The Effect of Education on Fertility: Evidence From Compulsory Schooling Laws*

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Abstract

Social scientists have long observed a strong negative relationship between education and fertility. Since the choice of schooling is not random, however, the question of whether this correlation is causal remains open. In this paper, I use 1950-1990 US Census data, along with information on compulsory attendance and child labor laws that affected women's schooling choices in their teenage years, to estimate the effect of education on total completed fertility accounting for the endogeneity of schooling. Instrumental variable estimates using changes in state compulsory schooling laws as a source of exogenous variation in education indicate that women with 3-4 additional years of schooling have on average one less child than they would have otherwise. Further analysis suggests that heterogeneity across individuals and, to a lesser extent, non-linearity in the fertility return to schooling, explain an important part of the difference between IV and OLS estimates. Moreover, this fertility-reducing effect of schooling does not appear to be mediated by a reduction in marriage rates, while there is evidence that education does increase the probability that a woman will reach the end of her fertile lifetime without children. These results are robust to a number of specification checks, and imply that rising levels of education can account for a sizable fraction of the decline in fertility rates for several Western countries in the second half of the 20th century.

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I. Introduction

Social scientists have long observed a strong relationship between education and fertility. Both across countries and over time, higher levels of schooling appear associated with fewer children per woman. In particular, the last forty years have seen widespread fertility declines accompanied by increases in educational attainment levels in most Western countries. The question of whether this correlation is causal remains open, however. Despite the various reasons to expect a causal relationship between schooling and fertility, empirical research to date has not provided a definitive and satisfactory answer.

The key challenge in estimating the effect of education on fertility decisions is that unobserved characteristics affecting schooling choices are potentially correlated with unobservable factors influencing the decision to have children. For instance, women with high ability levels, stronger tastes for work or low discount rates are relatively more likely to finish high school and attend college. At the same time, for any given level of education, they are likely to be more inclined to pursue a professional career and delay having children. Therefore, one might expect a negative relationship between years of schooling and number of children even in the absence of any causal effect of education on fertility. On the other hand, women with better access to credit markets may be more likely both to attend school and to have children, whereas females coming from less affluent households may lack the opportunities or incentives to get an education as well as the means to raise children and support an extended family. As a result, a positive spurious correlation between education and fertility is possible too. The presence of error in available measures of schooling can also introduce a bias towards zero, thus creating the appearance of a weaker correlation between the two variables than may exist in reality.

Estimating the impact of schooling on fertility may help clarify the role of education in demographic transitions. Moreover, this effect can be seen as yet another dimension of the social return to schooling. To the extent that schooling choices of individuals have social consequences in the form of fiscal costs or welfare benefits, which are not taken into account by individuals, then this constitutes another reason why the social return to education may be different from its private return.[1]

An analysis of the impact of schooling on fertility is also motivated by important fiscal and welfare policy implications. Falling fertility rates, along with longer life expectancies, result in population ageing. Unless immigration flows are large enough to offset this process (and countries are typically reluctant to let that happen), this tends to reduce the labor force relative to the elderly population. In other words, it raises the dependency ratio of retirees to working-age adults, thus putting pressure on public spending in pensions and health care. This is a major concern in many industrialized countries, where such fiscal burdens constitute a major threat to their current social security systems. If secondary- and post-secondary education enrollment rates are growing in those countries, no analysis of the sustainability of the welfare system can be complete without an assessment of the impact of higher education on fertility, which in turn affects the future size of the labor market. In developing countries, on the other hand, decreasing fertility may reduce health risks for both women and children, and contribute to improving welfare conditions, especially of rural households. Programs like the World Bank's Female Secondary Schooling Assistance Project aim to achieve these goals precisely by encouraging education of girls. The effectiveness of such ventures depends on whether, and to what extent, female education stimulates reductions in fertility.

In order to solve the identification problem outlined above and estimate the effect of education on fertility, I use changes in state compulsory schooling laws over time as a source of exogenous variation in individual schooling choices. Female teenagers typically faced compulsory attendance and child labor laws enacted by their state legislatures several years or even decades prior to those women's fertility decisions. Moreover, legislators appeared to be concerned with raising education levels and preventing children from entering the labor force too young, and did not seem to be acting in response to contemporaneous or anticipated changes in fertility patterns. Recent literature studying the causal links between education and labor market outcomes has used supply-

[1] Most of the research on social returns to education to date has been focused on the effect of an individual's schooling on the *wages* of other workers in her social group (Acemoglu and Angrist (2000), Heckman and Klenow (1998)), although some recent studies analyze the impact of individual schooling on other outcomes with social repercussions, such as crime (Lochner and Moretti (2002)), or mortality (Lleras-Muney (2002)).

side instruments such as child schooling laws: Angrist and Krueger (1991) first documented a relationship between quarter of birth and individual schooling and used it to analyze private returns to education; Acemoglu and Angrist (2000) studied wage spillovers; Lochner and Moretti (2004) analyzed effects on crime; and Lleras-Muney (2004) studied mortality. Very little or no work has been done linking schooling and fertility through the study of a natural experiment, however. An exception is McCrary and Royer (2003), who use birthday information for Texan and Californian women in the 1990s and a regression discontinuity approach to study the effect of mothers' education on infant mortality, by first establishing no impact of schooling on the probability of becoming a mother (an age-specific fertility rate). Instead of relying solely on school age entry laws (which is the rationale behind a strategy based on date of birth), this paper also uses information on other regulations such as minimum school dropout age, and minimum schooling requirements for leaving school and for entering the workforce, in order to obtain a more complete picture of the institutional constraints affecting education of women in all fifty contiguous states during five decades in the early- and mid-twentieth century, a period when high school attendance rates rose dramatically.[2]

Instrumental variable (IV) estimates using data on women aged 40-49 from the 1950-1990 US Censuses suggest a strong, negative relationship between education and fertility. A one-year increase in schooling is associated with a 0.33 reduction in the average number of children. The magnitude of this effect appears to be larger than the relationship uncovered by simple ordinary least squares (OLS) regressions, which suggests the presence of measurement error in schooling. Further analysis indicates that part of this difference is due to the existence of heterogeneity in the fertility return to schooling across individuals and to non-linearity across education levels. Since the OLS and IV estimators are different weighted sums of the distinct impacts of each additional year of schooling on the number of children, with the IV placing more "weight" on the levels of education which are most affected by the instruments,[3] the estimates using compulsory schooling laws as instruments provide an accurate approximation to the

[2] This is the education expansion known as the 'high school movement'. See Goldin (1998) for details.
[3] Angrist and Imbens (1995) show that 2SLS and OLS estimates can be written as weighted averages of individual IV estimators, and Lochner and Moretti (2004) derive the corresponding 2SLS and OLS weights as a function of observable quantities.

effect of additional schooling on the fertility of women who are induced to increase their education because of those laws.

Because education can affect fertility by reducing the likelihood that a woman will marry and start a family, I also study the impact of schooling on marriage rates. Estimates uncover no statistically significant relationship, which suggests that education may be reducing fertility by delaying, but not by preventing, the decision to get married. This hypothesis is further supported by estimates showing that schooling does indeed raise the probability that a woman will reach the end of her fertile life-cycle with no children. Finally, I use these estimates to calculate the contribution of education expansion to the dramatic fertility declines observed in several Western countries, and find that about a third of the documented reductions in fertility between 1960 and 1990 can be attributed to the observed increases in female schooling in those countries.

The remainder of the paper is organized as follows. Section II discusses the theory, develops the estimation framework, and highlights the econometric issues involved in attempting to identify the effect of education on fertility. Section III describes the data and presents and interprets the base empirical results. Section IV discusses some robustness checks and additional results and applications. Section V summarizes the paper and concludes.

II. Theoretical and Econometric Framework

A. Fertility and Schooling: Theory

The most accepted theories in the Demography and Economics literature (Willis (1973), Barro and Becker (1988), Livi-Baci (1997)) suggest that female education lowers fertility through an increase in the opportunity cost of women's time where the productive technology for children is time-intensive relative to the parents' technology for their standard of living. In fact, theoretical models that seek to explain the number of children born over the life-cycle highlight female wages as the key element in the opportunity cost of childbearing. The canonical one-period, full-certainty model of fertility (Montgomery and Trussell (1986)) where children are a normal good and their care requires time as well as money expenditures yields a shadow price of children that is a function of the wage rate.[4] Other models seek to explain fertility histories as stochastic processes, where the woman is assumed to solve a sequential decision problem under uncertainty. These include Wolpin (1984), Newman (1988), and Hotz and Miller (1988), and have not yet produced a consensus about an appropriate empirical specification for life cycle fertility. In any case, since returns to schooling are positive, this induces a negative relationship between education and fertility. There are, however, other channels through which schooling can affect a woman's decision to have children.

To borrow from Easterlin and Crimmins (1985)'s terminology, the above is the 'demand' component of the educational effect on fertility.[5] Schooling can also affect the 'supply' of children, however. More educated women may have better information about health. By increasing awareness of the importance of food care, balanced nutrition, personal hygiene or cleaning standards, education can raise the fecundity, or potential reproductive capacity, of women. In this framework, schooling can have an additional impact on fertility by reducing the psychological cost of fertility control, since education may increase the ability or willingness to adopt new birth-control methods. In the limit, by raising knowledge of the existence and functioning of contraceptives in the first place, schooling may even bring the 'price' of fertility control down from infinity, thus allowing women to have the chance to exercise some control on family size that would not have been available otherwise.

The theories briefly reviewed above do not consider some additional channels that can mediate the relationship between schooling and fertility. Completing additional years of education necessarily entails spending more time in school. There is naturally a rather mechanical effect of schooling on fertility if women tend not to have children while

[4] This is just the (compensated) substitution effect. An increase in mothers' schooling also brings about an increase in parents' income that encourages spending in all normal goods, including children, but it appears safe to assume that this income effect must be small enough and hence the wage effect dominates. In fact, more complex theories produce an even weaker income effect on the sheer number of children: Becker (1960) and Becker and Lewis (1973) incorporate the quality dimension of reproduction decisions in their "child quality" fertility model. Their model predicts that any increase in parents' income raises both the quantity and the quality of children. Since the income elasticity of the former is small compared to the income elasticity of the latter, Becker contends, then the resulting increase in the amount spent on children mainly takes the form of higher quality, thus allowing for the substitution (wage) effect to clearly dominate the income effect on the number of children.

