

That “Vision Thing”: The State of Theory in Social and Personality Psychology at the Edge of the New Millennium

Arie W. Kruglanski
University of Maryland

Social psychology's status as a theoretical discipline is assessed. Whereas it has excelled as an experimental science, the field has generally eschewed broad theorizing and tended to limit its conceptualizations to relatively narrow, “mid-range” notions closely linked to the operational level of analysis. Such “theory shyness” may have spawned several negative consequences, including the tendency to invent new names for old concepts, fragmentation of the field, and isolation from the general cultural dialogue. Recently, steps have been taken to encourage greater theoretical activity by social psychologists, and there are now several major outlets for theoretical contributions. Further initiatives are needed, however, to instigate theoretical creativity, including ways of overcoming disciplinary risk aversion and the training of young social psychologists in ways and means of theory construction.

As we take our first steps into the new millenium, it seems appropriate to engage in some stock taking and ask how well we are doing as a discipline. What I would like to ask is how well we are doing as a theoretical discipline. How effective are our theoretical skills? How much attention and effort do we devote to theorizing? How seriously is the world taking our theories? How seriously are we taking them ourselves? Do we need to improve in that regard? If so, how?

Theory construction represents one of the two great Lewinian legacies for our field, and we all recall his famous phrase that “there is nothing as practical as a good theory.” The other legacy, of course, was experimentation, and there seems to be little doubt that we have done extremely well in that domain. It is no mere accident that our most prestigious society has the word *experiment* in its name. Since pioneers like Kurt Lewin, Muzafer Sherif, or Solomon Asch first amazed the world by experimenting on topics such as leadership, social norms, or conformity, the experimental business in social psychology has thrived incredibly. The “social psychology experiment” has become a kind of “cultural icon,” adored by some, vilified by others, but clearly a lynchpin of our science that we have developed into an art form and into which we have poured our best creative energies.

How well are we doing as a theoretical discipline? That is a good question. On the one hand, we insist that our major publications make a substantial conceptual contribution; this suggests that we hold theory in high regard. But other indications tell a different story. Consider the following. Of the many hundreds of textbooks, edited books, research monographs, and so forth published in social psychology over the last several decades, I am aware of only three books devoted to social psychological theories. These are the Deutsch and Krauss volume published in 1965, the Shaw and Costanzo volume published in 1970, and the West and Wicklund

volume published in 1980. A received wisdom in social textbook writing has been to go light on theory and heavy on the empirical stuff, in particular that of the “cute” variety. An important textbook featuring the theoretical approach—the Jones and Gerard volume published in 1967—was not a great hit with classroom instructors, who complained of it being too conceptual and abstract.

In 1971, the *Journal for the Theory of Social Behavior* was launched, devoted to theoretical analyses of social phenomena by social psychologists, sociologists, social philosophers, and others of their ilk. Another similar journal, *Theory and Psychology*, followed suit, connected to the International Society for Theoretical Psychology, which conducts well-attended biannual meetings all over the globe. Experimental social psychologists like ourselves have been conspicuous in their absence from these and similar activities.

We are notoriously ill at ease about generalizing beyond our research findings, and we get quite anxious when invited to speculate beyond our data. Nor do we feel very comfortable about tracking and evaluating theoretical arguments. Emblematic of this attitude is the renowned “Princeton rule” for a successful job talk in social psychology. The idea is to get to the data within the first 10 min of the presentation, else all is lost. A similar sentiment is implicit in the wonderful handbook chapter by Aronson and Carlsmith (1968), who wrote that “where the ideas come from is not terribly important . . . the important and difficult feat involves translating a conceptual notion into a tight, workable, credible, meaningful set of experimental operations” (p. 37). Eliot Smith, the current editor of *Personality and Social Psychology Review*, commented in a recent conversation that “as a field we are more phenomenon and data driven and less theory driven” (personal communication, 1999). Many will agree with this assessment.

