



Theory and Model Building in Psychology

Edited by
Fabian Hutmacher · Alexander Nicolai Wendt

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Fabian Hutmacher

University of Würzburg, Würzburg, Germany

Alexander Nicolai Wendt

Sigmund Freud University, Vienna, Austria

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Preface

The products of scientific creativity are manifold, encompassing not only innovative machinery and experimental techniques, but, above all, ideas. Psychological scientists do not tire of engaging with the complexity and depth of mental life by exploring effects, devising constructs, or reflecting on concepts, whether these involve subtle mechanisms of the cognitive apparatus or the plenitude of conscious life. Without a doubt, it requires diverse epistemic means to do justice to the intricacies of such an enigmatic subject matter as the psyche. The variety of methods in the discipline is only surpassed by the diversity of psychological theories and models. Yet, it is this very diversity that poses a challenge to psychological discourse. What constitutes either a theory or a model remains unclear, given the multiplicity of approaches and traditions, and with it, psychology is in danger of losing orientation.

This has become particularly apparent during the recent crisis debates in psychology (see, e.g., Hutmacher & Franz, 2025). More specifically, it has been observed that many psychological studies cannot be replicated (*replication crisis*; Open Science Collaboration, 2015), that many psychological theories are local micro-theories that are rarely integrated into coherent wholes (*theory crisis*; Eronen & Bringmann, 2021; Oberauer & Lewandowsky, 2019), and that psychological research has not adequately captured the diversity and variability of psychological phenomena (*universality crisis*; Henrich et al., 2010). These crisis debates have resulted in a host of proposals and counterproposals that can be hard to follow. This book aims to counteract conceptual disorientation by surveying the plurality of attempts at theory and model building in psychology. Rather than subjecting the often-disconnected ways of psychological thought to a unitary perspective, it seeks to offer a platform that allows the discipline's different research cultures to speak for themselves. The collection of texts provides insight into the polyphony of meta-

psychology. The interrelatedness of these perspectives uncovers a clearer picture of the status quo in theoretical psychology.

The book includes contributions to three complementary aspects of the problem of understanding theory and model building in psychology. Chapters in the **first part** (“Foundational Issues in Theory and Model Building”) establish this problem’s conceptual and philosophical outline. By contextualizing the debate with historical and interdisciplinary arguments, these chapters provide a framework for further discussion. The **second part** (“Current Challenges for Theory and Model Building”) is dedicated to the challenges that emerge when inquiring into the nature of theory and model building in psychology. Psychology faces a unique set of meta-scientific concerns that any attempt at solution needs to recognize. Such attempts are discussed in the **third part** (“Possible Solutions for Theory and Model Building”) of the book. It contains both historical and contemporary viewpoints as well as critical and constructive lines of reasoning. Yet again, it is the resonance between the different parts of psychology that gives rise to the whole. The book concludes with a final chapter, in which we situate the individual contributions within the current debate on theory and model building in psychology.

The research traditions in psychology are not only distributed along ideal dimensions but also correlate with institutional and regional spheres. For this reason, the contributors to this collected volume bring unique backgrounds in personal biography and scientific affiliation alike. To advance the psychological enterprise, a collective effort is necessary. Even in its early stages—for example, at the earliest universities in medieval Europe—science became an institution of international understanding and cooperation. True to this spirit, new ideas for the foundations of psychology will require an effort of solidarity. This is to say that diverse standpoints should recognize one another to achieve progress in dialogue—even if this requires dispute at times.

The very beginning of this project was promoted by Gerd Jüttemann (1933–2023) who was an inspiration for both of us. In the German-

speaking psychological community, Jüttemann was a promoter of dialogue and sought to bring together inventive voices even if they contradicted his own. Unfortunately, Jüttemann was not able to see where his initial encouragement would ultimately take us. We wish to contribute to his legacy by upholding a pluralistic ideal of our discipline.

We would like to thank our contributors for all the thought-provoking ideas that they present in the different chapters. The book was advanced thanks to invaluable help. We wish to thank Steffen Kluck (Rostock) for helping with the peer review. We would also like to thank our student research assistants, Benedikt Luther and Franziska Schenk, for their support during the entire process of putting this volume together. Liam Inscoe-Jones from Palgrave Macmillan and Naveen Dass from Springer Nature provided reliable and friendly assistance in all editorial matters. Finally, we wish to thank our wives, Aliz and Mariangel, for their unfailing support.

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Fabian Hutmacher
Alexander Nicolai Wendt
Vienna, Austria
Würzburg, Germany
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Notes on Contributors

Barbara L. Andersen

is a Distinguished University Professor at The Ohio State University (OSU), USA. Trained as a clinical psychologist with expertise in behavioral medicine, her appointment is in the Department of Psychology. Also, a member of the OSU Comprehensive Cancer Center (CCC), her research portfolio focuses on biobehavioral aspects of cancer, studies of psychological mechanisms leading to changes in immunity, inflammation, and poor health outcomes, and psychological intervention development.

Matthias Borgstede

studied psychology at the University of Technology in Braunschweig, Germany. He works as a Senior Lecturer at the University of Bamberg, Germany, where he teaches research methodology and statistics. His research focuses on the methodological foundations of psychological measurement and the development of formal theory in the behavioral sciences.

Laura F. Bringmann

is an Associate Professor at the Department of Psychometrics and Statistics, University of Groningen, Netherlands, and heads the LaBlab (Laura Bringmann's intensive longitudinal data lab; <https://www.laurabringmannlab.com/>). She has authored over 50 articles in journals such as *Psychological Review*, *World Psychiatry*, *Psychological Methods*, and *Current Directions in Psychological Science*. Her research focuses on bridging the gap between fields such as statistics, philosophy, methodology, and clinical psychology. She received an NWO VENI grant (2019), and initiated and organizes the Measurement Is the New Black

“MITNB” consortium, a group of scientists interested in improving the validity of Experience Sampling Methods (ESM).

Carlos Cornejo

is a Full Professor at the Pontificia Universidad Católica de Chile. He earned his PhD from the University of Cologne, Germany, and completed postdoctoral training at the University of Heidelberg, Germany. His research and teaching focus on speech, human interaction, and the history and philosophy of psychology. He is the founder and director of the Laboratory of Language, Interaction, and Phenomenology (LIF) at the Pontificia Universidad Católica de Chile. His most recent book *Forgotten Streams in the History of 19th Century German Psychology*, co-edited with Cristián Hernández Maturana, was published in two volumes in 2025 by Springer.

Paul De Boeck

was trained in mathematical psychology and personality psychology and became a psychometrician and quantitative psychologist in his academic career. He is Emeritus Professor of the K.U. Leuven, Belgium, and The Ohio State University, USA. He is a former president of the Psychometric Society and the 2021 winner of the lifetime achievement award of the same society. His published work concerns intelligence, personality, explanatory measurement, a variety of random effects (e.g., random item effects), IRTree models, replication and psychological construct issues.

Maarten Derksen

is Associate Professor for Theory and History of Psychology at the University of Groningen, Netherlands. He is the author of *Histories of*

Human Engineering: Tact and Technology (2017, Cambridge University Press) in which he explored the dynamic between control and resistance in techniques to influence human behavior. He has also written about the crisis of confidence in psychology and is involved in an ethnographic study of replication research.

Berna Devezer

is Professor of Marketing at the University of Idaho, USA. She has an MS in statistics and a PhD in marketing from Washington State University, USA. Devezer is working on metascience methodology and theory with a specific focus on studying the interplay between scientific and statistical inference, replications, and reproducibility of scientific results from a statistical modeling perspective. Devezer's collaborative and interdisciplinary research program integrates insights from the history and philosophy of science with theoretical tools and modeling frameworks grounded in mathematical statistics.

Gesa S. Duden

is a Postdoctoral Researcher and Adjunct Assistant Professor at Concordia University in Montréal, Canada. She is a cultural psychologist whose research explores the cultural and systemic embeddedness of the human psyche, with a focus on sense of belonging, urban life, and mental health. Her work draws on interdisciplinary and mixed-methods approaches to investigate how migrants and other minoritized groups experience, interpret, and respond to their surroundings in everyday urban environments. She has conducted research in Germany, Ireland, Brazil, and Canada, including projects on migration, identity, and spatial belonging.

Ruben Ellinghaus

is working as a Postdoctoral Researcher at the FernUniversität in Hagen, Germany. He is a German psychologist and former doctoral student of Rolf Ulrich. He has previously held academic positions in Tübingen, Germany, and New Zealand. His research methods span the fields of mental chronometry, psychophysics, and cognitive modeling. For example, he has worked on testing empirical predictions from models that specify the processing of conflicting information, as well as on classic problems from psychophysics, such as the mechanisms underlying temporal context effects in magnitude judgments. Additionally, he is interested in the history and philosophy of psychology.

Markus I. Eronen

is an Associate Professor at the Department of Theoretical Philosophy, University of Groningen, Netherlands. He received his PhD from the University of Osnabrück, Germany, and after that worked as a postdoctoral researcher in Germany and Belgium. His research topics include philosophy of mental disorders, levels and the nature of hierarchical organization, and theory construction in psychology. He has written over 25 articles in journals such as *Philosophy of Science*, *British Journal for the Philosophy of Science*, *Perspectives on Psychological Science*, *Biology & Philosophy*, and *Philosophical Psychology*, and is an associate editor of *Theory & Psychology*.

Uljana Feest

is Professor of Philosophy at Leibniz University, Hannover, Germany. In her research, she specializes in the philosophy and history of the cognitive and behavioral sciences, with special focus on experimental practices of knowledge generation in these fields. She uses an approach that integrates the history of psychology with the philosophy of

psychology. Feest is the author of *Operationism in Psychology. An Epistemology of Exploration* (University of Chicago Press, 2025). She has recently begun to work on a new project, analyzing the history, investigative practices, and social relevance of personality research.

Joachim Funke

is Full Professor (retired) of Theoretical and Cognitive Psychology at the Department of Psychology, Heidelberg University, Germany. His primary interests are issues within problem solving and thinking research. He is one of the founders of the European approach to the study of complex problem processes using computer-simulated microworlds.

Nicolò Gaj

is an Associate Professor in the Philosophy of Science at the Catholic University of the Sacred Heart in Milan, Italy, and a licensed clinical psychologist. His work focuses on philosophical and meta-theoretical issues that inform and advance the discipline of psychology. His research interests span the theoretical foundations of psychological practice and the fragmentation within the field. On these themes, he authored *Unity and Fragmentation in Psychology. The Philosophical and Methodological Roots of the Discipline* (2016, Routledge). His works have been published in journals specializing in the (meta-)theoretical analysis of psychological issues, such as *Theory & Psychology*, and *Philosophical Psychology*.

Marcia Gralha

is an independent scholar of the Unified Theory of Knowledge (UTOK) and has authored professional publications on its application to

psychotherapy and curates the development of UTOK in Brazil. She obtained her M.A. in clinical psychology from Western Carolina University, North Carolina, USA, and her B.A. in psychology from the University of South Florida, USA.

Gregg Henriques

Professor of Graduate Psychology at James Madison University, USA, is the founder of UTOK, the Unified Theory of Knowledge, which is a new system of understanding that bridges the sciences and humanities into a coherent whole. He is the author of three books, numerous professional papers, and has a popular blog on Psychology Today, called *Theory of Knowledge*. He teaches classes in psychotherapy, personality, personality assessment, cognitive psychology, history and systems, and social psychology.

Fabian Hutmacher

is a Lecturer at the University of Würzburg, Germany. In his research, he combines empirical methods and (meta-)theoretical reflections to arrive at a better understanding of individual and collective processes of reasoning and remembering. He also holds a keen interest in the philosophy and history of psychology. He is the editor of two books and his works have been published in leading general interest journals such as *American Psychologist*, *Psychological Inquiry*, and *Psychological Science* as well as in journals specializing in meta-theoretical questions, such as *Theory & Psychology* and the *Journal of Theoretical and Philosophical Psychology*.

Lisa M. Osbeck

is Professor of Psychology at the University of West Georgia, USA. She is a Fellow of the American Psychological Association and the Center for Philosophy of Science, University of Pittsburgh. Works include *Ever not Quite: Pluralism(s) in William James and Contemporary Psychology*, with Saulo Araujo (Cambridge, 2023); *Psychological Studies of Science and Technology*, with Kieran O'Doherty, Ernst Schraube, and Jeffrey Yen (2019, Palgrave); *Values in Psychological Science* (2018, Cambridge); *Rational Intuition*, with Barbara Held (2014, Cambridge), and *Science as Psychology: Sense-Making and Identity in Science Practice*, with Nancy Nersessian, Kareen Malone, and Wendy Newstetter (Cambridge, 2010).

Jolynn Pek

is a Professor in the Department of Psychology at The Ohio State University, USA, where she directs the quantitative psychology graduate program. Her research focuses on quantifying different sources of uncertainty throughout the scientific process—including defining psychological constructs, power analysis in study design, modeling data, and research reporting. Her methodological work is motivated by the goal of bridging the gap between theories about modeling data, statistical innovation, and practical application, supporting the adoption of rigorous, accessible methods in psychological science.

Martin Peper

is Full Professor of Neuropsychology at the Faculty of Psychology, University of Marburg, Germany, since 2007. He completed his studies in psychology at the University of Heidelberg (Dr. phil., 1991). In 1992, he was Visiting Scholar at the Center for Neural Science, New York University (J.E. LeDoux). As Assistant Professor in the Section of Personality Psychology, Experimental and Biological Psychology (J. Fahrenberg) at the Department of Psychology, University of Freiburg, Germany, he completed his habilitation in 1997. In 2004, he earned a

Dr. rer. medic. degree from the Charité–Medical Faculty of the Humboldt University of Berlin.

Noemí Pizarroso López

holds a PhD in psychology from UCM (Madrid) and Université Paris V (France), and teaches history of psychology at UNED, Spain. Her research focuses on early twentieth-century French psychology, especially the tradition of historical psychology linked to Ignace Meyerson. She explores neglected figures such as Delacroix, Pradines, and Dambuyant, emphasizing the plurality of psychological approaches and their ties to disciplines like sociology, history, ethnology, and philosophy. She is particularly interested in how this tradition is engaged with a genealogical conception of the subject and experience, and how it resonates with contemporary debates in historiography and cultural psychology.

Pablo Rojas

is a social scientist with a background in psychology, anthropology, philosophy, and music. His research examines skilled sensory practices across various domains, including aeronautics, education, music, and wine appreciation. In recent years, he has led research and development work for the wine industry, focusing on consumption research and new product development. He holds a PhD in Social Sciences from The School of Advanced Studies in the Social Sciences (EHESS), Paris, France. His book, *Reprising Craftsmanship: An Expressive Approach to Skill*, is published by Springer.

Stephan Schleim

is a cognitive scientist and Associate Professor of Theoretical Psychology at the University of Groningen, Netherlands. His research focuses on the theory and science communication of psychology, neuroscience, and psychiatry. His chapter on the development of the concept of addiction brings together his interests in substance use and psychiatry. In addition to his academic work, he has been active as a science writer for over 20 years and frequently comments on scientific developments in the media. His most recent book is *Science and Free Will: Neurophilosophical Controversies and What It Means to Be Human* (Springer, 2024).

Kathleen L. Slaney

is a Professor in the Department of Psychology at Simon Fraser University, Burnaby, Canada. Her research has focused primarily on psychological metascience and conceptual analysis of psychological concepts. A Fellow of the American Psychological Association, she is author of *Validating Psychological Constructs* (2017), and co-editor of *A Wittgensteinian Perspective on the Use of Conceptual Analysis in Psychology* (2013), *The Wiley Handbook of Theoretical and Philosophical Psychology* (2015), and *Problematic Research Practices and Inertia in Scientific Psychology* (2021).

Henderikus J. Stam

is Professor Emeritus of Psychology at the University of Calgary, Canada, where he taught courses in the history of psychology and theoretical psychology. He recently co-authored *The Indispensability of Phenomenology, Experiment and History: Life and Work of Johannes Linschoten* with René van Hezewijk. Founding editor of *Theory & Psychology* (now Editor Emeritus), he is a former president of APA's Division 26 (Society for the History of Psychology), Division 24 (Society for Theoretical and Philosophical Psychology), and the International

Society for Theoretical Psychology. In 2015, he was awarded the Joseph Gittler prize by the American Psychological Foundation for scholarly contributions to the philosophical foundations of psychological knowledge.

Timo Storck

is Professor of Clinical Psychology and Psychotherapy at Psychologische Hochschule Berlin, Germany. He works in private psychoanalytic practice in Berlin and Heidelberg. In his research, he focuses on conceptual research in psychotherapy and psychoanalysis, film psychoanalysis, psychoanalysis and society, and psychoanalytic future studies.

Daniel Sullivan

is the Director of the Social and Personality Psychology Program at the University of Arizona in Tucson, USA. His research focuses on how cultural factors influence experiences of and responses to threats. He is the author of *Cultural-Existential Psychology* (2016) from Cambridge University Press.

Momoka Sunohara

is a cultural psychologist from Japan who earned her doctorate in Psychology at Concordia University in Montréal, Canada. Her research examines the universality and cultural specificity of psychological functions, with particular attention to the cultural shaping of mental health and psychological disorders. She has studied beliefs about mental illness in Japan and Canada, immigration, acculturation, psychosocial adaptation, and the mental health experiences of underrepresented minorities. She uses interdisciplinary and mixed-

methods approaches to challenge mainstream psychological theories and has led cross-cultural projects promoting international research collaborations and EDI conversations.

Brandon M. Turner

is a Professor in the Department of Psychology at The Ohio State University, USA. He received a B.S. from Missouri State University in mathematics and psychology in 2008, an MAS in statistics from The Ohio State University in 2010, and a Ph.D. from The Ohio State University in 2011. He spent one year as a postdoctoral researcher at the University of California, Irvine, and two years as a postdoctoral fellow at Stanford University. His research interests include dynamic models of cognition and decision making, methods for performing likelihood-free and likelihood-informed Bayesian inference, and unifying behavioral and neural explanations of cognition.

Rolf Ulrich

is a German psychologist whose research focuses on time perception, psychophysics, language and thought, as well as the mathematical modeling of cognitive processes, and the interaction between linguistic and non-linguistic cognitive processes. Additionally, he develops statistical and mathematical methods for various applications in empirical research. As a Heisenberg scholar, he conducted research at the University of California, San Diego, before holding professorships at Konstanz, Wuppertal, and Tübingen.

Hannes Wendler

studied psychology and philosophy in Innsbruck, Austria, and Heidelberg, Germany, with a research stay in Cambridge, UK. He is a

Mercator Fellow at the University of Cologne, Germany, a board member of the Association for Philosophy and Psychology, and a candidate in psychodynamic psychotherapy at the Institute for Psychoanalysis and Psychotherapy in the Rhineland. His research focuses on clinical psychology, theoretical psychology, phenomenology, value theory, and interdisciplinary work between philosophy and psychology.

Alexander Nicolai Wendt

is a Lecturer at the Sigmund Freud University, Vienna, Austria, and the Ruprecht Karls University of Heidelberg, Germany. Besides experimental cognitive psychology, he engages with foundational problems of psychology concerning the discipline's history, meta-theory, and methodology. Recent books are dedicated to these concerns: *What is Psychology About? The Philosophical Foundations of its Subject-Matter* (Palgrave Macmillan, 2025), *Phenomenological Psychology as Rigorous Science* (Springer Nature, 2024).

Frederick J. Wertz

Duquesne University PhD, is Professor Emeritus at Fordham University, USA. Former editor of the *Journal of Phenomenological Psychology*, he was co-author of *Five Ways of Doing Qualitative Analysis: Phenomenological Psychology, Grounded Theory, Discourse Analysis, Narrative Research, and Intuitive Inquiry* (2011). His scholarship has focused on the philosophical foundations and history of psychology, phenomenology, comparative research methodology, psychoanalysis, and indigenous psychologies. His research topics include perception, abnormality, criminal victimization, spirituality, and psychopathology. Now retired in Michigan, USA, he continues to work on scholarship, teaching, collaborations, and mentoring.

Part I

Foundational Issues in Theory and Model Building

1. On Theories and Models in Psychological Discourse: A Conceptual Approach

Kathleen L. Slaney¹ 

(¹) Simon Fraser University, Burnaby, BC, Canada

 **Kathleen L. Slaney**
Email: klslaney@sfu.ca

Keywords Theory building – Models – Psychology – Philosophy of science

Introduction

As with any science, two essential questions for psychological science are what constitutes good theory building and how are models and theories related to one another. Such questions, of course, presume that the meanings of the terms “theory” and “model” are reasonably well understood and shared, notwithstanding small departures in the specific definition one might invoke in a given usage of either term. Yet, scientific discourses—including those of psychological science—abound with references to both “theory” and “model,” and the presumption that we all mean the same thing when we use these terms might not be as obvious as it appears. Moreover, philosophers of science have written plenty on both theories and models and the roles they do or ought to play in science. Given there are both ordinary language and scientific meanings of both terms, I begin by briefly

exploring commonsense meanings, followed by a description of well-known philosophy of science accounts. Then I describe which senses of the concepts of theory and model, and which understandings of the relationship between them, are common in psychological science discourse, and whether these reflect or depart from well-understood philosophy of science accounts. The primary objective is to illuminate the rich conceptual terrain in which these two seemingly straightforward scientific concepts exist so that the core questions of what constitutes good theory building in psychology and what the relationship is between theories and models within psychological science can be constructively approached.

A Word on the Conceptual Analytic Approach

In examining a theoretical position or methodological approach, whether general or specific, clarification of the concepts central to that theoretical position or methodological approach is a necessary first step. One cannot adjudicate, for example, the success of a specific theory in explaining a given phenomenon, or whether an experimental model is representing well some aspect of the world under study, if the meanings of the concepts at play are ambiguous, unclear, or confusing. In the present context, this means that providing cogent and clear responses to the questions of what constitutes good theory building in psychology and how models and theories are related to one another requires some preliminary conceptual “housekeeping” of the focal concepts of “theory” and “model” such that the various senses of these concepts are explicated and the senses relevant to these questions are identified. There are many approaches that one could take to doing conceptual analytic work. Here, my approach is generally consistent with Wittgenstein’s later philosophy (Wittgenstein, [1953](#)), especially as interpreted by Wittgensteinian scholar Peter Hacker and his collaborators (e.g., Baker & Hacker, [1982](#), [2009](#); Bennett & Hacker, [2022](#)). In essence, the approach involves describing the “logical space” of a concept or set of concepts, including the uses of words and

expressions denoting the concept. It can be used to reveal ambiguous, inappropriate, or confusing uses of words and phrases, and serves the ultimate goal of clarifying what does and does not make sense with respect to a concept or set of concepts as used within a given context.

Ordinary Uses of “Theory” and “Model”

Although I will not spend much time dwelling on the ordinary language meanings of the terms “theory” and “model,” three points are worth raising. The first is simply that there do exist both ordinary and scientific senses of both concepts. The second is that it depends on the specific contexts in which these terms are used as to whether the scientific uses are parasitic upon (i.e., extend from) the ordinary sense or the reverse, and whether or not this is important to bear in mind in a given context. For example, what one makes of a casual reference to the human mind being modeled after a computer (e.g., “Humans are hardwired to recognize danger”) will differ from using the computer metaphor of mind as a theoretical model of cognition. Likewise, the popularity of the latter over the last several decades has influenced how lay-persons think and talk about the mind and the brain in relation to the computer metaphor. Third, scientific discourse includes instances of both ordinary and scientific senses of both concepts, and, again, whether or not this is important to bear in mind in a given context will depend on the intentions attached to a specific usage.

A full presentation of the grammatical landscape (or “logical space”) of the theory and model concepts is not possible here; however, it may be useful to provide a general ordinary language definition of each. The scientific senses of these terms will be the subject of the remainder of the chapter. Within ordinary parlance, “theory” refers to a perspective on, an account or explanation of some event or occurrence, or characteristic or quality of an object or phenomenon which is based on consideration of the facts, putative or undisputed. A theory is the “conceptual basis of a subject or area of study” and is often contrasted with practice (Oxford English Dictionary, [2024a](#)). It is a notion of

something to be done or the application of a method for doing it (e.g., “This should work, in theory”). Whereas the origins of the idea come from Ancient Greece (*θεωρία*), the origins of the English word go back to at least the sixteenth century, stemming from both the French *theorie* and Latin *theoria* (among others). The association of the notion of theory with science is long-standing; even very early usages were invoked in reference to scientific accounts of the cosmos or physical world. Common derivatives are “theoretical” (and the adverbial “theoretically”), “theorize,” and “theorist” (Oxford English Dictionary, [2024a](#)).

In common discourse, the relevant senses of the noun “model” generally refer to something that accurately resembles or represents something else, an imitation, a representation of an object or structure (e.g., a model train set), a prescribed or desired behavior (e.g., a model citizen), general comportment or course of action, an ideal version of an object or event (i.e., a prototype), or a simplified or idealized description of a complex system (Oxford English Dictionary, [2024b](#)). It derives from the French *modelle* and Latin *modellus* (Gerlee & Lundh, [2016](#); Oxford English Dictionary, [2024b](#)), and its uses also first appear in English text in the sixteenth century (Oxford English Dictionary, [2024b](#)). Its most common derivative is the transitive verb “to model” (Oxford English Dictionary, [2024b](#)).

Models of Scientific Theories and Theorizing

Although scientific theorizing has always been an integral part of science, explicit examinations of theory and theorizing and the existence of prescribed systems for theorizing did not become an independent area of inquiry until the establishment of philosophy of science as a separate branch of philosophy in the early twentieth century. Since that time, two major frameworks have dominated: the *syntactic view* and the *semantic view*.¹ A third view—the *pragmatic view*—differs from but shares in common with both the purpose of

providing a prescribed account of the composition and function of a scientific theory (Winther, [2021](#)).

The Syntactic View

The *syntactic view* (or “received view”²) originated in the early twentieth century through the philosophical movement of logical positivism.³ The formation of the movement was motivated in large part to address the “problem of theoretical terms,” which is that the truth or falsity of theoretical propositions (i.e., statements containing at least one theoretical term) cannot be unequivocally determined by direct reference to observables. Under this view, theoretical terms are held to be either meaningless computational devices or are fully reducible to observation terms (Slaney, [2001](#)).

Core members of the logical positivist movement—most notably Rudolf Carnap and Carl Hempel—sought to address this problem through the formalization of theories into “axiomatized sets of sentences in first-order predicate calculus” (Portides, [2017](#), p. 26). According to this approach, theories are conceived as “collections of sentences” wherein the sentences are composed of theoretical and observational language terms connected by logical operators. Propositions containing theoretical terms are interpretable only through the application of “correspondence rules,” that is, formal rules of inference or formal postulates that are presumed to establish a direct correspondence between the conclusions derived within a theory and observation statements. For example, a correspondence postulate could express a theoretical proposition about relative mass (a theoretical term) using the observation term “heavier than,” as measured in a chosen unit of weight (Carnap, [1956](#)).

The dominant model of theorizing within the syntactic view is the *hypothetico-deductive* (H-D) *model*. The essence of the logic of the H-D model is that theories are made up of theoretical postulates from which empirical statements can be deduced, translated into observation statements, and then compared with observations (Portides, [2017](#)). Theories are, thus construed, “sets of linguistic statements” or “logical

calculi” (Vorms, [2018](#), p. 199, p. 187). Moreover, they are “self-sufficient totalities,” or closed systems in which “a theory is all implicitly contained in its hypotheses” and “developing the theory only means deducing consequences from them” and determining if these predictions play out (Cellucci, [2016](#), p. 101).

By the mid-twentieth century, logical positivism and the H-D model of theorizing fell prey to continued failed attempts to make the model work in practice. Among the specific criticisms raised were that it failed to clearly specify how theories are initially formulated or developed (Cellucci, [2016](#)); that theories are underdetermined by observations (i.e., the same set of observations might confirm multiple different hypotheses); that it failed to clearly explicate the nature of theoretical terms and how they acquire meaning due to its emphasis on syntax at the cost of semantics (Portides, [2017](#)); and that theoretical predictions rarely turn out to be entailed unless supplemented with auxiliary hypotheses (Mills Boyd & Bogen, [2021](#)), which may only be valid within narrow applications of the theory (Gelfert, [2016](#)).

The Semantic View

The semantic view of theories arose in large part in critical response to the syntactic view propagated by logical positivists. Patrick Suppes (e.g., Suppes, [1967](#)), Frederick Suppe (e.g., Suppe, [1989](#)), Bas van Fraassen (e.g., van Fraassen, [1987](#)), and Ronald Giere (e.g., Giere, [1988](#)) are among the key proponents of this view. Unlike the syntactic view, according to which theories are sets of linguistic statements, proponents of the semantic view conceive of theories as “extra-linguistic” entities (Vorms, [2018](#), p. 199). More specifically, theories are defined as, or identified with, sets (or families) of models, wherein “model” is typically understood in a logico-mathematical sense (Vorms, [2018](#)). Whereas sentential logic forms the backbone of axiomatization of theory within the syntactic view, formal semantics and set theory does so for the semantic view. The focus is on the mathematical structure of theory as opposed to on the language of theory (Vorms, [2018](#)). To give a very general description (of an admittedly complex

notion), a semantic view of theory emphasizes the structural relationships among the axioms of theory, models of the theory, models of experiment, and models of data (Winther, [2021](#)).

Although one or another version of the semantic model of theorizing continues to dominate in current philosophy of science circles, several shortcomings of the semantic view have been identified, including difficulties in establishing formalized relationships between mathematical models (i.e., theories) and non-mathematical “target systems” (i.e., phenomena under study), reconciling fictional models with real objects and events, and treating scientific theories as fixed rather than the dynamic structures they are (Cellucci, [2016](#)).

The Pragmatic View

Despite their differences, the syntactic and semantic views are similar in that both are explicit attempts to formally reconstruct and axiomatize scientific theory and theory building (Vorms, [2018](#)). They also share several assumptions, including that theory is explicit, mathematical (or, at least, is best represented formally), abstract, systematic, readily individualizable, distinct from data and experiment, and fulfills explanatory and predictive functions (Winther, [2021](#), p. 31). A third perspective—the *pragmatic view*—embodies a quite different vision of the structure of scientific theories. Key proponents are Nancy Cartwright (e.g., Cartwright, [1983](#)) and Philip Kitcher (e.g., Kitcher, [1993](#)). The pragmatic view accepts that language and modeling are important to theorizing but no less so than are the problems, standards, skills, and values that are integrated into scientific practice. It is generally informed by the following theses: a) idealized theory may be too weak to ground predictive and explanatory work; b) theory structure is plural both in terms of the variegated components that make up theory and the different types of theory that actually operate in science; c) there are many nonformal aspects to theory structure (e.g., metaphors, analogies, values); d) the dynamics of theory structure include users, values, and purposes; and e) theory structure is continuous rather than dichotomous with practice (Winther, [2021](#), pp.

31–32). Together, these theses inform a view of scientific theorizing in which the focus switches from axiomatizing and identifying the structure of theories to the practices of science, including theorizing and modeling, where both are expanded to include nonformal components (e.g., metaphors, analogies, policy views). Proponents of the pragmatic view acknowledge “that scientists use and need different kinds of theories for a variety of purposes” and, thus, that there is “no one-size-fits-all structure of scientific theories” (Winther, [2021](#), p. 46).

In addition to these explicit models of theorizing, there are more general understandings among scientists of the notion of theory. For example, theories have been described as “unproven hunches,” “conceptual devices” (Suppe, [1998](#)); “repositories of scientific knowledge” (Vorms, [2018](#), p. 171); “a necessary part of science” (Thompson, [1962](#), p.22); attempts “to bind together in a systematic fashion the knowledge that one has of some particular aspect of the world of experience” (Ruse, [2005](#), p. 870); and “well-substantiated explanation[s] of some aspect of the natural world” (McComas, [2014](#)). Scientists—more so than most philosophers of science—embody a broader pragmatic stance as to their understandings of what theory consists of and are more likely to use the terms “theory,” “model,” and “hypothesis” interchangeably, most often with little consequence (Bailer-Jones, [2009](#)). It may well be a matter of debate whether scientists’ understandings of theory constitute accurate or useful characterizations. Although the current context is not the place for further elaboration of this, it is worth noting that philosophical accounts may not always intersect in obvious ways with more mundane understandings that are held by practicing scientists.

Models and Modeling in Science

To say that modeling within contemporary science is widespread is an understatement. So prolific are scientific models that Goodman (1968; as cited in Gelfert, [2016](#), p. 6) claimed: “Few terms are used in popular and scientific discourse more promiscuously than model.” Some have

even asserted that the use of models by the likes of Galileo and Newton played a significant role in bringing about the modern scientific revolution (Borbone & Brzechczyn, [2016](#)). Many different types of models are employed within science, as broad and far-reaching as it is. Models may be physical, mechanical, conceptual, graphical, mathematical, and, even, fictional. The natures of models may be concrete or abstract, theoretical, analogical, or scale. A model could be a replica of some phenomenon, or it could consist in a set of plans or a design for something. It might express a formal relationship between a structure of some sort and “target system,” that is, whatever part or feature of the world is under study. In that respect, what is being modeled may be a physical object or structure of some sort, or it may be an abstract entity, such as a relational property. Different types of models will differ with respect to both how and the extent to which they are representations, including the kinds of things they represent.

Within scientific discourses, theories and models are “habitually contrasted” (Bailer-Jones, [2009](#), p. 3) and are generally viewed as serving distinct functions within science. However, the relationship between theories and models is characterized quite differently within each of the major frameworks described above.⁴ Proponents of the syntactic view disagree on the importance of models, including whether they serve any useful function at all (Frigg & Hartmann, [2020](#)). Advocates of the syntactic view see models primarily as a means for interpreting formalized theory in concrete applications (Gelfert, [2016](#)) by the use of correspondence rules that allow the theory to be compared directly with observable phenomena (Borbone & Brzechczyn, [2016](#)). Importantly, according to the syntactic view, a good theory renders models theoretically and practically redundant and, hence, they are viewed as having only temporary benefit and, for some, no benefit at all (Bailer-Jones, [2009](#)). Models are understood as playing a meta-mathematical role, as “interpretative structures of subsets of sentences of the formal calculus,” and, thus, play no representational role outside of the formalism (Portides, [2017](#), p. 25). They merely “instantiate the axioms of a theory” (Gelfert, [2016](#), p. 8). Specifically, a

model is an interpretation of a theoretical postulate in which what is stated in the postulate is true (Vorms, [2018](#)).

Conversely, according to the semantic view, models are the *constituents* of theories and, thus, are “the building blocks of which theories are made up” (Frigg & Hartmann, [2020](#), p. 31). As such, models determine the content of theories (Vorms, [2018](#)). However, whereas theories define idealized systems, models “constitute a structure that satisfies these definitions” (Borbone & Brzechczyn, [2016](#), p. 5). Within the semantic view, models are viewed as either “state-spaces” or set/model-theoretic structures (Frigg & Hartmann, [2020](#); Winther, [2021](#)) “which represent their target systems in virtue of there being some kind of *morphism* (isomorphism, partial isomorphism, homomorphism, ...) between the two” (Frigg & Nguyen, [2021](#), p. 23; emphasis in original). Models are seen as *mediating* the complicated terrain between abstract theories and concrete empirical facts. Thus, within the semantic view, models serve both as integral parts of theories and as instruments for representing the specific phenomena to which a theory pertains. Typically, this involves contrasting models of data to models of theory (Portides, [2017](#)). More generally, the interpretation of the axioms of theory is understood in light of relationships among a hierarchy of models: from models of theory to models of experiment and then to models of data (i.e., statistical models; Winther, [2021](#)).

In contrast, within the pragmatic view, as with theories, no rigid criteria are placed on scientific models. Rather, models are accepted as being plural both in terms of the components of which they consist (which will vary across disciplines) and in the many different types of models that are used in science, not all of which are mathematical, “or even ideally recast as mathematical” (Winther, [2021](#), p. 35). For proponents of the pragmatic view, what a scientific model is and how it relates to the relevant theory depend in part on the purpose of the research and the beliefs and intentions of the scientist (Gelfert, [2016](#)).

Also important to note is that whereas proponents of the syntactic view emphasized the role of theory over that of models within science,

advocates of both the semantic and pragmatic views put models at the center of their accounts of theory, albeit based on very different reasoning. Whereas in the syntactic view models are regarded as providing a means for interpreting a formal system, in both the semantic and pragmatic views they are seen as essential tools for representing features of the world (Gelfert, [2016](#)).

As with theories, there are general understandings among scientists of what constitutes a scientific model and of the relationship between theories and models. For example, Bailer-Jones ([2002](#)) found that physicists viewed models as being essential to science, but they defined and described models in different ways. On the relationship between theories and models, scientists adopt a diversity of perspectives including there is no real difference between theories and models; models will eventually become theories when consistently confirmed by empirical test; models are necessarily oversimplified and incomplete, whereas theories are the most accurate and complete accounts of a given target of study; theories are more general than models; and modeling involves applying general theories to specific cases.

The Conceptual Landscape of Scientific Theories and Models

Given these (admittedly, truncated) descriptions of models *of* (theorizing in) science and models *in* science, some core features of the theory and model concepts as understood within science come to light. Some of these features are more clearly aligned with one or more of the three viewpoints on theory building summarized above; where relevant, this will be indicated in parentheses. Theories are higher-order descriptions or abstractions of specific states of affairs (for syntactic and semantic views). They enable description, prediction, and (for semantic and pragmatic views) explanation of phenomena. They are tools that enable deductive, inductive, and (for the semantic and pragmatic views) abductive inferences, from abstract principles to

particulars and the reverse. As higher-order descriptions, theories are representational of theoretical generalities and abstract principles and structural features of reality (especially for the semantic view). They are testable and, once well-substantiated, function as approximations of reality; however, they are ever-revisable in the face of compelling new evidence (especially for the semantic view). In terms of the relationship between theory and knowledge, theories play a repository role in representing the accumulation of knowledge with respect to a domain of study and, as such, are broad in scope. They are essential to science but not always directly useful on their own in making sense of observed phenomena. They may be subject to formalization or axiomatization (for syntactic and semantic views) and, as such, are often contrasted with concrete observations or experiences.

Like theories, models are “epistemic” tools.⁵ They may be formal (syntactic and semantic views), derived from theory (syntactic view), and based in analogy⁶ but need not be any of these (semantic and pragmatic views). As with theories, they are representational—this being one of their core features: all or most models serve to represent in one way or another (formally or iconically) some aspect of the world, or “target system.” They stand in for something else—a real object, event, process, or complex system—but are more closely connected to reality than theories (especially for the semantic view). They fulfill several roles, including simplifying complex theory (semantic and pragmatic); capturing core features of some domain; providing insights or mental aids; fulfilling heuristic or aesthetic functions (syntactic and pragmatic views); providing a bridge between theory and reality (semantic view). However, unlike theories which are approximations to truth (especially for the semantic view), models are always false, even if useful tools (especially for the pragmatic view). Models have varying ontologies, including physical, abstract, formal, and fictional. Models, although also essential to science, are narrower in scope than theories.

As noted, there may be differences between philosophical accounts of the relation between theories and models and practical understandings of scientists (e.g., Bailer-Jones, [2002](#)). Whereas both

theories and models have been put under the microscope by philosophers of science, theory and model use within science is pretty much taken for granted in contemporary science (Frigg & Hartmann, [2020](#)). Psychology is no exception and, like all scientific disciplines, both are core parts of the day-to-day activities of psychological researchers.

Theory and Models Within Psychology

In the final section of this chapter, I will describe some of the ways in which the theory and model concepts get put to use within psychology,^{[7](#)} with the aim of illuminating where and where not psychological researchers' uses of these terms reflect the philosophical frameworks described earlier in the chapter. As such, I remain agnostic on the questions of what constitutes good theory building in psychology and on precisely how theories and models are related. I leave these questions to some of the other contributors of this volume. My focus is, instead, on how these concepts are *already used* within psychological science discourse and on attempting to illuminate some of the understandings psychological researchers might have regarding the relationship between theories and models.

The idea of theory extends to several different subcategories within psychology. There are “grand theories” associated with the broad “schools” of psychology that have a place in the history of the discipline. The most commonly cited examples include psychodynamic theory, behavioral theory, humanistic theory, and cognitive theory. These are not so much specific theories but more so broad frameworks that provide a foundation for the development of more explicit theories regarding particular psychological phenomena under study (e.g., hysteria, classical or operant conditioning, self-realization, attentional processes). There are also the broad categories of theory associated with the larger subdisciplines of psychology such as developmental, social, personality, clinical and counseling, cognitive neuroscience, and forensic. Of course, there also exists a vast and ever-growing collection

of substantive theories falling within these and other subdisciplinary boundaries. These may be broad (e.g., attachment theory, social identity theory, trait theory, dissonance theory, rational choice theory) or narrow (e.g., reinforcement sensitivity theory, fuzzy trace theory, conflict-monitoring theory) in scope. Meta-theory and methodological theory also make up a large category of both broad (e.g., psychometric theory, validity theory, experimental design theory, sampling theory, statistical theory, grounded theory, ethnographic theory, phenomenological theory, critical theory) and specific (e.g., generalized Gaussian signal detection theory, Bayesian item response theory, latent state-trait theory, constructivist grounded theory, autoethnography) theories.

As with “theory,” the term “model” gets heavy use in psychological discourse. However, the many references to models are more difficult to categorize in terms of broad classes. However, it might be useful to think of models as falling in one or more of the following categories: *theoretical*, *methodological*, and what Harré ([1970](#), [1985](#)) called “real” or *iconic* models.⁸ In the first category, I include both general and specific *theoretical models*. Examples of general theoretical models include such models as connectionist models of cognitive processing, models of prosocial behavior, attachment models, and causal models. A small selection of specific theoretical models includes examples such as the need-threat model of interpersonal relations, conflict-control model of dual tasking, integrated self-categorization model of autism, and actor-partner model of social behavior. Construed in this way, theoretical models are really just theories which, as described above, range in scope and specificity. In other words, in the case of theoretical models, “model” is used pretty much synonymously with “theory.”

Methodological models are many but may be roughly distinguished in terms of a few broad categories. *Study design models* capture the procedures involved for collecting data (quantitative or qualitative) relevant to the phenomena under study. Examples include randomized designs of various types, cross-sectional designs, survey designs, nested factorial designs, clinical trials, naturalistic observation, case studies,

cohort studies, interviews, ethnographic research, focus groups, and many others. *Psychological measurement models* include both overarching frameworks and a large subset of statistical models that may be used to assess (or validate) one or more of the psychometric properties of a psychological measurement instrument or set of scores produced in administering a measure. These general frameworks have adhered to one of three measurement paradigms: the *representational*, the *operational*, and the *classical* (Michell, [1986](#)). For the most part, in practice these frameworks have hovered in the backdrop of most psychometric assessment, which has been less concerned with justifying the quantification per se of psychological attributes (behaviors, processes, etc.) and more concerned with using psychometric models to determine if psychological measurement instruments are performing as they are intended to, that is, are producing valid and reliable data used to test substantive hypotheses (see Slaney & Maraun, [2008](#) for common examples). Whereas most psychometric models can be classified as falling within one of two broad frameworks, namely, *classical test theory* (e.g., true-score model) or *modern test theory* (e.g., latent class or latent trait item response models, e.g., Rasch, two- and three-parameter, graded response), others do not fall neatly within one or the other category (e.g., multi-trait multi-method models, generalizability models). *Statistical models* constitute perhaps the largest category of methodological models and, in fact, subsume psychometric models. This category includes the vast class of general linear and nonlinear statistical models that are used to test hypotheses about individual and group differences, causal mechanisms, cognitive and neural processes, latent structures underlying multivariate distributions of sets of measured variables, change mechanisms, class membership, among others. Common examples include but are certainly not limited to analysis of variance (ANOVA) models, linear and nonlinear regression models, Bayesian models, structural equation models, multilevel models, mediation and moderation models, clustering models, machine learning models, and computational models.

Iconic models make up a third broad category of models used in science. Harré ([1985](#), p. 174) defined this type of model as a “real or imagined thing, or process, which behaves similarly to some other thing or process, or in some other way than its behaviour is similar to it.” Examples from psychological science include physical or diagrammatic models of the human brain and nervous system, pictorial illustrations of specific theories (e.g., Maslow’s hierarchy of needs, the “iceberg” model of the psychoanalytic theory of the unconscious, stages of developmental attachment model, multi-store model of memory), illustrations of psychological processes (e.g., attention, perception, learning), and path models of causal relations between “manifest” (i.e., measured) and the “latent” (i.e., unmeasured) variables that putatively “underlie” them.

An obvious question for the current purposes is whether these common uses of “theory” and “model” within psychological research discourse align with the philosophical viewpoints described above. The neo-behavioristic theory of Clark Hull (among others) is one historical example of an approach to theorizing that reflects elements of the syntactic view, in particular his use of formal operational definitions of theoretical terms (e.g., “drive,” “habit strength”) and the hypothetico-deductive approach to theory building. Although neo-behavioral approaches are well past their prime in psychology today, the commonplace operationalization of psychological constructs and deducing testable hypotheses from general theory betray the continued influence of the syntactic view of theories. Formalization of theory in terms of mathematical models, especially in subdisciplines such as mathematical psychology and many areas of cognitive science, is reflective of the semantic viewpoint, as is the common approach of representing the axioms and principles of theory into experimental models and then data models (i.e., statistical) to test concrete applications of the theory. Recently, there is a growing call among psychological researchers to strengthen psychological theory through the incorporation of formal theory (e.g., Eronen & Romeijn, [2020](#); Haslbeck et al., [2022](#); Borgstede & Eggert, [2023](#); Milner et al., [2020](#);

Smaldino, [2020](#)). However, whether these and similar efforts will lead to broad uptake of the semantic approach across the discipline remains to be seen. Moreover, it is also not always obvious whether the type of formal theory being advanced is more consistent with the syntactic or the semantic viewpoint. Likewise, although psychological science has no shortage of mathematical models (especially data models, which constitute some kind of mathematical paraphrase of theoretical propositions), clear adherence to either the syntactic or semantic conceptions of the relation between theories and models is often not obvious from the common ways psychological researchers use the terms “theory” and “model.” This ambiguity is exacerbated by the fact a given model can serve multiple roles at the same time. For example, a path diagram might function as a theoretical model, a methodological model, and an iconic model with the arrows potentially indicating theorized causal relations, estimates of parameters of a statistical model based on aggregated data, or an illustration of the complex interconnections between study variables and the phenomenon under study. This is not necessarily a problem, but it does complicate the ability to ascertain whether and where the philosophical accounts of theory building and of the relation between theories and models are reflected in psychological science. In fact, if I had to characterize psychology in terms of only one of the three views described above, I believe the pragmatic view best represents the plurality of uses of the terms “theory” and “model” within psychological science discourse, including syntactic and semantic senses. However, it is likely that this has more to do both with the breadth and diversity of psychology’s subject matter and how accommodating the pragmatic view is, as opposed to reflecting strong explicit commitments among psychological researchers to the pragmatic view as an explicit framework for theorizing and the role of models therein.

Conclusion

As noted from the outset, my aim is not to form a response to the questions of what constitutes good theory building in psychology or what is the relationship between theories and models. Rather, my goal has been to present some philosophical meanings that have been given to scientific conceptions of theories and models and juxtapose these with how psychological researchers understand theories and models, as reflected in their uses of the terms “theory” and “model.” In closing, a few points are worth raising. First, elements of all the three philosophical accounts of models of science—i.e., the syntactic, semantic, and pragmatic viewpoints—appear in psychological science discourses, at least implicitly. For instance, the legacy of the syntactic view continues to be seen in the ways in which specific hypotheses are deduced from general theory and put to empirical test. The ability to make accurate predictions also continues to be a major desideratum of some domains of psychological science (e.g., risk assessment). From the semantic view, the emphasis on representing real features of a target system through formal relations among theories, experiment, and data has become the methodological backbone of many commonly employed methods. The inescapability of values within science and implications of research findings are both now widely accepted research principles that are heavily informed by the pragmatic view.

Second, psychological scientists’ understandings of the meanings of the terms “theory” and “model” appear to more often be tacit and implicit rather than clearly aligned with a specific philosophical account. Moreover, together, these tacit and implied understandings extend to a quite broad set of potential meanings of “theory” and “model” that might provide useful insights when attempting to narrow in on what theory building in psychology is and on what the relationship between theories and models is.

Third, the subject matter of psychology is vast and diverse. Really, it is better to consider psychology to be an umbrella of subject *matters* (plural). As such, establishing a one-size-fits-all account of psychological theory building and the relationship between theories and models might be challenged from the start. Embracing pluralism in

both how we conceive of theorizing and of the tools (including models) we use to theorize about or otherwise understand the psychological subject matter at hand might be a more realistic goal (see Hutmacher & Franz, [2025](#) for a current example). This might facilitate the use of theory and models in ways that are epistemically well-suited to the research questions at hand, while also allowing scientists to better acknowledge and accommodate the unavoidable values that are woven throughout research, as well as the pragmatic realities of doing research.

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Footnotes

¹ Although these viewpoints are frequently presented as *the* major models of science, it should be noted the viewpoints in the current work are not presumed to be exhaustive of all the different accounts of theory and theorizing that have been given. Moreover, while these perspectives have gained significant exposure in English-language philosophy of science, other accounts have been given in other languages (e.g., König & Pulte, [1998](#)).

² As it would be called by early proponents of the semantic view. Also, although the general premises of both the syntactic and semantic views can be fairly clearly distinguished, individually each is more a collection of related views rather than a monolithic position.

³ The general movement of *logical positivism* is also captured under the label “logical empiricism,” though sometimes they are construed as overlapping but independent movements (see Godfrey-Smith, [2003](#); Slaney, [2017](#)).

[4](#) However, as Frigg and Hartmann ([2020](#)), p. 29) note, there is a “full spectrum” of positions on the relation between models and theories within and across these frameworks.

[5](#) This term is meant to capture generally the artifacts of science (objects, devices, ideas, procedures) that are used in knowledge production and should not be taken as a defense of a specific stance on the function of theories and models in science (in contrast to, e.g., Knuuttila, [2011](#)).

[6](#) In a practical sense, models and analogies serve many of the same functions in science; however, some philosophers of science have treated them as distinct and have framed analogy as a separate but integral part of scientific modeling (e.g., Bailer-Jones, [2009](#)); others see analogy as an extension of the model concept (e.g., Hesse, [1963/1966](#)).

[7](#) Herein, the focus will be on scientific psychology and will not include humanities-based approaches for which there has been growing recognition (e.g., Freeman, [2023](#); Teo, [2017](#)). Although most of the examples given below are drawn from research falling with quantitative research traditions, some allusions to research tools associated with qualitative research traditions also appear. A comparison of theory building and model use in quantitative and qualitative research traditions within psychology is beyond the scope of the current work; however, it is noted that there are relevant and important distinctions to be made between these two general traditions.

[8](#) I grant this framing is both an oversimplification and incomplete. The objective is not to provide a comprehensive survey of every kind of model that gets used in psychological science. Rather, it is to capture some broad categories of models that commonly appear in published research.

2. The “Oughts” and “Shoulds” of Integration in Psychology

Lisa M. Osbeck¹ 

(¹) Department of Psychology, University of West Georgia, Carrollton, GA, USA

 Lisa M. Osbeck

Email: losbeck@westga.edu

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The “Oughts” and “Shoulds” of Integration in Psychology

In his celebrated call for integrated accounts of psychological phenomena, John Dewey warned that “the material” of psychological life “is too great in mass and too varied in style to fit into pigeonholes” (1896, p. 357). The problem of organizing the subject matter of psychological science without losing its inherent complexity and variety remains formidable. The rampant disorganization of psychology troubles many: narrow specialization and hard lines of practice between intradisciplinary communities (e.g., applied and experimental psychologists, cognitive neuroscientists and socio-cultural psychologists, psychological science and psychological humanities) engender gaps or divides in the knowledge produced, especially in comparison with natural science (e.g., see Sternberg & Grigorenko,

[2001](#); Wertz, [2024](#)). For some, the obvious answer is more concerted efforts toward theory integration across the discipline.

Demand for integration in psychology has deep historical roots (James, [1892](#); Koch, [1951](#); Stern, [1938](#); Sternberg & Grigorenko, [2001](#)). However, detailed analysis of precisely what that means, how it might be accomplished, and most importantly, what it will do is less frequently articulated. This is not especially surprising. The vagueness and multiple meanings of concepts such as integration reflect that they take shape and evolve in networks, always connected to past meanings and in current cultural and temporal and disciplinary contexts (Hutmacher & Franz, [2025](#); Slaney, [2017](#)). The important point for the present purposes is that in addition to multiple meanings, attempts at integration of psychology's vast "material" reveal different goals, different visions of what integration is for, and thus why it is needed—its function and purpose. These matters relate to normative considerations, connected to the realm of values, both epistemic and other kinds (e.g., social, moral).

The essay began with a quote from Dewey, and thus it is worth noting that I view the discussion of the normative dimension of integration as broadly compatible with pragmatist and pluralist frameworks that both connect to Dewey ([1922](#)) and William James ([1905](#), [1907](#)) and find support in much contemporary philosophy of science. An important aspect of these frameworks is that knowledge is always partial and incomplete, owing to the complexity of both the world and the knower. The complexity of the world (including the human world that is the focus of psychological science) requires multiple levels of analysis, points of view, and modes of inquiry (e.g., see Mitchell, [2023](#)). The complexity of the knower means that the activity of theorizing is always inflected with values, interests, commitments, and passions (see also Polanyi, [1974/1958](#)), while at the same time it reflects the disciplinary and cultural traditions of the epistemic community in which the researcher participates. This is an important way in which we may describe knowing as perspectival, which is in turn tied to the idea that the activity of knowing is

constrained by the aims and assumptions of the researcher and the theoretical model that guides the inquiry (see Giere, [2019](#); Osbeck et al., [2011](#)). However, the moral or value-related aspect of knowledge so construed is less often brought into contemporary discussions (Araujo & Osbeck, [2023](#)).

Discussions of epistemic and moral values often aim at human science or psychology in toto. But more fine-grained and contextualized understanding is also required. Appreciation of the normative dimension in psychological science requires examination of specific examples—cases in which integration emerges as important to some task at hand, and this will be the method adopted here. After reviewing below the distinction between descriptive and normative concerns, we will examine three examples of contexts that express distinguishable goals, amounting to different ways of understanding the *responsibility* associated with a call for integration in psychology.

Descriptive and Normative Aspects of Theory Integration

At a most basic level, we may view problems of psychological theory relations through descriptive or normative lenses; they are different perspectives in their own right. A descriptive lens draws attention to what is entailed in theory combinatorial processes, the conditions under which theory continuities arise or may be drawn. To address such questions, we might consult cognitive science models of conceptual blending (e.g., Fauconnier & Turner, [2003](#); Holyoak & Thagard, [1997](#)) or explore processes by which hybrid models emerge through interdisciplinary science practice (e.g., Nersessian, [2022](#)). We might ask how various theories came to be related in scientific practice by analyzing important historical case examples (e.g., Nersessian, [1985](#); Thagard, [2018](#)), as well as cognitive and socio-political-cultural situations that support conceptual change. Descriptive questions also concern *what* becomes combined, *which* models or theories, and in what proportion or relation to one another. These kinds of questions

are important to ask in connection with *how* progress in theory or model building becomes possible, and in designing learning environments to support it, especially for interdisciplinary science.

But many questions remain, including how a blend or combination (at any level) may be judged to be a good one, useful and appropriate to the purposes at hand. Whether implicit or explicit, this judgment concerns what *ought* to or *should* be the case. The normative dimension is not divorced from the descriptive, of course. We could make the argument that historical, cognitive, or ecological theories have implications for how theories ought to relate to one another in psychology, but it is important to recognize that someone (singly or in collaboration) needs to draw the implications, and that they do so for some purpose, toward some end. Drawing implications reveals values, priorities, assumptions, concerns, and alignments.

Normative Stances on Theory Relations in Psychology

The normative dimension of theory relations is, of course, central to critical and theoretical psychology and to the project of this book, which asks among other questions what counts as good psychological theory and theory building. That psychology faces a “theory crisis” is a pervasive theme in recent decades (e.g., Eronen & Bringmann, [2021](#); Meehl, [1978](#); Oberauer & Lewandowsky, [2019](#); Sturm & Mülberger, [2012](#)). The label of “crisis” entails an obvious normative judgment, an indication that something is wrong, different than it ought to be. As others have noted, such judgments are hardly new; forms of crisis talk date to the late nineteenth century, reproaching psychology’s chaotic and disjointed pursuits, weak evidence, feeble theories, and general incoherence (Goertzen, [2008](#); Sturm & Mülberger, [2012](#)). In the final chapter of *Psychology: Briefer Course* ([1892](#)) opening dedicated to philosophy, James disparages the “string of raw facts” and “gossip and wrangle about opinions” without “a single law in the sense that physics shows us laws, not a single proposition from which any consequence

can causally be deduced” (James, [1892](#), p. 468). His summation of the state of theory relations in psychology echoes in later admonitions of psychology’s immaturity in comparison to the established sciences it emulates (Boring, [1933](#); Koch, [1951](#); Spence, [1956](#); Stern, [1938](#)).

Such charges of disciplinary incoherence and negative comparison with sciences such as physics reveal judgments of how psychological theories ought *not* to relate to each other, but do not in themselves offer guidance as to how psychological theories *should be* related, especially if the subject matter of psychology is acknowledged to differ fundamentally from physics. But stances that do offer positive statements about how psychological theories *should be* related to each other are easily found; they are most evident in the various schemes for unifying psychology theoretically or methodologically. Beginning in earnest in the 1980s and 1990s (see Staats, [1998](#)), theorists attested to the historically problematic nature of the discipline’s “fragmentation” and articulated new or revised visions for unification, again addressing what psychology “ought to be” by contrast with the discipline as we find it (e.g., Fishman, [1987](#); Yanchar & Slife, [1997](#); Staats, [1983](#); Sternberg, & Grigorenko, [2001](#); see Gaj, [2016](#) for historical overview and analysis). More recently, Gregg Henriques advanced the “Unified Theory of Knowledge” (Henriques, [2022](#); also this volume), which he frames as offering a solution to psychology’s theoretical shortcomings and a new meta-perspective for the discipline. As is the case in his proposal, unifying schemes typically bear the mark of both philosophical commitment and methodological prescription, though their requirements vary in form and degree. For Arthur Staats, an ardent supporter, unification poses comprehensive demands in both theory, method, and disciplinary structure (Staats, [1999](#)). On the other hand, Rychlak expects only “willing effort made” to remain conceptually grounded with other psychologists, “not a compliance to an order” (Rychlak, [1994](#), p. 894).

Juxtaposed to the calls for unification are pronouncements that psychology cannot, will not, and most importantly, should not be unified (e.g., Green, [2015](#); Koch, [1993](#); Stam, [2004](#)). The reasoning

driving this position takes various forms, emphasizing the importance of cultural variation, avoidance of epistemic hegemony, creative freedom, social justice, or experiential abundance, according to the context and priorities of the author.

If less obviously, normative assumptions and values are revealed in the concepts of “integration” and in calls for “pluralism,” though not in entirely consistent ways. Discussions of integration and pluralism appear across a wide array of research and scholarly contexts in psychology. In themselves they appear less controversial than “unity” and “fragmentation,” even though frequently linked to these concepts. We may claim that psychology would benefit from greater integration between its theories without raising alarms about hegemony. But what integration is for, why it is needed, and when it is successfully accomplished are questions of a normative kind. These questions are complicated by confusion surrounding what integration and pluralism mean, how they function, and the nature of the relation between them, and these issues themselves are difficult to sort out with any degree of precision. There are different forms or senses of both integration and pluralism, widely varied contexts in which they appear, and different (even contrasting) positions concerning their relation to each other.

The Value(s) of Integration

Integration is highly important to the argument for disciplinary unity and proposal of specific unification schemes, serving to connect theories and thus increase internal coherence. At the same time, integration is central to the goals of interdisciplinary research, which by merging psychology with other disciplines may threaten psychology’s distinct identity or boundaries. At the very least, it is clear that integration is directed toward very different projects in psychology. A comprehensive review of integration in psychology is beyond the present scope, but one can appreciate important differences in meaning, features, scope, and purposes of integration with an illustrative comparison of three contexts in which it figures

prominently: cognitive theory, personality theory, and psychotherapy. The comparison enables the distinction of three important variations: (1) integration in the service of discovery, (2) integration in the service of fidelity to subject matter, and (3) integration in the service of designing more effective interventions (targeted problem-solving). To make the differences apparent requires examination of each in turn, the purpose of the following section.

Integration for Discovery

We noted above a concern dating to the early history of psychology which centers on a need to strengthen psychology's epistemic foundations, increase conceptual integrity and coherence, and thus augment predictive power, equated with disciplinary progress. Unification schemes (e.g., Henriques, [2011](#); Staats, [2005](#)) address the effort to strengthen epistemic foundations, though the question of how unity is understood (e.g., whether unity requires homogeneity or allows heterogeneity) follows closely from these projects. Concern with expanding psychology's epistemic reach while solidifying its grounding also accompanies appeals to integration in some contexts. For example, in a helpful article titled "A Theory Integration Program," Gerd Gigerenzer identifies "the continuing development of its theoretical fundament" as "arguably the most critical task of psychology" ([2017](#), p. 133). He presents integration as the key to theoretical development, explained by contrast to two standard methods of strengthening scientific foundations, beginning with theory competition:

The first is the competition between existing theories, as envisioned by Karl Popper: science progresses by successively eliminating theories until ideally one survives that cannot be rejected. This program emphasizes the "context of justification" and has fueled progress in statistical hypothesis testing and model selection criteria. At the same time comparatively little attention has been paid to the "context of discovery," which

Popper tried to eliminate from the philosophy of science.
(Gigerenzer, [2017](#), p. 133)

Developing unified theoretical frameworks is the second route to strengthening foundations for which Gigerenzer cites Anderson ([1983](#)) and Newell ([1990](#)) as examples, as well as unified frameworks for decision-making that include his own (Gigerenzer et al., [2011](#)). Not only are theory competition and unification not the same; their goals may directly conflict. Unifying frameworks prioritize adherence over rivalry; in turn, “emphasis on competitive testing of low-level theories or on null hypothesis testing may divert from the ideal of building unified theories” (Gigerenzer, [2017](#), p. 134). Gigerenzer thus introduces a third, complementary path toward scientific progress, that of *integration*:

Scientific progress can result not only from the elimination of theories but also from integration. Whereas the growth of knowledge is sometimes pictured in analogy to the survival of the fittest and elimination of weak theories (Popper, [1972](#)), the theory integration program envisions growth through building networks between theories. Its general task is to detect coherence among current theories. (Gigerenzer, [2017](#), p. 134)

The goal of theory integration is “more modest” than that of either a comprehensive metatheory or program of eliminative reduction based on theory competition. Integration is intended merely “to achieve a higher degree of coherence within psychology” (p. 135).

Several points are important to note about integration as Gigerenzer presents it in this context. The first is that because integration plays an important role in discovery, it therefore promotes innovation and progress—toward new theory, new applications. The powerful potential for progress is obvious in the examples of theory integration Gigerenzer cites, including the discovery of DNA made possible only through the melding of Mendelian genetics and Darwinian evolutionary theory, as well as contemporary physicists’ integration of quantum theory with general relativity. Despite what Gigerenzer calls his modest

aims, we may take from these examples an assumption that psychology might become a more consequential science through integration of this kind.

The potential for consequential discovery, however, seems contingent on integrative efforts of a particular quality. That is, a second crucial point we may extract from Gigerenzer is that integration requires a high degree of theoretical and procedural precision, not a misty impression of connectedness or union:

Above all, integration requires meticulous attention to details, operational definitions, and clarity of explanatory concepts. It cannot be achieved at an abstract level alone. Rather, it may begin by analyzing a single pair of phenomena or concepts that first need to be operationally defined, followed by an investigation of how they are theoretically connected. If the connection can be worked out, the next phase is to add further phenomena (concepts) to create a network with precise relations. (Gigerenzer, [2017](#), pp. 134–135)

Gigerenzer's illustration of the two-step process with examples from his own theory integration program suggests a third, implication: integration requires effort, a deliberate act, whatever tools are used in service of that purpose. Motivations for such acts may be both personal and shared, reflecting common understandings and goals within an epistemic community. This is not to say that integrations are arbitrary constructions conforming to the agenda of the integrator. Rather, he compares the growth of knowledge through integration to an ongoing puzzle:

Progress is less a blind variation plus-selection process than a carefully reflected process, where scientific growth can result from deliberate integration rather than elimination of theories. Instead of seeing theories as fierce competitors, or as toothbrushes that no one wants to share, the theory integration program sees them as pieces of a puzzle that, carefully

combined, might form a greater picture. (Gigerenzer, [2017](#), p. 144)

Although a “greater picture” is attained, the picture serves the purpose of epistemic advance. That is, an aesthetic value of appreciation for wholeness and connectedness is not the dominant motivation for integration; it is not for the sake of a diffuse impression of connection and union. Indeed, integration is not a final state. It is never achieved to completion but is a matter of degree, always limited in scope and application.

Gigerenzer’s practical suggestions for enhancing integrative efforts draw focus to the role of the researcher, yet at the same time call for broader disciplinary change—a shift in psychology’s epistemic values and relationship with theory. The suggestions he makes “to get the theory integration program running” target every level: researchers could submit articles that analyze connections between seemingly disparate findings or ideas; journals could include sections devoted to integration of theories; institutionally, psychology departments could encourage and reward integration work as part of the important business of psychology. In short, this would amount to “a change in professional culture” (Gigerenzer, [2017](#), p. 144).

The example Gigerenzer provides is important because of his deliberate attention to integration itself as the focus of the paper, rather than on content that needs to be integrated. He offers a clear analysis of integration’s role in theory building. Numerous other contexts of psychological research and theory feature integration as a central concept, though rarely do other authors analyze integration itself in the detail Gigerenzer provides. Nevertheless, without the detailed analysis we may still extract a sense of what integration means in relation to what it is called upon to *do* in these other contexts, its function—the goals and purposes for which it is called, the problems it will address, and what it can accomplish. The following two contexts illustrate purposes and goals for integration that differ in important ways from the integration in Gigerenzer’s paper. For personality theory, I include a

summary of recent changes in the field but focus on a very well-known and influential paper by McAdams and Pals ([2006](#)), for the reason that it provides a clear account of the value of integration for understanding persons. In the case of psychotherapy research, however, I do not examine one specific paper in detail but instead refer more generally to the integrative psychotherapy framework as this has developed in recent years to highlight the role of integration in targeted intervention (problem-solving). The three contexts by no means exhaust the domains of psychological research in which calls for greater theoretical integration are prominent. They serve merely as examples, though they represent three prominent and somewhat distinct research specialties with long histories and contemporary influence in psychology.

Integration for Fidelity to Subject Matter

Personality psychology shows a decided shift in recent years, with authoritative scholars turning deliberately and resolutely toward integrative theory and models. This shift accompanies mounting dissatisfaction with the available options for conceptualizing, investigating, and teaching personality theory during the late twentieth and early twenty-first centuries. Walter Mischel provides a summary of the historical developments and what was at stake:

In 1968, the limitations of traditional approaches were becoming evident, but the alternatives were just beginning to emerge. Since then, the study of personality has expanded vigorously into an increasingly interdisciplinary science, renewing the hopes with which personality psychology was founded (Cervone & Mischel [2002](#)). The field was intended to ask the deepest questions about human nature, and to become the meta-discipline—the hub—for integrating the basic findings and general principles revealed by work at different levels of analysis within the larger science as they speak to the coherence and organization of the individual. The aim was to build on whatever was relevant, from the biological to the psychosocial

and cultural, to capture the unique patterning and organization of the functioning, distinctive “whole person” (Allport [1937](#), [1961](#)). (Mischel, [2004](#), p. 18)

Several things are worth noting in this brief passage alone. First, integration connects to an increasingly interdisciplinary science of personality, in recognition of the inadequacy of a bounded disciplinary approach to an inherently complex subject matter that requires multiple levels of analysis (for a comprehensive discussion of psychology’s complex subject matter, see Wendt, [2025](#)). Second, at least some of those who seek an integrative approach to personality science view the effort as a return to the original intent of personality psychology: concern with the unique “whole person.” Third, the origin story of personality is one not only of concern for the whole person but reflects traditionally philosophical inquiry, asking “the deepest questions about human nature.” Early personality psychologists intended the field to function as the “hub” of integration, a conceptual (and methodological?) nexus for the discipline of psychology in its entirety. Relatedly, integration is “unbounded” we might say; it is additive and inclusive, with no constraints on content, approach, or method other than relevance to the unique patterning of whole persons.

McAdams and Pals ([2006](#)) offer another very well-known formulation of an integrative science of personality. Going beyond Mischel’s narrative of dissatisfaction in the field of personality research, McAdams and Pals recall a “near-death experience” for personality psychology in the 1970s, the cause of which was a critical response highlighting myriad situational factors that influence personality development and human action. Following this crisis, new evidence for the stability of behavior over the life span led to a revival of interest in the personality “trait,” once the main target of situationist critique. The trait itself became an integrative concept, a blend of genetic influence and learning history (e.g., Goldberg, [1990](#)).

McAdams and Pals zealously promote a yet more comprehensive framework for personality, one that returns to the original intent of the subfield's founders: "Despite its recent revival, personality psychology still falls somewhat short, because it continues to retreat from its unique historical mission. That mission is to provide an integrative framework for understanding the whole person" (2006, p. 204). For McAdams and Pals, models incorporating multiple forms of data through divergent assessment methods are key to an adequate view of any unique individual (*a* person). But they also target the way "personality" as a concept or scholarly object tends to be conceptualized and studied, including in personality textbooks, and call for a more integrative approach to understanding persons in general: "*the* person." The implicit contrast to "the integrative point of view" emerges from the two kinds of texts they criticize: the parade of theories approach (presenting a range of incompatible alternatives from psychoanalysis to humanistic psychology) and the "smorgasbord of research topics and issues but no overarching conception for making sense of it all" (McAdams & Pals, 2006, p. 205). Instead, McAdams and Pals suggest "five big principles for a new integrative science of personality," which include evolution, dispositional structure, characteristic adaptation, life narrative and identity, and culture. Two of the five principles, namely, characteristic adaptations and life narratives, are themselves depicted as integrative, and all the principles bear a relation to everyday life and action, as well as developmental tasks and challenges (see p. 213).

Like Mischel, McAdams and Pals look back toward a more worthy goal, linking integrative models to the "historic mission" of personality psychology. They also similarly view personality in foundational relationship to psychology in general, promoting a more centralized position for personality within the discipline, representing this position as a requirement, a kind of charge:

Taken together, the five principles articulated in this article mark a newfound effort on the part of personality psychology to

assume a central and integrative position within the discipline of psychology and in the social sciences more generally. For too long now, personality psychologists have shirked their *responsibility* to do so. (McAdams & Pals, [2006](#), p. 215, emphasis mine)

We detect a similar note of urgency and responsibility in Gigerenzer's appeal for integration, especially that strengthening theoretical fundamentals is the most important task. However, Gigerenzer's integration is directed toward the possibility of discovery and the future, with no comparable sense of historic mission. A goal of theoretical progress is implicit for McAdams and Pals, but fidelity to the subject matter (the whole person) is their principal emphasis, such that evaluation of disciplinary progress must accord with that goal. Another difference is that Gigerenzer explicitly describes his aims for integration as modest and limited, intending only to promote coherence where possible, to shore up disciplinary foundations with a little progress at a time. By contrast, for McAdams and Pals, integration in the service of a comprehensive framework, at least as represented in the examples offered here, assumes a central position. This is also implied by the goal of a "meta-discipline" and "hub" (Mischel) in relation to the whole of psychology. Such an integrative framework resembles in this way a unification strategy, though personality psychology and not general psychology foundations provide the origin and grounds of unification.

Finally, in the context of personality, despite specification of *principles* to be included in an integrative framework (what is to be included), there is less emphasis on *how* integration takes place, on the process of integration, that is, on practical steps that might be taken to achieve it. This is not surprising given that the authors' ostensive focus in the examples examined is not on integration per se, but rather on the complex nature of the subject matter (the unique individual, the whole person) for which an integrative framework is demanded.¹ Integration functions principally as an adjective marking a comprehensive model or

the subject matter; it may also function as the ideal toward which to strive in personality theorizing.

It is worth briefly noting here that in earlier personality theory, the whole person emphasis entailed integration of the conscious and unconscious aspects of psyche as a goal or ideal, in keeping with a striving toward psychological health or growth. For example, integration is vital to Jung's description of the process of individuation, referring to a gradual balancing of opposing forces or energies as a person becomes whole (Jung, [1939](#)). A similar idea figures prominently in both Horney's model of the self (Horney, [1951](#); see Paris, [1999](#)) and Erikson's developmental model (Erikson, [1959](#)); integration is also the implicit contrast class of R.D. Laing's "divided self" (Laing, [1960](#)). The theoretical contexts of psychoanalysis and existential psychology are of course very different from those of McAdams and Pals and colleagues, in that the former invoke internal dynamics of self or psyche rather than interrelations of theories. The relevant point is only that integration and the processes by which it occurs may retain some of the elusive character of psychic integration when it is applied to personality theory relations. On the other hand, integration of the nervous system was also of growing interest in the early twentieth century, illustrated in the "integrative action by virtue of which the nervous system unifies from separate organs an animal possessing solidarity, an *individual* [...]" [i.e., an individual organism] (Sherrington, [1926](#), p. 2). It is not unreasonable to suspect that integration of both psyche and individual nervous system contributed to the original importance of integration to personality (the historic mission), and this complex mix continues to inflect our conceptions of whole persons.

Integration for Targeted Problem-Solving

Psychotherapy obviously draws from personality theory and the integrative frameworks that emerge in that context. McAdams and Pals specifically note that implications for psychotherapy naturally follow the integrative personality framework, and that data from personality assessment of different kinds inform a comprehensive picture of

individuals. That is, an integrative understanding of persons may be necessary for an effective integrative treatment design. However, research on psychotherapy is its own domain, despite obvious overlap. The goal is oriented around treatment effectiveness rather than ever more comprehensive understanding. The meaning of integration in the context of psychotherapy research is itself immensely complex and evolving, with several positions expressed on the relationship to “eclecticism” and on the broader purposes of integrative therapy (see Norcross & Goldfried, [2019](#)). On one hand integration represents a flexible approach specifically tailored to the needs and characteristics of the individual client (Castonguay et al., [2003](#)); on the other hand, “psychotherapy integration focuses on the relationship between an effective practice and its theoretical and empirical basis,” which appears to be a different goal (Zarbo et al., [2016](#), p. 3). Different forms of integration important for psychotherapy include identifying elements common to all approaches, matching interventions to problems or populations, and combining various therapeutic theories and techniques. The important point for our purposes is that integration with the deliberate aim to optimize interventions for a person or a targeted problem (e.g., anxiety) casts the value of integration quite differently from either discovery or comprehensive understanding and fidelity to the subject matter. Generalizable models may be a desirable outcome of integrative psychotherapy research, but they still serve the purpose of aiding prediction for problem-solving, for focused application. As such, pragmatic values rise to the fore.²

Although we may find (or draw) areas of overlap between integration in the three contexts examined here, the examples reveal different convictions concerning the need for theory integration, and thus distinguishable goals toward which integration should aim. I have characterized them here as relating to discovery and innovation, fidelity to the subject matter, and flexible tool use (interventions) in the service of problem-solving. In each case, appeal to integration may reflect concern with theoretical progress, though different conceptions of progress are implied. In turn the diverging conceptions of progress

accompany objectives and roles associated with different areas of psychology, but such differences still highlight the implicitly normative and value-centric nature of accounts of integration, and the way these values may be instantiated in the practices and aims of different research communities.

Integration and Pluralism

Integration is frequently associated or conjoined with pluralism, adding complexity to the task of understanding integration's meanings and purposes (e.g., Boxell & Marquis, [2022](#); Mitchell, [2023](#)). As is the case for integration, references to pluralism in psychology abound, in highly divergent contexts of use with different shades of meaning. Various contexts and meanings of pluralism in contemporary psychology are recently reviewed elsewhere (Araujo & Osbeck, [2023](#); Osbeck & Araujo, [2023](#)). Here I will mention only that confusion about the relation between integration and pluralism arises in part from the fact that “pluralism” is invoked both in arguments *for* disciplinary unity and in arguments against it. Those who connect integration to pluralism seem to imply that psychology's subject matter is tied by an invisible underlying connection despite apparent differences (e.g., Wertz, [1999](#)). In turn, other scholars cite pluralism as appropriate in view of psychology's hopeless or *necessary* fragmentation (e.g., Stam, [2004](#); Kirschner, [2006](#), Teo, [2010a](#)).

Though outside the scope of this chapter, confusion surrounding the relation of integration to pluralism (or vice versa) arises against what appear to be the very different origin stories of integration and pluralism in the course of Western thought, or at least the formal accounts of these origins. Integration was originally a mathematical concept, important to ancient calculus, entailing principles of area continuation later formalized by Leibniz (Amazigo & Rubenfeld, [1980](#)). Rosenthal ([1951](#)), for example, situates integration in the historical development of calculus:

[Calculus] starts with integration; computing areas, volumes, or lengths of arcs were the first problems occurring in the history of Calculus. Such problems were discussed by ancient Greek mathematicians, especially by Archimedes, whose outstanding and penetrating achievements mark the peak of all ancient mathematics and also the very beginning of the theory of integration. (Rosenthal, [1951](#), p. 75)

By contrast, pluralism emerged in broader contexts of ontology, that is, theory concerning the ultimate nature of reality and its forms. Arguably it is rooted in a pre-Socratic quandary over the problem of whether the world is one or many (e.g., see Kirschner, [2006](#)). However, what is translated from the German as “pluralism” itself appeared in contrast to an “absolutism” or absolute idealism (the idea that *some* absolute reality exists outside of the human capacity for knowing) attributed to Hegel and followers, along with “monism” (the idea that there is *one* supreme reality only). The surrounding debate played an important role in theological discourse outside of Germany into the late nineteenth century (see Bevir, [2012](#); Laner, [1905](#); Mander, [2011](#); Wahl, [1925](#) for historical details).

But in the twentieth century, both integration and pluralism became associated with the interaction of different biological systems (or levels) and their properties. In these contexts, pluralism implies analysis aimed at different levels of explanation to respond adequately to the complexity of living systems and aid predictive models, consistent with its contemporary meaning in the philosophy of biology (e.g., see Feibleman, [1954](#); Mitchell, [2023](#)).

Divergent values and motivations remain obvious in contemporary appeals to pluralism in psychology. In some contexts, pluralism is called in to promote epistemic gains. For example, “explanatory pluralism” relates to incorporation of multiple levels of analysis toward predictive model refinement and reduction of error (e.g., Sewell et al., [2011](#); Fabry, [2017](#)). Elsewhere, however, concerns are principally ethical: pluralism accompanies a mandate for foregrounding cultural diversity (Shweder

& Power, [2013](#)), including in psychological methods (Gergen, [2014](#)), education (Denson & Ing, [2014](#); Schwarzenhal et al, [2018](#)), and the workplace (Combs, [2002](#)). These positions hold that only a pluralistic framework can avoid elimination of voices and valuable sources of psychological knowledge, including insights generated through the humanities and artistic expression (e.g., Teo, [2017](#); Freeman, [2023](#)). Kirschner ([2006](#)), for example, notes that “it is socially and morally desirable for psychology to continue to be comprised of, and to offer to the larger culture, diverse lenses – multiple, sometimes incommensurable discourses” ([2006](#), p. 3). Incommensurable discourses surely cast doubt on the possibility of meaningful integration.

Conclusions: Normativity in Integration and Pluralism

From even the brief overview provided it is important to recognize the variability and complexity not only of meanings but of motivations driving debates on theoretical relations in psychology. An important point that emerges is methodological, that discussions of integration in psychology that aim at the discipline in general are complemented by examination of the purpose and function of integration in specific contexts, as is the case for pluralism (Araujo & Osbeck, [2023](#)).

The broader point, however, is that pursuit of integration and pluralism implicates values, instantiated in statements about the way the discipline *ought* to be structured, how the subject matter *should* be organized, research conducted, and practice applications pursued. Calls for integration in psychology involve judgments about the kind of relations one believes *ought to* pertain among theories, and which theories, and why. The motivations behind these positions on integration represent if not *conflicts* in values, at least different disciplinary priorities. To offer a very sweeping (and somewhat inaccurate) summary: concerns about scientific integrity (predictive power, replicability), theoretical coherence, and methodological rigor

bump up against concerns about reductive accounts of human experience, power imbalances, elimination of cultural variations and voice, restriction of form of expression, even epistemological violence (Teo, [2010b](#)).

In a recent paper, Hutmacher and Franz acknowledge that “knowing the available options and perspectives will ultimately also give us the opportunity to choose more carefully what kind of perspective on a psychological phenomenon seems most promising and useful in a given situation” (Hutmacher & Franz, [2025](#)). Similarly, the normative dimension of theory relations in psychology does not point to a problem to be solved, but rather an opportunity for more understanding and open discussion of different values underlying various epistemic aims, offering clearer direction in research, scholarship, and practice. This goal may itself serve integrative and epistemically expansive purposes, as Koch implies in promoting “psychological studies”: “We can hope, too, for more flexibility in the capacity of inquirers to enrich their vision by trying on the spectacles of their neighbors” (Koch, [1993](#), p. 902).

Importantly, the discussion of values implicates both personal and social dimensions of psychological science, understanding of which underscores the responsibility that attends its practice. These relations are explicitly explored, as is their connection to human emotion, in the earlier tradition of pragmatism (especially Dewey, [1922](#)), and a more recent tradition that foregrounds the personal and social dimensions of science without reducing it to subjective enterprise (e.g., see Osbeck et al., [2011](#); Polanyi, [1974](#)/1958). To this end, theoretical psychology may benefit from continued engagement with contemporary philosophy of science that explores more complex understandings of the relation of science and values (e.g., Douglas, [2021](#)).

Finally, the focus on normative questions concerning integration’s purposes and values should not be taken to indicate that descriptive questions concerning theory integration are unimportant. Indeed, if integration is an aim toward which to strive, it would follow that we should study how it might be facilitated. This kind of question is

informed by data, including data provided by the study of integrative success stories through historical or ethnographic research, along with what is less conducive to integration. Better understanding of the social conditions and quality of interaction in collaborative projects in interdisciplinary settings that promote integration is also required. These questions, in turn, invite reflection on how we conceptualize and evaluate successful and unsuccessful integration, a normative consideration. Understanding how questions concerning descriptive and normative questions about theory relations interweave and implicate one another in practice is as important as understanding how they differ in principle.

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Footnotes

¹ Of note is that the task of more precisely specifying the path to integration is taken up in an earlier paper on personality integration, wherein the focus is on describing relations between six main systems contributing to the formation and development of personality structure (Royce, [1983](#)).

² It is worth noting that Henriques ([2011](#)) advocates for theoretical unification on the grounds that the costs of fragmentation are high, which is a variation of a pragmatic argument for integration.

3. Toward a More Accurate Notion of Exploratory Research (And Why It Matters)

Uljana Feest¹✉ and Berna Devezer²✉

(1) Institute of Philosophy, Leibniz University, Hannover, Hannover, Germany

(2) College of Business and Economics, University of Idaho, Moscow, ID, USA

✉ **Uljana Feest (Corresponding author)**

Email: feest@philos.uni-hannover.de

✉ **Berna Devezer**

Email: bdevezer@uidaho.edu

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Introduction

In the debate about the replication crisis of the past 15 years, one focus has been on questionable research practices, such as HARKing (hypothesizing after the results are known)—the practice of re-analyzing experimental data and presenting the results as confirming a hypothesis other than the one researchers started out with (Kerr, [1998](#)). It is commonly understood and widely held that even though it is impermissible to treat the results of such a re-analysis as

confirmatory of the new hypothesis, such a re-analysis of the data should be accepted in the toolbox of exploratory research. In other words, data can be re-analyzed with the aim of generating new hypotheses, as long as these ideas are not presented as being confirmed by the same data that generated them (Bridges, [2022](#)). Within the open science movement, the requirement of preregistration was introduced, in part, to prevent researchers from HARKing and to distinguish clearly between confirmatory and exploratory data analysis (EDA). It thereby relies on two assumptions. The first assumption is that preregistration should make it harder for researchers to treat their data as confirming a hypothesis different from the one they set out to test. The second assumption is that “exploratory” data analysis is not a questionable research practice as long as it is properly tagged as such.

Recent work about the philosophy of scientific methodology has suggested that the case against HARKing is not as clear-cut as the above characterization makes it sound (e.g., Devezer et al., [2021](#); Rubin, [2017](#); Rubin & Donkin, [2022](#); Szollosi & Donkin, [2021](#)). Here we take a different approach, however. It is not our aim to discuss whether retrospective data analysis can be confirmatory. Rather, we want to dissect the notion of *exploratory research* as used in the metascience literature. Specifically, we will argue that this notion is (a) too narrow to capture other important ways in which research can be exploratory, and (b) too broad as it does not provide us with normative guidelines to distinguish between epistemically fruitful and epistemically deleterious forms of “exploratory” data analysis. We develop our argument by drawing on the philosophy of science literature about exploratory research (e.g., Colaço, [2018](#); Massimi, [2018](#), [2019](#); O’Malley, [2007](#); Steinle, [1997](#)). This literature, we will argue, provides us with an appreciation of the importance of exploration in both empirical research and theoretical modeling. It also highlights that if we restrict the notion of *exploration* to processes of data *analysis*, we risk neglecting the processes that led to their *creation*. This means that there is a danger of treating those data as given rather than debating their quality and relevance.

We begin by providing an overview of an influential articulation of the exploratory/confirmatory distinction within statistics and psychology offered in the early replication crisis literature (Wagenmakers et al., [2012](#)). We argue that current advocates of exploratory data analysis use the word “exploratory” in a way that diverges not only from a common-sense understanding of exploration but also from older treatments of exploratory data analysis (e.g., Tukey, [1977](#)). We then give some background about the practical and experimental turns within the philosophy of science in the early 1980s, highlighting that it was before the backdrop of these movements that interest in exploratory research emerged, both within the philosophy of experimentation and the philosophy of modeling. We show that similar impulses drove works stemming from these two traditions, and argue that exploratory and theoretical work need not be mutually exclusive. We further discuss how *exploratory modeling* was highlighted as a specific mode of exploratory research in the philosophy of modeling (e.g., Fisher et al., [2021](#)) and identify a particular strand of recent metascience work on exploratory modeling (e.g., Devezer & Buzbas, [2023](#); Guest & Martin, [2021](#)) as exemplifying this mode. After discussing the systematic variation of modeling or experimental parameters as a crucial ingredient of exploratory research, we move on to expound on another aspect of exploratory research, namely, that it takes place in a situation of conceptual openness. We point to the vagueness of psychological concepts (Meehl, [1978](#)) and draw connections to recent philosophical analyses of the relationship between exploratory research and conceptual development (e.g., Feest, [2010](#); Haueis, [2023](#)). In the conclusion, we return to examining whether the distinction between confirmatory and exploratory data analysis has the necessary precision to distinguish between productive exploratory work and questionable research practices. We argue that it does not.

On the Exploratory/Confirmatory Distinction in Statistical Data Processing

In the aftermath of the replication crisis, a growing literature has focused on distinguishing between confirmatory and exploratory research. Originally restricted to the null hypothesis significance testing (NHST) framework (despite subsequently being extended to other modes of inference), this dichotomy suggests that exploratory and confirmatory research primarily and firmly diverge in their analytical approaches. Confirmation is said to be characterized by the prior specification of the statistical analyses the researchers intend to carry out, which also means locking in the hypotheses to be tested in an NHST framework, whereas exploration can be performed via post hoc data analyses that Wagenmakers et al. ([2012](#)) refer to as “wonky”: “If you carry out a hypothesis test on the very data that inspired that test in the first place then the statistics are invalid (or “wonky”)” (p. 633). Such wonky analyses may overfit the observed data, cherry-pick the results, or misrepresent the procedures implemented, inflating the support for the hypothesis. Such practices, the authors argue, are illegitimate if they are treated as confirmatory of the hypothesis, but are legitimate so long as they are used as part of a process of exploration, not confirmation.¹

The literature discussed above suggests that researchers should be required to preregister their hypotheses and statistical analyses to prevent confirmatory value from being claimed for an analysis that is “merely” exploratory. Preregistration policies have a lot of *prima facie* plausibility, and we certainly endorse the underlying commitment to transparency and accountability. However, we are skeptical of how this policy has come to be tied to the distinction between confirmatory and exploratory research since those two concepts are less than clear. Here we set aside issues surrounding the definition of “confirmatory analysis” within Wagenmakers et al.’s framework. Instead, we focus on the features they attribute to *exploratory data analysis* to probe underlying assumptions about the nature of exploration.

First, Wagenmakers et al.'s definition of "exploration" strictly relies on the absence of a hypothesis specified before seeing the data. What the authors further imply is that within the context of exploratory research hypotheses would need to be generated based on data and data alone. In this framework, exploration informed by a hypothesis or theory is not explicitly considered a possibility. At any rate, the authors do not specify what a theory-informed exploration might look like. While using the broader label of "exploratory research," this framework is narrowly limited to exploratory statistical analysis of experimental data rather than providing a comprehensive perspective on exploration in science. Perhaps this statistical data analysis-centered view of exploration invites a comparison to an earlier framework for exploratory data analysis (EDA) envisioned by Tukey ([1980](#), [1993](#)). Tukey and Wagenmakers et al. have in common an exclusive focus on data analytical procedures and agree that for confirmatory analyses, data collection or analysis needs to follow a clear formulation of hypotheses. Tukey's view differs fundamentally in its objective and approach to exploratory analyses, however. Tukey's EDA aims to find questions not necessarily with an eye to generate new hypotheses but to inform how to conduct and analyze future experiments. Instead of recommending haphazardly performing wonky inferential statistical procedures, Tukey emphasizes a thorough numerical or graphical examination of the data without using probability in general or significance tests in particular. He recommends flexible techniques to describe structures that emerge in the data and cautions the analyst to not mistake these structures for natural laws.

Second, in Wagenmakers et al.'s dichotomous framework, performing invalid statistical inference is suggested to be an acceptable, if not recommended, scientific practice as long as it is correctly labeled as exploratory. We argue that even though it is conceivable that good hypotheses may be generated this way, it remains unclear why this method should work or how successful it would be. From a theoretical perspective, there is no reason to believe that misusing or abusing statistical procedures could reliably lead to scientific discoveries

(Devezer et al., [2021](#)). The view of exploratory practices as purely data-driven also does not quite fit within a common-sense understanding of *exploration*. For example, we would not send a team of explorers to the Arctic without a plan or reliable equipment and supplies. Why, then, would we send scientists to explore empirical and theoretical spaces unprepared and ill-informed? Moreover, it is not clear how we would distinguish between more or less promising exploration in Wagenmakers et al.'s framework. Good ([1983](#)), in his attempt at a philosophy of exploratory data analysis, suggests that a pattern found in EDA should be considered salient or interesting only if it has "a non-negligible subjective or logical probability of being potentially explicable, at least in part" (p. 290). He approaches exploratory analyses with a probabilistic mindset to support valid inferences from the data.² In the same spirit, coming out of statistics, there are procedures tailored toward exploratory data analysis which nonetheless return statistically valid results aimed at delineating successful exploratory approaches (e.g., Gelman, [2004](#); Goeman & Solari, [2011](#)). As such, exploration via invalid statistical inference remains contested and appears to be insufficient to capture the essence of the process of data exploration.

Third, in Wagenmakers et al.'s framework, selective reporting of desired results is legitimized. This is a particularly puzzling premise given the basic definition of "exploration," which typically contains some reference to a thorough investigation with the aim of learning about something unknown. For example, Tukey ([1993](#)) emphasizes that EDA should not exclusively focus on finding happenstance phenomena but also needs to describe some "continuing reality" (p. 7). Tukey appears to suggest that instead of singling out surprising patterns in the data, exploration should also remark on recurring, baseline effects, as well as negative or statistically nonsignificant patterns that could be important. Similarly, Höfler et al. ([2021](#)) suggest that a valuable exploration should be honest, transparent, and full instead of selectively analyzing the data or cherry-picking desirable results. Moreover, it seems that despite the commitment to a non-theory-driven

approach to exploratory data analysis, its selective nature is likely to import biases that will remain unchecked if it is not or cannot be modeled properly.

In light of the three points just mentioned, it seems that the usage of the term “exploratory” in writings such as Wagenmakers et al. pulls in two different directions, each of which is incompatible with plausible (common-sense and statistical) assumptions about exploration. On the one hand, they treat exploratory research as happening in the absence of prior hypotheses and, thus, as not being guided by any theoretical or conceptual assumptions. This goes counter to a common-sense notion of *exploration* as involving at least some basic preliminary understanding of the terrain that is being explored and the tools needed to explore it. On the other hand, they suggest that exploratory research does not need to engage in a systematic investigation of the entire terrain but can, rather, focus on some regularities that were generated by a rather arbitrary choice of procedures. This goes counter to the idea that exploration should be thorough and systematic. If, as we argue, exploration cannot be entirely free of preexisting theoretical and conceptual assumptions, it seems important to us that these assumptions be made as transparent as possible and that their theoretical warrant is discussed. When this doesn’t happen, the worry is that the exploratory process encourages specific conceptual biases to creep in through the back door.

In sum, Wagenmakers et al. (and the metascience literature following their lead) do not embrace the intuitive idea that exploration ought to be guided by some normative framework determining what constitutes good exploration. Nor does it follow prior works on exploratory data analysis (Tukey, Good). Instead, it construes exploration as aiming for serendipitous discoveries, free from scientific or statistical constraints. We see the danger that such a liberal and unprincipled approach is likely to yield invalid statistical inference or spurious results. By contrast, we construe exploration as aiming to provide a carefully planned, systematic mapping of unknown spaces. But this still leaves open the question of how to characterize such a

Carefully planned and systematic mapping. If we understand exploration to rely on prior assumptions, how should the relationship between those assumptions and the exploratory activity be construed? Can exploration be guided by hypotheses or theories? Does exploration need to be understood in contrast with confirmation? Is there exploratory research beyond data analysis and what does it look like? How flexible should exploratory research be? What criteria characterize good scientific exploration? In the following, we turn to the literature on exploratory research coming out of philosophy of science. The next section introduces the reader to the context in which a focus on exploratory research emerged within philosophy of science, followed by a section explicating some functions of exploratory modeling.

