Time Perception: Discussion Paper

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Study of the papers in this session reveals at least three major topics, each of which arises in three or more of the papers. They are: the quality of timing performance, clock models aimed at accounting for the variability in the behavior, and discovery of the scale for the subjective perception of time. I organize my remarks accordingly.

QUALITY OF TIMING PERFORMANCE

Perhaps the most obvious contrast in quality of timing is that between human and animal performance. In both cases considerable evidence is provided that mean times are quite accurate and that, to a first approximation anyhow, all distributions of normalized responses are the same. However, substantial differences exist between the animal and human data in the magnitude of the relative variability: the Weber fraction for the human data runs at about 5% and for the animals nearer to 50%.[1-3] However, in at least two respects these two classes of data are not comparable. First, the ranges over which they have been studied do not overlap, being between tens of milliseconds and a few seconds for the humans and from seconds to tens of seconds for the animals. Second, the pressure in the human experiments has been for precision of performance and it is far from clear that the animal studies have been designed with that in mind. The consquences for an animal who does not exhibit exact timing are really not very severe, being nothing worse than some unrewarded responding. Perhaps it would be useful for someone doing animal studies to attempt to establish the limits of their performance, which we have no reason to expect to be worse by an order of magnitude than that of people. And in the other camp, perhaps it would be useful to determine whether the 5% figure continues to hold into the region of tens of seconds. I do not underestimate the difficulties and effort required in each case, but both questions seem important.

All of us have been astonished by the precision of timing that Kristofferson and his associates have managed to achieve, and until he demonstrated it 15 years ago few of us would have anticipated that the variability in timing would remain constant or nearly so over any substantial region. What is new in the present data, and even more surprising, is the series of plateaus in estimates of the period of the clock (see below), which are spaced by factors of two over intervals that increase by factors of two. This, it seems to me, has important implications for modeling, to which I now turn.

CLOCK MODELS

A number of our authors envisage timing behavior as based upon a clock of some sort. Three of the papers[1,2,4] postulate a real-time digital device with timing arising from a count of the number of events. Hopkins and Kristofferson admit no variability in the clock itself, whereas Gibbon *et al.* explore various possibilities, rejecting as a primary source of the observed variability Poisson noise in the clock and favoring some form of scalar variability either in the clock or in the memory. (I should make clear that their finding that Poisson variability plays little role in timing in no way bears on the physiologically well justified Poisson representation of sensory intensity.) Assuming the period of the clock to be q, everyone agrees that the arrival of a signal will be random relative to the pulse train defining the clock, which introduces a uniformly distributed random variable over the interval $(0,q)$. Wing and Kristofferson[5] suggested that this is just one of three sources of variability leading to the observed variability, the other two being another, but independent, uniformly distributed one also on $(0,q)$, and the third an independent, normally distributed one associated with the response process. Hopkins has shown us that this model gives an almost perfect fit to his data; however, one would also like to see how well the data can be fit by other highly peaked distributions, such as the Laplace, instead of the triangular.

The major problem of that model, it seems to me, is this: Where does the second uniform distribution over $(0,q)$ come from? Hopkins attempted an argument along the following lines: After the count is achieved, the system exits the clock and initiates a response mechanism which is delayed in starting in much the same way the clock is, presumably because it cycles in a clock-like fashion. To fit the data, the two rates must be nearly the same, but to achieve approximate independence he assumed slightly different rates. This argument does not seem very persuasive to me, and I fear that it may run into difficulties with Kristofferson's findings about the plateaus.

Consider how the plateaus of variance may come about. One possibility is that the counter applied to the pulses of the clock has a maximum count, and when a time is wanted that exceeds the capacity of the counter, the system in essence counts every other pulse. This could be achieved by cells that are activated whenever two pulses occur within q time units but are refractory for considerably less than q time units, where we recall q is estimated to be about 12 msec. Such a model produces one uniformly distributed random variable on the interval $(0,2q)$, but I really don't see where the second one is to come from since there is no reason for the quantal character of the response process also to change scale. Because the second random variable seems to arise from exiting the clock and initiating the response process, its distribution should be controlled by the statistics of the response mechanism, not that of the timer. Once that dilemma is solved, then estimating even longer intervals simply involves repeated applications of the same type of cell that responds to every other pulse, but with even broader periods of integration. Such a mechanism generates the factors of two which Kristofferson has found. One cannot but wonder how many of these filtering cells can be arranged in series; presumably that can be estimated by extending Kristofferson's methods to appreciably longer times. It seems important to me that the distributions for $2q$ and $3q$ be studied with the same care the Hopkins has given q to see whether the fit of the convolution of a normal with two identical, independent uniform distributions continues to be equally satisfactory. For the reasons given above, I wonder if an asymmetry will not begin to evidence itself. If it does not, the second uniform distribution on $(0,2q)$ is an interesting theoretical challenge.

