THE RELATIONSHIP BETWEEN WOMEN'S EDUCATION AND MARRIAGE OUTCOMES*

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Abstract

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Abstract

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1. Introduction

The literature on the returns to education is undoubtedly one of the largest in labor economics. Notwithstanding, women have been largely ignored by this literature despite the fact that they have been increasing their educational attainment relative to men for some time; indeed, women in the United States are now *more* likely to attend college than are men. The focus on the returns of education for men has not been without reason. Historically, women frequently spent long periods of time outside of the formal labor market—limiting the impact of education on earnings and complicating the identification of the labor market return to education.[1]

Even for women with very limited labor force participation, there is reason to believe that education improves women's life outcomes. In particular, evidence suggests that education reduces mortality (Lleras-Muney, 2002), increases the cognitive ability of women's children (Murnane, 1981), reduces the incidence of criminal activity (Lochner and Moretti, 2001), aids in overcoming addiction (Sander, 1995), and improves the health of women's children (Thomas, Strauss, and Henriques, 1991 and Currie and Moretti, 2002).[2] Goldin (1992) presents suggestive evidence that college attendance may have improved the marriage outcomes of women who attended school in the 1960's and 1970's. In this paper we show that the marriage market is one of the primary channels through which education is correlated to women's wellbeing.

To do so, we rely primarily on 2000 Census data. Controlling for age and state fixed effects, we find that high school and college completion are strong predictors of marital status in 2000. Much of this effect is driven by marriage stability as opposed to the probability of having ever married. Furthermore, women's education is strongly related to husband income. College completion is particularly important—predicting an average increase in husband earnings of over $20,000 relative to women who only attended some college. Back of the envelope calculations suggest that half or more of the

[1] Papers that examine the labor market return of education for women include Buchinsky (2001) and Butcher and Case (1994).
[2] For a summary of the effects of schooling on well-being, see Haveman and Wolfe (1984).

correlation between women's education and consumption operates through the marriage market—not the labor market.

Although there may be an important causal effect of education, there could also be substantial bias resulting from unobserved heterogeneity, the joint determination of marriage and education decisions, selection into marriage, and educational investments after marriage. To provide evidence regarding the causality of the relationship, we examine the marriage outcomes of women from the 1980 Census. Using birth quarter as an instrument for education, we find that higher levels of education have a relatively small impact on the probability of marriage. On the other hand, education appears to have a substantial *causal* effect on husband earnings. More specifically, an extra year of education increases husband earnings by about $4,000. We conclude that selection bias is unlikely to be severe in this specific case, even if it is problematic more generally. Naturally, these results reflect only the experiences of women whose education was affected by their birth quarter.

Section 2 outlines the prior theoretical and empirical literature on assortative mating. In Section 3 we discuss the data that will be used for the analysis. In Section 4, we describe the relationship between women's education and marriage outcomes using data from the 2000 Census. In Section 5, we consider the causal channels that may operate between education and marital outcomes, as well as potential biases. In Section 6 we employ an instrumental variables strategy to provide preliminary evidence regarding the causal effect of education on marriage outcomes. We then conclude.

2. Background

Assortative mating is extremely well documented. In particular, researchers have found that individuals marry spouses who are like themselves over a number of dimensions. Citing a large number of studies, Mare (1991) asserts that spouses tend to be similar in terms of educational attainment, occupation (Hout, 1982), ethnic background (Pagnini and Morgan, 1990), religion (Johnson, 1980), and physical characteristics (Epstein and Guttman, 1984). Furthermore, men with high wages tend to marry women with favorable labor market prospects (Juhn and Murphy, 1997; and Cancian, Danziger, and Gottschalk, 1993). Pencavel (1999) shows that assortative mating on the basis of

educational attainment increased in importance over the last half of the 20th century. Much of the increase between 1940 and 1980 can be explained by school acting as an important marriage market. Rose (2004) examines changes in the distribution of women's educational attainment and marriage outcomes in the 1980, 1990, and 2000 Censuses. She presents evidence showing that the incidence of hypergamy (women marrying more educated men) decreased over this period. Also, the marriage rate of highly educated women converged toward that of other women.

There are a variety of theories that explain the existence of positive assortative mating. Weiss (1997) provides an insightful summary of marriage market models that can generate positive (or negative) assortative mating. Becker's (1991) model of marriage suggests that when one spouse specializes in household production, there will be negative assortative matching on wages though there is likely to be positive assortative mating on non-wage income and spouse characteristics. When household public goods are important, Lam (1988) shows that positive assortative mating on both wages and other characteristics is possible in equilibrium.

Despite the developed literature on the extent and causes of positive assortative mating, relatively little is known regarding the causal impact of an additional year of education on women's marriage outcomes. Two studies that focus on other questions, yet shed some light on the issue, include Behrman, Rosenzweig, and Taubman (1994) and Behrman and Rosenzweig (2002). In these studies, the authors examine how the educational attainment of identical twins correlates to the educational attainment of their spouses. An individual who receives an extra year of schooling relative to his or her twin marries a spouse with 0.3 years of additional education on average. These studies suggest that even controlling for genetic differences and a largely common environment, extra schooling is associated with improved marriage outcomes. The observed association may still fail to reflect a causal effect of education on marriage outcomes due to the joint determination of education and marriage decisions, unobserved differences between twins, selection bias, and post-marital education investments.

Also consistent with a causal link between education and marriage outcomes, Goldin (1992) argues that during the middle of the twentieth century many women attended college with the purpose of finding a college educated spouse. She supports this

argument by noting that the direct financial return of education for women was relatively small due to their sporadic participation in the labor force. On the other hand, marrying a college educated man was associated with significantly higher family income. Women who attended college were much more likely to marry college educated husbands during the same time period. Ge (2006) claims that marriage market considerations were still an important driver of women's educational investments in the 1980's. Using a dynamic structural model, she estimates that the college completion rate for women would have been six percentage points lower in the absences of marriage market considerations.

The objectives of this paper are three-fold. First, we will document in detail the relationship between a woman's education and her marriage outcomes. In doing so, we hope to establish that the marriage market is one of the primary mechanisms through which education is related to women's well-being—even for recent cohorts of women. Second, we discuss the obstacles to identifying the *causal* effect of education on marriage outcomes. This discussion is augmented by a simple theoretical framework in the appendix that more formally illustrates the challenges to identification. Third, we provide suggestive evidence regarding the causal effect of women's education on marriage market outcomes by taking advantage of birth quarter as an instrument for education.

