aps ASSOCIATION FOR PSYCHOLOGICAL SCIENCE

Perspectives on Psychological Science
7(2) 99–108

DOI: 10.1177/1745691611434210
http://pps.sagepub.com
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There Is Nothing So Theoretical as a Good Method

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Abstract

This article documents two facts that are provocative in juxtaposition. First: There is multidecade durability of theory controversies in psychology, demonstrated here in the subdisciplines of cognitive and social psychology. Second: There is a much greater frequency of Nobel science awards for contributions to method than for contributions to theory, shown here in an analysis of the last two decades of Nobel awards in physics, chemistry, and medicine. The available documentation of Nobel awards reveals two forms of method–theory synergy: (a) existing theories were often essential in enabling development of awarded methods, and (b) award-receiving methods often generated previously inconceivable data, which in turn inspired previously inconceivable theories. It is easy to find illustrations of these same synergies also in psychology. Perhaps greater recognition of the value of method in advancing theory can help to achieve resolutions of psychology's persistent theory controversies.

Keywords

method, theory, crucial experiments, Nobel Prizes

> "There is nothing so practical as a good theory"
>
> (Lewin, 1951)

Here is one interpretation of these nine words with which Kurt Lewin memorably proclaimed the value of theory: Established theories include rules of correspondence that connect the theory's concepts and principles to empirical observations. When a theory is "good" (in Lewin's sense), its rules of correspondence go beyond assigning conceptual labels to laboratory research procedures. They extend the theory's concepts and principles to the nonlaboratory world—in other words, to the possibility of useful applications. This article shamelessly uses Lewin's aphorism as the template for a further assertion—the one provided in this article's title—that celebrates method much as Lewin celebrated theory.

Lewin has had much company in praising theory. Theory is widely regarded as the most creative form of scientific contribution. Scientific disciplines that stress theory are characterized as "basic" or "pure." More empirically or practically focused disciplines are seen as "technical" or "applied"—labels that most will see as implying lower status. Journals that feature theoretical articles often stand as their disciplines' publication flagships. Psychology's most elite empirical journals often oblige authors to establish the value of submitted articles by making clear how their empirical work "advances theory."[1]

This article is the latest of the author's series of descriptions of the vicissitudes of theory in relation to advancement of psychological knowledge. The previous attempts (Greenwald, 1975, 2004; Greenwald, Pratkanis, Leippe, & Baumgardner, 1986) have been read by some as advocating the conduct of psychological research without reference to theory (e.g., by Greenberg, Solomon, Pyszczynski, & Steinberg, 1988). That was never the author's aim. Indeed, the idea of conducting research without reference to theory seems inconceivable. Those earlier efforts may have been read as antitheory because of their implications that psychologists worship excessively at the altar of theory.

Rather than being antitheory, the author's attitude toward theory may better be characterized as "skeptically reverential." The reverential portion recognizes the power of theory to achieve parsimonious understanding and to guide useful applications—precisely the wisdom of Lewin's statement. The skeptical portion comes from noticing the ability of theory to restrict open-mindedness. This article concludes by recognizing the power of theory in the context of describing method–theory synergy. But, first, a visit to the darker side.

Competition Between Theories

When alternative theories contest the interpretation of an interesting finding, researchers are drawn like moths to a

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flame. J. R. Platt (1964) gave the approving label "strong inference" to experiments that were designed as crucial empirical confrontations between theories that competed to explain a compellingly interesting empirical result. Platt regarded strong-inference (crucial) experiments as efficient vehicles for scientific advance—his 1964 article is subtitled "Certain systematic methods of scientific thinking may produce much more rapid progress than others" (p. 347). Advocates of the strong-inference strategy might reasonably expect, as did Platt, that empirical confrontations of the strong-inference variety should rapidly resolve theoretical controversies. The test of this rapidity could be to show, by historical analysis, that when theory controversies are pursued via empirical confrontations, resolutions follow rapidly.

Life expectancy of theory controversies

In 1897, a British reporter asked Samuel Clemens, then traveling in London, whether he had any reply to a New York newspaper's report that Mark Twain (Clemens's literary pseudonym) had just died. Clemens's famous comment was "Just say that the report of my death has been grossly exaggerated" (Paine, 1912, p. 1039). A similar response can be expected from almost any psychologist whose theory has just been reported to have been empirically falsified.

