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Teenage Childbearing Is Not So Bad After All... Or Is It? A Review of the New Literature

By Saul D. Hoffman

What are the socioeconomic effects of teenage childbearing for women and their families? A decade ago, the answer seemed clear, and it provided a solid basis for public policy. "Kids having kids," the research literature conclusively showed, was a very serious problem. It exacerbated the already difficult situation of young women from poor families and contributed importantly to the higher incidence of poverty and welfare dependence of these women.

The summary in *Risking the Future*, the 1987 report of the National Research Council, is well known: "Women who become parents as teenagers are at greater risk of social and economic disadvantage throughout their lives than those who delay childbearing."¹ President Clinton echoed this perspective in his 1995 State of the Union address, when he declared, with some understandable political hyperbole, that teenage pregnancy is "our most serious social problem."

Now the consensus of the research community is much less settled. Research conducted in the decade since *Risking the Future* has challenged the conventional view, arguing instead that the problem of teenage childbearing has been exaggerated, perhaps substantially. Authors of these studies contend that a teenage birth does not make a young woman's situation very much worse and that it may even have a beneficial effect on her socioeconomic status. These conclusions have gotten a great deal of attention, and they have a lot of researchers and policymakers scratching their heads.

What are we to make of all this? Should we be rethinking our public policy about

teenage pregnancy in light of the new findings? Is a policy intervention such as the National Campaign to Prevent Teen Pregnancy fundamentally misguided, undertaking a counterproductive mission? Even if we could do something about the high rate of teenage childbearing in the United States,* would it be a mistake?

In what follows, I review this new literature and appraise it critically, to assess whether the recent research persuasively makes the case that the effects of teenage childbearing are benign or even positive.

Effects of Teenage Childbearing

Before proceeding, it is worth emphasizing that there is no such thing as *the* effect of a teenage birth. Teenage mothers are individuals, so they naturally vary in their circumstances, their behavior and their well-being. (For example, they may be 14-year-olds still in junior high school or 19-year-old high school graduates.) And some of them end up doing rather well. This is an important finding of Furstenberg, Brooks-Gunn and Morgan's Baltimore study² and of a national study of teenage mothers by Duncan and Hoffman.³

Consequently, there can be no "one size fits all" conclusion here. But at the same time, it is possible and useful to describe the *average* effect of teenage childbearing, recognizing that the average conceals underlying variation in both directions.

Measuring the socioeconomic effects of early childbearing sounds like it ought to be a simple task. After all, everyone knows that teenage mothers are much worse off on many dimensions than women who delay childbearing. Their family incomes are lower, they are more likely to be poor and to be receiving welfare, and they are less likely to be married. Additionally, their children lag on standard measures of early development. But such facts by themselves do not establish that a teenage birth is the single cause of those problems—or that if

we could successfully intervene and change a woman's age at first birth and *nothing else* about her up to that point, we would greatly alter her life circumstances.

The crux of the research problem is obvious: Teenage mothers are clearly not a random sample of the population. They often carry with them a host of other disadvantages that also contribute to their poorer economic circumstances, like growing up in poor families and tough neighborhoods. And simply changing a woman's age at first birth would not necessarily change those conditions.

This may sound like another dry academic debate about distinguishing causality from correlation, but it has important policy implications. If we want to make a difference in the lives of these young women, we need to know where and when and how we might best intervene and offer services. Even a successful program to curtail teenage pregnancy might not affect most other aspects of an adolescent's life. It would not, for example, be likely to alter school, neighborhood and family conditions. Would a pregnancy prevention program with those limitations make a difference? The answer depends on whether the apparent negative effects of teenage childbearing reflect causality or just correlation.

Research Approaches

In most studies from the mid-1970s through the mid-1980s, researchers attempted to deal with the question of causality vs. correlation by controlling statistically for the many obvious and measurable differences between young women who had a teenage birth and those who did not—factors such as parental education and income, which affect both the likelihood that a woman has a teenage birth and her later socioeconomic position.

