Alternative Futures for Our Science

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When I think about the present and future of the field of social and personality psychology, the opening line from Dickens' *A Tale of Two Cities* comes to mind: "It was the best of times. It was the worst of times." To start with the worst of times (as psychologists always do), when there's a session about the future of a field you know it's because people see big troubles on the horizon — usually long after the troubles have already started to hit. Of course, social and personality psychologists have had deeply insightful self-awareness of problems, and crises of identity, for a long time — it's become part of an old tradition of self-criticism without reform.

The Chronic Problems

Half a dozen years ago, in a paper on "The proper study of social psychology," Hal Kelley (2000) listed what he considered the three chronic cardinal ailments of ocial psychology that kept it a marginalized field:

1. *Lack of focus* – loss of sense of direction; scattered, shifting, unstable goals.
2. *Self-hatred* – harsh criticism of each other's work; negative stereotyping of colleagues; labeling their work as trivial, obvious, just common sense — "grandma-knows-it psychology." And most important:
3. *No general theory* – just little local theories that don't add up, and fade away.

Many causes underlie the problems that Kelley noted and that still have not gone away. For decades, social psychologists were guided by a value system in which being the smartest kid on the block, and finding new and surprising phenomena fast, were goals far more important in the pursuit of tenure and glory than working toward a general theory, not to mention the building of a cumulative science. If getting and keeping your job and status in a field requires achieving "originality" by not building on anyone else's work, it is understandable that the field as Kelley saw it lacks focus, has shifting and unstable goals, and is marked by harsh criticism of everybody else, while failing to build a general theory and settling for local ones instead. It's a great way to stay marginalized.

For years it was possible to luxuriate in self-awareness about the field's problems — "yes we know, we know, and it's really too bad" — while continuing to do business as usual. That's different now, because this time the crisis challenges the field's survival — at least survival for the kind of research that many of us have gotten used to and that requires a lot of money, conducted within institutions whose existence now depends on federal funding. Well, the times are changing fast. Last year, I widely circulated some Paul Revere-type e-mails, warning that the NIMH close-down is coming for traditional social and personality psychology, it's really coming. And now it actually has come, at least for much of what has

been considered mainstream, hardcore, social-personality psychology. Grant applications — good ones — are being returned unreviewed, because they no longer fit the priorities considered fundable by the federal institutes.

To many, the shift in NIMH funding seems ironic, because it comes just at a moment that also feels like the best of times in our science. That's true at least for many whose work is moving in exciting research directions that often do address some of the key goals that federal funding agencies care about. But rather than defend our field, I'd like to consider some of the main alternatives and choices faced by those young enough to construct its future.

What Kind of Field Do You Want This One to Become?

Let me suggest two broad, constructive alternatives for consideration — and it's an approach-approach conflict because both are valuable.

The first alternative is a default option; namely, don't do anything much different from what you are doing already, it's probably very good. Social and personality psychology have distinguished and even grand histories and accomplishments. There's much to gain by continuing in traditions that consistently yield surprising and significant findings that reveal much about human behavior and the social forces that influence it.

The limitations of the field that Kelley pinpointed all have their good sides too. Being highly critical of everybody's work — including your own — and trained to be a methodological bloodhound, doesn't necessarily make one just another well-hated reviewer on the study section or the journal board. It also provides the skills and sophistication to do non-obvious and sometimes startlingly interesting and elegant studies on all sorts of phenomena. Sometimes if you can manage to do three in a row on the same topic, and survive a few more revisions, they even have a chance of becoming enshrined in the *Journal of Personality and Social Psychology*. More seriously, you might actually add something truly interesting to what is known about the remarkable characteristics, foibles, limitations, and powers of human beings and their social worlds.

Likewise, shifting goals in one's work and having no consistent focus, don't have to be negatives — they can be hallmarks of flexibility, scientific creativity, and a hunger for tackling diverse phenomena in the course of a career. So, alternative one is to keep on doing what you're probably doing already. But maybe think about smaller, shorter grants from the National Science Foundation and don't worry about not getting larger ones from NIMH — you'll be in good company.

The second alternative is also a default option because many in the field have been doing it already for years–but it takes a somewhat different course, perhaps driven by different goals and values. That course is described in great detail in a piece that Dan Cervone and I wrote (actually Dan wrote it 98 percent and I endorsed it 100 percent) for a volume called *Advances in Personality Science* (Cervone & Mischel, 2002).