[5] By 'demand for children', Easterlin and Crimmins (1985) refer to the number of children parents would want in order to achieve their desired family size, in the absence of any natural constraints and under the assumption that birth control mechanisms were known, available and costless. 'Supply of children' is, then, the number of children a couple would have, were they to make no deliberate attempt to limit family size.

continuing to attend high school or college, thus delaying the beginning of (and effectively shortening) their reproductive life. Other mechanisms for education to affect fertility include changes in tastes for children versus work. Schooling may alter or shape the views that women have on their traditionally assigned role in society, encouraging some women to devote themselves to a professional career to the expense of creating an extensive family (or even of having children at all).

To sum up, theory suggests that there are several channels how schooling impacts on fertility, all of them being negative expect for the ‘supply’ argument regarding health conditions and fecundity. The combined sign of the overall effect is ambiguous, although it seems reasonable to expect a negative relationship. In any case, the magnitude of such effect is a purely empirical question.

B. Empirical Specification

In order to capture the causal relationship of interest, consider the regression model:

$$y_{it} = \alpha + \beta \cdot s_{it} + z_i'\gamma + \delta_t + \varepsilon_{it} \,, \tag{1}$$

where y_{it} is a measure of total completed fertility, the total number of children ever born to woman i observed in the Census year t, s_{it} is her schooling, δ_t are Census year fixed effects, z_i is a vector of individual covariates that includes state-of-birth effects, year-of-birth effects, and other demographic variables, and ε_{it} is an individual error component. Standard OLS estimates of equation (2-1) will be biased if schooling, s_{it}, is correlated with unobserved determinants of individual fertility choices contained in the residual term ε_{it}. As argued above, this can be the case if ability, patience or tastes for work encourage schooling and produce a low demand for children, which creates a negative omitted variable bias, or if access to economic opportunities facilitates both education and raising children, in which case there is a source of positive confounding bias, or in the presence of measurement error, which creates attentuation bias towards zero (a positive bias, if β is indeed negative). Naturally, all these possibilities are not mutually exclusive.

To address the endogeneity of education and eliminate those sources of bias, I use compulsory schooling laws as instruments that exogenously affect schooling choices. The use of valid instruments for education should produce consistent estimates of β in equation (2-1). It is important to recognize, though, that the effect of education on fertility may be non-linear in schooling and may vary across individuals. In that case, the IV estimates must be interpreted as some weighted average of the heterogeneous marginal effects of schooling on the fertility of those women most induced to raise their education by the compulsory schooling laws being used as instruments.[6]

III. Data and Main Results

A. Data Sources and Descriptive Statistics

The analysis uses data on US-born white women from the 1950-1990 Census microdata extracts for whom all the relevant variables are reported. Individuals born in Hawaii or Alaska are excluded, since these states did not enter the Union until 1959, and no information on compulsory attendance or child labor laws during the early twentieth century is available. The sample is further restricted to women aged 40 to 49, who are reaching or are already past the end of their fertile lifetime and who were 14 years old between 1914 and 1964 (years for which information on compulsory schooling laws in effect in each of the 50 contiguous states is available). The schooling variable is highest grade completed, capped at 17 years to impose a uniform top-code across survey years. Other technical details on the extracts and the definitions of the variables are documented in the Data Appendix.

Table 1 provides the descriptive statistics for the extract. The average age is constant across censuses, while mean schooling increases by about 0.6 or 0.7 years between 1950-60, 1960-70 and 1970-80, and then by slightly more than a year between 1990 and 2000. Total completed fertility, measured as the number of children per woman, increases steadily from an average of 2.2 for individuals in the 1950 Census to 2.9 in 1980 (these are the mothers of the 'baby boom' children), and then goes back down to 2.2 by 1990. Table 2 reports total completed fertility by educational attainment. In each of the sample years higher education levels are associated with substantially lower average

[6] The monotonicity assumption (namely, that compulsory schooling laws only have a positive or no effect on individual schooling) is necessary for this interpretation of the IV estimator. See Angrist and Imbens (1995), Imbens and Angrist (1994), Heckman (1997), Heckman and Vytlacil (1997) for more details.

numbers of children. The key feature to notice in Table 2 is that, while the average moves around over time, the differences in fertility by education level are sizable and persistent over time. This is further evidenced in Figure 1, which shows total completed fertility by educational attainment for all cohorts of women born between 1885 and 1954. For example, women born in 1925 who did not complete high school had an average of 3.4 children, whereas those in the same cohort who obtained a college degree gave birth to 2.4 children on average. Looking at total fertility rates —the average number of children a woman could expect to bear in her lifetime if she were to experience current age-specific fertility rates— also reveals a similar pattern. As illustrated in Figures 2a and 2b, college graduates aged 27 or younger were less likely to give to birth to a child than high school dropouts both in 1960 and in 1990. Between ages 28 and 40, college graduates are slightly more likely to have given birth during the reference year, but that tiny advantage does not make up for the big difference in fertility rates for teenagers and women in their early-to-mid twenties.[7] Finally, Figure 3 illustrates how education affects fertility at different levels of schooling after controlling for state of birth, state of residence, Census year and year of birth dummies. The figure displays regression-adjusted total completed fertility rates, this is, the coefficient estimates obtained from regressing the number of children ever born on a full set of schooling dummy variables, in a model that also includes the state, year and cohort variables (reformulated so as to have mean zero in the dataset) as covariates. The figure shows a fairly steady decline in fertility with nearly every additional year of schooling after fourth grade, although the decline appears to be slightly larger at some particular stages (such as high school graduation).

In conclusion, it appears that college graduates typically have about one less children on average than high school dropouts. Next I will turn to regression analysis in order to control for state of birth, cohort of birth, and year effects, and then to use instrumental variables to identify whether this observed relationship can be interpreted as a causal effect of schooling.

[7] This translates into an estimated (using US Census data) TFR for high school dropouts of 3.7 in 1960 and 1.9 in 1990, compared to 2.7 and 1.3 for college graduates in 1960 and 1990, respectively.

B. OLS Estimates

Table 3 shows OLS estimates of equation (2-1) for the entire sample, and also separately by Census year. Education appears to be negatively correlated with total completed fertility after controlling for cohort of birth, state of birth, and year effects using OLS. Column (2), the regression equivalent to Figure 3, adds state of residence fixed effects in order to absorb potential heterogeneity across states in fertility patterns. Doing that leaves the point estimates practically unchanged. While the estimates using single censuses (columns 4 to 8) show a slight degree of variation across years,[8] the table suggests that on average an additional year of schooling appears to be associated with a reduction of about 0.13 in the average number of children. Put another way, women with four additional years of schooling appear to have on average 0.5 less children.

The OLS estimates presented here are consistent with the hypothesis that schooling reduces completed fertility. If so, the effect appears to be quantitatively and statistically significant and fairly persistent over time. However, these estimates may reflect the impact of unobserved characteristics that influence the probability of completing higher levels of schooling and the decision to have children. For example, as discussed in the previous section, women with lower discount rates are relatively less likely to invest in education and a professional career, and more likely to marry and start having children early. To the extent that variation in unobserved discount rates is important, OLS estimates could be overstating the effect of schooling on fertility. On the other hand, OLS could be underestimating that effect if measurement error in the schooling variable is significant, and/or if there are significant disparities in background and access to economic opportunities that promote education and child rearing.

C. Compulsory Schooling Laws as Instruments for Individual Schooling

The ideal instrumental variable induces exogenous variation in years of schooling while being uncorrelated with measurement error, discount rates, ability, tastes for work, or any other individual characteristics that can affect both education and fertility. I use state-mandated restrictions on child labor and compulsory school attendance laws as

[8] These deviations may be simply reflecting time variation in omitted state characteristics in the relationship between schooling and fertility, since these single-Census regressions do not include state of residence effects.

instruments for schooling. Compulsory attendance laws are condensed as the minimum number of years a child had to be in school before being allowed to drop out, and are the maximum of either the explicitly mandated minimum years of schooling in the state, or the difference between the minimum dropout age and the maximum enrollment age. Child labor laws are summarized as the minimum years of schooling required before obtaining a work permit. Since the major reason to leave school typically was to work, these act as constraints on schooling choices as well. Child labor laws are defined as the larger of the explicitly mandated minimum schooling required for a work permit, and the difference between the minimum age for work and the maximum school enrollment age.

Compulsory schooling laws in the first half of the twentieth century have been extensively studied by Margo and Finegan (1996), Acemoglu and Angrist (2000), Lleras-Muney (2004) and Lochner and Moretti (2004). Lleras-Muney (2002) documents their effectiveness for both men and women, and finds that they did not affect blacks. For this reason, I restrict my attention to white women. The compulsory attendance laws and child labor laws in effect in each of the 50 contiguous states were assigned to individuals in the sample based on the year in which they turned 14 (which is calculated from year of birth, estimated using age on Census day) and their state of birth (since no information on state of residence during adolescence is available in the Census).

In the years relevant for my sample, 1914-1964, states changed child labor and compulsor attendance laws several times, and generally upward.[9] This resulted in an increase over time in the fraction of women being exposed to more restrictive laws, as is evident from observing the bottom eight rows in Table 1. For example, while no woman in the sample from the 1950 Census had been exposed to laws requiring 9 or more years in school before obtaining a work permit, by 1990 such laws had been in place at age 14 in the state of birth of 43.5% of all women.

