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# **PERSPECTIVISM IN SOCIAL PSYCHOLOGY**

THE YIN AND YANG  
OF SCIENTIFIC PROGRESS

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# The Resting Parrot, the Dessert Stomach, and Other Perfectly Defensible Theories

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Perspectivism calls upon scientists to use empirical work to do deliberately the contextual exploration that they now do furtively while pretending to be doing hypothesis testing. (McGuire, 1989, p. 244)

## Prologue

In *Against Method*, Paul Feyerabend (1975) concluded that any attempt to specify bounds of scientific method would be misguided. The specified boundaries, he argued, would inevitably exclude methods that are valuable in the accumulation of scientific knowledge. Feyerabend's argument drew heavily on an analysis of Galileo's methodological flexibility in advancing Copernicus's heliocentric theory over the dominant geocentric theory.

One reason for my finding Feyerabend's argument compelling is that I have long questioned the wisdom of a principle of proper method in psychology that is now widely advocated: the belief that empirical research is valuable only to the extent that it advances theory. I see this principle advocated every time that, as editorial reviewer, I receive a copy of an editorial rejection declaring that the reported research, even though acknowledged as interesting, did not advance theory.

Fortunately, this principle has not always been used to decide what should and what should not be published. Among the important works that have managed to achieve publication without advancing theory are some of the major works by Asch (conformity), Sherif (norm formation), Milgram (obedience), and Zajonc (mere exposure). I take occasional pleasure in managing to get into print an article that contains no theory (e.g., Greenwald, Draine, & Abrams, 1996; Greenwald, McGhee, & Schwartz, 1998).

In a few methodological articles, I have tried to make the point that much time and effort can be wasted in excessive focus on confirming theories or in trying to resolve disagreements among theories. I have discovered that theory is so sacred a cow in our discipline that my methodological articles have led more than a few colleagues to conclude that I am generally an opponent of theory.

My contribution to this Festschrift takes a further tack in describing costs of overemphasis on theory. It will no doubt strengthen the impression that I

am against theory. Accordingly, I call attention to a quotation that I strongly endorse: "There is nothing so practical as a good theory" (Lewin, 1951, p. 20). I endorse it because I understand that the only way to relate empirical findings to practical applications is to have a theory that provides the basis for generalizing beyond the laboratory.

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A common property of many long-unresolved theoretical debates in psychology is the flexibility of the competing theories: They are readily modified to accommodate unanticipated findings. Although theory modification is essential to scientific progress, repeated modifications can make contending theories effectively interchangeable, in turn making their competition illusory. Because theory competitions can be either sustained or resolved by voluntary actions of researcher/theorists, there is no way, beyond generalizing from the past, to predict that a specific theory competition will be sustained in illusory fashion. This makes it a challenge to develop strategies that will protect against the waste of research resources on unresolvable theory competitions.

Of those who join here to celebrate Bill McGuire's scientific contributions, I can claim the opportunity to have been influenced by Bill almost as long ago as any. Bill was an instructor of a team-taught course at Yale on Human Culture and Behavior that I took as an undergraduate in 1957-1958. Bill's obviously favorable perspective on the Hovland, Janis, and Kelley (1953) *Communication and Persuasion* volume was no doubt an influence that pointed me toward choosing attitudes as my first area of research specialization. It wasn't until several years later that I began to read Bill's published work, starting with his theoretically ingenious and empirically convincing 1964 chapter on immunization-like processes in resistance to persuasion.

Bill's methodological works, starting with the famous "Yin and Yang" article (McGuire, 1973) and continuing through his several writings on perspectivism, have influenced me so much that I cannot identify which (if any) of the methodological points that I subsequently made in print may have occurred to me prior to my reading them in Bill's work. The ideas in this chapter build on a recurring theme in Bill's series of methodological works: the description of research strategies in terms of their potential to facilitate or hinder progress.

I thank this volume's editors for having obliged relatively short contributions. To cope with this limitation, I ask readers to assume that some of my more contentious points are plausible, after which I proceed as if I had established these points convincingly. If I am allowed to get away with this, I am sure that I will try it again in the future; it is remarkably liberating.

