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Causes of death in hepatitis B and C: a methodological issue

A methodological issue regarding calculation of person-time at risk has been brought to our attention in our study of causes of death in people diagnosed with hepatitis B and C.[1] In our analysis, to avoid notification bias, we excluded deaths that occurred within 6 months of diagnosis. For the remaining study population, time at risk commenced at the time of hepatitis diagnosis. This approach leads to lower estimates of standardised mortality ratios (SMRs), meaning that significantly raised SMRs can be interpreted with reasonable certainty. However, with this approach, time at risk is not left-truncated in a manner corresponding to the exclusion of deaths, and SMRs might be underestimated as a result.

We have reanalysed our data using the latter definition of time at risk (ie, including deaths within 6 months of diagnosis), and did find a modest increase in all SMRs. In terms of the major outcomes, the recalculated SMRs for liver-related mortality were 13 (95% CI 11·6–15·1) for hepatitis B and 18·3 (16·7–19·9) for hepatitis C, and those for drug-related mortality were 1·5 (1·1–2·2) and 21·5 (20·2–22·9), respectively. Similarly, we recalculated the values we gave in our response[2] to the letter by Völzke.[3] The associations between hepatitis B and C and death from atherosclerotic circulatory disorders (1·3, 1·1–1·7, and 2·0, 1·7–2·3, respectively) and circulatory deaths not related to atherosclerotic circulatory disorders (0·5, 0·4–0·6, and 1·1, 1·0–1·3) remained qualitatively the same, although slightly increased numerically.

The same point also applies to our linkage study of hepatitis C virus and hepatitis B virus notifications with the New South Wales cancer registry.[4] Again, recalculation of standardised incidence ratios (SIRs) gave broadly unchanged conclusions. Details of all recalculated SMRs and SIRs in both papers are available from the authors on request.

Although the conclusions of our study are not altered, we do feel that this is an important methodological point in linkage studies. We would recommend that observed events and person-time at risk are left-truncated in a similar manner, at least as sensitivity analyses.

We declare that we have no conflict of interest.

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Diagnosis of smear-negative tuberculosis in people with HIV/AIDS

The Public Health review by Haileyesus Getahun and colleagues (June 16, p 2042)[1] is a useful addition to recent articles and policy statements stressing the dire need for rapid, simple, and accurate tuberculosis diagnostic and drug susceptibility tests suitable for use in resource-poor settings.

However, by not considering articles published after May, 2005, the paper by Getahun and colleagues did not capture important recent developments in this active and rapidly evolving field. Among the promising new tools overlooked was the microscopic-observation drug-susceptibility assay (MODS).[2]

MODS is a low-technology, broth culture method that delivers highly sensitive bacteriological detection of *Mycobacterium tuberculosis* together with identification of isoniazid and rifampicin resistance in a median of 7 days and at a cost of US$2 per test. In a validation study in Peru involving almost 2000 patients, including 253 with HIV infection, the performance characteristics of MODS were shown to be at least equivalent to costly gold standard culture and drug susceptibility testing methods used in high income countries, with superior diagnostic sensitivity in patients both with and without HIV coinfection,[2] data that have since been replicated in Ethiopia[3] and in Honduras and Brazil.[4]

When MODS was coupled with the string test, a novel application of a simple, inexpensive approach to sample acquisition that outperforms sputum induction, bacteriological diagnosis of pulmonary tuberculosis in HIV-infected patients nearly doubled.[5]

It seems unfortunate that a review article intended to inform urgent policy changes in a field as dynamic as tuberculosis diagnosis was unable to capture the most up-to-date information. We propose that authors should be provided with the opportunity to update papers of this nature during the often lengthy delay between a paper's acceptance and publication.

We declare that we have no conflict of interest.

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WHO

Violence and the Millennium Development Goals

The association between domestic violence and the first five Millennium Development Goals (MDGs; July 28, p 292)[1] is bidirectional. Violence has a negative effect on efforts to alleviate poverty (MDG 1), and poverty has been shown to increase the likelihood of violence.[2] Similarly, education, women's empowerment, child mortality, and maternal health are all linked to domestic violence.