The Emergence of Interest in Exploratory Research in Philosophy of Science

Within the field of the history and philosophy of science (HPS) exploratory research was not an object of analysis until the late 1990s when two papers appeared the same year that each used the term “exploratory experiment” (Burian, [1997](#); Steinle, [1997](#)). These authors identified what they took to be a specific function of experiments that had not been covered by previous scholarship. The emergence of philosophical interest in exploratory experimentation has to be placed within the general context of two movements that emerged within HPS and STS that are sometimes referred to as the “practice turn” and the “new experimentalism,” respectively. In general terms, all of these approaches are characterized by a rejection of the distinction between the contexts of discovery and justification.³

The practice turn in philosophy of science was informed by several important insights. First, practices take place in space and time; they are situated and temporal. Looking at scientific practice, thus, decentered (at least for some scholars) the traditional interest in a purely logical relationship between theory and evidence in favor of a

more historically informed study of knowledge production. In addition, it drew attention to material aspects of science, such as experimentation, the use of scientific instruments, and practices of modeling. One feature shared by many instances of this turn to practice was the recognition of an epistemically important “middle realm” that does not easily fit in the traditional dichotomy between theory and evidence.

The spirit of experimentalism, as it emerged in the 1980s, is epitomized in Ian Hacking’s famous statement that “experimentation has a life of its own” (Hacking, [1983](#), p. 150), by which he meant to highlight that our philosophical interest in scientific experiments should not be restricted to their function in theory testing. Subsequently, one prominent alternative proposal concerning the function of experimentation was articulated with the concept of *exploratory experimentation* already mentioned above. For example, Friedrich Steinle ([1997](#)) took this concept to describe a practice of systematic variation of experimental parameters. A second (related) reading of the importance of exploratory experimentation is that sometimes experiments about a given subject matter take place in the absence of theory and, thus, in a context of conceptual openness (Burian, [1997](#); Steinle, [1997](#)). Other scholars also pointed to the poverty of existing philosophical accounts of the purpose of scientific data production. Bogen and Woodward ([1988](#)), in particular, argued against the common assumption that theories predict observations, claiming instead that theories predict phenomena, which are not straightforwardly observable. With this, they pointed to the local and idiosyncratic conditions under which experimental evidence for phenomena is produced. In addition, Woodward ([1989](#)) also argued that one of the functions of experimental research can be to establish the existence of specific phenomena in a given domain, as opposed to testing theories about them. In turn, Feest ([2010](#)) has argued that this kind of activity requires (at least) rudimentary concepts of the phenomena, which inform the ways in which tests about them are operationalized. Importantly, this tradition of thinking about

exploratory aspects of research focuses, among other things, on the *production* of data. This is in stark contrast with the literature about exploratory data analysis we discussed in the previous section, which starts off with *already existing* data.

The practice turn was not restricted to experimental practices, however, but also came with a rising interest in scientific *modeling* as a key feature of scientific practice. Just like Ian Hacking had resisted an understanding of science as aiming at representation (highlighting practices of experimental intervention instead), philosophers interested in modeling also moved away from the thinking of models as mere representations toward an understanding of models as tools of exploration (Gelfert, [2016](#)). This way of thinking was spearheaded by Morgan and Morrison ([1999](#)), who coined the expression “models as mediators,” thereby capturing both the idea of models as instantiating an existing theory (e.g., van Fraassen, [1980](#)) and the idea of modeling as being a form of theorizing, where the latter is construed as a process by which our scientific understanding of a specific subject matter is pushed forward (e.g., Bailer-Jones, [2000](#)). Importantly, on this understanding, theorizing about a given subject matter is not mutually exclusive with its exploration. In other words, models can be seen as allowing researchers to investigate features of their objects of investigation while at the same time engaging in a form of theoretical work about these objects.

When we look at the history of the philosophy of experimentation, it becomes clear that here, too, there was a gradual shift away from thinking of exploration only in connection with a specific type of experiment toward a more expansive understanding of exploration as a type of activity that integrates theoretical and experimental components. This development happened, in part, as philosophers of science engaged with Steinle’s initial juxtaposition of exploratory experimentation and theory-driven experimentation: “[T]heory-driven experiments are typically done with quite specific expectations of the various possible outcomes in mind,” whereas “[e]xploratory experimentation ... is driven by the elementary desire to obtain

empirical regularities” (Steinle, [1997](#), p. 70). This dichotomy was challenged by subsequent works that pointed out that some forms of experiments are aptly described as “exploratory” even though they clearly rely on a lot of theoretical assumptions, including both auxiliaries and assumptions about the very subject matter under exploration (see, e.g., Colaço, [2018](#); see also Feest & Steinle, [2016](#)).

If we follow this line of reasoning, it seems that exploratory experimentation can involve various degrees of theory, raising the question of how clear-cut the distinction between theory-driven and exploratory experiments really is. It is for this reason that we adopt the term “exploratory research,” which we take to refer not to a specific type of experiment but to an ongoing, iterative process of trying to establish empirical regularities and mechanisms that are connected to a given object of research, to delineate the boundaries of that object, and to provide theoretical descriptions of them (Feest, [2025](#), chapter 4). In this regard, we follow O’Malley ([2007](#)), who has argued that the very distinction between exploratory experimentation and theory-driven experimentation may be an oversimplification of real investigative processes, particularly in highly complex domains that increasingly use massive amounts of data. Accordingly, we wish to retain the notion of “exploratory” to do justice to Steinle’s insight about the importance of varying contexts as part of the dynamics of systematic explorations. Importantly, however, we argue that the systematic variation of parameters need not be restricted to the variation of *experimental* parameters. Mathematical modeling and computer simulations can play a very similar role and, thus, should be included in the repertoire of tools used in exploratory research.

On the Role of Mathematical Models in Exploratory Research

The upshot of the above is that we retain Steinle’s ([1997](#)) insight that exploratory research involves the systematic variation of parameters while following more recent work in philosophy of science in rejecting

the idea that such a research activity is (or should be) theory-free. In this section, we argue that this way of thinking about exploratory research dovetails with recent work on exploratory modeling, which has also argued that modeling can be exploratory despite being closely tied in with theoretical work.

The term “model” is notoriously hard to pin down as it refers to many kinds of things, ranging from physical objects to fictional and abstract objects to set-theoretic structures and equations (cf. Frigg & Hartmann, [2024](#)). There is widespread agreement, however, that models and modeling can serve a range of epistemic functions, such as prediction, explanation, guiding data collection, discovering new questions, bounding outcomes to plausible ranges, illuminating uncertainties, demonstrating tradeoffs, and challenging robustness of scientific theories by introducing perturbations (Epstein, [2008](#)). Many of these functions involve some level of exploration in the theoretical problem space, in the model space, or in the data.

To understand what this means, we highlight a key distinguishing characteristic of model-based approaches in science, namely, that they rely on mappings. These mappings can be physical, verbal, formal, and so on. It is by virtue of such mappings that modeling can help researchers explore, understand, and explain complex unknown phenomena (e.g., mind and behavior, see van Rooij, [2022](#)) by studying a simpler model system that resembles the target system in certain desired aspects. Modeling may, thus, allow researchers to engage in indirect exploration of a target system, even when they set out to test theoretically informed hypotheses about the behavior of the model system. For example, we may set up an *in silico* experiment to test whether a formal model behaves in a certain way because of a specific assumption and manipulate that assumption via simulations. As a result, we will be able to make a decision about the sensitivity of model outcomes to that assumption, but we won't have tested whether that assumption holds for the target system. We will, however, have explored the potential implications of certain conditions for the target system.

Clearly, the modeling activities described in the previous paragraph are “theoretical” in some sense, insofar as they rely (minimally) on decisions about how to represent the target system. In addition, the existing literature about formal modeling offers a range of proposals as to how exploratory modeling can play out for different target systems in ways that contribute to theorizing. For example, when the target system is a theoretical framework, formal modeling invites exploration into the nature of theories by enforcing an explication of their assumptions and implications (Smaldino, [2017](#)). Particularly since formal models need to be expressed in mathematical language or computational code, creating a full and transparent specification of the knowns and unknowns of the theory becomes a necessity. Suppes ([1962](#)) has argued that even in the absence of sufficient theoretical development, models of experiment and models of data can be used to guide scientific inference. This is relevant for our purposes because in psychology it is common to perform experiments with the aim of testing specific hypotheses in the absence of well-specified theories (Feest, [2025](#)). Guest and Martin ([2021](#)) suggest that in these situations formal modeling may induce open theorizing and help make opaque ideas underlying the hypothesis more transparent and explicit.

Suppes ([1962](#)) provides a broader analytical framework by conceptualizing research as involving a hierarchy of models. He defines *models of data* as concerning all aspects of the experiment that can be used in statistical tests aimed at testing the adequacy of theories such as homogeneity and invariance of effects. By contrast, he argues that *models of experiment* exist at a higher level in the model hierarchy and involve the choice of experimental parameters such as sample size. Crucially, Suppes believes that when theoretical knowledge consists of a collection of heuristics rather than a well-specified logical structure, models of experiment and data could still be employed to guide empirical research approaches. In this way, his account captures our contention that formal modeling can be exploratory. In the kind of model hierarchy proposed by Suppes, it’s not necessarily the nature of modeling activity that differs between levels so much as the target

system realized by the model, whether it be a theory, an experiment, or observations about a phenomenon. Regardless of the level models operate in, specifying what they are helps researchers articulate the logical arguments they are making about the relationship among parts within a target system, lay out all assumptions, and examine the appropriateness and implications of these assumptions. This counterfactual ability to infer conclusions that follow from given assumptions allows modelers to revise their models by juxtaposing them with reality. Such an iterative process aims to move between different levels of the model hierarchy by rigorous triangulating, testing, and updating (Devezer & Buzbas, [2023](#)).

This big-picture view of scientific modeling helps us appreciate the role models can play in exploratory research. In particular, the above sketch of formal modeling (a) captures the iterative character of exploration and (b) highlights the role of counterfactual reasoning in the exploratory process. Pursuing a similar argument, Massimi ([2019](#)) has argued that “targetless” and “hypothetical” models can help us generate modal knowledge about what is causally or objectively possible, “thanks to their specific kind of imagining or conceiving nonveridical scenarios” (Massimi, [2019](#), p. 871). In computational approaches, this is often achieved via performing simulations under a formal model and observing the sensitivity of the model to changes in initial conditions (Gelfert, [2016](#), p. 73). This type of procedure is also illustrated by Devezer et al. ([2019](#)), which advances a mathematical model of the scientific process to study the sensitivity of certain scientific outcomes (such as reproducibility of results and speed of discovery) to changes in parameter values (such as different research strategies, level of system noise, and choice of statistical methods). The authors argue that in-depth exploration of this model under full model specification allows the researchers to observe plausible outcomes under most conservative conditions as well as the boundaries of what is possible under extreme assumptions.

From the above discussion, it follows that modeling can be exploratory in at least four interrelated senses:

1. Theoretical exploration: Modeling can be used to explore the implications of an already fully formalized theory with a clear logical structure. In this sense, it may help generate theoretical predictions or hypotheses to be tested and guide data collection.
2. Conceptual exploration: Modeling may help explicate hazy theoretical assumptions about the subject matter, thereby making them less hazy and allowing for their robustness to be probed by introducing perturbations. In this sense, it aids in the gradual, iterative building of theories.
3. Data exploration: Empirical modeling may help explicate assumptions made in experimental design and explore the relationship between design constraints and the data generated. To this end, model exploration does not depend on any degree of formal theorizing and can be minimally or vaguely informed by some informal theoretical background.
4. Modal exploration: Modeling can be used to explore the boundaries of what is possible and what is inevitable under different theoretical assumptions or initial conditions. In this capacity, it may inform the discovery of new research questions, expose theoretical gaps, and identify plausible bounds for parameter values.

Conceptual Openness in Exploratory Research

In the previous section, we argued that model-based simulation as an exploratory activity is akin to exploratory experimentation as it involves varying model (vs. experiment) parameters and observing the implications to map out the outcome space. As we have laid out, both experimentation and modeling can be exploratory even if they (a) presuppose theoretical or conceptual assumptions as auxiliaries or (b) contribute to the development of a theory of the object of exploration.

In this section we turn to a second necessary ingredient of exploration that was briefly touched on above, namely, the idea that

exploration involves venturing into an unknown or little-understood space. Within the literature about exploratory experimentation, this has at times been described in terms of the idea of *conceptual openness*. Indeed, for Steinle ([1997](#)), a crucial feature of exploratory experimentation was that it could result in a conceptual restructuring of an entire field. Another way of putting this is to say that exploratory research is required when the descriptive and explanatory concepts for a novel domain are not yet well articulated. As laid out in the previous two sections, the notion of *conceptual openness* should not be confused with the idea that researchers start with a blank slate. The idea, rather, is that exploratory research is called for when it is not yet clear to what extent existing concepts are applicable. As a result of exploratory research, then, concepts can get extended and changed in sometimes unanticipated ways.

The notion of exploration as taking place in conceptually open spaces while necessarily building on existing concepts is also hinted at by Gelfert ([2016](#)), who points out that exploration can be specific (targeting a particular object) or nonspecific. Both types of exploration presuppose conceptual assumptions. They only differ in the extent to which they draw on prior assumptions and in the level of specificity of those assumptions. Illustrating this with a common-sense example, we may say that when someone explores an unknown environment in search of food, they will clearly presuppose not only the concept of *food* but also some basic criteria of how to distinguish food from non-food (such as smell and look). This is perfectly compatible with the idea that such exploration can result in the discovery of previously unknown forms of nutrition and extend our very concept of what is edible. Haueis ([2023](#), [2024](#)) has recently begun developing a principled philosophical account of the relationship between exploratory research and conceptual extension in science. Turning specifically to psychology, such an understanding of exploration as conceptual articulation and extension is also hinted at by Hutmacher and Franz ([2025](#)), who argue that the inherent vagueness of psychological concepts calls for research that explores the boundaries of their referents.

Within the history and methodology of psychology, we encounter a similar sentiment already in Paul Meehl's ([1978](#)) pronouncement that the scientific concepts of psychology are "open." According to Meehl, this openness arises as a result of "the indefinite extensibility of our provisional list of operational indicators, [...] the empirical fact that indicators are only probabilistically, rather than nomologically, linked to the inferred theoretical construct, and the fact that most of our theoretical entities are introduced by an implicit or contextual definition" (Meehl, [1978](#), p. 815). This statement acknowledges that psychological concepts are laden with pre-theoretical content, which needs to be both exploited and explored (see also Feest, [2025](#)). As Meehl points out, psychological concepts are rarely fully explicated, prompting him to call on psychologists to "learn to be sophisticated and rigorous in their metathinking about open concepts at the substantive level" (p. 815) and to embrace the possibility of "conceptual drift" (p. 816). Meehl envisions such conceptual drift to occur in close coordination with "psychometric drift" (p. 816), by which he means the principled improvement of measurement tools. This is important because it reveals an understanding of the exploratory process as involving an iterative process of improving measurement tools (and, thus, empirical data) and developing one's concepts. We argue that this vision of the investigative process in psychology is highly compatible with the account of exploratory research we have sketched in the previous sections. Recall, for example, that one of the rationales for exploratory modeling was that such modeling forces researchers to make the hazy content of their concepts explicit, thereby allowing for their investigation by way of systematic variation of model parameters. Likewise, Meehl's emphasis on the close connection between psychometric and conceptual progress also points to the need for a notion of exploratory research that does not begin after the process of data generation (in the service of a specific hypothesis test) has been completed but begins with the very design of the experiments and models as well as the choice of tests that enable the generation of data.

Summing up, we endorse a notion of *exploratory research* that highlights two crucial features: the systematic variation of experimental and model parameters on the one hand and the conceptual openness of the research situation on the other. As we have argued, both of these features are compatible with the idea that the exploratory process can be guided and constrained by conceptual or theoretical assumptions as well as with the idea that exploratory research can contribute to theory formation. For example, the systematic variation of parameters (both in experiments and models) can be a tool for theory construction and conceptual development.⁴ Moreover, we have pointed out that our analysis is compatible with a range of existing positions in data science, formal modeling, and philosophy of science going back several decades. On this account, it is an essential feature of exploratory research that it unfolds across a variety of research activities and projects over time rather than within the bounds of a single project or research article. In our view exploration can start from theories, vague conceptualizations, apparent paradoxes, observations, and even specific research results and it can continue until the target object is fully fleshed out and most of the questions surrounding it—whether they’re modal, theoretical, conceptual, or empirical—have been raised and examined. In this regard, our account of what is *exploratory* in exploratory research is—in a certain sense—much broader than the one that is implicit in the confirmatory/exploratory distinction (Wagenmakers et al., [2012](#)): it pertains not only to a specific kind of data analysis but to a larger investigative endeavor that can also include theorizing, modeling, experimenting, etc. Moreover, it provides us with diagnostic tools to talk about the standards that high-quality exploratory research should meet. As we will argue in our concluding section, those standards are, ultimately, more demanding than those implicit in Wagenmaker et al.’s account of exploratory data analysis. The remaining question, then, is why our analysis should be preferred.

Conclusion: Why it Matters

Per the analysis presented in this chapter, exploratory research is not an individual, clearly delineated, precise research activity but a process of mapping a given object of research in a theoretical and empirical space, by iteratively negotiating between theory and data to first make boundaries, unknowns, uncertainties explicit, and then gradually uncover what is knowable. In our analysis, exploratory research can occur both against the backdrop of strong theory and in situations that are characterized by conceptual openness, though (especially in this latter case) it needs to be informed by the aim to do a thorough search of the space of possibilities.

This understanding of exploratory research differs from exploratory data analysis as conceptualized by Wagenmakers et al. ([2012](#)) and adopted in the post-replication crisis metascience literature. As we explained above, that literature restricts exploration to statistical analyses that are formulated and performed after having seen experimental data. The only aim of such analyses is to generate hypotheses by singling out statistical patterns emerging in the data. Thus, according to the view we reject here, the defining characteristic of exploratory analysis is that it does not test hypotheses determined prior to any knowledge of the data.

Now, the question might be raised whether this is a merely terminological point. Surely, Wagenmakers et al. ([2012](#)) and other authors are entitled to use the term “exploratory” in any way they want. We have argued in this chapter that their notion of exploration is counterintuitive insofar as it does not seem to require background assumptions or systematic search. However, this does not imply that there might not be a perfectly legitimate, more restricted notion of *exploration* that applies to the kind of exploratory data analysis suggested by them. Perhaps our broader notion of exploration is compatible with the more narrow one proposed by them? To argue against this possibility, we need to show that a purely data-driven, non-theoretical understanding of exploratory data analysis is not only counterintuitive but also epistemically counterproductive.

To examine this question, let's pick up the common argument for preregistration that we presented at the outset, namely, that it is a valuable tool (a) to prevent researchers from misrepresenting their post hoc theorizing as confirming a hypothesis, and (b) to distinguish confirmatory from “merely” exploratory research. Our analysis grants that preregistration can contribute to transparency. However, we are skeptical of the way this argument has been tied to the distinction between confirmatory and exploratory data analysis. For one thing, a hypothesis can be confirmed (or disconfirmed) by the data regardless of whether the analysis was preregistered (provided that it was done by an honest researcher). But here we are interested in a more substantial question, namely, whether exploratory data analysis (again, in the sense of Wagenmakers et al.) is either necessary or sufficient for that analysis to be truly exploratory. We argue that it is not.

The non-necessary part would likely be acknowledged by advocates of exploratory data analysis since they don't need to commit to exploratory data analysis being the *only* kind of exploratory research. Once this is granted, however, this calls for a more in-depth analysis of the notion of *exploratory research*. Such an analysis has been provided in this chapter. A crucial upshot of our analysis is that exploratory research starts out by acknowledging the conceptual openness of the research situation, while also making use of existing presuppositions and exploring the relevant epistemic space. This is done by varying experimental or model parameters in order to further the theoretical or empirical understanding of a given subject matter. Once this is acknowledged, however, it seems that exploratory data analysis also cannot be sufficient for exploratory research. The data of such an analysis are the outcome of an experiment that was designed to test a specific hypothesis. So, there is a sense in which the data exhibit a path dependence on that previous hypothesis. But there is no guarantee as to the quality of that hypothesis, and hence it is not clear that the data are of any relevance to the subject matter. This means that the to-be-explored space is potentially already severely hampered by the availability of only a limited set of data. This runs counter to a feature

we have identified as important to exploration, namely, the aim to map the space of possibilities. If, on the other hand, researchers have conceptual or theoretical reasons for believing the data to be of interest, this reveals that they are implicitly acknowledging our thesis that exploratory research is an iterative and ongoing process that builds on and expands existing conceptual, theoretical, and instrumental tools. In that case, we argue, exploratory research requires that those tools be made explicit so that they can be analyzed, discussed, and probed more systematically.

Summing up, in this chapter we have drawn on some relevant philosophy of science literature to argue that the notion of exploration implicit in the dichotomy between confirmatory and exploratory research is quite impoverished, and therefore insufficient for fully capturing the nature of scientific exploration. By this, we mean not merely that it only covers a part of the exploratory process (i.e., the data analysis). Rather, we argue that if our understanding of exploration is adopted, this reveals either (a) that exploratory data analysis, on its own, does not meet the criteria for exploratory research at all (even if, by chance, it occasionally happens upon an interesting hypothesis), or (b) that whatever makes such analyses exploratory (e.g., drawing on existing conceptual and technological tools with the aim of investigating possibility spaces through modeling or experimenting) needs to be brought out into the open. This is the sense in which we fully embrace the open science movement's call for transparency (see also Leonelli, [2023](#) for an account of open science that we are sympathetic to).

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Footnotes

¹ This asymmetry between what is allowed in exploratory (vs. confirmatory) research is in line with the common perception that exploratory research is inherently less rigorous than confirmatory research. As will become clear in this chapter, we believe this common perception to be mistaken.

² Notably I. J. Good is a Bayesian statistician, and his perspective on EDA is consistent with Cromwell’s rule coined by another Bayesian statistician, Dennis Lindley—a rule that reminds us of the importance of maintaining a sense of doubt in our reasoning. Good goes beyond the categorical framing of Cromwell’s rule, however, to highlight that we should not readily accept an explanation emerging in exploratory data analysis if we attach to it a vanishingly small prior probability. On the flipside, this would also suggest that if we start exploration with too strong an idea of how to explain the patterns, we would not be able to change our minds in light of the data.

³ This distinction has traditionally often been understood as implying that the rationality of science plays out in the logic of theory testing only, whereas processes of theory-generation are not—and need not be—governed or constrained by rational method and therefore do not fall within the purview of philosophy of science. While this reading of the distinction seems to be at the heart of the intuitive appeal of the exploratory/confirmatory dichotomy, there is a long-standing consensus within the philosophy of science that it is problematic, though there are several different construals of what makes it problematic (Hoyningen-Huene, 1987). For our purposes, it suffices to say that normative analyses of knowledge-generating activities have become an established part of contemporary philosophy of science.

⁴ As similar examples of novel forms of exploratory experimentation, we may contrast the notion of metastudy by Baribault et al. (2018), who suggest radically randomizing experimental parameters within a so-called metastudy for the purposes of robust theory testing, with the notion of integrative experimental design by Almaatouq et al. (2024), who suggest mapping the design space of possible experiments associated with a research question to inform iterative theory development.

4. Theory and Model Building in Experimental Psychology: Learning from Past Mistakes?

Joachim Funke¹ 

(1) Department of Psychology, Heidelberg University, Heidelberg, Germany

 **Joachim Funke**

Email: joachim.funke@psychologie.uni-heidelberg.de

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Background¹

Psychology is in crisis—not for the first time (see also Hutmacher & Franz, [2025](#))! And we are not just talking about a *replication* crisis (see, e.g., Pashler & Wagenmakers, [2012](#); in opposition to that thesis: Fiedler & Schwarz, [2016](#); Fiedler et al., [2021](#)), but a crisis that could be due to a misguided methodological orientation. Large parts of our discipline, psychology, have a difficult relationship with everything that cannot be represented in objective figures.

In another essay, I posed the question of whether physics is the right model for the structure of psychology or whether biology, with its system-theoretical orientation, would be a more suitable model discipline (Funke, [2024](#)). I was inspired by the biologist Norbert

Bischof, whose most recent work “Theoretische Psychologie” [Theoretical Psychology] is nearing completion. He writes:

It seems to me that psychology came to a fork in the road relatively early on the way to its scientific self-image and took a wrong turn. As a result, it ended up in a dead end from which it has still not found its way out. (Bischof, [2024](#), p. 19; translated by J.F.)

Some people think that the problems in psychology could have to do with a lack of theorizing (e.g., Oberauer & Lewandowsky, [2019](#)). I am convinced, too, that the neglect for theoretical psychology which can be recognized, to give but one very specific example, by the institutional lack of chairs for Theoretical Psychology at German Psychology Departments is detrimental to our discipline.

What is the way out? Do we need to be even more careful in our data collection or has the over-emphasis on data collection perhaps been the mistake? Is the solution perhaps an increasing focus on theory rather than empiricism (see Funke, [2023](#))? This chapter will take a closer look at the process of theorizing. How does one build a good theory? What are the criteria for a good theory? Learning from past mistakes for the future: This could be one of the solutions.

The chapter starts with a description of the process of theory formation, illustrated by my own field of research, namely, sequential decision-making in complex environments. Then I will discuss the question of quality criteria for theories; finally I will come up with a conclusion.

A Theory of Problem Solving in Complex Environments

Sequential decision-making in complex environments is a field of research which can illustrate the process of theory formation, that is, show how a theory can be developed. I follow a deductive methodology

(Erdfelder & Funke, [2004](#)). Deductive methodology goes back to Sir Karl Popper ([2002](#)), whose ideas have been adapted to psychology by Bredenkamp ([1972](#)) or Gadenne ([2004](#)). We start by listing the components required for a theory.

The Components of a Theory

To create a theory, one needs various components: (1) A *domain* in which empirically observable (2) *phenomena* exist, to whose understanding the theory aims to contribute at a defined (3) *level of resolution*. There need to be (4) *theory elements* that are (5) *structurally related*, (6) *make specific predictions*, and (7) *prohibit certain events*. In addition, (8) *some other details* are desirable.

The Domain

The domain is the area in which the facts and phenomena of interest can be found. Newell and Simon ([1972](#)), for example, chose chess as the domain for their theory formation in the area of problem solving. For a theory of problem solving in complex environments, corresponding environments should be named and characterized in more detail (see, e.g., the criteria for this in Dörner & Funke, [2017](#)).

The Phenomena

The phenomena in need of explanation should be identified. At the same time, this provides indications of the data to which a theory should refer. Dörner's ([1989](#)) article on the "little green turtles" is a nice contribution that contains a plea for phenomenon-driven research. In Dörner's example, the little green turtles' strange movement behavior remains inexplicable to experimentalists but is recognized by an untrained child. While the child observes exactly what happens in the phenomenon area, the experimentalists are blinded by their theoretical preconceptions. In the phenomenon area of "problem solving in complex environments," for example, it needs to be explained why some people can deal with complex challenges quite well, while others are hopelessly overwhelmed. However, the obvious idea that

“analytical intelligence” is responsible for this competence must be quickly discarded (Wenke et al., [2005](#)). “Operational” intelligence (Dörner, [1986](#)) or—somewhat more modern—“systems competence” (Funke et al., [2018](#)) would be the most likely candidates.

The Resolution Level

The level of resolution determines the fineness or coarseness of the analysis: If the focus is on neuronal processes, a higher (finer) level of resolution is sought than if the focus is on actions. One could also speak here of the “unit of analysis” at stake. In my opinion, actions are the level that is optimal for psychologists. Those who are interested in neuronal or biochemical processes are better off in medicine (or biochemistry). I doubt whether we teach the necessary skills (for a medical or biochemical approach) in psychology studies. Instead of digging for gold on foreign terrain, we should dig in our own field—for me this means searching for cognitions, emotions, and motivations that guide our actions. Deciding on a level of resolution is often also linked to a decision on the research methodology to be used: If you want to investigate neuronal processes, electrophysiological measurement methods or imaging techniques are appropriate. If action-guiding factors are to be identified, different methods need to be used that are able to capture the *meaning* of human actions. Baumeister et al. ([2007](#)) point to degenerated research methods in which self-reports and finger movements on keyboards form the main source of data for psychologists, but the actual behavior of a person falls by the wayside. So, the appropriate resolution level for a theory of complex problem solving should be the actions that a problem solver shows during her or his solution attempts, including, besides cognitions, the ongoing emotions and motivations.

The Theory Elements

The theoretical elements of an artifact are listed in a “parts list” (e.g., perception units, memory unit, processing unit, output units). It may not be clear from the parts list how the individual parts can be put

together. But one thing is certain: If just *one* of the parts is missing, the overall result may be at risk (there are more important and less important parts, but their importance cannot be seen immediately). If a spring or a cog remains in a disassembled wristwatch, a mistake has probably been made during reassembly that calls the functionality into question.

For a theory of problem solving in complex environments, Frensch and Funke ([1995](#)) proposed a (rough) list of parts, for example, the task area consisting of “givens, goals, barriers, and tools.” But in contrast to a wristwatch, the parts are unclear; they consist of concepts. And in contrast to physical objects, psychological concepts are mostly “fuzzy” (see, e.g., Hutmacher & Franz, [2025](#)).

The Structure of the Elements

One of the most important concepts for describing the structure of the elements is the problem space. It contains the mental representation of the problem and, according to Simon and Lea ([1974](#)), comprises the *rule space* (with all known rules for solving the problem) and the *instance space* (with all possible states = instances). For well-defined problems, the instance space describes all possible states of the facts involved and thus always contains both the starting point and the goal of a simple problem.

For example (see Fig. [4.1](#)), the “Tower of Hanoi” (a “*Drosophila*” of problem-solving research) has for the variant with 3 pegs and 3 disks 27 possible rule-compliant arrangements. Typically, a problem solver starts with the arrangement at the top (all three disks on the left peg) and should reach the state at the bottom right (all three disks on the right peg) with as few steps as possible (ideally, $2^3 = 8$ moves).

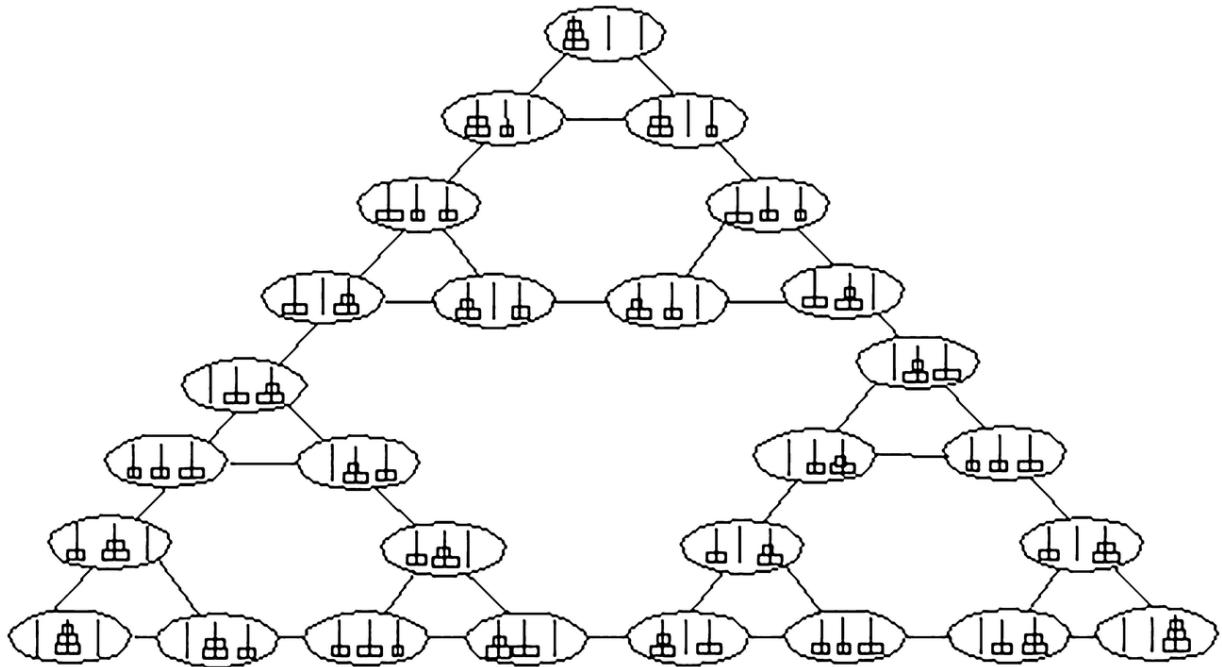


Fig. 4.1 Tower of Hanoi. Illustration of the complete problem space for the “Tower of Hanoi” with three pegs (starting point, on top; end point, bottom right). (Source: own drawing)

Process Assumptions

It is not enough to simply make structural assumptions. It is also important to specify the concrete processes. This can be done, for example, through “cognitive modeling,” as is possible within the framework of ACT-R (using the example of Donald Broadbent’s “SugarFactory”; Said et al., [2016](#)). Structural and process assumptions belong together: Processes run within certain structures (flowcharts are often used to illustrate process flows).

For example, think of the “structure and process model of information processing” (SPIV model; Hussy, [1983](#)) shown in Fig. [4.2](#) (it is a conceptual theory, not a model; see the differentiation below).

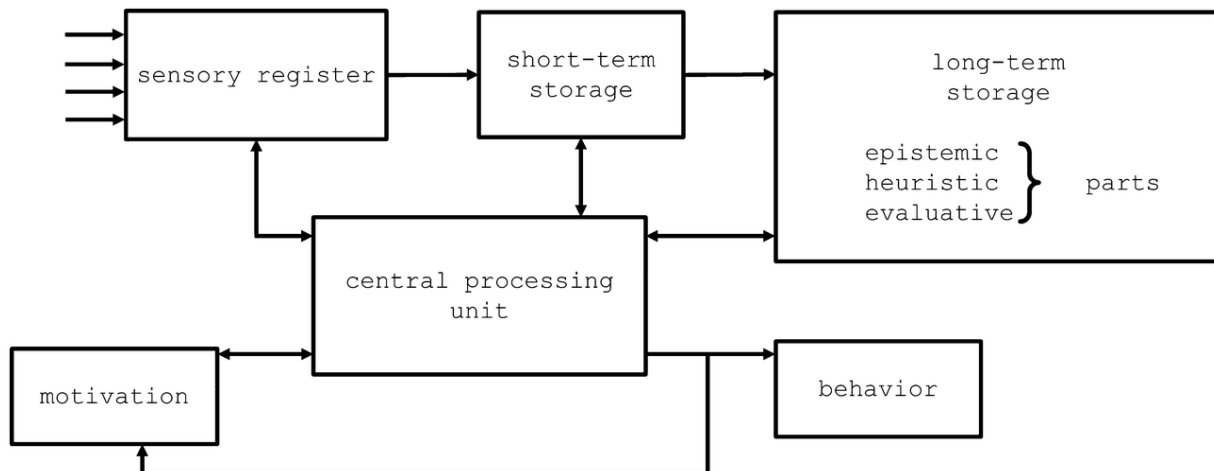


Fig. 4.2 Structural components of the SPIV model. Overview of the structural components of the SPIV model (SPIV = “Struktur- und Prozeßmodell der Informationsverarbeitung” i.e., structure and process model of information processing; from Funke & Hussy, [1979](#), p. 3). (Source: own drawing)

The model in Fig. [4.2](#) not only contains a list of parts, but the arrows between its parts also illustrate the sequence of hypostatized information processing (the interaction of the various structural components). It is not without reason that the SPIV model resembles the circuit diagram of a computer—it was the time when the computer metaphor of the mind (Gigerenzer & Goldstein, [1996](#)) was predominant. Today, we are celebrating the rediscovery of human versus artificial intelligence (Fuchs, [2022](#)). The computer metaphor has had its day; the differences are now clearly recognized.

This might be a good place to discuss the difference between a theory and a model: I will follow here the proposal made by Smaldino ([2020](#)). Theories are mostly presented in verbal form; they describe complex systems (like human cognition). Concerning models, Smaldino differentiates between equation-based models and agent-based models. Equation-based models use equations, for example, Weber’s law about the just-noticeable difference (JND, the smallest change in stimuli that can be perceived). Agent-based models “are a particular class of computational models in which individual agents (which are often meant to represent people, but agents can also represent anything from biological cells to economic firms to political

municipalities) are simulated as explicit computational entities” (Smaldino, [2020](#), p. 208). For example, Baumgaertner et al. ([2016](#)) were interested in the dynamics of opinions. They write:

We introduce a stochastic spatial agent-based model of opinion dynamics that includes a spectrum of opinion strengths and various possible rules for how the opinion strength of one individual affects the influence that this individual has on others. Through simulations, we find that even a small amount of amplification of opinion strength through interaction with like-minded neighbors can tip the scales in favor of polarization and deadlock. (p. 207)

Predictions

Predictions and hypotheses are at the heart of theories. They specify the world of expectations against the background of a theory. Good hypotheses should be testable (they should be able to fail, at least in principle) and have the widest possible scope.

Take an example. Against the background of Festinger’s dissonance theory (Festinger, [1964](#)), the hypothesis is formulated that the reduction pressure increases with increasing dissonance. You buy an object from a store A at a price of X and discover the same object in the neighboring store B at a price of Y, where $Y < X$. The greater the price difference (X-Y), the greater the pressure to justify a possibly overpriced and therefore bad purchase (possible lines of justification: Warranty conditions are better at A, A offers exchange options, retailer A is more reputable, etc.). In the area of dealing with complex environments, for example, predictions are such that analytical intelligence is not sufficient to predict good solutions (Wüstenberg et al., [2012](#)) or—to put it positively—that “operational” intelligence is required (Dörner, [1986](#); Firnhaber, [2022](#)). The reason for this judgment is that complex environments require—besides intelligence—emotional stability and motivational strengths (see Dörner & Funke, [2017](#)).

Predictions can concern point events (the alleged prediction of a solar eclipse on May 28, 585 BC, by Thales of Miletus; see Wenskus, [2016](#)) or entire patterns or functional processes (e.g., the prediction of case numbers of an infection event by the Gompertz function; see the blog post by Dörner, [2021](#)).

The Forbidden Events

In the spirit of fallibilism sensu Sir Karl Popper, a theory should not only make predictions, but also name “forbidden” events that could be used to falsify the theory. If a theory agrees with all occurrences, you have a “perfect” theory at first glance (weather prediction: “If the cock crows on the dunghill, the weather changes or it stays as it is”). However, if a theory such as the weather theory just mentioned does not prohibit *any* constellation of events, it has no explanatory value and is useless. The more events are forbidden, the better the theory because there are more opportunities for falsification. The theory takes an increased risk to fail. The position that psychology does not really do a great job at creating falsifiable theories can be found in Meehl ([1978](#)) and, more recently, in Eronen and Bringmann ([2021](#)) who summarize:

Meehl pointed out that psychological scientists are fond of developing new theories, but instead of resulting in cumulative theoretical progress, these theories tend to just come and go: Theories are neither decisively refuted nor accepted as part of established knowledge; they simply hang around until they are abandoned or forgotten. (p. 779)

When dealing with complex environments, for example, it must not be assumed that they are easier to handle than simple environments. It is forbidden that there is only one correct solution. Also, it is forbidden for people without systemic understanding to provide better solutions than those with systemic skills. This list can be (and should be!) extended.

Other Information

Of course, it would be desirable to reveal the image of humankind (in German: *Menschenbild*) that usually remains hidden. It is not irrelevant whether the theory is based on a mechanistic or humanistic view of man: Systems theory (describing technical systems) and action theory (describing systems with intentions) should be kept apart (Herrmann, [1982](#)), as technical and human systems are not the same. The medium must also be specified: Most theories of psychology are formulated in the medium of “language”; a few approaches use formal models (e.g., Steven’s power law), and others are (re-)constructed in structuralist terms (Westermann, [1987](#), [2004](#)). Of course, a transfer of verbally formulated theories into formal structures is a gain in precision and thus a desirable step that is a hallmark of “mature” science (Smaldino, [2020](#); but see Hutmacher & Franz, [2025](#), for a warning against premature formalization). And we have known since Habermas ([1968](#)) at the latest that there is an interest in knowledge behind all research. This applies not only to empirical work, but also to the development of theory. Nowadays, good practice in scientific work therefore includes the disclosure of possible conflicts of interest.

For the linguistically formulated theory of dealing with complex environments, which is the focus here, I assume a humanistic view of humankind (humankind as a goal-oriented agent). The interest in knowledge is driven by the major societal challenges (Wissenschaftsrat, [2015](#)) that humanity is confronted with and which place special demands on dealing with complex environments.

What Distinguishes Good Theories from less Good Ones?

It is obvious that theories can be differentiated with regard to a number of aspects. Is it possible to derive a “ranking of quality” from this? “An overview that has been tremendously helpful is the list of quality criteria compiled by Prof. Edgar Erdfelder ([1986](#)) of Mannheim University. This list contains three main and five secondary criteria that

can be used to decide on the quality of a theory. The three main criteria (the order of which is unimportant) are:

- (a) Logical consistency: Can no contradictory statements be derived from the theory with certainty?
- (b) Empirical content: Can the theory potentially be empirically refuted (falsified)? How large is the number of falsification possibilities?
- (c) Empirical proof: How many non-trivial falsification attempts has the theory successfully withstood? Is the theory burdened with findings contrary to the theory?

The secondary criteria (in order of importance) are:

1. Practical-technological relevance (applicability): Does the theory in an obvious way open up technological application possibilities?
2. Emancipatory relevance (in the sense of Holzkamp, [1970](#)): Does the theory contribute to people's self-enlightenment about their societal and social dependencies?
3. Heuristic value: Does the theory stimulate new empirical research paradigms and questions?
4. Integrative value: Are other theories summarized by the theory in question and thus rendered superfluous?
5. Precision: Is the theory explicable in a precise formal language and possibly even axiomatized?

An additional thought that is important in this context is the following: "It is not the proof that the possibility of an error is excluded, but the possibility that an error can be proven that is required for the empirical sciences and for empirical psychology in particular!" (Erdfelder, [1986](#),

translated by J.F.). This sentence is a “meta-statement” that lies behind the mentioned criteria—a kind of summarizing comment.

Are there “Fake” Theories?

When it comes to good and less good theories, one may also raise the question: Are there “fake” theories (i.e., propositional systems that look like a theory but *are* not a theory—kind of “cargo cult” in the sense of Feynman, [1974](#))? The answer is “yes”—there are theories that appear in the guise of a theory without rightly bearing the attributes (see also Macho, [2016](#)). We approach this question in three ways: (a) Using the example of conspiracy theories, (b) using the example of pseudo-theories, and (c) using the example of narratives.

Conspiracy Theories

The best-known example of fake theories is the conspiracy theories circulating in large numbers (see, e.g., the compilation in Nocun & Lamberty, [2020](#)), which are known to be dogmatic (not teachable) and should therefore be called conspiracy ideologies or conspiracy myths (Lewandowsky & Cook, [2020](#), describe how such ideologies and myths can be tackled). This type of “fake” theory can be easily recognized by its resistance to falsification attempts.

Pseudo-Theories

While conspiracy theories are relatively easy to debunk, this is more difficult with the following genre of “pseudo-theories.” In his essay “Traps and Gaps in Action Explanation,” the Hildesheim psychologist Werner Greve ([2001](#)) takes up an argument by Jochen Brandtstädter ([1984](#)) regarding “a priori elements” in research programs that first needs to be explained. A priori elements are those elements that are true from the outset (a priori means without any form of empiricism); Kant called such truths “analytic” in contrast to “synthetic” truths, which can only be described as true after verification in empirical research. The statement “bachelors are unmarried” is a priori true

without any perception of reality. To test the truth of the sentence “Smoking causes lung cancer,” on the other hand, good empirical research is required (see for this example Pearl & Mackenzie, [2018](#)). Brandtstädter ([1984](#)) has pointed out that hidden a priori elements could promote pseudo-empiricism, for example, sociological research programs on the marital status of “bachelors,” which could make this topic the subject of representative surveys. The pointlessness of such empiricism is, however, more difficult to recognize when it comes to sentences such as “Alcoholics suffer from loss of control even after consuming small amounts of alcohol” (this statement is not empirically substantive if the definition of an alcoholic sets loss of control as a criterion for alcoholism). Similar arguments against pseudo-empirical science can be found in Smedslund ([2016](#)).

Greve ([2001](#)) looks at one of the most successful theories in psychology, the “theory of planned behavior/reasoned action” (cf. Ajzen, [1991](#); Fishbein & Ajzen, [2010](#)). This theory predicts the occurrence of behavior with reference to a previous declaration of intention, which in turn is made dependent on the subjective norm, the attitude toward the behavior in question, and one’s own perceived behavioral control. Greve’s argument is the following: Is the relation between intention and behavior really a causal relationship, or is it rather an a priori element so that the empirical evidence seemingly confirming this relationship should rather be treated as a case of pseudo-empiricism? Doesn’t the corresponding intention logically belong to *every* conscious action? If intention is one of the a priori elements of an action (as it presumably is), is the repeated empirical confirmation of this connection not equivalent to the (fictitious) representative survey about the marital status of bachelors? One would hardly want to celebrate this as a result of empirical research—these are a priori elements in a research program that cannot be considered empirical.

Narrative

Finally, the question remains as to whether the term “narrative,” which has recently become popular in the political context (Hofmann et al., [2014](#)), describes a theory. Narratives are stories that revolve around certain events and are intended to give them meaning. Narratives are regarded as meaningful stories, as images with a high degree of appeal (for a recent review, see Green & Appel, [2024](#)). It is probably a buzzword from the feuilleton, but is also often used in the humanities, where it is more likely to be used to describe a method (e.g., Riessman, [2008](#)) than to characterize theoretical scientific ideas. Narratives are “fake” theories, as they do not provide any predictions and cannot be falsified; at best (from an evidence-based point of view), a narrative does not “fit” the facts it is supposed to describe. For instance, the narrative of “rags to riches” describes the American dream of social advancement, but as a narrative it is no substitute for a sociological class model for social advancement or social relegation.

What Were the Mistakes?

Although we seem to have sophisticated ideas about how to build theories and how to distinguish good theories from less good ones, psychology is facing crisis debates. This indicates that something must have gone wrong. So, what *were* the mistakes? And what can we learn from these mistakes? I want to mention at least three mistakes:

First, *the strong orientation toward the natural sciences* (see my previous comments about physics as the leading science): Psychology is not a purely natural science but in difference to a science of dead objects (i.e., physics) psychologists deal with living organisms in social contexts.

Second, *the devaluation of methods other than the experiment*. I recommend taking the following advice seriously: “The truth is that no method will provide a panacea: Different methods have different advantages and disadvantages, and by using multiple methods, one capitalizes on the strengths of the methods while helping to minimize the effects of their weaknesses” (Sternberg & Grigorenko, [2001](#), p.

1072f). This advice means that methodological pluralism is needed—not a monism that allows only one way to knowledge.

Third, *the underestimation of theoretical work compared to data collection*. Maybe this is the biggest mistake. To paraphrase Kant, who once said, “Gedanken ohne Inhalt sind leer, Anschauungen ohne Begriffe sind blind” (in English, usually translated as, “Thoughts without content are empty, intuitions without concepts are blind”), I would say: Data without theories are blind, theories without data are empty. The argument is not “theories first”: We need both theories and data. If psychology wants to be an empirical science, it needs data to be empirical and it needs theories to be called a science.

As an aside, it should also be noted that an orientation toward evolutionary biology might not be a bad idea. In my opinion, there is far too little comparative psychology (see, e.g., Heyes & Huber, [2000](#)). Maybe we should remind ourselves of the concluding sentences of Charles Darwin’s most influential work *On the Origin of Species*: “In the distant future I see open fields for far more important researches. Psychology will be based on a new foundation, that of the necessary acquirement of each mental power and capacity by gradation. Light will be thrown on the origin of man and his history” (Darwin, [1859](#), p. 488).

Conclusion

What can we learn from past crises and the current crisis? Psychology is a strong science in terms of its institutions and will, for sure, not disappear in the concert hall of science. But psychology is a weak science in terms of its theories and predictions. Many years ago, the famous physicist Richard Feynman described psychology once as a “cargo cult science”—he wrote:

I think the educational and psychological studies I mentioned are examples of what I would like to call Cargo Cult Science. In the South Seas there is a Cargo Cult of people. During the war they saw airplanes land with lots of good materials, and they

want the same thing to happen now. So they've arranged to make things like runways, to put fires along the sides of the runways, to make a wooden hut for a man to sit in, with two wooden pieces on his head like headphones and bars of bamboo sticking out like antennas—he's the controller—and they wait for the airplanes to land. They're doing everything right. The form is perfect. It looks exactly the way it looked before. But it doesn't work. No airplanes land. So I call these things Cargo Cult Science, because they follow all the apparent precepts and forms of scientific investigation, but they're missing something essential, because the planes don't land. (Feynman, [1974](#), p. 11)

This statement is from 1974—and hopefully, psychology is no longer suspected of being a “cargo cult science.” Feynman's examples go back to the 1930s and to parapsychology—but similar cases can be observed today. In neuroscience, there are “voodoo correlations” (Fiedler, [2011](#)). And because the “form was perfect,” a parapsychological paper on “feeling the future” with spooky content made it into an excellent top-ranking journal (Bem, [2011](#); for an immediate critique, see Wagenmakers et al., [2011](#)).

Do we need competition among theories? Of course we do! The famous “experimentum crucis,” which names one of two competing explanatory approaches as the clear winner, is rarely feasible. This makes it all the more important to practice “adversarial collaboration” along the lines of Daniel Kahneman and Gary Klein ([2009](#); see also Ceci et al., [2026](#)). It is also great to see that there is a “Journal of Controversial Ideas” (see <https://journalofcontroversialideas.org/>) since 2021!

Being in a crisis always means having opportunities for a new beginning. The subject of psychology has survived many a crisis. New beginnings are not always as new as they sound. Even in a field as dry as statistics, Cumming ([2014](#)) has suggested a fresh start. Maybe, psychology can learn from past mistakes? Brehmer ([1980](#)), however,

casts reasonable doubt on the ability of humans to learn from experience.

Wendt and Funke ([2022](#)) close their considerations about a “new course for psychology” with the following statement:

Taking responsibility on the ship called psychology means taking heart by proposing a course to the best of one’s knowledge and belief, even if one can – and even should – be criticised by others. We have the cards of theoretical psychology at our disposal to engage in a proper discourse with our critics about the direction of the ship. Defending our point of view, but also recognising better arguments, is a duty towards the solidly united crew, the ship, and the fleet of all sciences. There can be no certainty about the waters of new knowledge – the only certainty is that the waters in which we last travelled are already sufficiently well known. (p. 125; translated by J.F.)

There is nothing to add.

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Footnotes

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5. Theories and Models in Phenomenological Psychology

Alexander N. Wendt¹✉ and Frederick J. Wertz²✉

(1) Sigmund Freud University, Vienna, Austria

(2) Fordham University, New York City, NY, USA

✉ **Alexander N. Wendt (Corresponding author)**

Email: alexander.wendt@mail.sfu.ac.at

✉ **Frederick J. Wertz**

Email: wertz@fordham.edu

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Phenomenology and Psychology

At first sight, phenomenology appears to be a current of philosophy. Seen this way, the project of psychology seems to be contingent on interdisciplinarity—as if philosophy in general and phenomenology in particular were an external criterion or a commendable inspiration for psychology but nothing more. However, history teaches that the relationship between these fields of knowledge, psychology and phenomenology, is not as distant as it seems. When the phenomenological movement emerged in the scientific debate of the late nineteenth century, philosophy and psychology were still institutionally undivided (see Kessel, [2019](#)). Phenomenological

contributions were directed at psychological problems, and many protagonists of the movement belonged to psychological institutes, participated in psychological conferences, and even did empirical work (see Spiegelberg, [1972](#)). Early phenomenology belonged to psychology just as psychology was the topic of phenomenology. A phenomenological commentary on theoretical psychology is not simply optional or complementary advice; it has been and remains inherent to psychological discourse. Phenomenology deals with foundational issues, such as the nature of conscious life and how it can be psychologically studied, which are crucial for psychological research and theory. Phenomenology provides essential insights for psychology's identity and development.

To enable the critical contribution of phenomenology, certain misunderstandings about the movement need to be averted. Amedeo Giorgi ([1983](#)) suggests a list of ten prejudices which impede an appropriate understanding of the approach. He argues that phenomenology should not be seen as mere introspection, as subjectivism, as investigation of experience alone, as merely idiographic, as limited to private or inner realities, as an anti-science, as primarily propaedeutic, as speculation, as purely aprioristic, or as an antagonist for empirical psychology. The baseline of Giorgi's apologia of phenomenology is that it should not be mistaken for a champion for one of the two parties in the struggle of scientific cultures, natural science vs. the humanities. It does not simply affirm the descriptive and idiographic understanding of psychology which relies on verbal data, nor does it repudiate the claim or abandon the aim of nomothetic and explanatory research. In fact, recent developments in the phenomenological movement embrace experimental and quantitative procedures, for instance, so-called neurophenomenology (Varela, [1996](#)). In phenomenological investigations, first-, second-, and third-person phenomena all have their proper place. More specifically, phenomenology is dedicated to determining what these perspectives essentially are—how each accesses mental life, including their reach and limits.

Another misunderstanding concerns the supposed homogeneity of phenomenological contributions. Like other currents in the history of thought, the label of 'phenomenology' has various meanings and uses. Phenomenology unites or integrates heterogeneous standpoints that originated both inside and outside the phenomenological movement. Although not a doctrine or monolithic edifice, it offers a few coherent premises as well as a methodological and conceptual core that are shared among phenomenologists, for example, that lived experience is accessible through reflection and empathy, meaningfully structured, and available for conceptual investigation with evidence. The movement includes contrasts, such as Edmund Husserl's transcendental egology as opposed to Daniel Dennett's heterophenomenology,¹ Martin Heidegger's fundamental ontology in contrast to Frantz Fanon's critical phenomenology, or Franz Brentano's psychognosia which differs from Michel Henry's radical phenomenology. Because phenomenology aims at fidelity to its subject-matter and includes various topics, research questions, goals, sensibilities, and points of view, philosophical works have reached different conclusions. Given a common core method that analyses and conceptualises experience according to its intrinsic structures, in contrast to inferential methodologies that feature and are constrained by deduction and induction, these differences provide psychology with various resources that are heterogeneous and nonetheless coherent. As a distinct approach, the field of phenomenological research discloses a uniquely wide and complex discourse on the structure and meaning of phenomenality, that is, on what it means for something to appear and be experienced.

The epistemic vitality of phenomenology which manifests in controversy and internal self-criticism should not be seen as a weakness but as a critical part of and resource for science. The reach of its philosophical horizon allows scientific disciplines to be accountable and responsible for their underpinnings and constraints. Phenomenology offers orientation for empirical psychology as the prior reveals tacit conceptual commitments as well as novel epistemic paths

and thereby makes further knowledge attainable (e.g., Wertz, [1986b](#)). This relationship has been expressed by Heidegger when insinuating that “science does not think”² (Heidegger, [1954](#), p. 9; our translation). Evidently, such a statement affronts the self-esteem of modern research insofar as it brings back memories of philosophical hegemony over empirical research. Yet, it is not inevitable to interpret Heidegger’s charge as brute epistemological foundationalism, that is, as a dismissal of scientific autonomy. A more moderate standpoint would entail that philosophy is itself in an important way scientific and may help empirical sciences in understanding their own predicaments. Psychological research entails substantial foundational conundra—be it the mind-body problem, the problem of introspection, or the problem of other minds—and phenomenology offers a way of understanding them as it addresses the constitutional issues of theoretical psychology, such as the problems of ungroundedness, fragmentation, and problematic evidentiary claims.

Phenomenology’s contribution is not exhausted by a reflection on foundations, although it starts from it. Beyond that, it is a philosophy of science. More specifically, phenomenology provides a material philosophy of science which is not reducible to formal considerations about logic because it immediately engages with the phenomenality of mental life. For Husserl, philosophy is itself a rigorous science, one that is interrelated with all the positive sciences. Fields of research such as mathematics and logic investigate *formal and exact essences*, whereas empirical sciences, such as psychology, investigate phenomena that are essentially morphological, intrinsically inexact (see Wertz, [2024](#)). Thus, phenomenology contributes to identifying disciplinary boundaries and frontiers. Whereas other perspectives in the philosophy of science, for example, critical rationalism, explicitly withdraw from considerations about the contents and specific methods of the sciences, confining their task to determining the patterns of justification and critique, phenomenology addresses the epistemic, ontological, and axiological (e.g., normative) makeup of the psychological, thereby clarifying psychology as a scientific discipline. In other words, phenomenology

engages with the problem of subject-matter in psychology and, following from that, the research interests, questions, methods, and methodology proper for that subject-matter.

Phenomenology, as rigorously reflexive science, provides a framework that allows addressing the basic structure of psychology's scientificity. In this structure, the concepts of theory and model are elementary members. The terms 'theory' and 'model' have various meanings and contexts of use. Phenomenology critically examines them and can inform the psychological process of theory building. The contribution of phenomenological philosophy of science thereby becomes an orientation for the psychological discourse. This chapter articulates the phenomenological understanding of theories and models as well as their formation in psychology. Theorising and modelling will be analysed as types of epistemic acts which disclose the world. Regarding psychology, the basic claim is that both theories and models, in their varying forms, seek understanding of the encounter with mental life. Understanding is the purpose of science and an essential expression of rationality wedded to observable evidence. To rationalise psychological phenomena, theories attempt at *explaining* a part of mental life, whereas psychological models attempt at *translating* phenomena to a rational form. This line of argument will be devised in three steps: meta-psychology, the concept of theories, and the concept of models.

Formal and Regional Ontology in Meta-Psychology

Psychology is dedicated to a field of phenomena with a questionable, controversial, and historically changing identity. This field has been addressed as soul, mind, or behaviour in the past, and the *problem of subject-matter* remains equivocal (see Wendt, [2025](#)). Even though some have argued that the problem itself might be unsolvable, it is hardly debatable that psychology—in principle—does have its own distinctive disciplinary region of inquiry even as may include heterogeneous

subject-matters. If there were no such riddle of life called human consciousness, no psychological investigation would have ever been undertaken. Something unknown piques human curiosity, an original encounter with mental life within the irreducible phenomenon of persons vis-à-vis purely material or even merely biological phenomena. Psychology tries to make sense of its own distinctive subject-matter. Phenomenology offers methods that enable knowledge to move from an original (prescientific, naive) familiarity with its phenomenon to rigorous conceptual, theoretical clarity. It reflects on the conditions of psychology insofar as it discloses the primordially of its subject-matter.

As mathematics investigates the ideal relations between quantities and biology explores the structures of organic matter, psychology delves into yet another domain of reality. This calls for a *topology of being*, that is, a survey of the diverse forms of reality which may be subject to scientific inquiry. Such an investigation challenges the presuppositions of the natural scientific approach to psychology which interprets all reality as physical in favour of a variegated ontology that acknowledges the region of human persons, whose reality includes but is not reducible to materiality (see Wertz, [1983](#)). History of philosophy is replete with competing topologies of being, reaching from the classical antagonism of Parmenides and Heraclitus to contemporary approaches, such as Joseph LeDoux's *The Four Realms of Existence* (LeDoux, [2023](#)). In philosophy of science, such ontological pluralism has been discussed on several occasions (e.g., Cartwright, [1999](#)). To provide a systematic approach, Husserl calls for *formal ontology* or *formal apophantic* which is dedicated to "[t]he investigation of something or something as such, i.e., the objects as such as substrates of possible predicative meanings [...]" (Husserl, [1956](#), p. 28). This is to say that phenomenology occupies itself with determining the ontological reach of possible scientific subject-matters by recovering their meaning. Heidegger's renowned *fundamental ontology* is a cognate way of thinking (see Rogove, [2022](#)). Formal philosophical investigation is complemented by regional ontology which

differentiates the heterogeneous kinds of being as they were discovered by formal ontology. This regional ontology differentiates the physical, biological, and personal. The investigation of personal mental life discloses human beings in their psychophysical holism and structural unity, including the mutual implications and interdependencies of its constituents according to their meaning, teleology, and temporal development (see Wertz, [1995](#)).

Similar to other approaches, phenomenology establishes a critique of worldviews and epistemic attitudes, in particular by acknowledging and then surpassing the 'natural attitude' of experience with the 'phenomenological attitude' that investigates both prescientific life and 'scientific' practices as conscious, human performances.

Phenomenology acknowledges that conscious life and its evidence are a problem, and it devises a critical examination of various forms of evidence. However, what sets phenomenology apart from other approaches, such as critical psychology and Freudian meta-psychology, is its quest for the essential structure of psychological life, its attempt to determine the order of being which manifests not only pure logic but the distinctive being and reality of the psychological, with its intrinsic demands on empirical methods and theory. For this purpose, the movement offers methods, such as Husserl's eidetic analysis (see Wertz, [2010](#)), existential analysis in the tradition of Jean-Paul Sartre and others (see Hanscomb, [2006](#)), the reflections of personalism (see Wertz, [2025](#)), and Hans-Georg Gadamer's hermeneutics (see Frie, [2010](#)) with their methodological justification for the human sciences.

The dependency of cognitive science on concepts like 'cognition', 'information', or 'function' reveals a tacit foundational layer of psychological research (see Wendt, [2022](#)). In contrast, phenomenology questions such conceptual commitments to the extent that these empty words remain ungrounded in investigations of the matters to which they refer, and addresses the primordially that pertains to essences of the highest genera like 'physical thing' or 'person' by means of *regional ontology*. A distinct form of being belongs to each region according to

its essence, and to be genuine, empirical science must be conceptually faithful to its indigenous region of being.

The topic of 'association' serves as an example for psychology's conceptual presuppositions. It permeates contemporary psychology like its history: From Aristotle's associative structure of memory to David Hume's laws of association and Johann Friedrich Herbart's associationism, the idea prepares important discoveries like the stimulus-response connection, neural association, or free association in psychoanalysis. Yet, in most cases the concept of association itself remains an operative notion without itself becoming thematic. Conceptual analysis reveals that the expression is used in the generic sense of a connection or even coincidence on some occasions but in a more terminological sense in other contexts (cf. Warren, [1921](#)). Yet, the superficial appropriation of the term obscures its conceptual foundations in the matters to which the notion refers.

In his monograph on *Phenomenology of Association* (1971), Elmar Holenstein summarises the phenomenological concerns with the concept of association from a Husserlian vantage point. Holenstein identifies the central problem to be the non-egological constitution of experience: "The self-conscious and categorical self which, in classical idealism, has had exclusive responsibility for the meaningful constitution of the world is now to be dethroned" (Holenstein, [1971](#), p. 2; our translation). Hence, problems in philosophy and psychology of consciousness linger at the foundation of the concept of association and they remain tacitly relevant even when omitted in the psychological discourse. Husserl ([2001](#)) himself approached the origins of these presuppositions with concrete philosophical investigations of *passive synthesis*. In differentiating, describing, and interrelating passive (pre-egoic) and active (egoic) syntheses, Husserl disclosed the phenomena on which the notion of 'association' is based and also highlighted processes of the activities of mental life founded on but irreducible to 'association'. Such a contribution does not finally solve all the problems of association, but it addresses them by disclosing an otherwise

unknown primordial phenomenon of consciousness about which the scaffolding of discourse on association has been forged.

Phenomenological psychologists of Utrecht and Heidelberg, such as Johannes Linschoten and Carl Friedrich Graumann, have excogitated the so-called *situation analysis* (cf. Schott, [1991](#)) to identify the configuration of individual psychological phenomena and situations according to phenomenologically specified fields of meaning. Drawing on Linschoten, Graumann highlights four aspects of psychological situations, namely, (1) *corporeality*, “insofar as subjects have a location from where they perceive and act and according to which they perceive their environment and enact it”; (2) *landscape* as “the materiality and spatiality of the intentional environment”; (3) *historicity* in the sense of “the temporality of experience which reveals itself in the horizontal structure of the interaction of actuality and possibility”; and (4) *communication*, which means “that our horizon of experience is disclosed, expanded, and limited by others” (Graumann, [1985](#), pp. 45–47; our translation). This list might require further inspection and even revision, but it nevertheless demonstrates the phenomenological insight into the primordial dimensions of experiential constitution upon which the explanatory space for psychological theories is rendered. Psychological concepts emerge from lived experience and phenomenology retraces their original constitution. In this sense, it is a descriptive method which allows us to study personal life, thereby helping to clarify and compare the practices of psychological researchers and theorists (see Wertz, [2024](#)).

Phenomena Acquire Theory

The phenomenological movement as a whole has repeatedly engaged with the concept of theory, which is understood as a human achievement rooted in pre-theoretical, prescientific life. A proper and elaborate account of phenomenological *psychology* that focuses broadly on theory can be found in Adrian van Kaam’s [1966](#) *Existential Foundations of Psychology*. Van Kaam chooses an anthropological access

to the problem when articulating “Man’s thoughtful standing-out into reality” (van Kaam, [1966](#), p. 75). In a similar way, the phenomenologist Erwin Straus has portrayed humans as “inquisitive beings” (Straus, [1960](#)). Van Kaam identifies intentional givenness, that is, the encounter with the self-presentation of phenomena, as the basic property of experience. In other words, “[s]cience is a mode of contacting reality” (van Kaam, [1966](#), p. 74). This contact evolves over the course of human history, leading from a mythological presentiment to the emergence of critical reflection on the spontaneous vision of the world. With this idea, van Kaam approximates the philosophy of Ernst Cassirer, who treats mythos and science as different symbolic forms which explore and appropriate reality (see Cassirer, [1957](#)).

Prior to scientific theorising, experience is acquainted with phenomena which reveal themselves to it: “Observation is thus really preceiving [sic] what is ‘out there’ ” (van Kaam, [1966](#), p. 88). Such phenomenal givenness is the central topic and starting point of phenomenology, which then goes on to analyse the essences of phenomena, conceptually clarifying (with imaginative variation, hermeneutics, existential reflection, and critical scrutiny) their meanings, invariant structures, and typical variations. Martin Heidegger reconstructs the etymology of *phenomenon* as that which reveals itself and comes to the fore, thereby requiring to be understood *as something*. He argues that this hermeneutic disclosure of phenomena even predates their interpretation as mere appearance: “Only because something claims to show itself in accordance with its meaning at all, that is, claims to be a phenomenon, can it show itself as something it is not, or can it ‘only look like...’ ” (Heidegger, [1996](#) p. 25). Far from claiming that a simple solution is possible, the phenomenological discourse has continuously explored the essence of the phenomenon itself, for example, in the recent approaches of Michel Henry and Jean-Luc Marion, who emphasise the immediacy of givenness (see Marion, [2002](#)).

For van Kaam, engaging with phenomena constitutes the observant existence of science, in the living of scientists who institute and pass

down methods and theories of many different kinds, both descriptive and explanatory. He also argues that theory in turn guides perception when making sense of the primordial encounter with a phenomenon. Thus, “[m]an is an experimenter by nature, and man is by nature a theorist” (van Kaam, [1966](#), p. 75). This duality in which knowing is founded on living and also transforms our understanding of the world manifests as a constant tension and dialogue which is both unavoidable and never finally resolved. Van Kaam speaks of a dialectical relationship which allows the inquisitive mind to return from the theoretical knowledge of any sort to its existential basis as a whole. This involves a renewed investigation of general theory in light of singular phenomena. For psychology, van Kaam identifies the specific field of relevant phenomena as *behaviour*, no doubt reflecting the psychology of his time (1960s), perhaps prematurely proposing an all too limited and restricted solution to the problem of subject-matter. However, it would also follow from van Kaam’s insistence on the ‘existential foundations of psychology’ that a renewed cognisance of the expressiveness of behaviour in prescientific life would reveal the phenomenon of empathy and, with it, the mental life embodied in personal conduct.

Theory devises a *structure of ideas* which helps recognising the structuredness of phenomena. The psychologist “assembles behavioral phenomena to construct theories which reveal something about the structure of behavior” (van Kaam, [1966](#), p. 96). However, van Kaam recognises a second dialectical entanglement, alongside the mutual dependency of observation and theory. He speaks of *comprehensive theory* as opposed to *differential theory*. The prior proposes a structure of ideas to explain the variety of phenomena as described by differential theories. On the one hand, differential theory—such as psychoanalysis, stimulus-response behaviourism, or information processing theories of cognition—commits to addressing a partial dimension of what is existentially given and produces different and perhaps even seemingly incompatible constructs to understand a special theoretical field, for example, that of unconscious defence mechanisms, operant conditioning, or encoding. Comprehensive theory,

on the other hand, tries to make explicit what is implicit in the originally given whole of existence on which differential theories are based and to comprehensively conceptualise their interrelations in existence, thereby theorised as a whole. In the process of comprehensively theorising, excesses and infidelities of differential theories are critically identified and their apparent incompatibilities are resolved by clarifying their relations within the whole of concrete existence, from which they originated and which they represent in relative abstractions.

Comprehensive theory addresses 'behaviour' in its complex contextual holism. It provides "foundational-integrational constructs" (van Kaam, [1966](#), p. 133). As psychology progresses, both types of theory depend on and enhance each other. Comprehensive theory does not erase the one-sidedness of differential theories but rather fosters it, places it within a larger context. The source of and validity criterion for comprehensive theory include the phenomena uncovered by differential psychologists while they are, in turn, reconnected to the complete phenomenon by comprehensive theory. Ultimately, "[t]his interaction is possible because foundational and differential psychology have a common ground, phenomenology" (van Kaam, [1966](#), p. 142). Differential theories do not reflect their primordial origin and thus are enhanced by the methodological operations of the more comprehensive, existentially grounded and revelatory theorising. These operations are rooted in presuppositions which may be uncovered and critically assessed by phenomenology. Thus, existential-phenomenological descriptions provide the psychologist with constituents of knowledge that cease to be fragmentary and make a comprehensive theoretical structure possible.

How phenomenology guides the integration of differential theories may be seen in the debate about psychology of perception. The case of the so-called New Look exhibits the attempt of cognitive psychology to approximate ecologically valid situations of human perception. Scholars like Jerome Bruner attempted to include the person as a whole in the study of perception, thereby opposing the tradition of psychophysics.

Where the New Look described perception as determined by personal factors, the psychophysicists saw it as stimulus determined (see Wertz, [1983](#)). Yet, the differential theories of the New Look, focusing on unconscious defence mechanisms in one theory, values in another, and sensory-tonic processes in another, lacked any holistic comprehension of perception in ordinary human life situations. Instead of trying to recover existence as prescientifically lived and to critically evaluate the presuppositions of differential theorising by means of phenomenology, theory building made compromises with quantification, aiming for measurability rather than moving towards comprehensive theory. Regrettably, these theorists continued to treat the world as meaningless stimuli somehow transformed by disparate psychological factors rather than as a significant, multifaceted structure (see Wertz, [1986a](#)). The benefits of laboratory research are paid for with its alienation from the plenitude of the lifeworld's complex existential structure.

As a general criticism of the psychology of perception, the shortcomings of the New Look have also been expressed by Ulric Neisser, who concluded that “the researched phenomenon has been transformed under the demands of experimental manageability” (Wertz, [1986a](#), p. 255). Still, neither Neisser nor James Gibson, who promoted ecological psychology, was capable of radically addressing the conceptual presuppositions of perception. Where they could have engaged with the phenomenal constitution of personal corporeality intentionally relating to the world, they committed to an intellectualistic notion of ‘schema’ or the naive realism of environmental ‘affordances’ to explain perception, dividing theories from each other and from the phenomenon (see Wertz, [1987](#)). Although the abstractions of differential theories, such as psychophysics, New Look, or ecological psychology, have their merit and provide meaningful insights into aspects of perception, they require additional effort to hold them accountable to the original concrete phenomena they claim to understand.

Phenomenology provides psychology with *rigour* (see Wendt, [2024](#)) as an epistemic principle for theory building, theory evaluation, and

theory.integration. It is an attitude which is directed at the foundational examination of scientific claims. When reflecting on the “essential ontic limits for the objectification of the mind” (Scheler, [1973](#), p. 218), Max Scheler undertakes such a rigorous revision of psychology. He argues that the field of mental phenomena may only be partially objectivised and goes on by saying that “only a very small part of the objectifiable psychic realm is [...] observable and repeatable; and yet a smaller part of the observable psychic realm can be experimentally varied and influenced” (Scheler, [1973](#), p. 218; see also Wendt, [2025](#)). Scheler’s reasoning enables phenomenological critique of preexisting psychological theories as well as a criterion for devising new theories. In the context of the psychology of perception, corporeality and the embodiment of experience become the subject of phenomenological analysis and prove to be presuppositions of psychological constructs, such as Gibson’s concept of affordances (see Wertz, [1987](#)).

Psychological theories are created when science attempts to understand the manifold of mental phenomena, to see further and make translucent what is still opaque. This act of understanding is grounded in human *rationality*. Whereas other kinds of understanding may be independent of intellectual explanation, for example, empathetic understanding in compassion, psychological theorising takes—as Husserl (see Husserl, [1996](#), p. 337) argues—the form of *explanation* which is rooted in and differentially articulates a phenomenon’s structure by virtue of abstractive reasoning (and especially by logic). Such phenomena explained in their structural wholeness are not reducible to their elements or antecedent conditions. Phenomena comprise totalities and science need not confine itself to causal and quantifiable relationships, which often lose any connection to their indigenous motivational wholes.

When investigating mental phenomena, it is worth considering non-linear relations which are plentiful and may challenge a mechanistic view of unidirectional processes in nature (see Fuchs, [2017a](#)). Theories do not necessarily only explain but make a phenomenon comprehensible, revealing it amidst the shadows of its primordality.

Hence, they introduce ideas to get a hold of mental phenomena. Because the nature of the phenomenon can resist comprehensive understanding and knowledge, theories can draw on models which attempt to *translate* and *transform* the meaning of a phenomenon from its primordial occurrence to a form which is accessible to scientific reason. As van Kaam puts it, the psychologist tests “whether this theoretical structure is a model that points to what actually appears in the observable phenomena” (van Kaam, [1966](#), p. 96). Nonetheless, phenomenology cautions psychology about the critical differences among the original phenomenality, differential theoretical explanations, and an existentially comprehensive theory, which may remain an aspirational possibility requiring phenomenology.

The constructive potential of phenomenological reasoning about psychological methodology shows in light of the quantitative-qualitative dichotomy. Usually treated as a formal difference concerning issues like verbal in contrast to numerical data or the purpose of generalisation as opposed to the purpose of description, phenomenology addresses the relationship of empirical methods to the phenomena themselves. This is to say that phenomenology addresses the epistemic meaning of methodology: Qualitative knowledge provides a necessary foundation for empirical quantitative knowledge inasmuch as measurement presupposes what is measured. Numbers only determine the magnitude and frequency of phenomena, not what they are (see Wertz, [2018](#)). Qualitative inquiry contributes to the investigation into psychology’s subject-matter insofar as it discloses the quality of mental phenomena. Nonetheless, phenomenology is able to recognise the value and epistemic capacities of experimentation or mixed-methods research. Ultimately, the quantitative-qualitative dichotomy is dissolved by addressing the more foundational layer of primordial acquaintance with phenomena.

A Phenomenological Understanding of Models as Transformations

Theoretical psychology and philosophy of science have engaged with the notion of the model on different occasions (see Bailer-Jones, [1999](#)). The discourse exposes certain primary motifs that are at play when trying to understand what a model is. First of them is the contrast of realist and anti-realist accounts (see Schmid, [2015](#)). They tackle the claim to the truth of models. A default realist position is that models are meant to coincide with reality and to represent it (the perfect model model; see Teller, [2001](#)), whereas anti-realists emphasise the fallibility of reason. Either position has its pitfalls; the former faces the virtual impossibility to corroborate the entirety of any model's presuppositions—famously leading to the Duhem-Quine problem of confirmation holism. To anti-realism, on the other hand, any success in science must seem to be a miracle, as Hilary Putnam ([1975](#)) put it. Hence, anti-realism invites scepticism or relativism. Nonetheless, anti-realist positions took precedence in the recent debates, hosting approaches like the syntactic (Rudolf Carnap, Carl Gustav Hempel), structuralist (Patrick Suppes, Wolfgang Stegmüller), or semantic (Frederick Suppe) view of models. They focus on formalisation and even reduce the notion of theory to a set of models. A phenomenology of the model needs to engage with this discursive background.

Phenomenological philosophy of science has not yet spawned a proper account of model building. Günter Figal ([2007](#)) is one of the few phenomenologists who have tackled the concept. In his view, models *show* or *demonstrate* something (παράδειγμα—to point beyond). According to Figal, something appears indirectly via a demonstration which means that the model is a demonstration by virtue of its *disclosing character* and insofar as it makes something recognisable (*kenntlich*). Models must provide a growth in recognisability which Figal calls *intensification*. This increment in intensity provides orientation thanks to “an especially attentive directedness towards something” (Figal, [2007](#), p. 671; our translation). Against this background, Figal phenomenologically determines the primary characteristic of models to be *reflective* in the sense of demonstrations.

Their value resides in their affording recognition of what would otherwise be overlooked.

Figal's view treats models as magnifying glasses, as focuses of attention, as heuristics rather than as simple representations of what is modelled. As a miniature model of a town allows the spectator to contemplate the urban composition beyond the disquieting turmoil of everyday life, a psychological model of personality reflects the dimensions of character development without becoming absorbed in the drama of biography in all its thick details. Figal's contribution shows that phenomenology provides a new perspective for the discourse on the concept of the model because it moves beyond the idea of representation that has been the centrepiece of modelling since the Aristotelian idea of *mimesis* (μίμησις). According to phenomenology, something is intended by means of a model, but the relationship between the model and its intentional object is not that of two independent ontological entities. Modelling something is an epistemic relationship of presentation rather than representation. From this perspective, models have meaning, that is, they enable attention to meanings that may be given in reality. Thus, phenomenology promotes a non-naïve realist understanding of modelling, as a kind of intentional consciousness of reality, without assuming representationalism.

However, moving beyond Figal, the act of modelling does not only entail an increment of intensity but also a shift of or transformation in meaning-content. For example, the medical model of psychopathology draws attention to symptoms, pathological processes, and physiological causes of mental disorders but also profoundly limits and, if taken as a representation of psychopathology, distorts our understanding. To account for transformation of our relationship with the subject-matter in modelling, a refined phenomenological understanding of models would view demonstration and reflection as characteristics of models that acquire meaning through an epistemic act of *rationalisation*. Models not only highlight certain givens but make them attainable for reason. To revisit the example of personality psychology: Not only since Theophrastus of Eresos, humankind has been wondering about the

riddle of character but personality models like the infamous Big Five seem to satisfy this old curiosity, even if their answer entails a reductionism. Models are not only magnifying glasses but spectrometers, revealing meaningful structure in what were before more glaring flashes of light than could even be comprehended.

Phenomenology can dedicate itself to modelling as an epistemic act. In this regard but no other, it approximates the approach of so-called *model-based reasoning* (Ronald Giere, Nancy Nersessian). What sets them apart is that model-based reasoning treats models as analogues of existence and thus relapses into representationalism. Phenomenology allows us to understand the act of modelling as transformations of our consciousness of phenomena insofar as it *highlights orderly meaning in these phenomena*. What sets this act apart from other types of sense-making like metaphors is that models constitute rational order. This transformation precedes the recognition of mimetic similarity in representation. Reason probes into the phenomenon and establishes an order or scale which then enables the judgement of similarity, but this judgement is only secondary.

A classic example of the transformation in the understanding of phenomena through models is the idea of *proportion* which was already present in ancient and medieval architecture (in which resides the etymological origin of the term model). The bodily subject dwells in the *factum brutum* of space and then bestows order upon it, allowing the rationale of architecture, which means a transformation of the natural environment into the domain of culture. The sense-making of modelling transforms the primordial identity of the phenomenon by *abstraction* and *idealisation*. As Nancy Cartwright ([1989](#)) summarises, these two processes of thought have been the bone of contention in the debate of model between the Aristotelian and Galilean views throughout the history of philosophy. As opposed to these empiricist traditions, however, phenomenology contends that models do not derive from pre-given (sense) data but rather attain the rationalisation of the otherwise and previously opaque phenomenon.

From the phenomenological point of view, idealisation and abstraction are cases of fictionality or, in the words of the phenomenologist Thomas Fuchs ([2017b](#)), an *as-if function*. A phenomenon is treated *as if* it were an independent whole, thus revealing the meaningful order of its parts. The fallibility of this fiction becomes evident in the example of symmetry as a model of beauty which pervades the history of art. It took the humility of artists like Albrecht Dürer to realise that the profoundness of beauty cannot be modelled (see Mahr, [2003](#)). And yet, the attempt of understanding beauty through proportion was the condition for this insight. In an analogous case, Charles Spearman argued that the psychological method of factorial analysis was necessary to comprehend human intelligence because “our limited intellects can only hope to deal with the infinite complexity of Nature after analyzing it down into its bare unaesthetic elements” (Spearman, [1904](#), p. 203). The psychological model closed in on the phenomenal texture of personality, enabling rational discourse about cognitive abilities but nonetheless the essence of the person ultimately withdraws from the model.

Inspired by Goethian morphology, Claude Lévi-Strauss reached a similar understanding of the act of modelling in his *modèles réduits* (see Dvořák, [2023](#)). According to his account, models allow (we would suggest that *some* models *may* allow) the recognition of an event in its totality because the event’s circumstances are reduced so that the whole can shine forth. Put phenomenologically, this kind of reduction actualises a meaningful order by virtue of the model’s fictionality. As in the case of a miniature replica of a city, the model establishes the limits and confines of the location where in personal presence one would doubt where a place like a city truly ends. Still, the miniature does justice to the proportions of the city and thus expresses an essential aspect of the original phenomenon; it is more than a mere construction. The model’s fictionality allows the decomposition of the phenomenon, its *analysis*. The meaningful order which is encountered by means of the model has a specific mereological character. This is to say that modelling allows reason to identify and relate parts because the

phenomenon is treated as if experienced as a whole, that is, as if its structure could be expressed by the model. Where the fictionality of the model is forgotten, the 'as if' turns into a 'nothing but', as the phenomenologist Dietrich von Hildebrand ([1953](#)) argued, which means that any model bears the risk of reductionism.

Models may have different functionality in the scientific process. They can provide integration for an investigation which shifts attention to comprehension in the sense of van Kaam. This means that the model promotes theoretical unity and phenomenal closure. Such models purport to exhaust the phenomenal space and may thus be called *figurative models* because they establish a figure or gestalt. In generating a totality, these models are more than summaries. They ameliorate a theory or scientific argument. Their closure of a phenomenal space entails a (preliminary) termination and completion of the research process. Open ends and ambiguities resist the generation of a figurative model. Even a taxonomy like John Bowlby's list of four attachment styles can be a model like this.

Another class of models does not stand at the end of the research process as figurative models do. Instead, their characteristic feature is venturing into the pre-rational presence of the phenomenon. For this reason, they may be called *exploratory models*. Against the resistance of primordial phenomenality, an exploratory model attempts an orderly sense-making in light of a phenomenon. The model probes into the uncharted space and provides a tentative understanding (see also Feest & Devezer, this volume). An exploratory model may be, for example, a circle drawn on a piece of paper to idealise its self-referentiality or an equation to suggest a regularity. None of them are identical to the phenomenon, but they epistemically approximate a part of it or something (entailing a dynamism) that significantly shapes it, and they thereby procure knowledge about it.

A theory attempts to observe (which shows in the etymology of θεωρέειν/theoreein) a phenomenon in itself, but some phenomena resist their rationalisation. A model, be it figurative or exploratory, provides rational access to a phenomenon. This is achieved by an act of

transformation or translation (lat. *Translatio*—*carry over*). The ambiguity of the Latin prefix ‘trans’ represents the two types of models. A figurative model translates in the etymological sense of coming from one side to the other, of building a bridge and filling in gaps. It completes the understanding of a phenomenon. An exploratory model translates in the etymological sense of moving beyond. It renovates the understanding of the phenomenon. The target structure of modelling, for example, a symbolic system, can be preexisting, such as a language or mathematics, but it can also emerge spontaneously, inspired by the phenomenon itself. Such *impromptu models* are spontaneous works of creativity and resemble artistic inspiration. A classic example is August Kekulé’s dream about the alchemist symbol of the Ouroboros serpent, which unlocked the rational foundation for the chemical theory of the cyclical structure of benzene.

A model as the result of the act of modelling is ultimately alien to the phenomenon it attends to because it would otherwise be as incomprehensible as the phenomenon. A figurative model rationalises the totality of a phenomenon, thereby alienating the latter’s original inexhaustibility. An exploratory model rationalises the primordially of a phenomenon, thereby alienating the immediacy of its primordial presence. Translation means finding a counterpart in a different field of meaning as a *mode of presentation* for the phenomenon and thus invites two questions about the notion of the model: First, how is it possible to find a counterpart for something that is to be explained, viz., an unknown? Second, what are the criteria for the similarity between the model and its object? (for the problem of similarity, see also Teller, [2001](#)) Phenomenology is able to answer both questions and thereby establishes a phenomenological understanding of models for psychology.

A representationalist conception of modelling cannot answer the first question. Without a direct interface of phenomenon and model, no knowledge about the phenomenon is possible. This is a classical problem of epistemology. However, phenomenology does not subscribe to representationalism, which means that models are no analogies in

the sense of accidental and external ascriptions to a phenomenon that are the sole product of subjective activity, as constructivist accounts of modelling argue. The act of translation is familiar with the phenomenon; nobody could be a translator without knowing the language from which to translate. The phenomenon is not unknown in a strict sense; it is not epistemically absent in spite of not being rationally attainable without the model: "A demonstration [...] would not be understood [...] if it was not understood according to something which is not given by it [the demonstration] alone" (Figal, [2007](#), p. 674; our translation).

In the act of modelling, a subject apprehends a phenomenon by translating its pre-predicative (or pre-rational) structure to an intelligible field of meaning. This is possible because the subject partakes in rationality but is also acquainted with primordial phenomenality. Experience can be pre-reflective as well as reflective. The acquaintance with primordiality is a founding constituent of experience, for example, in the corporeal structure of consciousness: One's being in the world enacts a standpoint, and no conscious experience is possible without this pre-reflective trace of corporeality.

The crucial epistemic potential of models is their rationalising the pre-predicative presence of meaning, rendering it available for theory building. At the same time, the act of modelling is solicited on the occasion of theorising. Science does not spontaneously generate models but does it due to its research interest in explaining phenomena. If this interest faces the limits of its own rationality, a model is an attempt to trace the primordial presence of the phenomenon. More specifically, it is a search which enables rationality where it is otherwise epistemically unattainable. The phenomenon eludes or withdraws from the attempt of theoretical rationalisation and thus leaves a trace against the background of rationality. To give an example of a figurative model, trying to understand what intelligence is might call for a model which exhausts all aspects of the phenomenon. The model would provide a texture of aspects and relationships but to finalise the model, the phenomenon's totality has to be incorporated, this comprehensive

movement towards the phenomenon as a whole being foundational for the model. This is the phenomenological answer to the second question (about the criteria of illuminating relationship between a model and its object). Whenever a scientist uses a model, whether it is created by an algorithm or by divination, the immediate contact with the phenomenon provides the criterion of validity.

Rationality

Theorising and modelling serve the growth of scientific rationality. But what is rationality? This question stands at the centre of the phenomenological movement. Rather than presupposing a panlogical view of being which assumes that everything has an immediate expression in logic, phenomenology recognises the plenitude of existence which is never exhausted by our knowledge. It challenges the Galilean foundation of exact science according to which nature is a book written in the language of mathematics. This claim either underestimates the phenomenal plenitude of being or it overestimates the universality of mathematical or logical rationality.

As a philosophy of science, phenomenology addresses the resistance of being to the attempt of scientific rationalisation, and the field of psychological phenomena is particularly fierce in its resistance. The topology of being reveals that the structure of primordially is nothing short of heterogeneous and contains opacity. To determine its aspects allows a *morphology of rationality*, tracking failed and successful attempts of developing theories by means of describing and/or translating mental phenomena into a scientific form, which ultimately requires evidence.

Phenomenology makes a critical contribution to psychology by challenging its self-conception as an exact science. This is not a negative contribution but a revision of presuppositions about science and its phenomena, which can lead to the growth of the discipline. Phenomenological psychology provides the foundation for theory building, solicits methodological change, and provides a philosophy of

science to improve the foundation of the scientific processes in theoretical psychology and inspire innovation in psychological research. These perspectives for development are not external to psychology, but phenomenology belongs to the discourse and will continue to contribute from within.

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Footnotes

¹ It is debatable whether Dennett's contribution is phenomenological in the first place.

² Translations throughout the text are provided by the authors unless indicated otherwise.

6. Solving the Problem of Psychology Can Help with Theory and Model Building

Gregg Henriques¹✉ and Marcia Gralha¹

(1) James Madison University, Harrisonburg, VA, USA

✉ **Gregg Henriques**
Email: henriqgx@jmu.edu

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Solving the Problem of Psychology Can Help with Theory and Model Building

The field of psychology is highly active, producing thousands of publications annually, hosting numerous conferences, and drawing an increasing number of students at both the undergraduate and graduate levels (American Psychological Association, [2024a](#)). On the surface, this suggests the field is thriving. However, despite psychology's prolific output, the field faces significant conceptual challenges that have persisted for years. The field's frameworks, models, and findings generally remain confined to mid-level theorizing and struggle to build cumulative knowledge that extends beyond specific research programs.

We characterize the difficulties psychology faces in achieving cumulative knowledge as the “sandcastle problem” (Henriques, [2022](#)).

While the findings from individual empirical paradigms may be impressive, they often lack a solid theoretical foundation. Thus, like sandcastles, the findings are quickly “washed away” when new researchers introduce new methods and models. Other scholars have made similar observations (e.g., Hutmacher & Franz, [2025](#); Oberauer & Lewandowsky, [2019](#)).

We contend that the sandcastle problem arises from a deep-seated conceptual confusion about what psychology is. We can contrast this confusion with the state of affairs in biology. Biology is consensually recognized as the science of life, with the cell as a primary unit. Biology’s “Tree of Life” provides a macroscopic view of the field, and it is framed by the modern evolutionary synthesis (i.e., natural selection operating on genes and organisms across the generations). Biology, therefore, is aligned with a clear subject matter (life and living organisms), has consensus on its basic unit (the cell), and operates within a broad theoretical paradigm (the modern evolutionary synthesis).

In contrast, psychology is awash in conceptual ambiguity. Psychologists do not agree on their subject matter, a fundamental unit of analysis, or any shared overarching paradigm to organize their findings and theories. Instead, the field occupies a vague space between biology and the social sciences in terms of complexity. This is the field’s “vertical problem” (Henriques, [2011](#)): it is unclear where, exactly, psychology fits as we move from organisms to animals to humans and human groups. In terms of its core subject matter, psychology aligns with both “mind” and “behavior,” but these two concepts have never been effectively defined and integrated—a challenge we refer to as psychology’s “horizontal problem.” The field also struggles with its identity, torn between being a basic science, an applied profession, or a hybrid of both.

This confusion is apparent in the American Psychological Association’s “definition of psychology,” which reads as follows:

Psychology is the study of the mind and behavior. The discipline embraces all aspects of the human experience — from the functions of the brain to the actions of nations, from child development to care for the aged. In every conceivable setting from scientific research centers to mental healthcare services, “the understanding of behavior” is the enterprise of psychologists. (American Psychological Association, [2024b](#))

This definition is so ambiguous that it borders on being meaningless. A more straightforward definition would simply state, as Werner Traxl did in the 1970s, that “psychology is what psychologists do” (Wendt, [2024](#)).

Our argument builds from this critique to raise the following question: *If psychologists cannot clearly define their field or what they study, how can the mid-level models and theories they produce lead to a cohesive, cumulative understanding?* We characterize the crisis in model and theory building as the sandcastle problem, which is a useful metaphor to highlight the lack of a shared foundation in the field. We trace this issue to the historical “crisis in psychology,” which, we argue, emerged because of a profound gap in our understanding of the world in the wake of the scientific Enlightenment.

We then propose solutions. Specifically, we argue that scientific understanding can be framed by a “metaphysical-empirical continuum” of justification that ranges from specific empirical findings to abstract conceptual structures. The problem of psychology can be understood as the field’s failure to develop an effective descriptive metaphysical system that defines its core concepts (i.e., science, behavior, and mind) and a metatheoretical architecture that can assimilate and integrate the major schools of thought and empirical findings into a coherent whole. We then introduce UTOK, the Unified Theory of Knowledge. It has both a coherent descriptive metaphysical system and a metatheoretical architecture that can effectively address and resolve the problem of psychology. If adopted, UTOK can help psychology transcend the sandcastle problem and achieve cumulative knowledge.

The Sandcastle Problem

A New Synthesis for Solving the Problem of Psychology: Addressing the Enlightenment Gap describes the challenges of theory and model building in psychology as the sandcastle problem (Henriques, [2022](#)). It is a problem that has long been noted. Paul Meehl ([1978](#)) aptly captured it when he observed:

It is simply a sad fact that in soft psychology theories rise and decline, come and go, more as a function of baffled boredom than anything else; and the enterprise shows a disturbing absence of that *cumulative* [italics in original] character that is so impressive in disciplines like astronomy, molecular biology and genetics. (p. 807)

Psychological research often relies on operational definitions—ways of describing concepts that can be measured and analyzed. The sandcastle problem arises when these research efforts lack a unified conceptual framework, making it difficult to integrate new findings into a coherent whole. Like sandcastles washed away by the tide, each new wave of research, with its own operational definitions and methods, can overshadow previous findings, preventing the field from developing a cumulative body of knowledge. This is why mainstream psychology, despite an abundance of studies, often struggles to provide a clear, unified picture of its subject matter, leading to confusion about psychological phenomena and their place in the broader scientific discourse.

Consider, for example, how the concept of a “growth mindset”—which gained significant attention in the 2000s (Dweck, [2006](#))—relates to, builds on, or differs from Albert Bandura’s concept of “self-efficacy” from the 1980s or Julian Rotter’s “internal locus of control” from the 1950s. These concepts can be seen as overlapping yet disconnected “sandcastles,” representing similar ideas without being structured in a way that allows for cumulative integration. If we add more concepts, such as attribution theory, cognitive dissonance, psychodynamic

defense mechanisms, or the role of positive thinking, the issue only becomes more complex—like trying to assemble a puzzle with many disconnected pieces and no frame for the edges. We argue that this pattern holds true for virtually all of psychology’s major concepts and findings.

Viewed through the lens of the sandcastle problem, the need for a comprehensive framework that can integrate diverse research threads into a cohesive whole becomes evident. We are, of course, not the first to argue for a holistic approach to psychology. As we briefly review below, the field has a long-standing debate regarding whether a holistic approach is desirable or feasible (Wieser, [2018](#)). Our perspective is that a holistic view is desirable to the extent that it is feasible and that developments in metatheory over the last two decades have allowed such a perspective to be realized.

