

The IUD and Recovery Of Fertility

A digest that appeared recently in *Family Planning Perspectives* [A first pregnancy may be difficult to achieve after long-term use of an IUD, 2001, 33(4):181–182] summarizes at length and with accuracy a report by Helen Doll and coauthors that was originally published in the *British Journal of Obstetrics and Gynaecology*.¹ That report concluded that “long term use of an intrauterine device by a nulliparous woman increases the risk of impairment of fertility to a clinically important extent.” Because readers of *Family Planning Perspectives* may have limited access to articles and letters appearing in British journals, they would likely not be aware of our reanalysis of the data and our challenge to the authors’ interpretation of their results.²

Specifically, the researchers “assessed fertility by calculating the time taken from stopping contraception to plan pregnancy until the delivery at term.” Their finding is entirely statistical, and we reached quite different statistical conclusions in our reanalysis. We used the number of women still at risk, as shown by the authors in their tables, to recalculate the rates and to estimate standard errors. We found no statistically significant difference between long-term IUD users and long-term pill users at any time in the proportions of women with no delivery, nor was there a significant age difference. Indeed, we found a significant difference between short-term use of IUDs and long-term use through 30 months, but there was no difference thereafter in the proportion who did not give birth. Further, there was a significant difference in age between short-term IUD users and long-term users ($p=.035$). The longer-term IUD users were older when they discontinued use to plan a pregnancy.

Long-term IUD users were significantly older than were barrier method users ($p=.00004$), whom the authors did not an-

alyze by use-duration. Through 30 months after discontinuation, long-term IUD users had significantly higher rates of having had no birth, but by 36 months and thereafter, there was no statistically significant difference in these rates.

Tables 3 and 4 of the original paper showed that being 35 or older at discontinuation for planned pregnancy was the strongest factor adversely associated with having a term birth. IUD users represented fewer than one-third of all women studied, but were 50% of women aged 35 and older and were 46% of women aged 30–34 at discontinuation.

What Doll and her coauthors show is that younger nulliparous women conceive rather speedily, regardless of the contraceptive method they had been using previously. They have not demonstrated differences by contraceptive method in the proportions of women who have not had a child at three years or thereafter, nor have they shown any differences between IUD users and pill users. Moreover, they also have not demonstrated that the higher proportions of long-term IUD users who in the first 12–30 months following discontinuation have not had a child can be attributed to disease or to disease sequelae. Partnership dissolution (either marital or nonmarital) is not addressed, nor are other social circumstances, including changes of mind, which might well affect women who have remained childless through their early 30s.

The data strongly suggest that whatever differences exist in the recovery of fertility in this study among former barrier method users and former pill and IUD users are associated with age. The conclusion that nulliparous women should avoid long-term IUD use is therefore unwarranted by these data.

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References

1. Doll H, Vessey M and Painter R, Return of fertility in nulliparous women after discontinuation of the IUD: comparison with women discontinuing other methods of contraception, *British Journal of Obstetrics and Gynaecology*, 2001, 108(3):304–314.

2. Sivin I, Webb A and Stedman Y, Letter to the editor, *British Journal of Obstetrics and Gynaecology*, forthcoming.

The researchers respond:

While it is important that the issues raised in our recently published paper are discussed,¹ we believe that Sivin and colleagues, both in their letter to the *British Journal of Obstetrics and Gynaecology*² and in their letter here, misunderstand our statistical analyses and our findings.

The main misunderstanding centers on the general principles of survival analysis. Sivin and colleagues write that we simply compared the time taken to deliver between selected groups of subjects, with our findings being “entirely statistical.” As we stated in the article, this time to delivery was calculated as the time from stopping contraception to plan a pregnancy until term delivery; as in our previous analyses,³ we do not have information on the date of the last menstrual period before a pregnancy occurred.

In particular, it seems that the authors of the letter believe we did not take into account whether a woman remained in the study during follow-up, and thus was still “at risk.” The statistical methods section of our paper clearly states that we used routine life-table techniques in our analyses of the time taken to deliver—which, by definition and as stated, take into account women who were censored and who left the analysis at either the end of follow-up, loss to follow-up or resumption of contraception. Tables 1 and 2 in our original paper were for information only, and contain no other details (such as when women were censored and thus not at risk) that we did not use ourselves. Sivin and colleagues’ “reanalysis” of our data, while being unnecessary, is also inappropriate, since survival data should be analysed in terms of the whole survival curve rather than at specific follow-up points. In addition, the reanalyses are incorrect in terms of the estimated numbers of censored subjects, as well as being unadjusted for the effects of other variables.

Indeed, the letter authors largely disregard and apparently misunderstand the analyses we used to estimate relative survival experience (i.e., that independent of the effect of other factors) using multiple Cox regression. It would be entirely incorrect to conclude, on the basis of unadjusted data, that there is an association between increased risk of fertility impairment in nulliparous women and any

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one of the number of interrelated factors found to have such an individual association (Tables 2 and 3). However, almost all of Sivin and colleagues' arguments and all of their calculations are based on these unadjusted data; they note from our adjusted analyses (Table 4) only that age (and in particular being 35 or older) had the strongest effect on outcome. Yet only 10 subjects were 35 or older, with the effect being confined solely to women of this age and there being no significant linear trend across age-groups. Moreover, the significant effect of contraceptive method and duration of IUD use remained after we adjusted for age as well as other potentially confounding factors, such as the presence of gynecologic and selected other illnesses, which were more prevalent in women who had been using barrier methods of

contraception than among other women.

While factors other than fertility impairment, such as partnership dissolution and other social circumstances, could indeed explain reductions in conception and delivery, such factors would likely affect all women and their partners to the same extent, irrespective of the method of contraception that they stopped using. As stated in our paper, our study was a purely observational one and not intended to provide information on the reasons for the observations observed.

In conclusion, while we appreciate the attention given to our paper, it is unfortunate that this attention is based on apparent misunderstanding of both our statistical methods and data interpretation. These issues are considered in greater depth elsewhere.⁴

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