On reading an earlier draft of this chapter, one of this volume's editors commented that it seemed pessimistic concerning the functioning of theory in psychology. I hope not to be read as a pessimist. Some of the challenges that psychologists face in dealing with theory are caused by the youth of psychology as a discipline. By developing an understanding of these challenges, I hope to facilitate progress in dealing with them.

## Competition Among Theories

Competing theoretical accounts of novel findings attract researchers like moths to flame. J. R. Platt (1964) gave the approving label *strong inference* to experiments that are designed to choose between competing theoretical interpretations of a phenomenon. It is easy to conclude that competition between theories is generally desirable.

Is it? If competition among theories is a good way of doing science, we should expect theoretical controversies to have a short life expectancy. When a controversy occurs, we should expect that experiments designed to choose among the theories will resolve the controversy within perhaps a few years. It is therefore informative to examine the life expectancy of theoretical controversies.

My informal review of theoretical controversies reveals that it is easy to identify publications that initiated many well-known controversies, but it is virtually impossible to identify publications that brought any of these to resolution. For example, there has been a long-lasting controversy about the nature of mental representations that underlie the human ability to rotate objects mentally (Shepard & Metzler, 1971). Are these representations propositional (verbal or symbolic) or analog (involving visual features)? More than 30 years later, this debate shows little sign of ending. It is easy to identify other similarly durable controversies pertaining to representations underlying mental categorization (dating from Labov, 1973), serial versus parallel mental processes in memory search (dating from Sternberg, 1966), and bipolar versus orthogonal dimensions of positive and negative affect (dating from Nowlis & Nowlis, 1956). The life expectancy of these controversies is certainly long, and it may be indeterminate. A genuine competition between theories should eventually reach a conclusion. Psychology's well-known theoretical controversies appear to offer only the illusion of competition. Their prevalence calls into question the belief that theory controversy is a useful propellant for the advance of scientific knowledge.

### How to Determine Whether Theory Competitions Are Illusory

Alas, there is no way to determine conclusively whether psychology is plagued with illusory theory competitions. One possible method would be to establish by analytical reasoning that *all* competitions between theories are necessarily unresolvable. This assertion has indeed received careful attention from philosophers of science. Ironically, philosophical analyses of the prospects for using empirical methods to resolve theoretical disputes display their own lengthy, unresolved controversy, which can be found easily in the philosophical literature by searching for articles on the topic of "underdetermination of theory by data." This is the body of literature in which one runs repeatedly into the names of Quine, Duhem, Popper, Kuhn, Lakatos, and Feyerabend, who were the major contributors before about 1980.

Even if philosophy can break its own deadlock to establish that psychology's theoretical controversies are in principle resolvable, it would nevertheless be possible for scientists to pursue such controversies so as to indefinitely

avoid resolution. Also, perhaps a bit more surprisingly, even if philosophers could persuade us that some or all such controversies are in principle not resolvable, it would nevertheless be possible for scientists to resolve all such controversies.

As an example, consider a theoretical controversy that has recently occupied the time and attention of astronomers: Is the astronomical object Pluto a planet or is it a large comet? Regardless of any conclusion that philosophy of science might reach about the possibility of resolving this debate, astronomers have it in their power to prolong the debate endlessly, just as it is possible for them to achieve a speedy resolution. More generally, it is always an option for researchers either to prolong or to resolve any competition between theories. As a result, working scientists effectively make the philosophers' analyses irrelevant.<sup>1</sup>

To appreciate researchers' power to control the fate of any theoretical controversy, imagine that, when a research article claims that its findings refute an old theory and establish a new one, advocates of the old theory might say, "Our theory had a long life and is now gone. Long live the new theory." If that does not happen, imagine that advocates of the old theory publish a reply in which they argue that the apparent threat to their theory was entirely misguided. At that point, nothing need stop proponents of the new theory from declaring, "Hallelujah! The venerable old theory has risen. Let our upstart pretender rest in peace." Despite such possibilities for concluding any theoretical debate, psychological researchers generally choose to sustain debate. They choose with apparent uniformity to defend any attacked theory.

### **What Are the Consequences of Illusory Competition?**

Although illusory theory competitions do not achieve the goal of choosing among the competing theories, they might have other benefits that make them scientifically valuable. As an aid to thinking about costs and benefits of illusory competitions, I offer a fictitious illustration for which we can be sure that the controversy is illusory.