Consider the 13 controversies listed in Table 1, included there because they were prominent controversies that were pursued via the strong-inference strategy in two psychological subdisciplines with which the author is familiar—cognitive and social psychology. For each of the 13 controversies, the table has a column for both a controversy-initiating and a controversy-ending publication. This allows a simple subtractive computation to reveal each controversy's duration. However, for all but one of Table 1's controversies, the subtraction is impossible—one cannot locate a controversy-ending publication. The one of these controversies that appears to be resolved is left unidentified in Table 1 only so as not to deprive readers the challenge of trying to retrieve a resolution of one of these (revealed below) from their own knowledge.

An alternative to the claim that Table 1's controversies remain unresolved is that they have, rather, been abandoned—researchers have simply lost interest. That alternative can be appraised by determining whether recent publications treat the controversy as active or dormant. A search for recent publications revealed that all but one of Table 1's 13 controversies were treated as active in recent publications.[2] Three others, to be considered toward the end of this article, appear to be approaching resolutions, even though the publication record indicates that their controversies remain active.

Advocates of Platt's strong-inference method may be puzzled by the near absence of resolutions for Table 1's controversies. Persistence of these competitions suggests that decades of presumably crucial empirical confrontations designed to resolve them may represent more of an illusion than a reality of theory competition. A closer examination of the history of each of these controversies (not given here) will reveal that

Table 1. Some of Psychology's Theory Competitions

Phenomenon	Competing theories	Initial or early publication	Controversy-resolving publication
Sapir–Whorf hypothesis	Language/culture does (or does not) influence categorization	Whorf (1956)	
Structure of affect	Bipolar vs. independent positive and negative dimensions	Nowlis and Nowlis (1956)	
Counterattitudinal role playing	Dissonance vs. self-perception vs. impression management	Festinger and Carlsmith (1959)	
Memory search	Serial vs. parallel search	Sternberg (1966)	
Implicit learning	Rules vs. associative learning	Reber (1967)	
Mental rotation	Analog vs. propositional representation	Shepard and Metzler (1971)	
Semantic priming	Spreading activation vs. compound cueing	Meyer and Schvaneveldt (1971)	
Categorization	Features, exemplars, prototypes, rules	Labov (1973)	
Altruism	Intrinsic vs. extrinsic motivation	Cialdini, Darby, and Vincent (1973)	
Misleading information	Altered traces vs. independent traces	Loftus and Palmer (1974)	
Judgment under uncertainty	Heuristics and biases vs. rationality	Tversky and Kahneman (1974)	
Affect–cognition relationship	Affective primacy vs. cognitive primacy	Zajonc (1980)	
Memory dissociations	Modules vs. processes vs. thresholds	Jacoby and Dallas (1981)	

Note. The emptiness of the rightmost column is not an accident—see text.

publications that were treated by one side as crucial opposition-falsifying findings were generally greeted by the opposed side as conceptually or empirically flawed efforts.

Philosophy of science does not help

Is psychology fated to be plagued with aging, unresolved theory competitions? Perhaps analytical reasoning can establish that competitions among theories are necessarily irresolvable. The logical basis for this thesis has received careful attention from philosophers of science. Ironically, philosophers' analyses of the prospects for empirically resolving theoretical disputes display their own lengthy, unresolved controversy. The controversy can easily be found in the philosophical literature by searching for the topic "underdetermination of theory by data." This is a body of literature in which one repeatedly finds the names of Quine, Duhem, Popper, Kuhn, Lakatos, and Feyerabend. With the exception of Popper, these philosophers have not been encouraging about the prospects for resolving theoretical controversies.

Even if philosophy had an answer, it would not help

Curiously, even if philosophers of science could manage to break their own deadlock about whether controversies like those in Table 1 were in principle resolvable, scientists would nevertheless be free to pursue such controversies endlessly. Perhaps even more curiously, if philosophers could decisively establish that all theoretical controversies were in principle not resolvable, it would nevertheless be possible for scientists to resolve any and all theoretical controversies.

How can this be so? Although it was only a relatively minor theoretical controversy, consider an issue that, until quite recently, occupied substantial time and attention of astronomers (Luu & Jewitt, 1996): whether the astronomical object Pluto is a planet or some subplanetary body. Regardless of any conclusion that philosophers could reach about the possibility of resolving that debate, astronomers had it in their power either (a) to prolong the debate or (b) to achieve a speedy resolution. It seems commendable that astronomers recently resolved the controversy, even if it was not to the satisfaction of all. Meeting in 2006 in Prague, Czech Republic, the International Astronomical Union concluded that Pluto was not a planet but a lesser object—one of many "dwarf planets" in the solar system.

A much more significant theoretical controversy—over the role of human immunodeficiency virus (HIV) as the cause of AIDS—occupied many medical scientists in the late 20th century. In 2000, 17 years after the discovery of HIV, more than 5,000 doctoral-level scientists, convening in Durban, South Africa, signed a declaration asserting that "HIV causes AIDS" (The Durban Declaration, 2000).