The Crisis of Psychology Was Never Resolved and Has Become a Problem that Is Denied

The history of psychology is characterized by the rise of distinct schools of thought. The structuralists, like Edward Titchener, emphasized human subjective experiences examined via introspection. The functionalists, like William James, explored the adaptive functions of “mental life” in both animals and humans. Psychoanalysts following Sigmund Freud viewed psychology through the lens of unconscious mental processes, whereas behaviorists like John Watson rejected the mental domain altogether, focusing on the experimental study of observable behaviors in organisms. These differing perspectives address various aspects of reality, and psychology never was able to integrate all these various aspects into a unified system.

In [1927](#), Lev Vygotsky argued that these differences represented a “crisis” in psychology. Throughout the twentieth century, many scholars echoed this concern. In a series of publications spanning more than two decades, Henriques ([2003](#), [2008](#), [2011](#), [2022](#)) laid out the case why psychology’s crisis should be rechristened “the problem of psychology.” Early in psychology’s development, there were serious concerns about

its lack of a coherent subject matter, such that it was considered a crisis. However, over time, the discipline shifted to justify itself as a science by focusing on applying behavioral science methods to areas of interest (see, e.g., Mayrhofer & Hutmacher, [2020](#)). In doing so, the field largely ignored the conceptual challenge of defining its ontology—what in the world the field is fundamentally about. Thus, the crisis was avoided, but it was never resolved. Those who adopt a Freudian lens might say the problem was denied.

The Problem of Psychology Stems from the Enlightenment Gap

The scientific Enlightenment was a pivotal period in human thought. During this era, the natural sciences emerged, grounded in an epistemology that relied on objective, third-person observations to make truth claims. The rise of the “scientific method” for understanding nature through mathematics, logic, and empirical data gained powerful momentum with Newton’s work in classical mechanics. This established a worldview that embraced a materialistic scientific ontology and an external empirical epistemology. The result was to effectively divide the world into two realms. On one side was the objectively predictable physical world, framed as “primary qualities that could be quantified” and thus were accessible to scientific methods. On the other side was the unobservable realm of secondary qualities. These were the conscious, mental processes that could be subjectively experienced, but could not be explained via the normal process of physical and behavioral reduction (see, e.g., Chalmers, [1996](#)).

Although the mind-body problem has been present in one form or another since human consciousness reached the capacity to contemplate it, it nonetheless took on a new form with the emergence of modern science. Henriques ([2022](#)) characterized the modern form of the problem as the Enlightenment Gap. This gap explicitly refers to the difficulties that arose in the wake of the scientific Enlightenment for understanding the relationship between mind and matter and between scientific and subjective/social knowledge. Psychology, positioned as it

was at the crossroads of studying observable behaviors and subjective human experiences, found itself divided by the Enlightenment Gap. This division is evident in psychology's definition as the science of both "behavior" and "mind," where behavior aligns with the objective, physical world, and the mind with the subjective, mental world.

The field also faced challenges regarding its scientific identity, epistemology, mission, and values. For instance, much confusion and debate has reigned about whether psychology should be considered a natural science, a social science, or a humanistic profession. Drawing from C.P. Snow's ([1962](#)) lecture on the divide between scientific and humanistic cultures, Kimble ([1984](#)) identified a similar cultural split within psychology. He noted:

Psychology has an identity problem. After more than a century of official existence, it still lacks a coherent set of values. There is little harmony among groups of us who practice very different professions, and there is even debate over the definition of our subject matter. (p. 833)

This brief summary highlights the field's lack of consensus on its proper ontology, epistemology, and core identity. With it, we can ask: *What is needed for psychology to overcome its philosophical divides, scientific challenges, and issues of identity and coherence?*

The following sections start with an analysis of the conceptual structure of scientific understanding that places knowledge in psychology on a "metaphysical-empirical" continuum ranging from data-based findings to worldviews linked by definitions of concepts and categories. This continuum reveals that scientific psychology has dealt with the problem of psychology by focusing primarily on the empirical side of the continuum, while leaving the metaphysical and metatheoretical side ineffectively addressed. We then summarize a unified theory that affords the field a coherent descriptive metaphysical system and a metatheoretical architecture that can assimilate and

integrate major empirical findings and the key insights from the major schools of thought.

The Metaphysical-Empirical Continuum

Empiricism is arguably the cornerstone of modern scientific knowledge. The essence of science lies in basing claims on measurement and experimentation from an objective, third-person perspective. Data are analyzed through theoretical models that aim to provide causal explanations, helping to clarify the phenomena under investigation. Ideally, these models and theories are connected to a broader framework of understanding. For instance, if a researcher is studying a particular aspect of DNA, that aspect should ideally be understood within the larger context of genetics, cellular biology, and evolution.

However, as the sandcastle problem highlights, mainstream psychological research lacks a unified framework, or a true “paradigm,” as Kuhn ([1996](#)) originally described it. Over time, the term paradigm has evolved to mean either a comprehensive system of understanding that guides an entire field or the specific school of thought adopted by a subset of researchers. As such, psychological science can be considered to be simultaneously pre-paradigmatic and multi-paradigmatic (Henriques, [2022](#)). It is pre-paradigmatic because it lacks a unified paradigm in the original Kuhnian sense, and it is multi-paradigmatic because it consists of many different schools of thought.

With this point clarified, we can now introduce the metaphysical-empirical continuum, as developed by Henriques ([2019](#)), to help frame knowledge in psychology. At the concrete end of the continuum are the empirical data obtained by the methods of science. At the opposite, more abstract end, are the metaphysical concepts and categories that are used to map the territory at the highest levels of abstraction. Between these poles, the continuum frames knowledge production as unfolding across four levels: metatheory, paradigm or school of thought, theory, and hypothesis. Together, these levels form the

continuum starting with (a) metaphysical concepts and categories for mapping reality; (b) metatheory, which provides a trans-paradigmatic view of the field; (c) the paradigm or school of thought that organizes the subject matter; (d) the theory that provides the causal explanatory framework, grounded in a paradigm; (e) hypotheses, or testable predictions derived from theory; and finally, (f) the empirical data gathered through research methods.

When a science becomes fully paradigmatic in the original Kuhnian sense, there is a unification of the metaphysical side of the continuum in a way that aligns with the empirical side. For example, Newtonian mechanics was a “true” paradigm in this sense for over 200 years in physics, offering a set of assumptions and laws that framed the subject matter of physics and provided its explanatory structure in a way that accounted for its empirical findings.

In contrast, psychology is organized into paradigms in a weaker sense, referring to specific schools of thought shared by subsets of researchers. Psychology is lacking in a true paradigm that provides a shared framework its subject matter as well as a metatheoretical structure to ground its explanations. We argue that the core issue in psychological model and theory building today stems from confusion on the metaphysical side of the continuum.

It is important to clarify the term “metaphysics,” as it can lead to confusion. In some contexts, metaphysics is associated with the unknowable or abstract philosophical debates that are disconnected from empirical reality. For example, some might pejoratively label the question of how many angels can fit on the head of a pin a “metaphysical question” because it is not a question that can be answered empirically. The term is rarely discussed in scientific psychology, and this is a primary reason why. However, in its academic sense, metaphysics refers to a branch of philosophy that explores the nature of reality and the concepts and categories we use to understand it.

Henriques ([2022](#)) draws on a distinction that helps clarify the type of metaphysical considerations necessary for solving the problem of

psychology. Descriptive metaphysics (Strawson, [1959](#)) involves defining core concepts and categories to form a cohesive system of understanding. Unlike “pure” metaphysics, descriptive metaphysics is concerned with empirical referents; processes and phenomena that can be directly experienced or scientifically observed. Cahoon (2013) similarly advocated for a “systemic metaphysics” that clarified the concepts and categories that had been mapped by science. Framed in this way, metaphysics can refer to a descriptive system of concepts and categories that can effectively account for empirical phenomena in the world.

The problem of psychology can now be reframed as a problem of descriptive metaphysics. We do not have a conceptually clear understanding of the natural world and the necessary concepts like nature, science, mind, and behavior to specify what it is that psychology is about. Put differently, the science of psychology is lost in the Enlightenment Gap. According to this analysis, if we are to understand psychology as the “science of mind and behavior” in some form or another, we need big picture view that clearly defines the way science maps the natural world and enables us to have shared definitions of both behavior and mind. In other words, we need a descriptive metaphysical system for defining our key concepts and how they relate to one other.

A Descriptive Metaphysical-Metatheoretical System that Solves the Problem of Psychology

Henriques ([2022](#), [2024](#)) has developed a new framework called UTOK, the Unified Theory of Knowledge. It evolved from the Unified Theory (UT) first outlined in *A New Unified Theory of Psychology* (Henriques, [2011](#)). Consistent with the metaphysical-empirical continuum and the need to advance the abstract side of the spectrum, UTOK includes both a descriptive metaphysical system for natural science, behavior, and mind, as well as a metatheoretical architecture that ties these elements together in a new way.

Prior to elucidating these components, it is useful to briefly situate UTOK in the history of ideas in psychology, as it is structured in a way that connects to the field's historical roots and aspirations. In the 1920s, there was a movement called "emergent evolution" that attempted to provide a holistic conception of both the universe and our scientific knowledge of it that could effectively place psychology in the scheme. In Europe, "stratification theories" were extremely popular in the interwar period and influenced many fields, including neuroscience and psychology. The prototypical stratification theory is found in Aristotle's scales of nature, which identifies inanimate matter, life, animals, and humans as representing different domains. The philosopher Nicolai Hartmann built upon this and developed an ontological system that legitimized these different layers.

Wieser ([2018](#)) reviewed this history, and he summarized the rise and fall of stratification theories in psychology and related fields. Although these frameworks held much appeal, they fell out of favor for at least three reasons. One major problem was that there were many different models of stratification. Some had three layers, some had four layers, some had six or more. There were also different terms for the different layers and different dividing lines between them. The rise of scientific empiricism meant that ideas needed to be tied to data and have the potential to be falsified. A second major problem was that, although these frameworks provided potential for empirical investigation, they generally were not framed so that they could be tested against one another to determine which depiction was best. Finally, stratification ideas in the interwar period were often used to justify ideas of progress tied to Nazism and racial superiority. The rise of the postmodern sensibility in the postwar era reacted strongly against "grand narratives," especially ones that suggest that the author and his kind represented the highest forms of being.

As will become clear, UTOK is anchored to a stratified view of nature. However, it corrects many of the past errors. It transcends the stratification theories of the last century precisely because it provides the user with clear-cut definitions and does so in direct contrast to the

failure of modern empirical psychology. In UTOK, natural science, behavior, and mind are explicitly defined. In addition, UTOK includes and transcends the postmodern critique. It frames cultural knowledge as consisting of systems of justification and places science within that context. In short, UTOK succeeds where holistic and stratified approaches of the past have tried and failed.

The Tree of Knowledge System: A New Map of the Stratification of Nature and Science

The Tree of Knowledge (ToK) System offers a map of cosmological evolution and our current scientific knowledge of it (Fig. 6.1). It divides the evolution of behavioral complexification into five layers. The first layer, existing before the Big Bang, is framed as an Energy-Information Implicate Order. From this, four distinct planes of existence emerged in the following sequence: (1) Matter-Object; (2) Life-Organism; (3) Mind-Animal; and (4) Culture-Person.

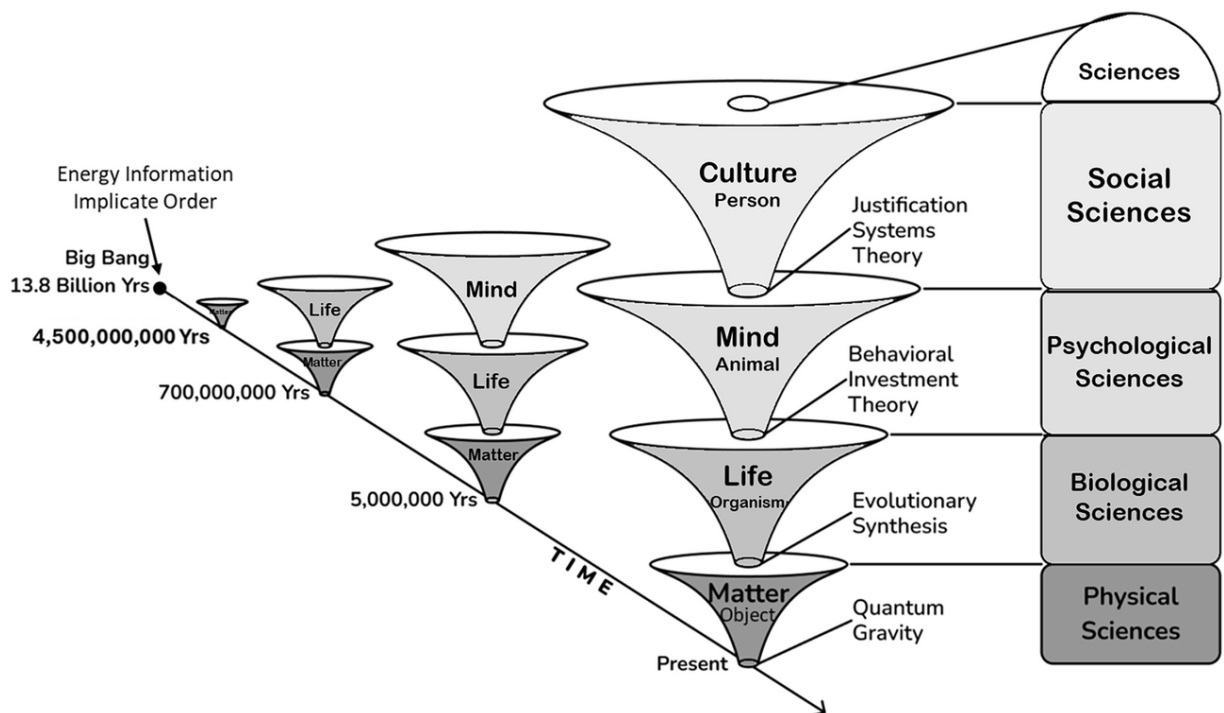


Fig. 6.1 The tree of knowledge system

The ToK System represents a new model of Big History (Henriques, [2022](#)). Big History, an interdisciplinary approach founded by David Christian in the mid-1980s, divides cosmic history into eight key “thresholds.” Henriques and Volk ([2023](#)) argue that the ToK System offers a more refined approach to Big History because it: (a) distinguishes general complexity from the specific trajectory of behavioral complexification that led to humans, (b) explains how this path involves both increasing organization and leaps into new planes or dimensions of existence, (c) clarifies how these jumps arise from evolutionary processes involving propagation, variation, selection, and retention feedback loops, and (d) identifies the Mind-Animal dimension as a distinct plane of existence.

Henriques ([2004](#)) demonstrated how the ToK System allows us to clearly define the subject matter of psychological science. It addresses psychology’s “vertical” problem by clearly positioning the Mind-Animal domain above the Life-Organism plane mapped by the biological sciences and below the Culture-Person plane examined by the social sciences. Additionally, it resolves psychology’s core “horizontal” conflict of subject matter between mentalism and behaviorism.

A key insight that stems from the ToK System is that the Mind-Animal plane consists of mental behaviors. These behaviors emerge as animals’ nervous systems become more complex. This dimension takes off during the Cambrian explosion, which saw the rise of animals with brains and complex active bodies (Trestman, [2013](#)). This perspective allows us to see both the continuity between Life and Mind and their critical differences. Even from a folk psychology standpoint, it’s easy to distinguish the behavior of trees, grass, or mushrooms from that of a squirrel cracking a nut. The concept of “mindedness” (Henriques, [2022](#)) highlights this distinction: animals move through their environment using a sensorimotor loop, coordinated by their complex, active bodies.

In addition to framing the four planes of existence, the ToK System also depicts science as part of its broad world map. It shows that science both exists within the Culture-Person dimension and emerges out of it in a way that allows it to map behavioral complexity in nature.

Henriques ([2022](#)) explains why this means that, in addition to framing nature in a way that allows us to see “Mind,” the ToK System also enables us to see clearly the relationship between science as a human construction and science as a tool that maps behaviors in nature. This relationship becomes much clearer via UTOK’s Periodic Table of Behavior.

The Periodic Table of Behaviors in Nature

The Periodic Table of Behavior (PTB; Fig. [6.2](#)) expands on the ToK System, offering a way to classify patterns of increasing behavioral complexity in nature and link them to scientific domains. It is a taxonomy that identifies three distinct patterns of emergence: (1) aggregates across scales, (2) levels of complexification, and (3) novel dimensions of complexity shaped by evolution.

The Periodic Table of Behavior					
	Dimensions of Complexity				
		MATTER	LIFE	MIND	CULTURE
		Physical	Biological	Psychological	Social
Object-Field Relations	Context of Behavior	Field	Ecology	Environment	Society
	Behavioral Entity	Object	Organism	Animal	Human Person
Three Primary Levels of Object Complexity (Part, Whole, Group)	Groups of Wholes	Molecule	Multicell/Colony	Family-Group	Family-Community-Nation
	Fundamental Whole	Atom	Cell	Mind-Brain System	Self-Consciousness System
	Fundamental Part	Particle	Gene	Neural Network	Symbolic Justification

Fig. 6.2 The periodic table of behavior

The top row of the PTB, labeled Domains of Reality and Science, mirrors the ToK System, aligning the physical sciences with matter, biological sciences with life, psychological sciences with mind, and

social sciences with culture. The second row, General Object-Field Relations, captures broad behavioral patterns across each plane, including aggregate arrangements across different scales in nature. For instance, material entities range from the tiny neutrinos to vast structures like the Hercules-Corona Borealis Great Wall, spanning 10 billion light years. Similarly, biology scales from methane molecules to the planet-wide system sometimes called “Gaia.”

The PTB suggests each plane of existence has a primary unit of organization: atoms for Matter-Object, cells for Life-Organism, animals with brains for Mind-Animal, and self-conscious persons for Culture-Person. In philosophy, mereology studies the relationships between parts, wholes, and groups. Recognizing each plane’s primary unit allows us to use mereology to see how these wholes are made up of parts and can form groups. The third row of the PTB identifies the primary units of analysis for each dimension, incorporating the part-whole-group relationship. This creates a 3-levels-by-4-dimensions table of behavioral complexification that traces the major steps from the Big Bang to modern humans.

UTOK argues that natural science is a system designed to describe, explain, and predict nature’s behavior patterns. This can be linked with the PTB’s taxonomy to predict that science’s main fields align with the PTB’s classification. The first six PTB floors align with the natural sciences, divided into physical and biological categories. For example, Floor 1 relates to subatomic particles and forces, mapped by quantum physics, while Floor 2 corresponds to atoms and the Periodic Table, and Floor 3 to molecules and chemistry. The PTB also covers other physical sciences, such as astronomy (examining large cosmic entities) and earth sciences, which combine physics and chemistry. Shifting to biology, Floor 4 marks the transition to biochemistry and molecular biology, focusing on cells and their components, while Floor 5 covers cytology, the study of cells. Eukaryotic cells, which emerged through a symbiotic event, paved the way for multicellular organisms. Fields like botany, mycology, and zoology align with Floor 6, representing these organisms.

In the Mind-Animal plane, animals are the primary unit of analysis. Neuroscience corresponds to Floor 7, while Floor 8 focuses on animal behavior, aligning with psychology and ethology, although confusion persists over where biology ends and psychology begins, a topic that we address in a separate section below. Floor 9 covers animal group behavior, linked to behavioral ecology and sociobiology.

At the Culture-Person level, the key unit is a self-conscious person who justifies actions in a social context. Floor 11 covers human psychology, addressing the issue of whether psychology focuses on all animals or just humans. Subfields like developmental, personality, and social psychology map human behavior in cultural contexts. Floor 10, bridging animals and humans, aligns with cognitive science, an interdisciplinary field studying how humans process information, make decisions, and communicate. Floor 12 focuses on human groups, corresponding to social sciences like anthropology, economics, and sociology.

Defining Psychology with the ToK and PTB

The ToK System and PTB offer a new view of behavior and science, helping us define the domain of the mental. The Mind-Animal plane explains how animal behavior differs from the Life-Organism plane, while the Culture-Person plane shows how human mental behaviors are similar to but also distinct from those of other animals.

Henriques ([2004](#)) highlighted a key distinction that divides psychology into two main branches: basic and human psychology. Basic psychology studies the animal-mental dimension, focusing on nervous system processes and behaviors in animals, aligning it with the natural sciences. It aims to objectively describe and explain animal behavior. Human psychology, though connected to basic psychology, is a separate branch focused on humans as self-conscious agents capable of justifying actions in social contexts. This branch aligns with the social sciences, addressing justification systems and social dynamics. Finally, both basic and human psychology can be separated from and aligned with professional psychology (see Henriques & Sternberg, [2004](#)).

The Map of Mind

The Map of Mind (Fig. 6.3) is the final element in UTOK's descriptive metaphysical system. It is necessary because, by themselves, the ToK System and PTB do not allow us to clearly delineate subjective conscious experience. The reason is that they adopt a third-person behavioral science perspective, and thus do not capture the first-person subjective point of view.

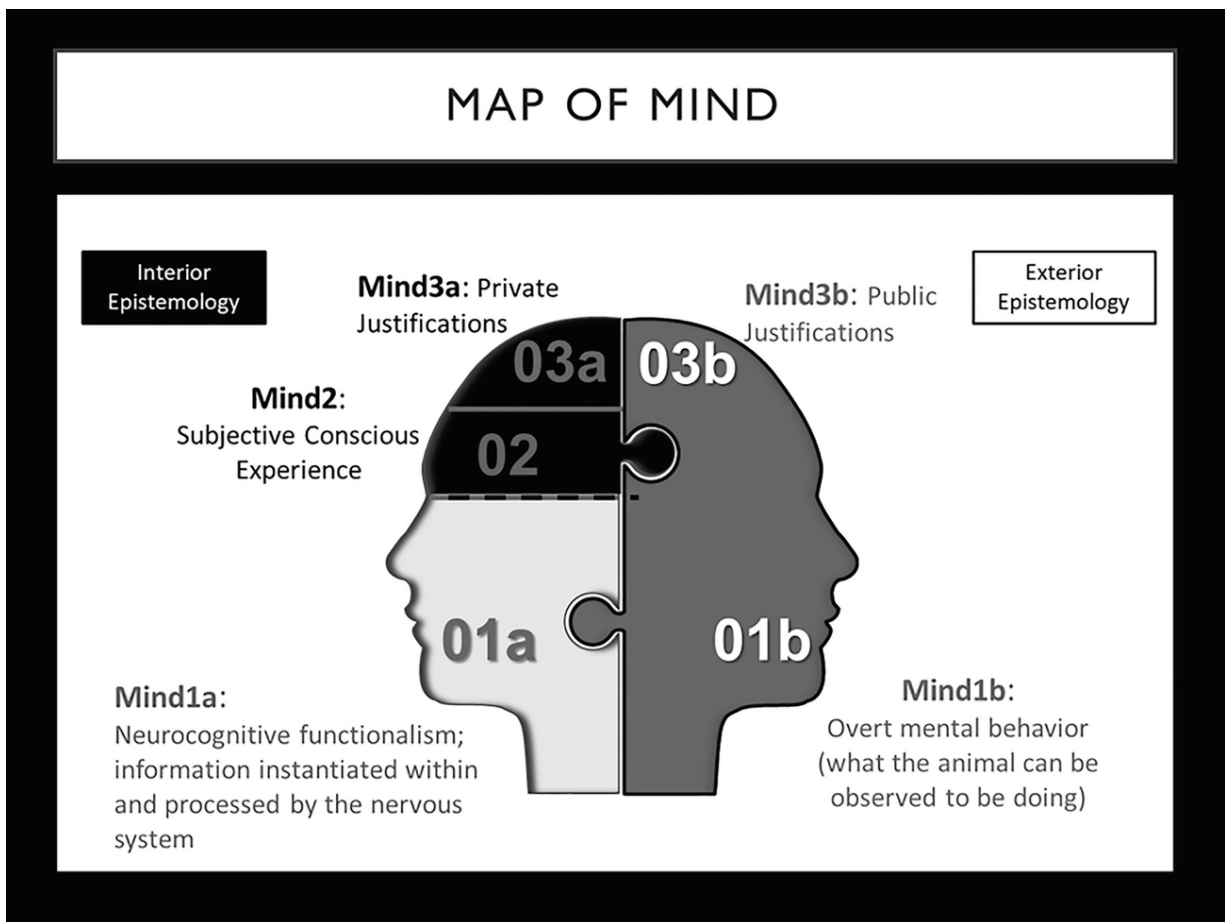


Fig. 6.3 The map of mind

The Map of Mind organizes the main referents for mind by: (1) distinguishing between the mental behavioral processes that take place within the animal from those that take place outside the animal, (2) differentiating between the interior (first-person) and exterior (third-person) epistemological positions, and (3) differentiating between neurocognitive processing and sociolinguistic processing, in alignment

with the ToK System's differentiation between the Mind-Animal and the Culture-Person planes of existence.

Three core domains of mental processes emerge from this taxonomy: Mind1, Mind2, and Mind3. These domains are further divided to capture different contexts and levels of mental processes. Namely, the letters "a" and "b" are used to differentiate mental behavior within the animal and outside the animal (between animal and environment), respectively. The resulting structure is given as: (1) Mind1 as the domain of the observable neurocognitive activity of the animal, with Mind1a referring to the covert neurocognitive activity in the nervous system and Mind1b referring to the overt actions happening between the animal and the environment; (2) Mind2 as the field of subjective conscious experience, not accessible via an exterior epistemology, and (3) Mind3 refers to the domain of self-reflective awareness and sociolinguistic justification, which is uniquely human. Mind3a refers to private egoic narration and Mind3b pertains to overt speech.

The Map of Mind clarifies the various domains of mental processes, offering a more precise and comprehensive vocabulary for psychology. With the descriptive metaphysics of behavior and mental processes clarified, the foundation is in place for the construction of causal explanatory metatheories that synthesize key paradigms in the field in a shared, unified language system.

Justification, Investment, and Influence: UTOK'S Metatheoretical Framework for Human Psychology

UTOK's ToK, PTB, and MoM frame the descriptive metaphysical end of the metaphysical-empirical continuum. The next point on the continuum is metatheory, and UTOK's Unified Theory of Psychology (Henriques, [2011](#)) is framed by three metatheoretically integrative ideas. Behavioral Investment Theory (BIT) maps the mental behaviors at the Mind-Animal plane from a neurocognitive functional perspective.

The Influence Matrix (IM) maps the relational field that emerges as mammals engage in intricate social dynamics for survival and reproduction, which evolves into the highly complex human relationship system. Finally, Justification Systems Theory (JUST) tracks the evolution of how self-consciousness and the Culture dimension of behavioral complexification emerged and transformed us into persons.

Behavioral Investment Theory

Behavioral Investment Theory (BIT) is UTOK's joint point for framing and explaining the emergence and evolution of the Mind-Animal plane of existence. BIT posits that the evolution of the brain and overall nervous system in animals is fundamentally linked to movement and the coordination of actions of the body as a whole. BIT contends that the nervous system evolved as an investment value system that processes information to coordinate behavior toward optimal paths of investment, considering factors like energy expenditure, risk, cost, and reward. BIT effectively merges B. F. Skinner's concept of behavioral selection with key insights from cognitive science, framing mental behavior as an evolutionarily adaptive process that combines neurocognitive processing with environmental feedback, enabling animals to adaptively pursue valued goals toward survival and reproduction.

As a metatheory, BIT integrates insights from evolutionary biology, cognitive science, behavioral science, cybernetic control theory, and neuroscience. It is structured around six principles: (1) *energy economics*, which grounds mental behavior in the physics of energy and entropy, framing it in terms of energy expenditure balanced against a cost-to-benefit ratio; (2) *evolution*, which situates behavior within the context of natural selection across the species' evolutionary history; (3) *behavioral genetics*, which highlights the role of genetic inheritance in shaping behavioral predispositions; (4) *neurocomputational control*, which views the nervous system as an information processing and control system that is working to predict and manage the flow of natural resources; (5) *learning*, which focuses on how environmental

associations and contingencies from direct experiences and observations shape the behavioral investments of animals; and (6) *lifespan development*, which considers the role of developmental stages in shaping behavior over the animal's lifetime.

BIT tracks the evolution of mental behavior across the animal kingdom. This progression moves from *reactive* animals, to *learning* animals, to *thinking* animals, and finally into *talking* animals—that is, human persons. This evolutionary trajectory is captured in the “Architecture of the Human Mind” diagram, which integrates modern models of memory to illustrate how neural-information processing is instantiated and flows through the evolved layers of mental behavior.

Animal mental behavior evolves through distinct stages of complexity: starting with reactive mental behaviors (e.g., reflexes and automatic responses), advancing to learning-based mental behaviors (e.g., adapting actions based on environmental feedback and experiences), moving to thinking-based mental behaviors (e.g., simulating possible future scenarios and planning actions accordingly), and culminating in human language-based mental behaviors (e.g., self-reflection and verbal justification both privately and publicly). BIT conceptualizes these levels as evolving layers of neurocognitive complexity that integrate perceptual, motivational, and emotional processing systems to optimize behavioral investments.

In understanding how animals, including humans, navigate their environments and optimize their actions, BIT lays the groundwork for exploring the relational complexities that define human social behavior. This brings us to the Influence Matrix, a key extension of BIT that maps the human relationship system.

The Influence Matrix

The Influence Matrix (IM) integrates insights from evolutionary theory, attachment theory, personality theory, and social psychology to explain the core dynamics that drive mental behaviors in the human relational world (Henriques, [2022](#)). It emphasizes two fundamental human social needs: *social influence*—the ability to influence others according to

one's interests—and *relational value*—the perceived sense of being seen, known, and valued by important others.

The IM is additionally structured around three key self-other process dimensions: power, love, and freedom. The power dimension maps social hierarchical and competitive dynamics, where individuals vary in rank and resources and coordinate their relations accordingly. This includes both direct power, such as authority roles in a boss-employee relationship, and indirect power, like subtle social status comparisons. The love dimension captures the spectrum from cooperation and affiliation to hostility and contempt, and represents how humans form bonds of affection or, conversely, reject or oppose others. Finally, the freedom dimension spans the continuum from dependency to autonomy, tracking the degree of social involvement and one's freedom to act independently versus being controlled by others. This dimension is important in understanding attachment styles and developmental processes that balance dependency needs and self-reliance.

The IM illustrates how individuals navigate the social world by continuously calibrating their positions along these dimensions, using strategies like care eliciting, competition, altruism, and autonomy to negotiate social influence and relational value. In this way, the IM provides a model for tracking state changes in response to social situations, describing how individuals navigate complex and fluid relational fields that blend cooperation and competition, dominance and submission, and dependency and autonomy in flexible and adaptive ways. By incorporating these elements coherently, the Influence Matrix offers an evolutionarily grounded framework for understanding human social motivation and emotion, effectively bridging insights from multiple lines of thought in psychological science.

Justification Systems Theory

Justification Systems Theory (JUST) is the metatheoretical framework that explains the evolutionary emergence of the Culture-Person plane

of existence. JUST provides insights into how humans transitioned from being primates to becoming persons with self-consciousness and complex cultural systems.

The core insight of JUST posits that there was a tipping point in hominid evolutionary history where we transitioned from symbolic tagging into the use of propositional language—language that allows for declarative statements. This shift created the *problem of justification* (Henriques, [2003](#)), a novel adaptive challenge that shaped both human self-consciousness and culture. The problem of justification refers to how the emergence of propositional language introduced question-and-answer dynamics, where statements about the world could be challenged, requiring humans to justify their thoughts, feelings, and actions on the social stage. The problem involves two interrelated dimensions: the analytic (truth or accuracy) and the evaluative (value or practical implications). The former concerns whether a proposition aligns with the current state of affairs, while the latter refers to implications for what ought to happen, both at the personal and social levels. According to JUST, navigating what is and is not justified across those dimensions shapes human thought, action, and social interactions, giving rise to justificatory self-conscious reflection and the narratives about the self.

These dynamics of justification set off a feedback loop of increasing complexity that shaped the structure and function of human self-consciousness. JUST maps the core domains of human consciousness through the Updated Tripartite Model (UTM), which divides consciousness into three domains, depicted below. The Experiential Self refers to the felt, embodied experience of being in the world and includes both perceptual awareness, motivational-emotional states, and relational dynamics involving social influence and relational value. It aligns with the Mind-Animal plane of existence, and it is shared with other mammals. The Private Ego is the self-reflective portion of consciousness that narrates experiences and generates justifications to maintain a coherent self-concept and avoid cognitive dissonance. Finally, the Public Persona manages social roles and projects an image to others on the social stage as a function of the relational dynamics

tracked by the Influence Matrix. This model emphasizes how human consciousness is fundamentally shaped by the need to justify actions, beliefs, and values both internally to oneself and externally to others.

According to JUST, justification dynamics also gave rise to large-scale justification systems, which are interlocking networks of language-based beliefs and values that legitimize a specific version of reality or worldview in human groups. These systems of justification function as the social constructs that coordinate people, provide a shared set of roles and expectations, and shape human cultures. In this way, JUST provides a framework for understanding the evolution of human culture and consciousness, marking the transition from primates to persons who navigate the context of justification on the Culture-Person plane of existence.

In framing the emergence of human culture as large-scale systems of justification, JUST connects individual-level cognition with the broader socio-cultural contexts that shape and are shaped by justification processes. Together, these insights allow JUST to function as a metatheory that ties together the evolution of human language, self-consciousness, and cultural systems, providing a comprehensive framework for understanding how humans became uniquely positioned as persons operating in a socially constructed reality.

The Dynamic Combination of Justification, Investment, and Influence

It's important to recognize that Justification Systems Theory, Behavioral Investment Theory, and the Influence Matrix work together to form a dynamic framework for understanding and explaining human mental behavior. Henriques ([2022](#)) explains how these three concepts align with folk psychology.

Folk psychology refers to how people use common sense and everyday language to describe human actions and explain why people behave the way they do. The most basic, intuitive, and practical way to approach this is through “belief-desire-action” psychology. In everyday situations, people explain their actions by referring to their beliefs and

desires. Beliefs are what a person consciously knows or understands, while desires represent what a person wants or hopes for. Actions are simply what people do in response to these beliefs and desires.

Compared to major psychological theories like psychoanalysis and radical behaviorism, UTOK (Unified Theory of Knowledge) aligns much more closely with folk psychology and its belief-desire framework. In fact, UTOK, through its theories of investment, influence, and justification, not only incorporates folk psychology but also goes beyond it by offering a more comprehensive set of constructs.

UTOK'S Implications for the Future of Model and Theory Building in Psychology

We have argued that there is a vague conceptual space that corresponds to the territory that psychological science represents in the world. The APA's definition of psychology makes this ambiguity clear; psychological science lacks a way to frame its foundation. It cannot frame the edges of the psychological puzzle and thus cannot put the parts together. If we use a sandcastle metaphor, the foundational ground for mid-level theories is soft and the empirical methods for obtaining the insights do not connect in obvious and clear ways to other lines of research. This is not a problem that can be solved with more research. The well-known replication crisis points to a significant difficulty in psychological science in achieving cumulative knowledge. However, unlike many, we do not believe that the primary solution is better methods and larger data sets. Rather, the primary solution is clarity about the fundamental conceptual ground that frames psychological science.

The takeaway from this chapter is that models and theories in psychology must be positioned along the metaphysical-empirical continuum. Researchers should clearly identify the paradigm within which their work operates and understand how it relates to the larger metaphysical and theoretical framework. This second step, which has been largely missing, is crucial. The crisis in psychology led to the belief

that developing a coherent, overarching perspective was impossible and could be disregarded. We argue that this perspective was not just ignored but actively denied, leading to the “sandcastle problem.” Now that a coherent metaphysical system with a comprehensive metatheoretical structure has been proposed, it is possible to start aligning mid-level paradigms with one another.

There are already many examples of this type of work in the literature. Henriques and Panizo ([2018](#)) demonstrated how the Unified Theory of Knowledge (UTOK) could be applied to understanding depression. While there are numerous models and theories of depression, the concept still lacks a clear, consensual definition. Henriques and Panizo used UTOK’s framework to show how depression can be viewed as a state of behavioral shutdown, connecting perspectives that describe it as a normal human response, a maladaptive psychological disorder, and a biological disease state.

Recently, we applied UTOK to psychotherapy (Henriques & Gralha, [2024](#)). The most consistent finding in psychotherapy is that various legitimate treatments tend to produce similar outcomes, a phenomenon known as the “dodo bird verdict,” where everyone wins and deserves a prize. Research suggests that the key factors driving change in psychotherapy include the therapist’s legitimacy in their societal role, the quality of the therapeutic relationship, the therapist’s ability to foster shared understanding of the client and their problem, and the willingness to engage in active interventions to promote change. We showed how UTOK’s metaphysical and metatheoretical structure could explain these common factors, elevating them from commonsense observations to a sophisticated, theory-driven explanation. We then argued that the major schools of thought in psychotherapy could be understood as helping individuals break maladaptive cycles through the lenses of justification, investment, and influence.

These examples illustrate that psychological theories and models must be grounded in both empirical findings and broader theoretical frameworks. When done effectively, this creates a robust network of descriptions and explanations. In addition to depression and

psychotherapy, UTOK has been applied to various phenomena, including intimate partner violence (Gow, [2007](#)), borderline personality disorder (Cechak, [2021](#)), well-being (Henriques et al., [2014](#)), mindfulness (Miller, [2022](#)), personality theory (Menzies, [2013](#)), and consciousness (Henriques, [2022](#)).

Conclusion

What is psychology and what does it correspond to in the world? We contend that the problem of psychology is real, important, and remains unresolved. We have argued that the reason for that is that it sits inside of a larger problem. The Enlightenment Gap means that, in the wake of modern science, we have never been able to effectively place mind in relationship to matter and science in relationship to subjective and social forms of knowing.

This confusion resulted in psychology increasingly relying on empirical methods, and ultimately defining itself as a science by virtue of applying such methods. The result of failing to find the edges of the discipline has been a chaotic fragmentation and proliferation of models. UTOK provides a new solution. It bridges the Enlightenment Gap and defines psychology both vertically and horizontally with a new descriptive metaphysical system. It also provides a broad, metatheoretical architecture that has been shown to be able to assimilate and integrate key ideas into a coherent whole. As such it is well positioned to help with the future of model and theory building, as scholars can now approach their work from both sides of the metaphysical-empirical continuum.

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Part II

Current Challenges for Theory and Model Building

7. The Unresolved Puzzle of Psychological Theories

Paul De Boeck¹✉, Jolynn Pek¹✉, Barbara L. Andersen¹✉ and
Brandon M. Turner¹✉

(¹) Department of Psychology, The Ohio State University, Columbus,
OH, USA

✉ **Paul De Boeck (Corresponding author)**

Email: deboeck.2@osu.edu

✉ **Jolynn Pek**

Email: pek.5@osu.edu

✉ **Barbara L. Andersen**

Email: andersen.1@osu.edu

✉ **Brandon M. Turner**

Email: turner.826@gmail.com

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Introduction

Psychologists understand the world in terms of concepts and theories. Understanding seems to be a basic human need that is met with explanations of what we, perhaps selectively, observe in the world. Theories provide us with these explanations. The need for

understanding, and hence, for theorizing, might stem from its instrumentality in developing expectations for the future (prediction) and to frame accommodations and adaptations in our interactions with the world (control). In science, we employ methodologies in the construction and appraisal of theories. It is known that psychology has a history of crises related to methodologies and theories (e.g., Sturm & Mülberger, [2012](#)), which might stem from slower progress than hoped for. The latest crises are about replication and theory (Green, [2021](#); Oberauer & Lewandowsky, [2019](#)). For non-replication across cultures and time, Hutmacher and Franz ([2025](#)) refer to a universality crisis. Not surprisingly, crises tend to come with proposals for how to improve methodology and theory.

Recently, proposals and recommendations have been published for theory appraisal and for theory construction. Theory appraisal is focused on the replication of effects (e.g., Asendorpf et al., [2013](#); Simons, [2014](#)) in line with the hypothetico-deductive (H-D) approach of testing (i.e., appraising) hypotheses deduced from theory. Theory construction is an activity for which a methodology is lacking in the H-D approach (e.g., Borsboom et al., [2021](#)).

While ineffective and flawed methodologies may occur, we see continuing issues in psychological science (replication failures, weak theories) as inherent to the complexity of human behavior. To deal with the complexity of human behavior (in a broad sense) and its explanation as a source of the continuing issues, our aim is to offer and detail three desiderata one might consider to make progress.

1. *Paradigmatic and theoretical pluralism.* Different paradigms¹ and theories may apply depending on the type of phenomena, subpopulations (e.g., culture, time period), and circumstances. The extent of how relevant theories and paradigms are can also occur in a continuous way, which may also depend on the individual (e.g., Bem & Funder, [1978](#)).
2. *Generalization and random components.* To avoid extremes of strict particularism and strict universalism, a generalizability perspective and the use of (ad hoc) random modeling components are useful

and the use of (advanced) random modeling components are useful as a formal way to model the complexity of psychological phenomena. They are tools to abstract away from less relevant specifics of a phenomenon, bringing focus to its core aspects if any.

3. *Theoretical and practical relevance of applications.* Theories can be informed by observations in application, and application can improve (e.g., clinical) practice. Applied questions can also drive theory development.

Before we expand on these desiderata, we introduce the problem of complexity in two ways: with a puzzle metaphor and with a more technical description of important features of human behavior as well as how major methodologies have dealt with the problem of complexity. To further set the stage for a discussion of the three methodological desiderata, we present a short and selective history of the scientific method in psychology, related to some recent developments. Finally, we expand on and discuss the three desiderata.

The Puzzle of Psychological Theories

The problem of complexity can be illustrated with a metaphor. Consider a puzzle with images on the pieces of the puzzle that together represent an object. Using the puzzle as a metaphor, suppose that a psychological theory about a phenomenon is a tentative answer to the question “What is represented on the puzzle?” For example, the puzzle may represent a flower or some other object. Each piece of the puzzle shows a part of the object (and perhaps also of its background). The images on the pieces are the metaphorical analogue of an empirical finding from an individual study or a set of similar studies regarding a phenomenon. For a single puzzle piece, generating an interpretation can be thought of as a mini-theory. For example, a mini-theory is interpreting a plain green piece without the shape of a leaf as representing part of a leaf. That the piece represents part of a leaf might be of interest itself, but

the leaf as a part of the puzzle might also fit with the overall picture of the puzzle (e.g., of a flower or vegetable; i.e., the theory).

To present the issues of complexity that psychology encounters, let us assume that the pieces of the metaphorical puzzle do not fit together in a way to form a visually coherent and specific image (e.g., of a specific flower). Instead, puzzle pieces are possible parts of the object in a more abstract way instead (e.g., a “flower”). Let us further assume there are several complications. (a) Some pieces provide poor information—the analogue of information provided in psychological studies—because they show a minor detail, have low resolution, lack accuracy (i.e., some may be distorted), or are irrelevant (e.g., representing background elements). (b) Pieces can seem inconsistent—the analogue of disjointed and contradictory findings—without disregarding the variation of specifics. For example, there may be a piece with thorns on the stem (as if for a rose) and another piece with tulip leaves, which may seem inconsistent unless one leaves aside the specifics of flower types. (c) The object may be atypical—the analogue of out of scope theories—a non-existing object (e.g., a humanized flower, with hands and feet), a combined object (e.g., a shirt with flower design), or an object in unexpected shape, etc. Expectations about what is represented on the puzzle can be misleading if one expects to see a clear and realistic representation. All three complications (presence of uninformative pieces, perceived inconsistencies between pieces, atypical objects) may prevent finding a solution for the puzzle (i.e., a good theory).

Although meant as a metaphor for the issues psychology encounters, the puzzle metaphor does not fully represent all challenges. Whereas a typical puzzle has a complete set of pieces to work with, studies in psychological research generate new pieces, for example, to test a hypothesis, and even so, there may be crucial missing pieces.

Continuing with the puzzle metaphor, the three discussed complications are each related to a major challenge psychological science is faced with. The first challenge concerns the information provided by the puzzle pieces (observations and their interpretation). The replication crisis and the fragmentation into isolated mini-theories

are examples observed in the field. The second challenge concerns the level of abstraction that interacts with what the pieces of the puzzle specifically show, that is, the level of abstraction required to find commonality across different expressions of the same phenomenon. Although all theories are abstract to some extent, the question is: What is the best level of abstraction for a set of findings? A one-fits-all level of abstraction is unlikely. Similar questions can be raised about psychological constructs. There is a discrepancy between the specifics of pieces and the abstractness of the represented object. The third challenge concerns cognitive biases. In terms of the typical puzzle, we expect (a nicely) coherent and familiar kind of picture when the pieces of the puzzle are put together. Mapping this onto psychological theories, we often have the (implicit) belief that parsimony is better and only one theory can be true. These and other tendencies toward familiar and intuitive notions can be a source of bias (cf. “knowing” what kind of picture to expect). For example, complex dynamics and the unconscious and blind copying of feelings and thoughts may be considered less plausible behavioral principles. Although the first challenge has attracted plenty of attention, the other two challenges are less noticeable in the literature.

Behavioral Features at the Basis of the Problem of Complexity

Temporarily leaving the puzzle metaphor but continuing to focus on complexities, we will now highlight two features of human behavior which necessarily lead to a high degree of complexity. These two features (many-to-many relations and multiple and higher-order dependencies) can explain why research findings vary from study to study, why useful models might not fit well to data, and why it is so difficult to build sufficiently successful theories.

We first introduce the notion of a *behavioral stream* as a scaffold to explain and understand these features. Behavioral stream refers to a sequence of overlapping overt and covert behavior (from the

perspective of the observer). For example, while we get up in the morning (overt) we may think of the activities planned for the day and we may experience a mixture of joy and stress (covert, inner and unexpressed thoughts and feelings). Behaviors have different sources and might also have downstream effects. Behaviors, their sources and effects, may in turn lead to other behaviors, and so on. Behaviors can be simple (e.g., check the time before leaving home) or composite (e.g., attending a class), sources of behavior can be events (e.g., missing a bus on the way to campus), experiences (e.g., failure and consequent frustration), states and traits (e.g., states of emotion, high activity level), and effects can fade away immediately (e.g., relief when making it to class in time) or they last (satisfaction with the result of a difficult quiz in class).

Most empirical studies in psychology are controlled interventions in the behavioral stream (e.g., manipulation in an experiment, being presented with a test). Indeed, such empirical studies are necessarily embedded in the natural behavioral stream of the day because the natural stream cannot be halted. Other (individual and/or shared) sources remain active and are not eliminated (e.g., an ongoing inflation).

Many-to-Many Relations

The notion of many-to-many relations as used here is inspired by set theory, with one set for sources and the other for effects, knowing also that behaviors can function as sources and as effects. The same source can precede a variety of behaviors and the same behavior can stem from different sources. For example, the same emotion (e.g., anger) can be expressed in different behaviors, for example, shouting versus disengaging. At the same time, the same behavior (e.g., disengagement behavior) may stem from different sources (e.g., feeling angry versus bored). It may even happen that the same behavior (e.g., shouting) stems from opposite (kinds of) sources (e.g., expressing versus suppressing aggression behavior).

For a correct understanding, we add three caveats. First, although we use terminology for relations that can easily be interpreted as causal (e.g., effect, leads to) we do not make claims about the basis of the relations. Second, the relations as mentioned between sources and effects are deemed probabilistic. Third, the relations are most likely structured to some extent instead of being idiographic and completely dependent on the context. However, unraveling the structures of relations would be challenging when the structures are complex.

Multiple and Higher-Order Dependencies

Many-to-many relations may be due to dependencies of two types. The first concerns dependencies between behaviors and dependencies between effects. The dependencies can be positive or negative. For example, let a difficult task be a source that evokes concentration and persistence as behaviors, with concentration enhancing persistence. Alternatively, abandonment of a difficult task (a behavior) can lead to feelings of self-blame at the cost of feelings of relief. Behavioral dependencies, positive or negative, can lead to long-term “specialization” (e.g., behavioral styles) which may show as multidimensionality. For example, the human need for control as a general source of behavior can lead to dominant and power-seeking behavior (social control), but it can also lead to an interest in mastery of technical problems (technical control) or to an intellectual interest to understanding the world (a kind of cognitive control). Although all these types of control-based behaviors may co-occur, at least some individuals become specialized in behavior patterns through experiences of positive or negative effects of these patterned behaviors. Further specializations can lead to more specific dimensions (e.g., an interest in psychology vs. physics), etc. To make things even more complex, one specialization may be instrumental for another. For example, a specialization in cognitive skills may be instrumental for social control. High dimensionality (e.g., many types of control beyond social, technical, cognitive) as well as dimensions within dimensions (e.g., different types of social control) is a serious complication to

theories about constructs and to theory construction. Negative dependencies and specialization imply that behaviors with the same source (e.g., a trait, an emotion) may not be positively correlated.

Second, there are also dependencies that follow the structure of a statistical interaction (also known as moderation) that make behaviors stemming from one source dependent on another source. Furthermore, interactions include scenarios in which effects of a behavior are dependent on behavioral sources and on other behaviors (i.e., behaviors can also be behavioral sources). For example, a behavior (e.g., loudly protesting) that is induced by an attitude (e.g., valuing social justice) as a source may depend on other behavioral sources such as personality traits (e.g., self-righteousness) and situations (e.g., an event involving one's ingroup) and may be tempered by inner self-talk of staying calm.

Limited dependencies (pairwise or even a triadic) of the positive versus negative type and of the interaction type can be rather easily dealt with using existing methods. However, the situation is different if there are very many dependencies, and especially if the dependencies are of a higher order (e.g., higher-order interactions). Dependencies that entail positive or negative feedback loops may require a dynamical systems approach. In general, the problem of complexity is dealt with in different ways depending on how one approaches research.

How Different Approaches Deal with Complexity

Complexity might be less of a problem for *qualitative approaches* because they allow for the interpretation of behaviors by considering individual and contextual aspects of behaviors. Here, the same behavior (e.g., disengagement in one of the above examples) can have different meanings depending on the individual person and the context. This perspective is not quite possible to adopt for the typical quantitative approach such as regression analysis and factor analysis because the meaning of a variable is the same for all persons and all contexts. Instead, qualitative methodology is interested in the complexity of its research objects and individual and contextual aspects (Mey, [2023](#)),

which can be a fruitful basis to notice general aspects across variations in the appearance of a phenomenon (e.g., Levitt, [2021](#)). Potential weaknesses are objective verifiability and a high interpretational flexibility in meeting generalizability criteria.

For *classical quantitative methods*, the typical strategy to deal with complexity is to use simplifying assumptions for the unexplained variance in observations not captured through the core model. The unexplained variance is treated as error, for example, in multiple regression and factor models, so that it can be ignored for the purpose of interpreting focal effects and is relevant only to the degree of uncertainty about inferences. This simplification can be misleading, however, when misspecifications of a model enter the error term. Although the simplification can be relaxed through appropriate model extensions, it may not be easy to discover which misspecifications go into the error term or lead to distortions. Sometimes, a different kind of model may be more appropriate. In a certain way, because qualitative methods pay more attention to the contexts and the specifics of behaviors, they have the potential to examine more closely what is unexplained and may go into the error term.

In the approach called *data analytics* (or machine learning), the complexities are dealt with by using as input a very large number of specific variables and observations. The models for these data allow for complex interaction effects, dependencies, and nonlinear effects, primarily for the purpose of prediction instead of explanation. Thus, data analytic models can better accommodate the complexities of human behavior compared to classical quantitative methods. The main limitation of data analytic methods is that they are black box methods that make predictions from data, but with lack of transparency of how the predictions are made and with issues of interpretability of the results (but see Kamath & Liu, [2021](#) for methods of interpretation).

Issues to do with behavioral complexity make it difficult to solve the metaphoric puzzle of interpreting findings in psychology. The complexity makes for an unclear picture in which it is difficult to unveil coherence that is a necessary condition for understanding observed

phenomena. This may explain the tendency to focus on specific topics (specific cognitive tasks, specific effects, specific paradigms, specific constructs) (De Boeck et al., [2023](#)), that is, scientists tend to focus on pieces of the puzzle instead of piecing together the broader picture of a phenomenon or set of phenomena. The contrast is not new as explained in the next section on the history of scientific methodology in psychology and it is illustrated again in the aftermath of the replication crisis.

A Short Selective History of Recent Scientific Methodology

According to Popper in his 1935 book on “Logik der Forschung,” what demarcates science from non-science is falsifiability (Thornton, [2023](#)). Subjecting statements to possible falsification by observations is an activity that belongs to “appraisal” versus “generation” of hypotheses and theories, terms used by Haig ([2005](#)). The falsifiability view aligns with the H-D approach, which consists of deducing hypotheses from theory for the purpose of comparing specific predictions with observations. Often, null hypothesis significance testing (NHST) is used for H-D based appraisal. In practice, by emphasizing the testing of specific predictions, building broader theories can be neglected and complications of building a theory for a highly complex behavioral reality can be swept under the rug. Mini-theories are sufficient to formulate testable hypotheses, but these mini-theories may draw attention away from further theorizing and may seem to offer more certainty.

This tension between theory and observable effects can be observed early in the history of psychology as well as more recently. For example, William James contrasts his perspective against the mundane approach of observable experimental effects: “The results that come from all this laboratory work seem to me to grow more and more disappointing and trivial. What is most needed is new ideas. For every man who has one of them one may find a hundred who are willing to drudge patiently at

some unimportant experiment” (James, [1986](#), part XII, December 7). James’ perspective led to his process theory of emotions, which assumes that bodily states (e.g., trembling) precede and lead to the experience of an emotion (e.g., fear). Wilhelm Wundt, the champion of the experimental method that generates observed effects, in contrast, did not develop theories for observed effects. To be fair, some other early experimentalists did, for example, Ebbinghaus and Fechner.

The most recent opposition between an interest in observable effects and theory is illustrated by efforts to replicate specific effects (e.g., Open Science Collaboration, [2015](#)) and the subsequent growing interest in theory construction (e.g., Borsboom et al., [2021](#)). Let us first briefly summarize the initial reactions to the replication crisis which were concentrated on how psychological studies and methods could be improved to obtain more trustworthy empirical findings (e.g., Asendorpf et al., [2013](#); Lebel et al., [2017](#); Schimmack, [2012](#)), mostly in the line of the H-D approach: higher power, large sample size, large-scale direct replications of effects, preregistration to avoid HARKing (hypothesizing after the results are known; Kerr, [1998](#)), alternatives for NHST as a method for making inferences (confidence intervals, the Bayes factor), and improving the psychometric qualities of measurements.

The recent interest in theory construction, by contrast, has led to proposals based on abduction and modeling (e.g., Borsboom et al., [2021](#); Haslbeck et al., [2022](#), van Rooij & Baggio, [2021](#)). We will use the acronym A&M for the approach and first explain the abduction component. In the view of Peirce ([1931–1958](#)), “abduction is the process of forming explanatory hypotheses. It is the only logical operation which introduces any new idea” (Collected Papers, 5.172). Abduction is reasoning from observations to explanation. In particular, abduction is an inference based on findings (bottom-up, similar to induction) and prior knowledge on the domain of the findings (top-down, similar to deductive predictions). The notion of abduction was historically meant for generation, whereas it was later also used for appraisal (Douven, [2021](#)) and in an explicit way in the view of Haig

([2005](#)). The term “inference to the best explanation,” as used by Harman ([1965](#)), suggests that abduction also helps in theory appraisal. In Rozeboom’s ([1997](#)) view on abduction, the distinction between generation and appraisal is unimportant. He used the term explanatory induction (EI) for his approach and presents abduction as a better way than the H-D approach to link theory (and psychological constructs: attributes and events) to observations. The H-D link is a top-down link (through a top-down prediction), whereas abduction is primarily a bottom-up link (from observations to explanations), as indicated by the term “induction” in EI, although EI requires the availability of prior knowledge and is therefore interactive between bottom-up and top-down. Abduction is also not unlike explanation as a notion in qualitative methodology (Osbeck, [2014](#)).

The second component of recent theory construction initiatives is modeling. Rodgers ([2010](#)) describes modeling as a quiet methodological revolution in psychology. Most models are statistical models, such as factor models, multilevel models, mixture models, etc. Formal terms in models have the potential to serve as intermediaries between theory and observations, in that the formal terms in a model (e.g., latent variables) can be meant to represent psychological concepts. At the same time, these formal terms are also linked to observations by making data a function of the model terms (e.g., Haines et al., [2025](#)). In the H-D approach, the link between theory and observations is provided through the independent (IV) and dependent (DV) variables at the basis of test statistics. When modeling is used within an H-D approach, it serves the purpose of measuring the variables for deriving a test statistic (e.g., a parameter estimate) or to evaluate the goodness of fit of a model with a specification that functions as a hypothesis (e.g., a one-factor model structure as a hypothesis). Modeling has primarily an instrumental role in the service of hypothesis testing.

In the A&M approach, the role of modeling is broader and serves the purpose of building good theories. The approach allows for going back and forth between theory and observations. Observations in the form of

data can be used to estimate the model, and hypothetical observations (data) can be generated from the model. In most cases, data are input for models, but data can also be output from models. It is possible to generate (i.e., simulate) data from a model to better see what complex models (and the corresponding theories) imply by comparing the patterns of generated hypothetical data with the results of data analysis and observed statistics that do not require modeling. In machine learning approaches, the generated data function as predictions. Generative models (used to generate hypothetical observations) are also called computational models because they use computer programs to generate (i.e., to compute) data from complex types of models. An early application of generative modeling is Atkinson and Birch's ([1970](#)) dynamics of action model inspired by achievement motivation theory. Especially because of machine learning, generative models are now more familiar. Abduction as explanatory induction can be used to formulate a model that can be estimated and used in turn to simulate data to check whether the produced data patterns are consistent with statistics derived from real data that are supposedly relevant for the theory in question (Haslbeck et al., [2022](#)). A repetition of this process is a way to iteratively build and improve a theory.

Returning to the puzzle metaphor, the initial reactions to the replication crisis emphasize the trustworthiness of the puzzle pieces, whereas the later reactions emphasize forming a meaningful object in an iterative process. In terms of theory building, the iterative process means that explanatory hypotheses are abducted and appraised in the process of forming a meaningful theory. Although the A&M approach offers an important alternative to the H-D method, it can also be used for mini-theories and specific topics. For example, focusing on a specific cognitive task can also be a fixation on pieces of the puzzle instead of also keeping a perspective on the more global picture.

For the H-D approach as well as for the A&M approach, theory is very important. That is, it is the subject of appraisal in the H-D approach and it is the objective in the A&M approach. The three desiderata that are briefly summarized in the introduction and

discussed in the next section are desiderata for theorizing (and corresponding modeling), with possible consequences for how a scientific methodology is used (H-D, A&M, and possibly others). Different methodologies may work, and there does not seem to be a consensus among philosophers of science on the best scientific method (Hepburn & Andersen, [2021](#)). Therefore, our desiderata can be considered as addenda to one's preferred methodology.

Three Desiderata for Dealing with the Problem of Complexity

Desideratum 1: Paradigmatic and Theoretical Pluralism

Commonly, paradigms and theories are pitted against each other based on the implicit or explicit belief that there is a single best paradigm and that only one theory can be true. Beauchaine ([2024](#)) calls this zero-sum science. If one paradigm or theory becomes more popular, it is at the cost of the other paradigms and theories. Beauchaine ([2024](#)) argues that the complexity of human behavior is a reason for openness or even a commitment to pluralism in the sense of using and combining more than one paradigm or theory. For the domain of developmental psychopathology, he offers two rationales for pluralism.

The first reason for pluralism is that it combines the findings regarding phenomena and mechanisms from (a) different source levels (genetic, neural, environmental, community-related) and (b) different levels of nesting (e.g., occasions nested within individuals, individuals nested within schools, etc.). For source levels, we use the term *multiscale* as used in the material and biological sciences (Horstemeyer, [2009](#); Qu et al., [2011](#)). Processes from lower scales (genetics, neurophysiological) trickle up and interact with processes from higher scales (social, cultural). For levels of nesting, we use the term *hierarchical*. Multilevel modeling for nested data is now a popular method in psychology, but multiscale modeling is not.

The second reason for pluralism refers to the common finding that a theory does not always apply across studies and appearances of the

phenomena under consideration, leaving room for other theories. For example, Wakschlag et al. ([2017](#)) point out that Patterson et al.'s ([1989](#)) coercion theory, in which disruptive behavior of children can be explained through negative reinforcement patterns, might need modification in terms of early neurodevelopmental pathways.

Epistemologically, the finding that different theories might apply at the same time can be understood from Mäki's ([2005](#)) local realism, which allows for different degrees of probability for a theory to be true. In terms of modeling, Bayesian model-averaging (e.g., Hinne et al., [2020](#)) allows for pluralism with model probabilities depending on prior probabilities of a set of models and on the data, possibly from multiple studies.

Desideratum 2: Generalization and Random Components

For a theory to be trustworthy, it needs to be supported by empirical findings. If the findings do not generalize well across studies with different samples, designs, operational definitions (e.g., specifications of the same IV and DV), the theory has at best a limited scope. The analogue for qualitative approaches is the exclusion of sufficiently different contexts or aspects of the same phenomenon. Although qualitative methods may help to find a proper level of abstraction to generalize and to assist the abduction process in building a theory, here we take a quantitative methods perspective.

Meta-analysis is an approach to investigate the generalizability across studies. Mixed models for meta-analysis allow for relevant findings (commonly called effects) to vary. The variation is called heterogeneity and is conceptualized as a random component across studies. We believe that heterogeneity is omnipresent and that the use of random components is a useful approach, which generalizes beyond meta-analysis and beyond participants as in factor models and item response theory models. All too often generalization is interpreted as generalization across samples and groups of individuals, whereas items of a test are assumed to have fixed contributions to the item responses. Instead, random item effects allow for generalization across items (De

Boeck, [2008](#)). In a similar way, randomness across cognitive tasks and effects that show in the tasks (e.g., choice tasks) can reveal the “contours” of generalization, for example, of the framing effect, how it does and does not apply in different ways, and how it can lead to a better integration of such effects across choice tasks (De Boeck et al., [2022](#)). Instead of meta-analysis of multiple studies, a simpler method is a meta-study with random components based on mini-studies and multiple ways to implement a mini-study (DeKay et al., [2022](#)).

The behavior reality can, of course, not be explained in its full complexity. Investigating the limits of generalization and the use of random components can be considered a compromise between the full behavioral complexity and oversimplifications (e.g., assuming independence of error terms vis-à-vis everything else and normal distributions). Random model components allow us to differentiate effects and structures of interest from background variations that are treated as noise. We believe that the balance between simplicity and complexity is (far) too tilted toward simplicity to be a reasonable representation for the (real extent of) complexity of human behavior. Such oversimplification has led to three undesirable consequences: (a) “biased” results, (b) underestimated uncertainty for statistical inference, and (c) insufficient separation of the effects and structures of interest from the complexities that are less relevant for the purpose of research. We will give two examples.

The first example is from Haines et al. ([2025](#)) concerning the Stroop effect (a difference in response time to congruent and incongruent stimuli), which highlights the discrepancy between a theory-driven data analysis based on model specifications of underlying processes (called a generative and conceptual model by the authors) and a descriptive model (called a traditional, behavior model by the authors). The generative model has random components for individuals per condition and experimental session, with an individual mean and standard deviation, which is estimated with a Bayesian approach that results in a simultaneous estimation of parameters at both levels.

Different assumptions are also used in the study to model the random components of response time (normal, lognormal, shifted lognormal). The lognormal accommodates a positive relation between mean and dispersion and the shifted lognormal version takes the assumed variation in non-decision time into account. The authors show that their modeling combined with a Bayesian approach led to a higher reliability of individual Stroop effect parameters and thus a better separation of figure and background (measurement error with possibly different assumptions than the common simple assumptions). As far as the lognormal distribution and non-decision time matter for validity of the effect estimates, taking these background components into account will also reduce potential bias.

The second example is from De Boeck et al. ([2024](#)) and concerns a higher-order random component, called adventitious error (Wu & Browne, [2015](#)), which is a random component that allows for the covariance matrix of observed variables to be random beyond sampling variation. The consequences of ignoring this higher-order random component are overestimated certainty of relations between variables, overestimated power, and biased estimates when the estimates are based on single studies. Adventitious error can also explain why all models are “wrong” and have only approximate, not perfect, goodness of fit (Wu & Browne, [2015](#)).

Random model components can be used for many purposes. For example, adding random components to a model is a tool for sensitivity analysis to investigate the robustness of hypothetical phenomena and their variation. Another example is that the model itself can be considered a nominal random variable in case different models (and thus theories) would apply.

Desideratum 3: The Theoretical and Practical Relevance of Applications

Although it is perhaps too extreme to state that nothing is as theoretical as a good and realistic practical application (reversing the famous aphorism that nothing is as practical as a good theory), we believe that

applications cannot be ignored. The main reason is that the circumstances in which a theory is applied matter, as theories can have poor ecological validity. Given the complexity of human behavior, theories might need iterative modifications, which simultaneously lead to better theory and to a higher practical value of theories.

In many cases, the implementation of principles derived from a scientific theory to prevent, process, or remedy problems requires additional research for the principles to work in practice. The additional research is the topic of implementation science. Nilsen ([2015](#)) in his overview of implementation science cites the context of the implementation of a theory as the primary reason why additional research is needed to support the implementation of theoretical principles. The problem is that direct translation of theory to practice is not straightforward.

Consider an example of implementation science in the broad domain of health, which is the recent trend toward multiple-behavior interventions focused on different health promoting behaviors instead of single-behavior interventions (Zhang et al., [2024](#)). In an extremely large meta-analysis, Zhang et al. ([2024](#)) surprisingly found that a sheer number of multiple-behavior recommendations (e.g., exercise, reducing fat intake) show a linear relation with the behavioral and clinical change across eight different domains, independent of the recommendations. The result implies carry-over effects (i.e., dependencies) between domains.

Summary

There are likely different opinions about what makes psychological theories and methodologies for constructing and appraising theories good, which is not surprising in the presence of the problem of complexity. We believe that there is room for pluralism and collaboration across all approaches, and that generalization and application perspectives are beneficial.

Returning to the puzzle metaphor, a major challenge is to disregard varying and irrelevant specifics while avoiding overly simplifying assumptions that may be misleading and distorting. In reality, one can easily be distracted by specifics. The optimal level of abstraction may be problem specific and depends on the kind and level of a theory. For a quantitative approach, the use of random components in a model can help to separate core from secondary aspects of variability. Falling leaves in autumn may have capricious paths and may not all end up on the ground. The specific paths may not be easy to discern and understand, but all leaves are subject to gravity. Using random components as well as qualitative method-based reasoning may come to such a conclusion. In case a separation between levels of abstraction is undesirable, multiscale methods can be used to connect the level of specifics with the more abstract level. In addition, instead of following cognitive ideals of simplicity and certainty, focusing on fixed paradigms and methodologies, we forward an openness to composites of paradigms and theories. Finally, we believe that applications are important and inspirational.

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Footnotes

¹ An organizing framework and set of assumptions (Beauchaine, [2024](#)).

8. Theory and Measurement: Why Representation Is Not Enough

Matthias Borgstede¹ 

(¹) University of Bamberg, Bamberg, Germany

 **Matthias Borgstede**

Email: matthias.borgstede@uni-bamberg.de

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Introduction

There is an intricate connection between theory and measurement. On the one hand, theory development may build on empirically derived relations between different measurements. On the other hand, the construction of measurement devices may itself be theory-driven. As science progresses from common sense concepts and lay theories to more sophisticated and empirically adequate descriptions of the world, theoretical concepts and measurement procedures co-evolve, such that neither can be understood independent of the other. Such a reciprocal coordination between theory and measurement has been argued to be essential to scientific concept formation and the development of theoretically meaningful measurement procedures (Borgstede & Eggert, [2023a](#), [2023b](#)). For example, in the case of temperature measurement, the development of more sophisticated measurement devices paved the way for the discovery of empirical relations between

temperature and other physical quantities (such as volume or pressure). These empirical discoveries, in turn, contributed to the development of a coherent theoretical embedding of the concept of temperature and ultimately led to the modern physical understanding of temperature as a molar phenomenon that emerges from the microscopic movement of particles (Chang, [2007](#)). The example of temperature illustrates how theory and measurement may co-evolve in the course of scientific discovery. However, the connection between measurement and theory building appears less clear when we move the focus from the natural to the social sciences. Particularly in psychology, measurement and theory building persist to pose substantial problems for basic and applied research, starting with a notorious disagreement about what the two terms are supposed to mean, and culminating in numerous issues concerning the meaning of scientific terms and the validity of psychological assessments (cf. Leising & Borgstede, [2019](#); Maraun & Gabriel, [2013](#); Michell, [1997](#), [2000](#); Slaney & Garcia, [2015](#); Slaney & Racine, [2013](#); Uher, [2018](#), [2021](#)). It is therefore difficult to find psychological examples that correspond to the co-evolution of thermodynamics and temperature measurement. One might argue that this lack of correspondence is not primarily a methodological problem but a result of a fundamental difference between physical and psychological phenomena (Slaney, [2023](#)). For example, it may well be that some psychological phenomena (e.g., preferences) simply do not adhere to the ideal of a fixed quantitative structure. In fact, there is considerable empirical evidence against fixed quantitative preferences (such as preference reversal when two choice options are presented in different contexts). Of course, one might object that the ideal of a fixed quantitative structure does not actually hold for physical length, either, since in relativistic space-time, the length of an object changes with its velocity. Nevertheless, even if one acknowledges the possibility of fundamental differences between the physical and the psychological world, it does not follow that the relation between theory and measurement differs between the two disciplines. In other words, if the terms “theory” and “measurement” can be conceptualized such that

they are independent of the peculiarities of the domain of phenomena, one may conclude that the relation between the two concepts should be the same for physical, psychological, or any other science.

Unfortunately, there is no consensus about what counts as a theory in psychology. For example, following Cronbach and Meehl ([1955](#)), a theory essentially is a nomological network that consists of hypotheses about the relations between theoretical constructs. In this view, for example, the hypothesis that there are five general factors of personality that underlie the results of different personality scales would count as a psychological theory. However, if theories are construed as formal models that provide a mechanistic description of observable phenomena (cf. Povich, [forthcoming](#)), the five-factor hypothesis would not count as a theory, since it does not say anything about what general factors are and how they might generate the observed results in a personality test. The notion of measurement in psychology is equally ambiguous. For example, Stevens ([1946](#)) defines measurement as the rule-based assignment of numerals (i.e., words or symbols that are borrowed from mathematical language) to objects. Consequently, the averaging of numeric responses in a personality test would qualify as an instance of measurement. On the other hand, according to Krantz et al. ([1971](#)), measurement is the construction of numerical representations that preserve the relational structure of empirical observations. Consequently, a personality test that builds on the averaging of numeric responses would not qualify as measurement, since the averaging operation presumes quantitative item responses and can thus be performed irrespective of any underlying empirical relational structure.

The aim of this chapter is to outline a consistent conceptual framework that unifies the notions of theory and measurement at a very general level, such that they equally apply to the natural sciences and the social sciences. Given such a conceptual framework, psychology may in fact learn a lot from the history of other scientific disciplines, such as physics, without committing to reductionism or physicalism.

Throughout this chapter, I will approach the concepts “theory” and “measurement” from a *relational* perspective, thereby revealing the exact nature of their connection, which gives rise to some important practical implications for psychological research. The main result of the analysis is that measurement without theory may be technically possible, but semantically vacuous, and that the key to giving meaning to measurements in terms of scientific concepts lies in integrating measurement and substantive theory by means of formal modeling. So understood, theory building, as opposed to statistical modeling of data, involves the formalization and explication of scientific concepts and their semantic relations such that their application to empirical observations naturally leads to theoretically meaningful measurement.

In the following sections, I will first elaborate on how theory and measurement can both be framed from a relational perspective, namely, “[The Semantic Approach to Scientific Theories](#)” section and “[The Representational Theory of Measurement](#)” section. I proceed with an integration of the two approaches to what Borgstede and Eggert (2023b) have coined “[Theory-Based Measurement](#)” section. Finally, I will sketch how theory-based measurement may solve some important issues in the context of psychological measurement and theory building.

The Semantic Approach to Scientific Theories

The relational perspective adopted in this chapter naturally leads to a *semantic* understanding of scientific theories. The semantic view originated as an attempt to overcome the many shortcomings of logical empiricism, which used to conceptualize scientific theories as collections of logical statements about the world (cf. Balzer et al., [1987](#); Suppe, [1989](#)). The idea behind logical positivism was that scientific theories should be stated in a formal language (e.g., first-order logic) such that they describe the empirical phenomena at hand by means of a minimal set of axioms. Following the positivist view, empirical tests of a theory require to link the abstract terms of the theory to concrete

observational terms by means of correspondence rules that state the exact empirical conditions under which a theoretical term applies. In other words, the relation between theory and measurement was thought to be specified by the operational definitions of the theoretical terms. Although the positivist view of scientific theories has become less popular in the natural sciences during the last decades, it appears to be still very common in psychology. Most prominently, it is a standard protocol to fix the meaning of theoretical terms in psychology by means of operational definitions. For example, when a personality psychologist claims that urban populations have a higher degree of “neuroticism” than suburban populations, it is essential to state the specific personality test that was used in the cited study—otherwise the theoretical term “neuroticism” would remain ambiguous. The rationale behind this practice is the idea that theoretical terms need to be defined by means of empirical operations (i.e., by correspondence rules). The semantic perspective, on the other hand, shifts the focus away from the syntactic form of axiom systems and emphasizes the class of empirical structures¹ they are meant to describe, instead (Suppe, [1989](#)). Following the terminology of mathematical logic, the class of structures described by an axiom system is called *semantics*. Semantics, understood in this sense, are usually given in terms of mathematical set-theory. For example, the semantics of the logical operator *AND* is given by the intersection of two sets. Logical predicates (such as *KNOWS*, *LIKES*, *PREFERS*...) are captured by mathematical relations between the sets of objects to which they apply. For example, the logical predicate *KNOWS* semantically corresponds to the set of all ordered pairs of individuals who know each other. In abstract logic, syntax and semantics are both mathematical objects—and the aim of mathematical logic is to prove correspondences between these two domains (i.e., soundness and completeness). In the case of scientific theories, soundness and completeness guarantee that a theory may equally well be specified syntactically (i.e., by its axiom system) or semantically (i.e., by the class of relational structures it is meant to describe).

Following the semantic view, scientific theories are identified by the class of relational structures they describe (as opposed to specific sets of axioms). Being formal descriptions of classes of structures, scientific theories may or may not correspond to real empirical structures. If an empirical relational structure turns out to be a member of the class of structures captured by the theory, we may say that the theory is *empirically adequate* with respect to this particular empirical relational structure. An empirically adequate theory can be said to *represent* the empirical relational structures that fall into the class of structures fixed by the theory. Such a representation usually requires a *model* that maps the abstract theoretical structure to a specific empirical structure. For example, the theory of thermodynamics describes a very general class of relational structures by means of a specialized scientific vocabulary (such as “entropy,” “energy,” or “pressure”). The correct usage of this vocabulary is fixed by the fundamental laws of the theory (e.g., the first law of thermodynamics, which demands the conservation of internal energy in closed systems), thereby restricting the class of empirical structures for which the theory can be empirically adequate.² It is important to note that the fundamental laws of a theory are not themselves empirical statements. They are formal restrictions of the class of structures that the theory aims to represent. In other words, the fundamental laws of a theory tell us *what the theory is about*. Any empirical application requires a dedicated model that allows for the application of the fundamental laws to specific empirical systems. In the case of thermodynamics, one such application might be the behavior of a steam engine. Naturally, a steam engine can only be described in thermodynamic terms if there is a corresponding model that accounts for the mechanical constraints of the engine, as well as for any deviations from the theoretical idealizations expressed in the fundamental laws of the theory. Given such a model, the theory of thermodynamics represents the physical structure of the steam engine and may be tested for its empirical adequacy by setting up particular experiments and comparing the observed results with the model predictions.

The semantic view has been used to reconstruct how scientific theories evolve over time, how they guide experimentation, and how they may be revised given contradictory empirical evidence (see, e.g., Thomson-Jones, [2006](#), Thompson, [1983](#), or Heise, [1993](#)). There were even applications of the framework to psychology, resulting in some ambitious attempts to reconstruct existing psychological theories (Westermann, [1987](#); Westmeyer, [1989](#), [1992](#)). For example, Kraiker ([1977](#)) proposed a semantic reconstruction of Skinner's theory of operant behavior. Kraiker introduced the law of effect as the fundamental principle of operant behavior, which basically asserts that the probability of a certain behavior, given a certain context, increases if the behavior is followed by a reinforcing event. Like in the physical example of thermodynamics, the law of effect does not say much about the world per se, but is merely a statement of how the constituting theoretical terms of behavior analysis are related to one another. Correspondingly, any empirical application of the theory requires a specific model that links the fundamental principle (i.e., the law of effect) to actual empirical systems (e.g., a pigeon that is exposed to a concurrent variable interval schedule in a two-choice task). As shown in Borgstede and Luque ([2021](#)), such model-based applications may in fact explain various patterns of operant matching that have been observed in behavioral experiments.

However, the semantic view did not have an enduring influence on psychological theory development. This was certainly partly caused by the high level of abstract formalism employed in the set-theoretic reconstructions of different psychological theories, which made it inaccessible to mathematically untrained psychologists. Also, the high amount of mathematical skill required to understand these reconstructions may appear unnecessarily complicated in light of the results produced. Moreover, the idea that psychological theories might be reconstructed using a formal language might have provoked objections regarding the nature of psychological phenomena. As noted above, despite the high impact of quantitative psychology, it is far from self-evident that a mathematical formalization can actually capture

psychological phenomena (which may, in part, be intrinsically qualitative). Such reservations, however, fail to acknowledge that “formal” does not necessarily imply “quantitative.” All that is meant by “formal” is that it is possible to give an unambiguous mathematical description of the structure that underlies the phenomena under study. Such a mathematical description *may* use numbers (if the structure is quantitative), but does not necessarily do so. In other words, there is much more to mathematics than numbers, including the study of highly complex, qualitative structures (cf. Rudolph, [2013](#)). Therefore, even if it might indeed be impossible to provide quantitative laws for certain fields of psychology, a semantic approach to psychological theories could still be fruitful.

Moreover, despite the limited success of the semantic view as a practical tool for theory building in psychology, it may nonetheless be valuable as a descriptive account of how empirical science has proceeded in the past. In other words, the semantic view does not require us to actually specify an explicit formalization of the theory core in terms of abstract set-theory³—it merely asserts that the core of a theory is equivalent to a class of relational structures that aim to represent certain empirical relational structures. The semantic view thus clarifies what we mean by a theory and how we can apply theories to empirical systems by means of formal modeling.

The Representational Theory of Measurement

The representational theory of measurement provides a formal reconstruction of measurement in terms of abstract set-theory (Krantz et al., [1971](#)). One of its greatest strengths is that it equally applies to physical and non-physical measurement, including measurement in psychology. Representational measurement builds on the idea that measurement can be understood as a mapping of objects from an empirical relational structure (e.g., the tipping of a beam balance or the behavior of an individual in a forced-choice task) to a numerical relational structure (e.g., cardinal numbers, real numbers, or metric

spaces), such that the mapping preserves the relations in the empirical structure (e.g., “better than”) by finding suitable relations in the numerical structure (e.g., $>$). Such a mapping (a so-called *homomorphism*) guarantees that the empirical structure is *represented* by the numerical structure (but not necessarily vice versa). The essential part of representational measurement is that the underlying relations are not themselves numerical to begin with. For example, although empirically derived, order relations that build on numeric test responses in a psychological test (such as a rating scale) do not constitute an empirical relational structure in the sense of representational measurement since they are themselves numeric—which would render the representation to a numerical relational structure trivial. Furthermore, it is important to stress that—in opposition to the positivist view that still dominates many psychological studies—representational measurement is *not* defined by operational rules for the assignment of numbers to objects. Instead, the mapping that is required for representational measurement is defined as an abstract mathematical concept that guarantees the structural equivalence between empirical phenomena and their formal representations. If the representation is successful, the resulting numerical objects (i.e., the scale values) correspond to different levels of an attribute. The representational theory of measurement deals with various types of empirical and numerical systems, including the well-known scale types that have been introduced by Stevens ([1946](#)), as well as more complex representations, such as n-dimensional force vectors or the construction of color spaces (Luce et al., [1990](#)).

The representational theory of measurement bears some striking similarities with the semantic account of scientific theories. Like in the semantic view, empirical science is construed in relational terms. Both approaches use abstract set-theory to formalize the relational structures that make up the theory—or the measurement model, respectively. In either case, the aim of empirical enquiry is to identify correspondences between the abstract relational structures and certain empirical relational structures such that the empirical structure may be

represented formally. In the case of representational measurement, the formal representations are limited to numerical systems (i.e., measurement requires numbers), whereas the semantic account of scientific theories makes no such restrictions. In other words, representational measurement—by definition—makes use of numbers; a scientific theory, on the other hand, *may* use numbers to represent certain aspects of the world, but *need not* necessarily do so. Consequently, although the term “representational measurement” is reserved exclusively for numerical scale values, the semantic view does not exclude non-numerical, or “qualitative,” representations (cf. Borgstede & Rau, [2023](#); Borgstede & Scholz, [2021](#)).⁴

One could argue that representational measurement is just an application of the semantic approach to a rather restrictive class of theories with a very narrow scope. However, representational measurement is more than that, since the results obtained from well-designed measurement devices can reveal hitherto unknown empirical regularities that may inspire the inductive formation of more general theoretical laws. Representational measurement enables researchers to make their assumptions about quantitative structures explicit and, thus, testable. If an empirical relational structure can indeed be represented by a quantitative scale, representational measurement provides the formal tools to justify specific number-generating procedures as measurement instruments. Thus, opposite to the common *modus operandi* in psychology, the quantitative nature of psychological attributes is not taken for granted but treated as an empirically testable hypothesis (Michell, [1999](#)). Measurement is thus not just the assignment of numbers to objects (cf. Stevens, [1946](#)), but the mapping of an empirical relational structure to a numerical relational structure. Such a mapping may or may not exist in a certain case, thereby determining whether measurement is possible or not.

However, the representational theory of measurement says nothing about *why* a certain structure is (or is not) quantitative. Neither does it specify the generative processes behind such structures. In other words, given a representational measurement model that is empirically

adequate, we may conclude that we measure something—but we do not know *what* we measure. For example, the empirical structure given by the comparison of volume changes in a gas thermometer can indeed be represented quantitatively by a ratio scale. The representational theory of measurement provides the tools to explicate and test the relational structure of volume changes in a gas thermometer (i.e., by checking for order, continuity, and additivity). However, such testing for quantitative structure and constructing a ratio-scale representation does not consider anything outside this very specific relational structure. Therefore, although we can rigorously test the ratio-scale hypothesis (or any other scale type), we cannot justify any claims that the numerical representations of volume changes in gas thermometers measure “temperature.” If we want the results of such measurement procedures to have a theoretical meaning over and above the fact that the constructed scale has a quantitative structure, we have to conclude that representational measurement alone does not suffice. Theoretically meaningful measurement requires to embed the representational theory of measurement in a larger conceptual framework that integrates theory and measurement. In the following section, I propose that the framework of *theory-based measurement* provides such an integration.

Theory-Based Measurement

Theory-based measurement was introduced by Borgstede and Eggert ([2023a](#), [2023b](#)). Building on the semantic view of scientific theories, Borgstede and Eggert propose that measurement should be understood as the *application of a substantive⁵ formal theory to a standardized empirical setting*. For example, temperature measurement by means of a gas thermometer is in fact nothing but the application of thermodynamics to a very specific physical system. On a purely operational level, the thermometer yields numbers that are read from a calibrated scale such that they are proportional to the change in volume of the gas that is inside the thermometer once the thermometer is in

contact with a probe of a certain temperature of interest. Changes in gas volume may be scaled according to the representational theory of measurement in a very straightforward way (i.e., by comparing the observed volumes inside the thermometer for various combinations of probes). However, even though the representational theory of measurement tells us that we are in fact dealing with a quantitative attribute, we would still have no idea what we were measuring unless we understand how the system that produces the observed numbers works. The question is: why does the gas thermometer produce changes in observed volume and how are these related to *temperature*? Note that the representation alone does not imply any semantics for the measured quantity. If the behavior of a thermometer can be represented by a quantitative scale, all we know is that we are measuring *something* and that this something is quantitative. The same problem arises when representational measurement is applied in psychology. Although most psychological studies do not provide explicit information about the empirical relational structures they seek to represent, there is a growing amount of work that systematically tests the structural assumptions behind (potentially) quantitative psychological scales. For example, Luce's model of general psychophysics builds on the hypothesis that perceived intensity forms a unique ratio scale across all perceptual domains (Steingrimsen, [2016](#)). A series of experiments where subjects were instructed to perform various qualitative comparisons between different combinations of physical stimuli (e.g., tones of different pitches) produced convincing evidence that some perceptual domains may be represented by a common intensity scale and that this scale is in fact quantitative and, hence, measurable in the representational sense (Steingrimsen, [2016](#)). Nevertheless, as with the temperature example, these results merely imply that there is *something quantitative* that can be measured across different perceptual domains. However, the global psychophysics model does not tell us what this something might be and why we should expect it to produce quantitative behavioral structures, in the first place.

The proposal of theory-based measurement implies that the results of a measurement procedure become meaningful if and only if there is a substantive formal theory that explains the observed empirical relational structure.⁶ In the case of temperature measurement such a theory luckily exists (but this was not always the case, cf. Chang, [2007](#)). The theory of thermodynamics provides an abstract description of how temperature relates to pressure and volume. More specifically, the *ideal gas law* states that the temperature T of an ideal gas is proportional to the product of pressure P and volume V divided by the number of particles in the system N : $T = PV/Nk$, where k is the Boltzmann constant. Notably, this formal description of the behavior of ideal gases can be directly applied to construct an abstract description of the gas thermometer in thermodynamic terms. Applying the abstract formalism of thermodynamics to the specific empirical system constituted by a gas thermometer, we can further derive that the observed volume of the gas will be greater for one probe than for another if and only if the first probe has a higher temperature than the second. In other words, because the thermometer can be fully described by the theory of thermodynamics and because this theory explains why the gas in the thermometer varies in volume in the first place, we know for sure that the numbers read from the scale of the thermometer must correspond to the temperature of the probe under consideration. The theoretical embedding of the measurement procedure provides meaning to the observed numbers. Without the abstract laws of thermodynamics, we would not even know what “temperature” is. The theory of thermodynamics tells us that the quantity measured by a thermometer is exactly T , and that T is related to the other abstract concepts of thermodynamics by the theoretical laws of the theory. Following the semantic view of scientific theories, these laws provide the semantics of abstract terms like temperature. Therefore, meaningful measurement is only possible in light of substantive formal theory.

Theory-based measurement may seem counterintuitive at first. Especially for scientists who are used to a positivist understanding of

theory and measurement. One might object, for example, that the quantitative structure of the attributes we seek to measure is not an empirical question at all—because attributes are *defined* as quantities at the level of the theory and, hence, untestable (cf. Kyburg, [1984](#)). A similar argument is given by Franz ([2022](#)) who argues that it is not even possible to state the question of measurability for psychological attributes in an empirically meaningful way since ordinary language does not allow us to talk about the additivity of mental objects. For example, a statement like “my current level of anxiety is exactly the sum of the level of anxiety I had yesterday and the level of anxiety I had the day before yesterday” has no meaningful interpretation in ordinary language (Franz, [2022](#)). In fact, theory-based measurement avoids the problem of meaningless ordinary language interpretations in the context of measurement because it does not rely on such concepts. Instead of using ordinary language concepts, theory-based measurement builds on theoretically defined concepts. In other words, while it may be true that there is no meaningful ordinary language interpretation for the addition of anxieties, it may still be possible that a theory of avoidance behavior includes a quantitative attribute that determines one’s tendency to avoid certain situations, for which additivity has a meaningful interpretation. For example, in a fear conditioning experiment, two discriminative stimuli may be established as cues for two different amounts of electric shock—rendering the question whether the joint presentation of stimuli leads to avoidance behavior that is consistent with the additivity condition an empirical question, indeed (cf. Krantz et al., [1971](#)). Still, it may be difficult (or even impossible in some domains) to find empirical relations for which the conditions of measurability (e.g., additivity) may be tested. However, this does not imply that such experiments are impossible in principle.

What does this mean for measurement in psychology? If we take the few cases where representational measurement has been successfully applied to human behavior (e.g., the intensity of perceived loudness or brightness), we must admit that substantive formal theory is sparse in

these domains and, consequently, theory-based measurement remains an ideal, rather than an actual practice. However, it might well be possible that future work will integrate the concept of perception intensity with a more general theory of behavior, such as the theory of operant selection, which identifies quantitative percepts with statistical predictors of expected changes in evolutionary fitness (cf. Borgstede, [2021](#), [2022](#); Borgstede & Eggert, [2021](#), [2023a](#)). Future work will show whether these (or other) theory-based approaches to measurement will succeed.

Discussion

In this chapter, I have argued that theory and measurement are intrinsically linked as they both are relational concepts. Whereas measurement may be formally characterized in representational terms, the meaning of the measurands necessarily builds on substantive formal theory. Therefore, I proposed theory-based measurement as an integrative account of measurement and theory building. Theory-based measurement states that measurement is the application of a substantive formal theory to an empirical system by means of an abstract description of the system in terms of the theory. If an empirical procedure measures something and if the procedure is adequately described by the theory, numbers generated by a measurement procedure acquire a theoretical meaning.

The physical examples used in the above sections were chosen primarily for didactical reasons—and one might object that what holds for physics does not necessarily apply to psychology. In fact, it might well be that the phenomena dealt with in psychology are completely different from the phenomena observed in physics. For example, one might argue that psychology deals with genuinely qualitative phenomena that cannot be quantified in a theoretically meaningful way—a position that Michell ([1997](#)) calls the *quantity objection*. Although mainstream psychology has established a *modus operandi* that routinely involves number-generating procedures (such as rating scales

or psychometric tests), the quantity objection does raise a valid empirical question regarding the structure of psychological attributes. In other words, it is certainly possible that the assumptions commonly made in psychometric assessments (e.g., regarding the quantitative structure of attributes or the adequacy of specific statistical modeling frameworks) are wrong and that psychometricians adhere to various conceptual confusions and logical fallacies, as some have argued (Maraun & Gabriel, [2013](#); Michell, [2000](#), [2009](#); Uher, [2021](#)).

However, even if the quantity objection were true, the concept of theory-based measurement would still be applicable in that it would allow us to identify exactly which psychological attributes are measurable and which are not. In fact, the semantic view does not even require theories to be *quantitative*. It merely asserts that theories are semantic constructions that refer to relational structures. The constituents of these structures may be quantitative or qualitative, internal or external, subjective or objective. Theory-based measurement takes this idea even further and states that meaningful measurement can never be achieved independent of substantive formal theory. To measure something means to represent its structure numerically. However, representation alone is not enough for *theoretically meaningful* measurement. Instead, any kind of measurement needs to be embedded in substantive formal theory, otherwise we would not know what it is that we are representing.

One important implication of the proposed view is that theory building and theory testing in psychology require more than fitting statistical models to questionnaire data. The key question in psychometrics is not *how* to measure something, but to justify *what* is being measured. Contrary to many existing approaches (most prominently, construct validation theory, cf. Cronbach & Meehl, [1955](#)), theory-based measurement asserts that measurement and theory are not independent, but that they are intrinsically connected (cf. Suppe, [1972](#)). In other words, there is no (empirical) quantitative theory without measurement, and there is no (meaningful) measurement without theory. Theory-based measurement emphasizes the reciprocal

coordination of empirical and abstract relational structures. Thereby it provides the necessary analytical tools to potentially bridge the gap between supposed psychological attributes and numeric test-scores.

At the current state, it is difficult to say whether there are any truly theory-based measurements in psychology. There are certainly instances of representable empirical relational structures, such as psychophysical intensities via cross-modal productions (Steingrimsen, [2016](#)) or subjective color spaces via subjective color matching (Krantz, [1975a](#), [1975b](#)). However, for none of the existing representations of psychological attributes there is a true theoretical understanding why the corresponding empirical phenomena have the observed structure. In other words, there is no substantive theoretical embedding of the formal representations established by the respective measurement procedures. In many other cases, there is even doubt about the adequacy of the intended representation, for example in the case of direct ratings by means of numeric scales, as they are often applied in questionnaires or assessments of subjective experience. This is not to say that common psychometric procedures necessarily fail to produce measurements. Of course, it is perfectly possible that numeric rating scales actually evoke quantitative comparisons within each individual and adequately represent these (internal and unobserved) empirical relational structures. However, the existence of such representations requires some very strong assumptions about the structure of psychological attributes and the cognitive processes underlying the use of rating scales. In other words, if there are psychological quantities, and if these quantities map onto numeric rating scales by means of mere instruction, psychological studies using such scales may actually produce valuable empirical results that might even inspire substantial theory building. However, if any of these assumptions is wrong, much of psychological research might be less reliable than often thought. Of course, one could still compare individuals on psychometric scales, for example, in the context of educational benchmarking or personnel selection. Such applications might in fact be useful to a certain degree. However, they would not advance the understanding of human

behavior on a scientific level (cf. Buntins et al., [2017](#)). Just like thermometers have been used long before we had a physical understanding of why they behave the way they do, the application of psychometric scales to guide individual decisions and selective processes does not build on substantive formal theory. Without such a foundation in substantive formal theory, current psychological testing procedures cannot qualify as scientific measurement but should rather be interpreted as social practices.

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Footnotes

¹ Due to its focus on structures between objects, rather than on intrinsic properties of objects, the semantic view is sometimes also called structuralist (cf., Sneed, [1971](#)). However, the term “structuralism” is used in various disciplines with completely different connotations (e.g., economy, architecture, or linguistics). Therefore, here I avoid the notion of structuralism and stick to the less ambiguous label “semantic view.” See also Lorenzano ([2013](#)) for a discussion of the terminology.

² Note that a theory that does not restrict the class of structures for which it can be empirically adequate is empirically vacuous—that is, it makes no testable predictions about the world whatsoever.

³ By set-theory, I mean the mathematical discipline that studies abstract relations between certain classes of objects, which are called sets, such as one set being a subset of the other, or the common objects of two sets (i.e., their intersection).

