

*EVALUATION OF QUANTITATIVE
THEORIES OF TIMING*

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Scalar timing theory is a clear, complete, modular, and precise theory of timing that explains much of the data from many timing procedures, but not all of the data from all of the procedures. The multiple-time-scale theory of timing provides an alternative representation of time that has not yet been tested with respect to its fit to timing data.

Key words: time perception, timing, scalar timing theory, scalar expectancy theory, pacemaker

The article by Staddon and Higa provides a criticism of scalar expectancy theory (SET) and presents an alternative multiple-time-scale (MTS) theory of timing.

Criticisms of Scalar Expectancy Theory

The authors report that “in terms of citations and numbers of published papers, SET is by far the most popular theory of interval timing” (p. 215). This may partly be an historical accident of having been developed earlier than others. Once a theory has been announced and found to be useful, alternative theories are held to the higher standard of being demonstrably better. Four particularly attractive features of SET are that it is clear, complete, precise, and modular (i.e., it consists of separable parts). Realistic alternative theories of time will undoubtedly also have these features.

A distinction should be made between the underlying formal model of scalar timing theory developed in the 1970s by Gibbon (1971, 1972, 1977) and the information-processing interpretation of this process developed in the 1980s (Church & Gibbon, 1982; Gibbon & Church, 1981, 1984; Gibbon, Church, & Meck, 1984). The criticisms of Staddon and Higa apply to the information-processing interpretation of scalar timing theory, particularly its representation of time.

In the information-processing interpretation of scalar timing theory, the assumption

was made that psychological time is a single function that changes in some regular way with physical time. This function was assumed to be produced by a pacemaker that emitted pulses at some rate and distribution form that were summed in an accumulator (Gibbon et al., 1984). Although a random emitter and a fixed emitter without variability would not, as the sole source of variance, produce the Weber-law property, and a fixed emitter without variability was far too regular, multiplicative sources of variance in memory storage and decision threshold produced the Weber law property of interval timing (Gibbon, 1992). One of the strengths of SET was that it did not require a particular distribution form of the interpulse interval to fit data functions that had the Weber law property. Staddon and Higa’s conclusion that pacemaker-accumulator mechanisms “are fundamentally at odds with the Weber law property of interval timing” (p. 215) suggests that no distribution of interpulse intervals has a constant coefficient of variation (a constant ratio of standard deviation to mean number of pulses at different times), but the Rayleigh distribution has this characteristic (Reid & Allen, 1998), as well as a Poisson or fixed emitter with a variable rate, either between or within trials (Gibbon, 1992). To distinguish between different assumptions about interpulse distributions requires data from extensive psychometric studies with very short time intervals (Fetterman & Killeen, 1995).

In their discussion of the pacemaker, Staddon and Higa gave the impression that the development of SET proceeded as follows: (a) A Poisson pacemaker was assumed, (b) it was found to be incompatible with Weber’s law, and (c) the deficiency was corrected by

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adding additional assumptions about memory and decision processes. This is historically incorrect. The development was that (a) an extensive review of the behavioral data supported Weber's law for timing (Gibbon, 1977), and subsequently, (b) an information-processing analysis of scalar timing theory with a Poisson pacemaker, and other distributions, was found to account quantitatively for the data (Church & Gibbon, 1982; Gibbon, 1991; Gibbon & Church, 1984; Gibbon et al., 1984).

Staddon and Higa conclude that the psychological representation of time is approximately a logarithmic, rather than a linear, function of time. This provides part of an explanation of the form of the psychophysical bisection function which is quite symmetrical when the duration of the stimulus is plotted in logarithmic units (e.g., Church & Deluty, 1977). Church and Deluty described a model containing three parts (clock, criterion, and response rule) and considered three functional relationships between physical and psychological time (linear, logarithmic, and reciprocal). They concluded that the simplest explanation of these data was the logarithmic representation of time, although they explicitly recognized that an alternative response rule could be consistent with a linear representation of time. One problem, however, was to resolve why the function relating mean response rate to time in the peak procedure is a fairly symmetrical function. In their first collaborative experiment, Gibbon and Church (1981) attempted to find a single function that would account for both the approximate symmetry of the psychophysical function on a logarithmic axis and the approximate symmetry of the peak function on a linear axis. Staddon and Higa assume that animals use the logarithmic function in the bisection procedure but the inverse function (antilog) in the peak procedure. This dual code may produce some conflicts in a bisection procedure in which the two manipulanda are always available (Platt & Davis, 1983). Can an animal use a linear representation of time to stop responding on one lever and start responding on the other, and a logarithmic representation of the time to decide when to switch between the levers?

In their discussion of the psychological representation of time, Staddon and Higa gave

the impression that the development of SET proceeded as follows: (a) A linear representation of time was assumed, (b) it was found to be incompatible with the psychophysical function relating the probability of a long response to stimulus duration, and (c) the deficiency was corrected by adding additional assumptions about memory and decision processes. This is historically incorrect. The development was that (a) in the initial development of an information-processing interpretation of scalar timing theory, there was no satisfactory basis to determine whether the psychological representation of time was linearly or logarithmically related to physical time, (b) the time-left procedure was designed to force the animal to do an operation with its psychological time scale before using it in behavior, (c) clear quantitative results of several time-left experiments were consistent with a linear representation of time, but not with a logarithmic representation of time (Gibbon & Church, 1981).

In their discussion of the analysis of individual trials of the peak procedure, Staddon and Higa gave another example that suggested that the development of SET proceeded as follows: (a) An assumption was made, (b) it was invalidated by data, so (c) a new feature was added to the theory to correct the flaw. They report that, in the peak procedure, (a) an initial prediction of SET was that the later an animal began responding the earlier it would stop, (b) the data showed that the later an animal began responding the later it would stop, therefore (c) SET was modified to correct the flaw. In fact, the purpose of the individual-trials analysis was to decompose the sources of variance from clock, memory, and decision processes based on a quantitative analysis of individual trials (Church, Meck, & Gibbon, 1994; Gibbon & Church, 1992). Previous analyses using mean response functions had emphasized the importance of memory variance, which would lead to the observed positive correlations between the times that an animal starts and stops responding in the peak and temporal generalization procedures (Church & Gibbon, 1982; Gibbon & Church, 1984; Gibbon et al., 1984).

Because scalar timing theory is clear, modular, and precise, it is possible to test the predictions of the theory with different experimental procedures and different dependent

measures. In some cases, the predictions have been outstandingly close to the data; in some cases they are obviously wrong; in some cases there are small but systematic differences between the predictions and the data. The failures to fit, even more than the successes, provide the impetus for theoretical development.

One example of a failure is that scalar timing theory (as described by Gibbon et al., 1984) makes no provision for extinction and various related phenomena. Another problem is that it does not account for the behavior in variable-interval schedules of reinforcement without changes that pertain only to some procedures (Brunner, Fairhurst, Stolovitzky, & Gibbon, 1997). A theory of the animal should apply to all procedures, or a procedure-classifier module should be added to the theory. These problems may not be present in vector memory representations used in some well-specified timing theories (Grossberg & Schmajuk, 1989; Machado, 1997), rather than the distribution memory representation used in scalar timing theory.

Examples of small but systematic differences between the predictions of scalar timing theory and the data have been reported in animal experiments (Church, Lacourse, & Crystal, 1998; Crystal, 1999; Crystal, Church, & Broadbent, 1997) and in human experiments (Collyer, Broadbent, & Church, 1992, 1994; Collyer & Church, 1998). These systematic effects may reveal a mechanism that produces a psychological representation of time that is approximately linear but that has local maxima and minima. A multiple-oscillator model of timing (Church & Broadbent, 1990) has provided some qualitative fits.

A Multiple-Time-Scale Theory of Timing

The phrase “theory of timing” is sometimes reserved for quantitative theories that are fully specified such that two independent investigators can apply the theory to fit the results produced by any procedure in the domain of the theory and obtain the same predictions. In order to predict behavior, a timing theory must include (a) a representation of the physical time since the occurrence of an event, (b) a memory of the time of reinforcement, and (c) a response rule.

The phrase “theory of timing” has also been used to refer to a set of ideas or theory fragments that may be worthy of consider-

ation. The primary idea of MTS is that time is represented as the output of a series of cascaded habituation units. The necessary and sufficient conditions for a function to be “pacemaker free” were not described, but the term may refer to a function that is continuous or nonlinear, or one that has multiple inflection points. Real-time conditioning models (such as that of Sutton & Barto, 1981) made use of stimulus traces as the representation of physical time. More recent versions of real-time conditioning models have used a cascade of stimulus traces (Moore & Choi, 1998). Cascades of functions have also been used in the spectral theory of timing (Grossberg & Schmajuk, 1989) and in a version of the behavioral theory of timing (Machado, 1997); parallel coding of multiple periodic functions was used in the multiple-oscillator theory of timing (Church & Broadbent, 1990). In these cases, physical time is coded as a vector of values of multiple functions that change in some regular way with physical time.

Without an explicit proposal about memory storage, memory retrieval, and decision processes, it is not clear the sense in which the MTS theory of interval timing can account for data from many different time-related experiments. Although the article contains 21 equations, there is no quantitative comparison of observed with predicted behavior. None of the 13 figures provides visual evidence of a correspondence between observed and predicted behavior. One approach would be to substitute the output of a series of cascaded habituation units for a pacemaker-accumulator system in scalar timing theory; that is, to change the perceptual representation of time without changing assumptions about memory storage or retrieval or the decision rule. This would provide a basis for a quantitative comparison of the two models (Church, 1997).

Staddon and Higa’s critique of SET suggests that it may be possible to develop a timing theory that is simpler, is applicable to a wider range of procedures, is connected more firmly to the biological basis of behavior, and makes more accurate predictions. In my judgment, such a theory will, like SET, be clear, complete, modular, and precise.

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TIMING WITHOUT A TIMER

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Inferred-process theories propose various internal timing mechanisms to provide discriminative stimuli for temporal control of behavior. Biobehaviorally informed computer simulations indicate that timing may emerge from the action of reinforcement on neural circuits without the need to postulate timers of any sort.

Key words: timing, neural networks, neuroscience, fixed-interval schedule

Three separable aspects of the Staddon-Higa article may be distinguished: (a) a general critique of accounts of temporal regularities in behavior that appeal to inferred entities such as “internal clocks” or “pacemakers,” (b) a proposal for an alternative approach to temporal control—the multiple-time-scale (MTS) model (Staddon & Higa, 1996), and (c) a comparison of MTS with a particular inferred-process theory—scalar expectancy theory (SET) (Gibbon, 1977; Gibbon & Church, 1990). The first and second aspects of the article are addressed in this commentary. (As a preliminary matter, we use the phrase *temporal control* as shorthand for an orderly relation between behavior and time measured from some event. Clearly, time itself is not a stimulus. There is no environmental or neural event that uniquely and uniformly covaries with time from an *arbitrary* stimulus and, hence, no possibility that natural selection could have produced a neural system for its detection.)

