Theory & Psychology

Personal Reflections on Theory and Psychology

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Abstract. Psychology's most important task is to integrate the various extant patchworks of theories into overarching theories. Theory integration is a longstanding concern in biology, economics, or physics, but not in psychology. We teach our students how to test theories, not the art of theory construction in the first place. As a consequence, in some parts of psychology, theories have become replaced by surrogates, such as circular restatements of the phenomenon, one-word explanations, and lists of general dichotomies. Moving backwards from existing models to labels is an odd event in science, which typically progresses in the opposite direction. Theory construction should be taught in graduate school, and editors of major journals should encourage submissions that make advances in theory integration.

Key Words: circular restatements, one-word explanations, theory construction, theory integration, tools-to-theories heuristic

When discussing psychological research, what surprises every economist or physicist is that psychology has no theory. It has many local ones but no overarching theory, not even a provisional one. Yet there is something even more surprising: a lack of awareness of the value of integration. Whereas the unification of theories, such as evolutionary theory and genetics, is a widely shared goal in physics and biology, it is barely visible in psychology. Few psychologists even consider theory integration as an objective. A textbook in economics starts with first principles that lead to an overarching theory and discusses how reality fits into this picture. A textbook in psychology lists

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dozens of theories in chapters on reasoning, intelligence, problem solving, and judgment and decision making—topics that appear closely related, but are populated by different researchers, published in different journals, and presented as independent enterprises. To the poor student, the relation between the various concepts in these different theories is never made clear. Why is present-day psychology such a patchwork of small territories, resembling, to use a political metaphor, Italy or Germany before unification around 1870? Why are psychologists so content working within the confines of their own small territories?

I have written a few pieces on this problem; these belong to my least cited papers. It appears to be a topic discussed less in journals than in letters. In response to my short essay entitled "Surrogates for Theory" in the APS *Observer* in 2009, which detailed some of psychologists' clever ways to avoid constructing theories, a professor from Indiana University wrote: "No field within psychology suffers more from the theoretical malaise than mine, educational psychology." Others working in social psychology, reasoning, and judgment and decision making claimed that the malaise is most virulent in their respective fields. A professor from a business school wrote: "I think the problem is that people in the social sciences have no idea how to build theory." Many letters came from distinguished professors emeritus. Young scholars appear to be busy with something else, building careers rather than theories.

When the editorial team for *Theory & Psychology* first met, we discussed what name to give the new journal. The common vision was to promote theory in a discipline where data are often the end rather than the means; some of us therefore proposed naming it "Journal of Theoretical Psychology." After all, there is a *Journal of Theoretical Biology* and an *International Journal of Theoretical Physics*; economic journals do not even need this qualifier because most economists would not consider publishing an article without formal theory. In many a discipline, those in the theoretical tower think more highly of themselves than of those in the empirical branch. That even holds for mathematics, where probability theorists tend to look down at statisticians and their "dirty hands" from dealing with the messy world. Einstein's assertion that he would reject the data before he would reject relativity theory illustrates the common trust in strong theory as opposed to noisy data (cited in Mahoney, 1979). Yet, as I recall, the title "Journal of Theoretical Psychology" encountered resistance, not only from the publisher. With such a precarious name, it would not sell. Few psychologists would open a journal with "theoretical" in the title, and even fewer would submit papers. The sad truth is that these objections were not off the mark. The final compromise was *Theory & Psychology*, which both distanced psychology from the problematic term and connected the two.[1] There appears to be something peculiar about psychology and its relation to theory. Here are some observations and proposals.

Teach Theory Construction?

The argument against teaching theory construction is that there is no single recipe for designing a theory. We had better teach experimental methodology and statistics instead, the argument continues, and hopefully theories will somehow emerge from significant results. The counter-argument is that there is no single formula for methodology and statistics either. Generations of psychology students have been deceived about this fact: psychological textbooks on statistics are typically silent about different statistical theories and instead present an incoherent mishmash of Fisher, Neyman-Pearson, and Bayes as an apparent monolithic truth. The result is statistical rituals, not statistical thinking (Gigerenzer, 2004). We should not teach theory construction in the same mindless way as statistics, but as the art of scientific thinking and discovery.