Another very significant theoretical controversy concerns the assertion that human activities contribute to global warming. Although there remains political opposition to that hypothesis, scientists are near unanimous in their willingness to declare that humans do indeed contribute to global warming (see the United Nations report: Solomon et al., 2007).

Unanimity among scientists is an unreasonable standard for resolution of theoretical controversies. Neither the 2000 Durban Declaration on HIV–AIDS nor the 2007 U.N. report on global warming has unanimous support among scientists. However, if any of Table 1's theoretical controversies similarly approached the near unanimity of scientists in support of those two conclusions, those persisting controversies would certainly now be regarded as having been resolved.

T. C. Chamberlin

Much of the blame for long-lasting theory competitions can be credited to a well-analyzed phenomenon—*confirmation bias*. Researchers are prone to accept as valid findings that agree with their theories and, simultaneously, to reject as invalid findings that disagree with their expectations. Confirmation bias is easily condemnable as a form of myopia or blindness. Notwithstanding that suggestion, confirmation bias has also been promoted as a beneficial cognitive strategy, as in these statements by Karl Popper and Thomas Kuhn:

> The dogmatic attitude of sticking to a theory as long as possible is of considerable significance. Without it we could never find out what is in a theory—we should give the theory up before we had a real opportunity of finding out its strength; and in consequence no theory would ever be able to play its role of bringing order into the world, of preparing us for future events, of drawing our attention to events we should otherwise never observe. (Popper, 1963, p. 312)

> By ensuring that the paradigm will not be too easily surrendered[,] resistance [i.e., confirmation bias] guarantees that scientists will not be lightly distracted and that the anomalies that lead to paradigm change will penetrate existing knowledge to the core. (Kuhn, 1970, p. 65)

But even Popper and Kuhn could not improve on the earlier views of geologist T. C. Chamberlin, who substantially predated both modern philosophy of science and modern social psychological conceptions of ego-involvement and self-enhancement bias with this statement from 1897:

> Important as the intellectual affections are as stimuli and as rewards, they are nevertheless dangerous factors in research. . . . The moment one has offered an original explanation for a phenomenon which seems satisfactory, that moment affection for his intellectual child springs into existence; and as the explanation grows into a definite theory, his parental affections cluster about his

> offspring and it grows more and more dear to him. . . . So soon as this parental affection takes possession of the mind, there is apt to be a rapid passage to the unreserved adoption of the theory. . . . The mind lingers with pleasure upon the facts that fall happily into the embrace of the theory, and feels a natural coldness toward those that assume a refractory attitude. . . . There springs up also unwittingly a pressing of the theory to make it fit the facts and a pressing of the facts to make them fit the theory. . . . The search for facts, the observation of phenomena, and their interpretation are all dominated by affection for the favored theory until it appears to its author or its advocate to have been overwhelmingly established. (Chamberlin, 1897, pp. 358–359)

To deal with this problem of "parental affection," Chamberlin proposed a method of multiple working hypotheses:

> [By bringing] into view every rational explanation of the phenomenon . . . [t]he investigator thus becomes the parent of a family of hypotheses; . . . the right use of the method requires the impartial adoption of all alike into the working family. The investigator [can then proceed] with a certain natural and enforced erectness of mental attitude to the inquiry. (p. 360)

Although Chamberlin recognized affective influences on scientific cognition, he apparently assumed that they could be overcome by force of will or, as he put it, "neutralized":

> The investigator thus at the outset puts himself in cordial sympathy and in parental relations (of adoption, if not of authorship) with every hypothesis that is at all applicable to the case under investigation. Having thus neutralized, so far as may be, the partialities of his emotional nature . . . .(p. 360)

Chamberlin's suggestion was imaginative, but it seems obvious that he never tried to implement it. Had he tried, he might have discovered that it did not work. His multiple-working-hypotheses method might be effective if researchers could indeed regard a competitor's theory with something approaching their affection for a beloved adoptee. But the lesson of histories such as those condensed in Table 1 is that researcher–theorists engaged in controversies typically treat a competitor's favored theory more like an obstreperous stepchild.

