Authors ignored main conclusion of study that they cited

EDITOR,—Paul M Fleiss and Frederick Hodges claim that epidemiological studies long ago disproved the "myth" that neonatal circumcision has a protective effect against penile cancer.[1] They quote only one such study, that of Maden *et al*,[2] and, curiously, omit its main conclusion—that "absence of neonatal circumcision and potential resulting complications are associated with penile cancer." The odds ratio for those never circumcised compared with those who had undergone neonatal circumcision was 3.2 (95% confidence interval 1.8 to 5.7), while for those circumcised later it was 3.0 (1.4 to 6.6).

I know of no analytical epidemiological study whose results support Fleiss and Hodges's claim.

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1 Fleiss PM, Hodges F. Neonatal circumcision does not protect against penile cancer. *BMJ* 1996;312:779-80. (23 March.)
2 Maden C, Sherman KJ, Beckmann AM, Hislop TG, Ten CZ, Ashley RC, *et al.* History of circumcision, medical conditions and sexual activity and risk of penile cancer. *JNCI* 1993;85:19-24.

Authors' reply

EDITOR,—Edgar J Schoen states his support of Wolbarst's uncontrolled, epidemiologically flawed study. Using the same pseudoscientific methods, Wolbarst declared: "It is generally accepted that irritation derived from a tight prepuce may be followed by nervous phenomena, among these being convulsions and outbreaks resembling epilepsy. It is therefore not at all improbable that in many infants who die in convulsions the real cause of death is a long or tight prepuce."[1] Does Schoen's uncritical admiration for the scientific methods of Wolbarst lead him to accept these concepts as well?

The results of Kochen and McCurdy's number shuffling are invalid, for they are based on the false premise that only uncircumcised men can develop penile cancer. Case reports of penile cancer in circumcised men abound and may represent only a tiny fraction of unreported cases. Some forms of carcinoma in situ are seen primarily in men who were circumcised as neonates.[2]

Alan Stanton will be interested to learn that if the results of Maden *et al*'s study are controlled for age the seemingly higher rate among elderly men who have not been circumcised is shown to be the result of the formerly low rate of neonatal circumcision among the rural uneducated classes.[3] Most newborn American male babies were not subjected to circumcision until the 1950s. If the associative risk factors persist then the future rates of penile cancer among men who were circumcised at birth in the 1950s and '60s will be equal to the present rates among elderly men born in the 1920s who were not circumcised.

Circumcisionists have not provided an aetiological model for penile cancer. The myth of smegma as a carcinogen is disproved.[4] Studies of clusters without reference to valid aetiology can be used to make any association seem significant. On the basis of any of Schoen's selected references it would be equally valid to claim that abstinence from pork prevents penile cancer. Smoking, however, is a genuine risk factor for cancer. None of these studies looked at the role of tobacco as an aetiological factor. Studies from countries without a history of circumcisionist domination have found long term tobacco use to be the single most important factor for penile cancer.[5] Thus the only option for those with a genuine interest in preventing penile cancer is to campaign actively against tobacco. We are confident that this will interest Schoen and Stanton, for we sincerely hope that they are more interested in preventing penile cancer than in perpetrating unethical, destructive, mutilative, antisexual, Bronze Age blood rituals on defenceless children.

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1 Wolbarst AL. Universal circumcision as a sanitary measure. *JAMA* 1914;62:92-7.
2 Wade TR, Kopf AW, Ackerman AB. Bowenoid papulosis of the penis. *Cancer* 1978;42:1890-903.
3 Maden C, Sherman KJ, Beckmann AM, Hislop TG, The CZ, Ashley RL, *et al.* History of circumcision, medical conditions, and sexual activity and risk of penile cancer. *JNCI* 1993;85:19-24.
4 Reddy DG, Baruah IK. Carcinogenic action of human smegma. *Arch Pathol* 1963;75:414-20.
5 Hellberg D, Valentin J, Eklund T, Nilsson S. Penile cancer: is there an epidemiological role for smoking and sexual behaviour? *BMJ* 1987;295:1306-8.

Prenatal and postnatal prevalence of Turner's syndrome

Data presented were insufficient to challenge specificity of prenatal diagnosis

EDITOR,—Claus Højbjerg Gravholt and colleagues challenge the specificity of prenatal examination in the diagnosis of Turner's syndrome.[1] This challenge is not supported by the data provided.

