**Reevaluating the Socioeconomic Effects of Teenage Childbearing:
A Counterfactual Approach***

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* The analysis in this paper is under way. Results and Discussion/Conclusion sections are outlined. I expect a full version of the paper to be completed by November, 2006.

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INTRODUCTION

Despite the heated debate over 30 years on the socioeconomic effects of teenage childbearing, the consensus on the findings of its "true" effects is still unsettled (Hoffman 1998; Ribar 1999; Wu and Wolfe 2001). The detrimental life cycle consequences have been well documented: If a woman has a teen birth, she is more likely to drop out of high school, to poorly perform in the labor market, and to be on welfare (An, Haveman, and Wolfe 1993; Hofferth and Hayes 1987). These apparently adverse socioeconomic outcomes, however, tend to conceal teen mothers' disadvantaged backgrounds in the first place. From a theoretical standpoint, it is unclear whether they should be best understood as adolescents or disadvantaged women (Geronimus, Korenman, and Hillemeier 1994). Their negative socioeconomic outcomes may result from the incidence of being teen mothers or from the disadvantages they faced during childhood and adolescence. Separating these two factors creates a considerable methodological difficulty, which is known as selection bias (Winship and Mare 1992). If both observable and unobservable preexisting differences can account for the relationship between teenage childbearing and its socioeconomic consequences, any assertion of its causal effects becomes vulnerable.

In the presence of selection bias, researchers have developed methodologically enhanced alternative models (e.g., Korenman, Kaestner, and Joyce 2001; See Hoffman (1998) for a review). If teenage childbearing occurs randomly to female adolescents, as in an experiment designed to estimate its treatment effect, one can obtain the causal effects of teenage childbearing; other than teen birth, any differences between teen mothers and their comparison group stem from the randomization process. Unfortunately, this ideal cannot be achieved in most observational studies. It has been shown that analyses based on standard regression methods are not robust due to their failure in adequately controlling for preexisting socioeconomic differences between teen mothers and their comparison group; hence, most of the alternative models have focused on finding better comparison groups. For example, within-family fixed-effects models are designed to control for unobserved family-level heterogeneity by comparing teen mothers with their sisters who gave birth after age 20 (Geronimus and Korenman 1993; Hoffman, Foster, and Furstenberg 1993). Quasi-natural experiment approaches attempt to estimate the causal effects of teenage childbearing in terms of the approximate randomization procedures with observational data. They treat those who gave twin birth or had a miscarriage as comparison groups because these events are considered to occur randomly (Grogger and Bonars 1993; Hotz, McElroy, and Sanders 1997). Instrumental variables methods take a different approach to mitigating the selection bias problem in ways to utilize variables that are closely related to teenage childbearing but have no direct influence on its socioeconomic consequences (Klepinger, Lundberg, and Plotnick 1995; Olsen and Farkas 1989). Although all these models are intuitively appealing, they have their own caveats. As discussed below, it is not uncommon to see that they are grounded on somewhat strong assumptions as well as exposed to the unrepresentativeness of the samples used.

In this paper I propose a counterfactual analysis of the socioeconomic effects of teenage childbearing in an explicit causal framework (Rosenbaum and Rubin 1983; Rubin 1977). As selection bias has been of most concern in the literature, I employ propensity score matching to construct more reliable comparison group to teen mothers. This approach enables one 1) to find adolescents who did not give birth but are similar in all other characteristics to teen mothers based on a propensity to give birth; 2) to compare various socioeconomic outcomes between those who are teen mothers ("treated" group) and those who are not ("control" group) using semiparametric and nonparametric estimators. Since this sort of counterfactual analysis focuses mainly on selection bias due to observed characteristics, I also conduct a sensitivity analysis developed by Rosenbaum (2002) and DiPrete and Gangl (2004) to address selection bias due to unobserved characteristics. The counterfactual approach taken here is expected to produce new insights into the selection bias problem that is severe in research on teenage childbearing and its subsequent consequences.

This study also extends the literature on adolescent fertility by taking into account recent changes in the social context and trends in teenage childbearing. U.S. teen birth rate has steadily declined since 1991, when it was at its peak (Child Trends 2005; see Figure 1). The birth rate for teens between the ages of 15 and 19 was 41.7 births per 1,000 teens in 2003, which is a 33 percent decline from the high of 61.8 per 1,000 teens in 1991. This trend is accompanied by policy implementation for reducing teenage childbearing, rise in the proportion of teenagers who delay sex until older ages, prevalence of the use of contraceptives, and an increasing unfavorable attitude of teenagers toward early childbearing. In addition, over the past 15 years, economic returns to education have strengthened, a new welfare policy entitled the Personal Responsibility and Work Opportunity Act (PRWORA) of 1996 replaced the Aid to Families with Dependent Children (AFDC) with the Temporary Assistance for Needy Families (TANF), and the influx of immigrants, especially Hispanics, has continued. The implications of all of these changes are ambiguous with respect to the direction and magnitude of the socioeconomic effects of teenage childbearing. For instance, the decline in the teen birth rate is observed across all racial/ethnic groups, nevertheless Hispanic teens (82.2 per 1,000 teens) have been left behind other groups (27.5 per 1,000 teens for non-Hispanic whites and 64.8 per 1,000 teens for non-Hispanic blacks) in terms of reducing teenage childbearing (Ryan, Franzetta, and Manlove 2005). While the tightened education-employment nexus may give more penalties to teen mothers due to their lower educational attainment regardless of the changing composition of teen mothers, it may result in little harm to young mothers insomuch as teenage childbearing is endurable and even culturally acceptable for a non-trivial portion of them. Without data that reflect these social contextual changes, therefore, it is hard to examine the socioeconomic effects of teenage childbearing.

