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ESSAYS IN FAMILY ECONOMICS

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ABSTRACT

The first chapter examines the effect of the number of years children spend living with a single-parent family instead of a two-parent family on children's completed schooling, based on a sample of children from the PSID. To deal with the endogeneity of mothers' family structure decisions, I exploit the variation across states and over time in unilateral divorce laws, unmarried fertility ratios, welfare rules, earned income tax credit rates, and labor market conditions that generate plausibly exogenous changes in mothers' family structure choices. I construct a set of extensive measures for these contextual variables and use them as instruments to estimate a child's human capital production function. Instrumental variable estimation indicates that one additional year spent in a single-parent family during childhood (ages 0–15) can cause a loss of 0.145 years in schooling. This result implies that the differences in family structure experiences over the early life course between white and nonwhite children can explain roughly 76% of the gap in educational attainment between the two groups. On the other hand, ordinary least-squares estimation only suggests 13%.

Children born to unmarried parents may receive lower human capital investments in youth, and therefore may be less likely to finish high school or to attend college. The second chapter explores these effects empirically using state level data over the period 1940-2000. We find that a steady-state increase in unmarried fertility ratio of 100 per 1,000 child births could lead to a 4.6 percent drop in high school graduation rate and a steady-state 4.2 percent decline in secondary school enrollment in the long-run. This result is important since Heckman and Lafontain (2010) found that since the late 1960s

the high school graduation rate has fallen by 4-5 percentage points, despite the growing wage differentials between high school graduates and dropouts. Our analysis implies that the rise in unmarried fertility predicts a ceteris paribus drop in high school graduation rate of about 6.6% in the same time period, thus provides an important explanation for the dropout problem in recent decades.. Moreover, our results indicate a very weak link between abortion and child education, in contrast to the strong effect of abortion on crime documented in the literature.

DEDICATION

To my wife Jin and my parents, Zhangyin Bai and Guifang Feng. Their love and support mean the world to me.

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CHAPTER ONE

THE EFFECT OF FAMILY STRUCTURE ON CHILDREN'S EDUCATION: AN INSTRUMENTAL VARIABLE APPROACH

1. Introduction

A large and growing literature has found a significant correlation between family structure and children's development and future well-being. On average, children growing up in two-parent families tend to fare better than children growing up in other family types in terms of a large variety of outcomes, including improved cognitive, emotional, and physical well-being, better performance in school and labor market, and lower risk of teenage or nonmarital childbearing.[1] However, it is difficult to interpret this large literature causally since family structure is not exogenously assigned but selected by parents. This paper asks whether children raised in a two-parent family would achieve better educational results compared to children raised in a single-parent family. This investigation is done using instrumental variables (IVs) to control for the endogeneity of family structures.

Most earlier findings are based on simple correlation studies and fail to account for endogeneity or self-selection problems. The main limitations lie in the fact that family structure changes are not exogenous or random events independent of other determinants of children's outcomes. There may be unobserved variables or processes that jointly determine family structure and children's outcomes. To be more specific, two key sources of selection bias could arise: (1) Women who choose to marry or cohabitate with

[1]Ginther and Pollak 2004; McLanahan and Sandefur 1994; Astone and McLanahan 1994; Fronstin, Greenberg, and Robins 2001; Cherlin, Kiernan, and Chase-Lansdale 1995.

a partner may be systematically different from women who choose to be single in both unobserved preferences and ability. (2) A child's unobserved ability or schooling prospects may affect the mother's marital decisions.

To illustrate the first problem, suppose that mothers who are more child-oriented are more likely to exit low-quality marriage or to be more careful and patient in their search for a partner. Then, the estimated effect of living in single-parent families on child education will be upwardly biased. To illustrate the second problem, suppose that mothers with low ability are more likely to have low-ability children and are more likely to compensate their children by entering or staying in a marriage for a potential extra earner or caregiver. In this case, the estimated effect of single-motherhood on child education will also be biased upward. Therefore, both unobserved characteristics of mothers and children may influence mothers' decisions on family structure. The presence of these unobserved characteristics makes it very difficult to estimate the effect of family structure decisions on children's outcomes.

To make things worse, measuring family structure correctly is very challenging given the recent rise of more complex family arrangements, such as cohabitation and stepfamilies. Due to the ambiguity of family boundaries associated with these family forms, reporting them is often inconsistent among family members and is dependent on the measurement strategies.[2] If the measurement error of family structure is random, then the estimated effect of family structure on child outcomes would be biased toward 0.

[2]See Brown and Manning (2009) for a recent review of the related studies.

To address the above-mentioned issues, this paper estimates the effect of family structure on child educational attainments by using IVs that are related to the mothers' family structure decisions but otherwise unrelated to child outcomes. In particular, I examine the effect of the number of years a child spends living with a single-parent family from birth to age 15 on the child's number of completed schooling years by the age of 25. My estimation is based on a sample of children from the Panel Study of Income Dynamics (PSID) born between 1968 and 1982. I choose this sample since the childhood years of these children (1968–1997) witnessed major changes from the following four sources that may lead to plausibly exogenous variations in mothers' family structure decisions: (1) unilateral divorce laws, which were adopted by most states during the 1970s and which increased the ease of divorce by not requiring the consent of both partners; (2) the sharp increase in unmarried fertility ratio (UFR) since the 1960s, which could imply a waning social stigma against single-motherhood; (3) Welfare Waivers between 1993 and 1996, and the Temporary Assistance for Needy Families (TANF), which replaced the Aid to Family with Dependent Children (AFDC) in 1996, imposing time limit and work requirement restrictions on welfare recipients; (4) Earned Income Tax Credit (EITC), the expansions of which in 1986, 1991, 1994, and 1996 led to a larger marriage tax penalty.

I construct an extensive set of measures for the state-level unilateral divorce regulations, UFRs, welfare rules, EITC rates, and local labor market conditions over the first 16 years of a child's life, and I use them as instruments for the mother's choices about family structures over the same childhood years in the estimation of the child's human capital production function. These variables turn out to be valid instruments and are reasonably powerful in explaining mothers' marital behaviors. Moreover, the substantial variations in these contextual variables across states and over time provide the basis for identification.

One problem with an extensive list of IVs is that the two-stage least-squares (2SLS) estimates can be severely biased toward the probability limit of the corresponding ordinary least-squares (OLS) estimates when there are many overidentifying instruments (see, e.g., Stock and Yogo 2004; Andrews and Stock 2006; and Hansen et al. 2008). To overcome this problem, I use the limited information maximum likelihood (LIML) estimator, which is approximately unbiased for the overidentified model (see Flores-Lagunes 2007 and Hansen et al. 2008). The 2SLS estimates lie between those of OLS and LIML, implying that 2SLS does suffer from a strong bias toward OLS. Stock and Yogo proposed a test on whether such many-instrument (or weak-instrument) biases are tolerable compared to the OLS bias. The test suggests a strong bias for 2SLS but does not signal a bias problem for LIML.

The main results indicate that living in a single-parent family has a significant and sizeable detrimental effect on children's educational outcomes. In particular, one additional year spent in a single-parent family during childhood (ages 0–15) can cause the child to lose 0.145 years in schooling. This result is robust to a wide range of alternative sets of instruments. This is quite comforting since IV estimates are known to only estimate a local average treatment effect and are very sensitive to the instruments used. My findings also suggest a strong downward bias associated with OLS estimation.

Based on the PSID sample, the LIML estimate implies that the differences in family structure experiences over the early life course between white and nonwhite children can explain roughly 76% of the gap in educational attainment between the two groups, holding everything else equal. On the other hand, OLS suggests only 13%.

The rest of the paper is organized as follows. Section II reviews alternative methods for dealing with the endogeneity problem. Section III presents the empirical model. Section IV describes the sources of the instruments. Sections V and VI present the results and conclusions, respectively.

2. Literature Review

The literature that examines the relationship between family structure and children's well-being is extremely large. However, most studies describe the correlation between child outcomes and family structures and suffer from the endogeneity biases discussed above. Many studies acknowledge this problem, and some attempt to overcome it using the following methods.

If the bias comes from omitted variables that are related to both family structure and children's outcomes, the most straightforward approach is to add these omitted variables in the equation. However, it is difficult to identify all the omitted variables, let alone find good measures of these. In practice, researchers include numerous variables to serve as potential indirect control or proxy variables, among which family resources and background information are the most common. For example, McLanahan and Sanderfur (1994) showed that the difference in income accounts for as much as half of the difference in school achievement between children from two-parent families and one-parent families, and their finding holds across three US surveys (PSID, NLSY, and NSFH). Fronstin, Greenberg and Robins (2001) found similar results from UK data and recorded weaker effects of parental disruption on labor market performance for both males and females after controlling predisruption characteristics. Almost all such studies find a weaker linkage between family structure and child well-being as they add controls. However, to the extent that these extensive controls are imperfect approximations of the actual omitted variables, the results may still be biased. Moreover, this approach could not address biases from reversal causality (e.g., parents take their children's future outcomes into consideration when making family structure choices).

Sibling comparison could be used to control unobserved family-specific variables, but this control does not eliminate biases that come from other error structures. For example, the unobserved factors specific to each sibling within one family still cannot be accounted for. Using data from US and UK, respectively, both Sandefur and Wells (1999) and Ermisch and Francesconi (2001) indicated that sibling controls weaken the relationship between family structure and a child's schooling but do not eliminate it. In comparison, based on an NLSY sibling sample from 1986 to 1994, the analysis of Ginther and Pollak (2003) shows no statistically significant link between schooling and family structure. Sibling analysis relies on the differences in the siblings' childhood experiences in alternative family structures to explain the differences in their future outcomes. Families with relatively stable family arrangements (a majority of the families) do not contribute much to this analysis. Moreover, sibling analysis cannot address the problems of reverse causality or measurement errors.

Another approach would be to look for a natural experiment. While it is clearly not feasible to randomly assign households to different family types, Corak (2001) and Lang and Zagorsky (2001) used parental death as a quasi-natural experiment for single parenthood. Both found that parental death has much less impact on children than parental absence because of divorce. Their studies suffer from two main problems. First, families that experienced parental death can be significantly different from families that did not. Numerous studies find a link between mortality, and marital quality.[3] Second, the effect of parental deaths can be different from divorces or separations. For example, there are differences in the financial and social support that widowed and divorced single-parent families receive. The distress and behavioral patterns of family members under each situation may also differ. Thus, it is difficult to find a feasible quasi-natural experiment situation where family structure changes are exogenous, and people who experience them are representative of the whole population.

Very few studies have attempted to use IVs. Manski, Sandefur, McLanahan, and Powers (1992) evaluated alternative parametric and prior information assumptions in estimating the effect of family structure on high school completion. The standard probit model and the endogenous switching regression model generate very similar results, and both indicate that residing in a nonintact family at age 14 decreases the probability of graduating from high school. Both results also fall within the nonparametric bounds. So,

[3]See Coyne et al. (2001) for example.

the authors concluded that there is little evidence to support the endogeneity of family structure. However, the instruments used in the endogenous switching regressions—the region indicators and parents' educational differences—are questionable since it is difficult to rule out the possibility that these variables have direct impact on children' high school outcomes.[4]

3. Empirical Methods

The following human capital production function could be estimated to examine the impact of the family structure experience during childhood on the children's educational attainment:

$$H_i = \alpha_0 + \alpha_1 \overline{M}_i + \alpha_2 \ln\overline{I}_i + \alpha_3 X_i + \alpha_4\delta_a + \omega_i + \tau_i^H + \epsilon_i^H \qquad (1)$$

where H_i represents the completed years of schooling for child i by age 25; $\overline{M}_i$ measures the number of years child i spent in a single-parent family as opposed to a two-parent family from birth through age 15; $\overline{I}_i$ is the average family income need ratio[5] for child i also from birth to age 15 (the family income need ratio is a better measure than just the family income for economic resources accessible to child i since it is adjusted according to the family size and the needs of all family members[6]); X_i is a vector of observed family/child characteristics, including the mother's education, the mother's age

[4]They also assume that the sex of children would not affect family structure. Dahl and Moretti (2008) showed that women with first-born daughters are less likely to marry and more likely to be divorced, and they also found that fathers are more likely to obtain custody of sons than daughters after a divorce.

[5]The family income need ratio is computed by dividing the family income by the need standard specific for the family for a certain year. Both the family income and the need level are in 1983 dollars. Family income includes both taxable income (e.g., labor income and asset income) and transfer income. Need standard is Orshansky-type poverty threshold based on the annual food need standard with an additional adjustment for diseconomies of small households (in rent, etc.).

[6]The estimation results are robust to alternative income measures, such as per capita family income.

at the child's birth, number of children born to the mother, child i's gender, birth order to the mother, birth weight, race, religion, and urban/rural factors; δ_a is a dummy for child i's birth year to control for the birth cohort fixed effect; ω_i is child i's unobserved ability endowment; τ_i^H is the mother's taste for child educational attainment; and ϵ_i^H is the random error.

Following the human capital literature, I made several assumptions in Equation (1). First, child educational attainment is determined by the cumulative experience of family arrangements and cumulative economic inputs. Second, family income measures could approximate for the economic inputs in child human capital developments. In addition, the time-invariant family/child characteristics, including the unobserved ability endowment, have a constant effect over time.

The endogeneity problem arises because inputs $\overline{M}_i$ and $\overline{I}_i$ may be correlated with the child's unobserved ability ω_i and the mother's preference τ_i. To further clarify this problem, assume the mother's reduced-form decision rule for family structures over child i's childhood years ($\overline{M}_i$) to be

$$\overline{M}_i = \beta_0 + \beta_1\,\omega_i + \beta_2\,X_i + \beta_3\,\overline{R}_i + \tau_i^M + \epsilon_i^M \qquad (2)$$

where $\overline{R}_i$ is a set of contextual variables that form the state-level legal, social, and economic environments that may influence the mother's marital decisions from birth to age 15 of child i, including the cumulative measures over the same span of unilateral divorce laws, UFRs, welfare rules, EITC rates, and labor market conditions; τ_i^M is the mother's marriage preference; and ϵ_i^M is a random error.

Child ability endowment ω_i enters Equation (2) since the mother's family structure decisions may be affected by the child's ability. For example, mothers with low ability are more likely to have low-ability children and are more likely to compensate their children by entering or staying in a marriage for help from a potential extra earner or caregiver. Mother's preferences for child education τ_i^H may also influence family structure choices through her marriage preference τ_i^M. For example, mothers with the strongest preference for child education are more likely to exit low-quality marriage or to be more careful and patient in their search for a partner. Therefore, the estimated coefficient on family structure by OLS in Equation (1) is very likely to be inconsistent.

$\bar{R}_i$ enters Equation (2) for two reason. First, these family-formation-related contextual variables can directly influence a mother's family structure choices. For example, unilateral divorce laws make divorce easier by not requiring consent from both partners. Second, since the mother's decisions on family structure and labor supply are made jointly, any factors that affect her labor supply can indirectly influence marital decisions. Changes in transfer program policies as well as market demand conditions all have important effects on work decisions.

$\bar{R}_i$ can serve as valid instruments for $\bar{M}_i$ in a child's human capital production function (1), assuming that these contextual variables are uncorrelated with unobserved child ability endowment ω_i and the mother's preference for child outcomes τ_i in Equation (1). This exogeneity assumption seems plausible.

The cumulative income $\bar{I}_i$ in Equation (1) is also potentially endogenous for two reasons. First, family income depends on the mother's marital and work decisions, and so it is potentially correlated with a child's ability, which plays an important role in determining marital and work choices. Second, income depends on the mother's ability endowment. To the extent that the mother's ability is not fully captured by her observed characteristics, such as education, and that the residual part is correlated with the child ability, this will also generate a potential correlation between income and unobserved child ability. Thus, I need to instrument for family income in Equation (1). Again, $\overline{R}_i$ could serve as a set of plausible instruments since these state-level contextual measures may have important effects on income, while at the same time they are not related to an individual child/mother's ability.

By using the set of variables in $\overline{R}_i$ as instruments, I can address the endogeneity issues associated with family structures and establish the causal link between family structure and a child's educational outcomes by estimating the child's human capital production function (1).

4. Instrumental Variables

To construct the instruments, I collect detailed information about the substantial changes in divorce law, UFRs, welfare rules, EITC rates, and labor market conditions during the past few decades, which may be the main driving forces behind the changes in family structures. Table 1.1 presents the list of IVs used in this paper. In the following sections, I briefly outline the main relevant aspects of the sources for these instruments and discuss previous findings from the literature on each of them.

A. Unilateral Divorce Laws

Under traditional state divorce regulations, divorce requires consent from both spouses. In the 1970s, to remove the high transaction costs and legal inefficiencies of the divorce process, there was a movement toward the unilateral divorce laws that allow divorce with the consent of either spouse.[7] Most states enacted the laws in 1970s with five states even before 1970, while 17 other states never adopted the law. See the Table 1.9 for more details. The dramatic change in divorce regulations across states in the 1970s is accompanied with the sharp rise in divorce rates for the past few decades (see Friedberg 1998, Figure 1). It appears that, by making divorce easier, the enactment of unilateral divorce laws leads to higher divorce rates.

Many studies have examined the effect of divorce laws on divorce rates. Peters (1998) argued that under the assumptions of symmetric information and no transaction costs, the change in law from mutual divorce to unilateral divorce would simply move the property rights from the spouse who wished to remain in the marriage to the spouse who wished to leave, without making divorce more likely. To support her theory, she conducted a cross-sectional analysis based on a sample of women in 1979 and found no impact of the unilateral divorce. On the other hand, Allen (1992) used the same cross-sectional data but found a significant role of unilateral divorce on divorce. Friedberg (1998) revisited this question using state-level panel data from 1968 to 1988. By including state fixed effects and flexible state time trend controls, her study reveals a strong positive association between unilateral divorce and divorce rates. Based on 40

[7]The other important feature of divorce laws reforms is the move to a no-fault divorce, which was already in place in many states before 1970. Since the change to no-fault divorce laws had little impact on divorce, it is not the focus of my discussion.

years of census data, Gruber (2004) confirmed that unilateral divorce regulations do significantly increase the incidence of divorce. Wolfers (2006) indicates that Friedberg (1998)'s analysis fails to "separate out preexisting trend from the dynamic effects of a policy shock" (p. 1802), and his modified estimation concludes that much of the effects arise soon after the change in divorce regulations, and the rise in divorce is reversed in the long run.