Inferred-Process Theories

Staddon and Higa reiterate the behavior-analytic critique of attempts to explain regularities in behavior by invoking structures and

processes that are based solely on inferences from behavior. In the case of SET, functional relations between behavioral measures (rate and choice) and time from the onsets of stimuli serve as the basis for inferring that behavior is controlled by the output of an internal pacemaker. The recourse to inferred processes is a hallmark of cognitive theorizing generally (e.g., Atkinson & Shiffrin, 1968), and SET fits squarely within that tradition. In contrast, behavior analysis regards theories employing inferred processes as “having all the virtues of theft over honest toil,” as Bertrand Russell remarked in another context. Staddon and Higa note that theories of this sort postulate that which they seek to explain. Such theories endow the organism with the very capabilities that are needed to encompass the relation in question. In so doing, inferred-process theories embrace an essentialist view of science (Palmer & Donahoe, 1992) that impedes genuine explanation and that inadvertently encourages circular reasoning and the nominal fallacy (Donahoe & Palmer, 1994, pp. 9, 152; Skinner, 1938).

Temporal control presents special temptations to inferred processes: Behavior changes in an orderly fashion in the absence of a correlated change in the external environment. Consider responding during a fixed-interval schedule. Following the reinforcing stimulus, it is reasonable to regard responding as under the control of the discriminative effects of the reinforcer because food never occurs during this period. The sequence of events instantiates a three-term contingency that is known to establish a stimulus as discriminative for infrequent responding. This account

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of the postreinforcement pause in fixed-interval schedules meets the criteria for scientific interpretation: *Independent* evidence demonstrates that responding declines in frequency under conditions that are satisfied by the interpreted situation (see Donahoe & Palmer, 1994, pp. 126–129). Moreover, the interpretation can be experimentally evaluated because the event of concern—the reinforcing stimulus—can be manipulated. For example, the reinforcer can be introduced as a probe stimulus into the fixed interval at a time when responding would otherwise be substantial. Under these circumstances, the reinforcing stimulus brings responding to a halt, thereby documenting its discriminative effects. (Reinforcing stimuli have other than discriminative effects, but this complication does not detract from the point being made here; cf. Papini & Hollingsworth, 1998.)

Responses that occur somewhat later in the fixed interval present quite a different challenge. Independent experimental analysis does not indicate, in general, that putative discriminative stimuli can acquire control over responses when the two events are separated by such long intervals. Moreover, the time between these later responses and the subsequent reinforcer is too long for the behavior to be maintained by that reinforcer. Again, independent experimental analysis does not confirm, in general, that behavior can be strengthened by putative reinforcers that occur after such intervals (but see Lattal & Gleeson, 1990). The interpretive challenge is complicated further by the fact that environmental stimuli within the test chamber remain constant throughout the fixed interval. These invariant stimuli cannot support changes in responding in any obvious fashion (cf. Donahoe, Palmer, & Burgos, 1997; Shull, 1995). The question remains, how can we interpret behavior that covaries with time in a constant environment?

Two complementary strategies for addressing temporal control are consistent with behavior analysis: (a) The behavior that occurs in the situation of interest may be further analyzed in that situation. (b) A subset of the events that occur in the original situation may be analyzed in other situations. In either case, the additional information is then used to interpret behavior in the original situation (see Donahoe, 1993). As an illustration of the first

strategy, other characteristics of the operant can be measured, such as interresponse times (e.g., Anger, 1956; Williams, 1968). These measures reveal the discriminative effects of stimuli that are occasioned by immediately preceding responses (Morse, 1966). As another example, responses in addition to the operant may be concurrently measured during the interval, responses such as the adjunctive behavior of drinking (Roper, 1978) or general activity (Killeen, Hanson, & Osborne, 1978; see also Domjan, 1994; Silva, Timberlake, & Cevik, 1998). Temporally constrained adjunctive behavior is a potential source of immediately preceding stimuli that can exert discriminative control over the operant. Indeed, such stimuli have been exploited in the interpretation of temporal control by the behavioral theory of timing (BeT) (Killeen & Fetterman, 1988) and related formulations (Machado, 1997). Staddon and Higa ironically remark that interpretations based on the discriminative effects of adjunctive behavior address the temporal problem “in a less ad hoc (and more easily disprovable) way” (p. 215) than models such as SET. We prefer to characterize such behavioral models as incomplete rather than incorrect. Scientific interpretations can never be wholly wrong if they are based on valid experimental analysis. Indeed, this is the great advantage of scientific interpretations over inferred-process theories such as SET and, we believe, MTS as well.

The second strategy for understanding temporal control is to identify possible variables that may have acted in the original situation and then to subject them to experimental analysis in a different situation. The new observations may be made at any level—behavioral, neural, cellular, biochemical—without violating the basic strategy. At the behavioral level, it is often assumed that some stimulus, such as an “aftereffect” of the reinforcer, changes progressively during the fixed interval and that the operant is controlled by this aftereffect in proportion to its magnitude (cf. Capaldi, 1966). To determine whether events correlated with time since a reinforcer can serve a discriminative function, Ferster and Skinner (1957) introduced into a fixed-interval schedule an environmental stimulus whose characteristics varied with time since the preceding reinforcer. To wit,

the response key grew brighter as time passed. When the values of this stimulus (called an added clock) were later manipulated, responding changed in a manner that was consistent with the presumed role of the unobserved stimuli within a standard fixed-interval schedule. (This finding is a necessary but not a sufficient condition for the proposition that otherwise comparable, but unobserved, stimuli are discriminated in fixed-interval schedules without added clocks.) As a second behavioral example, a stimulus of either of two durations may be presented followed by a two-choice concurrent schedule in which the reinforced component is a function of the duration of the first stimulus. To wit, after a short stimulus duration on a middle key, pecking the left concurrent key is reinforced, whereas after a long duration, pecking the right key is reinforced. If the duration of the first stimulus is correlated with distinctive stimuli that change with time, then those stimuli should acquire control over concurrent responding, as, indeed, they do (e.g., Rilling, 1967). The formation of a concurrent temporal discrimination under these circumstances is consistent with the interpretation of fixed-interval performance as a product of control by unobserved stimuli that vary with the passage of time.

Neural processes are particularly likely to be invoked in discussions of temporal control. Because the external environment is constant, internal time-dependent stimuli are proposed to serve as the controlling stimuli. SET appeals to internal clocks, and even BeT relies on a pacemaker to coordinate transitions between behavioral states. "For our characterization of the pacemaker we are immediately driven inside the organism. . . . The pacemaker itself is almost certainly a biological oscillator" (Killeen & Fetterman, 1988, p. 289). The tactic of moving to a finer level of analysis when the explanatory variance at the behavioral level has been exhausted is consistent with the behavior-analytic approach:

The physiologist of the future will tell us all that can be known about what is happening inside the behaving organism. His account will be an important advance over a behavioral analysis, because the latter is necessarily "historical"—that is to say, it is confined to functional relations showing temporal gaps. . . . What [the physiologist] discovers cannot in-

validate the laws of a science of behavior, but it will make the picture of human action more nearly complete. (Skinner, 1974, pp. 236–237)

And, commenting on the interval "between the stimulating action of the environment and the response of the organism, . . . only brain science can fill these gaps. In doing so, it completes the account; it does not give a different account of the same thing" (Skinner, 1953, p. 18; see also Michael, 1998, p. 160).

Staddon and Higa criticize the notions of timers and pacemakers (rightly so in our view) but then introduce in their place a series of hypothetical units with decreasing rates of habituation. According to MTS, temporal discriminations are dependent on comparisons between a "long-term memory" of the output of this cascade—stored at times when the operant was reinforced in the past—and the present value of the output ("short-term memory"). If the difference between the two memories is less than some threshold value (i.e., if the present time is similar to past times when the response was reinforced), then the operant is emitted. In evaluating MTS and other proposals, recall that an account meets the demands of scientific interpretation only if it draws upon events and processes that have been subjected to independent experimental analysis. What experimental evidence at the neural level substantiates the existence of clocks, pacemakers, and habituation units (or systems of such units) that operate in the behaviorally relevant range of seconds and minutes? To our knowledge, none exists. And others concur: "There are no physiological data that support the existence of pacemakers and counters processing temporal information on the order of [even] hundreds of milliseconds" (Buonomano & Merzenich, 1997, p. 139). The habituation units of MTS are no more the product of independent experimental analysis than are the pacemakers of SET. Habituation units are pacemakers under another name.¹ Thus, it is possible to

¹ The reference cited in support of biological pacemakers (Spitzer & Sejnowski, 1997) does not appear to be relevant to timing mechanisms of the type proposed in SET and MTS. The finding that units "deeper" in the brain are less likely to be activated as the time between stimulus presentations decreases (Williamson, described

agree with Staddon and Higa (p. 245) that “the jury is still out on whether [pacemaker] theories or the MTS theory have the stronger claim to biological plausibility” without endorsing either theory. Of course, circuits have been identified that respond differentially to very small differences in the time of arrival of biologically important stimuli within the same modality, such as those that mediate echolocation in bats (see Carr, 1993). Also, neural circuits have been identified that mediate circadian rhythms, although these are strongly entrained by environmental stimuli (see Rusak, Abe, Mason, Piggons, & Yang, 1993). Finally, circuits have been identified that temporally coordinate repetitive motor patterns, such as the tongue movements that accompany drinking (see Marder & Calabrese, 1996). Note, however, that all of these circuits mediate temporal relations between events that are biologically important over the evolutionary history of the species. None of these circuits mediates temporal control by *arbitrary* stimuli over the time ranges that are encountered in behavioral experiments. The remainder of this commentary describes an approach to temporal control that is more nearly consistent with the demands of experimental analysis.