Firstly, postnatal karyotyping was performed in only 13 of 100 cases. Postnatal karyotypes in cases of fetal death or terminated pregnancy (76%) were unavailable. Fetal death occurs more commonly in non-mosaic Turner's syndrome. Termination is more likely when there are additional ultrasonographic findings. The authors did not correct for anatomical defects detected after cytogenetic diagnosis. These are more likely to occur in non-mosaic aneuploidy.[2] The cases in which postnatal karyotyping was performed are therefore not representative of the whole group.

Secondly, the diagnostic test that led to the erroneous antenatal diagnoses (amniocentesis or chorionic villus sampling) was not specified. It is well recognised that Turner's syndrome in any form cannot be reliably diagnosed from direct preparations of chorionic villi.[3]

Thirdly, mosaicism was involved in all the cases in which there was a discrepancy between prenatal and postnatal karyotypes. Mosaicism in chorionic villi is not at all rare and is usually confined to the placenta. Mosaicism in amniotic fluid cells is rare but usually real. The finding of any form of mosaicism in amniotic fluid cells poses problems with regard to counselling since the phenotype in true mosaicism is extremely variable.[2] Parents should be informed about the inherent possibility of another karyotype being found postnatally.

We recently performed a survey among all Dutch centres performing prenatal diagnosis to study the decisions made by parents when sex chromosome aneuploidy was detected prenatally. During the study (1988-93) 62 of 96 couples who had been given a diagnosis of Turner's syndrome decided on termination (54 cases of 45,X karyotype and eight of mosaic Turner's syndrome karyotype). Twenty four pregnancies were terminated after chorionic villus sampling (all cases of 45,X karyotype) and 38 after amniocentesis. The remaining 34 couples decided to continue the pregnancy (18 cases of 45,X karyotype and 16 of mosaic Turner's syndrome karyotype). Overall, in this study postnatal karyotyping was successful in 94 of the 179 patients with any prenatal diagnosis of sex chromosome aneuploidy. Ten discrepancies involving Turner's syndrome were found. Six were found on chorionic villus sampling (of which three involved mosaicism) and four on amniocentesis (all of which involved mosaicism). Non-mosaic diagnoses of Turner's syndrome in amniotic fluid were always confirmed.

In conclusion, the eight discrepancies between the prenatal and postnatal karyotypes reported by Gravholt and colleagues represent insufficient evidence to challenge the prenatal diagnosis of Turner's syndrome by amniocentesis.

Data were collected by the Dutch Working Party on Prenatal Diagnosis.

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3 Leschot NJ, Wolf H, Verjaal M, van Prooijen-Knegt AC, Boer K. Monosomy X found at first trimester CVS: a diagnostic and counselling dilemma. *Clin Genet* 1990;37:236-7.

Authors' reply

EDITOR,—We will be most interested to see the results of C J M van der Sijs-Bos and colleagues' study when they are published. The authors found 96 pregnancies involving a Turner's syndrome karyotype, and in 62 cases the pregnancy was terminated (unfortunately, no information is given about spontaneous abortion). In 34 cases the pregnancy continued. In 10 (10%) of the 96 cases there was a discrepancy between the results of prenatal and postnatal karyotyping (in four cases after amniocentesis and in six cases after chorion villus sampling). The true percentage is probably higher because not all cases were ascertained postnatally, though the exact figure is not given in the letter. It is thus impossible to discern exactly how large the discrepancy is between prenatal and postnatal testing after amniocentesis and after chorion villus sampling. Unfortunately, no karyotypes are given in the letter.

Van der Sijs-Bos and colleagues do not discuss the main evidence from our study—the prevalence study. Here we found that among infant girls the prevalence of Turner's syndrome was 32/100 000. When female fetuses tested by amniocentesis were compared with those in untested pregnancies (after the exclusion of cases referred for prenatal testing because of the results of ultrasound scanning) the relative risk of the syndrome was 5.68. Among female fetuses tested by chorion villus sampling the relative risk was 13.3. In our paper, as an adjunct to the prenatal study, we also presented supplementary evidence from our study of 24 liveborn fetuses with a prenatal diagnosis of Turner's syndrome karyotype (initially ascertained by amniocentesis in 22 and by chorionic villus sampling in two). We included this study to see whether it refuted the surprising results of the prevalence study. This was not the case.