Before I turn to my last topic, let me say how pleased I am to find growing evidence for the existence of both good mental clocks and accurate mental counters, which some years ago David Green and I[6] suggested would provide a parsimonious account of some psychophysical speed–accuracy data.

SUBJECTIVE PERCEPTION OF TIME

When we turn to the subjective aspects of the perception of time, the only phrase that comes to my mind is "a can of worms." It is a familiar can to those who, like myself, have fished in psychophysical waters.

With the exception of Eisler (see below), those who have spoken of clock models have postulated periodicity in real time, and to the degree that the models are successful, which is considerable, that can be taken as *prima facie* evidence that at a certain level the perception of time is proportional to physical time. In this view, subjective scales are no more than useful constructs in a theory, and certainly many important constructs of physics—energy, momentum, entropy, and force—gained their status only via theory. However, as psychologists we have, in addition, strong intuitions about the lively existence of subjective attributes that cannot possibly be linear with the usual physical measures as well as the added knowledge that when we ask human subjects, more or less directly, about these attributes, we usually obtain results that are far from linear with physical measures. That makes suspect, but by no means rules out, the proportionality of subjective to physical time which is posited, with success, in these models.

Some[5] observe that the distributional data are describable as arising from a single distribution through scale changes, which is what Weber's law amounts to, and suggest that this in essence determines the needed transformation of time—which transformation is located in memory and not in the clock. This is the original strategy of Fechner, one that postulates a solution which, at least in psychophysics, has been found wanting an empirical basis.

So, one says, almost reflexively, let's decide the matter empirically. It is perhaps well to begin with the blunt admission that psychophysicists have never evolved a way to do so that has commanded wide assent. The direct scaling methods of S.S. Stevens[7] to which Eisler made reference, rest upon a mode of communication that is entirely language based; in fact, these methods rely upon the instruction to the subjects that the numbers they assign to stimuli shall preserve subjective ratios. In whatever way our subjects understand this instruction, they do give consistent, repeatable data; nonetheless, whatever the instruction does mean, we do not have the slightest idea how to communicate it nonverbally. Moreover, through the work of King and Lockhead,[8] we know that magnitude estimates are highly malleable, and quite different functions can be obtained by altering the feedback subjects receive. In brief, we simply do not know how to do scaling experiments with subjects who do not speak our language. Yet, that is exactly what two, and perhaps three, of our authors have claimed to be doing.[1,3,] Do I misunderstand and have they solved the century-old dilemma of the psychophysics of big differences, of what I call global psychophysics? I think not.

So far as I can tell, the researchers working with animals are doing temporal discrimination studies which, just like the discrimination studies of psychophysics, do not tell us much about the overall apprehension of an attribute. The fact that the indifference point between a variable and a standard time interval sometimes is approximately at the standard in no way implies that a linear scale in involved, and the fact that Weber's law holds does not dictate a particular nonlinear transformation. Eisler is quite aware that neither tack will do, but I believe he has slipped into two other traps. First, he has avoided reducing the problem to one of simple discrimination by assuming that the subject selects the second interval not to be equal to the first one, but to be subjectively one-half of the total interval. The motivation for this bit of indirection on the part of the animal, although not the author, escapes me. How does the animal know to use 1/2 rather than any other fraction? Second, and rather more serious, he has used the human data to establish a region within which he reinforced the animals' responses in what amounts to a discrimination study with 10 discriminative stimuli, and the animals—at least two of eight rats—quite reasonably took into account their own variability and stayed well within the reinforced region, thereby nearly reproducing the human behavior. Since we know from years of operant work that animals are quite sensitive to temporal reinforcement and from human work that magnitude estimation scales are malleable, these results persuade me of nothing whatsoever about temporal perception in animals.

I do not wish to disparage efforts toward finding objective ways of eliciting information about internal states, which is what I believe a subjective scale to be, but it is surely going to require a more complex idea than either just discrimination or just reinforcement. For the moment we may have to be satisfied with models of the sort that Kristofferson and his students and Gibbon and Church have been working on to account for these highly regular and, I believe, important temporal discrimination and timing data.

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