3. Data

The analysis relies on the 2000 and 1980 Census 5 percent Public Use Samples (PUMS). The 2000 Census 5 percent Public Use Sample is attractive primarily due to its massive sample size, which allows us to explore the association between education and marriage with depth and precision. Additionally, the data set contains information on marital status as well as labor force outcomes and husband characteristics. Unfortunately, the Census data provides only a limited set of covariates. Using these data, we explore the relationship between educational attainment and marriage outcomes for white women between the ages of 30 and 45 who were born and residing in the U.S. The vast majority of these women had completed their education and been married at least once. Additionally, almost all of their husbands would have completed their

education but were not yet retired. Thus their current income is a reasonable proxy for permanent income. The final sample consists of 1,170,626 women.

Table 1 shows summary statistics for these women which are computed using the sampling weights. Not surprisingly, the average age for women in this sample is 37.86—about the middle of the range of ages we're examining. Nearly 70 percent are married, though only 12 percent had not ever been married. About three quarters of women (and over 90 percent of their husbands) report being in the labor force. The average income for women in our sample is $24,921, most of which can be accounted for by earnings. This is less than half of husband income which is $57,350. In the 2000 PUMS, education is reported in categories as opposed to years. For our analysis, we use the categories provided in the Census except that we group together masters and professional degrees. We also combine all individuals who attended college but did not earn a bachelor's degree into a single category. Finally, all individuals who did not complete first grade are treated as if they have attended no school. In the sample, the large majority of women and their husbands have completed high school. The modal education category is some college though nearly a third of women and their husbands have at least completed their undergraduate degrees.

We also use the 1980 PUMS. This dataset shares many of the same advantages with the 2000 PUMS. It is enormous, including information of 835,935 white women between the ages of 30 and 45. Additionally, the 1980 PUMS provides information on quarter of birth, which is a potential instrument to investigate the causal effect of education on marriage outcomes. Examining Table 2, we see that the age of women in the sample is slightly lower relative to the 2000 PUMS, though the range of ages is the same. Nearly 80 percent are married and less than two thirds are in the labor force. Average income for these women is $6,310—much less than their husbands' income of $22,579. Fewer than 20 percent of women completed college. The husbands are slightly more educated than the women but have less education than is the case in 2000. Examining the timing of births over the year, we see that women are slightly more likely to have been born in the second quarter and slightly less likely to have been born in the first quarter than would be expected if births were timed randomly throughout the year.

4. The Relationship between Women's Education and Marriage Outcomes

The Relationship between Education and Marriage Outcomes by Education Level

We begin our analysis with the relationship between education and marriage outcomes using the 2000 PUMS. As mentioned earlier, education is reported in categories as opposed to years. For this reason we focus on the marginal change in marriage outcome associated with moving up to the next educational category. Consider the following regression equation:

(1) $$y = \delta^{OLS}(E) + X\beta + \varepsilon$$

where y is one of several measures of marital outcome discussed below, E is the education category and $\delta^{OLS}(E)$ is a nonlinear function captured by a set of dummy variables; X includes age and state of residence fixed effects.[3] For reasons discussed in the next section, $\delta^{OLS}(E)$ is almost surely an inconsistent estimator for the causal effect of education on marital outcomes. Despite this, $\delta^{OLS}(E)$ is well suited for illustrating the extremely strong correlation between education and marriage.

Figure 1 shows the relationship between education and the change in probability of being married in 2000, the probability of ever having been married, and the probability of being married conditional upon marriage at some point. The numbers shown in the figure correspond to $\delta^{OLS}(E) - \delta^{OLS}(E-1)$ for each outcome. Note that since E is measured in categories and not years, coefficients reflect the change associated with moving up one *category*, not moving up one *year*. Due to the enormous sample size used in estimation, the standard errors are negligible and not shown. The figure suggests that high school and college completion are associated with particularly large increases in the probability of being married in 2000. Women who complete high school are nearly 8 percentage points more likely to be married than those who dropped out in the twelfth grade. The college completion coefficient is about half as large. The fact that the

[3] The education effects may well vary by age. This would not be taken into account with age fixed effects. The estimates should be viewed as an average relationship for women of the ages in our sample (ages 30-45). In addition to the included controls, the estimated coefficients are robust to the inclusion of a polynomial in husband age.

marriage rate rises with education suggests that, for most women, husband quality and the utility of being married increase faster with education than the utility of being single. We discuss this in greater detail in the appendix. Note, however, that increases in the marriage rate are driven primarily by marriage stability (or the rate of remarriage) as opposed to the probability of ever having been married.[4] Indeed, beyond high school, education is associated with reductions in the probability of ever having been married. Beyond college completion, additional education is associated with fewer and less stable marriages. These results, including the apparent high school and college sheepskin effects, are quite similar to those found in Rose (2004). As discussed in Section 5, there are good reasons to think the OLS coefficients do not represent causal effects; this may be a particular concern for the negative estimates found in later years.

Figure 2 shows how husband annual income changes with increases in education. Once again, the numbers reflect marginal effects. We examine income in levels as opposed to the perhaps more natural log specification because some husbands have zero or negative incomes. In a log specification, the results can be fairly sensitive to the treatment of such husbands. Later we examine results from quantile regressions that allow us to examine both logs and levels. Figure 2 shows that attending the 12th grade predicts about a $5,000 increase in husband income. The increase associated with completing high school is about the same size. Attending some college predicts an additional $9,000, while college completion is correlated to an increase in husband earnings of a whopping $22,000. Completing a master's degree or a PhD has a much smaller marginal association with husband earnings. While one cannot ascribe a causal interpretation to the results, the findings from Figure 1 and 2 are consistent with high school and college attendance substantially improving women's marriage prospects.