⁴ Since such non-quantitative representations may in fact be of primary interest in psychology, a generalization of the representational measurement approach to non-numerical structures appears to be a promising route. However, since a formal integration of qualitative representations is far from trivial, I adhere to the standard definition of representational measurement throughout

this article. See, however, Borgstede and Eggert ([2023b](#)) for a less restrictive definition of measurement within the semantic view.

[5](#) A substantive theory is a theory that includes statements about the generative processes of the phenomena under study. Substantive *formal* theory is given in a formal language that allows for the construction of computational models. In contrast to most statistical models, the main goal of such computational models is not merely to provide a parsimonious description of observed data, but to formalize the hypothesized processes behind the data generation (cf. Eronen & Romeijn, [2020](#)).

[6](#) Note that the underlying theory need not necessarily be *true* in order to justify theory-based measurement, since there might be more than one formal theory that is compatible with the observed empirical structures (i.e., empirical observations may be theoretically underdetermined). In other words, measurement can only be meaningful with respect to a specific theory—which may, of course, be wrong.

9. Scientism in Contemporary Psychology, and Its Underlying Mythic Order

Carlos Cornejo¹✉ and Pablo Rojas¹
(¹) Escuela de Psicología, Santiago, Chile

✉ Carlos Cornejo
Email: cca@uc.cl

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Introduction

Many psychologists agree on a dismissive attitude toward theory within the discipline (Eronen & Bringmann, [2021](#); Gigerenzer, [2010](#); Valsiner, [1991](#)). While experimental research grows rapidly, supported by technical advancements that allow for increasingly precise measurements of psychological variables, the development of theory does not keep up with this progress. The view that theoretical analysis is often met with disdain or even aversion has persisted since the inception of scientific psychology (Danziger, [1983](#); Dilthey, 1883/[1989](#); Koch, [1992a](#)). Jaan Valsiner has referred to this attitude as “theory-phobic” (Valsiner, [1991](#), p. 481), and Gerd Gigerenzer as a “theory aversion” (Gigerenzer, [2024](#), p. 298).

Historically, the main argument for critiquing the aversion to theory in psychology has pointed toward the inadequacy of natural science’s

methods in capturing the complexity of psychological phenomena (Danziger, [1985](#); Groeben, [1986](#); Teo, [2024](#)). Yet, interestingly enough, researchers specializing in quantitative methods have argued on the importance of theory in science in recent years. Thus, the methodological “replication crisis” has led to a “theory crisis” (Eronen & Bringmann, [2021](#); Eronen et al., [2024](#)), prompting a reappraisal of the role of theory in formulating more precise hypotheses, which in turn allows for the development of more consistent and robust theoretical models (Muthukrishna & Henrich, [2019](#); Oberauer & Lewandowsky, [2019](#); Reber, [2016](#)).

To improve the effectiveness of hypothesis testing is undoubtedly a necessary and important task. However, because this effort focuses on improving quantitative methods in psychology, it may overlook several metatheoretical assumptions that can only be reevaluated from a philosophical perspective. Although theory aversion does indeed hamper experimental research, its roots go beyond the misuse of statistics or the clumsiness in theory building. This chapter inquires the reasons behind psychology’s theory aversion. To achieve this, we will briefly review the implicit theory of science held by psychologists, which has led them to growingly abandon theorizing in favor of an almost exclusive dedication to empirical research.

The chapter is organized as follows. The next section provides a brief account of the implicit philosophy of science that continues to influence psychology to this day. This philosophy allows for a strict separation between theory and empirical data. The third section summarizes developments in philosophy and history of science from the 1960s onward, highlighting the convergence of different traditions on the impossibility of drawing an absolute boundary between theory and empirical research. In the fourth section, we analyze why psychology continues to adhere to a philosophy of science that has been outdated for decades. As psychology understands that compliance with a logical empiricist model is a necessary and sufficient condition to advance knowledge, we argue that much of contemporary empirical psychology displays elements of scientism. In the concluding section,

we propose that one way to overcome scientism in psychology is to study the ubiquitous mythical background of scientific knowledge more seriously.

Psychology's Tacit Philosophy of Science

Whether psychologists are aware of it or not, their reluctance to accept conceptual and theoretical analyses as relevant forms of scientific practice is the corollary of philosophical—indeed, metaphysical—procedures. Modern psychology appeared alongside a massive strive for rigor that extended both throughout philosophy and the emerging social sciences. The most successful and renowned movement embodying this strife is perhaps logical positivism. Adopting the metaphysics devised by Wittgenstein in his *Tractatus Logico-Philosophicus* (1921/[2001](#)), logical positivists embraced the idea that logic is the key to understanding the relation between language and reality. The ontological gap between facts and their linguistic accounts can be bridged by carefully formulating our thoughts as propositions that represent the logical form of the facts that compose the world. As Wittgenstein stated, logic is more than a discipline; it is “the scaffolding of the world” (Wittgenstein, 1921/[2001](#), §5.61, §6.124). “Logic pervades the world: the limits of the world are also its limits” (Wittgenstein, 1921/[2001](#), §5.61); and “Logic is transcendental” (Wittgenstein, 1921/[2001](#), §6.13).

Logical positivism followed the *Tractatus* by committing to two blocks of tenets for doing science: *logicism* and *physicalism* (Carnap, [1959](#)). Firstly, scientific language should be logically construed. Since reality behaves logically, scientific language must respect such logic in order to accurately describe what happens “out there” in the world. Ultimately, an accurate understanding of reality requires a clarification—that is, a logical formulation—of our language. Theoretical language is tolerable provided its terms guarantee an unequivocal translation into some form of observational claim. It is then assumed that a methodically constructed language must reflect the logical composition

of reality. Therefore, one has to expect a clear abstractive reason from observational sentences, through singular terms, to abstract affirmations. A disambiguated language would assure the upwards—or inductive—direction of increasing abstraction as well as a downward, deductive direction, that is, from theory to observation. Secondly, concerning physicalism, logical positivism states that every scientific formulation should be translatable into an observational language, where absolute clarity exists regarding the truth conditions of propositions. Only such a physicalist language guarantees the reflex reproduction of the logical image of reality. By means of logicism and physicalism, logical positivism reduced the scope of meaningful statements to empirically observable ones (added to those theoretical statements that are translatable into observable ones).

Psychology absorbed this thriving conception of science following the prescription of pouring its mental terms into physical equivalents. Adopting a distorted version of P. W. Bridgman's (1927/[1960](#)) notion of "operational analysis," psychology equated the meaning of a concept with its observable manifestations (see Koch, [1992b](#)). With this "operational definition," psychologists translated complex psychological concepts (e.g., experience, learning, reasoning, speaking) into physical (observational) descriptions. Crucially, psychologists inherited the belief that a behavioral translation exhausts a concept's meaning, hence circumventing the task of accurately defining theoretical concepts that, paradoxically, give life and meaning to such operational manifestations.

Popper's criticism of logical positivism did not undermine psychology's faith in the observational basis for every scientific statement nor its reluctance to use theoretical formulations. Popper (1935/[2002](#)) pointed out the impossibility of verifying a hypothesis by empirical evidence, for singular empirical data can never suffice to uphold a theoretical statement of a general nature. Nonetheless, scientific theories can be falsified by empirical evidence. As a consequence, scientific theories end up being conjectural. What distinguishes scientific knowledge from any other form of knowledge is its permanent proneness to be falsified. These arguments led to the

methodological prescription to actively look for evidence that potentially contradicts a held hypothesis.

It is relevant to notice that Popper's arguments against hypothesis verification do not touch the architecture of logical positivism. He proposed to reverse the inductivist method by a hypothetico-deductive one.¹ He also replaced the confirmatory procedure—the search for evidence favoring an initial hypothesis—with falsifiability, namely, the supererogatory imperative of actively searching for evidence that disconfirms a held hypothesis. However, for Popper, scientific language still corresponds to a logically consistent system, where theoretical statements are bound to observational statements by logical connectives. Thus, despite its differences, Popper's model perpetuates a metaphysical separation between theory and evidence.

In psychology, Popper's philosophy was assimilated only at a methodological level. Psychologists omitted his arguments against verification but adopted his proposal of falsifiability as part of the methodological prescriptions of doing science correctly. Thus, Popper's scheme of "falsification instead of verification" was replaced by "verification + falsification." While dismissing its philosophical background, psychology interpreted falsifiability as a methodological prescription complementary to the logical positivist program. According to this theoretically inconsistent, Carnapian-Popperian hybrid philosophy of science, theory must be tied by logic to the realm of empirical evidence which, in turn, allegedly both verifies *and* falsifies hypotheses. By engaging in this methodological coalescence, psychologists fall under the misleading spell of reaching double rigor.

Bewitched by the Myth of the Given

Despite its logical inconsistencies, the Carnapian-Popperian hybrid conception of science continues to operate implicitly in psychology. From the point of view of the history of science, it is remarkably odd that a long-abandoned philosophy that supports such ontological schism between our language and the natural world is still alive in the

research practices and methodological rituals of the social sciences. Taken as a case study of the social sciences, psychology has adhered to the ontological belief that there is a “world of facts” susceptible to be objectively and neutrally described by a refined observational language—a belief aptly named “the myth of the given” by Wilfrid Sellars ([1956](#)). Not only has psychology forgotten that “the given” is already a product of metaphysical definitions, but has also—paradoxically—committed to the abandonment of any metaphysics as capable to be informative of the world as given.²

Relevant to expose the underlying motions in scientific practice is biologist and physician Ludwick Fleck’s notion of science as composed of diverse *thought styles* (Fleck, 1935/[1979](#)) where scientific theories are both set forth and constrained by the socio-historical and cultural framework of the society in which they are nested. Building from a historiographic analysis on the diagnosis and treatment of syphilis, Fleck concludes that the larger sociocultural setting directly conditions the pertinence of scientific questions and the acceptability of scientific answers. Fleck’s ideas pioneered a fruitful program aiming to a historiographic (Kuhn, [1962](#); Feyerabend, [1970](#)) and sociological (Knorr, [1979](#); Knorr-Cetina, [1981](#); Latour & Woolgar, [1979](#)) examination of the contextual factors at stake in the emergence and development of scientific theories and technological advances (Bijker et al., [2012](#)).

Yet it would be erroneous to think that the critique of the metaphysics of logical-empiricism and the scientific practices that arise from it originates only from social scientists who disagree with “the received view of science” (Suppe, [1974](#)). In 1951, analytical philosopher Willard Van Orman Quine made a fundamental argument against the Kantian distinction between analytical and synthetic propositions—the transcendental ground of theory-evidence schism. While the former’s truth-value does not require knowing the semantic content of their terms (e.g., *John is John*), the latter does (*John is bald*). An *analytic* claim such as *John is John* does not require to empirically know who *John* is to decide whether it is true or not. *Synthetic* claims,

on the other hand, do require empirical evidence for its truth-value assessment. Quine demonstrates that whatever way one uses to define *analyticity* (via definition, substitution, or semantic rules), it will involve taking the notion of synonymy for granted, implying that analyticity depends on a real-world subject who identifies the terms as interchangeable in meaning. Analyticity is hence ultimately bound to the subjectivity which it originally claimed to discard. Quine famously concludes: “That there is such a distinction to be drawn at all is an unempirical dogma of empiricists, a metaphysical article of faith” (Quine, 1951/[1964](#), p. 42). The whole enterprise of logical positivism depends on the separation between a domain of pure language and a domain of facts, that is, between theory and evidence. Quine argued that the analytic-synthetic distinction, far from supporting such distinction, rests on empirical grounds. Indeed, a reduction of theoretical terms to empirical ones is unfounded from the very start.

Later on, Quine ([1960](#)) conceived the *underdetermination thesis*, also called the Duhem-Quine thesis, according to which evidence underdetermines any scientific theory. The same empirical evidence can be read (“translated” in Quine’s words) from two different theoretical perspectives, both utterly consistent with empirical data but logically inconsistent with each other. In other terms, the same behavior can be explained satisfactorily by more than one theoretical language. There is no domain of facts “out there,” awaiting to be described once and for all with a single ideal observational language.

Quine’s critique served as the initial crack that led to the collapse of logical-empirical metaphysics. Among many other post-analytical philosophers, Davidson ([1973](#)), Putnam ([1997](#)), and Rorty ([1979](#)) have expanded their own critiques to the foundationalist aspirations of logical positivism. It seems there is no form to draw a well-defined line between theory and evidence. Any theoretical work inexorably bears empirical consequences. Conversely, whenever discussing evidence, we back the pertinence of our observations on theoretical assumptions—even inadvertently.

In sum, since the mid-twentieth century we observe an important convergence between very dissimilar philosophical traditions that agree on the issue of a foundationalist theory as unsustainable—all which applies to the tacitly agreed-upon perspective in psychology. Whether the philosophical approach is analytical, hermeneutic, or phenomenological, the resulting conclusions are relatively similar. None of these approaches supports the idea of “raw facts” that would function as objective building blocks to edify a logical system of knowledge. There is no relevant evidence without theory, or distinct theory without evidence. In consideration of this almost 100-year-old philosophical development, it is all the more intriguing that psychology continues to tacitly adhere to an utterly outdated philosophy of science. The dissociation between contemporary philosophy of science and the implicit conception of science operating in psychology is not ascribable to sheer ignorance of relatively recent advances in philosophy or to a bent for a particular philosophical stream that aligns with current psychological research practices. We must dig deeper into the discipline’s research and teaching practices to answer the question: Why does psychology still operate with a (tacit) conception of science that has been philosophically superseded decades ago?

The Quest for “Real” Science

The peculiar way in which psychology teaches, applies, and evaluates its methodology has been diagnosed in terms of a “methodolatry” (May, [1969](#)), “method-fetishism” (Koch, [1992a](#)), and a “cult of empiricism” (Toulmin & Leary, [1992](#)).³ Cowles ([2020](#)) argues that the notion of “the scientific method,” as psychology and other social sciences understand it, originated from a mythic reproduction in training new generations of psychologists. In particular, Rudolph ([2005](#)) traces back the origins of the “myth of the scientific method” to John Dewey’s ([1910](#)) *How We Think*, a short textbook for teachers intended to summarize the research about children’s thinking. In chapter six, Dewey formulates five steps of a “complete act of thought”: “(1) a felt difficulty; (2) its

location and definition; (3) suggestion of possible solution; (4) development by reasoning of the bearings of the suggestion; [and] (5) further observation and experiment leading to its acceptance or rejection” (Dewey, [1910](#), p. 72). Rudolph evidenced how Dewey’s five steps for scientific thinking rapidly became a short-hand for exhausting any scientific initiative: such crystallized simplification became *the* scientific method for the outside educator.

As Rudolph points out, the huge success of Dewey’s five-step account was partially due to depicting scientific rationality as a general attitude of ordinary people rather than a highly specialized capacity of scientists. Additionally, Dewey’s five steps “[...] allowed educators to embrace the rhetoric of science and, thus, take advantage of the cultural authority science possessed following the wave of late nineteenth-century popularization” (Rudolph, [2005](#), p. 368). It is relevant to highlight key phrases in this quote, such as “the rhetoric of science” and “cultural authority.” Rudolph implies that the promotion of a single, automated scientific method did not solely obey to epistemic reasons but also to non-epistemic considerations. As with psychology, the search for a reliable method would be tightly related to a desire to be acknowledged as a natural science proper. Modern psychology has developed over a long cultural process whereby it has accepted a mechanistic cosmology, which views the method of physics as the most appropriate way to explain reality as a whole—including the “inner world” of mental life. Remarkable technological advancements that improved citizens’ life quality confirmed the benefits of a scientific worldview. The expansion of scientific thinking—parallel to the decay of religious faith—led to the “late nineteenth-century popularization” of science, which enhanced its “cultural authority.” This historical and cultural background must be accounted for when analyzing the particular way in which psychology adopted the so-called scientific method (Dijksterhuis, [1961](#)).

To nineteenth-century psychology, adopting *the* scientific method meant more than just gaining a means of understanding its subject matter. It crucially represented veering off from philosophy toward an

embrace of the natural sciences. During this time, philosophy began to be perceived as a fuzzy pursuit, gradually overshadowed by more advanced technological forms of knowledge developed by the natural sciences. In Germany, for instance, the dominant idealist philosophy dramatically conceded its hegemony to the positivist tradition (Cornejo & Hernández Maturana, [2025](#)). The successful adoption of mathematical methods in physiology aided the establishment of psychophysics and then of physiological psychology, which, alongside the spread of Darwinism, made the dream of scientific psychology feasible. Not without a sense of pride, in 1894 Wilhelm Windelband declared during his inaugural speech as Rector of the University of Strasbourg that “psychology falls unambiguously within the domain of the natural sciences” (Windelband, 1894/[1980](#), p. 176).⁴

Notice that psychology’s adoption of the scientific method encrypts a worldview conversion. Following the rules of the scientific method may superficially aim at granting a reliable method of knowing. At a deeper level, however, it contains the refrained desire to become a legitimate science. Thus, psychology’s rejection of theory manifests more than the avoidance of an epistemically dubitable procedure: it expresses a fear toward philosophy as it would throw the discipline back into its overcome, “pre-scientific” past. The ritualization of the scientific method, along with the uncritical acceptance of established methods and the automated rejection of ideas outside these methods, suggests that psychology employs the scientific method for extra-scientific purposes. An example is the use of methods as ready-made and the belief that they are infallible because of their nature rather than their adequate use. A case in question is the use of statistics as a fix-all, end-all hypothesis testing tool, particularly in the case of psychologists who often lack a proper background in mathematics to flexibilize its use (see De Boeck et al., [2020](#)). Additionally, psychology’s doctrinal teaching of methods as transcendental truths, in many cases without acknowledging neither their authors nor the historical contexts in which they were created, further reinforces this diagnosis. This brings us to believe that the “scientific method,” as it is employed by

psychology, while seeking to fulfill epistemic functions, also fulfills *mythic* ones. We will come back to this issue in further depth at the end of this chapter.

One finds similarities between the way psychology has implemented the scientific method and the ritualized form of science called *scientism* (Hayek, [1952](#)). The rigid use of this method suggests that, for a significant proportion of contemporary psychology, science is not only a systematic approach to knowledge but an ultimate goal in itself. It follows that much of contemporary psychology may be not scientific, but rather *scientistic*. Susan Haack ([2017](#)) summarizes some characteristic indicators of scientism. According to her, scientism is the pervasive attitude of sanctifying “science,” making people prone to accept whatever finding or statement comes adjectivized as “scientific,” mythologizing “method,” and fortifying the demarcation line between science and the pretenders. Not infrequently, scientism has led to raids against “pseudo-science,” echoing religious persecution of infidels. Another sign of scientism, Haack ([2017](#)) argues, is dressing up unworthy content, which involves using scientific tools and jargon not to promote genuine inquiry but to disguise a lack of proper rigor or conceptual clarity. Haack believes, this feature is particularly prominent in the social sciences:

Dressing up dreck is even commoner, probably, in the social sciences, where lengthy introductory chapters on “methodology” are sometimes only window-dressing, and graphs, tables, and statistics sometimes focus attention on variables that can be measured rather than those that really matter, or represent variables so poorly defined that no reasonable conclusion can be drawn. (Haack, [2017](#), p. 41)

To uphold an anti-dogmatic stance that encourages the permanent critical openness to revise statements and procedures is among the key traits of science. To paraphrase Brandom’s ([1994](#)) insightful description, science is the language game of giving and asking for

reasons. This is the foundation upon which the *fallibility* of science rests. Science is an activity that supposes and prompts the art of questioning. Yet scientism denies the fallibility of science. As used by scientism, the scientific method ceases to be a means to answer questions and becomes an end in itself. There is importance in finding a sound answer, but it is all the more central to address it by using a specific (narrow) understanding of the scientific method. In this framework the careful replication of inherited methods is held higher than reflecting on their aptness to study the issue at hand. Such an attitude ultimately leads to the ritualization of methodological tools, as if this quasi-religious respect would warrant scientific standards, disregarding conceptual clarity in regards to the phenomenon and sought answers.

In this, however, *scientistic* psychology confounds means and ends. Science's commitment should reside in the unveiling of truth, not in the tools used to unveil it. In the case of psychology, the ultimate goal is to describe, explain, and understand human beings in all their complexity—encompassing biological, neurophysiological, behavioral, social, symbolic, and phenomenological dimensions. Whatever methods it should choose, they must support this objective. If, conversely, the methods themselves become revered, the process of knowledge is turned upside down and the actual understanding of the issue at stake gets pushed to the background. Hayek ([1952](#)) characterized scientism as an attitude that is “decidedly unscientific” (Hayek, [1952](#), p. 16): “The scientistic as distinguished from the scientific view is not an unprejudiced but a very prejudiced approach which, before it has considered its subject, claims to know what is the most appropriate way of investigating it” (Hayek, [1952](#), p. 16).

Scientistic psychology's confusion between means and ends has been convergently pointed out by Wendt ([2024](#)) establishing the distinction between *exactitude* and *rigor*. Expanding on the Husserlian notion of *rigor*, Wendt criticizes its alleged synonymy with *exactitude*. While the latter emerges in mathematical discourse as accuracy, the former is rather a “*meta-attitude*, an attitude in which other attitudes

become thematic [...] Thus, rigor allows science to emancipate from its presuppositions, leading to a discourse on its foundations” (Wendt, [2024](#), p. 17, italics in the original). The “exact sciences” are thus called because they rely upon mathematical modeling, and their exactitude comes from mathematics. But the mere use of mathematics cannot be equated to rigor. By applying the (natural) scientific method, psychologists do not necessarily improve the conceptual clarity of their subject matter. Science, argues Wendt ([2024](#)), does not become rigorous simply by introducing mathematical tools. It becomes rigorous when it is fallibilistically prone to provide a rational “foundation for its explanatory principles” (Wendt, [2024](#), p. 17). When providing a rational foundation for science is a priori banned from the game of “giving and asking for reasons,” for example, by rejecting theoretical debates for being philosophical (and, therefore, supposedly unempirical), we encounter a form of scientific bigotry hidden underneath a more or less sophisticated experimental design. Wittgenstein’s words still resound: “For in psychology, there are experimental methods *and conceptual confusion*” (Wittgenstein, 1953/[2009](#), §371, italics in the original).

Further Symptoms of Scientism in Psychology

We have introduced the theory aversion as a feature of psychological scientism. However, there are isolatable symptoms that are worth noting as detrimental.

Assessing Scientific Worth for its Methodology

There is a tendency in psychology to reduce the scientific debate mainly, if not exclusively, to its methodological aspect. Often, the evaluation of article proposals focuses solely on the proposed design or on the instruments used, neglecting the novelty and pertinence of the research question or how reasonable the proposed hypothesis is. One of the current debates focuses on the *replication crisis*. Much work has been published attempting to answer the astonishing finding made by

Ioannidis ([2005](#)), concluding that most empirical studies in either health, the natural or social sciences are difficult or impossible to replicate. Manifestations of this crisis (e.g., retrospective hypotheses formulation, cherry-picking data, p-hacking), as well as the proposed solutions, are mostly methodological (Cumming, [2014](#); Nosek et al., [2015](#); Wagenmakers et al., [2012](#)). A ritualized methodological approach implies a non-reflective use of statistical tools, among others. But aside from a deficient use of statistical proof, the replication crisis usually involves a careless treatment of the theoretical concepts implied in the research question (Wiggins & Christopherson, [2019](#); Schnepf & Groeben, [2024](#)). The deficitary conceptual definition of technical terms composing the theoretical models sustaining these studies has been less explored and should be.

Blindness to Social Factors in Science

As psychology has surrendered its capacity to rationally analyze its own foundations, it has become blind to the inevitable influence of social, political, and cultural factors in its historical development as a discipline. Carnapian-Popperian hybrid epistemology assumes scientific activity is governed by the morals of individual scientists. This view actively ignores the human and social aspects of science. We know certainly that most scientific research takes place in universities which are part of societal orders and pressures that condition scientific work (Callard, [2022](#)). Scientists are workers obliged to publish (or perish), and the more, the better. Maintaining their jobs depends on performance evaluations that consider awarded grants, published papers, and, increasingly, their capacity to transfer ideas into marketable technological products. Science has increasingly become a technological device serving a socioeconomic system obsessed with producing more quantity over quality (Bok, [2003](#); Noble, [2003](#)). Surprisingly, these social conditions are overlooked when analyzing the development of scientific theories as if scientific activity were not executed by real people, but by epistemic agents detached from society and culture.

An additional symptom of the blindness to the impact of social factors on scientific practice is psychology's inability to critically disengage from fashionable topics and technologies. Modern psychology tends to overestimate what the natural sciences have to say about humankind. This mythical faith results in a willfulness to choose oversimplified visions of psychological phenomena, as if all psychological conundrums were to be resolved by the mere introduction of new technologies. Two relevant examples are contemporary *neuromania* and *Darwinitis* (Tallis, [2011](#)). The interest in brain imaging techniques resulted in a substantial growth of the neurophysiological grounds of cognitive research and fostered the rapid spread of cognitive neuroscience over psychology. While offering valuable insights into human cognition, it is hard to overlook that the a-critical reception of cognitive neuroscience contributed to an overemphasis on the brain functioning as if all aspects of human life could be satisfactorily explained as results of brain activity. By ignoring subjective aspects of human experience, such as intentionality, phenomenology, and personal identity, as well as the sociocultural origins of the human mind, psychology suffers from *neuromania*, aspiring to obtain through neuroscience something more than the neurophysiological basis of cognitive processes (Gabriel, [2017](#)).

According to Tallis ([2011](#)), something similar happened to evolutionary theory in the nineties. Evolutionary psychology was born claiming that evolutionary theory can account for every facet of human life, including our deepest moral and subjective experiences. For sure, such a claim goes far beyond the original insights of evolutionary theory in biology. As a revival of late nineteenth-century Social Darwinism (represented by researchers such as Herbert Spencer and Francis Galton), evolutionary psychology expanded biological evolutionary thought to explain gender roles, cultural practices, aggression, cooperation, jealousy, and mating strategies, among others. By doing this, evolutionary psychologists ignore the non-deterministic character of biological predispositions and minimize social and cultural factors in accounting for human behavior and experience.

Fossilization of Theoretical Models

According to psychology's implicit theory of science, the constant procedures of confirmation/falsification assure the growth of knowledge. The more scientists become involved in scientific work, the faster inadequate theories will be displaced by novel ones. However, the exact opposite seems to be the case. Chu and Evans ([2021](#)) conducted a bibliometric study—with over 90 million papers across different areas—concluding that the more articles are published in a given field, the lesser the very small set of best-cited articles unsettles the field or brings novel discoveries. This suggests that an excess of papers published each year in a field can be detrimental to the advancement of science. Chu and Evans ([2021](#)) conclude: “The more-is-better, quantity metric-driven nature of today's scientific enterprise may ironically retard fundamental progress in the largest scientific fields” (Chu and Evans, [2021](#), p. 4). Thus, contrary to the tacit conception of science most psychologists adhere to, the credibility attributed to a canonical core of theories may stem from a confirmatory bias resulting from publication-overproduction: a new article increases its chances of being published if its authors cite the established canon, while the chances of publication decrease if the article introduce novel ideas and/or methods. Thus, the socioeconomically conditioned overproduction machine fossilizes a science instead of instigating its further development.

Performative Self-Contradiction

As we argued in Section “Bewitched by the Myth of the Given”, studying a phenomenon empirically requires the simultaneous introduction of theoretical distinctions. The major scientific contributions of the discipline have emerged from theoretical argumentation and not from empirical studies. Interestingly, this observation applies not only to theories of lesser empirical strength, such as humanism and psychoanalysis, but also to well-established theories of scientific psychology, like behaviorism, the cognitive sciences, and neuroscience. A remarkable example in behaviorism was brought up by John Watson

([1913](#)), who launched his empiricist crusade with a manifesto, not by empirical evidence, published in *Psychological Review*, leveraging his position as editor for the journal. In this exemplary case of performative self-contradiction, one can see how several approaches to scientific psychology have denounced the vices of theory and yet still promoted the advantages of empirical research through successful theoretical trials.

The Psychologist's Fallacy

In 1890, William James warned: “The great snare of the psychologist is the confusion of his own standpoint with that of the mental fact about which he is making his report” (James, [1890](#), p. 196). James cautioned about this risk mainly by considering introspection as a method, for it is straightforward to inadvertently attribute the qualities of one's own perception of an object to others' perception. Strikingly, in contemporary scientific psychology the risk of distortion is heightened because of methodological reasons. The urge to quantify psychological phenomena has progressively led psychology to adapt these phenomena to fit available experimental techniques rather than adjusting the methods to suit the studied object. This approach can distort psychological processes, as critical aspects of psychological life—such as freedom, creativity, autonomy, reflective capacity, affectivity, and spirituality—are often excluded or reduced to operationally measurable constructs. These distortions and dismissals, made to save the method, bear no theoretical justification.

Although this list is not exhaustive, it is sufficiently telling that psychology would continue to strive for an ideal of natural science that has arguably long been surpassed. As a consequence of resigning its capacity to reflect on its foundations, psychology has sacralized and rigidized the use of the methods of the natural sciences, even at the high cost of curtailing psychological phenomena, stalling scientific advance, and ignoring social factors that impact science's development. Psychology appears to have forgotten the inherent fallibility of scientific knowledge, while keeping a rigid notion of “scientific method” that only

embodies the carcass of logical structure in critical thinking. The pursuit of understanding and explaining psychological phenomena seems to have shifted toward a ritualized adherence to a methodological canon that *while rational is not always reasonable*. How can we envision a psychological science that overcomes the uncritical reverence for an idealized scientific method?

Conclusion

Throughout the chapter, we argue that the ideal of fallible rationality is not equivalent to implementing the methods used in the natural sciences. Believing that the rigid application of these methods automatically guarantees fallible rationality is a basic epistemological error in psychology. This misconception carries a constant risk of achieving methodological sophistication while lacking conceptual clarity. When we conceive science as a human endeavor rather than an automated process, scientific activity unavoidably produces an “excess of meaning.” If we fail to reflect on this appropriately, there is a risk that science may inadvertently evolve into a ritual stripped of its original purpose: the old art of giving and asking for reasons.

The application of the scientific method for unexamined purposes results in several symptoms that hinder the further development of psychological science. Among them, we have mentioned the ritualized use of methods of analysis (e.g., Null-hypothesis Significance Test), the doctrinal teaching of methodology, an a priori rejection of theoretical analysis, blindness to the influence of social, political, and economic factors on the development of psychology, and a vulnerability to techno-scientific trends (neuromania, Darwinitis). These issues, among others, contribute to the diagnosis of *scientism* in psychology.

It is not difficult to recognize that the implicit epistemology operating in psychology works on a specific model of the human being. In this anthropology, the scientist is expected to act rationally and dispassionately. Acting rationally means a permanent willingness to question traditional truths and, in Kant’s spirit, to have the courage to

think for oneself. Acting dispassionately means objectively analyzing reality while minimizing the influence of personal desires and biases. Therefore, science should strive to eliminate or at least reduce factors related to the researcher's subjectivity, for these can distort the objective apprehension of reality. Consequently, researchers must position themselves as a subject outside whichever reality is being studied, which then becomes the object of their investigation. This perspective aligns with the notion of *homo rationalis*, which operates as the agent of knowledge within the implicit epistemology of psychology.

Contemporary psychology faces challenges in accepting that the scientific method can fulfill mythical functions due to the belief that science and mythical are inherently contradictory. According to the modern vision of knowledge, science is seen as a remedy for the mythical understanding of earlier times. Science would be the cure for myth. However, this conception of myth rests on the anthropological perspective that assumes the scientist as *homo rationalis*. As long as psychology does not fully dismantle such inherited anthropology, which underpins its tacit epistemology, it will struggle to recognize that its reliance on ritualized replication of methods from the natural sciences responds, at a deeper level, to a basic anthropological need that has been ignored from its inception by the idea of *homo rationalis*.

Crucial to our critique is to understand what we mean by a mythic dimension in scientific practices. In its commonsensical, everyday use, a myth is a wrongly upheld belief that can usually be stated as a clear narrative (i.e., urban myths, Homeric texts, etc.) and bearing a superstitious, religious, fictional, and specifically pejorative character. Yet, the—technical/anthropological—use of the term we invoke here holds little or no relationship with its actual truth-value (Kolakowski, [1989](#)). We are here rather appealing to a form that encapsulates a dispositional, yet orderly worldview or a relation with the cosmos that falls outside the realm of a rational or even factual conception of the human and/or other non-human beings. However, its non-rational character does not overshadow its value, which resists translation into justifiable terms and reveals a dimension of experience where emotion

and thing are manifested in a single unit (Cassirer, [2013](#)). Rather than being upheld by veracity as a core value, myth is ruled by the expressivity of their coalescing figures—as what we now call *personification* and *physiognomic perception* shows. Part of Ernst Cassirer's analyses expose how we tend to think of myth as present in more “primitive” cultural forms—usually considered to be present in the genesis of religion—but not manifest in other kinds of cultural forms, least of all science. But it is precisely its presence throughout cultural manifestations that must be acknowledged so that it can be understood in its own terms instead of being turned into a phantasmatic enemy. Cassirer has shown how myth can be found—sometimes as a vestige—in language, religion, art, and technical activity, to name a few. Would it not be odd for science to be completely devoid of this human dimension?

The received view of science makes us think that mythical thinking is a vestige of primitive forms of knowledge, so that every formally educated person, and a fortiori a scientist, does not exhibit such kind of thinking. Far from being an empirical fact, this belief rests upon an idealized rationality whose existence has been systematically questioned throughout the twentieth century by philosophy, anthropology, and other disciplines in the human and social sciences. If we don't find myths in contemporary science, it is because our vision of myth is too shallow. As modern citizens we tend to think of myth as. Contemporary mythologies are not anymore anthropomorphic or animistic; they are scientific and mechanomorphic. This is precisely why recognizing them is often so arduous: “[I]t is because our contemporary myths are scientific ones that we fail to acknowledge them as being myths at all” (Toulmin, [1982](#), p. 24).

It is as unrealistic to think that scientists act solely rationally, without affect, passion, or interest, as one is to believe that science can erase the mythical dimension that has accompanied humanity since time immemorial. Against the rationalist anthropology of the received view of science, there is no definitive demarcation line between science and myth. Insofar as science is conducted by people, rather by

transcendental agents, it is not surprising that scientific theories inadvertently end up performing functions beyond the epistemic. “[A] scientist’s *methods* may be completely empirical, yet his investigations will have no direction without the guidance of a pre-existing body of ideas, some of which may turn out under scrutiny to be survivals from surprisingly far back” (Toulmin, [1982](#), 25 f.).

Our critique of scientism in psychology ought to be interpreted at two levels. First, it points out a blind spot in scientific psychology, where the insistence on importing methods from the natural sciences has paradoxically hindered scientific progress. Second, our critique serves as an invitation to reflexively examine, both empirically and theoretically, the motives underlying the reverence for an idealized version of natural science.

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Footnotes

[1](#) A structural similarity between Popper’s falsifiability and the widely spread methodology of null hypothesis significance test (NHST) seems to exist, according to which experiments assume a default hypothesis (the “null hypothesis”) wherein there is no difference between groups and its viability is decided by means of a yes-no answer using an arbitrary limit (typically a p value ≤ 0.05). The NHST has been strongly criticized for being misused and misconceived (Gigerenzer et al., [1989](#); Gigerenzer, [2004](#)) and signaled as one of the sources of the contemporary replication crisis in the social sciences (Ioannidis, [2008](#)). More important to our purposes is reminding that the NHST and Popper’s philosophy of science only hold a mere family resemblance (Koch, [1992b](#)). While Popper argued for the need to introduce severe disconfirmatory tests, “the null hypothesis is perhaps the weakest test that could be devised” (Smith, [2018](#), p. 3). Some authors have pointed out that the NHST is a “travesty of Popper’s views on the proper way to conduct science” (Oakes, [1986](#), p. 70). While Popper urged scientists to test their theories explicitly against empirical evidence, the NHST only allows to establish an indirect contrast by indicating which hypothesis does not fit the observed data.

[2](#) Psychology seems to have studiously followed Wittgenstein’s advice in the *Tractatus*: “My propositions serve as elucidations in the following way: anyone who understands me eventually recognizes them as nonsensical, when he has used them—as steps—to climb up beyond them. (He must, so to speak, throw away the ladder after he has climbed up it)” (Wittgenstein, 1921/[2001](#), §6.54).

[3](#) It is worth mentioning that decrying method-fetishism only to quantitative research and attempting to overcome it by introducing qualitative methods is misleading, as the phenomenon transcends the quantitative versus qualitative cleavage. Methodolatry has indeed pervaded qualitative research (Musa et al., [2015](#)). Thus, “[m]uch of our published qualitative research continues to be flavored with implicit positivist (and empiricist) assumptions” (Chamberlain, [2000](#)).

[4](#) It may be controversial to place Windelband as leaning more toward the scientific pole rather than adopting the integrative stance his distinction between nomothetic and idiographic sciences aims to promote (see the thorough work of Gundlach, [2017](#)). However, one of us has argued for the primacy of nomothetic language at the metalinguistic level within Windelband’s nomothetic vs. idiographic distinction (Cornejo, [2012](#)).

10. On Worms, Mirror Neurons, and Explaining Human Behavior

Markus I. Eronen¹✉ and Laura F. Bringmann²✉

(1) Department of Theoretical Philosophy, University of Groningen, Groningen, The Netherlands

(2) Department of Psychometrics and Statistics, University of Groningen, Groningen, The Netherlands

✉ **Markus I. Eronen (Corresponding author)**

Email: m.i.eronen@rug.nl

✉ **Laura F. Bringmann**

Email: l.f.bringmann@rug.nl

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Introduction

A central aim in psychology and neuroscience is finding theories that can successfully explain or predict human behavior. The lack of such theories has recently led to calls for more emphasis on theoretical and conceptual research (e.g., Borsboom et al., [2021](#); Eronen & Bringmann, [2021](#); Guest & Martin, [2021](#); Levenstein et al., [2023](#); van Rooij & Baggio, [2021](#)). However, humans and their behavior are extremely complex, and can be studied at many levels, ranging from molecules to social factors. Thus, a key question for efforts at finding theories to explain

human behavior is the following: Is there a privileged level, or levels, where we have the best prospects of finding successful theories to predict and explain behavior?

In different forms, this question has preoccupied philosophers and scientists for decades, if not even centuries, and there is no consensus in sight (see, e.g., Eronen, [2021](#); Fried, [2022](#); Miller, [2010](#)). However, in practice the dominant view continues to be that the best explanations can potentially be found at the “lower” levels, including neurobiology, genes, and biochemistry. This view, which can be called *explanatory reductionism* (Eronen, [2021](#)), has been explicitly endorsed by many neuroscientists (e.g., Gordon, [2016](#); Insel & Cuthbert, [2015](#)) and some philosophers (e.g., Barwich, [2021](#); Bickle, [2006](#)). It is also clearly reflected in how research funding is allocated: For example, the National Institute of Mental Health, the biggest funder of mental health research in the world, has overwhelmingly favored neuroscience and genetic research over more traditional psychological research in the past decades (Frances, [2020](#)).

In this chapter, we argue against this view and show that there is no reason to privilege lower levels when looking for explanations and theories of human behavior. To make our case stronger, we start from the perspective of lower levels and from a best-case scenario for the reductionist. More specifically, we focus on the attempts at explaining the behavior of *C. elegans* and show that even in this relatively simple biological case, it is extremely hard to find good theories and explanations based on the lower levels. In contrast, context and behavior remain the best sources for powerful predictions and explanations. We then discuss the implications this has for finding neuroscientific theories and explanations for human behavior, using the case of mirror neurons as an example. Finally, we argue that theories that explain or predict human behavior should build on the (folk) psychological and behavioral levels.

Before we start, it is important to emphasize that this chapter is about theories that can (causally) explain or predict human behavior. Although explanation and prediction are central goals in science, a

theory may be successful even if it does not reach these aims, for example, if it provides adequate description or understanding. We briefly return to these other aims at the end of this chapter, but our main arguments do not directly concern them. Another important clarification is that “theory” should be understood very broadly here, including both local and case-specific theories, such as mechanistic models (see, e.g., Bechtel, [2009](#)), and broader theories, such as the theory of thermodynamics. Also, the notion of “levels” can be understood in a very loose and heuristic sense, roughly referring to things that are similar to each other in relevant ways (e.g., “the level of neurons”), and in most cases could also be replaced with “size scales.”

C. elegans

Caenorhabditis elegans is a nematode (roundworm) of roughly 1 mm in length. It has been used as a model organism since the 1960s, starting in the lab of Sydney Brenner, who later received the Nobel Prize for his work on *C. elegans* (Ankeny, [2001](#)). What makes this tiny worm attractive for neuroscience is that it has one of the smallest nervous systems in nature, consisting of only 302 neurons. It is the first organism for which both the genome and the connectome or “wiring diagram” of the nervous system have been completely mapped out (Ankeny, [2001](#)). In addition, the behavior, development, and biochemistry of *C. elegans* have been extensively studied. It is also possible to intervene on specific neurons and track the results of the intervention throughout the nervous system, for example, with optogenetics, a technique with which specific genetically engineered neurons can be activated with light (Leifer et al., [2011](#)).

This combination of relative simplicity and large amount of relevant knowledge makes *C. elegans* a “reductionist’s delight” (Schaffner, [2000](#)) and an ideal starting point for our discussion of the limits and possibilities of explanatory reductionism in neuroscience. Early on, the study of *C. elegans* was accompanied with much optimism, and the hope was to determine how exactly genes and elements in the nervous

system are linked to behavior (Ankeny, [2001](#)). The idea was that explanations for the behavior of the worm could be found based on the connections in the wiring diagram. Many links between behavior and neurobiology have indeed been discovered. Within the nervous system of the worm, several circuits have been identified and linked to behavior (Flavell & Gordus, [2022](#)). For example, among the motor neurons, groups of neurons have been linked to forward motion and backward motion, respectively (Roberts et al., [2016](#)), and models have been proposed for circuits of neurons associated with sleep (Bechtel & Bich, [2023](#); Maluck et al., [2020](#)).

However, in spite of these successes, and the amount of neurobiological knowledge available, understanding of the biological basis of the behavior of *C. elegans* remains limited (see also Krakauer et al., [2017](#); Stiefel & Brooks, [2019](#)). The dynamics of the nervous system and the “cognition” of *C. elegans* have turned out to be immensely more complicated than originally anticipated.

To start with, the anatomical wiring diagram does not give an adequate picture of how the system functions. The same anatomical connections can play different functional roles depending on the context. Whole-brain imaging and other techniques have revealed that the system is not as stable and invariant as suggested by the wiring diagram, but highly dynamic and evolving, and exhibiting different functions in different sensory and motor contexts (Bargmann, [2012](#); Flavell & Gordus, [2022](#)). An important factor giving rise to this context-dependence is *neuromodulation*: The activity of a neuron, and the dynamics of a whole circuit, can be modified by molecules released by other neurons (Bargmann, [2012](#)). For example, the switches between dwelling states (slow movement with frequent reversals) and roaming states (faster movement with less reversals) of the worm are partly regulated by a non-synaptic neuromodulatory circuit (Flavell & Gordus, [2022](#); Roberts et al., [2016](#)). More specifically, due to neuromodulation, the activity of one specific neuron (named AIA) can be correlated with either forward-moving or dwelling behavior, which are basically opposites to each other, depending on whether food odors are present

in the environment (Flavell & Gordus, [2022](#)). These are just a few examples: Such changes in function due to neuromodulation are common in the *C. elegans*, and in vertebrate organisms in general (Bargmann, [2012](#)).

The first point that we want draw from this is that even when studying an organism as tiny as a nematode, we need to take into account multiple levels or scales, as well as contextual factors (Stiefel & Brooks, [2019](#)). As Sarma et al. ([2018](#)) put it: “Nowhere is the need for models that encompass multiple scales more evident than in the [...] nematode’s network of 302 neurons, where simple crawling and swimming behaviours remain unexplained [...] Despite decades of effort, we struggle to describe how individual neurons give rise to such diverse organismal behaviour” (p. 2). The information flow in the circuits of *C. elegans* depends on the context and neuromodulation, and the highly dynamical network of neurons can exhibit far more states and behaviors than the wiring diagram alone would suggest (Bargmann, [2012](#)). Importantly, higher-level or higher-scale factors are not just needed for understanding overall behavior or dynamics of the worm: As the example of neuromodulation shows, we cannot even decipher the function of a *single neuron* without taking its context and the overall dynamics of the system into account.

The second point we would like to emphasize is that we still do not have good explanatory or predictive theories for the behavior of the worm. It is not possible to reliably predict the behavior of the worm based on its nervous system (Hallinen et al., [2021](#)), and the neural mechanisms underlying the decision-making or even the movement patterns of the worm are not well understood (Cook et al., [2019](#); Gjorgjieva et al., [2014](#); Hallinen et al., [2021](#)). This is striking, considering the available resources mentioned earlier: The genome and the wiring diagram of the worm is known, it is possible to manipulate specific neurons, and the development and biochemistry of the worm is well studied. Interestingly, more reliable and efficient predictions can be made with models based on behavioral data, for example, based on video recordings of the movement patterns of the worms (Costa et al.,

[2024](#); Daniels et al., [2019](#))—a point to which we return later in this chapter.

All in all, this does not bode well for efforts at explaining and predicting far more complex systems, such as the human nervous system. Let us thus move on from worms to our main interest in this chapter, namely, human behavior and cognition.

Mirror Neurons

As neurons are the basic building blocks of the nervous system, an important approach in neuroscience has been building up theories starting from the level of individual neurons. This is evident, for example, in mapping-the-human-connectome projects (Toga et al., [2012](#)) or in mirror neuron research. However, if we take into consideration the problems in explaining the behavior of *C. elegans* discussed above, it is very unlikely that this would be a feasible way of developing predictive or explanatory theories of human behavior. To illustrate this, let us look more closely at the case of mirror neuron research, which explicitly starts from the level of neurons.

Mirror neuron research was highly influential and visible in neuroscience in the first decades of this century and is still actively pursued (Bonini et al., [2022](#)). Mirror neurons were discovered by Giacomo Rizzolatti and colleagues at the University of Parma in the early 1990s, when investigating the activity of individual brain cells of macaque monkeys (Rizzolatti et al., [1996](#)). These researchers noticed a peculiar phenomenon: Some neurons in the premotor cortex of the monkeys were firing not only when an action was performed (e.g., a monkey grabbed a banana), but also when the same action was perceived (e.g., the monkey saw another monkey grab a banana).

Rizzolatti and his colleagues soon saw immense potential in their discovery. They proposed that mirror neurons could explain how animals understand the actions of others in a direct way, “from within,” by simulating these actions in the motor area of their own brain (Rizzolatti et al., [1996](#)). In further research, mirror neurons were found

in multiple brain regions and in different animal species (Bonini et al., [2022](#)). In humans, it is difficult to measure the activity of individual neurons (you have to open the skull to do so), but one can measure the activity of the “mirror neuron system” indirectly, for example, using functional magnetic resonance imaging (fMRI; Bonini et al., [2022](#)). Over the years, mirror neurons have been proposed to (partially) explain a broad range of phenomena, such as language acquisition, autism, imitation, or empathy (Cook et al., [2014](#)).

But what does the firing of a neuron in a given context really mean? Does it really give away its role in the mechanism or the brain? Importantly, mirror neurons are not morphologically or developmentally distinguishable from other neurons, so the only thing that makes them “mirror” neurons is their pattern of firing (Hickok, [2014](#)). And the problem, considering the *C. elegans* case, is that it is highly implausible that we could derive the role of a neuron just from its firing activity in a specific context. As we saw, the firing of a neuron in *C. elegans* can signify different things, depending on the context and the overall state of the network.

This context-dependence of neuronal behavior is not just some peculiar feature of *C. elegans*: Instead, neuromodulation is a widespread mechanism in organisms, including mammals (Bargmann, [2012](#)). And due to the fact that neurons in the macaque or human cortex are on average connected to thousands of other neurons (Yuste, [2015](#)), their activity and function in the overall system is likely to be much *more* context-dependent than that of *C. elegans* neurons. Interestingly, also some proponents of mirror neuron research acknowledge this complication: “it is clear that a neuron endowed with a given *functional property* (e.g., discharging during grasping) does not necessarily play a relevant *functional role* in that specific function (e.g., controlling grasping execution). Indeed, the anatomical location and connections of neurons with similar functional properties enable them to play (or not to play) a role in a given function” (Bonini, [2017](#), p. 59). In other words, the fact that the firing of a neuron is correlated with a specific function

or behavior does not imply that it is a neuron *for* that specific function or behavior.

There are also further complications: Mirror neurons do not exclusively fire in the mirroring context, but also often fire to some extent in response to other stimuli (Cook et al., [2014](#)). There is also clear evidence that mirror neurons can be “rewired”: For example, at the end of one study, mirror neurons were firing not just for grasping hand movements, but also when the monkey saw a plier or a stick, which had been used in the experiment and therefore repeatedly observed by the monkey (Heyes & Catmur, [2022](#); Hickok, [2014](#)). All of this suggests that we probably do not know much at all about the role of mirror neurons in the macaque or human nervous system, and it is not even clear that they form a distinct class of neurons (as the title “mirror neuron” suggests).

More generally, it is highly doubtful whether the level of neurons is a plausible starting point when looking for explanations for higher-level phenomena like imitation, action understanding, or autism (Churchland, [2011](#); Hickok, [2014](#); Krakauer et al., [2017](#)). Even in the case of *C. elegans*, explanations for complex behaviors require higher-level dynamics and variables (Sarma et al., [2018](#); Stiefel & Brooks, [2019](#)). In order to get closer to explaining phenomena like action understanding or autism, we first need a good understanding of what these phenomena even are (see also Bringmann et al., [2022](#)). Then, we need to learn more about the cognitive mechanisms underlying or producing these phenomena (Bechtel, [2009](#); Craver, [2007](#)). And (mirror) neurons that are firing do not yet make up a cognitive mechanism. At best, they are a small part of such a mechanism, and to know what role (if any) these small parts are playing, we need to know more about the overall mechanism, at various levels or scales. When a mirror neuron is firing, what are the neurons it is sending information to? From what kinds of neurons is the mirror neuron receiving input? What is the mechanism that this circuit is part of, and how are things like movements and perceptual contents exactly represented in this mechanism? How does this mechanism, or the interaction of several

mechanisms, give rise to the higher-level phenomenon of interest (e.g., action understanding)? Considering that a neuron in the macaque or human cortex can receive inputs from, and send out information to, thousands of other neurons, answers to these questions at the level of neurons are likely to be intractable in practice (see also Rich et al., [2021](#)). Thus, taking higher-level factors into account is necessary, and looking for mechanistic explanations for phenomena such as action understanding or autism at the level of neurons is rather hopeless.

Reductionists and mirror neuron researchers would probably largely agree with this and point out that (1) the mirror neuron theory is not just about single neurons, but also about the “mirror neuron system,” and (2) cognitive neuroscientists are usually looking for multilevel mechanisms and not just for explanations starting from the level of neurons. We will consider the second point in the next section and focus here on the first one. According to mirror neuron proponents, “mirroring” can also be detected with brain imaging at the level of brain areas, by showing that some brain areas are active both when observing and executing an action. However, it is doubtful whether such mirroring patterns have been actually found in fMRI data (Hickok, [2014](#)). And more importantly, what is being measured with fMRI is increased activity in very large populations of neurons: One voxel, which is the smallest unit fMRI can measure, usually contains at least 100,000 neurons (Miller et al., [2014](#)). This is over 300 times the number of neurons in the nervous system of *C. elegans*, which already is complex enough to prevent simple explanations for the roles of neurons. In addition, considering that even in the mirroring areas of monkeys, only around one-third of the neurons are actual mirror neurons (Bonini et al., [2022](#)), the majority of neurons in any given voxel or brain area will not be mirror neurons. All of this suggests that the existence of a mirror neuron system in humans is a wild hypothesis at best (see Hickok, [2014](#); Heyes & Catmur, [2022](#) for further criticism).

To summarize, the functional role of mirror neurons is unknown. It cannot be derived from the firing pattern only, and we also do not have plausible evidence for the existence of a mirror neuron system or

mechanism.¹ In general, looking at the neuronal level is not a plausible starting point for explaining higher-level phenomena such as action understanding, autism, or empathy. And if we already have higher-level explanations, it is not at all clear what neuron-level findings can add to these explanations—a topic to which we return in the next two sections.

Cognitive Neuroscience

In the previous section, we have argued that starting from the level of neurons is not a feasible approach when explaining human behavior. But what are the prospects for cognitive neuroscience in general, if we consider also higher-level and multiscale approaches?

When discussing the *C. elegans*, we pointed out that we still do not have neuroscientific explanations or theories to causally explain or predict the behavior of this worm. And it is crucial to keep in mind just how small the *C. elegans* brain really is, with only 302 neurons. Now imagine a system that is a 100 times larger, with around 30,000 neurons. This system would already pose far greater challenges for modeling, as many aspects of complexity increase exponentially, not linearly. For example, whereas a network of 300 nodes has around 45,000 possible (undirected) connections between the nodes, a network of 30,000 nodes has around 450,000,000 possible (undirected) connections between nodes.² The same pattern can be observed in actual biological cases: Whereas *C. elegans* has around 5000 synaptic connections between neurons (Cook et al., [2019](#)), a fruit fly larva with around 3000 neurons already has more than 500,000 synapses (Winding et al., [2023](#)).

However, we are still nowhere close to the complexity of the human brain. For that, you have to imagine a system that is around 280,000,000 times larger than the nervous system of *C. elegans*. According to current estimates, the human brain consists of around 85,000,000,000 neurons (von Bartheld et al., [2016](#)), most of these neurons synaptically connecting to thousands or tens of thousands

other neurons (Yuste, [2015](#)). If a system that is a “reductionist’s delight” and nearly 300,000,000 times smaller the human brain is already sufficiently complex to make the development of explanatory theories extremely difficult, what are the prospects of developing neuroscientific theories of human behavior? Prima facie, rather hopeless—see also Rich et al. ([2021](#)), who argue that inferring the structure of our cognitive systems based on empirical data is a computationally intractable problem.

However, one source of hope is theoretical breakthroughs in neuroscience, which might allow us deal with complexity in a more efficient way. Something like this has happened in physics. For example, the behavior of gas molecules is extremely complex and often computationally intractable. One liter of oxygen contains around 2×10^{22} gas molecules, which is more than a million times the number of neurons in the human brain. Fortunately, to efficiently explain and predict the behavior of gases, we do not have to model each individual gas molecule, but we can use macrovariables such as pressure, volume, and temperature. As James Woodward ([2021](#)) points out, in physics we have found variables and relationships which allow us to effectively ignore or screen off the details at the lower levels. Woodward calls this “conditional irrelevance”: Conditional on the values of the higher-level variables (e.g., pressure and volume), further variations in the lower-level variables (e.g., positions of individual gas molecules) are not relevant for the phenomenon of interest (Woodward, [2021](#)). Once we have found causally explanatory higher-level variables for which conditional irrelevance holds with respect to lower-level variables, we can just use the higher-level variables with little or no loss of explanatory power (see also Gebharder & Eronen, [2023](#)).

If something like this could be found in neuroscience, that would be a significant breakthrough, and a way out of the conundrum described in this section (see also Stiefel & Brooks, [2019](#)). However, there are at least three problems or challenges with this approach: (1) current methods dominating cognitive neuroscience are not very conducive to reaching this aim, (2) cognitive neuroscience is very different from

physics, so there might not even be useful higher-level variables to discover, and (3) if we discover them, they may turn out to be psychological or behavioral variables instead of neuroscientific ones. The third point is a topic for the next section, so let us briefly consider the first two.

Regarding the first point: In physics, figuring out useful higher-level variables has required careful theoretical and conceptual work (Bringmann & Eronen, [2016](#); Chang, [2004](#); Eronen & Romeijn, [2020](#)). These variables did not just fall out of the data, but were the result of centuries of iterative cycles, where measurement, conceptualization, and mathematical theory interacted (Bringmann et al., [2022](#); Chang, [2004](#)). In contrast, in cognitive neuroscience there is relatively little focus on theoretical and conceptual work, and often, conceptual unclarity abounds: “cognitive neuroscience is awash in a sea of conflicting terms and concepts” (Poldrack & Yarkoni, [2016](#), p. 588).

In practice, cognitive neuroscience is largely technologically and empirically driven, which is not a fruitful approach to figuring out the causal structure of the system, or finding the kinds of higher-level variables discussed above. This is nicely illustrated by Jonas and Kording ([2017](#)), who applied state-of-the-art neuroscience methods to study a microprocessor. The idea was that if we apply these methods to a system whose structure and organization is known, we can assess how much of the structure can be revealed with those methods. The conclusion was that although the methods produced results that looked plausible and interesting, they provided little insights into the actual functioning and causal structure of the system (Jonas & Kording, [2017](#)). Indeed, as far as we know, cognitive neuroscientists have not yet found any causally explanatory higher-level variables in the nervous system.

Then to the second point. Even if cognitive neuroscience focuses efforts on finding higher-level variables and regularities, it is likely that things will not work out as neatly as in physics. In the end, we have not yet found good causal theories even for the behavior of *C. elegans*, with a nervous system about 300,000,000 times smaller than the human one. One aspect that makes neuroscience different from physics is that

biology is very messy: Whereas all gas molecules in a gas are the same, and it is straightforward to take a statistical average of their behavior, each neuron is a distinct biological individual. Each neuron has its own developmental history, extremely complex organization that can differ in significant ways from neurons of the same type, and highly context-dependent function (Gjorgjieva et al., [2016](#); Huang, [2009](#), see also previous section). This suggests that the behavior of populations of neurons might not result in higher-level variables and relationships for which conditional irrelevance with regard to lower levels holds. In other words, it may be that nature has no useful higher-level variables to offer in this case, as the behavior of the system is sensitive to the specific features of individual neurons.

This raises a rather unsettling possibility: On the one hand, we cannot predict and explain human behavior at the level of neurons, due to the overwhelming complexity at that level. But on the other hand, we also cannot predict and explain human behavior with higher-level neuroscientific variables, because there are not enough useful higher-level neuroscientific variables to be found. This could be called “ill-behaved complexity”: Complexity that is (nearly) intractable if we start at lower levels and also does not result in sufficiently well-behaved mechanisms or causal regularities at higher levels.³ If this, or even something close to it, is the case, the prospects of developing predictive or explanatory theories of human behavior with cognitive neuroscience are very dim. Therefore, it may be better to direct efforts to the psychological and behavioral levels, to which we now turn to.

Mindreading and Mindshaping

Based on the pessimism of the previous section, one might wonder if predicting human behavior is possible at all. But in practice, we seem to be quite successful in dealing with each other in everyday life and anticipating each other's behavior. For example, when my partner shouts “bye!” and turns around, I can predict that they will very likely walk out of the door and close the door behind them. So how do we

manage to predict and explain each other, in the face of the intractable complexity of the human brain?

The first part of the answer is that we manage this by tracking real patterns of behavior (Bermúdez, [2003](#); Dennett, [1987](#)). Even in the case of *C. elegans*, currently the most efficient and reliable way of predicting its behavior is by starting from patterns of behavior, and not from neurobiology (Costa et al., [2024](#)). If there is a lot of food available, the worms will probably dwell; if you remove the food, they will probably switch to food searching behavior (Flavell et al., [2020](#)). Similar robust patterns are evident in human behavior: When the light turns green at a crossroads, I can predict that the pedestrians who have been waiting will start crossing the street, whereas the people in cars will stop driving.

As these examples suggest, *context* is crucial for successful prediction. If you put a traffic light into a lecture hall, people will not start walking when it turns green. When someone is screaming “Help!” in a theater play, it is no cause for concern, but if someone is screaming “Help!” next to a burning building, we should act quickly. And so on. Thus, the combination of contextual information and knowledge of robust patterns of behavior gets us very far in predicting human behavior (see also Bermúdez, [2003](#); Hutto, [2007](#)).

However, this is not yet enough. Sometimes people think before they act, make intricate plans, collaborate in complex projects, and so on. Thus, often we have to figure out what other people think and to engage in “mindreading.” By mindreading, we refer to the practice of attributing mental states to agents in order to make sense of and predict their behavior (Spaulding, [2020](#)). Consider the following example:

By attributing to Steve the desire for a banana and the belief that there are no more bananas at home but there are some left at the local grocery store, you can: (i) *explain* why Steve has just left home; (ii) *predict* where Steve is heading; and (iii) *coordinate* your behavior with his (meet him at the store, or

prepare a surprise party while he is gone). Without mindreading, (i)–(iii) do not come easily—if they come at all. (Barlassina, [2017](#))

This example illustrates the power of mindreading based on “folk psychological” mental states such as beliefs, desires, and emotions. Although there are wide-ranging debates on the nature of folk psychology and mindreading, it is widely agreed that humans commonly engage in mindreading, that it is crucial for our social lives, and that it often works (Barlassina, [2017](#); Dennett, [1987](#); Fodor, [1987](#); Zawidzki, [2013](#)). If we treat other people as agents with beliefs, desires, and other mental states, we can predict and explain their behavior remarkably well (see also Dennett, [1987](#), [1991](#)).

This may still seem puzzling considering the near-intractable complexity of the brain discussed in the previous section, and the problems in understanding the cognition of an organism as simple as *C. elegans*. Indeed, it has been pointed out in the literature that figuring out what mental states another person has, and predicting their next actions based on those mental states, should be a computationally intractable problem (see, e.g., Rich et al., [2021](#); Zawidzki, [2013](#)). There are just too many possible beliefs, thoughts, or other mental states that one could in principle have in any given situation.

To reconcile this difficulty with our evident success in mindreading, several authors have proposed in recent years that we humans are “mindshaping” ourselves and others in order to make life more tractable (Heyes, [2018](#); McGeer, [2007](#), [2021](#); Peters, [2019](#); Zawidzki, [2013](#)). The hypothesis is that the framework of mindreading and folk psychology has developed through (cultural) evolution as a way to make behavior predictable and to coordinate behavior so that we can function together efficiently (Heyes, [2018](#); Slors, [2021](#); Sterelny, [2012](#)). Humans are a thoroughly cooperative species, and our capability to cooperate efficiently (e.g., when hunting large mammals) is a key factor in our evolutionary success (Sterelny, [2012](#)). Through norms, agreements, rules, and shared vocabulary we *make* our behavior predictable and

understandable so that we can better get along, and coordination and cooperation become easier (Peters, [2019](#)). As children grow up, they learn about beliefs, thoughts, and intentions, how to appropriately use such concepts, how they ought to think and act in certain situations, and so on (Heyes, [2018](#); McGeer, [2007](#)). If they fail to follow these norms and conventions, they are corrected or even disciplined.

We can predict the behavior of people based on these norms, rules, and regularities, because people have learned to follow them. As a species, we are very proficient at shaping our minds, shaping other minds, and tracking these shapes or patterns, and all of this results in the predictive success of mindreading. Hence, whereas the human nervous system is (nearly) intractably complex, human behavior is not. It follows real patterns that are (at least partly) the result of mindshaping by pedagogy, rules, conventions, etc. These patterns are to a large extent normative: People follow them because they *ought to* follow them. However, this normative dimension, and the predictive and explanatory power that comes with it, is completely lost if we depart from folk psychology and search for theories and explanations in the brain (see also Borsboom et al., [2019](#)). Norms are not visible in brain states. Therefore, without the normative dimension, we are faced with the ocean of complexity described in the previous section.

To summarize, humans have developed mindshaping and mindreading, which are based on norms and conventions, as a solution for dealing with the potentially intractable complexity of human behavior. It is unclear how we could explain and predict human behavior without relying on context, behavior, and the normative patterns resulting from mindshaping.

Conclusions

We can conclude that any realistic attempt to construct a theory to predict and explain human behavior has to rely on behavioral patterns and (folk) psychology.⁴ But importantly, we should not just accept folk psychological concepts as they are. Instead, they should be amenable to

improvement and adjustment based on empirical findings and conceptual work (Bringmann et al., [2022](#); Eronen, [2020b](#)). In practice, psychological science has been very much empirically and statistically driven (similarly to cognitive neuroscience), which partly explains the lack of successful theories in psychology (Eronen & Bringmann, [2021](#)). In order to make progress, it is fundamentally important to focus on the basic concepts and how to iteratively improve them (Bringmann et al., [2022](#)). However, psychological theory constructors should also keep in mind that finding good psychological theories is extremely hard (see also Eronen & Bringmann, [2021](#)), so progress is likely to be very slow.

A closely related point is that there are other goals in psychology and neuroscience besides prediction and causal explanation of behavior, and these other goals may in fact be more realistic and fruitful. For example, psychology and neuroscience often aim at detecting phenomena or developing purely descriptive models. Also, non-mainstream approaches such as phenomenology (Wertz, [2024](#)) or psychological humanities (Teo, [2017](#)) do not have prediction and causal explanation as goals, but rather, for example, understanding. Our arguments do not apply to these other goals and approaches. In fact, the *C. elegans* case nicely illustrates the importance and usefulness of purely descriptive research. Arguably the greatest success in *C. elegans* research has been developing a very complete descriptive model of the biology of the worm (Ankeny, [2000](#), [2001](#)), which also provides a springboard for eventual causal theories of the behavior of the worm. Sufficient knowledge of relevant robust phenomena and the organization of the system can even be seen as a prerequisite for developing good explanatory theories of the system (Eronen & Bringmann, [2021](#)). Also, discoveries of robust phenomena can be important as such: Consider examples such as cognitive biases or inattentional blindness, which are very important and societally relevant discoveries, even though we do not know the underlying mechanisms (Eronen, [2020a](#); Haig, [2013](#)). In addition, discovering new patterns in the interaction of psychological states, behavior, and context can be very fruitful and important (e.g., studying in which contexts we

are most likely to experience inattentional blindness). In this sense, our arguments can be seen as supporting the importance of non-causal descriptive research and phenomena detection, both in neuroscience and in psychology.

We started this chapter with the question: Is there a privileged level, or levels, where we have the best prospects of finding successful theories to predict and explain human behavior? Based on our analysis, we offer a preliminary answer: The best prospects are not at biological or neuroscientific levels, but at the level of psychology and behavior.

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Footnotes

¹ The mirror neuron theory has been closely associated with the simulation theory of mindreading and with embodied and enactive approaches to cognition. However, these do not depend on the mirror neuron theory, and our criticism is not aimed at them.

² Calculated with the equation $(n(n - 1))/2$, where n is the number of nodes, see, for example, https://en.wikipedia.org/wiki/Complete_graph

³ Woodward ([2021](#)) also mentions this possibility, and refers to it as “model chaos.”

⁴ In philosophical terms, we are defending a strong form of epistemic or explanatory antireductionism in psychology. However, this does not imply or require any ontological antireductionism, or even realism regarding (folk) psychology. The predictive power of (folk) psychological states is compatible with a range of ontological positions, ranging from Dennettian instrumentalism to realism (see Eronen, [2020b](#), for more).

11. Addiction as an Example of Concept and Theory Formation in Psychology and Psychiatry: Lessons Since the Nineteenth Century

Stephan Schleim¹ 

(¹) Theory and History of Psychology, Heymans Institute for Psychological Research, Faculty of Behavioral and Social Sciences, University of Groningen, Groningen, Netherlands

 **Stephan Schleim**

Email: s.schleim@rug.nl

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Despite endless efforts, psychology has not yet been able to attain the rank of an exact science, not least because it was forced to form its basic concepts independently of brain science. (Flechsigt, [1896](#), p. 7; transl.)

Introduction

Where do scientists get their concepts from? A central and, in my opinion, still valid result of Kant's epistemology was that there can be

no pure observation without concepts. Or to put it in more modern terms: Scientific observation presumes definitions. From today's perspective, it is therefore not surprising that the positivism¹ of the Vienna Circle was doomed to fail, at least in its early and naive form: Even the allegedly purely scientific protocol sentences (cf. Neurath, [1933](#)) of the form "Stephan's protocol at 1:25 pm was: Stephan stood by the lake and saw a swan" presuppose the concept of a swan. Now, the question of what a swan is may be considered largely undisputed in today's biology—but, as we will see below, in the border areas of our knowledge and our sciences, such a consensus is often lacking.

Where do psychology and psychiatry in particular get their concepts from? To this day, representatives of these disciplines have to ask themselves what their subject is all about (e.g. Kendler, [2016](#)). Is it natural science? Life science? Social science? Humanities? Already since the nineteenth century, the reduction to neurobiology in particular was intended to provide these disciplines with their ontological foundation (cf. Marx, [1970](#); Schleim, [2025](#)). As is well known, this thinking or paradigm has led to an immense number of research projects and even more publications—without, however, being able to reduce psychological processes or mental disorders to something biological in a strong sense. We will look at this in more detail for the concepts of addiction and dependence. This choice is partially contingent on the author's previous work (e.g. Schleim, [2023](#)) but also suitable in that it concerns various scientific and clinical disciplines, theory and practice, society and politics, and has an interesting history back into the nineteenth century.

In psychological textbooks, the consensus remains that psychology is the science of mind (or consciousness) and behavior, which of course only shifts the subject matter to a different level. And this may be less controversial for behavior than for mind/consciousness. These problems were already encountered in the classical mind-body problem: To what extent is the psyche different from the body (or matter) and how do they relate to each other, especially in the light of scientific findings? Despite all the eagerness to reduce the former to the

latter or—in the other camp—to save “the psyche” from this reduction and thus from being absorbed into the physical, it is often overlooked that the concept of the body (matter) is by no means unproblematic. Montero ([1999](#)) pointed out that in this sense we not only have a mind-body problem, but also a body problem: If one wants to reduce mental to physical processes, then one should also know what a *physical* process is (cf. Schleim, [2020a](#)). The pragmatic answer that “physical” is what physicists do is obviously problematic: This view would amount to question-begging when interpretations of quantum mechanics invoke consciousness (e.g. de Barros & Montemayor, [2019](#)). Then the latter would have to be seen as a physical category, simply because physicists deal with it. This is clearly not a viable way to decide the nature of scientific concepts.

For the time being, an essentialist model—like that of the chemical elements (cf. Kendler et al., [2011](#))—that would allow researchers in psychology and related disciplines to “carve nature at its joints” is unavailable. Good scientific practice then requires them to operationalize their concepts (or “constructs,” often the independent and dependent variables) transparently: The procedure in the methods section of a paper should describe how the scientists measured their constructs such that this can be repeated by third parties. This is useful in the sense of Thomas Kuhn’s “normal science” (Kuhn, [1962](#)) because it allows researchers to pursue their activities productively, instead of endless disputes on what, say, attention *really* is. In the long term, however, it is likely to incur the accusation of conceptual vagueness and lack of theoretical integration (e.g. Eronen & Bringmann, [2021](#); Hutmacher & Franz, [2025](#)). For it is then not self-evident that even research on a fundamental process such as attention deals with the same topic when talking about “attention” (cf. Hommel et al., [2019](#)).

A culmination of these problems is current consciousness research: At the turn of the millennium, scholars celebrated the fact that this topic had been put back on the research agenda. And not just somewhere, but right at the top of the agenda thanks to framing consciousness as one of the last great mysteries of humanity (cf. Miller,

[2005](#)). Recently, established representatives of this research accused each other of practicing “pseudoscience,” while different groups are competing for scarce funds (Lenharo, [2024](#)). What is more interesting for us in terms of terminology is that in this prevailing paradigm, the concept of consciousness cannot be clearly distinguished from that of attention or memory (Koch et al., [2016](#)). Today, a multitude of approaches with different operationalizations and thus also *explananda* (viz., what is to be explained) exist side by side (Northoff & Lamme, [2020](#); Signorelli et al., [2021](#)). According to Signorelli and colleagues, these preliminary approaches do not deserve the status of a theory, perhaps not even a model of consciousness; rather, one should also look more closely at the epistemological question of what “to explain” should mean here. And instead of the brain positivistically “giving” us the answer as to what the correct concept of consciousness is, it is rather the other way around: Depending on the concept of consciousness, neuroscientists associate completely different brain regions with the explanandum, for example, frontal versus posterior neuronal networks (Northoff & Lamme, [2020](#)).

As I wrote at the very beginning with reference to Kant’s epistemology: Without concepts and their definitions there can be no scientific observation. Above, I have tried to show in more general terms what this can mean for disciplines such as psychology and related fields. In the following, I will work this out in more detail using the concepts of addiction and dependence—which influence research groups and millions of patients around the world—in three parts: The first section will show how “addiction” gained prominence in medicine in response to socio-political changes around 1900; then I will examine how it was interpreted neurobiologically in the 1990s, the “Decade of the Brain”; in the third section, I will move on to the pragmatic understanding of addiction as it has become established in the relevant diagnostic manuals today. In the concluding section at the end, I will relate this to the question of what this means for psychological-psychiatric conceptualization and theory formation.

A Brief History of the Concept of Addiction

Where does the term “addiction” come from? One possible derivation is etymological: In Roman law, the Latin “addicere” (literally: to speak to, to assign to) had the meaning of assigning slaves to their masters (Rosenthal & Faris, [2019](#)). Interestingly, the Dutch word “ver-slav-ing” used today for addiction literally means “en-slave-ment” (from Dutch “slaaf”, slave). From the mid-sixteenth century, addiction referred to the state of being devoted to something. This could be positive or negative—the former, for example (in a Christian context), in the case of devotion to God, the latter in the case of excessive gambling.

However, we have other languages and therefore other cultures that derive the term for addiction differently. Despite the family ties between English, Dutch and German, the term “Sucht” used today in the latter language is derived from Middle High German “siech,” which meant “sick.” The relationship to the English term or Dutch “ziek” is obvious, while the German “krank” that is used today is of a completely different origin. “Sucht” was therefore much closer to a medical-pathological meaning than “addiction” or “verslaving” from the very beginning. Do the terms in the different languages then denote the same or different phenomena? Interestingly, the relative frequency of the corresponding words in German and English books also shows different patterns (Fig. [11.1](#)). It should be borne in mind that German was a dominant language of science until the Second World War.

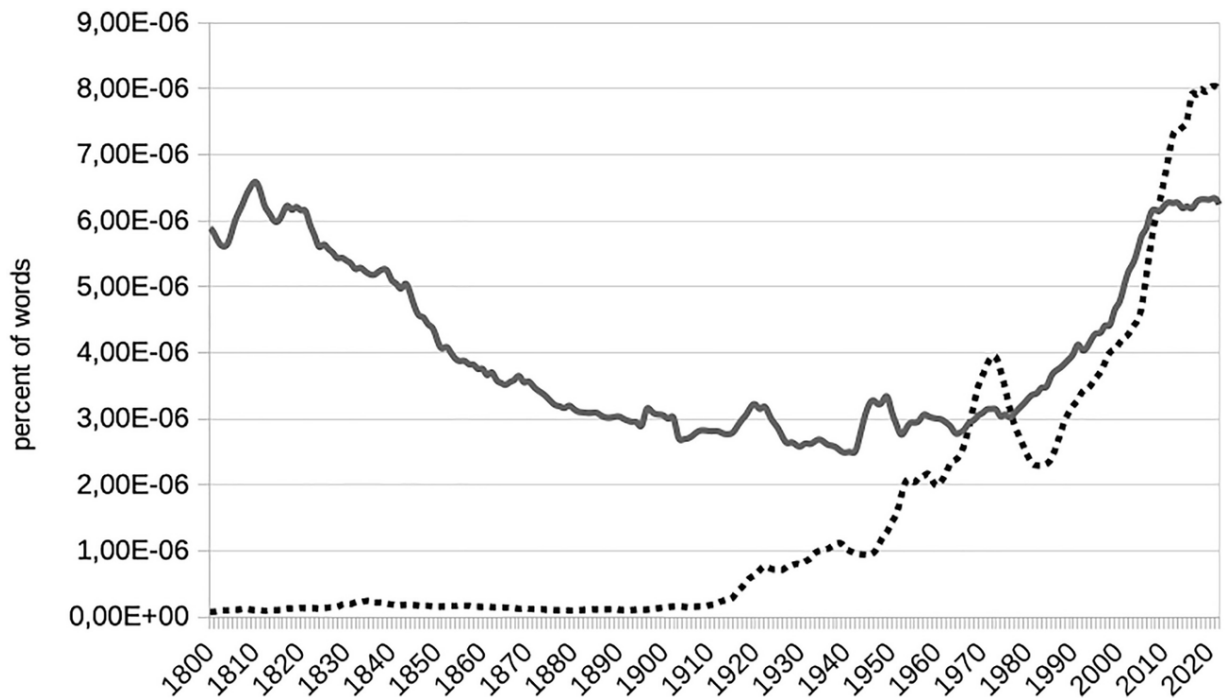


Fig. 11.1 “Addiction” and “Sucht” over time. In line with explanation in the text, it can be seen that “addiction” (dotted line) is only increasingly discussed in English-language books since the 1920s. In contrast, the course of the relative frequency of “Sucht” (gray line) in German-language books indicates a shift in the meaning of illness to addiction in the modern sense. (Source: Ngram, courtesy of Google)

However, etymological derivation carries the risk of confusing the origin of a term with its actual meaning in a particular time and culture. We therefore need to look at how a term was actually used. Konrad Rieger (1855–1939), professor of psychiatry and director of the psychiatric clinic in Würzburg, Germany, demonstrated the diversity of the term “Sucht” in German around the year 1900 (Rieger, [1905](#)). He distinguished (a) 15 types of addiction that are purely moral in nature, (b) 17 pathological types and (c) 19 mixed cases. For (a), it is significant that “Sucht” can also be derived from “suchen” (i.e. seeking), which is reminiscent of the older English meaning of devotion to something: Examples of this include “Habsucht” (greed) and “Selbstsucht” (selfishness)—but in fact also “Spielsucht” (an intense passion for gambling). By contrast, (b) does not include what we consider as addiction today, but actually somatic-organic diseases such as “Fallsucht” (epilepsy), “Fettsucht” (obesity), “Mondsucht” (lunacy) or

“Schwindsucht” (tuberculosis). Examples of (c), the mixed cases, refer on the one hand to substance use, which we think of today in particular with addiction, such as “Cocainsucht” (cocaine addiction), “Haschischsucht” (cannabis addiction) or “Trunksucht” (alcohol addiction). On the other hand, Rieger also lists potentially problematic behaviors such as “Eifersucht” (jealousy), “Stehlsucht” (cleptomania) or “Zerstörungssucht” (a strong desire to destroy).

This distinction between pathological and non-pathological forms can be explained most simply in terms of substances: People could, for example, have a passion for consuming alcoholic beverages, that in itself would not be pathological, according to Rieger. However, people could also drink so much that they have acutely pathological experiences or behavioral patterns; this would correspond most closely to our current concept of drug abuse or intoxication (see below). And frequent and intensive drinking could ultimately lead to chronic disease states in the sense of organic damage. Rieger’s examples in which no substances are involved become pathological when there is a loss of control, that is, when they occur compulsively and to an extent that can no longer be considered normal. This may sound vague—but, in fact, psychiatrists and clinical psychologists nowadays and thus 120 years later use the DSM criteria² to assess whether a social-psychological impairment is “clinically significant” or, for example, when grief after the loss of a loved one no longer is “culturally appropriate” and becomes Prolonged Grief Disorder (APA, [2022](#)).

Rieger’s account is more than just a historical example. Even today, the German language has over 50 nouns ending in “Sucht” (Duden, [2015](#)). As we will see, the distinction between pathological and non-pathological forms, especially in mixed cases, still poses a challenge today—for which an understanding of what is to be regarded as “normal” and in this sense is influenced by normative moral ideas is significant. One could say that morality is much more involved in the German concept of addiction than in English, where “addiction” and other disease names are more separate. In any case, Rieger and

contemporaries did not yet have a single medical concept of addiction—and certainly didn't focus primarily on substances as we do today.

The pharmacologist, toxicologist and pioneer in addiction research Louis Lewin (1855–1939) painted a similar picture. Commenting on a draft law to combat alcohol abuse, he discussed the distinction between “Trunkenheit” (drunkenness) and “Trunksucht” (alcohol addiction; Lewin, [1891](#)). The former corresponds to Rieger's acute pathological case, the latter to the chronic one. As we will see below, the definition of the latter is surprisingly modern:

According to medical opinion, alcoholism [Trunksucht] is a passion of certain people to consume alcoholic beverages frequently in such quantities that it causes demonstrable harm to their physical and mental health and, as a result, a neglect of social or family duties. (Lewin, [1891](#), p. 1199; transl.)

The fact that there is also no general concept of addiction in his work is shown by the discussion of other substances such as cannabis, cocaine, morphine and opium, where only morphine addiction is named as such. Cocaine is referred to as “cocaineism”, opium as “opium consumption” and that many millions of people are “slaves to opium” (Lewin, [1891](#), p. 1200). Even in Lewin's 470-page magnum opus, published 26 years later, “Suchten” are only mentioned on one page, in a quote that will be useful to us later:

In contrast to morally deviant addictions [Suchten], such as gambling addiction, the passion for narcotic substances has a tangible, material basis, namely the drug in question, which has an altering effect on the function of the cerebrum. Whatever other consequences may result from this – one is the decisive one, namely the loss of willpower in the face of the lure of morphine, cocaine, etc. The impulses thus produced are so pleasant to the user that moral resistance is also broken. (Lewin, [1927](#), p. 52; transl.)

Interestingly, the [1998](#) English translation does not use the term “addiction” but “passion” (Lewin, [1927](#)/1998, p. 28). Like Rieger, Lewin attaches “Sucht” to concepts like love, gambling and alcohol, but speaks of morphinism, cocaineism, nicotinism or caffeinism related to other substances (Lewin, [1927](#)/1998, p. 237). Pharmacologist Margit Kreutel’s historical dissertation on opium addiction expresses her astonishment that opium, despite its widespread use in many Eastern and Western cultures both as a medicine and as a stimulant, was not described in connection with addiction for centuries (Kreutel, [1988](#)). According to the conclusion suggested above, this was probably simply due to the fact that there was no concept of addiction as we know it today.

The psychiatrist Thomas Szasz (1920–2012) described that it was only with the *Standard Classified Nomenclature of Disease* of 1934 (Logie, [1934](#)), the forerunner of the DSM, that “drug addiction” was officially included in a standard medical work for the first time (Szasz, [1974](#), p. 7).³ According to him, this was preceded by decades of political and religious demonization campaigns (cf. Jay, [2010](#); Schleim, [2023](#)). Thus, although doctors and especially psychiatrists on both sides of the Atlantic Ocean had been treating problematic substance use for some time, this was only subsumed under the general term of drug addiction from the 1930s onward. Even if I cannot develop an exhaustive history of the concept of addiction here, the described processes could perhaps be understood in the sense of Ian Hacking’s (1936–2023) “making up people”—as a socio-religious-political-medical construction of the class of drug addicts with the help of the category of drug addiction introduced in 1934 (cf. Hacking, [2007](#)).

In any case, it is fitting that, as Kreutel describes, in the following years and decades and in many countries, great difficulties were encountered in defining addiction (Kreutel, [1988](#), pp. 5–6). In particular, experts attempted to define a medical concept of addiction in contrast to the everyday concept of habituation. In 1950, the “Expert Committee on Drugs Liable to Produce Addiction” of the World Health Organization (WHO) took on this task. An initial proposal turned out to

be unsatisfactory, and only a few years later, in 1957, it was replaced by the following definition:

Drug addiction is a state of periodic or chronic intoxication caused by the repeated use of a natural or synthetic drug. Its characteristics include: 1) an irrepressible craving or need (compulsion) to continue taking the drug and to obtain it by any means; 2) a tendency to increase the dose; 3) a psychological (psychologic) and generally a physical dependence on the drug; 4) harmful effects for the individual and for society. (as cited in Kreutel, [1988](#), p. 6 transl.)

And in contrast:

Drug habituation is a condition that results from repeated use of a drug. Its characteristics include: 1) a desire (but not a compulsion) to continue taking the drug because of the euphoria it produces; 2) little or no tendency to increase the dose; 3) some degree of psychological dependence on the effects of the drug, but the absence of physical dependence and therefore of an abstinence syndrome; 4) harmful effects, if any, primarily to the individual. (as cited in Kreutel, [1988](#), p. 6; transl.)

The main differences between drug addiction and habituation were therefore that in the former the craving had to be uncontrollably strong and the dose tended to be increased, that there had to be a physical as well as a psychological dependence and that in the case of addiction there also had to be social damage. Even at first glance, it is noticeable that formulations such as “tendency” or “in general” are somewhat vague. From today’s perspective, psychological dependence is actually another formulation for (strong) desire or craving; physical dependence is usually understood as (severe) pain, seizures or delirium during drug withdrawal (cf. Rothenhäusler & Täschner, [2013](#)).⁴ Social factors also likely play a role for the social damage: If, for example, heroin users have to share syringes and therefore infect each other

with viruses such as HIV or hepatitis and then possibly also infect uninvolved third parties via sexual intercourse, the consequences to public health will be greater than under better hygienic conditions.⁵

As early as 1960, the medical profession acknowledged the inadequacy of these definitions and in 1964 the commission was given a new name: “WHO Expert Committee on Drug-Dependence” (Kreutel, [1988](#), p. 7). This committee recognized the diversity of addictive states and finally published the following definition in 1969:

Drug addiction is a psychological and sometimes physical condition caused by the interaction of a living organism and a drug. It is characterized by reactions that always include the compulsion to take the drug regularly or periodically. On the one hand, there is the need to experience the psychological effects of the drug, and on the other, the urge to avoid symptoms of abstinence. Tolerance may or may not be present. An individual can be dependent on more than one drug. (Kreutel, [1988](#), p. 7)

In order to confirm that the medical profession, by officially adopting the concept of addiction—which would later promote the establishment of addiction medicine, addiction centers and so on—was satisfying a political-religious need in the 1930s in the USA, as Szasz ([1974](#)) suspected and is suggested on the basis of numerous media reports, extensive archival work would probably be necessary. However, it is widely described and known that after the end of alcohol prohibition, US authorities strongly targeted cannabis and opium and its many derivatives, sometimes with propaganda films and racist motives (cf. Jay, [2010](#); Schleim, [2023](#)). It is also striking that these drugs were originally regarded as important medicines, but were then demonized, stigmatized and finally banned altogether surprisingly quickly and violently (de Ridder, [2000](#); Kreutel, [1988](#)). Indeed, for many centuries opium had been considered *the* essential medicine par excellence in much of the world; suddenly it was framed as the scourge of humanity.

Why are these political and sociological facts of interest for our investigation? They support the assumption that drug addiction can be constructed by influential actors with certain interests precisely because it is—to use Hacking’s term—not a “fixed” but a “moving target” (Hacking, [2007](#)). This brief history of the concept of addiction is thus also intended as a template for what conceptual-historical studies of other psychological or psychiatric constructs such as attention or empathy might look like (cf. Schleim, [2015](#)). After all, such concepts have to come from somewhere and from someone.⁶ After a brief excursion into the “Decade of the Brain,” the 1990s, we then look at today’s conceptual practice of addiction and dependency.

Addiction as a Brain Disease

From 2002 to 2015, the American psychiatrist and neuroscientist Thomas R. Insel was director of the National Institute of Mental Health (NIMH), a public institution for the treatment and research of psychological-psychiatric disorders with a budget in the billions.⁷ Insel was thus an important pioneer in research, particularly in the field of biological or neuropsychiatry. In a popular science article in 2010, he described the disorders diagnosed millions of times today as the effects of “faulty circuits” in the brain (Insel, [2010](#)). Interestingly, he wrote about the historical development of his field, that “scientific approaches based on modern biology, neuroscience and genomics are replacing nearly a century of purely psychological theories, yielding new approaches to the treatment of mental illnesses” (Insel, [2010](#), p. 44).

In the last section, we saw Louis Lewin’s quote from his main work on substance use from 1927, in which he explained that the “passion for narcotic substances,” in contrast to a “morally aberrant” addiction such as gambling, is based on a “material basis,” namely, the change in brain function caused by the consumption of the substance (Lewin, [1927](#), p. 52). In fact, the term “brain” appears 182 times in Lewin’s 470-page monograph on drugs and thus much more frequently than “Sucht” (addiction) in all its variants. And of course there were already more or

less successful attempts in the nineteenth century in both psychology and psychiatry to explain mental processes, as we would say today: neuroscientifically (cf. Marx, [1970](#)). The spread of psychosurgery, psychotropic drugs and brain stimulation over large parts of the twentieth century testify to the fact that the brain was repeatedly placed at the center of research and clinical practice (cf. Schleim, [2021](#), [2025](#)).

However, a strong increase in the biological paradigm can be observed since the 1980s. A crucial point of this paradigm shift could be Nancy C. Andreasen's much-cited popular science book *The Broken Brain: The Biological Revolution in Psychiatry*—at least it expresses the prevailing zeitgeist well (Andreasen, [1984](#); cf. Schleim, [2025](#)). From the DSM-III of 1980 onward, its editors wanted to put psychiatry on a scientific footing and leave the Freudian psychodynamic categories behind (cf. Wilson, [1993](#)). Progress in neuroscience fostered these high hopes. In the proclamation “The Decade of the Brain” from 1990, the highest political level not only called for more research into neurological diseases such as Alzheimer's or Parkinson's, but also explicitly for fighting against drugs and addiction:

Research may also prove valuable in our war on drugs, as studies provide greater insight into how people become addicted to drugs and how drugs affect the brain. These studies may also help produce effective treatments for chemical dependency and help us to understand and prevent the harm done to the preborn children of pregnant women who abuse drugs and alcohol.^{[8](#)}

The course was thus set for researching and explaining substance use and associated problems from a neuroscientific perspective. Around two years after the proclamation, the National Institute on Drug Abuse (NIDA) was incorporated into the National Institutes of Health and thus became a sister organization of the aforementioned NIMH. In 1997, Alan I. Leshner, who, as a psychologist—which was very unusual—had

been director of the NIMH from 1990 to 1992 and then became director of the NIDA from 1994 to 2001, published the programmatic and widely quoted two-page commentary “Addiction is a Brain Disease, and it Matters” in *Science* (Leshner, [1997](#)).

If we remember from the previous section that the term “addiction” was actually abandoned by the WHO back in the 1960s, we can wonder about this title: Something that cannot be clearly defined is supposed to be a brain disease? This is reminiscent of the optimism widespread in neurophilosophy, which we already encountered in the introduction, to define difficult terms such as “consciousness” with the aid of the brain (cf. Churchland, [2002](#)). As we have seen, this is limited by a circularity, since the study of consciousness (just like counting swans) already presupposes a concept of it! And for the empirical results, the concept—we remember the different findings in the frontal and posterior cortex, respectively—is demonstrably of great importance (e.g. Northoff & Lamme, [2020](#)). In a Kantian vein, concept and perception cannot be separated here. Leshner’s core argument is best expressed in his own words:

That addiction is tied to changes in brain structure and function is what makes it, fundamentally, a brain disease. A metaphorical switch in the brain seems to be thrown as a result of prolonged drug use. Initially, drug use is a voluntary behavior, but when that switch is thrown, the individual moves into the state of addiction, characterized by compulsive drug seeking and use [...]. (Leshner, [1997](#), p. 46)

To be fair, it must be added that Leshner also mentioned social factors of addiction, for example, the (nowadays still generally underappreciated) finding that many US soldiers returning from the Vietnam War—where the opium derivative heroine was relatively easy, cheap and available in high quality—gave up their habit without much trouble (cf. de Ridder, [2000](#); Schleim, [2023](#)). This suggests that substance use should be understood—at least in part—as a means of

coping with problems in a particular situation, that is, as *instrumental* use (Müller, [2020](#); Schleim, [2020b](#)). Now it is a triviality that our experiences and our behavior change our brain. We call this “plasticity.” Even if the rate of change of such processes decreases with age, it never reaches zero according to current knowledge (cf. Schleim, [2025](#)). For Leshner, the pathological criterion, the essential factor for distinguishing between substance use and addiction, is *voluntary control*, as he further explained in a footnote to the previous quote:

The state of addiction – both the clinical condition and the brain state – is qualitatively different from the effects of large amounts of drugs. The individual, once addicted, has moved from a state where drug use is voluntary and controlled to one where drug craving, seeking, and use are no longer under the same kind of voluntary control, and these changes reflect changes in brain function. (Leshner, [1997](#), p. 47, n. 11)

We need not exhaustively discuss the merits of this definitional criterion here. Leshner’s description of addiction as a brain disease also provoked many critical responses (e.g. Levy, [2013](#); Satel & Lilienfeld, [2014](#)). This is, of course, a specific case of the general question of the extent to which psychological-psychiatric disorders can be reduced to brain processes, a special case of the mind-body reduction addressed above. There are various arguments as to why this is not possible, for example, the normativity of the disorders (Stier, [2013](#)), their conceptual complexity (Schleim, [2022](#)) or the fact that they are not only realized in the brain, but also embodied and embedded in an environment (Fuchs, [2017](#)). Moreover, we know today, some 30 years after Leshner’s programmatic publication, that even in the latest edition of the DSM from 2022, there is still not a single reliable diagnostic biomarker among the hundreds of disorders (APA, [2022](#)).

In the last section, we had to leave open the question of the extent to which the official classification of addiction as a medical-psychiatric problem was shaped by political-religious aspirations. In the case of the

narrative of addiction as a brain disease, Leshner's own later words are available: In his short time as director of the NIMH, he had seen that describing psychological-psychiatric disorders as brain diseases secured funding for this research from the US Congress. He then applied this strategy to his new role as director of the NIDA, as the highest representative of addiction research in the USA and perhaps even worldwide: "Mental health advocates started referring to schizophrenia as a 'brain disease' and showing brain scans to members of congress to get them to increase funding for research. It really worked" (as cited in Satel & Lilienfeld, [2014](#), p. 4).

But if the concept of addiction cannot be clearly defined and cannot be reduced to the brain,⁹ how should we deal with problematic substance use? We find a way out in the pragmatics of medical practice.

From Addiction to Dependence from a Pragmatic Medical Perspective

As we saw above, the WHO abandoned the concept of addiction back in the 1960s and replaced it with that of dependence. It is a special feature of clinical psychology and psychiatry that their disorders are not only classified in the international ICD of the WHO, but also in the North American DSM. The publishers of the latter only followed the WHO's decision for the DSM-III-R of 1987. Charles P. O'Brien was a member of the corresponding committee and would later (2007–2013) even become chairman of the commission to revise the substance use disorders for the DSM-5. In a widely cited editorial together with Nora D. Volkow, who succeeded Leshner as director of NIDA, he described the reasons for this decision (O'Brien et al., [2006](#)). Therefore, the removal of the term "addiction" from the DSM was very controversial and was decided by only *one vote* in majority. According to these authors, there were efforts to align the DSM more closely with the ICD on the one hand and to harmonize the treatment of the various psychoactive substances on the other. Psychiatrists also wanted to avoid stigmatizing the large number of alcohol and nicotine users as

“drug addicts.” As a result, the term “dependence” became established. However, O’Brien leaves no doubt that he considered this decision to be flawed: “This decision was a serious mistake. [...] Addiction is a perfectly acceptable word” (O’Brien et al., [2006](#), pp. 764–765). On the one hand, addiction could be validly and reliably defined as “compulsive drug-seeking behavior” and, on the other hand, “dependence” in common parlance only conveyed the aspect of withdrawal symptoms, which experts refer to as physical dependence (O’Brien et al., [2006](#), pp. 764–765).