Carving Nature at the Wrong Joints

We made the case for the importance and timeliness of building a science of persons in their social worlds, studied at multiple levels. We noted that academic disciplines rarely cut nature at its natural joints — and ours certainly has not. The question is: How can we become a more cumulative science that

takes better account of the aspects of nature we care about, regardless of the academic boundaries that cut us up historically? An example comes from the birth of cognitive science. Old disciplinary boundaries were quickly crossed on bridges that rapidly linked psychologists with anthropologists, computer scientists, linguists, neuroscientists, and philosophers. The bridges worked because they enabled the convergence of their common interests in the acquisition and representation of knowledge at complementary levels. The participants benefited from their neighbors' theoretical and methodological tools and began to see and analyze the same basic phenomena from different vantage points, at different levels, and with increasing depth. That example illustrates the building of a cumulative science that continues to grow as cognitive science and cognitive neuroscience become increasingly connected, morphing into other areas of science, including molecular biology.

I think we are now not just in the worst of times but also in the very best of times, because many people in our field are working hard and well to create this kind of future. Such work is illustrated in the bridge-building that is accelerating in social and personality psychology currently. You can see it in an in-press volume called *Bridging Social Psychology*, edited by Paul Van Lange (in press). More than 60 leading personality and social psychologists contributed chapters on the bridges they are building to related sub-disciplines and other fields. A dizzying array of bridges is connecting social-personality psychology to many areas of psychological science and other fields. The bridges go in all directions. They're connecting to cognitive science, biology, neuroscience, motivation, emotion, development, speech science, health and behavioral medicine, economics, political science, culture, and more.

It is happening not because bridge-building is in vogue, but because social and personality psychologists need these bridges to clarify phenomena that lie at the intersections among multiple disciplines, and unfold at multiple levels. Therefore, they are best illuminated by analyses that share collaboratively the tools and the perspectives contributed by those different levels and disciplines. Such analyses can encompass, for example, what the individual experiences, thinks, and does at the psychological level; what the brain is doing; and, ultimately, how the relevant biological processes play out at all levels.

Fine illustrations of this kind were in the program of the present conference, for example in meetings dealing with "social cognitive neuroscience" — and notice that from its title on that it juxtaposes social and cognitive and neuroscience all in one unhyphenated breath. These are steps toward building a cumulative science, and without the reductionism that most of us dread, because each level has its own importance and is enriched by its connections with the others. And the causal direction never goes one way but always involves reciprocal interactions. A vivid recent example of such interaction comes in the finding reported recently that the perception of being stressed (as well as objective indices of stress) make one biologically years older, as revealed by changes in the telomeres of the DNA (Epel and colleagues, 2004).

The example makes clear that the principle of *reciprocal determinism*, basic for understanding the interaction between persons and situations, holds equally for the mutual influences between biological factors and psychosocial influences, as John Cacioppo and others have long noted. It reminds us that interactionism is a basic rule in sciences dealing with living organisms. Ehrlich (2000), for example, focusing on the interplay of genetic endowment and environmental experience, commented that the psychologist's typical strategy of partitioning the determinants of behavioral characteristics into separate genetic versus environmental causes is no more sensible than asking which areas of a rectangle are mostly due to length and which to width. The future may be brighter if we leave the old partitioning

strategy behind. But that's easy to recommend and difficult to do.

The Person-Situation Split: The Worst Cut of All

For me, the classic partitioning most unnatural and destructive to the building of a cumulative science of mind and social behavior is the one that traditionally has split the person apart from the situation, treating each as if it were an independent cause of behavior. How the field deals with this split will significantly influence the future it constructs for itself. The fact that there still is such a splitting is astonishing, given that Kurt Lewin already in 1936 placed both the person and the situation at the core of social psychology. Seventy years ago he argued that the field's key aim needs to be to understand how individuals try to make sense of their social environments in light of their goals and interpretations. In such a science, the person and the situation — always the perceived, construed, psychological situation — have to be conceptualized and analyzed in tandem, not artificially partitioned into separate entities or split into different sub-disciplines with separate training programs and missions.

What has made it so difficult to pursue Lewin's goal? I think the culprit is a long, deeply ingrained tradition of Western thinking about persons and situations that is extremely difficult to shed. That tradition has incorrectly assumed that the person and the situation are independent causes of behavior and indeed separable entities that have to be split. Those old assumptions may have made sense early in the last century, when positivism and behaviorism were in full force and the mechanistic Cartesian worldview that goes back to Aristotle was still predominant. That view split the phenomena and the fields of social and personality psychology, and the two disciplines themselves, in exactly the worst way — a way that undermined Lewin's goal and continues to hamper the progress of our science.