Several studies examine the other impacts of unilateral divorce laws. Gruber (2004) provided some evidence for increased entry into marriage. He also found that children growing up in a unilateral divorce regime are less well educated, enjoy lower incomes, and are more likely to marry earlier. Alesina and Giuliano (2006) implied that women who plan to have a child are more willing to have the child within marriage as unilateral divorce regulations make it easier to escape marriage.

To fully capture the dynamic effect of unilateral divorce laws on a child's family structure experience during childhood years, I use as an instrument the total number of years the child resides in a state with the laws in place from birth through age 15. The average childhood years exposed to a unilateral regime is slightly above 7 years with a standard deviation of 7.33. My findings indicate that exposure to a unilateral regime increase the number of years living in a single-parent family over childhood.

B. Unmarried Fertility Ratio (UFR)

The UFR for state i in year t is defined as

$$UFR_{it} = 1{,}000\left[\frac{births\ to\ unmarried\ \ women_{it}}{total\ \ live\ births_{it}}\right]$$

As shown in Figure 1.2, the national UFR grew slowly first and then started to rise rapidly after the 1960s. In addition to standard economic incentive-based models (e.g., welfare payments for unmarried mothers), there are also a host of more cultural/behavioral models claiming that the growth in out-of-wedlock childbearing may be associated with the decreased social stigma against single-motherhood. In general, stigma may serve as a substitute for legal restrictions on nonmarital childbirth (see Posner 2000, Chapter 5). I argue that the UFR could be used as a proxy for the level of social norms against single parenthood. Also, UFR is much higher among nonwhites than whites, which corresponds to a stronger social disapproval of single motherhood among whites.

Using panel data on state-level UFR by race,[8] I constructed the average value of UFR for each child from birth to age 15, specific to the child's birth year, race, and state of residence. My first-stage results indicated that this UFR measure has positive effects on the number of childhood years for a child to live with a single mother.

C. AFDC, Waivers, and 1996 Welfare Reform

Between January 1993 and August 1996, the Department of Health and Human Services approved welfare waivers in 43 states under Section 1115 of Title IV-A of the Social Security Act. These waivers can be considered the first phase of welfare reform;

[8]UFR data are taken from Kendall and Tamura (2010).

many of the policies and concepts included in the state waiver requests were later incorporated into the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996. This comprehensive legislation changed the welfare system into one requiring work in exchange for time-limited assistance. It created the TANF program, which replaced AFDC. Under TANF, states and territories operate their own programs, so a great deal of heterogeneity in welfare rules across states has emerged. The main changes introduced by both Section 1115 Waivers and the 1996 welfare reform that are relevant to my analysis are time limits and work requirement rules.

Time Limit: Under the AFDC rules, families were entitled to receive assistance for as long as they met the eligibility standards. Due to concerns that families were becoming dependent on AFDC and accepting welfare as a way of life, a number of states applied for and received waivers that allowed them to set time limits on welfare receipt. By the time AFDC was repealed, a total of 32 states had received waivers authorizing some form of time limits. Under TANF, all states could set their own time limits, though they are forbidden to use federal funds to provide assistance to a family that includes an adult who has received assistance for 60 months. A great deal of heterogeneity across states emerged as a result. For example, Florida and Georgia set the limits at 48 months, lower than the standard 60, while New York did not impose a time limit. See Table 1.10,[9] for more details about time limits. I construct three instruments to capture the effects of time limits, all of which are the average values over a child's childhood years of the

[9]Special thanks to Bernal and Keane (2011) for sharing with me their data on welfare rules shown in Appendix, Tables 1.2–1.4.

following variables: a dummy for whether the state had a time limit, the length of the time limit, and years elapsed since the time limit was first implemented.

Work Requirement: TANF provides that states must require adults to work after they have received assistance for 24 months, or earlier at state option. States differ greatly in their work requirements rules. In 1998, 21 states require welfare recipients to commence work immediately, and 24 states set a more generous work requirement time limit of 24 months. See Table 1.11 for more details about work requirement. Like benefit termination time limit, I construct three instruments to capture the effects of work requirement, all of which are the average values over a child's childhood years of the following variables: a dummy for whether the state had a work requirement time limit; the length of the work requirement time limit; and years elapsed since the work requirement time limit was first implemented.

AFDC/TANF Benefit Levels: AFDC/TANF benefit levels vary greatly across states and over time (see Table 1.12 for more details about benefit levels). To capture the effect of the benefit levels on the family structure experiences of children, I use the average value of the maximum real (in 1983 dollars) benefit levels for families with three children specific to the state a child grew up in and over the childhood period of the child.

Three of the four stated goals of PRWORA involved reducing nonmarital births and encouraging marriage. States that reduced out-of-wedlock childbearing without raising abortion rates qualified for special bonuses. Changes in public assistance should have reduced the incentives to become a single mother and should have increased the incentives to marry. Time limits, sanctions, diversion activities, and work incentives all make it harder to receive public assistance as a single mother without also engaging in work-related activities.

Moffitt (1998) reviewed the extensive literature on the effect of welfare benefits on family behavior and concludes that there is evidence of a small positive effect of welfare on female headship, though this effect is sensitive to estimation specifications. Based on fertility and marital history records up to age 23 of the eight birth cohorts of women in the NLSY, Rosenzweig (1999) found that higher AFDC benefit levels and lower marital prospects induce young women to choose to have a child outside of marriage. Hoffman and Foster (2000) also confirmed the positive association between welfare benefits and single-motherhood among disadvantaged young women. A more recent study by Light and Omori (2008) reveals that increased AFDC or TANF benefits are expected to decrease the likelihood of single-to-marriage transitions but will increase the likelihood of single-to-cohabiting transitions. The authors also found that welfare benefits are positively associated with divorce for black women, but not for other groups.

Recent findings on the effect of welfare reform are also mixed. Bitler et al. (2004) found that the transition from AFDC to TANF led to more marriages and less divorce, while Fitzgerald and Ribar (2004) found little effect of TANF on marriage rates. These conflicting results are consistent with the fact that TANF programs simultaneously encourage marriage by increasing eligibility for married women and discourage marriage by promoting female employment.

D. The EITC

The EITC is a refundable federal income tax credit that supplements wages for low-income families. Since its inception in 1975, the EITC has undergone major expansions in 1986, 1991, 1994, and 1996 and has grown into the largest federally funded means-tested cash assistance program in the United States. In addition, 15 states have enacted state EITC that supplements the federal credit by 2000. The EITC rate increases with the number of children in a family.[10]

Hotz and Scholz (2003) provided evidence for marriage penalty associated with EITC, and this penalty has increased during the major expansions, especially from 1994 to 1996. They argued that the effect of EITC on marital behavior mirrors that of AFDC/TANF benefits. Blau and Van der Klaauw (2011) found that the tax treatment of children affects family structure in a significant way. Several studies also found that the EITC expansions have different impacts on women's labor supply depending on their marital status. Essia and Hoynes (2004) reported that that the expansions reduced total family labor supply of married women by just over a full percentage point. On the other hand, Meyer and Rosenbaum (2001) showed that the unprecedented increase in the employment and hours of single mothers during 1984-1996 can be largely attributed to the expansion of the EITC. To account for these effects, I construct the EITC phase-in rate using federal- and state-level EITC rules together. I use as instrument the average value over the childhood period of a child of the EITC rates for families with one child.

5. PSID Data

[10]For examples, in 2000, the subsidy rates for families with one and two-plus children were 34% and 40%, respectively.

The sample used in this paper comes from the 1968–2007 waves of the PSID. The PSID began in 1968 with about 5,000 households consisting of 18,000 individuals, which is a national representative sample with an oversample of low-income families. Information about families, individuals within the families, and direct descendants of the original families are collected annually from 1968 to 1997 and biennially after 1997.

I confine my sample to children who were born after 1967 and who were present for all waves until they reach the age of 25. I eliminate the group of children whose mothers were not in the household when they were growing up (age 0–15). This strategy creates a sample of 2004 children born between 1968 and 1982 who were present in every wave from the birth to age 25 and who grew up with their mothers in the house in all their childhood years.

Information on their socioeconomic and demographic characteristics as well as family compositions was collected on each interview date. Family income includes both taxable income (from wages, asset, investment, etc.) and welfare transfers from all family members. The income/need ratio will be used in my analysis to adjust family income for the family size, where need is the Orshansky-type poverty threshold based on annual food need standards, family size, and diseconomies of small households. Children's educational outcomes are measured as the completed years of schooling by the age of 25.

The 1985–2007 Marriage History File of PSID provides history of marriage, divorce, cohabitation, and separation as well as retrospective marriage histories for years before 1985 for all PSID individuals. By linking mothers' marriage history information to the children sample, I was able to create the complete family structure experiences for each child from birth to age 15. In particular, I compute the number of years a child spent living in a single-parent family (a mother-only family, to be more precise, since all children in the sample grew up with their mother always present in the family) during their childhood period. I include both marriage and cohabitation in the definition of two-parent families, and I do not distinguish step-parent families from two-biological-parent families.

PSID provides the residence state at each survey dates. This enables me to merge the PSID sample by state and year with the contextual variables, including divorce laws, UFRs, welfare rules, EITC rates, and labor market conditions described in Section IV. Then, I construct the set of IVs by taking the average of these variables over the age span of 0–15 of each child as measures of the different legal, social, policy, and economic environments that may have important effects on the mother's family structure decisions over the child's childhood years. One advantage of using the PSID sample is that the children from the sample grew up in 1968–1997, a period that witnessed most of the major changes in the contextual variables mentioned above.

Figure 1.1 shows that the percent of children living in a single-parent family increases with child age. Only 15.37% of the sample were born to a single-parent family, while 24% of these children lived with a single mother by the age of 15.

Table 1.2 presents the main characteristics of the children and their mothers in the final sample. Of the 2004 children, 65% are white and 52% are female. The average completed years of schooling for children is 13.64, with the white children achieving higher educational attainment than the nonwhite (13.95 vs 13.06). The gap between white children and nonwhite children are even more substantial in their family structure experiences and in the access to family income. Over the entire childhood period (age 0–15), a typical white child spent about 3 years in a single-parent family and enjoyed an average of 0.91 for the log value of income/need ratio compared to 6.06 and 0.19 for her nonwhite counterpart.

6. Results

A. Reduced Form Regressions for the Endogenous Variables

Table 1.3 presents the results of the first stage of 2SLS or the reduced form regressions in LIML, which uses the instruments listed in Table 1.1, together with the exogenous variables in Equation (1) to predict the two endogenous variables: the number of years living in a single-parent family and the average income measures, both from birth to age 15. I suppress the exogenous variables listed in Table 1.2 to conserve on space.

The upper panel of Table 1.3 shows reasonable coefficients on the instrumental variables in general. For cumulative childhood family structure experiences (column 1), the following four instruments prove to be the most important predictors: the unilateral divorce laws, which significantly increase the number of years a child spends in a single-parent family by making divorce easier; the UFRs, higher values of which imply lower level of social stigma against unwed mothers, thus leading to a positive effect; the welfare benefit levels, which turn out to have a negative effect; and the average state wage rate, which has a positive effect. The welfare reform rules (e.g., time limit and work requirement) have no influence on mothers' decision making except for a marginally significant negative effect from the years elapsed since the implementation of work requirements. This result is not surprising given the mixed findings from prior empirical work (see Section IV, Part C) and considering the fact that only a small fraction of mothers in the sample were affected by the welfare reform.[11]

The bottom panel of Table 1.3 provides a summary table of some diagnostic statistics that are useful in identifying weak instruments. The partial R^2 is the correlation between an endogenous variable and the excluded instruments after controlling for the exogenous variables, and Shea's partial R^2 further partials out the correlation of the endogenous variable with the fitted values of other endogenous variables. For the cumulative family structure experiences, these are 0.0243 and 0.0247, respectively. The F-test is for the joint significance of the excluded instruments. This is 3.63 for family structure with a P-value of 0.0000. These statistics suggest that the instruments for family structure are reasonably powerful. See further evidence for this claim below.

Now, I turn to the average income measure in column 2. The partial R^2 and Shea's partial R^2 are 0.0395 and 0.0401, respectively, and the P-value for the F-test is 0.0000. Thus, the instruments also have reasonable influence on the average income. The regression results show that the most important IVs for income are UFRs, welfare benefit levels, time limit rules, and unemployment rates.

B. Main Results

[11]Only mothers of the 713 children born after 1977 were affected by the welfare reform toward the end of their children's childhood years.

Table 1.4 reports the estimation results of Equation (1) from several methods. The OLS estimation indicates a small but statistically significant negative effect of single-parent family arrangements on children's educational attainments. Spending one more year in a single-parent family instead of a two-parent family is associated with a loss of 0.026 years of schooling. In contrast, the 2SLS estimate using the 12 instruments listed in Table 1.1 implies a much larger loss of 0.1284 years of schooling. However, as discussed in Section I, 2SLS can be severely biased toward the probability limit of the corresponding OLS with such a large set of instruments. I overcome this issue by using LIML, which produces an estimate of –0.1450 schooling years for each additional year spent with a single mother. I regard this as the preferred estimate since it is statistically significant at 5% and, moreover, almost unbiased, as I will discuss in the next part.

The LIML estimation implies that spending one more year in a single-parent family can cause the child to lose 0.1450 years in completed education. This is a substantial effect. To view this more clearly, now I use the LIML estimate to examine how much the racial difference in family structures can explain the white-nonwhite gap in educational outcomes. An average white child in the sample spent 1.4 years in a mother-only family, while an average nonwhite child spent 6.06 years. Assuming the detrimental effect of nonintact family experiences to be constant across racial groups and holding everything else equal, this difference implies that the white child will obtain 0.68[12] more years of schooling than her nonwhite counterpart, which can explain roughly

[12]This is computed as 0.1450*(6.06-1.4).

76% of the actual gap in completed schooling (0.89)[13] in my sample. In comparison, based on the OLS estimate (–0.026), an average white child would only obtain 0.12 more years of education than an average nonwhite, which can only account for 13% of the actual racial educational gap in the sample.

Consistent with the literature, the OLS estimate of the correlation between family structure and education is quantitatively small when controlling for family income and background information. Once the instruments are used, the estimated negative effect of living in a single-parent family rather than a two-parent family on child education becomes about 4.5 times larger (–0.1450 vs –0.026), which implies a substantial upward bias for OLS. There are two possible explanations for this. As discussed in the Introduction, the endogeneity problem associated with mothers' family structure choices can cause an upward bias when mothers who are more child-oriented or who have higher-ability children are more likely to stay single. In addition, the increasing family structure measurement errors due to the rise of more complex family arrangements, such as cohabitation and stepfamilies, further bias the effect of family structure to 0.

Table 1.4 also shows that the estimated effect of the average income falls by 26% (from 0.8179 to 0.6058) and becomes statistically insignificant when one uses the instruments. The LIML estimate suggests that the doubling average income would increase the finished schooling by (0.6058)*(ln2) = 0.41 years. In contrast, mothers' education stays highly significant and quantitatively sizable. One more year of schooling for mothers can be translated to about 0.24 (LIML) more years of schooling for their

[13]As shown in Table 1.2, on average, a typical white child will obtain 13.95 years of schooling by the age of 25, as compared to 13.06 for a nonwhite child.

children. In addition, column 4 shows that conditional on family background information (e.g., family structure, income, and mothers' education), female children tend to achieve half-year more schooling than male children and that white children also tend to obtain about half-year less schooling.

C. Comparison of Alternative Estimation Methods

As discussed above, the bias in 2SLS toward the corresponding OLS increases with the number of overidenifying instruments. In comparison, the LIML estimator is approximately median-unbiased for overidentified models and provides a finite-sample bias reduction. Columns (1), (3), and (4) in Table 1.4 confirm this as the 2SLS estimate falls between OLS and LIML and is shifted about 16.4% of the way toward OLS.

Stock and Yogo (2004) proposed a test for whether such many-instrument (or weak-instrument) biases are tolerable compared to the OLS bias. The weak-instrument test statistic was originally proposed by Cragg and Donald (1993), which is reported in the next to the last row in Table 1.4. Note that the test statistic is 3.998 for all IV estimations using the same 12 instruments (OLS, GMM, and LIML). Stock and Yogo developed the critical values for this test statistic for testing the null that instruments are weak, where weak instruments are those that can lead to an asymptotic relative bias greater than a certain percentage of OLS bias. The critical values for the null that the 2SLS bias may exceed 20% and 10% of the OLS bias are 19.40 and 10.78, respectively. Thus, we cannot even reject the null that the 2SLS bias may exceed 20%. In comparison, the Stock and Yogo critical value for the null that the LIML bias may exceed 10% of the

OLS bias is 3.58, which is smaller than the Cragg and Donald test statistic of 3.998. Thus, there is no evidence of serious bias associated with LIML estimators.

An alternative way to solve the many-instrument bias problem is to reduce the number of instruments by using factor analysis. Four factors were obtained to summarize the information contained in the original 12 instruments using the principal factor method with varimax rotation. Column 5 in Table 1.4 presents the LIML results based on the four rotated factors. Using four instruments instead of the 12 instruments slightly increases the effect of family structure to –0.1559. The Cragg and Donald test statistic is 7.54, well above the 10% critical value of 4.72. However, the estimate with the reduced instrument set of four variables suffers from some efficiency loss and is only statistically significant at a 10% level. Thus, I do not adopt this as my preferred method.

D. Robustness of the Results with Respect to the Instrument List

Due to unobserved heterogeneity in treatment effects, IVs provide an estimate for a specific group—people whose behaviors can be manipulated by these IVs. Therefore, the IV estimates can be sensitive to the instruments used. Table 1.5 compares the LIML estimates using the original list of 12 instruments in column 1 and those using variants of the list in columns 2–6. The first stage results for the five variant instrument lists are not reported, but all five sets of instruments provide reasonable explanation of the independent variation of the two endogenous variables.[14]

[14]The only exception is the IV set that consists of only unilateral divorce laws and unmarried fertility ratios (column 5 of Table 1.5), which are not stronger predictors of family income.

In column 2 in Table 1.5, I remove all nine instruments related to welfare benefits and reforms (time limit and work requirement), and this decreases the estimated effect of spending one more year in a single-parent family to –0.1360 years of schooling. In column 3, I exclude labor market variables, which also reduces the estimate slightly to –0.1339. In column 4, I drop the two most powerful predictors of changes in family structure from the list: instruments specific to unilateral divorce laws and UFRs. The effect is very similar to column 1 but becomes marginally insignificant (P-value = 0.109). In comparison, column 5 uses only these two instruments, and the estimate on family structure is very similar to that in column 4. However, the estimate on family income becomes highly insignificant, which can be explained by the two instruments' weak explanation of family income (see footnote 14).