Data from the National Longitudinal Study of Adolescent Health (Add Health) are exploited to disentangle the relationship between teenage childbearing and its socioeconomic consequences from the latest cohort of young adults. Collected between 1994 and 2002, Add Health best fits the objective of this study because of its longitudinal design and timeliness that can capture recent changes in the social context. Add Health allows me, for example, to indirectly address how the increase of Hispanics in the U.S. population and the post-1996 welfare policy would have impacts on teenage childbearing and its socioeconomic outcomes.

TEENAGE CHILDBEARING AND ITS SOCIOECONOMIC EFFECTS: IS IT CAUSAL?

Theoretical Views

The negative socioeconomic consequences of teenage childbearing seem apparent. Insights from human capital theory provide theoretically convincing reasons for why (Becker 1993): The incidence of early childbearing tends to raise the opportunity costs of accumulation in human capital. First of all, being a mother during adolescence may be at odds with human capital investment because it is during this critical period that one's educational attainment is accumulated. Without a high school degree, the U.S. educational system does not allow one to move on to obtain a college degree, which is more valued in labor markets.[1] In addition, teenage motherhood may keep young mothers

[1] The General Educational Development (GED) has been another route to postsecondary education. Upon dropping out of high school, teen mothers are no less likely than other female high school dropouts to obtain the GED (Upchurch and McCarthy 1990). But the GED's intended

from labor force participation. It is due in most part to their low educational attainment and the unlikely compatibility between employment and child rearing. As a result, teen mothers tend to be more welfare-dependent and trapped in poverty. This point of view, thus, places more emphasis on teen mothers as adolescents caught in a harmful event rather than as disadvantaged women: Since they are still at a developmental stage of life, they would not be likely to take the appropriate economic, social, and psychological responsibilities of their own attainment and child rearing (Furstenberg 1991). Although teen mothers mostly come from disadvantaged families, having a teen birth further lowers their chances to escape prolonged poverty. Preexisting socioeconomic differences between teen mothers and other young women could reduce but not fully account for the detrimental consequences of early childbearing.

The other view, meanwhile, claims that teenage childbearing does not necessarily cause negative consequences for young mothers (Geronimus 1991; Geronimus, Korenman, and Hillemeier 1994). It takes seriously the fact that the majority of teen mothers come from impoverished families and neighborhoods, because their disadvantaged backgrounds are also a powerful factor to explain their subsequent attainment. What this fact implies is that it is highly unlikely that disadvantaged young women would get out of poverty regardless of postponing teenage childbearing. Furthermore, encountered with poor conditions and bleak prospects, they tend to adjust their attitudes and behaviors to sustain their socioeconomic viability. As opposed to young women from less disadvantaged groups for whom early motherhood is likely to be an obstacle to their future attainment, teenage childbearing may be a culturally rational response to poverty for disadvantaged young women, given the possibility of socioeconomic support from extended families and neighborhoods (Geronimus,

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effect is unclear: male GED recipients are akin to high school dropouts rather than high school graduates (Cameron and Heckman 1993), whereas female GED recipients fare better than high school dropouts but worse than high school graduates (Cao, Stromsdorfer, and Weeks 1996).

Korenman, and Hillemeier 1994). Hence, they may have an incentive for early childbearing as an adaptive strategy. From this revisionist point of view, teen mothers are viewed as disadvantaged young women rather than as adolescents experiencing an unplanned event. Since the adverse consequences of teenage childbearing may be an artifact of preexisting socioeconomic disadvantages faced by teen mothers, this view calls for focusing on substantive knowledge about these preconditioned differences between teen mothers and other young women as well as teenage childbearing per se in order to obtain its causal effects.

Alternative Models

Empirical consideration of these two contrasting views raises an important question as to how one can take the selection bias problem into account. If teen mothers are systematically different from other young women in terms of preexisting characteristics, teenage childbearing is endogenous to teen mothers' subsequent outcomes and so its causality is not established. For this reason, a variety of novel approaches have been taken to clarify the causal link between teenage childbearing and its socioeconomic consequences (e.g., Korenman, Kaestner, and Joyce 2001). Those alternative models carefully attempt to account for not only observable but also unobservable differences between teen mothers and other young women.

