

bon and Staddon–Higa theories acknowledge the equivalence of timing and counting. What remains perplexing and elusive are those events that control the sorts of behaviors Staddon and Higa and all the other clever researchers in this domain have attempted to capture. The rest of us may exclaim in Viola’s words from *Twelfth Night*:

O Time, thou must untangle this, not I;
It is too hard a knot for me t’ untie!

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TOLERANCE IN A RIGOROUS SCIENCE

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Scientists often evaluate other people’s theories by the same standards they apply to their own work; it is as though scientists may believe that these criteria are independent of their own personal priorities and standards. As a result of this probably implicit belief, they sometimes may make less useful judgments than they otherwise might if they were able and willing to evaluate a specific theory at least partly in terms of the standards appropriate to that theory. Journal editors can play an especially constructive role in managing this diversity of standards and opinion.

Key words: tolerance, diversity, truth, conviction, parsimony, historicity

Staddon and Higa’s paper is one of most stimulating and provocative I have read for

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some time in the literature on timing. I hugely enjoyed reading it, and I strongly supported its publication. I did not do this because the theory seems true, because it addresses core timing data that I believe any theory of timing must address, because it best satisfies a law of parsimony, and especially, I did not support publication because reading the paper con-

vinced me of anything in particular. Generally speaking, I supported it because I found it interesting, and I suspect it will stimulate thinking about what timing is and about how timing is related to other processes, such as memory dynamics. Also, I see the introductory section as a model of constructive conceptual analysis and as virtually a defining exemplar of the idea that to understand a theory one needs to understand its historical development. I also see the new model as one the appreciation of which by many researchers will require tolerance for a view which is legitimate but not in any way yet proven to be true. My comments address a few issues related both to peer review in general and to the evaluation of Staddon and Higa's article specifically.

Historicism: Knowing the Historical Development of a Theory Is Part of What It Means to Understand the Theory

I especially like the introduction section, in which scalar expectancy theory (SET) is conceptually and historically analyzed. In my opinion, an historical analysis can clarify the theoretical choice points encountered in the development of a theory, can clarify why a theory has assumed its current form, and can suggest possible alternatives. History can be read in many legitimate and different ways, however, so it would not be surprising or disconcerting if, for example, the actual authors of SET deny having made the choices Staddon and Higa attribute to them.

Staddon and Higa's historical analysis reminds me of a tradition including Vico, Goethe, Hegel, Nietzsche, Hanson, Kuhn, and increasingly many contemporary scholars. This historicist tradition highlights processes of change in science, and suggests that an ahistorical sketch of a momentary condition of a science, such as its current condition, no matter how brilliant its logic, can only hint at what the science is all about. Some researchers may be impatient with what to them might be an irrelevant and distracting historical analysis, but such a positivist and ahistoricist position seems now on the defensive after its long hegemony during much of the 20th century. I also see Staddon and Higa's position as being compatible in this sense with Skinner's own historicist position, according to which to understand behavior one must look to its history.

Parsimony: Simplicity Might Be a Good Thing, If We Knew What It Meant

Theorists often invoke parsimony when they describe the virtues of their work. I do not ever recall a theorist proudly proclaiming his or her theory to be complex. But what is parsimony? If a theory involves 6th grade algebra, then we read the claim that elementary algebra is simple and accessible and makes clear predictions consisting of smooth and simple curves. If a theory involves nonlinear differential equations for which one must resort to numerical approximations, then we read the claim that this complexity is more than justified because the theory deals with critical issues of behavior dynamics in a clear and, yes, simple way, considering the spectacularly difficult nature of the problem. And, even if a theory is so transcendently complex that computer simulations can scarcely describe its behavior, then we still encounter the claim that the theory is parsimonious because, even though it is admittedly complex, it can address otherwise entirely inaccessible issues of correspondingly transcendent importance. The common claim in this case is that in the long run, the theory will be seen to be more elegantly simple than cumbersome elaborations originating in simpler assumptions.

Classic examples of this latter type of parsimony include the Copernican heliocentric conception of the solar system, as opposed to the initially simpler Ptolemaic geocentric conception, and within psychology, the hierarchical conception of the structure of memory as opposed to the initially simpler linear conception. In short, parsimony seems to depend on the eye of the scientist and on the historical context. What is simple to one theorist is oversimplified to a second and perversely complicated to still a third. The complexity of simplicity has been explicitly addressed (Harper & Hooker, 1976; Nersessian, 1987; Sober, 1975, 1988). I would expect to see some critics dispute whether Staddon and Higa's theory is appropriately simple: Some might see it as too simple and others as not simple enough.