There is a sizable and statistically significant relationship between individual schooling and sets of dummies for both types of compulsory schooling laws. This is shown in Table 4, which display estimates for the first-stage regressions of years of schooling on dummies for child labor laws requiring 7, 8, and 9 or more years in school, and/or dummies for compulsory attendance laws mandating 9, 10, and 11 or more years

[9] See Lleras-Muney (2002) and Lochner and Moretti (2004).

of schooling (the omitted categories are the least restrictive groups for child labor and compulsory schooling laws); equations also include controls for state of birth, year of birth and Census year. For instance, the entries in column 1 show that women born in states requiring 9 or more years in school to issue a work permit ended up with .38 more years of schooling completed than those born in states with a child labor law that required 6 or less years. In general, the estimated coefficients are consistent with the notion that the more stringent the legislation, the stronger is its effect on average years of schooling.[10] Moreover, the hypothesis of no joint significance of the estimated coefficients is soundly rejected in every column (F-statistics are reported in the table).

The identifying assumption is that the timing of within-state changes in compulsory schooling laws over time is orthogonal to unobservable characteristics of women that may affect their fertility decisions years later, once other potential confounding factors have been taken into account by conditioning on state of birth, cohort of birth and Census year (and, in some specifications, also state of residence). This hypothesis is reinforced by the results shown in Table 5. The columns in this table report the estimated coefficients from regressions of a dummy for whether a woman completed discrete levels of education as indicated in the column heading. The effect of compulsory schooling laws is strongest for completion of high school levels of schooling, and is smaller, and in most cases statistically insignificant, in columns corresponding to higher levels of education. Finding that the laws increased the proportion attending college as well as the fraction completing high school would have suggested that they might have been correlated with underlying trends in education or other omitted factors such as tastes or family background, and are therefore not exogenous, thus invalidating them as instruments for schooling. Instead, the results indicate that this is not a problem in the data, showing that the laws were not endogenous during this period.[11]

[10] Such effects were first documented for men in Acemoglu and Angrist (2000). The effects for women hereby reported are qualitatively and quantitatively comparable.

[11] It is interesting to note that, up to 12th grade, the laws have a significant positive effect on schooling above required levels. Possible explanations include peer effects, educational sorting (Lang and Kropp (1986)), or the fact that educational decisions are "lumpy" (Acemoglu and Angrist (2000)). On the other hand, the appearance of quantitatively small, but statistically significant *negative* effects of compulsory schooling laws on higher levels of schooling might reflect shifts in state resources away from local colleges and to high schools concurrently with the enactment of laws requiring additional years of schooling.

The different columns in Table 6, which report estimated coefficients from the first-stage regression for subsamples of the data, show that the impact of the child labor and compulsory attendance laws dummies remain significant after excluding each Census year, therefore ensuring that the effectiveness of compulsory schooling laws is consistent over time and across cohorts in the sample. Overall, the evidence seems to support the validity of these laws as instruments for schooling.

D. IV Estimates and Interpretation

The 2SLS estimates of equation (2-1) are reported in Table 7. Controlling for state of residence, state of birth, year, and year of birth main effects, and using compulsory schooling laws as instruments generates an estimate of the effect of education on total completed fertility of -0.330 (with a standard error of .061) when dummies for both child labor and compulsory attendance regulations are employed.[12] This is still negative, and significantly larger in magnitude than the OLS estimate (the 95 percent confidence interval for this coefficient is [-0.445,-0.210] and comfortably excludes the OLS estimate of -0.131), and indicates that, other things equal, having completed three additional years of schooling reduces the number of children ever born by one. Using compulsory attendance laws alone yields somewhat smaller (in absolute value) estimates, although less precise and not significantly different from those including child labor laws or both types of laws in the set of instruments.

In order to further analyze the causal effect of education on fertility, and given that Table 2 seems to suggest that the impact of schooling on fertility may be greatest at high school completion, I estimate a model of fertility using an indicator for high school graduation rather than total years of schooling. This will answer the question: how does a change in the fraction of women graduating from high school affect fertility? The answer to such question should be interesting also because fertility declines have been associated historically with education expansions that increased female rates of enrollment in, and of completion of, secondary education. Arguably, these are also the most relevant levels of schooling for policy intervention.

[12] All standard errors reported in this paper are corrected for state-of-birth/year clustering.

The first two columns in Table 8 report OLS estimates of a model where the regressor of interest is a dummy for having graduated from high school. The change in fertility, net of state, year and cohort effects, associated with completion of secondary school is -0.63. This is fairly consistent with the previous estimates for the linear model in schooling, given that high school graduates have on average more than just one extra year of schooling than high school dropouts. Next I move to IV estimation. Since Table 4 documented that child labor and compulsory attendance laws did induce an increase in the fraction of women graduating from high school, the same identification strategy remains valid in this case as in the model linear in years of schooling. The estimated effect of high school graduation on fertility using the instruments is close to -1. The most precisely estimated, which uses both sets of compulsory schooling laws, is -0.9. This implies that completing secondary education leads women to having, on average, approximately one less child. This supports the notion that the effect of schooling on fertility is likely to be largest at twelfth grade, and reinforces the finding that estimates that account for endogeneity show a sizable negative effect of education on fertility.

The difference between OLS and 2SLS estimates of the fertility return to education appears to suggest that OLS understates the magnitude of the causal relationship of interest. In previous sections, I pointed at measurement error in schooling, and at unobserved differences in access to economic opportunities encouraging both education and fertility, as possible explanations for a positive bias in OLS estimates. Measurement error is probably not a strong candidate in this case, however, since the results in Table 3 showed that pulling 1990 Census data from the sample does not affect the OLS estimates. If attenuation bias were responsible for the discrepancy between the OLS and 2SLS coefficients, and given that the measure of schooling is noisier in 1990, then one would expect the OLS estimate from a sample that excludes that year to be significantly closer to the 2SLS estimate than the regular OLS from using all of the data. That does not appear to be the case, however, which indicates that a positive correlation between schooling and fertility induced by some unobserved factor such as access to credit is likely to have caused the OLS estimate to be biased upwards.[13]

[13] Similarly, if more educated women tend to marry relatively more educated, wealthier men, and demand for the sheer number of children decreases with income, then women with high levels of schooling are less

Another plausible reason for the 2SLS estimates to differ from their OLS counterparts is that the causal effect of an additional year of schooling on fertility might not be constant at all levels of education or across all individuals. The discussion thus far involved a homogeneous-effects, linear model of the effect of education, but that is unlikely to be the case in reality. Even in the absence of any endogeneity in education, IV and OLS estimates of the fertility return to schooling will typically differ because each is generated by variation for a particular group of individuals over a limited range of variation in the explanatory variable. More precisely, the reason for this disparity lies in the difference between the weighing function underlying each estimator. IV estimates reflect a weighted average of causal responses to each single-year change in completed schooling, with the weights depending on the fraction of individuals who are induced to make each transition by the compulsory schooling laws used as instruments, while OLS estimates weigh individuals in proportion to their contribution to the total variation in schooling, irrespective of the instruments.[14] In the presence of non-linearity and/or individual heterogeneity in the effect of schooling on fertility, the IV will almost surely differ from the OLS estimate.

The OLS weighting function for each value of s_i is illustrated in Figure 4, along with the histogram of schooling. Not surprisingly, given the distribution of schooling, the OLS weighting scheme places the most influence on values between 8 and 16. While 38% of the sample has 12 years of schooling, however, OLS gives more weight than the histogram to other schooling values like 9, 10, 11, where (as Figure 3 shows) the fertility return to schooling is relatively smaller (less negative); or 14 and 15, where (again, according to Figure 3), it is zero or even positive. This is the reason why the OLS estimate (-0.128) is considerably less negative than the population average of the covariate-adjusted fertility return to schooling (-0.187).[15]

likely to show higher fertility rates on average, thus creating a negative spurious correlation between these two variables. To the extent that compulsory schooling laws do not alter matching decisions in the marriage market, then, 2SLS estimates are more negative than OLS because the instruments eliminate this additional source of positive endogeneity bias.

[14] In other words, IV uses only the variation in schooling that is correlated with the instrument. See Angrist and Imbens (1995) for more details.

[15] This is obtained as the weighted average of the covariate-adjusted difference in average fertility at each schooling increment, using the relative frequencies from the distribution of schooling in the sample as weights. See Angrist and Krueger (1999).

Figure 5a plots the OLS and 2SLS weights for the case where child labor laws as used as instruments, as well as the difference between the two. Figure 5b does the same for compulsory attendance laws. These weights are computed using the formulae derived in Lochner and Moretti (2004). As expected given the results shown in Table 5 (indicating that the instruments induce changes in schooling at the secondary education level, but not at post-high school levels), the 2SLS weights are larger than the OLS weights for levels of education corresponding to high school, and comparable if not lower for levels below or beyond high school. To the extent that the greatest impact of schooling on fertility decisions is associated with changes at the secondary level of education, one should expect the 2SLS estimates to be larger than OLS estimates. In fact, using the 2SLS weights to re-weight the observed fertility responses to each additional year of schooling (i.e.: the year-by-year changes in regression-adjusted fertility means net of state, year and cohort effects) produces an estimate that is slightly larger (more negative) than the OLS estimate and closer to the 2SLS estimate, although not by much. Using the 2SLS weights to re-weight the changes in the regression-adjusted average number of children by years of schooling (the estimates of the fertility-schooling conditional expectation function shown in Figure 3) produces an estimate of -0.151 or -0.146 (for the case where child labor laws or compulsory attendance laws are used, respectively), compared with -0.128 from using the OLS weights. This suggests that only a small part of the reason why 2SLS are more negative than OLS estimates lies in differences in the fertility return to schooling across levels of education.