Temporal Control as an Emergent Effect of Reinforcement

Biobehaviorally informed computer simulations indicate that behavior that covaries

in Glanz, 1998) is consistent with MTS, but it can be accommodated without recourse to progressively more slowly habituating units—an assumption for which there is no direct experimental evidence. There is direct evidence, however, that all neurons have a refractory period after firing, and this fact is sufficient to account for the findings as presented. Let p be the probability that a neuron is in a state in which it can be activated by its inputs (i.e., not in a refractory period) and let there be n neurons in a series. In order for the n th neuron in the series to be activated by stimulating the first neuron, all neurons in the chain must become activated. Thus, the probability that the n th neuron is not activated (p_{na}) is $1 - p^n$ (ignoring complications such as spontaneous firing and parallel processing, neither of which affect the basic point). The probability p_{na} decreases as a function of n and as a function of p , which in turn decreases as the rate of stimulation increases. (Given any fixed time interval, the proportion of that interval in which an element is in a refractory period increases as the rate of stimulation increases.) In short, the finding is equally consistent with a series of units all of which have the *same* habituation rate.

with time in a constant environment can emerge from the action of reinforcement on neural networks without invoking timers, pacemakers, or differentially habituating units (Burgos, 1995, 1997; Burgos & Donahoe, in press; see also Buonomano & Merzenich, 1995). The neural network consisted of input units (activated by simulated environmental events), sensory-association units, motor-association units, and output units (simulating the behavior of the network). Connection weights (simulating synaptic efficacies) to motor and motor-association units were modified by a nonspecifically projecting system (simulating the dopaminergic neuromodulatory system subserving reinforcement). Connection weights to sensory-association units were modified by a second nonspecifically projecting system (simulating projections from the hippocampus to sensory-association cortex) that, in turn, was also modulated by the reinforcing system. When input units were stimulated by constant environmental stimuli, activity propagated through the network and potentially activated the output unit simulating the operant. Connection weights were modified (updated) in successive time steps that simulated the continuous passage of real time. The weights increased whenever pre- and postsynaptic units were coactivated and a reinforcing signal was present. The weights decreased whenever the pre- and postsynaptic units were coactive but the reinforcing signal was absent (see Donahoe, Burgos, & Palmer, 1993, and Donahoe, 1997, for additional information about the learning algorithm).

Figure 1 shows a cumulative record of activations of the operant unit over a series of 10 time-step fixed intervals (Burgos & Donahoe, in press). In the training regimen, a reinforcing stimulus was presented when the operant unit had an activation greater than zero on the 10th time step. After the reinforcer, all activations were returned to their spontaneous levels and the next 10 time-step fixed interval began. With training, the operant unit was activated at very low levels immediately after the reinforcer, but was increasingly activated as the time to the next reinforcer approached. Thus, the simulated behavior bore an orderly relation to time within the 10 time-step fixed interval, but there was no timing mechanism. How did this

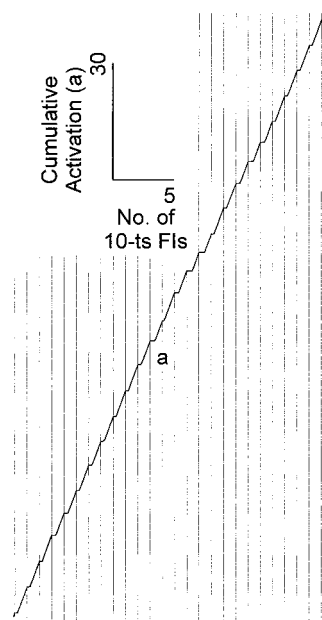


Fig. 1. Cumulative record of the activations ($0 < a < 1$) of an operant unit trained with a 10 time-step (ts) fixed-interval (FI) schedule of reinforcement. Vertical lines indicate boundaries between successive fixed intervals. Performance is shown for FI 276 through 300 of training. (Adapted from Burgos & Donahoe, in press.)

come about? The process whereby temporal control developed may be sketched as follows: Early in training, the reinforcer strengthened connection weights that caused the operant to be activated on the next time step in which the stimulus occurred (i.e., the onset of the next fixed interval). However, early activations of the operant unit were not reinforced and, therefore, the set of connection weights that mediated early responses weakened. With continued training, the reinforcing signal came to select a set of connection weights that permitted the constant environment to activate the operant unit only later in the fixed interval. An examination of the cumulative record of activations in Figure 1 provides some sense of this process. At point *a*, the postreinforcement pause was unusually brief. These nonreinforced responses caused the set of connection weights that supported early responding to weaken. As a result, postreinforcement pauses were unusually long during the immediately following fixed intervals. The temporal pattern of responding reveals a dynamic process of acqui-

sition and extinction across successive fixed intervals.

We wish to make it clear that we do not regard the foregoing as a full simulation of behavior reinforced on a fixed-interval schedule. Many other variables would have to be taken into account in a complete simulation. However, the simulation does demonstrate that temporal control can develop in a constant environment as an emergent effect of reinforcement without recourse to pacemakers and the like. Nothing in the network resembles a timer, and the parameter values for all the units were identical to those used in previous simulations (e.g., Donahoe et al., 1993). Instead of pacemakers or differentially habituating units, reinforcement simply selected a set of connection weights such that only prolonged exposure to the constant environment was sufficient to strongly activate the operant. After exposure to the contingencies of reinforcement, the network acted *as if* there were a timer, but in fact none existed. Moreover, the characteristics of the network were constrained by information obtained from independent experimental analyses carried out at the neural level. These included the general architecture of the network, the effects of the hippocampal and the dopaminergic systems on synaptic efficacies in sensory association and motor cortex, the effect of dopamine on long-lasting long-term potentiation, temporal summation of presynaptic inputs, and competition between presynaptic inputs for control of postsynaptic receptors among others (see Donahoe, 1997). A more adequate simulation would be more fully informed by such information. We are only at the beginning of biobehaviorally constrained interpretations of complex contingencies of reinforcement.

The fruitfulness of a biobehavioral approach is further illustrated by other results that demonstrate a relation between the time of maximal responsiveness and the variance about that time. Simulations were conducted in which networks were trained in Pavlovian procedures to respond to interstimulus intervals (ISIs) of different durations: four, eight, or 16 time steps (Burgos, 1995, 1997). These simulations used a hybrid genetic-developmental-learning algorithm. The genetic algorithm simulated evolution through natural selection to determine the network architec-

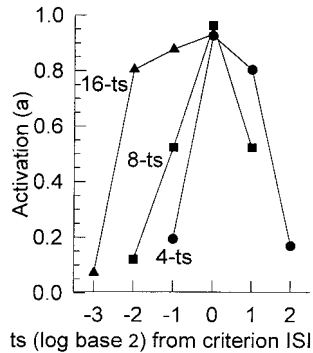


Fig. 2. Mean activations of the output units to probe stimuli of various durations. Different networks were given 200 trials of Pavlovian training with the criterion interstimulus intervals (ISIs) of either four, eight, or 16 time steps (ts). The ISI of the probe stimulus that was the same as the training ISI is designated as zero on the x axis. The differences between the ISIs of the probe stimuli and the criterion ISI are expressed in logarithmic units (base 2). (Adapted from Burgos, 1995, 1997.)

ture. The developmental algorithm simulated the epigenetic processes that affect the initial connectivity of the networks produced by the genetic algorithm. The learning algorithm modified the connection weights of the networks produced by the combined actions of the genetic and developmental algorithms. The final networks were the result of as many as 100 generations, with each generation consisting of successive implementations of the three algorithms.

Figure 2 shows the activation levels of conditioned response units during probe stimuli presented at the conclusion of the evolutionary process. The intervals for which selection occurred during evolution (i.e., the criterion ISIs) were four, eight, or 16 time steps. The value of zero on the x axis indicates that the ISI of the probe stimulus was the same as the value of the criterion ISI during evolution. The durations of the other probe stimuli were either shorter (negative values) or longer (positive values) than the criterion ISI. The values shown are the activation levels at the time steps immediately prior to the end of the probe stimulus. Two aspects of the results are pertinent here. First, regardless of the criterion ISI, architectures and connection weights were selected that permitted the networks to respond maximally to the selected ISI. Thus, the contingencies of selection were sufficient without recourse to either

pacemakers or habituation units. Second, the variance in responding increased as the criterion ISI increased. Increased variance in temporal control occurred automatically as an emergent product of selection. (Interpretations of responding to ISI values greater than those encountered during the selection process are problematic; cf. Staddon & Higa, p. 226.) In short, Weber-law-like temporal control appeared without the need to appeal to pacemakers, hypothetical units with differential habituation rates, or transformations applied to linear processes.

Conclusion

The core problem of temporal control is to explain how behavior can vary with time in a constant environment. The problem arises because experimental analysis has shown that operants are almost invariably discriminated (i.e., under the control of some antecedent) and that the controlling stimulus must occur almost immediately before the response in question. What antecedent stimulus is responsible for temporal control? SET hypothesizes a pacemaker and associated cognitive machinery to supply the missing events. MTS hypothesizes a cascade of differentially habituating units together with various memory and comparison processes. BeT looks toward stimuli produced by temporally constrained adjunctive behavior, with a pacemaker to coordinate transitions between behaviorally defined states. What we suggest is that timing can occur without a timer of any sort. Reinforcers simply select a set of synaptic efficacies such that, when the neural system is acted upon by a constant environment, the operant is emitted with changing probability. Before the action of reinforcement, no timer (of whatever form) existed. After reinforcement, no timer exists. The system simply functions *as if* there were a timer. Natural selection has given organisms a network of richly interconnected neurons with the capability for their synapses to be modified by reinforcement, and this appears to be sufficient for the acquisition of temporal control.

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CAN A DECAY PROCESS EXPLAIN THE TIMING OF
CONDITIONED RESPONSES?

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To explain time-scale invariant distributions of response latencies, it appears to be necessary to postulate scalar noise in the remembered intervals, against which the subjective measure of the currently elapsing interval is compared. At least in some cases, the observed variability cannot be due to variability in the subjective intervals written to memory; it must come from noise (variability) in the reading of a memory. The Staddon and Higa proposal offers no explanation for the observed variability, and it is unclear what noise assumption would yield the observed variability, given their assumption that intervals are timed by a nonlinear decay process. The decay process cannot plausibly be represented by the logarithmic function, because it begins and ends at infinity. The assumption of any form of nonlinear timing is inconsistent with the most important result of the time-left experiment, which is that the changeover time increases linearly with the comparison-standard difference.

Key words: subjective time, time-scale invariance, scalar variability, response latencies, temporal psychophysics

The data that Gibbon's scalar expectancy theory (SET) tries to explain are primarily data on the latencies of conditioned responses. Conditioned responding tends to begin when a certain proportion of the reinforcement latency has elapsed. My comments focus on the assumptions crucial to the explanation of these response latencies.

*The Origins of the Noise in
Temporal Decision Making*

The opening and closing parts of Staddon and Higa's article seem to imply that the principal problem with Gibbon's SET is that it does not enable us to derive the variability observed in the latencies of conditioned behavior. To explain these response latencies, SET assumes a timer, a memory that stores outputs from the timer, and a comparison process. The comparison process generates a response when the ratio of the current value from the timer to the comparison value retrieved from memory exceeds a threshold. To model the timer, Gibbon suggested a Poisson pacemaker feeding an accumulator. The essential feature of this timer is that subjective

time (the internal measure of the duration of an interval) is proportional to objective time.