Imagine that you and a friend are 18th-century Yale philosophy professors time-transported to New Haven in the year 2003 as part of a laboratory exercise by electronic beings of the 24th century. (We shall assume that this laboratory exercise has been approved by the local review network for research on carbon-based forms.) Arriving at the Elm–Broadway intersection in 2003, you and your friend immediately notice the noisy vehicles that dominate the street. Your electronic hosts decide to make use of your fascina-

<sup>1</sup>At the Festschrift conference, I showed a video recording of Monty Python's "Dead Parrot Sketch" (Chapman, Monty Python, 1969/1989) to illustrate the opportunities that exist for optionally prolonging theoretical debates. In that episode, a customer returns a parrot to the pet shop from which he recently purchased it and presents the pet shop owner with the complaint (theory) that the parrot is now dead and was indeed dead at the time of purchase. The pet shop owner presents and defends several alternative views, especially (and repeatedly) the theory that the parrot is resting (providing part of the title of this chapter). In this inspired and hilarious piece of comedy, the shop owner's persistent and imaginative refutations of the dead parrot theory illustrate the possibility of prolonging a theoretical debate indefinitely by defending alternative interpretations even in the presence of compelling data.

tion with automobiles in order to test your intelligence. They inform you that these vehicles are moved by something that is located under the hood and they challenge you to explain how the movement is produced. They permit you to observe the operation of these vehicles either as a passenger or by watching from the street. (It is significant that you do not have the opportunity to look under the hood—this is crucial to the metaphor for behavioral psychology.)

Taking up the challenge and the position of front-seat passenger, you notice that whenever the driver applies steady foot pressure to a certain pedal, the vehicle steadily gains speed. In addition, you hear a noise that rises in pitch as the vehicle moves faster. Looking out the window, you also notice that there are places at which several vehicles are stopped while their drivers supervise the injection of some unseen substance into the vehicles' bodies. After pondering your observations, you conclude that the injected substance fills a hidden trough from which it is consumed by a powerful beast that runs on a treadmill. This treadmill is the source of both the power for movement and the sound; you theorize that the rising pitch of the sound is produced by the treadmill's increasing speed. In your theory, the pedal acts much like a horse's reins. The foot pressure on the pedal transmits a signal to the unseen beast, causing it to run on the treadmill and thereby to produce the power needed to turn the vehicle's wheels.

Your colleague has made similar observations but, unlike you, has observed that as the vehicle gains speed, the accompanying sound first rises in pitch, then drops, and then rises again. Based on this evidence, your colleague declares your theory to be hopelessly incorrect and proposes in its place a two-beast theory. In the two-beast theory, each beast has its own treadmill. The second beast starts running when the first one is fatigued. The two-beast theory includes an elegant explanation of why the second sound does not rise to a higher pitch than the first even though the vehicle has gained more speed. The explanation appeals to a gearing mechanism that allows the vehicle's wheels to turn faster for the same treadmill movement.

After conducting additional observations while listening more carefully, you conclude that your colleague's observations are valid. Nevertheless, you see no reason to abandon your one-beast theory. To explain all of the data, you need only to add a second treadmill, which you hypothesize to be operated by the one and only beast that resides under the hood. The audible changes occur when the beast steps off the first treadmill, causing it to slow down, and before the second treadmill gets up to speed. You are excited to have identified a well-defined difference between the two theories. You and your colleague immediately begin to discuss possibilities for tests that will decisively determine whether each vehicle houses one or two beasts.

A relatively sophisticated early 21st-century automobile user will, of course, see these developments as unfortunate. You and your colleague are about to waste a good part of your visit to the 21st century on the illusion of a competition between two plausible, but somewhat misdirected, theories. Also, while you are engaged in this competition, you will be ignoring the interesting idea that your two theories share—the idea of a transmission that allows an engine with a limited range of revolution speeds to control wheels that have a much wider range of revolution speeds. This theory, on which the two of you agree, actually explains the sound patterns that were the original source of

your disagreement. However, in your eagerness to focus on the disagreement between your theories, you failed to appreciate the value of a theoretical proposition on which you agree.