The fact that the heterogeneity in of fertility returns to schooling across education levels is small does not necessarily imply that there is no heterogeneity in the effect of schooling *across individuals*. If, for example, the women most affected by compulsory schooling laws happen to be those with the highest discount rates and/or with the lowest access to credit, who have a larger fertility return to schooling relative to the average individual in the distribution, then the IV estimator will be capturing the average marginal effect for those women, and will not be an estimate of the average effect in the population. An alternative approach for examining the relationship of interest for women with education levels between 8 and 12 years of schooling consists in using completed years of high school as the endogenous regressor. Unlike the model that uses a dummy for high school graduation, this method has the advantage that it is not vulnerable to the miscoded binary treatment problem (see Imbens and Agrist (1994)). Table 9 presents estimates of the effect of an additional year of high school on fertility. As shown in columns 1 and 2, the OLS estimates are now higher in value (around -0.22) and much closer to the IV. In particular, the coefficients obtained by including compulsory attendance laws in the set of instruments (also -0.22, and -0.26 when including all instruments), while comparable to those obtained in the model with total years of schooling, become no longer significantly different from the OLS estimates in columns 1 and 2 (even the IV estimates from using only child labor laws –columns 3 and 4– are not statistically distinguishable from the OLS estimates, although in this case this is just due to the loss in precision). This reconciliation of the OLS and IV coefficients in this specification is further evidence that the effect of education on fertility is largest for women who completed at least some high school education, which are the individuals most affected by the instruments. Indeed, given the evidence that suggests that compulsory schooling laws encouraged further schooling of women who would have otherwise dropped out from high school, the IV estimates are a better assessment than OLS estimates of the likely fertility outcomes of education expansions that increase female graduation rates from high school.

IV. Additional Results and Applications

A. The Impact of Schooling on Marriage and Childlessness

One of the possible ways for education to affect fertility is through marriage status. If schooling reduces the likelihood that a woman will marry, then naturally higher schooling levels will bring about reductions in the number of children. I explore this possibility by estimating models of the probability of having ever been married on years of schooling. Panel A of Table 10 reports OLS and 2SLS estimates of the impact of education on marriage status. While OLS shows a small, but statistically significant, negative coefficient, the 2SLS estimates show no systematic relationship and are all insignificant. Hence there is no evidence that schooling affects the probability that a woman will marry. The documented fertility-reducing effect of education is not due to more educated women being significantly more likely to remain single.

Since the negative impact of schooling on fertility does not seem to operate through marriage, it seems natural to ask whether schooling does affect the probability that a woman will remain childless until the end of her fertile years. Panel B of Table 10 shows estimates of models of the probability of not having had any children on years of schooling. In this case, both 2SLS and OLS regressions produce statistically and quantitatively significant coefficients. Using compulsory attendance laws yields an estimate of 0.18, which implies that an additional year of schooling raises the probability of not having any children by almost 2 percentage points. Other IV specifications produce somewhat smaller but not very precisely estimated, and hence not significantly different coefficients. Overall, this constitutes suggestive evidence that schooling may be reducing fertility partly by increasing the proportion of women who reach the end of their fertile lives without children, even though these women may still be about as likely to get married as their less educated counterparts.

B. An Application: The Role of Education Expansions in Fertility Declines

To put the above estimates of the impact of schooling on fertility into perspective, it is useful to look at several countries that experienced dramatic fertility declines and education expansions in the second half of the twentieth century, and compare the actual reductions in fertility with those implied by these estimates from the observed increases in education levels of women.

Although still high in many parts of the world, total fertility rates have been decreasing dramatically during the twentieth century, mostly after 1960.[16] As populations become more educated, the number of children per woman falls. In particular, the last forty years have seen widespread fertility declines accompanied by increases in educational attainment levels in most Western countries. While in 1960 the average woman in North America or in industrialized Europe would have 3.4 and 2.6 children

[16] The worldwide Total Fertility Rate fell from around 6 children per woman in 1900 to 2.7 in 2003. In 1960, this rate was 4.9. *Source*: Population Division of the Department of Economic and Social Affairs of the United Nations Secretariat, *World Population Prospects: The 2002 Revision* and *World Urbanization Prospects: The 2001 Revision*, http://esa.un.org/unpp

respectively, fertility rates in every developed nation are now below the replacement rate of 2.1, ranging from 2.0 in the United States to 1.6 in France and Canada, 1.4 in Japan, and just 1.2 in Italy.[17] In some cases where education expansion has been relatively more recent and initial levels were particularly low, the decline in fertility between 1960 and 2000 has been even more dramatic: it went from 2.9 to 1.1 in Spain, from 3.2 to 1.4 in Portugal, and from 3.8 to 1.9 in Ireland.

Table 11 reports data from six European countries that underwent significant reductions in fertility between 1960 and 1990. At the same time, average female education increased in all of them. It is natural to ask: to what extent did education expansion contribute to the decline in fertility in each one of those countries? Using the 2SLS estimates from Table 6 it is possible to provide an answer to that question. The predicted fall in fertility from the observed rise in female education is computed for each country and presented in columns (5) and (6), each using one of the extremes in the range of 2SLS estimates obtained from the different specifications in Table 7. The fact that these countries saw increases in education at or around the high school levels makes the exercise particularly meaningful if the 2SLS estimates are capturing the marginal effect of education on fertility for women at those education levels.

The estimated effects of schooling on fertility imply, for example, that between 21% and 28% of the fertility drop in Italy between 1960 and 1990 can be explained by the increase in education experienced by its female population during that period. In general, about a quarter of the fertility decline in Germany, Italy and Ireland, a third of the drop in Portugal and Spain, and as much as half of the fall in Greece can be attributed to the effect of rising female education.

V. Concluding Remarks

There are a number of reasons to expect that education reduces fertility. By raising wages, education increases the opportunity cost of having children and spending time

[17] According to the latest *Population Bulletin* of the Population Reference Bureau (March 2004), as of 2003 all industrialized countries have fertility rates below 2.1 children per woman, the level needed to ensure the long-term replacement of the population. Moreover, the United Nations projected in 2002 that fertility levels will likely fall below replacement in three out of four developing countries by 2050 (UN Press Release POP/850).

away from work. Education may also make women more aware of methods of birth control, and more accepting of alternative lifestyles that do not necessarily include marrying early and having children. It is also possible that more educated women enjoy higher husband's earnings, if there is assertive matching, and that may encourage demand for children. Empirical evidence on the effect of education on fertility has implications for welfare and fiscal policy and is also of interest for economic theory.

In order to identify the magnitude of the relationship between schooling and total completed fertility, this paper uses changes in state compulsory attendance and child labor laws over time to generate exogenous variation in schooling of women in an extended sample from the US Census that includes 1950 through 1990 data. The finding that three additional years of schooling result in one less children per woman on average is consistent and robust to a number of specification checks. Instrumental variables estimates also suggest that most of this effect is not channeled through lower marriage rates. Educated women are not less likely to marry, however they are more likely to reach the end of their fertile lifecycle without having any children, which is consistent with the hypothesis that education delays marriage and family formation.

I further argue that the estimated impact of schooling on the decision to have children can shed some light on the dramatic demographic changes experienced in the last few decades by countries with large expansions in education. As much as a third or more of the fertility decline observed in several Western countries such as Spain or Ireland can be attributed to a rise in their female education levels. This suggests that the fertility-reducing impact of schooling, while constituting an external benefit in less developed countries with high population densities and growth rates, can be regarded as an external cost in more advanced societies where the fertility rate is already significantly below replacement, the dependency rates of workers to pension recipients is already low, and the expected contribution of the average young person to the public budget is clearly positive, as is currently the case in most of the industrialized world. In those countries, the schooling effects on fertility represent a negative external effect of education that should contribute to better understand the overall social impacts of human capital accumulation.

Appendix

A. Data Appendix

This study uses data from the 1950 General (1/330 sample), 1960 General (1% sample), 1970 Form 1 State and Form 2 State (both 1% samples), 1980 5% State A (a 5% sample), and 1990 1% unweighted (a 1% random self-weighted sample created by IPUMS) Census IPUMS files. See Ruggles and Sobek (1997) for more details on the IPUMS system.

The extracts include all US-born (except Alaska and Hawaii) white women aged 40-49 at the time of the Census survey. The 1950 sample is limited to "sample line" individuals (this is, those with long-form responses), the only for whom information on children born is available. The schooling variable used for 1950 through 1980 is the Census extracts variable HIGRADE (general), the IPUMS recode of the highest grade completed. The 1990 Census only reports schooling in broader categories; therefore it is not directly comparable with the information from previous surveys. Years of schooling in that year were assigned from group means for white women in each category reported in Park (1994, Table 5), who uses a one-time overlap questionnaire from the February 1990 Current Population Survey to construct averages for the categories found in the 1990 Census. Finally, in order to ensure consistency of the schooling measure across all five Census years, the resulting variable was capped at 17, the highest grade completed available in the 1950 Census.

The compulsory attendance laws and child labor laws in effect in each of the 50 contiguous states in the years 1914-1964 were assigned to all individuals in the sample on the basis of their state of birth and the year in which they turned 14 (which is calculated from year of birth, estimated using age on Census day). More details on the data sources for these laws are given in Appendix B of Acemoglu and Angrist (2000). Baseline regressions that use compulsory schooling laws matched by age at 16 produced qualitatively similar results.

