A recommended method of analysis for presentiment experiments

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Studies designed to detect the phenomenon usually called "presentiment" have been among the most popular in experimental parapsychology over the past few years. It is claimed that collectively such studies have produced strong evidence that physiological processes can be influenced by future events through a form of precognition.

Generally these studies involve exposing subjects to a series of stimuli while some physiological variable is monitored. The idea of presentiment is that the stimulus can have a retroactive effect on the measured variable, and that this effect can be detected before the stimulus is applied.

The most common form of presentiment experiment consists of a number of sessions, each consisting of a sequence of trials which are designated - randomly and independently of one another - as either active trials, in which a stimulus is applied, or control trials, in which there is no stimulus. Typically the results of such experiments have been analysed by calculating an average value of the physiological variable for active trials, measured immediately before the application of the stimulus, and comparing this with an average value for control trials. A significant difference between the averages has been seen as evidence of a presentiment effect.

Unfortunately it is known that if these averages are calculated first for individual sessions, and then further averaged over all the sessions in the experiment, they are liable to a subtle statistical bias. This can produce an artefactual difference between the averages for active and control trials, which may mimic a presentiment effect even though none is present.

Various methods have been proposed for either reducing or eliminating this bias. Essentially, the problem is to find a way of doing this while avoiding an unacceptable sacrifice of sensitivity to any genuine presentiment effect that may be present.

To assist in the choice between the different approaches, the accompanying mathematical note presents expressions for the mean and variance of four of the statistics available for the analysis of presentiment experiments. These include exact expressions for the mean, derived using very general assumptions, which indicate whether the statistic is biased or unbiased. There are also approximations for the variance, applicable when the number of trials per session, and/or the number of sessions per experiment are large. The size of the variance determines how sensitive the statistic would be to a genuine presentiment effect. (Note that the approximate expressions for variance are intended only to clarify the properties of the statistics, and not to be used directly in analysing experimental data.)

In the light of these results, the recommendation is to use the exactly unbiased statistic called W^* in the accompanying note. Rather than being based on averages, W^* is defined in terms of suitably rescaled sums of the physiological variable for the two groups of trials within each session - active and passive - with a simple estimate of the initial baseline value first being subtracted from the measured values.

To define W^* in mathematical terms, suppose that each session contains N trials, of which the first M trials are used to estimate the initial baseline. Suppose also that the value of the physiological

variable measured in the *n*th trial is X_n, and that the probability that a trial is active is *a*. Then *W** is the average over all sessions of the statistic *H**, defined for each session by

$$H^* = \frac{1}{a(N-M)} \sum_{\text{active},\, n=M=1}^{N} \left(X_n - \overline{X}^{(-)} \right) - \frac{1}{(1-a)(N-M)} \sum_{\text{control},\, n=M+1}^{N} \left(X_n - \overline{X}^{(-)} \right) \quad (1)$$

in which the initial baseline estimate $\overline{X}^{(-)}$ is the average of X_n over the first *M* trials:

$$\overline{X}^{(-)} = \frac{1}{M} \sum_{n=1}^{M} X_n \quad (2)$$

Thus, for each session, *H** is simply the sum of the measured values of the physiological variable in active trials (with the initial baseline estimate subtracted), divided by the expected number of active trials, with the corresponding quantity for control trials subtracted. The sums are restricted to trials after the *M*th trial, to avoid re-using the values already used to estimate the initial baseline.

The mathematical results suggest that, as a result of the subtraction of the initial baseline estimate, the sensitivity of the statistic *W** to any genuine presentiment effect should not be too much lower than that of the biased statistic used originally for the analysis of presentiment experiments, provided the baseline of the physiological variable does not drift too far during each experimental session. The value of *M* that maximises the sensitivity of *W** will depend on both the characteristics of baseline drift and the variability of the physiological response from trial to trial. For the analysis of a particular experiment, the optimal value of *M* can be chosen empirically, to minimise the variance of *W** and thereby maximise its sensitivity.

In practical terms, the following two-stage procedure is recommended:

(1) A small number of sessions are designated as pilot sessions and used to estimate the optimal value of *M*. For each possible value of *M* (from 1 to $N-1$), equations (1) and (2) are used to calculate the value of *H** for each pilot session and the variance of *H** between pilot sessions. Then *M* is chosen to be the value which gives the smallest value for the variance. Data from the pilot sessions must be excluded from further analysis.

(2) With the value of *M* fixed, the values of *H** are then calculated for the remaining sessions, and averaged to give the statistic *W**. Because it is the average of a number of independent and identically distributed contributions from the individual sessions, the statistical significance of the resulting value of *W** can be determined by standard methods such as the Student's t test. (It is therefore not necessary to make any assumption about the variance of *H**. As already mentioned, the approximate results presented in the mathematical note are not intended to be used directly in analysing experimental data.)

The results obtained by this procedure will be entirely free of bias. But it should be emphasised that, in order to achieve this, it is essential that both the first *M* trials of each session, used to estimate the initial baseline, and the pilot sessions used to estimate the optimal value of *M* must be excluded from any further analysis.

Two further points are worth noting:

(1) It is also shown in the mathematical note that the definition of W^* can be extended straightforwardly to the situation where the trial types are not specified independently of one another (see Appendix, Section A.4, of the mathematical note). An example is a counterbalanced design, in which every session is constrained to contain the same number of active trials. The extension is essentially just a matter of adjusting the coefficients in equation (1) to reflect the fact that, at any point, the probability that the next trial will be active depends on the types of all the preceding trials.

(2) The recommended procedure can be used to reanalyse existing experimental data, provided that measured data for individual trials have been recorded, rather than just averages per session.

Finally, although the recommended procedure should be capable of eliminating bias with an acceptable sacrifice of sensitivity, it is still well worth considering an alternative suggestion made by Kennedy. In this approach, rather than comparing the results of measurements for the two types of trials, an attempt would be made to use the data to predict the type of each trial in advance. If a suitable prediction criterion could be identified, this would have several advantages in addition to eliminating bias. It would allow exact statistical analysis of experimental results using the binomial distribution, and might also have the potential to enhance the statistical power of experiments considerably.