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*The United States continues to have the highest teenage birthrate of any developed country. The U.S. rate is twice that of Great Britain; 5–6 times that of France, Sweden and Denmark; and 15 times the Japanese rate. (Source: Child Trends, *Facts at a Glance*, Washington, DC: Child Trends, 1995.)

This time-honored approach yielded two consistent findings: Some of the apparent differences in well-being between teenage mothers and other young women are due to factors other than age at first birth; and the remaining effects of a teenage birth are negative and substantial. Studies like these were the basis for the negative prognosis in *Risking the Future*.

But even the best of these studies were unable to control for enough factors to convincingly establish that teenage pregnancy was the cause of the remaining effect. Researchers increasingly became aware of both the impact of unmeasured factors that might influence coefficient estimates and the need to correct, or adjust for, this selectivity bias. For example, parental income and education are undoubtedly important factors in the socioeconomic well-being of young adults, but so are parental attitudes and involvement, and other intangible factors that defy easy measurement. Many critics appropriately view research using extensive controls with some skepticism. At best, it probably provides an upper-bound estimate of the causal effect of teenage childbearing.

The newer research has gone in a different direction. Rather than trying to extend previous work by attempting to measure and control directly for more and more confounding factors, researchers have sought to isolate and measure the causal effect of a teenage birth by finding better “natural” comparison groups. The idea here is to identify a group of women who are reasonably similar to teenage mothers in most meaningful ways, so that one may infer that differences between the groups are largely the result of early childbearing. Three very clever and important approaches have been developed along these lines.*

First, Geronimus and Korenman proposed comparing sisters who had first births at different ages.⁴ Since sisters share a family and neighborhood background, including many family characteristics that are hard to measure, the differences in socioeconomic outcomes between them ought to represent primarily the effect of the difference in their age at first birth. This method is not perfect, because even sisters are not exactly alike, and their differences could affect both their fertility and other outcomes. But it should tell us something useful and possibly different than the traditional approaches.

Second, Grogger and Bronars compared teenage women who had twins with those who had a singleton birth.⁵ The idea is that the difference between having one child and having two children might be a rea-

sonable measure of the difference between having zero and having one—that is, between having and not having a birth as a teenager. And since having twins is a random event, the two sets of mothers should not differ in any systematic way.

Most recently, Hotz, McElroy and Sanders compared mothers who had become pregnant at age 17 or younger with teenagers who had conceived by the same age but had a miscarriage.⁶ Since most miscarriages occur randomly, these two groups of young women ought to be sufficiently similar that we can look at the subsequent differences between them to gauge the effects of a teenage birth. If teenage births are costly, the women who miscarried should be doing better, on average, than the mothers, because the miscarriage forced them to delay the start of childbearing.

Statistically, these approaches are somewhat different from one another. The sisters study was based on family fixed-effect analyses, in which the impact of unmeasured family factors was eliminated by comparing sisters. The other two approaches were more akin to “natural experiments,” in that the observed teenage childbearing status occurred randomly within the sample considered. In these two approaches, unobserved family and perhaps individual factors that may have affected the outcomes of interest remained and were not controlled for, but because of the randomness, these factors were uncorrelated with teenage childbearing. Thus, the omission of these factors from the analyses did not bias the estimate of the effect of teenage childbearing.

Research Findings

So, what did these studies tell us about the causal effects of a teenage birth? The sisters study clearly indicated that the traditional analyses overstated the negative impact of a teenage birth. Indeed, it seemed to show not only that the true causal effects of a teenage birth were much smaller than they appeared in the earlier work, but that they were very small—often essentially zero. The authors reported that while teenage mothers were less likely than their sisters who delayed childbearing to have had any postsecondary schooling or to be currently married, they did no worse, on average, on most economic measures. For example, differences in the sisters’ family incomes from their late 20s through their mid-30s were quite small and were not statistically significant.[†]