Note that there are strong sheepskin effects at high school and college completion. The college sheepskin effect is not consistent with a strong role for college as a marriage market, but may indicate a role for an occupational marriage market where education

[4] Ideally, to examine marriage stability one would want to employ a period-by-period hazard model. The current specification suffers from the fact that more highly education women may marry later and thus have had less time to divorce. To investigate this possibility, we also examined women between the ages of 40 and 45. The vast majority of these women would have been married for a number of years and have had ample time to divorce. The results for this sample are quite similar.

drives occupational prospects. Alternatively, these sheepskin effects may be driven by the unobserved quality of the women that is correlated with her education decision.[5]

The Relationship between Education and the Distribution of Husband Income

If education affects the available pool of husbands from which a woman finds a mate, it may have important effects on the distribution of husband income—not just the average. For example, college attendance may allow women the opportunity of meeting men in very high earnings occupations (e.g. doctors and lawyers). On the other hand, secondary school attendance and completion might protect women from marrying husbands with very low incomes (e.g. those outside of the labor force). Such relationships allow that the education component may interact with the error term. By examining the relationship between education and the quantiles of the husband income distribution we investigate these possibilities.

Figure 3 shows how education is related to the 10th, 25th, 50th, 75th, and 90th percentiles of husband income. Each point gives $\delta^q(E) - \delta^q(E-1)$ where q is the quantile of the error term. 12th grade attendance and high school completion are each associated with approximately \$4,000 to \$5,000 increases in husband income across the different quantiles—though this absolute sum is likely to have the largest utility effect for individuals in the lower quantiles. On the other hand, college attendance is associated with the largest increases in husband earnings at the top of the earnings distribution. College completion is associated with nearly a \$50,000 increase in the 90th percentile of husband earnings. This is still small, however, compared with the \$113,000 increase in the 90th percentile of husband earnings associated with completing a master's or a professional degree. Completing a PhD is associated with marginal declines in the 90th percentile of husband earnings. Moving away from the top decile, college and graduate education is still generally associated with increases in husband income but the magnitudes of the effects are much smaller.

One advantage of quantile regressions is that we can examine the log of husband income and our coefficient estimates will be unaffected by how we deal with zero and

[5] Thanks to Ryan Johnson for pointing this out.

negative income values. In Figure 4, we show the estimated marginal increase in log income associated with each education level. Zero and negative values are given a value of 0 for log income. The figure shows that in percentage terms, 12th grade attendance and completion is associated with the largest improvements in husband quality at the bottom of the income distribution. High school completion is associated with over a 40 percent increase in the 10th percentile of husband income but only a 9 percent increase in the median. This suggests that secondary school may protect women from the very worst marriage outcomes. On the other hand, the figure confirms the finding that college completion and a master's or professional degree is associated with massive improvements in the quality of husbands at the top of the distribution. While college completion and a master's degree are associated with gains of 24 percent and 8 percent in the median of husband earnings, both are associated with over a 40 percent increase in the 90th percentile of husband earnings. These findings are consistent with higher education acting as a gateway to the very best marriage outcomes.

How Much of the Relationship Between Education on Women's Net Income Operates Through Husband Income?

The magnitude of the relationship between education and husband income suggests that much of the improvement in a woman's wellbeing associated with education may operate through the marriage market as opposed to the labor market. It is quite difficult to calculate how much of the correlation operates through the marriage market. In addition to uncertainty regarding economies of scale in households, it is unclear how resources within a family are shared and how this would change with an increase in women's education. Further, husbands and wives derive utility from being together that is unrelated to family finances, so that even without income effects marriage can have a strong effect on wellbeing.

Despite the inherent difficulties, it is worth performing a back of the envelope calculation to determine what fraction of the benefit associated with education can be plausibly attributed to the marriage market. Bear in mind that while the relationships discussed do not have a causal interpretation, they do underscore the importance of additional research into the effect of education on marriage outcomes.

One simple decomposition would involve examining how much of the correlation between a *married* woman's education and family income operates through husband quality as opposed to a woman's own income. To do so, we divide the regression coefficient of husband income on women's education by the sum of this coefficient and the coefficient of a regression of the wife's own income on her education. We do so by year of education. The results are given in column 9 of Table 3. Between the end of high school and college completion, about 60 to 70 percent of the relationship between married women's education and family income is attributable to husband income as opposed to a woman's own income. This falls substantially, however, for highly educated women. Thus for women who marry, education is more closely linked to a rise in her husband's income than to a rise in her own, suggesting that the marriage market returns to education may dominate the labor market returns.

The above decomposition shows the importance of the marriage market as a mechanism for education to improve women's life outcomes but it fails to take into account that many women in our sample are single. We can perform only a very rough calculation that includes these women. Assuming no economies of scale, no utility benefit from marriage, and equal resource allocation between couples, the consumption of women in a specific education category can be captured by the following equation:

$$(2) \qquad NI(E) = p(E)\left[\frac{1}{2}H(E) + \frac{1}{2}I_m(E)\right] + [1 - p(E)]I_s(E),$$

where $NI(E)$ is the expected net income available to the woman and is a function of her education, E; $p(E)$ is the probability that a woman is married, $H(E)$ is husband income, $I_m(E)$ is a woman's income if she's married, and $I_s(E)$ is a woman's income if she's single. Taking the derivative of this expression, we find:

$$(3) \qquad \frac{dNI}{dE} = \frac{dp}{dE}\left[\frac{1}{2}H + \frac{1}{2}I_m - I_s\right] + \frac{1}{2}p\frac{dH}{dE} + \frac{1}{2}p\frac{dI_m}{dE} + (1-p)\frac{dI_s}{dE}.$$

The first two (of the four) terms show the portion of the relationship between a woman's education and her consumption that is attributable to the marriage market. The first shows that as education increases, a woman is more likely to gain the average net income associated with marriage at the cost of her income if she remains single. The second term shows that for a fixed probability of marriage, as education increases, net income changes

with husband income. The third term shows that education may be associated with increases in the income of married women (though this must be shared with the husband). The final term captures the increase in income associated with higher education if a woman remains single.

Fortunately, we have data available on the probability of marriage and the average incomes of husbands, married women, and single women by year of education. We also can compute the changes in these variables associated with increases in education. This allows us to compute the fraction of the total effect arriving from the first two terms. Column 10 of Table 3 shows the fraction of the total relationship between women's education and consumption by year of schooling. The other columns contain all of the numbers necessary to calculate this fraction. Between the end of high school and college completion, about half of improvements in women's consumption associated with education operate through the marriage market as opposed to the labor market. It is somewhat lower for master's and professional degrees and actually turns negative for women with PhDs.[6]

Though this table contains only back of the envelope calculations, they suggest that the marriage market is an area of first order importance in understanding the impact of education on the lives of women. Furthermore, these effects are understated if marriage allows women to enjoy economies of scale in household production, risk sharing within the household, or some other utility benefit of marriage.

