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The Socioeconomic Costs of Teenage Childbearing: Evidence and Interpretation*

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Until recently, the belief that teenage childbearing makes a substantial *causal* contribution to persistent socioeconomic disadvantage was pervasive. The scientific evidence used to support this interpretation, however, was open to the criticism that estimated effects of teen childbearing on long-term socioeconomic outcomes were biased by failure to account for unobserved heterogeneity; fertility timing varies systematically across populations, and early fertility is much more common in socioeconomically disadvantaged communities. Investigators therefore made efforts to control for background differences among mothers who had first births at different ages. We were concerned, however, that measures of family background used in these studies might have been inadequate. Although sibling comparisons have well-known limitations (for example, nonrepresentativeness and small samples; Griliches 1979), they offer a valuable complement to more standard cross-sectional approaches. We undertook the sibling approach as a matched comparison group study in which the set of match characteristics is more complete than that provided by matching measured or observable family background characteristics. The recent replication of our study (Geronimus and Korenman 1992a) by Hoffman, Foster, and Furstenberg (1993) supports our principal conclusions that 1) standard cross-sectional estimates of the consequences of teen childbearing are biased by failure to control adequately for family background differences between women who have first births as teenagers and those who have first births at later ages, and 2) previous estimates are likely to have exaggerated the costs of teen childbearing.[1]

Yet we have two concerns: 1) that Hoffman et al. (1993) leave the reader with the impression that "within-family" estimates (i.e., sister differences) derived from the Panel Study of Income Dynamics (PSID) or the National Longitudinal Survey of Youth (NLSY) are to be preferred to those based on the National Longitudinal Survey Young Women's

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Sample (NLSYW); and 2) that because the PSID and NLSY within-family estimates are larger than the NLSYW estimates, the "weight of the evidence" suggests that the effects of teen parenthood on subsequent socioeconomic status are likely to be sizable, although smaller than has often been assumed. Below we 1) take up the question of the relative validity of the various estimates, and 2) discuss the larger issue of the correspondence between within-family estimates from any data set and the "true effects" of a teen birth.

COMPARING ESTIMATES FROM DIFFERENT DATA SETS

In Geronimus and Korenman (1992a)[2] we analyzed three samples: NLSYW women who were age 28–38 in 1982, PSID women who were 28–38 in 1985, and NLSY women who were 23–31 in 1988. Hoffman et al. analyzed a sample of PSID women who were 21–33 in 1987. Within-family point estimates derived from either of the PSID samples (theirs or ours) are larger than those from the NLSYW, as are those based on our NLSY sample. Because of the unsettling range of within-family estimates, among other reasons, we concluded that we were far from being able to write the final word on the size of any true causal effects of teen childbearing. In contrast, Hoffman et al. suggest that their PSID estimates are to be preferred and that the NLSYW estimates should be considered "outliers".

The PSID sisters sample that Hoffman et al. study is (as they note) more recent, larger, and in some respects "more representative" than the NLSYW sample that we study. However, the importance of these sample characteristics to the study results has not been demonstrated; as we explain below, it is questionable. Meanwhile, Hoffman et al. do not mention weaknesses that are specific to the PSID (for examples, see Geronimus and Korenman 1992a, Section II) or balance the strengths and the weaknesses of the data sets. For example, although their sample is larger than our NLSYW sample, as they recognize, the precision of within-family estimates is not high in any of the samples. Furthermore, although it appears that Hoffman et al. have 428 sister pairs and we have only 156, Hoffman et al.'s sample of 428 includes women who have not yet had births. In Hoffman et al.'s model specifications, the estimated effects of birth timing are identified by comparing those women who had teen births to those who had later births. Restricting their sample to such women reduces their sample size by well over half, from 428 to 188 sister pairs (Hoffman et al. 1993, Table 1).[3]

In addition, the greater number of sibling pairs in their sample is obtained at the cost of including some very young women in the analysis (e.g., those between the ages of 21 and 25). An overarching purpose of our study was to provide a long-term follow up of teenage mothers. Because most teen births occur in the late teens, measuring outcomes in the early twenty's cannot provide estimates of long-term effects. In studying older women, we built explicitly upon earlier research by Furstenberg and his colleagues (1987a), who found that early assessments of the effects of teen births led to overpessimistic assessments of later socioeconomic status.