Reasons for Our Theoretical Aversions

Historically, we have been rather averse to “heavy duty” theorizing, and we have taken much more to Kurt Lewin's injunction about experimentation than to his plea for theorizing, thus discriminating against one of his intellectual offspring in favor of the

Correspondence concerning this article should be addressed to Arie W. Kruglanski, Department of Psychology, University of Maryland, College Park, Maryland 20742-4411. Electronic mail may be sent to arie@psyc.umd.edu.

other. The question is why? There could be several reasons. One is the disenchantment in scientific psychology with the grand theoretical systems of the 1930s and the 1940s, including Freud's psychoanalytic theory, the Hull-Spence theory of behavior, and Lewin's own field theory. It was a similar disenchantment with general systems in sociology (Parsons' in particular) that drove Robert Merton to advocate in 1957 a shift to "mid-range" theorizing, an idea that found natural resonance among social psychologists. Writing in 1965, Deutsch and Krauss (1965) commented that "social psychological theorizing is moving in the direction of developing 'theories of the middle range'" (p. 5), referring to theories by Lewin's students, such as Leon Festinger, Morton Deutsch, Dorwin Cartwright and Jack French, or John Thibaut and Harold Kelley, who, according to Deutsch and Krauss (1965), "unlike Lewin . . . have not been theorists in the grand manner" (p. 62). Everything is relative, however, and it seems that they were grand enough compared with much contemporary work. To borrow a metaphor from the world of fashion, if post-Lewinian theories were "middies," many subsequent formulations were more like "minis"; middi and mini theoretical skirts, that is.

The Case Against High-Level Theorizing

Be that sartorial detail however it may, Merton's (1957) major argument for mid-range theories was that abstract or general formulations are notoriously difficult to verify. By contrast, as he put it, "middle range theories are close enough to observed data to be incorporated in propositions that permit empirical testing" (p. 39). Thus, we had better stick with smaller, mid-range theories and proceed to build on them before we deign to advance to loftier theoretical plateaus. A kindred idea was that the social sciences are not quite ready for sweeping theories, and we had better first establish a secure database from which to theorize. In Merton's terms, it is a mistake to think that "systems of thought can be effectively developed before a great mass of basic observations has been accumulated" (p. 46). And, in his opinion, "we are not ready Not enough preparatory work has been done" (p. 45).

Was Merton (1957) Right?

But how compelling are Merton's (1957) arguments for low-level theorizing (whether of the "middi" or "mini" variety)? Take the assertion that we need first a firm database before we theorize. The history of Western science seems to belie that proposition. Some of the most impactful theories in the history of physics, for example, were advanced well before a substantial body of relevant empirical data was even collected. There was no telescopic evidence to bolster Copernicus's heliocentric theory, and Galileo had only partial evidence. The more decisive evidence required powerful telescopes; these were available only in the 19th century, that is, a full 2 centuries later. Thus, Copernicanism won out in the 17th century because of parsimony rather than because of decisive evidence. Einstein's special theory of relativity, proposed in 1905, had a major impact on physics because of its harmony and elegance not only before there was any evidence to support it but despite experimental evidence to the contrary. The great public success of Einstein's general theory of relativity, published in 1915, did not occur until Eddington confirmed one of its major predictions (the gravitational bending of light) in 1918. Another

major prediction of the theory (the gravitational redshift of spectral lines) was not confirmed till 1960. Yet the sheer formulation of the theory 45 years earlier was already hailed as a great achievement on the basis of its compelling coherence and aesthetic beauty. Closer to home, Festinger's (1954) theory of social comparison processes was buttressed by very little evidence when it was first presented, as was the theory of cognitive dissonance, or Schachter and Singer's (1962) two-factor theory of emotion, yet the importance and subsequent impact of these formulations may not be doubted.

In fact, requiring extensive data before a theory is published may drain it of its heuristic potential and its ability to stimulate further research. As Serge Moscovici recently observed, with only the slightest grin, "too many facts wear out the Truth" (personal communication, 1998). From that perspective, we need to develop criteria for evaluating a theory beyond the sheer amount of empirical verification it has already received.