In column 6, I add the interactions between mothers' education and all welfare related instruments (i.e., welfare benefit levels, time limit, work requirement, and EITC) to the original instrument list. These new instruments allow the effects of welfare variables to vary with mothers' educational level. Most coefficients on these interaction instruments have negative signs in the reduced form regressions on family structure, consistent with the notion that more educated mothers are less likely to use welfare. Adding these new instruments lead to a substantially smaller effect of family structure (from –0.1450 to –0.1151).

To sum up, it is comforting to find that the effect of the number of years spent in a one-parent family instead of a two-parent family on children's completed schooling is robust to alternative sets of instruments, with the estimated effect ranging from –0.1360 to –0.1465 (with only one exception of –0.1151). Moreover, all instrument sets passed the Hansen's J/overidentification test reported in the next to the last row of Table 1.5.

E. Other Future Child Outcomes

Now we turn to other measures of children's future outcomes. Table 1.6 estimates Equation (1) but use as the dependent variable the dummy variable indicating whether the child graduate high school by age 18. The results are very consistent with those in Table 1.4. The OLS estimation shows a small and marginally statistically significant negative effect of single-parent family arrangements on children's probability of graduating high school. Spending one more year in a single-parent family instead of a two-parent family is associated with a loss of .36% probability of graduating from high school. In comparison, using the same 12 instruments listed in Table 1.1 LIML estimation implies a much larger loss of 4.55%. To put this number in perspective, a nonwhite child in my sample spent 4.66 more childhood years with a single parent than its white counterpart on average, which implies that an average nonwhite child would be about 21.2% less likely to graduate high school than an average white child due to family arrangement differences, ceteris paribus. The LIML estimation passed both week-instrument and overidentification tests as shown on the last two rows of Column (2) of Table 1.6.

Table 1.7 replicates all the estimations of Table 1.6 for the probability a child ever repeat a grade before age 18. While both OLS and LIML estimations imply no statistically significant association between family structure and probability of repeating grades, it is worth noticing in LIML that one additional year of single-parent family experience may make a child 1.75% less likely to repeat a grade before 18.

Table 1.8 focuses only on the female sample (1044 females with 665 white and 379 nonwhite) to study the probability of having early childbirth before 19 as those girls grow up. Again, OLS estimation implies a small but statistically significant effect of growing up in a single-parent family on the probability of early childbirth. Spending one additional year in a single-mother family instead of a two-parent family is associated with 0.89% increase in the probability of having childbirth before 19 in the future. The LIML implies a much stronger effect, 2.39% increase in the probability. However, it is statistically insignificant due to weak instruments (The LIML failed to pass the weak instrument tests)

7. Conclusion

This paper examines the effect of the number of years a child spends living with a single-parent family instead of a two-parent family on the child's potential to complete schooling. This study is based on a sample of children from PSID born between 1968 and 1982. To deal with the endogeneity of mothers' family structure decisions, I take advantage of the variation across states and over time in unilateral divorce laws, UFRs, welfare rules, EITC rates, and labor market conditions that generate plausibly exogenous variation in mothers' family structure decisions. I construct an extensive set of variables for these legal, social, policy, and economic environment measures and use them as instruments to estimate a child's human capital production function.

The main results indicate that living in a single-parent family has a significant and sizeable detrimental effect on children's educational outcomes. In particular, one additional year spent in a single-parent family during childhood (ages 0–6) can cause the child to lose 0.145 years in completed schooling. This result is robust to a wide range of alternative sets of instruments. This is quite comforting since IV estimates are known to only estimate a local average treatment effect and are very sensitive to the instruments used.

My findings also suggest a severe downward bias associated with OLS estimation. Based on the PSID sample, the LIML estimate implies that the differences in family structure experiences over the early life course between white and nonwhite children can explain roughly 76% of the gap in educational attainment between the two groups, holding everything else equal. In comparison, OLS suggests only 13%. There are two possible explanations for this. First, the endogeneity problem associated with mothers' family structure choices can cause an upward bias if mothers who are more child-oriented or who have higher-ability children are more likely to stay single. Second, the increasing family structure measurement errors due to the rise of more complex family arrangements (such as cohabitation and stepfamilies) further bias the effect of family structure to 0.

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Figure 1.1. Percent of Children Living in Mother-only Families from Birth to Age 15

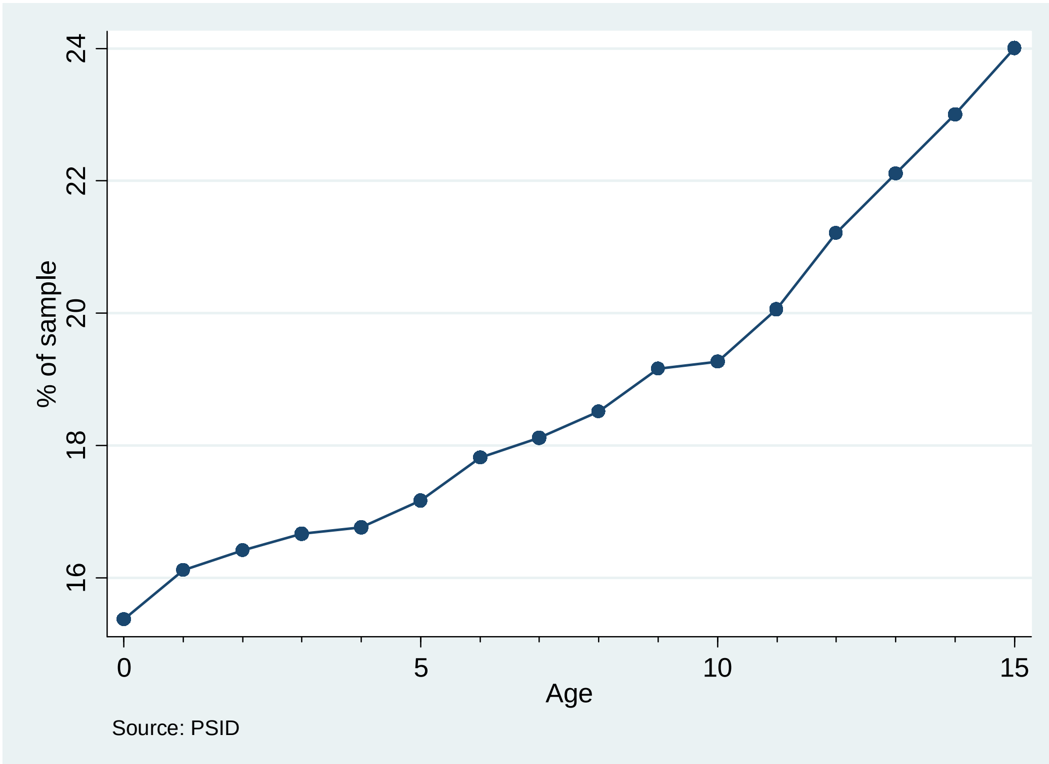

Table 1.1. Instrumental Variable List

Variables	Description	Mean	SD
	Unilateral Divorce Laws		
UNI_i	No. of years living in a state with Unilateral Divorce Laws over child age 0-15	7.137	(7.333)
	UFR		
UFR_i	Unmarried Fertility Ratio (the average value over child age 0-15)	260.569	(195.719)
	Welfare benefit & reform		
BEN_i	Real AFDC/TANF maximum benefits for a family of four (the average value over child age 0-15)	386.864	(156.679)
TL_i	No. of years living in a state with Time limit over child age 0-15	0.007	(0.024)
TL_LENGTH_i	Time limit length in months (the average value over child age 0-15)	214.876	(4.212)
$TL\_ELAPSED_i$	Years elapsed since the implementation of time limit (the average value over child age 0-15)	0.008	(0.035)
WR_i	No. of years living in a state with work requirement over child age 0-15	0.009	(0.031)
WR_LENGTH_i	Work requirement time limit length in months (the average value over child age 0-15)	214.174	(6.234)
$WR\_ELAPSED_i$	Years elapsed since the implementation of work requirement (the average value over child age 0-15)	0.013	(0.053)
	EITC		
$EITC_i$	The EITC phase in rate for a family with one child (the average value over child age 0-15)	0.108	(0.035)
	Local labor market		
$UNEMP_i$	Unemployment in the state (the average value over child age 0-15)	0.069	(0.013)
$WAGE_i$	Mean weekly earnings of production workers in manufacturing (the average value over child age 0-15)	323.849	(85.283)

Notes: No. of observations: 2004. Time limit length equals 216 months (18 years) if the state does not have time limit. This also applies to work requirement time limit.

Table 1.2. PSID Summary Statistics

Variable	All		White		Nonwhite	
	Mean	SD	Mean	SD	Mean	SD
Child completed Education	13.64	(1.99)	13.95	(1.97)	13.06	(1.89)
Child whether graduate HS	0.92	(0.26)	0.94	(0.23)	0.89	(0.31)
Child ever repeat grade before 18	0.12	(0.32)	0.09	(0.29)	0.17	(0.38)
Child having childbirth before 19	0.20	(0.40)	0.11	(0.31)	0.36	(0.48)
Years in a one-parent family						
Ages 0-15	3.02	(5.25)	1.40	(3.41)	6.06	(6.59)
Birth	0.15	(0.36)	0.04	(0.19)	0.37	(0.48)
Ages 1 to 5	0.83	(1.71)	0.32	(1.05)	1.79	(2.23)
Ages 6 to 10	0.93	(1.83)	0.44	(1.30)	1.84	(2.29)
Ages 11 to 15	1.10	(1.97)	0.60	(1.50)	2.06	(2.36)
Ln (avg income/need ratio)						
Ages 0-15	0.66	(0.65)	0.91	(0.49)	0.19	(0.67)
Birth	0.56	(0.72)	0.81	(0.57)	0.07	(0.70)
Ages 1 to 5	0.63	(0.64)	0.86	(0.49)	0.19	(0.68)
Ages 6 to 10	0.63	(0.71)	0.88	(0.55)	0.15	(0.75)
Ages 11 to 15	0.65	(0.78)	0.92	(0.62)	0.14	(0.80)
Mother's Education	12.93	(2.13)	13.31	(2.05)	12.20	(2.10)
Mother's # of Children	3.17	(1.66)	2.86	(1.22)	3.76	(2.16)
Mother's age at birth	24.55	(5.27)	25.16	(4.97)	23.40	(5.61)
Whether first child	0.40	(0.49)	0.42	(0.49)	0.36	(0.48)
Low birth weight	0.07	(0.25)	0.06	(0.23)	0.09	(0.29)
Birth year	1974.81	(4.29)	1974.97	(4.18)	1974.51	(4.46)
White	0.65	(0.48)				
Female	0.52	(0.50)	0.51	(0.50)	0.55	(0.50)
Urban	0.51	(0.50)	0.44	(0.50)	0.64	(0.48)
Religion	0.99	(0.12)	0.98	(0.13)	0.99	(0.08)
No. of observations	2004		1309		695	

Notes: Variable "Child having childbirth before 19" is for female sample only: 665 white female and 379 nonwhite female.

Table 1.3. Reduced Form Regressions for the Endogenous Variables

	Years in a one-parent family	Ln(avg income need ratio)
UNI_i	0.0356**	0.0012
	(0.0159)	(0.0016)
UFR_i	0.0079**	-0.0008***
	(0.0025)	(0.0002)
BEN_i	-0.0023*	0.0005***
	(0.0009)	(0.0001)
TL_i	-13.6599	-11.5891**
	(46.6665)	(5.0830)
TL_LENGTH_i	-0.0775	-0.0671**
	(0.2787)	(0.0311)
$TL\_ELAPSED_i$	-2.3871	-0.7661
	(11.5880)	(1.2610)
WR_i	7.3001	8.5183
	(74.7334)	(8.0166)
WR_LENGTH_i	-0.0960	0.0531
	(0.3748)	(0.0400)
$WR\_ELAPSED_i$	-10.4030	1.0805
	(6.6636)	(0.7182)
$EITC_i$	45.5581	-2.1633
	(42.0647)	(4.4562)
$UNEMP_i$	5.0923	-2.2450**
	(10.1813)	(0.9899)
$WAGE_i$	0.0106***	-0.0004
	(0.0032)	(0.0003)
N	2004	2004
R^2	0.2602	0.5494
Partial R^2	0.0243	0.0395
Shea's Partial R^2	0.0247	0.0401
F-statistics	3.6300	6.5000
P-value	0.0000	0.0000

Notes: Standard errors in parenthesis. * $p < 0.10$, ** $p < 0.50$, *** $p < 0.01$. All exogenous variables in equation (1) – see Table 2- are suppressed to conserve on space.

Table 1.4. Comparison of Results by Estimation Methods

	(1) OLS	(2) GMM	(3) 2SLS	(4) LIML	(5) LIML_Factor
One-parent family years for ages 0-15	-0.0249***	-0.1100*	-0.1253**	-0.1450**	-0.1559*
	(0.0092)	(0.0600)	(0.0607)	(0.0729)	(0.0808)
Ln(avg income/need ratio) for ages 0-15	0.8179***	0.6429	0.6414	0.6058	0.8469
	(0.0923)	(0.4806)	(0.4905)	(0.5496)	(0.5774)
Mother's Education	0.2262***	0.2424***	0.2397***	0.2424***	0.2234***
	(0.0226)	(0.0425)	(0.0431)	(0.0473)	(0.0494)
White	-0.1454	-0.3953	-0.4663	-0.5286	-0.6814
	(0.0967)	(0.2656)	(0.2695)	(0.3096)	(0.3562)
Female	0.4748***	0.4999***	0.4961***	0.5003***	0.5057***
	(0.0778)	(0.0797)	(0.0804)	(0.0818)	(0.0832)
N	2004	2004	2004	2004	2004
R^2	0.2598	0.2269	0.2134	0.1935	0.1674
Weak/many-instrument test		3.998	3.998	3.998	7.54
P-value, Hansen's J-statistics		0.6262	0.6262	0.6428	0.5662

Notes: The dependent variable is the completed schooling years by the age of 25. In columns 2-5, instrumental variables listed in Table 1.1 are used. Standard errors are in parenthesis. * $p < 0.10$, ** $p < 0.50$, *** $p < 0.01$.

Table 1.5. Robustness with Respect to the Instrument List

	(1) Original set	(2) Excludes Welfare Ben, TL & WR	(3) Excludes Labor Market	(4) Excludes Divorce Laws & UFR	(5) Only Divorce Laws & UFR	(6) Adding Interactions with Mother's Edu
One-parent family years for ages 0-15						
	-0.1450**	-0.1360**	-0.1339	-0.1444	-0.1465	-0.1151
	(0.0729)	(0.0683)	(0.0868)	(0.0900)	(0.0912)	(0.0736)
Ln(avg income/need ratio) for ages 0-15						
	0.6058	0.6659	0.4070	0.8296	-0.1319	0.7960
	(0.5496)	(0.7663)	(0.5495)	(0.5883)	(0.9563)	(0.5112)
Mother's education	0.2424***	0.2377***	0.2581***	0.2248***	0.3006***	0.2276***
	(0.0473)	(0.0631)	(0.0470)	(0.0506)	(0.0768)	(0.0451)
N	2004	2004	2004	2004	2004	2004
R^2	0.1935	0.2014	0.2097	0.1837	0.1906	0.2178
Weak/many-instrument test	3.998	6.177	3.065	3.229	9.20	2.85
P-value, Hansen's J-statistics	0.64	0.52	0.67	0.58	-	0.55
No. Instruments	12	5	10	10	2	19

Notes: The dependent variable is the completed schooling years by the age of 25. Standard errors are in parenthesis. * $p < 0.10$, ** $p < 0.50$, *** $p < 0.01$.

Table 1.6. The Probability of Graduating High School by Age 18

	(1) OLS	(2) LIML
One-parent family years for ages 0-15	-0.0036*	-0.0455**
	(0.0017)	(0.0166)
Ln(avg income/need ratio) for ages 0-15	0.0641***	0.0545
	(0.0158)	(0.1111)
Mother's Education	0.0119***	0.0125
	(0.0035)	(0.0093)
White	-0.0381*	-0.2007**
	(0.0148)	(0.0658)
Female	0.0189	0.0285
	(0.0115)	(0.0147)
N	2004	2004
r2	0.0842	0.4268
Weak/many-instrument test		3.688
P-value, Hansen's J-statistics		0.2385

Notes: Standard errors are in parenthesis.

Table 1.7. The Probability of Repeating Grades Before Age 18

	(1) OLS	(2) LIML
One-parent family years for ages 0-15	0.0008	-0.0175
	(0.0020)	(0.0125)
Ln(avg income/need ratio) for ages 0-15	-0.1018***	-0.2476**
	(0.0184)	(0.0919)
Mother's Education	-0.0111**	0.0003
	(0.0040)	(0.0079)
White	-0.0084	-0.0153
	(0.0190)	(0.0520)
Female	-0.0679***	-0.0654***
	(0.0141)	(0.0146)
N	2004	2004
r2	0.0828	0.1397
Weak/many-instrument test		3.688
P-value, Hansen's J-statistics		0.3877

Notes: Standard errors are in parenthesis.

Table 1.8. The Probability of Childbirth Before Age 19

	(1) OLS	(2) LIML
One-parent family years for ages 0-15	0.0089**	0.0239
	(0.0031)	(0.0353)
Ln(avg income/need ratio) for ages 0-15	-0.0453	0.2228
	(0.0301)	(0.2378)
Mother's Education	-0.0170**	-0.0388*
	(0.0062)	(0.0194)
White	-0.1117***	-0.1713
	(0.0320)	(0.0931)
N	1044	1044
r2	0.1983	0.1269
Weak/many-instrument test		2.180
P-value, Hansen’s J-statistics		13.579

Notes: Standard errors are in parenthesis.

CHAPTER TWO

UNMARRIED FERTILITY, EDUCATION AND SOCIAL STIGMA

1. Introduction

Nonmarital childbirth has increased substantially over the past half century: the percentage of births to unmarried women rose from 3.8% in 1940 to 33.2% in 2000 (Martin et al, 2002). Great concerns have been raised in social science about the adverse consequences of out-of-wedlock childbirth for children's development and well-being. In this paper, we focus on the long-run relationship of unmarried fertility and children's education. Using state-level data from the United States between 1940 and 2000, we find that a steady-state increase of 100 nonmarital births per 1,000 live births is associated with a decrease in high school graduation rates of 4.6 percentage points. This result is important since Heckman and Lafontain (2010) find that since the late 1960s the high school graduation rate has fallen by 4-5 percentage points, despite the growing wage differentials between high school graduates and dropouts. Our analysis shows that the rise in unmarried fertility predicts a ceteris paribus drop in high school graduation rate of about 7.1% between 1965 and 2000. In reality, the national high school graduation rate in our sample drops from 80.15% to 69.76%, a roughly 10% decrease. Therefore, our estimated effects of unmarried fertility can explain about 68% of decline in high school graduation in recent decades.