Aside from differences in sample size, Hoffman et al. offer two additional reasons to question the NLSYW estimates: 1) that the NLSYW sisters sampling requirement—that sisters were linked to the same household at baseline, in 1968, at ages 14 to 24—leads to downwardly biased within-family estimates; or 2) that genuine cohort differences exist, which render the NLSYW estimates outdated. These are interesting hypotheses but, as we discuss below, there is little evidence to support them. In addition, other evidence might lead one to favor, or certainly not to dismiss, the results based on the NLSYW.

THE EFFECT OF SAMPLING

Hoffman et al. attempt to test their hypothesis that the NLS sampling restriction leads to downwardly biased estimates by simulating the NLSYW sampling restriction with PSID data. Their results are inconclusive. As they hypothesize, the within-family effect of a teen birth on income declines somewhat when they restrict their sample to sisters who were coresident in 1980 at ages 14 to 24. The standard error on this estimate is large, however, and provides no statistical grounds to reject the hypothesis that the sample restriction is of no consequence. The coefficient from the simulated "biased" sample differs by only one standard error from the estimate from the "unbiased" sample.[4] Furthermore, their "simulated" NLSYW sample definition departs substantially from the actual definition.[5]

We mimicked the NLSYW sampling restriction in PSID data in a way that reflects the restriction more accurately. We studied a sample of PSID sisters who were 14 to 24 in 1968 and were linked to their parental dwelling unit, and we followed them to ages 28–38 (Geronimus and Korenman 1992a, Table VII). These PSID within-family point estimates remained much larger than the corresponding estimates from the NLSYW. (In fact, rather than being lower estimates, they actually *increased* slightly in comparison to the PSID sisters sample without such a restriction, although the increase did not differ statistically significantly from our earlier estimates.) Although tests of this type necessarily involve the discarding of observations (and therefore tend to be imprecise), our findings are inconsistent with the hypothesis that the NLS sampling restriction accounts for the differences in results across data sets.

COHORT DIFFERENCES

Hoffman et al. also offer the hypothesis that genuine cohort differences may exist between our NLSYW sample and their PSID sample. In their paper, this hypothesis is offered as a matter of speculation without any empirical test. They argue that the more recent cohort may suffer larger costs of a teen birth because teen births currently are more likely to be out-of-wedlock births. Hoffman et al., however, find comparatively weak and insignificant within-family effects of teen childbearing on the three marital status variables for their study cohort. The within-family effect of a teen birth on the probability of being married at follow-up (presumably an important mechanism through which having an out-of-wedlock birth affects current income) appears smaller in their PSID sample and in our NLSY sample, which are based on younger cohorts, than in our NLSYW sample. This finding casts some doubt on the hypothesis that the increasing association of teen childbearing with out-of-wedlock childbearing explains the differences in the results across the samples.

The more general question—whether cohort changes have affected outcomes of early childbearing—has received some attention. For example, whereas differences in marriage probabilities between teen mothers and women who delay childbearing have grown, differences in the probability of high school completion and in subsequent fertility appear smaller in more recent cohorts than in earlier cohorts (Dechter and Furstenberg 1992a, 1992b; Dechter, Furstenberg, and Harris 1992; Upchurch and McCarthy 1989). The various changes over time that have been described affect economic status in opposite directions, so it is difficult to predict their net effect on differences in economic status of siblings across cohorts. In addition, an alternative hypothesis is suggested by the results of Furstenberg, Brooks-Gunn, and Morgan (1987a, 1987b), that the apparently larger effects in the more

recent PSID and NLSY cohorts are due to the short follow-up period and may diminish with age. This hypothesis also seems worthy of further empirical study.