B. Mathematical Appendix

This appendix clarifies the concepts behind the OLS and 2SLS weighting functions used in Section III for the regression of total completed fertility on years of schooling. Consider the model in equation (1) where all covariates have been partialed out and the fertility return to schooling is assumed to vary with the level of schooling $s_i \in \{0, 17\}$, but not across individuals with the same education level. Equation (1) can then be rewritten as:

$$y_i = \sum_{j=0}^{17} \beta_j + \eta_i , \qquad \text{(A1)}$$

where η_i is a mean-zero, individual-specific error term (that results from projecting ε_i onto the space spanned by the covariates in equation (1)). Both the OLS and 2SLS estimates of equation (1) can be written as weighted averages of the causal responses to each unit increase in schooling, the set of β_j's, as shown in Lochner and Moretti (2004). The weights for the OLS regression are:

$$\omega_j^{OLS} = \frac{prob(s_i \geq j) \cdot \{E[s_i | s_i \geq j] - E[s_i]\}}{E[s_i^2]} \quad . \qquad \text{(A2)}$$

The weighting function for the 2SLS regression that uses a set of three indicator variables as instruments (CL7 through CL9, where CL7 is a dummy for child labor laws requiring 7 years of schooling before being allowed to work, and so on, and the omitted category is CL6) is:

$$\omega_j^{2SLS} = \frac{\sum_{k=7}^{9} prob(CL=7) \cdot \{E[s_i | CL=k] - E[s_i]\} \cdot [prob(s_i \geq j | CL=k) - prob(s_i \geq j | CL=6)]}{\sum_{k=7}^{9} prob(CL=7) \cdot \{E[s_i | CL=k] - E[s_i]\} \cdot \{E[s_i | CL=k] - E[s_i | CL=6]\}} \quad . \qquad \text{(A3)}$$

Given that the instruments satisfy the monotonicity assumption of Angrist and Imbens (1995), the last factor in each term in the numerator can be rewritten as $prob(s_{CLk} \geq j \geq s_{CL6})$, where s_{CLk} is the schooling choice of individuals exposed to the instrument CL*k*. An analogous formula to (A3) can be derived for the case where compulsory attendance laws (CA) are used as instruments.

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***Figure 1* : Total Completed Fertility, by education and cohort: 1885-1954**

Source: Author's calculations from 1950-1990 Census IPUMS data

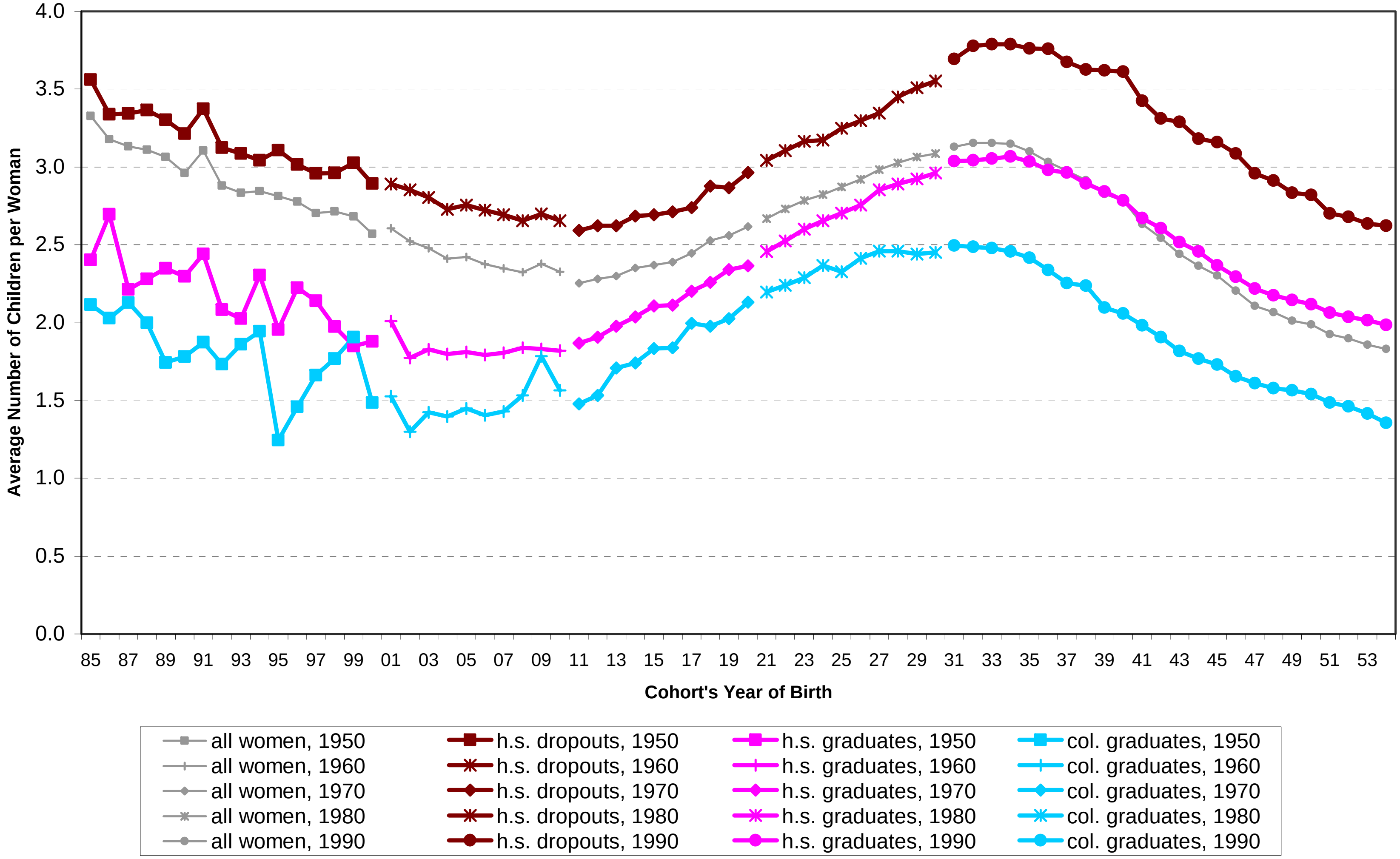

Figure 2a **: Age-Specific Fertility Rates, by Education: 1960**
Source: Author's calculations from 1960 Census IPUMS

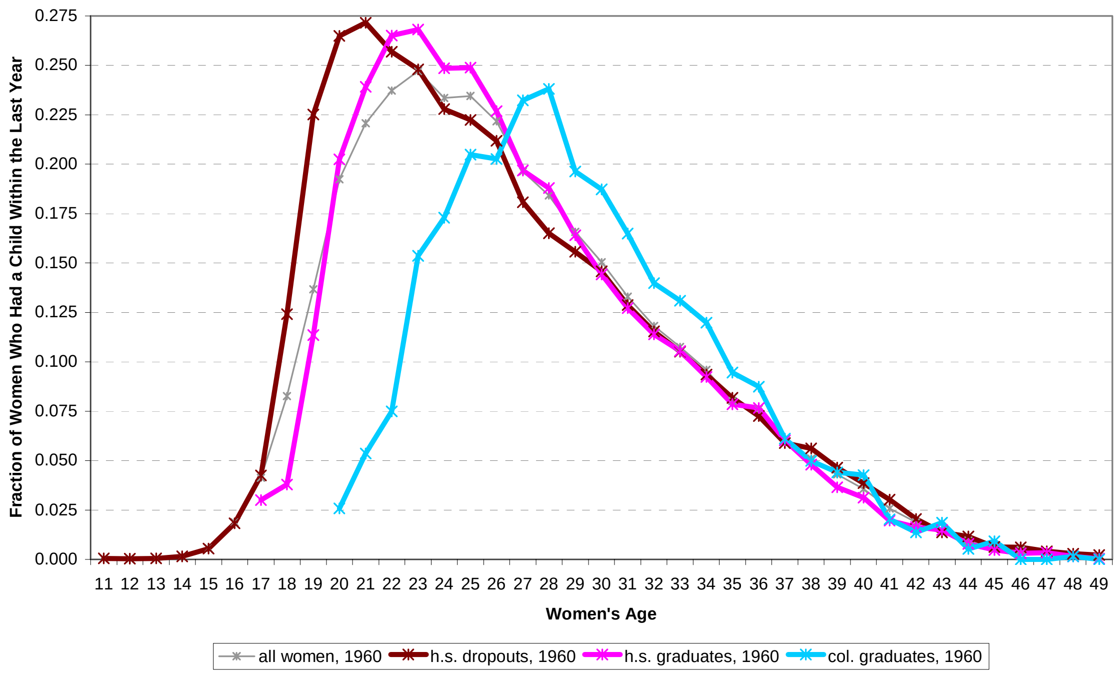

Figure 2b **: Age-Specific Fertility Rates, by Education: 1990**
Source: Author's calculations from 1990 Census IPUMS

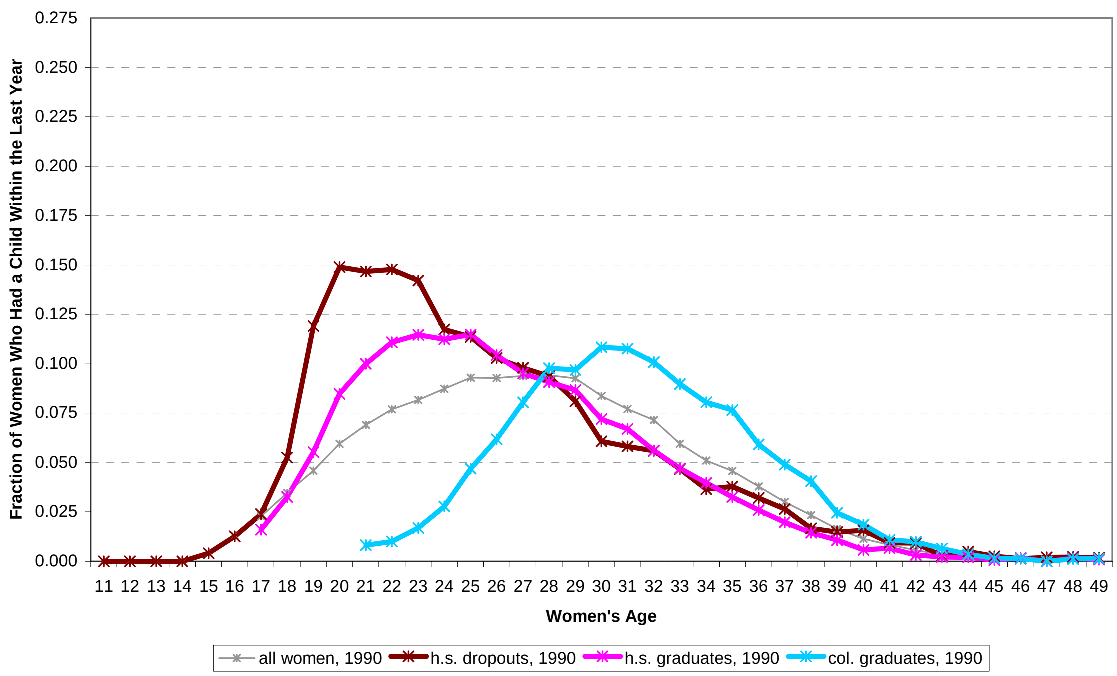

Figure 3: **Regression-Adjusted Total Completed Fertility, by Years of Schooling**