Simwaka and colleagues[3] discussed the association between women's empowerment and violence against women and poor access and control over resources, and recommended putting gender issues in the African agenda to achieve MDG 5. Hence, monitoring the progress in preventing violence should not be separated from monitoring the development process in developing countries.

Other challenges such as discrimination, inequity, extremism, religious fanaticism, human rights violations, and the faded democracy process have hampered efforts to combat violence in these countries. Ammar[4] stated that "Egypt would be able to combat public violence (eg, terrorism) better if it addresses co-occurrence of spousal and child abuse than by changing its school curriculum". Moreover, we will not be able to estimate properly the magnitude of domestic violence if its economic costs are not investigated.

Therefore, the growing political will to take action against violence is not enough in itself, especially when women feel that spousal abuse is justified and when judges and lawyers are part of a culture that tolerates violence against women.[4]

I declare that I have no conflict of interest.

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Department of Error

Annane D, Vignon P, Renault A, et al, for the CATS Study Group. Norepinephrine plus dobutamine versus epinephrine alone for management of septic shock: a randomised trial. Lancet *2007;* **370:** *676–84*—The address for Prof E Bellissant in this Article (Aug 25) should read: "Centre d'Investigation Clinique INSERM 0203, Unité de Pharmacologie Clinique, Hôpital de Pontchaillou (CHU de Rennes), Université de Rennes 1, Rennes, France". The first two sentences of the third paragraph of the Results section should read: "Mean blood pressure increased to much the same extent in both groups after randomisation (figure 4). Compared with the norepinephrine plus dobutamine group, arterial pH was significantly lower on day 1 (p<0·0001), day 2 (p=0·0008), day 3 (p=0·0019), and day 4 (p=0·0007) in the epinephrine group (figure 4)." The second to last line of the same paragraph should read: "Likewise, there were no significant differences between the two groups in terms of the time to haemodynamic success (log-rank p=0·67; figure 5) and the time to vasopressor withdrawal (log-rank p=0·09; figure 5)." Also, an incorrect version of figure 2 appeared. The correct version is shown here.

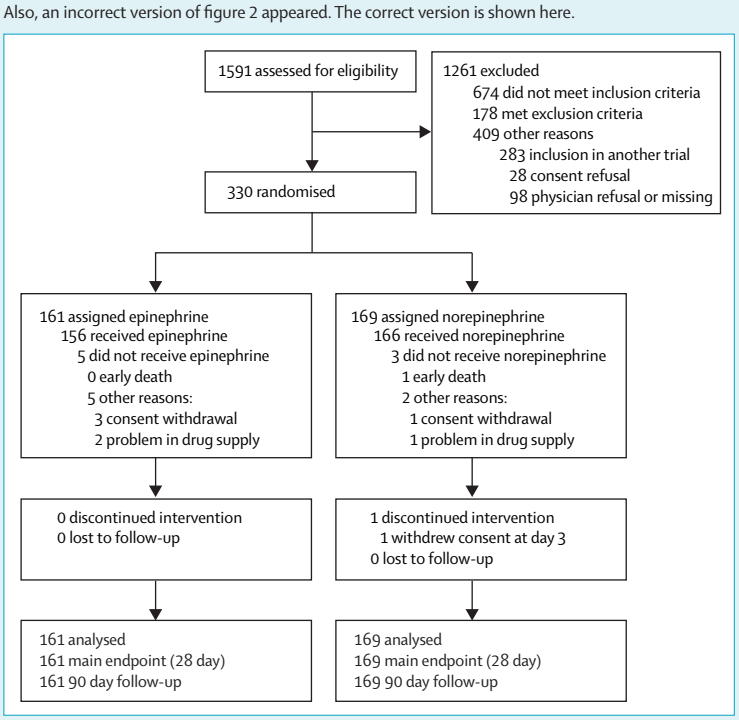

Figure 2: **Trial profile**