Comments from JABA Readers

FURTHER REAPPRAISAL OF MOMENTARY TIME SAMPLING AND PARTIAL-INTERVAL RECORDING

We found the comments of Suen, Ary, and Covalt (1991) on our paper (Harrop & Daniels, 1986) to be very interesting, and we appreciated the general clarity of presentation. On some important points, however, we disagree with their analysis. The comment that "for any given true total duration, MTS can be expected to record behavior in the same number of intervals whether that behavior is massed in one bout or is distributed among many bouts" (p. 804) is potentially misleading. Consider, for example, a behavior, perhaps a student banging a desk lid in school, that occurs for a total duration of 6 min in a 60-min lesson and that is observed using momentary time samples (MTS) undertaken every 30 s. If the 6 min of behavior occur in one bout, the MTS record would show 12 (perhaps 13) instances recorded, or 12/120 of the observations made, which is the correct proportion. If, on the other hand, the behavior occurred in bouts of 3 s each, then the MTS record could conceivably show any number of instances of behavior occurring from 0 to 120 (i.e., from 0/120 to 120/120). That *on average*, the number recorded in these circumstances would be 12/120 (as may be demonstrated by sampling theory) is, in practice, of little use or interest to the practitioner who relies on single periods of observation. Herein lies the crux of the matter, the implications of which we believe have not been acknowledged.

The sampling theory analysis provided by Suen et al. (1991) correctly demonstrates that MTS provides unbiased estimates of duration in the sense that on average the MTS estimates will match the actual durations. But an unbiased estimate is not

necessarily accurate when based on a single session of observations, as shown by the above example. In practice, we need to know both the degree of systematic error (bias) produced by a sampling method and how much unidentified error is involved. That MTS exhibits no inherent systematic error, whereas partial interval recording (PIR) systematically overestimates duration, does not imply that MTS exhibits less unidentified error than PIR does.

The thrust of our 1986 paper was that we need to take into account how and why sampling procedures are used by the practitioner. In this respect, an abstract mathematical analysis that shows the degree of systematic error involved in various observational methods may not be particularly relevant or valuable. The most important consideration is that individual investigators do not usually average their data across several observation sessions. Nor are they necessarily interested in estimating accurately the actual absolute levels of behavior. Instead, they are usually interested in comparing scores from different sessions to determine whether or not any change has occurred in the amount of behavior. For example, a teacher who observes a student's desk banging probably isn't interested in being able to demonstrate that the student bangs the desk 10% of the time. Of more concern is to find out whether or not an intervention is effective in changing (reducing) this level of behavior. The teacher would choose a sampling method that is sensitive to relative changes in behavior in preference to one that is unbiased but insensitive to change. This is the point we were trying to make about MTS and PIR in our 1986 paper. MTS is unbiased, whereas PIR systematically overestimates duration. On the other hand, PIR is more capable than MTS of detecting relative changes in the duration of a

behavior. This perhaps surprising finding is easily explainable in terms of PIR exhibiting less unidentified error than an equivalent MTS procedure.

The Harrop and Daniels (1986) paper was specifically designed to examine the question of the sensitivity to change of MTS and PIR. The results demonstrated that PIR is more sensitive to change than MTS is. Suen et al. (1991) say that our data were apparently based upon "an analysis that recorded how many seconds were counted by PIR in each interval" and that "this information is not available in practice" (p. 804). If our analyses had been based upon such information, Suen et al. would be correct in their criticism. In fact, our analyses for PIR used only the standard scoring method (i.e., we used the number of intervals recorded). The analyses of variance of regression that we carried out are complex. However, the sensitivity index derived from these analyses is simply a measure of the *correlation* between PIR or MTS scores and actual durations calculated for data obtained within each of the levels of probability of onset that we used for the simulations. Because only small (random) variations in actual duration occurred within each probability level, the correlations obtained provide an indication of how well the PIR and MTS scores reflect these small changes. Indirectly, the sensitivity index may be said to assess the unidentified (rather than systematic) error inherent within an observational method. The higher sensitivity we reported for PIR in all our simulations means that in practice PIR is better able than MTS to detect changes in the actual duration of behavior.

As we stated at the time, this does not mean that PIR provides an accurate estimate of the degree of change (in fact PIR tends to underestimate change).

In conclusion, we reiterate that we found PIR to be more sensitive than MTS in detecting relative changes in behavioral levels, and we recommend its use under the circumstances discussed in our 1986 paper. Furthermore, we believe our results demonstrate the usefulness of computer simulation studies. Such studies may produce data that complement or challenge the conclusions obtained from a purely mathematical analysis. As such, they may encourage a more sophisticated mathematical treatment than that currently available. In particular, we suggest that mathematical analysis of time sampling focus on the issue of the unidentified error involved in various observational methods.

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