The thesis that general or abstract theories are more difficult to test than are more concrete or middle range theories also is problematic. One should not confuse abstractness with vagueness. Vague theories are difficult to operationalize and to test, not abstract theories. Take a theory about mammals, which is more abstract, in some sense, than a theory about dogs. Is it more difficult to test a theory about mammals than about dogs? Not really, because what constitutes a *mammal* may be quite clear and explicit, no less so than what constitutes a *dog*.

For Popper (1959) and other philosophers of science, the generality or abstractness of a theory is a plus, not a minus, because the more general a theory is, the greater its empirical content and, hence, the more testable and falsifiable it is. According to Popper, it takes some guts (and Popper was a great advocate of "guts" in science) to propose a highly general theory, precisely because there are so many opportunities to falsify it. There is nothing scientifically or philosophically wrong with sweeping or abstract theorizing. The issue is psychological rather than philosophical: Does one have the guts, is one prepared to take the risk?

Risk Aversion and Prevention Focus in Social and Personality Psychology

It is precisely in the departments of boldness, audacity, and the readiness to take risks that we as a field might be lacking. Indeed, several commentators have noted that we tend to be highly risk averse and rather circumspect about what we say or do. Harry Reis and Jerome Stiller (1992), in a recent paper, discussed such risk aversion and tied it to the "crisis" of the 1970s and the allegation that social and personality psychology is not a true science; this may have increased our disciplinary motivation to become ever more exacting and tougher in our evaluation of research. Tory Higgins (1992) concurred, adding that this may have introduced a "prevention" focus as our essential *modus operandi*. As he put it, "to avoid the perception of mistakes, it is best to work within traditional boundaries, use conventional paradims and interpret results in accordance with established theories" (p. 491). Needless to say, such risk aversion or prevention focus is inimical to bold theorizing.

Negative Consequences of Our Theory Shyness

Inventing New (or Distinct) Names for Old (or the Same) Concepts

However, risk aversion and circumspection do not come without a price. In fact, they may prevent us from doing good science, if by that it is meant the discovery of underlying principles and mechanisms behind seemingly diverse phenomena. Our tendency to “myopically focus on the particulars,” as Barry Schlenker (1974, p. 8) put it, may have led us to rediscover the wheel or, as Yogi Berra put it, to have that “*deja vu* feeling all over again.” Norman Miller and his colleague argued recently that such an “invention of new names for old concepts” is “first on the list of impediments to scientific progress in contemporary social psychology” (Miller & Pedersen, 1999, p. 150). As a consequence of these common practices, they assert, “contemporary social psychology is rife with implicit, but unsubstantiated claims of discriminative construct validity” (p. 150).

Fragmentation of the Field

If “rediscovery of the wheel” refers to a failure to notice commonalities across time, “fragmentation” refers to a failure to notice commonalities across domains. Vallacher and Nowak (1997) decried the fact that social psychology is an “undeniably fragmented discipline” (p. 95) in which “a distinct set of factors often large in number, tends to be invoked to explain different phenomena, fostering a highly differentiated conceptual landscape for the field as a whole” (p. 74).

Declining Interest Value of Our Articles

Another unintended consequence of our reluctance to theorize and go beyond our phenomena might be our tendency to beat them, if not entirely to death, at least to the point where they are no longer interesting. In a survey of the *Journal of Personality and Social Psychology* readers conducted by Kay Deaux in 1988, the most frequently listed complaint was that “journal articles just aren’t interesting anymore.” Tory Higgins (1992) tied this, again, to our risk aversion and prevention focus. In his words, “it would not be surprising if articles ‘playing it safe’ were less interesting than articles ‘shooting for the stars’” (p. 491).