Let us leave this thought odyssey behind, hoping that its absurdity will not prevent appreciation that the competition it describes may share characteristics with many of psychology's theoretical controversies. Specifically, the one-beast-versus-two-beast controversy has three characteristics: First, the competition is unresolvable in the sense that the competing theories are flexible enough to be empirically indistinguishable. Second, the competition has the potential to generate a progression of ultimately unimportant findings. Third, the competition distracts attention from interesting ideas on which the competitors agree.

Proper evaluation of the proposition "Illusory competitions are likely to generate inconsequential findings and divert attention from more valuable research efforts" requires a more detailed analysis of currently unresolved controversies than can be offered here. Once again, I limit my goal to trying to establish plausibility. Therefore, consider that (a) it seems unlikely that an extended debate over whether Pluto is a planet or a comet will generate important findings, (b) researchers' attempts to decide whether altruism is intrinsically or extrinsically motivated may be diverting effort away from interesting questions about how to influence the frequency of altruistic acts, and (c) for all the journal pages that have been devoted to them, studies designed to test whether mental categories have structures identifiable as features, prototypes, exemplars, or rules have yielded relatively little gain in understanding categories beyond Jerome Bruner's (1957) point that categories go "beyond the information given."

Unresolved controversies may have their most positive impact in encouraging the development of new observational methods. The attempt to propel the controversy puts pressure on researchers to generate observations that go beyond already available evidence. So, in seeking to resolve whether Pluto is a comet or a planet, astronomers may find ways to make observations of previously unobservable characteristics of Pluto. Those new observational methods may prove valuable even though they will not resolve whether Pluto is a planet or a comet. It will not resolve the debate because the debate, insofar as it is an illusory competition, is only about how the words "planet" and "comet" should be used. Similarly, the debate about altruism is largely one about the meanings of the terms "intrinsic motivation" and "extrinsic motivation." Debates about word meanings can be handled much more efficiently by negotiation among those who need to use the words than by gathering new data that allow different groups to modify existing meanings in different fashions.

### **Why Are There Illusory Competitions?**

The reader is again reminded that I am proceeding as if my superficially illustrated arguments have been established convincingly. On that basis, I have concluded that there is indeed an epidemic of illusory theory competitions in psychology; these competitions are illusory in that they turn out to be debates about the meaning of words rather than about the nature of relationships

among observable empirical phenomena. Having established this point, I am now free to ask, "Why do these competitions develop and thrive in psychology?"

I have a simple two-part answer: First, researchers tend to be ego-involved advocates of *their* theories. This point is far from new, and it does not apply uniquely to psychology. The point was made very well by the geologist T. C. Chamberlin (1890/1965) when he wrote the 19th-century predecessor of J. R. Platt's (1964) "strong inference" article. The second reason, because it does apply specifically to psychology, seems the more important reason: Most of psychology's theories concern unobservable mental entities—things such as representations, motives, and traits. It is a constant challenge to psychologists to come up with measures for these unobservable entities. The difficulty of measuring unobservable mental entities makes it relatively easy for well-trained researchers to challenge the validity of any such measure. This explains equally the ease of attacking the empirical evidence for any theory and the ease of defending a theory against empirical attacks.

### *A Conflict of Interest*

Because researchers tend to be ego-involved advocates of their theories, researchers' self-respect is tied to judgments about the validity of their theories. Most psychological researchers have many occasions to experience a conflict of interest between their self-respect and their scientific objectivity. This conflict can occur whenever they encounter results that appear to refute theories with which they are identified. Perhaps regrettably, it is all too easy for psychologists to resolve the conflict in favor of the preferred theory. I have developed a three-item questionnaire intended to demonstrate the conflict. Readers are invited to answer these questions not for themselves but as descriptions of "other typical researchers." However, the questions are indeed intended to provoke self-questioning. (The italicized answer to each question indicates resolution of a conflict of interest in favor of self-respect.)

1. When conducting multiple parallel tests of a prediction from a preferred theory, the typical researcher will most likely conclude that a test that produced a statistically significant confirmation was more valid than one that produced a statistically nonsignificant result. (*True* or *False*?)
2. When an experiment fails to produce a result predicted by a preferred theory, the typical researcher will actively seek publication of that theory-refuting result. (*True* or *False*?)
3. When reviewing a manuscript that bears on a preferred theory, the typical researcher will be more likely to write a favorable review when the manuscript's research supports the theory than when it does not. (*True* or *False*?)