A curriculum on theory construction might start with good examples, first principles, and the ability to detect surrogates for theory (see below). First principles could be learned from a dip into the history of science, such as Kuhn's list of features of good theories: accuracy, consistency, broad scope, simplicity, and fruitfulness. But it should not begin and end with the history and philosophy of science, which for the most part does not deal with psychology. Rather, comparing research practices in the natural sciences with those in psychology is a first step towards getting a graduate student to think. For instance, many psychological theories—prospect theory is a prominent example—differ from those in the natural sciences in their abundant use of free parameters. Although the laws of classical physics do have parameters, the purpose is to measure these as precisely as possible rather than to fit them to each data set anew. In much of psychological research, however, the opposite is common practice: parameters are never fixed but are fitted to every new data set, with the goal to increase R^2, or some other measure of goodness of fit. The nature of parameters in a theory is only one dimension on which psychological theories differ from one another and from those of other disciplines. Others include: formal versus verbal theories, optimization versus heuristic processes, as-if models versus process models, and domain-specific versus domain-general theories.

To illustrate, the common approach in research on cognition and decision making builds formal models with many free parameters that assume some optimization processes (e.g., Bayesian or expected utility maximization); they predict behavior but do not model cognitive processes (as-if models) and are proposed as domain-general theories of cognition. In my own work on bounded rationality (Gigerenzer, 2008), I design formal models of heuristics with zero-free parameters that reveal clearly what the model can predict and what it cannot; these models assume cognitive processes that are of heuristic nature rather than concerned with optimizing some function (because optimization is

rarely feasible in an uncertain world). That is, cognitive processes employ limited search and ignore information in order to be robust rather than "optimal." These models are process models that specify a search rule (the direction of search for information in the search space), a stopping rule (when search is stopped), and how a decision is finally made. Finally, there is not one general heuristic but an adaptive "toolbox" containing domain-specific heuristics. As a consequence, I study the "ecological rationality" of each heuristic, that is, the worlds in which they are more or less successful.

With these and other distinctions in mind, students can understand the structure of current theories and alternative ways of building theories.

From Tools to Theories

One of the most instructive ways of teaching theory construction is to investigate the origins of existing theories. The neglect of theory construction goes hand in hand with the inductive story that theories emerge from data, yet in spite of being repeatedly asserted, it is too simplistic an explanation (Holton, 1988). Discovery entails more than data and theories; it also involves scientific tools and practice. In fact, one major source of psychological theories is researchers' tools, such as statistics and computers. When psychologists grow accustomed to a new tool for data processing, the tool is repeatedly proposed as the way the mind works. I have called this principle of discovery the *tools-to-theories heuristic* (Gigerenzer, 1991). For instance, signal detection theory serves as a rich template for psychological theories, from sensory discrimination to recognition memory to eyewitness testimony in courts. It is a fruitful theory in the sense that it provides a highly useful conceptual language (hit rate, false alarm rate, decision criterion, sensitivity d′) and has been applied broadly. Originally proposed by Tanner and Swets (1954), signal detection theory did not emerge from data but from a statistical tool, the Neyman-Pearson hypotheses testing method. Tanner called his model mind a "Neyman-Pearson detector," and the origins of the theory are described in detail by Gigerenzer and Murray (1987, chap. 2). Instead of simply emerging from data, the new theory in fact changed the very nature of the data psychologists were generating. Just as in Neyman-Pearson theory, the novel data were hits and false alarms, unlike in the earlier work on sensory discrimination, from Fechner to Thurstone, which was based on measuring thresholds or psychological differences between stimuli. Here, we have a fascinating story about how a major new theory was discovered. After talking with some of today's major proponents of signal detection theory, I realized that few are aware of the origin of their own theory, with most believing that it stemmed from new experimental data. Yet the tool inspired the theory, and the theory inspired new kinds of data to be generated, which in turn were used to test the new theory.