5. What Explains the Correlation between Women's Education and Marriage Outcomes?

In this section we consider the channels through which education and marriage outcomes may occur. After considering causal channels, we address biases that confound estimation of the causal effect, such as unobserved heterogeneity, joint determination, selection bias, and timing bias.

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[6] This is because obtaining a PhD is associated with fairly large drops in the probability that woman is married (relative to a woman who stopped her education at a master's degree). Because husbands usually earn more than wives, the reduction in the probability of marriage is associated with lower net income.

Causal Channels

There is reason to believe that at least some of the correlation between women's education and marriage outcomes may be causal. Indeed, there are several mechanisms through which education could increase husband quality and the probability of marriage. Many women meet their future spouses as they attend high school or college (see Mare, 1991; Pencavel, 1999; and Goldin, 1992). This would imply that women who complete high school are more likely to marry high school graduates. Similarly, college attendance would be positively correlated to the probability of marrying a college graduate. For other women, education may change their occupations and social circles, allowing them to meet high quality men after completing their schooling. In addition to affecting the type of men women meet, education may make women more desirable to the high ability men with whom a woman comes in contact. This could be because education prepares women to earn more in the labor force and/or be more effective in household production.[7] Alternatively, education may simply signal a woman's quality.[8]

A woman's education may also change the behavior of a given husband. For example, it is well established that married men earn more than unmarried men. If this premium is caused by marriage, a high quality or highly educated wife may induce a higher marriage premium. A wife's education may be an input to her husband's productivity either directly or by allowing him to specialize in market work.[9] Additionally, a woman's education may increase her bargaining power within a relationship—allowing her to increase her husband's labor supply.[10] Lastly, the positive income effect associated with marriage to a high education (and income) wife may induce a husband to work less or enter a more enjoyable but lower paying occupation.[11]

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[7] See Currie and Moretti (2002) for a discussion of how maternal education is related to infant health and mortality.

[8] The argument holds even for an extra year of secondary education, the most relevant case for our instrumental variables analysis. In particular, an extra year or secondary school may allow women to develop cognitive and literacy skills which might be attractive to potential husband directly or through the labor market. Additionally, women who drop out instead of attending high school may have more time to engage in high risk behaviors and interact with low quality men. Finally, graduating high school may serve as a strong signal regarding a woman's quality as a mate.

[9] Chun and Lee (2001) find support for the importance of household specialization. Hersch and Stratton (2000) do not.

[10] See Chiappori, Fortin, and Lacroix (2002).

[11] We thank Dick Butler and Jim Kearl for these insights.

As seen in Figure 2, education also has an impact on divorce and remarriage rates. Inasmuch as the marriage market is comparable to the job market, one can use insights from the job search and matching literature to see why "marriage turnover" might be lower among the well educated. Job turnover is lower when the information regarding match quality is precise and there are rents associated with job-specific human capital. Thus, it may be that educated women have more precise information regarding match quality—reducing the incidence of divorce due to later shocks about realized husband quality. This might be true if highly educated women were more likely to cohabitate before marriage (in which case many matches fail before they are legally recognized), tended to marry later when they had better information regarding their *own* personality and long-term goals as well as those of their husbands, or simply had the experience or cognitive skills to better predict match quality. Additionally, educated partners may engage in greater relationship-specific investments that reduce the incidence of divorce just as job-specific capital reduces firm turnover rates.[12]

These channels plausibly explain why higher education levels should have a causal effect on marriage outcomes. Unfortunately, recovering this causal effect is not straightforward. Next we discuss several problems that arise in uncovering the causal effect of education on marriage outcomes. In the appendix we present a simple model that more formally addresses some of the identification difficulties discussed below.

Unobserved Heterogeneity and the Joint Determination of Education and Marriage Outcomes

Education is endogenous to marriage and labor market expectations and realizations. Additionally, factors affecting the cost of education may have an independent effect on marriage outcomes. Because of this, women who find it optimal to invest heavily in education are likely to have systematically different marriage outcomes than those who invest little. To the extent that we do not observe all factors affecting education and marriage choices, the estimated impact of education is likely to reflect

[12] Although not introduced in the context of the marriage market, the matching model presented in Ljungqvist and Sargent (2000) is readily adaptable to it.

omitted variables bias. For example, high intelligence lowers a woman's cost of educational investment and may independently improve her marriage prospects, generating a positive correlation between the two outcomes. To the extent that one does not control for a woman's intelligence, the estimated impact of education on marriage outcomes will be biased upwards.

The bias due to unobserved heterogeneity need not be positive, however. This is most clear when considering how women might alter educational investments in response to marriage market expectations. For example, a particularly attractive woman might expect to marry a high income man. Such a woman might find it optimal to make relatively small educational investments due to low expected labor supply and diminishing marginal utility of consumption. In this example, a woman's attractiveness is positively correlated to marriage market expectations and negatively correlated to educational investments. If one does not control adequately for a woman's attractiveness, the estimated impact of education is likely to be biased downwards.

Women likely respond not only to marriage market *expectations*, but also to marriage market *realizations*. To the extent that education has a positive marriage market return, once a woman marries she may no longer find additional educational investments optimal.[13] If a woman happens to marry a higher income husband than expected, reduced labor supply and diminishing marginal utility of consumption will further reduce the incentive to invest. If favorable marriage realizations reduce educational investments, the estimated impact of education will be biased downwards.

In general, unobserved heterogeneity and the joint determination of education and marriage outcomes will bias OLS estimates of the impact of education on marriage outcomes in an indeterminate direction. Instrumenting for education can circumvent this problem as long as the instrument affects a woman's choice of education but has no independent impact on marriage outcomes.

Selection Bias

[13] Additionally, childbearing may increase the cost of going to school. A woman might also find it optimal to work and use the proceeds to invest in her husband's education.

Another obstacle to identifying the effect of education on husband income is selection bias. Husband income is observed only for those women who choose to marry. It seems plausible that the latent quality of marriage offers for those who choose not to marry is somewhat worse. If education has a positive impact on the quality of marriage offers, as education increases, women with marginal marriage prospects may choose to marry. This will pull down the average husband income for highly educated married women and generate a downward bias that instrumenting (or even random assignment of education) cannot solve. In general, one would need an additional variable that affects the probability of marriage but not husband quality to properly address this issue.