First, within-family fixed-effect models compare sisters whose childbearing was timed at different ages. Sisters share the same family and neighborhood characteristics so that comparing sisters is expected to eliminate unmeasured environmental factors. Geronimus and Korenman (1993) show that the cross-sectional studies overstate the correlation between teenage childbearing and its negative socioeconomic outcomes and the effects of early motherhood are minimal in most cases. When applying within-family fixed-effects models to multiple data sets (e.g., Hoffman, Foster, and Furstenberg 1993), however, the negative consequences for teen mothers amount to be small but remain statistically significant. In addition, the sister comparison gives rise to several substantive concerns. Its estimation of the causal effects of teenage childbearing is based on somewhat small samples in which sisters coresided at the time of survey. Even comparing teen mothers and their sisters who formed their own households does not result in strengthening the representativeness of the sample, because the sister study requires to select its sample from large families. More importantly, within-family fixed-effects models are not so powerful in capturing individual differences—especially time-varying—between teen mothers and their sisters. If teen mothers have lower levels of cognitive and noncognitive abilities than their sisters, a failure in controlling for these will make the negative effects of teenage childbearing biased upwardly; on the other hand, if a female adolescent gives birth as a strategic response to poverty and her sisters with grim socioeconomic prospects are more likely to coreside at home, the negative effects of teenage childbearing will be biased downwardly.

Second, instrumental variables methods are designed to take endogeneity of teenage childbearing into account, with most attention paid to finding such variables that satisfy the identification and exclusion problems (Klepinger, Lundberg, and Plotnick 1999; Olsen and Farkas 1989; Ribar 1994). The instrumental variables must meet the condition that they have direct influence on teenage childbearing but no influence on its socioeconomic outcomes; it is only through teenage childbearing that the instrumental variables affect the outcomes. For instance, age at menarche as well as a variety measures regarding abortion has been utilized as the instrumental variables. Despite that all these variables are justified in a statistical sense, a concern about theoretical justification remains to be resolved as the findings using the instrumental variables methods are mixed.[2] Usually, the assumption is untestable that age at menarche or the number of state abortion facilities has no relation to young women's attainment. It would not be unreasonable to infer that the instrumental variables used in the research may still be endogenous if they are correlated with race and poverty, which are known to have impacts on young women's socioeconomic consequences. Also, if the proposed instrumental variables are correlated with unmeasured variables that might affect the outcomes of interest, it can bias estimates of the causal effects of teenage childbearing (Bound, Jaeger, and Baker 1995).

Lastly, quasi-natural experiment approaches identify as the comparison groups teen mothers who gave twin birth or female adolescents who had miscarriages. Because of the random characteristic of twin birth and miscarriages, teen mothers would not systematically differ from these comparison groups. Then differences in socioeconomic consequences between teen mothers and non-teen mothers are approximated by differences between having two children and having one child or between teen mothers and adolescent women who experienced miscarriages. The twin study shows that teenage childbearing has modest but adverse effects on women's attainment (Grogger and Bonars 1993), while the miscarriage study finds most of the negative effects of teenage childbearing short-lived and its effects positive when teen mothers reach their mid- and late 20s (Hotz, Mullin, and Sanders 1997; Hotz, McElroy, and Sanders 2005). Although creative as they are, the findings from both studies need to be cautious to interpret due to the fact that twin birth and miscarriages are rare events. Moreover, as Hoffman (1998) pointed out, teen mothers with twins might benefit from economies of scale, compared with teen mothers with one child. This suggests that an approximation of the effects of teenage childbearing would be underestimated, because economies of scale should not

[2] For example, with different specifications on the endogeneity of teenage childbearing, Olsen and Farkas (1989) and Ribar (1994) find the negative effects of teenage childbearing disappear, whereas Klepinger, Lundberg, and Plotnick (1999) report that those effects do not disappear.

exist for teen mothers with one child, compared with non-teen mothers. Also, the miscarriage study would make the effects of teenage childbearing underestimated to the extent that the underreporting of miscarriages and/or abortions removes from the comparison group young women whose early pregnancy is more likely to be stigmatized.

A COUNTERFACTUAL APPROACH TO THE SOCIOECONOMIC EFFECTS OF TEENAGE CHILDBEARING

A Matching Framework for Causal Inference

As depicted in the alternative models above, an assessment of the "true" effects of teenage childbearing on socioeconomic consequences involves the fundamental problem of causal inference: One cannot simultaneously observe the outcomes of interest when a female adolescent gave birth (being a treated subject) and when she did not (being a control subject) (Holland 1986). In the experimental setup, this problem is solved by randomization. The treatment group is identical to the control group on all characteristics except for treatment assignment. Any differences in the outcome between the two groups are regarded as the causal effect of the treatment. In most social scientific studies, however, random assignment is infeasible: teenage childbearing is likely to occur nonrandomly. This selection bias problem invokes an important, but often neglected, issue: *The causal effect of teenage childbearing we try to assess is not the average treatment effect but the average treatment effect for the treated* (Dehejia and Wahba 2002; Harding 2003). In other words, given that teenage childbearing is concentrated among the disadvantaged subpopulation, a meaningful causal inference can be achieved by comparing teen mothers with teen mothers-to-be as better counterfactuals than non-teen mothers-to-be. Standard regression methods tend to produce unrealistic average treatment effect estimates by overlooking serious mismatches between those who gave teen birth and those who never did so.