These results suggested that teenage mothers’ serious problems might be due largely to factors that were part of the rea-

son that some teenagers had births and others did not; that is certainly the conclusion that Geronimus and Korenman drew. Geronimus went on to argue on the basis of these and health-related findings that early childbearing might be optimal in some disadvantaged subpopulations.⁷

But when Geronimus and Korenman⁸ and a second set of researchers⁹ attempted to confirm these findings with other data sets, the results were much less dramatic. For example, both analyses showed that the average difference in economic well-being (family income divided by the poverty line) between sisters was not zero, but rather about one-third. This is smaller than the estimates from analyses that did not control for unmeasured family factors, but it is certainly not small. (It is, for example, bigger than the gap in earnings between men and women who work full-time.) The sisters also differed substantially in their probability of being poor or receiving welfare, and in their educational attainment. These findings were quite consistent across the data sets and research teams.

The approach comparing teenage mothers of twins with those who had a singleton birth yielded negative but relatively modest estimates of the effects of a teenage birth. For example, the probability of graduating from high school was about four percentage points lower for the mothers of twins, and their family income was about \$1,100 (or probably about 5–10%) less. The mothers of twins were also slightly more likely to be in poverty and on welfare.

Finally, the study comparing teenage mothers with teenagers who had a miscarriage produced the most surprising and controversial results. The researchers looked at how the two groups fared between ages 16 and 30; this allowed them to distinguish between short-run and longer run impacts, which is an important difference not fully considered in previous research.

By the mid- to late 20s, the teenage mothers did better over a wide range of outcomes than those who had a miscarriage. For example, they worked more regularly and earned more than their counterparts, and their spouses had higher incomes than the husbands of women in the comparison group. Differences in educational attainment (including receiving

*I exclude a fourth revisionist point of view, which is primarily interpretive and relies on secondary sources. (Source: Luker K, *Dubious Conceptions*, Cambridge, MA: Harvard University Press, 1996.)

†Because the samples were quite small, especially for analyses of dichotomous dependent variables, many fairly large differences were not statistically significant. But in most cases, the estimated coefficients were relatively small.

a GED) and income from welfare between the two groups were very small. The teenage mothers were worse off only on two categories: They had more births by age 30, and they spent more time as single mothers than those who had a miscarriage. (The study did not look at two common outcomes—the probability of being officially poor and total family income.)

Evaluation

These studies were genuinely innovative, and collectively, they cast considerable doubt on the received wisdom about the consequences of teenage childbearing. Their findings are swiftly making their way into the established literature and are cited regularly by scholars.

But just how much confidence ought we have in these new findings? Is it time to rethink appropriate public policy? Some researchers may think that because these recent approaches are so compelling and clever, the results must be correct. But even clever methods may be undermined by problematic data or execution. Indeed, there are many small and large problems, and almost all of them tend to make the resulting estimates too small—maybe much too small.

One problem plagues all of this research, although the scholars themselves are not to blame: The demographic characteristics of teenage mothers and the economic environment have changed so quickly and dramatically that the samples available to researchers may simply not be relevant today. To examine longer run effects of teenage childbearing requires using data on women who were teenage mothers quite some years ago. For example, the teenage births studied by Hotz and colleagues occurred between 1971 and 1982, when the proportion of teenage births that were nonmarital ranged from about 25% to 50%; today that rate is 75%.¹⁰ The worlds of welfare and work were quite different, too. Welfare reform was still years in the future, as was the deteriorating labor market that confronts less educated workers in the 1990s.

Thus, even if these studies were internally flawless, one might want to take

*In 1968, 32% of 18–19-year-olds and 74% of 20–24-year-olds were no longer living with their parents; 20% and 57%, respectively, were already married. (Source: U.S. Bureau of the Census, Marital status and family status, March 1968, *Current Population Reports*, 1969, Series P-20, No. 187, Table 2.)

†Hoffman and colleagues chose the age range so that virtually no potential sisters were omitted. Geronimus and Korenman's samples extended to age ranges at which some sisters might be omitted, but less so than in the original analysis.

their policy prescriptions cautiously. However, each has a variety of problems that add to this inherent weakness.