Evidence from earlier in the paper suggests that this may be a valid concern. More specifically, Figure 1 maps the change in the probability of marriage associated with higher education. Over most levels of education, there is a positive correlation between the two. The predicted probability of marriage for someone without a high school degree is 12 percentage points lower than for a college graduate. This suggests that increased education may lead women with marginal prospects to marry.

In practice, the severity of this selection bias may vary substantially across applications. In the context of the particular instrumental variables strategy we pursue, we show in the next section that selection bias is of minimal importance. Although the problem may be more severe for other identifying strategies, our estimates appear robust to assuming even very large differences between the distribution of single and married women unobservables.

Timing Bias

Thinking about marriage and education in a dynamic context reveals another difficulty. Educational investments may occur before and *after* marriage. We expect, however, that educational investments after marriage may have a very different impact on husband income than education at the time of marriage. In particular, barring divorce, education after marriage cannot change the identity of a particular husband. This suggests that the relationship between education at the time of marriage and marriage outcomes may be stronger than the relationship between completed education and marriage outcomes. To the extent that one wishes to identify the impact of education at

the time of marriage, performing the analysis using completed education will yield an estimate that's biased downwards. We will refer to this as "timing bias".

To see that this is the case, consider the following simple analysis. Suppose we are interested in how education completed prior to marriage affects marriage outcomes. The regressor of interest is education at time of marriage, which we'll call E_1. The regressor we have available to use is total education, E_T, which is a noisy measure of E_1 due to post-marital education investments, E_2. Consider a simple regression designed to recover the linear effect of education at time of marriage on husband income, *y*. Let the causal relationship be

(4) $$y = \delta_1 E_1 + \delta_2 E_2 + \eta,$$

where we have allowed education after marriage to have an effect that differs from education before marriage. The goal is to recover δ_1. This equation can be rewritten in the following way:

(5) $$y = \delta_1 E_T + \nu,$$

where $\nu = (\delta_2 - \delta_1)E_2 + \eta$ is a composite error term that takes into account that completed education is a noisy measure of education at the time of marriage. Now suppose that we have access to an instrument that is uncorrelated with the error term, η. If the instrument generates variation in post-marital education, it will still be correlated to the composite error term, ν. In this case, the resulting IV estimates will be biased relative to the coefficient of interest (downwards, if one assumes $\delta_1 > \delta_2$). To identify the impact of education at the time of marriage, one needs an instrument that generates variation in education, but only before marriage outcomes occur. Of course this restriction only applies if one wishes to examine the impact of education at the time of marriage. The impact of "total education"—regardless of timing—can still be identified.

6. Quarter of Birth Evidence Regarding the Causal Effect of Women's Education on Marriage Market Outcomes

The multiple factors driving the relationship between education and marriage outcomes make it difficult to interpret the observed correlation. Still, a careful

examination of the causal relationship between a woman's education and marriage outcomes is interesting for a number of reasons. First, it underscores the need to take into account marriage outcomes when examining the impact of education on women's lives. Second, it sheds light on the potential importance of school as a marriage market. Third, to the extent that the relationship between education and husband quality is in large part causal, a large return to education in the marriage market provides insight into why many women choose to invest heavily in education, even though the returns in the labor market may be limited by sporadic labor force participation. In the remainder of the paper we use an IV strategy to provide evidence regarding the causal effect of education on marriage outcomes for a set of women whose educational attainment was affected by their quarter of birth.

Of the problems discussed above, unobserved heterogeneity and joint determination can be *solved* by instrumenting for education. To solve the selection and timing criticisms, one must estimate a dynamic joint model of labor and marriage markets for men and women. This requires exclusion restrictions across equations as well as at least some functional form assumptions for unobserved variables.[14] Exclusion restrictions are often difficult to come by and often not as compelling as one might like. Pending innovation on this front, we defer this approach to future work. Although the selection and timing problems are not *solved* by our instrument, we provide evidence that the estimates are reasonably robust to these problems.

Instrumenting for education can solve problems of unobserved heterogeneity and joint determination. This requires observable variation in a determinant of women's education that does not affect marriage probability and husband income directly. Most of the instruments that have been used in the past to examine the labor market returns to education for men are unlikely to be successful for investigating marriage outcomes.

In education research, one oft-used instrument is the historical change in compulsory schooling laws across the United States. The idea is that compulsory schooling laws create a change in the costs of schooling—generating variation in

[14] Ge (2006) presents and estimates a model of women's marriage, education, and labor market decisions. Her model is identified primarily through distributional assumptions as opposed to exclusion restrictions.

educational attainment. This approach will not work in our context, because any change in schooling laws will change the schooling of all potential husbands and competing women. Thus the variable affects husband income and quality regardless of a woman's own education. The same goes for instrumenting using distance from college.[15] In fact, *any* variation caused by aggregate changes in education policy or residential location is likely to affect women in the sample, their potential husbands, and the competitors with whom they are vying for a good husband. For this reason, such instruments are likely to yield coefficient estimates that are hopelessly biased.

A valid instrument would be one that varied across women within a given time and place—thus imposing no general equilibrium effects. Many instruments fitting this description are unsatisfying for other reasons. For example, family background characteristics are almost certain to have independent effects on marriage outcomes, outside of their effect on education, thus failing to provide identifying information. One strategy that could work is to use birth quarter as an instrument as it meets the stringent test of providing individual-specific variation. In the appendix, we more formally discuss instrumental variables identification in the context of a simple behavioral model.

Angrist and Krueger (1991) discuss how birth quarter interacts with a January first enrollment cutoff and compulsory schooling laws to generate variation in the level of schooling. For example, 16 year olds born in January will have completed less schooling when they are able to drop out than similar individuals born in December. This is because they will have enrolled in school a year later but be eligible to drop out at roughly the same time. The authors show that children born in the fourth quarter of 1960 started school about .4 years later than students born in the first quarter. Angrist and Krueger also use variation in compulsory schooling laws across states to show that these laws drive the relationship between quarter of birth and school enrollment.[16] These findings suggest that quarter of birth may be an appropriate instrument for educational attainment. If birth quarter generates variation in the cost of education that is not

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[15] See Card (1995).

[16] Lleras-Muney (2001) provides additional evidence that compulsory schooling laws affected educational attainment in the first part of the twentieth century.

otherwise correlated with marriage outcomes, the instrument solves the unobserved heterogeneity and joint determination problems discussed above.