### *Who Cares About This Conflict of Interest?*

It is of interest that the American Psychological Association's (2002) *Ethical Principles of Psychologists and Code of Conduct* contains no mention of researchers' conflict of interest when it comes to evaluating their own theories.



The closest reference is one brief statement in Ethical Principle 8.10(a) to the effect that “Psychologists do not fabricate data” (APA, 2002). The Fifth Edition of the APA *Publication Manual* gives a brief elaboration of this principle: “Errors of omission also are prohibited. Psychologists do not omit troublesome observations from their reports so as to present a more convincing story” (APA, 2001, p. 348). This reference to “troublesome observations” is the sum total of the American Psychological Association’s ethical advice bearing on the possibility of researcher self-interest influencing the conduct of research.

It is provocative to compare research psychologists with litigators who actively work to construct a one-sided case for their clients. Lawyers’ professional ethics allow this unmitigated partiality in the context of an adversarial system in which each contending party has its own advocate. In contrast, in its near silence on the topic of researchers’ conflicts between self-interest and scientific objectivity, APA’s code neither encourages nor discourages researchers’ bias in advocating their own theories.<sup>2</sup>

### *Resolving the Conflict of Interest*

Solutions to the conflict between self-interest and scientific objectivity can be divided into those that would legislate the conflict out of existence (the first three of the following) and those that recognize and try to manage the conflict of interest (the last two).

1. A MODIFIED CODE OF ETHICS. When researchers make editorial decisions and when they review grant proposals or manuscripts submitted to journals, they act in the role of judge or jury. In the courts, judges or jurors are expected not to participate in cases in which they have a relationship with a contending party. By analogy, researchers could excuse themselves from reviewing manuscripts or grant proposals that concern theories with which they are associated. A serious problem with this suggestion is that it would exclude reviewers and editors from participating in just those reviews for which they are most qualified. An even more severe problem is that this proposal does not touch on the frequently arising conflicts that occur in the process of dealing with one’s own research results.

2. SEPARATING THE ROLES OF THEORIST AND DATA COLLECTOR. If theorists played no role in producing the data that test their theories, their partiality would not enter into evaluations of their theories. This proposal draws on the traditional separation of theorist and experimentalist roles in the discipline of physics. However, this otherwise reasonable solution seems impractical for psychology because of the radical shift that it would require in the culture of the discipline.

3. SEPARATING THE ROLES OF RESEARCHER AND GATEKEEPER. Another role-separation solution to conflicts between self-interest and scientific objectivity

<sup>2</sup>The American Psychological Association disagrees with the interpretation of the Ethics Code as stated here. This statement should therefore not be interpreted as representing an official position of the American Psychological Association.

is to define separate career paths for potential researchers and potential decision makers (such as editors and grant givers). A model is provided by the legal profession's separation of the career path for judges from that for advocates. Again, however, this change would require a radical change of the culture of scientific psychology and therefore seems quite impractical.

4. CHAMBERLIN'S (1890/1965) METHOD OF "MULTIPLE WORKING HYPOTHESES." Researchers in many disciplines are under pressure from reviewers and editors to establish the theory relevance of results that they report in manuscripts or of the research that they propose in grant applications. This aspect of publication culture encourages one-sided theory advocacy as an expository device in reporting research. An alternative means of emphasizing theory is to stress multiplicity and complementarity among theories. This is a point that Bill McGuire, in his perspectivist approach, has urged repeatedly. It relates to a suggestion that Chamberlin made after he commented on the tendency of scientific theorists to have a parental affection for their theories:

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 intellectual offspring, and it grows more and more dear to him, so that, while he holds it seemingly tentative, it is still lovingly tentative, and not impartially tentative. . . . There is an unconscious selection and magnifying of the phenomena that fall into harmony with the theory and support it, and an unconscious neglect of those that fail of coincidence. 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 seem refractory. Instinctively, there is a special searching-out of phenomena that support it, for the mind is led by its desires. There springs up, also, an unconscious pressing of the theory to make it fit the facts, and a pressing of the facts to make them fit the theory. (1890/1965, p. 755)

Instead of asking researchers to present results that show how theories fare in competition with one another, researchers can be asked to show how theories complement one another—locating the boundaries between domains in which different theories apply. For those familiar with the language of construct validity, this is a suggestion to include evidence for discriminant validity in research designs and reports.