In spirit of the counterfactual analysis with observational data, the analytic approach taken in this study seeks to identify a reliable comparison group that is similar in preexisting observed characteristics to adolescent mothers (Levine and Painter 2003). Rubin (1977) proves that conditional on covariates that are observed prior to the treatment—i.e., teenage childbearing here—, treatment assignment is independent of the outcome of interest; and then the average treatment effect for the treated is identified. This proposition can be written as:

$$\begin{aligned}
&\tau \mid_{T=1} = E(\tau_i \mid T_i = 1) = E(Y_{i1} \mid T_i = 1) - E(Y_{i0} \mid T_i = 1),\\
&\text{if } Y_{i1}, Y_{i0} \coprod T_i, \text{ then } E(Y_{i0} \mid T_i = 1) = E(Y_{i0} \mid T_i = 0) = E(Y_i \mid T_i = 0), \text{ and}\\
&Y_{i0} \coprod T_i \mid X_i \text{ for each } i,
\end{aligned}$$

where i indexes the population under consideration, τ is the treatment effect, T_i designates if the ith unit was assigned to treatment (1) or control (0), Y_{i1} and Y_{i0} are the values of the outcome of interest when unit i is subject to treatment (1) or control (0), respectively, $\coprod$ is the symbol for independence, $Y_i = T_iY_{i1} + (1 - T_i)Y_{i0}$, and X_i is a vector of pretreatment covariates. It assumes that unobserved covariates have nothing to do with treatment assignment. Unlike a randomized experiment where both observed and unobserved characteristics are balanced between the treatment and control group, one only balances these two groups on observed characteristics in this counterfactual framework with observational data. This ignorable treatment assignment assumption can be relaxed by conducting a sensitivity analysis described below by which the impact of an unobserved covariate is bounded.

To obtain the treatment effect for the treated, the counterfactual analysis matches two young women with the same preexisting observed characteristics, one of whom is a teen mother and the other is not. Basically each unit of observation could be stratified into subgroups in terms of a specific value of covariates, which is identical to conditioning on these preexisting observed characteristics. However, as more covariates are needed to ensure the determinants of teenage childbearing, there should be an increasing number of cells that contain no comparison unit.[3] Rosenbaum and Rubin (1983) suggest that propensity score matching greatly reduces the high dimensionality of the observed covariates. The propensity score, $p(X_i)$, is defined as the probability that a unit i receives treatment assignment. They show that:

$$\text{If } Y_{i1}, Y_{i0} \amalg T_i \mid X_i, \text{ then } Y_{i1}, Y_{i0} \amalg T_i \mid p(X_i).$$
$$\text{Thus, } \tau\mid_{T=1} = E_{p(X)}\left[\tau\mid_{T=1,p(X)} \mid T_i = 1\right]$$

If the true propensity score is known, a pair of the treated and the comparison groups matched by their true propensity scores would, in expectation, be balanced on both observed and unobserved preexisting characteristics. Since this is improbable in practice, estimated propensity scores obtained from a logit (or probit) model are used for matching the treated and the control groups based on observed covariates.

This approach takes the following procedure: 1) the estimated propensity scores are calculated with a logit model predicting whether or not a female adolescent becomes a teen mother[4]; 2) teen mothers are matched to non-teen mothers based on their propensity scores. Among a variety of matching algorithms, this study considers single-nearest-neighbor matching with replacement, caliper matching for a reasonable range of calipers, and kernel matching that weights each non-teen mother based on its distance from a teen mother (Morgan and Harding 2006)[5]; 3) whether matched groups are balanced on

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[3] Even if all n covariates are binary, the number of possible values for the covariates will increase exponentially, resulting in 2^n (Dehejia and Wahba 2002). For instance, if 10 binary variables have an effect on the incidence of teenage childbearing, there will be 1024 cells, each of which has to have a teen mother as well as a non-teen mother.

[4] In this model, the determinants of the incidence of teenage childbearing should be carefully sorted out due to potential selection bias in assessing its causal effects. See the data and measures section below. Again, the issue of selection bias on unobserved characteristics is discussed later in light of a sensitivity analysis.