Staddon and Higa suggest that the theoretically relevant property of this model for the timer is what it predicts about the variability in the repeated timing of the same interval. They note that the *variance* in subjective time for a given duration of the interval being timed will be equal to the mean value of the obtained accumulations. If the variance is proportional to the mean, then the *standard deviation* is proportional to the square root of the mean. Thus, the greater the mean, the smaller the standard deviation in proportion to the mean. In his original formulation of SET, Gibbon (1977) made this same point, and he drew the necessary conclusion, namely, that Poisson variability in accumulations (subjective intervals) could not explain the scalar variability in the response latencies. That is, it could not explain why the standard deviations of the obtained distributions are proportional to the means (and, more generally, why normalized distributions, regardless of their shape, are superimposable). It is unclear why this issue is revisited at this late date; it has been a settled issue from the beginning.

Gibbon (1977) suggested reasons why proportional variability might occur, focusing on possible trial-to-trial variations in the rate at which the pacemaker ran. Subsequent work, however, has shown that the scalar variability in

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the response distributions cannot be due primarily to variability in the original timing of the remembered intervals. Rather, it must be due to variability in the process of reading a value stored in memory in order to use it as the comparison value in a decision. The necessity of attributing the variability in response latencies primarily to the memory reading process is shown most clearly by the results obtained from the two-standard version of the time-left task, because in this experiment, the central tendency or expectation read from memory has no counterpart in the input intervals. None of the input intervals is anywhere near the expectation of the distribution of those intervals. Thus, the scalar variability must arise from the process of reading the expectation itself, not from the distribution of intervals on which that expectation is based.

In the time-left task, the subject compares the time left until reward on one side (the so-called comparison or time-left side) against the unvarying expectation on the so-called standard side. The delay on the time-left side gets shorter as the trial proceeds, while the delay on the standard side does not. When the subject estimates that the time left on the comparison side is shorter than the delay on the standard side, it switches from the standard side to the comparison side. In the two-standard version of this task, the expectation on the standard side comes from an experienced population of two randomly intermixed standard delays: On some trials, the standard delay is very short, for example, 15 s; on others, it is very long, for example, 240 s. The subject never knows which delay is in force on the standard side on any trial, so it must base its behavior on the expectation it computes from the bimodal population of two very different delays. The time left at which the subject changes over tells us what the subject takes to be the expectation or central tendency in this bimodal distribution of delays on the standard side. The changeover turns out to occur at about the harmonic mean (e.g., Brunner, Gibbon, & Fairhurst, 1994), which, for the above illustrative values of the two standard intervals, is 30 s.

The crucial result for present purposes is that the distribution of changeover responses (the distribution of elapsed times at which the subject switches from the standard side to the comparison side) is essentially the same in the

two-standard case as it is in the case in which there is but a single standard interval at 30 s. Thus, the distribution of changeover responses cannot be a reflection of variability in the timing of the very short and very long intervals that compose the population of standard intervals. The population in the two-standard version is very different from the population in the one-standard version. Only the expectations are the same in these two cases, not the populations on which those expectations are based. Yet, these expectations produce the same distribution of changeover latencies. One is forced to conclude that the proportional standard deviation (the scalar variability) observed in the distribution of response latencies reflects trial-to-trial variability in the target values retrieved from memory rather than trial-to-trial variability in the original inputs to memory (the "accumulations" in the accumulator).

Put another way, one is forced to conclude that time-scale invariance is a basic property of the noise in a remembered temporal interval, no matter how that interval was originally derived, whether directly from the timer or from a computation of the expectation of a distribution. Time-scale invariance means that one cannot deduce the time scale of the experiment from the distribution of response latencies. That is, from looking at this distribution, one cannot estimate what the objective duration of the interval to be remembered was. If the variability in the observed distribution were not proportional to its mean, then one would be able to deduce the time scale of the experiment (the reinforcement latency) in the absence of a scale factor for the x axis (numerically labeled ticks). The proportion between the width of the distribution and its location along the x axis would indicate the time scale of the experiment (the reinforcement latency). In short, the assumption of Gaussian noise in the signal that comes from memory, with a standard deviation proportional to its mean value, is indeed a postulate in SET, not a deduction. It is best seen, however, as a manifestation of a more general property of conditioned behavior, namely, time-scale invariance.

The postulation of scale-invariant noise in the values read from memory is a "deeper problem" with SET (Staddon and Higa, p. 227) only if there are other assumptions about the sources of variability, from which the variability in response latencies may be

deduced. No such assumptions are presented in this paper. In fact, Staddon and Higa refuse to say anything about the characteristics of the noise in the signals that determine behavior in their model. A problem I have with any proposal that the subjective measure of time is given by a decay function is that this makes it difficult to obtain a time-scale invariant model. The assumed rate of decay imposes a time scale, making it difficult to have a time-scale invariant model.

One could explain the scalar variability of response latencies if one postulated (a) that subjective time is a logarithmic function of objective time and (b) that the variability or noise in the subjective time signal is independent of the magnitude of that signal (i.e., constant, rather than varying as a function of signal strength). However, as explained below, the postulation of a logarithmic relation is incompatible with the assumption that time is measured by a decay process, which appears to be the foundational assumption in the Staddon and Higa model. Second, the assumption that the noise in the signal is independent of signal level is physically implausible. Finally, this physically implausible postulate is just as much a postulate as the postulation of scalar variability in SET. Thus, the Staddon and Higa model does not derive the observed scalar variability from more basic assumptions. Indeed, it does not even provide an explanation of the observed scalar variability.

The Function Relating Subjective Intervals to Objective Intervals

The essential feature of the accumulator model of the timer is that the subjective interval (the quantity in the accumulator at the end of an objective interval) is proportional to the objective interval. SET would not be in any consequential way altered if the Poisson pacemaker assumption were abandoned, leaving only the foundational assumption that subjective intervals are proportional to objective intervals. Empirically, this function appears to be a linear function rather than a strictly proportional one, but the deviation of the intercept from the origin is small, so, for most purposes, the function can be treated as one of simple proportionality, which is what I will assume hereafter.

Staddon and Higa suggest an alternative to

this assumption. Actually, they suggest at least two alternatives, possibly three. One alternative is that the subjective interval decreases in accord with the sum of several exponentially decaying terms (their multiple-time-scale function).¹ This seems to be the assumption that they are in the end most deeply committed to. The other alternative, which they suggest is equivalent to the first for practical purposes, is that the subjective quantity (the quantity in the head) that corresponds to an objective interval decreases as the negative logarithm of the objective interval. A third suggestion, entertained at various points in their argument, is that the relation is a power function with a negative exponent. I do not agree that these alternatives are for practical purposes equivalent. This makes it hard to assess the viability of Staddon and Higa's proposal, because they sometimes rest their argument on properties of the logarithmic function, while at other times they assume that the sum-of-exponentials function best describes the relation. These are mutually incompatible assumptions. The claim (on p. 220) that "There are several other functions that have very similar properties to the logarithmic: power . . . , the sum of exponentials, and others . . . (Figure 1)" is not defensible, either on the grounds by which the properties of functions are usually compared or on the grounds that the differences between the functions are not great enough within a reasonable range of intervals to matter in practice.

One important property of a function is its behavior at the extremes of its argument. The negative logarithmic function of time [$\tau = -\log(t)$] goes to plus infinity as time goes to zero. This property, all by itself, is an obstacle to the assumption that this function could describe the quantitative relation between objective time intervals and the signals (or traces) in the head that they give rise to. Moreover, as time goes to infinity, the nega-

¹ It may seem odd to speak of a decreasing measure of an increasing function, but that is inescapable in a model that uses the state of decay to measure the magnitude of an increasing variable (as in, e.g., carbon 14 dating). As Staddon and Higa note, this inverse measure (a measure that gets smaller as the thing being measured gets bigger) is more or less okay, as long as the relation between the quantity measured and the measure is monotonic. However, one consequence of using a decay measure is that the measure must be nonlinearly related to the thing measured. This leads to problems, as explained below.

tive log function goes to minus infinity. This is a decidedly odd property for a “decay” process to have. Usually, the more something has decayed, the closer its absolute value is to zero. For time intervals greater than one, the more the negative logarithm “decays,” the farther its absolute value gets from zero.

The power function also goes to plus infinity as time goes to zero (again an obstacle to postulating it), but at least it goes to zero as time goes to infinity, as a decay function should. This means, however, that as time intervals get larger, the difference between any logarithmic function of time and any power function of time becomes arbitrarily large. Thus, these two functions are not interchangeable over any very large range of intervals. (Remember that a straight line is a good approximation to any smooth function over a short enough interval.)

Finally, Staddon and Higa’s MTS function—a sum of decaying exponentials—differs by arbitrarily large amounts from the negative log function at both ends. Any sum of exponentials is finite at both extremes of its argument, whereas the logarithmic function is infinite at both extremes.

Another very important property of the proposed functions in the present context is how they relate objective differences and ratios to subjective differences and ratios. SET proposes that subjective time is proportional to objective time. More formally, it is a scalar function of objective time, that is, $\tau = kt$, where τ represents the subjective duration of an interval, t is its objective duration, and k is a constant of proportionality (scaling factor). This relation has the unique and theoretically very important property that equal objective differences map to equal subjective differences and equal objective ratios map to equal subjective ratios. This means, for example, that the difference between the subjective durations corresponding to objective durations of 10 and 20 s is the same as the difference between the subjective durations corresponding to objective durations of 40 and 50 s. Thus, equal differences in the world map to equal differences in the head. And similarly, the ratio between the subjective durations corresponding to objective durations of 1 and 10 s is the same as the ratio between subjective durations corresponding to objective durations of 5 and 50 s. Thus, equal ratios

in the world map to equal ratios in the head. (Formally, this is because, if $t_1 - t_2 = t_3 - t_4$, then $kt_1 - kt_2 = kt_3 - kt_4$, and if $t_1/t_2 = t_3/t_4$, then $kt_1/kt_2 = kt_3/kt_4$, where $t_1 \neq t_2$ and $t_1 \neq t_3$.) All of the other proposed functions (mappings from the world to the head) lack one or both of these two important properties.