EVIDENCE ON THE REPRESENTATIVENESS OF THE PSID AND NLS ESTIMATES

Sibling samples drawn from the PSID or either of the NLS data sets are likely to be unrepresentative in a number of ways. As Hoffman et al. note, the NLS sampling scheme overrepresents the unmarried. Sister samples drawn from any of the data sets will overrepresent large families. Moreover, sibling samples may exaggerate any problems associated with attrition. One way to gauge the effect of any "nonrepresentativeness" on the teen childbearing *coefficients* (the important issue) is to compare estimates based on the sisters samples with estimates based on the full samples (i.e., samples that are not restricted to women with sisters in the sample).

In Table 1 we compare *cross-sectional* estimates of the effects of teen births on income/needs ratios based on the sisters subsample with those based on the entire sample (that is, all mothers are included, whether or not they have a sister in the sample) for the NLSYW, the PSID, and the NLSY. On the basis of this internal comparison, the NLSYW sisters subsample appears to yield more "representative" coefficient estimates than the PSID

Table 1. Comparison of Cross-Sectional Estimates of the Effect of a Teen Birth on Income/Needs

Dependent Variable = Ln(Income/Needs)

	Coefficients (SEs)			
	GK[a] NLSYW 28–38 1982	GK[a] PSID 28–38 1985	GK[a] NLSY 23–31 1988	HFF[b] PSID 21–33 1987
Controls for Race, Urban Residence, and Age Only				
Entire sample	−.32	−.41	−.36	?
	(.03)	(.06)	(.03)	
Sisters subsample	−.33	−.47	−.56	?
	(.09)	(.09)	(.09)	
Controls for Race, Urban Residence, Age, and Family Background				
Entire sample	−.26	−.36	−.27	?
	(.03)	(.06)	(.03)	
Sisters subsample	−.26	−.43	−.44	−.42
	(.09)	(.09)	(.09)	(.06)

[a] Geronimus and Korenman (1992a, 1992b, various tables). Family background controls included by G and K are number of siblings and dummy variables for mother's and father's education (three each) and whether respondent lived in a single-parent family at age 14.

[b] Hoffman, Foster, and Furstenberg (1993, Table 2). Family background controls included by H, F and F are mother's education, residence in the northeast or north central region, parental family income/needs ratio, and whether the family received income from AFDC, all measured at age 14.

or the NLSY (compare Row 2 to Row 1 or Row 4 to Row 3). The apparently "too large" cross-sectional estimates derived from the PSID and the NLSY sisters subsamples, but not from the NLSYW sisters subsample (relative to estimates based on the entire sample of any of the data sets), may explain why the within-family effects remain substantial in the PSID and the NLSY, even though the within-family effects are smaller than the cross-sectional effects in all three data sets.

In their discussion, Hoffman et al. focus on the different "within-family" estimates derived from different data sets, where those derived from the NLSYW are the smallest. The figures in Table 1, however, show that the cross-sectional estimates also may be much larger in the PSID and the NLSY than in the NLSYW. Even if one accepts the hypothesis that the NLSYW sisters subsample is not representative of sisters age 14 to 24, there is no reason to suspect that estimates based on all women in the NLSYW would be unrepresentative. Women between ages 14 and 24 who have formed households independent of their parental dwelling units would be represented in the full sample. This point is further evidence that the NLSYW sampling restriction does not explain the different results. In addition, it reveals a great advantage of the NLSYW sisters sample: it is the only sisters sample that yields cross-sectional estimates of the effect of a teen birth on income/needs which are similar to those based on the entire data set.

