LETTERS

Studying the Health Effects of Induced Abortion

A great deal of research has been conducted on the question of possible adverse health effects of induced abortion. Unfortunately, much of it has serious methodological problems or is irrelevant to today's conditions. A careful analysis of the literature established minimum methodological standards for research examining the effect of induced abortion on subsequent pregnancies.[1] These criteria eliminated studies that were subject to recall bias, that evaluated outmoded abortion procedures, that commingled spontaneous and induced abortions and that used inappropriate comparison groups.

A digest that appeared in a recent issue of *Family Planning Perspectives* [After abortion, Danish women's odds of preterm delivery are doubled, 2000, 32(4):200] describes two research studies—one involving the impact of induced abortion on the duration of subsequent pregnancies[2] and one on the effect of abortion on low birth weight[3]—that violate the last of these standards. The researchers compare first births among women who have had an induced abortion with second and subsequent births among women who have never had an abortion. It is well known that first births are more risky than later births, and it is impossible in these studies to distinguish the risk associated with a first birth from the risk resulting from a prior abortion.

In the case of low birth weight, the data presented in the original study suggest that the identified risk may be completely unrelated to abortion. While 5.0% of first births among women with one prior abortion were low birth weight (Table 3 of the article),[4] the same was true of 5.2% of the first births among those in the control group (Table 1 of the article).[5] The analysts find an elevated risk when they compare women whose first pregnancy was terminated with women having their second or subsequent birth, among whom the rate of low birth weight is 3.5–4.7%.

In fact, women with a prior abortion were more likely to smoke; when this is taken into account, the data suggest that a previous abortion may have been somewhat protective against low birth weight when the first birth occurred within six months of the abortion. However, a multivariate analysis would need to be carried out for this hypothesis to be tested properly.

The analysis focusing on premature and postterm delivery suffers from the same methodological weakness, although the difference between first births and subsequent births is not as great as it was with low birth weight. Even if an appropriate comparison showed increased risks associated with abortion, however, the finding that women who had had an abortion were at heightened risk of either preterm or postterm delivery would be unconvincing because of the absence of controls for other risk factors. For example, the dataset contains no measure of smoking status, although the authors recognize that smoking is correlated with having had an abortion.

What the researchers have shown is that induced abortion does not protect against the risks of delivery as much as a prior birth does. This finding has little practical importance, since few women look to abortion to reduce the risk of their first birth. The way in which the studies were presented, however, would lead most readers to conclude that having an abortion increases the risk of an adverse outcome associated with their first birth. Such misleading reports can cause unnecessary anxiety to patients and can also have political repercussions. (Already, antichoice groups in the United States, pointing to one of these studies,[6] have argued that state laws should require physicians to warn patients that an induced abortion would increase the risk of prematurity in future births.) Investigators studying the potential complications or side effects of politically sensitive procedures such as induced abortion have a special duty to be certain that their research approach is free of bias and is presented in a way that is not conducive to misinterpretation.

Stanley K. Henshaw
The Alan Guttmacher Institute
New York

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3. Zhou W, Sorensen HT and Olsen J, Induced abortion and low birth weight in the following pregnancy, *International Journal of Epidemiology*, 2000, 29(1):100–106.

4. Ibid., p. 103.

5. Ibid., p. 102.

6. Zhou W, Sorensen HT and Olsen J, 1999, op. cit. (see reference 2).

The authors reply:

While we controlled for gravidity in our study, not for parity, it is possible that this does not provide the best adjustment for the background risk associated with reproductive history. Whether adjustment for parity will provide an unbiased adjustment is, however, also open to question.[1]

The association seen between parity or gravidity and reproductive failures may reflect selection rather than risk. Reproduction is being planned and controlled partly by using the reproductive history, which unfortunately may destroy the ability to make unbiased observational analyses, regardless of the adjustments made. It is possible, perhaps even likely, that these forces of selection act differently for women who choose an abortion than for women who do not. It was for this reason that we chose to study only the first part

of the reproductive career for the cohort members. In light of these limitations, conclusions must be drawn with caution; we stress that our results were seen only for subgroups of the population, and that the overall risk was small and detectable only in a very large study. Most other studies have been too small to detect a risk of this magnitude, including those that we conducted in China.[2]

In our study, we actually also compared results with the first pregnancy among controls and found similar results for preterm birth but not for low birth weight, as Henshaw has indicated. Although these analyses were made for women of the same parity (nulliparous women), the results may still be subject to bias, since controls were selected only if they had a subsequent pregnancy. Unlike Henshaw, we are not convinced that this comparison is better that the one we make in the tables.

