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ABSTRACT

Causal Effects of Education on Marriage

Many nations have experienced both rising education levels and declining marriage rates. However, cross-sectional comparisons within countries often indicate that more highly educated individuals are more likely to be married. Economic theory suggests ambiguous causal effects of education on marriage. This study uses a novel instrumental variable approach and finds that education decreases the probability of being married for younger persons but not for older persons. However, education increases the probability of never marrying even by ages 45-54. Education also reduces the likelihood of being divorced or separated, which partially offsets effects on being never married in overall marriage rates.

JEL Classification: I20, J12, J24
Keywords: marriage, education, human capital, instrumental variables

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1. Introduction

Economic development, demographic transition, and related factors have led to rising education levels and falling marriage rates in many countries around the world. A more inclusive economy for women opens up employment opportunities, increases investments in human capital, and delays marriage (Goldin 2006). Access to formal education appears to play an especially important role in these trends. However, education does not necessarily have a negative causal effect on marriage. In fact, recent cross-sectional comparisons suggest that more highly educated individuals are more likely to be married than their less educated counterparts (Kalmijn 2013; Lundberg et al. 2016). Thus, time series and cross-sectional data are often at odds. Furthermore, economic theory suggests that causal effects of education on marriage are ambiguous and likely heterogeneous. For example, education may make an individual a more attractive marriage partner, but it may also increase their own independence and reduce their own desire to get married. Highly educated individuals can also better afford to be more patient and more selective in choosing a marriage partner that is a good match. Additionally, the time spent investing in education and career may have a direct causal effect on delaying marriage without reducing the likelihood of eventual marriage. Effects of education on marriage may also vary by gender. Despite the topic's importance, there is very little previous research attempting to estimate causal effects of education on marriage outcomes. We seek to fill this gap.

We combine individual-level data from the pooled 2006-2019 American Community Survey (ACS) with cohort-level data from the 1980 and 1990 decennial census long form microdata samples to estimate causal effects of education on marriage outcomes. More specifically, we use the 1980 and 1990 data to construct cohort-level means for maternal education and other cohort parental characteristics. We define cohorts based on state of birth,

year of birth, and ancestry group. We then merge the census cohort-level data with the ACS data and estimate two-stage least squares (2SLS) regressions that instrument for individual education with the cohort mean maternal education level.[1] We include numerous controls including fixed effects for state-of-birth interacted with year of birth and ancestry group interacted with year of birth. Thus, we are exploiting variation across ancestry groups born within the same state and year, while controlling for national-level differences among ancestry groups over time.

We have multiple important results. First, formal education significantly decreases the probability of being married during ages 25-34 but does not have a significant effect on the probability of being married during ages 45-54. Thus, education appears to delay marriage. However, marriage is a dynamic process that involves first marriages, possible dissolution, and possible remarriage. Education significantly increases the probability of never getting married even through ages 45-54. At the same time, education decreases the probability of being divorced. Thus, for ages 45-54 education significantly reduces the probability of ever being married but not the probability of being currently married because those who do ever marry are more likely to remain married. Additional analysis suggests that education increases the likelihood of marrying a college-educated educated spouse.

Previous literature has suggested that a positive association is emerging between education and marriage (White and Rogers 2000; Goldstein and Kenney 2001; Cherlin 2009; Torr 2011). However, this literature is mostly descriptive and does not address potential endogeneity. To our knowledge, Lefgren and McIntyre (2006) is the only other study using

[1] We chose 2SLS over an IV probit model because it does not require the restrictive assumption of a normally distributed error term, making it more robust under broader conditions. The continuous nature of the endogenous variable also aligns naturally with the linear framework of 2SLS. Moreover, 2SLS provides coefficients that are directly interpretable as linear marginal effects, allowing for clearer interpretation of the results. Also, when large numbers of fixed effects are included as in our case, probit models often have difficulty converging and are prone to bias from incidental parameters problems.

instrumental variables to attempt to estimate the causal effect of education on the probability of marriage. Lefgren and McIntyre (2006) use quarter of birth as an instrument and find that women's education has no effect on the probability of marriage. However, quarter of birth as an instrument has come under criticism because parents' decisions about birth timing correlate with often unobserved characteristics such as marital status and mother's age, potentially undermining its exogeneity (Buckles and Hungerman 2013), so more research is needed. We adopt a novel instrumental variables strategy using cohort-level mean maternal education.[2] Our 2SLS results differ from descriptive relationships, and we dig deeper into multiple aspects of marriage outcomes. The benefits and costs of marriage are likely heterogeneous across individuals, but it is still an important institution to study and there is considerable interest among academics and the general public in marriage trends over time and how various factors like education may affect marriage (Stevenson and Wolfers 2007; Taylor 2010). Education has become increasingly important in the lives and labor market outcomes of both men and women, and it is vital to better understand whether and how education affects marriage outcomes.