Comparisons with cross-sectional estimates derived from other sources provide additional suggestive evidence that it is premature to label the NLSYW estimates "outliers". In the 1990 June CPS (a recent sample that is larger than the NLSYW, the NLSY or the PSID, and is not subject to attrition over many years), the estimated effect (s.e.) of teen childbearing on ln (income/needs) for mothers ages 28–38 is −0.31 (0.02), with controls similar to those in the first row of Table 1 (race, urban residence, and age). Bronars and Grogger (1992, Table 3) report similar findings from the 1980 census. Although none of these estimates is a "gold standard,"[6] collectively they and the evidence reported in Table 1 show that further investigation is required before the NLSYW estimates are ruled out in favor of the PSID estimates. In fact, they could be said to support the idea that the NLSYW yields more representative estimates of the effects of a teen birth.

Finally, within-family estimates of a magnitude similar to our NLSYW estimates do not appear unreasonable in light of previous study findings. Although we questioned the conclusions of studies that failed to account for unobserved heterogeneity, we built on their findings in hypothesizing that the negative effects of teen childbearing may not be inherent (Geronimus 1987; Geronimus and Korenman 1992a). A close reading, not of summaries but of the cross-sectional studies that preceded our study, suggests that the evidence on the effects of teen childbearing on economic status may have been more mixed than is widely appreciated.[7] Even in earlier comparisons between teenage and older childbearers that lacked detailed controls for family background, differences by maternal age were modest for some socioeconomic outcomes (e.g., Furstenberg et al. 1987b). In light of the acknowledgment that such differences may represent upwardly biased estimates, it would not be surprising to obtain further reduced estimates of effects of a teen birth in the presence of detailed controls.

INTERPRETING THE ESTIMATES

An important aspect of any evaluation of the weight of the evidence is the interpretation of residual differences between sisters in indicators of socioeconomic status. In all of the samples and data sets, within-family effects for some outcomes were large and statistically significant. Hoffman et al. appear more willing than we are to interpret these residual differences between sisters as estimates of the causal effects of a teen birth.

Moreover, although they acknowledge that other, even countervailing, interpretations are possible, they state that their estimates "are likely to be conservative." In contrast, we argue that the within-family estimates are likely to overstate any "true" effects of teen childbearing.

Hoffman et al. believe that their estimates "are likely to be conservative" because "many of the childless women will have post-teen births . . . and these women do particularly well in terms of their income and educational attainment." Here they raise an interesting point, which in fact strengthens the case for studying women at older ages. Although no one knows what will happen as younger cohorts age and have children, we found the following in our study of older women in the PSID and the NLSYW: when we included women who were over age 19 but who had not yet had first births by specifying a model that grouped them with women who had births after age 19, the within-family effects of a teen birth on the income/needs ratio (and on other outcomes) barely changed (Geronimus and Korenman 1992b, Appendix II).

Why, instead, might the within-family estimates overstate true effects? First, within-family heterogeneity may bias sibling estimates upward. Several personal characteristics that may vary between sisters and may affect subsequent educational attainment or economic status have been identified as antecedents to teen childbearing; these include poor school performance or negative attitudes toward school and relatively low AFQT scores. These differences have been described even within fairly homogeneous populations (Abrahamse, Morrison, and Waite 1988; Hayes 1987; Moore and Snyder 1991; Plotnick 1992; Zabin, Hirsch, and Boscia 1990). There is no a priori reason to believe that similar selectivity does not operate within families. Some ethnographic observations suggest that it may do so (Burton 1990; Ladner 1971; Stack and Burton, forthcoming); in fact we found that teen mothers in the NLSY had somewhat lower AFQT scores than their sisters who postponed childbearing. Finally, several researchers (Olsen and Farkas 1989; Ribar 1992; Rindfuss, St. John, and Bumpass 1984; Upchurch and McCarthy 1990) who have studied the endogeneity of fertility timing in models of educational attainment argue that fertility timing and education are determined jointly, and find evidence to support the importance of endogeneity bias.

A related theoretical concern is that families may distribute resources to mitigate or exacerbate differences among siblings (Griliches 1979). The extent or direction of any resulting bias in within-family estimates has not been established. One of the possibilities is that such efforts might tend to mitigate the upward bias induced by within-family heterogeneity. It seems less plausible to us, however, that attempts by socioeconomically disadvantaged parents to equalize economic outcomes among their children could be so effective as to reverse the net (upward) direction of bias caused by differences in ability, motivation, and achievement. Of course, this question remains empirical.