Method–Theory Synergy: Evidence From Nobel Prizes

To this point, it has been difficult to produce evidence to support the idea that theory competition—such as Platt's strong inference or Chamberlin's method of multiple working hypotheses—offers an efficient route to scientific progress. At the time of this article's preparation, the average age of the theory competitions in Table 1 was 44 years. This means that, on average, they have been unresolved for durations that approximate or exceed the expectable length of a productive scientist's research career.[3]

Table 1 suggests that strong inference is not working effectively in psychology. But it is unsatisfying to rely on Table 1. Perhaps the unresolved controversies in Table 1 indicate a problem more in how cognitive and social psychologists manage theory competitions than with the strategy of theory competition itself. Perhaps other subdisciplines of psychology or other sciences come closer to using empirical confrontation in the effective fashion envisaged by Platt or Chamberlin.

Unfortunately, it was beyond the author's expertise to analyze theory competitions for psychology subdisciplines other than cognitive and social psychology, let alone other scientific disciplines. But it was possible to find some useful data from other scientific disciplines. To learn about the role of theory in other sciences, the author consulted a site that provides detailed information about Nobel Prize–winning contributions. It was relatively easy to learn details of the contributions that have been so highly valued as to have been recognized in the form of Nobel Prizes in physics, chemistry, and medicine.[4]

Awards for method and theory

At the Nobel Website, each Nobel Prize is described by a one-sentence award citation, accompanied by substantial elaboration in a press release. Examination of the citations and press releases made it apparent that Nobel science awards could readily be sorted into the two classes of awards for (a) developments of theory and (b) method-based contributions.

For contributions to theory, the citation almost invariably included the word "theory." Contributions to method were usually recognizable by inclusion of one or more of the words "method," "studies," or "invention." Appearance of "discovery" in citations was equivocal. Most "discovery" citations were for methods that permitted previously impossible observations, but in a minority of these, "discovery" indicated a theoretical contribution. Method contributions were further classifiable as being for new methods created by awardees (71%) or for contributions that derived from ingenious use of previously developed methods (29%). This distinction among awards for methods will be considered further, below, in the discussion of method–theory synergy.

All 77 of the physics, chemistry, and medicine awards for 1991 through 2011 were classified (by the author) as awards for either method or theory, based on information in the award citations and in the accompanying press releases.[5] Only two awards were difficult to code. For one of these, the contribution was described as including both theory and method, and for that reason, it was coded half for each. The other difficult one could not be assigned confidently to either category, so there was no better solution than to code it as half for each.

As shown in Table 2, 82% of the contributions for the 21-year period were for method, and 18% were for theory.

Physics had the highest percentage for theory, but it was still a minority—29%. Chemistry had 85% for method, 15% for theory. Medicine had 91% for method, 9% for theory. This severe imbalance in favor of method contributions was not expected.[6]

Relations of theory to method evident in Nobel Prize awards

Even in physics, a discipline that is stereotyped as prizing theory above all else, Nobel Prizes were given almost three times as often for method contributions as for theory contributions. Why? A hypothesis that sustains the preeminence of theory is that important contributions to theory are so difficult in physics that important contributions to method will occur much more frequently. Alternatively, it may be that lucrative patents and royalties offer incentives to physicists to focus greatest effort on technical method contributions. Because of these alternative possible explanations, the author sought further understanding of the relation of method to theory in the Nobel awards by examining more closely the mentions of theory that appeared in descriptions of prizes that were coded as having been awarded for methods. These mentions of theory were of two types, both of which appeared frequently.

Existing theories played important roles in developing awarded methods. Some of the press releases for the Nobel awards explained how awardees used existing theories to design the methods for which their awards were given. One of the earliest Nobel Prizes in physics was awarded to Albert Michelson in 1907 "for his optical precision instruments and the spectroscopic and metrological investigations carried out with their aid." One of the purposes for which Michelson had designed these instruments was to test the theorized existence of a medium (the ether) that was assumed to propagate the vibrations of light rays. Michelson and Morley's (1887) famous experiment was conducted in expectation that existence of the ether would be confirmed by showing that measurements of the speed of light would vary with the direction of the light's travel, thereby establishing movement of the earth relative to the ether. To the contrary, however, their findings obliged them to conclude, apparently disappointedly, that "It appears . . . reasonably certain that if there be any relative motion between the earth and the luminiferous ether, it must be small." The use of the words "theory" or "theoretical" five times in the opening pages of their 1887 article made clear that theory played an important role in guiding the design of their apparatus.

Table 2. Categorization of Nobel Science Awards, 1991–2011

	Medicine	Chemistry	Physics	Total
Theory	2	4	8	14
Method	21	22	20	63

Note. Entries in this table summarize the author's categorizations based on award descriptions available at the Nobel Foundation's Website, http://nobelprize.org. A spreadsheet containing the information used to make these judgments, together with links to the pages at which one can find detailed information for each award, is available in this article's online supplement at http://pps.sagepub.com/supplemental-data.