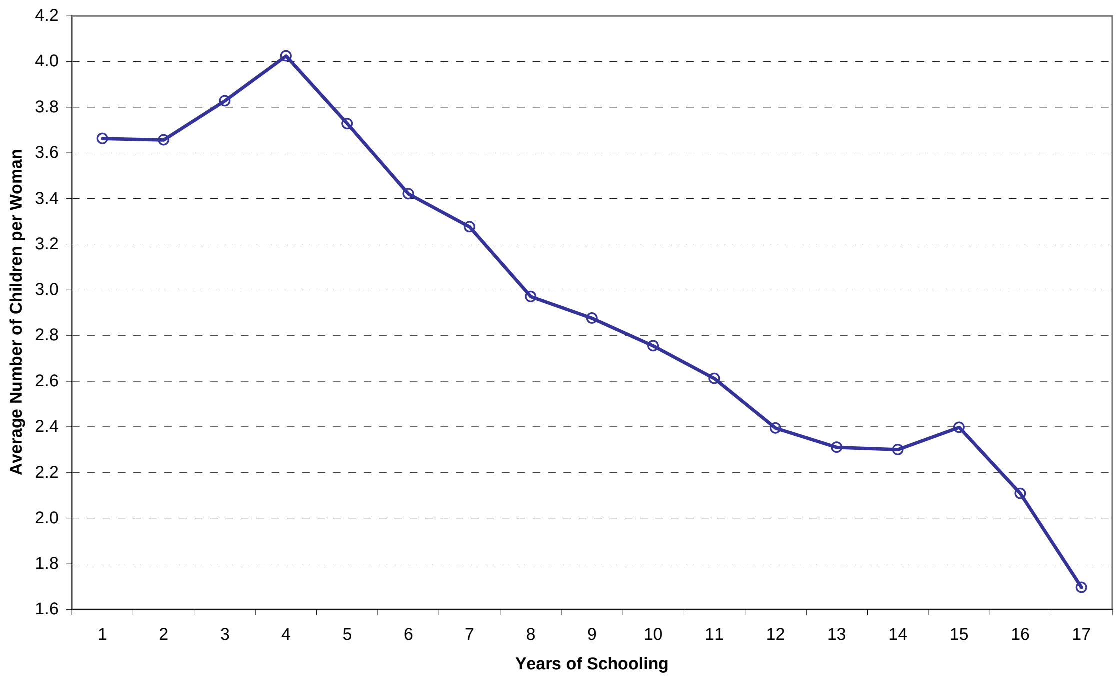

Figure 4: **Schooling Histogram and OLS Weights**

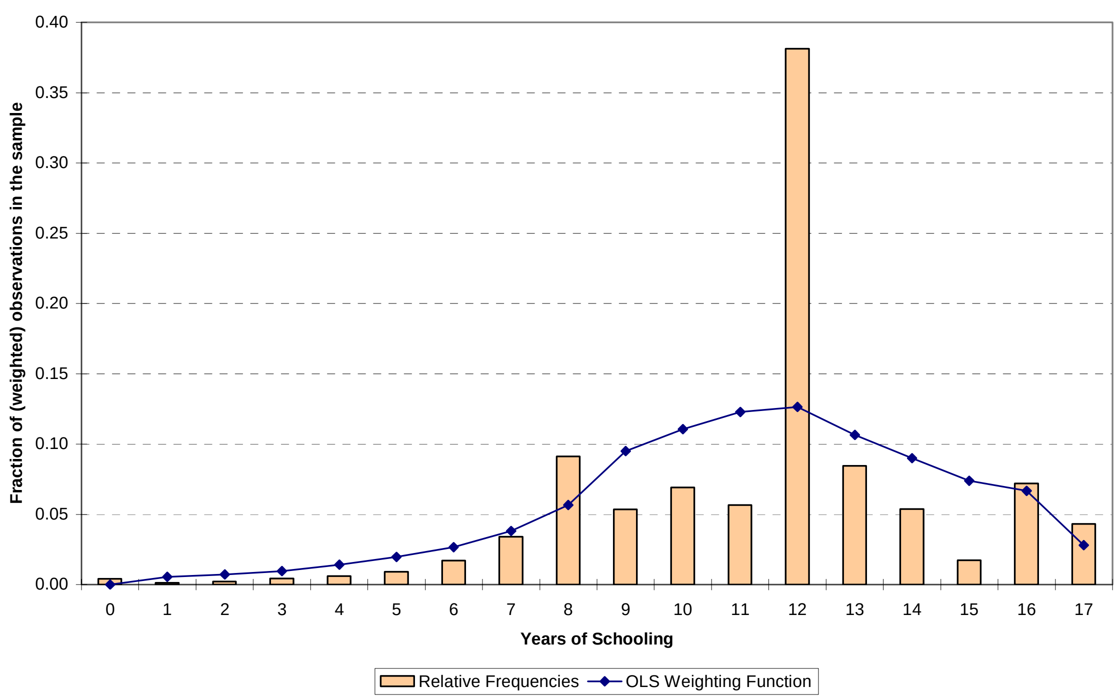

Figure 5a **: 2SLS and OLS Weighting Functions**
Instruments: Child Labor Laws

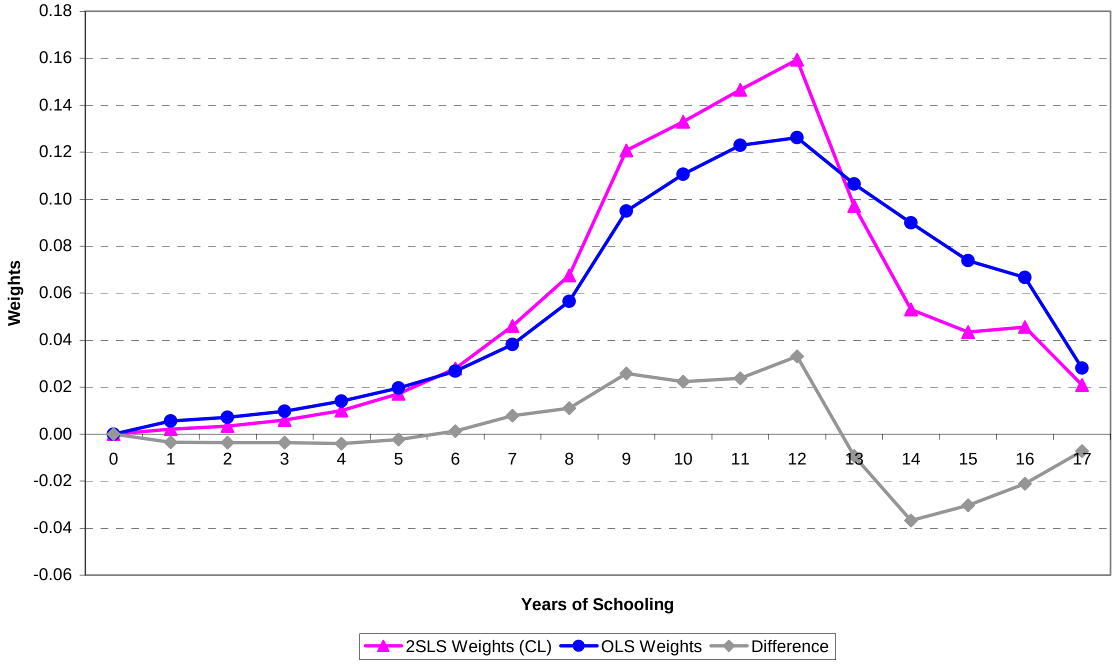

Figure 5b **: 2SLS and OLS Weighting Functions**
Instruments: Compulsory Attendance Laws