A second basis for our hypothesis that the residual effects may overstate any true effect of a teen birth on economic status is that much of the within-family difference in income between teen and nonteen mothers in any of the samples was accounted for by the greater chances of being married at follow up among the sisters who had nonteen first births (Geronimus and Korenman 1992a, Table VI). Therefore, the assessment of whether teen childbearing has adverse effects on economic status depends largely on whether teen childbearing itself makes teen childbearers less likely to be married at follow-up. To answer this question will require continued research that should take into account possible biases due to heterogeneity and endogeneity, following the recent research on the relation between teen childbearing and educational attainment.

In addition, we raised the possibility that differences in marital status at the time of the follow-up interview may not reflect permanent differences in marital stability. The reason is that a woman who had a teen birth probably married earlier than her sister who postponed

childbearing, and therefore would have been exposed longer to the risk of marital separation at the time of the follow-up interview.[8] In our preliminary work in analyzing first marriages that began during the PSID sample period, estimates from fixed-effects (sister-differenced) *proportional hazard models* (e.g., Ridder and Tunali 1992) that account for differences in exposure suggest that the marital dissolution hazard is no higher among teen marriages than among nonteen marriages. This finding does not prove that life-cycle differences account for differences in income between women who had births as teenagers and their sisters who had births at later ages. Nevertheless it highlights the need for renewed effort and further research before interpreting the remaining within-family difference in income as an accurate estimate of the effect of an exogenously determined teen birth.

SUMMARY AND CONCLUSION

There is little evidence to support the reasons suggested by Hoffman et al. for treating the results based on the NLSYW as outliers. There is even some evidence that might lead one to favor the NLSYW estimates. After some investigation, which of the range of within-family estimates across data sets is most accurate remains unsettled (although exploring differences in cross-sectional estimates from the sisters subsamples seems promising). In addition, we believe there is evidence to support the hypothesis that within-family estimates are upwardly biased because of within-family heterogeneity and endogeneity, but the importance and magnitude of such bias is also an open question.

Although we have highlighted here what we believe to be the main points of disagreement between ourselves and Hoffman et al., we hope readers will not lose sight of the areas of agreement between the two studies, which are substantial, or of the empirical support for our key findings that Hoffman et al.'s replication study has provided. To us, the findings of both studies suggest that future research should account empirically for potential biases from (possibly unmeasured) heterogeneity in family background. Because the prevailing beliefs about the consequences of teen childbearing have been based on cross-sectional comparisons that lack detailed family background controls, these beliefs now should be open for reconsideration and should be subjected to reevaluation. Several recent empirical attempts have been made to take heterogeneity or endogeneity bias into account. These studies support this conclusion and caution against drawing causal inferences from existing estimates of the effects of teen births. We continue to recognize the limitations of currently available methods and data for accounting for unobserved heterogeneity and selectivity (e.g., Griliches 1979; Manski 1989). Therefore we encourage the enhancement of data sets and the continued empirical investigation of questions that have been raised about possible biases of sibling estimation and other methodological approaches. We hope that with new rounds of research, advances will continue to further the understanding of these important social processes. Given the difficulty of accounting adequately for selection into teen childbearing across and within populations, and even within families, and given the conflicting within-family estimates, we believe that the size of any "true effects" of teen births on socioeconomic status must be considered an open question.

NOTES

[1] Other recent empirical studies supporting this conclusion are Bronars and Grogger (1992); Geronimus and Korenman (1993); Geronimus, Korenman, and Hillemeier (1992); Lundberg and Plotnick (1989); McCrate (1992); Ribar (1992); Rosenzweig and Wolpin (1992); Olsen and Farkas (1989); Upchurch (1992); Upchurch and McCarthy (1990). For a recent study that reaches the opposite

conclusion see Moore, Myers, Morrison, Nord, Brown and Edmonston (1992). For a summary of recent evidence see Bachrach and Carver (1992).