Conveniently, the nature of quarter of birth mitigates the timing bias discussed earlier. Birth quarter has predictive power primarily for secondary school attendance and high school completion. To the extent that nearly all women marry after this point, our instrument induces variation in education that occurs prior to marriage. This solution is not perfect. In the 1980 Census data, 14.8 percent of women are married by the age of 18, suggesting that there is still room for problems with consistency. On the other hand, the bias should still be much smaller than might result when estimating effects through college and graduate work—about 70 percent of ever-married women in the sample were first married between the ages of 18 and 24.

The other problem which instrumenting does not address is selection. This problem does not appear to be severe in the context of our strategy; we show later that after instrumenting with birth quarter, education has no significant effect on the probability that a woman is married. Furthermore, the estimates are extremely robust to assumptions about the latent marriage offers of unmarried women. It therefore seems plausible that the observed marriage effects are not substantially biased due to selection into marriage.

Because quarter of birth does not have large problems with timing bias or selection, and offers individual-level variation that is free of general equilibrium effects, these instruments meet the specific requirements for identifying the impact of education on marriage outcomes. It is important, however, to consider whether quarter of birth satisfies the conditions *generally* necessary for satisfactory instrumental variables identification. Instruments will yield biased estimates if they are only weakly correlated with the endogenous variable, they do not have a monotonic impact on the endogenous variable, or they fail to meet the necessary exclusion restrictions. To evaluate the usefulness of quarter of birth, it is worthwhile to consider each of these concerns in turn.

First, if instruments are only weakly correlated with the endogenous variable, the second stage estimates will be biased toward the OLS coefficients. Staiger and Stock (1997) show that the finite-sample bias is approximately proportional to the inverse of the F-statistic of the instruments in the first stage. Bound et al. (1995) discuss this source of

bias specifically in the context of quarter of birth instruments. In Angrist and Krueger's (1991) original work, weak instrument bias arises as a substantial problem since quarter of birth is interacted with state fixed effects. The resulting first stage F-statistic of their myriad instruments is quite low. We use a more parsimonious specification that alleviates this particular concern.

Second, Imbens and Angrist (1994) discuss the consequences of instruments that have a non-monotonic relationship with the endogenous variable. They note that if an instrument encourages some agents to receive treatment but simultaneously discourages others from doing so, the resulting estimator is uninterpretable as a treatment effect for *any* group of individuals. Examining the differences in the education CDF of women born in the first and fourth quarters, it appears that quarter of birth at least has a monotonic impact across education levels. Furthermore, the mechanism through which quarter of birth plausibly affects educational investments also suggests a monotonic effect.

Finally, quarter of birth may not satisfy the necessary exclusion restriction. In other words, quarter of birth may be correlated to education but may also affect marriage outcomes through some other mechanism. Bound, Jaeger, and Baker (1995) cite a number of studies suggesting that quarter of birth may be correlated to such diverse afflictions as schizophrenia (Sham et al., 1992) and autism (Gillberg, 1990). Furthermore, we show that for children between the ages of 5 and 15 in the 1960 Census, quarter of birth is related to family income. This evidence raises concerns that quarter of birth may not satisfy the required exclusion restriction. There is no way to *assure* the validity of our assumed exclusion restrictions. Later in the section, however, we consider a variety of robustness checks and a specification test of the joint validity of our instruments. Our findings are robust to these checks, which increases our confidence in the usefulness of the estimates.

Implementation of the Quarter of Birth Identification Strategy

To implement the quarter of birth identification strategy, we specify the following second stage equation:

(6) $y = \delta^{IV} E + X\beta + \varepsilon$,

where y represents the marriage outcome of interest, E is now years (not categories) of education, X is a vector of control variables, and ε is a residual. In this framework, δ^{IV} represents the causal effect of an additional year of education on future marriage outcomes. As Figure 2 makes clear, a *linear* specification is inconsistent with the highly nonlinear correlation between education and marriage outcomes. As we discuss below, birth quarter is only able to identify a *local* treatment effect, centered around the end of high school. Thus we hope to identify the average linear effect of additional secondary education for the subsample of women affected by quarter of birth. For the range of education levels and the subset of women examined, a linear education effect is likely to be an adequate, albeit still imperfect, approximation. The comparable OLS estimate captures an average *linear* association between women's education and marriage outcomes. Naturally, the association for any particular set of women need not be close to the OLS estimate. Furthermore, because the OLS weights observations differently from IV, the resulting coefficient need not be similar—even if education is exogenous.

To deal with correlation between the error term and education, we specify the following first stage equation:

(7) $$E = \alpha_0 * Q2 + \alpha_1 * Q3 + \alpha_2 * Q4 + X\Gamma + \nu,$$

where $Q2$, $Q3$, and $Q4$ are dummy variables that take on a value of 1 for women who were born in the second, third, or fourth quarter respectively. It is assumed that birth quarters are excludable from (6) because they capture only that variation in educational attainment induced by the interaction of quarter of birth with compulsory schooling.

We use white women between the ages of 30 and 45 in the 1980 PUMS extract. This is the last year for which quarter of birth information was recorded. The switch to measuring education in years instead of categories is required by the change of data. Our choice of covariates is also limited by what is available in the 1980 Census. In addition to state of birth fixed effects (state of residence could be endogenous), we control for a second order polynomial in age.[17] Doing so ensures that our estimates are unlikely to be affected by a mechanical correlation between quarter of birth and a woman's age. Angrist and Krueger (1991) and Acemoglu and Angrist (1999) control for year of birth

[17] The age variable takes into account differences in age across women born in different quarters.

dummy variables.[18] Because one of the primary sources of variation we want to use is between the fourth and first quarters of adjacent years, we control for a smooth function of age rather than use birth year dummies that eliminate this source of variation.[19]

In our preferred specification we choose to instrument only with quarter of birth indicator variables. While interacting quarter of birth with state of birth or year of birth may increase the power in the first stage, it can also lead to bias associated with the use of multiple weak instruments (see for example Bound, Jaeger, and Baker, 1995 and Staiger and Stock, 1997). For this reason, we rely on a very parsimonious set of instruments. We do, however, perform robustness checks in which we control for age category dummies and instrument using these dummies interacted with quarter of birth.

