Dear Sir,

The criticisms by Vorberg and Schwarz [1], of a series of papers by Pöppel and coworkers [2–7], on the apparent, strong periodicities observable in choice reaction time distributions, are much less well founded than at first appears to be the case. The actual sample sizes have ranged between 100 and 30000 trials on one subject, and not always been the 260 cited by Vorberg and Schwarz (Pöppel, personal communication).

However, Vorberg's and Schwarz's critique does not address the real identification problem which arises in this context. Their simulations are treated as frequency distributions of one variable outside real time, whereas Pöppel's model, and the related phenomena he and coworkers report, is strictly concerned with a distribution of endpoints in time of a process that terminates (i.e., a response is made), and the data are observed in sequential experiments so that these endpoints are themselves elements of long time series. The data, therefore, imply considerably more relevant statistical information than that used by Vorberg and Schwarz.

The visual inspection method used by Pöppel [2–7] appears to work, in the sense of giving repeatedly the same graphic impression, if the records within a single subject are phase-locked onto the first peak, if they have a bimodal distribution of peak heights, and if they are state-dependent (we have to know if the subject is or is not intoxicated or anaesthetised).

It is, therefore, not sufficient to show that they are not full of „bumps" (where by „bumps" we mean the false quasiperiodic peaks due to lumping a distribution in little bins to create histograms) but we have to show that the periodicity of the peaks is stable in the sense of being both phase-locked onto the occurrence of the first peak, and also in the sense that the distribution of the interpeak intervals (i.p.i.) and the associated distribution of the magnitude of these peaks as deviations from the troughs between them, are of a form not generated by artefacts of lumping into bins.

As Pöppel's argument rests in part on the modal i.p.i. of 30 ms (and not a mean i.p.i. as Vorberg and Schwarz state) we need information on the statistic

V_r = variance (i.p.i.) / mean (i.p.i.)

as this should tend to zero as the system becomes strongly periodic, within the data for a single subject. The legitimacy of pooling data over different subjects in this context is dubious, but the distribution of V_r over a group of subjects should be examined. Vorberg and Schwarz give the standard error of the mean interbump interval over a long series (1000) of simulations, but it is not that statistic we require here, but V_r. A distribution of both periodic and aperiodic bump densities might have the property of a mean of 30 ms and a very small s.e. without in any sense being strongly periodic within each separate realization of the process.

It is worth noting that the statistical methods which are necessary to disentangle the identification problem arising here were not available in sufficient detail before the 1980's, and Pöppel's work precedes them by a decade. It is, therefore, gratuitous for Vorberg and Schwarz to write of „data analysis procedures which inevitably confirm the hypothesis one wants to test" without bothering on their part to be very clear what the hypothesis is, or what means existed for testing it at the time that it was first legitimately advanced.

Yours sincerely,

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Reply to Gregson

The aim of our paper was to study the pitfalls to be expected if reaction time data are examined by Pöppel's method [1]. Consequently, we were concerned with how Pöppel's data have actually been analyzed rather than with how they might have been analyzed, e.g., by time-series methods. In order to do so, we followed his method as faithfully as possible. We are in complete agreement with Gregson when he objects to the data analysis procedure employed in our simulations which, however, is that advocated by Pöppel; to demonstrate its inadequacy for testing the oscillatory hypothesis is the very point of our paper. The informed reader will notice that Gregson does not address any of our detailed criticisms except for the following two points:

Number of observations: Sample sizes reported in Pöppel's basic papers [1] do not exceed $N = 260$; we ignored the fact that, due to erroneous responses, actual sample sizes were even smaller.

Mode vs mean: We concede that the 30 ms estimate of the osciallatory period given by Pöppel was based on the mode of the empirical interpeak interval distribution although we do not see any theoretical justification for using the mode instead of the mean. Anyway, we reran our simulations of Pöppel's method with modal rather than mean interpeak intervals – as expected, the results essentially remain the same.

Thus, we have no reason to retract our original conclusions [2]:

- Pöppel's method does not discriminate between multimodal and unimodal reaction-time distributions;
- Pöppel's method yields artifactual estimates of the oscillatory period which are determined by the histogram bin width rather than by the true intermode distance of multimodal latency distributions.

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