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Authors’ reply

Like Alexander Wood and colleagues, we would have also expected deprivation to be associated with violence in this military population. Using the data that we had available to us, our study1 looked at the socio-demographic factors which would be expected to be associated with violence based on what we know from the general population. We included education and military rank, both of which are related to socioeconomic status and to deprivation to some extent. We did not have another measure of deprivation but we suspect that other measures of socioeconomic disadvantage in addition to education and rank would have had only a marginal additional effect.

In response to the concerns about the method used to classify participants in a combat or non-combat role, and the assertion that medical personnel are often deployed in a combat role, we would like to make some clarifications. Medics are not deployed in a combat role. The Geneva Convention specifically prevents medical personnel from using lethal force other than to protect themselves or their patients. Being in a combat role is not the same as being deployed in a forward area. However to take account of the possible discrepancy between the roles that military personnel have on deployment and their trade description, we used participants’ own description of their role on deployment and not the trade description derived from their cap badge. The questionnaire asked participants to select their main duty on deployment from a list of 20 roles. If a participant ticked both combat and any other duty, combat trumped all others. The classification of role in modern Armed Forces is difficult and neatly dividing all deployed participants into two groups if anything will underestimate rather than overestimate the true difference due to combat exposure.

We shared the concern about attrition in a cohort study. Hence we linked all the participants from the beginning of the cohort study with their official criminal records. Therefore, we had no loss to follow-up. But bias could still arise through non-response to the questionnaire at the beginning of the study. The variables that are associated with non-response are younger age, male gender, and lower rank;1 which are also associated with violence.2 But we weighted our analysis for response rate to account for these factors. If such adjustment was not entirely effective non-response would have given an underestimation of violence.

Unfortunately the information provided by the Police National Computer records did not provide information on the context in which the violence post-deployment occurred. With regard to Wood and colleagues’ observations about behavioural change in military personnel following deployment, in particular increased nightclub attendance, we reiterate that we did not find that deployment per se was associated with increased risk of violence. Our analyses of the effect of combat role and exposure to traumatic events on subsequent violent offending involved only those who had been deployed. Therefore all participants in these analyses, according to Wood and colleagues’ observations, would have been more likely to attend nightclubs more frequently, and this would not help to explain the increase in violent offending associated with combat role or traumatic events on deployment.

We declare that we have no conflicts of interest other than those disclosed in the original paper.

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Tuberculosis vaccine trials

Michele Tameris and colleagues’ data (March 23, p 1021)1 offer a valuable opportunity to reflect on the utility of diagnostic approaches for tuberculosis in the clinical trial setting.

In this study, a hierarchical approach was used for diagnosis of tuberculosis consisting of progressively less stringent endpoints 1, 2, and 3.1 Although unstated, there was also an implicit endpoint 0, microbiologically confirmed tuberculosis, which was a subset of endpoint 1 (microbiological