This paper is a natural extension of Kendall and Tamura (2010). Their paper used the same illegitimacy date starting in 1923,[15] and found that states with higher unmarried fertility rates have higher crime rates in the future. Instead of focusing on crime, we examine the other aspect of the unfavorable outcomes of unmarried fertility for children: the decline of their educational attainment. Based on state-level data on high school graduation rates, secondary and higher education enrollment rates, we find that children born out-of-wedlock may also receive less education. If there is causal link between education and crime, this paper presents an important channel through which unmarried fertility poses an impact on future crime. Lochner and Moretti (2004) provide evidence that schooling significantly reduces the probability of incarceration and arrest by using compulsory schooling laws as instruments for education.

The social stigma attached to nonmarital fertility wanes greatly during our sample period from 1940 to 2000. The long time series data allows us to examine the change of the relationship between out-of-wedlock childbirth and children's future education over time, and thus reveal whether the degree of social stigma would affect this relationship.[16] In theory, the change in social norms is associated with the variation in parental match quality of the marginal out-of-wedlock childbirth. In particular, when the social stigma is high, only the lowest quality matches fail to marry – ones in which children may not have been much better off, or possibly even worse off, had their parents married. When unwed

[15] Kendall and Tamura (2010) used a different lag structure to calculate the effective unmarried fertility ratios for crime.
[16] In general, stigma and similar social norms may serve as a substitute for legal restrictions on non-marital childbirth (see the discussion in Posner, 2000, chapter 5). Our findings illustrate how the level of social stigma in a society can change the outcomes of formal laws and policies regarding marriage and childbirth.

childbearing is more culturally acceptable, many high match quality marriages that would have benefited the children are foregone.

Kendall and Tamura (2010) provide some evidence for the theory in terms of child crime outcomes. They find that some marriages in the 1940s and 50s were of such low quality that the children involved would have been less likely to commit a crime in the future in single-parent households. This finding is reversed in the 1960s and thereafter. However, our results show that the correlation between unmarried fertility and children's educational outcomes remains negative for all these periods, implying that when it comes to education, even the worse matched marriage in the 1940s could benefit the children.

There exists an extensive literature showing that children born out-of-wedlock fare worse than children growing up in two-parent families on a range of outcome measures[17]. However, little attention was paid to the long-run effect on children's schooling achievement. While the individual-level data used in many of previous studies is highly important for some purposes, our state-level data allows us to analyze the general effect of unmarried fertility on the overall high school graduation rate[18], to examine this effect over a longer time period, and to control fully for the effects of abortion.[19] We are also able to examine why and how this effect has changed over time. Nevertheless, our results are generally consistent with this literature.

[17] Baldwin et al. 1980; Card 1981; Haveman et al. 1997; Kahn et al. 1992; Mclanahan et al. 1994; Moore et al. 1997. Also see Waldfogel, Craigie, and Brooks-Gumm 2010 for a recent review.
[18] As well as secondary and higher education enrollment rates, respectively.
[19] See part 2 in section II.

The rest of the paper is organized as follows. Section 2 discusses the theory and model. Section 3 described the data and empirical methods. Section 4 presents our main results, and Section 5 Concludes.

2. Theory

A. Unmarried Fertility

The unmarried fertility ratio[20] (*UFR*) for state *i* in year *t* is defined as:

$$UFR_{it} = 1{,}000\left[\frac{births\ to\ unmarried\ women_{it}}{total\ live\ births_{it}}\right]$$

As shown in Figure 2.1[21], the national *UFR* rose slowly until the early 1960s, then increased rapidly after. The cause-in-fact of any change in the UFR must be either: (a) a change in the population share of unmarried women; or (b) a change in the fertility of unmarried women relative to married women. There seems to be some evidence for both, as may be expected given the large increases visible in Figure 2.1.[22] Proximate causes for increases in the unmarried fertility ratio are many and controversial, including important government policies such as welfare payments for unmarried mothers and child support laws.[23] Our findings show that the social value of policies that affect marriage and childbirth incentives may depend on the level of social stigma prevalent in society.

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[20] An alternative measure of unmarried fertility is the birth rate for unmarried women, ages 15-44. However, this latter measure is not generally available for subnational regions over long time series, and, moreover, UFR is thought to be more relevant for the social consequences of unmarried fertility (Cutright and Smith, 1988).

[21] National UFR are weighted average based on 32 states for which data on mother's marital status are available with relatively little interpolation. See Appendix for details.

[22] Gray, et al, (2006) show that the population share of unmarried women has increased significantly, while Smith, et al. (1996) find an increase in unmarried fertility, and Ventura and Bachrach (2000) cite declines in birth rates for marrieds and increases in intercourse frequency among unmarrieds.

[23] There is a substantial literature. Some proximate causes that have been suggested include: lower returns from specialization in marriage (Becker, 1981); the legalization of abortion (as discussed in the following subsection)

Children born to unmarried parents may receive lower human capital investments, because the partnership between the parents is less stable, and sometimes nonexistent (Becker, 1981). Rather than spend most or all of their childhood living with two biological parents, children born to an unmarried couple may live in a variety of nontraditional circumstances. Among other possibilities, she may be raised by: a single parent alone; both parents, who married shortly after the birth; both parents, who cohabitate without marrying; one parent, married or cohabitating with a step-parent; one parent and a grandparent, or any combination of these at different times during youth.

The literature on the relationship between family structure and child outcome is astonishingly vast. Most research suggest that children growing up in nontraditional family arrangement tend to fare worse in a large number of dimensions both during their childhood ai-nd when they reach adulthood, relative to children raised up in a two-parent family.[24] This paper mainly focuses on the educational outcome for children born to unmarried parents.

Some research has noted that outcomes attributable to incomplete family structure may also be caused by other economic and social factors, which themselves may affect family structure. The factor that attracts most attention is poverty.[25] McLanahan and Sandefur (1994) found that about half the disadvantage associated with growing up in a

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(Akerlof, et al, 1996); changes in racial composition (Korenman et al, 2006); changes in social norms regarding premarital sex (Nechyba, 2001); generosity of welfare programs towards unmarried women (Ellwood and Bane, 1994); loosening of child support rules (Aizer and McLanahan, 2006); increases in male unemployment and imprisonment rates (Wilson, 1987); and declines in religiosity (Berggren, 1997); economic "despair" caused by income inequality for low socioeconomic status women (Keaeney et al, 2011).

[24] Ginther and Pollak 2003; McLanahan and Sandefur 1994; Astone and McLanahan 1994; Fronstin, Greenberg and Robins 2001; Cherlin, Kiernan, and Chase-Lansdale 1995.

[25] Maternal education is another important factor. Ellwood and Jencks (2004) shows that since 1965 nonmarital childbearing rose far more rapidly among the less educated, leads to a rising correlation between a mother's marital status and her education.

single-parent family was explained by the income difference. Ellwood and Jencks (2004) show that unmarried childbirth is three times more common among poor women as among affluent women. Given the limitation inherent in our data, we cannot fully rule out the possibility that poverty, among other possible factors driving the unmarried fertility, may be the root cause of educational failure.[26] However, the appropriate policy recommendation that can be derived from our findings – whether to target unmarried fertility directly or some more primitive factors that cause it – is beyond the scope of this paper.

This paper also examines whether the social stigma associated with unmarried motherhood can change the effects of unmarried childbirth on children's future educational achievement. The theory is described below.

Our model is a variant of Kendall and Tamura (2010). Assume individuals live two periods. In the first period, pregnancy happens to a unit measure of parents, and these parents then decide whether to marry. In the second period, children born in the first period are adults. Since we are interested in the relationship between out-of-wedlock childbirth and future human capital investments, we ignore parents who are married before pregnancy. For simplicity, we also assume that each parental match has only one child. We can show that relaxing this restriction would not change the results.

Parents care about their marriage quality and their children's future human capital. We define a parent's utility function as:

$$U = U(Z, H)$$

[26] Moreover the state-level correlation between the poverty rate and UFR is only 0.20. In a regression with fixed state and year effects, the estimated effect of poverty on UFR is not statistically significant, even at the 10% level.

where Z is household output and H represents children's human capital. For the purposes of our analysis, we define H as the possibility that a child succeeds in graduating from high school[27]; thus, U is strictly increasing in both arguments.

Let a parent's household output be increasing in their marriage quality, if they are married. Specifically, denote output Z as:

$$Z = \begin{cases} g(m) & \text{if married} \\ q - s & \text{otherwise} \end{cases}$$

where $m \in [0,1]$ denotes parental match quality, g is an increasing function in m, q denotes utility from being single, and s denotes utility loss from social stigma associated with single parenthood.

Let the probability H that a child succeeds to graduate high school be given by

$$H = \begin{cases} f(m) & \text{if parents married} \\ h & \text{otherwise} \end{cases}$$

with f as an increasing function, implying that higher match quality allows for greater human capital investments in children. Given $h > 0$, having married parents increases the probability of graduating high school for all children born to parents of match quality $m > m^* = f^{-1}(h)$. Thus, children born to parents of very low match quality ($m < m^* = f^{-1}(h)$) may be worse off if their parents marry, since low match quality

[27] For the estimation of effects on college enrollment rate, we can denote H as the possibility of a child attending college.

parents may fight often, be substance-abusers, or otherwise be unreliable in rearing children.[28] Therefore, parents of match quality *m* will marry if and only if

$$U(g(m), f(m)) \geq U(q-s, h) \quad [1]$$

Note that *U* is a strictly increasing function in m. Denote the match quality *m* for which equation [1] binds as $\overline{m}$. Then all matches $m \geq \overline{m}$ will marry in the first period.

First consider the simplest case, in which match quality is uniformly distributed among parents. In this case, the number of non-marital births is simply $\overline{m}$.[29] Then given the definition of the probability of graduating high school, *H*, high school graduation rate in period 2 can be written as:[30]

$$H = h\overline{m} + \int_{\overline{m}}^{1} f(m)dm . \qquad [2]$$

Then the change in period 2 high school graduation rate associated with an incremental change in period 1 unmarried fertility is $\frac{\partial H}{\partial \overline{m}} = h - f(\overline{m})$. This expression is positive if $\overline{m}$ < *m**, and negative if $\overline{m}$ > *m** (recall that $m^* = f^{-1}(h)$). Thus, for low levels of unmarried fertility ($\overline{m}$ < *m**), high school graduation rate is increasing in unmarried births. This is so because these children's parents are of such low match

[28] It is possible that $m^* \leq 0$, in which case no matter how poor the match quality is, children are better off with married parents.

[29] Note that $\overline{m}$ is not the *UFR*; all of the parents in our model conceive before deciding whether to marry, but obviously many other parents are already married at the time of conception. Nevertheless, under weak assumptions, *UFR* would be monotonic in $\overline{m}$.

[30] Like $\overline{m}$, the high school graduation rate here is only the rate among children who are born to unmarried parents. Nevertheless, under weak assumptions, the real high school graduation rate we intend to estimate would be monotonic in the rate here.

quality that they are actually better off if their parents do not marry. In general, the effect of unmarried fertility on high school graduation rate is concave, see Figure 2.3. [31]

Now suppose that the social stigma associated with unmarried parenthood, s, varies between locations over some reasonably small interval, $[\underline{s}, \bar{s}]$. From equation [1], it can be seen that $\bar{m}$ will differ across locations, depending on the value of s. Therefore, denote the number of out-of-wedlock childbirths in a location with social stigma s as $\bar{m}(s)$. Comparative statics then implies that locations with greater social stigma will have fewer out-of-wedlock children, since

$$\frac{\partial \bar{m}}{\partial s} = \left[\frac{-\frac{\partial U}{\partial Z}}{\frac{\partial U}{\partial Z}\frac{\partial g}{\partial m} + \frac{\partial U}{\partial H}\frac{\partial f}{\partial m}} \right] < 0 .$$

Then if social stigma in all locations is high, such that $\bar{m}(\underline{s}) < m^*$, cross-sectional comparisons will show that locations with more unmarried fertility in period 1 have *higher* graduation rates in period 2. On the other hand, if social stigma is quite low across most locations – say if $\bar{m}(\bar{s}) > m^*$ – then cross-sectional comparisons will show that locations with more unmarried fertility in period 1 have *lower* graduation rates in period 2. In general, as social stigma falls globally over time, the cross-sectional relationship between unmarried fertility and education will grow more negative. It can also be shown that this pattern is evident even if the assumptions of one child per couple and uniformly distributed match quality are relaxed.

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[31] $\frac{\partial^2 H}{\partial \bar{m}^2} = -f'(\bar{m}) < 0$

The theory implies that, if the social stigma declines over time, the relationship between unmarried fertility and education should be more negative in more recent years than in earlier years. If the social stigma is weaker among nonwhites than among whites, we should observe a stronger negative effect of unwed childbirth on children's educational achievements among nonwhites.

B. Abortion

Legalized abortion provides a powerful birth control tool which enables women to eliminate unwanted pregnancies and to optimize the timing of childbearing, thus may create a more favorable environment for children's development. Levine et al. (1999) finds that teenagers, unmarried women and economically disadvantaged are all more likely to seek abortions. Gruber et al. (1999) document that the children on the margin of abortion suffer difficulties in many dimensions in early childhood: infant mortality, poverty, and single-parenthood. The children from unwanted pregnancy, had they been born, would have likely grown up in an environment unlikely to foster robust human capital investment, thus having low schooling achievement.

However, there are two important biases in identifying such an effect empirically, one measurement-based and one structural. The measurement issue relates to the fact that it is actually "wantedness", not abortion, that influences children's well-being. If parents use abortion as a way of reducing unwantedness, then abortion will increase children's educational attainment; however, abortion is also a substitute for other forms of birth control, such as prophylactics and abstention. Therefore, some variation in the number of abortions may simply signify variation in the use of substitute birth control methods, in which case lower levels of abortion will actually correlate with less unwantedness, and therefore, higher educational attainment.[32] Therefore, to find the true effect of a policy change that liberalizes abortion, looking directly at a measure of unwantedness, such as the unmarried fertility ratio, may be more appropriate.[33]

Analyzing abortion in concert with unmarried fertility also addresses an important structural issue, first raised by Akerlof, et al. (1996). They proposed that the technological shock of abortion and female contraception may have played a major role in the rise of unmarried fertility in the later 20th century. They argue that legalizing abortion made unmarried women more willing to participate in uncommitted, premarital sex by reducing the cost of sexual activity. Even women who were opposed to abortions engage in more premarital sex to compete for boyfriends. When such women became pregnant, however, they could no longer rely on social pressure to ensure that their boyfriend married them. Nonmarital births therefore rose. This means that abortion legalization may increase the rate of unmarried fertility. As evidence for this effect, Ventura and Bachrach (2000) report national survey evidence that about 78% of out-of-wedlock pregnancies were unwanted in 1994, long after the legalization of abortion. If unwantedness is negatively associated with children's educational outcomes, then abortion may lead to lower levels of educational achievement.

[32] In other words, it is difficult to distinguish shifts in the demand curve for abortions from movements along the demand curve.

[33] On the other hand, as discussed above, UFR may be a function of stigma, government policies, and other incentives facing parents, so it is not a perfect measure of unwantedness either. At the least, however, our analysis complements abortion-based analyses.

We expect to find empirically that, after controlling for unmarried fertility, the effect of abortion to benefit children's educational attainment should be stronger. This is because the effect of abortion on education, conditional on contemporaneous unmarried fertility, is estimated primarily through variation in unwanted or mistimed pregnancies in married households, untempered by abortion's potential for causing increase in unwantedness.

3. **Data and Empirical Methods**

We use three different measures for children's educational achievements: high school graduation rate, secondary school enrollment rate and higher education enrollment rate. The high school graduation rate for state *i* in year *t* is defined as

$$\text{High School Graduation Rate}_{it} = \left[\frac{\text{number of high school graduates}_{it}}{\text{population aged } 17_{it}} \right]$$

This measure is same to the national 17-year old graduation ratio published by the National Center of Educational Statistics (NCES), [34] which is computed by dividing the sum of public and private high school graduate numbers by the size of 17-year-old population in each year. Following NCES and also previous literature,[35] we exclude GED recipients and only count those who receive high school graduate diploma as high school graduates. One problem arises in that state-level data on private high school graduate numbers are only available for a limited number of years (see Appendix for more details).

[34] The only difference is that we construct this ratio for each of the state rather than for the nation as a whole.

[35] Cameron and Heckman (1993) and Heckman and LaFontaine (2006, 2008) find that GED recipients performs significantly worse than high school graduates in almost all dimensions, and are more equivalent to high school dropouts in terms of economic and social outcomes.

To solve this problem, we use available data on private high school enrollment, public high school enrollment and public high school graduate numbers to generate estimated values for private high school graduate numbers for the missing years. Removing those missing years, however, does not change our main results. Another problem is that population were reported in 5-year categories for states, thus we use the 15- to 19-year-old population to construct 17-year-old population. Nonetheless, the national ratios calculated as the weighted average of all states from our data shows a generally similar trend as reported by NCES (see Figure 2.4).

Another popular measure, often referred to as high school completion rate, quantifies the proportion of freshmen high school students who receive a high school diploma four years later. We did not adopt this method due to the scarcity of grade-level enrollment data for earlier years. Reassuringly, the estimation based on state-level high school completion data from Warren (2005) for the more recent period 1975-2000 generates similar results to those from our data for the same time period. Moreover, the national trend from Warren (2005) also matches ours most of the time (see Figure 2.4).

Figure 2.4 shows that in US, high school graduation rate increases from 1940, peaks in 1965 at about 80 %, and then starts to fall. In 2000, the national rate is roughly 70%, a nearly 10% drop from 1965, which is surprising given the growing wage differentials between high school graduates and high school dropouts during that time. We will show that a significant part of this high school dropout issue can be explained by the sharp increase in unmarried fertility.