The power function does not carry equal objective differences into equal subjective differences. Assuming again that $t_1 \neq t_2$ and $t_1 \neq t_3$: If $t_1 - t_2 = t_3 - t_4$, then $t_1^p - t_2^p \neq t_3^p - t_4^p$. For example, $1^2 - 2^2 \neq 2^2 - 3^2$. However, the power function does carry equal objective ratios into equal subjective ratios: If $t_1/t_2 = t_3/t_4$, then $t_1^p/t_2^p = t_3^p/t_4^p$. For numerical illustration, note that $1^2/2^2 = 2^2/4^2$. Neither of the other two functions has this property.

The log function does not carry equal objective differences into equal subjective differences or equal objective ratios into equal subjective ratios; rather, it carries equal objective ratios into equal subjective differences (a property that Staddon and Higa make extensive use of). Thus, under the same conditions on the t s as above, if $t_1 - t_2 = t_3 - t_4$, then $\log(t_1) - \log(t_2) \neq \log(t_3) - \log(t_4)$, and if $t_1/t_2 = t_3/t_4$, then $\log(t_1)/\log(t_2) \neq \log(t_3)/\log(t_4)$. However, when $t_1/t_2 = t_3/t_4$, then $\log(t_1) - \log(t_2) = \log(t_3) - \log(t_4)$. For numerical illustration, note that $\log(1) - \log(0) = 0 + \infty \neq \log(2) - \log(1) = 0.3$, and $\log(2)/\log(1) = 0.3/0 = \infty \neq \log(4)/\log(2) = 0.6/0.3 = 2$. However, $\log(2) - \log(1) = 0.3 - 0 = \log(4) - \log(2) = 0.6 - 0.3$.

The exponential function has the inverse property; it carries equal objective differences into equal subjective ratios. Under the same conditions on the t s as above, if $t_1 - t_2 = t_3 - t_4$, then $b^{\alpha t_1} - b^{\alpha t_2} \neq b^{\alpha t_3} - b^{\alpha t_4}$, and if $t^1/t^2 = t^3/t^4$, then $b^{\alpha t_1}/b^{\alpha t_2} \neq b^{\alpha t_3}/b^{\alpha t_4}$. However, if $t^1 - t^2 = t^3 - t^4$, then $b^{\alpha t_1}/b^{\alpha t_2} = b^{\alpha t_3}/b^{\alpha t_4}$. For numerical illustration, note that $2^2 - 2^1 \neq 2^3 - 2^2$ and $2^2/2^1 = 2 \neq 2^4/2^2 = 4$. However, $2^2/2^1 = 2^3/2^2 = 2$.

A sum of exponentials is not itself an exponential function, so the MTS function suggested by Staddon and Higa does not possess any of these potentially useful properties. The function that Staddon and Higa graph as the MTS function in their Figure 1 was obtained by a simulation in which the weighting of the different exponential terms was free to vary. The actual weighting that produced the graph is apparently not known. However, the

following power, logarithmic, and sum-of-exponentials functions produce curves as close or closer to each other than those in their Figure 1. The power function is the one Staddon and Higa assumed. The exponentials have the decay constants that they assumed. The negative log function has been scaled and displaced vertically so as to be as close to these two functions as possible over the range covered by Staddon and Higa's Figure 1. Thus, these functions allow us to estimate the extent to which the MTS function has the properties of the logarithmic function within the range graphed by Staddon and Higa (the situation is much worse outside this range): power: $\tau = t^{-0.45}$; negative log: $\tau = -0.39\log(t) + 0.8$; sum of exponentials: $\tau = \text{MTS}(t) = (1/2.5)(e^{-0.36t} + e^{-0.105t} + 0.5e^{-0.006t})$. From the third function, we can calculate that $\text{MTS}(1) - \text{MTS}(2) = 0.12 \neq \text{MTS}(3) - \text{MTS}(4) = 0.07$ and $\text{MTS}(1)/\text{MTS}(10) = 2.47 \neq \text{MTS}(5)/\text{MTS}(50) = 3.302$, and moreover, $\text{MTS}(1)/\text{MTS}(31) = 4.62 \neq \text{MTS}(20)/\text{MTS}(50) = 1.508$ and $\text{MTS}(1) - \text{MTS}(10) = 0.5 \neq \text{MTS}(5) - \text{MTS}(50) = 0.35$. These numerical examples show, when time is measured by a sum of decaying exponentials, that equal objective differences do not correspond to equal subjective differences, equal objective ratios do not correspond to equal subjective ratios, equal objective differences do not map to equal subjective ratios, and, finally, equal objective ratios do not map to equal subjective differences (as they would if the MTS function could be substituted for the log function and vice versa).

The numerical examples given above limit the values of t to the range graphed by Staddon and Higa in their Figure 1, and yet the discrepancies between the functions are substantial—great enough to yield measurably different predictions even when the range of t is thus limited. In fact, however, SET applies to experiments covering a considerably broader range—from a few seconds to 3,000 s (about three orders of magnitude). Over this range, the differences between the functions that Staddon and Higa suggest are equivalent for practical purposes are very large and completely unmistakable. For example, when $t = 1,000$, the MTS function in Staddon and Higa's Figure 1 is already effectively zero, whereas the power function is 0.04 and declining very slowly, and the vertically

displaced negative logarithmic function is well below zero at -0.37 and growing ever more negative (ever farther from the zero value at which a decay function ought to terminate). Thus, before we can evaluate Staddon and Higa's proposals, they will have to settle on a form for the relation between subjective intervals (the signals in the head) and the objective intervals, because the predictions of an MTS model cannot in fact be reasonably approximated by the predictions of a logarithmic model and vice versa.

The relation between differences and ratios in the subjective realm and differences and ratios in the corresponding objective quantities is fundamental to the issues raised by Staddon and Higa's article. We are here concerned with contrasting models for the processes in the brain that "process," "operate on," or "do computations with" subjective intervals to produce behavior. The subjective intervals are signals or traces in the brain that are at least monotonically related to objective intervals, and therefore can "encode" those intervals. Models for the processes in the brain that determine the timing of conditioned behavior are evaluated on the basis of how well they predict the timing of the animal's responses given various objective intervals (usually reinforcement latencies). The predictions depend jointly on the postulated quantitative relation between the subjective intervals and the objective intervals, on the form and sources for the noise in these neural signals, and on the formal properties of the operations or processes in the brain into which these subjective intervals enter in order to determine the observed behavior.

Many experimental results in the timing literature are very accurately accounted for by a model that makes the defining assumptions in SET, which are that (a) subjective intervals are proportional to objective intervals; (b) the noise in remembered intervals is Gaussian with a standard deviation proportional to the interval being remembered; and (c) the decision variable—the quantity that generates a response when it exceeds a decision threshold—is a ratio of subjective intervals. Put another way, this last assumption is that the measure of the similarity of two intervals is the ratio of their subjective measures. A ratio of one indicates perfect similarity. Decisions

to respond occur when this measure of similarity exceeds a threshold, which is usually appreciably less than one. Decisions to stop responding occur when this measure of similarity is appreciably greater than one. The success of SET at giving quantitatively accurate explanations of the experimental data is evidence for the correctness of its assumptions, including, of course, the assumption that subjective intervals are proportional to objective intervals. However, the experimental evidence most directly relevant to this particular assumption comes from the time-left experiment, to which Staddon and Higa's discussion does not do justice.

The fundamental idea behind the time-left experiment is that if subjective intervals are proportional to the logarithms of objective intervals, then when the brain subtracts one such quantity from another, it is equivalent to dividing the corresponding objective intervals. The subjective result of this operation (the signal generated when the signal for the elapsed interval is subtracted from the signal for the comparison interval) corresponds to the dimensionless quantity that is obtained by dividing one objective interval by another, that is, to the ratio of two objective intervals. This follows directly from the property of the logarithmic function stressed above, namely that it carries equal objective ratios into equal subjective differences. Thus, differences in the head correspond to ratios in the world.

Because a logarithmic encoding of objective intervals converts subjective subtraction (subtraction in the head) into objective division, the assumption of such an encoding makes startling predictions about what will happen when a subject is faced with a task in which it must compare a subjective interval obtained by subtraction with another, separately specified, subjective interval. This is what the time-left task does. The subject must compare the time left until reward is obtained on the so-called comparison (*C*) side with the standard (*S*) delay of reward on the other side. The time-left to reward on the comparison side gets shorter as the trial continues, whereas the standard delay does not. Thus, there comes a point in the trial at which the rational thing to do is to switch responding from the standard side, which has the shorter expected delay at trial onset, to the comparison (time-left) side, which has

the shorter expected delay after some interval has elapsed. The only way to estimate this point—the elapsed interval (*E*) at which it pays to change over—is to subtract the elapsed interval from the fixed and known value of the comparison interval at the start of the trial.

Staddon and Higa's discussion of this experiment focuses on the question of the subjective value of the first half of a comparison interval versus the second half, which is not the proper focus. The most powerful result from these time-left experiments (as was stressed in the original publication by Gibbon & Church, 1981) is the relation between the midpoint of the cumulative changeover function (hereafter, the changeover point) and the absolute values of the comparison and standard intervals, when the ratio of these two reference intervals (the *C/S* ratio) is held constant. What matters is not *where* the changeover point is located for any particular values of *C* and *S*, which is what Staddon and Higa focus on. What matters is what happens to this changeover point as one increases the values of *C* and *S* proportionately (leaving their ratio unchanged). If subjective intervals are proportional to the logarithms of objective intervals, then the midpoint of the changeover distribution should be determined by the *C/S* ratio, which means that it should be independent of the actual values of *C* and *S*. This seems a priori exceedingly unlikely, and it is, in fact, contrary to experimental fact. Empirically, the changeover point increases linearly with the values of *C* and *S*. This result is fatal to the assumption that the magnitudes being subtracted in the head are the logarithms of the corresponding objective intervals.

In the end, Staddon and Higa seem to recognize the impossibility of explaining the time-left result while maintaining the assumption that the computation of the time left is carried out with quantities that are proportional to the logarithms of the intervals they represent. They write, "The claim is that no matter what the animal's internal code for elapsed time, it will also have some kind of compensatory perceptual constancy mechanism . . . that allows it to behave appropriately with respect to the real world (i.e., real time)" (p. 222). In Footnote 2, they write,

Time may well be (and is, we contend) encoded nonlinearly, in the sense that it is mapped on to some internal variable that increases [sic] with elapsed time in a negatively accelerated way. Nevertheless, subjective time, like subjective weight and the other examples, is roughly proportional to real time. We argue that encoding determines experimental results that depend on discriminability, but subjective value determines results that depend on value (e.g., choice experiments).