[5] I use exact matching for age, race/ethnicity, and county of residence. Levine and Painter (2003) employ a within-school propensity score matching estimator to capture unobserved school-level characteristics such as peer influence. Yet if teen mothers and teen mothers-to-be are more likely to have friends who are not in school, matching those groups on county of residence makes more sense.

observed covariates is examined. If the propensity score model is well specified, there is little difference between teen mothers and non-teen mothers in terms of preexisting observed characteristics[6]; and 4) the simple χ^2 statistic assesses differences between matched teen mothers and non-teen mothers in subsequent socioeconomic outcomes that include educational attainment, employment status, and welfare dependency.[7] For comparison, estimates of the effects of teenage childbearing are presented with standard regression methods. This would reveal that how much of selection bias due to preexisting observed characteristics has an influence on estimating the causal effects of teenage childbearing.

The propensity score matching approach has clear advantages over previous studies using parametric approaches. While standard regression methods assume a specific function form, the matching estimators used here are nonparametric so that they do not need such an assumption. These estimators are known to be more efficient and free from collinearity because only the estimated propensity scores are required. On the other hand, there are several concerns with this counterfactual approach. There are good reasons to believe that a certain portion of teen mothers would still not have their counterfactuals. In this case, one can only estimate the causal effects of teenage childbearing for the subset of the treated group that overlaps with its comparison group (Heckman, Ichimura, and Todd 1998). This common support problem, however, also can shed light on how comparable teen mothers and non-teen mothers are to each other in terms of preexisting observed characteristics, which is not well understood in prior research. Of most concern as depicted earlier is that the propensity score matching approach itself cannot take

[6] It should be noted that to achieve full optimal balance, the entire joint distribution of the matching variables must be the same. Diamond and Sekhon (2005) deal with this issue in more detail.

[7] As in previous studies, the counterfactual analysis in this study yields estimates of the *total* effects of teenage childbearing. Attention should be paid to growing interest in the relationship between adolescent fertility and nonmarital childbearing and yet it requires a more complex modeling (Cherlin 2001).

selection bias due to unobserved variables into account. The sensitivity analysis described below can be employed to examine how robust matching estimates of the causal effects of teenage childbearing are in the presence of an unobserved covariate.

Sensitivity Analysis

The counterfactual analysis of teenage childbearing used in this study may be sensitive to “hidden bias” due to preexisting unobserved characteristics that influence both the incidence of teenage childbearing and its socioeconomic outcomes, even if this approach achieves the balance between teen mothers and non-teen mothers in terms of preexisting observed characteristics. If these two groups systematically differ in an unobserved fashion, the estimates of the causal effects of teenage childbearing obtained from the matching method will be biased. For example, researcher may not grasp the exact decision making process by families of where to live; if the process matters both for the incidence of teenage childbearing and for its subsequent outcomes, the true effects of early motherhood will not be estimated. The sensitivity analysis developed by Rosenbaum (2002) addresses the strength of such an unobserved variable that would relate to being a teen mother and resulting in a particular socioeconomic status in order to dismiss the causal inference made from the counterfactual analysis (See Appendix for a formal notation).

The Rosenbaum bounds method of sensitivity analysis assumes that a confounding unobserved covariate, U, exists that affects the odds of being assigned to the treatment, T, conditional on observed covariates, X. If U has nothing to do with T, then the assignment process is regarded as random. But as the influence of U on T becomes stronger, the confidence interval on the estimated effect of T becomes wider, and the significance level of the test of the null hypothesis of no effect of T on the outcome increases (i.e., the p-value goes up). In this scenario, one gauges the end points on the bounds for the significance level of the test of the null hypothesis for each assumed level of association between U and T. This enables one to find the case where the effect of U on the outcome is so strong that knowledge about U would almost perfectly predict the level of the outcome, whether or not a unit of observation received treatment assignment. Hence, the Rosenbaum bounds method provides a basis for assessing the endogeneity problem by making explicit the extent to which the ignorability assumption underlying the propensity score matching is vulnerable (DiPrete and Gangl 2004). That is, it is possible to benchmark the strength of unobserved confounding variables against observed variables, given many of the determinants of the incidence of teenage childbearing that have been identified in the literature. For instance, family structure is known to have a powerful effect on early motherhood and its socioeconomic consequences (Wu and Martinson 1993). By computing how large the magnitude of this effect should be for an unobserved covariate to reach a specific level of the Rosenbaum bounds where the effect of teenage childbearing becomes insignificant, we can examine the strength of hidden bias required to alter the causal inference about the effects of teenage childbearing on socioeconomic attainment.

DATA AND MESURES

Data

This study uses data from the National Longitudinal Study of Adolescent Health (Add Health). Add Health is a nationally representative, school-based, longitudinal study of adolescents in grade 7 to 12 in 1994-1995 (see Harris et al. (2003) for more information). The data at the individual, family, and neighborhood levels were collected in two waves between 1994 and 1996. The Add Health study employs a school-based design to select a stratified sample of 80 high schools with selection proportional to size. A feeder school per each high school was selected as well with probability proportional to its student contribution to the high school. Therefore, the school-based sample has a pair of schools in each of 80 communities. An In-school questionnaire was administered to more than 90,000 adolescents who attended each selected school on a particular day during the period of September 1994 to April 1995. Part of adolescents from the In-school survey was selected for In-home interviews. Based on the school rosters, a random sample of about 200 students from each high school and feeder school pair was collected to yield the core In-home sample of about 12,000 adolescents. The In-home interviews added special over-samples that included racial/ethnic minorities, physically disabled adolescents, and a genetic sample. These Wave I data produced a total sample size of 20,745 adolescents, 10,480 of which are female. Their parents also interviewed in Wave I. In 2001 and 2002, approximately 15,200 Wave I respondents, 8,030 of which are female, of ages 18 to 26 years old were re-interviewed in Wave III to investigate the influences that experiences in adolescence have on young adulthood.