We have now seen repeatedly that the field of addiction and its definition is characterized by moral-political-religious and financial interests as well as by different views in the professional world. In the field of clinical psychology and psychiatry, these are not only academic debates, but also influence the nature and availability of psychological and medical services worldwide. The organization and composition of the relevant committees for the classification of disorders with their different interests and possible conflicts of interest is a topic in itself and of a more sociological nature (e.g. Davis et al., [2024](#)).

We can content ourselves here with looking at the result in ICD-11, which was adopted by the WHO in 2019 and came into force in 2022. It contains a long list of substances and behaviors, but “addiction” is only used for so-called *behavioral* addictions—so far only gambling and gaming disorder. However, regardless of whether it is a substance or a behavior, the criteria for dependence are consistent (e.g. Matone et al., [2022](#)). These are best illustrated using an example:

6C41.2 Cannabis dependence: Cannabis dependence is a disorder of regulation of cannabis use arising from repeated or continuous use of cannabis. The characteristic feature is a strong internal drive to use cannabis, which is manifested by impaired ability to control use, increasing priority given to use over other activities and persistence of use despite harm or negative consequences. These experiences are often accompanied by a subjective sensation of urge or craving to use cannabis.

Physiological features of dependence may also be present, including tolerance to the effects of cannabis, withdrawal symptoms following cessation or reduction in use of cannabis, or repeated use of cannabis or pharmacologically similar substances to prevent or alleviate withdrawal symptoms. The features of dependence are usually evident over a period of at least 12 months but the diagnosis may be made if cannabis use is continuous (daily or almost daily) for at least 3 months.¹⁰

The diagnostic criteria derived from this then relate to (1) the loss of control (one usually craves the drug and consumes it earlier, more frequently or for longer than one actually wants to); (2) physiological characteristics such as tolerance development and withdrawal symptoms and (3) the domination of life by consumption (one neglects other interests or obligations in life, for example, and even continues despite problems).

Finally, we can compare this with what we discussed above: Lewin's definition of "Trunksucht" (alcohol addiction) 130 years earlier is similar to the classification in ICD-11 depending on what he understands by "passion" (for consumption) (Lewin, [1891](#)). The dysfunctional aspect of the psychological, physical or social sphere is already included in his classification. Rieger, on the other hand, placed the desire for a substance or behavior in the moral, non-pathological domain (Rieger, [1905](#)). The acute negative effects of consumption are treated separately in the ICD-11¹¹ and the chronic damage is only hinted at in the case of dependence with dysfunction.

Loss of control or compulsion, on the other hand, runs through the definitions of the WHO from 1969 (Kreutel, [1988](#), p. 7), Leshner ([1997](#)) and O'Brien and colleagues ([2006](#)). This implies that substance use *without* loss of control and psychological, physical or social harm is by definition *not* a medical problem. And from a theoretical point of view, the pathological concept of dependence is therefore primarily oriented toward the occurrence of individual or social suffering or harm. We can

now attempt a synthesis and venture an outlook on the question of psychological concepts in general.

Synthesis, Conclusion and Outlook

Theory-building can be said to serve two main purposes. One is to *predict* the occurrence of events or outcomes of experiments, and thus to anticipate new facts. The other is to *explain*, or to make intelligible facts which have already been recorded. (von Wright, [1971](#), p. 1)

I started the chapter with a quote from the then important psychiatrist and brain researcher Paul E. Flechsig (1847–1929). He believed that psychology was not an exact science because it emerged before it was able to derive its basic concepts from brain research (Flechsig, [1896](#)). Even after decades of biological, neuro- and evolutionary psychology, cognitive neuroscience and neuroscientific consciousness research, this breakthrough has yet to occur. This and the current state of biological psychiatry raise the question of whether this is even possible (cf. Noble et al., [2024](#); Schleim, [2024](#), [2025](#)). Incidentally, at the end of his lecture, Flechsig warned of the harmful effects of alcohol, particularly on the brain, and recommended “general education on the hygiene of brain life” for prevention (Flechsig, [1896](#), p. 35; transl.). Over a century and many decades with a focus on brain research later, even highly developed countries with elite universities like the USA are plagued by bigger drug problems than ever (e.g. Pieters, [2023](#)).

I have not gone into the wider variety of models to explain substance use and dependence, which include biological (e.g. the neuroadaptive model or that of addiction memory), psychological (e.g. learning theory or stress-coping models) and sociological approaches (e.g. the socioeconomic model; cf. Ullrich, [2018](#)). It should have become clear that addiction and dependence are no natural kinds or “fixed targets,” which leaves plenty of room for pragmatism and social

construction (cf. Hacking, [2007](#); Kendler et al., [2011](#)). Looking back over the past 130 years, it would be naïve to assume that the terms will not change in the future. Biological, psychological and social research could and will continue to inform science—but will need to gain widespread acceptance in the committees of the relevant medical institutions to become widely effective.

Signorelli et al. ([2021](#)) made the designation as an “approach,” “model,” or “theory” of consciousness dependent on its explanatory power—while acknowledging that the concept of explanation in consciousness research is not at all clear. In defining addiction or dependence, the pragmatic context of everyday clinical practice comes in handy: The concept that best predicts, diagnoses and treats problematic substance use or addictive behavior should ultimately prevail. Otherwise, we would have to accept concepts that are in line with the wider epistemic beliefs of the scientific community (such as neuro-reductionism), but which are less useful in practice. Particularly in clinical contexts, practical utility should weigh more than epistemic fashion, for the sake of patients.

Can anything be concluded from this for psychology as a whole? I am not the first to relativize the significance of neuroscience for psychology. For example, in the period around 1900, from which many of the historical sources discussed here originate, the view of mind-body parallelism was widespread in psychology (e.g. Wundt, [1906](#)). And even today there are new old advances such as those of 4E cognition, which is oriented more toward ecological rather than molecular biology (e.g. Fuchs, [2017](#)) or phenomenological psychology (e.g. Wendt, [2024](#)). It is perhaps an advantage that political, financial and moral interests generally play less of a role in psychological science than the health market; however, even in less practice-oriented fields, there are career paths to follow and different schools are fighting for recognition and funding (cf. Lenharo, [2024](#); Schleim, [2014](#)). Academic psychology could orient itself more toward folk psychology to increase its practical relevance. In times of never-ending crises within and outside the discipline (cf. Eronen & Bringmann, [2021](#); Hutmacher &

Franz, [2025](#)), it would perhaps be a good idea to focus more on social problems and needs. After all, virtually all human-made problems involve consciousness and behavior, thus psychology. Otherwise, the society that finances psychological science might one day consider it dispensable. In any case, without agreement on the meaning of concepts, the chances of theoretical integration are poor from the outset.

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Footnotes

¹ In short, the avoidance of all speculative elements in philosophy and science. But when we use language, we must begin somewhere. For a more sophisticated understanding of the term, see Ayer (1959).

² DSM stands for the *Diagnostics and Statistical Manual* of the American Psychiatric Association, which has been available since 2022 in a linguistically revised version of the fifth edition from 2013 (DSM-5-TR).

³ According to the influential psychiatrist Karl Menninger (1893–1990), it was primarily a document intended to standardize the psychiatrists' use of terms, but that "was never popular" (Menninger, [1985](#), p. 474). Psychiatrists quickly worked on an improvement using psychodynamic principles, which ultimately led to the DSM-I after the Second World War. The 1934 classification divided mental disorders into 24 groups, including "5. Alcoholic psychoses," "6. Psychoses due to drugs [...]" with opium and its derivatives as a special case and "other drugs" as a subcategory, and finally a broad category "23. Without psychoses," including "b. Alcoholism" and "c. Drug addiction" (Menninger, [1985](#), pp. 473–474).

⁴ From an embodied-phenomenological perspective, this distinction should be questioned: If one assumes the embodiment of the "psyche," is desire not then also physical and are pain, convulsions and delirium not then also mental? We cannot go into this in depth here, so please refer to the literature on 4E cognition (e.g. Fuchs, [2017](#)).

⁵ Interestingly, this definition of habituation fits relatively well with Rieger's non-pathological preference for a substance and that of addiction with its chronic pathological form (Rieger, [1905](#)). Both forms *could* lead to the acute pathological state, that is, the third form, but do not necessarily have to.

⁶ For psychology, in particular, the relation between folk and scientific psychology would be an interesting topic for a follow-up investigation.

⁷ <https://www.nimh.nih.gov/about/budget>

⁸ Presidential Proclamation 6158 of July 17, 1990, online at: <https://www.govinfo.gov/content/pkg/STATUTE-104/pdf/STATUTE-104-Pg5324.pdf>

⁹ This raises the question of whether this irreducibility is a temporary or a principled phenomenon. Given that the debate about mind-brain reductionism can be traced back to

antiquity (cf. Schleim, [2024](#)), it will probably never be settled once and for all. In my view, the argument from normativity is very strong: If experts decide that a condition should be called, for example, pathological grief if it persists at least *six months* in minors and *one year* in adults, we should not expect to find biomarkers in any strong sense (cf. Schleim, [2023](#)).

[10](#) ICD-11, 06: Mental, behavioral or neurodevelopmental disorders → Disorders due to substance use or addictive behaviors → 6C41 Disorders due to use of cannabis, <https://icd.who.int/browse/2024-01/mms/en#1129015467>

[11](#) Namely, for cannabis, as substance-induced intoxication (6C41.3), delirium (6C41.5) and psychotic disorder (6C41.6).

12. Theory and Practice in Psychology

Maarten Derksen¹ 

(¹) Department of Theory and History of Psychology, University of Groningen, Groningen, The Netherlands

 **Maarten Derksen**

Email: m.derksen@rug.nl

Keywords Theory crisis – Theory and practice in psychology – Bruno Latour – Participatory action research

Introduction: Concerns About Theory and Practice

Around 2019 a spate of articles appeared urging psychologists to attend to the problematic nature of theorizing in psychology.¹ The solution to psychology's problems was not, or not only, to conduct replication studies and test the trustworthiness of earlier results. Even more urgent than replication studies, it was argued, was the improvement of psychological theory. Firstly, the ultimate aim of science is not to discover empirical regularities, but to explain and understand them. Secondly, without a good theory empirical research, including replication, is aimless and uninterpretable. Hypothesis testing requires a theory that implies precise predictions and allows a strong line of derivation from theory to statistical hypothesis. Whereas in the early years of the reform movement attention had focused on the

relation between hypothesis and data/results, increasingly calls were made to shift attention to the relation between theory and hypothesis.

Oberauer and Lewandowsky's ([2019](#)) contribution to this debate illustrates the general tenor of the calls for better theory, but it inadvertently also points towards what is lacking in the debate. Oberauer and Lewandowsky focus on a gap: the gap between theory and data. This gap is ideally bridged by two sets of inferences: from theory to hypothesis and back, and from hypothesis to data and back. At the latter, empirical level, hypotheses about the relation between variables imply data, and vice versa data can support or question hypotheses. This is the level that much of the discussion about problems in psychological research has focussed on. P-hacking, for example, affects the extent to which data can support a hypothesis, and a failure to replicate an effect casts doubt on the empirical generalization about the relation between variables that the hypothesis entails.

There has been less attention, according to Oberauer and Lewandowsky, to the theoretical level. At the theoretical level theories imply hypotheses: we can derive predictions about the relations between variables from the theory. Vice versa, support for a hypothesis at the empirical level can offer support for a theory at the theoretical level. If an empirical generalization appears to hold, we may infer that the theory that predicts that generalization is confirmed. Oberauer and Lewandowsky note however, as others have before them (e.g. Meehl, [1978](#)), that often in psychology a theory merely predicts that a generalization *may* hold, not that it *must* hold. Theories typically predict that effects can happen, not that they will happen. The weak link between theory and hypothesis allows researchers to explain away failures. Confirmations of hypotheses are counted as support for the theory, but failures merely show that this particular way of testing the theory does not work. In psychology, Oberauer and Lewandowsky argue, there is too much exploratory, discovery-oriented research that masquerades as theory-testing research.

Oberauer and Lewandowsky focus on the weakness of the bridge between the theoretical and empirical level, but psychology neither ends with results nor with theory. The field of psychology is not exhaustively described by Oberauer and Lewandowsky's theoretical and empirical level: there is a practical level too. Professional psychologists work for companies and institutions where they apply the insights that were developed by researchers. That, at least, is the idea, but for a long time there have been worries about a gap between research and practice: researchers complain that professional psychologists often seem to ignore scientific evidence, while practitioners bewail the irrelevance of the theories that the researchers produce (Tkachenko et al., [2017](#)). The replication crisis and the theory crisis invite another look at this research-practice gap. How should the gap between theory and practice be bridged? What does the weakness of many psychological theories, that Oberauer and Lewandowsky as well as many others have noted, mean for the application of these theories in the work of, for example, psychotherapists, coaches, and management consultants? And what does the replication crisis imply for the relation between theory and practice?

In this chapter, I focus on theories of behaviour change and their applications. Whereas the research-practice gap is much discussed in clinical psychology (APA Presidential Task Force on Evidence-Based Practice, [2006](#); Willemssen, [2022](#)) and industrial-organizational psychology (Tkachenko et al., [2017](#)), the field of behaviour change seems to be dominated by optimistic statements about its actual and potential practical accomplishments (e.g. Bargh, [2018](#); Thaler & Sunstein, [2021](#)). Discussions in the clinical and industrial-organizational fields focus to a large extent on the question why practitioners do not (or not always) base their work on scientific evidence, but that does not appear to be an issue in behaviour change. In response to the replication crisis, however, a small number of authors have started addressing questions regarding the relation between theory, evidence, and application. I will analyse their proposals using a well-known theory of practice, that of Bruno Latour.

Based originally on his study of Louis Pasteur's development of the anthrax vaccine, Latour's ideas about the relation between science and society became very influential in the study of science and technology. What makes applying these ideas to the question of the research-practice gap in psychology particularly interesting is that according to Latour we should not look at the relation between research and practice in terms of a gap at all.

Extending the Lab

In Latour's view, laboratories are places where the distinction between inside and outside (the lab and the "real world"), which is at the same time a distinction between small-scale work and large-scale processes, is being undone. That is what laboratories are for. Real-world phenomena are brought into the lab and turned into small-scale, visible, and controllable processes among purified entities. Next, in order to "apply" the processes that have been created in the lab to solve the problems of other people, the laboratory is extended into their world: the crucial laboratory conditions are recreated there in order to allow the same control over processes. This is essential: "Scientific facts are like trains, they do not work off their rails" (Latour, [1983](#), p. 155).

Crucial in this process is the work of translation. Real-world problems have to be translated into terms that scientists can work with. Everyday descriptions of situations and their causes must be redescribed in the language of scientific theories. At the same time, the interests of scientists and other actors outside science have to become aligned, so that the solutions that scientists propose are relevant to the people concerned. These translations create a network that connects actors, both human and non-human, along which scientific facts may travel. Only in the context of this network do the products of science have the power to explain and change the world. In this sense, "there is no outside of science" (Latour, [1983](#), p. 167).

Extending the lab requires scripting, prescribing the actions of the people who must work with the technology that science offers (Akrich,

[1992](#)). Latour notes that in the case of Pasteur's anthrax vaccine, Pasteur got involved in several controversies when others could not make his vaccine work. Each time, Pasteur would ask to check that all the proper procedures had been followed, and then the problem would be solved. The results of the laboratory could only be replicated by others if they handled the technology in the right way. Likewise, devices such as washing machines come with manuals that prescribe the proper operating procedures. If these are not followed, the device is not guaranteed to work.

Although Latour puts less emphasis on this aspect than on translation, an important part of extending the lab is to create a sphere of operation for the technology that is sufficiently bounded to ward off disturbing influences. Latour writes that “no one has ever seen a laboratory fact move outside unless the lab is first brought to bear on an ‘outside’ situation and that situation is transformed so that it fits laboratory prescriptions” (Latour, [1983](#), p. 166). Creating a situation that fits laboratory prescriptions will usually require some form of insulation. This was noted long ago by French philosopher of science Gaston Bachelard, who remarked that a device can be described negatively, in terms of the disturbances against which it is shielded, “le technique de son isolement” (Bachelard, [2000](#), p. 222). A device is a closed system. Electronic devices, for example, rely on the insulation of the copper wiring and on the casing for proper operation. The insulation and the casing protect the effect (a current travelling across a wire) from the influence of variables (moisture, other currents) that would moderate the effect or prevent it altogether.

Extending the Psychological Laboratory

In psychology, an argument very similar to Latour's views was made by Richard Henshel ([1980](#)). Henshel rejected the criticism, common during the crisis of confidence in psychology in the 1970s, that psychological experiments create highly artificial situations that for that reason do not translate to real-world applications. Although such

artificiality may be a problem when the experiment is meant to test hypotheses about regularities in the world outside the laboratory, artificial conditions are a great benefit if the aim is to discover new regularities. To apply such findings in the real world the laboratory conditions have to be recreated there. “The theory guiding the experiment can be used to create that result beyond the laboratory walls by deliberately reproducing the strange, hitherto nonexistent conditions in the outer world” (Henshel, [1980](#), p. 475). This, he adds, is “the norm in natural and physical science” (Henshel, [1980](#), p. 475), where many lawful relations are only obtained under artificial circumstances. To apply such findings, one has to “make the external world match the laboratory, not to make the laboratory match the external world”, a lesson, he adds ruefully, that seems lost on psychologists (Henshel, [1980](#), p. 475). Applying psychological results produced in the laboratory (or other artificial research conditions) then becomes a form of engineering.

More recently, a spirited defence of the value of basic research for applied psychology was published by Klaus Fiedler, in his contribution to an edited volume titled “Applications of Social Psychology: How Social Psychology Can Contribute to the Solution of Real-World Problems” (Forgas et al., [2020a](#)). Fiedler ([2020](#)) is robustly positive about the beneficial impact that psychology already has on society, and rejects calls for more field research. On the contrary, research in natural settings has many pitfalls and is often impossible for practical, ethical, or legal reasons. Fiedler draws on a classic paper by Douglas Mook ([1983](#)) in which Mook (who drew among others on Henshel) defended “external invalidity”. Mook ([1983](#), p. 379) argued that a laboratory experiment is meant to uncover a process that *can* happen, not necessarily one that *does* happen in the real world. Experiments are artificial in order to allow control “for extraneous variables and to permit separation of factors that do not come separately in Nature-as-you-find-it” (Mook, [1983](#), p. 384), thus enabling us to discover processes that also operate in the real world.

However, in the introduction to the same volume, Forgas, Fiedler, and Crano also stress that it may be useful to make the experiment more representative of the world to which its results should be relevant. Borrowing Egon Brunswik's concept of "representative research designs" they writes that "in principle, internal and external validity could be simultaneously maximized with experimental designs that rely on the representative sampling of the situations (person, contexts, stimuli, etc.) to which the predictions are expected to generalize" (Forgas et al., [2020b](#), p. 13). Furthermore, basic research has to be followed by "translational research". The road from basic research to successful application is long and the goal is rarely accomplished, but chances of success are increased significantly when one engages in "translational research [...] anchored in a clearly spelled-out theory" (Fiedler, [2020](#), p. 24). For example, insights from basic research into face recognition were further developed in laboratory experiments with identifying suspects in line-ups. The results of those experiments were implemented in the police force in new procedures for suspect line-ups that minimized false positives. In Fiedler's example, the boundary between lab and outside world is successively dissolved, first by turning the lab into a miniature police station where different line-up procedures could be compared, then by implementing the best version in real police stations. This process was helped by the fact that the line-up was always already a procedure that took place in a secluded, controlled environment, and has similarities with the discrimination and recognition tasks that are common in psychological laboratories. The gap between theory and practice, laboratory and police station, was not great to begin with.

An ambitious, far-reaching view of translation and the dissolution of the boundary between lab and wider world was put forward by Michael Hallsworth in his recent "manifesto for applying behavioural science". Responding to scepticism about the efficacy of behavioural interventions, Hallsworth listed improvements that could make behavioural science (even) more successful. His first two proposals are to make behavioural science integral to the operation of organizations

and to stop seeing it as a separate tool. Behavioural science should be “a lens that can be applied to any public or private issue” (Hallsworth, [2023](#), p. 311), and be part of all the processes in an organization. Ultimately, people should become their own behavioural scientists. Hallsworth foresees “a future where policy or product designers act less like (choice) architects and more like facilitators, brokers and partnership builders” (Hallsworth, [2023](#), p. 316). In Hallsworth’s vision, entire organizations are transformed into psychological laboratories peopled by behavioural engineers. Only this all-enveloping extension of the lab will allow the full realization of the potential of behavioural science.

Better Theory

Optimism about the current state of basic research in psychology has been severely dented by the replication crisis. A common response is to call for theory-driven research that specifically focuses on the moderators and boundary conditions of psychological effects in order to make behavioural interventions more effective and efficient. Ap Dijksterhuis responded to the non-replication (e.g. Shanks et al., [2013](#)) of his famous “professor-priming” effect² (Dijksterhuis & van Knippenberg, [1998](#)) with a commentary titled “Welcome back theory!” (Dijksterhuis, [2014](#)). Behavioural priming, he noted, is applied for all sorts of important ends (weight loss, fighting addictions, etc.) and too much scepticism could threaten such “laudable initiatives” (Dijksterhuis, [2014](#), p. 74). All the same, we need to understand better what moderates these priming effects, so that we can explain and predict when they do and do not occur. Theory-driven research is needed according to Dijksterhuis to map the boundary conditions of priming so that these applications may be made more efficient. A similar call, although coming from the kind of critical perspective that Dijksterhuis warned against, can be found in the closing remarks of Szaszi et al.’s ([2018](#)) report of their scoping review of the choice architecture literature. So-called nudges have become a very popular way to steer people’s behaviour in the right direction, but the literature

currently does not offer practitioners the “accompanying instructions for how and when to use the intervention techniques” (Szasz et al., [2018](#), p. 364). What is needed is theory-driven research to uncover the boundary conditions of nudging effects and develop theoretical frameworks that allow precise predictions of when nudges are effective. Four years later Szasz (this time with different co-authors) repeated this call (Szasz et al., [2022](#)). They noted that, despite the enthusiasm about nudges, their reported effects are very heterogeneous, and we therefore need to understand better under what conditions nudge interventions work. Bryan et al. ([2021](#)) recently called for a “heterogeneity revolution”, in which the heterogeneity of treatment effects is turned from a nuisance into a focus of study that can be harnessed for theory development and increasing the effectiveness of interventions. We must better understand the moderation of intervention effects, they urged, so that we can answer the question “For whom, and under what conditions, does an effect appear, and why?” [no page number]. Heterogeneity is also a concern for Hallsworth ([2023](#)), whose manifesto for applying behavioural science calls on behavioural scientists to recognize that interventions may not work, or work equally well, in all settings, and for all groups. Ijzerman et al. ([2020](#)) likewise stress that we must take into account the heterogeneity that is introduced by the different cultural, historical, political, and structural contexts when we apply basic research in applied settings.

Taming heterogeneity by charting the moderators of effects should, in principle, go a long way towards bridging the gap between theory and practice. The theory then better maps the complexity of the world in which it will be applied. In Latour’s terms: a finer, more articulated network has been constructed by drawing in and connecting more actors. Hallsworth and Bryan et al. are among several authors who call for field experiments to achieve this mapping: the world must be turned into a laboratory. According to Hallsworth, heterogeneity in the effects of interventions stems from two factors: differences in the context and differences in the people who are targeted. Behavioural

scientists should therefore “create multi-site studies to systematically study heterogeneity in a wider range of contexts and participants” (Hallsworth, [2023](#), p. 313). Berkman and Wilson ([2021](#)) likewise urge psychologists to leave the confines of the laboratory. They approvingly cite Robert Cialdini’s call to engage with “messy field studies” (Berkman & Wilson, [2021](#), p. 867).

For both Hallsworth and Berkman and Wilson this kind of research, that tries to get a grip on the heterogeneity of effects, must result in theories that are above all practical. For Hallsworth, such practical theories will connect theoretical frameworks such as, in his case, dual-systems theories, with the concrete practice of behavioural interventions, by specifying—among others—when and where a prediction drawn from such frameworks will hold. Hallsworth quotes Berkman & Wilson, according to whom “(a) practical theory is one that suggests actionable steps toward solving a problem that currently exists in a particular context in the real world” (Berkman & Wilson, [2021](#), p. 864). In other words, a practical theory *is* the bridge between theory and practice that successful interventions require.

History

As we have seen, Hallsworth identified two sources of heterogeneity: contextual and individual variability. A third source of complexity emerges when the context of social behaviour is theorized as historically variable. Social psychologists, including Dijksterhuis, have responded to the replication crisis by emphasizing that social behaviour is very sensitive to context, and will change when the context has changed. Given that variability, we simply cannot always expect a direct replication of a study conducted earlier to produce the same results (e.g. Crandall & Sherman, [2016](#); Iso-Ahola, [2017](#); Stroebe & Strack, [2014](#)). An experimental manipulation that worked with a sample of psychology students at a university in the American midwest in the 1980s may not work again with an online sample of MTurk workers in 2024, because their cultural and historical context is very

different. The stimuli used in the manipulation may have acquired a different meaning, for example. What was scary or erotic in 1980 may be merely funny in 2024.

This argument offers a ready explanation for any failure of direct replication in domains of psychology that work with variables that are contextually sensitive. According to these authors, direct replications are therefore of little use in basic research, where conceptual replications are the proper way to test and refine theories about the underlying processes that produce behaviour. They do add, however, that in applied research direct replications are of great importance in order to test whether a treatment or intervention is in fact reliably effective and will repeatedly produce the same outcome (Crandall & Sherman, [2016](#); Stroebe & Strack, [2014](#)).

These authors do not dwell on the topic, but this historicity adds another layer of complexity to the application of psychological theories. Behaviour is situated, these situations are locally and historically variable, and they influence different individuals in different ways. Therefore, theoretically charting the moderators of a particular effect does not make the effect of interventions more predictable, unless the current, local, and individual “value” of these moderating variables has been determined as well. Extending the laboratory and applying the results of basic research is only possible if this variability is somehow taken into account.

Situated Conceptualizations

Thus, the historical, cultural, and individual variability in the determinants of behaviour poses a considerable challenge for practitioners involved in behaviour change. As an illustration of this challenge, consider Lawrence Barsalou’s ([2016](#)) application of his theory of situated conceptualizations to the phenomenon of priming. The theory states that we store our everyday experiences as so-called situated conceptualizations, meaning that we remember situations rather than isolated concepts. When we later encounter a similar

situation, the situated conceptualization becomes active again as a simulation of the earlier experience, a re-enactment of the associated brain states. These simulations act as pattern completion inferences, telling us what is likely to happen in this situation:

By reenacting the original situation, the pattern completion process provides a concrete sense of being in the situation physically, acting in it, and responding to it emotionally. By superimposing the simulated situated conceptualization on the current situation, the pattern completion process guides perception, action, and affective states as the situation unfolds. (Barsalou, [2016](#), p. 7)

This is what happens in priming: a feature of the situation triggers a situated conceptualization, which produces inferences as the brain completes the pattern that is experienced, and the associated ideas, behaviour, and/or emotions follow. But this also explains why priming effects are both abundant and difficult to reproduce. The theory explains why “just about any element of a social situation can prime some other aspect of the situation”, as research has shown according to Barsalou ([2016](#), p. 8). But the theory also implies that the effect of the prime can be modulated by any other element of the situation that was stored in a situated conceptualization. Moreover, different people will have different situated conceptualizations, so their responses to a prime will also vary. “Given the diverse situational experience that people have, both in the past and in the current moment, it seems likely that diverse responses to a prime will occur regularly” (Barsalou, [2016](#), p. 9).

Note that Barsalou’s theory revolves, like Latour’s conception of the application of scientific research, around a distinction between inside and outside, and how this distinction is dissolved. In their minds, people simulate and re-enact experiences they had with the world, with their bodies, and with their own minds. These simulations form the conceptualizations that form our understanding of the world, and they

are situated conceptualizations because we experience the objects in the world as embedded in various contexts. Re-enacting the situated conceptualization then creates the experience of being in the situation. The boundary between inside and outside has been overcome: “the situation is not represented as detached and separate from the conceptualizer” (Barsalou, [2009](#), p. 1283). We represent concepts as situated and represent them by simulating our perceptions of and actions with them, thus putting ourselves in that situation.

Barsalou’s theory implies that priming effects *have to be* difficult to reproduce in direct replications.³ But by the same token they also imply, although Barsalou does not draw this conclusion, that it must be very difficult to apply the insights of priming research in practice. The practitioner will be confronted with the question how this world, teeming with possible behavioural triggers, can be charted. Not only are there many possible primes, what will act as a prime depends on the situated conceptualizations of each individual. How can all these primes be made observable and charted, if everyone lives in their own particular world of primes? To that can be added another question. In the laboratory, researchers have some control over the situation that they present the participant with. The lab is their space. The world beyond the lab however is not at the disposal of the practitioner. How then can one put to practice the effects that were produced in the rarified, controlled atmosphere of the laboratory, when that practice takes place where the practitioner does not have that control?

In Barsalou’s extraordinary reflection on priming and replication, through the lens of his theory of situated conceptualizations, a world appears in which neither people nor the psychologists who study them have much control over the processes of unconscious influence that determine their lives to a great extent. Theory describes the mechanisms behind priming and other forms of unconscious influence, and theory details the largely automatic nature of human behaviour more generally, but it does not seem to offer much in the way of power over these processes, neither to the psychologists nor to their participants. Psychological theories of priming, nudging, and other

automatic aspects of our behaviour seem to have the practitioner in a bind: they explain the causes of problematic behaviour very well (why we stereotype, why we fail at dieting, why we enter into conflicts) but by the same token they seem to make solving such problems very difficult. They picture a world of myriad unconscious influences that not only thwart people's good intentions, but also threaten to scupper the psychologist's intervention at any time.

Situating and Insulating Interventions

Esther Papies ([2017](#)) describes the problem in a paper on situating interventions.⁴ Papies argues that the so-called intention-behaviour gap (not doing what we know is good for us) is the result of the many situational cues that can unconsciously influence our behaviour by triggering habits, impulses, and goals that are at odds with our good intentions. Our behaviour is to a large extent automatic, but our automatisms are extremely context-sensitive. Papies's proposed solution to this problem is to "situate" interventions. Papies uses Barsalou's theory as a general framework for applying any laboratory-generated theories about human behaviour in context. Any intervention must take into account, create, or exploit the situational features that the theory says are connected to the behaviour that is to be produced or prevented. The intervention must be situated, either by including cues for better behaviour in the critical situation ("cueing") or by adapting people's situated conceptualization of those real-world situations ("training"). Situating an intervention thus creates a link between a theory and a problem by "integrating" the intervention into the situation or the situation into the intervention (Papies, [2017](#), p. 5). Situating an intervention means integrating intervention and situation along the lines drawn in Barsalou's theory of situated conceptualizations, drawing theory and practice as closely together as possible. By situating the intervention, the laboratory is extended into a context that the psychologist has created.

The basic problem remains that the number of such environmental influences is very large and variable over people as well as situations. People may encounter different influences in different situations, and to what extent they're influenced by them differs between individuals. The world is full of triggers for bad behaviour, but we are not all triggered by the same things. Some of Papies's strategies aim to attenuate this issue. One of them is "attentional bias modification training", which trains people not to pay attention to the wrong stimuli (Papies, [2017](#), p. 8). Another strategy employs mindfulness training to break the connection between stimulus and behaviour, teaching people to "surf the urge", for example (Keesman et al., [2017](#); Papies, [2017](#), p. 9). Both these strategies basically break or weaken the link between triggers and behaviour, making the number and variety of triggers in the world irrelevant. These strategies isolate the subject, creating an insulated sphere, a mini-laboratory, in which the intervention can take place.

Residue

In Papies's conceptualization of behaviour change, the challenges posed by the context-sensitivity of human behaviour are overcome by situating the intervention and by insulating it from outside interference. Cuing and training make sure that the intervention fits the environment, and further training (attentional bias, mindfulness) helps to make the subject impervious to the wrong kind of influences. Papies presents it as an evidence-based, theory-driven practice. It is an extension of the laboratory into the world of weight loss. Nevertheless, residual challenges remain that cannot be dealt with by situating the intervention. Firstly, Papies acknowledges that interventions may have to be tailored to individuals, adapted to their specific traits. Tailoring, which she sees as complementary to situating an intervention, "requires good insights into the characteristics of an individual" (Papies, [2017](#), p. 11). She does not say where these insights must come from.

Secondly, ideally one would want the effects of the intervention to generalize to other situations than the one they were produced in. Situating an intervention creates a network along which the theorized effect can travel, but the life of a person with weight problems is not restricted to those particular situations. Papies admits that “the reasoning underlying situated interventions suggests that the effects should in principle be limited to situations with critical features like the ones targeted in the intervention” (Papies, [2017](#), p. 12). However, Papies speculates that the subject may learn to generalize the intervention, and further research is needed to study how this learning process can be stimulated.

Other authors have also hinted at skills required for effective behaviour change that go beyond what the laboratory produces. Extending the lab may not, or at least not yet, be enough to create behaviour change. Hallsworth writes that expertise in behaviour change is more than knowing the theories in the abstract, and not enough is known yet about the process of successfully implementing these theories. Behavioural scientists “should therefore learn from practitioners how they adapted specific interventions to new contexts” and strive to “codify and cultivate” these skills (Hallsworth, [2023](#), p. 313). Hallsworth emphasizes, moreover, that behavioural scientists and practitioners must engage with the ethical aspects of their work, recognizing, for example, that behaviour change interventions may increase as well as decrease inequality. Berkman and Wilson likewise point out that people “have been betrayed by scientists in the past” and suggest that “ethical community-based participatory research” may prevent this from happening in the future (Berkman & Wilson, [2021](#), p. 866).

Participatory research, in which interventions are developed in collaboration with the people involved in the issues that require change, takes the extension of the lab and the dissolution of the boundaries between theory and practice a step further than most psychologists are willing to go. Ironically, this approach was pioneered by Kurt Lewin, whose dictum that “there is nothing as practical as a

good theory” is nowadays often cited in support of the view that basic research is essential for successful applications.⁵ But what the various forms of “residue” that I have pointed out in the texts of Papies, Hallsworth, and Berkman and Wilson indicate is that basic research may be essential, but not sufficient for practice. In response to the replication crisis and its implications for psychological practice, a more complex vision of the relation between theory and practice is starting to emerge in the field of behaviour change. Participatory research may be a step too far for most psychologists, but it appears to be increasingly recognized that extending the lab to solve real-world problems is a more complicated process than is suggested by the term “application”. A science that revolves around the situatedness of human behaviour and theorizes those situations and their influence as individually, culturally, and historically variable faces considerable challenges creating effective and transportable techniques of intervention. To make such techniques work reliably in a world of shifting, individually variable influences on human behaviour appears to require skills that go beyond following the procedures dictated by theory. It puts demands on what Kant called “judgment”: the ability to apply general principles to particular cases.⁶ All the more so because some of the principles involved are not scientific but ethical in nature.

It might be worthwhile for behavioural engineers to take note of the discussion taking place in clinical fields about their research-practice gap. A recurring response to the common concern about therapists whose work is not sufficiently evidence-based is precisely to argue that successful psychotherapy requires more than what is currently furnished by scientific research. Jochem Willemsen, for example, has noted that therapists “have to operate in contexts that are characterized by uniqueness, uncertainty, and value conflicts” (Willemsen, [2022](#), p. 293). That work, he argues, requires a different epistemic style—thinking in cases—than the statistical thinking that dominates the psychological laboratory. Thinking in cases foregrounds the uniqueness of each client, their problems and their circumstances, and the ethical issues involved in those problems and the potential treatments for

them. To bridge the gap between research and practice, Willemsen argues, studies of the efficacy of therapies are not enough: they must be complemented with studies of clinical practice in its natural setting that employ qualitative as well as quantitative methods and provide thick descriptions of the therapeutic process.

Conclusion

I have employed Bruno Latour's concept of "extending the lab" to analyse the challenges posed by putting psychological theories in practice and the solutions that have been proposed. I have shown that better theory is not enough to extend the lab and develop successful behaviour technology. The various forms of "residue" that I have pointed out in the texts by Papies, Hallsworth, and Berkman and Wilson all point to skills and practices that appear to go beyond what psychological laboratories currently offer. In fact they appear necessary *because* theory falls short in fully guiding practice. Theories of behaviour change put great emphasis on the context-sensitivity of behaviour and on its individual variability, but they cannot address specific contexts or individuals, nor do they theorize their ethical aspects. Berkman and Wilson mention participatory action research as a way to deal with this problem. In such research, solving a practical problem becomes its own research project, conducted together with the people involved. A metaphorical temporary laboratory is constructed that covers this particular issue and its stakeholders and enables them to collaborate on a solution, creating a connection between theory and practice for this specific case.

Participatory action research remains a rather marginal approach in psychology, although it has been applied in psychotherapy (Becker et al., [2009](#)). More often, it befalls to the practitioner to mediate between the theory and the specifics of a particular case, and to work out how this particular client, with their specific situation, background, and preferences, is best helped. This mediating work of practitioners should get full attention in the efforts to improve the practice of behaviour

change. What is needed for better practice is more than better theories of human behaviour as called for by Oberauer and Lewandowsky and others who have diagnosed a theory crisis. Better practice requires not just theories *for* practice, but also theories *of* practice. Extending the psychological laboratory more successfully than is currently the case requires behavioural psychologists to study what happens when interventions are successful. They need to study the “thinking in cases” that is typical for practitioners who deal with unique individuals and contexts, precisely because their own theories increasingly emphasize individual and contextual heterogeneity. Behavioural engineers would do well to look over the fence and explore the debates in clinical psychology.

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Footnotes

¹ See, for example, the papers collected in a special issue on “Theory in Psychological Science” of *Perspectives on Psychological Science*, 16(4).

² Participants performed better at Trivial Pursuit if they had first imagined themselves as a professor instead of as a hooligan.

³ In fact, it raises the question how it is possible to produce these effects in the first place; Barsalou’s reasoning suggests a file drawer of failed attempts to find statistically significant results in priming experiments.

⁴ Both Fiedler (2020) and Dijksterhuis (2014) mention Papies’s work as an example of successful application of basic psychological theory.

⁵ See Michael Billig’s analysis of this rhetoric (Billig, 2015). Fiedler (2020) uses Lewin’s quote in this way.

⁶ “Urteilstkraft”, see *Kritik der reinen Vernunft* A132-134/B171-174.

13. Psychological Theory, Practice, and Real-Life Circumstances: An Epistemological Analysis of Their Intersections

Nicolò Gaj¹ 

(1) Department of Psychology, Catholic University of the Sacred Heart, Milano, Italy

 Nicolò Gaj

Email: nicolo.gaj@unicatt.it

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Introduction

Modern psychology originated as a theoretical discipline grounded in empirical evidence (Mecacci, [2019](#); Woody & Viney, [2024](#)). Its practical development on the other hand—that is to say, the application of psychological knowledge to practical problems—is relatively recent and was initially driven by societal pressure to address issues arising from the two World Wars and the advancements of industrial society (Thomson, [1968/2013](#)). In the present day, practical or applied psychology has gained significant institutional and social recognition thanks to its effectiveness in addressing real-life issues across diverse

ecological contexts. It therefore deserves consideration from practitioners as well as theorists.

The relationship between theory and practice in psychology is an underexplored and complex area of study, with unique aspects that warrant closer examination. To introduce this topic, it is important to point out two common concerns within the scientific community. First, many scholars have observed a disconnect between theory and practice (Henriques, [2016](#); Sternberg, [2005](#); Yanchar & Slife, [1997](#)): the theoretical dimension tends to evolve rapidly via empirical research and results in the progressive building up of a continuously developing—though not necessarily cohesive—body of knowledge, while practice is often seen as slow to assimilate theoretical research outcomes. According to this view, practice has diverged so far from theory in terms of its content and methods that it appears to have almost decoupled from it (Legrenzi et al., [2017](#)). Similarly, practitioners may perceive theoretical knowledge as largely irrelevant for practical purposes (Hopwood et al., [2025](#); Stewart et al., [2012](#); Wilson et al., [2009](#)).

Second, it is a fact that a plurality of practical psychologies exists. The ever-increasing number of orientations, approaches, techniques, and methods—even within the same subfields—makes identifying the conceptual core of psychology as a practical discipline a challenging task, and this is before tackling the differences among different practical subfields dealing with different kinds of issues and contexts. Such plurality not only pertains to practice, but also involves theory (Sternberg & Grigorenko, [2003](#)) and may be more appropriately labeled as *fragmentation*. For our purposes here, I define fragmentation as a state of relative theoretical and/or methodological isolation, whereby approaches are validated by their own traditions and operate independently from the approaches of other traditions (Gaj, [2016](#)). Thus, we observe both vertical fragmentation (between theory and practice) and horizontal fragmentation (among theories *and* among practices).

From these brief observations, it becomes clear that psychology exhibits a degree of disunity, which in part relates to the disconnect between theory and professional practice and, in part, to the need to adapt practical principles to the ecological settings where they are implemented. Against this backdrop, the aim of this chapter is to outline the key steps in the conceptual journey from theoretical concepts to real-world changes brought about with the aid of psychological tools, with a focus on intervention design and delivery. To this end, I will first clarify the relationship between theory and practice by examining their respective functions, the nature of their two-way connection, and the apparent divergence between them. Next, I will turn to the relationship between psychological practice and real-world situations, which will lead to a discussion of the principles that guide the design of effective practical interventions. My overall goal is to sketch an overarching theory of practice that can “accommodate” the numerous practical approaches that have sprung from different theoretical traditions (e.g., psychodynamic theories, behaviorism, cognitivism), independently of their many possible contexts of application (e.g., clinical settings, organizations, schools). Leveraging principles from the field of design, I will attempt to lay out a theoretical framework that describes the progression from theoretical knowledge to practical know-how and real-life situations. This should enhance our understanding of the foundational principles underpinning professional practice, thus offering a preliminary account of the common conceptual core of all psychological intervention.

From Theory to Practice

First, it is essential to clarify the nature of theory and practice and their interrelationship. Theory is descriptive and explanatory in nature, given that its purpose is to describe and explain phenomena: its goals, therefore, are epistemic.¹ Theoretical work entails formulating and testing hypotheses to attain reliable epistemic outcomes regarding the phenomena under study by means of rigorous scientific tools. The

greater the conceptual and methodological consistency of these tools, the greater the validity and reliability of the resulting knowledge.

Practice, on the other hand, is prescriptive in character, given that its purpose is to bring about or facilitate change in a portion of the world (Corradini, [2017](#)). Its prescriptive nature involves the selection and endorsement of certain values² (arranged in a hierarchy) that can lead to transformative ends in contingent circumstances. In other words, practice requires values in order to be directed toward desirable ends. In general terms, practice is aimed at shifting systems (such as individuals, couples, groups, organizations) from (undesirable) State A to (desirable) State B, within highly constrained environments (namely, the real-life circumstances under which the intervention takes place). In essence, the value-laden principles of practice serve as a guide for designing the sequence of steps required to achieve change in any particular setting, given limited resources.

In view of the above, what is the link between theory and practice? First, theory does not inherently require practice—although the direction of theoretical exploration may be inspired by it—given that the epistemic goal of understanding psychological phenomena can stand on its own. In contrast, practice demands theory, because theory provides the epistemic background for identifying the targets of change and the intervention procedures to be carried out. Therefore, practice is dependent on theory, but not vice versa. However, it should be noted that the outcomes achieved by practice serve to justify both the practical principles that produced these results and the theoretical principles that informed this particular approach to practice. Thus, the outcomes of practice indirectly validate the underlying theoretical framework (see Fig. [13.1](#)) although neither necessarily nor under all conditions. Indeed, the failure of practice to achieve transformative goals cannot be univocally traced back to the underlying theory. Rather, other-than-theoretical factors – related to the contingent conditions of application – may be responsible for such negative outcomes, while the validity of the theoretical counterpart remains intact.³ Here, the expression “conditions of application” refers to the contingent

characteristics of real-life settings in which interventions take place. As Van Bavel and colleagues (2016) argued in addressing the issue of reproducibility, contextual factors play an important role, since their investigation may help clarify the circumstances under which theories hold. In the same vein, factors that turn out to play a (more or less) systematic role in the nexus between practice and theory may be incorporated into the theoretical structure, whose original formulation should then be regarded as partial. This, however, does not affect the justificatory connection between practical outcomes and theory described above. Furthermore, theory and practice are linked to one another via a web of semantic meanings: it follows that a certain theory may inspire more than one practical approach. This last consideration may account—at least in part—for the vast array of psychological practices that exist, as pointed out at the beginning of this chapter.

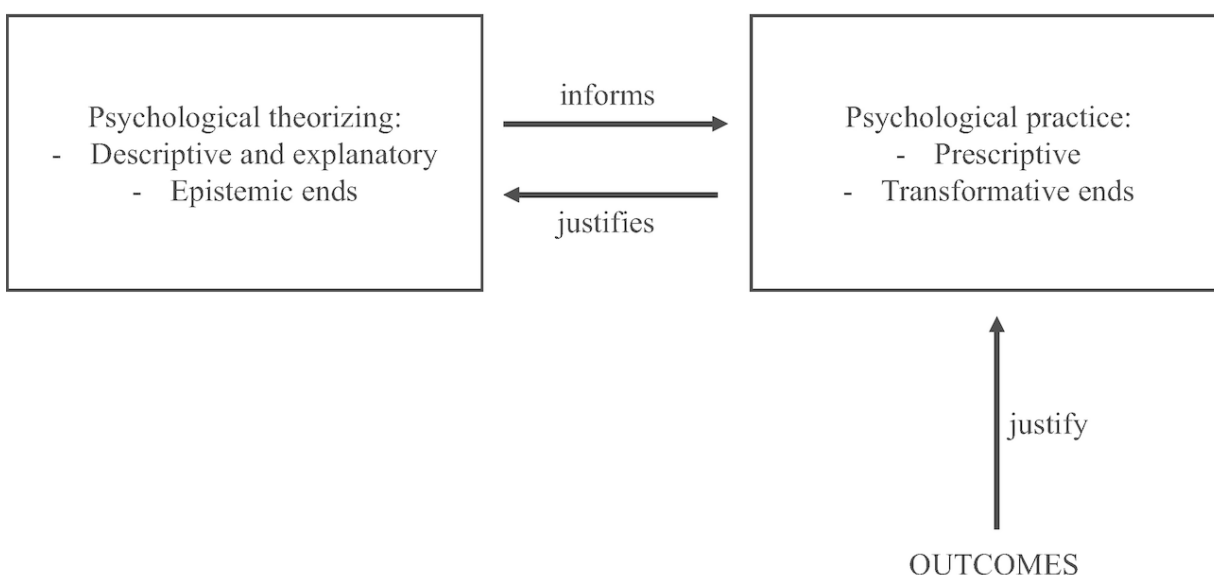


Fig. 13.1 The relationship between theory and practice

Despite these connections, theory and practice appear to have diverged from one another, as briefly outlined earlier. What is the source of this gap between the two? In exploring this issue, I will examine both theory and practice, beginning with theory. The history of psychology teaches us that psychology (as a theoretical endeavor with epistemic goals) has consistently emphasized epistemic reliability,

effectiveness, and reproducibility, in the mold of the exact sciences; this has led to a tendency to adopt methods from the tradition of the natural sciences, viewed as the highest standard of scientific inquiry (Danziger, [1990](#); Leahey, [1992](#); Woody & Viney, [2024](#)). Hence, psychology's approach to phenomena has been heavily influenced by the experimental method, *the scientific dispositive par excellence*, whose main methodological aim is to limit the interference of contextual contingent factors in the relations among the variables under study. This has prompted researchers to break complex phenomena down into simplified, idealized systems of variables. At the same time, it has fostered the large-scale adoption of quantitative methods to provide numerical accounts of phenomena of interest. This methodological stance has prioritized the use of certain methods over investigation of the peculiar features of objects of inquiry: in other words, it has been sought to adapt the object to the characteristics of the method rather than vice versa (Danziger, [1985](#); Machado et al., [2000](#); Mayrhofer & Hutmacher, [2020](#)). The preference for experimental and quantitative methods has gone hand in hand with an emphasis on generalization, in terms of a tendency to study individuals as members of homogeneous classes with shared features. From this generalizing stance, the unique features that differentiate individuals (features that do not fit with certain class partitions) have inevitably been viewed as “noise”, that is to say, as confounding variables to be “filtered out”. In sum, in its efforts to be recognized as a mature science, mainstream⁴ psychology quickly embraced the approaches and methods of the natural sciences, focusing on quantification and generalization, and—following experimental methodology—breaking down its objects of interest into discrete variables.⁵ However, as will soon become evident, this choice has had key implications for the applied domain.

Let us now turn to practice. When taking action aimed at fostering change, practitioners typically focus on the unique aspects of the real-world situations where change is required. This approach prioritizes aligning practical outcomes with specific goals over strictly adhering to reliable and reproducible methods. In striving to attain their

transformative goals, practitioners concentrate their analysis on the specific characteristics of the environments where they operate, seeking to identify their peculiarities rather than their similarities with broad homogeneous categories. This approach is understandable because effecting change *in a specific real-life setting and set of circumstances* requires considering what makes that context unique rather than what makes it similar to other contexts. Accordingly, practitioners methodologically prioritize inquiry into particulars, that is, they tend to focus on what makes an event unique, rather than subsuming it under generalizing principles. Such an approach—in opposition to that of psychological theorizing, which as we have seen prioritizes the identification of general principles—recalls the never fully resolved debate between nomothetic and idiographic stances (see von Wright, [1971](#)) and re-actualizes it within the interplay between theory and practice.

By now, it should be clear that the divergence of theory and practice is more down to theory than it is to practice. More specifically, the responsibility for this bifurcation lies with a particular understanding of psychological theorizing, namely, a naturalistic understanding, according to which only what can be framed and understood via the methods of the natural sciences may aspire to being considered “real” knowledge. Indeed, such an approach to psychological theorizing is associated with another key factor responsible for the divergence of theory from practice: that is to say, a preference for focusing inquiry at the subpersonal level rather than at the personal level.⁶ This is a crucial point, in that this feature severely hinders the formulation of theoretical principles compatible with practice. Why so? Well, inquiry at the personal level takes human beings to be intentional agents, while inquiry at the subpersonal level views its object as especially subject to forces acting below the level of consciousness and intentionality (e.g., material, impersonal, unconscious forces). A personal understanding of psychological issues is key when working with people toward change, in that change may only be facilitated and only occurs in settings where the subject is attributed with intentionality, rationality, and capacity for

free choice. Thus, a personal understanding of human beings offers a robust bridge between theory and practice because it connects them in a way that is both meaningful and consistent (Gaj, [2021](#)), thereby providing valid epistemic grounds for conducting psychological practices. Conversely, by promoting an understanding of human beings based on the categories of the natural sciences, most mainstream psychological outlooks tend to explain human phenomena (i.e., thoughts, actions, desires, beliefs) in terms of subpersonal (e.g., mechanistic, physiological, unconscious) processes that are (completely or partially) *detached* from personal experiences. Such a perspective is unlikely to provide a useful theoretical foundation for practice, because it tends to be largely unrelated (or only loosely related) to the real-life experiences that practice consists of and aims to address.

In support of this hypothesis, let us take a deeper look at the nature of psychological interventions. The clients of any intervention—defined as the end users of psychological services—are referred to the practitioner because of one or more issues. Clients believe (more or less explicitly) that these issues fall within the purview of psychology and, thus, seek expert intervention. Importantly, the issue(s) that motivated the request for intervention is *always* framed by the client(s) in personal terms, that is to say, the clients themselves formulate hypotheses concerning mental distress or requirements for change that are situated at the level of personal interactions. Indeed, lay people ordinarily make use of personalistic attributions (i.e., attributions at the level of the person) to account for and make sense of their own and others' behaviors. In addition, the methods of intervention that practitioners use to facilitate change are also framed in personal terms, with change occurring as an interpersonal outcome. Of course, the issue(s) at stake—although formulated in personal terms—can be influenced by other-than-personal factors, such as subpersonal factors (in terms of events or processes taking place within the individual) or superpersonal factors (in terms of unfavorable social conditions) (Gaj, [2025](#)). However, it should be noted that both subpersonal and superpersonal factors exert their influence via the mediation of the

personal level, that is to say, in the form of *personally experienced* adverse conditions. In other words, both subpersonal and superpersonal factors may play a part in prompting a request for intervention, but only under the condition that their effects are somehow (directly or indirectly) experienced at the personal level. Therefore, the primacy of the personal level remains unchanged: both the proximal determinants giving rise to the consultation and the work toward change occur at this level.

Let us pause to summarize what has been claimed so far. We have seen that theory and practice have different aims and that practice relies on theory as its foundational base of knowledge. Although they are conceptually inseparable, theory and practice currently appear to have diverged quite significantly from one another. The key driver of this bifurcation is a naturalistic interpretation of psychological theory. This disconnect is mainly rooted in the personal/subpersonal distinction: practice is necessarily focused on the personal level of inquiry, while theory, understood in naturalistic terms, tends to prioritize inquiry at the subpersonal level. Thus, prioritizing inquiry at the personal level within psychological theorizing is essential if we are to restore the desirable level of continuity between theory and practice.

From Practice to Real-Life Circumstances

From the above, a key factor in differentiating between theory and practice appears to be the general vs. particular axis. It has been suggested that theorizing mostly addresses the general, while practice focuses on the particular, and that this distinction may be linked to their different purposes. However, upon closer examination, the situation is more complex. Again, let us begin by addressing theory: by definition, theories can be more or less general. With a certain degree of approximation, theories are considered general when they identify a relatively small number of relevant factors to explain a broad class of similar phenomena. For example, according to the theory of free fall, all unsupported objects near the surface of the earth speed up as they fall.

This is true irrespective of the color of an object, how it is made, its shape, and so on. In psychology, according to the theory of operant conditioning, all behaviors that are reinforced are more likely to occur again in the future. This is true irrespective of the circumstances, the type of response, the type of stimulus, and so on. In both examples, the second set of factors is irrelevant to accounting for the phenomenon, whereas the first set of factors is crucial to explaining it. Specifically, in the first example, the object must have mass, must be unsupported, and must be located near the surface of the earth (Kosso, [1992](#)). In the second example, only three aspects must be specified to account for the interaction between any organism and its environment: the situation in dependence of which the response occurs, the response itself, and the resulting reinforcement (Skinner, [1969](#)). Yet not all theories are general: some are specific and focus on a (more or less) small class of phenomena. We may further extend this line of reasoning by considering a scenario where the class is so narrowly defined that it consists of only one instance. In this case, the theory pertains to a single case or, alternatively, a class that numbers one. Another intersection between theory and the particular occurs when a (more or less) general theory is applied to provide a detailed description and explanation of a single case, which serves as an example or prototype of a broader class of similar events. In any case, it is clear that theory intersects with both the general *and* the particular.

What about practice? On the one hand, practice is constituted by prescriptive principles aimed at bringing about transformation, that is, at facilitating change. The formulation of these principles is necessarily general, in the sense that it is *prima facie* independent of the specific contingent circumstances of its application in real-life contexts. Hence, at this level, practical principles are general in character. On the other hand, practical principles are meant to be instantiated and adapted to specific real-life circumstances, which are situated and unique particulars, meaning that they are shaped by the contingent constraints of the setting where the intervention takes place. At this level, practice engages with what have been termed the “ultimate particulars” (Nelson

& Stolterman, [2012](#)), via the creative design of unique (i.e., particular) actions constrained by the contingent conditions defining the setting in which they will be delivered. Therefore, both theory and practice deal with both the general and the particular, as summarized in Table [13.1](#).

Table 13.1 How theory and practice intersect with the general vs. particular axis.

	General	Particular
Theory	Describes and explains a wide range of events by identifying a small number of key properties necessary for membership in the class.	Describes and explains a class with a membership of one or sample cases (i.e., prototypes).
Practice	Formulates general prescriptive principles with transformative ends.	Designs and delivers unique instances of action under contingent constraints.

Given that fostering change entails adapting general prescriptive principles to unique circumstances, practitioners are tasked with applying broad prescriptions (see the lower left box in Table [13.1](#)) to the design of customized services while navigating the constraints imposed by the specific setting in which they operate (see the lower right box in Table [13.1](#)). Despite its importance, the bridge between general prescriptive statements and real-life circumstances has never been addressed systematically within psychology and is often left to the experience, creativity, and expertise of individual practitioners. The insufficient theoretical attention devoted to the process of designing interventions (for notable exceptions, see Lane & Corrie, [2006](#); Polkinghorne, [2004](#); Trierweiler & Stricker, [1998](#)) has gradually led to the development of a form of “expert common sense” or “wisdom” internally to the different theoretical schools or perspectives. Such practical approaches are frequently passed down from one generation of practitioners to the next, often via supervised or hands-on learning. While it is important to acknowledge the enormous value of this received wisdom, what is lacking is a theoretical framework grounded in general principles that all psychologists can relate to, regardless of their particular theoretical perspective or the interests of their objectual domain. Any effort in this direction would enhance

communication among practitioners and encourage reflection on the nature and nuances of psychological intervention.

To address this gap, a provisional framework is provided here that draws on conceptual tools from design science. According to Nelson and Stolterman ([2012](#)), design is a distinctive intellectual tradition of inquiry. Its principles, while independent, complement rather than stand in opposition to those of other traditions such as science or art. In the case in hand, design may usefully be integrated with science, which serves as the foundational theoretical and practical knowledge upon which interventions are designed and delivered. So, how can we specifically define design? For Nelson and Stolterman, when the creation of things is driven by human intentions, design takes place. More exactly, it is “the ability to imagine that-which-does-not-yet-exist, to make it appear in concrete form” (Nelson & Stolterman, [2012](#), p. 12).

Accordingly, when practitioners face the challenge of serving their clients by purposefully designing and delivering an intervention, they are engaging in design. To further illustrate the principles that govern the process of designing and delivering interventions (Table [13.1](#), lower right box), I now propose a modified version of the so-called *Double Diamond design process model*, a dual-sided framework that effectively conceptualizes the key components of the processes we are examining here. The model was first introduced by the British Design Council in 2005,^{[7](#)} but I have drawn more closely here from the modified version developed by Norman ([2013](#)). As illustrated in Fig. [13.2a](#),^{[8](#)} I propose dividing the process of designing and delivering psychological interventions into two distinct phases. Both phases follow a divergence-convergence pattern, which serve, respectively, to thoroughly define the problem and identify the most effective solution. Over the following sections, I will provide a detailed illustration of these phases.

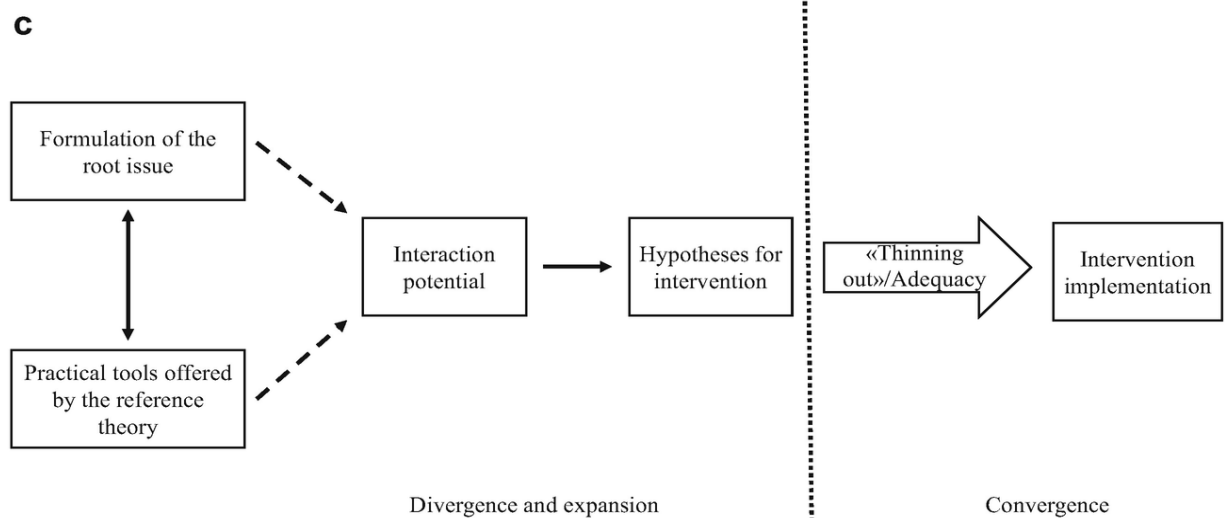
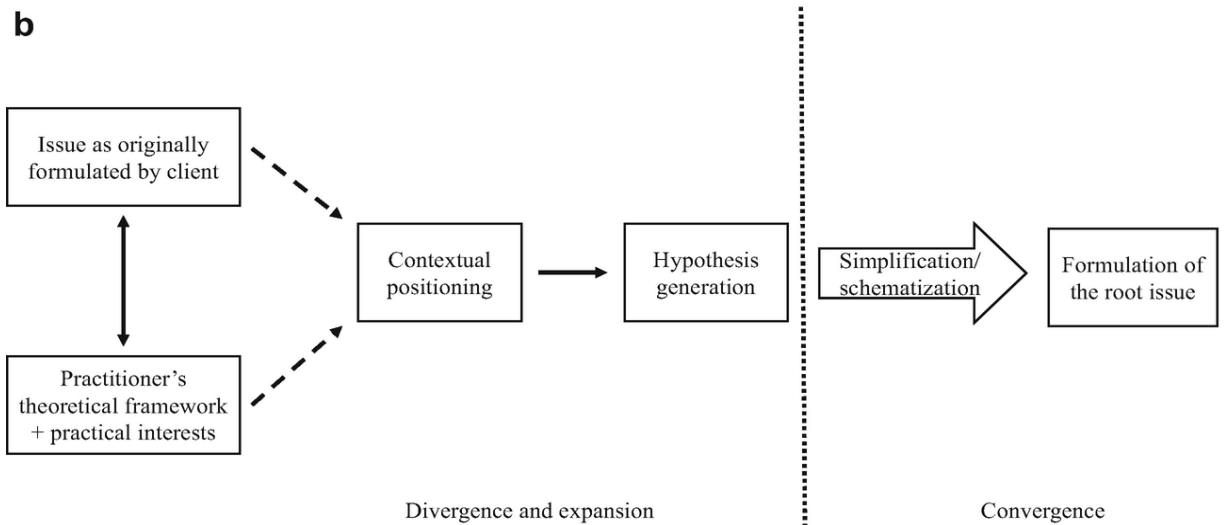
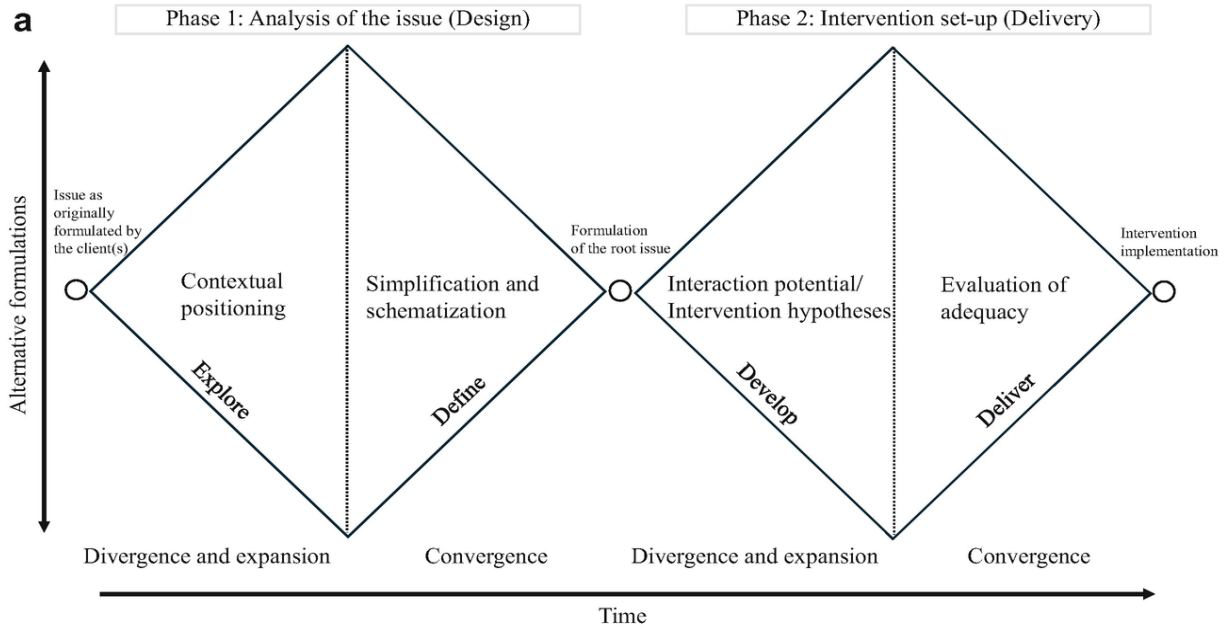


Fig. 13.2 The application of psychological practice to real-world circumstances. Adaptation of the Double Diamond design process to illustrate the two phases in applying psychological practice to real-life circumstances. The overall process is represented in (a), while exploded views of the two individual phases are shown in (b) and (c). First, (b) provides a concise exploded view of the left-hand diamond in (a). The issue as originally formulated by the client is analyzed using the practitioner's theoretical tools. The analysis aims to uncover the client's implicit needs and desires, while situating the issue within a broader semantic network of contextual elements. The outcome is a set of possible hypotheses that converge toward the formulation of the root issue, whose adequacy is both interest- and practice-relative. Next, (c) presents a concise exploded view of the right-hand diamond in (a). Here, the formulation of the root issue, combined with the practical tools available to the practitioner, generates a certain interaction potential. This includes multiple possible forms of intervention (i.e., hypotheses), whose adequacy is evaluated in order to select the mode of intervention most suitable in light of the specific situation

Analysis of the Issue (Design)

Phase 1, represented by the left diamond in Fig. [13.2a](#), is focused on analyzing the issue presented by the client so as to *design* the framework of the intervention and develop a basis for its implementation (see the right diamond in Fig. [13.2a](#)). The issue brought by the client often represents, or is part of, a “wicked problem” (Nelson & Stolterman, [2012](#), p. 16), namely, a problem characterized by complexity and resistance to conventional problem-solving methods. What makes problems wicked? First, they cannot be fully articulated because their intricate nature defies definitive formulation. Second, they cannot be resolved via a simple set of actions or pre-existing solutions. Third, they permit numerous descriptions and explanations, each leading to different repertoires of possible solutions. Accordingly, wicked problems are characterized by open boundaries and, thus, are inherently unique (Dörner & Funke, [2017](#)). In principle, they may be assigned to superordinate categories, but this is more likely to hinder than to help their practical management. They need to be addressed in all their uniqueness. In the case of psychological interventions, they constitute the “raw material” from which to embark upon the design and intervention process.

In psychological settings, a promising approach to tackling wicked problems is to begin with the client's needs and desires. These provide

direction in complex contexts and constrain the range of possible interpretations of the situation. Furthermore, needs and desires help to define the client's ideal vision of the world following the anticipated transformative influence of the psychological intervention. Often stemming from suffering and feelings of alienation, needs and desires can drive transformational change and pave the way for the emergence of new possibilities. In essence, they represent the basic framework of the (wicked) problem presented to the practitioner.

Nonetheless, needs and desires are seldom clear. Practitioners need to listen and observe carefully to grasp what remains unexpressed as much as what is being outwardly expressed. This involves meeting two key requirements for which the practitioner is directly responsible. First, the practitioner must adopt an awareness that is patient and open, rather than selective. Second, the practitioner must establish a balanced power relationship among all participants in the intervention. Both clients and practitioners need to recognize that delivering a psychological intervention demands cooperative interaction, where both parties are fully and authentically engaged in a reciprocal process, with shared responsibility, while maintaining their respective roles and types of expertise.

These requirements must be fulfilled to facilitate the emergence of the *expected unexpected* (Nelson & Stolterman, [2012](#)), that is, the emergence of needs and desires that are either implicitly experienced or not yet within the clients' conscious awareness. The unexpected is nevertheless to some degree also expected, in that clients consulting practitioners often expect to benefit from the latter's specific expertise, which would not otherwise be available to the client. This is a standard feature of most professional activities. Hence, the psychological practitioner should transcend the explicitly stated and facilitate the emergence of what is not, to enhance the client's understanding of the problem and attain a complete picture of the situation.

Thus, practitioners should never exclusively base their intervention on the issue as initially formulated by the client, which should be viewed as a provisional rather than final statement. Before taking

action, practitioners must strive to understand the “basic, fundamental (root) issue (that) needs to be addressed” (Norman, [2013](#), p. 219), the goal of Phase 1. Rather than prematurely converging upon a solution, the task at this early stage is to *diverge* from the original formulation and *expand* the available hypotheses. To be effective, this early phase of diverging and expanding must be complemented by subsequent steps toward decreasing the number of hypotheses and converging toward a single formulation, which is needed to establish a framework for achieving practical goals. Let us now examine the dynamic divergence/convergence process represented by the left diamond in Fig. [13.2a](#) in greater detail.

First, the practitioner develops a thorough understanding of the client(s)’ relational networks, including any personal, interpersonal, cultural, social, and physical constraints impinging upon the context(s) of interest. This will demand time and collaborative effort and may lead the client(s) to feel as though they are “deviating from the core problem”. Nevertheless, it is a crucial step in situating the issue within the set of contextual circumstances where it originated, with a view to shedding light on its underlying meaning.

Second, the practitioner generates provisional hypotheses that link the issue (as originally formulated by the client) with the relational context. More specifically, it is herein assumed that psychological intervention is requested when a gap arises between the demands of one (or more) contexts inhabited by the client(s) and the client(s)’ ability to address these demands. Thus, the presenting issue is understood as a response/reaction to the gap. Hence, the framework proposed here will take into account both the situated etiology of the problem for which psychological intervention has been sought and the meaningful semantic connections between the disruption of the client(s)’ relationship with a portion of their environment and their request for psychological help (Carli & Paniccia, [2003](#); Johnstone et al., [2018](#); United Nations Human Rights Council [UNHRC], [2020](#)). Accordingly, the request for intervention is conjectured to have been driven by relational imbalances caused by events or processes that

have had the effect of unsettling a party (an individual, a group, an organization, and so on) to an otherwise non-problematic relationship, within a specific ecological context. At this stage, several hypothetical semantic connections between the issue and the context are established.

Third, the practitioner must progressively select from among the working hypotheses so as to converge upon the most appropriate. Elements are intentionally simplified, schematized, ignored, or emphasized with a view to coming up with an adequate, albeit partial, formulation. A working hypothesis is viewed as adequate if it captures pertinent connections between the issue brought by the client and the salient features of the context. This task is inherently interest-relative and as such will be markedly influenced by the theoretical and practical framework adopted by the practitioner, as well as by the meanings negotiated with the clients. It follows that outcomes will vary as a function of the individual practitioner's theoretical perspective and practical approach.

The final step is the formulation of the root issue, which may be defined as an improved (i.e., reformulated) version of the issue as originally presented by the client. It includes elements from the client's implicit dimension and is influenced by the practitioner's theoretical interests and transformative values. Specifically, identifying the root issue involves *formulating* the issue in a way that (1) defines what constitutes the foreground and the background by purposefully "isolating" the salient elements; (2) interprets reality with the aim of envisioning possible futures and facilitating change; (3) holds local value, meaning that its heuristic value is limited to the specific context in question. Figure [13.2b](#) offers a synthetic illustration of these steps.

Let us attempt to apply these notions to a simple practical example. Imagine two parents consulting a psychologist for their autistic child. Whatever issue they present, the psychologist faces a wicked problem related to the clients' (implicit as well as explicitly stated) needs and desires. After establishing multiple semantic links between the issue and elements of the broader context, the practitioner will converge

upon a selected, simplified, interest-relative formulation of the root issue, which takes into account the implicit dimension (parents' and child's expectations, parents' and child's representations of the problem, client's ambivalent feelings about the intervention, and so on), and the contextual positioning (e.g., relationships within the family, the relationship with the school, the child's peer relationships, and so on). Evidently, this formulation will influence the range of possible interventions. In other words, different formulations will lead to different proposed interventions within differently framed settings (e.g., individual support to the child, family therapy, a home-based intervention, and so on) and with different prospects of change.

Setting up the Intervention (Delivery)

The above makes clear that the formulation of the root issue is not only the goal of Phase 1 but rather also the starting point of Phase 2. The latter phase is devoted to the setting up and *delivery* of the intervention: in other words, it entails identifying and selecting from among the practical implications of the root issue to implement a customized intervention.

Phase 2 is synthetically summed up in the right-hand diamond in Fig. [13.2a](#). Its starting point is the formulation of the root issue. This formulation is informed by a partial framing of the problem, hence it is perspectival, in the sense that it reflects the constraints and limitations (e.g., practical interests, values, theoretical outlook, negotiated goals) of the perspective from which it has been made (Giere, [2006](#); Osbeck, [2019](#)). This perspectival, situated knowledge reveals specific possibilities for action on the part of practitioners, enabling various potential intervention scenarios within the limits of the theoretical framework being adopted. In other words, it provides and encourages certain sets of competent actions (practical strategies supported by the theory of reference) while excluding or discouraging others. For practitioners, these actions represent opportunities to develop

strategies for delivering interventions. For clients, this repertoire constitutes a set of opportunities, or affordances, for change.

Given its salience to our topic, the notion of affordance deserves to be more fully introduced so as to set the stage for the rest of the chapter. This concept was first introduced by James J. Gibson to describe the relationship between organisms and their environments (Gibson, [1979](#)). Specifically, affordances refer to a complementarity between the animal and what the environment offers, both in terms of perception and possible actions (Burlamaqui & Dong, [2015](#); Chong & Proctor, [2020](#)). Hence, affordances are relational properties, in that they concern the interaction between specific features of the environment and the agent (Barsingerhorn et al., [2012](#)).

Since it was first proposed, the concept of affordance has evolved and there are currently many different definitions of it (see Burlamaqui & Dong, [2015](#); Chong & Proctor, [2020](#); Dohn, [2009](#)). In design science, the concept has garnered much attention for its ability to capture key aspects of the relationship between “the properties of an object and the capabilities of the agent that determine just how the object could possibly be used” (Norman, [2013](#), p. 11). In this specific field, affordances may be understood as cues that invite agents to act upon an artifact according to its designed function (Burlamaqui & Dong, [2015](#)), the latter being jointly determined by the features of the objects and the abilities of the interacting agent. It follows that in the field of design, social and cultural experiences play an especially important role: people learn to *see* and *use* affordances based on their store of previous knowledge. Thus, on the one hand, meaning resides in the environment, that is, in the opportunities offered. However, meaning is necessarily *for* someone: not in the sense that someone must be aware of the meaning here and now, but in the sense that meaning is also given by what actions someone is able to perform. Accordingly, the notion of affordance may be further understood as relational, cultural- and skill-relative (Dohn, [2009](#)).

After this brief digression, let us return to our core topic. Based on our discussion to date, the repertoire of competent actions available to

the practitioner is linked to the affordances to be made available to the client. In this context, affordances may be viewed as relational properties that define the intervention setting, encompassing all interactional, social, symbolic, cultural, and physical stimuli that form the environment in which the psychological intervention is delivered (see Tomej & Xiang, [2020](#)). These affordances offered to the client are shaped by the perspective established through the formulation of the root issue as well as by the practical tools offered by the chosen theoretical framework. In other words, the resulting sets of affordances may be viewed as the outcome of the interaction between the formulation of the root issue *and* the practical tools at the practitioner's disposal. Overall, these affordances constitute an "interaction potential" (Dohn, [2009](#), p. 154), which is a collection of affordances that enables a range of possible actions and opens up various intervention possibilities. Thus, during the divergence/expansion phase of the second diamond, the practitioner's goal is to arrange and manage the controllable elements of the setting to provide as many affordances for change as possible. The left side of Fig. [13.2c](#) provides a concise illustration of these steps.

Other than the constraints imposed by the chosen theoretical perspective, the interaction potential is, ideally speaking, not bound by any of the other constraints characterizing the intervention setting. That is why it must be "thinned out" in light of all the remaining contextual variables. Indeed, the need to identify the path and methods to achieve change within a specific real-world environment requires the progressive selection and evaluation of the appropriateness of these hypotheses.

The first crucial limiting factor is the goal of the intervention. The formulation of this goal is partially informed by the root issue, which suggests a range of possible avenues for change. However, other constraints must also be considered, such as the characteristics of the setting, the client(s), and the practitioner. Consequently, affordances will be constrained based on these contextual factors (see Burlamaqui & Dong, [2015](#)). In addition to considering these constraining factors,

the path to service delivery involves evaluating the adequacy of intervention hypotheses, understood as structured setups of affordances, in terms of their suitability within a constrained scenario. Drawing from Wendy Parker's (2020) discussion on model evaluation,⁹ adequacy is first assessed in terms of *reliability in a type of use*, or context of use. The practitioner might ask: Have cases similar to the one currently under analysis (as laid out in the formulation of the root issue) been successfully addressed before using a certain intervention hypothesis? Here, the evaluation is focused on how well the hypothesis fits a certain *type* of context with a specific *type* of client, one or a set of practical tools, and a given range of circumstances. This could be viewed as a coarse-grained evaluation of the adequacy of the intervention hypothesis. Second, adequacy is assessed in terms of the degree of success obtained in a *particular instance (i.e., token) of use*. This represents the final step in the process and roughly corresponds to the delivery of the service. It evaluates how well a specific intervention hypothesis achieves the practical goal in a specific instance of use, constituting the more fine-grained evaluation of the adequacy of the intervention hypothesis. This second definition of adequacy is conceptually derived from the first and may be assessed by simply waiting to see whether the goal is achieved in that given instance. The right-hand side of Fig. 13.2c illustrates these steps.

Let us now summarize the entire process shown in Fig. 13.2a, using the previous example. The formulation of the root issue, combined with the practical framework offered by the reference theory, generates a dense interaction potential that, in turn, is associated with many intervention hypotheses (i.e., opportunities for competent actions). These hypotheses are then narrowed down by considering constraints that the intervention hypotheses must accommodate: Are there sufficient economic resources available? Are the parents willing to participate in the delivery of the intervention for their child's benefit? How will the practitioner handle the expectations of both the child and the parents? Such questions are crucial when thinning out the hypotheses. Next, the practitioner assesses the appropriateness of the

remaining hypotheses in a *type* of context, mainly by evaluating how well the intervention hypothesis(es) aligns with successful interventions in similar scenarios and asking what aspects make these other scenarios comparable. Then, the practitioner will evaluate the adequacy of these hypotheses in this *specific instance*. Here, the focus is on what makes the circumstances unique: the unique features of the setting, the specific characteristics of the client(s), the practitioner, and the background conditions under which the intervention is being implemented.

One final, yet significant consideration: the processes represented by the two diamonds should be viewed as an iterative cycle that is highly responsive to feedback collected in the setting. Designing and implementing a psychological intervention is not an isolated endeavor; rather, it involves delivering a service that relies on dynamic cooperation and interaction. Accordingly, the outcome of this process will be subjected to continuous fine-tuning throughout its implementation and should not be considered complete until the intervention itself has been fully concluded.

Conclusion

Based on the above, it is clear that the design approach (as shown in Table [13.1](#), lower right-hand box) may serve as a crucial hinge point between psychological practice and real-life situations, as illustrated in Fig. [13.3](#), which complements Fig. [13.1](#). This approach aims to offer conceptual tools for bridging the gap between the generality of scientific knowledge and the uniqueness of real-world contexts.

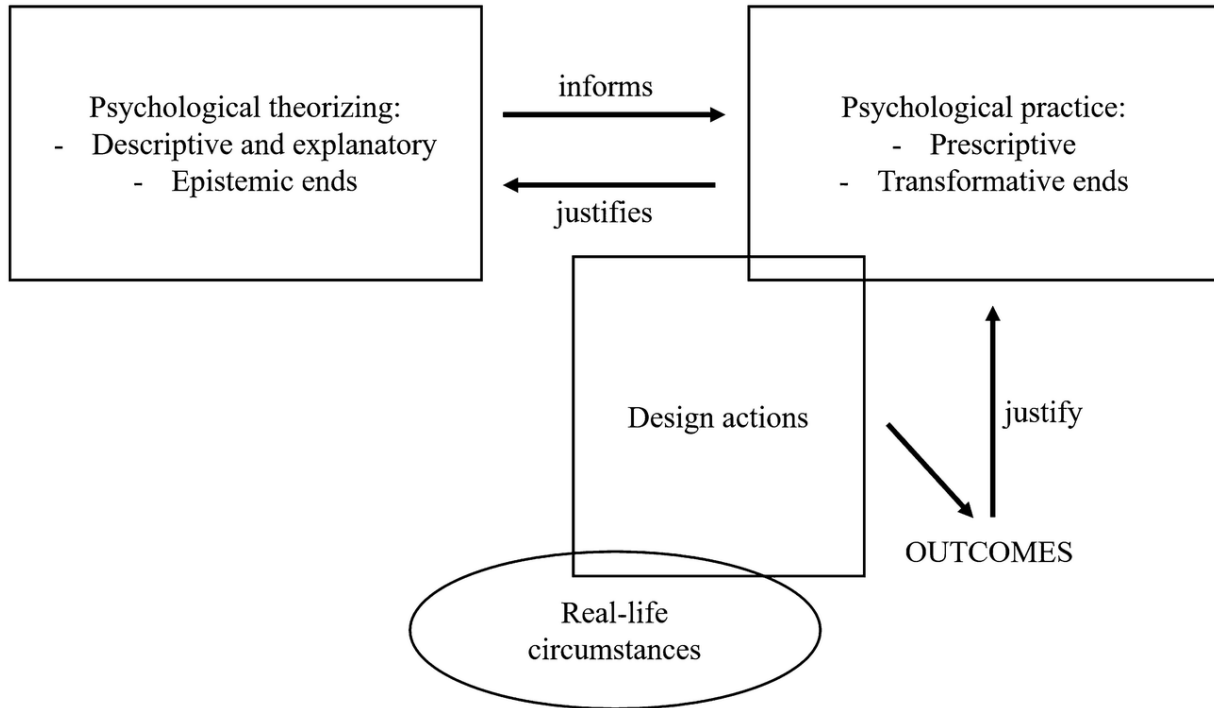


Fig. 13.3 The relationship between theory, practice, and real-life circumstances

The framework outlined makes it clear that the essence of any theory of psychological intervention is *personal* and *relational*. These features surely play a role in generating the plurality inherent in the practical dimension of psychology. However, they also unveil important aspects that can advance psychology both theoretically and practically. On the one hand, the personal dimension provides direction for the development of psychological theorizing, now informed by the requirements of practice. On the other hand, the relational dimension is key to the design of any intervention: not merely in the obvious sense that interventions require human interaction but rather in the more crucial sense that the core focus of any psychological intervention is on *the relationship* between, on the one hand, the client(s) and their context(s) and, on the other hand, between the practitioner and the client(s). These inherently situated aspects contribute to the complexity of the topic, making it challenging to encapsulate within a broad conceptual framework that successfully captures the main aspects of professional practice. This chapter aims to contribute to the endeavor by fostering reflection on these critical issues among both

practitioners and scientists and stimulating debate around the guiding principles of psychological practice.

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Footnotes

¹ Although there is clearly a significant difference between describing and explaining a phenomenon, this distinction is not relevant for present purposes.

² This set of values comprises *both* values embedded in the adopted theory and its corresponding theory of practice *and* values negotiated and endorsed by the parties in the contingent situation bearing upon the intervention. I will return to this point later.

³ Here, the expression “conditions of application” refers to the contingent characteristics of real-life settings in which interventions take place. As Van Bavel and colleagues (2016) argued in addressing the issue of reproducibility, contextual factors play an important role, since their investigation may help clarify the circumstances under which theories hold. In the same vein, factors that turn out to play a (more or less) systematic role in the nexus between practice and theory may be incorporated into the theoretical structure, whose original formulation should then be regarded as partial. This, however, does not affect the justificatory connection between practical outcomes and theory described above.

⁴ It is worth noting that many “off-stream” voices have been raised in the recent past (see, e.g., Malich & Rehmann-Sutter, [2022](#); Teo, [2017](#)) laying claim to the need for pluralism in psychology.

⁵ According to Danziger ([1985](#)), because these variables are required to undergo numerical manipulation, they are defined as independent of one another and assumed to remain the same regardless of varying conditions. It is clear that such assumptions are not neutral when framing mental functioning.

⁶ The personal level of inquiry involves attributing psychological states or processes (e.g., thoughts, beliefs, intentions, and desires) to oneself or other people, with a view to accounting for their behaviors. It is the common, everyday way of framing events of psychological salience. The subpersonal level, on the other hand, adopts constructs below the level of the person, reducing psychological states or processes to the workings of *parts* of the person (Bermudez, [2005](#)).

⁷ See <https://www.designcouncil.org.uk/our-resources/the-double-diamond/>

⁸ This and the following figure panels (2b and 2c) should be understood as design schemas, as described by Nelson and Stolterman ([2012](#), pp. 7–8). Rather than exactly representing how things really are or should be, they serve to describe aspects of ideal solutions from a range of possibilities. In other words, they are strategies or tactics for design inquiry that offer guidance on how to organize and make sense of infinitely complex information, with the goal of supporting design inquiry and effective action.

⁹ Given that our focus here is on professional practice, the proposed evaluation process is more informal than the evaluation of scientific models proposed by Parker. However, the author herself seems to suggest that the usefulness of her approach may extend beyond scientific models, to the point of calling for the two definitions of adequacy to be applied to “other tools as well” (Parker, [2020](#), p. 461).

Part III

Possible Solutions for Theory and Model Building

14. Mathematical Models in Theoretical Psychophysics: A Historical Perspective

Ruben Ellinghaus¹  and Rolf Ulrich² 

- (1) Department of Psychology, General Psychology: Judgment, Decision, and Action, FernUniversität in Hagen, Hagen, Germany
(2) Department of Psychology, Cognition and Perception, University of Tübingen, Tübingen, Germany

 **Ruben Ellinghaus (Corresponding author)**

Email: ruben.ellinghaus@fernuni-hagen.de

 **Rolf Ulrich**

Email: rolf.ulrich@uni-tuebingen.de

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Introduction

The application of experimental methods to questions that had traditionally been within the realm of philosophy marked a groundbreaking step in the development of psychology. For example, although the general question of how knowledge of the physical world is mediated through the senses has been explored in philosophy since Aristotle (e.g., Marks, [1978](#)), genuinely experimental inquiries into

human perceptual abilities did not begin until the nineteenth century (Boring, [1942](#)). Although Wundt, with good reason, is often credited as the founding father of psychology for establishing the first psychological laboratory in 1879, this article aims to highlight the fundamental importance of the work of Helmholtz and Fechner. As argued by Boring ([1950](#)), the contributions of these two authors may be even more essential to the foundation of experimental psychology than those of Wundt, particularly in the context of human perception (cf. Mausfeld, [1994](#); however, see Fahrenberg, [2015](#), for a different perspective).

In this article, we argue that classical psychophysics, in the tradition of Fechner, was groundbreaking in the discovery of psychophysical laws and the development of methods for quantifying perceptual performance. However, we argue that this research tradition was theoretically limited in understanding the cognitive mechanisms that constitute the perceptual abilities it aimed to measure (e.g., stimulus discrimination). Conversely, we believe that a research tradition paved the way for significant theoretical advancement in psychophysics—pioneered by Thurstone ([1927a](#), [1927b](#))—that rigorously underscored the mathematical specification of non-observable entities. This particular emphasis on non-observables and mathematical rigor, in turn, echoes a central meta-theoretical conviction laid out by Helmholtz ([1867](#)). We illustrate how the Helmholtzian approach has led to substantial theoretical progress concerning perceptual abilities beyond mere stimulus discrimination, such as color vision. Finally, we discuss the broader contribution of this abstract and mechanistic tradition in cognitive psychology.

Empirical Laws in Classic Psychophysics

The determination of the law by which the discrimination sensitivity depends on the magnitude of the stimuli, [...], is one of the most important and consequential tasks of outer psychophysics.—Gustav T. Fechner ([1860](#), p. 48)

As Fechner envisioned, the primary goal of psychophysics was to establish a research program that uncovered the lawful relationships between physically defined aspects of sensory stimuli and our perceptions of these stimuli. To better understand Fechner's vision, as articulated in the initial quote, it might be worthwhile first to examine the concept of scientific laws more generally. Although there is no consensus regarding the definition and the role of laws in science (e.g., Cartwright, [1983](#)), consulting Carnap ([1995](#), p. 3) as a prominent authority, we find that "the observations we make in everyday life as well as the more systematic observations of science reveal certain repetitions or regularities in the world. [...] The laws of science are nothing more than statements expressing these regularities as precisely as possible."

The first seminal contributions to the literature that employed ideas about scientific laws aligning with modern conceptualizations presumably originated in the seventeenth century (Zilsel, [1942](#)). For example, Galilei's law of free fall, stating that all freely falling objects (without resistance) accelerate at the same rate, can be expressed as

$$\frac{v(t)}{t} = g \quad (14.1)$$

where $v(t)$ reflects the velocity given at time t and g reflects the gravitational constant (about 9.81 m/s^2 on planet Earth). In Fechner's time, formal laws had played a crucial role in astronomy and physics for some time, making it revolutionary to consider psychological research in a similar light. As a broadly and highly educated scholar in both philosophy and science, Fechner followed in the tradition of Leibniz and Descartes, envisioning a *Mathesis Universalis* (Feuerhahn, [2004](#); Mausfeld, [1994](#); Mittelstrass, [1979](#)), where science ultimately aims to subsume phenomena from various fields of inquiry under universal mathematical principles. For Fechner, the foundation for his research program was established by the physiologist Ernst Heinrich Weber, whom Fechner himself referred to as "the father of psychophysics" (Fechner, [1860](#), p. VIII). Weber ([1846](#), [1905](#)) introduced the concept of the difference limen (DL), which later became widely recognized as the just noticeable difference (JND). The DL represents the minimal difference in physical magnitude between two stimuli that allows for

reliable discrimination. For the development of Fechnerian psychophysics, Weber's most groundbreaking observation was that the *DL* turned out to be a constant fraction of the referent magnitude. For example, he observed that if a 10.3-ounce weight could just be distinguished from a 10-ounce weight, a 10.3-gram weight could just be distinguished from a 10-gram weight. Put differently, Weber measured the *DL* for weight discrimination to equal 3% of the referent stimulus. By similar experimentation with line length and tone frequency, he observed that the *DL* also increased proportionally to the magnitude of the reference stimulus for these stimulus features. For Weber, these results supported the general view that humans perceive the relation between magnitudes irrespective of the absolute magnitudes in many cases (Weber, [1905](#), p. 117).

Fechner's Search for the Psychophysical Law

Crucially, Weber never formulated his insight about the relative constancy of the *DL* in the form of a mathematical law. Rather, the corresponding formalism was given by Fechner as

$$\frac{DL}{S} = k \quad (14.2)$$

where *DL* refers to the Difference Limen, *S* reflects the magnitude of the referent stimulus, and *k* is the Weber constant (roughly 0.03 for weight discrimination, given in the same unit as the *DL*, for example, kg or ounces).

Equation ([14.2](#)) is commonly known as Weber's law. It is crucial to recognize that this law is not truly psychophysical in the strict sense of Fechner's perspective but is instead physical; all variables constituting the law are defined by physical measurement, and therefore, the law does not directly connect any physical to any psychological magnitude (cf. Algom, [2021](#)). Fechner, however, believed that a genuine psychophysical law could be derived from Weber's law with some additional steps. To achieve this, he introduced a new variable, Ψ , representing the magnitude of subjective sensation arranged along an interval scale. Thus, he proposed that the numerical distance between any two adjacent values of Ψ is equal across all levels of the scale:

$$\Delta\Psi = m\left(\frac{\Delta S}{S}\right) \quad (14.3)$$

where m represents a proportionality constant. With $\Delta S = DL$, Eqn. (14.3) postulates that the minimally perceivable increase in physical magnitude feels subjectively equal at all intensities. After combining Eqn. (14.3) with Eqn. (14.2), Fechner finally arrived at the formula well known as the Weber-Fechner law, sometimes simply called Fechner's law:

$$\Psi(S) = m \cdot \ln\left(\frac{S}{S_0}\right) \quad (14.4)$$

expressing that sensation magnitude Ψ increases logarithmically with physical magnitude S , while S_0 represents the absolute threshold (e.g., the minimally required luminance of a light patch to cause a sensation of brightness reliably).

The Difference Limen: A Quantitative Measure of Discrimination Performance

So far, we have broadly outlined the historical origins of psychophysics as pioneered by Weber and Fechner. However, it is important to recognize that Fechner's empirical methodology is at least as significant a contribution as his theoretical deduction of the psychophysical law. While Eqn. (14.4) is commonly known as the Weber-Fechner law or simply Fechner's law (Lewis, 1960), Fechner himself referred to this equation as *Massformel* (measurement formula) (Algom, 2021; Boring, 1942). For Fechner, the logarithmic law emphasized which quantities needed to be measured in the first place, so that the proposed law describing the relationship between stimulus and sensation could eventually be defined.

Indeed, Fechner's dedication to an exact empirical estimation of the quantities defined by the *Massformel*, under strict laboratory conditions, is evident in the numerous methods of threshold estimation which are discussed in his *Elements* (Fechner, 1860). Many of these methods are still being used in modern-day psychophysics, such as a procedure termed as *Methode der richtigen und falschen Fälle* (*method of right and wrong cases*). The most prominent variant of this procedure

is the two-alternative forced-choice task (2AFC), originally developed by Hegelmaier (1852), who was a student of Karl von Vierordt in Tübingen (cf. Laming & Laming, 1992). In this task, a standard s with constant magnitude and a comparison c varying on a certain physical quantity (e.g., weight) are presented successively on each experiment trial. After each trial, participants are instructed to indicate whether the first or second stimulus had a higher magnitude (e.g., which one was heavier). In practice, DL can be measured by fitting a *psychometric function* to the data (Gescheider, 1997), a procedure already foreshadowed by Fechner (Link, 1994), but not fully developed into its current standard form until Urban (1910). This psychometric function plots the probability that c is judged to be larger than s on the y-axis as a function of the physical magnitude of c on the x-axis. The psychometric functions increase monotonically from 0 to 1 as the value of c increases (Fig. 14.1). Importantly, DL is captured by the slope of the psychometric function. That is, the steeper the psychometric function, the smaller the DL and hence the higher the participant's sensitivity. By convention (Gescheider, 1997), half the distance between the levels of c corresponding to the 0.25 and 0.75 probabilities (i.e., half the interquartile range) defines DL .