The Nobel Prizes for Physics in 1992, 1994, 1995, and 2002 were awarded for designs of apparatus and methods to detect subatomic particles whose existence had been theorized but never empirically observed. For these four awards, existing theory played roles both in designing the particle detectors and in guiding statistical analyses of collision-generated images, allowing conclusions that the theorized particles had indeed been observed.

Awarded methods produced unanticipated observations that suggested new theory. The Nobel award press releases frequently explained how, after initial publication of an award-receiving method, results produced by the new method led to previously inconceivable theory. Some examples of the method-generates-data-inspires-theory sequence are found in these quotations from Nobel Prize press releases:

> [D]ue to his work particle physicists have been able to focus their interest on very rare particle interactions, which often reveal the secrets of the inner parts of matter. (1992 physics award to Georges Charpak, "for his invention and development of particle detectors, in particular the multiwire proportional chamber")

> [D]iscovery of the tau was the first sign that a third "family" of fundamental building blocks existed. (1995 physics award to Martin L. Perl, "for the discovery of the tau lepton")

> Kornberg's . . . crystallographic pictures . . . are so detailed that separate atoms can be distinguished and this makes it possible to understand the mechanisms of transcription and how it is regulated. (2006 chemistry award to Roger D. Kornberg, "for his studies of the molecular basis of eukaryotic transcription")

> The two German cell physiologists . . . have together developed a technique that allows the registration of . . . incredibly small electrical current (amounting to a picoampere—10^{-12}A) that passes through a single ion channel. . . . [T]his new analytical tool has during the past ten years revolutionized modern biology. (1991 medicine award to Erwin Neher and Bert Sakmann, "for their discoveries concerning 'reversible protein phosphorylation as a biological regulatory mechanism'")

Impact of empirical discoveries achieved with existing methods. A reviewer of this article suggested the possible usefulness of distinguishing between awards for method-based discoveries that were produced with newly created methods and those achieved with previously existing methods. To make this judgment, the author classified a method as "previously existing" if the press release either explicitly stated its

earlier origin or if the press release contained no statement indicating that the awardee had created the method.

Awards achieved with existing methods were relatively infrequent in physics (3 of 20 method awards) and chemistry (4 of 22) but were noticeably more frequent in medicine (11 1/3 of 21; the fraction is because just one of three co-recipients of the 2000 award used existing methods). Awards made for research using existing methods sometimes involved the awardee pushing an existing method well beyond the limits of its previous uses. For example, the 1997 medicine award to Stanley Prusiner ("for his discovery of prions") was for a long series of studies using methods previously used to isolate genes and demonstrate their functions. Awards classified as based on work with existing methods often included existing methods that received a previous or subsequent Nobel Prize. For example, Prusiner's discovery of prions used "knockout" gene technology that received a Nobel Prize 10 years later, in 2007.

In summary, Nobel science awards of the last 21 years have recognized contributions to method considerably more often than they have contributions to theory. For medicine, chemistry, and physics, the imbalance was 63 awards for method to 14 for theory. Nonsystematic perusal of earlier awards indicated that the recent method-favoring imbalance likely characterizes the entire history of Nobel Prizes in the sciences. For 1901, the first year the Nobel was awarded, two of the three science awards were for method (Wilhelm Conrad Röntgen in physics for methods producing X-rays and Emil von Behring in medicine for developing an effective serum therapy treatment for diphtheria). The 1901 award in chemistry was for theory—to Jacobus H. van 't Hoff for "discovery of the laws of chemical dynamics and osmotic pressure in solutions."

Nobel Prizes to Psychologists

Since World War II, nine Nobel Prizes, seven in medicine and two in economics, have been given for work done by psychological scientists (see Table 3). Among these, the seven awards in medicine were all for method-based contributions. The 1978 economics award to Herbert Simon was for theoretical work on decision making in organizations. The 2002 economics award to Daniel Kahneman was partly for method ("Kahneman's main findings concern decision-making under uncertainty, where he has demonstrated how human decisions may systematically depart from those predicted by standard economic theory") and partly for theory ("Together with Amos Tversky [deceased in 1996], he has formulated prospect theory as an alternative, that better accounts for observed behavior"). The preponderance of awards for method among those given to psychologists indicates that the Nobel Foundation's preference for method relative to theory does not treat psychological scientists differently from scientists in other disciplines.

