When comparing the recent report by Olsen *et al*[3] with the original paper by Carlsen *et al* I noticed differences in the graphs. Figure 1 of the original paper contains data for only 31 of the 61 publications listed in the table: 30 data points are missing. Furthermore, the difference in the circles' areas is greater than expected: the ratio of the maximum to the minimum number of subjects in the studies, expressed as a logarithm, is 4·3, but the difference in the areas of the circles is far larger (fig 1a). Using the data given in the original paper's table, I redrew the figure (fig 1b). The overall impression is quite different.

I wonder about the reason(s) for these mistakes. As this paper had a considerable impact not only in the scientific community but also in the lay press, it is difficult to comprehend why these severe errors have been overlooked both before and after publication.

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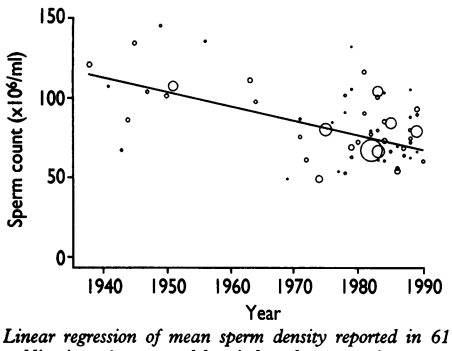
1 Carlsen E, Giwercman A, Keiding N, Skakkebaek NE. Evidence for decreasing quality of semen during past 50 years. *BMJ* 1992;305:609-13.
2 Brake A, Krause W. Decreasing quality of sperm. *BMJ* 1992; 305:1498.
3 Olsen GW, Bodner KM, Ramlow JM, Ross CE, Lipshultz LI. Have sperm counts been reduced 50 percent in 50 years? A statistical model revisited. *Fertil Steril* 1995;63:887-93.
4 Bromwich P, Cohen J, Stewart I, Walker A. Decline in sperm counts: an artefact of changed reference range of "normal"? *BMJ* 1994;309:19-22.

Authors' reply

EDITOR,—As Alexander Lerchl points out, figure 1 of our overview indicating that sperm concentrations have decreased during the past 50 years is deficient. During the final preparation for publication, for reasons that we cannot trace, some of the points were omitted. The regression analysis in the paper is unaffected by this: the new regression line (weighted by the number of subjects) had a slope of $-0{\cdot}934\times10^6$/ml per year (SE 0·157; $P<0{\cdot}0001$), and that line was correctly included in our original figure. A better impression of the regression analysis is provided by the figure in this letter, in which the areas of the circles are proportional to the number of subjects in each publication. There is no reason for Lerchl's scepticism.

Lerchl quotes several criticisms of our paper but omits our detailed and specific responses as well as the subsequently published empirical evidence, which points in the same direction as our paper.

Specifically, Lerchl quotes Brake and Krause, who, on the basis of our data, claimed that sperm concentration had significantly increased since 1970. In fact, Brake and Krause made a mistake in their calculation: the increase they quoted is non-significant ($P=0{\cdot}36$). Lerchl quotes Bromwich *et al*, who offered a speculative, elementary statistical argument with no empirical basis or verification. Lerchl fails to quote our earlier detailed comments on this theoretical exercise.[1,2] Lerchl finally quotes the recent report by Olsen *et al*, who also did not add new empirical evidence: they performed various unsurprising reanalyses of our data, all of which agreed about a significant decline in sperm concentration. We have submitted detailed comments on these reanalyses elsewhere.



Linear regression of mean sperm density reported in 61 publications (represented by circles whose area is proportional to number of subjects in study), each weighted according to number of subjects, 1938-90

Lerchl omits to refer to the additional empirical evidence that has been published. Auger *et al* (who were originally motivated by serious scepticism about our original report) studied 1351 healthy men volunteering to donate sperm in one clinic in Paris between 1973 and 1992.[3] Carefully separating age effects from cohort effects (year of birth), they documented a highly significant decrease in sperm count of 2·1% per year (from 89×10^6/ml in 1973 to 60×10^6/ml in 1992) and concomitant decreases in the percentages of mobile and normal spermatozoa. Three additional, shorter reports have been published, also based on data from one clinic and all with similar conclusions.