They seem here to be making a distinction between how time is “really” encoded in the nervous system and how it is encoded when the animal has to do something that depends on its time estimates. In the latter case, they concede that “subjective time . . . is roughly proportional to real time.” It is in the nature of behavioral data that they can only be used to determine how a thing is represented at the point in the brain at which the signal that does the representing enters into a combinatorial computation that has behaviorally observable consequences.² The time-left experiment determines the relation between the subjective interval and the objective interval at the point in the behavior-generating process at which the brain determines the time left. At that point, the relation appears to be one of proportionality. Elsewhere in the brain where intervals are represented, the relation might have a different form. If so (and evidence of this remains to be found), then it will be difficult to defend the claim that the form at one point in the brain’s processing is the “real” form, whereas the form elsewhere is the “?” form (virtual? imaginary? complex?—it is not clear what alternative to “real” would be appropriate to plug in here). The process that determines when the subject changes over from the standard option to the comparison option is presumably a real

process, so the variables that enter into it are presumably themselves just as real (tangible, measurable, etc.).

Explaining the Distributions of Response Latencies

Thus, the question becomes whether there is evidence that at some point in some of the processes underlying at least some timing tasks, the form of the relation between the interval signal in the head and the objective interval is approximately logarithmic or, alternatively, a decay function. As indicated above, these are mutually exclusive hypotheses, because decay functions go to zero whereas the negative logarithm goes to minus infinity. The majority of the tasks to which SET has been applied are tasks that look at the distribution of response latencies relative to the reinforcement latency. SET does a good job of accounting for these distributions. By contrast, the observed distributions are not predicted by a model that assumes that (a) the relation between subjective intervals and objective intervals is either approximately logarithmic or is determined by a decay process and (b) the noise in a subjective interval is Gaussian with constant standard deviation. If the underlying measure of the ever-lengthening objective interval is really decaying to an asymptotic value of zero, as any decay model ought to assume, then it is going to be even more interesting to see what kind of assumptions about underlying noise will be required to explain the scalar variability that is so salient a property of the observed distributions. The particular MTS function that Staddon and Higa use in their Figure 1 is effectively zero by the time that 1,000 s have elapsed. Beyond that interval, there is nothing left to decay, so all objective intervals longer than that are subjectively the same. However, the likelihood of a pigeon’s having resumed responding when a given proportion of the fixed interval in a fixed-interval schedule has elapsed is the same when the fixed interval between rewards is 30 s as when it is 3,000 s (Dews, 1970). This is a particularly striking and simple example of scalar variability in the timing of an operant response. How this could be explained by a sum of exponentials (or any other true decay function) and what the noise assumptions

²A computation in which the values of two different variables combine to determine the result. For example, in SET, the subjective measure of the currently elapsing interval is divided by the expectation retrieved from memory (a combinatorial operation) to produce the measure of similarity, which is compared to a threshold to determine whether the animal will or will not begin to respond. The comparison is also a combinatorial operation, namely, ordination (is the measure of similarity greater than the threshold). All of the basic operations of arithmetic (addition, subtraction, multiplication, division, and ordination) are combinatorial operations. Inversion, taking the log, raising to a power, and exponentiation are examples of noncombinatorial operations.

would have to be to get scalar variability over that range of intervals are very unclear.

Staddon and Higa do not make any suggestions about the sources or the form of the noise in the processes that generate timed responses. As a consequence, they do not offer an account of the distributions that are observed experimentally. Rather, they seem to argue that it is not reasonable to try to explain these distributions. If so, then there is little reason to offer alternatives to SET, because that is what SET principally explains. In refusing to try to explain the distributions of timed responses, Staddon and Higa abandon the field on which SET most often operates. More important, they avoid wrestling with some of the more difficult consequences of the assumption that time is measured in the head by a decay process. One of the more intractable problems with this assumption is reconciling it with the scalar variability observed in the distribution of response times.

At several points, Staddon and Higa seem to argue that SET is founded on the explanation of experiments on temporal discrimination. This assumption seems to underlie the paragraph (p. 223) whose second sentence begins, "The fundamental flaw in the time-left argument is in fact *conceptual*" and culminating in the sentence, "The general point is that discriminability does not determine perceived value." This is a puzzling paragraph, because SET does not rest on the Fechnerian error of assuming that just noticeable differences are subjectively equal. In

fact, it is well known that this assumption led to Staddon and Higa's sometime postulate (that the relation between the subjective quantity and the objective quantity is logarithmic), not to Gibbon's postulate. Indeed, the postulates that constitute SET are incompatible with the assumption that just noticeable differences are equal. Whatever SET's faults, the assumption that discriminability determines perceived value is not among them.

On the other hand, the failure to explain the scalar variability in the distributions of conditioned responses is a serious fault in Staddon and Higa's model. Scalar variability is a very well-established fact. It appears to be a manifestation of a deeper and broader principle, the principle of time-scale invariance. And, it appears to be irreconcilable with the assumption that temporal intervals are measured by a decay process, which is the central assumption in Staddon and Higa's model.

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MULTIPLE TIME SCALES IS WELL NAMED

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Staddon and Higa's article is a critique of scalar expectancy theory, and a proposed alternative, multiple time scales. The critique is generally flawed, both factually and logically. The alternative is bewildering in its flexibility, opaque in its quantitative description, and never addressed to real data.

Key words: timing, scalar expectancy theory

The article by Staddon and Higa is first a critique of, or more properly a diatribe on, scalar expectancy theory (SET), and second a proposed alternative. The paper is not satisfactory in either section, for very different reasons. The criticisms of SET are often ill taken and occasionally bizarre. On the other hand, the multiple-time-scales (MTS) alternative proposed next is a bit too aptly named. Staddon and Higa mix and match time scales to qualitative descriptions of effects in the timing literature with a bewildering variety. Power scales, log scales, summed exponential scales (MTS), and even linear scales (see below, time-left explanation) are used almost at will.

When the original version of the article was submitted to *JEAB*, the editor requested a review from me, and I submitted a signed review. The paper was subsequently revised, re-submitted, and accepted to be published with commentary. My reading of the revision is that not much has changed (with the exception of Figure 6, discussed below). I am therefore providing a commentary which is essentially portions of my original review, with discussion of minor points deleted and edited for clarity.

Poisson Variance

The authors begin with a central theme, namely that the pacemaker-accumulator idea in SET is difficult to reconcile with Weber's law. Indeed, the description implies that SET is continuously scrambling to get around this difficulty by adding parameters to the ac-

count. A central feature of a pacemaker accumulator is Poisson variance, which does not accord with the scalar property: proportional rescaling of timing distributions. Actually, from the outset the information-processing account of SET was designed with just the scalar property in mind. The idea was that a system of this kind (a) must account for the scalar property and (b) may do so in more than one way. The clock, memory, and decision process stages identified three potential sources of scalar variance, all of them with the critical property that random variation in the system be multiplicative. The three sources so identified were pacemaker rate variation (by the way, it makes no difference whether one assumes trial-to-trial variation or within-trial variation; cf. Gibbon, 1992), memory translation multipliers in storage and retrieval, and threshold variation. The authors have, I think, misunderstood the way in which the notion of a Poisson pacemaker (in both early and late versions of SET) was conceived. Gibbon (1992) showed that even small sources of multiplicative variance do indeed render Poisson variability negligible. The assumption of a Poisson pacemaker was a convenient one that is physiologically plausible, because there are many neural systems with Poisson variability. The key features of the theory did not rely on the Poisson pacemaker idea but rather on a mechanism that integrates activity over time with multiplicative variance.

Logarithmic Subjective Time Scale

Staddon and Higa then go on to posit a logarithmic perceptual subjective time scale. They argue that the temporal bisection result with indifference at the geometric mean is a natural and straightforward instantiation of equal subjective distance from the two anchor

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points, short and long. As Staddon and Higa state, the geometric mean finding drops out of the log time scale rather simply. As long as variance is constant and symmetric on the log scale, the midpoint between two remembered values should be at the geometric mean. In fact this was the motivation for examining the subjective time scale in the time-left experiments (see below). For bisection, given the linear scale that SET assumes, the psychophysical function is obtained not by simple differences on the subjective scale but rather by similarities on this scale, where similarity is defined, like other discriminative functions in SET, as a ratio, in this case of the probe to the referents. Staddon and Higa have not described the SET analysis of bisection accurately, in my view. Gibbon (1981) analyzed logarithmic and linear scales using a difference rule for the logarithmic scale and the ratio rule for the linear scale. Although it is obviously true that differences on a log scale reduce to ratios on a linear scale, it is not true that the form of the psychometric function is well fit by the logarithmic scale. Indeed, it is shown that symmetry of remembered distributions of the standards on the log scale results in a poor fit to the psychometric functions, albeit with an appropriate indifference point at the geometric mean.

There are other data that deal with the form of the psychophysical function as well, which again make for difficulties with the log scale. Church and Gibbon (1982) examined what form temporal generalization functions ought to take when animals discriminate whether a probe stimulus is the same or different from a reinforced standard. The scalar property is again found, but with near symmetry on the real time axis, which is not to be expected from a subjective log scale. Rather a negative skew, which is never found in real data, is predicted for the log scale.

Time-Left Experiments

The time-left experiments (Gibbon & Church, 1981) were motivated precisely by the geometric-mean finding for bisection. But Staddon and Higa do not seem to have grasped the fundamental features of these experiments, particularly Experiment 2. Subjects choosing between an elapsing alternative and another, the standard, which is fixed (beginning "right now"), should show choic-

es reflecting proximity on the log scale to food, which is much shorter in the middle of the interval for the time-left side than it is for the standard. But importantly, if a log unit is added to both sides by doubling the elapsing and standard interval, as was done in the second experiment, then a discrimination based on proximity to food on the log scale predicts *no change* in the point of indifference between the two. And this of course is ruled out by the data from Experiment 2.

Even with omission of Experiment 2, Staddon and Higa evidently find the log scale not tenable for the time-left experiments, and perform a bizarre contortion to obtain the desired result. They argue that subjects perceive time in a logarithmic fashion but then take *inverses* (i.e., antilogs) and then calculate immediacies (expectancies) in real time! That is, subjects perceive time logarithmically but have the good sense to ignore their perceptions (or the good sense to pick the antilog transform) when faced with a choice between delays to food. They use real time, as though the log scale associated with perception of time were irrelevant. SET does not need such convoluted reasoning to arrive at the appropriate result, because the point of indifference should scale linearly with real time provided that time is perceived linearly with real time.

Staddon and Higa claim a kind of "perceptual constancy mechanism" to account for the use of linear time. They argue "for a separation between the animal's capacity to assess reinforcement rates and its capacity to use a decaying memory trace as a stimulus" (p. 222). The example espoused to justify such a convoluted interpretation is that there is a smaller two-point threshold on the hand than on the back "but we do not feel that our hands are larger than our back" (p. 223). This kind of reasoning from just noticeable differences is exactly the sort that led Fechner to the log scale. Do the authors wish to challenge his inference? In any case, this example makes no sense applied to the time scale. The reason for judging that the hands are smaller than the back has nothing to do with the two-point threshold. Visual sensory data are undoubtedly used in determining size, but for temporal judgments there is no alternative receptor. What are the alternative data used when judging time durations?