Thanks to its strong emphases on social contexts such as families, schools, and neighborhoods, and its broad definition of health-related behaviors, Add Health provides valuable information suitable to this study. First, the Wave III sample contains event history data on fertility, educational attainment, labor market performance, and welfare receipt. Second, the Wave I sample provides rich sets of multilevel variables that measure observed covariates prior to the incidence of teenage childbearing, which are found to affect not only early motherhood but also adolescent mothers' socioeconomic attainment. Among these potential preexisting characteristics are differences in family influences, schooling, and neighborhood environment as well as individual differences in cognitive abilities and attitudes and behaviors. Teen mothers are more likely than non-teen mothers to grow up in poverty, to experience family instability, and to have academic and social problems in school. Third, the Wave I sample contains sets of variables that are previously unmeasured but considered key factors of teenage childbearing and various socioeconomic outcomes. For example, recent evidence shows that noncognitive skills such as "soft skills" have an important role in teenage childbearing and subsequent attainment, but the previous literature has not taken those measures into account (Heckman, Stixrud, and Urzua 2006).

Finally, and more importantly, Add Health allows this study to explore the socioeconomic effects of teenage childbearing in the late 1990s and the early 2000s, whereas past research has been limited to examine its effects at best in the early 1990s (Levine and Painter 2003). The issue of timeliness is important here because of significant social contextual changes during that period (Hoffman 1998). The 1990s witnessed growing economic return to education, changes in welfare policy, and the increase of Hispanics in the U.S. adolescent population, all of which could have influences on adolescents' fertility behavior. This study is not able to capture the direct impacts of these social changes, whether positively or negatively; nevertheless, I expect to reduce the ambiguities regarding the direction and magnitude of these changes by comparing the effects estimates of teenage childbearing from Add Health with those reported from previous studies.

Measures

TEENAGE CHILDBEARING This study obtains a measure of teenage childbearing from the Add Health life history calendar of the Wave III sample. It documents when a respondent gave birth. I treat a woman as a teen mother if she gave birth before or at age 18. To check the robustness of the results, I create a second indicator of teenage childbearing that treats a woman as a teen mother if she gave birth before or at age 20.

EDUCATIONAL ATTAINMENT As one of the key dependent variables, educational attainment represents socioeconomic status of young women. This study evaluates differences between teen mothers and non-teen mothers with four measures of educational attainment: dropping out of high school; graduating from high school; receiving the GED; and attending college. In a case where GED recipients are considered high school graduates, it is found that there is little difference between teen mothers and non-teen mothers (Upchurch and McCarthy 1990). In the other case, there is ample evidence that they are more akin to high school dropouts (Cameron and Heckman 1993; Heckman and Rubinstein 2001). Thus, this study reports the results in both cases where high school graduates contain GED recipients or does not.

LABOR MARKET PERFORMANCE For a second dependent variable, this study measures teen mothers' labor market performance relative to that of non-teen mothers with employment status. With a limited analytic sample consisting of respondents who were enrolled in school at Wave III, I compare teen mothers with non-teen mothers in terms of whether they were employed, unemployed, or on the job training and then in terms of whether they worked full-time or part-time among the employed.

WELFARE DEPENDENCY As a third dependent variable, welfare receipt signifies an important dimension of young women's socioeconomic consequences. This study identifies a respondent as welfare-dependent if she received AFDC, public assistance, or welfare payments within the subsequent years until 2001. I take out welfare receipt before teenage childbearing from this part of the analysis.

INDIVIDUAL CHARACTERISTICS A set of individual-level variables measured at Wave I are included in the analysis to calculate the propensity score of whether an adolescent gave birth. These variables are also found to influence young adults' socioeconomic attainment. This study measures age, race/ethnicity, cognitive ability, noncognitive skills, religiosity, and risk behaviors. Race/ethnicity is classified as non-Hispanic whites, non-Hispanic blacks, Hispanics, and Asians. For a measure of cognitive ability, the Add Health Wave I sample provides data on the Add Health Picture Vocabulary Test (AHPVT), an abbreviated version of the Peabody Picture Vocabulary Test with age-standardized scores for adolescents. This study improves on the literature by including measures of noncognitive skills, which are referred to as both attitudinal and behavioral personal traits that are correlated with but distinct from cognitive skills. While unmeasured in most research on the effects of teenage childbearing, noncognitive skills are found to play a significant role in early motherhood and various adult outcomes (Heckman, Stixrud, and Urzua 2006; Plotnick 1992). The Add Health Wave I sample provides a combination of the Rotter's locus of control scale and the Rosenberg's self-esteem scale.[8] In addition, a measure of the quality of social relationships is constructed. How well a respondent gets along with others, such as parents, teachers, and friends, is an important component of noncognitive skills. This study constructs a composite measure based on the respondent's relationship with each group of significant others. Religiosity is a composite measure of attendance to religious services (from once a week or more to never), the importance of religion (from very important to not important at all), and the oftenness of prayer (from at least once a day to never). Risk behaviors are measured with the questions of how many days a respondent did smoke or drink during the past 12 months.