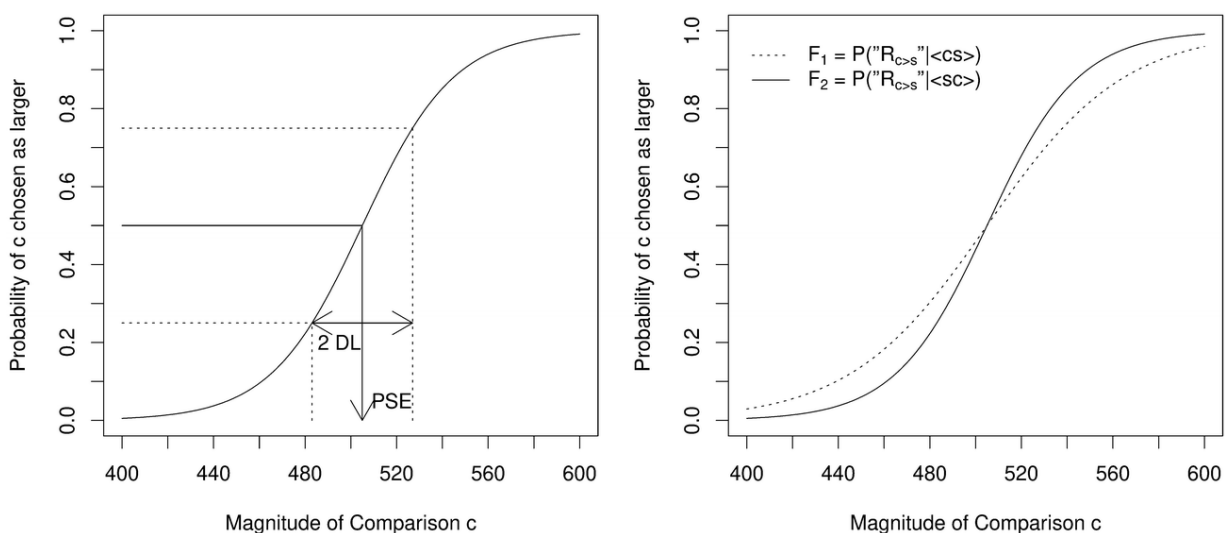


Fig. 14.1 Psychometric functions of a 2AFC duration discrimination experiment. Left Plot: Hypothetical logistic psychometric function of a 2AFC duration discrimination experiment with a standard s of 500 msec. The location of the function defines the PSE , or point of subjective

equality. Half the distance between the levels of c , respectively, corresponding to $c_{25\%}$ and $c_{75\%}$ (i.e., half the interquartile range) defines DL . Hence, DL is a measure of sensitivity reflected in the steepness of the psychometric function. **Right Plot:** Hypothetical order-dependent logistic psychometric functions $F_1 = P(R_c > s | \langle cs \rangle)$ and $F_2 = P(R_c > s | \langle sc \rangle)$ of a 2AFC duration discrimination experiment with a standard s of 500 msec. Note that both functions have identical locations corresponding to an identical PSE for both stimulus orders. However, F_2 is steeper than F_1 , implying a larger DL for $\langle cs \rangle$ trials than for $\langle sc \rangle$ trials and hence exhibiting a negative TBE

Laws and Explanations: The Role of Mathematical Theory in Science

It was mathematics, the non-empirical science par excellence, wherein the mind appears to play only with itself, that turned out to be the science of sciences, delivering the key to those laws of nature and the universe that are concealed by appearances.—
Hannah Arendt (*The Life of the Mind*, [1978](#), p. 7)

As noted above, scientific laws, such as Galileo's law of free fall or Weber's law, are mathematically precise descriptions of empirically observed regularities. However, these laws are not necessarily explanatory in the sense of specifying the underlying causes of the observed regularities in question. For example, according to Bechtel and Wright ([2019](#)), the established psychophysical laws—while elegantly describing the relationship between physical quantities and mental phenomena—remain unexplained themselves. Put differently, empirical laws answer the question *what* will be observed under specific circumstances, but leave open the question *why* this particular observation will unfold.

Prominent philosophers such as Carnap ([1995](#)) and Popper ([1959](#)), although differing in their epistemological frameworks, both acknowledged that references to non-observable entities and principles contribute to scientific explanation. For instance, a theoretical account for Galilei's law is typically given by pointing to the laws of Newtonian mechanics, according to which gravitation exerts a constant force on an object, thus causing a constant acceleration rate (Hrabovsky &

Susskind, [2020](#)). In detail, Newton's second law of mechanics can be stated as

$$F_a = \frac{dp}{dt} = m \cdot \frac{dv}{dt} = m \cdot a \quad (14.5)$$

where $p = m \cdot v$ denotes the momentum of a body in motion with acceleration a . Since a equals g near the Earth's surface, after integrating both sides of the latter two terms, Eqn. ([14.1](#)) directly follows.

By the deductive power of formal logic, scientific theories allow researchers to derive testable predictions from general laws and initial conditions—a view famously formulated in the deductive-nomological model of explanation (Hempel & Oppenheim, [1948](#)). Importantly, such predictions are often informative when they are *dis-confirmed* rather than confirmed by experiments (Lakatos, [1978](#); Popper, [1959](#); Popper, [1963](#)). Deviations from the predictions of established theories are often regarded to constitute *empirical anomalies* (Kuhn, [1970](#)). According to Kuhn ([1970](#)), one rather natural way for scientists to react to such anomalies is to doubt them, as, for example, when one attributes them to problems of measurement. However, in case empirical anomalies can be convincingly demonstrated with robust evidential value, constructive turns in science are sometimes made by modifying established theories (Lakatos, [1978](#)). For instance, empirical anomalies that challenged Newtonian mechanics consisted in experiments demonstrating that the definition of momentum $p = m \cdot v$ does not hold at very high velocities, which led physicists to modify classic mechanics formulae by including the Lorentz factor $\gamma = 1 / \sqrt{1 - \frac{v^2}{c^2}}$, where c denotes the speed of light. Thus, with the refined basic equation $p = \gamma \cdot m \cdot v$, a modified version of Newton's second law can be stated as

$$P = \frac{m \cdot v}{\sqrt{1 - \frac{v^2}{c^2}}}. \quad (14.6)$$

Coherently, the classic law $p = m \cdot v$, then, is a special case, holding only for low velocities, of the more general law expressed in Eqn. ([14.6](#)).

Theoretical Psychophysics of Stimulus Discrimination: The Classic View

Five papers in 1927 describe my efforts to establish some law and order in psychophysics, but I should never claim that they are more than a beginning in the development of psychophysical theory.—L.L. Thurstone ([1952](#), p. 308)

While the laws of motion belong to the *explananda* of physics, according to Cummins ([1983](#)), the primary explananda of psychology are *mental capacities*. One fundamental capacity of humans and other intelligent organisms is the ability to make distinctions, such as distinguishing ripe from rotten fruit through color and smell discrimination. We have already seen how certain aspects of this capacity, like discrimination sensitivity, can be experimentally measured using comparative judgment tasks, and how observed regularities can be formally expressed as empirical laws. Next, we examine the post-Fechnerian history of psychophysics, highlighting key theoretical developments initiated by Louis L. Thurstone.

As outlined below, Thurstone's work led not only to the first rigorous mathematical models of stimulus discrimination but also to the identification of deviations from these models (i.e., empirical anomalies), thereby enabling later theoretical refinements. In contrast to previous psychophysicists, Thurstone was firmly interested in the role of *cognitive representations* involved when people judge objects based on comparing some feature of interest. Specifically, Thurstone introduced the concept of the *discriminal process*, which refers to the mapping of the values of some physical continuum (e.g., levels of luminance) onto representations of these levels (e.g., the perceived intensity of brightness) arranged alongside a perceptual continuum (e.g., ranging from the minimally to maximally perceivable brightness). Crucially, due to *discriminal dispersion* inherent in the discrimininal process, Thurstone modeled the represented magnitudes X_1 and X_2 of any two compared stimuli S_1 and S_2 as random variables with variances σ_1^2 and σ_2^2 . According to Thurstone's law of comparative judgment (LCJ),

participants in any trial n of a 2AFC experiment base their decision concerning the relative magnitude of the stimuli on the difference between the two represented magnitudes:

$$D_n = X_{1,n} - X_{2,n} \quad (14.7)$$

where D_n is the perceived magnitude difference on that trial. Adhering to a more general formulation of LCJ, the statistical distribution of the perceived magnitude difference D thus amounts to

$$D = X_1 - X_2 = Z_{1,2} \cdot \sqrt{\sigma_1^2 + \sigma_2^2} \quad (14.8)$$

where $Z_{1,2}$ refers to the statistical dispersion of the experimentally observed proportions that S_1 is judged greater than S_2 .

Most importantly, unlike Fechner's psychophysical law, LCJ is not merely empirical but theoretical, as it specifies theoretical entities such as cognitive representations. Coherently, by making (and combining) different assumptions regarding the features of these entities, such as the shapes and the correlation of the distributions modeling the magnitude representations, a multitude of exact and testable predictions can be deduced (e.g., Luce, [1977](#); Weissmann et al., [1975](#)). For instance, Thurstone ([1927b](#)) demonstrated how the Fechnerian postulate, as expressed in Eqn. ([14.3](#)), can be tested empirically, and he found that it was false. Most important to the present investigation, many influential psychophysical theories, for example, signal detection theory (*SDT*, Green & Swets, [1966](#)) and other prominent psychophysical *difference models* (cf. Luce & Galanter, [1963](#)), are directly based on LCJ.

Challenging the Classic View: Effects of Stimulus Order on Discrimination Sensitivity

In essence, the aforementioned difference models are rooted in the Thurstonian postulate that humans base their comparative magnitude judgment between any two stimuli on the *cognitive representations* of these stimuli (Baird & Noma, [1978](#)). Consequently, these models predict that the ability to discriminate between any two stimuli should not depend on their temporal order, but only on the parameters

reflecting the physical quantities and discriminial dispersions (for a detailed analysis, see Yeshurun et al., [2008](#)).

Empirically, however, this particular assumption of order-independence does not hold. For example, Nachmias ([2006](#), p. 2462) noted the slope of the psychometric function to be steeper for $\langle sc \rangle$ than for $\langle cs \rangle$ trials in a shape discrimination task, thus indicating better discrimination performance for trials in which the standard precedes rather than follows the comparison. Importantly, this observation reflects an instance of the *Type B effect* (TBE, Ulrich & Vorberg, [2009](#)), reflecting the technical term for the data pattern that discrimination sensitivity differs as a function of the temporal order of s and c in a 2AFC task (for an illustration, see Fig. [14.1](#)).¹ Since Nachmias' seminal paper, the TBE has been established as a robust and general phenomenon across a range of stimulus modalities and discrimination tasks (for a meta-analytic review, see Ellinghaus et al., [2024](#)).

For historical accuracy, it should be noted that while Nachmias was the first to identify the TBE as an empirical contradiction to the predictions of Thurstonian difference models, the effect itself had been discovered much earlier by Lillie J. Martin and Georg E. Müller during their exploration of weight discrimination in Göttingen (Martin & Müller, [1899](#)). We can only speculate about why Thurstone and other theoretical psychophysicists in his tradition did not consider this earlier documentation of order-dependent sensitivity differences. Notably, according to Murray and Link ([2021](#)), many psychophysicists in the US accessed the work of Müller and his colleagues through Edward Titchener, who had published English summaries of some articles reporting the experiments conducted in Müller's lab. In fact, Titchener ([1905](#), pp. 303–309) did give an English description of the relevant findings of Martin and Müller and praised them highly: “There can be no doubt that the work of Martin and Müller will stand as a landmark in the history of experimental psychology, comparable, perhaps, with such books as Herings Lichtsinn or Ebbinghaus' Gedächtnis” (p. 309). Recall, however, that Thurstone was quite skeptical of Titchener's technical and measurement-focused approach

to psychophysics, and hence might not have fully recognized some passages of his writings.

After it became clear that the TBE challenges established models of comparative judgment, numerous psychophysical researchers have sought to find its mechanistic origin.² At least two extensions of the classic Thurstonian model have been proposed that can account for order-dependent sensitivity differences. First, based on a suggestion by Durlach and Braida (1969), Nachmias (2006) reasoned that judgments would be based on comparing the second stimulus presented on a trial with a virtual standard. Elaborating on this idea, Lapid et al. (2008) and Dyjas et al. (2012) formalized a cognitive mechanism according to which such internal reference I , akin to Nachmias' virtual standard, is established throughout an experiment.³ According to this Internal Reference Model (IRM), participants complete their task in a current trial n by comparing the internal representation $X_{2,n}$ of the second stimulus in this trial against the current internal reference I_n , which is a conglomerate of previous and current stimulus instances and is continuously updated from trial to trial. As the subjective difference D_n on the current trial—similar to LCJ as described in Eqn. (14.8)—results from this comparison process, it can be stated as

$$D_n = I_n - X_{2,n} \quad (14.9)$$

where the internal reference $I_n = g \cdot I_{n-1} + (1 - g) \cdot X_{1,n}$ on trial n is a weighted sum of the internal representation of the first stimulus $X_{1,n}$ in the current trial and the internal reference I_{n-1} from the previous trial $n - 1$, with constant weight g , $0 \leq g < 1$. If $D_n > 0$, participants perceive that the first stimulus is larger than the second stimulus, whereas when $D_n < 0$, they perceive that the second stimulus is larger than the first stimulus. Crucially, Dyjas et al. (2012) demonstrated that the mechanism specified by IRM implies a better discrimination performance when s precedes rather than follows c , and thus predicts a (negative) TBE.

A second framework that takes into account the effects of stimulus order on discrimination sensitivity is the Sensation Weighting Model

(SWM; Hellström, [1977](#), [1985](#)). In contrast to IRM, SWM assumes that the magnitudes of the internal representations for the first and second stimuli are differently weighted when the stimuli are compared.

According to Dyjas and Ulrich ([2014](#)), basic SWM can be formalized as

$$D = w_1 \cdot X_1 - w_2 \cdot X_2 + u \quad (14.10)$$

where w_1 and w_2 denote the weighting coefficients associated with the internal representations of the first and second stimulus, respectively, and u is a constant to adjust the mean of D . As shown by Dyjas and Ulrich ([2014](#)), SWM can also predict a TBE.

In conclusion, both IRM and SWM are extensions of the classic Difference Model, which are able to predict a TBE as repeatedly observed across various tasks and dimensions. In order to distinguish between these two accounts, it would be diagnostic to find experimental conditions that render data patterns consistent with one of the models but at the same time inconsistent with the other model. For example, as discussed by Ellinghaus et al. ([2024](#)), the validity of IRM could be challenged by the presentation of strong empirical evidence demonstrating positive TBEs under specific (a priori definable) boundary conditions. Thus, it seems essential for future psychophysical research to identify such boundary conditions and interpret their implications for existing models of stimulus discrimination.

Cognition Beyond Discrimination: The Value of Mathematical Rigor

Thus far, we have selectively outlined the history of psychophysics of magnitude discrimination to illustrate how the application of mathematically formalized models has enabled researchers to gain a deeper understanding of the unobservable mechanisms underlying this fundamental perceptual ability. As a question more broadly relevant to psychological theorizing, it seems valuable to examine whether this particular scientific approach has also proven helpful in understanding some cognitive abilities beyond mere stimulus discrimination. To that

end, we will first describe how mathematical psychophysics carved the way for understanding the physiological basis of human color vision. Finally, we will argue that the meta-theoretical principles advocated by Helmholtz, which have proven fruitful in the study of color perception, have also demonstrated great promise in other areas of human cognition research and continue to hold significant relevance today.

The Psychophysical Origins of Trichromatic Color Vision

Today, it is widely accepted that human color vision is based on three distinct cone types located on the retina, each being most sensitive to short (red), medium (green), and long (blue) wavelengths of light, respectively (Palmer, [1999](#)). From a historical perspective, it is noteworthy that the physiological anchoring of trichromatic color vision could only be established after theoretical psychophysics in combination with behavioral experiments had paved the way.

The mathematical foundation of the theory was initiated by Hermann G. Grassmann, who pointed to the fact that colored light adheres to the principle of additivity. For instance, he stated that any color sensation perceivable by humans could be obtained by mixing white light of a certain intensity with a certain colored light of particular intensity. According to Grassmann ([1853](#)), the law of color mixture can be stated as

$$L_i(P) \equiv L_1(W) + L_2(Q) \quad (14.11)$$

where P represents an arbitrary color stimulus with luminance L_i , W represents white light with luminance L_1 , and Q represents a particular color stimulus with luminance L_2 .

An important insight from this analysis is that any colored light can be altered to match any color sensation by only varying three physical parameters (corresponding to the dimensions of hue, brightness, and saturation in modern terminology). Based on this notion, Clerk Maxwell ([1857](#)) reasoned that the majority of color sensations may be obtained by mixing no more than three light sources corresponding to the primary colors red, green, and blue, provided that each of the three lights is included at a specific level of relative intensity. Coherently,

following Maxwell, a generalized version of the law of color mixture (cf. von Campenhausen, [1993](#)) can be stated as

$$L_i(P) \equiv L_1(R) + L_2(G) + L_3(B) \quad (14.12)$$

where L_1 , L_2 , and L_3 reflect the luminances of the primary colors R , G , B , and P denotes a colored light with luminance L_i . Behavioral evidence for this trivariance of color vision was first given by Maxwell ([1857](#)) and later by Helmholtz ([1867](#)). For example, the latter demonstrated that by adjusting the relative intensities of three monochromatic light sources, participants were able to produce a colored light appearing subjectively equal to an arbitrary comparison color (cf. von Campenhausen, [1993](#)). These color matching experiments were interpreted by Helmholtz ([1867](#)) as supporting the trichromatic theory of vision, as earlier articulated by Thomas Young, who had proposed in 1801 that color vision is based on three different receptor types with overlapping sensitivity areas (Sherman et al., [1982](#)). In particular, Helmholtz assumed the existence of three distinct sets of nervous fibers in the retina, each responding preferentially to light of a specific wavelength, such that any color sensation results from the combined activation of these fibers. Helmholtz's hypothesis has since been repeatedly corroborated by neurophysiological studies (Palmer, [1999](#)).

Hermann von Helmholtz: A Pioneer of Cognitive Psychology

Although Helmholtz placed great emphasis on synthesizing the multidisciplinary research on color phenomena of his time to provide a common neurophysiological foundation, it would be incorrect to conclude that he considered psychological theorizing of only secondary interest. Rather, Helmholtz believed that phenomena related to *perception* (as opposed to *sensation*) involved a cognitive component of judgment and thus required a theoretical analysis that is genuinely psychological:

Since perceptions of external objects belong to representations, and representations are always acts of our mental activity,

perceptions can also only come about through mental activity. Therefore, the study of perceptions properly belongs to the domain of psychology, particularly insofar as it involves examining the nature of the corresponding mental activities and determining their laws. (von Helmholtz, [1867](#), p. 427, translated by the authors)

To elaborate, Helmholtz thought that perception's primary function was to inform an agent about the nature of external events and objects. Yet, since sensations (e.g., excitation of nerve cells on the retina) merely constitute the effects of such external entities (e.g., an object, say, an apple, reflecting light onto the retina), they demand an additional step in processing before meaningful percepts (e.g., the cognitive representation of an apple) can arise. This additional step—called *unconscious inference* in his sign theory of vision—deduces from a given sensation the representation of an external event as its cause. Furthermore, because sensory data are inherently ambiguous or incomplete, perceptual judgment often depends on learned regularities from past experience to some extent. This strategy is generally useful but can lead to errors when the situation significantly diverges from the previous experiences used as a reference point. Helmholtz's explanation of colored shadows illustrates the supposed function of prior knowledge in guiding the formation of perceptual representations.

The latter phenomenon arises in an experimental setup where an object (e.g., a pencil) is placed on a white area (e.g., a piece of paper) that is illuminated not only by white light but also by a second non-white light source (e.g., yellowish light from a candle). By adherence to this procedure, the pencil casts two shadows, one from the candle and one from the daylight. The shadow cast by daylight appears yellowish, consistent with the candle's illumination. However, the candle's shadow appears blueish, which is inconsistent with its illumination by white light. The question of how to explain this counterintuitive finding had puzzled prominent scholars, including Fechner. Helmholtz' explanation

goes as follows: First, since the shadow continues to appear blue when seen in isolation through a tube, even when the illumination is changed while staring through the tube (Fechner, [1838](#)), the blue color cannot be attributed to physical features of the light. Instead, as goes the argument, the perceived color must be the result of a cognitive judgment which is based on prior reference experience; since it usually does not happen that one part of a surface is unaffected by the general lighting conditions, the observer assumes that both the candle and the daylight illuminate the shadow. If this assumption were valid, the shadow would cause its actual physical excitation only when it was a blue object (the complementary color of yellow), which is what is perceived.

Noteworthy is that while Helmholtz distinguished psychology from physiology in terms of theoretical analysis, he held that all sciences should rigorously adhere to exact observation and mathematical modeling, as traditionally known in mechanics (cf. Mausfeld, [1994](#)). Analogous to later critical theorists like Thurstone, he highlighted the special significance of non-observable entities. As Helmholtz ([1867](#), p. 40, translated by the authors) put it, any observable phenomenon is

only fully explained when it has been reduced to the ultimate natural forces that underlie it and are effective within it. Since we can never perceive forces themselves, but only through their effects, we must, in any explanation of natural phenomena, move beyond the realm of the senses and turn to imperceptible entities defined solely through concepts.

About 150 years later, we may conclude that Helmholtz's general meta-theoretical approach, that is, reserving for psychology a distinct level of abstract theoretical analysis which is informed by rigorous behavioral experimentation, has proven utterly successful in various branches of cognitive research. Beyond the historical overview of theoretical psychophysics presented in this chapter, we propose that David Marr's multilevel approach to cognitive research stands out as a particularly

remarkable contribution. According to Marr ([2010](#)), any cognitive process is best understood when tackled at three distinct levels of theoretical analysis, namely, a computational level (defining the function of a cognitive process), an algorithmic level (defining the computational operations undertaken to fulfill the function), and an implementational level, which describe the physical realization of the process. While the first two levels typically fall within the realm of psychophysics and cognitive psychology, the latter is mainly relevant for neuroscience and physiology. Interestingly, within the last decade, neuroscientists have increasingly recognized that understanding the neural processes underlying cognition requires behavioral research guided by abstract theorizing to a much larger degree than previously thought (e.g., Krakauer et al., [2017](#); Piantadosi & Gallistel, [2024](#)).

Therefore, it appears that—at least in certain branches of neuroscience—an increasingly anti-reductionist perspective is being adopted, one that Marr had previously articulated in his magnum opus, which resonates with Helmholtz’s meta-theoretical beliefs: “Studying vision by studying only neurons is like studying bird flight by studying only feathers: It simply cannot be done” (Marr, [2010](#), p. 27). The call for a stronger development of formal theory also emerged in the context of the so-called *replication crisis*, which refers to a debate surrounding the evaluation of relatively low replication rates across various fields of psychology (Open Science Collaboration, [2015](#); Pashler & Harris, [2012](#); Simmons et al., [2011](#)). Many authors have attributed these findings to either a deficient statistical rigor (Button & Munafò, [2017](#); Stanley et al., [2018](#)) or to insufficient prevention of questionable research practices, such as p-hacking (Simmons et al., [2011](#)). On the contrary, others have pointed to the crucial role of theory-guided hypothesis formation (Fiedler, [2017](#); Muthukrishna & Henrich, [2019](#); Oberauer & Lewandowsky, [2019](#); Ulrich & Miller, [2020](#)). For instance, Oberauer and Lewandowsky ([2019](#)) argued that formal modeling can strengthen the logical link between theory and hypothesis, thereby increasing the a priori probability of an experiment providing meaningful evidential value concerning a phenomenon of interest (cf. Borsboom et al., [2021](#);

Eronen & Bringmann, [2021](#). Consistently, replication rates are higher in areas of psychology that typically embrace mathematically formalized theories (such as cognitive psychology and psychophysics), compared to areas that devote less rigor to theory formation (Open Science Collaboration, [2015](#)).

Currently, it remains a topic of debate whether psychology should generally follow a meta-theoretical framework closely modeled on the research logic of physics, which heavily relies on formal theory and idealization (Borgstede & Eggert, [2023](#); Bringmann & Eronen, [2016](#); Eronen et al., [2024](#); Meehl, [1967](#); Slaney, [2023](#); Trafimow & Fiedler, [2026](#)). While some authors have pointed to the potential of a physics-oriented style of research in psychology (Borgstede & Eggert, [2023](#); Bringmann & Eronen, [2016](#); Trafimow & Fiedler, [2026](#); Ulrich, [2009](#)), others articulate motives for caution regarding such an analogy between psychology and the physical sciences (Hutmacher & Franz, [2025](#); Sanbonmatsu & Johnston, [2019](#); Slaney, [2023](#); Trendler, [2009](#)). For instance, Hutmacher and Franz ([2025](#)) pointed out that psychological concepts may be inherently more vague than those typically studied in the physical sciences, and expressed caution regarding the uncritical application of formal theory and exact measurement in psychology. While acknowledging that formalization can be a valuable tool, they emphasized that it is unlikely to resolve all conceptual and methodological challenges.

In any case, although it is currently difficult to predict the role formal theory should assume in various areas of psychology, we believe it is becoming increasingly evident that theoretical work in general will play a pivotal role in shaping the future of our discipline (Fiedler, [2017](#); Frischkorn et al., [2022](#); Funke, [2023](#); Glöckner & Fiedler, [2024](#); Muthukrishna & Henrich, [2019](#); Oberauer & Lewandowsky, [2019](#); Wendt & Wolfradt, [2022](#)).

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Footnotes

¹ Ulrich and Vorberg (2009) introduced the notion *Type B effect* (TBE) to distinguish it from the classic time order error (Fechner, 1860; for a comprehensive historical overview, see Hellström, 1985), which Ulrich and Vorberg refer to as *Type A effect*. In detail, one speaks of a *negative* TBE if *DL* is larger for ⟨*cs*⟩-trials than for ⟨*sc*⟩-trials, while a *positive* TBE refers to the opposite result pattern, that is, smaller *DL* for ⟨*cs*⟩-trials than for ⟨*sc*⟩-trial (for a review, see Ellinghaus et al., 2024).

² Interestingly, some psychophysicists have even doubted the mere existence of the TBE but instead claimed that it constitutes a methodological artifact; as noted above, this is quite a

common (and in terms of debate valuable) reaction of established scientists when confronted with empirical anomalies. For example, García-Pérez and Alcalá-Quintana ([2010](#)) argued that the higher difference thresholds in $\langle cs \rangle$ compared to $\langle sc \rangle$ trials as observed by Lapid et al. ([2008](#)) are due to a supposedly erroneous procedure of DL estimation. However, Ellinghaus et al. ([2024](#)) reported conclusive meta-analytic evidence of the TBE across a range of heterogeneous analysis procedures. Thus, the claim of García-Pérez and Alcalá-Quintana ([2010](#)) appears highly doubtful at this point.

[3](#) Note that several closely related ideas have been proposed within a Bayesian framework (Glasauer & Shi, [2021](#); Jazayeri & Shadlen, [2010](#); de Jong et al., [2021](#); Schumacher & Voss, [2023](#); Shi et al., [2013](#); Wiener et al., [2014](#)).

15. Integrating Cultural Perspectives into the Theory Building of Mainstream Psychology

Gesa S. Duden¹✉ and Momoka Sunohara²✉

(1) Department of Psychology, Concordia University, Montreal, QC, Canada

(2) Department of Political Science, Concordia University, Montreal, QC, Canada

✉ **Gesa S. Duden (Corresponding author)**

Email: gesa.duden@concordia.ca

✉ **Momoka Sunohara**

Email: momoka.watanabe@concordia.ca

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Introduction

Culture profoundly influences how we think, feel, and act—making it essential to *integrate cultural perspectives into mainstream psychology* to ensure its global relevance and accuracy. Furthermore, *Psychology itself is a cultural product*, developed within specific societal and historical contexts that have shaped its dominant theories and methods (Gergen et al., [1996](#)). Although not a new insight, psychological science has often underexamined its own cultural underpinnings. Much of the

discipline continues to rely on research conducted by and with people from affluent Western societies while often making problematic assumptions of universality (Adams et al., [2015](#); Henrich et al., [2010](#); Tam & Milfont, [2020](#)). This chapter outlines how psychological model and theory building can benefit from reflecting on itself as a cultural product and from integrating cultural perspectives into its core.

Culture, Mind, and Cultural Psychology

Culture and mind have dynamic and multifaceted relationships (Lehman et al., [2004](#)). We understand culture as the complex and ever-changing systems of meanings encompassing values, beliefs, behavioural norms, as well as cultural products, but not as a synonym for ethnicity or nationality. Despite varied definitions, culture is widely understood to involve *implicit* elements, including tacit assumptions that individuals are often unable to articulate. Further common elements of definitions of culture describe it *as patterns of meaning*, *as systematic*, *as existing in the head and in the world*, *as mutually constituted and shared* and *as transmitted* (Kirmayer, [2007](#); Ryder et al., [2011](#)). Cultural knowledge assists humans in addressing a wide range of problems and can provide guidance by defining what is considered normal or deviant. For instance, a study comparing Indian and US-Anglo-American participants on their preferences on co-sleeping (Shweder et al., [1995](#)) found that US-American and Indian participants focused on different moral values when deciding on sleeping arrangements. While Indian participants highlighted the protection of vulnerable, female chastity anxiety and respect for hierarchy, US-American participants focused on the “sacred couple” and the autonomy ideal in their decisions.

Cultural knowledge is transmitted over generations and from those familiar with the context to newcomers. Thereby cultural models of parenting play a central role. Parenting styles and goals vary widely across cultures, shaping how children are socialized into the values and ways of being specific to their cultural context (Keller et al., [2006](#),

[2010](#)). For example, German middle-class mothers emphasize independence while Indian mothers emphasize autonomous relatedness in their parenting styles—orientations that shape early interactions and ongoing socialization (Keller et al., [2010](#)).

Cultural psychology assumes that culture and mind are mutually constitutive (Shweder, [1991](#)), that is, that psychological processes are shaped by cultural experiences and vice versa. The discipline aims to understand this relationship and how it creates psychological variation (Markus & Kitayama, [1991](#); Ryder et al., [2011](#)). In contrast, much of mainstream psychology implicitly takes an absolutist or universalist standpoint, assuming that psychological phenomena operate following a set of universal and natural laws, independently of context—context is seen as “unwanted noise”. Acknowledging the cultural shaping of psychological phenomena does not mean rejecting the possibility of universals altogether. Rather, it calls for caution in assuming universality without cross-cultural evidence. From this perspective, universality is not a starting assumption but a conclusion to be earned through culturally diverse research. Norenzayan and Heine ([2005](#)) proposed a framework for evaluating degrees of universality in psychological processes, ranging from accessibility universals (present and functioning similarly across cultures) to existential universals (present but functioning differently), to non-universals. Few psychological phenomena have met the criteria for accessibility universals (see Brown, [1991](#)) including having an understanding of the number “two” or smiling when you are happy. Existential universals, such as achievement motivation, show important cultural variation in their expression: for instance, while success tends to motivate performance among North Americans, Japanese seem to be more motivated after failures than after successes (Heine et al., [2001](#)).

The fact that much of psychological research has historically overlooked cultural variability, treated the mind as independent of culture and assumed universal cognitive and emotional processes is reflected in the overreliance on WEIRD samples.

WEIRD Samples and WEIRD Researchers

The acronym WEIRD highlights that, while many psychological studies make universal claims, they often examine phenomena in a minority of people—those from Western, Educated, Industrialized, Rich, and Democratic countries who make up only about 12% of the world's population (Henrich et al., [2010](#); Hutmacher & Franz, [2025](#); Tam & Milfont, [2020](#)). Despite widespread recognition of the WEIRD problem since its formulation in 2010, over 90% of experimental participants still come from the United States, Canada, and a few Western European countries. Meanwhile, 88% of the global population account for only 4 to 8% of all participants in psychology and cognitive science (see Fig. [15.1](#); Henrich, [2024](#); Hruschka et al., [2018](#); Nielsen et al., [2017](#); Rad et al., [2018](#)).

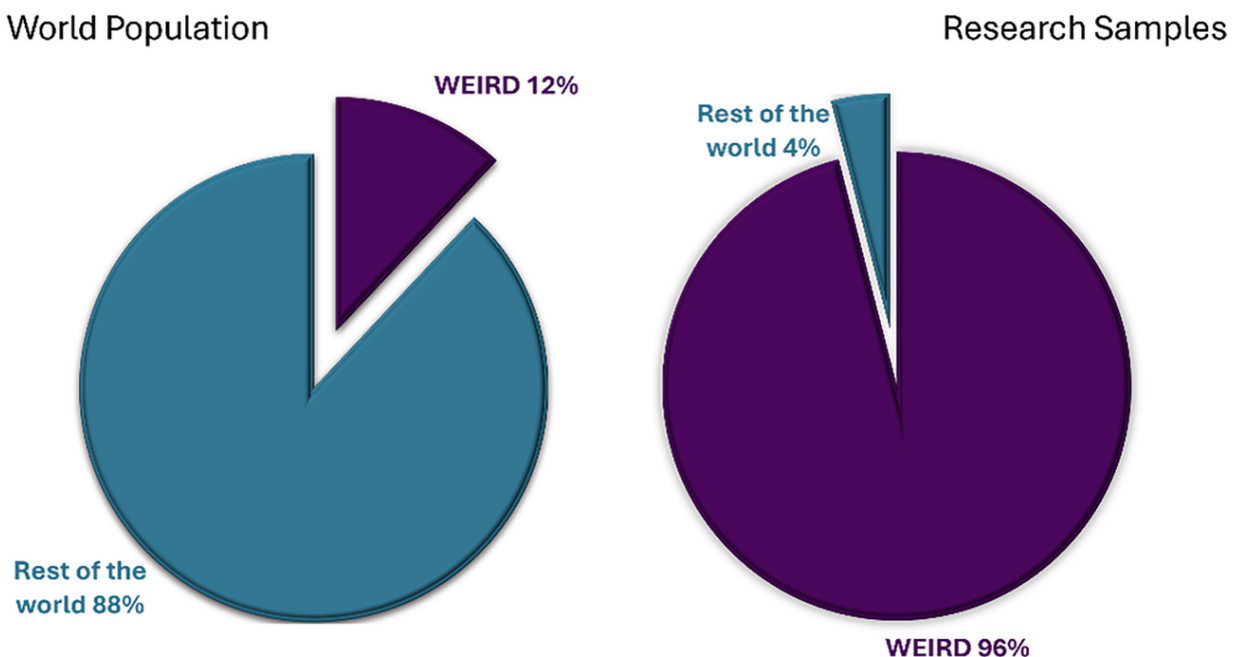


Fig. 15.1 Samples in psychological research compared to world population. Data taken from Arnett ([2008](#)) and Henrich et al. ([2010](#))

The WEIRD-sample limitation is often overlooked when findings from Western studies are generalized (Adetula et al., [2022](#)). For example, studies from the Global South are far more likely to specify the country of their participants and institutions than those from the

Global North (Castro Torres & Alburez-Gutierrez, [2022](#)). This lack of geographic context can create misleading assumptions of universality—studies without location details are often seen as describing general psychological truths, while those that name a context are viewed as locally bound.

The problem of generalizing from WEIRD samples is heightened by findings showing that WEIRD people are weird indeed—in many aspects they differ from other populations. The classic example is that of the fundamental attribution error (FAE), which describes the tendency to attribute behaviour to personal traits while downplaying situational influences (Gilbert & Malone, [1995](#)). The FAE was long thought of as universal with researchers exploring its cognitive mechanisms rather than questioning its cross-cultural validity (Brady et al., [2018](#)). Miller ([1984](#)) challenged this assumption by showing that middle-class US-Americans made more dispositional attributions than middle-class Hindu Indians, who placed greater emphasis on situational factors. He argued that cultural meaning systems shape attribution styles: Western cultures prioritize individual agency, making dispositional attributions more intuitive, whereas Eastern cultures foster holistic thinking, leading to greater consideration of context. Evidence for this was also seen in the fact that Anglo-Indian participants with greater exposure to Western norms made more dispositional attributions than their Hindu Indian counterparts. Later studies replicated and expanded Miller's findings (e.g., Choi et al., [1999](#)).

The term WEIRD has been criticized for emphasizing national differences while overlooking other key aspects of diversity, such as ethnicity (Syed & Kathawalla, [2022](#)). It can also reinforce Western-centric views by implicitly framing non-WEIRD populations as uneducated, poor, or undemocratic—echoing colonial and racist narratives. Still, the acronym continues to be useful in highlighting the limited scope of much psychological research.

Yet unrepresentative samples are only part of the problem. The dominance of WEIRD samples is tied to the geographical concentration

of research institutions and funding with most psychological research stemming from Western institutions and researchers. For example, an analysis of 1691 articles in developmental psychology journals found that 61% of first authors were affiliated with US institutions, 81% with institutions in English-speaking countries, 15% in non-English-speaking European countries, and only 4% in Asian countries and Israel. Strikingly, only two studies had first authors based in Central or South America, and none from the Middle East or Africa (Nielsen et al., [2017](#)). This geographic imbalance is not just a matter of who gets studied, but also of who formulates research questions, sets agendas, and defines what is considered valid knowledge. As a result, the cultural assumptions embedded in WEIRD academic environments continue to shape the psychological theories that are generalized to humanity.

Psychology as a Cultural Product—The Influence of Philosophical Axioms

Psychological science, like all scientific disciplines, rests on foundational philosophical assumptions—so-called axioms (Yanchar & Hill, [2003](#)). Axioms are basic, self-evident beliefs that guide how we define reality (ontology) and how we claim to know it (epistemology). Though often implicit, axioms shape what researchers consider valid questions, appropriate methods, and meaningful findings. In this way, abstract philosophical assumptions have concrete effects on how research is designed, conducted, and interpreted (see Fig. [15.2](#)).

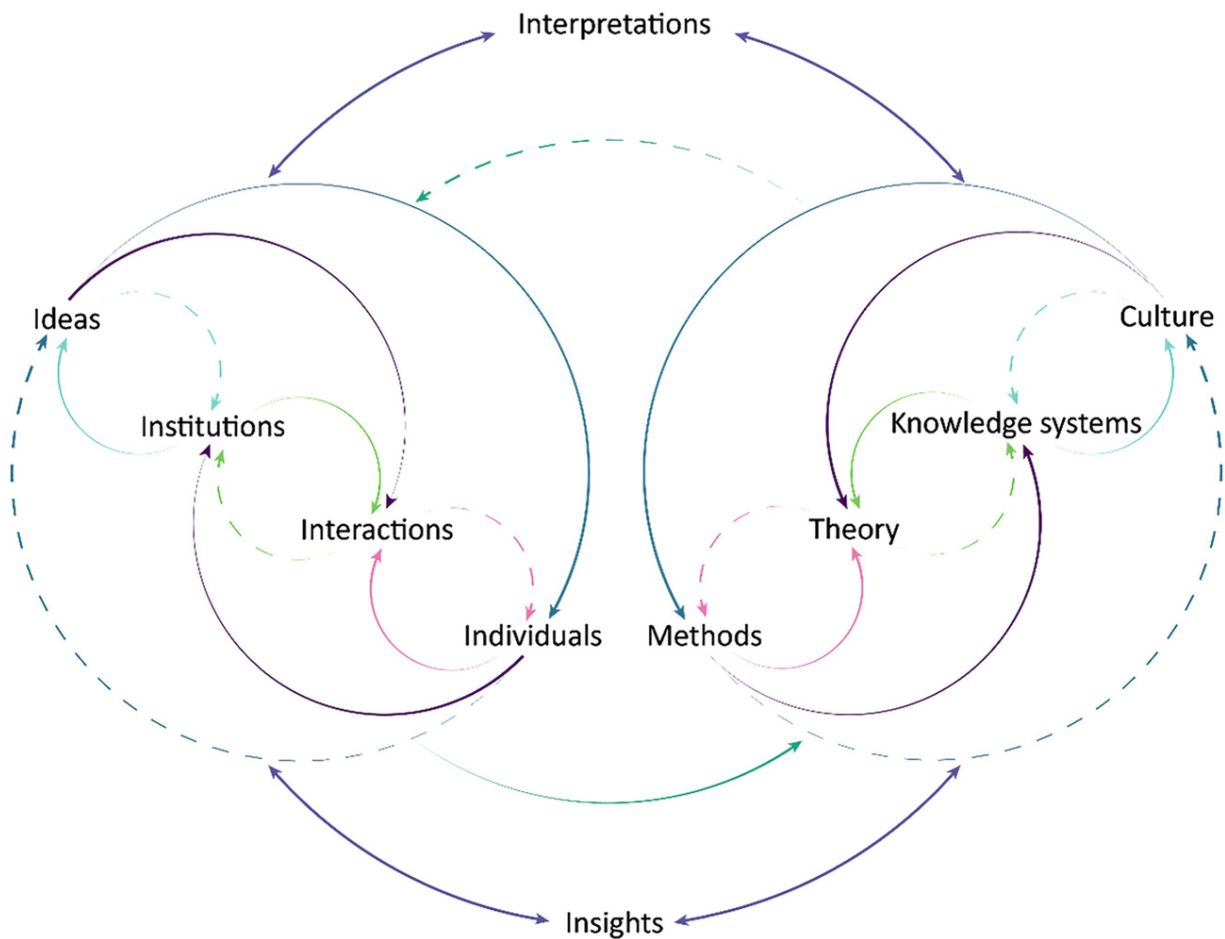


Fig. 15.2 The dynamics of knowledge production in psychology

The rise of logical positivism in the Global North around the early twentieth century significantly shaped psychology's development as an empirical science (Yanchar & Hill, [2003](#)). Logical positivism assumes that systematic, causal relationships exist in the social world and that strict research methods can uncover them (Stainton Rogers, [2009](#)). In teaching the history of psychology, it is often claimed that psychology became a science when Wundt and others broke from philosophy by establishing laboratories and conducting experiments (Hatfield, [2002](#)). However, this portrayal misrepresents Wundt's own perspective, who, rather than rejecting philosophy, sought to integrate empirical methods with philosophical inquiry, envisioning psychology as both a natural and a cultural science (Fahrenberg, [2012](#); Jovanović, [2021](#)). Yet, psychologists pushed to have their discipline recognized as a natural

science by adopting experimental methods from physics and physiology (Hatfield, [2002](#)). This historical pursuit may still subtly impact the field today, limiting engagement with philosophical questions of ontology and epistemology (Eronen et al., [2024](#)). Psychology's scientific authority continues to rest largely on natural science methods—along with their implicit ontological assumptions (Kovary, [2019](#)).

The preference for positivist empiricism as psychology's default epistemology has often occurred and continues to occur without critically examining its suitability for the subject matter at hand (Yanchar & Hill, [2003](#); Wertz, [2011](#)). One consequence is the field's disproportionate focus on methodology, a phenomenon referred to as *methodologism* (Teo, [2018](#)). This emphasis can obstruct deeper reflection on the nature of psychological phenomena themselves. Teo ([2018](#)) argues that psychology rarely engages with the question of what *truth* means, instead relying on an implicit methodological theory of truth—namely, that truth is whatever results from following accepted quantitative procedures. The resulting abstract formalism is evident in many academic publications (Mayrhofer & Hutmacher, [2020](#)).

Another angle on psychology as a cultural product comes from postcolonial theories which argue that the philosophical foundations of psychology's emergence as a science were based on a Eurocentric understanding of human life and shaped by the political motives of European colonial expansion (Adams et al., [2015](#)). According to Nyamnjoh ([2012](#)), colonization led to the replacement of diverse Indigenous epistemologies with a dominant Western epistemology. Coloniality is still present in psychological research, influencing what is studied, how it is studied, and who conducts and funds the research (Sengupta, [2021](#)). For example, Western psychological research tends to treat whiteness as the default, positioning other racial groups as deviations—reflecting the implicit assumption that white samples are raceless (Roberts & Mortenson, [2023](#)). In response to colonial research practices, Indigenous scholars and communities have increasingly advocated for research methodologies that foster respectful and

inclusive knowledge production (Foxworth & Ellenwood, [2022](#); Hernandez, [2022](#)).

While postcolonial perspectives are essential for exposing the power dynamics embedded in psychological science, the call for cultural contextualization can also be grounded in a broader philosophical critique. Diverse traditions—from phenomenology to Buddhism—offer alternative conceptions of subjectivity that challenge Western notions. These perspectives resist both universalist and relativist extremes, suggesting that cultural variation is rooted in deeper ontological differences in how human experience is structured and understood. Engaging with such plural philosophies of subjectivity opens a path towards a more inclusive foundation for psychology—one that honours both the shared and culturally shaped dimensions of being human.

The philosophical axioms forming psychology are reflected in the conceptual core of the discipline. The following subsections illustrate how foundational concepts—such as the self, attachment, mind-body relations, and mental disorders—are deeply embedded in specific cultural and philosophical traditions, most of which have been historically rooted in Western contexts.

Culture Making up Psychological Science: Self and Relational Styles

Psychology's cultural imprint is especially evident in its conceptualizations of the self, individual, and personality—core topics of the discipline. Western science and culture have encouraged the view of a stable, core personality that persists across contexts (Kirmayer et al., [2008](#)). This perspective often overlooks sociocultural embeddedness, focusing on individuals in isolation rather than in relation to family, community, and environment—a critique raised by systemic and feminist approaches and emphasized in Global South psychologies (Deterville, [2016](#)).

Cultural psychology research suggests that the boundaries between self and others vary depending on an individual's cultural contexts. For

example, studies show that in East Asian societies people tend to conceptualize their self in relational terms compared to the emphasis on individual autonomy in the West (Heine, [2001](#); Markus & Kitayama, [1991](#)). Many Indigenous communities describe an *ecocentric* view of personhood, where individuals are embedded in a broader web of life (Kirmayer et al., [2008](#)). How we define the self—as an isolated entity or as being in reciprocal interaction with contexts—impacts both research and practice (Kirmayer et al., [2008](#)). For instance, therapeutic models based on an individualistic view of the self may focus on personal insight and autonomy, while approaches grounded in relational or ecocentric understandings may prioritize community involvement, environmental connection, or healing relationships.

Attachment theory, which posits that infants and caregivers are biologically predisposed to form close bonds (Bowlby, [1969](#)), presents another example of psychology's cultural embeddedness. It defines secure attachment as ideal, while avoidant and anxious-ambivalent styles are seen as problematic. Research confirms that the secure base phenomenon occurs in infant-mother dyads across industrialized and developing societies, but critics argue that this emphasis reflects a Western bias favouring individuation and independence (Harwood, [1992](#); Rothbaum et al., [2000](#)). Indeed, cultural differences are apparent in infants' exploration behaviours and attachment styles. Many non-Western cultures emphasize dependence over exploration, for instance, Dogon infants in Mali explore less due to environmental risks (True et al., [2001](#)), and Puerto Rican mothers favour proximity-seeking over exploratory behaviour (Harwood, [1992](#)). Overall, cultural context plays a key role in shaping secure base expressions and attachment styles challenging the assumption of one ideal style (Molitor & Hsu, [2019](#)).

Culture Making up Psychological Science: Emotional Models and Expression of Distress

The ways in which emotions and psychological distress are experienced, expressed, and interpreted are deeply shaped by cultural frameworks. Across cultures, emotions and distress may be

communicated through thoughts, behaviours, or bodily symptoms—each of which holds different significance depending on the cultural context. What one context may label as psychological suffering, another may interpret as spiritual imbalance or physical ailment. These culturally embedded models shape not only how individuals articulate their experience, but also how others—family members, clinicians, and researchers—respond to it.

Cross-cultural research shows that cultural norms shape how emotions are expressed and regulated (Chentsova-Dutton & Ryder, [2019](#)), which can affect how psychological distress manifests and is perceived (Tamir et al., [2024](#)). One prominent example of culturally shaped distress expression are *psychologization* (*foregrounding cognitive-affective symptoms articulated in mental terms*) and *somatization* (the manifestation of psychological distress through physical symptoms). Some cultures are more likely to foreground bodily expressions of distress, while others emphasize emotional or cognitive symptoms. Originally, psychoanalysts described somatization as an immature defense mechanism, framing it as a sign of lower psychological sophistication (Ryder & Chentsova-Dutton, [2012](#)). Similarly, some linguistic perspectives have suggested that somatization arises from a lack of emotional vocabulary in certain languages, reinforcing Eurocentric assumptions and dualist thinking.

Instead of evaluating these patterns through a Western developmental lens, these cultural differences in distress expression might be understood in light of divergent philosophical traditions—particularly mind–body dualism in the West and holistic thinking in many non-Western contexts. Many non-Western healing traditions adopt a holistic view, treating mind and body as interconnected. Within these frameworks, physical symptoms serve as a legitimate form of communication with health professionals (Mbaegbu, [2016](#); Ip et al., [2023](#); Xiang et al., [2023](#)). The close connection between somatic changes and emotions is well documented across cultures, as seen, for example, in Sub-Saharan Africa, where the bodily expression of emotions reflects a deeply integrated view of body

and mind in the local understanding of personhood (Bovey et al., [2024](#); Mbaegbu, [2016](#); Yik & Chen, [2023](#)). Furthermore, conditions such as *shenjing shuairuo* (neurasthenia) and *shen kui* (kidney deficiency) in China, as well as *dhat syndrome* (semen-loss anxiety) in South Asia, exemplify how emotional distress can manifest through somatic complaints (Xiang et al., [2023](#)).

Differences in emotional expression and appraisal develop already during childhood: studies indicate that European US-American parents elaborate more on emotions when conversing with their children compared to East Asian parents (Fivush & Wang, [2005](#)). In North American contexts, emotional expertise is linked to psychological, physical, and interpersonal well-being (Ford et al., [2014](#)). In contrast, East Asian and West African cultures tend to cultivate greater bodily awareness (e.g., Choi et al., [2016](#)). These cultural differences in experiential expertise may lead to varied experiences of distress, as individuals monitor culturally emphasized domains and perceive changes in these areas as more distressing. Therefore, cultural differences in emotional experiences may reflect variations in the domains that receive attention (Choi et al., [2016](#)).

In many Western contexts distress tends to be psychologized, with an emphasis on cognitive-affective symptoms rather than somatic symptoms, even when the latter are present. This pattern reflects the legacy of Cartesian mind-body dualism, which has profoundly influenced biomedical models and research designs. While Cartesian dualism has dominated Western thought, it has also been challenged within Western philosophy itself—for example, by phenomenological, existential, and systems-theoretical perspectives that advocate for more integrated mind-body models. Despite these critiques, contemporary psychology continues to reflect largely dualistic assumptions, especially in its emotion theories. These models, often based on English-speaking, highly educated populations, prioritize internal mental states when describing emotional experiences. Among these populations, using mental state language to describe emotions is linked to greater emotional granularity and well-being, reinforcing the model's perceived

validity. However, such emphasis may obscure the somatic and embodied dimensions that are equally integral to emotional life even within Western contexts (Ip et al., [2023](#)).

Culture Making up Psychological Science and Vice Versa: Categorization of Mental Distress, Explanatory Models, and Looping Effects

What constitutes normal and abnormal experience and expression varies cross-culturally, influencing how distress is distinguished from normative functioning (Duden et al., [2026](#)). For example, while depression in European US-Americans is associated with *decreased* emotional expressiveness, among Asian US-Americans it may involve *increased* emotional expressiveness. Although these symptoms appear to be opposites, both represent deviations from the norms for emotional expression within their cultural contexts (Chentsova-Dutton & Ryder, [2019](#)). Similarly, emotional numbness may be reported less frequently in cultures where its expression aligns more closely with cultural norms, making it harder to recognize (Chentsova-Dutton & Ryder, [2020](#)).

The role of culture in psychological phenomena is gaining increasing recognition. The *Diagnostic and Statistical Manual of Mental Disorders 5* (DSM-5), for example, places greater emphasis on cultural considerations than previous editions (Xiang et al., [2023](#)). However, the *DSM* itself remains a cultural product—rooted in Western assumptions about mind, self, and disorder—and may not be universally applicable (Gone & Kirmayer, [2010](#); Duden et al., [2022](#), [2023](#)).

Historical and cross-cultural shifts in how distress is categorized reveal the culturally embedded nature of diagnostic systems. Some changes might reflect scientific progress or evolving norms (e.g., the removal of homosexuality as a disorder), while others may indicate shifts in the experience of distress itself. For instance, *bulimia nervosa* only emerged recently, possibly shaped by new cultural ideals around privacy and food availability (Chentsova-Dutton & Ryder, [2025](#); Keel & Klump, [2003](#)). Eating disorders more generally are changing in relation

to cultural models of beauty and body (Weissman, [2019](#)), but also in relation to cultural models of disorders: in 1990s Hong Kong, anorexia was expressed without fat phobia, which only became prominent after exposure to Western diagnostic frameworks (Watters, [2010](#)).

Symptom conceptualizations and experiences are continually evolving, often in response to cultural shifts. Emerging syndromes such as *sluggish cognitive tempo* (Barkley, [2014](#)), *hikikomori* (Pozza et al., [2019](#); Saito, [1998](#)), and *resignation syndrome* (Von Knorring & Hultcrantz, [2020](#)) have emerged alongside changing social pressures, while others, such as *shenjing shuairuo*, have faded or merged into Western constructs like depression (Bhola & Chaturvedi, [2020](#)). In Japan, debates around *modern-type depression* illustrate how cultural shifts influence symptom expression: modern-type depression contrasts with traditional depression by emphasizing blaming others and fluctuating moods rather than guilt and endurance, potentially reflecting tensions between collectivist norms and growing individualism (Kato & Kanba, [2017](#)).

One might object that culture merely colours how people *talk about* psychological phenomena, whereas the phenomena themselves—such as depression—exist independently of this cultural variation. From this perspective, cultural factors are seen as surface-level “noise” that researchers must filter out to access the disorder’s true nature. Yet this view neglects the crucial role of *explanatory models*—culturally shaped understandings of symptoms, causes, and appropriate responses—in impacting not only how distress is expressed, but *how it is actually experienced and enacted* (Kleinman, [1980](#); Kirmayer & Bhugra, [2009](#)). Studies show that in physical illness, beliefs about the self (e.g., self-efficacy) and about symptoms (e.g., whether they should be monitored or are seen as part of the illness) can directly impact the symptom experience and management (Halm et al., [2006](#); Petrie & Weinman, [2012](#)). Because many symptoms lack objective markers, individuals rely on culturally available categories and labels to make sense of their suffering. This can lead to symptom experiences that reflect local norms and values (Chentsova-Dutton & Ryder, [2020](#)).

Explanatory models also shape reality through what Ian Hacking ([1998](#)) calls *looping effects*, the recursive feedback between human experience, classifications systems, and social institutions (see Fig. [15.3](#)). Once a diagnostic category is introduced and institutionalized (e.g., through the DSM), it influences how individuals interpret their experiences and how institutions treat them. Over time, these interpretations feed back into the category itself, subtly reshaping it (Hacking, [2004](#); Kirmayer & Sartorius, [2007](#)). For example, the widespread adoption of the DSM's definition of depression has altered how people describe and understand sadness, leading more individuals to identify as depressed and seek treatment accordingly. This shift has consequences for clinical practice, research, public health messaging, and even pharmaceutical development (Gómez-Carrillo & Kirmayer, [2023](#)). Looping effects are particularly pronounced with normative classifications such as those found in the DSM or ICD. These diagnostic systems do not only describe symptoms, but define norms about what is considered healthy or pathological. As such, they exert a powerful influence on how individuals interpret their own experiences and how society responds to them (Hacking, [2004](#)).

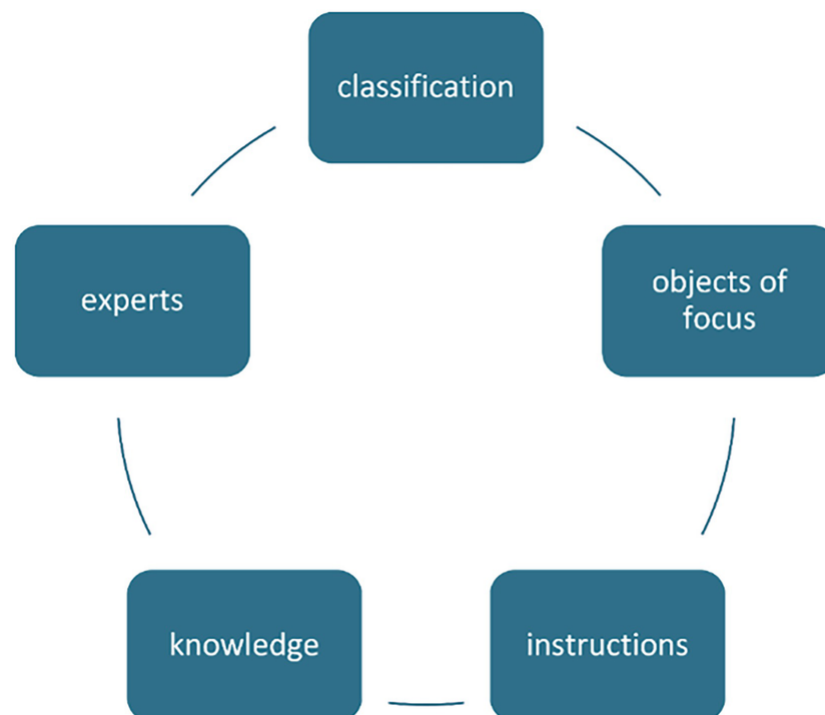


Fig. 15.3 Hacking's looping effects. Adapted from Lauriault ([2012](#))

Crucially, these effects are not limited to individual experience. Diagnostic categories influence broader systems—insurance coverage, school accommodations, healthcare access—and create incentives that reinforce their authority and relevance. Scientific language becomes part of everyday discourse, as in the increasing use of “anxiety” and “depression” to describe ordinary struggles in many Western societies (Brinkmann, [2016](#); Xiao et al., [2023](#)). Cultural talk is thus not a distraction from the study of mental disorders—it is constitutive of them. Psychological categories are not neutral containers for universal phenomena but are shaped by, and help shape, the cultural worlds they inhabit. Recognizing the dynamic interplay between culture, classification, and lived experience is essential for building a more inclusive and reflexive psychological science.

Integrating Culture into Our Models and Theories

To develop theories that genuinely reflect the diversity of human experience, psychology must recognize that its models are often built on culturally narrow foundations (Brady et al., [2018](#)). When psychological research overlooks its own cultural embeddedness as well as the cultural variability of human beings and draws only from homogenous samples, it risks producing distorted understandings of human functioning. A more robust science emerges not by erasing complexity, but by embracing it—considering who is studied, how cultural meanings shape both participants and researchers, and where and why research takes place.

Designing Studies with Cross-Cultural Validity

Studying culture-mind interactions requires more than comparing groups—it demands designs that are culturally meaningful across contexts. Researchers must avoid the *category fallacy* (Kleinman,

[1977](#)): assuming that psychological constructs (e.g., depression) are equivalent in meaning across cultures without validating them locally. This challenge is particularly evident in global mental health research or clinical psychological research. If categories like depression lack cultural resonance in a given setting, comparing prevalence rates or symptoms across countries becomes scientifically questionable. This poses many challenges. For instance, how do we measure mental disorders in different contexts, if we cannot be sure that our categories are even meaningful within these contexts?

One important step is enabling local researchers to conduct psychological studies in their own contexts. Most psychological knowledge continues to be produced in the Global North. Between 2017 and 2019, nearly 92% of first authors in psychology publications were affiliated with institutions in Western countries (Lin & Li, [2023](#)). Researchers from the Global South face significant structural barriers, including language, funding, and access to resources (Sengupta, [2021](#); Raval et al., [2023](#)). Furthermore, their work is often devalued as “too local” or dismissed unless tied to a Western comparison group (Raval et al., [2023](#)). If we aim to build psychological theories that are both scientifically sound and globally relevant, it is essential to create conditions in which researchers from diverse geographical and cultural contexts can lead the way—posing research questions that emerge from local realities, developing methods suited to those contexts, and interpreting findings through culturally grounded lenses.

Furthermore, to move beyond the constraints of culturally bounded models, international and interdisciplinary collaboration is essential (Duden, [2021](#)). Cultural sensitivity in such partnerships involves recognizing diverse epistemologies, avoiding dominance of any single worldview, and ensuring equitable participation of local institutions (Duden, [2021](#); Urassa et al., [2021](#)). Many intercultural research projects still lack equal participation from local research institutions (Urassa et al., [2021](#)). True collaboration involves shared authorship, mutual learning, and reflection on the power dynamics embedded in research. Multilingual teams and the citation of scholarship beyond English-

language sources can also expand the scope of psychological theorizing (Duden, [2021](#); Wertz, [2011](#)).

Reflexivity—long embraced in qualitative research—can aid this process. Researchers are encouraged to examine how their epistemic values and cultural assumptions influence their interpretations. Furthermore, reflexivity also examines researchers' moral or political norms, which can influence research topics and study designs, and the selection of study subjects (Nielsen et al., [2021](#)). First suggestions on how to integrate reflexivity in quantitative research have already been made (e.g., Jamieson et al., [2023](#)).

Transforming Methodology, Sampling, and Education

One avenue for transforming the discipline in practice is to re-evaluate how we design studies, engage with communities, justify our sampling choices, and educate future psychologists. Participatory and context-sensitive approaches offer promising ways to ground psychological research in local realities. Participatory Action Research (PAR), for example, involves communities in co-producing knowledge (Gibbs, [2001](#)). This approach not only democratizes research by treating participants as experts in their own right but also generates questions and methods that resonate more deeply with local realities (Duden & Bagheri Sarvestani, [2023](#)). A study by Abarca Brown et al. ([2022](#)) in São Paulo, conducted during the COVID-19 pandemic, exemplifies this. It reimagined urban mental health by focusing on community knowledge and local coping strategies—both contributing to science and strengthening communal ties. Furthermore, qualitative and mixed-methods approaches could advance the field. Quantitative methods alone often reduce culture to categorical variables (e.g., ethnicity), overlooking contextual meanings and lived experiences. Qualitative approaches can give insight into how cultural norms, values, and practices shape psychological phenomena, providing depth to cross-cultural comparisons. Mixed-methods research integrates quantitative generalizability with qualitative insights, addressing both universal patterns and cultural specificity (Douceirain et al., [2016](#)). To advance

psychology and integrate culture into its cores, researchers could adopt *pragmatic mixed-methods designs*—prioritizing research questions over methodological dogma—and ensure integration at all stages (e.g., using qualitative data to refine quantitative measures). This approach balances rigor with cultural nuance, fostering interdisciplinary dialogue (Doucerein et al., [2016](#)).

Such methodological shifts go hand in hand with more intentional and transparent sampling practices. Improving research design means explicitly attending to who is studied and why. Ideally, researchers should think about the diversity of their sample and report on the key demographic characteristics such as age, socioeconomic status, ethnicity, religion, and nationality. Where this is not possible, the absence should be acknowledged. More fundamentally, researchers could be asked to justify their choice of sample—just as they already justify sample size (Brady et al., [2018](#)). For instance, studying US undergraduates may be appropriate for testing a theory initially, but such limitations need to be clearly stated to hinder overgeneralization and invite further research across diverse populations. Framing conclusions in context not only enhances accuracy but sparks curiosity about how these dynamics play out in other settings.

Discussion sections could include considerations on how culture might affect the generalizability of findings. Rather than assuming universality, researchers could begin with what their data tells them about the specific group under investigation and reflect on whether and how this might—or might not—translate across cultures (Rad et al., [2018](#)). This mindset encourages more thoughtful theorizing and fosters new lines of research grounded in contextual variation rather than abstract generality. At the same time, it is important to guard against the tendency to pay mere lip service to cultural variability—for instance, by routinely calling for cross-cultural validation without taking concrete steps towards conducting such studies. Meaningful engagement with cultural context requires not only critical reflection in writing but also deliberate shifts in research practices (Hutmacher & Franz, [2025](#)) including broader sampling and international

collaboration. Guidelines and checklists for equitable and inclusive practices at every stage of the research are provided by the APA ([2022](#)) and others (e.g., Dawson et al., [2020](#)).

These transformations in research practice must also be reflected in how psychology is taught. Psychology curricula shape the discipline's trajectory by influencing how emerging scholars conceptualize research problems and methodological norms. Many Western programmes prioritize methodological rigor over epistemological reflection, reinforcing a view of psychology as a singular, positivist science (Yanchar & Hill, [2003](#)). Although the qualitative-quantitative methods debate in psychology is slowly subsiding (Duden, [2021](#); Duden & Barz, [2024](#); Wertz, [2011](#)), a strong bias towards quantitative, natural science-based approaches remains entrenched in both curricula and publication expectations. Reimagining psychological education means fostering critical thinking about the assumptions embedded in research practices, encouraging reflexivity, and introducing students to culturally diverse epistemologies from the start of their training. Without such changes in educational practice, efforts towards culturally grounded research may remain superficial.

Conclusion

Psychology cannot claim to be a universal science without accounting for the cultural contexts in which human cognition, emotion, and behaviour unfold. As this chapter aimed to show, psychological models themselves are shaped by historical, social, and epistemological forces that influence what is studied, how it is studied, and whose experiences are considered representative. Recognizing the reciprocal relationship between culture and mind challenges psychology's reliance on WEIRD samples and calls for models that better reflect human diversity. A more rigorous and inclusive psychological science requires moving beyond implicit cultural assumptions and engaging with diverse perspectives. This shift is not just about adding cultural variables but about rethinking foundational theories to acknowledge the dynamic

interplay between individuals and their sociocultural environments. By making cultural perspectives central to model-building, researchers can develop frameworks that are not only scientifically robust but also contextually meaningful, ultimately fostering a more globally relevant understanding of human cognition, emotion, and behaviour.

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16. Psychoanalysis in the Midst of Psychology's Crisis

Hannes Wendler¹✉ and Timo Storck²✉

(1) University of Cologne, Cologne, Germany

(2) Psychologische Hochschule Berlin, Berlin, Germany

✉ **Hannes Wendler (Corresponding author)**

Email: hwendler@uni-koeln.de

✉ **Timo Storck**

Email: t.storck@phb.de

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Pluralistic Narratives and Psychology's Crises

Academic psychology and psychoanalysis share a troubled, albeit intertwined history.¹ In the wake of psychology's tumultuous present, the call to turn to theory building has been voiced with increasing volume, ultimately leading some to reinterpret psychology's replication crisis (OSC, [2015](#)) in terms of a theory crisis (Oberauer & Lewandowsky, [2019](#)). While clinical psychology might be said to have been slow to react to this state of affairs at first (Tackett et al., [2019](#)), large-scale proposals of reform are now gaining traction, usually either leaning towards neuroscience (e.g., Cuthbert & Insel, [2013](#)) or psychometrics (e.g., Kotov et al., [2022](#)). In spite of its reputation as a

head-heavy approach, psychoanalysis, as of now, has not put forward a significant perspective on psychology's crisis (never mind proposed a remedy), further attesting to the hiatus separating it from the so-called mainstream discourse.

Change is direly needed and we want to outline some venues for cooperation. Academic psychology and psychoanalysis do not have to stand in (unfruitful) opposition, but can and should mutually illuminate each other. As is often the case, dialogues that start from highly divergent perspectives might be particularly difficile (and sometimes seem discomforting or even pointless), but soon prove to be invaluable. We want to stimulate such a dialogue between academic and psychoanalytic psychology.

We start out by challenging some pre-conceptions about psychology's crisis by way of reconstructing and reflecting its history. Afterwards we highlight parallels in the development of psychoanalysis and discuss the intertwinement of their respective histories. This procedure will shed light on the historical genesis of the hiatus between academic psychology and psychoanalysis, on the one hand, and pose the question of its logical justification, on the other hand.

Both academic psychology and psychoanalysis look back on a long-winded history of crises. Taking note of the fact that the replication or theory crisis was *not* psychology's first crisis is not merely of historical interest (Maiers, [2022](#)). Rather, it can help shed light on the scientific structure of psychology as well as its relationship to psychoanalysis. When psychology was in upheaval at the verge of the twentieth century, there was considerable disagreement about its scientific status. Drawing on Fahrenberg's (Fahrenberg, [2015](#), pp. 538–539) overview, it is instructive to consider that the entire logical space of possible perspectives was held by the proponents of the discourse:

- Some held that *psychology's crisis was inseparably linked to philosophy's crisis* (that ensued earlier in the nineteenth century) and due to their untimely separation (e.g., Windelband).
- Some believed that if psychology was to be a proper science, it must rid itself of philosophy's metaphysical ballast and be established as a

unitary science (e.g., Ebbinghaus).

- Some argued that *psychology was essentially dualistic*, dividing into approaches from the natural sciences and the humanities (e.g., Vygotsky).
- Some posited that there never was 'one psychology'. In fact, there are '*many psychologies*' and any attempt to unify them is doomed to misconstrue the plurality of approaches (e.g., Binswanger).

On the verge of the twentieth century, researchers with the self-understanding as psychologists were so productive that ever-new interpretations of psychology's scientific status started to come into conflict with each other. While neither theoretical argument nor empirical evidence seemed to suffice to privilege one perspective over and against the others, this heterogenous pluralism of perspectives proved to be immensely fruitful in terms of theoretical innovation.

In the midst of apparent stalemate, Bühler ([1927](#)) put forward his seminal discussion on *the crisis in psychology*. When reflecting on the creative output of theoretical perspectives by his contemporaries, Bühler coined the idea of a *structural crisis* and, thereby, put one more logical possibility on the table. What if psychology is structured in such a way that it cannot resolve these issues? Asserting a structural crisis in psychology is to say that its scientific structure is such that it must, in principle and not only accidentally, spawn crises.

In contemporary psychology, there exists the popular narrative (Leahey, [1992](#)) that psychology underwent two Kuhnian revolutions before reaching a stage of normal science that lasts to this day. For illustration, consider the *narrative of two revolutions*:

1. The revolution from structuralism (Wundt, Titchener) to behaviourism (Pavlov, Skinner, Watson)
2. The revolution from behaviourism to cognitivism (Neisser, Piaget, Newell, Simon, Nisbett)

Drawing on Bühler's concept of structural crisis, this revolutionist narrative can be challenged on several fronts. One pertains to the incompleteness of the narrative, another concerns its framing altogether. Addressing the former, two additional milestones ought to be added:

0. The emergence of scientific psychology from its pre-scientific or philosophical predecessors, for instance, through the founding of Wundt's and James's laboratories. This transformation from philosophical to scientific psychology has important predecessors in Herbart's mathematical psychology and Fechner's psychophysics.
3. The present-day crisis of cognitivism in terms of replication crisis or theory crisis. Of course, it can only be speculated whether the integrative form of contemporary cognitivism will prevail through the crisis or be replaced by connectionism or another contender.

Substituting the missing historical dates, however, will not lead to a complete picture of psychology's identity. One is better advised with opting for perspectives like Fahrenberg's ([2015](#)) theoretical psychology that conceives of psychology's identity as a *unitas multiplex*. Instead of trying to dissolve the complexities of psychology's history, the discipline's identity is framed as the systematics of its controversies, thereby accounting for the pluralism of perspectives within an explicit and theoretical meta-narrative. From the theoretician's and historian's point of view, a sober and open-minded posture is most profitable—even if it comes at the price of questioning seemingly secured presuppositions about our discipline's past.

This already points to the second challenge to the revolutionist narrative. In simple terms, Kuhn ([1992](#)) pictures scientific development as paradigm shifts. These paradigm shifts, again, are conceived as successively repeating cycles of the phases of normal science, crises and revolutions. Hence, the notion of crisis is engrained into the very core of the narrative of two revolutions, because without preceding crises,

there can be no revolution in the first place. However, it is anything but clear whether this narrative can be applied to capture psychology's development, because it presupposes that psychology had first reached a phase of normal science and, therefore, overcame its preparadigmatic status. This, of course, can be doubted and it remains a contentious issue whether psychology's status as a science can be modelled in this sense (Weimer & Palermo, [1973](#)). Additionally, if Bühler is right and psychology's crises are an expression of its underlying structural features, then psychology's crises can neither be truly resolved through revolutions and consequent paradigm shifts.

From this follows an either-or situation: either psychologists have to come to terms with psychology's ever-recurring crises or they have to find a novel theoretical groundwork for psychology altogether.

On this fundamental layer, these considerations lead to the question of psychology's subject matter. Must the ultimate reasons for psychology's crises not be sought in the nature of its subject matter? While it might be assumed to be rather straightforward to assert that any science must have a subject matter, psychology is troubled by a controversial discourse regarding precisely this assertion. Without being able to elaborate in detail (see Wendler & Ramminger, [2023](#)), this issue can be illustrated by considering the *standard definition* of psychology as the science of experience and behaviour (as provided by the APA, for instance). As Graumann ([2001](#)) has argued, it is noteworthy that this definition is formulated as a logical conjunction of two distinct subject matters, namely, experience *and* behaviour. Before this backdrop, the structural feature giving rise to psychology's crises can be identified in the dual nature of its subject matter. Approaches that are fine honed either for experiential or behavioural phenomena are bound to come up with conflicting conceptions of what amounts to 'good' psychological science. If this is the case, then psychological science can, in turn, not be established as a unitary science by appealing to the unity of its subject matter (for it is twofold).

Graumann ([2001](#)) posits that instead of searching for psychology's *unity*, the question can be transformed into that of psychology's

identity. Experience and behaviour amount to no exclusive dichotomy, but an inclusive duality. In this picture, psychology's identity is not challenged by the dual nature of its subject matter, but by centripetal forces from other disciplines that change the structure of psychology's discourse by introducing external modes of explanation. This principle, centripetal forces can alter psychology until it would become something else altogether, for instance, something along the lines of the Inselian dictum that 'psychiatry is nothing but applied neuroscience' (Insel & Quirion, [2005](#)). The second challenge to psychology's identity occurs through centrifugal forces from within psychology, which threaten to pull psychology apart into a manifold of distinct disciplines (see Osbeck, this volume). This sometimes is denoted by the term 'psychological sciences'. Against this backdrop, psychology's crises can be understood in terms of *identity crises* and be associated either to centripetal (e.g., introduction of measure of exactitude from the physical sciences; cf. Straus, [2013](#)) or centrifugal forces (e.g., isolated discourses on psychology's crises that remain unconnected; cf. Hutmacher & Franz, [2025](#)).

In sum, identifying the roots of psychology's crises is anything but an easy task, albeit not impossible. The upshot of the analysis so far is that if psychology's crises stem from its underlying identity conflict, then a clinical perspective on identity development might just be what is missing and what psychoanalysis could provide. These tensions that arise from structural features at the foundational level of psychological science might just turn out to be productive. In the following sections, we will flesh out this thought and probe whether it is of more than just allegorical value.

Scientific Structure of Psychoanalysis

Psychology's history can be understood as a continuous pursuit of scientific exactitude. The primary reason why the current crisis poses a deep and serious challenge to psychology's identity is that low replicability rates of only one-third to one-half of the original findings

call psychology's status as a science into question or at the very least pose the question of how far developed it can be regarded as of now (OSC, [2015](#)). Balancing the scales on this question is not an easy task; nevertheless, it is precisely what current debates are after, for instance, the one pertaining to whether psychology's crisis is a methodological crisis or a theory crisis (Oberauer & Lewandowsky, [2019](#); Fiedler, [2018](#)). While vulnerable to substantial criticism, remedies, such as the introduction of badges that acknowledge open science practices like preregistration or open access data (Kidwell et al., [2016](#)), react to this disruptive finding by incentivizing psychologists to work more rigorously, and, more importantly, by formalizing the criteria which 'good psychological science' ought to observe. Needless to say, these large-scale remedies enjoy widespread popularity and attempt to re-orient psychology's conventions for scientific conduct on models from natural science.

Consider now that psychology's pursuit of scientific exactitude is intimately coupled with the history of psychoanalysis. In the formative discourses of the late nineteenth and the early twentieth century, the trend became predominant to establish psychology as a positivistic science. Along the lines of Comte's ([1975](#)) law, the philosophical stadium of psychology's development was to be replaced by a truly scientific stadium that relies on empirical evidence and experimental methods, while freeing itself from metaphysical ballast and normative statements (cf. Albert & Topitsch, [1971](#)). The same holds for the influential, falsificationist current from theory of science, whose founding father, Popper ([1962](#)), discredited psychoanalysis as mere pseudo-science by using examples from its canon to illustrate unfalsifiable statements: Psychoanalytical statements, hence, were too vague and its hypothesis ultimately untestable. In this vein and alongside perspectives, such as historical materialism or existentialism, psychoanalysis has gotten into the crosshairs of behaviourists, psychometricians and logicians (Feigl & Scriven, [1956](#))—not least because its claim was not limited to treatment or even cultural interpretation, but to be a novel and foundational approach to studying

the psyche. In spite of the fact that Freud's institutionalization of the first private practise for psychotherapy in Vienna should provide a paradigm for the history of clinical psychology, his thought was criticized as esoteric, his methods as unfounded, his concepts as oblique and his worldview as scandalous (see Mayer, [2002](#)). In addition, and from the perspective of the sociology of science, during the 1930s and 1940s psychoanalysis was systematically discriminated within its countries of origin during the rise to power of national socialism, leading to deep-running, institutional changes within German-speaking universities, especially within psychological faculties, and large-scale migration of psychoanalytical scholars (Greuter, [1984](#)).

In short, within the confines of the psychology's dominant self-narrative of a newly founded science that defines itself by the pursuit of scientific exactitude in terms of strict methodology, psychoanalysis did not make the cut. More than that, the very identity of academic psychology can be said to have constituted itself in contrast to psychoanalysis. Interestingly enough, the same cannot be said of other academic disciplines, such as sociology or cultural studies, where psychoanalytical writings are firmly rooted in the theoretical canon.

Several issues go along with such a framing. It would be meticulous to discuss whether the appraisal of psychoanalytical theory is fair, since there is ample evidence of Freud changing his theoretical commitments in light of contradicting empirical findings (Grünbaum, [2008](#)). More strikingly, however, what is overlooked in such appraisals is the *heterogeneity of psychoanalysis*. Adopting a broader, historically informed approach reveals that such accounts can only lead to one-sided evaluations of psychoanalysis' status as a science and, consequently, fail to do justice to its pluralistic perspectives on its subject matter. Just as for the case of psychology, one might better speak of *many psychoanalyses* instead of one psychoanalysis (that would seemingly be identifiable with Freud's writings). Needless to say, such pluralism comes at a price. For instance, theoretical psychoanalysts have gone to considerable lengths when grappling with the so-called consensus problem that addresses the difficulty to

ascertain intersubjectively shared interpretations of clinical observations or materials (Seitz, [1966](#)).

In the following paragraphs, we will outline some of the complexities of psychoanalytical thought concerning its theory of science, theoretical traditions and its functions.

Nomological, Idiographic and Alternative Interpretations of Psychoanalysis

Psychoanalysis is a discourse. This means that it comprises several, partly irreconcilable theoretical positions, akin to psychology itself. Nevertheless, these opposing psychoanalytical positions are bound to each other by a common outlook and cross-cutting issues, again, similar to psychology. Dealing with such a difficult situation requires a certain degree of tolerance for ambiguity and a willingness to bracket (at least temporarily) seemingly certain teachings. On top of that, the theoretical heterogeneity is best developed by drawing from its heterogeneous history (Graumann, [1982](#)).

Beginning with theory of science, it should be noted that the psychoanalytical discourse cannot be pinned down to a single perspective, be it hermeneutical or mechanistic. Amongst other things, this entails that psychology cannot exile psychoanalysis simply by appealing to scientific exactitude and defining it by appealing to positivistic or falsificationist criteria, because certain readings of psychoanalysis do just the same. For illustration, we want to differentiate three key positions from psychoanalytical theory of science in broad strokes by appealing to Windelband's ([1894](#)) classical distinction between nomothetical and idiographic sciences.

- *Psychoanalysis as natural/nomothetical science*: Especially in early writings, Freud envisioned psychoanalysis as a natural science that operates by postulating law-like statements, explains its observations in mechanistic terms and is firmly wedded to the assumption of psychic determinism. After all, the drives that were said to govern the life of the unconscious should be seen as natural phenomena (and as

such accessible to methods honed to an enlightenment conception of nature).

- *Psychoanalysis as hermeneutical/idiographic science*: In the later discourse on psychoanalysis, another, concurring interpretation took hold that conceived of it as a hermeneutical science, interested not in laws, but individuals, primarily not invested in explanation, but understanding and decidedly not mechanistic, but more concerned with the linguistic side of the therapeutic dialogue and the language of the unconscious. This interpretation of psychoanalysis gained traction in the wake of its assimilation through critical theory.
- *Alternative conceptualizations of psychoanalysis*: Several other accounts of the scientific structure of psychoanalysis can be differentiated, which do not fit neatly into either of these two alternatives. In this vein, Lorenzer ([1988](#)) coined psychoanalysis as 'hermeneutics of the body'. Other accounts have stressed the emancipatory faculty of psychoanalysis. For instance, following Apel's ([1965](#)) characterization of the social sciences, psychoanalysis can be understood as a science of reflection, meaning that the patients, that is, the objects of psychoanalytic theories, can incorporate psychoanalytic theories in the language of their self-understanding as (responsible) subjects.

The Five+ Psychologies of Psychoanalysis

Moving on from the general perspective on the scientific status of psychoanalysis to a more concrete, content-based appraisal, several schools of thought or traditions ought to be differentiated. Over three decades ago, Pine ([1988](#)) put forward a now classical classification of the psychoanalytic discourse into four distinct 'psychologies' or traditions. Adapting Pine's perspective to the contemporary state of research, one more tradition has to be added to his list, resulting in *five psychologies of psychoanalysis*.

- *Psychology of drive*: From the perspective of the psychology of drive, the individual is seen as the subject of the unconscious workings of the drives, which are expressed on the surface level of experience

(e.g., in phantasies, dreams or slip-ups) and lead to pathogenic conflicts in experience.

- *Ego psychology*: The standpoint of ego psychology developed out of an attempt to expand on the old drive-conflict psychology and introduced concepts such as an adaption to the (average expectable) environment, defence and reality testing.
- *Psychology of object relations*: A more radically different outlook is provided by the psychology of object relations, in which experience is analysed in terms of an internal drama composed of an inner stage that is inhabited by inner objects that stand in manifold, meaningful relations to each other and the experiencing individual. The individual enacts the roles of these objects or re-enacts the situations in which they were formed.
- *Self psychology*: From the vantage point of self psychology, the individual is conceived as an ongoing subjective state that is engaged with its self-delineation in terms of boundaries, continuity and esteem. Object relations are seen in the light of structure formation within the self and a new concept of narcissism is put forth.
- *Relational psychology*: The most current development that was not yet considered by Pine in the psychoanalytical discourse, intersubjective psychology, recasts psychoanalysis as a systems theory. This means that psychic determinism is replaced by intersubjective contextualism, resulting in a theoretical vocabulary of organizing principles, (mal-)attunements and reciprocal co-constitution that is fine-tuned to the ever-shifting character of mental life.

Against this backdrop, the talk of psychoanalysis singular rings with some naivete. This holds true especially considering that isolating exactly five psychologies of psychoanalysis is, in a sense, arbitrary, since many more traditions could be enumerated, such as Lacanian structural psychoanalysis, Barangerian psychoanalytic field theory and many more. This is why the notion of the *five+ psychologies of psychoanalysis* is more accurate, while admittedly also being clunkier.

Psychoanalysis comprises a multiplicity of different approaches, which complement and overcome each other, and, therefore, might be said to constitute a *unitas multiplex* akin to psychology itself. Also in line with this microcosmic structural parallelism of part and whole, the psychologies of psychoanalysis are connected through common themes (the unconscious, mental defence, resistance, etc.) and methods (interpretation, free association, imagination, etc.) as well as cross-cutting issues (the consensus problem, abstinence, empathy, etc.). Taking the fact that psychoanalysis cannot be identified with a singular perspective to be evidence of its insufficient development would be to misunderstand the matter completely. The origins of the polyphony of psychoanalysis run deeper and mirror the multivoicedness of its subjects, be it analysts or analysands.

Tracing this line of thought further requires a more focused look at the manner in which psychoanalysis proceeds treating individual clinical cases to formulating generalized statements about the human psyche.

Theory Building in Psychoanalysis

Freud's ([1926](#), p. 256) often-quoted dictum of an 'inseparable bond' ('Junktim') between treatment and research refers first of all to the fact that, as Freud writes, early psychoanalysts could not have conducted treatments without learning something new (about psychoanalysis, that is). This means that in the early days of psychoanalysis, concepts emerged from concrete experience in the consulting room, as a response to clinical experience and challenges, as it were. In the early history of psychoanalysis, examples of this are the concepts of unconscious fantasy, resistance, transference and countertransference. The 'direction' of theory formation can thus be described as moving from the individual clinical case (and case presentation) to the development of concepts ('bottom-up'). At the same time, this is only one dimension, as the opposite direction, which can be described more as top-down, can also be found: Freud's major case presentations (e.g., Freud, [1909](#)) always also consist of illustrating the significance of the

concepts, showing their relevance to clinical processes and testing them, albeit not in a nomothetic sense, for their suitability in practice.

Even if later on the conceptual framework no longer had to be developed in the advancing history of psychoanalysis, the figure of the 'inseparable bond' has remained a guiding principle for processes of theory formation, namely, in terms of conceiving theory from the standpoint of the individual case. The application of the psychoanalytic treatment method also does not operate in the sense of a mere application of general principles (e.g., what has proven to be effective for the treatment of people suffering from depression), but is rather rooted in the concrete encounter of the therapeutic relationship in the treatment session. This is not to say that manualization is not possible or useful, but that the approach of psychoanalysis itself, although it can very well be combined with these other approaches, has some particular elements to it. The individual treatment is not simply the replication of a previous one which turned out to be successful. This may prove to be a helpful idea with regard to the replication crisis in psychology, and we will return to it.

However, this raises the question of generalization in psychoanalysis. Does this approach mean that only each treatment can stand on its own, without one treatment being able to inform and guide a subsequent one? That would be rather peculiar, especially as a new set of theoretical instruments would then have to be developed for each single treatment.

Generalization and the Clinical Case

Generalization usually refers to the fact that something that applies to an individual case can also be assumed to have general validity. Every inductive conclusion is based on this idea, that is, on the justifiable assumption that conclusions can be drawn from the particular to the general. In scientific psychology, this is usually ensured by a sufficiently large and representative sample. The benefit of generalization lies in being able to make predictions for future individual cases that are sufficiently similar in terms of key variables, for example, that the use of

a certain method in the treatment of a person suffering from depression is likely to be successful.

Even if psychoanalysis is not opposed to this logic in the context of psychotherapy research, it does provide a different approach to generalization. Its generalization, we propose, is not at the level of predictions or the application of what inductive conclusions tell us, but at the level of theory formation. It can be said for psychoanalysis (cf. Zepf, [2006](#), p. 263) that it does not make statements about how individuals are in general, but *general* statements about *particular* individuals. In other words: they are general, theoretical statements about the particular, not statements about generalities. While academic psychology is usually concerned with the 'average human being' in order to describe structures and functions in general, psychoanalysis formulates general claims as well, yet these concern what can be said about the individual.

A comparison with neuropsychology can help to further illustrate the difference between these perspectives: When the human brain is studied using scientific methods, the object of investigation is the 'average' brain. The aim is to make statements about how the human brain functions. This requires a large sample and at the end of a particular study, something has been discovered about the human brain in general. This does not tell us anything about individual experience and neither something about the specific brain of an individual test subject. The conclusions drawn from the study results remain general.

In psychotherapy research, we can also examine the 'average' patient. Evidence-based research works according to this logic: What proves to be helpful for the treatment of people suffering from major depression? Only against this background can we speak of 'treatment resistance' in psychopharmacological terms or, more generally, of so-called non-responders.

What psychoanalysis now has to contribute, and this is a legacy of Freud's assertion of an inseparable bond, is not merely an individual case-related, qualitative and understanding approach, but also the perspective on the particular patient in relation to generalization. If we

say that the generalization of psychoanalysis lies at the level of theory formation, then this means that concepts are developed and refined from the individual case in its particular structure, which do not allow predictions about similar future cases, but rather provide a framework through which future cases, in their particular structure, can be understood (yet not explained and also not predicted). In other words: Where academic psychology provides ways to *predict* future cases and the impact of certain actions, psychoanalysis, by means of its general theory of the particular, provides ways to *understand* future cases.

Take a fictionalized patient, Mr. A., who enters psychoanalytic therapy. He presents with symptoms of depression, and over the course of treatment, both his therapist and Mr. A have come to recognize that his feelings of diminished self-worth are connected to the experience of having his emotional needs consistently unmet by caregiving figures throughout his life. Also, he was never able to express his feelings of disappointment to others, yet rather resorted to self-degradation. Even though these conceptualizations are far from being completely original, the therapist also includes some thoughts of the longing for a sibling Mr. A. didn't have who would have made things more complex but also less lonesome in childhood. In a bottom-up way, the therapist might develop the idea of a longed-for-sibling-complex as a novel contribution to psychoanalytic theory of depression. Thus, he generalizes by means of proposing a new conceptual element. Subsequently, we cannot state that this configuration plays a role in the etiopathogenesis of depression under any circumstances, yet we have a framework for understanding further cases. We do not operate on the level of prediction but on the level of understanding, yet still by means of generalization.

Psychoanalytical Perspectives on the Crisis in Psychology

What can psychoanalysis contribute to psychology's crisis? For psychoanalysis, replication is not the repetition of an observation

against a background of sufficiently comparable conditions. This is the case not only because of the difficulty of controlling all variables, but also considering the ‘place’ of generalization in psychoanalysis. Nevertheless, psychoanalysis provides access to being able to understand one individual case after the other against the background of something general (namely, a theory of the particular structure of cases).

Psychoanalysis can also come into play when it comes to reflecting on the interest-driven nature of psychological research. The replication crisis does not essentially arise due to a lack of scientific honesty or rigour (leaving aside cases such as HARK-ing), but rather due to structural difficulties, that is, due to the structure of a hypothesis-testing procedure. Here it can be argued that the choice of certain parameters in a study setting is always already permeated by how researchers themselves relate to their investigation, what kind of relationship exists between them and their subject.

In the closing sections of our investigation, we want to highlight some promising routes of how psychoanalytic thought could aid in psychology’s crisis. Some of these routes are already trodden, others still await exploration. We use the format of a ‘proof of concept’, because we want to sketch the outline of an idea, but cannot provide extensive elaboration at this point for reasons of space. As stated in the introduction, our ultimate aim is to stimulate a productive dialogue on crisis between academic psychology and psychoanalysis, and, accordingly, we understand our ‘proofs of concept’ as an incentivization for additional research.

Proof of Concept 1: ‘Good science’ Through Regulations

In the previous sections we have already touched upon current efforts to get a grip on psychology’s crisis by increasing and formalizing regulations of scientific conduct. This pertains to both the introduction of research practice guidelines such as open data, preregistration or badges (Kidwell et al., [2016](#)) that are external to the research question, but also to the manner in which research questions should be

formulated (e.g., falsifiable) or the manner in which the logical link between theory, hypothesis and findings ought to be construed (e.g., through formalized or mathematical modelling; Oberauer & Lewandowsky, [2019](#)).

Within the contemporary discourse, the idea that ‘good science’ could be achieved through increasing regulation has been subjected to substantial critique. For instance, Fiedler ([2018](#)) invoked Meehl’s creative cycle between tightening and softening processes in order to make conspicuous that the current trend on regulation one-sidedly favours tightening processes. Therefore, the innovative and risk-seeking side of ‘good’ psychological research is suffocated or at least insufficiently developed. What is more is that the loosening part of psychology’s creative cycle is askew because of what Fiedler ([2018](#)) calls *sexy hypotheses*, that is, the incentivization for publishing surprising or seemingly unlikely research findings.

The discourse on regulation rests on the premise that there exists an intrinsic link between the logic and sociology of scientific research. Psychoanalysis can offer an additional and unique perspective on this link. In his seminal study in ethnopsychanalysis, Devereux ([2014](#)) turns to the psychodynamic underpinnings of scientific methodology. Within the researcher’s mind, so the argument goes, methods in general and methodical exactitude and rigour in particular establish security when the behavioural phenomena cause anxiety. Devereux demonstrates this dynamic by discussing over 400 case studies from the domain of behavioural sciences, ranging from anthropological observation of indigenous mutilation practises to psychotherapeutic settings on the couch. Sticking to (ideally formalized) regulations, here, tames the behavioural phenomena and renders them manageable within the ‘scientific attitude’.

This approach can be transferred to psychology’s crisis. The perchance unconscious wish in the backdrop is that increasing regulations spares psychology a head on confrontation with its difficile foundational discourse. Speaking metaphorically, top ranking journals prescribe ever more regulations in the hopes of their anxiolytic effects.

From this vantage point, the psychoanalytic perspective can enable us to deal with the anxiety in a different manner, namely, by scrutinizing its roots. Psychoanalysis, here, discloses that scientific research is always also an existential endeavour, in which the researcher comes into contact with the unknown, not only intellectually, but also emotionally. This existential dimension, whilst always and irreducibly present, is merely masked and obscured by method and regulation.

Proof of Concept 2: Psychodynamics of Scientific Discourse

Devereux's work demonstrated that there exists a road from anxiety to method within behavioural science. This could be utilized to shed light on the psychodynamics of Meehl's tightening processes that were identified, for the sake of argument, with interventions such as preregistration, open data or good science badges. The psychodynamics of scientific discourse, however, extend beyond specific analyses of regulatory interventions.

In this vein, psychoanalysis can shed light on what the crisis actually means for psychology. Drawing on reflections on the relationship between subject and object, a psychodynamic interpretation of psychology's crisis in terms of a theory crisis (Oberauer & Lewandowsky, [2019](#)) can be outlined. Concerning the researcher (subject) and their theories (object), Zysman ([2018](#)) put forward an account that conceives scientific theories to have the status of inner objects for researchers. Apart from psychoanalytical terminology, this means that our theories also have an emotional meaning for us, we have grown fond of them, they prove to be faithful companions and give us orientation. This implies that if this special bond is shaken in the context of the theory crisis, a personal crisis is at hand simultaneously. This can and should not be discarded but rather reflected in terms of choices and decisions that are being made in planning and conducting psychological experiments and interpreting their outcome.