Method–Theory Synergy in Psychology

Psychology offers many examples of synergy between method and theory. To keep this section brief, the author makes no attempt here to be more than illustrative. Also, the illustrations

Table 3. Nobel Prizes for Psychology-Related Contributions Since World War II

Year	Prize	Awardee(s)	Citation
1949	Medicine	Walter Rudolf Hess	For his discovery of the functional organization of the interbrain as a coordinator of the activities of the internal organs
1961	Medicine	Georg von Békésy	For his discoveries of the physical mechanism of stimulation within the cochlea
1967	Medicine	Ragnar Granit, Haldan K. Hartline, George Wald	For their discoveries concerning the primary physiological and chemical visual processes in the eye
1973	Medicine	Karl von Frisch, Konrad Lorenz, Nikolaas Tinbergen	For their discoveries concerning organization and elicitation of individual and social behavior patterns
1978	Economics	Herbert A. Simon	For his pioneering research into the decision-making process within economic organizations
1981	Medicine	Roger W. Sperry	For his discoveries concerning the functional specialization of the cerebral hemispheres
1981	Medicine	David H. Hubel, Torsten N. Wiesel	For their discoveries concerning information processing in the visual system
2000	Medicine	Avid Carlsson, Paul Greengard, Eric Kandel	For their discoveries concerning signal transduction in the nervous system
2002	Economics	Daniel Kahneman	For having integrated insights from psychological research into economic science, especially concerning human judgment and decision-making under uncertainty

Note. All information in this table was obtained from http://nobelprize.org. All of the citations in the Citation column are quotations from that site.

given here were deliberately oversampled from the works of psychologists whose theories appear in Table 1.

Using theory to develop new methods

A cognitive psychology example of using theory to develop new methods is Sternberg's (1969) development of the additive factors method. Another is Jacoby's (1991) use of theory in developing the process dissociation method. In social psychology, Festinger and Carlsmith (1959) used Festinger's (1957) cognitive dissonance theory to create their counterattitudinal role-playing method. Murphy and Zajonc (1993) used Zajonc's (1980) theory of affective primacy to develop their affective priming method.

Using method-produced results to develop new theories

An example of theory prompted by findings resulting from innovative method is prospect theory (Kahneman & Tversky, 1979), which developed in part from Kahneman and Tversky's method of generating thought experiments (as described by Kahneman, 2007). Another is the spreading-activation theoretical model based on Meyer and Schvaneveldt's (1971) priming method. Their demonstration of the priming method's value also spawned many variants, one of which was Murphy and Zajonc's (1993) affective priming method. In social psychology, theoretical principles resulting from data produced by new methods include (a) norm formation, resulting from Sherif's (1936) autokinetic-effect method; (b) conformity, resulting from Asch's (1951) unanimous-incorrect-majority method; (c) obedience to authority, resulting from Milgram's (1963) teacher–learner method; (d) diffusion of responsibility, resulting from Darley and Latané's (1968) bystander intervention method; (e) identity formation, resulting from Tajfel's minimal group method (Tajfel, Billig, Bundy, & Flament, 1971); (f) aversive racism, resulting in part from Gaertner and Bickman's (1971) wrong-number method; and (g) the elaboration likelihood theory (Cacioppo, Petty, Kao, & Rodriguez, 1986), based in part on data obtained with Brock's (1967) listed-thought method. As can be seen in Table 1, some of these methods spawned multiple theories that proceeded to compete for many years.

Can the Controversies Be Resolved?

With all but one of Table 1's 13 controversies still unresolved after durations averaging more than 40 years, it is tempting to conclude that researchers' predilection to defend theories is retarding scientific advance. But it seemed premature to reach that conclusion without examining the most recent publications on the 13 controversies. That effort was rewarded. Recent publications confirmed that most of the controversies were still active. The reward was in discovering that for three of the controversies, recent publications showed some prospects for resolution.

Representations of categories: (Brain) pictures worth many words

On the topic of mental categorization, Smith and Grossman (2008) recently reviewed research that combined behavioral measures with brain imaging data, including studies of both neurological patients and typical control samples. While observing that single-system explanations of individual studies remained viable, Smith and Grossman also described the findings they reviewed as supporting an interpretation in terms of "multiple systems," which corresponded to rule-based and similarity-based categorization processes.