FAMILY BACKGROUND As one of the most important factors of the incidence of teenage childbearing as well as its socioeconomic consequences, the family deserves attention. Family background covers family structure, parental education, parenting, and the number of siblings. Family structure is categorized as two-parent biological families, two-parent step families, single-mother families, single-father families, and other families (e.g., foster families). Parental education is measured with the highest level of education either of the parents obtained and categorized as less than high school, high school

[8] Locus of control measures the degree of control individuals feel ranging from external to internal. According to Rotter (1966), individuals who believe that outcomes are due to luck have an external locus of control while individuals who believe that outcomes are due to their own efforts have an internal locus of control. The self-esteem scale measures perceptions of self worth (Rosenberg 1965). Both scales have been commonly employed in past research of the effects of noncognitive skills on socioeconomic outcomes.

graduate, some college experience, and college graduate or more, with an indicator of missing observations on parental education. Parenting is measured by parental monitoring, which indicates how involved parents are in children's activities. To construct this measure, I calculate the total count of their activities monitored by parents, ranging from 0 to 7, including curfews, friendships, TV watching, and food and dress choices.

SCHOOL ENVIRONMENT School facilitates interactions of adolescents with teachers and peers by way of providing role models and developing adaptive strategies. I focus on measures of the collective socialization (Coleman 1990). These include (1) school's structural characteristics, such as the percentage of white in school, school type (private/public), and school location and (2) school climate, such as school mean of GPA, school-level degree of participation in extracurricular activities, school-level expectations of the future, and school mean of family income.

NEIGHBORHOOD ENVIRONMENT Socioeconomic conditions of neighborhood might define individual's opportunity structure and the normative climate during adolescence and subsequently affect their future outcomes (Massey and Denton 1993; Wilson 1987). Several measures of local labor market contexts are constructed from both data sources: the percent idle, which means young people who were neither at work nor in school; an index of racial heterogeneity; total unemployment rate; and the percentage of families of which income is below the poverty level.

RESULTS

Descriptive Statistics

- Full sample / Teen mothers / Non-teen mothers

Matching Results

- Results from the propensity score logit model
- Descriptive results from matched sample for balance check
- Results from propensity score matching: educational attainment, employment status, and welfare dependency; results from standard logit regressions for comparison
- Results from the sensitivity analysis

- Comparison between matching results from Add Health and those from previous studies

DISCUSSION AND CONCLUSION

- Summary
- Theoretical, methodological, and policy implications
- What-to-do for future research

APPENDIX: A SENSITIVITY ANALYSIS

The Rosenbaum bounds method (Rosenbaum 2002) of sensitivity analysis is complementary to the estimation of treatment effects using data on matched pairs. Although Rosenbaum developed the theory for a more general case, I limit the focus to his treatment of the case of matched pairs (See Chapter 4 of Rosenbaum (2002) and DiPrete and Gangl (2004) for more details).

Test statistics in the family of sign score statistics have the form

$$T = t(Z, r) = \sum_{s=1}^{S} d_s \sum_{i=1}^{2} c_{si} Z_{si}, \qquad \text{(A.1)}$$

where Z is the variable that designates which of each of the s pairs was treated, and r measures the outcome for each case in the S pairs. Z_{si} equals 1 if a case is treated, and 0 otherwise; c is defined as follows:

$$\begin{aligned} c_{s1} &= 1, c_{s2} = 0 \quad \text{if} \quad r_{s1} > r_{s2}, \\ c_{s1} &= 0, c_{s2} = 1 \quad \text{if} \quad r_{s1} < r_{s2}, \\ c_{s1} &= 0, c_{s2} = 0 \quad \text{if} \quad r_{s1} = r_{s2}. \end{aligned}$$

Finally, d_s is the rank of $|r_{s1} - r_{s2}|$ with average ranks used for ties. The product of the c and Z variables causes pairs to be selected where the outcome for the treatment was greater than the outcome for the control. The ranks of these cases are summed and compared with the distribution of the test statistic under the null hypothesis that the treatment has no effect.

