THE WHIRLIGIG OF TIME: SOME THOUGHTS ON STADDON AND HIGA

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Staddon and Higa's theory of timing finds analogy with physics' concern with the relativity of time and irreversible processes. Their model raises general issues about the nature and function of models and, specifically, the extent to which it has captured the stimulus events in temporal control.

Key words: memory, models, relativity, timing

> The time is out of joint. O cursed sprite,
> That ever I was born to set it right!

Hamlet's lament may echo many of those researchers who have taken up the task of understanding the nature of time itself. Time as an entity of scientific concern has largely been the province of physics, where time stands as one of the four fundamental dimensions along with length, mass, and charge, whose combinations define virtually all other physical variables. Yet, time remains the most mysterious and contentious of the set. Two major issues have vexed the physicist. Both were raised by Newton who asserted the absolute quality of time, and, moreover, in creating "a system of the world," founded a physics where time had no direction. The absoluteness of time was challenged and resolved by Einstein, who showed that measures of time were dependent on states of motion. The second, that of understanding nature's ubiquitous irreversible processes in the face of a temporally reversible mechanics, is still a hotly debated issue (e.g., Coveney & Highfield, 1990; Davies, 1995; Price, 1996). The relativity of time and temporal irreversibility are both touched by the contributions of behavior analysts and others to the psychophysics of temporal control. Staddon and Higa's article reflects this in ways I will try to point out.

The relationship of psychophysics to relativity theory is only metaphorical but I think conceptually useful. Let us confine ourselves to special relativity for the sake of simplicity, although that is not a requirement. Special relativity is founded on only two postulates: (a) The speed of light is a constant independent of the uniform motion of the observer, and (b) the laws of physics hold independently of the uniform motion of the observer (general relativity extended these postulates to nonuniform motion). From these two postulates, the Lorenz transformation can be derived, which yields values of fundamental units of length, mass, and time for observers moving relative to each other. For example, the time t' in one inertial frame, K', can be determined from another K, at any time t, by a multiplicative factor that depends on the relative velocity of the frames. The scale with velocity is essentially hyperbolic. A key point here is that there is no privileged frame. Assuming each has a proper clock, both have the correct time relative to their frame of reference, and there is no "true" time outside to serve as an absolute standard. The same, of course, is true of the other fundamental measures.

Psychophysics and extensions into the general domain of temporal discrimination and differentiation deal with the question of what transformation rules apply to carry us from the experimenter's frame to the subject's frame and back. Another way of looking at this question is how does the subject scale the variables thrown at it by the experimenter. Although experimenters may think they have a privileged frame, they do not; they simply have another scale, and often a crude one at that. The term *psychophysics* has implied to some the description of a field devoted to understanding the relationships between the "physical" world and a "mental" world, and thus is reflective of a dualist position even if unintended. But all we are concerned with

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here is physical in the sense that the organisms we study are as physical as the clocks we study them with. Moreover, perhaps by determining the transformation rules between one mechanism and another, we might understand how each works. In the typical experimental arrangement, we take for granted one scale or mechanism, say, a laboratory clock, and use it as a calibrated gauge to study the scaling and possible mechanism of our experimental subject.

Now, one may assume that because we use and understand the mechanism of our gauge or standard, the experimental organism possesses a similar gauge that it then uses to transform our imposed values into its own. In other words, if we impose a temporal contingency, discrimination or differentiation, upon an organism, it must then consult its own clock and report back its readings to us. Thus begins the search for the internal clock. It is easy to see why the notion of an internal clock is so seductive. The construct is a sort of copy theory in the same way we might attempt to understand how we see by putting a picture of the world in the head that we then look at from the inside. Similarly, to understand how the behavior of an organism might come under control of a putative temporal contingency, theories like those of Church and Gibbon (e. g., Church, 1989) endow the creature with an internal clock. The research program is then devoted to understanding the properties of this clock. With this commitment, the clock must require a host of other constructs to make it work: a gate, an accumulator, and an interplay between a reference memory and a working memory system through a comparator. Despite this rococo structure, we are still unable to determine how the clock or any of the other constructs are activated or controlled. How is the clock read? How does this system instigate behavior? One might build a program or device with all these features, including the missing functions, but this kind of model has no particular biological reference or significance. More significantly, as Staddon and Higa argue masterfully, the model does not consistently work. All the Ptolemaic epicyclic fiddling to make the model fit only diminishes its credibility.