2. Conceptual Framework

An individual will seek to enter a consenting marital union if and when the expected lifetime benefits of doing so exceed the costs. Individuals do not know the future benefits and costs and must form expectations. A marriage occurs if and when two people view marrying each other as their best option. Marriages can be dissolved, but marital dissolution involves significant financial and psychological costs, so we assume that individuals enter marriage with the expectation of a lifetime union. While numerous factors affect an individual's costs and

[2] Ahn and Winters (2023) use a similar identification strategy to examine the causal effects of education on self-employment outcomes. Otherwise, this identification strategy has not yet been widely used in the research literature.

benefits of entering a marriage, we focus on their productivity in market production and home production following a long literature (Becker 1981; Bardasi and Taylor 2006; Chiappori et al. 2009; Chiappori et al. 2018; Gousse et al. 2017).

We are specifically interested in the effect of education on marriage. Education is expected to increase productivity in both market production and home production, which makes the individual a more attractive potential marriage partner. However, additional education may reduce an individual's own benefit from marrying. If they become more productive, they may receive less benefit or less need to partner with another person. Marrying may push some women toward home production and away from market production; women who view their career and independence as important parts of their identity, may be hesitant to marry or at least less likely to marry at the margin (Goldin 2006). Education may also increase opportunities for and benefits from non-marital relationships, which could decrease an individual's net benefit of marrying.

Effects of education on marriage may also vary with age. For example, education may reduce the likelihood of marrying while relatively young, but negative effects of education on marriage propensity may diminish or even disappear as individuals age. This may occur if individuals view marriage as advantageous in childrearing. Education may encourage people to delay having children until older ages, but biological factors discourage them from putting off having children too long. Thus, some people may put off marriage until they are ready to have children, leading to heterogeneous effects on marriage by age. Additionally, biological factors related to aging may differ for men and women and lead to heterogeneous effects of education on marriage by gender. While we assume that individuals enter marriage with the expectation that they are life-long, subsequent experiences can alter expectations and lead to marital dissolution.

Education may lead to better initial matches, greater financial security, increased costs of marital dissolution, and more stable marriages.

Notably, observed relationships between education and marriage outcomes are likely influenced by frequently unobserved factors correlated with both such as personality, time preferences, and various dimensions of ability. For example, persons who are very disagreeable, very neurotic, or very myopic may have both low education levels and low marriage rates (Lundberg 2012; Dupuy and Galichon 2014). Persons with high cognitive ability may excel at education and make attractive marriage partners. Thus, simple descriptive analysis may lead to biased estimates of the relationship between education and marriage.

3. Data and Methods

We measure individual marriage outcomes using the pooled 2006-2019 American Community Survey obtained from IPUMS (Ruggles et al. 2021). The ACS is administered to a one percent random sample of the U.S. population each year and contains information on individual demographics, education, and other characteristics. The sample is randomly selected each year, and we are unable to link individuals across years. We pool multiple years to increase sample size and estimation precision. The ACS collects information on marital status at the time of the survey and codes individuals as married with spouse present in the household, married with spouse absent, separated, divorced, widowed, or never married. Our main measure of marital status is a married indicator that equals one for married persons regardless of spousal presence, but we also consider additional analysis using detailed marital spouse category indicators as outcomes. The ACS is a household level survey, so we also observe spousal characteristics for married individuals with spouse present.

We merge individual records in the ACS with cohort-level parental characteristics computed via the 1980 and 1990 decennial census long form survey 5% samples. The census and ACS data provide parental characteristic information only for individuals living in the same household as their parent(s). Since most adults do not live with their parents and those that do are a non-random sample, we cannot directly observe individual-level parental education for our adult ACS sample. We also cannot link the same individuals over time. Instead, we link cohort-level data over time. We define cohorts based on state of birth, year of birth, and ancestry group. State of birth is directly reported in both the census and ACS microdata. Year of birth is computed as survey year minus age at the time of the survey.[3] We define 12 ancestry groups including Dutch, English, French, German, Irish, Italian, Polish, Scandinavian, Scottish, Other Eastern European, Other Western European, and all other. We limit the analysis to persons who are white and non-Hispanic to increase comparability across ancestry groups. Cohort-level parental characteristics for other racial and ethnic groups would also be challenged by small sample sizes, missing data, and possibly changing composition of unobservable characteristics.

We use the 1980 census microdata to compute cohort parental characteristics for children born 1963-1972 and ages 8-17 in the 1980 census. Similarly, we use the 1990 census to compute cohort parental characteristics for children born 1973-1990 and ages 0-17 in 1990. Following IPUMS, we treat biological, adoptive, and step-parents equivalently if they live in the same household as the child. However, parents living in a different household as their child cannot be linked to their child in the census microdata. While some children live with no parents, the

[3] This involves some measurement error depending on when during the year an individual was born and when they were surveyed. This will induce some noise in our instrument and partially reduce its first-stage explanatory power, but it should not bias our second-stage results. We show below that our instrument is very strong despite this measurement error.

overwhelming majority in our survey do. 95 percent of the combined 1980 and 1990 census sample of children live with a mother and 84 percent live with a father.

Our main instrument is the cohort mean years of schooling among mothers of children who have a mother in the household.[4] We also explore a similar alternative instrument based on the mean years of college education for mothers of children with a mother in the household. We additionally construct similar instruments for paternal education, but our preferred instrument relies on maternal education because children are more likely to live with mothers than fathers, children typically spend more time with their mothers, and parent-child human capital transmission is stronger for parents that spend more time with their children (Gould et al. 2020).

