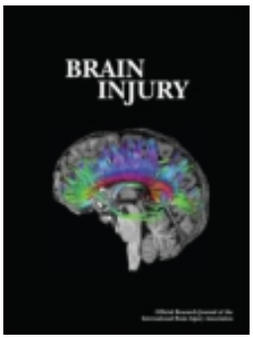

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# Letter to the Editor—Response

Peter Schofield & Iain Perkes

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## LETTER TO THE EDITOR

# Letter to the Editor—Response

The author is quite correct in drawing attention to differences in a range of demographic and relevant exposure characteristics between the prisoner and community groups in our study. Such 'discrepancies' were unavoidable and potentially informative. As we stated in the methods section of the paper, selection of variables for inclusion in multiple regression modelling was 'based on biological plausibility and the presence of statistically significant univariate associations both with group membership and TBI'. Neither marital status nor aboriginal or Torres Strait Islander status were associated with TBI in our sample and thus could not play a role as confounders in the association between TBI and group membership and therefore did not warrant inclusion in the model. Although there were significant associations between playing a contact sport and both TBI and group membership (more likely among prisoners than controls) the latter finding seemed of questionable relevance other than as might be mediated through TBI. In any event, if we include 'played a contact sport' in our regression analyses, it further diminishes the (already non-significant) association between TBI and group membership.

Our statement that there was 'no significant association between TBI frequency or severity and custody/community group membership' represents the report of a result rather than a conclusion. It is simply what we found in multivariate analyses.

The point of our comment in the paper about causality was simply to remind readers that even if, after adjustment for possible confounders, we had found an association between TBI and group (i.e. more in prisoners) in this cross-sectional study, we would still need to be cautious about (over) interpreting the finding to mean that TBI leads to offending (although unquestionably such a mechanism could account for such a finding). In our multivariate analyses, however, there was no significant association between TBI and prisoner status, hardly supportive, we would contend, of a causal link between TBI and subsequent offending. On the other hand, impulsivity and dissocial characteristics were highly associated with prisoner status in multivariate analyses which included TBI. Under these circumstances our statement that 'these analyses provide little support for the notion that TBI leads to much offending behaviour and *invite speculation* that personality characteristics (impulsivity and dissocial) *may be* considerably more salient' (emphasis added for this letter) still seems to us to be cautious, fair and balanced.

The author of the letter states: 'Several differences should be acknowledged in how data about TBI were collected from the custody and community samples'. In fact much of the discussion of our paper is devoted to the issue of the methodological limitations of our study to which he/she refers. Our paper states that 'the telephone survey inquired about the first head injury, then their second head injury and so on until the fifth'. This does not seem to us to be ambiguous: it refers to the study participants' first head injury, their second head injury, etc. We do not pretend to have ventured the last word on the complex relationship between TBI and offending behaviour. However, for all the inherent and, we had thought, comprehensively described limitations of our paper, our data were hard won and we owed it to all study participants to publish the outcome. We remain quite comfortable with our cautious interpretations of the findings. We agree with the writer that validated tools for eliciting a history of TBI are to be welcomed.

Peter Schofield, Iain Perkes,
Tony Butler & Stephanie Hollis

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