Past skepticism about brain imaging methods may on occasion have implied that finding locations of brain functions via imaging methods does little to advance theoretical understanding of cognitive processes. That skeptical view has been effectively laid to rest by works such as those by Kosslyn and colleagues (Ganis, Keenan, Kosslyn, & Pascual-Leone, 2000; Kosslyn, Digirolamo, Thompson, & Alpert, 1998) and by Smith and Grossman (2008). Those works show how findings of associations of brain regions with cognitive processes can effectively address theoretical issues. For mental rotation, the resolution was in favor of mediation by visual imagery rather than by propositional representations. For categorization, a possible resolution suggested by Smith and Grossman came from identifying empirically defined boundaries between domains in which rule-based and similarity-based categorization processes operate.[7]

Memory search: Mathematics used to draw boundaries

Sternberg (1966) reported a striking experimental observation of increasing linear functions that related reaction time (to recognize a digit as one of up to six that had just been studied) to the number of digits that had been studied. Sternberg interpreted the linear increase as indicating that items held in memory were being retrieved and checked one at a time—in series. However, an explanation of the linear relationship in terms of parallel memory search was soon advanced. The ensuing (initially) intense competition between the serial and parallel search theories may now have achieved a resolution as the result of work by Townsend and colleagues (e.g., Townsend, 1990; Townsend & Ashby, 1983; Townsend & Fifić, 2004), who developed a method of empirically distinguishing parallel from serial search processes. Townsend and Fifić's method estimates parameters that contribute to a "survivor interaction contrast function." Their article used the relation of the survivor-interaction contrast function to time since stimulus presentation and to reveal "strong evidence for pure serial or pure parallel processing, with some striking apparent differences across individuals and interstimulus [interval] conditions" (Townsend & Fifić, 2004, p. 953).

A good reason for thinking that Townsend's method has provided a resolution is the nonappearance, since publication of the article by Townsend and Fifić (2004), of any opposition

to their conclusions. Similarly to the controversy concerning representations underlying categorization, this (possible) resolution was achieved through use of a nonbehavioral method in conjunction with behavioral data. In this case, the nonbehavioral method was mathematical analysis.

Counterattitudinal role-playing: Resolution by translation

This controversy began with Festinger and Carlsmith's (1959) confirmation of a surprising prediction from cognitive dissonance theory (Festinger, 1957). Festinger and Carlsmith had offered $1 to some of their subjects for, in effect, lying. The lie was to describe, to a confederate posing as the experiment's next subject, a very boring task that they had just completed by saying that the task was very interesting. Festinger had predicted that cognitive dissonance produced by the paltry justification for this lie would be resolved by the "counterattitudinal role player" (i.e., the lying subject) coming to judge the boring task as actually interesting. This prediction was confirmed. Subjects who had been paid $1 later described the dull task as more interesting than did subjects who went through the same procedure with the much more generous incentive of $20, which presumably produced less dissonance.

After several years of controversy about whether this counterattitudinal role-playing result was reproducible (it was), alternative theoretical interpretations began to appear. The first was Bem's (1965) self-perception theory. Recent accounts of the history of this theoretical controversy have appeared in reviews by Olson and Stone (2005) and by Harmon-Jones, Amodio, and Harmon-Jones (2009). These two reviews make clear that the controversy has not disappeared—several competing theories remain viable. Nevertheless, it is remarkable that there now appears to be no trace of the contentiousness that surrounded this controversy at its peak in the 1960s and 1970s. Rather, there appears to be a truce—one in which advocates of each contending theory appear comfortable in claiming to command the entire field, while doing so without disparaging the field's co-occupants.[8]

The phrase "cognitive dissonance" was established as a staple in intelligent lay discourse perhaps 30 years ago. Educated nonpsychologists therefore understand "cognitive dissonance" as a label for rationalizations offered to explain otherwise surprising behavior. For social psychologists, cognitive dissonance remains a technical term that identifies both (a) a collection of well-established empirical findings that were regarded as counterintuitive when they first appeared in the 1960s and (b) the set of theories that still compete to explain these findings. The relationship among these competing theories is like that among speakers of different languages who observe the same events while using very different words to describe them—and do so with no apparent disagreement about what they are describing. The juxtaposed statements of the multiple theories amount to the tablets of a Rosetta Stone that could be used to construct intertranslations among the theories.[9]

Conclusion

This article urges recognition of the value of method in advancing theory. The case for this conclusion started with the observation that the research strategy of "strong inference" (Platt, 1964), which called for empirical confrontations between contending theories, appears to have failed dismally in psychology. This failure was documented with a summary of a history of long-unresolved theory controversies in cognitive and social psychology (see Table 1), each of which involved many back-and-forth empirical confrontations of the strong-inference variety.

An analysis of the recent history of Nobel Prizes in science unexpectedly revealed that these awards were given much more often for creation of methods and for method-based discoveries than for developments of new theory. Nobel awards of the past two decades in physics, chemistry, and medicine repeatedly demonstrated two interesting forms of method–theory synergy. One was that existing theories often provided the basis for design of awarded methods. A more dramatic synergy was evident in Nobel Foundation press release descriptions of how awarded methods had served to generate previously inconceivable research findings, which in turn led to previously inconceivable theories.