Historically, the assumption that the person and the situation are independent causes of behavior led to making personality psychology the field devoted to the person apart from the situation. It treated the situation as the error term that needs to be removed or aggregated away. To see the person, you had to remove the effects of the situation, either by making it completely ambiguous, as on a Rorschach inkblot in projective testing, or by getting rid of it on situation-free global measures of what the person is like "on the whole." Consequently, the situation was — and in much current practice still is — deliberately removed or aggregated out to ask about the general effects of persons, regardless of situations. In contrast, much of social psychology became defined as the study of the effects of situations, usually regardless of the kinds of persons in them. So, for each field, the main variables of the other constituted the error variance that needed to be removed. As Leon Festinger said to me 40 years ago while discussing my interest in personality and individual differences, "Your independent variables are my noise." And I told him that his noise was my essence.

The boundaries between personality and social psychology, and between the person and the situation, made little sense to me when *Personality and Assessment* was published in 1968. They make even less sense now, because treating the person and the psychological situation as independent causes of behavior flies in the face of the reciprocal interactions that our science is finding, of what the cognitive revolution taught us years ago, and of what is again being found in cognitive neuroscience and biology. Therefore with the goal of studying the person and the situation at their natural joints — rather than at their old academic joints — Yuichi Shoda and I and our colleagues for many years have focused on the situation as well as the person jointly, and on their intrinsic interconnections in the head of the perceiver and in the behaviors that are generated in the social world (e.g., Mischel, 2004).

Because the intrapersonal and interpersonal are two integrally connected sides of one phenomenon, our science needs a seamless bridge between the sub-disciplines of personality and social psychology, and a unifying, integrative theory for studying person-situation interaction. Fortunately, several directions of research and theory-building have converged in recent years, providing at least the outlines for such a framework. By drawing on cognitive science and neural network models of a broadly connectionist type, personality can be conceptualized as a dynamic cognitive-affective processing system ” a system that mediates how the person selects, construes, and processes social information and generates social behaviors in continuous interaction with the perceived features of social situations. While the details are sure to keep changing, this kind of broad framework also may become a bridge to connect the kind of basic interpersonal situations identified by Harold Kelley and colleagues in their comprehensive *Atlas of Interpersonal Situations* (2003) to the psychological “chemistry” — the intrapersonal and interpersonal cognitive-affective processing dynamics — of the people dealing with those situations and construing them in their own terms.

Toward a More Cumulative Science?

If we want to build a stronger, more integrative and cumulative science, what is needed? Hal Kelley noted that one chronic problem is that our field lacks big or general theories. As the previous editor of *Psychological Review*, I spent much of the last few years worrying about the advancement of theory and what makes a good one — more than I would otherwise have ever done. In fact, one of my favorite Amos Tversky remarks was that for every 10 years of hard empirical work in psychology you earn 10 minutes for talking about theory. And I can understand why most social psychologists have shied away from big theory-making as if it were the plague.

In the early history of psychology, the big, grand theories were too often the grandiose theories, in which one person tried to spin de novo an all-encompassing brand new view of human nature in which a few antecedents — usually tucked away in early childhood or the unconscious — accounted for virtually everything. They also had the advantage of being based on little data and cast so that they could not be disconfirmed. The nice part was that it guaranteed them long lives in textbooks for decades after their impact had evaporated. That’s not what *Psychological Review* was looking for during my term, and it’s not what I suggest now.

Instead, a cumulative science can flourish if many small but solidly data-based theories become integrated into bigger ones ” stronger, broader, multi-level ones—as many in the field already have been doing for a long time. That requires developing and using common shared tools and a common language, so that researchers can relate to each other’s work and gain from it, rather than isolating each other in the struggle to claim total — rather than necessarily constrained — uniqueness in their efforts. It makes spelling out the conditions that enable replication a requirement for publishing empirical contributions in our research literature. It requires placing high value on demonstrating robust and replicable effects about important phenomena and processes, more than on finding cute new ones, especially if they don’t replicate. And it means that not being replicated — which is excusable when everybody uses somewhat different language, tools, and procedures — becomes the terror that it should be in a serious science.

All this may sound good, but it’s unlikely to be facilitated unless students and young faculty find that they can try to pursue such goals without committing early professional suicide. And that won’t happen if tenure and promotion decisions are based on criteria that make such efforts a route to premature career

failure, because we expect people to have their own original theory or novel phenomenon within a half dozen years of the PhD. And that's unlikely to change as long as the "toothbrush problem" holds. As one wit — whose name I am sorry to have forgotten, but whose message I keep repeating — wrote many years ago: We psychologists treat our theories like toothbrushes; no self-respecting person wants to use anyone else's. Perhaps it's time to rethink our values and our practices, if we want to build a more cumulative psychological science in which social and personality psychologists have a central place in the future.

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