Correlation between Quarter of Birth and Educational Attainment

Before describing our IV estimates, we document the relationship between quarter of birth and educational attainment for women in our sample. For each woman we detrend the data by subtracting the mean education outcome of women born in the two quarters before and two quarters after her birth.[20] Table 4 shows that women's education is monotonically increasing in the quarter of birth—this is consistent with women born earlier in the year becoming eligible to drop out at a lower grade. The F-statistic of the joint significance of these coefficients exceeds 35. The results suggest that a woman born in the fourth quarter obtains about .08 years more education than women born in the first quarter. Examining alternative measures of educational attainment, it is apparent that quarter of birth has a particularly large effect on the probability that a woman completes high school. The relationship between quarter of birth and higher levels of educational attainment is weaker, though still statistically significant. The relationship also changes in nature. While the third and fourth quarters are associated with higher secondary education, only the second quarter is associated with higher levels of college education.

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[18] Acemoglu and Angrist (1999) acknowledge in a footnote the possibility that their estimates may be biased due to inadequate controls for age. They assert that for the men they are examining, the age-earnings profile is sufficiently flat that this is not a problem.
[19] Bound and Jaeger (1996) also make this point.
[20] This is identical to the detrending done in Angrist and Krueger (1991).

This suggests that the correlation between quarter of birth and higher education is unlikely to operate through compulsory schooling—at least for women in our sample.

One possible explanation for this is that quarter of birth among white children between the ages of 5 and 15 in the 1960 Census is correlated to family income.[21] More specifically, children born in the second quarter are in families with incomes that are $171 higher than children born in the first quarter. This difference is highly significant with a t-statistic exceeding 6. There is no statistically significant difference between the family incomes of children born in the first, third, and fourth quarters. This evidence suggests that the correlation between second quarter birth and higher levels of secondary education is likely driven by differences in family income. To the extent that this is true, using variation in second quarter birth as an instrument may be akin to using differences in family background. There is no evidence, however, that the high levels of education associated with a third or fourth quarter birth are driven by family background. We consider possible biases from this second quarter effect later when we check the robustness of the estimates.

Estimates of the Effect of Education on Marriage Outcomes

Having shown that quarter of birth is indeed correlated to a woman's educational attainment, we will now describe the results from the IV estimation strategy. Columns 1 and 2 of Table 5 show OLS and IV estimates of the linear effect of women's education on the probability that they are married in 1980. Examining column 1, we see that the OLS estimate of the linear effect of education on marriage is extremely small, though statistically significant. An additional year of education reduces the probability of marriage by less than one tenth of one percentage point. Column 2 shows the estimates from our IV strategy. Examining the column, we see that the quarter of birth instruments have an F-statistic of over 20 in the first stage, suggesting that the bias due to over-fitting in the first stage is likely to be minimal. Additionally, a chi-squared test of overidentifying restrictions is insignificant.[22] The IV point estimate on education

[21] We choose this range of ages because they would have been nearly the same age as the women in our 1980 sample. Additionally, they would have been young enough to still be at home.
[22] See Newey (1985).

suggests that an additional year of schooling reduces the probability of marriage by about 1.5 percentage points. This effect is statistically insignificant, however. Unfortunately, the lack of precision on the point estimate makes it difficult to rule out a moderate sized effect of either sign. The fact that the IV estimate of education on the marriage probability is relatively small and statistically insignificant is fortunate for us. It suggests that selection into marriage is unlikely to be an important source of bias when examining measures of husband quality.

In Specifications 3 and 4, we explore the linear relationship between women's education and husband yearly income. We do so in levels because some husbands have zero or negative incomes; excluding such men from the analysis may bias our estimates. Specification 3 suggests that an additional year of education is associated with an increase in husband earnings of $1,665. From Specification 4, we again see that our instruments have a fairly high first stage F-statistic and that the test of over-identifying restrictions does not suggest invalid instruments. The IV point estimate of $4,129 is more than twice as large as the OLS estimate of $1,665. This coefficient is statistically significant with a t-statistic of nearly 4. The standard errors are sufficiently large, however, that a fairly broad range of effect sizes (though not the very precise OLS point estimate) would be captured in a 95 percent confidence interval. This is a large effect—representing about *18 percent* of the average annual earnings of husbands in the sample. As a causal effect, this implies that the marriage market can have enormous repercussions for the future family income of women affected by our instrument.

In Section 4, we use several strong assumptions to form a rather simplistic comparison between the returns to education for women operating through the labor market as opposed to the marriage market. We found that for most women, using the OLS coefficients, about half of the correlation between education and wellbeing is attributable to the marriage market. We can now reproduce this analysis using the IV estimates.

Since there is little evidence of a causal effect on the probability of marriage (dp/dE), we simplify the calculation by assuming it to be zero. Thus, all we need are labor market returns to own education for married and single women and the probability of being married. In 1980, 79 percent of those with 11 years of schooling were married.

The effects of education on labor market earnings for single and married women are computed using the same birth quarter IV strategy and are subject to the same caveats. We separate the sample into married and single women and run separate IV regressions of own income on education using the same control variables. For married women, we find an own income return of $1,865 and for single women, $2,106. Plugging this into equation (3) we compute that fully 58 percent of the rise in women's consumption due to education operates through the marriage market, not the labor market. This is inline with the estimates found in Table 3. Of course, as we discuss in Section 4, this calculation is only an approximation and should be viewed as suggestive.

Understanding the Local Average Treatment Effect

Having described our IV estimates of the effect of education on marriage outcomes, it is important to emphasize that even if the instruments are valid, the estimated treatment effects correspond to a very specific local average treatment effect (LATE).[23] Unless education has a constant linear treatment effect, our estimate is informative only for those women whose educational attainment was affected by their quarter of birth. Given the non-linearity in the OLS relationship between education and husband income and the difference across the distribution of effects, the estimated treatment effect is unlikely to generalize to other education levels.[24] While the scope of our empirical evidence is somewhat narrow, it lends credence to the hypothesis that education may be an important causal determinant of marriage outcomes for many women.