Recent histories of three of Table 1's long-unresolved theory controversies provided further support for the value of method. Brain-imaging methods were used to decisively resolve one theory competition that would otherwise not have achieved resolution. Two other controversies now appear to be approaching method-facilitated resolutions by locating boundaries between empirical domains of competing theories. One more controversy is perhaps close to resolution in the form of finding intertranslations among the conceptual languages of theories that appear not to make different empirical predictions.

What about the as-yet-unresolved controversies? Perhaps researchers who were active during the peaks of at least some of those controversies might consider collaborating on reports to describe the current state of their controversy. These collaborations might start by identifying empirical results that are accepted by all parties to the controversy. This could provide a path to discovering resolutions of the boundary-drawing or intertranslation variety. Such collaborative controversy resolutions might prove very welcome to historically opposed competitors who still seek resolution. They might also serve as models that could accelerate the resolution of younger controversies.

Editor's Note: Comments on this article (especially regarding other long-unresolved theoretical debates or the status of the debates mentioned here) are welcome in the online comment system. Go to http://pps.sagepub.com/content/7/2.toc, *click on the text for the article, then click on "submit a comment" under "Reader Responses."*

Acknowledgments

The author is grateful to numerous colleagues (too many to list individually) who have commented on this work in response to various

oral presentations in the past decade and have provided perspectives on the various theoretical controversies mentioned in this article.

Declaration of Conflicting Interests

The author declares that he had no conflicts of interest with respect to his authorship or the publication of this article.

Funding

This research was supported by National Institute of Mental Health Grants MH-01533, MH-41328, and MH-57672 and by the Implicit Cognition Research Fund at the University of Washington.

Notes

1. The author has much soft data to support this observation. Numerous times, the prime reason communicated for editorial rejection, either of his submissions or of others that he had favorably reviewed, was that they "did not advance theory." The author's most highly cited article, for which he readily adds one citation here (Greenwald, McGhee, & Schwartz, 1998), was recommended for rejection by a reviewer who pointed out (correctly) that it "did not advance theory."
2. Citations of these recent publications bearing on Table 1's controversies appear in this article's online supplement at http://pps.sagepub.com/supplemental-data.
3. The one debate in Table 1 that can be treated as resolved is about cognitive representations used to perform mental rotations. Studies using positron emission tomography scans (Kosslyn, Digirolamo, Thompson, & Alpert, 1998) and transcranial magnetic stimulation (Ganis, Keenan, Kosslyn, & Pascual-Leone, 2000) have yielded findings that clearly favor the theory that this skill depends on visual representations.
4. The medicine award is fully identified as an award for physiology or medicine. The Nobel Foundation's site is http://nobelprize.org/nobel_prizes. The present analysis focuses on empirical sciences and therefore does not include economics, for which the majority of awards are for contributions to theory.
5. The explanation for there being 77 awards in three disciplines over a 21-year period is that on 14 occasions, two distinct awards—for different contributions—were given in a single discipline in the same year. This happened seven times in physics, five times in chemistry, and twice in medicine. This is different from having multiple awardees named for one award. The total number of awardees over the 21 years in the three disciplines is 144.
6. The basis for the coding summarized in Table 2 is available in a spreadsheet that contains (a) all of the (one-sentence) award citations, (b) relevant quotes extracted from the press releases, and (c) active links to the complete press releases (which are at http://nobelprize.org/nobel_prizes). The spreadsheet is included in this article's online supplement at http://pps.sagepub.com/supplemental-data.
7. The difficulty of achieving resolution of competing theories of category representation is suggested by a reviewer's reaction to a draft of this article. This is a quote from the review: "If the Smith and Grossman (2008) paper really ends the debate on category representation (both how many systems there are and how they are parsed/defined), I will eat my hat. Swear to God."
8. A partial qualification of this assertion is needed. Most of the contending theories posit a motivational process (Festinger called it "dissonance arousal"), and the evidence has largely supported the conclusion that dissonance manipulations such as the one in the Festinger and Carlsmith (1959) experiment produce a motivational state that can be characterized as arousal. Bem's (1965) self-perception theory was the one approach that eschewed any such motivational assumption, as a consequence of which it can be considered less successful than the other competitors in explaining the entire empirical corpus of cognitive dissonance phenomena.
9. Such a resolution by translation was described for the mental rotation debate by Anderson (1978), 20 years prior to the brain-imaging resolution by Kosslyn and colleagues.

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