5. COLLABORATIONS INVOLVING THEORETICAL ANTAGONISTS. If proponents of competing theories could work together on the design of research, several desirable consequences, beyond the added resources of combined forces, could be expected. The most important desirable consequence is that debates about meanings of words would be played out in private without flowing into journal pages. Second, the collaborative research would likely be designed to give both (or all) theories their best chances for confirmation, including the possibility that different theories would be demonstrated to be valid under different conditions (again pointing to the desirability of evidence for discriminant validity). An illustration that this is possible, even if difficult, can be found in

the recent “adversarial collaboration” exercise by Mellers, Hertwig, and Kahneman (2001).

### Epilogue

My wife and children still recall the moment of what may have been my most brilliant theoretical creation—never mind that I was later disappointed to discover that others had preceded me in the theory. It was at a hotel in downtown Columbus, Ohio, to which our family had gone for a Sunday buffet. After filling ourselves on various combinations of shrimp, cheese, salads, casseroles, vegetables, fish, turkey, and, in my case, at least one more piece of roast beef than seemed prudent—to the point of being simply unable to eat more—we all discovered that it was still possible to make a trip to the dessert table and to eat at least one dessert. Primed by the title of this chapter, the reader will by now have anticipated my theoretical insight—the dessert stomach. The dessert stomach is an extra digestive organ that functions like the pinch hitter in baseball, resting on the sidelines until it is needed late in the game.

The dessert stomach theory neatly explained the otherwise puzzling all-you-can-eat-buffet data. However, the true test of this theory, like any other, is whether it can be applied successfully. Can the dessert stomach theory be put to use to achieve a result that would not have been thought possible until this theory came along? I reasoned that, if the extra capacity of the dessert stomach indeed existed, then it should be possible to put that capacity to some other use. That was the moment when—already filled to capacity—I got up and approached the dessert table. I hypothesized that an approach to the dessert table would suffice for my unseen internal digestive machinery to switch open the dessert stomach. After arriving at the dessert table, I stopped, changed direction, and walked instead toward the table at which pieces of juicy, rare roast beef were being carved. As those in my family well recall, that was the first observation of a piece of roast beef being tricked into the dessert stomach.

I bring up this accomplishment not merely to impress you with my theoretical skills, but because it may be an illustration that helps point the way to psychology’s future. The dessert stomach theory could be perfectly defensible in any journal that obliged researchers to rely only on behavioral data. It is easy to imagine an unending controversy between advocates and critics of the theory. On the basis of nothing but behavioral data, the dessert stomach theory is, as the title of this chapter suggests, “perfectly defensible.” However, as soon as any researcher would undertake to look inside the abdomen for the dessert stomach, the theoretical fate of this hypothesized digestive organ would be promptly resolved.

In contemporary psychology, only rarely are we able to look inside, or under the hood, in order to locate and identify our theoretical constructs. When we can do that, we sometimes get answers to questions that would otherwise be candidates for an unending theoretical controversy.<sup>3</sup> With the application of

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<sup>3</sup>See the articles by Kosslyn, Digirolamo, Thompson, and Alpert, (1998), and Ganis, Keenan, Kosslyn, and Pascual-Leone (2000). These indicate a possible path toward resolution of the previously described debate about representations underlying mental rotation by using evidence obtained from looking under the hood (using positron emission tomography scans) and otherwise tinkering under the hood (administering transcranial magnetic stimulation).

computers to the processing of event-related potentials and the increasing temporal and spatial resolution of brain-imaging devices, we are perhaps at the beginning of an era in which psychology's theorized entities will be increasingly tied to identifiable inside-the-brain structures. In that coming era, we may not see the kinds of prolonged theoretical controversies that are so readily found in today's psychology. This coming era may well arrive during the professional lifetimes of many of those who join in this volume to celebrate Bill McGuire's career. Its arrival may even be hastened if, in the meantime, we can divert less of our collective professional resources to illusory competitions among theories.

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