The psychodynamic interpretation of the theory crisis offers ripe opportunity for additional research, albeit research of quite a different character than what one would usually expect to read. Without going

into detail, we want to denote two promising venues for future investigation. The first venue concerns the *psychological interpretation of falsification*. Critical rationalism is situated in the first row of 'mainstream' theoretical psychology since Popper's founding days. Falsification, here, is regarded in terms of a logical link between empirical observations and theoretical statements (*modus tollens*); in fact, it is regarded as the only decisive logical link between the two. However, falsification can also be interpreted psychologically. If theories are (also) regarded as inner objects, then their falsification (also) has a psychological meaning. The question could be: What does it feel like to be falsified? Psychodynamic interpretations of hurt desires of recognition, biased appraisals of conflicting evidence and resistance to change lie ready at hand. From this vantage point, psychoanalytic perspectives on theory of science can help unravel the old conundrum of the sociology of science, namely, explain why scientific change takes place *after* the objective situation has swayed so often.

The second venue for further research concerns the *psychological interpretation of the context of discovery*. Since its founding days, Reichenbach's ([1938](#)) distinction between context of discovery and context of justification has been basic for the discourse on theory of science. The standard interpretation holds that only statements within the context of justification are of concern with regard to a possible refutation of a theory and that the manner in which a theoretical statement is conjectured, be it via logical reasoning or dreaming, is only of secondary importance. Challenging this standard interpretation invites the danger of psychologism, albeit without being impossible (Wendt, [2022](#)). It would be a low hanging fruit to argue that if theories are researcher's inner objects, then the context of justification must be structured emotionally as well. However, it is more instructive to demonstrate how the psychoanalytic perspective can enrich even those accounts that uphold Reichenbach's distinction. Theories become inner objects not only after they are construed objectively, but rather are construed through psychological operations such as thought and

intuition. These in turn can be subjected to a psychodynamic interpretation.

Take the logic of the classical method of free association, for example. The idea behind this method is that if persons are allowed to associate freely, their phantasy is still bound by their personality structure, which is the reason why ideas they produce reveal something about them. In other words, even though the 'experimental situation' does not impose any restriction on the subject's behaviour, the subject's personality and capabilities introduce certain bounds to his possible behaviour. In this sense, researchers' personalities delimit the psychological space in which they can make a discovery. Transferring this to the context of discovery in general, psychodynamic investigation of researcher's personality will reveal novel insights into the nature of scientific discovery.²

Proof of Concept 3: Observer Effects in the Scientific Setting

Whereas the prior two proofs were interconnected, the now following one is independent and concerns the logical make-up of scientific investigation. Reflection on the scientific setting is missing, except from some noteworthy exceptions. In his phenomenological analysis, Wendt ([2018](#)) has argued that the experimental situation in psychology is constituted through controlling the confounding variables. By keeping confounding variables identical throughout experimental variations or by excluding them altogether, the theoretically specified link between dependent and independent variable can be investigated empirically. Identifying and controlling confounding variables on the level of behaviour is one of the key conceptual achievements that enable psychological experimentation.

Failures to control confounding variables such as observer effects that occur through the presence of the experimenter or the degree of their information about the intentions behind the experimental set-up have also played a role in the replication crisis. This was the case in the perhaps most infamous example of failure to replicate in psychology,

concerning behavioural priming experiments that used stereotypical age stimuli to modify walking speed (Doyen et al., [2012](#)).

Psychoanalysis has opted for a different route. Instead of *controlling and excluding* confounding variables, it is interested in *exploiting* them. Whereas the insight that the scientist's presence and personality can influence experimental results has been a thorn in the side of rigorous psychological research, the analogous insight into the therapists influence on the clinical situation has been central to the development of the therapeutic setting. Following Mayer's ([2002](#)) historical treatment, the therapeutic situation originated with Freud's discovery of the transference phenomenon, that is, the phenomenon that patients project feelings onto the therapist that originally are about someone else. This insight is central, because it means that even seemingly conspicuous behavioural material such as speech is distorted by partly unconscious meanings that are not directly observable. These distortions cannot, in principle, be controlled or excluded, because parts of mental life are essentially distorted. Psychoanalysis therefore must study the structure of these distortions in order to reveal insights about the phenomena of real life. These were said to convey psychic depth, whereas artificially isolated mental events that are observable in experimental settings are, in comparison, superficial. It might seem counterintuitive, but in this sense it follows that forcing the psychological realm into logical categories is a more worrisome distortion than recognizing that mental life always transports an excess of meaning, that is, is always more than it seems to be at first sight.

Recognizing that parts of mental life are irreducibly distorted does not imply that it would be irrational. Rather, psychoanalysis is concerned with unravelling the *logic of psychological distortions*. This entails coming to terms not only with transference phenomena, that is, the influence of the patient's personality and biographical experience on the therapeutic setting, but also with so-called countertransference, that is, the therapist's partly unconscious projection of unresolved conflicts and feelings originally directed towards someone else onto the patient. While the fact that transference and countertransference are

entangled certainly makes matters more complicated, it has spawned a plenitude of inspired contributions in the psychoanalytic literature and lies at the heart of the transference focused approach to psychotherapy (Kernberg et al., [2008](#)). Even more importantly, this insight is the ultimate justification for subjecting psychoanalysts in training to self-exploration therapy. The reason being that a psychoanalyst must be familiar with his own participation in distorting the clinical material in order to identify the sources of distortion that stem from the patient. In other words, being able to recognize patterns of distortion introduced by their personality structure or manner of conduct enables psychoanalysts to systematically exploit the distortions that occur in the therapeutic setting in order to support the patient's self-understanding or engage in novel modes of intersubjective encounter. In this way, distortion is not only one of the most valuable sources of information for psychoanalysis, but also a key variable for therapeutic change.

A psychoanalytical perspective on observer effects in psychological science introduces a different outlook on the status of confounding variables. Whereas the classical idea of the psychological laboratory rests on controlling or excluding confounding variables, a psychoanalytic interpretation of the experimental setting reorients the scientist's interest towards exploiting the presence of confounding variables. Confounds are part of life and observing mental life within a psychological vacuum is a major distortion on its own (cf. Holzkamp, [1972](#)). A psychoanalytical interpretation of the logic of psychological science would not stop at the doorstep of the laboratory, but include the scientist's personality as one of its essential constituents. An enormous field of research awaits investigation, posing questions such as: At what level of analysis does the scientists personality influence scientific work other than in the sense of observer effects by the conductor? Do his interests guide literature research, do his unresolved conflicts produce blind spots for certain topics, etc.? What would it mean for psychological science if it was built on the axiom that the

scientists must ‘master’ their own personality similarly to how therapists must ‘master’ their own?

‘How may I be of help?’—Recommendations Pertaining to Psychology’s Crisis

The goal of this investigation has been to take a first step towards a productive dialogue between mainstream psychology and psychoanalysis about psychology’s current crisis. For the most part, this endeavour has taken on the form of providing psychoanalytically informed perspectives on psychology’s crisis and highlighting venues for fruitful cooperation. Coming to an end, one should be reminded that the term ‘crisis’ originally comes from the medical context and concerns that point of an affliction’s curve at which it can either aggravate or alleviate (Benetka, [2018](#)). Accordingly, we want to provide some concrete, psychoanalytic recommendations for helping with psychology’s crisis.

- Psychological self-awareness and self-reflection can prove to be a useful tool to improve quality of teaching, scientific dispute and scientific research.
- Ask yourself what would happen if you made use of what appears as a ‘distortion’ in the experimental setting rather than trying to control or exclude it.
- Consider the psychological background of the logic of science, for instance, what are the emotional consequences for a researcher ‘who is falsified’ and how does he or she try to cope with them in later research?
- Reflect upon the interplay of the ‘average’ person and the ‘particular’ person in psychological research and its implications.
- Distinguish between generalization on the level of predictions and generalization on the level of theory building.
- Conceive of psychology’s crisis also as a personal crisis of psychologists.

We want to encourage researchers to consider this open-ended list creatively in any way that seems profitable for their own research. A productive venue for the future of psychoanalytical contributions to theoretical psychology lies in developing this list further into a—pun intended—*checklist for psychoanalytically informed research planning and theory building*.

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Footnotes

¹ We use the term ‘academic psychology’ in opposition to psychoanalysis for several reasons. On the one hand, want to avoid undifferentiated talk of ‘mainstream psychology’. On the other hand, it highlights that the reality of psychoanalysis nowadays does not primarily take place at universities, but at dedicated institutes. This terminology will allow us to challenge the discursive presuppositions that undergird the process in which psychoanalysis was ostracized from academic psychology.

² This thesis must not be conflated with psychologism.

17. The Nine Lives of Phenomenological Psychology

Henderikus J. Stam¹ 

(¹) University of Calgary, Calgary, Canada

 **Henderikus J. Stam**

Email: stam@ucalgary.ca

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The Nine Lives of Phenomenological Psychology

It has become a stable, but time-worn cliché that is repeated in textbooks and psychology courses throughout the world: psychology is the science of mind and behavior.¹ There are multiple variations on this definition but most still rely heavily on some link between mind/consciousness and behavior. And while behavior is, on the face of it, an obvious feature of human and animal life (but note the traps that emerge as soon as one tries to define it), mind and consciousness remain more opaque. My aim in this chapter is not to address the vast literature on mind and/or consciousness—it is instead to point out that what waylays the definition of those terms is what philosophers have called “first-person consciousness” or what elsewhere in the social sciences is simply referred to as “subjectivity.” It remains one of the most difficult and at once seemingly easiest to understand, namely, that

psychology must at some point in its investigations come to address the problem of first-person consciousness or first-person experience. That it has not been able to satisfactorily address this is most obviously visible in its fragmentation and divisive approaches to all things subjective. Although equally divided, psychoanalysis has at least given us a narrative on subjectivity that targets the right problem. Psychology's variegated means of addressing subjectivity can only be accounted for by the inability to find a lever on this, one of the central questions of the discipline.

The most serious and in a way most successful approaches to subjectivity derive in one way or another from phenomenology. Alexander Wendt ([2024](#)) recently made a cogent argument that phenomenology can provide a "rigorous discourse on foundation." Contemporary psychological approaches to understanding first-person subjectivity are indebted to phenomenology as a mode of theorizing and conducting research. This is not always obvious or necessarily directly derivative and I will consider several approaches later in this chapter to illustrate the case. The question then is, does it matter? Is there any sense in which it makes a difference to acknowledge a precursor or an important foundational position? Psychology, in its most scientific moments eschews most forms of historical acknowledgment. Short of a potted history that is trotted out on occasion to legitimize its status as a science (Wundt, Watson, Skinner, followed by a host of cognitive scientists), psychology has little use for endless foundational exhumations. My claim here will be that such historical acknowledgments are necessary if only to throw light on the somewhat limited and often trivial approaches that count as understanding "subjectivity." On the other hand, I don't want to suggest we should turn back the clock to a phenomenological psychology that was not only difficult because of its introduction of an entirely new discourse, but also found few takers in North America. The point is to see what phenomenology can still teach us by way of a discourse on foundation.

Phenomenological Psychology

Phenomenological psychology has remained a constant in or around psychology for more than a century. Frequently associated with the work of Edmund Husserl (1859–1938),² it remains a small but apparently viable orientation in contemporary psychology. It would be difficult to call it a “theory” or a “practice” because there are multiple versions of phenomenology at play. In addition, since the term *phenomenology* is often used to describe experience more broadly, we can expect it to show up in various ways in psychology. The Merriam-Webster dictionary ([n.d.](#)) includes the following definition under the heading “Medical Definition”: “the way in which one perceives and interprets events and one’s relationship to them, in contrast, both to one’s objective responses to stimuli and to any inferred unconscious motivation for one’s behavior.” Here phenomenology refers to little more than the perception and interpretation of the world and is far removed from the origins and developments of the philosophical position formally recognized as *phenomenology*.

Phenomenology is a polysemic concept, at once difficult, sometimes obscure, and frequently used as a stand-in for subjectivity, first-person experience, and the like. Phenomenological societies, journals, and books are present on every continent of the globe, without showing any signs of diminishing. Nonetheless, phenomenological *psychology* does not denote a coherent school, approach, or tradition; it serves as a configuration for a very heterogeneous set of projects. There is a large variety of post-Husserlian phenomenologies, some of which have taken root while others have dried on the vine (e.g., Miron, [2016](#)). In these varieties of phenomenological psychology there exists a stubborn continuity that addresses a feature of human existence not captured by other psychologies.

Phenomenology, as is well known, started as a much more serious attempt to settle philosophical problems. Edmund Husserl was the founder of this philosophy but a clear statement of the position did not emerge until the two-volume publication of *Logical Investigations*

published in 1900–1901. From the very beginning, however, it was clear that the new experimental psychology of the late nineteenth century was going to be a foil for the new philosophy of phenomenology. Indeed, as Kusch ([1995](#)) has noted, Husserl was constantly preoccupied with psychology throughout his lifetime. Originally this came out of his embeddedness in the debate of the late nineteenth century on *psychologism* or what was known as the *Psychologismus-Streit* in German philosophical circles from, roughly, 1890 to 1914.

This particular argument began when Husserl published his philosophy of arithmetic in [1891](#), after having completed a PhD in mathematics in Vienna in 1883 and a habilitation on the concept of number in Halle under Carl Stumpf in 1887. His philosophy of arithmetic, *Philosophie der Arithmetik: Logische und Psychologische Untersuchungen*, was reviewed by Gottlob Frege (1848–1925) who argued that Husserl’s introduction of psychological principles in logic and mathematics was misguided and a form of subjectivism. Husserl’s response came six years later with his two-volume *Logische Untersuchungen (Logical Investigations)*. Having thoroughly accepted Frege’s critique, Husserl now furthers the argument over psychologism by saying that logic is a practical-normative discipline whose task it is to evaluate methods of scientific justification (Kusch, [1995](#)). The so-called *Psychologismus-Streit* that engages German-speaking philosophers up to World War I places philosophy and psychology in an opposition that was part real, part manufactured largely because of the academic resources at issue.

While having taken a position at Göttingen University from 1901 to 1916, Husserl continued the development of his phenomenology. He redefined the word phenomenology to mean a science of consciousness and moved it away from Brentano’s use as “descriptive psychology.” The first volume of the *Logical Investigations* was primarily concerned with a detailed critique of psychologism. It was in the second volume that Husserl argued that “pure” phenomenology was a neutral foundation of the sciences.

It is, on the one hand, an ancillary to *psychology* conceived as an *empirical science*. Proceeding in purely intuitive fashion, it analyses and describes in their essential generality—in the specific guise of a phenomenology of thought and knowledge—the experiences of presentation, judgment and knowledge, experiences which, treated as classes of real events in the natural context of zoological reality, receive a scientific probing at the hands of empirical psychology. Phenomenology, on the other hand, lays bare the ‘sources’ from which the basic concepts and ideal laws of *pure* logic ‘flow,’ and back to which they must once more be traced so as to give them all the ‘clearness and distinctness’ needed for an understanding, and for an epistemological critique, of pure logic. (Husserl, 1900–1901/[1968](#), p. xxviii)

From the outset then phenomenology was contrasted to the newly emerging, empirical psychology that had by 1900 embedded itself in the German-speaking universities. By clarifying the role of logic, Husserl was able to contend that logic was a preliminary to science, making logic something beyond psychology. However, more important was the clarification that Husserl provided to Brentano’s notion of intentionality. Brentano had revived this notion once current in medieval scholasticism by arguing that

Every mental phenomenon includes something as object within itself, although they do not do so in the same way. In presentation, something is presented, in judgment something is affirmed or denied, in love loved, in hate hated, in desire desired and so on. This intentional inexistence is characteristic exclusively of mental phenomena. No physical phenomenon exhibits anything like it. We can, therefore, define mental phenomena by saying that they are those phenomena which contain an object intentionally within themselves. (Brentano, 1874/[1995](#), pp. 88–89)

Husserl argued that some presentations have no physical object. In the second volume of the *Logical Investigations* he distinguished between intuition and representation. As Moran ([2001](#), p. xxxviii) noted,

By the late 1890s Husserl had developed the main elements of his account of the relations between signs and things signified, between intentions and their intuitive fulfilments, but it seems likely that his crucial distinction between *sensuous* acts and acts of *categorical* intuition did not emerge until he began writing the six *Investigations* themselves. This notion of categorical intuition, a distinct intuition of complexes founded on sensory intuition, opened up the proper domain of phenomenological viewing as Husserl would develop it after 1901.

Having been dissatisfied with parts of the *Logical Investigations*, Husserl published his major work in 1913: *Ideen zu einer reinen Phänomenologie und phänomenologischen Philosophie* (*Ideas: General Introduction to Pure Phenomenology* (Husserl, 1913/ [1931](#)). Here however it is important to note again that this was not an exercise in philosophy alone. The rapid emergence in experimental psychology in the late nineteenth century and first decade of the twentieth century in German-speaking universities was carried out exclusively in philosophy departments. Unlike their American colleagues (who were mostly trained in German institutions), German psychologists occupied philosophy chairs and created institutes, divisions, and seminars within philosophy departments. Psychology was philosophy! As Kusch ([1995](#), pp. 122–127) points out however, by 1913, 10 of the 44 professorships in philosophy in German-speaking universities were held by a new generation of philosophers, experimental psychologists.³

What began as a branch of philosophy soon became a source of frustration to the philosophers. Kusch ([1995](#), [2024](#)) recounts the debates involving Husserl, Wilhelm Dilthey, Wilhelm Windelband, and Heinrich Rickert among others. In 1913, the same year that Husserl published his major work on phenomenology, a petition circulated

among German-speaking philosophers which was signed by 107 philosophers from Germany, Austria, and Switzerland. The petition demanded that experimental psychologists should be prevented from occupying any more philosophical chairs. All German-language universities and ministries of education received a copy of the petition and called on governments to create independent departments of psychology since psychology was an independent discipline.

World War I put an end to that debate as almost all academics supported the war effort at the outset. Nonetheless, what had begun as a question about psychology had also created the search for a philosophy that was true to the tradition and unbound from the vagaries of experimental psychology—phenomenology was an obvious candidate. Some psychologists meanwhile were engaged during the war in the training and testing of soldiers, just as American psychologists mark the beginning of a huge testing industry in the testing of soldiers in World War I—this was a clear division of labor (Kusch, [1995](#)). Following the war, the Weimar Republic was characterized by a period of crises, at least until 1923 and again after the beginning of the depression in 1929 (Weitz, [2018](#)). According to Kusch ([1995](#)) “both academic pure philosophy and experimental psychology had to cope with, and accommodate to, an intellectual environment that was hostile to science, rationality and systematic knowledge” (p. 206) perhaps best characterized by Oswald Spengler’s popular book *The Decline of the West*.

Martin Heidegger created the intellectual counterpoint to Spengler’s anti-science *Lebensphilosophie*. But Husserl was now the major representative of phenomenology and consciousness, as expressed in his *Ideen*, which became one of the central questions of the new philosophy. During his lifetime however not much of Husserl’s work was actually published and only in the last few decades have scholars analyzed the vast legacy of Husserl’s writings. Nonetheless, some of his late-published works turned out to be important for his views on psychology. In the last work that appeared during his lifetime, *The Crisis of European Sciences and Transcendental Phenomenology* (Husserl,

1936/ [1970](#)), Husserl's claims for a transcendental phenomenology were again adjudicated against empirical psychology, which he viewed as a failed science (Moran, [2012](#)). Psychology could not comprehend subjectivity as it was still caught in the Cartesian split between objectivity and subjectivity. A true phenomenological psychology was yet to be created but it would precede and subsume empirical studies in psychology. In the *Crisis* he defends his transcendental idealism. By this he means that, according to Mohanty ([2006](#)) consciousness comes to possess "absolute being." He wishes to locate phenomenology in the place of the first science, preceding any and all claims of the sciences. Here too then psychology comes *after* the phenomenological grasp of essences; it is the descriptive theory of the "essence of pure experience." By bracketing one suspends judgment to arrive at the phenomena as they are given in consciousness.⁴

Both Max Scheler, an important figure for his influence on Dutch phenomenology, and Heidegger would take up the new phenomenology almost immediately and move it in different directions. Having rejected experimental psychology as a foundation for phenomenology in the late 1890s and early 1900s, this theme was taken up again in the 1920s as psychology gradually established itself as a discipline in the German-speaking countries and expanded rapidly in North America. Phenomenologists in the 1920s followed Husserl in relegating psychology to a minor science and an unimportant one at that. An objective science of consciousness could only be achieved by a phenomenological psychology that is subsumed under a transcendental phenomenological philosophy.

The ability of phenomenologists to come to unprejudiced justifications of their views on the world would lead many to question the validity of phenomenology. Husserl however saw it as his task to show how phenomenology must not only precede psychology but would create a new kind of psychology, a phenomenological psychology. It would create a transcendently directed phenomenology that would change psychology, producing a descriptive psychology with an "original intuitional essence" (Beyer, [2022](#), p. 26). In his Amsterdam

lectures of 1928, Husserl again addressed phenomenological psychology and transcendental phenomenology (Husserl, 1928/ [1997](#)). Husserl introduced transcendental phenomenology as a “first philosophy,” or “the philosophical science of the sources from which philosophy springs” (p. 214). Phenomenological psychology would, however, provide the foundations for a scientific psychology. Phenomenological psychology was independent of scientific psychology since the former was dependent on intuition and reflection as the source of its knowledge.

Before Husserl’s death and the salvaging of his unpublished writings by Herman van Breda (Horsten, [2018](#)), there were already numerous interpretations of his work. I have mentioned Heidegger as the most obvious and immediate of Husserl’s heirs but even Maurice Merleau-Ponty already referred to “second generation” and “third generation” Husserlians in his course notes of 1959–1961 (Merleau-Ponty, [2002](#)). Furthermore, Merleau-Ponty also notes that phenomenology remains unfinished, “what is essential is not said [...] the public has not yet learned this language [...] if one seeks to didactical, to define everything, one is understood even less [...] the new thought is not visible in relief” (Merleau-Ponty, [2002](#), 11–12). So for Merleau-Ponty, Husserl cannot proceed progressively, the work is always “preparatory”: “His work is a *trace* of his monologue” (p. 12).

In his major essay on phenomenology and psychology, Merleau-Ponty (1961/ [1964](#)) repeatedly returned to the Husserl’s conflicted approach to the relationship between philosophy and psychology. Ultimately Merleau-Ponty believed that the antipathy gave way to Husserl’s understanding of philosophy and psychology as one of “interdependence and reciprocity” (p. 94). The opposition was “only a point of departure.” Psychology, however, argues Merleau-Ponty, is neither inductive (as empiricists would have it) nor is it a return to an a priori as some philosophers would have it. Psychological reflection is a finding, the meaning of which can only be found through an “effective contact with my own behavior and that of others. Phenomenological psychology is, therefore, a search for the essence, or meaning but not

apart from the facts” (Merleau-Ponty, 1961/ [1964](#), p. 95). Elsewhere Merleau-Ponty had suggested that “phenomenology can be practiced and identified as a *manner or style* of thinking [...] before arriving at complete awareness of itself as a philosophy” (Merleau-Ponty, [1962](#), p. viii). It leaves open the question, raised by Steven Crowell ([2002](#), p. 420) among others, that phenomenology also consists in an “orientation toward a common set of issues defined by commitment to a relatively explicitly protocol; a mentality willing to limit itself to what can be pursued methodologically and to leave alone all that cannot be; in short, something like a *research program*” (italics in original). But is phenomenology anything like a research program? Here is the rub. In the attempts to create a viable phenomenological psychology psychologists want to know: What does it look like and can it ever be systematized in a fashion acceptable to the standards of psychology as the discipline exists today?

In both Husserl and Merleau-Ponty, phenomenology as a foundational philosophy could not provide a solution to the problem of psychology—that is—to the problem of just what a phenomenological psychology should look like and how it could compete with experimental psychology beyond claiming that it should be the foundation of any experimental psychology. On the presumptions of the extant experimental psychologies, as they were developing in the mid-twentieth century, psychology should be a science, even if the notion of science was not agreed upon. That is, even if not all sciences looked like the positivist-inspired versions focused on experience (observation) as the final arbiter of scientific results, psychology should be open and accountable through the potential replication of research forming the basis of any and all claims. On the other hand, there came the emergence of a phenomenological psychology that, independent of philosophy, struggled with the possibility of trying to achieve what looked like something Husserl had hoped for as a phenomenological psychology. What is clear about these attempts is that there was no blueprint or clear outline of this psychology. It required a working out of the foundations and methodologies, particularly the latter, given

psychology's overwhelming penchant for what would later be called "methodolatry."⁵

The Search for a Phenomenological Psychology⁶

It was not long after World War II that several European and North American psychologists took up Husserl's challenge and began to articulate a phenomenological psychology. In addition, a number of psychologists who had been involved in research activities of various sorts adopted a phenomenological approach, not always calling it that. For example, the Belgian psychologist Albert Michotte published his groundbreaking work *La perception de la causalité* in 1946. It was often seen as an important phenomenal if not phenomenological aspect of perception (e.g., Bertamini & Hulleman, [2006](#); Thinès et al., [1991](#)). Indeed, Bertamini and Hulleman refer to Michotte's "experimental phenomenology" when describing a set of Michotte's experiments on regions visible through holes. Others have noted that Michotte's work on causal perception was a form of "experimental phenomenology" (Wagemans et al., [2006](#)). Despite this, Michotte was not out to create an experimental phenomenological psychology. That he happened upon this path was noted especially by others after the fact.⁷ Elsewhere German psychologists were also concerned with the creation of a phenomenological psychology, perhaps best known among them was Carl Graumann (e.g., Graumann, [1964](#)).⁸

The Netherlands was the site of one of the most ambitious attempts to create a phenomenological psychology. Centered at the University of Utrecht, it included colleagues in psychiatry, criminology, and psychology and was promptly mislabeled as the "Utrecht School" of phenomenology. It never functioned as a proper school despite the overlapping interests of the colleagues associated with it. As van Hezewijk and I have pointed out in detail (van Hezewijk & Stam, [2024](#)), the notion of a school arose from the perspective of outsiders looking in

and the particularities of a “school of thought” were never quite present. I will focus here briefly on two figures at Utrecht University, Frederik Buytendijk and his successor, Johannes Linschoten. There were others at the university involved in creating the impression of a school of phenomenology in the social sciences, these two figures remain central. Buytendijk, trained as a physician and physiologist, was appointed to the chair of psychology in Utrecht in 1946 which gave him the freedom to attempt the creation of a phenomenological psychology in this program. Before the expansion of the universities in the early 1960s, it was still possible to conduct seminars and classes with small numbers of students, ask them to read complex philosophical works as well as literature, and lecture in a magisterial manner.

What made the work of Buytendijk and the earlier work of Linschoten unique was their emphasis on phenomenology as a theoretical and practical kind of occupation while both worked in the laboratory on much more traditional topics. Indeed, by the end of his career (cut short by his early death at the age of 38) Linschoten had written a book that appeared to those not familiar with the majority of his work to have undermined and critiqued the very possibility of a phenomenological psychology (Linschoten, [1964](#)). But prior to this more critical review of phenomenological psychology, Linschoten too had written extensive articles on such topics as sleep, humor, sexuality, and so on. Both Buytendijk and Linschoten were actively involved in various social organizations, both Roman Catholic and secular. Their understanding of psychology included a vision of the discipline as a socially relevant, interventionist one capable of addressing real social problems. Various organizations emerged in the post-war period that supported this role, such as The Netherlands Institute of Psychologists which was founded in 1938, just before the outbreak of World War II.

Neither Buytendijk nor Linschoten (nor any of their colleagues in Utrecht) formulated a coherent phenomenological psychology. Linschoten came closest however with the publication of his most well-known book internationally (translated into English and German) on William James (Linschoten, [1959](#)). Here he qualifies James as a

forerunner of phenomenological psychology by showing how James just stopped short of a phenomenological psychology. Husserl read James (although not the crucial “A world of pure experience”) but James appeared not to have read any of Husserl’s work. Linschoten argues that James could not make the final step to a phenomenological psychology because he lacked a conception of intentionality, which Husserl derived and explicated from Brentano’s notion of “intentional inexistence.” The crucial notion in Husserl is as follows: the intentional act is not dependent on the existence of an object independently of the act itself allowing for the phenomenological reduction through bracketing.

Much ink has been spilled on various explanations, extensions, and critiques of the problem of intentionality in phenomenology. However, one other feature of James that attracted Linschoten was the former’s appeal to physiology or an objective psychology that was to be included in the frame of a descriptive psychology (Linschoten, [1959](#), p. 229). As van Hezewijk and I ([2024](#)) have noted, Linschoten’s final work published after his death was taken publicly to be a rejection of phenomenology in favor of an experimental, positivist scientific psychology. We argue that this is in fact a false assumption and that taken as a whole, Linschoten’s work was an attempt to wed an experimental psychology with phenomenological insights, historically constituted. Nonetheless, with Linschoten’s death the explicit emphasis on phenomenology at Utrecht University came to an end. The influx of students and the growing popularity of a standardized form of education that could be formulaically taught to undergraduates made it almost impossible to continue the phenomenological approach. Nonetheless, individuals in the Netherlands who had been associated with phenomenology continued to publish such works well into the following decades.

In other locations throughout Europe, various attempts were made to connect Husserl’s phenomenological psychology to a research program in psychology. I do not have the space to provide a review of these efforts since some no longer exist while others are in various

stages of change (see, e.g., the Copenhagen Center for Phenomenological Psychology and Aesthetics⁹). What is more important is the fact they exist at all and that they exist mostly in parallel with mainstream psychology (even if there are major exceptions such as Varela et al., [1991](#)). The overlap is minimal and can be understood primarily as a function of the difficult philosophical content required to master phenomenology in the first instance. However, after World War II there was no consensus on what the new psychology would look like in European universities, what its methodologies would include, how its content would be decided, and how much of the historical remnants of German psychology would make it into a post-war psychology. Furthermore, American psychology had excelled at establishing itself within the institutional structures of the modern universities in a way that was only beginning in European countries (Capshew, [1999](#); Dehue, [1995](#); Métraux, [1985](#); Pickren, [2007](#)). Post WWII, many European psychologists received their education or would spend time in America in various laboratories. As American psychology (and the English language) became dominant in European psychology programs, a standard form of psychology education slowly began to take hold eventually coalescing in what could be called a North-Atlantic model. Although there were holdouts, especially in Eastern European countries before 1989, an experimental psychology and an evidence-based clinical psychology would, by the beginning of the twenty-first century be a stable feature or an aspirational one of most psychology programs in Europe and North America. I will now turn to the American context of phenomenological psychology, a complicated history at best.

Phenomenological Psychology in America

Humanistic Psychology

Humanistic psychology was considered, for some time, an offshoot of European philosophical movements such as phenomenology and existentialism. For example, the collection by James Bugental ([1967](#)),

which contains contributions by what was then the majority of self-labeled participants in humanistic psychology, was actually a collection of positions trying to articulate some coherence to this new direction. Occasionally referred to as a “third way” to distinguish itself from behaviorism and psychoanalysis, it included individuals who were affiliated with US universities or were engaged in some form of clinical practice. Indeed, right from the outset humanistic psychology was seen as both a corrective to clinical and counseling psychology (e.g., Carl Rogers, Rollo May) as well as a new form of research psychology (e.g., Abraham Maslow, Joseph Royce). But in its resistance to behaviorism and psychoanalysis it did not forge a unitary phenomenological psychology and instead created a number of branches. Much of this was likely due to its need to focus on clinical and counselling psychology and its inability to compete with the research traditions of what became experimental, scientific psychology. It was the latter after all that would be richly rewarded with research grants, laboratories, and publication outlets after World War II in the context of the Cold War (e.g., Robin, [2003](#)).

Duquesne University

The most visible of the attempts at creating a phenomenological psychology came with the foundation of Duquesne’s psychology department. Misiak and Sexton ([1966](#), p. 62) called Duquesne University “the capital of phenomenological psychology in the New World,” an overstatement by several degrees. However, it was Father Adrian van Kaam, who was the first head of the psychology department and who ensured the foundations of what, for him, were the *existential* foundations of the psychology program (see Van Kaam, [1966](#)). Although not a major academic institution, van Kaam realized that as a catholic university it was possible to create a different kind of program there even if the undergraduate students came largely from the working classes of Pittsburgh in the early years of the program (van Kaam, [2004](#)). Amedeo Giorgi became the most visible of those contributors in the early years and he has pursued a phenomenological program for his

entire career. Several hundred graduates of that program have continued the traditions, even if they are not explicitly phenomenological but more broadly considered “human science” traditions. A history of that program was published in 2002 (Smith, [2002](#)) and a history of phenomenological psychology in the United States more generally was published by Churchill et al. ([2021](#)). Giorgi himself has written about the difficulties associated with founding the *Journal of Phenomenological Psychology* (Giorgi, [1998](#), [2012a](#)).

Amedeo Giorgi has tended to be understood as generally representative of the late twentieth-century phenomenological psychology in the English-speaking world. He has published several books and numerous journal articles outlining his approach and he has supervised several hundred dissertations (Giorgi, [1970](#), [2009](#)). In his early years at Duquesne he traveled to the Netherlands to meet, among others, Buytendijk and Linschoten. He also traveled to Bonn in Germany to meet Graumann. In fact, Linschoten, Graumann, and Giorgi met together in Bonn and apparently agreed to be the vanguard of the new phenomenological psychology (Giorgi, [1999](#)).¹⁰ In 1986, Giorgi moved to Saybrook University (with a five-year interlude at the University of Quebec at Montreal). His life has been devoted to the expression of a particular method of phenomenological psychology as clarified in his writings, perhaps best summarized as the descriptive phenomenological psychological method (Giorgi, [2012a](#)).

For Giorgi, this method begins by assuming a correct attitude, the attitude of the phenomenological reduction. The bracketing that is required is for Giorgi crucial, requiring a “psychological attitude.” Giorgi separates the transcendental phenomenological reduction from a psychological phenomenological reduction which includes only the “object of consciousness” (Giorgi, [2012a](#), p. 5). The method consists in gradually transforming “meaning units” into expressions of psychological import. I am obviously not doing justice to the process in such a short description but Giorgi himself has published multiple guides to this method as have his many students (e.g., Giorgi, [2009](#)).

What is crucial in this context is in fact the notion of method in the first place. A North American psychology student or even a psychologist will frequently ask, but how do I do this? Hence the clear articulation of a methodology meant to show the student the way. Giorgi himself acknowledged this in another interview:

And I would say developing a method where there was a vacuum probably is the biggest thing, and probably I would almost say “too big,” because it has dominated my life [laughter]. I didn’t mean for method to take such a role in my life—people heard there was this method, and they wanted me to teach a workshop, or go and teach. And my idea was to meet [...] the task: Duquesne wanted me to go there and develop a method [...] So, after fifty years of doing method, I’m tired! [...] It has been a good thing, but also a distraction, in the sense that I couldn’t let go of method, that’s where everybody was pulling me, toward the method. (Giorgi, [2012b](#))

In addition to Duquesne, programs at Saybrook University, the University of Dallas, University of West Georgia, as well as other locations continued to promote, if not phenomenological psychology, then at least a humanistic, alternative approach to psychology that was sometimes influenced by phenomenology. What is interesting about these developments is that individuals who were faculty in these locations developed unique approaches to phenomenological psychology. There is no singular program or research tradition that is called “phenomenological psychology” but multiple, inspired, and sometimes clinically oriented approaches. They can be influenced not only by Husserl but also by thinkers as diverse as Rollo May, Maurice Merleau-Ponty, Paul Ricoeur, Jean-Paul Sartre, among others (see Churchill et al., [2021](#), for an outline of developments in the US).

Qualitative Methods

It will not escape even the most superficial overview of phenomenological psychology that much of it is couched in terms of

“qualitative methods.” At least in the English-speaking universities of the world if not elsewhere (and I include here many European universities that teach psychology exclusively in English such as those in the Netherlands), a concern with so-called qualitative methods has been formalized in recent years in courses, journals, research approaches, and even learned societies. With the demise of postmodernism (which I will not attempt to define here save to note its use as a label), “qualitative methods” became a code-word for alternative approaches to psychology. This included discourse analysis, grounded theory, social constructionism, and more. Phenomenological psychology, which was always a kind of qualitative method when considered methodologically (but not in the hands of philosophers), seems to have settled into this niche. Sometimes this is explicit as in Jonathan Smith’s “interpretative phenomenological analysis” which advertises itself as a qualitative method (Smith et al., [2022](#)). As a now recognized approach to psychological research, qualitative methods have reinforced what has been psychology’s primacy of method over theory and the requirement of justification. It seems that for the past hundred years or so, psychology without a clear method is not a psychology worthy of the name. Phenomenological psychology has suffered under this requirement.

Conclusions

This very brief sortie through a theoretically sophisticated understanding is perhaps not particularly illuminating save for one question that bedevils the discipline of psychology. Just what is subjectivity and how do we come to understand the Other in our quest to develop a psychology. Our experimental methods, the neurosciences, personality inventories, and the like are fine so far as they have illuminated limited, local aspects of human experience. But they only delve so deep. After more than a century we have not come very far, I would tender, in our understanding of people. Both psychology’s methods and its commitment to a particular kind of functionalism have

hamstrung the discipline for much of its history. The kinds of theories the discipline has produced are best characterized by what I have called elsewhere *indeterminate functionalism* (Stam, [2010](#), [2015](#)). By indeterminate functionalism I mean those functions that are described in most research studies by functions-as-activities. Hence remembering a list of words (to take a trivial example) leads one to speculate on “short-term memory.” But the act of remembering consists in an *activity* of trying to recall a list of words. This is obvious, except the resulting concept of a short-term memory is open-ended. It provides a flexible category that can be expanded indefinitely. So we have not only short-term memory but when the concept no longer fits we can expand it to include any of a hundred different kinds of memory concepts (Tulving, [2007](#)). Because it is a *functional* entity and not a material object it has an inherent flexibility. However, there is no in principle limit to the way we can expand functional categories. One can imagine a new procedure to expand the boundaries of one’s functional categories. In practice, researchers hang promissory notes on their functional categories, by, for example, arguing that there may be some neurological foundation found for the latest functional memory category. But, (also) in practice, these never seem to show up and account for the very functional properties at play. Hence the inherent flexibility of functional categories allows the psychological project to continue without end. However, is science not like this, one might argue. Every finding opens up multiple new questions. But unlike the search for, say, additives in plastics that do not poison the catalyst in recycling (Ngu et al., [2025](#)), the functional categories do not at some point locate themselves in a material categories, they float instead on a sea of possible extensions.

Phenomenological psychology, one might argue, is not subject to these constraints. At least if we take Husserl seriously we may arrive at “the things themselves.” But it is a difficult journey and psychology is a practical task master. It continually demands to “show me.” Hence a subtle practicing of an *epoche* leaves the scientific hypothesis outside its frame of reference. And since the *epoche* is meant to return us a concrete lived experience, we are not in the realm of generating

functional categories. Eidetic intuition then leads to clarification or essences on some accounts. This is a long way from any kind of quick psychological explanation; it is also a difficult road for the neophyte and not taught in a quick undergraduate course.

What is left then for a theory of subjectivity? Although the notion of “subjectivity” was revived with the advent of “postmodern” authors such as Michel Foucault, Jacques Derrida, and Jacques Lacan as well as feminist thinkers such as Luce Irigaray and Julia Kristeva, its use in psychology has been limited. This is surprising since one might argue that it constitutes the core of the discipline! Just what is subjectivity? How does one understand first-person consciousness? What is it to create a proper theory of subjectivity? Or are we thrown back on psychoanalysis again, as we were a hundred years ago except now in some post-Lacanian version? Phenomenological psychology was a robust attempt to break through this impasse. It still lives out there, it deserves our attention.

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Footnotes

¹ It is also the first element of the definition of psychology in the APA dictionary. <https://dictionary.apa.org/psychology>

² Thanks to Alexander Wendt for pointing out that there are multiple origins for phenomenology, some of which are noted in general histories of phenomenology (e.g., Smith, [2018](#); Spiegelberg, [1960](#)). Husserl himself studied with Franz Brentano from 1884 to 1886 and Herzog ([1992](#)) includes William James, Brentano, Henri Bergson, Carl Stumpf, and Ernst Mach as important forerunners. Obviously, I don't have the space to outline a full history here.

³ Seven of these were first- or second-generation students of Wundt, whose capacity to graduate Ph.D.'s was remarkable. Of the 186 dissertations which he supervised, many would go on to be founding members of psychology departments or psychology laboratories around the world.

⁴ For a recent discussion of this see a special issue of the *Journal of Phenomenological Psychology*, 54, issue 1 of May 2023.

⁵ The origins of the widely used portmanteau, “methodolatry,” are obscure since many people have used it to describe psychology's peculiar obsession with methods and methodology. Furthermore, many people have claimed to falsely know the provenance of the term when, in fact, it originated in early twentieth-century philosophy and probably entered psychology via Gordon Allport and David Bakan. The most likely source is Jacob Loewenberg (Stam & Warren, [2015](#)).

⁶ I take Wendt's ([2024](#)) point that one can distinguish between “phenomenological psychology” and “psychological phenomenology” as a useful way of distinguishing narrower focused programs from those of a philosophical nature.

7 Michotte was interested in multiple aspects of science and psychology, as well as philosophy, the subject of his dissertation in 1900. In addition, he spent time in Wundt's lab in Leipzig as well as with Oswald Külpe in Würzburg. As a psychologist with a philosophical education he was typically interested in a wide variety of topics.

8 For a comprehensive overview of phenomenological psychology in Germany in the twentieth century see Herzog ([1992](#)). An updated review can be found in Wendt ([2024](#)).

9 <https://psychology.ku.dk/cppa/>

10 With Linschoten's early death and Giorgi's disappointment in Linschoten's apparent turn to experimental work, this did not pan out.

18. Toward a Neuro-Environmental Lens Meta-Model of Person-Situation Transactions

Martin Peper¹ 

(1) Faculty of Psychology, Neuropsychology Section, University of Marburg, Marburg, Germany

 **Martin Peper**

Email: martin.peper@uni-marburg.de

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Introduction

The theory crisis debate in psychology has identified several problems (e.g., Hutmacher & Franz, [2025](#)). A primary concern addresses the dominance of a multitude of isolated micro-theories that focus on narrow aspects of psychological phenomena without contributing to a coherent, overarching framework (Oberauer & Lewandowsky, [2019](#)). Psychology has long been considered highly fragmented, with subfields and research traditions operating relatively independently of one another (Royce & Powell, [1983](#)). Such fragmentation is visible in the diversity of concepts, methods, and epistemologies, making it difficult to establish shared foundations or to integrate findings (Fahrenberg, [2015](#)). Moreover, ambiguity of poorly defined psychological constructs complicates the development of robust, testable theories (Hutmacher &

Franz, [2025](#)). This lack of integration within psychology and across disciplines not only impedes internal progress, but also makes it difficult for interdisciplinary fields, such as neuroscientific psychology, to build on psychological theories and derive operationalizations (Ramminger et al., [2023](#)).

In response to these challenges, this chapter offers a framework that addresses some core issues of this crisis. An expanded *Brunswikian lens-type meta-model* is developed for studying the interactions between individual factors on the person side and contextual factors on the situation side. The term meta-model is used here because it does not describe a single, domain-specific theory. Instead, it operates at a higher level of abstraction by integrating psychological and situational perspectives into a unified framework. It provides principles and concepts for organizing, comparing, and guiding the development of diverse psychological theories, rather than advancing isolated hypotheses about specific phenomena. The model may thus serve as a lattice for theory construction and integration across multiple domains. It is theoretically open and permits the integration of various explanatory phenomena (e.g., action control, emotion regulation, reward processing, etc.) without being obliged to any specific paradigm (e.g., evolutionism, behaviorism, cognitivism, constructivism, etc.).

By providing an overarching meta-theory that suggests to systematically integrate neurobiological, psychological, and environmental levels of analysis, the current approach may help to select and guide specific research interests. It is intended to provide a unifying structure, enabling the comparison, integration, and further development of theories and models, and hopefully represents a step toward more coherent theory building. To this end, a broad range of relevant contributions are considered, particularly those from the field of evolutionary psychology (Buss, [1991](#)), functional neuroanatomy (Nieuwenhuis et al., [2008](#)), neuropsychology (Peper, [2018](#); Ramminger et al., [2023](#)), environmental psychology (Barker, [1968](#)), ambulatory psychophysiology (Fahrenberg & Myrtek, [1996](#); Peper & Loeffler, [2014](#)), phenomenological psychology (Graumann, [2000](#)), probabilistic

functionalism (Brunswik, [1952](#)), and multivariate reliability theory (Peper, [2018](#); Wittmann, [1988](#)).

In contrast to the naturalistic tradition suggesting a simple sequence of situational stimulus, neurocognitive processing, and subsequent behavior, the present account reflects multiple causal transactions including the possibility that environmental contexts shape brain-mediated behaviors and that neurocognitive states control environmental engagement. These domains co-constitute one another through irreducible transactions of emergent phenomena. This view is consistent with previous contributions regarding the ecological embedding of mental functions (Fuchs, [2018](#)), suggesting that the brain should be understood as deeply interconnected with the body and environment, emphasizing the embodied nature of cognition and perception and challenging the view of the brain as a detached computational system.

By integrating neural and environmental dimensions, guided by the organization of prefrontal brain systems that coordinate motivational, emotional, cognitive, and motor control, this model also aims to further the long-standing debate about the interactions between personality and situational factors (e.g., Buss, [2009](#); Rauthmann et al., [2020](#)). This may have implications for developing ecologically valid models and refining assessment procedures, particularly at the interface of environmental and neuroscientific psychology (Kühn, [2024](#)).

The chapter is organized as follows: First, a brief overview of foundational theories and models that address the relations between the brain, cognition, and the environment is provided. Second, the neuro-lens model (NLM; Peper, [2018](#)) is summarized. Third, an environmentally extended meta-model is proposed to coordinate the neuropsychological and environmental levels of explanation.

Meta-theoretical Considerations on Person-Environment Relations

Evolutionary Perspective

Recurrent adaptive problems in ancestral environments led to the development of specialized neural mechanisms that promoted survival and reproductive success (Buss, [2009](#); Neel et al., [2020](#)). Behavioral flexibility is essential to this end, as it allows the organism to adapt responses to changes in environmental demands (Međedović, [2018](#)): effective decision-making depends on the detection of relevant environmental cues and the evaluation of shifting cost-benefit trade-offs. These responses reflect automatic, biologically prepared processes to ancestrally significant stimuli. The proximate psychological mechanisms, which are rooted in neurogenetically determined architectures, have evolved to support this adaptive flexibility (Neuberg et al., [2010](#)).

Behavioral flexibility provides support for major adaptive goals, such as resource acquisition, threat avoidance, and social coordination (Buss, [1991](#)). Importantly, evolutionary theories emphasized that behavioral flexibility itself is context-dependent and incurs costs (Međedović, [2018](#)): Intermediate levels of plasticity may optimize fitness across variable environments, whereas rigidity or stability may be more adaptive under highly unpredictable or stable conditions. The development of executive control systems that support the detection of environmental cues and behavioral adaptation is therefore likely to have required considerable evolutionary investment.

For instance, rodents construct complex spatial maps that also enable them to navigate resources and regulate social interactions within their habitat (Wallace et al., [2002](#)). This suggests that the rodent brain, and by extension the human brain, is evolutionarily equipped not only to react to physical properties, but also to actively shape the environment corresponding to biologically prepared motivational mechanisms. Advanced learning systems integrate motivational, emotional and cognitive signals into multidimensional representations of the environment that enhance strategic adaptation. These capacities for both cognitive environmental learning and socio-emotional regulation provide a neuropsychological foundation for modeling

human person-situation transactions, as discussed in more detail in the subsequent sections.

Person and Environment

From a biological perspective, environments are defined by perceptual and operational constraints. Sensory-motor capacities, innate instincts and executive control functions determine how organisms perceive and act. Human environments also incorporate informational, emotional, and social elements (Lewin, [2012](#)/1963). Individuals actively reflect on, engage with, and modify their surroundings, constructing meaningful contexts through action. Environmental experiences are thus fundamentally shaped by cultural and social relationships, through which meaning is acquired via communication, emotional exchange and shared practice (Graumann, [2000](#)).

Human environments are designed to offer multiple affordance structures that promote navigation, engagement, and consumption (e.g., Barker, [1968](#)). Urban environments are structured to elicit specific behaviors and experiences: various landmarks and cues require individuals to form adaptive mental maps that guide motivated perception, decision-making and behavior. Adaptively responding to environments requires the integrated functioning of motivational, emotional, cognitive, and motor systems, which is coordinated by executive brain functions. Understanding these neuropsychological processes as well as their situational affordance counterparts is essential for modeling the transactional nature of person-situation relationships in psychology.

An essential step toward transactions lies in examining how individuals perceive, interpret, and respond to contexts (e.g., Rauthmann et al., [2016](#)). Early theoretical accounts emphasized that environmental stimuli are not perceived passively but undergo subjective transformation. Murray ([1947](#)) called this subjective interpretation of situational cues the *beta press*, in contrast to the *alpha press*, which was thought to be concerned with the 'objective' side of situational stimuli. The distinction between an objective reality of

stimuli and their individual perception corresponds to a more recent notion of hierarchically structured environmental meaning with 'objective' cues at the lowest level (e.g., Rauthmann et al., [2020](#)).

However, such approaches may fail to account for the evolutionary organization of neurocognitive systems: Even very early stages of visual processing are modulated by prior learning, expectations, and emotional biases (cf. Schmidt-Atzert et al., [2026](#)), rendering the supposition of an objectively perceived reality improbable. An emergentist lens model might help to rethink previous suggestions without claiming to have solved the reality problem: 'objective' reality might be reframed as a type of intersubjective, higher-level property emerging from beta press experiences.

Within both the neuropsychological system as well as the structures of the physical environment, separate and categorically different emergence phenomena can be assumed: (1) Neuropsychological functions are realized within the physical reality of neurobiology and exhibit non-reductive emergence: for instance, higher-order executive control functions arise through the binding of cognitive, motivational and emotional subfunctions. This corresponds to a *strong emergence* theory (e.g., Chalmers, [2006](#)), where novel neuropsychological properties are irreducible to the functional states observed at the subordinate level of neuron assemblies. (2i) Within the environment, affordance structures (Gibson, [1979](#)) are constituted by emergent dimensions of various domains of meaningful cues that exhibit layered properties and form a hierarchy of situational classes (*weak emergence*). Affordances are clearly located in the natural or human-designed physical reality; however, the arising meaning and function cannot be reduced to a local accumulation of physical matter (*strong emergence*). The perspective adopted in the following is that of ontologically *indifferent monism* and *methodological parallelism* (e.g., Fahrenberg, [2015](#)).

Probabilistic Models

Behavior in the environment typically occurs under conditions of uncertainty. Environmental cues often fail to provide definitive or deterministic information, instead offering probabilistic indications of the underlying conditions. Probabilistic functionalism is a perspective that takes into account the complex and uncertain structure of real-world environments, as well as the adaptive and context-sensitive nature of mental functioning. Within this tradition, the lens model of Brunswik ([1952](#)) interprets perception as a process within a probabilistic network, where individuals interact with latent environmental properties such as object characteristics or risks through observable but imperfect cues. The effectiveness of perception was believed to depend on the degree to which these cues predict the relevant situational variables. The lens model thus describes probabilistic relationships between distal variables (the environment) and proximal cues (the information available to the individual). Each cue carries varying degrees of reliability. Through experience, individuals learn to select, weight, and integrate the most valid cues to optimize adaptive decision-making. Subsequent work has expanded Brunswik's lens model to various psychological domains (e.g., Hammond & Stewart, [2001](#); Wittmann, [1988](#)).

Specifically, Peper and colleagues proposed its application to the fields of neuropsychological assessment and emotion psychology (Peper, [2018](#); Ramminger et al., [2023](#); Schmidt-Atzert et al., [2026](#)). The lens and neuroscience models share some common ideas: For example, in the *posterior cortex*, the structure of the primary visual cortex and the surrounding *extrastriate areas* give rise to integration processes at different hierarchical levels of object recognition, which can be represented by a lens model (Barlow, [2001](#)). In the frontal cortex, the predictive coding model (e.g., Clark, [2013](#)) has suggested a multilayered framework where hierarchically organized prefrontal networks extend from rostral regions (coding abstract rules and task sets) to caudal regions (coding specific stimulus-response associations) (e.g., Alexander & Brown, [2018](#)). Thus, uncertainty is controlled by integrating probabilistic situational cues and continuously updating the

internal model in light of new information. It is therefore plausible to argue that the principles of probabilistic functionalism apply not only to perception processes, but also to the executive processes involved in achieving and planning goals in environmental transactions.

Brunswik's functionalism thus appears to reflect the brain's ongoing effort to optimize utilization of environmental cues, calibrating perceptual and executive processes to the ecological validity and reliability of available information. The brain continuously refines the selection and weighting of cues based on the statistical structure of the uncertain and variable environment. As no single cue is fully reliable in all contexts, the principle of multimodal integration and redundancy within neural networks is widely accepted as a means of compensating for weak, noisy or missing signals. Higher-order neurocognitive functions draw on accumulated regularities to optimize behavioral strategies. Rather than pursuing absolute accuracy, the brain prioritizes adaptive efficiency, optimizing behavior to fit the probabilistic structures of the environment. This perspective highlights the essential role of flexible, context-sensitive processing in supporting goal-directed action—a central consideration for the lens-based meta-model proposed below.

The research traditions reviewed above suggest that psychological phenomena can be interpreted as the product of evolved neurocognitive mechanisms interacting with structured environmental affordances under conditions of uncertainty. To capture this interaction systematically, the following sections summarize the neuro-lens model (NLM; Peper, [2018](#)) and provide an expanded Environmental Lens Model (ELM) to better describe the probabilistic relationships between brain, mental function, and environment in a coherent framework.

Modeling Environmental Brain-Behavior Relationships

The Neuro-Lens Model

The neuro-lens model (NLM) proposes a systematic correspondence between neural indicators and psychological functions, suggesting a probabilistic and bidirectional mapping structure that may enhance interpretation across psychological and neurobiological domains (Peper, [2018](#)). Here, the hierarchical structure of the adapted NLM reflects both neuroanatomical principles such as the ventral-dorsal organization (Pandya & Yeterian, [2001](#)) and the functional specialization across different prefrontal cortex (PFC) subregions (Fig. [18.1](#)). This model establishes a link between neuropsychological functions and neuroanatomical substrates, suggesting that higher-order executive control processes are organized in interlocking layers.

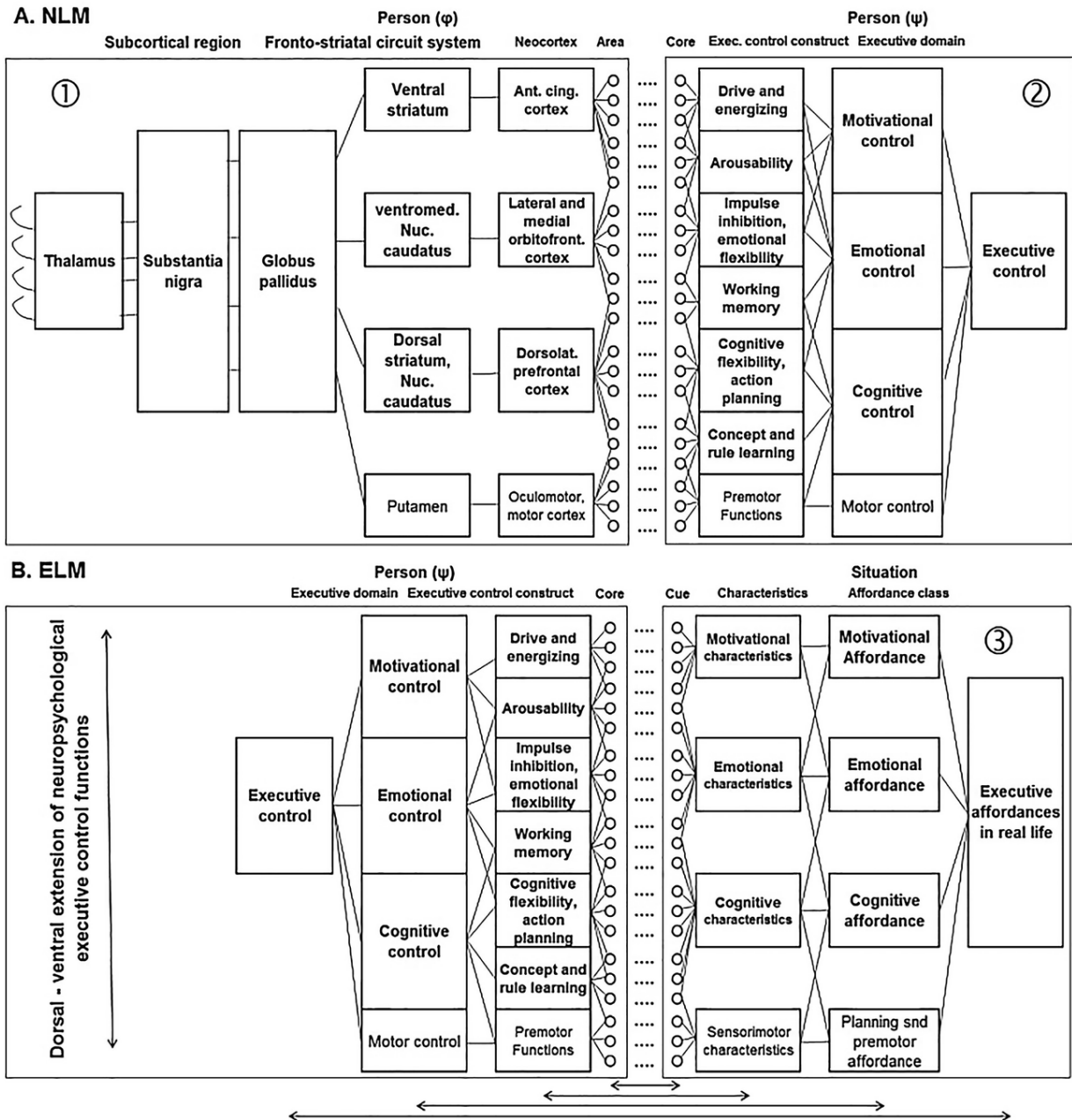


Fig. 18.1 Neuro-lens model and environmental lens model. A: Adapted version of the neuro-lens model (NLM; Peper, 2018), comprising (1) a simplified neuroanatomy of fronto-striatal system layers (φ) and (2) their associated neuropsychological functions (ψ). B: The environmental lens model (ELM) includes the neuropsychological function layers and (3) environmental affordance layers (see text for details). *Note:* The neuropsychological interpretation of the person side of the ELM (2) suggests an executive control suprasystem comprising at least three levels of abstraction: *core operations* (specific elementary functions), *executive control constructs* (functional compounds that share common features and coordinate elementary operations), and *domains* (broad types of functions that encompass multiple executive constructs). With regard to the situation (3), the hierarchy of the affordance suprasystem may include *cues* (specific perceivable features of the environment that show action-relevant properties), *characteristics* (categories that

group together multiple cues with shared affordance attributes), and *classes* (broad types or affordance domains with recurring patterns of characteristics).

The Neural Layers

The neural part of the NLM exhibits a hierarchical organization, as demonstrated by consistent patterns of structural and effective connectivity that allow for the formulation of latent neurobiological constructs, which have been evaluated through neurocomputational modeling and empirical data. The neural part of the lens comprises multilayered architectures with input, intermediate, and output levels that may also contain recurrent connections (see Peper, [2018](#), for details). A well-established framework is the *cortico-striato-pallido-thalamic functional loop model* (Alexander et al., [1986](#)). This model describes five parallel, functionally segregated loops extending from rostral to dorsal regions of the PFC, which facilitate the adaptive interaction of motivation, emotion, cognition, and motor function. It describes the vertical organization of motivational representations, spanning from the cortex through subcortical nuclei to the thalamus and back (Nieuwenhuis et al., [2008](#)). Three of these circuits (motivation, emotion, cognition) are particularly relevant in the present context as they show specific specializations: while the *ventral striato-pallidal system* is closely associated with emotional and motivational regulation, the *dorsal striato-pallidal system* primarily supports cognitive processes such as planning and problem-solving. Two further circuits of the PFC regulate motor planning, execution, and modulation, involving *cortical*, *basal ganglia*, and *brainstem* structures (described in detail in Schmidt-Atzert et al., [2026](#)). These systems are horizontally interconnected, allowing for a continuum from highly automatic processes to intentionally controlled motor actions.

The (Neuro-)Psychological Layers

Psychological construct networks also exhibit a hierarchical organization: In the cognitive domain, a prominent example is the three-stratum model of intelligence (e.g., Carroll, [1993](#)). In the personality domain, Royce and Powell ([1981a](#), [1983](#)) suggested a

multifactor-system theory proposing a cognitive style subsystem interacting with an affective value subsystem. A third stratum suprasystem governs overall personality organization resulting in the emergence of phenomena such as worldviews. These models typically comprise multiple levels. In the NLM, the functional (psychological) domains are coordinated with and constrained by the neurobiological systems summarized above: At the lowest level, specific, observable indicators such as behavioral responses, test scores, or individual items are located, while higher levels represent aggregated latent constructs, functions, or traits (Peper, [2018](#)) representing the motivational, emotional, cognitive, and motor planning domains. Because the brain also generates prediction error signals when actual outcomes deviate from internal expectation, the corresponding errors are also highlighted for each domain.

Motivational domain.

The regulation of fundamental motivational states is realized by the *medial prefrontal cortex (mPFC)*, as part of the *limbic fronto-striatal circuit* (e.g., Sewards & Sewards, [2003](#)). The resolution of motivational conflicts and the prioritization of behavioral tendencies, such as in approach-avoidance conflicts, are supported by the *anterior cingulate cortex (ACC)* (Corr, [2008](#)). The deviation of a predicted motivational expectation and the current state generates a motivational prediction error. The mismatch between expected and received outcomes, for instance, is processed in the *nucleus accumbens*, in interaction with *midbrain dopaminergic* neurons (e.g., Knutson & Cooper, [2005](#)). These signals are conveyed to the ACC and the *orbitofrontal cortex (OFC)*, where the ACC detects motivational conflict and signals the need for behavioral adjustment (Botvinick et al., [2004](#)) and the OFC evaluates outcome discrepancies to modify future behavior (e.g., O'Doherty et al., [2003](#)).

Emotional domain.

The regulation of emotion is governed by ventral areas of the ACC and their limbic connections (Bush et al., [2000](#)). The ACC is activated not only by personal experiences of pain, conflict, and errors but also by observing and empathizing with these experiences in others, highlighting its role in emotional perspective-taking (e.g., Decety & Jackson, [2004](#)). The mPFC plays a critical role in emotional regulation, evaluating emotional stimuli, processing reward and punishment values, and modulating affective responses (Ochsner & Gross, [2005](#)). The ACC and mPFC predict and mitigate aversive experiences, with the dorsal ACC being involved in anticipating negative outcomes, while the mPFC integrates these processes to promote flexible emotional adaptation and behavioral control in response to changing environmental demands (Ridderinkhof et al., [2007](#)). In decision-making, the PFC evaluates the reward value and associated effort costs of behavioral alternatives, selecting strategies that maximize rewards, with distinct neural responses to gains and losses observed in different regions of the prefrontal cortex. Contradictions between predicted emotional expectations and actual perceptions may generate emotionally triggered prediction error signals in the PFC and ACC (e.g., Kennerley et al., [2011](#)).

Cognitive domain.

Cognitive and attentional control and executive monitoring are realized as a function of the dorsal ACC (Bush et al., [2000](#)). Cognition is an important component of executive functioning, where attention is directed toward relevant information, and goal progression is monitored and updated within working memory (Smith & Jonides, [1999](#)). To maximize the likelihood of achieving a goal and satisfying a need, the organism evaluates available action options. When a goal is deemed unattainable, it becomes necessary to restructure the hierarchy of goals and needs. The *dorsolateral prefrontal cortex* (dlPFC) gives rise to cognitive flexibility, enabling the inhibition of specific actions to facilitate adaptive decision-making (e.g., Jones & Graff-Radford, [2021](#)). Cognitive flexibility is assessed by the ability to shift strategies based on

feedback, adjusting behavioral tendencies to meet situational demands. Effective regulation requires the ability to restructure goals when needs remain unmet, including the re-prioritization of previously subordinate needs, such as survival or well-being, in response to threats. Cognitive prediction error signals a mismatch between expected and observed outcomes. The dorsal ACC generates an error-related negativity indicating deviations from internal task goals, so that the dlPFC can initiate cognitive adjustments.

Sensory-motor domain.

The coordination of voluntary *motor* processing is realized by the motor circuits of the PFC described above. Successful execution of motor tasks, such as tool use, also involves structures for the integration of motor skills, cognitive planning, and environmental cues, including tool availability and task demands. Discrepancies between expected sensory inputs and actual task demands are processed by a *fronto-parietal* network including the *superior frontal cortex* and *posterior parietal cortex* to adjust behavior (e.g., Corbetta & Shulman, [2002](#)). Based on prediction errors, *supplementary* and *primary motor areas* contribute to adapting motor plans (Shadmehr et al., [2010](#)).

The Environmental Lens Model

In the following, an extension of the NLM is described, which includes the situation domain and relates it to the person component (cf. Figure [18.1](#) (3)). The environmental part of the lens also comprises multiple hierarchical levels of generality: At the lower layer, discrete environmental affordance cues stand opposite to the basic neuropsychological functions evaluating their relevance for the individual (cf. Scherer & Peper, [2001](#)). As human-made environments have been shaped—reflective of their designers' neurocognitive system—by multiple agents for diverse purposes, they are inherently multimodal and contain various classes of affordances. From a neuropsychological perspective, the affordance structure corresponds to the different functional modalities of the PFC summarized above as

the latter have evolved in response to them. To construe this part of the lens, a hierarchical taxonomy must be derived from the situational characteristics.

Supplementing an Environmental Half-lens

Wagerman and Funder ([2009](#)) have suggested a taxonomy of situational characteristics to describe their effects on behavior, and several questionnaires have been developed to clarify situational effects on personality (Sherman et al., [2010](#)). These instruments have typically been used for characterizing a wide spectrum of features of situations from controlled laboratory settings to everyday life experiences, to assess the perceived situational similarity and cross-cultural patterns in situation perception. Situations were described based on subjective construals of participants or evaluations by independent observers to establish profiles that can be associated with behavioral tendencies or personality traits.

Thus, to construe a hierarchical structure of the lens, three layers including specific *cues*, psychologically salient attributes (*characteristics*), and categories of situations sharing common features (*classes*) can be postulated (e.g., Rauthmann et al., [2020](#)). Research has described typical classes of situations named the “situational eight” (Sherman et al., [2010](#)). These situational traits are assumed to influence how individuals behave, think, and feel in different life contexts, which, in turn, affect personality traits. They are viewed as representations of unique characteristics of situations that are assumed to elicit specific emotional and cognitive processes, as well as social interactions.

The classes of the situational eight model are regrouped here by linking them to the hypothesized neuropsychological affordance structure: Motivational situations (cf. Rauthmann, [2016](#)) refer to, for instance, (1) *Mating* situations, where reproduction and social cues stimulate behaviors related to attraction and dominance regulation on the person side. Emotional situations are associated with (2) *Positivity* cues, which are accompanied by pleasurable and rewarding experiences linked to extraversion and well-being on the person

side. (3) *Negativity* and (4) *Adversity* both reflect emotional cues that elicit unpleasantness, engaging neuroticism and emotion regulation mechanisms. Cognitive situations refer to (5) *Intellect*, including cues eliciting reasoning and problem-solving behavior. (6) *Deception* also involves situations that require the detection of manipulation, use of theory of mind and strategic cognition. Finally, situations that include executive and action-oriented affordances include (7) *Duty* and (8) *Sociality*: the former reflects obligations and norm adherence, activating executive control and conscientiousness, whereas the latter involves interpersonal coordination, social planning, and behavioral regulation.

However, individuals are often confronted with multiple situational demands simultaneously, and perceive and respond to the same situation differently depending on their current emotional or motivational state, subjective interpretation, and cultural background. While the situational eight model highlights the relevance of situational influences, it appears to disregard other critical factors, such as neuropsychological predispositions (e.g., affective style, regulatory focus), stable personality traits, and individual life histories. In particular, it seems not to take into account the role of enduring neurobiological dispositions that consistently regulate behavior across time and contexts.

A Neuropsychologically Constrained ELM

The current neuropsychologically informed model for environmental affordances suggests that human interaction with the environment is structured through affordances that are functionally coordinated with the basic neuropsychological domains of prefrontal functioning: motivation, emotion, cognition, and sensory-motor execution and control. These affordances are not simply passive environmental features, but actively engage neurobiological systems for perception, action, and behavior. Drawing on evolutionary, neuropsychological, and ecological perspectives, the model suggests that specific environmental characteristics correspond to distinct functional domains in the brain

(due to previous influences of multiple human designers), thereby shaping the behavior of the users of these environments in predictable ways.

Motivationally salient environmental properties.

Environmental stimuli can activate motivational systems that evolved to support survival and reproduction. These include sensory cues, such as olfactory traces associated with food rewards or conspecifics. Motivational drives such as reproductive behavior and species preservation are linked to environmental affordances such as partner availability, mating displays, and reproductive cues. Similarly, achievement-related incentives—such as competitive settings or performance-based rewards—may trigger approach behaviors and are associated with dopaminergic systems underlying reward anticipation and effort mobilization.

Emotionally adaptive environmental properties.

Environmental affordances are closely linked to emotional regulation, particularly through the processing of threat and safety cues. Features such as signs of contamination, foul odors, or visual cues associated with decay elicit disgust responses to prevent disease and contamination. Similarly, fear-related cues—including darkness, confined spaces, or sudden noises—engage the *amygdala* and *periaqueductal gray* to trigger defensive behaviors.

Cognitive environmental properties.

The environment clearly presents affordances that support cognitive processing and rule-based behavior regulation. This includes features such as informational signage, prohibitive signs, and instructional cues, which help individuals to make context-appropriate decisions. These elements reduce cognitive load and support executive functioning by externalizing decision rules and guiding behavior in complex or ambiguous situations. These affordances engage the PFC and associated

networks responsible for rule-following, response inhibition, and goal-directed behavior.

Sensory-motor Properties

Finally, executive-motor integration involves the planning and execution of action sequences, supported by sensory-motor networks and the PFC. The usability of an environment, therefore, is dependent on the availability of action-congruent structures or affordances that match the individual's executive capabilities.

The mechanisms included in the neuropsychological part of the lens provide an internal forward model that facilitates real-time correction and learning processes by minimizing discrepancies between predicted and actual experiences within the environment. It can be postulated that it is the extent of increasingly aggregated error-related activity that constitutes a core principle to shape the functional hierarchy of the neuropsychological part of the lens. Thus, various types of prediction errors occur in response to different situational affordances at lower and higher levels of integration. It can be assumed that these signals are increasingly aggregated at higher levels. The current lens model suggests the use of probabilistic modeling to account for the various types and levels of error signals. The ecological validity of research designs could be improved by more broadly representing the different error domains.

Revisiting the Transaction Process

Transactions have been defined as reciprocal interactions between person and environment emphasizing the mutual adjustment of behavior in response to environmental cues and vice versa (Rauthmann, [2021](#)). Here, a transaction is also viewed as a bidirectional interaction or coupling between neuropsychological states and environmental demands (arrows in the Figure).

Person Transactions

The person perspective suggests that the causal construct can be located within the person and its subsystems. Neuropsychological states of the person emerge from the associated neural activity of the subsystems described above. The lens model postulates that—similar to multifactor-system theory (Royce & Powell, [1981b](#))—different subsystems constitute a personality suprasystem. Person transactions occur in situations where either compatibility or conflict with external affordances is registered, eliciting a specific, causally effective neuropsychological state. In the case of compatibility, specific feedback information flows bottom-up into the broader suprasystem-level structures thereby enhancing *assimilative* activities that lead to a greater stability of the personality system. In contrast, in the case of conflict, the neuropsychological process generates aggregable prediction errors informing the suprasystem to perform an adaptive response, that is, to enhance *accommodative* activities that initiate change of the person (cf. Royce & Powell, [1981a](#), [1981b](#)).

With respect to *motivation*, individuals actively shape contexts by selecting or designing environments that match with their motivational traits and goal hierarchies. Through the regulatory activity of reward circuits, individuals automatically evaluate, modify, or disengage from environments that do not support goal attainment—thereby selecting or constructing environments conducive to sustained effort. *Emotional* dispositions and regulation strategies allow individuals to influence the emotional ambience. Through mechanisms of reappraisal, suppression, or expression, individuals may diffuse tension, evoke empathy, or elicit social support. These transaction processes, governed by PFC-limbic interactions, facilitate interpersonal coordination and help establish emotionally congruent environments.

Cognitive capacities shape how individuals interpret and structure their environments. Through executive functions, individuals engage in planning, problem-solving, and hypothesis testing, transforming complex or ambiguous situations into structured, goal-relevant contexts. The PFC may establish order and strategic aims, thus allowing for environmental reconfiguration such as reorganizing physical or

informational resources to optimize task performance. Finally, through skilled and goal-directed *motor* behavior, individuals alter their physical environments. Motor execution, guided by internal sensory-motor representations, enables tool use, construction, and spatial navigation. The interaction of motor cortex, basal ganglia, and cerebellum facilitates precision and adaptability, allowing individuals to compensate for changes in the environment.

Transactions between the subsystems and the suprasystem of the person as a whole can be understood as an integration of lower-layer neurocognitive-affective states into coherent personality structures. Transactions can thus be seen as neuropsychological processes generating momentary adjustments to environmental cues based on internal predictions, while the suprasystem coordinates these into evolving and coherent traits. The goal of these transactions—balance between *assimilation* (predictive homeostasis) and *accommodation* (flexibility)—thus represents the functional correlate of personality integration and identity of the person.

The transactional fit or mismatch also depends on the degree of the person's current neuropsychological integrative abilities (e.g., focusing attention, emotional regulation) converging with the affordance profile of the situation. From a neuropsychological perspective, this involves integrating different functional domains, such as those summarized above, into a coherent response to situational demands as a whole. This is likely to be mediated by the coordinated activity in PFC-working memory networks. At the level of the personality suprasystem, a transactional profile may correspond to the probability pattern that a person will identify specific situational cues and respond appropriately to potential mismatches in affordances.

Situational Transactions

In the situation perspective, the causal constructs can be seen as located within the environment, that is, the initiating agents are situational affordances, constraints, or presses influencing the individual's neuropsychological system. The direction of effect is on

how these external conditions elicit or constrain functional states, potentially triggering regulatory adaptation. Thus, this type of transaction follows environment-initiated paths: Specific cues (e.g., reward or threat signals) activate affordances and associated latent capacities to elicit or inhibit responses on the side of the person. This transactional view postulates that the situation provides a multidimensional affordance profile that causally modulates the individual's neuropsychological action readiness in a bottom-up manner. Thus, situations contain several of the following latent demands simultaneously, potentially triggering specific 'vulnerabilities' on the person side, that is, the success of the transaction depends on whether these affordances are perceived and evaluated.

Motivational affordances. Novel or urgent situational affordance cues may bypass prefrontal control and directly activate subcortical survival systems. In particular, goal salience, task difficulty, and contextual rewards critically shape motivational responding. These affordances activate neurocognitive mechanisms, which evaluate effort and reward value. High-value cues in the environment stimulate the mesolimbic dopamine system and mobilize energetic resources, enhancing the willingness to engage in effortful behavior.

Emotional affordances. Emotionally significant features of the environment, such as threats, losses, or social signals, stimulate neural systems involving the *amygdala*, *insula*, and PFC. Contextual affordances such as safety cues or emotional support trigger appropriate emotional responses by shaping the deployment of emotion regulation strategies, facilitating adaptive responses through PFC-limbic interactions.

Cognitive affordances. Environmental structures that demonstrate information density, clarity of objectives, and feedback availability can influence cognitive functioning directly. These task-relevant cues are detected and processed through *fronto-parietal networks* and real-time feedback modulates ACC and OFC activities, promoting monitoring, error detection, and strategic adjustment.

Sensory-motor affordances. Visuospatial affordances provide critical input for *motor* planning. These inputs are processed through dorsal stream visual pathways and *cerebellar circuits*, enabling real-time motor coordination. Sensory feedback guides online adjustment to maintain motor precision.

Discussion

For many decades, directional pluralism has been viewed as a symptom of crisis in psychology, as illustrated by competing theories, constructs, and methods that coexist without a clear relational structure (Royce & Powell, [1983](#)). This unresolved fragmentation, before synthesis becomes possible, has been considered a sign of a young, developing discipline. However, the diversity of models and epistemologies persists to this day, reflecting the continuing multitheoretical complexity of the subject matter (Fahrenberg, [2015](#)). Thus, the crisis could be that of a mature science making it even more difficult to establish shared foundations or integrate findings. The present lens model does not claim to offer a simple solution to this crisis or eliminate pluralism. Instead, its aim is to contribute to the organization of important issues, such as person-environment interactions, by encouraging meta-theoretical reflection and to clarify the construct validation process.

The ELM suggests that model building might profit from systematically aligning internal construct categories with external affordance structures. The transaction concept draws on the ideas of assimilation and accommodation (Royce & Powell, [1983](#); Piaget, [1970](#)), describing how individuals adjust their internal models to fit external realities and vice versa. Lens models have previously contributed to clarifying reliability and validity issues in many psychological fields (Wittmann, [1988](#)). Here, they predict that asymmetries arising from mismatches between the selected psychological domain and the field of situational affordances may result in weak or misleading transactional associations. Determining convergent construct validity would be more difficult if a specific neurocognitive function is in the focus of analysis,

while the situational context primarily includes emotional affordances—resulting in a domain misalignment. The lens model suggests a systematic alignment of functional domains and environmental affordances across multiple levels of analysis.

Moreover, mismatches in aggregation levels between internal representations and situational affordances may undermine construct validation. This type of mismatch arises when predictors are aggregated at differing levels of generality, for example, combining low-level predictors with high-level criteria or vice versa. A low-level predictor (e.g., a narrowly defined neuropsychological function) paired with a broad criterion (e.g., multidimensional situational condition) may lead to underprediction or misclassification. Conversely, linking a broadly defined predictor (e.g., a general marker of emotional well-being) with a specific situational cue might also introduce asymmetry and stimulate overgeneralization. Similarly, differences in the temporal stability and resolution of measurements taken from the person (e.g., states versus trait-like ‘style’ constructs) and the situation (e.g., momentary versus permanent affordances) complicate the validation process. The lens model thus clarifies the need for symmetry for both domain and aggregation levels to ensure meaningful prediction and construct validation.

A further key issue in partially resolving the crisis diagnosed above is establishing robust causal relationships. However, symmetry of both sides of the lens does not confirm causality. This does indeed require additional experimental rigor from both sides of the lens (Peper, [2018](#)). To infer causality, experimental manipulations must be applied to either the person side (e.g., targeted manipulation of PFC functions such as working memory or emotional regulation) or the situation side (e.g., manipulation of environmental affordances such as spatial layout, stress cues, etc.). The variance structure of measured outcomes may then allow for identifying the causal effects of the manipulated condition converging for related construct on the other side of the lens, but diverging for unrelated constructs, thereby clarifying causally relevant convergence or divergence patterns depending on person or

situation manipulations. Reverse experimentation procedures can be implemented to increase validity—such as manipulating situational affordances to measure changes in neuropsychological function, and subsequently manipulating these functions to observe changes in environmental measures (for a description of experimental lens models see Peper, [2018](#); Wittmann, [1990](#)).

Recent advances in artificial intelligence have fostered a broader discussion about pluralistic modeling approaches. These developments have revealed opportunities but also clarified fundamental limitations (e.g., Helbing, [2010](#)), which are applicable to lens modeling with complex environments as well. According to this, many simplified models—including the lens model—may risk oversimplifying complex processes by assuming linear and stable cue-criterion relationships that, however, may not reflect dynamic or emergent phenomena. Indeed, ‘fast heuristics’ (Dhimi et al., [2004](#)) may specifically govern motivational or emotional control mechanisms characterized by different types of cue utilization processes, such as automatic, impulsive, or conditioned responses to rewards or outrageous errors that incur high costs. The ELM suggests an expanded set of cues and criteria to better explore the conditionality of these transactions.

Although Brunswik called for representative designs (e.g., Scholz, [2018](#)), many lens models used simple or artificial environments that inadequately sample real-world cue structures (Dhimi et al., [2004](#)). In contrast, the ELM suggests to broadly model real-world complexity within the criterion side of the lens. Nevertheless, problems with calibration and parameter specification may arise when the model becomes too detailed and the relevant variables are interdependent and difficult to operationalize in the real-world setting.

While lens models are typically based on linear regression and optimal cue weighting, human decision-making, particularly in redundant real-world environments, also employs heuristic, non-linear strategies that prioritize salient cues or adhere to simple stopping rules (Karelaia & Hogarth, [2008](#); Katsikopoulos, [2009](#)). The criticism that lens models show overreliance on averages, thereby masking individual

variability in strategies and outcomes, could be addressed through the exploitation of the full persons/variables x occasions/situations structure to allow lens models to be applied idiographically and contextually, enabling an exploration of person-specific as well as domain- and situation-sensitive transactions.

A pluralistic or possibilistic modeling approach that integrates multiple theoretical interpretations is in tune with the times (Helbing, [2010](#)). Nevertheless, the present lens model must recognize that an empirical evaluation is not yet available. The constraints of the model's scope must be acknowledged; otherwise, it may lead to misleading inferences. This is particularly important when applying the model to real-world, non-stationary systems characterized by unpredictability, non-linearity, and limited empirical replicability (cf. Helbing, [2010](#)). Therefore, the current model and its extensions can be understood as a meta-model, the aim of which is to provide guidance for conducting and implementing individual research projects in environmental neuropsychology and neuroscience.

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19. Historical Psychology: What Is It?

Daniel Sullivan¹ 

(¹) Department of Psychology, University of Arizona, Tucson, AZ, USA

 **Daniel Sullivan**

Email: swolf22@arizona.edu

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Introduction

There is a long tradition of debate concerning the relationship between history and psychology, dating virtually to the inception of “modern” nineteenth-century psychology and social science in European/American culture. Humans are historical beings, and societies are historical entities, in almost any of the many senses of the term “historical” that one might imply. Accordingly, many discussions about the philosophy of social science have revolved around the role of history in theory, understanding of causality, application of research, and so on.

In this essay I survey different responses to the question, “What is historical psychology?” which have been offered so far, with the ultimate aim of clearing ground for a historical psychology that will represent an interdisciplinary approach to social science. Yet it will also become clear (particularly in light of the broader themes of this volume) that sustained consideration of the history/psychology relationship quickly leads to perennial questions about the role of

theory building in psychological science. Many scholars have argued that historical data can and should be incorporated into prominent theories, models, and methods of psychology with little or no modification of the latter. On the other hand, in keeping with other chapters in this volume (see, e.g., Duden & Sunohara, this volume; Pizarroso, this volume), there are others who argue that the discipline of history provides a unique epistemology suited to human societies and mentalities, with which psychological theories need to engage. I hope this chapter makes a contribution not only toward the overdue emergence of a truly historical psychology, but also toward broader questions of theory development in psychology.

A few preliminary remarks are in order. Throughout this essay I focus on how history relates specifically to *social* psychology. My reasons are twofold: Social psychology is the sub-discipline concerned with people in social contexts, and therefore the field for which the history of those contexts is most obviously relevant; and it is the sub-discipline from which the most cries for a historical psychology have sounded, including in recent years. Indeed, I will pay special critical attention to the “new,” cultural-evolutionary version of historical psychology. This version shares methodological overlap with other contemporary projects such as *cultural dynamics* research (Varnum & Grossmann, [2021](#)) or Turchin’s ([2023](#)) *cliodynamics*. However, I am singling out the cultural-evolutionary work for detailed consideration because it is the most recent to explicitly claim the label *historical psychology*, and the one that appears to be gathering the most momentum. If we desire a truly historical psychology—and I do—we should pause to consider the direction in which this “new” version is taking us.

What Is History? What Is Psychology?

To address the role of historical data and a historical epistemology in social science, it is immediately important to consider what is meant by *history*. This question is not at all straightforward, especially for

psychologists unused to pondering it. Horkheimer (1936/[1972](#); 1932/[1993](#)) argued that at least three senses of history can be distinguished with corresponding methodological implications. First, what he called “traditional theory”—which connotes the positivist or empiricist worldview typical of the natural sciences and most contemporary psychology—views history as an objective series of material events basically irrelevant to the science of experimentation (indeed, the latter attempts to neutralize objective history by methodically isolating variables to observe lawful, “ahistorical” relationships). Second, history can refer to issues of ontogeny or development, as in the life history of an individual, the cultural history of a group (e.g., collective memory), or even the phylogenetic history of a species. Finally, history can refer to what is commonly implied by the term *historicity*: Humans’ reflexive awareness that all knowledge and experience is historically situated. The historicity of human experience suggests for many scholars that valid social science takes the form of *hermeneutics*: Not the study of the “objective” material circumstances of individuals and groups, but rather of the culturally constructed knowledge of those circumstances produced by individuals and groups (Bauman, [1977](#)).

Horkheimer’s three senses of history correspond to different philosophical frameworks for temporality distinguished by Sewell ([2005](#)). Sewell elaborates on the empiricist understanding of “objective” history—which he calls *experimental temporality*—as an approach that focuses on local, situational determinants of behavior rather than the unfolding of temporal processes. In other words, prior events, behaviors, or reports do not need to be understood in historical context, but can be wrested from that context, quantified and categorized in datasets of similarly abstracted events approximating the statistical ideal of independent observations. “The experimental conception of temporality [...] can be imposed only by what Burawoy aptly dubs ‘freezing history’—and [...] by cutting up the congealed block of historical time into artificially interchangeable units” (Sewell, [2005](#),

p. 95). On the other hand, the ontogenetic or developmental understanding of history corresponds to a *teleological temporality*.

A teleological explanation is the attribution of the cause of a historical happening neither to the actions and reactions that constitute the happening nor to concrete and specifiable conditions that shape or constrain the actions and reactions but rather to abstract transhistorical processes leading to some future historical state. (Sewell, [2005](#), p. 84)

From Sewell's ([2005](#)) perspective, the limitation of teleological temporality is that it reduces history to the steady unfolding of a small number of lawlike processes, a viewpoint which is ultimately compatible with experimental temporality.

Sewell contrasts both the experimental and teleological conceptions with what he calls an *eventful* conceptualization of temporality, one which acknowledges historicity.

The experimental conception [of temporality] rests on two fundamental assumptions: a uniformity of causal laws across time and a causal independence of every sequence of occurrences from previous and subsequent occurrences. The eventful conception of temporality denies both of these assumptions [...] [It] assumes that contingency is global, that it characterizes not only the surface but the core or the depths of social relations. Contingent, unexpected, and inherently unpredictable events, this view assumes, can undo or alter the most apparently durable trends of history [...] [It also assumes] events bring about historical changes in part by transforming the very cultural categories that shape and constrain human action. Because the causalities that operate in social relations depend at least in part on the contents and relations of cultural categories, events have the power to transform social causality. (pp. 100–102)

For social scientists like Horkheimer and Sewell, any scholarly claim about human history must engage on some level with the conditions of historicity and eventful temporality, which we cannot simply exit through methodological demarcation or containment (e.g., the “vacuum” of the lab). Any narrative a scholar produces about historical events is characterized by two realities: (1) the narrative concerns historical individuals who themselves possessed their own historical narrative of the events, potentially radically different from that of the scholar; and (2) the scholar is themselves situated in the same “stream” of historical events as those historical individuals about whom they are constructing a narrative (Olafson, [1979](#)). It therefore must be acknowledged that if a social scientist is analyzing human history using empiricist methods and experimental temporality, they are at best employing a useful methodological heuristic which cannot claim unilateral validity in comparison to other historical narratives.

Given these realities, it is unsurprising that the relationship between historical description and causal inference has been a recurrent topic in the philosophy of (social) science (for overviews, see Dray, [1966](#); Martin & McIntyre, [1994](#)). A common demarcating view suggests that the historian and the social scientist/psychologist pursue disparate aims through different methods. These arguments usually adopt the distinction between *explanation* and *understanding* as alternate modes of investigating human action (see Dallmayr & McCarthy, [1977](#)). In the philosophy of science, to provide an *explanation* for an event has come to mean to investigate it using the hypothetico-deductive model. This typically implies we have correctly isolated the event’s causal origin, and that we have further described this cause-effect sequence in terms of a nomothetic “covering law” which we expect to generalize to any future similar situation. By contrast, in this philosophical tradition to *understand* a human action or motive means to interpret it in terms of the rationale actors would typically give for their own behavior; and such interpretation can only be accomplished if the researcher has a thorough understanding of the social context in which such a rationale might arise or be evaluated.

This demarcating view implies “the autonomy of historical understanding” (Mink, [1966](#)): Historical analysis involves a different way of interpreting data (and indeed a different understanding of history) than that of a hypothetico-deductive analysis. Although most work in the philosophy of science now acknowledges the historically situated nature of scientific knowledge (Rheinberger, [2010](#); Schickore, [2011](#)), scientists working in psychology and the natural sciences often persist in understanding history purely in the empiricist sense—from the standpoint of experimental temporality. The psychologist hopes to generalize from their data to predict future events, and views history as a material record mostly irrelevant to their scientific aims; whereas the historian draws on their own “synoptic insight” (Mink, [1966](#)) as a “counter-method” to resist the abstraction of covering laws and offer instead a multi-causal understanding of why an individual or social group pursued one course of action and not another in a particular temporal-spatial setting (Barzun, [1974](#)). And whereas the psychologist believes knowledge progresses in a cumulative fashion as more covering laws and cause-effect relationships are identified, the historian sees knowledge (at least of human society) as an expanding network of hermeneutic circles—cultural contexts which may be approached, understood, and compared, but *emically* on their own terms and not *etically* according to those of a transcendent framework (Bauman, [1977](#)).

These demarcating views were attractive to nineteenth-century psychologists who waged a struggle to separate their discipline from other human sciences. Thus for many decades psychologists engaged with the discipline of history only to argue that historical perspectives were irrelevant. Such a tradition represents the identification of psychology with empiricism and the natural sciences, a recurrent argument (see Wertz, [2024](#)). Perhaps because the border with history is fuzziest when it comes to social psychology, prominent social psychologists have often downplayed any apparent overlap between their work and that of historians. Famously, Kurt Lewin sought through explicit philosophical discourse to make psychology an “ahistorical

science, the basic requirement of which is that all ‘causality’ must be dealt with in terms of factors that are contemporaneous with the effects” (Chein, [1947](#), p. 151; cf. Deutsch, [1954](#); Sullivan, [2020](#)).

Arguably the death knell of scholarly attention to history in social psychology was sounded during the social cognition movement of the 1980s–1990s, which re-aligned the sub-discipline with overarching trends and methods in general psychology.

It could well be argued that academic European/American social psychology started to ignore history because it eventually accepted the dichotomy between objective history and hypothetico-deductive methods, on the one hand, and historicity and interpretive methods on the other. In this regard, the field’s Lewinian obsession with “the situation” is both telling and unsurprising in a longer view of intellectual history. Most social psychological models (e.g., Ross & Nisbett, [1991](#)) identify the field as uniquely concerned with individual *construal* of social situations—in other words, researchers explain why a certain behavior is elicited if they explain how an individual consciously and/or unconsciously interprets the situation in which behavior occurs. In this, mainstream social psychology is only one of the more recent incarnations of a long European philosophical tradition of theories of *representation* and *mediation* (Dreyfus & Taylor, [2015](#); Rorty, [1979](#)), which in turn is a legacy of capitalism and cultural individualism (Horkheimer & Adorno, 1947/[2002](#)). Mainstream social psychology’s principle of situational construal is based on the assumption that individuals are constantly interpreting and representing the material world on a mental level; and when this assumption is wedded to a hypothetico-deductive scientific method, it means that scientific knowledge (of humans) is the arrangement of representations along a hierarchy of accuracy (Rorty, [1979](#)). Hence the field’s preoccupation with identifying the true, nonconscious causes of behavior which lie outside of people’s apparently largely inaccurate representations. Simultaneously, the field’s tendency to place historical understanding outside its purview is also easily rationalized in light of these commitments. If social psychology is the quest to unmask

inaccurate representations by identifying underlying causal processes, then the history—and especially the historicity—of a particular group of people is largely irrelevant to inquiry, constituting little more than the content of people's erroneous *a priori* causal theories for their behavior.

However, the question of the relationship between history and psychology surely cannot be resolved in as simple a manner as the demarcating view suggests. Despite the best defensive efforts of leading theorists, cries to integrate the disciplines in a form of *historical psychology* have periodically and repeatedly sounded. Although many have been more or less marginalized, some have gained traction at times and produced important studies (for reviews, see Gergen & Gergen, [1984](#); Sullivan, [2020](#)). In these theoretical and empirical gestures toward a historical psychology two common but distinct approaches may be discerned.

What Is Historical Psychology?

Though sharing a commitment to the intellectual mergence of history and psychology in some form, the visions of historical psychology that have been presented over the decades have also differed from each other, and indeed can be sorted into two major “camps.” The major point of contention between these camps concerns whether we view history through the lenses of experimental and teleological temporality, or through that of eventful temporality. In the work of scholars belonging to the first camp, history is reduced to psychology: History is a storehouse of additional data with which to test general laws posited to equally apply to contemporary (and future) individuals; it is a source of information, but not of lessons. For scholars in the second camp, psychology is ultimately absorbed by history: Theorizing is a procedure for situating psychological data in historical context and sensitizing society to possible futures. For these scholars, history represents a truly undiscovered country whose patterns and customs will alter the way they think about their own; it is a source not only of information, but of lessons.

Historical Data The *historical data* camp encompasses those social psychologists who have advocated for incorporating data from social history into our studies, but not for radical changes to our methods and theories. As Sewell (2005) notes of such perspectives: “If historical [psychology] is merely the [psychology] of the past, it is valuable above all because it increases the available number of data points” (p. 83). Manifestos written in favor of historical data present variations on arguments originally made by Webb et al. (1966), who categorized what they called *actuarial records* as one of several examples of “unobtrusive measures” which could be utilized alongside more conventional laboratory measures in a multi-trait, multi-method approach. Since Webb et al., proponents of historical data have tended to list the same advantages and disadvantages of such observations: Historical data sacrifice internal validity insofar as they are inefficient for establishing causal relationships, but increase external validity insofar as they represent the actual record of social behavior untainted by experimental artifacts.

