Novel findings in our population-based survey, which had participation rates of 48% in men and 54% (not 40%, as wrongly mentioned by Morris et al.) in women, suggest, but by no means prove, the existence of non-trivial associations of male circumcision with frequent orgasm difficulties in men and with a range of frequent sexual difficulties in women, including orgasm difficulties, dyspareunia and a sense of incomplete sexual needs fulfillment. Morris et al. should not be blamed for feeling unconvinced by our findings. However, as these critics repeatedly refer to Morris’ pro-circumcision manifesto as their source of knowledge, their objectivity must be questioned.

Morris et al. express concern over possible overfitting in our logistic regression models because we included a number of potentially confounding variables that differed between circumcised and uncircumcised men and between women with circumcised and uncircumcised spouses. However, as seen in Tables 3–6 of our paper, models with adjustment only for age provided odds ratios (ORs) similar to those obtained in the fully adjusted model, suggesting that this is mostly a theoretical concern. Next, Morris et al. suggest that we should have corrected for multiple testing even though such statistical manoeuvres are, at best, unnecessary and, at worst, deleterious to sound statistical inference in most epidemiological studies. Morris et al. also claim that prevalence ratios would have been more appropriate measures of association than ORs. However, despite Morris et al.’s firm statement to the contrary, there is nothing inherently inappropriate about using ORs in cross-sectional studies, even in situations with common outcomes. In such situations, however, ORs should not be misinterpreted as prevalence ratios. We would have been wrong to claim that our OR of 3.26 implied that frequent sexual difficulties were 3.26 times more common in women with...
circumcised spouses than in women with uncircumcised spouses. Nowhere in our paper did we interpret ORs in such a flawed manner. In accordance with the cited reference we simply noted that frequent sexual difficulties were more common in women with circumcised spouses and that the associated fully adjusted OR was 3.26.

Next, Morris et al. argue that our finding of considerably higher rates of frequent orgasm difficulties in (partially) circumcised than uncircumcised Danish men (11 vs 4%, OR = 3.26) may not apply in countries where circumcision means complete amputation of the foreskin. This may well be the case. If partial amputation of the foreskin truly entails frequent orgasm difficulties in a noticeable proportion of men (as experienced by 11% of circumcised men in our study), comparable proportions may well be larger and associated ORs even higher in countries where circumcised men experience greater tissue loss due to more extensive circumcision procedures. Obviously, more data are needed from rigorous studies using carefully constructed questionnaires. The questionnaires used to assess potential sexual problems in the two cited randomized controlled trials in Kenya and Uganda were not presented in detail in the original publications. Rather than blindly accepting such findings as any more trustworthy than other findings in the literature, it should be recalled that a strong study design, such as a randomized controlled trial, does not offset the need for high-quality questionnaires. Having obtained the questionnaires from the authors (RH Gray and RC Bailey, personal communication), I am not surprised that these studies provided little evidence of a link between circumcision and various sexual difficulties. Several questions were too vague to capture possible differences between circumcised and not-yet circumcised participants (e.g. lack of a clear distinction between intercourse and masturbation-related sexual problems and no distinction between premature ejaculation and trouble or inability to reach orgasm). Thus, non-differential misclassification of sexual outcomes in these African trials probably favoured the null hypothesis of no difference, whether an association was truly present or not.

Morris et al. should be commended for their creative attempt to dismiss the higher prevalence of frequent dyspareunia in women with circumcised (12%) than uncircumcised (4%) spouses (ORs between 4.17 and 9.00). They suggest that Danish women with circumcised spouses may be so psychologically troubled by the shape of their spouse’s penis that it might result in painful intercourse. A more plausible explanation would be that reduced penile sensitivity may raise the need among some circumcised men for more vigorous and, to some women, painful stimulation during intercourse in their pursuit of orgasm.