• *Sisters studies.* Consider the sisters analyses. Here, the issue is which set of findings to accept—the original one, showing little or no effect, or the two later ones, showing much bigger effects. The sample of sisters used in the original analysis was potentially unrepresentative in ways that could easily bias the results. The data came from the National Longitudinal Survey of Young Women, which was based on a large sample of women who were aged 14–24 in 1968. All women in that age range in a sampled housing unit were interviewed, so the survey included siblings who were living together but not those who had already formed their own independent households.* The problem, unfortunately, is that the resulting sample is representative only of coresiding sisters, rather than of all sisters.

Consequently, the sample contains a conspicuously small number of sisters. Could this bias the results? Suppose, not unreasonably, that the less successful older sisters of teenage mothers were more likely to be still living at home and thus included in the sample than were more successful ones. Then the differences between coresiding sisters might well underestimate the true effects of teenage childbearing.

The reanalyses were based on the National Longitudinal Survey of Youth and the Panel Study of Income Dynamics, both of which provided a much larger and more representative sample of sisters than the original data set. In both, the sample of sisters could be chosen so as to minimize or avoid the potential problem of missing older sisters.† The reanalyses are also far more timely, since the teenage births included occurred between the mid-1970s and the mid-1980s.

Given all this, there is no question about which set of sisters findings we should rely on. The weight of the evidence and the quality and timeliness of the data clearly support the reanalyses. The sisters approach is important, and Geronimus and Korenman deserve great credit for being the first to address the selectivity problem directly. They emphatically and successfully disabused researchers of the idea that one could ignore unmeasured variables in analyzing a problem like this one. But this literature establishes not that the effects of a teenage birth are small, only that they are smaller than previously thought.

• *Twins study.* In the twins study, the critical question is whether a comparison of teenage mothers of twins with teenagers who had a singleton birth provides a rea-

sonable estimate of the effect of having a teenage birth. The authors note, quite correctly, that the comparison is appropriate if and only if the impact on socioeconomic outcomes of having twins is exactly twice the impact of having one birth—that is, if the effect of a teenage birth is a “linear” function of the number of children (at least up to two). If there are economies of scale (i.e., if the cost of caring for two children is less than twice the cost of caring for one), the effect of a single teenage birth would be underestimated; if there are diseconomies of scale (the cost of caring for two is more than twice that of caring for one), the effect would be overestimated. Casual speculation might suggest that economies of scale exist: Two children typically do not seem to be twice as much trouble as one, and three are not three times the trouble.

It is not easy to get information about economies of scale in the amount of time spent caring for children, in part because it is not easy to measure the quality of care in the first place. Yet, to their credit, the authors attempted to provide some empirical evidence about this issue. Presumably, the negative effects of teenage childbearing on schooling and labor-market outcomes derive from the time that is directed toward child care rather than these other pursuits. Therefore, the authors compared the amount of time that the mothers of twins (in this case, not necessarily teenage mothers) reported spending on housework with the amount reported by mothers with one child. The difference would then be an estimate of the additional time costs of having twins.

The analysis revealed substantial economies of scale. On average, mothers with twins spent 50% more time (rather than twice as much time) caring for their children. And the authors found other evidence in the child development literature indicating that the mothers of twins spent about 40% more time on child care than did mothers of singleton infants. Thus, the twins comparison is severely compromised as an estimate of the effect of having a teenage birth. (It is, however, perfectly fine for estimating the cost of a teenage twin birth vs. a teenage singleton birth.)