Following the theory presented in Section 2, we estimate the effect of unmarried fertility and abortion on education. The abortion ratio for state *i* in year *t* is given by:

$$Abortion\ Ratio_{it} = 1{,}000\left[\frac{abortions_{it}}{total\ live\ births_{it}}\right]$$

We construct effective Abortion ratio as 17-year lagged values of abortions per 1,000 live births:[36]

$$Effective\ Abortion\ Ratio_{it} = Abortion\ Ratio_{it-17}$$

There are two sources of state-level data on abortion: surveys of abortion providers from the Alan Guttmacher Institute (AGI), and state health department reports collected by the Centers for Disease Control (CDC). We used the latter source, as Joyce (2003) argues that the CDC data is more reliable, since it includes estimates as early as 1970, while AGI data begin with national legalization in 1973. The drawback of CDC data is that some states do not mandate reporting of abortion data, so missing data is problematic.[37] However, due to limitations in the data on unmarried fertility, as described below, the missing CDC abortion data will not limit our analysis.[38]

Similarly, we calculate the effective UFR for high school graduation rate as:

$$Effective\ UFR_{it} = UFR_{it-17}$$

Unmarried fertility ratios are calculated based on data from birth certificates. A serious measurement problem is that many states historically have not required a statement of marital status on the birth certificate, and the number of states that do require

[36] Our estimation is robust to 18-year lags or the average of 17- and 18-year lags.
[37] Kendall and Tamura (2010) used the same abortion data (i.e. the CDC data).
[38] Another drawback of both data sources is that no reliable data on illegal abortions before legalization is available. Following previous literature, we assume a zero abortion ratio before legalization in all states, though see Joyce (2003) for a critique of this assumption.

such information varies slightly over time.[39] Since 1980, computer technology has allowed for inference of marital status based on paternity acknowledgements or by matching surnames of father and child; however, before 1980, data is simply missing for a significant number of states. With minimal interpolation between years in a small number of cases, reliable time series data is available for 32 states over most of the years between 1923 and 2002.[40] The set of reporting states is geographically diverse, although several well-populated states, including California, New York, and Massachusetts are unfortunately missing.[41] The set of states with available data in each year is described fully in the Data Appendix.

In addition to high school graduation rate, we examine two other important educational measures: secondary school enrollment rate and higher education enrollment rate. Secondary school enrollment rate quantifies the percentage of the population ages 14-17 enrolled in high school in state i and year t. Since high school students are roughly evenly distributed in the age range of 14 to 17, we define the effective UFR and effective abortion ratios for secondary school enrollment rates as:

$$Effective\,UFR_{it} = \left(\sum_{a=14}^{17} UFR_{it-a}\right)/\,4$$

[39] Another source of unreliability in the data is the systematic underreporting of out-of-wedlock childbirth in Texas and Michigan over the 1990-1993 period due to legislation passed in those states. We make no attempt to correct for this problem because children born during this period are, at most, 11 years old in 2000, the last year for which education data is used in estimation, and we focus on secondary and higher education measures.

[40] While the number of interpolated observations is small, the analysis of Murphy and Topel (1985) suggests interpolation may bias estimated standard errors. Our results are robust to exclusion of all interpolated data.

[41] Alternatively, states may be grouped into census regions and race-specific unmarried fertility ratios for other states within the same region may be applied to states with missing data. This procedure has been generally used to estimate national trends in unmarried fertility, though less so in recent years as the break in trend between 1979 data calculated in this way and 1980 data calculated using the computerized inferral methods described in the text seem to imply significant flaws in the grouping procedure.

$$Effective\ Abortion\ Ratio_{it} = (\sum_{a=14}^{17} Abortion\ Ratio_{it-a})/4$$

Finally, the higher education enrollment rates measures the percentage of 18-to 24-year olds who are enrolled in college in state i and year t. Similarly, we calculate effective UFR and abortion ratios for higher education enrollment rates as:

$$Effective\,UFR_{it} = (\sum_{a=18}^{24} UFR_{it-a})/7$$

$$Effective\ Abortion\ Ratio_{it} = (\sum_{a=18}^{24} Abortion\ Ratio_{it-a})/7$$

For each educational measure, panel data for between 20 and 32 states over years 1940-2000

(1947-2000 for higher education) are available to carry out the estimation of the equation below:[42]

$$\ln(Education)_{it} = \beta_1(Eff.UFR_{it}) + \beta_2(Eff.\ Abortion\ Ratio_{it}) + \Gamma X_{it} + \alpha_i + \delta_t + \varepsilon_{it} \qquad [3]$$

where α_i and δ_t represent state and year fixed effects, respectively, and X is a vector of state-level covariates including school measures, such as average class size and relative teacher salary; economic measures, such as output per worker, years of schooling in the labor force, female labor force participation rate, church membership rates, urbanization and gender ratios, a measure of racial heterogeneity[43]. The data appendix

[42] The differences in the number of states available in each year are due to the fact that not all states required registration of births as early as 1923. Effective UFRs may be calculated for 20 states in 1957, but for 29 states by 1961, and 32 states after 1966.

[43] Calculated as a "Herfindahl"-style index, racial heterogeneity = 1-[(%white)^2+(%black)^2+(%American Indian)^2+(%Asian)^2+(%other)^2]. Thus, larger values are associated with greater heterogeneity, with a maximum value of 0.6875 (perfect heterogeneity), and a minimum value of 0 (perfect homogeneity).

gives details on the collection of each of these variables, and Table 2.1 provides some summary statistics for reference.

4. Results

A. Unmarried Fertility and Educational Attainment, 1940-2000

In any year of the sample period, the effective UFR is generally strongly correlated with education. Figure 2.2 shows a scatter plot of effective UFRs and the high school graduation rate (in natural log) for the 21 states for which data is available in 1940 and the 32 states for which data is available in 2000. Since the average age of high school graduates is around 17, a state's effective UFR in a given year should be of a magnitude similar to its actual UFR about 17 years earlier. In 2000, the effective UFRs in most states are in the range of 75-312, representing 75-312 unmarried births for every 1,000 live births (the only exception is District of Columbia, with an effective UFR of 558). In comparison, the effective UFRs in 1940 are substantially lower, in the range of roughly 7-85.

Figure 2.2 illustrates a clear negative relationship between high school graduation rates and effective UFR across states in both years (similar relationships also hold for other education measures). Of course, this analysis is purely cross-sectional, nevertheless, we will show that formal empirical analysis generate similar results.

Table 2.2 presents regression results of equation [2] for high school graduation rate using panel date from 1940 to 2000. Year- and state-fixed effects are included in all regressions to control for national secular trend and invariant state characteristics, and observations are weighted by total state population. All the estimates we presented are adjusted for heteroskedasticity, temporal correlation across states, and an AR(1) process for within-state autocorrelation using a panel-data Prais-Winsten approach.

The first column presents an estimate of the effect of unmarried fertility on high school graduation rates without any control variables (other than year- and state-fixed effects). Though not statistically significant, the coefficient is negative, implying that higher UFRs are associated with lower high school graduation rates. The second column in Table 2.2 includes all covariates except for the effective abortion rate. Including these covariates helps to control for omitted variables, but to the extent that unmarried fertility has indirect effects on high school graduation rate by changing the levels of the covariates, their inclusion may be inappropriate. It can be seen that inclusion of these controls strengthens the measured relationship between UFR and high school graduation rates. The coefficient on UFR implies that a steady-state increase in the effective UFR of 100 per 1,000 live births is associated with about 4.6 percent decrease in high school graduation rates.

In column 3, following our discussion on abortion in section 2.2, we estimate the effect of abortion ratios on high school graduation rates while excluding unmarried fertility. The estimate of the coefficient shows that effective abortion ratios by itself have positive but weak and statistically insignificant impacts on high school graduation rates. An increase in the effective abortion ratio of 100 per 1,000 live births is associated with only 0.3% increase in high school graduation rates. It is somewhat surprising given the substantial impact of abortion ratios on crime found by Donohue and Levitt (2001) and

Kendall and Tamura (2010)[44]. One possible explanation is that an average aborted child, had him been born, was at higher risk of dropping out of high school only once, while he was more likely to commit crimes repeatedly.

In column 4, we estimate the effects of unmarried fertility and abortion jointly on murder, and column 5 extends this model to include all the other covariates.[45] As discussed earlier, abortion may affect education in two structural ways: it may reduce the number or timing of unwanted children (as in Donohue and Levitt, 2001), leading to a long run increase in education, and it may cause some women who do not wish to make use of abortion technology to have more unwanted children, leading to a long run decrease in education (as in Akerlof, et al, 1996). The inclusion of both effective UFR and the effective abortion ratio allows us to separate these effects in the data, thus estimating the effects of abortion on crime through changes in the fertility behavior of married parents only. Comparison between columns 3 and 5 reveals that the controlling for UFR does not significantly increase the estimated effect of abortion; while comparison between columns 2 and 5 shows that the estimated effect of UFR stays roughly unchanged by the inclusion of abortion control. Thus it is not evident that abortion legalization will decrease high school graduation rate by increasing unmarried fertility.

We now use results in column 5 to conduct counterfactual policy experiments. After the continuous growth in the first half of last century, US high school graduation

[44] Using same abortion data over period 1957-2000, Kendall and Tamura (2010) found that an increase of 100 abortions per 1,000 live births is associated with 10% less murder.
[45] A concern with the inclusion of both abortion and UFR is that both are proxies for unwantedness, potentially measured with error, and it is difficult to definitively sign any biases associated with measurement error when the error may be correlated across regressors.

rates started to decline since 1965[46]. This dropout problem is particularly surprising, given the rising wage differential between a high school graduate and a high school dropout since 1980. The declines in high school graduation since the mid-1960s (for cohorts born in 1950) coincide with the start of explosion in out-of-wedlock births in the 1950s. Between 1965 and 2000, the effective UFR for high school graduation rose from 39.43 to 193.12, a change of 153.69. Using estimates on the coefficient of effective UFR in column 5, our results imply a ceteris paribus decrease in high school graduation rate of 7.1%. In reality, the national high school graduation rate in our sample drops from 80.15% to 69.76%, a roughly 10% decrease. Therefore, our estimated effects of unmarried fertility can explain about 68% of decline in high school graduation in recent decades. For further evidence, Table 2.3 presents the predicted high school graduation rates in 2000 for each state if the effective UFR stayed at the lower level in 1965.

For policy purposes, a comparison in the relative sizes of the coefficients on unmarried fertility and abortion suggests that abortion is quite a blunt policy lever for enhancing education, relative to policies that promote effective family formation directly. Using the standard deviations listed in Table 2.1 for the 1971-2000 period, and the coefficients in the fifth column of Table 2.2, an increase of one standard deviation in the effective abortion ratio (184.40) is associated with a 0.7% increase in long-run high school graduation rates; while a one standard deviation increase in the effective UFR (108.57) is associated with a 5.0% decrease in long-run high school graduation rates.

[46] Heckman and Lafontain (2010) finds that since the late 1960s the high school graduation rate has fallen by 4-5 percentage points, despite the growing wage differentials between high school graduates and dropouts. Also see Warren (2005) for state-level trends. Definition of high school graduation rates in both works are slightly different from the one used in our paper. See section 3 for more discussion.

From these comparisons, policies that would incentivize more marriage seem to have higher productivity than those that would incentivize more abortion. However, this analysis is highly incomplete since it does not take into account the potentially different costs of such policies.

Finally, Column 6 includes a quadratic term in effective UFR in order to test in a simple way the implication of our model that the relationship between UFR and child educational development could be concave as shown in Figure 2.3. The estimates from column 6 do not seem to support the nonmonotonic relationship between effective UFR and high school graduation rates for our sample.[47] However, simply including a quadratic term may be inappropriate if the effects of the covariates on the crime rate differ over time, and later we will analyze the effects of UFR on crime across different periods to control for such problems.

Now we turn to the other two educational measures. Table 2.4 replicates all the estimations of Table 2.2 for the secondary school enrollment rate. Comparison between the two tables reveals a very similar effect of effective UFRs: an increase of 100 out-of-wedlock child births per 1,000 live births is associated with 4.2 percent decrease in secondary school enrollment rate, in comparison to 4.6 percent decrease in high school graduation rate. The estimated effects of abortion ratios on the two educational measures are also very comparable in magnitude: an increase of 100 abortions per 1,000 live births is associated with 0.6 percent increase in secondary school enrollment, compared to 0.4 percent increase in high school graduation rate. The similarity in findings across two

[47] The implied minimum graduation rate level from the quadratic is achieved when the effective UFR is 620, which is higher than all effective UFR values observed in our sample.

educational measures gives us stronger confidence in the negative implications of unmarried fertility on children's human capital development.

Column 6 of Table 2.4 weakly supports a concave relationship between unmarried fertility and secondary school enrollment rate, consistent with our theory discussed in section 2. The implied maximum point from the quadratic occurs when effective UFR is 71.4. This means only the first 7.14 percent of parents who choose not to marry seem to benefit their children. Any more unmarried fertility beyond this point is detrimental to children's educational development. Since the national average level of effective UFR for secondary school enrollment ranges from 27 to 207, therefore we mainly observe negative association between UFR and secondary school enrollment rate in our sample.

Table 2.5 presents the same analysis for higher education enrollment. Contrary to our theory, we find positive relationship between unmarried fertility and higher education enrollment rate. On possible explanation for this inconsistent result is the migration across state borders for college education. If the effective UFR is higher in the state that has a larger size of the net immigration of college students (the number of out-of-state students minus the number of resident students attending colleges in other states), the effect of UFR on college enrollment can be biased upward. A good example is District of Columbia[48]. In 1992, only 7 percent of freshmen students were DC residents, while its effective UFR is the highest among all states in our sample, 399.7 (the next highest UFR is about 199 in Mississippi).

[48] Excluding only DC does not change the results much.

For the remainder of the section, we perform robustness checks on our results. Table 2.6 presents several checks on the robustness of the measured effects in Tables 2.2 and 2.4. For readability, only the coefficient on effective UFR is presented, although the regression specification from which these coefficients are derived is the same as in column 5 of both Tables, where all covariates are included.

The "baseline" row presents results from regressions identical to those in column 5 of Tables 2.2 and 2.4. To the extent one is concerned that effective UFR is highly correlated with current UFR, and thus in some complicated way might simply be a proxy for current social conditions in a state, inclusion of the contemporaneous value for UFR would ameliorate these concerns. Row 2 shows that controlling for contemporaneous UFR does not have much effect on the relationship between effective UFR and education.[49]

Next, eliminating the population-based weighting scheme treats all states equally. As seen in the third row, this moderately lowers the estimated effect of unmarried fertility on high school graduation rates while increasing the effect on secondary school enrollment rate. Changing our assumptions regarding the structure of the regression errors to ignore autocorrelation and cluster only at the state level nearly double the measured effects on both educational measures. The inclusion of a state-specific time trend soaks up almost all of the variation in the regressions; as a result, the coefficients on unmarried fertility for both measures become small and statistically insignificant. Excluding Washington, D.C. increases the estimated effect on high school graduation,

[49] The coefficient on contemporaneous UFR is insignificant. Alternatively, a falsification test by which we simply replace effective UFR with the contemporaneous UFR reveals the same result: a small and insignificant coefficient on the contemporaneous UFR variable.

but decreases the effect on secondary school enrollment, yet the latter effect remains statistically significant at 5% level.

Next, one may be concerned about how moments of the income distribution other than the mean might affect crime. Controlling for the percentage of the state population in poverty, the effect of UFR on high school graduation disappears, whereas the effect on secondary school enrollment stay statistically significant, though decreased substantially[50]. Controlling for racial population shares slightly lowers the coefficients for both measures. The inclusion of region-year fixed effects controls for any unmeasured factors that vary over time within nine census regions. It appears that these effects reduced the effects somehow, but the results stay statistically significant.

One may be concerned that the long-time trend of high school graduation rate and secondary school enrollment rate can be driven by the rising wage premium associated with a high school diploma. We collected panel data on the average wage ratios between high school graduates and high school dropouts, between college dropouts and high school dropouts, and between college graduate and high school dropouts. Controlling for all these ratios does not change our main results, though they reduce the estimated effects slightly. Inclusion of dummies for Vietnam War period does not affect the results as well.

Finally, we include contemporaneous measure of marriage behaviors in an attempt to address the question of whether illegitimacy is the fundamental cause of low educational achievement as presented in our model or it is a proxy for the effect of two-parent household on children's educational development. . In the latter case, marriage

[50] Smaller sample can be a possible explanation, because poverty rate is not available at the state level before 1969, except for the (t-1) census year, 1959. Thus, this row in Table 2.6 uses fewer data points than those in Tables 2.2 and 2.4.

may have an ameliorative effect on children's future prospects even if the children were initially unwanted. To distinguish these effects in a simple way, we collected additional data on the share of adults 16 and older who were married in each state, from census records 1940-2000, inclusive. Inclusion of this variable reduced the estimated effects of unmarried fertility on high school graduation rate and secondary school enrollment rates by 17% and 5%, respectively. However, illegitimacy remains significantly negatively related to both educational measures.

B. "Social Stigma" Hypothesis Tests

The theory discussed in section II implies that as the social stigma associated with unmarried childbirth decreases, the relationship between unmarried fertility and children's educational attainment should become more negative, since the marginal child born to unmarried parents becomes more likely to have benefited from having married parents. In this subsection, we seek to find empirical evidence for the theory. First, we consider how the effect of unmarried fertility on education differs over time, since social opinions towards unwed parents have changed substantially in the U.S over our sample period 1940-2000. Second, we look at how the effect differs across racial groups, since stigma against unmarried fertility has generally been higher among whites than among nonwhites.

Table 2.7 estimates the effects of unmarried fertility on education by repeating the analysis in column 5 of Table 2.2. The only difference is that we now divide the sample into two time periods, 1940-1970, and 1971-2000.

The first two columns imply that unmarried fertility is negatively related with high school graduation rate in the 1940-1970 period, but this relationship seems to disappear in the 1971-2000 period. These results contradict the theory, which suggests that in later years unmarried childbirth should induce greater losses for these children, since the waning social stigma leads parents of higher match quality to forego their marriage. Results from regressions on secondary school enrollment rate are also inconsistent with the social stigma theory. The unmarried fertility is negatively associated with secondary school enrollment rate for both periods, but the relationship becomes weaker in the latter period.

A possible explanation for these results is a change in the composition of unmarried fertility towards parents in higher socio-demographic strata. Unmarried fertility has become less concentrated among teen mothers, less concentrated among nonwhites, and more children of unwed mothers are now living with both parents in a cohabitive home, as opposed to in single-parent homes (Ventura and Bachrach, 2000).

As a second test of the "social stigma" hypothesis, we decompose the effects of unmarried fertility on education by racial group. Unwed mothers have long faced lower levels of social stigma among nonwhite groups than among whites (Graefe and Lichter, 2002, Cutright and Smith, 1988, Hogan and Kitagawa, 1985). This may be part of the reason why unmarried fertility has generally been significantly higher among nonwhites: the averages over the 1923-1990 span are 89.29 per 1,000 live births for whites, but 264.99 per 1,000 live births for nonwhites.