In the absence of this fiddling, the model does not generate the proper transformation rules. Analogous with relativity theory, what these rules are depends on the contingencies imposed and the contexts in which they operate, as Zeiler (in press) has emphasized. This is not surprising. For example, in temporal differentiation, one tries by various procedures to make the organism into a clock; in temporal discrimination, one tries by various procedures to see how good a clock the organism can be. Is it any wonder the scaling is not comparable? Adding more and more internal clocks to account for the varieties of findings is clearly not a satisfactory strategy.

An internal clock is not per se implausible. There is substantial evidence for several such clocks to help account for various infradian, circadian, and ultradian cycles. The clocks themselves appear to result from dynamic limit cycles in protein synthesis that, in turn, control neural or hormonal activity. At least some of these clocks may be entrained by external stimulation (*zeitgebers*), showing that different rhythms or phases of the same clock may be generated depending on the environment. A shift in sleep time with significant changes in time zones is a familiar example. Those who advocate an internal clock to account for the effects of a temporal contingency could view such a contingency as a kind of zeitgeber that entrains behavior to conform more or less to the ongoing requirements. One metaphor is resonance as we tune a capacitor in a radio LC circuit to receive a desired frequency. Another metaphor is a coupled pendulum system wherein the motion of one pendulum entrains the motion of another. How useful such metaphors might be for developing models of timing remains to be seen.

Staddon and Higa provide a model based on an RC circuit called an integrator (also known as a low-pass filter). Equivalent circuits in neurons and synaptic junctions control neural integration. As the name implies, inputs to such a circuit are integrated via a charging capacitor. The output depends on the shape and frequency of the input signal and on the time constant of the capacitor. These devices may be cascaded using capacitors with different time constants to yield the sort of properties Staddon and Higa require to conform to some available data. This technique is, of course, another kind of fiddling to make things fit. If the data do not fit with

n such units, then make it $n + 1$. This, of course, piles parameter upon parameter. At least, Staddon and Higa might argue, their model only comprises one *kind* of device. This limits the model's applicability as they acknowledge. For example, the model cannot account for what surely is the most interesting property of performance under fixed-interval (FI) schedules—the temporal patterning. I would be curious how Staddon and Higa might approach the scale-invariant properties of fixed-interval patterning shown by Dews (1970), or his "Cheshire cat" phenomenon using multiple S^{Δ}s (Dews, 1962). There was also no treatment of interresponse-time (IRT) schedules, either properties of IRT distributions or the power law relations between IRT requirements and emitted IRTs (see, e.g., Zeiler, 1979). With schedules like IRT $> t$, reinforcers occur aperiodically, and thus a leaky integrative system like the one proposed would have to be modified to include a response memory. Also with IRT $> t$ and FI t, as well as many other contingencies, models will have to deal with variations in responding controlled or induced by the dynamic interplay between patterns of behavior and patterns of reinforcement.

The role of a memory system touches the topic of temporal irreversibility, the second contentious issue mentioned above in the attempts to understand the nature of time. Staddon and Higa do not waste much time, however, on the most salient manifestation of irreversibility, namely learning or acquisition, but rather assume all that has already taken place. The Staddon and Higa model is based on a "memory-as-stimulus" principle; in other words, salient events produce a temporary state in the organism that varies with time. Values of that state constitute a stimulus that controls when responding will occur. Conceptually, this sort of model suffers from the same problems encountered by its alternatives, namely how do states, traces, counts, and so forth, become behavior? Perhaps the question is not quite fair, because we rarely have the answer to this question regarding any physiological process. Nevertheless, in this model, there is no particular push for physiological plausibility. The issue here is whether an analytical account requires a commitment to any particular picture, for example, a fantasy physiology. Indeed, the advantage of a mathematical account is that, once developed, it may be relatively free of pictures. Maxwell's equations, once formulated, no longer depended on Faraday's lines of force or Maxwell's own hydrodynamical ether machine (see, e.g., Marr, 1993). What is essential to a successful model is a pattern of functional relations that properly encompasses the measurable variables of interest. A picture may be but a heuristic crutch, and a dangerous one at that. Such pictures are all too often taken literally.

I mentioned earlier the possibility of building a device as proposed in the Church and Gibbon account. So one might with the Staddon and Higa model. This reflects the engineering aspect of modeling; that is, the model may be instantiated in an actual device that displays the needed properties. The model itself is, of course, derived from known properties of such devices. Many different models may be analytically equivalent, so in the absence of understanding the real organism, perhaps one should be free to use any device that works. But such an approach could not be considered organism based, in the sense that Staddon has advocated in earlier papers (Marr, 1993; Staddon, 1993, 1997).