Isolation From the General Cultural Dialogue

This brings me to the last, though not the least important, cost of our reluctance to theorize and go beyond the information given: our relatively modest voice in the general intellectual debate about societal issues. The nonspecialists do not care much about the elegance of our designs and the sophistication of our procedures. They want to know what it all means and whether our take-home message is novel and exciting. As Donal Carlston (personal communication, 1999) recently observed, they are interested in our theory much more than in our data. Indeed, one would hope that our theories about the interface of the individual and society would have great impact on the national and international agenda in regard to major topics facing the world today. They should be at the forefront of discussions about education, foreign policy, world economics, peace negotiations, and so on. It is my impression,

however, that we have not yet lived to our potential in this regard and that other social science disciplines (e.g., economics, political science, social philosophy, sociology) have been more of a presence in the public sphere than we have been. In John Levine’s (personal communication, 1999) view, this may be due to the fear of ostracism from our in-group, which may discourage us from communicating with the “out-group,” the other social sciences and the general public, in so far as such communication may require us to go beyond our data.

Kenneth Gergen (1994a, 1994b) has commented repeatedly on this point over the last decades, lamenting the fact that our message is not heard much outside the boundaries of our own discipline and is available almost exclusively to the specialists. Social psychology, according to Gergen, could play a “coloratura role” in the exciting cultural dialogue “about values, policies, and goals” (Gergen, 1994b, p. 133). It should have “the capacity to challenge the guiding assumptions of the culture, to raise fundamental questions regarding contemporary social life, to foster reconsideration of that which is ‘taken for granted,’ and thereby to generate fresh alternatives for social action” (Gergen, 1994a, p. 109). But it has not done so, at least not thus far.

I had a personal opportunity to ponder this issue last year at Stanford when I attended a lecture by Jacques Derrida, the father of deconstructionism. It was a real happening. The entire campus was abuzz. Thousands of eager, bright undergraduates, graduate students, and faculty virtually fought for seats in Stanford’s largest auditorium. The same would probably have been true if, instead of Derrida, the speaker had been Habermas, Foucault, Bourdieu, Putnam, Milton Friedman, or Fukuyama. And I could not help wondering whether any of the great social psychologists whose work has charted the course of our discipline would have been accorded a similarly enthusiastic reception and, if not, why not.

The Shifting Publication Scene in Social and Personality Psychology

The concerns I have been describing are not exactly new, and they are not exclusively mine. Indeed, I have been citing some of our most distinguished colleagues, who have been expressing them for years. Why don’t we, therefore, do something about it? The truth of the matter is that, indeed, we do. Several years ago, Mark Zanna worried that social and personality psychologists have too little say in journals like *Psychological Review* or *Psychological Bulletin* and that maybe this accounts for the paucity of our theoretical contributions. Zanna’s vision bore a productive fruit: establishment of *Personality and Social Psychology Review*, now in its 3rd year of operation. In parallel, social and personality psychologists have been increasingly involved in *Psychological Review* and *Psychological Bulletin*. For the last 10 years running, social and personality psychologists have served as associate editors of *Psychological Review*, and the incoming editor in chief of *Psychological Review* is our own Walter Mischel. The current editor of *Psychological Bulletin*, Nancy Eisenberg, is also a social psychologist and a member of the Society for Experimental Social Psychology. Last but not least important, we have *Psychological Inquiry*, with Roy Baumeister and Constantine Sedidikes at the helm. In short, we have resolutely confronted and, by and large, resolved the problem of theoretical outlets for social and person-

ality psychologists, and the dilemma we are facing now is the happy one of a multiplicity of riches.

In parallel, it is encouraging to witness a growth in the number of theoretical articles authored by social and personality psychologists (see Figure 1). I have conducted a rough-and-ready survey of social and personality articles published in *Psychological Review* over the last 3 decades. (Some data at long last, sigh you with relief!) Whereas between the years 1965–1968 about 8.4% of *Psychological Review* publications were centered on social and personality psychology, the corresponding figure between 1975–1978 was 14%, between 1985–1988 it was 29%, and between 1995–1998 it remained unchanged at about 27%.