Grogger and Bronars are very open about the limitations of their research. They conclude that “economies of scale exist in time spent in childrearing. This ... suggests that the twins approach provides a conservative estimate of the effect of an unplanned teenage singleton first birth.”¹¹

Once findings enter the public domain, caveats and qualifications are often forgotten. For example, Hotz, McElroy and

Sanders summarized the sisters studies as suggesting that the effects of teenage childbearing are “positive,” which is an overstatement even for the original findings and utterly incorrect for the reanalyses. They described the results of the twins study as showing a “slightly negative” effect, which is correct, but they failed to acknowledge the authors’ own assessment that the results are highly conservative estimates.¹²

• *Teenage birth vs. miscarriage.* Finally, the study comparing teenage mothers with teenagers who miscarried, while careful and thoughtful, also has some weaknesses that need to be considered. There are three general areas of concern.

First, information on teenagers who had abortions is needed, because some women who miscarry would otherwise have had an abortion, and the authors wanted to identify only women who had been willing to carry a pregnancy to term. However, it is very difficult to get good information on teenage abortions or miscarriages from survey data. Understandably, some young women might have an incentive to conceal an early pregnancy termination.

Abortions and miscarriages clearly were underreported in the National Longitudinal Survey of Youth, the data set used for this analysis. Whereas national data for the period covered indicate that 40% of teenage pregnancies ended in abortion and another 12–13% in miscarriage,^{*13} among the women studied by Hotz and colleagues, approximately 25% of reported teenage pregnancies ended in abortion and 7% in miscarriage. Thus, only two-thirds the expected number of abortions and barely more than half the expected number of miscarriages were reported.

This underreporting undermines the analysis in several ways. The comparison group included only 68 women who had a miscarriage during their teen years. For many of the outcomes examined, including such critical ones as the woman’s own earnings and her spouse’s earnings, the effective samples were much smaller. For example, there were only 25–40 observations of husband’s earnings at a given age for women who had a miscarriage, because only some of these women married and not all of them were observed at all ages. Similarly, the analysis of women’s own earnings at a given age was based on very small samples of women who miscarried, because not all women were working and not all were observed at all ages.

When samples are that small and when there is so much misreporting, the sample may not be representative. Suppose, for example, that misreporting is highest among

young women who viewed their early pregnancy as an unfortunate error and who went on to do very well. Then the sample of women with acknowledged teenage miscarriages would be more disadvantaged than the general population of women with teenage miscarriages. In other work, the authors used a variety of techniques to address this problem.¹⁴ But there is no good substitute for the right data.

The second problem with this study is potentially more severe. Think of the teenage childbearing problem as a classic medical study, in which women who had a teenage birth constitute the treatment group and those who had a miscarriage are the controls. (This is exactly the way the authors urge

us to think about the problem.) As in a medical study, women are, for the most part, randomly assigned to one group or the other. Obviously, it is imperative that the groups remain distinct; otherwise, the experiment is contaminated and the differences in outcomes between them cannot be attributed to the treatment.

However, more than half of the women who had a miscarriage went on to give birth before age 20 (meeting the conventional definition of a teenage birth), and about one-third conceived again before age 18 and had a live birth (meeting this study’s definition of a teenage birth). Thus, while a miscarriage may cause a random delay of childbearing, it does not preclude a teenage birth; additionally, the delay may be far shorter than the one contemplated in most discussions of teenage childbearing. (As a practical matter, a miscarriage might delay a birth by as little as 3–4 months.) Since the control group was relatively small in the first place, this contamination of the control sample creates a serious problem.

Another way to appreciate this problem is to consider the underlying policy comparison. Virtually all previous research on the effects of teenage childbearing compared teenage mothers with some group—women in general, sisters etc.—*none of whom* had had a birth before age 20. But this was clearly not the case here. The authors noted that a miscarriage delayed the median age at first birth by nearly three years (from about age 16.5 to 19). However, the median delay does not necessarily tell the whole story. The benefits of delaying may not be a simple linear function of the length of delay, but may depend on the age until which young women postpone their first birth. For example, delaying a first birth from age 15 to age 17 may

yield little or no benefit for an outcome like high school graduation, but a two-year delay from age 17 to 19 may have a considerable effect. Furthermore, because the sample of women who had a miscarriage is so small, the authors could not investigate whether those who delayed their first birth longer fared better than those whose delay was quite short.