I am unclear as to the nature of the stimulus that controls behavior in the Staddon and Higa model. Indeed, the long history of the behavior analysis of timing has not provided much enlightenment on this issue, regardless of theory. Catania (1970), in his classic paper on timing, asserted that "Duration, like frequency, intensity, or spatial extent, is a discriminable property of stimuli" (p. 36). But what sort of feature is duration? And what of time itself? If the only property of time is that it has duration, then this leads us to the pointless conclusion that the only property of time is time. Years ago, the philosopher Jack Smart argued the illusory character of the "temporal stream." How fast does it flow? Presumably one second per second! Moreover, as the physicist David Park shows, it is impossible to perform an experiment demonstrating the passage of time (for a discussion of these points, see Davies, 1995). Relativity teaches us that we dwell in a space–time continuum; time no more flows than does length. Time can only be keyed by events. In Emily Dickinson's words, "Forever—is composed of Nows." Less elegantly, time is just one damn thing after another. Control via time is control by events, including behavior itself. A clock is a generator ("tick-tock") or marker ("October") of events. Both the Church–Gibbon 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

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

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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, Nietsche, 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 transcendentally 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, Ziriax, 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, scalar 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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fit the prediction. Most of the data showed that the relation actually was described by a fractional-exponent power function (exponents between 0.5 and 0.8 rather than the predicted 1.0), whether the standard was taken as the duration requirement or the durations that actually were followed by the reinforcing stimulus.

Staddon and Higa have accomplished such a perceptive critical analysis of scalar expectancy theory that nothing more need be said. My own comments begin with a reflection on the nature of contemporary behavior theory. Both the Staddon–Higa memory model and internal clock models share an emphasis on what animals bring with them in interacting with their environment, even though they differ in what these processes may be. Hypothesized properties of the animal interact with environmental demands to determine behavior. Behavior-analytic theory has often scrupulously avoided processes internal to the animal. The importance of history in influencing current behavior is well recognized, but history is treated as effects of variables imposed on the animals in the past rather than as those events filtered through an animal. In that type of theory, animals become vehicles for displaying how variables exert their effects on behavior rather than being the processors of environmental events. From the vehicle perspective, invoking either memorial processes or internal clocks is meaningless in explaining behavior. Temporal regularities in behavior are attributed to temporal regularities in the environment, not to the measurement of those temporal events by an internal clock and the translation of those readings into action. Nobody attributes the regularity of a pendulum to the pendulum's time sense or to its memory, so why attribute the temporal regularity of an animal's behavior either to a time measuring device or to a memory process?

But explaining behavior patterns solely as the outcome of the present and past environmental events that result in their appearance is to treat behavior as a purely physical system that is divorced from biology. No biological activity can be understood just by citing the environmental conditions under which it occurs. Explanations require a full understanding of what animals bring with them in their dealings with the environment. The importance of the animal is highlighted in the theory of evolution by natural selection, where animals inherit processes that have enabled their species to adapt to the environment. A biological approach to behavior treats the environment as the poser of adaptive problems that the animal solves with its internal resources. Environments occasion behavior; they do not produce it by themselves.

Memory decay occurs in the animal, not in the environment. As such, it is an inferred process. As a start in identifying its properties, Staddon and Higa draw on the characteristics of habituation, which is defined behaviorally as the waning of reflexive responding to the same stimulus when it is presented repeatedly. Their model to explain habituation, and thereby to explain memory decay in general, invokes other internal processes like reflex strength, a leaky integrator, and memory-trace strength, all tied together into a relatively simple and straightforward quantitative model. They go on to extend their approach to a multiple-time-scale model that follows the same principles. The result of even their preliminary efforts is the ability to integrate a considerable range of phenomena with this plausible model of event memory.

Why is this model any better than one based on an internal clock? One reason is that the memory model pulls together what would appear to be unrelated data without any need to introduce a novel concept such as an internal clock. The internal clock concept has been invoked only in the context of temporal control, whereas the memory model has much broader scope. That it can explain the timing data without reference to an internal time measuring device is both unexpected and impressive. Another reason is that the memory model contains intrinsic non-ad hoc principles for explaining why timing, as well as other behavioral phenomena, should differ substantially depending on the precise conditions under which it is studied. Theories based on internal clocks, which have largely ignored such data, probably could deal with these experimental results only by invoking arbitrary fitting parameters. A third reason is that consideration of the evidence suggests that organisms have not evolved with an internal clock that they use to judge time. The remainder of this paper deals with that issue.