A significant previous literature has used an individual's own parent's education to instrument for the child's education (Card 1999; Block et al. 2002; Hoogerheide et al. 2012). However, our study adopts a distinct approach by using cohort-level means of maternal education as an instrument. This method mitigates potential biases arising from unobservable individual-level factors, such as innate ability or family-specific resources, that may simultaneously affect both education and adult outcomes. By aggregating to the cohort level, our analysis reduces the influence of individual-level noise and ensures greater robustness against endogeneity. Rather than directly linking children to their actual mothers, we associate them with the maternal education levels of their cohort mates. While this approach may introduce some trade-offs in terms of precision compared to individual-level instruments, it provides sufficient variation for identification and offers a reliable framework for estimating causal effects.

We also construct additional cohort-level parental variables for inclusion as controls. These include the average annual income and employment rate for mothers among children with

[4] Thus, the instrument is constructed based on the 95 percent of children who live with their mother and not based on the five percent of children who do not live with their mother.

mothers in the household and the average income and employment rate for fathers among children with fathers in the household. We also compute the percentage of children who live with their mother, the percentage who live with their father, and the percentage who have married parents that both live in the household.

We merge the census cohort-level data to individuals in the ACS with the same year of birth, state of birth, and ancestry. Our preferred estimates are from two-stage least squares regressions that instrument for individual education with the cohort mean maternal education level, but we also report results from ordinary least squares (OLS). In addition to the other cohort-level parental characteristics included as controls, we also include detailed fixed effects for state-of-birth interacted with year of birth and ancestry group interacted with year of birth. We also include controls for survey year and age at the time of the ACS. Thus, our linear regression model has the following form:

$$Y_{isacot} = \beta YearsSchooling_{isacot} + \theta Z_{sac} + \gamma_{sc} + \xi_{ac} + \varphi_o + \delta_t + \varepsilon_{isacot}$$

, where Y_{isacot} is a marriage outcome dependent variable for individual i born in state s from ancestry group a in birth-year c and observed at age o in ACS year t. Years of schooling is measured at the individual level but will be instrumented using mean maternal schooling in 2SLS regressions. Z_{sac} includes the controls for parental characteristics discussed above. The additional explanatory variables ($\gamma_{sc}, \xi_{ac}, \varphi_o, \delta_t$) include detailed fixed effects for birth-state×birth-year, ancestry×birth-year, age, and survey year.[5] Inclusion of the detailed fixed effects means that we leverage variation across ancestry groups born in the same state and year, controlling for national-level differences among ancestry groups over time. ε_{isacot} is an error

[5] We use the reghdfe and ivreghdfe commands in Stata (Correia 2017, 2018).

term. We use ACS survey weights and cluster standard errors by birth state. We also conduct sub-sample analysis by gender and age group.

In order for 2SLS to produce consistent estimates of causal effects of education on marriage, the excluded instrument must be relevant and exogenous. The relevance assumption requires that the instrument have strong predictive power in the first stage. We report first-stage results below to support this assumption. The exogeneity assumption requires that the instrument is only correlated with the second-stage dependent variable through its effect on the first-stage dependent variable. In our case, this requires that cohort-level maternal education levels from the census data only affect individual marriage outcomes observed in the ACS via their effect on individual education levels in the ACS. Specifically, there should be no unobserved factors conditionally correlated with cohort-level maternal education and marriage outcomes. Controlling for other parental characteristics (including income and employment rates) should partially help with this as should including the extensive set of fixed effects.

The rationale for the instrument is that maternal education levels have risen over time in ways that vary across states and across ancestry groups within states and rising maternal education is expected to translate into greater education levels for the next generation. Rising female education levels are heavily influenced by changing gender norms and changing expectations and opportunities for female involvement in the labor force (Goldin 2006). However, ancestral cultural norms and state economic conditions influenced the timing and magnitude of increases in female education. For example, young women from more progressive ancestry groups and from states with strong labor market opportunities for highly educated women are expected to invest more in education than their counterparts from more traditional ancestry groups or from states with weaker employment opportunities for highly educated

women. We leverage differences in cohort-level maternal education across ancestry groups within the same state of birth and year of birth, while controlling for national level ancestry differences by year of birth. Since we control for parental characteristics including maternal employment rate and income, we believe there is no reason why cohort-level maternal education levels would affect their child's marital outcomes other than through the effect of maternal education on their child's education. Thus, we argue that our instrument is conditionally exogenous and our 2SLS strategy should give consistent estimates of causal effects of schooling on marriage outcomes. Additionally, we also conduct sensitivity analysis that simultaneously uses maternal and paternal education levels as instruments in order to conduct a test for overidentification.

Table 1 presents analytical sample means for selected variables by gender for ages 25-34, 35-44, and 45-54. Not surprisingly, the proportion who are married varies by age and gender. In particular, 56 percent of women ages 25-34 are married, but the rate is 70 percent and 69 percent for women ages 35-44 and 45-54, respectively. Only 47 percent of men ages 25-34 are married, but 67 percent of men ages 35-44 and 45-54 are married.[6] The overwhelming majority of married persons live in the same household with their spouse. Rates of being separated are consistently low and not much different across the groups in Table 1. Rates of being divorced (and not remarried) increase with age and are higher for women than men. Rates of being widowed are low for our sample but increase with age and are higher for women than men. Women have higher mean years of schooling than men, and the gap is largest among the youngest age group. Parental schooling means are highest for the youngest age group reflecting increasing parental education levels over time.

