

LETTERS

# The circumcision controversy

## ■ TO THE EDITOR:

Kinkade and Meadows pose the question, “Does neonatal circumcision decrease morbidity?” (*J Fam Prac* 2005; 54 [1]:81–82). Their piece fails to answer their own question.

The authors uncritically accept Wiswell’s contentions<sup>1</sup> of a 10- to 12-fold increase in urinary tract infection (UTI). This paper essentially is a rewrite of Wiswell’s retrospective 1985 data,<sup>2</sup> which the task force on circumcision of the American Academy of Pediatrics (AAP) found to be methodologically flawed due to failure to control for numerous confounding factors such as breastfeeding.<sup>3</sup> The AAP does not recommend circumcision to reduce the incidence of UTI in infants.<sup>4</sup>

The authors propose circumcision to prevent penile and cervical cancer. The primary risk factors for both cancers are infection with human papilloma virus (HPV) (acquired through sexual intercourse) and the use of tobacco. Circumcision prevents neither sexual intercourse nor the use of tobacco. Also, a bivalent HPV vaccine has been successfully tested, which is expected to protect against these cancers.<sup>5</sup> The authors cite a meta-analysis that claims circumcision is protective against HIV infection. They overlook, however, the much better Cochrane Review that found all studies to be methodologically flawed due to failure to adequately control for the numerous confounding factors and concluded: “We found insufficient evidence to support an interventional effect of male circumcision on HIV acquisition in heterosexual men.”<sup>6</sup>

Van Howe calculated that that the practice of neonatal circumcision costs 15.30 patient well-years per 1000 patients, an indication of significant morbidity.<sup>7</sup> Van Howe’s cost-utility analysis concluded that the adverse effects of male neonatal circumcision outweigh any benefits by a wide margin. Infant circumcision causes a dramatic increase in post-circumcision healthcare costs.<sup>7</sup> We, therefore, answer the authors’ unanswered question as follows: *No, neonatal circumcision causes an increase in child and adult morbidity and mortality.*

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## ■ DR KINKADE RESPONDS:

I appreciate that the comments allow amplification of points made in my clinical inquiry. It is clear that discussions of the science regarding neonatal circumcision generate extremely strong opinions. I was disappointed that their letter describes me as biased and claims that I ignored the majority of the medical literature on circumcision. I neither promote nor criticize circumcision in my practice.

Ms. Meadows, as the expert search librarian, conducted an extensive evidence-based search using the standard Clinical Inquiries database protocol. For each article that is included, I read at least 10 to 15 additional articles. It would be nice to cite all appropriate evidence; however, the *JOURNAL OF FAMILY PRACTICE* limits these reviews to 700 words (including the references) and 10 references. For comparison, the letter that I received was about 720 words.

Since my assigned topic was about potential benefits of circumcision, I did not comment on the risks that Drs Denniston and Hill mention. That would be the other half of the equation that one would need to make a decision about circumcision.

Regarding the increased risk of HPV infection among uncircumcised men, the correspondents suggest I cited a flawed study that had the most extreme findings.<sup>8</sup> This study from the International Agency for Research on Cancer (IARC) was robust and had the best external validity (including 1520 men from 5 different countries) and maintained internal validity. The attempt to adjust the results from this study based on results from another HPV detection study<sup>9</sup> is completely unsound. The IARC study obtained 2 HPV samples from the penis: one from the glans/coronal sulcus and the other from the distal urethra. The study by Weaver obtained samples from the penile shaft, foreskin, glans, scrotum, and urine. Specifically, the study by Weaver did not include samples from the distal urethra, which, as they note, yields detection rates of 12% to 31%. The attempt to correct the IARC results by dividing by half does not take into account the increased yield obtained from urethral specimens. The results of the IARC study are not the “most extreme.” Where the study I cited shows a protective effect of circumcision for HPV infection with an odds ratio of 0.37 (95% confidence interval [CI], 0.16–0.85), a well done study by Svare<sup>10</sup> reports an odds ratio (OR) of 0.2 (95% CI, 0.06–0.6) and another by Baldwin<sup>11</sup> reports an OR of 0.35 (95% CI, 0.21–0.57). Furthermore, the technique for obtaining HPV samples in the Svare study is similar to that recommended by Denniston and Hill (swabs from the penile shaft/perineal region and from the glans/coronal sulcus).

I appreciate the authors alerting me to the recently published update of the Cochrane review on circumcision and the decreased risk of HIV infection.<sup>12</sup> This is an issue that is confusing in the sense that it is difficult to arrive at a true point-estimate of the benefit. Almost all epidemiological studies of HIV risk and circumcision status show a benefit to circumcision. For this reason, 3 large (n=2276, 2500, and 3500) randomized clinical trials are underway in Africa to try and definitively quantify the benefit of circumcision with regards to HIV transmission.<sup>12</sup> Until those trials are completed in 2006–2007, I agree that the evidence is not strong enough to actually recommend circumcision as a means of preventing HIV infection. The criticism that the meta-analysis of HIV risk I cited<sup>13</sup> does not include the “largest study” (by Grosskurth<sup>14</sup>) addressing this question is defended by the authors of the meta-analysis. The group that published the meta-analysis also published the study that is purportedly the largest. Why did they not include their own study? The study “was not included as the study population formed the basis for a case-control study that allowed adjustment for confounding variables.”<sup>13</sup> They included their own follow-up study that was stronger methodologically.<sup>15</sup> The Cochrane review on this topic<sup>12</sup> faults the Grosskurth report because it reported a “harmful effect of circumcision” with “an adjusted OR of 1.25, but did not report CIs.” Contrary to the claims by Denniston and Hill that the largest study on this topic was not included is misinformed; the largest study is by Kelly<sup>16</sup> has about 1000 more subjects and was included in the meta-analysis<sup>13</sup> and the Cochrane review.<sup>12</sup> The meta-analysis was cited because it was well done and provided a point estimate. The Cochrane reviewers describe a similar protective effect, but because there is heterogeneity of the studies and there are randomized controlled trials pending, they chose not to combine the results in a meta-analysis. Their systematic review is illuminating and worth commenting on to put perspective on this issue. Of the 18 low-risk (general population) studies they include, 11 of 18 showed a protective effect of circumcision. Nine of these had statistically significant results; 7 in favor of circumcision and 2 showing no benefit. Of the 18 studies in high-risk groups, all show a protective effect of circumcision, with 13 of these reaching statistical significance.

I appreciate the opportunity to defend the Clinical Inquiries system of evidence-based answers. Although the practice of neonatal circumcision is lacking in randomized clinical trials, I feel that reasonable assessments of its risks and benefits and be made.

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