Tables 2.8 and 2.9 separate white and nonwhite out-of-wedlock births in their effects on education. Problematically, some states, particularly in the 1920s and 1930s, did not report racially-disaggregated UFR data, leading to a reduction from the 1,856 (1,863) observations previously employed over the 1940-2000 period to only 1,553 (1,560) observations for high school graduation rate (secondary school enrollment rate). Many of the states that did report such data were concentrated in the South. Thus, composition bias may cause these results to differ from the earlier analysis. To check for this possibility, column 1 of Table 2.8 (Table 2.9) replicate the analysis from Table 2.2 (Table 2.4), using the total effective UFR for only the observations for which racially-disaggregated data is available. Comparisons between this column and those in Tables 2.2 (Table 2.4)) suggest a high degree of similarity.

The rest of the columns estimate the effects of illegitimacy rates on education by racial groups. Columns 2 and 3 suggest that white unmarried fertility is negatively correlated with high school graduation rate but uncorrelated with secondary school enrollment rates. On the other hand, the coefficients on estimates of nonwhite unmarried fertility are much smaller than its white counterpart, and also not statistically different from 0 for both educational measures. It seems that the decline in high school graduation rate is mainly driven by the sharp increase in white UFR. These results also fail to support the theory, which suggest that nonwhite out-of-wedlock children may suffer from higher loss since better match quality parents tend to choose not to marry due to lower social stigma levels among nonwhites.

5. Conclusion

The paper has analyzed the relationship between unmarried fertility and children's educational attainment over the period 1940-2000. Using state-level data from the United States between 1940 and 2000, we find that a steady-state increase in unmarried fertility ratio of 100 per 1,000 child births could lead to a 4.6 percent drop in high school graduation rate and a steady-state 4.2 percent decline in secondary school enrollment rate in the long-run.. This result is important since Heckman and Lafontain (2010) finds that since the late 1960s the high school graduation rate has fallen by 4-5 percentage points, despite the growing wage differentials between high school graduates and dropouts. Our analysis shows that the rise in unmarried fertility predicts a ceteris paribus drop in high school graduation rate of about 7.1% between 1965 and 2000. In reality, the national high school graduation rate in our sample drops from 80.15% to 69.76%, a roughly 10% decrease. Therefore, our estimated effects of unmarried fertility can explain about 68% of decline in high school graduation in recent decades.

Next, our results show that the correlation between unmarried fertility and children's educational outcomes remains negative for all periods. These effects have generally decreased over time and that white unmarried fertility tends to be more correlated with child educational outcomes than nonwhite unmarried fertility.

Moreover, we also find that after controls for unmarried fertility, a steady-state increase of 100 abortions per 1,000 live births is associated with 0.6 percent increase in secondary school enrollment and 0.4 percent increase in high school graduation rate. For policy purposes, a comparison in the relative sizes of the coefficients on unmarried fertility and abortion suggests that abortion is quite a blunt policy lever for enhancing education, relative to policies that promote effective family formation directly. In particular, an increase of one standard deviation in the effective abortion ratio (184.40) is associated with a 0.7% increase in long-run high school graduation rates; while a one standard deviation increase in the effective UFR (108.57) is associated with a 5.0% decrease in long-run high school graduation rates. From these comparisons, policies that would incentivize more marriage seem to have higher productivity than those that would incentivize more abortion. However, this analysis is highly incomplete since it does not take into account the potentially different costs of such policies.

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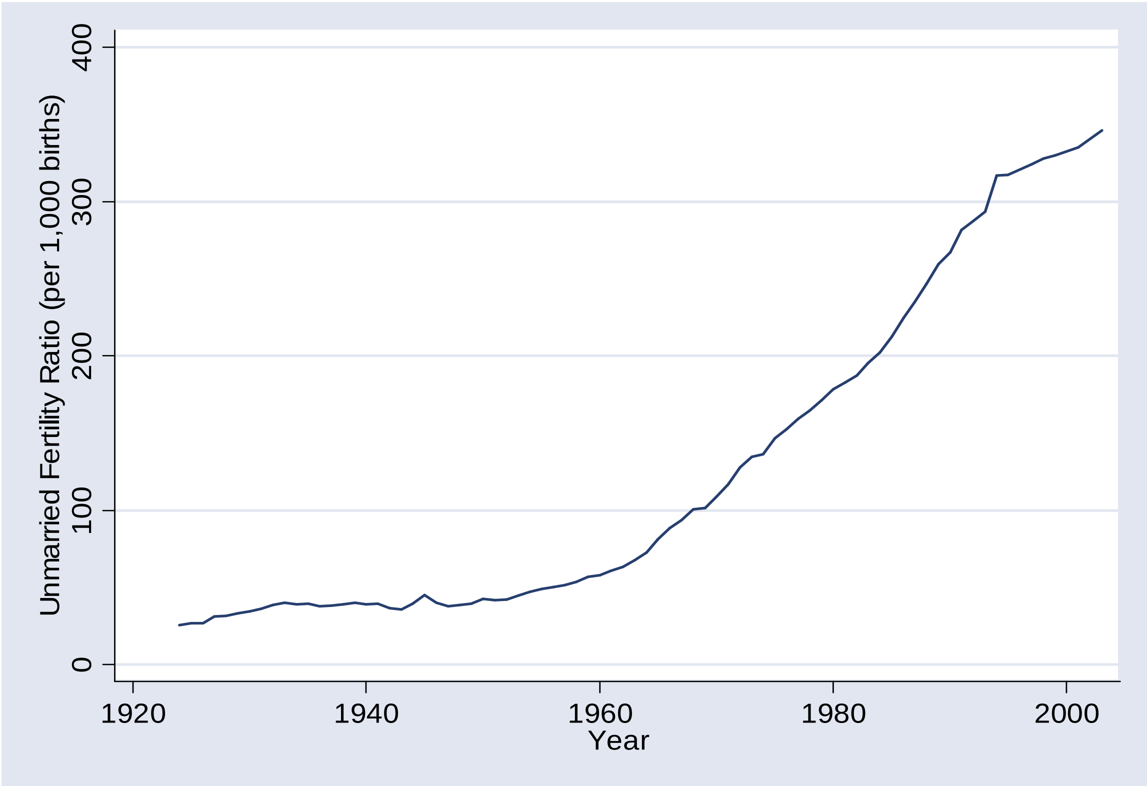
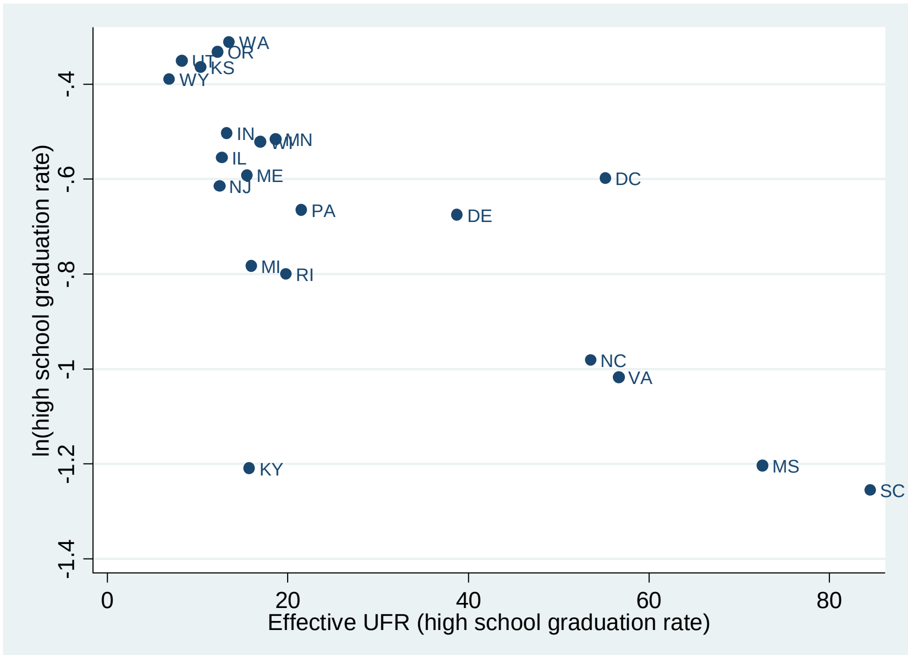
Figure 2.1: Unmarried Fertility Ratio: 1923-2002

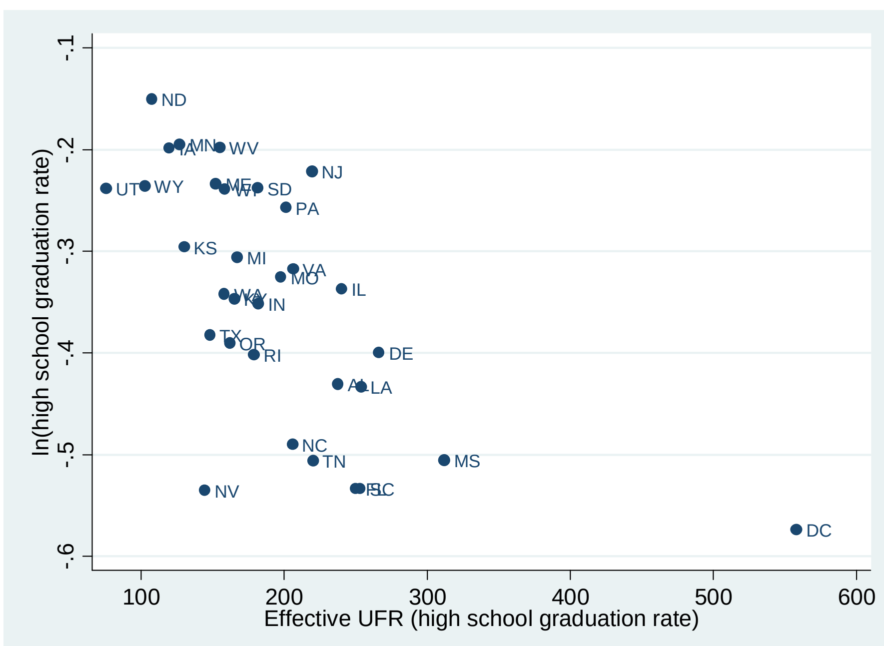


Notes**:** The unmarried fertility ratio (*UFR*), 1923-2002, calculated as births to unmarried women per 1,000 live births. Calculations are based on 32 states for which data on mother's marital status is available with relatively little interpolation (see data appendix for details).

Figure 2.2: Effective UFR vs. High School Graduation Rate, 1940 and 2000



1940



1960

Notes**:** Observations represent states with available time-series data on unmarried fertility ratios (UFR), calculated as childbirths to unmarried women per 1,000 live births.

Figure 2.3: Predicted Relationship between unmarried childbirth and the children's educational attainment

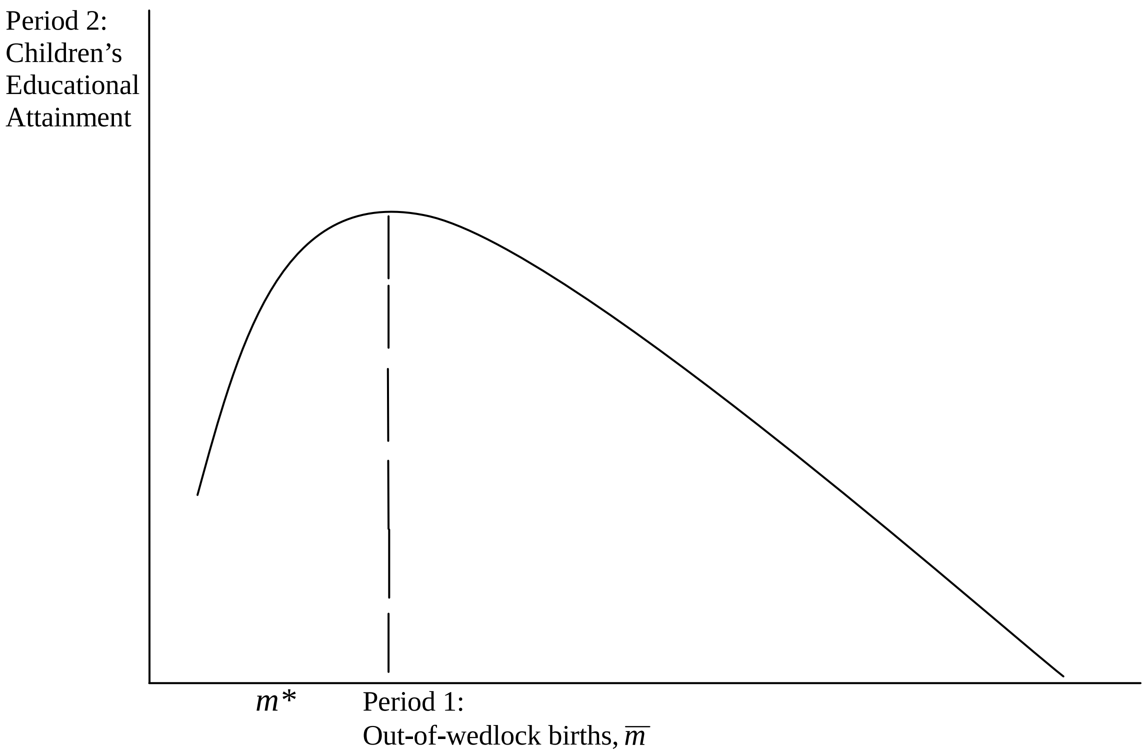
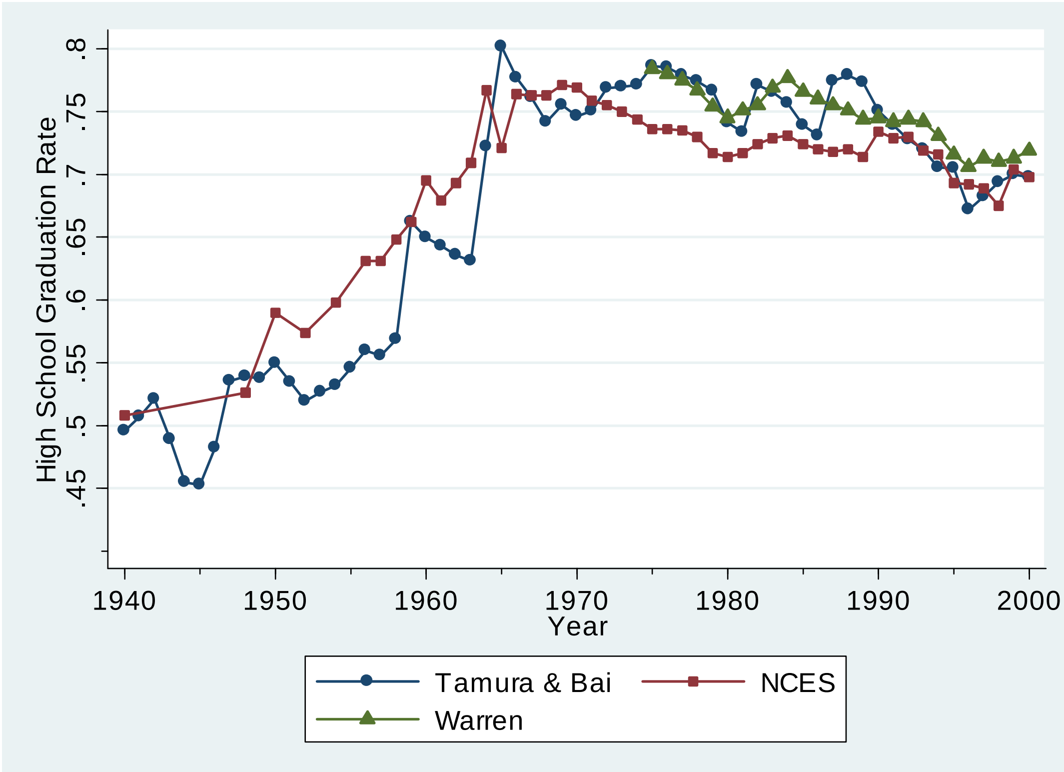


Figure 2.4: Three Measures of National High School Graduation Rate



Notes: High school graduation ratio is the percentage of 17-year olds who have a high school diploma. High school graduation ratio (Tamura and Bai) is the weighted average of state-level ratios constructed by authors, NCES high school graduation ratio is from Digest of Education Statistics. High School completion data is the percentage of entering high school freshmen who obtain a high school diploma a few years in the future. High school completion data is from Warren (2005).

Table 2.1 Summary Statistics

	1940-70 (N=903)		1971-2000 (N=960)	
	Mean	SD	Mean	SD
High school gradution rates	0.61	0.15	0.75	0.09
Secondary school enrollment rates	0.81	0.14	0.92	0.06
Higher education enrollment rates	0.18	0.10	0.46	0.11
UFR per 1,000 live births	58.81	45.37	228.33	108.78
White UFR per 1,000 live births	27.80	20.55	120.14	57.41
Nonwhite UFR per 1,000 live births	204.46	84.33	483.89	139.19
Effective UFR per 1,000 live births:				
High school gradution rates	38.02	27.37	108.57	77.87
Secondary school enrollment rates	39.03	28.09	116.48	80.92
Higher education enrollment rates	38.45	26.90	90.10	67.97
Effective abortions per 1,000 live births:				
High school gradution rates			221.29	184.40
Secondary school enrollment rates			230.38	186.72
Higher education enrollment rates			194.18	147.65
Female labor force participation rate	0.34	0.08	0.64	0.09
Pupil teacher ratio	24.48	3.67	19.60	3.10
Relative teacher salary	2.23	0.35	1.99	0.37
Average years of schooling	9.66	1.05	12.23	0.87
fraction population with < 8 years schooling	0.39	0.13	0.16	0.07
Real output per worker ($1997)	27566.67	6469.51	43827.28	7041.18
AFDC payments per family	3201.95	2772.64	6868.63	2623.78
% of population church members	37.12	11.72	24.49	11.49
% Urban	58.38	16.79	67.73	14.78
% Male	49.73	1.09	48.79	0.81
Racial Heterogeneity Index	0.18	0.15	0.24	0.13
% 15+ population married	65.55	3.29	58.34	5.41
New marriages per 100 marriage stock	3.35	6.95	2.98	4.59
Divorces per 100 marriage stock	0.68	1.12	1.10	0.48

Notes: Out of a theoretical maximum of 992 observations between 1940 and 1970, missing data limits the number of observations. Data on AFDC payments per family are lagged 15 years, and so correspond to 1925-1955 and 1956-1985.