Third, continuing with the analogy of treatment vs. controls, the controls in a medical experiment typically receive a placebo, an intervention designed to have no effect whatsoever. But is a miscarriage analogous to a placebo? Could it not have independent

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effects on schooling or psychological well-being and on subsequent outcomes? If it has, then the difference between the two groups no longer provides an accurate estimate of the effect of having a teenage birth.[†] If, for example a miscarriage has an independent negative effect, then the experiment will underestimate the true effects.

The Bottom Line

My reading of the evidence, old and new, leads to a centrist, conservative position. It is certainly fair to say that current research no longer supports the notion that teenage childbearing is a devastating event. We are a long way not only from Campbell’s famous conclusion that “when a 16 year old girl has a child ... 90 percent of her life’s script is written for her,”¹⁵ but perhaps also from the tone of *Risking the Future*.

The new research quite appropriately recognizes and focuses attention on the important contribution of other factors—especially family and individual characteristics that are difficult to measure—to the poor average outcomes of teenage mothers. But neither is there yet compelling evidence that the independent

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*These figures include women through age 19, not age 17, as in the study by Hotz and colleagues. I am aware of no data on abortion and miscarriage exclusively for adolescents younger than age 18.

†The researchers are aware of at least the first and third problems discussed here; they argue, not persuasively in my opinion, that their results are not affected by them. A further issue concerns whether enough miscarriages are nonrandom to create a problem. The authors try to account for some sources of nonrandomness, such as tobacco and alcohol use, but do not control for others, such as domestic abuse. It is therefore possible that prior to the pregnancy, the young women who had a miscarriage were systematically worse off than the teenagers who gave birth; this would bias the results.

sites, receipt of this counseling was not a randomized intervention in the project. The inclusion of a comparison group would have strengthened the study design.

Follow-up data were collected at different postinitiation points for implant users (six months) and DMPA users (one year); this no doubt yielded more recall bias among DMPA users. Because the number of women who reported having had more than one sexual partner during both rounds of data collection was quite small, any results comparing these women to those with one sexual partner should be considered preliminary. Among the few women who reported more than one partner during the study period, we did not distinguish between consecutive and concurrent partners.

We recognize that our decision to dichotomize the condom-use variable was not optimal for identifying women at extremely low risk of STDs (i.e., those who always used condoms); however, too few women always used condoms at both survey rounds to permit an analysis of always used versus sometimes, rarely or never used, which would have been the preferable approach. Further, our analysis did not include a measure of the length of a couple's relationship, which has been shown to be associated with the likelihood of condom use.¹¹

Finally, our only indicators of risk for STDs were more than one sexual partner over the study period and perceived risk of infection with HIV; our analysis did not

examine other risk factors, such as intravenous drug use or whether respondents' partners had had multiple sexual partners. Moreover, women's perceptions regarding their risk of HIV infection could have changed over the study period. Nonetheless, their perception of risk measured at baseline likely influenced their decision to use condoms once they adopted their new hormonal method.

Despite these limitations, this study nonetheless reveals that condom use declined among women relying on long-term hormonal contraception and identifies several predictors of condom use that are relevant for family planning counselors and providers. We suggest that future research in this area focus on the content and impact of counseling messages, and on condom use among women in the other high-risk categories that were not included in our analysis.

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causal effects of teenage parenting are positive, zero or even just marginally negative. There are hints and suggestions, and clever research avenues that ought to be pursued. We have been greatly and meaningfully informed by the new research. But the evidence is simply not yet solid enough. The "experiments" are not good enough, and almost all of the biases would tend to make the estimates too small.

Accordingly, it is much too early to conclude that policy efforts to reduce teenage pregnancy and childbearing are misguided. Reduction of early parenthood will not eliminate the powerful effects of growing up in poverty and disadvantage. But it represents a potentially productive strategy for widening the pathways out of poverty or, at the very least, not compounding the handicaps imposed by social disadvantage.

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