But is this enough? This trend certainly is encouraging, but now is not the time to rest on our laurels. Even though the theoretical ferment in social psychology may be on the rise, our work still seems to be profoundly phenomenon driven rather than theory driven. Susan Anderson, currently an associate editor at *Psychological Review*, admits to “having been surprised by the large numbers of papers even by top notch players in the field that are really literature reviews and don’t present a well-developed, internally coherent theory that is in fact new” (personal communication, 1999). Marilyn Brewer, our outgoing editor of *Personality and Social Psychology Review (PSPR)* adds that “submissions to *PSPR* have not produced much by way of broad, general theory” (personal communication, 1999). It seems, then, that even though we have an ample opportunity to feature our work in major theoretical journals and even though we have been increasingly taking advantage of this opportunity, something is still amiss. That something could simply be the knowledge of how to theorize.

Learning How to Theorize

The implicit assumption in the field today seems to be that theorizing, as compared with research methods, is a matter of inspiration, intuition, and imagination, the “three *is*” that one either has or has not and that simply cannot be imparted to others. Accordingly, our graduate programs are very emphatic on teaching methodological and data-analytic skills, whereas theorizing is left to the temperament and personality of the individual investigator.

But is it true that theory construction cannot be taught? Even art, the epitome of creativity and inspiration, is widely taught; creative writing is widely taught; why cannot theory construction in science be systematically taught? The truth is, in other fields of science, it is. In physics, for example, graduate students choose between theoretical and experimental programs, and those who choose theory follow a very different educational trajectory than the experimentalists do. They take different courses and address very different research problems under the mentorship of working theoretical physicists. In short, they eat, breathe, and drink theory throughout their graduate studies to ultimately emerge as theoreticians themselves, a skill that they acquired and developed in the course of their graduate training.

If it is true that in social and personality psychology theory is underemphasized, it may be difficult to follow the physics model, simply because there would not be enough theoretical mentors to go around. To “jump start” the process, we may have to resort to a different method. One possibility would be to mobilize our existing theoretical potential and use it to the benefit of many. This could be accomplished through a series of theory construction workshops, perhaps sponsored by the National Science Foundation or the National Institutes of Health or attached to our major

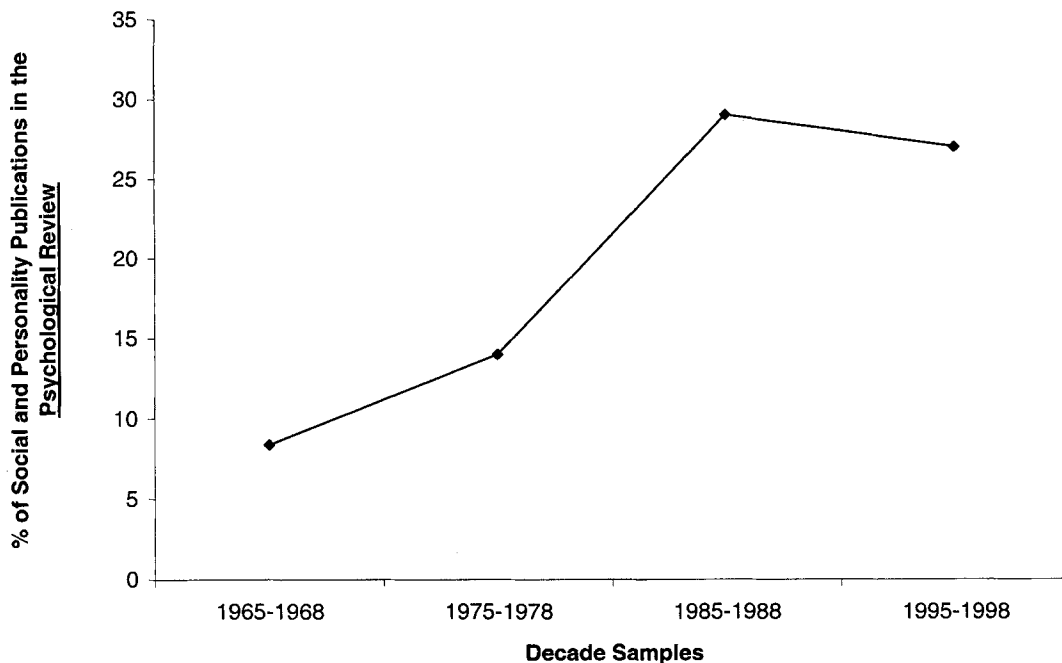


Figure 1. Percentage of social and personality publications in *Psychological Review*, sampled over 4 decades.