Table 2.2. The Effects of Out-of-Wedlock Childbirth on High School Graduation Rate, 1940-2000

	(1)		(2)		(3)		(4)		(5)		(6)	
Effective UFR (x 100)	-0.006	(0.021)	-0.046	(0.019)			-0.006	(0.021)	-0.046	(0.019)	-0.060	(0.032)
Effective UFR^2 (x 10,000)											0.004	(0.007)
Effe Abortion Ratio (x100)					0.003	(0.004)	0.006	(0.005)	0.004	(0.004)	0.003	(0.005)
Mom's School Years			-0.015	(0.018)	-0.016	(0.018)			-0.016	(0.018)	-0.017	(0.018)
Female(<=44) Labor Force Participation Rate			-0.003	(0.198)	0.125	(0.196)			0.014	(0.198)	0.025	(0.199)
Pupil Teacher Ratio (L.1-12)			-0.011	(0.003)	-0.012	(0.003)			-0.011	(0.003)	-0.011	(0.003)
Relative Teacher Salary (L.1-12)			0.042	(0.031)	0.046	(0.031)			0.043	(0.031)	0.046	(0.030)
Average School Years			0.139	(0.021)	0.143	(0.022)			0.139	(0.021)	0.138	(0.022)
% with <8 yrs of schooling			-0.136	(0.155)	-0.024	(0.159)			-0.136	(0.154)	-0.152	(0.158)
ln (real output per worker) (1997$)			-0.119	(0.040)	-0.115	(0.040)			-0.117	(0.040)	-0.116	(0.040)
AFDC			-0.025	(0.016)	-0.025	(0.017)			-0.025	(0.016)	-0.026	(0.016)
% Church			0.298	(0.076)	0.276	(0.075)			0.296	(0.076)	0.297	(0.075)
% Urban			0.156	(0.138)	0.151	(0.139)			0.150	(0.139)	0.158	(0.138)
% Male			4.468	(1.639)	4.056	(1.633)			4.525	(1.637)	4.613	(1.646)
Racial heterogeneity index			-0.120	(0.122)	-0.131	(0.124)			-0.124	(0.122)	-0.110	(0.123)
State FE	Yes		Yes		Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes		Yes		Yes	
R-squared	0.87		0.92		0.92		0.87		0.92		0.92	
Obs.	1856		1856		1856		1856		1856		1856	

Notes: The dependent variable is the natural log of the high school graduation rate. Standard errors are in parentheses. Regressions based on 1,856 observations for 32 states over the 1940-2002 period. Year and state fixed effects are included in all regressions, and observations are weighted by total state population. Coefficients and standard errors are adjusted for heteroskedasticity, temporal correlation across states, and an AR(1) process for within-state autocorrelation using a panel-data Prais-Winsten approach.

Table 2.3. The Counterfactual Predictions for High School Graduation Rates for All States in 2000 (To Be Continued.)

state	Whether UFR data is missing	High School Graduation Rate				
		Actual Values		Predicted Values in 2000		
		1965	2000	If **Effecitve UFR** stayed at the 1965 level	If **Racial heterogeneity index** stayed at the 1965 level	If **both varibales** stayed at the 1965 level
New England						
Maine	0	0.87	0.79	0.84	0.80	0.84
Rhode Island	0	0.77	0.67	0.72	0.69	0.74
Connecticut	1	0.87	0.83	0.88	0.85	0.91
Massachusetts	1	0.89	0.75	0.80	0.77	0.82
New Hampshire	1	0.89	0.79	0.84	0.79	0.85
Vermont	1	0.77	0.87	0.93	0.88	0.94
Middle Atlantic						
New Jersey	0	0.88	0.80	0.88	0.83	0.90
Pennsylvania	0	0.90	0.77	0.83	0.78	0.85
New York	1	0.81	0.65	0.70	0.67	0.73
East North Central						
Delaware	0	0.82	0.67	0.73	0.68	0.75
District of Columbia	0	0.56	0.56	0.70	0.57	0.70
Florida	0	0.70	0.59	0.64	0.59	0.65
North Carolina	0	0.73	0.61	0.65	0.62	0.66
South Carolina	0	0.66	0.59	0.63	0.59	0.63
Virginia	0	0.68	0.73	0.78	0.74	0.79
West Virginia	0	0.83	0.82	0.86	0.82	0.86
Georgia	1	0.68	0.58	0.62	0.59	0.63
Maryland	1	0.79	0.76	0.82	0.78	0.85

Table 2.3. The Counterfactual Predictions for High School Graduation Rates for All States in 2000 (Continued.)

state	Whether UFR data is missing	High School Graduation Rate				
		Actual Values		Predicted Values in 2000		
		1965	2000	If **Effecitve UFR** stayed at the 1965 level	If **Racial heterogeneity index** stayed at the 1965 level	If **both varibales** stayed at the 1965 level
West North Central						
Alabama	0	0.73	0.65	0.70	0.65	0.70
Kentucky	0	0.68	0.71	0.76	0.71	0.76
Mississippi	0	0.62	0.60	0.67	0.60	0.67
Tennessee	0	0.71	0.60	0.65	0.61	0.66
South Atlantic						
Louisiana	0	0.69	0.65	0.70	0.65	0.71
Texas	0	0.68	0.68	0.72	0.70	0.74
Arkansas	1	0.76	0.72	0.77	0.73	0.77
Oklahoma	1	0.85	0.74	0.78	0.76	0.80
East South Central						
Nevada	0	0.85	0.59	0.62	0.60	0.64
Utah	0	0.88	0.79	0.81	0.80	0.83
Wyoming	0	0.89	0.79	0.82	0.80	0.83
Arizona	1	0.73	0.55	0.57	0.56	0.59
Colorado	1	0.82	0.67	0.70	0.69	0.72
Idaho	1	0.89	0.76	0.79	0.77	0.81
Montana	1	0.87	0.80	0.84	0.81	0.85
New Mexico	1	0.76	0.66	0.69	0.69	0.72

Table 2.3. The Counterfactual Predictions for High School Graduation Rates for All States in 2000 (Continued.)

state	Whether UFR data is missing	High School Graduation Rate: Actual Values 1965	2000	Predicted Values in 2000: If **Effecitve UFR** stayed at the 1965 level	If **Racial heterogeneity index** stayed at the 1965 level	If **both varibales** stayed at the 1965 level
West South Central						
Oregon	0	0.94	0.67	0.71	0.68	0.73
Washington	0	0.92	0.71	0.76	0.73	0.78
Alaska	1	0.52	0.67	0.71	0.68	0.72
California	1	0.84	0.68	0.73	0.72	0.77
Hawaii	1	0.81	0.82	0.88	0.84	0.89
Mountain						
Iowa	0	0.92	0.82	0.86	0.83	0.87
Kansas	0	0.80	0.74	0.78	0.76	0.80
Minnesota	0	0.94	0.82	0.86	0.84	0.88
Missouri	0	0.79	0.72	0.78	0.73	0.79
North Dakota	0	0.87	0.86	0.90	0.87	0.91
South Dakota	0	0.85	0.79	0.85	0.80	0.86
Nebraska	1	0.90	0.84	0.89	0.86	0.91
Pacific						
Illinois	0	0.80	0.71	0.79	0.73	0.81
Indiana	0	0.83	0.68	0.73	0.69	0.74
Michigan	0	0.80	0.74	0.79	0.75	0.80
Wisconsin	0	0.93	0.79	0.84	0.80	0.85
Ohio	1	0.84	0.76	0.83	0.77	0.84

Notes: Predictions are based on regression results in column 5 of Table 2. For states where UFR data are missing, the average UFR values for non-missing states within the same region are used as proxies.

Table 2.4. The Effects of Out-of-Wedlock Childbirth on Secondary school enrollment Rate, 1940-2000

	(1)		(2)		(3)		(4)		(5)		(6)	
Effective UFR (x 100)	-0.010	(0.020)	-0.040	(0.017)			-0.014	(0.020)	-0.042	(0.016)	0.020	(0.033)
Effective UFR^2 (x 10,000)											-0.014	(0.007)
Effe Abortion Ratio (x100)					0.004	(0.006)	0.013	(0.007)	0.006	(0.006)	0.010	(0.006)
Mom's School Years			-0.012	(0.013)	-0.015	(0.014)			-0.014	(0.014)	-0.009	(0.014)
Female(<=44) Labor Force Participation Rate			-0.271	(0.137)	-0.173	(0.135)			-0.252	(0.134)	-0.284	(0.133)
Pupil Teacher Ratio (L.1-12)			-0.005	(0.002)	-0.005	(0.002)			-0.005	(0.002)	-0.004	(0.002)
Relative Teacher Salary (L.1-12)			0.019	(0.022)	0.022	(0.022)			0.021	(0.022)	0.010	(0.021)
School Years			0.063	(0.013)	0.066	(0.013)			0.063	(0.013)	0.069	(0.014)
% with <8 yrs of schooling			-0.037	(0.095)	0.040	(0.093)			-0.045	(0.094)	0.006	(0.098)
ln(real output per worker) (1997$)			0.034	(0.022)	0.037	(0.022)			0.035	(0.022)	0.037	(0.022)
AFDC			-0.005	(0.008)	-0.005	(0.008)			-0.005	(0.008)	-0.005	(0.008)
% Church			0.177	(0.057)	0.158	(0.056)			0.175	(0.057)	0.167	(0.056)
% Urban			0.474	(0.090)	0.482	(0.090)			0.468	(0.089)	0.434	(0.090)
% Male			4.396	(1.155)	4.032	(1.142)			4.509	(1.150)	4.158	(1.131)
Racial heterogeneity index			-0.203	(0.087)	-0.216	(0.088)			-0.211	(0.087)	-0.276	(0.090)
State FE	Yes		Yes		Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes		Yes		Yes	
R-squared	0.70		0.83		0.83		0.71		0.83		0.83	
Obs.	1863		1863		1863		1863		1863		1863	

Notes: The dependent variable is the natural log of the secondary school enrollment rate. Standard errors are in parentheses. Regressions based on 1,863 observations for 32 states over the 1940-2002 period. Year and state fixed effects are included in all regressions, and observations are weighted by total state population. Coefficients and standard errors are adjusted for heteroskedasticity, temporal correlation across states, and an AR(1) process for within-state autocorrelation using a panel-data Prais-Winsten approach.

Table 2.5. The Effects of Out-of-Wedlock Childbirth on Higher Education Rate, 1947-2000

	(1)		(2)		(3)		(4)		(5)		(6)	
Effective UFR (x 100)	0.054	(0.033)	-0.120	(0.207)			0.052	(0.034)	0.063	(0.031)	0.092	(0.056)
Effective UFR^2 (x 10,000)											-0.007	(0.011)
Effe Abortion Ratio (x100)					0.002	(0.014)	0.004	(0.016)	-0.002	(0.014)	0.002	(0.016)
Mom's School Years			-0.027	(0.027)	-0.024	(0.028)			-0.026	(0.027)	-0.024	(0.027)
Female(<=44) Labor Force Participation Rate			-0.186	(0.305)	-0.330	(0.297)			-0.194	(0.300)	-0.188	(0.300)
Pupil Teacher Ratio (L.1-12)			-0.024	(0.005)	-0.023	(0.005)			-0.024	(0.005)	-0.024	(0.005)
Relative Teacher Salary (L.1-12)			-0.003	(0.038)	-0.003	(0.038)			-0.003	(0.038)	-0.009	(0.039)
Average School Years			-0.019	(0.024)	-0.024	(0.024)			-0.019	(0.024)	-0.017	(0.024)
% with <8 yrs of schooling			-0.280	(0.164)	-0.344	(0.164)			-0.278	(0.164)	-0.264	(0.166)
ln (real output per worker) (1997$)			0.002	(0.038)	0.000	(0.038)			0.002	(0.038)	0.002	(0.038)
AFDC			0.004	(0.009)	0.003	(0.009)			0.004	(0.009)	0.004	(0.009)
% Church			-0.126	(0.120)	-0.109	(0.120)			-0.126	(0.120)	-0.130	(0.120)
% Urban			0.413	(0.238)	0.415	(0.233)			0.415	(0.232)	0.392	(0.235)
% Male			-13.182	(3.229)	-12.675	(3.214)			-13.183	(3.190)	-13.206	(3.190)
Racial heterogeneity index			-0.579	(0.219)	-0.566	(0.218)			-0.576	(0.217)	-0.608	(0.223)
State FE	Yes		Yes		Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes		Yes		Yes	
R-squared	0.97		0.98		0.97		0.97		0.98		0.98	
Obs.	1652		1652		1652		1652		1652		1652	

Notes: The dependent variable is the natural log of the higher education enrollment rate. Standard errors are in parentheses. Regressions based on 1,652 observations for 32 states over the 1947-2002 period. Year and state fixed effects are included in all regressions, and observations are weighted by total state population. Coefficients and standard errors are adjusted for heteroskedasticity, temporal correlation across states, and an AR(1) process for within-state autocorrelation using a panel-data Prais-Winsten approach.

Table 2.6. Robustness Checks

	HS grad. rate		Sec. enrollment rate	
Baseline	-0.046	(2.40)	-0.042	(2.60)
Include contemporaneous UFR control	-0.045	(2.36)	-0.042	(2.63)
Unweighted regression	-0.039	(2.21)	-0.064	(2.96)
Assume no autocorrelation, cluster errors at state level	-0.075	(6.38)	-0.077	(9.92)
Include a state-specific linear time trend	-0.020	(0.82)	-0.003	(0.12)
Exclude D.C.	-0.056	(2.46)	-0.030	(1.65)
Include poverty rate (1049, 1056)	-0.002	(0.12)	-0.028	(1.49)
Include % black	-0.039	(2.05)	-0.034	(2.09)
Include (region x year) fixed effects	-0.030	(1.51)	-0.027	(1.44)
Include wage ratios btw hs (college) grad and hs dropout	-0.038	(2.01)	-0.032	(1.99)
include dummies for vietnam war period	-0.046	(2.40)	-0.042	(2.60)
Include % married	-0.038	(1.86)	-0.040	(2.31)

Notes: Values are coefficients on effective unmarried fertility ratio (UFR). Coefficients on other covariates are suppressed for readability. The t-statistics are in parentheses.

Table 2.7. Temporal Variation in the Effect of Unmarried Fertility on Education

	HS grad. Rate				Sec. enrollment rate			
	1940-1970		1971-2000		1940-1970		1971-2000	
Effective UFR (x 100)	-0.198	(0.051)	0.016	(0.019)	-0.034	(0.045)	-0.016	(0.021)
Effe Abortion Ratio (x100)			0.007	(0.004)			0.013	(0.006)
Mom's School Years	-0.019	(0.042)	-0.059	(0.018)	-0.035	(0.022)	-0.043	(0.020)
Female(<=44) Labor Force Participation Rate	-0.077	(0.342)	-0.445	(0.298)	0.058	(0.219)	-0.723	(0.238)
Pupil Teacher Ratio (L.1-12)	0.003	(0.004)	-0.014	(0.005)	-0.001	(0.003)	0.002	(0.004)
Relative Teacher Salary (L.1-12)	0.036	(0.035)	-0.038	(0.039)	-0.013	(0.023)	-0.010	(0.031)
School Years	0.198	(0.031)	0.054	(0.026)	0.112	(0.024)	0.031	(0.017)
% with <8 yrs of schooling	-0.904	(0.292)	-0.034	(0.179)	-0.547	(0.145)	0.139	(0.128)
ln (real output per worker) (1997$)	-0.120	(0.053)	-0.119	(0.066)	0.065	(0.024)	-0.015	(0.042)
AFDC	-0.074	(0.029)	0.008	(0.016)	-0.017	(0.013)	0.004	(0.010)
% Church	0.084	(0.131)	0.380	(0.223)	-0.128	(0.079)	0.246	(0.186)
% Urban	0.374	(0.212)	0.034	(0.185)	0.675	(0.166)	0.137	(0.130)
% Male	1.864	(1.604)	-1.236	(2.814)	2.206	(1.106)	-1.044	(1.798)
Racial heterogeneity index	-0.258	(0.263)	-0.044	(0.212)	-0.597	(0.179)	-0.140	(0.134)
State FE	Yes		Yes		Yes		Yes	
Year FE	Yes		Yes		Yes		Yes	
R-squared	0.96		0.96		0.92		0.76	
Obs.	903		953		903		960	

Notes: Standard errors in parentheses. Regressions based on observations for 32 states over the 1940-2000 time period. Missing data limits the number of observations, however. Year and state fixed effects are included in all regressions, and observations are weighted by total state population. Coefficients and standard errors are adjusted for heteroskedasticity, temporal correlation across states, and an AR(1) process for within-state autocorrelation using a panel-data Prais-Winsten approach.

Table 2.8. Racially Disaggregated Effect of Unmarried Fertility on High School Graduation Rates

	High School Graduation Rate					
	Entire Population		Disaggregated by Race			
Effective UFR (x 100)	-0.041	(0.020)				
Effective UFR (white) (x 100)			-0.070	(0.029)	-0.149	(0.058)
Effective UFR^2 (white) (x 100)					0.042	(0.025)
Effective UFR (nonwhite) (x 100)			0.001	(0.005)	-0.003	(0.011)
Effective UFR^2 (nonwhite) (x 100)					0.001	(0.002)
Effe Abortion Ratio (x100)	0.004	(0.004)	0.003	(0.004)	0.003	(0.004)
Mom's School Years	0.000	(0.017)	-0.007	(0.017)	0.000	(0.000)
Female(<=44) Labor Force Participation Rate	0.108	(0.207)	0.142	(0.203)	0.000	(0.000)
Pupil Teacher Ratio (L.1-12)	-0.014	(0.003)	-0.014	(0.003)	-0.009	(0.017)
Relative Teacher Salary (L.1-12)	-0.030	(0.031)	-0.020	(0.030)	0.153	(0.200)
School Years	0.096	(0.024)	0.092	(0.023)	-0.013	(0.003)
% with <8 yrs of schooling	-0.060	(0.167)	-0.038	(0.174)	-0.021	(0.030)
ln (real output per worker) (1997$)	-0.154	(0.050)	-0.153	(0.050)	0.095	(0.023)
AFDC	-0.015	(0.018)	-0.015	(0.018)	-0.014	(0.171)
% Church	0.227	(0.080)	0.207	(0.079)	-0.150	(0.049)
% Urban	-0.143	(0.157)	-0.089	(0.153)	-0.016	(0.018)
% Male	-0.438	(1.973)	-0.240	(1.988)	0.210	(0.077)
Racial heterogeneity index	0.237	(0.133)	0.269	(0.133)	-0.093	(0.150)
State FE	Yes		Yes		Yes	
Year FE	Yes		Yes		Yes	
R-squared	0.94		0.94		0.94	
Obs.	1553		1553		1553	

Notes: Standard errors in parentheses. Regressions based on observations for 32 states over the 1940-2000 time period. Missing data limits the number of observations, however. Year and state fixed effects are included in all regressions, and observations are weighted by total state population. Coefficients and standard errors are adjusted for heteroskedasticity, temporal correlation across states, and an AR(1) process for within-state autocorrelation using a panel-data Prais-Winsten approach. All variables used as controls in Table 2.2 are also included in these regressions.