Truth: Truth Might Be a Good Thing, If It Means More Than Tradition, Uniformity, Standardization, Convention, Conformity, and Strongly Held Opinion

"Truth" can justify, in my opinion, a scientific form of intellectual intolerance when

scientists act as though they believe they know what is true, or as though their beliefs are objective and value free. The neutral objectivity once attributed to science has been challenged by a realization that science is often, and perhaps even always, value laden, implying, among many other things, that a theory may carry with it its own evaluative standards. The history of behavior analysis shows several occasions on which, in my opinion, these two conceptions of science, either as theory laden or as objective and theory neutral, are clearly revealed (Hineline, Silberberg, Zirriax, Timberlake, & Vaughan, 1987; Shimp, 1990; Williams, 1990).

Evaluation can be unaware self-portraiture if a reviewer sees someone else's scientific contribution only through the lens of his or her own perspective. This seems to happen frequently. Authors may be trapped on tilted playing fields when this happens: They have to defend their own theories against reviewers' evaluative standards, which are likely to derive from subtle differences in unstated metatheoretical views between authors and reviewers. I expect a goodly part of the overall commentary of Staddon and Higa's article will consist of indirect measures of the difference between their evaluative standards and theoretical goals on the one hand and those of their reviewers on the other. This is a difficult problem, because a reviewer may believe to be only upholding standards, not inflicting the reviewer's own personal standards on someone else. In any case, it is not uncommon to find a scientist evaluating someone else's theory as though it were a misguided, error-ridden, weak, confused application of his or her own values and standards, rather than as an altogether different approach, or even as an attempt to break away from those very values and standards.

Another issue related to the role of "truth" in evaluation is that of conformity and standardization. I once wrote a theoretical article that I thought had the virtue of developing new and importantly revealing types of data by which the theory could be evaluated. A reviewer, however, denounced these novel approaches on the grounds that once one begins to consider new predictions involving unfamiliar types of data, it is not clear where it all will lead. The criticism was that it was not clear how we could preserve our rigorous

standards if we begin to permit all sorts of novel predictions involving unfamiliar data and requiring unfamiliar evaluative standards. That, of course, was my point, but from the opposite side of the fence. I thought that we should not let a science stagnate on behalf of preserving standards and conformity, which might be in the end, for all we knew, arbitrary and counterproductive. I saw, and still see, this particular review as the scientific equivalent of a culture that suppresses and disparages nonstandard approaches. Some scientists, like some members of society, seem to fear diversity. Interestingly, it has been suggested that one of the prominent characteristics of a science is both that it suppresses novelty and that it has sufficient theoretical depth and clarity that it guides experimental research in ways intuition may find obscure, irrelevant, arcane, or even meaningless (Kuhn, 1970). I would like to suggest that it might be constructive to acknowledge and even to encourage the development of new alternatives. Why not try to develop a science of behavior so that it can benefit from, rather than suffer from, intellectual diversity? In short, I would not be surprised to find that commentary on Staddon and Higa's theory includes suggestions that it fails to address the "correct" data, where "correct" is defined with respect either to currently dominant theories of timing or just plain intuition.

Who Cares If a Reviewer Is Not "Convinced"?

How often has an author read that a reviewer is "not convinced"? This implies that peer review has uncovered some kind of weakness, as in logic, in the degree to which an argument is buttressed by relevant data, and so on. For a reviewer not to be convinced is a horrible thing. But wait! Just how bad is it? Consider two not uncommon cases: (a) A reviewer has worked for years on a theory radically different from the author's, or (b) the reviewer is deeply suspicious of all explicit theory.

In either of these cases, the observation that an author's argument is "not convincing" sounds like a neutral and direct judgment about the author's argument. Perhaps instead, however, it is an indirect means of describing the reviewer's own views. One scarcely ever sees a reviewer acknowledging

what personal criteria are used to determine what is convincing. The art and science of persuasion are extremely complex (Austen, 1818/1972; Myers, 1990; Petty & Cacioppo, 1981). Persuasion may be no clearer as an evaluative tool than simplicity is.