Two of the authors, Morris and Waskett, both internationally recognized circumcision activists, forget to declare their conflicts of interest. Even in situations that are out of context, Morris promotes himself as a neutral ‘authority on the extensive medical benefits of this simple surgical procedure’, whereas at the same time he argues that neonatal male circumcision ‘should be made compulsory’ and that ‘any parents not wanting their child circumcised really need good talking to’. In contrast, we conducted our survey without setting up any a priori hypotheses, because the limited and inconclusive literature on possible sexual consequences of circumcision would permit almost any imaginable a priori hypothesis. We had no intent to prove an already known ‘truth’ or disprove its contradiction. It is ironic that Morris et al. question the credibility of our findings, postulating that I have an ‘active involvement in opposition to male circumcision’. I have never expressed any objection, ethical, medical or other, against male circumcision as such. Unlike Morris, who believes that ‘circumcision is a biomedical imperative for the 21st century’, I could not care less whether fully informed, healthy adults choose to get circumcised or not. Likewise, when foreskin pathology is present (which does not include the physiological tightness of the foreskin experienced transiently by most boys), and the problem cannot be treated conservatively, preputioplasty or partial circumcision may be a relevant solution, even in minors and others who are unable to consent to the operation. However, because ethical discussions about ritual circumcision are sometimes distorted by strong personal views, I openly declared that I have participated in national debates over ethical issues surrounding male and female circumcision.

Like in critical letters to the editor following other recent studies that failed to support their agenda, Morris et al. air a series of harsh criticisms against our study. As seen, however, the points raised are not well founded. It seems that the main purpose, as with prior letters, is to be able in future writings to refer to our study as an ‘outlier study’ or one that has been ‘debunked’, ‘rejected by credible researchers’ or ‘shown wrong in subsequent proper statistical analysis’. This in spite of the fact that our study was carried out using conventional epidemiological and statistical methods, underwent peer-review and was published in an international top-ranking epidemiology journal. I would like to thank the IJE editors for withstanding the pressure from one particularly discourteous and bullying reviewer who went to extremes to prevent our study from being published. After the paper’s online publication, I have received emails from colleagues around the world who felt our contribution was useful and potentially important. One colleague informed me that the angry reviewer was the first author of the above letter to the editor. In an email, Morris had called people on his mailing list to arms against our study, openly admitting that he was the reviewer and that he had tried to get the paper
Humans and models: converging ‘truths’
From TIM A BRUCKNER\textsuperscript{1*} and CLAIRE MARGERISON-ZILKO\textsuperscript{2}

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\textsuperscript{*}Corresponding author. Program in Public Health & Department of Planning, Policy, and Design, University of California, Irvine, CA, USA and tary1 regarding our recent manuscript in which we
We thank Jay Kaufman for his thoughtful commen-
tation\textsuperscript{1} regarding our recent manuscript in which we
we called for critical letters in abundance to the
\textit{IJE} editors. Breaking unwritten confidentiality and
courtesy rules of the peer-review process, Morris dis-
tributed his slandering criticism of our study to people
working for the same cause. Rather than resorting to
such selective distribution among friends, Morris
should make both reviews freely available on the
internet by posting them in their entirety on
his pro circumcision homepage (www.circinfo.net).
Alternatively, interested readers should feel free to
request them from me at the e-mail address above.

Despite poorly founded criticisms and attempts at
obstruction our findings suggest that male circumci-
sion may be associated with hitherto unappreciated
negative sexual consequences in a non-trivial propor-
tion of men and women. Further carefully conducted
studies are needed.

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with many of Kaufman’s points, we would like to
respond to a key question that holds relevance to
most analyses using time series data: how should epi-
demiologists approximate the counterfactual value of
a population exposed at a specific point in time?

One approach to deriving counterfactual values in
time involves using a model-based framework. In
our analysis of the Cherokee response to acute and
large cash disbursements from a local Casino, we
employed a Poisson regression with a conventional
log-linear functional form.\textsuperscript{3} To control for confounding
by temporal patterns in accidental deaths (e.g.
seasonality), we included indicator variables for cal-
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