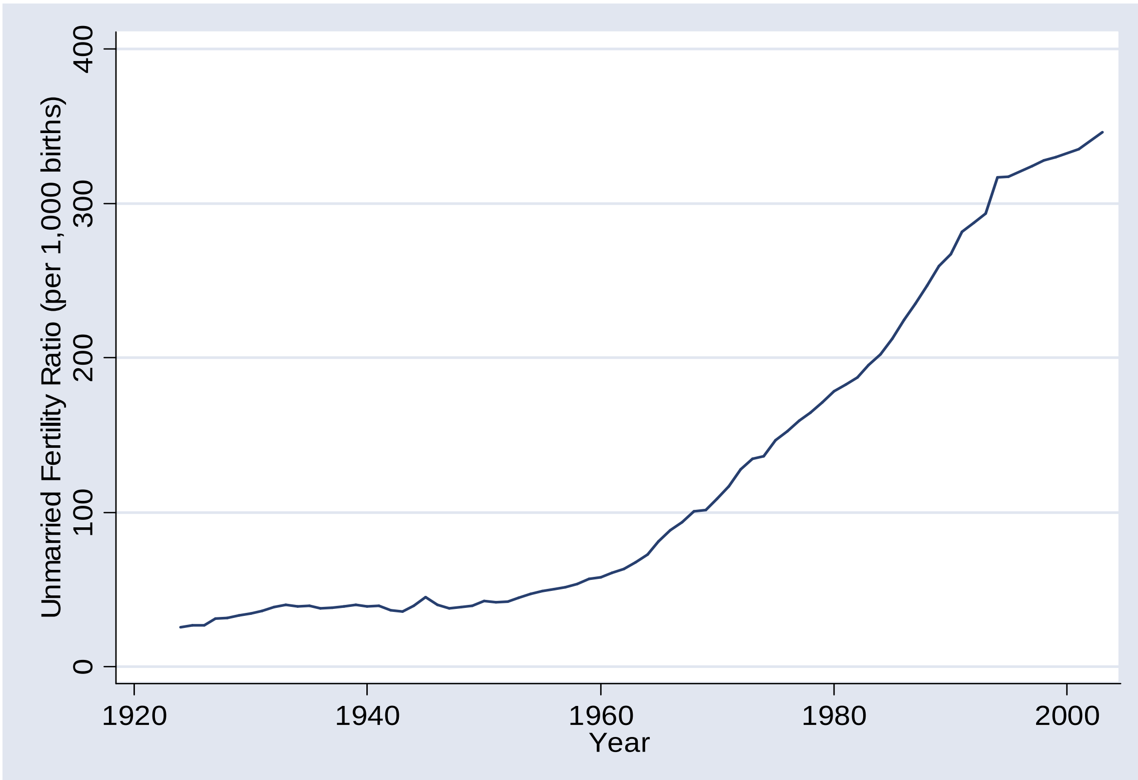
Table 2.9. Racially Disaggregated Effect of Unmarried Fertility on Secondary School Enrollment Rates

	Secondary Enrollment Rate					
	Entire Population		Disaggregated by Race			
Effective UFR (x 100)	-0.038	(0.016)				
Effective UFR (white) (x 100)			-0.026	(0.027)	-0.103	(0.063)
Effective UFR^2 (white) (x 100)					0.035	(0.026)
Effective UFR (nonwhite) (x 100)			-0.007	(0.006)	0.002	(0.014)
Effective UFR^2 (nonwhite) (x 100)					-0.001	(0.002)
Effe Abortion Ratio (x100)	0.005	(0.006)	0.003	(0.006)	0.003	(0.006)
Mom's School Years	0.009	(0.014)	0.006	(0.015)	0.000	(0.000)
Female(<=44) Labor Force Participation Rate	-0.305	(0.156)	-0.247	(0.153)	0.000	(0.000)
Pupil Teacher Ratio (L.1-12)	-0.002	(0.002)	-0.002	(0.003)	0.005	(0.015)
Relative Teacher Salary (L.1-12)	-0.036	(0.024)	-0.031	(0.024)	-0.243	(0.151)
School Years	0.021	(0.014)	0.017	(0.015)	-0.002	(0.003)
% with <8 yrs of schooling	-0.055	(0.103)	-0.056	(0.109)	-0.031	(0.024)
ln (real output per worker) (1997$)	0.038	(0.027)	0.036	(0.028)	0.019	(0.015)
AFDC	0.003	(0.008)	0.003	(0.008)	-0.050	(0.110)
% Church	0.138	(0.061)	0.122	(0.061)	0.035	(0.028)
% Urban	0.170	(0.107)	0.205	(0.106)	0.003	(0.008)
% Male	-1.949	(1.543)	-1.670	(1.581)	0.112	(0.061)
Racial heterogeneity index	0.140	(0.092)	0.148	(0.094)	0.200	(0.105)
State FE	Yes		Yes		Yes	
Year FE	Yes		Yes		Yes	
R-squared	0.86		0.86		0.86	
Obs.	1560		1560		1560	

Notes: Standard errors in parentheses. Regressions based on observations for 32 states over the 1940-2000 time period. Missing data limits the number of observations, however. Year and state fixed effects are included in all regressions, and observations are weighted by total state population. Coefficients and standard errors are adjusted for heteroskedasticity, temporal correlation across states, and an AR(1) process for within-state autocorrelation using a panel-data Prais-Winsten approach. All variables used as controls in Table 2.4 are also included in these regressions.

APPENDICES

Figure 1.2. Unmarried Fertility Ratio: 1923-2002



Source: Kendall and Tamura (2010)

Table 1.9. Divorce Laws by States

state	No-Fault Year	Unilateral Year	Unilateral, No-Fault Settlement	state	No-Fault Year	Unilateral Year	Unilateral, No-Fault settlement
Alabama	1971	1971	0	Montana	1973	1973	1
Alaska	1935	1935	1	Nebraska	1972	1972	1
Arizona	1931	1973	1	Nevada	1931	1967	1
Arkansas	1937			New Hampshire	1971	1971	0
California	1970	1970	1	New Jersey	1971		
Colorado	1972	1972	1	New Mexico	1933	1933	1
Connecticut	1973	1973	0	New York	1967		
Delaware	1957			North Carolina	1910		
DC	1966			North Dakota	1971	1971	0
Florida	1971	1971	0	Ohio	1974		
Georgia	1973	1973	0	Oklahoma	1953	1953	1[b]
Hawaii	1965	1972	1	Oregon	1971	1971	1
Idaho	1945	1971	0	Pennsylvania	1980		
Illinois	1984			Rhode Island	1910	1975	0
Indiana	1973	1973	1	South Carolina	1969		
Iowa	1970	1970	0	South Dakota	1985	1985	0
Kansas	1969	1969	0	Tennessee	1963		
Kentucky	1962	1972	1[a]	Texas	pre-1910	1970	0
Louisiana	1916			Utah	1943	1987	1
Maine	1973	1973	1	Vermont	1969		
Maryland	1969			Virginia	1960		
Massachusetts	1975	1975	0	Washington	1921	1973	1
Michigan	1972	1972	0	West Virginia	1969		
Minnesota	1933	1974	1	Wisconsin	pre-1910	1978	1
Mississippi	1978			Wyoming	1977	1977	0
Missouri	1974						

Source: Friedberg(1998); Gruber(2004)
Notes: a. starts in 1987; b. starts in 1975.

Table 1.10. Time limits under waivers and TANF

State	Waiver			TANF		
	month	year	limit	month	year	limit
Alabama				11	1996	60
Alaska				7	1997	60
Arizona	11	1995	24	10	1996	24
Arkansas				7	1998	24
California				1	1998	60
Colorado				7	1997	60
Connecticut	1	1996	21	10	1996	21
Delaware	10	1995	48	3	1997	48
DC				3	1997	60
Florida				10	1996	48
Georgia				1	1997	48
Hawaii	12	1996	60	7	1997	60
Idaho				7	1997	24
Illinois				7	1997	60
Indiana	4	1995	24	10	1996	24
Iowa				1	1997	60
Kansas				10	1996	60
Kentucky				10	1996	60
Louisiana	1	1997	24	1	1997	60
Maine				11	1996	60
Maryland				1	1997	60
Massachusetts				12	1996	
Michigan				9	1996	
Minnesota				7	1997	60
Mississippi				10	1996	60
Missouri	4	1995	48	7	1997	60
Montana				2	1997	60
Nebraska	1	1996	24	12	1996	60
Nevada				1	1998	60
New Hampshire				10	1996	60
New Jersey				2	1997	60
New Mexico				7	1997	60
New York				12	1996	
North Carolina	8	1996	24	1	1997	60
North Dakota				7	1997	60
Ohio	6	1996	36	10	1997	60
Oklahoma				10	1996	60
Oregon	7	1995	24	6	1996	
Pennsylvania				3	1997	60
Rhode Island				5	1997	60
South Carolina	6	1996	24	10	1996	60
South Dakota				12	1996	60
Tennessee	9	1996	18	10	1996	60
Texas	5	1996	24	1	1997	60
Utah				1	1997	36
Vermont				9	1996	
Virginia	7	1995	24	2	1997	60
Washington	1	1996	48	8	1997	60
West Virginia				1	1997	60
Wisconsin				9	1996	60
Wyoming				1	1997	60

Table 1.11. Work Requirement Time limits under Waiver and TANF

State	Waiver			1st TANF Plan			2nd TANF Plan		
	month1	year1	work TL	month2	year2	work TL	month3	year3	work TL
Alabama				11	1996	0			
Alaska				7	1997	24			
Arizona				10	1996	24	10	1999	0
Arkansas				7	1997	0			
California	9	1995	22	1	1998	0			
Colorado				7	1997	24			
Connecticut				10	1996	0			
Delaware	10	1995	24	3	1997	24	1	2000	0
District of Columbia				3	1997	24	10	1999	1
Florida				10	1996	0			
Georgia				1	1997	0	10	1999	24
Hawaii				7	1997	24			
Idaho				7	1997	0			
Illinois	10	1995	12	7	1997	24	10	1999	0
Indiana				10	1996	0			
Iowa				1	1997	0			
Kansas				10	1996	0			
Kentucky				10	1996	6	10	2002	24
Louisiana				1	1997	24			
Maine				11	1996	24	10	2002	0
Maryland				1	1997	0			
Massachusetts	10	1995	2	9	1996	2			
Michigan	10	1994	12	9	1996	2			
Minnesota				7	1997	24	10	2002	0
Mississippi				10	1996	24			
Missouri	4	1995	24	7	1997	24			
Montana	2	1996	24	2	1997	24	1	2000	0
Nebraska				12	1996	0			
Nevada				12	1996	24	10	2002	0
New Hampshire	7	1996	0	10	1996	0			

New Jersey				2	1997	24	10	2001	0
New Mexico				2	1997	2	12	1999	3
New York				12	1996	24			
North Carolina				1	1997	24	10	1999	3
North Dakota				7	1997	24	10	1999	0
Ohio				10	1996	24	10	2002	0
Oklahoma				10	1996	0			
Oregon				10	1996	0			
Pennsylvania				3	1997	24	10	2002	0
Rhode Island				5	1997	24	10	1999	2
South Carolina				10	1996	24	10	1999	0
South Dakota	4	1994	24	12	1996	2	10	2002	0
Tennessee				10	1996	24	10	1999	0
Texas				11	1996	0			
Utah				10	1996	0			
Vermont	7	1994	30	9	1996	30			
Virginia				2	1997	0	1	2000	3
Washington				8	1997	0			
West Virginia				1	1997	24	1	2000	0
Wisconsin				9	1996	0			
Wyoming				1	1997	0			

Table 1.12. AFDC/TANF Real Benefit Levels: Selected Years

State	Real Benefit for Family of 2				Real Benefit per Child			
	1978	1996	1997	2002	1978	1996	1997	2002
Alabama	135	85	83	74	46	19	19	17
Alaska	537	523	511	455	77	66	64	58
Arizona	212	176	172	153	57	45	44	39
Arkansas	206	103	100	89	41	27	27	24
California	443	323	285	296	103	64	67	63
Colorado	324	178	174	156	74	48	47	42
Connecticut	356	285	279	248	199	61	60	53
Delaware	311	171	168	150	64	44	43	38
District of Columbia	307	212	207	164	87	55	40	47
Florida	209	154	151	135	46	39	38	34
Georgia	156	147	143	128	35	32	31	28
Hawaii	598	360	352	314	120	94	92	82
Idaho	428	161	157	163	67	41	40	0
Illinois	330	217	212	189	90	24	23	21
Indiana	268	147	143	128	77	37	36	32
Iowa	437	228	222	198	84	44	43	38
Kansas	399	214	209	186	80	43	42	38
Kentucky	207	126	123	110	77	41	40	36
Louisiana	166	93	91	109	49	28	27	24
Maine	282	198	193	190	100	69	67	67
Maryland	265	189	186	193	72	49	49	51
Massachusetts	426	312	305	297	90	57	55	54
Michigan	465	226	221	197	112	66	65	58
Minnesota	543	282	276	246	54	57	55	49
Mississippi	110	61	60	81	37	15	15	13
Missouri	276	154	151	135	58	32	31	28
Montana	275	233	217	220	117	47	55	54
Nebraska	383	187	183	163	92	45	44	39
Nevada	285	184	179	160	69	38	37	33

New Hampshire	414	310	303	298	58	40	39	35
New Jersey	426	229	224	200	74	41	40	36
New Mexico	253	187	193	172	49	56	50	44
New York	479	298	291	260	126	70	69	61
North Carolina	255	157	154	137	26	16	16	14
North Dakota	359	220	215	202	104	55	54	52
Ohio	275	166	163	158	86	51	50	49
Oklahoma	299	149	146	124	87	47	45	38
Oregon	514	226	221	197	52	67	65	58
Pennsylvania	354	197	193	172	109	60	59	52
Rhode Island	520	302	295	263	15	51	50	44
South Carolina	120	101	99	89	35	26	26	23
South Dakota	399	243	237	212	61	31	31	27
Tennessee	147	92	90	80	40	26	26	23
Texas	141	96	93	89	37	24	24	22
Utah	374	226	221	208	100	46	45	43
Vermont	560	358	362	304	86	48	34	46
Virginia	213	150	146	146	150	36	35	32
Washington	477	287	280	250	98	61	60	53
West Virginia	250	124	121	193	66	38	37	59
Wisconsin	479	265	259	374	112	64	63	0
Wyoming	391	210	206	189	38	19	19	0

Note: All numbers are in 1983 dollars.

APPENDIX TO CHAPTER II

Data Appendix

Abortion ratio

Abortion data is from Centers for Disease Control, "Abortion Surveillance Report" [annual].

AFDC

Total payments divided by families receiving payments, from United States Statistical Abstract [annual]

Church Membership

Number of church members declared by 114 religious bodies in each state, divided by the total population. Studies were performed in 1952, 1971, 1980, 1990, and 2000 (e.g., Quinn, et al, 1982). Data for other years is linearly interpolated.

Enrollment Rates

Percent of population aged 14-17, and 18-24, enrolled in high school and college, respectively. See data appendix of Turner, et al. (2007) for details.

High School Graduate Numbers, Public

Public school graduate number data between 1945 and 2003 is available from United States Statistical Abstract [annual]. Missing data in 1951, 53, 55, 61, 83, 84, 86 are linearly interpolated.

High School Graduate Numbers, Private

Private School graduate number data for 1964-1980 and 1991-2002 is available from Digest of Educational Statistics [annual]. Data in 1940, 41 and 47 is available from Biennial Survey of Education [biennial]

Low Human Capital Population

Percent of state labor force members with fewer than 9 years of schooling, adjusted for migration, from Turner, et al. (2007).

Percent Urban

Percent of resident population living in metropolitan statistical areas, from Bureau of the Census United States Statistical Abstract [annual]

Population by Age and Gender

From Estimates for the United States, Regions, Divisions, and States by 5 Year Age Groups and Sex: Annual Time Series Estimates, U.S. Census Bureau [annual].

Real output per worker

Income per worker, from Bureau of Economic Analysis, converted to 2003 dollars with the consumer price index. See data appendix of Turner, et al. (2007) for details.

Schooling, Average Years

Average years of schooling among labor force participants. See data appendix of Turner, et al. (2007).

Unemployment

Figures used represent the percent unemployed among civilian non-institutional population 16 years and older, with total unemployment estimates based on the Current Population Survey, taken from Bureau of the Census United States Statistical Abstract [annual].

Unmarried Fertility Ratio

Births to unmarried mothers, as a fraction of 1,000 live births. 1925-1936: Data are from Bureau of the Census, Statistical Abstract of the United States [annual]. Some states supply race-specific UFR data, particularly Southern states, but others do not. 1937-2002: Data are from National Center for Health Statistics, Vital Statistics of the United States, Natality [annual], NCHS collected the data from birth certificate records, using either a 50% or 100% sample in each state. However, not all states ask about marital status on the birth certificate, and the number of states that do falls over the time period. With interpolation of fewer than 9 years in any particular state, UFRs lagged 8-34 years are available for calculating "effective" UFR for the following states starting in 1957: Delaware, District of Columbia, Illinois, Indiana, Kansas, Kentucky, Maine, Michigan, Minnesota, Mississippi, New Jersey, North Carolina, Oregon, Pennsylvania, Rhode Island, South Carolina, Utah, Virginia, Washington, Wisconsin, and Wyoming. In addition, effective UFRs are calculated beginning in years *after* 1957 for the following states: Alabama (1961), Florida (1958), Iowa (1958), Louisiana (1961), Missouri (1961), Nevada (1963), North Dakota (1958), South Dakota (1966), Tennessee (1961), Texas (1967), West Virginia (1959). The missing early data for these states is generally due to the fact that they did not require birth certification until some year after 1923.