[2] Greater detail on several of the points made here can be found in Geronimus and Korenman (1992a). Geronimus and Korenman (1992b) contains additional results that could not be included in Geronimus and Korenman (1992a) because of space limitations. We also refer readers to the exchange between Geronimus (1991, 1992) and Furstenberg (1991, 1992) in *Family Relations*.

[3] Missing income information reduces the Geronimus and Korenman NLSYW sample size to 129 pairs (for the income analyses only), in which neither member had missing income information. There appears to be no missing income information in the Hoffman et al. sample (and hence no loss in sample size due to missing data). The reason why there is no loss of sample from missing income information, however, is that "assignments [are] made by the editors in order to recreate data missing from an interview" (ISR 1989, p. 5). Although income is imputed for only a small fraction of cases, missing data problems will be exacerbated by sibling differences because income differences would be missing or would have to be imputed if information is missing for either sibling.

[4] In Note 11 of their paper, Hoffman et al. (1993) report a t-statistic of 1.35 for the point estimate of 0.18. This implies that the standard error of this estimate is 0.13.

[5] Hoffman et al. selected sister pairs who coresided *in 1980* at ages 14 to 24, and followed them to ages 21 to 33. Because of age, period, or cohort differences, this is not the same as selecting a pair of coresident sisters from all coresident sisters aged 14 to 24 in 1968 and following them to age 28 to 38 in 1982, a more literal simulation of our NLSYW analysis. In addition, in order to enter their sample of sisters who coresided in 1980, it appears that a pair of sisters who were selected randomly from all coresident sisters (under age 15) in a family in 1968 had to "survive" as a coresident pair from 1968 to 1980. This is not the same as selecting a random pair of sisters who coresided in 1980 at ages 14 to 24 because an entire family is lost if either of the two sisters selected as coresidents in 1968 moved out before 1980, even if two or more sisters in the family coresided at ages 14 to 24 in 1980. (By our tabulations, at least one-third of households in the PSID that contain two sisters under age 15 in 1968 contain more than two sisters.) This selection by Hoffman et al. represents another departure from our sample definition; it must contribute to the reduction in sample size and the loss of precision they encounter when restricting their sample to pairs who survived intact as coresidents from 1968 to 1980.

[6] The CPS estimates are for mothers age 28 to 38 in June 1990. Controls include age as well as dummy variables for urban/rural residence, black and Hispanic racial identification, and teen first birth. The dependent variable is ln(income/needs), where income is taken as the midpoint of categories from a categorical income variable. Needs ratios cannot be calculated for members of subfamilies. The Bronars and Grogger (1992) estimates are for mothers age 28 to 38 in the 1980 census PUMS sample whose children are living with them. The controls are those listed above. The teen childbearing effect (s.e.) on ln (household income) is -0.16 (0.02). Our experience suggests that a teen coefficient from a regression where ln(income/needs) is the dependent variable is no more than 50% larger than the corresponding coefficient from a regression where ln(income) is the dependent variable.

[7] In fact, not all estimated effects of teen births were adverse. For example, Hofferth and Moore (1979) report that the total (direct plus indirect) effect of delaying birth for one year among women who had teen first births (i.e., under age 19, whose average age at first birth is 17.15) is to *lower* family income at age 27 by approximately $250 (Hofferth and Moore 1979, Table 8, first panel). In another paper cited in *Risking the Future* (Hayes 1987) focusing on economic status, Haggstrom, Blaschke, Kanouse, Lisowksi, and Morrison 1981 concluded that their study of late teen childbearers (ages 18 and 19) "calls into question the prevalent notion that early parenthood, by itself, has a destructive impact on the ambitions and attainments of teenagers." Another early study with conclusions in a similar vein is Rindfuss et al. (1984).

[8] Neither we nor Hoffman et al. find substantial within-family differences in the probability of ever marrying.

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