[6] Gender differences in marriage rates for our analytical sample partially reflect corresponding differences among age groups not examined and differences among same-sex married couples.

4. Empirical Results

4.1 Probability of Being Married

We first examine the relationship between years of schooling and the probability of being married at the time of the ACS. Table 2 presents OLS and 2SLS results by gender for the full age range in our sample. Table 3 reports results by gender separately for ages 25-34, 35-44, and 45-54.[7] In all cases, the OLS coefficient estimates for years of schooling are positive and statistically significant at the one percent level. However, the 2SLS coefficient estimates in Tables 2 and 3 are never positive. Endogeneity test results for years of schooling in the OLS specifications are reported at the bottom of the tables and generally indicate that individual years of schooling is endogenous and OLS is biased and inconsistent. This endogeneity may stem from unobserved individual characteristics, such as innate ability, personality traits (e.g., patience, perseverance), or family background, which jointly affect both schooling decisions and marriage outcomes. For example, individuals with greater innate ability or stronger family support may attain higher levels of education and exhibit a higher likelihood of marriage, introducing upward bias in OLS estimates. The first-stage results for the 2SLS models consistently indicate that the instrument has strong first-stage predictive power. The first-stage F-statistics well exceed conventional thresholds (e.g., $F > 10$), confirming the relevance of the instrument.[8]

To further assess the instrument's predictive power, we conducted an auxiliary reduced form OLS regression with the binary dependent variable for being married regressed directly on

[7] Appendix Tables A1 and A2 report results for parental characteristics controls for the 2SLS regressions.
[8] Based on a reviewer suggestion, we also examined the partial correlations between the residuals of the instrument and the endogenous regressor; these are modest (0.0287 for women, 0.0327 for men), but they remain statistically significant. These partial correlations alone do not fully capture the strength of the instrument; instead, the first-stage F-statistics provide robust evidence of its adequacy.

cohort-level maternal education, controlling for the same covariates and fixed effects used in our main models. The results, presented in Appendix Table A3, indicate a significant negative relationship with coefficients of –0.005 for females and –0.007 for males. As expected, these coefficients are smaller in magnitude than the main 2SLS estimates (–0.018 for females and –0.019 for males) but have the same sign.

There is also some potential concern that the cohort-level maternal education instrument could be correlated with marriage outcomes of the second generation independent of the impact through the children's education. We cannot completely rule this out, but we think it is unlikely to qualitatively impact our results because we include additional cohort-level characteristics as control variables and because OLS estimates appear to be positively biased. If cohort maternal education had its own separate relationship independent of the impact through the second-generation's education, we would expect that relationship to be positive similar to the own education OLS coefficients of the second-generation. I.e., if the instrument is biased, we would expect it to induce a positive bias in 2SLS, but our 2SLS results still yield negative coefficient estimates for the impact of education on marriage. Thus, the instrument is likely either unbiased or the bias is small and does not alter the direction of the 2SLS estimates. Furthermore, our instrument is driven by social, cultural, and economic conditions across ancestry groups within states that varied over time and differentially impacted the educational investments of maternal cohorts. By the time the second generation makes marital decisions, the original conditions influencing their mother's education have considerably changed and appear unlikely to influence the child's marital decisions except through the impact on intergenerational transmission of education. I.e., some mothers got more education when they were younger for reasons that are no

longer relevant when their children enter the marriage market but the impact of maternal education has a long-run effect by increasing their children's education.

The 2SLS second-stage coefficient estimates in Table 2 are significantly negative for both men and women. However, the 2SLS results in Table 3 indicate nuanced effects of years of schooling on marriage by age group. Specifically, the effect is negative and statistically significant for ages 25-34, with coefficients of -0.036 and -0.040 for women and men, respectively. This indicates that an additional year of schooling reduces the probability of being married by roughly four percentage points for this age group. However, the effect of schooling is not significant for the 35-44 and 45-54 age groups, and the point estimates for the latter are very close to zero. Thus, the 2SLS estimates indicate that additional schooling reduces the probability of being married for younger adults but not for their older counterparts. This suggests that schooling may delay marriage among young people without impacting eventual rates of being married.

Table 4 reports 2SLS results for four alternative specifications by gender and age group. Panel A uses cohort-level paternal mean years of schooling as an instrument, and Panel B uses cohort-level maternal and paternal schooling as instruments. The results are qualitatively similar to the preferred 2SLS specification results in Table 3, and the overidentification test p-values in Panel B fail to reject the null that the model is exactly identified. Panel C examines the influence of excluding the parental characteristic control variables, and the results are qualitatively robust. Panel D measures education by years of college completed instead of years of schooling for both the individuals in the ACS and the maternal education instrument. Results are again similar.