Hailing from the data camp, McGuire (1976) anticipated the “new” historical psychology in many ways. In an exemplification of what Sewell called experimental temporality, McGuire’s view situates *cross-era comparison* as a sub-species of the different comparative approaches in psychology and social science, to be modeled after the *cross-group comparisons* associated with quasi-experiments and cross-cultural psychology. Advocating for the use of time series data to uncover broad causal associations across long swaths of historical time (e.g., does cultural change drive technology advancement or vice versa?), McGuire devotes most of his attention to technical issues that arise in trying to test such hypotheses with such data. As another exemplar, Simonton (2003) provides a compelling overview of ways historical data have been used in social and personality psychology, for instance, in famous studies of trends in political/religious authoritarianism and intergroup/racist violence. He details both quantitative and qualitative uses of historical data, and discusses the difficulty of translating the work of historians and biographers into

records amenable to social scientific analyses. Overall, despite certain challenges, Simonton takes the stance that such data can be subjected to similar analyses for similar purposes as in more conventional use of experimental and survey data.

The writings of the historical data camp scholars are identifiable, in part, because of how different they are from early philosophical handwringing about disciplinary boundaries with history. Rather than reflect on the epistemological questions raised by the study of history, these writers assume obvious advantages to adding historical data to the psychologist's stockpile, and devote the bulk of their energy to questions of data quality and translation that arise when such records are used. Technical and analytic methods for handling these issues may change as new tools become available: For instance, the digitization of archives can greatly increase access to useful data, or interpretive issues of autocorrelation in time series data may be addressed through techniques such as instrumental variable analysis (Obschonka et al., [2018](#); Varnum & Grossmann, [2017](#)). But inside the historical data camp, the fundamental issues and overarching goal remain the same: "We assume that the goal of the social sciences [regardless of data type] is always to achieve interpretable comparisons, and that the goal of methodology is to rule out those plausible rival hypotheses which make comparisons ambiguous and tentative" (Webb et al., [1966](#), p. 5).

The "New" Historical Psychology Earlier forays in the "historical data" camp superficially differ from the most recent version of historical psychology proclaimed by scholars such as Atari, Henrich, and Muthukrishna. The earlier efforts were formulated prior to the rise of extensive digital archives; and they tended to be more pluralistic, encouraging an openness of psychology to history independent of any particular theoretical commitments. Yet I argue that despite some unique characteristics, the "new" historical psychology belongs firmly in the historical data camp, insofar as it encourages the use of historical data without any substantive modification to the commitments of empiricist social psychology.

The new historical psychologists promise a trove of unexplored additional data from “historical subjects” with which to test psychological theories. For example, Atari and Henrich ([2023](#)) write, “Historical texts, art, and archeological sources serve as a kind of ‘psychological fossil record’ (Muthukrishna et al., [2021](#)) that opens up an opportunity to access data from dead minds” (p. 178). Muthukrishna et al. ([2021](#)) elaborate on the pragmatic and scientific benefits of such data collection from the deceased: “The dead represent a remarkably diverse subject pool, especially compared to the samples typically studied by psychologists. They thus constitute an important and relatively untapped source of data for psychological researchers [...] Long-dead subjects do not require payment or human subject approval” (p. 732).¹ The contemporary advocates are also clear about why the time is ripe for historical psychology: In an age of digital compilation the relevant historical records more than ever take the form of big data, widely available for use by psychologists unconnected to the circumstances and attendant expertise involved in their original collection and purpose (see Muthukrishna et al., [2021](#), p. 740). But perhaps more important from the advocates’ viewpoint is the fact that we now possess theory adequate to the task of grappling with such data.

Equipped with the field of cultural evolution, with its integration of evolutionary theory, focus on adaptive learning, and application of formal mathematical models, Muthukrishna et al. ([2021](#)) argued that for psychology to develop into a mature science of human behavior, it needs to develop into a historical science. [...] Historical psychology holds the potential to deepen our understanding of human behavior, and when paired with evolutionarily informed theories and state-of-the-art methodologies in natural language processing (NLP), it can provide widely relevant insights into psychological science. (Atari & Henrich, [2023](#), p. 177)

In essence, from this viewpoint historical psychology is cultural-evolutionary theory combined with big data (and requisite analytic methods). The recent acquisition of this theory, these data, and these tools apparently neutralizes the disciplinary threat of history from which psychology has repeatedly recoiled.

The new historical psychology adopts experimental temporality when its practitioners frequently describe historical comparison as merely a special case of synchronic comparison (see Muthukrishna et al., [2021](#)). Even further, these researchers directly describe historical events as “natural experiments” which have value for science insofar as they can be assimilated to the method of comparison (e.g., Atari & Henrich, [2023](#), p. 178). Given their emphasis on cultural-evolutionary theory, it is also possible to view the new historical psychologists as embracing a variant of teleological temporality. The relationship of evolutionary thought in general, and cultural-evolutionary theory in particular, to teleological modes of explanation has been widely debated (see Fracchia & Lewontin, [1999](#); Nagel, [1979](#)). Most cultural evolutionists are content to simply dismiss the idea that modern evolutionary biology is tied to notions of teleological progress as the result of sheer ignorance and unfounded (see Lewens, [2015](#)). New historical psychologists of cultural-evolutionary persuasion claim that they do not endorse teleological temporality because they understand history as “path-dependent” (Muthukrishna et al., [2021](#), p. 719). However, they immediately qualify such remarks by stating that the constraints of historical path dependence are only likely to occur “in the absence of sufficiently strong countervailing selection processes” (p. 719). In other words, the cultural-evolutionary theoretical approach is one in which “the built-in directionality of underlying causal forces guarantees that local variations are mere surface perturbations with no long-term effect on the course of history” (Sewell, [2005](#), p. 102).

For instance, probably the major achievement of the new historical psychology thus far has been Henrich’s ([2020](#)) study of the role of Medieval European restructuring of family size and inheritance practices in changing the trajectory of European/American culture.

According to Henrich ([2020](#)), institutional changes to kinship structure set certain European countries on an idiosyncratic path with cascading effects, leading eventually to the global economic predominance of what is often called the Global North of wealthy capitalist countries. Teleological temporality is much in evidence when Henrich ([2020](#)) devotes substantial portions of his book (pp. 430–490) to the argument that analyses of European/American modernity which hinge on capitalism and colonialism cannot account for the historical primacy of religious changes to family life. From Henrich's teleological perspective, modifications of kinship structure set in motion an inexorable historical process of which capitalism, imperialism, and globalization are merely peripheral downstream consequences.

Importantly, advocates stress that historical psychology practiced in this manner will not only supply further grist for the empiricist mill, but will cause us to re-think our assumptions about human nature by expanding the diversity of our participants. Whereas such diversity is conventionally considered in contemporary demographic terms, the advocates contend we can also construe diversity in temporal fashion. For instance, Henrich's teleological argument about family structure in Europe (and then North America) is intended as a contribution to scientific understanding of how unique the individualist psychology of this cultural region is, and therefore as a corrective to universalist psychological assumptions. It is clear enough that working with more diverse populations causes psychologists to re-think their theories, and it stands to reason that confronting historical data will do so as well. However, if the new historical psychology is basically the application of an existing theoretical framework and the most modern versions of established methods to newly digitized data, the question may be raised as to what extent this approach will actually cause us to query our assumptions about human psychology in a substantive way.

Authors such as Atari, Henrich, and Muthukrishna see historical data as valuable only insofar as they can be processed to fulfill the demands of mainstream theoretical and statistical approaches. Like earlier authors in the historical data camp, they devote most of their

relevant writing to the techniques required to rip historical facts from context and sort them into databases amenable to large-scale analysis and generalization. In the process they bemoan that “attempts to advocate for closer coordination between the humanities and the sciences [...] have typically fallen on deaf ears among humanities scholars. Large-scale database projects [...] have had to reconfigure their projects to include features or functions appealing to humanities scholars but orthogonal to the original scientific purpose” (Muthukrishna et al., [2021](#), p. 740). Apparently the new historical psychologists have not quite succeeded in the endeavor to “domesticate the historical discipline as the research assistant of science” and to establish the desired “clear division of labor [in which] historians discover and collect data about ‘interesting’ social-cultural phenomena, while evolutionary theorists raise the general level of analysis by subjecting those data to the selectionist covering law” (Fracchia & Lewontin, [1999](#), p. 15). Perhaps historians would perceive more “professional incentive to take part in scientific studies” (Muthukrishna et al., [2021](#)) if contemporary psychologists were more willing to consider the epistemological implications of historical analysis (for an argument in the historians’ own words, see Case & Vander Weele, [2024](#)).

In any particular instance, cultural-evolutionary theorizing may be defended against the charge that its conceptualization of temporality is only experimental or teleological (see Sanderson, [2007](#)). But from a broad vantage, it seems clear that the currently advocated historical psychology does not seek to alter the epistemological project of mainstream quantitative and comparative psychology, an alteration which would require sustained philosophical engagement with the historical nature of human society. Indeed, Kroupin et al. ([2025](#)) have recently put forward a “principle of articulation” which they hope will act as a rearguard defense of assumptions of universal psychological generalizability against the specter of hermeneutics (the dreaded “postmodern holiday,” p. 6). The latest in a series of frameworks perpetuating Lewin’s situationism, the principle of articulation posits

that theoretical generalizability constraints should be formulated not at the level of the individual but rather in terms of the environments which select for and afford particular patterns of behavior. In other words, a cultural-evolutionary approach ultimately reduces historical forces to environmental or situational forces (cf. Schmitt et al., [2021](#)). Such an approach offers the illusory promise of addressing historical causality by translating the classic concept of the psychological situation into an (ecologically determined) eternal recurrence (the covering law simply now takes the form: “When and wherever *X* environment forms, *Y* behavior occurs”). All this is to say that the new historical psychology is essentially mainstream psychology by other means; or rather with other data. From this perspective, the confrontation with history should not cause us to change theory and model building in psychology; rather, we should embrace historical data because the proper form of theory—a framework derived from evolutionary-biological consilience—conveniently affords the assimilation of such data.

Historical Epistemology

As a sharp contrast, several scholars who have endorsed some version of historical psychology have done so not at all in the service of hegemonic psychology (see Pizarroso, this volume, for an overview of one alternative tradition). Rather, those in the *historical epistemology* camp have drawn on the themes of the philosophical debate between psychology and history. Like the earlier participants in this debate, the historical epistemologists have argued that there are profound differences in the ways psychologists and historians explain human activity, differences that cannot be ignored or overcome by the tinkering of methodological “translation”; and yet unlike the founders of psychology, these scholars also refuse to resolve the issues by simply carving society at its joints and offering the disciplines joint custody. Instead, those in the historical epistemology camp propose that psychology acknowledge the historical nature of human society, and

thus become a discipline more aligned with history, either by embracing methodological pluralism or by altering its aims altogether.

Levine ([1976](#)) wrote a short essay which succinctly expresses the implications of historical epistemology for psychological research, in a series of observations that largely map onto the explanation/understanding debate discussed previously. Levine proposed that mainstream social psychology is dominated by a *naturalistic metaphysics* that assumes transhistorical laws which hold for all times and places and which operate outside the control of human consciousness. By contrast, he advocates for a *historical metaphysics* that assumes explanations are applicable only to delimited times and places, and that the mechanisms they describe are created and modifiable by humans. Regarding human knowledge, the naturalistic metaphysics assumes humans know by direct observation, and hence “data are the only form of knowledge” (Levine, [1976](#), p. 389); further, as a consequence, knowledge is cumulative because observational data can simply be added to each other indefinitely. However, the historical metaphysics assumes knowledge of society is holistic, meaning each society is a unique entity about which assumptions must be made which go beyond the direct observation of any individual piece of data.

Probably the best-known exponent of the historical epistemology camp in social psychology is Kenneth Gergen, who wrote a series of groundbreaking essays in the 1970s and 1980s that challenged mainstream researchers to confront the historical nature of human society. Gergen ([1984](#)) argued that social psychology’s assumption of “phenomenal immutability” needs to be challenged on two grounds. First, there is the challenge from historical context: The fact that psychological variables are always embedded in an intricate social context which has developed over time and which continues to develop, limiting the extent to which researchers can generalize their findings across time. For example, attitudes toward gender are formed against the backdrop of power and policy structures, division of labor, means of subsistence, and cultural models of identity, and cannot be readily extracted from or even fully understood outside of this socio-historical

nexus (e.g., Gill-Peterson, [2024](#); Lewis, [2022](#)). The second challenge is associated with what social scientists refer to as *reflexivity* (Jamieson et al., [2023](#)): The reality that psychological researcher and research are part of the society in which they do their work. The researcher's social position influences their choice of problems to study and the concepts with which they engage those problems; furthermore, those concepts in turn tend to become known by the broader public and to directly influence the social phenomena they were intended to neutrally describe. To effectively confront these challenges, Gergen ([1984](#)) proposes a move away from “synchronic theory [...] embodied within the mechanistic model of traditional social psychology [...] which deals with the static state of a given entity” (compatible with Sewell's experimental temporality) to “diachronic theory, which [...] [emphasizes] the direction of and relationships among social events across time” (p. 6; compatible with Sewell's eventful temporality).

Prospects for a Broader Historical Psychology

We have seen that although mainstream psychology at large, and social psychology in particular, has tended to draw lines of demarcation between history and psychology, there has nevertheless been a continuous countercurrent of work suggesting they may be profitably fused or interwoven. But within this countercurrent there have been still divergent streams: One camp, culminating in the proclaimed “new” historical psychology, tends to argue that historical data can be incorporated into existing psychological theories and methods with only relatively minor tasks of data preparation. Yet as Sewell ([2005](#)) asks, “Is history just a matter of more data points? Doesn't making [psychology] historical imply introducing ideas of temporality that are radically foreign to normal [psychological] thinking?” (p. 83). In keeping with this logic, scholars in the other camp have addressed the question: What might a historical psychology be if it actually considered the implications of historical epistemology; if it were truly a new approach to psychological theory?

Returning to Horkheimer and Sewell's different understandings of history/temporality, the logical empiricism of mainstream psychology adheres to an experimental temporality which attempts to freeze time through the logic of experiment and the quantification of variables. The new, cultural-evolutionary historical psychology ventures to some degree beyond the bounds of this experimental logic—through, for instance, the use of large historical datasets—but ultimately either is reducible to experimental temporality (in its insistence, e.g., that diachronic and synchronic comparisons are functionally similar) or remains bound to a teleological temporality which emphasizes a few ultimate causes (e.g., family and kin dynamics) at the expense of historical contingency. In this sense, for cultural-evolutionary historical psychology, historical causality is effectively reduced to situational or environmental causality.

In my opinion, a true historical psychology would incorporate both historical data and a historical sensibility—in other words, it would take seriously what Sewell and many historians conceptualize as the eventful temporality of human social existence. Such a psychology would try to understand history as a *subject-object dialectic* at multiple levels: That of human societies reacting to the other-than-human world and being shaped by it in turn; that of societies reacting to other societies and being shaped by them in turn; that of individual agents reacting to broader social structures and being shaped by them in turn (see Sullivan et al., [2022](#)). From this perspective, human history and that of the other-than-human world are not separate but intricately interpenetrating; at the same time, it is crucial to acknowledge the historicity of human experience, the fact that relationships and dynamics in human life are not given of natural necessity but actively produced and constructed, and therefore fundamentally malleable (Fracchia, [2021](#); Olafson, [1979](#); Schmidt, [1981](#)). The result is a radical contingency of human life as humans modify their environments, and subsequent generations (as well as the other-than-human environment and other interdependent cultural groups) respond to those modifications.

If a truly historical psychology must take such contingency into account, I propose that it should build off the following core theoretical assumptions, suggested by the frameworks of the historical epistemology camp scholars (see also Hutmacher & Mayrhofer, [2023](#); Sullivan, [2020](#)):

1. *Historical context:* People and psychological processes should be understood as inextricable from the unique historical contexts in which they live and occur;
2. *Reflexivity:* Research occurs, and researchers live, in unique historical contexts, and the situatedness of social research in history and society needs to be critically taken into account in the process, dissemination, and application of that research;
3. *Deep interdisciplinarity and methodological pluralism:* Rather than an assimilationist approach which reduces diachronic processes to synchronic hypotheses, mixed methods and disciplinary approaches should be leveraged to move between the synchronic and diachronic “moments” of social theory.

The third assumption is crucial because, as argued by philosophers of science, human society and psychology require and receive both causal explanations and interpretive acts of understanding from those who research them (Fay & Moon, [1994](#)). Similarly, both quantitative and qualitative forms of data analysis are valid and required for the complete study of society and psychology. Historical events are necessarily understood against the background of available data on trends and comparative cases; and historical trends are never fully grasped until they are brought into interpretive contact with concrete cases, which always outpace them in some fashion.

As an example of a recent study which exemplifies these principles, I would point to Shannon Elizabeth Bell’s ([2016](#)) *Fighting King Coal*, a multi-method investigation of the contamination and hazards caused by mountaintop removal mining in the Central Appalachian region of

West Virginia, as well as the community (non-)response to these hazards. Bell ([2016](#)) performed her study in Boone County, one of the top coal-producing counties in the state. As in many other parts of Appalachia, this county has (since the turn of the nineteenth century) been subject to the “boom-and-bust” cycles of the coal industry, which initially transformed a kind of subsistence economy into one based on wage labor but subsequently left jobs and livelihoods precariously at the mercy of supply and demand (for a quantitative historical psychology account, see Obschonka et al., [2018](#)). Specifically, the gradual loss of mining jobs since the mid-twentieth century has led to a fivefold reduction in West Virginian miners, a general population loss in the state of 38%, and a loss of over 20,000 people to migration in Boone County (Bell, [2016](#), pp. 23, 65). Since the 1990s, the shift toward increased mountaintop removal mining has de-territorialized the landscape, with mountains physically obliterated in up to 20% of the region (Willow, [2019](#)). Such new mining processes appear to have accelerated the environmental degradation caused by coal extraction in the form of increased floods, contamination of stream and well water, and exposure to dust-polluted air (Bell, [2016](#), pp. 24–33). In her study, Bell essentially asks: Why has such economic and public health devastation met with so little (or ineffective) community resistance over the decades?

What is remarkable is the diversity of methods and the openness to historical contingency which Bell brings to her analysis in order to comprehensively answer this question. She moves fluidly between synchronic and diachronic comparisons to build up an account which is equal parts social psychology and history, with neither epistemological framework wholly subsumed to the other. For instance, her initial documentation of the environmental damage caused by mountaintop removal mining combines a reconstruction of the history of the coal industry in West Virginia with a creative and rigorous “viewshed analysis” drawing on geospatial data (Bell, [2016](#), pp. 111–112). This work demonstrates that much of the current coal mining is conducted under the auspices of absentee landowners, which means that much of

the destructive activity occurs on land that is not actively inhabited by locals (although the consequences impact residential land). Further, less than a quarter of the total surface mining area is visible from county highways and roads, and less than 5% of the area is visible from U.S. and state highways (Bell, [2016](#), p. 116). Similarly, Bell's analysis of barriers to collective action involves dialectical movement between a contemporary-comparative and a historical understanding. For instance, she develops a quantitative index of social capital to apply to a random sample in Boone County and a demographically comparable West Virginia county not subject to coal mining; through on-the-ground surveying she determines that the coal region community is indeed hindered by diminished access to social capital (Bell, [2016](#), pp. 61–64). But in order to fully understand why this is the case, Bell has to reconstruct the local history of the coal town she surveyed. In doing so, she discovers a generational shift in social capital related to an incident of corporate union takeover which occurred in the early 1980s (Bell, [2016](#), pp. 66–70).

Bell's case study does not stand out as exemplary of the kind of historical psychology for which I am advocating solely or even primarily because of its employ of diverse methods and standpoints. Rather, it is because Bell marshals this diversity of information in the service of establishing *historical*, rather than causal-nomothetic, validity claims (Sullivan, [2020](#)). Historical data and epistemology are both primary for Bell, because she seeks to understand why a particular series of events occurred in a particular spatial-temporal context when they might not have occurred. Furthermore, the interpretation she generates cannot be easily transported into any other spatial-temporal context; rather, a network of interrelated contingent factors must be taken into account to understand whether a similar or different series of events might take place in another setting (this is what Gergen referred to as social psychology's aim of "sensitization," rather than prediction; Sullivan, [2020](#)). This does not mean that the results of Bell's research do not generalize, but rather

that a skeptical and multi-causal approach to issues of external validity is called for.

What is essential for theory and model building in psychology is not to respond to our multifaceted reality through a divide-and-conquer strategy: Demarcation of disciplines and occasional attempts to infringe on the ontological domain and data of a related discipline. Rather, part of our work as psychologists should be to fully recognize the multiple epistemologies and methods required to engage in the project of generating emancipatory (social) psychological knowledge. This “recognition” entails not merely acknowledging the work of scholars in other disciplines as valid or occasionally amenable to assimilation, but learning what it means to fully engage in their work. A historical psychology should therefore strive for a different “principle of articulation” in theory than that advocated by Kroupin et al. (2025). Sewell (2005) observes that “articulation implies the attachment or ‘jointing’ of distinct discourses to one another” (p. 339). A truly historical psychology would approximate Rorty’s (1979) understanding of the hermeneutical enterprise: A space between disciplines in which the concerns of each may meet; where alternative theories and methods are brought into dialogue for mutual enrichment and reflection, rather than in the imperative mode.

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Footnotes

¹ Given the now widely known ethical controversies surrounding the use of data from indigenous communities—and historical/ancestral data in particular (Bardill et al., [2018](#); Radin, [2017](#))—the cavalier, unqualified nature of these statements about the benefits of working with “dead subjects” is quite surprising. Perhaps more surprising is that these issues were actually recognized by earlier social psychologists who advocated for historical psychology, as in McGuire ([1976](#)): “Even though our subjects tend to be dead and thus beyond being bothered by our scoring of them, we may give offense to their descendants (either biological, ethnic, or ideological)” (p. 176). Honest in the acknowledgment of the ethical dilemma, McGuire ([1976](#)) was equally honest in owning the colonial nature of the enterprise: “But we must be prepared to shrug, and go on with the work even if our discovery of our *Mauvais Sang* [bad blood] leads us to say, like Rimbaud, that *Ma race ne se souleva jamais que pour piller* [My race only ever rose up to plunder]” (p. 176).

20. Past and Present Historical Psychologies: How to Deal with Variation in and Plurality of Mental Architectures in Psychological Research

Noemí Pizarroso López¹ 

(1) Universidad Nacional de Educación a Distancia, Madrid, Spain

 **Noemí Pizarroso López**

Email: npizarroso@psi.uned.es

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Introduction

New disciplines related to the study of our mind and brain's past have lately appeared that focus on the temporal or genetic dimension of neuronal and mental activity. For instance, there is paleoneurology, which studies the anatomy of the brain of extinct hominins; neuroarcheology, which examines the behavior typical of these same hominins (such as stone flaking) in modern human brains; and cognitive archeology, which tries to infer the cognitive skills of different hominins. Of them all, cognitive archeology most closely pursues issues pertaining to the realm of psychology. And in doing so it tries to go beyond the study of the brain, bringing body and environment into the equation as well. It speaks of a “bodily cognition” that involves haptic,

kinesthetic, and psychomotor skills and takes close account of how our cognition depends on our technology, in the understanding that the network of tools we use is so much a part of our cognitive system that we even delegate cognitive functions to external elements of our nervous system (Bruner et al., [2018](#)).

While the history of humanity is rife from end to end with radical technological change, the temporal dimension of cognitive archeology seems to come to a dead stop at the arrival of the human species, or at least it confines itself to a very “primitive” stage of human existence. It is no coincidence that cognitive archeology, defined as “reconstructing prehistoric ‘cognition’ ” (Cauvin, [2002](#), as cited in Henley & Kardas, [2020](#), p. 3), is also referred to as “prehistoric psychology.”

As a multidisciplinary field that fundamentally melds the field of archeology with the field of psychology, cognitive archeology is linked fundamentally to an evolutionary psychology (Henley & Kardas, [2020](#)). According to this view, which embraces the modularity of the mind, our psychological architecture is a property of our nervous system, which is the result of our genotype, which in turn is the result of millions of years of biological evolution (Tooby & Cosmides, [1992](#)).

To narrow in on the idea of possible post-prehistory changes in cognitive architecture, in human history itself, there is another discipline that has recently arisen, or rather re-arisen, whose goal is precisely to address the modifications that have occurred on the *Homo sapiens* time scale. This is “historical psychology” as proposed by authors such as Joseph Henrich and his colleagues (see Atari & Henrich, [2023](#); Muthukrishna et al., [2021](#)).

The New Historical Psychology

The “new” historical psychology (Muthukrishna et al., [2021](#)) gives center stage to the study of variations in the human species the world over (spanning areas such as the field of normative behavior, the organization of working memory or attentional bias, the role of intention in moral judgments, and the presence and structure of

personality traits) and to the need to account for the processes, environments, and conditioning factors that have eventually given rise to such heterogeneity.

The starting point of this “new” discipline is an article by cultural anthropologist Joseph Henrich and social psychologists Ara Norenzayan and Steve Heine, “The weirdest people in the world?” ([2010](#)), written to point out that most psychological research uses a highly biased sample the authors refer to as “WEIRD” populations (Western, Educated, Industrialized, Rich, and Democratic). While this paper was in a way limited to getting the mere existence of unstudied diversity out on the table, Henrich ([2020](#)) later went about explaining where the singularity of WEIRD populations came from. WEIRD psychology is characterized by traits like individualism, self-obsession, concern over self-esteem (more than others’ esteem), self-control, patience, nonconformism (less respect for authority), management of a wide network of social relationships, acceptance of rules and standards for coexistence (less nepotism), the attribution of intentions, the feeling of guilt (as opposed to shame), and analytical thinking (as opposed to holistic thinking).

These traits are thought to be the outcome of a long process whose beginnings lay in Europe’s Middle Ages, where a new psychology silently brewed. Henrich pinned the start of this new psychology to the Early Middle Ages and most particularly the role of the Catholic Church and its imposition of monogamy and other restrictions and prohibitions (a package of measures that eventually led to what Henrich called “the Marriage and Family Program” [MFP]). The erosion of the kin-based institutions specific to premodern tribal populations was “the proverbial pebble that started the avalanche to the modern world in Europe” (Henrich, [2020](#), p. 159). The weakening of family ties, together with the relational and residential mobility such measures necessitated, was accompanied by a series of psychological consequences: the need to stand out from the crowd on the basis of one’s own attributes and achievements, more pro-sociality and impersonal trust, less intragroup loyalty and more universal morals, the attribution of mental states and intention in criminal liability, and so

on. This entire set of social and psychological changes is thought to have affected the development of an array of new forms of social organization (voluntary forms not defined by clan or lineage), from the first cities and guilds to the various monastic orders and universities. These organizations gradually created their own laws, principles, rules, and systems of government, invoking rights, obligations, and duties that emphasized the idea of the individual. Urban development and impersonal markets (trading with strangers), with their respective laws and codes, merely accentuated the traits of the proto-WEIRD psychology (greater cultivation of personal reputation, hard work, efficacy, self-control, patience, punctuality), which hit its apogee in sixteenth-century Protestantism. The Reform lent an air of holiness to the psychological complex that was coalescing in Europe. WEIRD attitudes were only made more acute by mass literacy, early forms of democratic government (from the statutes and constitutions of free cities to Enlightenment theories about the social contract), and the Industrial Revolution, an authentic economic and technological tidal wave.

Henrich feels that the usual explanations of the history of the Industrial Revolution and modern times in general tend to ignore the role played by psychological changes, which worked to dismantle kinship-based institutions. In his view, such changes are not incidental, but key to understanding history: they lurked behind all new forms of social organization (markets, guilds, religious communities, scientific societies) and were in turn strengthened by the new organizational patterns. They were both cause and effect. These “deeper social and psychological footings” (Henrich, [2020](#), p. 435), on which he seeks not to reject but to build the existing explanations, are what he calls the “dark matter that flows behind the scenes throughout history” (p. 470), a slippery, shapeshifting matter that he tries to pin down with all the tools available to him (ethnographic studies, experimental research, surveys, historical records).

The historical psychology he espouses in his later articles (Atari & Henrich, [2023](#); Muthukrishna et al., [2021](#)) is meant to expand upon and

systematize the idea. It aspires to understand where intercultural differences come from and how the change happens. Like cognitive archeology, this “new” historical psychology is set in an evolutionist framework. However, it especially emphasizes cultural evolution, learning through others and cumulative cultural evolution sedimented on technologies, languages, and institutions.

To understand people’s psychology we have to consider not only our genetic inheritance but also how our minds have adapted ontogenetically and culturally to the local technologies and institutions. (Henrich, [2020](#), p. 469)

From this perspective, culture is not simply the product of a preestablished cognitive architecture anchored in the brain’s anatomy and physiology and dictated by our genetic heritage; instead, culture is a genuine driving force that can trigger new cognitive skills without the need for any genetic alteration.¹ That is, cultural innovations can reshape our cognitive abilities through unexpected secondary effects and may also induce changes in our neural substrate without necessarily involving any genetic changes at all. Culture, therefore, far from being a mere product of an innate cognitive architecture, steers the evolution of our psychology, even rewiring our brain, as literacy does (Dehaene et al., [2015](#), as cited by Henrich, [2020](#), p. 21).

The social norms governing human societies are thought to form part of this cumulative cultural evolution. For Henrich, evolution can give rise to a wide variety of normative systems, but not all of them have the same chance of surviving. Norms are tested when different groups, under different precepts, compete with each other. According to Henrich’s thesis, norms that favor success in inter-group competition, norms that foster interdependence and cooperation within the group in an ever-widening social sphere, will tend to endure and spread. This kind of competition, Henrich says, “has certainly driven the scaling up of human societies over the last 12,000 years” (Henrich, [2020](#), p. 69). In this sense, religion is supposed to have played a fundamental role in

societies' growth by setting rules (like the rules forcing people to broaden their social circle beyond their family ties) and overseeing the enforcement of those rules.

This is precisely the first example Muthukrishna et al. ([2021](#)) give to illustrate psychology as historical science. Religious beliefs in gods or cosmic forces (e.g., Karma) that make us feel watched and, if need be, punished are thus more likely to appear and spread. That would explain why religions based on universal gods (or cosmic forces) watching over compliance with moral codes became so prevalent around the fifth century BC.

The authors particularly appreciate the fact that this theory of the evolution of religions based on godly omnipotence enables predictions to be made not only regarding what we can expect to find in the historical record, but about current behavior in different societies and religions (transcultural research). In this case, experimental research using subjects from different forms of society and religions, including Buddhism, Christianity, Hinduism, and local traditions of ancestral god worship, confirmed that the people who believe most firmly in powerful moralizing gods behave most fairly toward their fellows, even strangers (Lang et al., [2019](#)).

The new historical psychology offers us many other examples linking past cultural practices with current cognitive or behavioral patterns, such as democratic behavior, trust in strangers, sex and gender differences, and differences in personality traits in different societies. Interested as it is in distinguishing clearly between arbitrary correlations and causal relationships, all these examples are supported by present experimental data to validate the cause-effect explanation. However, explaining the present psychological variation in terms of past cultural dynamics is not the only concern. New historical psychology also proposes to study diachronic psychological diversity, that is, to explore past forms of cognition.

It claims to have rich empirical materials to draw upon in that effort: all kinds of archeological remains and ancient texts bearing the fingerprints of past cognitive patterns. "History serves as a kind of

psychological fossil record, which opens up an exciting, and hitherto mostly untouched, source of ‘data from dead minds’ ” (Muthukrishna et al., [2021](#), p. 720). The new historical psychology does not hesitate to proclaim its worth, forestalling any methodological or epistemological critics: although the subjects cannot be manipulated experimentally or observed directly, historical artifacts, as “fossils of human thought,” enable us to trace out both explicit and implicit “patterns of past cognition.”

According to Muthukrishna et al. ([2021](#)), all that material, plus the specialized secondary literature analyzing it, forms an immense source of research possibilities for psychology, although it must be firmly understood that handling these sources and weighing their psychological significance will require interpretation by experts in the original tongue and the correct cultural context. That said, this “new” historical psychology considers that, in order for these materials to be useful to psychology, we must aspire to convert them into “quantitative data that can then be analyzed statistically” (Muthukrishna et al., [2021](#), p. 733). The best thing would be to have databases where cultural history is pre-coded, “ideally accompanied by coding rubrics broad enough to answer a wide range of research questions” (p. 734). As existing databases are not yet mature enough to be useful for psychological research, the challenge is to code the materials, but this is no easy task: the variables are many and subject to interpretation. Moreover, the necessary cooperation of experts in the domains and languages in question is rarely achieved (Slingerland et al., [2020](#)).

Faced with these difficulties regarding highly structured data, new historical psychologists propose using tools for the quantitative analysis of relatively unstructured historical data. This is basically a matter of content analysis techniques, such as word collocation/word embedding, topic modeling, or hierarchical cluster analysis. Analysis may be based on word or word-group frequency, or on human-annotation-based methods, starting with manual coding with expert support, to train a supervised automatic learning model that can identify the target construct in new corpora (Atari & Henrich, [2023](#)). In

short, the new historical psychology proposes a fundamentally quantitative methodology that delegates the task of extracting psychological information from large-scale historical corpora to computational methods emerging from natural language processing (Hutmacher & Mayrhofer, [2023](#)).

Apart from the hitherto unthinkable possibilities opened in this respect by the new digital humanities, the fact is that the history of late nineteenth-century and twentieth-century psychology provides us with multiple efforts to study different forms of cognition, their variety, and their changes over time, both through anthropological observations and through artifacts and ancient texts. These efforts, however, are conspicuous by their absence in the new proposals.

To be precise, Atari and Henrich ([2023](#)) briefly mention Lev Vygotsky, Michael Cole, and Kenneth Gergen. They acknowledge that, while these traditions are “out of date” in some respects, they still provide a useful framework with which to work. Indeed, Gergen ([1973](#)) squarely declared more than half a century ago that the observed regularities, and thus the major theoretical principles of social psychology, including Festinger’s cognitive dissonance theory and Milgram’s obedience phenomenon, “are firmly wedded to historical circumstances” (Gergen, [1973](#), p. 315)—which agrees with Henrich’s ([2020](#)) main argument. But Atari and Henrich ([2023](#)) never delve any deeper into that framework; they merely accept that these constructivist and constructionist approaches converge or are compatible with the evolutionist framework within which they place their own work.

I believe that, in response to this fresh and promising effort to shake psychology out of its WEIRD blindness, that is, its tendency to extrapolate WEIRD-based findings to the species as a whole and to assume that its own object of study is fixed and unchanging, it is worthwhile to revisit some of the earlier efforts made in this direction.

Old Historical Psychologies

The need to incorporate a sociohistorical stance into psychology research and account for the synchronic variety and diachronic variation of the mind is as old as empirical psychology itself. Confirmation of this statement can be found in Lazarus and Steinthal's original project concerning a Humboldt-inspired *Völkerpsychologie*, and in the later reclamation of the project by Wundt himself in what has been called his "second program" (which he considered an absolutely necessary complement to his "first program" of physiological or experimental psychology). The *Handbook of Cognitive Archaeology* starts precisely with a chapter that reminds us that

It was Wundt who [...] pointed out that individual human psychological processes are conditioned by an earlier history of the community to which they have no direct access. And it was Wundt who asserted that genetic (historical, developmental) methods are needed to deal with culturally mediated, historically contingent, 'higher' psychological processes. (Cole, [1996](#), p. 98, as cited in Henley & Kardas, [2020](#), p. 4)

The quotation is from Michael Cole, who, in his substantiation of cultural psychology, defended Lazarus, Steinthal, and Wundt's *Völkerpsychologie* traditions as well as Wilhelm Dilthey's descriptive psychology (for a brief introduction to these approaches and their specificities, see Jahoda, [2007](#)). Beyond these first attempts, claimed as antecedents of a cultural psychology, Cole's proposal is basically inspired by Vygotsky, whose work he helped introduce to the Anglo-Saxon world.

Vygotsky's sociohistorical school tackles above all the ontogenetic plane, understood as a crossroads between the phylogenetic and the cultural-historical (among the extensive existing literature, see, e.g., Van der Veer & Valsiner, [1994](#)). It is here where he mainly does his research, creating experimental situations to find out how children use different tools (both physical and symbolic) and the modifications entailed by the mediation of such artifacts, such as the internalization of systems of

signs (language, writing, numbers; see Vygotsky, [1978](#)). The most directly cultural-historical research Vygotsky engaged in was the transcultural work he and Luria did in Uzbekistan, in Central Asia (Luria, [1971](#)). These were tradition-steeped rural regions the Soviet government was pushing toward modern Western lifestyles through literacy drives and basic economic education. For Vygotsky and Luria, this dynamic formed a true natural experiment they could observe in detail to study the mental changes such sociocultural changes trigger.

Although an essential reference for any psychology interested in the psychological transformation worked by culture through artifacts, the work of Vygotsky—as well as that of current cultural psychology inspired by him—focuses on ontogenetic transformations more than the tracking of changes throughout history through the interpretation of ancient texts and artifacts, understood as the footprints or externalizations of a past cognition.

There are nevertheless other, much less well-known traditions that concentrate on this cultural-historical plane. For instance, in Germany we have Gerd Jüttemann (1933–2023), whose impact on psychology has been slight, though, despite his many publications. Hutmacher ([2022](#)) has recently pointed out several shortcomings that make it difficult to follow in his footsteps, such as the Eurocentric, linear nature of his fundamental theory and the scarcity of concrete work historicizing specific aspects of mental life. The fact that he published only in German and has not been translated hasn't helped either.

This latter difficulty is certainly one of the main obstacles hampering other attempts, like those made in the French-speaking world throughout the twentieth century, where we find a rich landscape of historical psychologies. Lucien Febvre (1878–1956), a founding member of the *Annales* history school, from which Jüttemann drew some of his inspiration, considered it a mistake for historians to use twentieth-century psychology to interpret the emotions, decisions, and reasoning of individuals from other times. Together with his colleague, psychologist Charles Blondel (*Introduction à la psychologie collective*, [1928](#)), Febvre asserted that different mentalities clearly exist,

and he firmly envisioned a historical psychology that aspired not only to describe these differences but to explain them. To this end, he set out to inventory the mental baggage (“outillage mental”), the set of instruments with which people think, feel, and act in a given civilization or era (Febvre, [1942](#)). Although the *Annales* historians gradually abandoned the label “historical psychology,” they continued to cultivate the idea through the new label of a “history of mentalities.”

While Febvre set out to develop this sort of collective psychology of past times, a similar project, but critical of the notion of mentality, was taking shape within psychology itself. It was led by Ignace Meyerson (1888–1987), who until then had led a career as an experimental psychologist and neuropsychiatrist, although in close dialogue with the human and social sciences (Di Donato, [1996](#); Parot, [2000](#)).² His approach was a convergence of the functionalist and genetic tradition in psychology, like that represented in France by James Mark Baldwin and Pierre Janet, with the social history project of the categories of thought by Durkheim and Mauss’s sociological school.³ It is in this interdisciplinary dialogue where we find probably one of the most sophisticated, best consolidated traditions in terms of putting psychology to the test of history (Pizarroso, [2013](#), [2018](#), [2019](#), [2020](#)).

Ignace Meyerson: A French Pioneer of Historical Psychology

Without denying that there may be “permanent functional aspects” that can be considered a sort of “primary psychological equipment,” Meyerson saw it as imperative to cast off the fixist prejudice that dominated psychology (Meyerson, 1948/[1995](#), p. 12). He certainly appreciated the value of work like Lévy-Bruhl’s ([1922](#)) on the primitive (mystical and prelogical) mentality, which stressed the diversity of modes of thought. But he found problematic the tendency to make all functions converge in a homogeneous system and the establishment of a mentality defined in opposition to another, a modern one. Instead of establishing two (or more) mentalities as uniform blocks, which

characterize historical periods of the spirit during which people's ways of thinking, feeling, and acting are articulated homogeneously, Meyerson proposed to observe what happens on various planes of activity while respecting the respective specificity of each and their heterogeneity over the course of time. In any case, for psychology to be able to deal with all this complexity and diversity, it had to broaden the repertoire of behaviors to be studied.

So far, "desirous of coming as close as possible to the methods of the physical sciences, [psychology] has preferred to study rather simple phenomena and functions, those that have responded best to the application of such methods" (Meyerson, 1948/[1995](#), p. 9). But the time has come, as Meyerson stated, to leave the lab and broaden psychology's scope to the works and institutions to which human activity gives rise.

Works, according to Meyerson, include the most diverse constructions human activity has been able to dream up: social and juridical phenomena, social rules; religious and magical facts, beliefs, rites, and myths; sciences, techniques, and experimentation; artistic phenomena; language, signs, and systems of signs. Ultimately, works as a whole constitute our world, which is "a nature transformed" by our activity, but one that is "incessantly and diversely humanized" (Meyerson, 1953/[1987a](#), p. 81). Instead of speaking of a human world, we really ought to be speaking of human worlds.

Meyerson's approach, which Jérôme Bruner ([1996](#)) called "France's best-kept secret," aimed directly at throwing light on the inner side of the history of institutions (Henrich's dark matter of history). He strove to do so precisely by analyzing all the sorts of documents that the "new historical psychology" now claims as legitimate for use in psychology. It is not just that these ancient texts and artifacts open a door to past forms of cognition, finding a work-around to the problem of the impossibility of doing any other kind of research (experiments, interviews, or tests) with long-dead subjects. The point is that because such texts and artifacts (which Meyerson referred to as "works") are the product of human activity, their study affords as valid an analytical

perspective on human behavior as do experimental analysis or clinical observation, to which Meyerson devoted himself throughout most of his career.⁴

Psychology therefore uses the same materials as history and other disciplines, like sociology and philology, but the questions it asks them are different. Psychology has to interpret the materials, one might say, decipher them, into psychological language, in connection with the mental operations or functions involved in their production: what forms of thought, perception, feeling, action can be glimpsed in them? When Meyerson spoke of “psychological functions,” he meant “what we have alternately called faculties of the soul, categories of the understanding or mental categories, and which it is better to call simply psychological functions,”⁵ to emphasize their operational character and overcome the tendency to substantialize mental facts.

His approach was to start with the psychological vocabulary in vogue in handbooks and treatises, with the caveat that the inventory of terms was surely neither full nor definitive. Nor universal. It was more a question of starting with this potentially anachronistic vocabulary and putting it to the test of history: to see if these functions, with their multiple component aspects, have always existed and, if so, in what form. This he did, with different functions, the foremost indubitably being the “person.”

His entire approach can in fact be boiled down to the historical analysis of the person, the self, a function that Meyerson used to exemplify his method for the first time in his thesis *Les fonctions psychologiques et les oeuvres* [*Psychological Functions and Works*], 1948, and to which he continued to devote much of his later efforts. What we understand as the person or the self is a complex set of many aspects: aspects related to our body and our image of it; socio-institutional aspects, like our name, our marital status, our rights and obligations, our profession, our sense of responsibility; religious aspects, like participation in a community, a church or worship, our personal relationship with the divine and what that relationship means in our inner life; our social relationships; and our sense of originality. One

faction of psychology (authors like Henri Bergson and William James) takes this original, intimate, inner self as a primeval phenomenon, the starting point of our activity. As opposed to this introspective psychology, which says immediate knowledge of the self can be gained by looking inward, Meyerson's historical psychology proposes to sift through the history of institutions and linguistic, legal and religious phenomena, with the additional support of ethnographic descriptions, to ascertain how each of the self's component aspects has taken shape. In short, to show how, through all such mediations, what we call "inner life" is constructed.

Meyerson's work merely expanded upon and examined in greater detail the outline of the history of this category drawn by Marcel Mauss ([1938](#)), Meyerson's colleague and friend. Mauss proposed adding complexity to the evolutionist scheme that stated that the individual is a notion of modern societies. Mauss had very quickly outlined a path that ran from the use of ritual masks by the Zunis, the Kwakiutl people, and Aboriginal Australians, where the notion of a "character" is found, to modernity's "psychological conscience," in which Protestantism played a fundamental role (with the idea of individual conscience, direct communication with God and an inner god). In between, he noted the development of some aspects of the person as a legal phenomenon in Rome, the moral dimension of the person in Stoicism and its examination of conscience (how to be conscientious, independent, autonomous, free, and responsible), and the metaphysical foundations of the person after centuries of churchmen's discussion about the individual soul. Mauss said this history was contingent. He pointed out how a certain notion of the individual and even of the self took a shaky form in Brahman India and ancient China long before it appeared in Christianity, but did not last.

Meyerson (1948/[1995](#)) supplemented Mauss's outline with an analysis of religion in Greece (in the cult of Dionysus, the mysteries and Orphism); the influence of eastern religions in Rome (which introduced the promise of immortality and with it a series of feelings like purity, rigor, and austerity); and the moral developments of Stoics and

Neoplatonic philosophers, especially concerning the notion of will. He then added an analysis of institutional thought in Greece and the transformations implied by the organization of democratic life in the city. His review ended with an articulation of the person with the notion of “genius,” defined as the “supreme culmination of the person” (Meyerson, 1948/[1995](#), p. 179), an identification that hit its apogee in German Romanticism.

At all events, Meyerson knew that a project like this required collective interdisciplinary work. In 1960 he organized a grand colloquium to continue exploring the question (Meyerson, [1973](#)). The event brought together luminaries like Jean Pierre Vernant, Pierre Hadot, and Louis Dumont, future leading lights in the study of the history of subjectivity and the origin of individualism, in addition to psychiatrists, writers, artists, linguists, and developmental psychologists. The historical review expanded upon and delved into issues Meyerson had already discussed in his thesis, adding many others and working forward as far as the seventeenth century, with industrial capitalism, Protestant Pietism, Romanticism, early historicism, and nationalisms. The reigning conception there was that of the “monad” individual, later reflected in late nineteenth-century novels, while later literature (Proust, Pirandello, Joyce, and Woolf) offered a different, more fragmented, fluctuating notion of the person.

The history of the person, which Meyerson continued to explore, especially through private diaries, in a way condenses Meyerson’s entire approach to a history of the psychological functions, which is merely a genealogy of the modern psychological subject—in a way, the same WEIRD psychology whose origins Henrich ([2020](#)) reconstructed. At all events, Meyerson devoted separate study to many other functions, like memory and its relationship with historical thought, work, scientific reasoning, and color perception. On the issue of color perception, he also called together a whole series of specialists in a colloquium, “Problèmes de la couleur,” at the end of which he concluded that, with the one set of “basic data,” humans have constructed color perception in different ways. There is not just one system of perception,

but systems of perception, created differently in different civilizations through incessant interaction with the environment—or more correctly “environments”—that people are perpetually constructing and reconstructing (Meyerson, 1957/[1987b](#)).

To account for variation and change, beyond the fact that each generation must relearn and recreate everything that is transmitted to it (with the innovations this recreation potentially implies), Meyerson insisted on the plurality of realms of activity (science, language, arts, religion, law), the experimentation we do in each of those domains, and the multiple interferences and interactions among the domains. While there may be impromptu convergences between realms, Meyerson warned us of the risks of accentuating convergences too heavily and focusing on synchronic coherences.⁶

Meyerson was critical of the idea of generalized, slow, constant, linear progress handed down by evolutionism. Instead of the idea of cumulative, gradual change, he insisted on the existence of actual “mutations, breaks, sharp turns, departures, and retreats” (Meyerson, 1948/[1995](#), p. 145).⁷ Without denying that progress has been made in certain realms and under certain circumstances, we have only to look at the history of sciences, languages, or art to see that they are full of crises. What is more, this “polyphonic” history (p. 61), where transformations are often unpredictable, is not closed. Thus, the psychological functions not only are unfinished, but are “essentially unfinishable” (p. 189).

The historical psychologist’s job is to follow the trail of the transformations, in a delicate job of interpreting texts and artifacts (a “second-degree effort”) in search of the operations cast into play in their production. We must carefully tilt back and forth, from functions to works and from works to functions. Because the job demands a solid mastery (knowledge) of the realm of works in question and their respective contexts, it requires close communication with specialists in the fields concerned, if not direct specialization, as in fact happened to Meyerson’s most faithful disciples: Jean Pierre Vernant became an unorthodox Hellenist devoted to throwing light on the transformations

of psychism in the transition from mythical Greece to Classical Greece, and Marinette Dambuyant an Indian specialist who focused on topics including the study of the category of person and will in ancient India (see Pizarroso, [2019](#)). Reciprocally, specialists in various areas, such as linguist Émile Benveniste, found that Meyerson's work enlightened them about the psychological realities they encounter without always recognizing them.⁸

To Meyerson's greatest regret, his historical psychology developed outside the bounds of psychology and the academic institutions that constituted its breeding ground. The reaction of his former colleagues, like Paul Guillaume, with whom he had performed a series of experiments on simian intelligence (Guillaume & Meyerson, [1987](#)), revealed experimental psychology's reluctance to embrace his focus (Pizarroso, [2008](#)). After his exile to Toulouse, he made various vain attempts to secure a position in psychology at the Sorbonne, where he had taught before, but he only managed to return to Paris in the sixth section of the École Pratique des Hautes Études (EPHE; from 1975 EHESS, École des Hautes Études en Sciences Sociales). Meyerson pursued his life's work inside a small circle of avid disciples and a wide constellation of specialists from the most diverse disciplines, who participated in the meetings and colloquiums he organized at the Center of Comparative Psychology, whence he wielded a quite diffuse influence.

Apart from this circle, but under the influence of shared concerns and a shared sensibility, other efforts have been made more recently to study the historicity of psychological categories and its performative nature, like Kurt Danziger ([1997](#), [2008](#)) and Roger Smith ([2005](#)). Nonetheless, the idea that the mind and psychological functions are historical objects remains quite marginal to the core of psychology as a scientific discipline.

Concluding Notes

We can only be glad that the “new historical psychology” has laid its focus on the cultural variety and temporal variation of psychology, insisting that working with WEIRD samples leads to partiality and mistaken universalization. Henrich’s work on the origins of WEIRD psychology from the Early Middle Ages to our days combines an enormous quantity of experimental, ethnographic, and historical research. Quite remarkably, it has succeeded at orchestrating a veritable host of empirical evidence into an overall view that is well argued and well written. And the pathway it has opened to future research into the psychological diversity revealed by exploration of the past is certainly promising.

Now then, a rereading of the work revolving around what we might call the “old” historical psychology, including the sociohistorical and cultural tradition, can not only enrich the historical narrative of current so-called WEIRD psychology, adding details and nuances, but also help to rethink the theoretical and methodological foundations of the “new” historical psychology. I refer, firstly, to the not-altogether-justified recourse to a series of psychological mechanisms supposedly formed in the co-evolution of genetics and culture, like a psychology oriented toward recognizing and internalizing rules, evaluating interdependence and developing a sense of tribal identity, as devices incorporated at a given time to a pre-existing cognitive system. A similar situation applies to the recourse to a “faith instinct” to explain learning from others, as well as to a mentalization skill as a glitch, and an inclination toward body-mind dualism to explain a tendency to believe in God, which gives account of a somewhat circular reasoning. Apart from these issues, related to its evolutionary framework, I also especially refer to the risks entailed in the implicit concept of a WEIRD mentality, a mentality that tends to emphasize synchronic coherences and to be defined by opposition to a non-WEIRD psychology, with the danger of over-homogenizing one and the other. We need to pay attention to the multiple fields where human activity is engaged, to the plurality (polyphony) of history, and to its discontinuities, gaps, and even retrogressions, as opposed to the idea of slow, steady, linear progress.

Apart from these general precautions, there is specific work on different categories and functions that could make the WEIRD psychology story fuller and richer. Much of it is related to some of the psychological changes Henrich tracks, such as analytical thinking, the attribution of intention, the experience of time and individualism. With respect to intention and responsibility, for example, the work by Vernant (1972/[1990](#)) on how will is portrayed in Greek tragedy shows how, back at the birth of the genre (the fifth century BC), the Greeks depicted an early awareness of self-as-a-responsible-agent, something hitherto unseen; it arrived with the transition from a mythical, religious thought to a new civil ideal where citizens participate in political life.

Without questioning the decisive role of the Catholic Church in the formation of current Western psychology, a little more attention could well be spent on this previous breeding ground as well as on developments in other traditions and civilizations, like Taoist meditation and pre-Buddhist India, where Brahman texts hint at an early certain notion of the intentional, deliberate act (Dambuyant, [1959](#)). We can thus refine the multiple aspects making up the psychological concepts we are looking at and add a little more perspective to a tale that claims the origins of individualism—and the traits associated with it (attribution of intention, analytical thinking, etc., and by insinuation prosperity itself)—depend overwhelmingly on the monogamy imposed by the medieval church.

There is indubitable interest in distinguishing between correlations and causal relationships to account for transformations in history, especially transformations involving complex, reciprocal causalities, like those at issue here. And yet perhaps too much emphasis is placed on explaining current psychological variety in terms of causal past cultural dynamics. We run the risk of falling into cultural determinism, which combines with an application of a series of universal psychological mechanisms—whether biologically anchored (instincts) or forged by genetic-cultural co-evolution—to make the new historical psychology a science more concerned with predicting behavior⁹ than with understanding diversity and otherness.

In connection with this, we might ask how far we can combine historical data with experimental data to “provide broad and rigorous tests of psychological hypothesis” (Muthukrishna et al., [2021](#), p. 739). How far can we isolate factors and transpose their effects on societies having such different socio-institutional, technological, and psychological fabrics? For example, research has been done into the relationship between belief in great moralizing gods and greater social equity in extremely different societies and religions. Can we assume that the experience of divinity is the same in them all, though? And if we find (as in fact we do) that atheist subjects do not respond to religious priming but do respond to a reminder of police or the courts, then what specific role can we assign to religious belief? And how is that related to legal experience?

With regard to the efforts of the new historical psychology to adhere to the standards of a supposedly scientific psychology, we should also step back and examine whether we are placing too much trust in the automatic quantitative processing of source data. True, the digital humanities open up possibilities we cannot refuse. And yet we cannot ignore that it is tricky to select, organize, and critically evaluate the pertinent sources when studying such a slippery subject, or that it is equally tricky to interpret the sources’ psychological content or meaning. For texts and artifacts to speak, we must carefully learn to ask the right questions and be open to the need to reword our queries. To delegate this task to an automatic processor seems rash to say the least.

In short, if we assume that there is a reciprocal interaction between institutions and psychology, that cultural and technological innovations have unexpected secondary effects, with the resulting neural rewiring and cascade of associated psychological—and institutional—changes, then we must be prepared for a wide margin of unpredictability and contingency in history. We can explain retrospectively, but we are hard put to make predictions, despite all the regularities we certainly find in human behaviors. It is a matter of understanding the present not as the inevitable result of evolution, no matter how cultural, but as an unfinished, complex, and relatively random construction. If we do not

accentuate these aspects, we may end up making monogamy and a belief in punishing gods the ultimate necessary cause of not only individualism, but progress, democracy, and prosperity.

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Footnotes

¹ This is what Colagè and d’Errico (2020) call a “top-down-also” model of cognitive evolution, in contrast to a “bottom-up-only” model.

² For a comparison between Meyerson’s approach and the historical psychology of Febvre and his disciple Robert Mandrou, see Vernant (1991) and Revel (1996).

[3](#) Hence, Meyerson's historical psychology can be indirectly connected to Wundt's *Völkerpsychologie*, of which Durkheim and Mauss's project is to some extent a reinterpretation. At all events, it is a version stripped of the remnants of teleology and philosophy of history still present in Wundt.

[4](#) He formulated his historical psychology in the 1940s, during his exile to Toulouse in the Second World War (see Pizarroso, [2026](#)).

[5](#) Meyerson, Ignace, Leçon du 17 novembre 1952, arch. Nat. 19920046/06.

[6](#) That is what other approaches, like Febvre's, do when they resort to the concept of mentality, which Meyerson mistrusted, as we have seen above. Other endeavors such as those made by Dutch psychiatrist Jan Hendrik van den Berg (*Metabletica: Principles of a Historical Psychology*, 1956) and social psychologist Zevedei Barbu (*Problems of Historical Psychology*, 1960) received similar criticism (Vernant, [1965](#)).

[7](#) He recognized that other psychologists, like James Mark Baldwin and Maurice Pradines, had admitted the possibility of such mutations, but they had confined them to a very ancient prehistoric period of human development.

[8](#) Letter from Émile Benveniste to Ignace Meyerson, August 18th, 1948, Arch. nat. 19920046/45.

[9](#) Varnum and Grossmann ([2017](#)) roundly affirm, "Ultimately the goal of a psychology of cultural change should be to produce a predictive science" (p. 961).

Part IV

Conclusion

21. Theory and Model Building in Psychology: Mapping the Current Debate

Fabian Hutmacher¹✉ and Alexander Nicolai Wendt²✉

(1) University of Würzburg, Würzburg, Germany

(2) Sigmund Freud University, Vienna, Austria

✉ **Fabian Hutmacher (Corresponding author)**

Email: fabian.hutmacher@uni-wuerzburg.de

✉ **Alexander Nicolai Wendt**

Email: alexander.wendt@sfu.ac.at

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Introduction

The idea that psychology is in crisis is not new; it dates back to the early days of psychology as an academic discipline (see Maiers, [2022](#); Sturm & Mülberger, [2012](#)). One prominent framing of this crisis, which has gained considerable attention in recent years, is the claim that psychology is experiencing a *theory crisis*, as indicated by a lack of cumulative progress over time, with existing theories remaining relatively vague and unspecific (van Dongen et al., [2026](#); Eronen & Bringmann, [2021](#); Muthukrishna & Henrich, [2019](#); Oberauer &

Lewandowsky, [2019](#); see also Hutmacher, [2026](#); Hutmacher & Franz, [2025](#)). Note that the theory crisis is more than an abstract academic problem; it can also be seen as a personal crisis (Wendler & Storck, this volume): It concerns us as psychologists doing (empirical) research who usually hope that the projects that we pursue have a positive impact on knowledge generation and ultimately also on society. If our theories are—as the narrative of the theory crisis suggests—not as good as we expect and want them to be, this also means that we fall short of this goal.

Of course, facing the theory crisis is easier said than done: Different schools of thought diverge substantially on the question of how good theories can be developed and what constitutes a good psychological theory. Against this background, a single collected volume such as the present one cannot solve all problems or provide answers to all questions. Our goal was more basic: As different traditions often coexist, while taking little notice of one another, we wanted to offer a platform for scholars from these various (meta-)theoretical backgrounds to present and discuss their points of view. In this concluding chapter, we connect the contributions to this volume to the existing literature in order to map the current debate. More specifically, we describe five sticking points that reappear across chapters and that point to long-standing debates in our field that have bugged psychologists for decades and—in some cases—even centuries. Based on this, we provide some ideas and suggestions that could in our opinion help to move the discourse on theory building in psychology into the right direction.

Sticking Point #1: Theories and Models in Psychology

Researchers ceaselessly develop and publish new psychological theories and models. However, there is little debate about what these terms actually mean. Consequently, the understanding of theories and models tends to remain implicit (Slaney, this volume). As there are

many ways to conceptualize theories and models, as well as their relationship (Bailer-Jones, [2009](#)), this lack of debate and precision is likely to have detrimental effects. If we do not know what we are talking about, debates on theory and model building in psychology are unlikely to make any progress. One way of gaining a deeper understanding of the meaning of the terms *theory* and *model* is to look at their (etymological) history. What one realizes when doing so is not merely the trivial insight that the meanings of the terms *theory* and *model* have changed over time, but that these changes are associated with changes regarding the ontological and epistemological commitments that individuals make. To put it differently, the way we talk about theories and models is more than a manner of speaking: It reveals something about the way we see the world.

Let us begin with the term *theory*. In ancient Greece, a *theoros* initially was someone who kept and preserved an oracle's prophecies (for details regarding the following considerations, see Koller, [1958](#)). Then, the meaning of the term shifted toward an official title for a messenger for religious festivities, connecting the concept to the act of traveling. Eventually, the experience of traveling took predominance over the religious meaning insofar as the messenger gained knowledge about foreign customs and institutions while traveling. The verb *theorein* thus became connected to exploration and knowledge, carrying *theoria* to its ultimate meaning of observation, contemplation, and understanding. For Aristotle, theorizing consequently is an activity of the highest region of the mind (*nous*). Dedicating oneself to theory in the contemplative life (*bios theoretikos*) thereby becomes the style of living that possesses the greatest dignity. Such an understanding of theory that focuses on self-reflection had a lasting impact on the cultural self-esteem of academic life and inspired not only the scholastic but also the Arabic understanding of intellectual virtues for the following two millennia. In other words, the classic concept of theory did not depend on a connection with empirical investigations in the sense of inductive generalization.

The concept of theory was subject to change in modernity, beginning with pre-enlightenment empiricism, in which it became increasingly important to connect theories to hypotheses which in turn had to comply with empirical phenomena. As a result of the growth of the philosophy of science by the early twentieth century, the understanding of the essence and conceptualization of theory was further deepened and diversified. Default positions include the empiricist account, according to which theories serve to explain phenomena by determining the relations between observed entities, and the propositional account, which treats theories as sets or networks of statements which allow the investigation of phenomena. In contemporary debates, one can—broadly speaking—distinguish a syntactic, a semantic, and a pragmatic approach to theory (for details, see Slaney, this volume; see also Cellucci, [2016](#); Portides, [2017](#); Vorms, [2018](#); Winther, [2021](#)). The syntactic view (as most prominently represented in the hypothetico-deductive model; see, e.g., Carnap, [1937](#); Hempel & Oppenheim, [1948](#)) sees theories as collections of statements or propositions expressed in a formal language, from which empirical claims can be logically derived and subsequently tested against actual observations. In contrast, the semantic view (for some classics, see Giere, [1988](#); Suppe, [1989](#); Suppes, [1967](#); van Fraassen, [1980](#)) understands theories as sets of models that capture patterns or relationships in the world, focusing more on the structural aspects than on the language used to describe them. In the words of Bas van Fraassen ([1980](#)):

The syntactic picture of a theory identifies it with a body of theorems, stated in one particular language chosen for the expression of that theory. This should be contrasted with the alternative of presenting a theory in the first instance by identifying a class of structures as its models. In this second, semantic, approach the language used to express the theory is neither basic nor unique; the same class of structures could well

be described in radically different ways, each with its own limitations. (p. 44)

Finally, the pragmatic view (e.g., Cartwright, [1983](#); Kitcher, [1993](#)) shifts attention from the formal structure of theories to the practical contexts in which they are developed and applied. This perspective sees theories as tools for explaining, predicting, and intervening in the world, often shaped by context and the goals of scientists. Rather than aiming for universal truth, the pragmatic view focuses on how well a theory works in specific situations. As this brief overview demonstrates and as already indicated above, the contrast between the syntactic, the semantic, and the pragmatic view of theories is not only a matter of style or idiosyncratic preferences but is deeply infiltrated by differences in worldviews and philosophical presuppositions: Can theory and theory building be formalized and axiomatized? Is there a language of theory that is able to mirror the “language” of the universe? Is there an objective truth to be described and discovered through theories? What role does the individual researcher play in the process of theorizing? In short, the discourse on theory is embedded in the environment of controversies that permeate the entire history of science.

This general insight also holds true for the term *model* as well as for the different ways of conceptualizing the relationship between theories and models. Via Vulgar Latin *modellus*, the etymology of *model* traces back to Latin *modulus* which is a diminutive derivation of *modus*. The word contains the Indo-Germanic *mod*, connecting it to the Greek *medomai* as an expression for consideration, thought, and even meditation, as well as *medimnos*, which is an ancient unit of volume for corn, allowing an association with *quantity*. The etymology of *model* also relates it with the word *module* which was used in a terminological sense to address organization and partitioning of spaces since antiquity, especially in Roman architecture. By the time of the Renaissance, the term *model* deviated from the concept of *module* (Mahr, [2003](#)): While the *module* of architecture served a concrete purpose in the process of building (like a construction plan), the *model*

meant to illustrate and provide a representation (like a blueprint): “Models thus become the medium of ideas” (Mahr, [2003](#), p. 75; our translation). Such an emancipation from concrete applications was the foundation for further abstractions in the following history of science.

As in the case of theories, the conceptualizations of *models* have drastically diversified over the last century. In the field of economic studies alone, Zschocke ([1995](#)) discusses 65 different definitions of the term, including models as isomorphic representations of reality or models as technical devices that enable creativity and freedom, as in the case of a model in the artistic process (for an example of the latter, see also Campill & Valsiner, [2023](#)). In a similar vein, Fried ([2020](#)) mentions different forms of models such as “verbal or mathematical models (e.g., different descriptions of how neurons interact in the brain), diagrams (e.g., the hydrological cycle that moves water on Earth), or mechanical models (e.g., a physical model of the DNA double helix)” (p. 336). Going beyond such taxonomies, one might also attempt to establish a proper definition of models as such. What such a definition looks like will crucially depend on the perspective that one holds regarding theories (see Slaney, this volume; Wendt & Wertz, this volume; see also Gelfert, [2016](#); Portides, [2017](#); Vorms, [2018](#); Winther, [2021](#)).

From a syntactic view, models are secondary to theories—they are ways of interpreting or instantiating a theory rather than fundamental entities in themselves. This perspective is fundamentally different from the semantic view, in which a theory is defined as a class or family of models. Take Alfred Tarski’s ([1954](#)) mathematical *model theory* as an example. For Tarski, a model is a sequence of objects that are semantically constructed in accordance with a *formal language*. Within this formal language, the model satisfies a logical function which connects the language with semantics while the syntax of the language determines the number of possible models. To illustrate this, consider structural equation models (SEMs) that are often used in psychology. The logic of SEMs delimits a set of possible permutations of the system. That is, not all relations may be modeled by SEMs but only those that

can be emulated by recombining the elements of the underlying formal language. Moreover, SEMs only become semantically meaningful when they are assigned to a domain of (psychological) research, inheriting the meaning from the empirical phenomenon.

In the pragmatic view, models are not just representations or structures—they are tools used by scientists for specific purposes to think, explain, and interact with phenomena. Importantly, models and theories have a flexible, dynamic relationship: That is, theories provide loose frameworks that guide and are shaped by modeling. In other words, theories and models co-evolve through practice, experimentation, and application. As both are epistemic devices (Knuuttila, [2011](#)), the distinction between models and theories may become less conceptually precise, shifting instead toward the circumstantial demands of the specific research context. An adjacent example for a broad understanding of models can be found in Gigerenzer ([1981](#)) who follows a constructivist approach to interpret models as a five-place relation of the subject, a purpose, a subject area, an empirical system, and a numerical system. This complex proposal moves beyond a common understanding of measurement models of mappings of empirical and numerical sets insofar as the researcher and the research interest become part of the model itself. Once more, the differences between these perspectives on models and modeling are not superficial and ephemeral but reveal a certain take on the philosophy of science (e.g., on the debate regarding realism versus anti-realism). Hence, just as in the case of theories, the concept of the model points beyond psychology, adumbrating a meta-psychological and transdisciplinary controversy between competing philosophies (see also Wendt & Wertz, this volume). To address and articulate these arguments is a task for theoretical psychology.

Sticking Point #2: The Challenges of Building (Good) Theories

As already mentioned in the introduction, recent years have seen different researchers raising the concern that there is a lack of good theories and cumulative progress in psychology (Eronen & Bringmann, [2021](#); Muthukrishna & Henrich, [2019](#); Oberauer & Lewandowsky, [2019](#); see also Hutmacher & Franz, [2025](#)). Henriques and Gralha (this volume) try to capture this observation using the metaphor of a sandcastle: Like sandcastles, psychological theories might look impressive at first sight but are always quickly washed away when the next generation of researchers enters the scene, introducing new ideas and building new sandcastles. Interestingly, similar observations were already made several decades ago. In a paper that has since become a classic, Paul Meehl ([1978](#)) wrote, for instance:

It is simply a sad fact that in soft psychology theories rise and decline, come and go, more as a function of baffled boredom than anything else; and the enterprise shows a disturbing absence of that cumulative character that is so impressive in disciplines like astronomy, molecular biology and genetics. (p. 807; for another early critique, see Pereboom, [1971](#))

In times in which psychology is probably more popular than ever (Clay, [2017](#)), this may seem surprising. However, it has repeatedly been pointed out that theory building in psychology is difficult as the subject matter that psychologists are dealing with (i.e., human mind and behavior) is highly complex (see Wendt, [2025](#)). The complexity of the human psyche can be described in different ways. On the one hand, one can point to the abundance of many-to-many relations (De Boeck et al., this volume): The same mental state or behavior can be caused by a wide range of different factors, and the same factor can cause a wide range of different mental states and behaviors—not to mention higher-order interactions between these factors. On the other hand, human thinking and behavior is often claimed to be situated, that is, to be dependent on a host of situational, cultural, and historical circumstances that substantially complicate the generalizability of

psychological findings and theories (Derksen, this volume; see also Gergen, [1973](#); Smedslund, [2016](#)). Against this background, some authors even state that talking about a theory crisis in psychology is misleading as “theories in psychology and other social and behavioral sciences will always fall short of traditional scientific benchmarks because of the complexity of the topics that are studied” (Sanbonmatsu et al., [2025](#), p. 1). Such fundamental objections notwithstanding, other authors have pointed to specific shortcomings in the process of theory building in contemporary psychology that should be addressed.

Building Better Theories: From Formalization to Creating Better Research Environments

One argument is to say that the theory crisis is rooted in the underestimation of theoretical work and conceptual clarification compared to data collection (Cornejo & Rojas, this volume; Funke, this volume; see also Bringmann et al., [2022](#); Eronen & Bringmann, [2021](#); Oberauer & Lewandowsky, [2019](#)), ultimately maybe even culminating in a “cult of empiricism” (Toulmin & Leary, [1985](#)). While psychologists have certainly observed a host of fascinating effects and collected a lot of interesting data, the theoretical meaning of these effects and data has often remained unclear (see Katzko, [2002](#)). In addition, the focus on “sexy hypothesis testing” (Fiedler, [2017](#)) has overemphasized originality and surprise value, giving priority to unexpected and unlikely findings (for an early critique of such a “fun-and-games attitude,” see Ring, [1967](#)). This also implies, however, that trying to improve psychological research practices through increasing the power of studies (Button et al., [2013](#)), preventing questionable research practices such as p-hacking (Simmons et al., [2011](#)), or reducing publication bias (Ferguson & Heene, [2012](#)) may be necessary, but is certainly not sufficient. What is needed is more attention to the theoretical underpinnings and the conceptual basis of psychological research.

The key question is, of course, how devoting more attention to theory building should look like. One suggestion that has gained

considerable momentum in recent years is the idea that theory and model building in psychology could substantially be improved through formalization and mathematization (Borgstede, this volume; De Boeck et al., this volume; Ellinghaus & Ulrich, this volume; see also Borsboom et al., [2021](#); Fried, [2020](#); Guest & Martin, [2021](#); Oberauer & Lewandowsky, [2019](#); Robinaugh et al., [2021](#); Smaldino, [2020](#); Trafimow & Fiedler, [2026](#); van Rooij & Baggio, [2021](#)). The basic idea behind calls for (mathematical) formalization is that formalization could help to reduce the vagueness and indeterminacy of verbal theories, thereby tightening the link between theory, hypotheses, and data—ultimately also increasing the replicability and reliability of psychological studies. While not necessarily denying that formalization can be a useful tool, others have pointed to its inherent limitations and challenges (Eronen & Bringmann, [2021](#); Hutmacher, [2026](#); Hutmacher & Franz, [2025](#); Oude Maatman, [2021](#); Sanbonmatsu & Johnston, [2019](#); Slaney, [2023](#)). For instance, formalization alone is not enough to guarantee robust phenomena, well-defined constructs, or valid ways of measuring these constructs (Eronen & Bringmann, [2021](#)). In addition, the model specifications that researchers choose crucially depend on their knowledge and their assumptions about the phenomena under investigation, suggesting that formal theories cannot simply replace verbal theories (Hutmacher, [2026](#); Hutmacher & Franz, [2025](#); Oude Maatman, [2021](#)).

No matter to what degree formalization and mathematization can contribute to the improvement of theory building in psychology; however, it seems important to acknowledge that theorizing is not done in an abstract, empty void but within certain societal and institutional structures (Cornejo & Rojas, this volume; Duden & Sunohara, this volume; see also Munafò et al., [2020](#); Rahal et al., [2023](#)). One frequently mentioned problem in this regard concerns the *publish or perish* culture that requires researchers to publish as much as possible in order to succeed in an academic career (Trueblood et al., [2025](#)) and that might rather undermine than support the generation of knowledge (Chu & Evans, [2021](#)). Although competition for the best arguments, methods,

and theories is integral to any intellectual endeavor, overemphasizing it undermines the equally essential potential for cooperation (cf. Altenmüller & Gollwitzer, [2022](#); Anderson et al., [2007](#)). For instance, competition for funding can have a wide range of economic costs (e.g., time to write proposals that might never be accepted), epistemic costs (e.g., balancing groundbreaking and incremental, basic and applied research) as well as social and ethical costs (e.g., creating and reinforcing distinct groups of winners and losers; Schweiger et al., [2024](#); see also Giner-Sorolla, [2019](#); Lilienfeld, [2017](#)). In a similar vein, the peer review system on which academic publishing is based has been criticized for being deeply biased (for or against authors, topics, methods, groups, countries) as well as for a general lack of quality (e.g., lack of qualified reviewers, insufficient scrutiny) and reliability (e.g., limited agreement and frequent disagreement between reviewers; Aczel et al., [2025](#)). On an even more fundamental level, it has been emphasized that psychological theorizing is too much focused on Western, educated, industrialized, rich, and democratic countries (so-called WEIRD countries; Apicella et al., [2020](#); Henrich et al., [2010](#); see also Duden & Sunohara, this volume) and that not only the state of untenured researchers is precarious but potentially also the status of participants who might be underpaid and exploited in some cases (Callard, [2022](#)).

But What Is a Good Theory, Actually?

Assume that all the abovementioned obstacles that arguably keep psychologists from building better theories could be removed. Then, a fundamental question would still remain unanswered: What criteria define *good* theories—and how do we decide whether a theory meets these criteria (Fried, [2020](#); van Rooij et al., [2024](#))? As far as potential criteria are concerned, there is no lack of suggestions: Typical candidates include logical consistency, discursive survival, and the ability to withstand falsification attempts, as well as the potential to explicate the relationship between theory and observation (Funke, this volume; Guest, [2024](#); for a detailed discussion of different criteria, see

also Royce, [1978](#)). Others emphasize that good theories are able to provide explanations (van Rooij & Baggio, [2021](#)), that they are useful in the sense that they help to solve problems that researchers are interested in (Levenstein et al., [2024](#); Yarkoni, [2020](#)), that they help to identify new problems that deserve attention (Adolfi et al., [2024](#)), or that they have an emancipatory potential while minimizing the harm that they bring to individuals and societies (Guest, [2024](#)).

Instead of asking for criteria for success, some researchers articulate the pitfalls of theory building by discussing the nature of *weak theories*. Hans Jürgen Eysenck ([1985](#)) argues that weak theories in psychology are not necessarily a problem and that “the value of a weak theory [...] lies in the fact that it directs attention to those problems which most repay study from a systematic point of view” (Eysenck, [1985](#), p. 24). In contrast, Royce ([1978](#)) holds that weak theories are merely programmatic, holding only heuristic value insofar as they accommodate new data. He suggests that stronger theory building is either descriptive—developing a tight nomological network—or explanatory, meaning it aims to maximize theoretical comprehensiveness. Gigerenzer ([1998](#)) critiques four forms of weak theory building, which he refers to as *surrogates of theory*, namely, one-word explanations (such as *anchoring*), redescription (such as attributing aggressive behavior to an aggressive disposition), muddy dichotomies (such as nature vs. nurture), and data fitting (i.e., replacing theoretical interpretations with concerns about model fit).

Naturally, none of the abovementioned quality criteria remain undisputed. Take the criterion of parsimony, for instance: While the usual view seems to be that theories should be as simple as possible—not making more assumptions than are strictly necessary to describe, explain, or predict a certain phenomenon—it has recently been argued that highly complex models can be desirable and helpful for scientific progress under certain circumstances (Dubova et al., [2025](#)). That being said, all researchers at least implicitly make constant judgments about the quality of theories, such as when they decide which theory to work with, which open research questions to address, and which arguments

or evidence to criticize. In essence, this also suggests that the way individuals conduct psychological research is shaped by a host of epistemic and non-epistemic values (Longino, [2004](#); Osbeck, [2019](#)). Whether there is a viable way to more systematic consensus-building processes and how it could take shape is part of an ongoing debate (e.g., Asendorpf & Gebauer, [2022](#); Hilbig et al., [2022](#); Leising et al., [2022](#), [2024](#)). In any case, this debate serves as an important reminder that simply creating more or different theories without reflecting on why these theories are needed or why they are better than existing ones will certainly not advance the field.

Sticking Point #3: The Subject Matter(s) of Psychology

At first glance, it may seem intuitive to sort academic psychologists into one of two camps (cf. Kimble, [1984](#)): either they hold a natural science perspective on psychology, or they embrace approaches rooted in the psychological humanities. There certainly is something to this distinction that has been discussed in one way or another for more than a century now (for some classics, see Dilthey, 1883/[1989](#); Ward, [1904](#); see also Koch, [1981](#)): There are indeed many psychologists who believe that psychological research could greatly benefit from using the natural science as a role model and who also argue that psychology has been most successful in the past whenever it followed this recommendation (Gigerenzer, [2024](#); Proctor & Warren, [2024](#); Trafimow & Fiedler, [2026](#); see also Borgstede, this volume; Ellinghaus & Ulrich, this volume). At the same time, many others argue that a natural science perspective on psychology is insufficient because it cannot account for the societal, cultural, and historical embeddedness and variability of psychological phenomena and fails to do justice to the reflexivity and intentionality of the individual person (Freeman, [2023](#); Malich & Rehmann-Sutter, [2022](#); Schnepf & Groeben, [2024](#); Teo, [2017](#), [2024](#); see also Cornejo & Rojas, this volume; Duden & Sunohara, this volume; Eronen & Bringmann, this volume; Gaj, this volume; Stam, this volume).

At second glance, however, things become more complicated (Eronen et al., [2024](#); see also Tafreshi & Slaney, [2024](#)): Most of those who think that the natural sciences would be a good role model for psychology admit that psychological phenomena can be described and analyzed from a wide range of perspectives, and most of those who reject the idea that psychology can ever become like physics, chemistry, or biology admit that approaches rooted in the natural sciences may be appropriate at least in some circumstances, provided that their limitations are kept in mind. In other words, there seems to be some consensus that the subject matter of psychology falls *somewhere in between*, which is to say that it does not plainly coincide with either alternative (for some examples in this volume, see Cornejo & Rojas, this volume; Funke, this volume; Henriques & Gralha, this volume; Schleim, this volume; Sullivan, this volume; for a comprehensive discussion, see Wendt, [2024](#), [2025](#)).

Broadly speaking, there are two possible reactions to this “somewhere in between” status (see Osbeck, this volume; see also Goertzen, [2008](#)): On the one hand, there are psychologists who see the discipline’s inability to agree upon a common denominator as a sign of fragmentation, arguing that this is an unfortunate state that should be overcome through efforts at unification (e.g., Fishman, [1987](#); Henriques, [2011](#); Staats, [1991](#); Sternberg & Grigorenko, [2001](#); Yanchar & Slife, [1997](#); for an overview, see Gaj, [2016](#); see also Henriques & Gralha, this volume; Peper, this volume). Here, the general idea seems to be that psychology will only really begin to thrive as an academic discipline once it has managed to agree on its subject matter. Importantly, proponents of this perspective emphasize that the usual definition that “psychology is the study of the mind and behavior” (American Psychological Association, [2024](#)) is not sufficient to guarantee a common understanding, as the terms invoked in this definition are highly ambiguous and require explanation themselves (Wendt, [2024](#), [2025](#); see also Henriques & Gralha, this volume). Moreover, thinking about the subject matter of psychology may require thinking about the discipline’s metaphysical underpinnings (Hibberd,

[2014](#)). On the other hand, there are psychologists who dispute that the alleged fragmentation is a symptom of a crisis: They hold that the apparent diversity in psychological research stems from the multilayered nature of the subject matter, meaning that there can be no successful unification attempts, as they would always end up reducing the complexity of human mind and behavior (e.g., Green, [2015](#); Koch, [1993](#); Mitchell, [2023](#); Stam, [2004](#); Teo, [2010](#); see also Slaney, this volume; Sullivan, this volume). The upshot of this perspective is that psychologists should embrace the fact that psychology's subject matter is diverse instead of trying to unify what cannot be unified.

Naturally, pleas for both unification and pluralism entail their own unique challenges. Those striving for unification have to find a convincing answer to the question of how the deeper philosophical—namely, ontological and epistemological—differences behind the various perspectives on the subject matter of psychology can be overcome. For instance, one may consider fundamental questions such as the mind-body problem or debates about indeterminism versus determinism and atomism versus holism (Goertzen, [2008](#); see also Kimble, [1984](#)). If unification really is the goal, staying neutral regarding these issues does not seem to be a viable option. If authors commit to a certain perspective, however, this will almost certainly spark disagreement and counterarguments – undermining the very aim of unification. Proponents of pluralistic theorizing face exactly the opposite problem (e.g., Goertzen & Smythe, [2010](#); Healy, [2012](#); Watanabe, [2010](#); for recent discussions of pluralism, see, e.g., Araujo & Osbeck, [2023](#); Hutmacher, [2023](#); Osbeck & Araujo, [2023](#); see also Osbeck, this volume): If there are indeed different legitimate ways of theorizing about human mind and behavior, how do we know which perspective is most adequate in a certain situation? And, maybe even more importantly: How can a productive dialogue between different perspectives be structured to allow a rigorous evaluation of their strengths and weaknesses? That is, how do we coordinate theoretical pluralism?

Sticking Point #4: Theory and Empirical Research

The debates about the subject matter(s) of psychology are closely related to debates about the appropriate methods for psychological research: Researchers who promote a natural science perspective on human mind and behavior usually also favor quantitative-experimental methods, while those skeptical of this perspective typically prefer qualitative-interpretative methods. Again, however, this dichotomous distinction is overly simplistic. To begin with, the distinction between quantitative and qualitative methods is not as clear-cut as one might initially think (cf. Hammersley, [1992](#)): Numerous distinct methods fall under each umbrella term, and there are many possibilities for crossing methodological boundaries. Even more importantly, those who believe that methods rooted in the natural sciences are promising do not necessarily insist that these methods *must be used* but rather that there are examples demonstrating that these methods *can be useful* (see, e.g., De Boeck et al., this volume; Ellinghaus & Ulrich, this volume). In a similar vein, critics of the dominance of quantitative-experimental methods who emphasize that their limitations should not be overlooked usually claim that a broader methodological repertoire is needed rather than stating that quantitative-experimental methods should be discarded (e.g., Dale et al., [2023](#); Danziger, [1985](#); Lamiell & Slaney, [2021](#); Mayrhofer & Hutmacher, [2020](#); Schnepf & Groeben, [2024](#); Toulmin & Leary, [1985](#); see also Cornejo & Rojas, this volume). Importantly, calls for methodological pluralism and for ending the devaluation of methods other than the experiment are sometimes even made by those who use methods derived from the natural sciences in their own work (see, e.g., De Boeck et al., this volume; Funke, this volume; see also Sternberg & Grigorenko, [2001](#)). If methodological pluralism is desirable, however, a similar problem arises as in the case of theoretical pluralism: How can we compare, evaluate, triangulate, and integrate the findings obtained through different methods? And

how do we determine whether (and to what degree) a certain method does justice to a psychological phenomenon (cf. Teo, [2021](#))?

These abstract debates can be illustrated through the case of historical psychology. Broadly speaking, historical psychology aims to understand and reconstruct the thinking, feeling, and behavior of past cultures and societies (Atari & Henrich, [2023](#); Hutmacher, [2022](#); Hutmacher & Mayrhofer, [2023](#); Muthukrishna et al., [2021](#); Pizarroso, [2020](#); Sullivan, [2020](#), see also Pizarroso, this volume; Sullivan, this volume). Often, this is done with the goal of either showcasing the (in-)variability of psychological phenomena, demonstrating how past societies differ from present-day societies, or understanding how the present-day societies evolved and came into being (for exemplary discussions regarding the concept of stress, see Hutmacher, [2019](#), [2021](#)). With these aims, historical psychology is closely aligned with neighboring research programs such as cognitive archeology (Henley et al., [2019](#)), historical anthropology (Wulf, [2013](#)), and cultural history (Burke, [2019](#)). Scholars working in historical psychology can arguably be divided into a “historical data camp,” which subscribes to quantitative methods and statistical analyses and a “historical epistemology camp,” which emphasizes the need for researcher reflexivity, a commitment to interdisciplinarity as well as methodological pluralism (Sullivan, this volume; see also Pizarroso, this volume). In a nutshell, the debates between these two groups thus reflect the broader discussions about the appropriate methods for psychological investigations more generally. As we see it, this observation also holds a more general lesson: Many of the debates about theory building in psychology, which may initially seem abstract, quickly take a concrete meaning in specific contexts; at the same time, debates that appear context-specific often reveal more general conflicts that are relevant to the discipline as a whole.

Moving beyond the question of which methods are best suited for investigating psychological phenomena, scholars have repeatedly highlighted the intricate relationship between theory and data. Two aspects seem particularly noteworthy. First, theories and data are not

independent: For one, the theories that researchers (implicitly or explicitly) hold about psychological phenomena influence the paradigms that they choose for investigating them as well as their analytic strategies and their interpretations, an observation that is commonly referred to as the theory-ladenness of observation (Boyd & Bogen, [2021](#); Dale et al., [2023](#); see also Borgstede, this volume). However, the opposite is also true: Theories are usually data-laden, that is, they emerge from observations that researchers have made. This creates a chicken-and-egg problem: “When a new scientific programme emerges, is it a new theory [...] that leads to new methods [...] and experimentation? Or is it a discovery in unrelated or theoretically unconstrained observation that leads to development of theory?” (Dale et al., [2023](#), p. 62) Arguably, this also has important consequences for the conceptualization of exploratory research: While some argue that exploratory research is equivalent to a theory-free exploration of data (e.g., Wagenmakers et al., [2012](#)), the idea that there can, in principle, be no theory-free exploration suggests that exploratory research should instead be understood as a principled endeavor that combines systematic variation and conceptual openness (Feest & Devezzer, this volume).

Second, there are multiple issues with measurement in psychology: To begin with, it has repeatedly been observed that “measurement instruments that supposedly measure a given construct often turn out to measure different things (i.e. the jingle fallacy), and that measurement instruments for different constructs often turn out to measure the same thing (i.e. the jangle fallacy)” (Peters & Crutzen, [2024](#), p. 1; see also Flake & Fried, [2020](#)). This is related to another observation: It is typically not raw data (e.g., verbal reports, reaction times, answers to questionnaire items) that shape theory building; rather, data are treated as indicators of psychological phenomena—and these phenomena inform theory building (Borsboom et al., [2021](#); Eronen & Bringmann, [2021](#); Haig, [2013](#); Woodward, [1989](#)). This means, however, that building psychological theories not only requires reliable data and reliable phenomena but also careful consideration of how

phenomena can be reliably delineated from the data (see, e.g., Eronen & Bringmann, [2021](#); Feest, [2024](#); Sanbonmatsu et al., [2025](#)).¹ On an even more fundamental level, there is an ongoing controversy over whether psychological attributes are measurable and quantifiable and whether measurement in psychology is comparable to measurement in the natural sciences (Eronen, [2025](#); Franz, [2022](#); Humphry, [2017](#); Leising & Borgstede, [2019](#); Maraun, [1998](#); Michell, [1997](#); Saint-Mont, [2012](#); Slaney & Racine, [2013](#)). In any case, all these debates about the interconnections between theory and data indicate that empirical psychological research faces not only many practical challenges—such as reducing questionable research practices or ensuring the reliability of findings (cf. John et al., [2012](#); Munafò et al., [2017](#))—but also more foundational issues that concern the very core of theorizing and collecting data.

Sticking Point #5: Theory and Practice

In an ideal world, theory and practice go hand in hand: Theory building informs psychological practice—and the problems and phenomena observed in psychological practice inspire research and theory building. One historical example illustrates this dynamic (Wendler & Storck, this volume): In the early days of psychoanalysis, experiences with clients during therapy sessions directly led to refinements in psychoanalytic theories, which in turn were immediately applied in therapeutic practice. Of course, the nexus between theory and practice does not always have to be this close: Understanding psychological phenomena can be an epistemic goal in and of itself, just as understanding the particles of the universe can be an epistemic goal in and of itself. At the same time, the resources available for academic research are limited. Hence, each discipline must justify its existence, as well as the selection of problems and phenomena it chooses to study. Given that addressing social issues almost always involves psychology, keeping the usefulness of psychological theories in mind seems natural (Schleim, this volume).² Already more than 50 years ago, Kenneth Ring

([1967](#)) encouraged psychologists to ask themselves the following question before initiating research: “Does this research really deal with a problem of some broad [...] human significance, or does it represent (be honest now!) merely an interest in one’s own pet quiddities?” (p. 122)

Against this background, it is particularly worrying that researchers have repeatedly observed a disconnect between theory and practice in psychology (Berkman & Wilson, [2021](#); Giner-Sorolla, [2019](#); for earlier examples, see Dewey, [1904](#); Holzkamp, 1970/[2009](#); see also Derksen, this volume; Gaj, this volume; Schleim, this volume). Practitioners often ignore the available scientific evidence or are at least hesitant to apply it in their everyday work; crucially, however, this resistance can at least partly be attributed to the irrelevance of many psychological theories for practical purposes (Lilienfeld et al., [2013](#); Stewart et al., [2012](#)). One way of explaining—and in some ways justifying—the disconnect between theory and practice is to point to the different logic behind empirical research and practical applications. According to this line of reasoning, researchers are primarily interested in identifying general patterns, while practitioners must deal with specific individuals in specific real-life settings. That is, academic psychologists want to find out how *the* human mind works, while psychologists working in applied settings need to understand how the mind of a particular individual works in a given situation and context. However, this distinction might actually be less clear-cut (Gaj, this volume): On the one hand, psychological theorizing can be more or less general, ranging from theories that explain small sets of phenomena to theories that are relatively broad. On the other hand, practitioners do not normally start from scratch in every single case but follow general prescriptive principles and use a certain set of methods that are adapted to a specific context.

Instead of construing theory and practice as strictly separate opposites, it therefore seems advisable to consider how translation processes between theory and practice (and vice versa) can be improved. Regarding the improvement of translation from practice to

theory, a relatively common suggestion is to demand more applied research (e.g., Andersen, [2025](#); Giner-Sorolla, [2019](#)) and to reduce the “laboratory-based, mental masturbation, valid in its own right, but devoid of contact with mundane reality” (Helmreich, [1975](#), p. 558). In his classic paper, Douglas Mook ([1983](#)) countered such demands with a *defense of external invalidity*: He argues that there are cases in which researchers are not so much concerned with applying their findings to real-world settings but rather seek to demonstrate that certain mechanisms exist or that certain effects can be made to occur under specific circumstances. In these cases, however, the artificiality of the laboratory is exactly what is needed (see also Fiedler, [2020](#); Henshel, [1980](#)). Nevertheless, one might still hold that contemporary psychological theorizing is too much focused on *effects that can be made to occur* and too little on *effects that do occur* (Brauer & Kennedy, [2023](#); see also Derksen, this volume). In other words, the argument is that psychological research should aim to determine which of the effects that can be observed in highly controlled settings are actually also relevant outside the laboratory.

Regarding the translation from theory to practice, two aspects deserve attention. First, the decision of how to translate theoretical knowledge into practical applications is often left to the “judgment,” “expertise,” “wisdom,” or “experience” of the practitioner (Derksen, this volume; Gaj, this volume). If translation is to be successful, more research is needed into practitioners’ thinking and behavior to understand the processes that are demarcated by these buzzwords. Second, an alternative to creating study settings that more closely resemble real-life scenarios is to create real-life scenarios that more closely resemble study settings (Derksen, this volume). If empirical research has determined, for instance, how food should be arranged in a canteen so that more individuals choose the healthy lunch option or how eyewitness interviews should be conducted to prevent memory distortions, one can create real-world settings that follow these insights. An ethical issue that arises in this context is whether it is morally justifiable to steer or “nudge” individuals in a certain direction

or whether policy approaches should always focus on empowering citizens and helping them make their own decisions (Hallsworth, [2023](#); Herzog & Hertwig, [2025](#); see also Holzkamp, 1970/[2009](#)). In other words, debates about (in-)adequate applications of psychological theories bring us back to one of the sticking points mentioned above, namely, the question of what makes a good psychological theory and whether the emancipatory potential of a theory should be included in its evaluation.

Conclusion

As emphasized at various points throughout this chapter, the five sticking points that emerged across the contributions to this collected volume are not new but have repeatedly been observed in the history of psychology as an academic discipline. This raises the question of whether these sticking points can be solved and overcome or whether they are permanent and will stay with the discipline for the foreseeable future. We feel that we are not in a position to answer this question. In fact, one of us (ANW) is more optimistic in this regard than the other (FH), mirroring the divide among the contributors. However, even if (some of) the debates sketched above should remain unresolvable, this does not mean that these debates cannot at least become more productive and clearer in their argumentative structure. To establish order and structure in theoretical psychology, it is necessary to articulate and communicate differences among sub-disciplinary fields and approaches, rather than retreat into mere coexistence. Coexistence is not proper pluralism: Quite the contrary, enacting pluralism *sensu strictu* requires a communicative effort to establish a scientific culture of debate (Wendt & Funke, [2022](#)). True scientific progress depends not on avoiding disagreement, but on embracing it through principled and transparent exchange. Controversy, when grounded in mutual respect and shared standards, is not a threat to psychology—it is its driving force.

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Footnotes

¹ Even more so, it may require explicating the concept of the *phenomenon* itself (Borsboom et al., 2021; see also Wendt & Wertz, this volume). If this is the case, then the abovementioned *chicken-and-egg problem* of theory and data may, in fact, resemble a kind of *three body problem*, raised by the question of whether phenomena, theories, or data provide the starting point for psychological research. One may even ask whether this tripartite relationship is still too simplistic: Perhaps scientific work involves additional constitutive elements, such as the basic logical or transcendental conditions for conducting research—elements that require yet another layer of critical corroboration.

[2](#) Note, however, that focusing on the usefulness or applicability of psychological theories may introduce a host of new questions and problems, such as: How do we deal with the fact that it might not be clear from the outset which theory will eventually turn out to be useful? Who defines which applications of psychological research are desirable and which are not?

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