We also performed sensitivity analyses to estimate the possible effect of confounding by smoking. Our results indicate that it is unlikely that smoking explains all of the excess risk seen in the studies.[3] A fully adjusted analysis should include data from the entire pregnancy period, on the use of contraceptive methods, about exposure to all confounders and on the desire for a given family size. It is possible that in such an analysis the results may change in either direction.

Weijin Zhou
Jørn Olsen
Danish Epidemiology Science Centre
University of Aarhus
Aarhus, Denmark

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Are U.S. Women Interested In Long-Acting Methods?

The article by Koray Tanfer and colleagues [Why are U.S. women not using long-acting contraceptives? 2000, 32(4):176–183 & 191)] asks worthwhile questions but bases its answers on outdated and incomplete information.

The data that they use in their analysis cover the years 1993–1995, the first three years in which the contraceptive injectable Depo-Provera was on the market in the United States. They do not acknowledge that today, five years later, the injectable owns about 10% of the prescription birth control market in the United States.[1] In addition, during the past 12 months, new prescriptions are up 15%.[2]

Furthermore, the data used in the article were collected from women who were 20–37 in 1991. Long-acting methods are well-suited for women who want to postpone pregnancy for several years. Market research shows that women who are younger than 20 or older than 37 are also satisfied users of this method.[3] The needs and concerns of neither of these age-groups are represented in the data, providing a skewed view of the use of the hormonal injectable.

Government studies show that the injectable follows only oral contraceptives and the condom in frequency of use: the diaphragm, the implant and the IUD all are used less frequently than the injectable.[4] Studies also have shown that long-acting contraceptives have helped reduce the pregnancy rate among young women in the United States.[5] In addition, while sterilization is the most commonly used method of contraception in the United States, recent studies have shown that more than 10% of women younger than 30 who have been sterilized regret their decision.[6] Clearly, U.S. women need more contraceptive choices.

The hormonal injectable presents a viable, effective and convenient option. By being based on outdated and incomplete information, the article by Tanfer and colleagues does a disservice to American women by drawing conclusions that do nothing to promote U.S. women's contraceptive options.

Kristin Elliott
Senior Director,
Public Relations and Patient Education
Pharmacia Corporation
Peapack, NJ

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Don't Downplay Market For the Hormonal Injectable

As a practicing gynecologist, I would like to offer some comments on the article by Koray Tanfer and colleagues on women's use of injectables and implants [Why are U.S. women not using long-acting contraceptives? 2000, 32(4):176–183 & 191)]. Few women in the study were using depot medroxyprogesterone acetate (marketed as Depo Provera), an injectable hormonal contraceptive that provides contraceptive protection lasting three months. The authors conclude as a result that "long-acting reversible contraception has not fulfilled its promise" [page 183].

This research has many flaws, however. Only women aged 24–41 were surveyed, thus omitting a huge number of potentially sexually active women at both ends of the reproductive spectrum. Also, the interviewing was done in 1995, when more than one-third of the women surveyed said they either had never heard of Depo Provera or lacked sufficient knowledge of the method. Since the injectable (while used by millions of women worldwide for decades) was only introduced in the United States in 1993, this certainly does not allow much time for a contraceptive method to have caught on.

Another flaw is the authors' focus on amenorrhea as one of the major side effects among injectable users. This seems inappropriate: Once most women are counseled that not getting a period is normal during injectable use, they are comfortable about not having regular menses. Clearly, when they are educated about potential side effects, most women do well. Indeed, the authors cite that "fewer than 5% of users who reported side effects said they would stop using the injectable within the year" [page 179].