conventions. In such workshops, major theoreticians in social and personality psychology would share their various “tricks of the trade” with interested graduate students, postdocs and young faculty. I do not mean workshops that merely talk about theory but rather ones that actively work through various theory construction problems under the supervision of seasoned and successful theorists. I envisage workshops in which the participants gain hands-on experience with theorizing in their own domains of interest and become explicitly cognizant of its challenges and pitfalls. The idea is to learn from our theoretical successes and failures; familiarize ourselves with different modes of theorizing; develop proper criteria for evaluating theory (beyond mere verification); address problems associated with “revolutionary” versus “paradigmatic” theorizing; learn about effective ways of introducing a theory to the social and personality community, the broader social science community, and the intellectual community at large; discuss the “obligations” that proposing a theory entails for its progenitor; and so forth.

Such workshop series could then spawn specific graduate courses or seminars in theory construction, instigate book writing on social and personality theories, and revitalize the interest in the broad theoretical issues that our field must confront. If the concerns we have all been voicing are real, such a development could make a palpable difference to the contribution and impact of social and personality psychology as a field of science. It is just possible that greater familiarization with theory construction could allay the anxieties it seems presently to engender, reduce the perceived riskiness of theorizing, and restore the balance in our field by shifting our orientation from a prevention to a promotion focus and creating the conditions for new generations of social and personality psychologists to try their hand at “shooting for the stars.”

References

- Aronson, E., & Carlsmith, J. M. (1968). Experimentation in social psychology. In G. Lindzey & E. Aronson (Eds.), *Handbook of social psychology* (Vol. 2, pp. 1–79). Reading, MA: Addison Wesley.
- Deaux, K. (1988). *JPSP survey*. Unpublished manuscript, City University of New York.
- Deutsch, M., & Krauss, R. M. (1965). *Theories in social psychology*. New York: Basic Books.
- Festinger, L. (1954). A theory of social comparison processes. *Human Relations*, 7, 117–140.
- Gergen, K. J. (1994a). *Realities and relationships: Soundings in social construction*. Cambridge, MA: Harvard University Press.
- Gergen, K. J. (1994b). *Toward transformation in social knowledge* (2nd ed.). London: Sage.
- Higgins, E. T. (1992). Increasingly complex but less interesting articles: Scientific progress or regulatory problem? *Personality and Social Psychology Bulletin*, 18, 489–492.
- Jones, E. E., & Gerard, H. B. (1967). *Foundations of social psychology*. New York: Wiley.
- Merton, R. K. (1957). *Social theory and social structure*. (Rev. ed.). Glencoe, IL: Free Press.
- Miller, N., & Pedersen, W. C. (1999). Assessing process distinctiveness. *Psychological Inquiry*, 10, 150–155.
- Popper, K. R. (1959). The logic of scientific discovery. New York: Harper. (Original work published as *Logik der Forschung*, 1935)
- Reis, H. T., & Stiller, J. (1992). Publication trends in *JPSP*: A three decade review. *Personality and Social Psychology Bulletin*, 18, 465–472.
- Schacter, S., & Singer, J. (1962). Cognitive, social, and physiological determinants of emotional state. *Psychological Review*, 65, 379–399.
- Schlenker, B. R. (1974). Social psychology and science. *Journal of Personality and Social Psychology*, 29, 1–15.
- Shaw, M. E., & Costanzo, P. R. (1970). *Theories of social psychology*. New York: McGraw-Hill.
- Vallacher, R. R., & Nowak, A. (1997). The emergence of dynamical social psychology. *Psychological Inquiry*, 8, 73–99.
- West, S. G., & Wicklund, R. A. (1980). *A primer of social psychological theories*. New York: Brooks/Cole.

Received February 7, 2000

Accepted February 9, 2000 ■