The origin of a theory alone, however, does not tell us whether the theory is fruitful or accurate. But it does tell us something about its assumptions and possible alternatives. For instance, two quite different statistical theories of inference also turned into theories of the mind: Fisher's analysis of variance (ANOVA) and Bayes' probability updating rule. These two tools along with signal detection theory implied noticeably different pictures of the mind. Fisher's ANOVA provided the template for several cognitive theories, most prominently causal attribution theory (Kelley & Michaela, 1980). Unlike in signal detection theory, Fisher's null hypothesis testing deals with only one kind of error, and thus causal attribution theory has no decision criteria that balance hits and false alarms (Gigerenzer, 1991). Nor is causal attribution based on prior probabilities, using evidence to revise these into posterior probabilities, as are Bayesian models of mind. Theories that originate from Fisherian statistics tend to picture the mind as mostly data driven, very different from earlier theories of causal perception by Piaget or Michotte.

The fingerprint of the tool is both a theory's strength and its limitation. For Bayesian theories of cognition, which are presently in vogue, every mental task appears to involve computing a posterior probability distribution. The tools-to-theories process of discovery is also evident in exemplar models of categorization, where multidimensional scaling turned into a theory of mind, as well as in the analogy between the computer and the mind (Gigerenzer & Goldstein, 1996).

By studying the features of statistical and computer tools that turned into theories of mind, students can understand the structures of current theories and alternative ways of building theories.

Surrogates for Theory

Opening students' eyes to the origins of theories and alternatives for construction of theories should set the stage to develop psychology into a more theoretical discipline. Opening their eyes to *what is not a theory* can be of equal help. In some areas of psychology, the development of theories goes backwards, from extant genuine theories to surrogates that pretend to be psychological theories. I describe three techniques for avoiding theories by creating surrogates.

Circular Restatements as Explanations

The most primitive means of avoiding theories is to simply restate the phenomenon in question in different words and pretend to have offered an explanation. This technique is also known as re-description or tautology, and has a long tradition. Recall Molière's parody of the Aristotelian doctrine of substantial forms: Why does opium make us sleepy? Answer: because of its

dormative properties. Such circular restatements are not limited to attributing behavior *X* to an essence or trait *X*. A closer look at the structure of psychological explanations shows abundant uses of restatements in published research (Gigerenzer, 1996; Katzko, 2006; L. Wallach & Wallach, 1994; M.A. Wallach & Wallach, 1998). Here are examples from prominent research in the respective fields. In research on the "belief-bias effect," participants were instructed to judge the logical validity of syllogisms, such as: "No addictive things are inexpensive. Some cigarettes are inexpensive. Therefore, some addictive things are not cigarettes" (a logically invalid but believable conclusion). The observation was that judgments depended on both the logical validity and the believability of the conclusion. The explanation offered was: "Dual-process accounts propose that although participants attempt to reason logically in accord with the instructions, the influence of prior beliefs is extremely difficult to suppress and effectively competes for control of the response made" (Evans, 2003, p. 455). The phenomenon that both logical structure and prior belief influence judgments is "explained" by restating the phenomenon in other words. Dijksterhuis and van Knippenberg (1998) reported the interesting result that priming intelligent behavior by inducing a professor stereotype resulted in more intelligent behavior. This result was explained in this way: "In concrete terms, activation of the professor stereotype is expected to result in intelligent behavior because activation of the professor stereotype leads to activation of intelligence" (p. 872). Katzko (2006) has analyzed this and other surrogate explanations in detail. There is nothing wrong about not having an explanation for how priming (writing down everything that comes to mind about a "professor") can lead to higher performance in a general knowledge task. Yet circular restatements pretend to have an explanation and thus distract from finding a model about how the specific priming task could have an effect on a general knowledge task. Restatements both create a theoretical void and cover it up.