4.2 Additional Marriage Outcomes

Table 5 reports 2SLS results for the effects of years of schooling on additional marriage outcomes. Panel A examines the effect on being married with spouse present; results are similar to the main results in Table 3 for the overall effect on the probability of being married. Panel B examines the effect on being married with spouse absent; three of the coefficients are significantly positive but the magnitudes are relatively small.[9] Panels C, D, and E examine separate effects on being separated, divorced, and widowed, respectively; the coefficient estimates are negative and significant for roughly half of these regressions; none are significantly positive. Thus, there is some evidence that education reduces the probability of being separated or divorced, i.e., education appears to create more stable marriage outcomes. The significant negative effect on being widowed occurs for women in all three age groups but not for men; this is not easy to interpret without more detailed characteristics on their deceased spouse and the timing of their marriage. However, this may suggest that less educated women marry spouses with higher risk of death such as due to worse health or riskier occupations or it may suggest that less educated widows are less likely to remarry than more educated widows. Panel F combines the effect on being separated, divorced, or widowed using a dependent variable equal to one if the individual is any of the three. The effect is negative and significant in all six columns of Panel F.

Panel G examines the effect of schooling on the probability of having never married (yet), and the effect is significantly positive in all six columns. The effect of an additional year of schooling is roughly two to three percentage points for ages 45-54. Thus, schooling may permanently reduce the likelihood of ever marrying for some individuals and increase the likelihood of being permanently single. This is somewhat surprising given the null effects on the

[9] The small positive effect may reflect dual-career "power couples" that sometimes maintain separate residences in different labor markets. Increased education may make this more likely.

probability of being married for ages 45-54 in Table 3. However, this is explained by offsetting effects on being separated, divorced, and widowed. Education makes some people permanently less likely to marry, but those who do marry are more likely to stay married, so that the overall effect of schooling on the probability of being currently married by ages 45-54 is effectively zero. In other words, education leads to less marital entry and less marital exit; these effects partially cancel each other out so that years of schooling does not significantly affect the probability of being married during ages 45-54.

4.3 Spousal Education

The remaining panels of Table 5 examine the effects of individual schooling on spousal education characteristics. An important and growing literature has documented positive assortative mating based on education (Pencavel 1998; Schwartz and Mare 2005; Greenwood et al. 2016). Similarly, researchers have suggested that some of the private benefit of education operates through marriage market effects (Iyigun and Walsh 2007; Chiappori et al. 2009; Bruze 2015; Wiswall and Zafar 2021). An individual's own education likely makes them more attractive to more educated potential marriage partners and increases their likelihood of having a more educated spouse. We next examine an indicator for having a spouse with a bachelor's degree or higher education. One complication is that we only observe spousal education for people that are currently married with spouse present in the household. Therefore, we examine spousal education both conditional on being married with spouse present and as a joint outcome with having a present spouse. The conditional analysis limits the sample to those who are married with spouse present. The joint outcome analysis includes the full sample and sets spousal education as zero for those without a spouse present.

Results in Table 5 Panels H and I suggest that an individual's own education increases the probability of being married to a college graduate spouse. The effect is moderately larger for women than men in both panels and there is some variation across age groups, but all relevant coefficients in Panels H and I are positive and statistically significant. Possible non-random sorting into marriage complicates interpretation of the conditional analysis, but the joint analysis results provide some insight into lower bound effects. Results in Panel I suggest that an additional year of schooling increases the joint probability of being married to a college graduate present in the household anywhere from 5.3 percentage points to 12.9 percentage points for the various groups. In other words, one's own education increases the likelihood of marrying a highly educated spouse and the magnitude is meaningfully large.

4.4 Further Analysis

Appendix Table A4 reports exploratory results examining the effect of years of schooling on having children. While there is some previous literature related to this topic (Björklund 2006; Monstad et al. 2008; Cygan-Rehm and Maeder 2013; Fort et al. 2016), previous results are somewhat mixed and there is no strong consensus on the effect of education on fertility. Unfortunately, the ACS does not contain information on completed fertility; it has information on the number of children in the household, but people have children at different ages, and children often live separately from parents, especially after reaching adulthood. While we cannot examine lifetime fertility using ACS data, the results in Appendix Table A4 do suggest that education reduces the probability of having children and the number of children during ages 25-34. Thus, reductions in marriage and childbearing for ages 25-34 appear connected. Education causes fewer marriages and fewer children while relatively young. However,

education appears to increase female fertility at older ages. The net effect is difficult to interpret and we reiterate that this analysis has data limitations, but the results may weakly suggest that education reduces overall fertility levels.

5. Conclusion

Increasing education levels have coincided with falling marriage rates in many countries, but cross-sectional comparisons often indicate that more educated individuals are more likely to be married. This inconsistency between time-series and cross-sectional data presents a puzzle about the causal effect of education on marriage. We merge cohort-level data from the 1980 and 1990 USA decennial censuses with the 2006-2019 American Community Survey and use two-stage least squares to estimate causal effects of formal education on marriage outcomes.