The authors go on to argue for a log time scale on other grounds (p. 224). However, I confess to be completely lost on Equations 8 and 9. How is it that “internal effects” are related to time differentials by these equations? Multiplying through by t or b , they seem to be saying that a small change in t (dt) is equal to the standard deviation of t times the “internal effects” or differential on z , $dz = \sigma_t dz$. But what is z ? And where is the variance in this system? Although it is certainly true that Equations 10 and 11 follow by integration, the justification for Equations 8 and 9 remains mysterious. This is but one of several examples of what might be called mini models in this paper, none of which are developed in sufficient detail for us to evaluate them. Moreover, it is not always clear which mini model is being espoused, because power, log, and MTS are frequently given equal weight, and are introduced where convenient for one or another purpose. It is also unclear how the mini models relate to the presumably central, more developed MTS model. Sometimes, as in Figure 1, they are touted as equivalent, but of course in many other respects, such as slopes, they are clearly not.

Start/Stop/Spread Correlations in the Peak Procedure

Staddon and Higa insinuate that positive start-stop correlations (as opposed to negative start-spread correlations) are a difficulty to be overcome in SET. In fact this analysis was designed to attempt to isolate the relative contributions of memory and decision variance in this procedure. Far from being unexpected, the analysis showed that both sources of variance are present.

MTS, in contrast, appears to have only a start threshold. How would such a system handle the results of the peak procedure? If a stop threshold is to be permitted, how does the animal “know” where to place it, or even come to “expect” reinforcement at a particular time, and so detect its omission?

MTS Memory Timing Model and the Variance Problem

Figure 6 in the current article is very different from that in the original. The original Weber fraction was shown to decrease about 55% over an eightfold range in interfood in-

terval (a Poisson system would show a Weber fraction decrease of about 65% over this range). The description in the original, however, closely matched that in the current version (pp. 232–233). The text appears to describe the Weber fraction as resulting from the decreasing slope of the memory trace.

The new Figure 6 shows rising and falling functions. The degree of change near the origin is small for values of $\lambda \geq 1.5$. However, small on what scale? Ordinate values are omitted from the new Figure 6. Indeed, it would be useful to have more than simply ordinate values in the new Figure 6; it would be much more compelling to see actual Weber fractions from real data obtained over these ranges.

The problem with where variance arises in this system is endemic to all of the discussion. Staddon, at a recent meeting (personal communication), described the Weber fraction as obtained from a simulation of MTS with threshold variance. That is, a given threshold on the memory decay trace functions with constant variability would induce the curves shown in Figure 6. Is threshold variance then the source of variability producing the Weber fraction? The description in the text does not make this clear at all. Indeed, there is no talk of variance throughout this paper, and the quantitative mechanisms underlying variance, or even those underlying the scales, are generally opaque (MTS is never described with closed forms). And if the lack of detail on variance mechanisms is a problem for me, I suspect I am not alone.

It is almost as though animals are perfectly accurate but somehow the slope of the subjective time functions induces a Weber fraction without any variability (at least in Equations 19 and 20). If threshold variance is indeed what induces the Weber fraction here, then that description needs to be made explicit. Threshold variance is one of the sources of scalar variance in SET, but it is always explicitly so. Here it is not clear (a) whether the Weber fraction increases or decreases or (b) whether the functions in Figure 6 are a result of a simulation with true variability or are the result of some calculations on slopes, as the text suggests.

Even on a qualitative level, it seems that the MTS decay functions, which can approach arbitrarily close to zero over a rather short

range (see Gallistel's commentary), must pose processing problems for real live subjects that have internal processing noise; for example, constant threshold variance must avoid negative threshold values as the MTS subjective scale approaches zero. But truncating the threshold distribution at zero means that it is no longer constant, and is less so the closer the decay function is to zero.

Deterministic accounts are in principle error free, but a major thrust of psychophysics for many years has been to understand sources of variability and error. We need to know more about how errors are produced in this system before an evaluation can be made. On

its face, MTS appears to avoid the variance problem by simply remaining silent.

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MODELING MODELING

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Models are tools; they need to fit both the hand and the task. Presence or absence of a feature such as a pacemaker or a cascade is not in itself good. Or bad. Criteria for model evaluation involve benefit-cost ratios, with the numerator a function of the range of phenomena explained, goodness of fit, consistency with other nearby models, and intangibles such as beauty. The denominator is a function of complexity, the number of phenomena that must be ignored, and the effort necessary to incorporate the model into one's parlance. Neither part of the ratio can yet be evaluated for MTS, whose authors provide some cogent challenges to SET.

Key words: models, pacemakers, theories

If you think models are about the truth, or that there is a best timing model, then you are in trouble. There is no best model, any more than there is a best car model or swimsuit model, even though each of us may have our favorites. It all depends on what you want to do with the model. Nor are models theoreticians' guns of domination, any more than data are empiricists' bullets of assault. War

games can be fun, however, especially when you do not have entangling alliances with the principles, and can just watch them swat it out. Will the grapplers or the punchers win this year? It happens to empiricists too, failing to replicate and sniffing about controls, but somehow it is more fun when it is the guys in suits, the guys who prefer *ln* to *log*, the guys who try to explain *your* data to *you*, are going at it.

And it is a good thing for them to do, too. Do *you* want to attempt to tell a Gibbon that his integral is improper? Or a Staddon that he might have one too many layers in his

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leaky cascade? Besides, if John ****on shows that John ****on's models are wrong or irrelevant, well, then, so much less for us to feel guilty about not mastering; and if we wait a few years, with any luck J*** H*** will upset the survivor from this round.

So, what do we mortals hope to learn from this paper, other than that, apparently, heavy experimental equipment cannot be operated after doing too many integrals? Perhaps that conceptual analysis is always relevant, no matter how mathematical a theory. Or how verbal. Pictures, either explicit (e.g., Staddon & Higa's Figures 3 and 4) or implicit (e.g., Skinner's reflex reserve, pictured in Killeen, 1988), are the foundation of models. They are the skeletons that get muscled by mathematics and padded by words. Until recently models were valued to the extent that they had such a visualizable (*anschaulich*) foundation (Miller, 1984). One is never wrong to refer back to pictures for guidance.

Models are go-betweens: They go between the data and our sense of understanding. This is as true of Skinner's models (e.g., the three-term contingency, reinforcement, the mand) as it is of the paraphernalia of timing. To the extent that models fail to make contact with the data or with our sense of understanding, they fail. In the first case their death is quick; in the second, the models languish until Burked by boredom.

If we sit out too many rounds, we might wind up as mere paying spectators, couch potatoes, Monday morning quarterbacks. This is safer, retrospectively smarter, but ultimately a less reinforcing posture. After all, it is *we* who are paying *them* with our attention. This is a game worth getting into.

And play we must, because all understanding involves models—reference to systems that exist in a different domain than the thing studied. Loose models make vague reference to ambiguous or ad hoc causes. Tighter models are more careful about definitions and avoid gratuitous entities. Models of phenomena are not causes of phenomena; they are descriptions of hypothetical structures or functions that aid explanation, prediction, and control. Their promulgators may believe that such structures exist, or that they have a particular instantiation, or that they were sighted last year in the medulla oblongata; interesting, certainly, but such beliefs are not

what is most important about the models. A model may be functionally brilliant without being structurally enlightening. Such was the case for Newton's mechanics. To get to the heart of a matter does not require that the matter have ventricles.

Washing the Baby

To use models effectively, and to understand those of others, scientists must undergo some training. In many cases this means studying some math. In *The Behavior of Organisms*, Skinner (1938) formulated rich verbal descriptions of acquisition, discrimination, and extinction that presaged Hull's more mathematical treatments, and were directly translatable into them. He inveighed against the "paper dolls" of models only after his logarithmic extinction curves were criticized (by Ellson, 1939). It was perhaps easier for Skinner to throw out his model baby than to learn how to bathe it. It is an advantage of a model and its packaging, however—not a defect—that its emissions and omissions can be discriminated. Most other parents would have held their breath and cleaned.

Staddon and Higa detect an odor arising from Gibbon and Church's baby, and don't like the way it is dealt with. "Too many layers of diapers" they seem to say. But it ain't their kid, and because they are raising one of their own, it is best just to sluice out the nest.

Watching the Baby

So, how about their neonate, the pacemaker-free model of timing? This issue records the village tour, as we peer into the pram, listen to the neighbors' comments, and make a few of our own. But the new infant is scarcely old enough to have soiled itself, let alone to have done much with its limbs; we will have to judge it from its looks, not its parents' promises. Analysis of its sibling (Staddon & Higa, 1997) would be worthwhile; but this one and its commentaries are already demanding enough, and we just do not have enough attention left in our wallets, what with the other game we hoped to catch. In fact, it is easier to listen to the local gossip about the kid than to study its features, and that is why some of you skipped ahead to here just now.

Making Paces

So then, what about some of its parents' claims for the multiple-time-scale (MTS)

model: Is it possible to have a pacemaker-free model of timing? What does a pacemaker add that makes it worth having, and with what have Staddon and Higa replaced it?

A pacemaker is a model generator. Say you want to boil an egg for 4 minutes, and have to decide when it is time to quench it. You need to subtract the time of day you started it from the current time of day, and compare the difference to a model—a model of 4 minutes. A prosthesis helps. You can buy an egg timer that is a sand clock; invert it and it takes 4 minutes for the sand to empty into the bottom vessel. Inverting it resets the origin to zero, and its completion represents a model of 4 minutes.

If you want to grill a fish for 10 minutes, you need a different model. Imagine a kitchen with a row of sand clocks, sitting under the row of spices, ranging from a garlicky 15 seconds up to a turkey 4 hours. Hinged, perhaps, to invert upon a touch. Quaint, but inefficient. There is a better prosthesis available: one of those wind-up timers that we use to alert us when to change the pigeons. It contains all of the sand clocks in a single portable device. Buy one of those, and you have bought yourself a pacemaker. It can model any interval up to an hour, and ring a bell as a discriminative stimulus for you to leave the meeting. Those are handy devices in colloquia, even if you do not have any pigeons running.

