generalised epidemics.[13] We recommend that future versions of the ASSA model should also incorporate this capability of uncertainty analysis in order to present model estimates with 95% confidence bounds. This would greatly improve the comparison of ASSA outputs with empirical data from national population-based surveys.

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Professor Dorrington responds:

Three major concerns about the analysis and reporting of the latest HSRC results were raised in my article, namely the potential for bias (given the low response rates), the lack of acknowledgement of uncertainty in the results, and the use of the results from the 2002 survey as the basis for implying trend. None of these issues has been dealt with in a satisfactory manner by Rehle and Shisana.

Their response does not address the question of bias. However, through providing survey results not published in the report, they inadvertently provide grounds for further concern on this issue with their comparisons in Tables III and IV, which purport to show the similarity between estimates from the survey and those from the 2007 antenatal survey (to which they assume the 2008 survey will be 'similar'). Bearing in mind the need for upward adjustment of the 2007 antenatal survey figures,[1,2] both comparisons show the prevalence from the HSRC survey to be somewhat lower (2.7% lower than the correct figure for 2007[1,2] in the case of the black women aged 15 - 49 years) than the figures with which the authors argue they should be comparable. Of course, given that probably around 90%[3] of those tested in the national antenatal sample are black women, one must wonder why the authors chose not to use the prevalence among pregnant black women in their comparisons, as in past surveys (instead of pregnant women in Table III and all black women in Table IV).

The potential for bias is a crucial question deserving more debate. Arguments presented elsewhere by the authors (e.g. South African National AIDS Council and a UCT research seminar) that suggest that the survey is unbiased, either on the basis of research by Mishra and colleagues[4,5] into this question with respect to household prevalence surveys carried out as part of the DHS surveys or on the basis of comparisons of the characteristics of the people who answered the questionnaire but did or did not agree to be tested, are problematic.

To the criticism that it would be more useful and honest to acknowledge the uncertainty and publish the confidence intervals, the authors' response is that they regard 'epidemiological plausibility' as being 'more important' than statistical significance. They argue that the decrease in prevalence among children aged 2 - 14, from an implausible 5.6% in 2002 to a more sensible 2.5% in 2008, is 'real' based on the 'contextual evidence' that coverage of effective PMTCT programmes has increased. They present no quantitative evidence to explain how a programme preventing infection in infants and with low coverage between 2002 and 2005 might explain, in an 'epidemiologically plausible' way, that the bulk of the drop (5.6% to 3.3%) in prevalence among children aged 2 - 14 occurred between 2002 and 2005!

Similarly they argue that the drop in prevalence in the youth is plausible in the light of their 'reported substantive behavioural changes'. Ignoring the question about whether reported behaviour is actual behaviour, it is curious that the reported drop in knowledge (also statistically significant) in the youth, the increase in percentage of males with more than one partner in the past year, and the fall in the age of sexual debut of males are not mentioned as indicators of changes in sexual behaviour.

Furthermore, the argument that comparing the *trend* in prevalence in 15 - 19-year-olds as measured by the survey with the *trend* in the prevalence of 15 - 19-year-olds attending public antenatal clinics is 'like comparing apples and oranges' misses the point. If condoms are being used to prevent the spread of the disease and are the major source of contraception, then: (*i*) one would expect to see a change in the age distribution of women attending public antenatal clinics (which one doesn't see); and, more importantly, (*ii*) if the prevalence in young women is falling to the extent suggested by the report, then surely one would have expected to see the prevalence among pregnant women (as measured by the antenatal survey) falling too (which it doesn't appear to be doing). Unless, of course, the suggestion is that prevalence is only falling in women who wouldn't have fallen pregnant had they had unprotected sex!

Finally, of the concern that 2002 is used as a basis for inferring trend the authors point out, quite correctly, that the change in prevalence from the HSRC survey shown in Table III of the article is that for the population aged 2 and older, whereas it would be more appropriate to consider the change in prevalence for the population aged 15 - 49. However, they fail to remedy this error, described by them as 'some serious inaccuracies', by providing the figures for women aged 15 - 49, preferring to argue that the trend implied by differencing the prevalence rates from the 2002 and 2008 surveys is 'in good agreement' with the trend from the antenatal surveys on the grounds that 6 of the 9 provinces showed change in the same direction. Aside from the fact that the chances of getting such a result or better are about 75% if one allocates the up and down arrows randomly, their comparison misses the point. It was the conclusion, based on the comparison from 2002 that prevalence had *dropped* in 4 provinces, which was at issue. The table with the correct figures is reproduced below (Table I). It is interesting to note that not only do the corrected figures not change the argument, but in the case of 2 of the 4 provinces (Western Cape and Gauteng) the differences are even more marked.

Table I. Difference in prevalence (%), 15 - 49 years, HSRC (2002 - 2008) v. antenatal surveys (2002 - 2007)

Province	HSRC survey (2002 - 2008)	Antenatal survey (2002 - 2007)*
Western Cape	–7.9	+2.9
Northern Cape	–0.6	+1.5
Free State	–0.9	+2.7
Gauteng	–5.1	–1.0

*These values ignore the impact of the expansion of the sample in 2006 which if allowed for would probably increase these differences by at least 1% and by as much as 3% for the Northern Cape in particular.

Rehle and Shisana also argue that change in overall prevalence over the period of the two surveys is very similar. The problems with this argument are: (*i*) as mentioned in the footnote to Table I there was a significant change in the sample used by the antenatal survey in 2006 and this, if anything, probably leads to an underestimate of the trend between 2002 and 2007; and (*ii*) the prevalence in 2007 that is comparable to the 2002 figure is not 28.0% but 29.3%,[1,2] and hence the implied increase in prevalence in women attending public antenatal clinics is at least 2.8% (which is a good deal higher than the 1.3% they report for the national household prevalence survey).

It should be noted that none of the points above have been argued on the basis of a model (ASSA's or otherwise). My purpose was not to argue that models are better than empirical data or that the HSRC survey is wrong (at least not in any way that suggested fault on the part of the investigators) or that PMTCT and ARV aren't having an effect or that behaviour is not changing towards the less risky, but to suggest that interpretation of the results should be more cautious and scientific and prepared to acknowledge the limitations of the survey.

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