We find that increased education reduces the likelihood of being married for young adults ages 25-34, but there is no significant effect on the probability of being married for adults ages 45-54. This pattern holds for women and men and with roughly equal coefficient magnitudes by gender. Furthermore, OLS yields biased and inconsistent positive coefficient estimates, likely because individuals with greater advantages—such as higher innate ability, stronger family support, or other unobserved factors—are more likely to achieve higher education and to marry. This inflates the estimated effect of schooling on marriage. Therefore, a 2SLS identification strategy is essential for uncovering causal effects of education on marriage. Cross-sectional studies of education and marriage that do not leverage quasi-experimental variation are likely to be inaccurate and misleading.

We also examine more detailed marriage outcomes and find additional nuance. While education does not reduce the overall probability of being currently married for ages 45-54, it

simultaneously increases the probability of being never married and appears to reduce the probability of being divorced, separated, or widowed. Thus, education appears to reduce the probability that some people will ever marry but increases marital stability for those that do marry. These effects roughly cancel out when looking at the effect of education on the probability individuals ages 45-54 are currently married, so that the estimated coefficient is close to zero and not significant.

We also offer some implications for research literatures related to assortative mating and fertility. We find that increasing an individual's education increases their probability of marrying a college graduate spouse, consistent with positive assortative mating. We also present suggestive evidence that in addition to delaying marriage, education reduces the number of children individuals have while relatively young. Education and marriage are two of the most important decisions an individual can make. We provide evidence that education has important causal effects on marriage outcomes.

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Table 1: Sample Means by Gender and Age Group

	(1)	(2)	(3)	(4)	(5)	(6)
	Ages 25-34		Ages 35-44		Ages 45-54	
	Women	Men	Women	Men	Women	Men
I. Dependent Variables						
Married	0.561	0.474	0.697	0.668	0.687	0.670
Spouse Present	0.545	0.458	0.685	0.653	0.674	0.655
Spouse Absent	0.016	0.016	0.013	0.015	0.013	0.015
Separated	0.022	0.013	0.026	0.019	0.023	0.019
Divorced	0.070	0.052	0.135	0.113	0.175	0.154
Widowed	0.003	0.001	0.009	0.003	0.021	0.008
Single/ Never married	0.345	0.459	0.132	0.197	0.094	0.148
Married*Spouse College Graduate	0.215	0.217	0.273	0.304	0.249	0.274
II. Main Explanatory Variables						
Years of Schooling	14.37	13.80	14.29	13.87	14.03	13.73
Years of College	2.50	1.98	2.43	2.06	2.19	1.96
III. Cohort-level Variables						
Mean Maternal Years of Schooling	12.99	13.00	12.51	12.52	12.11	12.11
Mean Paternal Years of Schooling	13.37	13.38	12.96	12.97	12.55	12.56
Mean Maternal Years of College	1.31	1.31	1.00	1.01	0.78	0.79
Mean Paternal Years of College	1.33	1.33	1.33	1.34	1.40	1.40
Mean Maternal Annual Income	22,576	22,579	20,498	20,560	18,075	18,066
Mean Paternal Annual Income	73,081	73,063	75,742	75,790	76,046	76,048
Percentage of Mothers Employed	0.61	0.61	0.61	0.61	0.56	0.56
Percentage of Fathers Employed	0.93	0.93	0.92	0.92	0.92	0.92
Pct. of Children with Mother Present	0.96	0.96	0.94	0.94	0.93	0.93
Pct. of Children with Father Present	0.86	0.86	0.84	0.84	0.83	0.83
Pct. of Children with Married Parents	0.81	0.81	0.80	0.80	0.81	0.81

Notes: The full sample includes white, non-Hispanic individuals born in the USA in years 1963-1990 and observed in the 2006-2019 American Community Survey. Sample means for cohort-level control variables are measured using the 1980 and 1990 decennial census microdata by state of birth, year of birth, and ancestry group. Married*Spouse College Graduate is a joint outcome equal to one if the individual is married with a college graduate spouse present in the household and zero otherwise.

Table 2: OLS and 2SLS Effects of Years of Schooling on Marriage Probability

	(1) Women Ages 25-54	(2) Men Ages 25-54
A. OLS		
Years of Schooling	0.024***	0.031***
	(0.001)	(0.001)
B. 2SLS		
Years of Schooling	-0.018**	-0.019*
	(0.009)	(0.010)
First-Stage Results		
Cohort Maternal Schooling	0.299***	0.348***
	(0.026)	(0.025)
F-Statistic	134.1	201.6
OLS Endogeneity Test P-value	< 0.001	< 0.001
Observations	4,097,486	4,068,507

Notes: Regressions include cohort parental characteristics control variables and a large number of fixed effects including interactions of birth-state and birth-year, interactions of ancestry group and birth-year, age at the time of the ACS, and ACS year. Standard errors in parentheses and first-stage F-statistics account for clustering by birth-state. The test for OLS endogeneity is the Durbin-Wu-Hausman test. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