What most clocks do, and what all accurate clocks do, is make and measure paces. Water clocks and candles do not have pacemakers; they move a measured quantity of matter at a relatively constant rate, and signal when it is all gone. They are inaccurate. Mechanical and electrical clocks meter the number of cycles of an oscillator. They measure them by mapping them to a spatial interval, such as the circuit of the clock face; or they count them by incrementing an integer, as do digital watches. The pacemaker is an oscillator, as is anything that rotates or revolves or swings—anything, that is, that can be modeled as an oscillator.

To use the pacemaker, you need to count or measure its output. The finer the scale and the more accurate the period, the more model intervals you can generate. The earth is a pacemaker. It rotates around its axis. Its course can be subdivided with a sundial to

time the day. Its rotations can be counted on a calendar to time the year. Calendar reforms have made this a fine art, primarily by knowing when to reset the year counter to maintain a correspondence between the months and the seasons of the year, the latter generated by an independent pacemaker—earth's revolutions around the sun.

Counting them. Our location in time is recorded as years (from an arbitrary origin) plus months plus days plus hours, and so forth. Whenever a second elapses, our pacemaker increments a seconds counter and resets to zero; when that reaches 60 it increments a minutes counter and resets to zero; when that reaches 12 it increments a p.m. counter and resets to zero. And so on. This is a counting cascade, with the output from faster registers being passed to slower registers.

The Good News

Pacemaker-counter systems are a great invention because the more oscillations you count, the less important is accuracy in the pacemaker. The subdivision of the period of the sun by a sundial is inaccurate. Better to multiply a fast oscillator than divide a slow one. In a pacemaker-counter system, even the worst pacemaker—one whose n th tick gives absolutely no information about when its next tick will come—can be part of an accurate clock, if the pacemaker is fast enough. The Poisson emitter is the mathematical model of the worst possible pacemaker. It has a constant probability of ticking at any instant in time, and this probability is independent of when the last tick occurred. If you count enough of these ticks, however, you can be as accurate as desired. In particular, your relative inaccuracy—the standard deviation divided by the mean—decreases as $1/\sqrt{n}$.

If I ask you to say when 10 seconds have elapsed without use of a watch, you are likely to count “one-thous-and-one, one-thous-and-two, . . .” You are counting quarter seconds, which you produce by rhythmic motions of mouth and tongue. This is more accurate than if you just guessed. You are timing by counting: You are using a pacemaker-counter system. Skinner forgive, for you have just gainfully employed one instantiation of the insidious hypothetical construct, *pacemaker*. If you refuse to play my game again and use

your watch, well, by this point you should know what I would say.

The Bad News

The Poisson system—the worst possible constant-rate pacemaker and the best possible counter—is mathematically a relatively simple model. That is why it is used. It is a subset of recurrent processes; you can replace the Poisson emitter with a more accurate oscillator, and the relative accuracy will improve further. But it cannot get worse. That is also the bad news, because when animals time intervals, their accuracy does not increase as the inverse square root of the interval to be timed. In fact their relative accuracy is relatively constant. This is Weber's law. It is also a keystone of scalar expectancy theory (SET). So, whereas the Poisson system is a good way to model time, it is a bad way to model animals that time. To do that, the clock has to be detuned.

Detuning the Clock

Where can error be added? The pacemaker is already almost as bad as it can get. One way to make it worse is to add error to the average rate of the pacemaker, a source of variance invoked by SET. Another is to add error to the counter. SET does that by having the storage of the count add an error proportional to the magnitude of the count. The behavioral theory of timing (BeT; Killeen & Fetterman, 1988) does it by having the pacemaker run faster for shorter intervals and slower for longer intervals, as though there were a ceiling on how high the counter could go. (The mechanism for this adjustment is assumed to be the higher levels of arousal typically associated with the higher rates of reinforcement on short intervals.) Machado's (1997) learning to time (LeT), a continuous version of BeT, also lets the speed of the pacemaker vary with the interval to be timed. The connectionist model of timing (Church & Broadbent, 1991) adds error to a register of counters that is proportional to the magnitude of the numbers they count.

Couching such models in mathematical terms can be useful, because they can then make more precise predictions that are susceptible to test. If they fail those tests, they must be fixed or discarded. For instance, one obscure mathematical model of pacemaker-

counter systems shows that under many different kinds of component error, the optimal period of the pacemaker should be independent of the interval to be timed; it predicts the value of that optimal interval in terms of the variances of the components; and it shows that, under the stated assumptions, the origin of Weber error must reside in the counter (Killeen & Weiss, 1987).

Why maintain a pacemaker-counter model if it must be fixed? Well, it is still a simple model, and a simple model that is accurate is much better than no model at all. All that is required is the replacement of a simplifying assumption (error-free counters) with a more realistic one (error in counting). Many feel that it is best to start with the simplest models and adjust them only when the data require it. This tactic keeps models understandable until some of that simplicity starts to exact a cost. Furthermore, the pacemaker-counter system has some face validity, at least in the realm in which intervals are long enough to be counted. Finally, it has some ecological validity: All of the best clocks do it that way.

How MTS Works

How do Staddon and Higa get worse than Poisson timing? They have a cascade of different interval timers, each of different (increasing) natural periods (much like the egg timers, arranged in order). The response output is the positive difference between the current input and the "memory" (V_i ; their Equation 15). The effect of a single input decays proportionally with its magnitude (Equation 16), which entails an exponential decay of its strength. One such unit feeds into (cascades into) the next, with the next having a slower rate constant (a_i).

The cascade acts as a low-pass filter. If temporal stimuli are coming at high frequencies, the earliest-fastest units in the cascade get saturated, and that blocks the signals from getting through to the slower units downstream. The first-fastest do all the work: It is variance in their level of saturation that accounts for most of the variance in the animal's response. If signals come more slowly, the fast units are not saturated when they recur; their response output to the next level of the cascade is therefore greater (Equation 15), and those slower levels start to pick up some of the burden. This is a good thing, because the accu-

racy of any one level decreases exponentially with its level of saturation. One can see this by noting that the change in time corresponding to a small change (error) in trace strength V grows as $dt/dV = e^{at}/a$. This is much worse than Poisson timing; much too much worse.

So, how do Staddon and Higa move in the other direction, and improve model performance to get better than exponential error? The secret is in the cascade. By passing the ball to the slower units at longer intervals, the system can operate in a closer to optimal range. Furthermore, by passing the ball most forcibly when the units are most accurate (Equation 15 shows that the signal strength decreases as a unit saturates) the bad performance near saturation is not propagated downstream. Their Figure 6 shows that by selecting the rate at which these downstream units are slowed, one can generate a range of Weber functions.

The MTS Counter

The MTS model is a counting cascade, with the output from faster registers being passed to slower registers. A start-time marker must discharge all the units so they can start timing an interval afresh, like a bar that restores all the egg timers to upright. (The authors profitably speculate on empirical consequences of weak start signals.) It must simultaneously initiate a start pulse; because all registers have been cleared, this will get through equally (weighted by the b_i parameters) to all units. For the system to come to equilibrium, a series of input pulses must then be sent that condition (fill) the various reservoirs, layers, or units to their various asymptotic levels.

The state of each unit is summed by Equation 17. Whereas counters are digital all-or-none devices, Equation 17 sums continuous variables for a continuous aggregate memory strength. It is more accurate to call the Staddon-Higa counter an “accumulator.” Same function, slightly different capabilities: If you count things small enough, then counters are not discriminable from accumulators. In the process of accumulation, MTS does not retain information about the individual registers. Those are weighted (by the b parameters) and summed. This is just as well, because animals usually make binary temporal decisions and primarily need to discrimi-

nate “not yet” from “now.” Any more information could be counterproductive.

This is a dynamic system, so it is not surprising that it is path dependent: The effect of a single time interval depends on those which came before, and on the disequilibrium state of the system. To be understood, it really has to be simulated. This is not hard to do. Equations 15 through 18 are simple when you work them through. Just pick a discrete time interval small enough (say, 0.1 s) and input timing signals at longer intervals (say, 10 s) and play with the parameters. Try it. That is the only way you will understand it. If it doesn't work, you can blame the authors. You might even get a publication out of it.

The MTS Pacemaker

MTS does not have a pacemaker. It has a series of continuous modelettes of temporal intervals (each of the layers) whose accuracy decreases exponentially with the interval to be timed. It is as though there were as many pacemakers as layers, each with a different resonance. This aspect is similar to the spectral approach of Grossberg and Schmajuk (1989) and the delay lines of Moore and Desmond (1992). The layers hum as an increasing function of the proximity of the stimulus to their resonant frequency, and the accumulator sums the chorus. They achieve their ability to predict Weber functions, as well as functions not so Weberian, from the tuning of this aeolian harp. Octaves (Figure 6, $\lambda = 2$) do quite well. Deviations from a perfectly horizontal line are also interesting, as they may come closer to real data than a perfect Weber's law.

Do We Keep It or Go for the Hose?

The parents tell us that this kid is going to grow up to be president, after having served in the space corps and composing some tone poems of local reknown. Furthermore, the kid will be inexpensive to keep, not needing a pacemaker, which the village can now sell to a used clock shop up the coast. Best of all, it is unsullied, so far. So, what do we think?

Nice thing is, a village needs more than one model. And this one is quite different; diversity is important, which we knew even before our bureaucrats told us so, and remains true despite them. But more than models, a village needs teams: an MTS team

and an SET team and an LeT team. The MTS coaches have scored some points on the SET coaches, but we have yet to see how well they can defend their own goal. It is worth nurturing this little model for a while; see if it lives up to its parents' expectations. It has the potential, but more players are desperately needed for a meaningful competition.

Modeling Modeling

To think about models, we need models of the process; restrictions on form and criteria for success and handicapping for assumptions. We have yet to evolve consensus on this most central preoccupation of scientists. I think of models as structures that exist in a domain different than the subject, and are constructed according to design principles that are dictated by theories. Parsimony (simplicity), power (the range of phenomena that can be covered), and accuracy (not too many mispredictions or omissions) are all important. But so also is excitement, the sense that models are comprehensible and testable, and that each of us can use them to score goals of our own. Science has many game-like aspects; if we model it as a game, with referees, teams, rules, matches, prizes, and penalties, we can come to some important conclusions: One match is not a season; a loss is less shameful than refusing to play; clear rules and agreed-upon goals are important; unbiased referees are essential. Perhaps most important, it is not a zero-sum game. Models

that can take the field are to everyone's advantage; contests such as this will not only improve all teams, but they will draw participants from other domains to refresh the experimental analysis of behavior.

Now then, anyone for an expansion team?

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*THE WHIRLIGIG OF TIME:
SOME THOUGHTS ON STADDON AND HIGA*

JACK MARR

GEORGIA TECH

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 roco 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^As (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-Gib-

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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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 impor-

tance 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 Stadon–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 con-

siderations. 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 sen-

sitivity 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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