In a recent international effort the temporal trends in semen quality were viewed in a broader context.[4] There have been similar temporal increases in the incidence of testicular cancer and frequently of hypospadias and cryptorchidism, and geographical covariation of several of these symptoms as well as male breast cancer has been documented. In our view it would be irresponsible to disregard this evidence, even if the link to possible determinants is far from definitively established.

Although Lerchl points out a (qualitatively unimportant) deficiency in figure 1 of our paper, we hope that this will not delay a dedicated, wide ranging research effort to clarify these issues.

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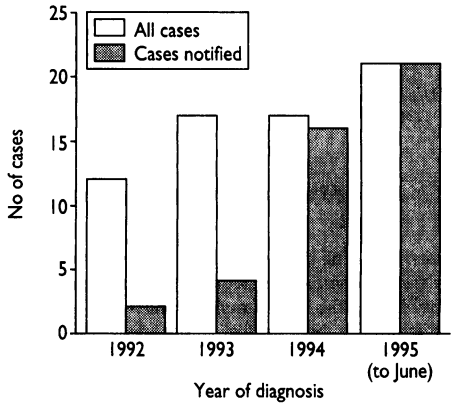
1 Keiding N, Giwercman A, Carlsen E, Skakkebaek NF. Importance of empirical evidence. *BMJ* 1994;309:22. [Commentary on: Bromwich P, Cohen J, Stewart I, Walker A. Decline in sperm counts: an artefact of changed reference range of "normal"? *BMJ* 1994;309:119-22.]
2 Skakkebaek NE, Keiding N. Changes in semen and the testis. *BMJ* 1994;309:1316-7.
3 Auger J, Kunstmann JM, Czyglik F, Jouannet P. Decline in semen quality among fertile men in Paris during the past 20 years. *N Engl J Med* 1995;332:281-5.
4 Male reproductive health and environmental oestrogens [editorial]. *Lancet* 1995;345:933-5.

Reasons for increased incidence of tuberculosis

Audit suggests that undernotification is common

EDITOR,—In her editorial Janet H Darbyshire suggests that undernotification of tuberculosis, particularly in association with HIV infection, is still common.[1] If sufficiently widespread, undernotification could result in underestimation of the incidence of tuberculosis, particularly in patients coinfected with HIV, with considerable public health implications. We recently audited notification of tuberculosis in patients known to be infected with HIV who were attending our hospital.

A database on all patients with mycobacterial infection was established by searching microbiology, histopathology, and clinical computerised records systems. Case notes were then examined for all patients. Patients were considered to have tuberculosis on the basis of a positive result of culture of a specimen from any site or either histological or radiographic changes compatible with tuberculosis and a response to standard antituberculous treatment. This database was then cross referenced with a record of notifications for the whole hospital. The figure shows the results.



Number of cases of tuberculosis diagnosed and notified in patients with HIV infection, 1992-5

Tuberculosis was considerably undernotified in 1992. The reasons for this were not clear from this audit, but the appointment of a clinical nurse specialist who had specific responsibility for notification of, and contact tracing in, cases of tuberculosis and HIV infection led to a substantial improvement in the rate of notification. This suggests that clinicians' concerns about patient confidentiality were not the prime reason for undernotification. In addition, a considerable increase in the numbers of cases of tuberculosis in patients also infected with HIV has been seen this year. Although the number of notifications of tuberculosis from our hospital has risen, from 99 in 1992 to 60 in the first six months of this year, the proportion of patients with HIV infection has increased from 17% to 32% over the same period. This seems to be due to increased screening for HIV infection in patients with tuberculosis. We have thus shown that although undernotification of tuberculosis in patients with HIV infection occurs, improved notification may also lead to increased recognition of coinfection with HIV.

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1 Derbyshire JH. Tuberculosis: old reasons for a new increase. *BMJ* 1995;310:954-5. (15 April.)

Large immigrant population may have confounded study

EDITOR,—N Bhatti and colleagues present an interesting analysis of changing rates of notification of tuberculosis based on national notifications and local data from Hackney.[1] The findings are interpreted as suggesting that the national increase is largely due to socioeconomic factors that have affected the white population and established ethnic minority communities to a similar extent. The authors suggest that recent immigration has made only a small contribution to this increase. The study's findings do not justify these conclusions.

As quoted in the paper, markers of socio-