Table 3: OLS and 2SLS Effects of Years of Schooling on Marriage by Age Groups

	(1)	(2)	(3)	(4)	(5)	(6)
	Ages 25-34		Ages 35-44		Ages 45-54	
	Women	Men	Women	Men	Women	Men
A. OLS						
Years of Schooling	0.020***	0.022***	0.029***	0.037***	0.022***	0.034***
	(0.001)	(0.001)	(0.0004)	(0.0005)	(0.0003)	(0.0005)
B. 2SLS						
Years of Schooling	-0.036***	-0.040***	-0.016	-0.013	-0.003	-0.005
	(0.011)	(0.012)	(0.011)	(0.012)	(0.015)	(0.012)
First-Stage Results						
Cohort Maternal Schooling	0.276***	0.324***	0.305***	0.356***	0.290***	0.339***
	(0.032)	(0.024)	(0.026)	(0.030)	(0.030)	(0.030)
F-Statistic	73.36	189.5	135.1	143.5	93.33	127.7
OLS Endog. Test P-value	< 0.001	< 0.001	< 0.001	< 0.001	0.056	0.001
Observations	1,396,424	1,396,717	1,623,377	1,615,877	1,028,617	1,009,074

Notes: Regressions include cohort parental characteristics control variables and a large number of fixed effects including interactions of birth-state and birth-year, interactions of ancestry group and birth-year, age at the time of the ACS, and ACS year. Standard errors in parentheses and first-stage F-statistics account for clustering by birth-state. The test for OLS endogeneity is the Durbin-Wu-Hausman test. ***Significant at 1% level.

Table 4: Robustness Checks for 2SLS Effects of Years of Schooling on Marriage

	(1)	(2)	(3)	(4)	(5)	(6)
	Ages 25-34		Ages 35-44		Ages 45-54	
	Women	Men	Women	Men	Women	Men
A. Using Paternal Schooling IV						
Years of Schooling	-0.036***	-0.043***	-0.013	-0.019	-0.008	-0.014
	(0.010)	(0.012)	(0.011)	(0.012)	(0.014)	(0.010)
B. Using Both Parental Schooling IV						
Years of Schooling	-0.036***	-0.042***	-0.014	-0.016	-0.006	-0.011
	(0.010)	(0.012)	(0.011)	(0.012)	(0.014)	(0.010)
Overident. Test P-value	0.996	0.802	0.752	0.468	0.853	0.597
C. Excluding Cohort Parental Controls						
Years of Schooling	-0.028***	-0.033***	-0.011	-0.012	0.001	-0.001
	(0.010)	(0.011)	(0.008)	(0.011)	(0.009)	(0.008)
D. Estimating Effects of Years of College						
Years of College	-0.045***	-0.049***	-0.026*	-0.014	-0.017	-0.006
	(0.016)	(0.016)	(0.014)	(0.012)	(0.018)	(0.016)

Notes: Specifications are similar to Table 3 Panel B except as indicated in the panel name. First-stage results are similar to Table 3 and are suppressed to conserve space. *Significant at 10% level; ***Significant at 1% level.

Table 5: 2SLS Effects of Years of Schooling on Alternative Marital Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Ages 25-34		Ages 35-44		Ages 45-54	
	Women	Men	Women	Men	Women	Men
A. Spouse Present						
Years of Schooling	-0.040***	-0.043***	-0.016	-0.017	-0.006	-0.008
	(0.012)	(0.013)	(0.012)	(0.013)	(0.015)	(0.012)
B. Spouse Absent						
Years of Schooling	0.004*	0.003*	-0.0004	0.004***	0.003	0.003
	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.003)
C. Separated						
Years of College	-0.014***	-0.005***	-0.007***	-0.003	-0.003	-0.002
	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
D. Divorced						
Years of Schooling	-0.010**	-0.008**	-0.011	-0.013***	-0.009	-0.014*
	(0.005)	(0.003)	(0.007)	(0.004)	(0.011)	(0.008)
E. Widowed						
Years of Schooling	-0.004***	1.50e-07	-0.003*	-0.001	-0.006**	-0.002
	(0.001)	(0.0005)	(0.001)	(0.001)	(0.003)	(0.001)
F. Separated, Divorced, Widowed						
Years of Schooling	-0.028***	-0.021***	-0.018*	-0.013***	-0.016***	-0.018**
	(0.005)	(0.008)	(0.010)	(0.004)	(0.005)	(0.008)
G. Never Married /Single						
Years of Schooling	0.064***	0.037***	0.021***	0.052***	0.030***	0.023***
	(0.011)	(0.007)	(0.008)	(0.013)	(0.010)	(0.007)
H. Spouse College Graduate Conditional Analysis						
Years of Schooling	0.237***	0.148***	0.201***	0.130***	0.184***	0.140***
	(0.0270)	(0.0108)	(0.00876)	(0.00760)	(0.0133)	(0.00803)
I. Spouse College Graduate Joint Outcome						
Years of Schooling	0.094***	0.053***	0.129***	0.077***	0.111***	0.094***
	(0.009)	(0.007)	(0.006)	(0.008)	(0.008)	(0.008)

Notes: The dependent variables for Panels A-G are indicators for detailed marital status. The dependent variables for Panels H & I are indicators for having a spouse with a bachelor's degree or higher. The conditional analyses in Panel H limits the sample to married persons with spouse present. The joint outcome analyses in Panel I include the full sample and set the dependent variables equal to zero if the person is not married with spouse present. First-stage results are similar to Table 3 and are suppressed to conserve space. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