One-Word Explanations

The second technique for avoiding theories is equally abundant. The observation that some factor *X* influences people's judgments more than factor *Y* is explained by saying that *X* is more "salient," "available," "relevant," "representative," or "vivid." One-word explanations are so perfectly flexible that they can, after the fact, account for almost every observed behavior. This technique is also known as the use of labels instead of models. To illustrate, one-word explanations can account for both phenomenon *A* and its opposite, *non-A* (see Ayton & Fischer, 2004). Consider the gambler's fallacy: after a series of *n* reds on the roulette table, the intuition is that the chance of another red *decreases*. This intuition was explained by people's reliance on "representativeness" by saying that "the occurrence of black will result in a

more representative sequence than the occurrence of an additional red" (Tversky & Kahneman, 1974, p. 1125). Next consider the hot-hand fallacy, which is the opposite belief: after a basketball player scores a series of *n* hits, the intuition is that the chance for another hit *increases*. This intuition was also attributed to representativeness, because "even short random sequences are thought to be highly representative of their generating process" (Gilovich, Vallone, & Tversky, 1985, p. 295). No formal model of similarity ("representativeness") can predict a phenomenon and its contrary, but a label can do this by changing its meaning. To account for the gambler's fallacy, the term alludes to a higher similarity between the series of $n + 1$ outcomes and the underlying chance process, whereas to account for the hot-hand fallacy, it alludes to a similarity between a series of *n* and a new observation $n + 1$ (Gigerenzer & Brighton, 2009). One-word explanations can be neither proved nor disproved, and hence do not enhance our understanding of how the mind works.

Lists of Dichotomies

A third way to avoid building theories are yin–yang lists of general dichotomies: *associative*, *unconscious, effortless*, *heuristic*, and *suboptimal* processes (assumed to foster "intuitive" judgments) versus *rule-based*, *conscious, effortful, analytic*, and *rational* processes (assumed to characterize "deliberate" judgments). The first list is called System 1, the second System 2, and both lists together are called dual-process theories of reasoning (e.g., Sloman, 1996). Today, we witness an avalanche of dual-systems or dual-process theories. In response to Sloman's original paper, Terry Regier and I (Gigerenzer & Regier, 1996) showed in some detail that much clarity is lost when one subsumes existing conceptual distinctions—such as Smolensky's intuitive processor versus rule interpreter, Hinton's intuitive versus rational processing, Schneider and Shiffrin's automatic versus controlled processes, and Freud's primary versus secondary processes—into one associative versus rule-based dichotomy, and the same holds for the other dichotomies. The problems we pointed out in our article have never been addressed or answered, nor is this article cited in current reviews on dual-process theories (e.g., Evans, 2008; Evans & Frankish, 2009). Dual-systems theories of reasoning exemplify the backwards development from precise theories to surrogates. Consider the work on the adaptive decision maker (Payne, Bettman, & Johnson, 1993) and on the adaptive toolbox (Gigerenzer, Todd, & ABC Research Group, 1999), which specifies a theory of heuristics, their building blocks, their ecological rationality, and the core cognitive capacities that heuristics exploit. I have seen none of this theoretical and experimental work incorporated into dual-process theories—this wealth of knowledge is now bundled together and renamed a "System 1" that is said to operate in a heuristic mode. The resulting problem with two-system theories "is the lack of any predictive power and the tendency to employ them as an after-the-fact

explanation" (Keren & Schul, 2009, p. 544). Last but not least, the "yin" and "yang" dichotomies do not even match. For instance, the same heuristic can be used both intuitively and deliberately (Kruglanski & Gigerenzer, in press).

All three surrogates are obstacles to building rich and precise theories and, in fact, represent a step away from already existing theories. Moving "backwards" from existing models to labels is an odd occurrence in science, which typically proceeds in the opposite direction.