As a health care provider in the United States, I believe that we have an obligation to educate women on all methods of contraception. There is also a strong need to do so, with about one-half of all pregnancies in this country being unintended. While many women who become pregnant may describe themselves as "using contraceptives" when they conceived, those methods may never have made it out of the drawer (in the case of the diaphragm or the condom), they might have been defective (a condom with

a hole) or they might have been used incorrectly (some missed birth control pills).

An injectable method has the advantage of not letting contraception depend on using a diaphragm or on remembering to take a pill. It also allows women who are not quite ready to decide about sterilization to postpone that decision but remain almost completely protected from pregnancy. So rather than downplaying the possibilities of injectable contraception, I would rather see the authors search for ways to better educate women about their options, while newer methods are being researched.

Mary Jane Minkin
Department of Obstetrics and Gynecology
Yale University School of Medicine
New Haven, CT

The authors reply:

Kristen Elliott and Mary Jane Minkin criticize our article for being "based on outdated and incomplete information," for doing "a disservice to American women" and for "having many flaws." We disagree.

First, the intent of the research was neither to deny that the hormonal injectable presents a viable, effective and convenient option, nor to suggest that U.S. women do not need more contraceptive choices. On the contrary, we focused on why women are not using these viable and effective contraceptive methods, when there is a clear need for more contraceptive choices. We assumed that understanding the reluctance of women to use these methods may prove to be useful in increasing these methods' use through further research, social marketing and behavioral interventions.

Moreover, while data from 1995 might be considered outdated in the year 2000, at the time this article was written, these data were the only national data available on the use and reasons for nonuse of these methods. The only other comparable data set available at the time, the National Survey of Family Growth (NSFG), has contraceptive use data but no information on why a particular method was not used. The data cited by Elliott are neither population-based nor useful for the kind of analysis that we presented. Further, the increases in both the number of new prescriptions and the market share of the injectable that are cited by Elliot are irrelevant and misleading, as they are based on provider data and use an inappropriate denominator.

Both Eliott and Minkin object to the age restriction in our sample. While our data have a restricted age range, they nonetheless cover about two-thirds of women of reproductive age. In any case, including younger (aged 15–19) and older (aged 40–44) women does not reveal higher levels of use for either the implant or the injectable.[1] In fact, use of these methods is highest in the age-groups included in our study, not in the age-groups that were excluded. Hence, we believe that adding the younger and older women would not have changed our conclusions. There is no evidence other than Pharmacia's own market research to show a higher level of use among younger or older women. The number of prescriptions written or filled and the increase in market share do not necessarily reflect usage accurately, because of all-too-frequent noncompliance.

Elliot erroneously contends that "long-acting contraceptives have helped reduce the pregnancy rate among young women in the United States," citing an article that refers to unintended pregnancies only and shows no direct evidence that such a decline was even partially a result of increased use of injectables.[2] What that study says is that the availability of two new highly effective contraceptives "*may* [emphasis added] have prevented a disproportionate number of pregnancies." Yet during the period in question, only condom use increased significantly. The decrease in the rate of unintended pregnancy is the continuation of a trend that began long before the two methods were introduced—due primarily to an overall increase in the proportion of women who practiced contraception.

Minkin sees as a flaw our focus on amenorrhea as one of the major side effects among injectable users. We referred to amenorrhea briefly [pages 177 & 179] as being among the side effects, but not as a major side effect. On the contrary, we state: "Women using the injectable were even more likely….to report side effects, although they were *less likely to describe them as major*" [emphasis added]. In hindsight, we should have placed more emphasis on irregular bleeding and amenorrhea, because according to Pharmacia & Upjohn, more than 5% of the 3,900 women in the clinical trial reported these adverse reactions.[3] Further, Pharmacia & Upjohn, the producer of the injectable, warn that "most women…experience disruption of menstrual bleeding patterns," including heavy and continuous bleeding, and that as women continue using the injectable, fewer experience irregular bleeding but more experience amenorrhea.[4]

Elliott and Minkin suggest that we have an obligation to promote U.S. women's contraceptive options and to better educate women on all methods of contraception (which we also suggest in our paper). We believe, however, that the onus of such obligations falls on representatives of the pharmaceutical industry and on service providers who prescribe the products. As social scientists, we do not produce, market, prescribe or implement the use of such products; we simply report the facts.

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