Appendix Tables

Table A1: Parental Characteristic Variable 2SLS Results for Marriage Probability

	(1) Women Ages 25-54	(2) Men Ages 25-54
% w/ Married Parents	0.361	0.632**
	(0.241)	(0.287)
% w/ Mother in Household	-0.023	0.037
	(0.029)	(0.027)
% w/ Father in Household	-0.024	0.026
	(0.036)	(0.033)
% of Mothers Employed	-0.007	-0.017
	(0.011)	(0.011)
% of Fathers Employed	0.060***	0.057**
	(0.020)	(0.022)
Mean Income of Mothers	2.38e-08	1.95e-08
	(7.65e-08)	(5.19e-08)
Mean Income of Fathers	7.39e-09	-3.44e-08
	(4.51e-08)	(4.90e-08)
Observations	4,097,486	4,068,507

Notes: Parental characteristics are measured as cohort level means from the 1980 and 1990 decennial census microdata. Results correspond to the 2SLS specification in Table 2 Panel B. **Significant at 5% level; ***Significant at 1% level.

Table A2: Parental Characteristic Variable 2SLS Results for Marriage by Age Groups

	Age 25-34		Age 35-44		Age 45-54	
	(1)	(2)	(3)	(4)	(5)	(6)
	Women	Men	Women	Men	Women	Men
% w/ Married Parents	0.114**	0.090**	0.137***	0.061	0.096	0.049
	(0.051)	(0.039)	(0.047)	(0.054)	(0.091)	(0.094)
% w/ Mother in Household	-0.071*	0.052	-0.014	0.039	0.005	-0.006
	(0.040)	(0.046)	(0.042)	(0.044)	(0.059)	(0.054)
% w/ Father in Household	-0.017	-0.003	-0.046	0.003	-0.053	0.072
	(0.053)	(0.041)	(0.043)	(0.053)	(0.086)	(0.083)
% of Mothers Employed	0.005	-0.011	-0.021	-0.022	0.031	0.008
	(0.02)	(0.018)	(0.015)	(0.015)	(0.020)	(0.021)
% of Fathers Employed	0.033	0.031	0.107***	0.058**	0.007	0.089**
	(0.030)	(0.031)	(0.034)	(0.026)	(0.028)	(0.041)
Mean Income of Mothers	3.39e-08	3.12e-08	1.13e-08	5.24e-08	1.22e-07	-9.84e-08
	(7.38e-08)	(6.37e-08)	(1.39e-07)	(9.46e-08)	(2.20e-07)	(2.05e-07)
Mean Income of Fathers	2.18e-08	-2.54e-09	-2.66e-07	-5.12e-07**	-5.62e-07	1.26e-07
	(4.43e-08)	(4.40e-08)	(2.12e-07)	(2.07e-07)	(4.43e-07)	(3.92e-07)
Observations	1,396,424	1,396,717	1,623,377	1,615,877	1,028,617	1,009,074

Notes: Parental characteristics are measured as cohort level means from the 1980 and 1990 decennial census microdata. Results correspond to the 2SLS specification in Table 3 Panel B.
**Significant at 5% level; ***Significant at 1% level.

Table A3: Reduced Form OLS Results for Cohort-Level Maternal Years of Schooling on Marriage Probability

	(1) Women Ages 25-54	(2) Men Ages 25-54
Years of Schooling	-0.005**	-0.007**
	(0.002)	(0.003)
Observations	4,097,486	4,068,507

Notes: Regressions include cohort parental characteristics control variables and a large number of fixed effects including interactions of birth-state and birth-year, interactions of ancestry group and birth-year, age at the time of the ACS, and ACS year. Standard errors in parentheses are clustered by birth-state. This regression directly examines the relationship between cohort-level maternal years of schooling and marriage probabilities. **Significant at 5% level.

Table A4: 2SLS Effects of Years of Schooling on Having Children

	(1)	(2)	(3)	(4)	(5)	(6)
	Ages 25-34		Ages 35-44		Ages 45-54	
	Women	Men	Women	Men	Women	Men
A. Number of Own Children						
Years of Schooling	-0.264***	-0.139***	-0.025	-0.036	0.062**	0.033
	(0.049)	(0.037)	(0.028)	(0.026)	(0.025)	(0.022)
B. Has One or More Child						
Years of Schooling	-0.131***	-0.083***	-0.044***	-0.034***	-0.001	0.009
	(0.011)	(0.012)	(0.007)	(0.009)	(0.011)	(0.009)
C. Child Born during Past Year						
Years of Schooling	-0.006		0.006**		0.001	
	(0.006)		(0.002)		(0.001)	
Sample Means						
Number of Own Children	1.05	0.71	1.56	1.33	0.99	0.99
Has One or More Child	0.54	0.38	0.74	0.63	0.57	0.53
Child Born during Past Year	0.11	-	0.04	-	0.01	-

Notes: The dependent variable for Panel A is the number of own children residing with the individual including step-children, adoptive children, and biological children. The dependent variable for Panel B is an indicator equal to one if the individual had any own children in the household. The dependent variable for Panel C is an indicator equal to one for women who had given birth to any children in the past 12 months; this is only available for women age 50 and under. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.