For the reasons I have described, among many others, I am not convinced that it should matter much whether a reviewer is convinced. Do we expect an advocate of one theory to be convinced by another, especially one that might attack or undermine the reviewer's own approach? Do we really believe that reading a paper submitted for publication might convince a reviewer along the following lines? "Oh well, here I have worked all these years to advance the theory of (whatever), and now I see clearly that I was wrong all this time. This new theory convinces me that one of the most important parts of my theory is wrong. Starting tomorrow morning I had better just start all over again from scratch. Thank goodness this new theory convinces me my own theory is really dumb." Perhaps more likely is something like, "What a dumb theory this person X is trying to develop. It's really too bad, and a terrific waste, that theorist X can't see that these are the wrong data, the wrong methods, the wrong analytical tools, the wrong logic, the wrong concept of parsimony, the wrong (whatever)." I would not be surprised to find Staddon and Higa's theory criticized on the grounds that one or more reviewers are not "convinced."

In addition to these two cases in which a reviewer might fail to be convinced, there is a third case that is so common it needs at least passing mention. That is the case in which mechanical hypothesis testing replaces common sense and professional and scientific judgment. In my opinion, more counterproductive nonsense has been written about the objective virtues of falsificationism and hypothesis testing, especially in the context of the use of classical inferential statistics, than in any other situation in which behavioral scientists decide whether a position is "convincing." Fortunately, behavior analysis has regularly drawn attention to this issue, perhaps more so than any other branch of behavioral science, so there is no need to belabor the point here.

Tolerance

In summary, perhaps sometimes it might be fruitful, given the diverse ways good science is conducted, to acknowledge that when we evaluate an article for publication, there are lots of potential problems with the evaluative standards of standardization and conformity, undefined "simplicity," whether a reviewer is "convinced," and "testing" this or that. Here are a few tentative rules of thumb that I suggest might help to promote greater intellectual tolerance in peer review of theory in behavior analysis. To begin with, try to adopt the author's point of view, if possible. Ask if a theory is coherent, imaginative, and rigorous from the author's point of view. Ask about the extent to which a theory integrates empirical phenomena that are otherwise unrelated. Ask if the theory seems to have the potential to be developed, articulated, and generalized. Ask if the theory integrates data that otherwise seem unrelated. Ask if the theory reveals how data that are intuitively unimportant are actually theoretically diagnostic. In short, struggle to see the world from the author's point of view.

There is an intentional ambiguity in the previous paragraph. Who is supposed to adopt these rules of thumb? An author in the process of evaluating a theory other than the author's own? A reviewer? An editor? A reader? My feeling is that there is a need for greater tolerance overall, yet the very tolerance I recommend probably needs to permit authors and reviewers to express strongly held and, in fact, intolerant opinions. The challenge is for the management of peer review, as in editorial decisions about how to handle divisive and controversial opinions, to simultaneously maintain rigorous standards and intellectual tolerance. The editorial challenge is not entirely unlike that which faces a nation wishing to preserve the highest standards of humanity while preserving the rights of individuals to disagree about what those very standards should be.

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TIME WITHOUT CLOCKS

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Staddon and Higa show that the ability to time events derives from principles of memory rather than from an internal device for measuring the duration of events. This insightful timing theory is parsimonious, fits the data, has potential widespread generality, and is evolutionarily plausible.

Key words: timing theory, temporal control, memory, internal clock

After more than 20 years in the limelight, scalar expectancy theory is in trouble. Not only has it sprouted seemingly infinite parameters that make it inelegantly cumbersome, but the embroidery no longer allows it even to predict Weber's law. Staddon and Higa explain why scalar expectancy theory may be neither internally consistent nor even solidly conceptually based.

Maybe this is the ultimate fate of any theory that firmly maintains its essential truth in the face of all data and simply adds what seems necessary to handle discrepancies. Has such a Ptolemaic endeavor ever worked? Perhaps a successful example can be found in the history of science, but none comes to mind. In psychology, Hullian learning theory also finally fell of its own weight, even though a better alternative never appeared. But, sca-

lar expectancy theory has an even more serious problem. Its seemingly endless collection of cycles and epicycles are replaced by a remarkably simple theory that invokes no timing processes at all. Staddon and Higa not only analyze the shortcomings in scalar theory; their far simpler theory explains more data more precisely. This indeed is an exciting advance in our understanding of how animals deal with timing problems.

Scalar theory never was comprehensive. Staddon and Higa mention that the proponents of the theory have ignored the large body of data available on cyclic interval schedules. Scalar theorizing also has ignored most of the published data on temporal differentiation. The shortcoming was evident even in the first scalar timing paper (Gibbon, 1977), and it has not been remedied since. The theory predicted a linear relationship between the duration of a behavior pattern and the time requirement put on that duration, but the only data discussed were the few that

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