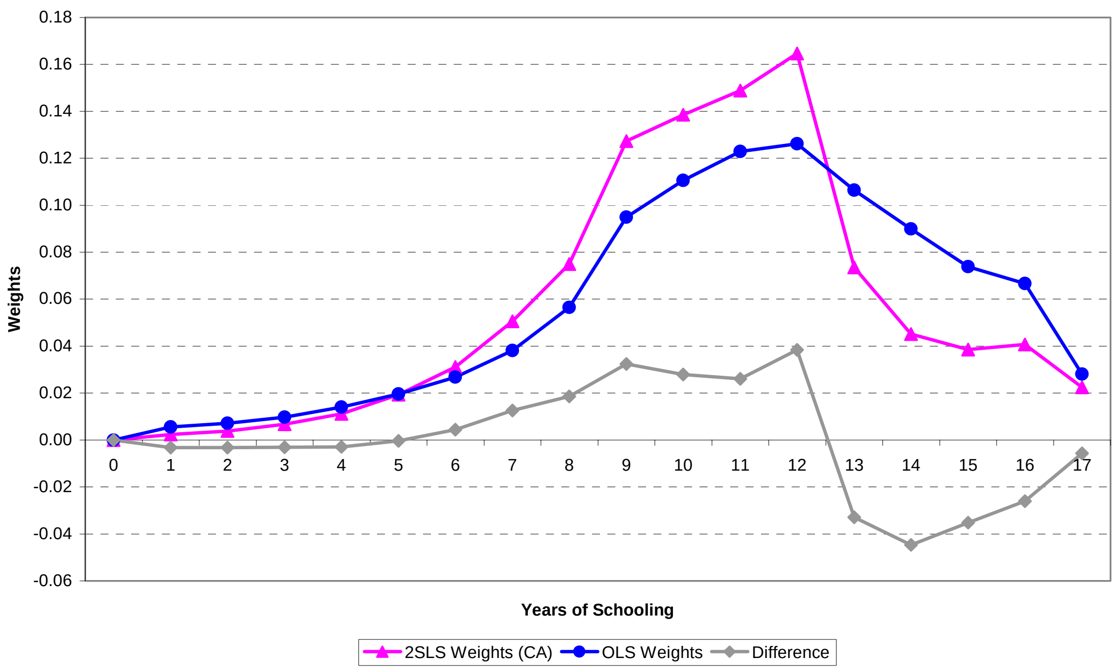

Table 1
Descriptive Statistics for the Census IPUMS extraction

Variables	1950-90	1950	1960
	Dependent Variable		
#Children Ever Born (Completed Fertility)	2.53 (1.95)	2.21 (2.26)	2.31 (2.00)
Childless	.154 (.361)	.252 (.434)	.194 (.396)
Ever Married	.948 (.222)	.928 (.258)	.944 (.230)
	Regressor		
Years of Schooling	11.56 (2.89)	9.96 (3.24)	10.61 (2.89)
	Covariates		
Age	44.37 (2.87)	44.23 (2.89)	44.33 (2.85)
	Instruments		
Percent Child Labor 6 or less	.1801	.4805	.2267
Percent Child Labor 7	.2753	.4498	.3731
Percent Child Labor 8	.3759	.0698	.3487
Percent Child Labor 9+	.1686	.0000	.0515
Percent Compulsory Attendance 8 or less	.2642	.6066	.3507
Percent Compulsory Attendance 9	.4445	.3753	.5225
Percent Compulsory Attendance 10	.0650	.0181	.0605
Percent Compulsory Attendance 11+	.2263	.0000	.0663
N	888,420	23,315	93,743

NOTE: The data are from the Census IPUMS for 1950 through 1990, with the sample restricted to white women born in the US (except Alaska and Hawaii) and aged 40-49 in the Census Year. Standard deviations are in parentheses. All other entries are means.

Table 1 (continued)
Descriptive Statistics for the Census IPUMS extraction

Variables	1970	1980	1990
		Dependent Variable	
#Children Ever Born (Completed Fertility)	2.80 (2.02)	2.94 (1.86)	2.16 (1.45)
Childless	.125 (.330)	.101 (.302)	.148 (.355)
Ever Married	.985 (.208)	.960 (.195)	.944 (.230)
		Regressor	
Years of Schooling	11.38 (2.65)	12.10 (2.51)	13.19 (2.34)
		Covariates	
Age	44.55 (2.86)	44.46 (2.91)	44.13 (2.85)
		Instruments	
Percent Child Labor 6 or less	.1942	.0533	.0295
Percent Child Labor 7	.2445	.2408	.1667
Percent Child Labor 8	.4956	.4141	.3687
Percent Child Labor 9+	.0657	.2917	.4351
Percent Compulsory Attendance 8 or less	.2524	.1190	.1115
Percent Compulsory Attendance 9	.4403	.4374	.4399
Percent Compulsory Attendance 10	.0783	.0894	.0585
Percent Compulsory Attendance 11+	.2289	.3542	.3902
N	194,279	454,712	122,371

Table 2

Total Completed Fertility by Educational Attainment

	1950-90	1950	1960	1970	1980	1990
All women	2.53	2.21	2.31	2.80	2.94	2.16
College graduates	1.93	1.28	1.77	2.39	2.29	1.67
Some College	2.31	1.68	1.99	2.69	2.78	2.10
High School Graduates	2.48	1.69	2.06	2.67	2.90	2.29
High School Dropouts	2.89	2.60	2.62	3.09	3.45	2.82

NOTE: The data are from the Census IPUMS for 1950 through 1990, with the sample restricted to white women born in the US (except Alaska and Hawaii) and aged 40-49 in the Census Year. All entries are means. ‘College Graduates’ is defined as having completed 16 or more years of schooling; ‘Some College’ as having completed more than 12 but less than 16 years of schooling; ‘High School Graduates’ as having completed exactly 12 years of schooling, and ‘High School Dropouts’ as having completed less than 12 years of schooling.

Table 3
OLS Estimates of the Effect of Schooling on Total Completed Fertility

	1950-90 (1)	1950-90 (2)	1950-80 (3)	1950 (4)	1960 (5)	1970 (6)	1980 (7)	1990 (8)
Years of Schooling	-.131 (.002)	-.128 (.002)	-.131 (.002)	-.166 (.006)	-.133 (.005)	-.104 (.005)	-.138 (.003)	-.131 (.003)
State of Residence Main Effects	No	Yes	Yes	No	No	No	No	No
R-squared	.074	.078	.068	.086	.055	.034	.048	.076
N	888,420	888,420	766,049	23,315	93,743	194,279	454,712	122,371

NOTE: The data are from the Census IPUMS for 1950 through 1990, with the sample restricted to women aged 40-49 in the Census Year. Standard errors corrected for state-year clustering are shown in parentheses. Entries are estimates of the effect of years of schooling on the measure of Total Completed Fertility, namely the discrete choice variable ‘Children Ever Born’. All regressions contain Census year, year of birth and state of birth main effects.

Table 4

Compulsory Schooling Laws as Instruments for Years of Schooling: First-Stage Estimates

Variables	(1)	(2)	(3)	(4)	(5)	(6)
			Child Labor Laws			
CL7 (Percent Child Labor 7)	.175 (.028)	.184 (.027)			.155 (.030)	.164 (.030)
CL8 (Percent Child Labor 8)	.163 (.027)	.174 (.026)			.137 (.029)	.148 (.029)
CL9 (Percent Child Labor 9+)	.386 (.034)	.385 (.034)			.336 (.037)	.340 (.037)
			Compulsory Attendance Laws			
CA9 (Percent Compulsory Attendance 9)			.079 (.023)	.092 (.023)	.024 (.023)	.034 (.023)
CA10 (Percent Compulsory Attendance 10)			.126 (.031)	.118 (.031)	.094 (.033)	.086 (.033)
CA11 (Percent Compulsory Attendance 11+)			.222 (.029)	.208 (.028)	.115 (.031)	.099 (.031)
State of Residence Main Effects	No	Yes	No	Yes	No	Yes
F-statistic (p-value)	47.02 (.000)	45.29 (.000)	20.61 (.000)	18.09 (.000)	27.52 (.000)	25.43 (.000)
R-squared	.171	.178	.171	.177	.171	.178

NOTE: The data are from the Census IPUMS for 1950 through 1990, with the sample restricted to white women born in the US (except Alaska and Hawaii) and aged 40-49 in the Census Year. Standard errors corrected for state-of-birth/year-of-birth clustering are shown in parentheses. All regressions contain Census year, year of birth and state of birth main effects. The sample size is 888,420.

Table 5
Effects of Compulsory Schooling Laws on Discrete Levels of Schooling

Variables	Completed 8+ Years of Schooling (1)	Completed 10+ Years of Schooling (2)	Completed 12+ Years of Schooling (3)	Completed 14+ Years of Schooling (4)	Completed 16+ Years of Schooling (5)
Dependent Variable Mean	.922	.777	.652	.186	.115
			A. Child Labor Laws		
CL7 (Percent Child Labor 7)	.031 (.004)	.023 (.004)	.015 (.005)	.004 (.003)	.002 (.002)
CL8 (Percent Child Labor 8)	.037 (.004)	.025 (.004)	.026 (.005)	-.008 (.004)	-.010 (.003)
CL9 (Percent Child Labor 9+)	.064 (.005)	.057 (.005)	.057 (.006)	-.005 (.005)	-.006 (.004)
F-statistic (p-value)	60.02 (.0000)	44.28 (.0000)	35.84 (.0000)	6.08 (.0004)	10.08 (.0001)
R-squared	.085	.135	.146	.048	.050
			B. Compulsory Attendance Laws		
CA9 (Percent Compulsory Attendance 9)	.029 (.003)	.012 (.003)	.019 (.004)	-.009 (.003)	-.009 (.002)
CA10 (Percent Compulsory Attendance 10)	.018 (.004)	.024 (.006)	.039 (.006)	-.005 (.003)	-.007 (.002)
CA11 (Percent Compulsory Attendance 11+)	.025 (.004)	.044 (.006)	.051 (.005)	-.003 (.004)	-.007 (.003)
F-statistic (p-value)	27.48 (.0000)	26.04 (.0000)	34.81 (.0000)	4.32 (.0048)	6.04 (.0004)
R-squared	.084	.135	.146	.048	.050

NOTE: The data are from the Census IPUMS for 1950 through 1990, with the sample restricted to white women born in the US (except Alaska and Hawaii) and aged 40-49 in the Census Year. Standard errors corrected for state-of-birth/year-of-birth clustering are shown in parentheses. All regressions contain Census year, year of birth, state of birth and state of residence main effects. The sample size is 888,420.

Table 6

Effects of Compulsory Schooling Laws on Years of Schooling: Robustness Checks

Variables	Excluding 1950 (1)	Excluding 1960 (2)	Excluding 1970 (3)	Excluding 1980 (4)	Excluding 1990 (5)
			A. Child Labor Laws		
CL7	.133	.203	.239	.180	.153
(Percent Child Labor 7)	(.025)	(.033)	(.033)	(.030)	(.029)
CL8	.154	.181	.204	.191	.121
(Percent Child Labor 8)	(.023)	(.030)	(.036)	(.031)	(.029)
CL9	.344	.374	.473	.425	.259
(Percent Child Labor 9+)	(.031)	(.037)	(.041)	(.043)	(.040)
F-statistic	46.28	37.10	51.50	34.30	15.54
(p-value)	(.000)	(.000)	(.000)	(.000)	(.000)
R-squared	.156	.181	.224	.189	.113
			B. Compulsory Attendance Laws		
CA9	.107	.097	.148	.087	.027
(Percent Compulsory Attendance 9)	(.021)	(.026)	(.029)	(.026)	(.025)
CA10	.155	.064	.114	.130	.103
(Percent Compulsory Attendance 10)	(.026)	(.035)	(.045)	(.036)	(.036)
CA11	.229	.180	.307	.206	.133
(Percent Compulsory Attendance 11+)	(.025)	(.031)	(.039)	(.032)	(.033)
F-statistic	27.71	13.43	21.54	13.61	6.78
(p-value)	(.000)	(.000)	(.000)	(.000)	(.000)
R-squared	.156	.180	.223	.188	.113
N	865,105	794,677	694,141	433,708	766,049

NOTE: The data are from the Census IPUMS for 1950 through 1990, with the sample restricted to white women born in the US (except Alaska and Hawaii) and aged 40-49 in the Census Year. Standard errors corrected for state-of-birth/year-of-birth clustering are shown in parentheses. All regressions contain Census year, year of birth and state of birth main effects.