Pursue Theory Integration?

In physics, the integration of extant theories is a primary goal. An example is the struggle to combine general relativity theory, which describes cosmic behavior on a large scale, with quantum theory, which describes behavior on a subatomic scale. In psychology, in contrast, the goal of bridging different theories is rarely pursued. Many years ago, Michael Watkins (1984) wrote that a cognitive theory "is a bit like someone else's toothbrush—it is fine for that individual's use, but for the rest of us ... well, we would just rather not, thank you" (p. 86). That attitude has changed little over the last decades: 25 years later Walter Mischel (2009) repeated that many psychologists still tend to treat theories like toothbrushes—no self-respecting person wants to use anyone else's. This aversion of entering others' territory is associated with the widespread but flawed methodological practice of testing only one theory—one's own toothbrush—against data, as opposed to testing two or more theories comparatively. The toothbrush analogy may be amusing, but it is embarrassing that researchers show little interest even in the neighboring subcommunities that work on the same topics, not to speak of other disciplines. For instance, psychologists who study intelligence using tests rarely talk to those who study thinking using experiments, and both communities tend to avoid philosophers and economists who study rational decision making.

There are two paradigms for conducting psychological research. One is territorial, the second factual. In the first paradigm, researchers identify with a subdiscipline, such as developmental psychology or social psychology, including its methodology, publication outlets, and conferences, and ignore everything outside this professional frame. In the second paradigm, researchers identify with a topic, such as decision-making or moral behavior, and ignore disciplinary borders, using all methodologies and knowledge available to understand the topic. The first professional pathway is, in my observation, the one that most psychologists have set foot on. It is good enough for making a career but prevents psychology from ever becoming a cumulative science. The second professional pathway, less trod upon, opens the possibility for integrating different theories on the same topic.

Let me insert a personal note. Before I became director at the Max Planck Institute for Human Development, I was professor at the University of

Chicago, my favorite university in the world. The reason why I left for Max Planck was the opportunity to set up an interdisciplinary, international research group that is funded long-term and enables a few dozen smart researchers from different disciplines to work together to develop a theory of heuristic decision making under uncertainty. Currently, I have psychologists, mathematicians, computer scientists, behavioral economists, evolutionary biologists, philosophers, engineers, and others working together. This group has existed for 15 years to date and has been able to make major contributions, including to the study of the adaptive toolbox and ecological rationality, or what can be called the science of heuristics (Gigerenzer et al., 1999). Bringing researchers from different disciplines and subdisciplines together has enabled progress in both theory construction and theory integration. Examples for theory integration from our research group are the integration of ACT-R theory with the recognition heuristic model, resulting in new insights on when systematic forgetting helps to make better inferences (Schooler & Hertwig, 2005), and the integration of signal detection theory with fast-and-frugal trees, another class of heuristics, which allows the connections between the concepts in both frameworks to be understood in a way not possible before (Luan, Schooler, & Gigerenzer, in press). None of this would have happened if I had worked within the territorial paradigm. Last but not least, I learn something new every day from my colleagues with different backgrounds and am never bored.

Final Thought

Many researchers believe that in order to have a successful career, they need to publish as much as possible, the proliferation of so-called "least-publishable units." Yet excellent departments want to hire researchers who have made a theoretical contribution and usually discourage mere quantity by asking applicants to submit their six best papers only. This procedure generates incentives to write something better than one has written before, not just more of the same. Similarly, the editors of major journals might consider writing editorials that discourage toothbrush culture and surrogates, and explicitly inviting articles that make advances in theory integration. This will make it easier for young researchers to combine pursuing a career with developing psychology into a theoretical enterprise.

Note

1. There is of course the International Society for Theoretical Psychology, home to *Theory & Psychology*. Yet this society is only loosely connected with what one would expect to read in the *Psychological Review*, the theoretical flagship of psychology.

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