An internal clock presumably records the duration of events and allows behavior to adjust to those durations. The focus of the Staddon–Higa paper is on interval timing, that is, on the animal's ability to judge the duration of a stimulus or to estimate the time between successive events (Carr & Wilkie, 1997). Evolution probably did not have our laboratory procedures in mind when designing a clock to handle interval timing. Evolution had no way of ''knowing'' what events would have to be timed and what the specific intervals might be. So, if the laboratory taps into abilities and processes that influenced survival and reproductive success in the history of the species, our procedures must invoke interval timing processes that evolved for handling other situations in which the events to be timed were essentially arbitrary and thereby unpredictable from one individual to the next. If the same clock services all or even many of these potential situations, it must operate independently of the particular events being timed. An alternative is that the clock is domain specific rather than domain general. If that is the case, theories based on a general-purpose internal clock are simply inadequate to cope with reality and at best must be qualified in terms of the situations to which they apply. Existing theories based on internal clocks seem to opt for enough domain generality as to make them blind to the possible need for such qualifications. The consequence is that consistency in the property of the internal clock should appear across different situations.

The following data are discussed in detail elsewhere (Zeiler, in press), so are only summarized here. Temporal differentiation requirements have been applied to different kinds of behavior. In every case, the particular response or sequence produced different conclusions about temporal differentiation, and different temporal properties of the same response yielded different conclusions as well. A similar divergence in characteristics of timing has been seen when different aspects of behavior are considered in the peak procedure. That is not all. Most experiments have shown that the properties of timing in temporal differentiation did not correspond with those seen in temporal discrimination. This was also the case with differentiation and discrimination versions of the temporal bisection procedure. When the durations to be bisected were raised to a power such that these power means matched the bisection point, the majority of discrimination procedures yielded positive exponents, but differentiation procedures yielded negative exponents. Experiments on both temporal differentiation and temporal discrimination have compared behavior in closed and open feeding economies. In either case, properties of timing varied considerably, and functions even reversed their direction depending on the particular feeding economy. For example, Weber fractions rose with longer time requirements in the open economy, but they decreased with longer requirements in the closed. All of these results sound more like domain specificity than an evolved interval timing system driven by a general-purpose internal clock.

Such observations fit the hypothesis that a general-purpose mechanism for dealing with interval timing has not evolved or, if ever present, did not survive over the course of biological evolution. Why should such a mechanism have evolved? It is not easy to find examples of a serious need for interval timing in the everyday life of humans, and it is even harder to come up with examples of interval timing in other species. Basic biological functioning does not require animals to keep track of time, because other stimuli are available to indicate when to eat, when to sleep, when to wake up, when to mate, when to avoid predators, or when to tend to offspring. The situation is quite different for humans, but that is because of their unique dependence on certain types of social interactions. People need to know about time in order to coordinate their behavior with others, yet people do not do very well in meeting their temporal needs without the support of external mechanical or electronic timekeeping devices. ''Without [a common language of time measurement] and without general access to instruments accurate enough to provide uniform indications of location in time, urban life and civilization as we know it would be impossible. Just about everything we do depends in some way on going and coming, meeting and parting'' (Landes, 1983, p. 2). But even social animals like chimpanzees and gorillas do not seem to schedule meetings or meet their children based on temporal considerations. Concern with time is distinctly human, and the necessary internal devices for meeting such demands are either nonexistent or are grossly inadequate. Astronomers and navigators always needed to know about time, but their own inherent resources could not do the job satisfactorily. If we could deal with time adequately without them, watches and clocks would not be so important to us.

I am suggesting that an internal clock for judging and measuring time never evolved at all. Maybe mechanisms for judging time intervals never existed to be selected and refined, or maybe an incipient internal clock was uneconomical because it served no important purpose. So how can the animals in our experiments display such lawful behavior when subjected to temporal demands? What Staddon and Higa have shown so eloquently and rigorously is that a clock for interval timing is unnecessary for animals to show the kind of behavior that others have interpreted as indicative of control by time. The general processes of memory are sufficiently flexible and powerful to produce such apparent sensitivity without reference to any sort of specialized timing system. This is a major breakthrough in our understanding of how behavior comes under temporal control.

Staddon and Higa have taken a big step forward in the direction of an economical and general theory of operant behavior. Their ability to deal with timing without an internal clock may well lead to a comprehensive domain-general theory of adaptive behavior and thereby does not require specialized processes for each situation. The future is bright, albeit clockless.

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