Table 7
2SLS Estimates of the Effect of Schooling on Total Completed Fertility

	(1)	(2)	(3)	(4)	(5)	(6)
Instruments	CL	CL	CA	CA	CL & CA	CL & CA
Years of Schooling	-.327 (.065)	-.344 (.066)	-.264 (.085)	-.295 (.091)	-.302 (.058)	-.330 (.061)
State of Residence Main Effects	No	Yes	No	Yes	No	Yes
			First Stage for Schooling			
CL 7	.175 (.028)	.184 (.027)			.155 (.030)	.164 (.030)
CL 8	.163 (.027)	.174 (.026)			.137 (.029)	.148 (.029)
CL 9	.386 (.034)	.385 (.034)			.336 (.037)	.340 (.037)
CA 9			.079 (.023)	.092 (.023)	.024 (.023)	.034 (.023)
CA 10			.126 (.031)	.118 (.031)	.094 (.033)	.086 (.033)
CA 11			.222 (.029)	.208 (.028)	.115 (.031)	.099 (.031)

NOTE: The data are from the Census IPUMS for 1950 through 1990, with the sample restricted to women aged 40-49 in the Census Year. Standard errors corrected for state-year clustering are shown in parentheses. Entries are two-stage least squares estimates of the effect of years of schooling on the measure of Total Completed Fertility, namely the discrete choice variable 'Children Ever Born', using the excluded instruments indicated, i.e.: a set of dummies indicating state and year specific labor and school attendance laws that were in effect in the state of birth of the individual at age 14. All regressions contain Census year, year of birth and state of birth main effects. The sample size for all columns is 888,420.

Table 8
OLS and 2SLS Estimates of the Effect of High School Graduation on Total Completed Fertility

	OLS (1)	OLS (2)	2SLS (3)	2SLS (4)	2SLS (5)	2SLS (6)	2SLS (7)	2SLS (8)
Instruments			CL	CL	CA	CA	CL & CA	CL & CA
High School Graduate (Completed 12+ years of schooling)	-.635 (.013)	-.624 (.012)	-1.002 (.352)	-1.163 (.370)	-.943 (.346)	-.994 (.362)	-.897 (.269)	-.900 (.286)
State of Residence Main Effects	No	Yes	No	Yes	No	Yes	No	Yes
R-squared	.074	.078						
First Stage for High School Graduation								
CL 7			.017 (.005)	.015 (.005)			.010 (.005)	.007 (.005)
CL 8			.027 (.005)	.026 (.005)			.018 (.005)	.016 (.005)
CL 9			.056 (.006)	.057 (.006)			.041 (.006)	.040 (.006)
CA 9					.018 (.004)	.019 (.004)	.012 (.004)	.013 (.004)
CA 10					.035 (.006)	.039 (.006)	.033 (.006)	.035 (.006)
CA 11					.049 (.005)	.051 (.005)	.037 (.006)	.038 (.005)

NOTE: The data are from the Census IPUMS for 1950 through 1990, with the sample restricted to women aged 40-49 in the Census Year. Standard errors corrected for state-year clustering are shown in parentheses. Entries are estimates of the effect of high school graduation on of Total Completed Fertility, namely the discrete choice variable ‘Children Ever Born’. High school graduation is defined as a binary variable that equals one if the individual completed 12 or more years of schooling, and zero otherwise. Entries in the 2SLS columns are two-stage least squares estimates using the excluded instruments indicated, i.e.: a set of dummies indicating state and year specific labor and school attendance laws that were in effect in the state of birth of the individual at age 14. All regressions contain Census year, year of birth and state of birth main effects. The sample size for all columns is 888,420.

Table 9
OLS and 2SLS Estimates of the Effect of Years of High School on Total Completed Fertility

	OLS (1)	OLS (2)	2SLS (3)	2SLS (4)	2SLS (5)	2SLS (6)	2SLS (7)	2SLS (8)
Instruments			CL	CL	CA	CA	CL & CA	CL & CA
Years of High School	-.218 (.005)	-.214 (.004)	-.375 (.092)	-.387 (.093)	-.228 (.094)	-.212 (.097)	-.264 (.073)	-.265 (.075)
State of Residence Main Effects	No	Yes	No	Yes	No	Yes	No	Yes
R-squared	.068	.072						

NOTE: The data are from the Census IPUMS for 1950 through 1990, with the sample restricted to women aged 40-49 in the Census Year. Standard errors corrected for state-year clustering are shown in parentheses. Entries are estimates of the effect of completed years of high school on Total Completed Fertility, namely the discrete choice variable ‘Children Ever Born’. Entries in the 2SLS columns are two-stage least squares estimates using the excluded instruments indicated, i.e.: a set of dummies indicating state and year specific labor and school attendance laws that were in effect in the state of birth of the individual at age 14. All regressions contain Census year, year of birth and state of birth main effects. The sample size for all columns is 888,420.

Table 10

OLS and 2SLS Estimates of the Effect of Schooling on Marital Status and Childlessness

	OLS (1)	OLS (2)	2SLS (3)	2SLS (4)	2SLS (5)	2SLS (6)	2SLS (7)	2SLS (8)
				A. Probability of Marriage				
Instruments			CL	CL	CA	CA	CL & CA	CL & CA
Years of Schooling	-.0028 (.0002)	-.0028 (.0002)	.0058 (.0049)	.0059 (.0049)	.0011 (.0078)	-.0014 (.0079)	.0031 (.0046)	.0022 (.0047)
State of Residence Main Effects	No	Yes	No	Yes	No	Yes	No	Yes
R-squared	.009	.011						
				B. Childlessness				
Instruments			CL	CL	CA	CA	CL & CA	CL & CA
Years of Schooling	.0091 (.0002)	.0088 (.0002)	.0061 (.0060)	.0058 (.0061)	.0178 (.0076)	.0194 (.0078)	.0087 (.0050)	.0086 (.0052)
State of Residence Main Effects	No	Yes	No	Yes	No	Yes	No	Yes
R-squared	.027	.029						

NOTE: The data are from the Census IPUMS for 1950 through 1990, with the sample restricted to women aged 40-49 in the Census Year. Standard errors corrected for state-year clustering are shown in parentheses. Entries in Panel A are estimates of the effect of years of schooling on the probability of marriage, constructed as a binary variable that equals one if the women was ever married, and zero otherwise. Entries in Panel B are estimates of the effect of years of schooling on childlessness, defined as a binary variable that equals one if the women never had any children, and zero otherwise. Entries in the 2SLS columns are two-stage least squares estimates using the excluded instruments indicated, i.e.: a set of dummies indicating state and year specific labor and school attendance laws that were in effect in the state of birth of the individual at age 14. All regressions contain Census year, year of birth and state of birth main effects. The sample size for all columns is 888,420.

Table 11
Implied changes in fertility for selected EU countries, 1960-90

Country	Year	**Avge. Years of Schooling**	**Average Fertility Rate**	$yschool_{90}$-$yschool_{60}$	$fertility_{90}$-$fertility_{60}$	β*$\Delta fertility_{60-90}$		**% explained**	
						β=-.264	β=-.353	β=-.264	β=-.353
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Spain	1960	**4.16**	**2.9**						
Spain	1970	**4.31**	**2.9**						
Spain	1980	**4.75**	**2.2**						
Spain	1990	**6.05**	**1.3**	1.74	-1.6	-0.459	-0.614	**28.71**	**38.39**
Portugal	1960	**1.53**	**3.2**						
Portugal	1970	**1.92**	**3.0**						
Portugal	1980	**2.84**	**2.2**						
Portugal	1990	**3.26**	**1.6**	1.34	-1.4	-0.354	-0.473	**25.27**	**33.79**
Ireland	1960	**6.67**	**3.8**						
Ireland	1970	**6.61**	**3.9**						
Ireland	1980	**7.65**	**3.3**						
Ireland	1990	**8.13**	**2.1**	1.52	-1.8	-0.401	-0.537	**22.29**	**29.81**
Greece	1960	**3.51**	**2.3**						
Greece	1970	**4.43**	**2.4**						
Greece	1980	**5.79**	**2.2**						
Greece	1990	**6.36**	**1.4**	1.93	-1.0	-0.510	-0.681	**50.95**	**68.13**
Germany	1960	**7.76**	**2.4**						
Germany	1970	**8.03**	**2.0**						
Germany	1980	**8.28**	**1.6**						
Germany	1990	**8.45**	**1.5**	0.42	-0.5	-0.111	-0.148	**22.18**	**29.65**
Italy	1960	**4.2**	**2.4**						
Italy	1970	**4.79**	**2.4**						
Italy	1980	**4.77**	**1.6**						
Italy	1990	**5.67**	**1.3**	0.88	-1.1	-0.232	-0.311	**21.12**	**28.24**

NOTE: The fertility data come from Eurostat, as reported in E. Phillip Davis 'Population Aging and Retirement Income Provision in the European Union' (1998). The education data come from the Barro-Lee dataset ['see Barro, Robert and J.W. Lee, "International Measures of Schooling Years and Schooling Quality, AER, Papers and Proceedings, 86(2), pp. 218-223] which includes estimates of average schooling years in the female population aged 25+ for 126 countries in the world.