In the case where the assignment to treatment is not random, the above test statistic can be bounded. It is assumed that there is an unmeasured variable, U, that affects the probability of receiving the treatment. If we let π_i be the probability that the ith unit receives the treatment, and X is the vector of observed covariates that determine treatment and that also determine the outcome variable, then the following treatment assignment equation applies:

$$\log\left(\frac{\pi_i}{1-\pi_i}\right) = \kappa(X_i) + \gamma U_i, \quad \text{where} \quad 0 \le U_i \le 1. \tag{A.2}$$

Rosenbaum (2002) shows that this relationship implies the following bounds on the ratio of the odds that either of two cases which are matched on X—or alternatively on the propensity score $p(X)$—will receive the treatment

$$\frac{1}{\Gamma} \le \frac{\pi_{s,1}(1-\pi_{s,2})}{\pi_{s,2}(1-\pi_{s,1})} \le \Gamma, \tag{A.3}$$

where s indexes the matched pair, $s = 1,\ldots,S$, and $\Gamma = \exp(\gamma)$.

Under the assumption that a confounding variable U exists, equation (A.1) becomes the sum of S independent random variables where the sth pair equals d_s with probability

$$p_s = \frac{c_{s1}\exp(\gamma u_{s1}) + c_{s2}\exp(\gamma u_{s2})}{\exp(\gamma u_{s1}) + \exp(\gamma u_{s2})}$$

and equals 0 with probability $1 - p_s$. Define

$$p_s^+ = \begin{cases} 0 & \text{if} \quad c_{s1} = c_{s2} = 0, \\ 1 & \text{if} \quad c_{s1} = c_{s2} = 1, \\ \frac{\Gamma}{1+\Gamma} & \text{if} \quad c_{s1} \ne c_{s2}, \end{cases} \quad \text{and} \quad p_s^- = \begin{cases} 0 & \text{if} \quad c_{s1} = c_{s2} = 0, \\ 1 & \text{if} \quad c_{s1} = c_{s2} = 1, \\ \frac{1}{1+\Gamma} & \text{if} \quad c_{s1} \ne c_{s2}. \end{cases}$$

Rosenbaum (2002) shows that for any specific γ, the null distribution of $t(Z,r)$ is bounded by two known distributions for T^+ and T^- that are attained at values of U, which perfectly predict the signs of c_{si} in equation (A.1), where

$$E(T^+) = \sum_{s=1}^{S} d_s p_s^+ \quad \text{and} \quad \text{var}(T^+) = \sum_{s=1}^{S} d_s^2 p_s^+ (1-p_s^+),$$
$$E(T^-) = \sum_{s=1}^{S} d_s p_s^- \quad \text{and} \quad \text{var}(T^-) = \sum_{s=1}^{S} d_s^2 p_s^- (1-p_s^-).$$

One can use these formulae to compute the significance level of the null hypothesis of no treatment effect. For any specific Γ, we compute

$$(T - E(T^+)) / \sqrt{\mathrm{var}(T^+)} \quad \text{and} \quad (T - E(T^-)) / \sqrt{\mathrm{var}(T^-)}$$

where T is the Wilcoxon signed rank statistic. These two values give bounds of the significance level of a one-sided test for no treatment effect.

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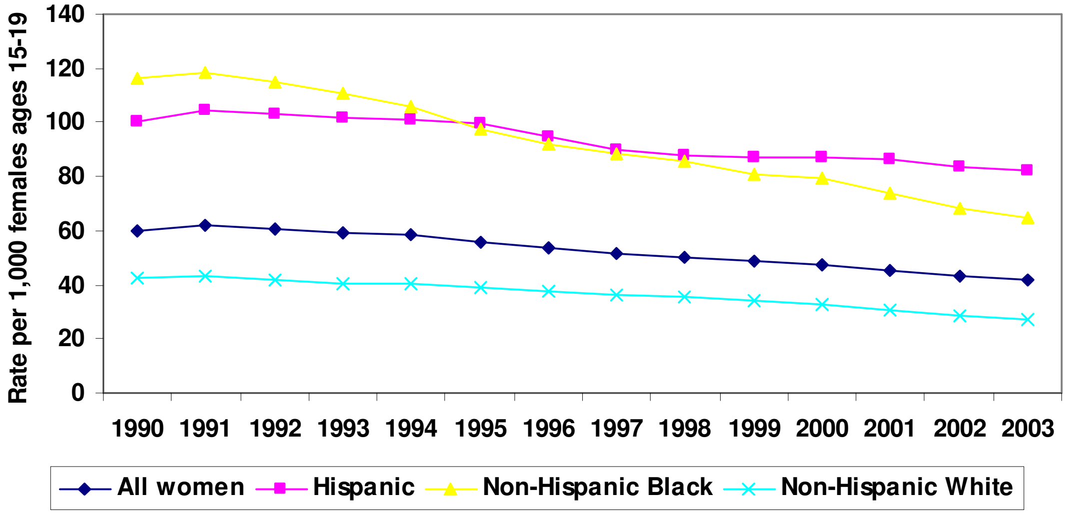
Figure 1. U.S. Teen Birth Rates, 1990-2003



Data source: National Surveys of Families and Households (NSFH).