We examine the distributional difference in education between women who were born in the first and fourth quarters (the quarters with the largest difference in educational attainment) to provide insight regarding which women were impacted by quarter of birth. Angrist and Imbens (1995) show how the difference in CDF's across birth quarters is proportional to the weight placed on each individual year of schooling in the LATE. To

[23] See Imbens and Angrist (1994).
[24] The IV estimates also do not address the problems that result if education has different effects for those of high and low ability, as suggested by the quantile estimates provided in Figure 4. The discussion in the paper is based on the assumption of a locational model where effects are the same across the distribution of error terms.

perform this analysis, for each year of schooling we regress whether or not an individual completed that amount of education or less on a fourth quarter dummy variable (we omit individuals born in the second and third quarters), age, and age squared. The coefficients on the fourth quarter indicator variable represent the difference in CDF at a given level of education. Figure 5 shows these differences.

It appears that quarter of birth is most highly correlated to changes in secondary school attendance—with the largest impacts operating between grades 9 and 12. This is generally consistent with the effects one might expect from an instrument operating through compulsory schooling. They are also quite similar to Angrist and Imbens' (1995) findings for men during the same period. This figure provides some credibility to the instrumental variables strategy. Additionally, it suggests that the IV estimates are likely to reflect the impact of education on a very different set of individuals than the OLS. This is one candidate explanation for the fact that the linear IV estimate is much larger than the corresponding OLS estimate.

To evaluate this explanation, we estimate the year-by-year relationship between women's education and marriage outcomes—much as we did earlier in the paper except we focus on women in the 1980 Census sample. Figure 6 shows the relationship between education and probability of marriage. The figure suggests that over the years of education used to identify our LATE, the OLS relationship between education and probability of being married is fairly small—well within a 95 percent confidence interval of the IV estimate. Weighting each of the OLS coefficients by the weights implied by Figure 5 yields an estimate quite close to 0. This estimate weights each year of schooling in a way that is comparable to the IV estimate.

Figure 7 shows the year-by-year correlation between women's education and husband income. All of the coefficients are substantially below the IV estimate of over $4,000. The coefficients on the 12^{th} and 13^{th} years are somewhat closer at over $3,000. Again, one can use the weights implied by Figure 7 to construct a linear education effect from the year-by-year OLS. The procedure yields an estimate of $1,669, which is still outside of the 95 percent confidence interval of the IV coefficient.

Why Might the IV Estimate Be Larger than the OLS Estimate?

It is perhaps surprising the IV estimate is so much larger than comparable OLS estimates—even taking into account which years of education are affected by quarter of birth. This is typical, however, when examining labor market returns to education. Summarizing the literature, Card (2000) examines 11 quasi-experimental studies examining the labor market return to education for men. He finds that in virtually all of the studies, the IV estimate of the return to education exceeds the OLS estimate. This is true whether the instrument induces variation in higher education (see Card, 1995; Kane and Rouse, 1993; Lemieux and Card, 1998; and Conneely and Uusitalo, 1997) or secondary education (see Staiger and Stock, 1997 and Harmon and Walker, 1995).[25] Examining mortality for men and women, Lleras-Muney (2002) uses compulsory schooling to identify IV effect sizes on mortality that are *three to four* times larger than the OLS estimates for both men and women. In light of the broader literature on the returns to education, finding IV estimates that are twice as large as the OLS estimates is unsurprising. Indeed, estimating IV returns to education *smaller* than the corresponding OLS estimates would be out of line with the broader literature.

Even though finding high IV estimates is not particularly unusual, it is still helpful to understand why and how this could be the case. One possible explanation is negative selection into education (i.e. women with the best expected or realized marriage outcomes cut short their educational investments). Given the time period, this seems plausible for women who are deciding whether to finish college or go to graduate school. This explanation seems less compelling for women considering the completion of an additional year of secondary school. A second possible reason for the high returns is measurement error in our education variable. According to Card (2000), however, this can account for at most a 10 percent difference between the OLS and IV effects. In our opinion, the most plausible explanation is that there exist heterogeneous returns to education (even compared to other women with a similar level of education) and our instruments generate the most variation in the education of those women with relatively large benefits of additional schooling. While this is inherently difficult to test,

[25] Because the IV estimates exceed the OLS estimates for instruments that generate variation in both high school and college attendance, it is implausible that the difference between the OLS and IV could simply be explained by the IV estimates overweighting those years of education that offer the highest returns.

Chernozhukov and Hansen (2003) use quarter of birth and an instrumental quantile regression method to estimate the labor market returns to education for men across the distribution of unobservables. In percentage terms, men at the bottom of the earnings distribution enjoy the largest returns to education.[26] Their findings suggest that the returns to education can vary substantially—even for individuals with similar levels of schooling. This lends credence to the hypothesis that our large IV estimate could be caused by heterogeneity in the marriage market returns to education.

Robustness Checks

Having described the quarter of birth estimates of the causal impact of women's education on marriage outcomes, it is helpful to examine in more detail the possible shortcomings of the strategy. As mentioned earlier, the problems with quarter of birth are several. First, quarter of birth (along with every other instrument) does not correct the bias associated with the non-random selection into marriage. Second, weak instruments can generate asymptotic bias toward the OLS coefficients due to over-fitting in the first stage. Third, if the instrument encourages some women to increase education and discourages others, the resulting IV estimate will fail to identify a valid LATE. Fourth, weak instruments can lead to strongly biased estimates if the instruments have even a small correlation with the error term.

The fact that after instrumenting, education has no significant effect on the probability of marriage suggests that the importance of selection is likely to be minimal. To further investigate the possibility, we used our IV coefficients to impute latent husband qualities for unmarried women. We then made varying assumptions regarding the residual for these women. Assuming that unmarried women had latent husband incomes of one standard deviation less or more ($16,875) than similar married women changed the point estimates by less than $100. The same was true if we assumed that the residuals for unmarried women were strongly negatively or positively correlated to

[26] Though not reported, it seems that men at the top of the earnings distribution enjoy the largest absolute benefit from schooling.

education. In essence, the instrumented relationship between education and marriage is so small that selection is not a concern.

We can examine the second problem directly by testing the power of our instruments in the first stage. Fortunately, using a more parsimonious set of instruments than Angrist and Krueger (1991) greatly limits the impact of this type of bias for the analysis.[27] Given that our F-statistic exceeds 15 for all specifications, the bias due to over-fitting in the first stage is likely negligible.

It is difficult to definitively prove that quarter of birth has a monotone impact on education. The mechanism through which quarter of birth plausibly affects educational attainment, however, certainly suggests that the impact should operate in a single direction. Figure 5 suggests that relative to a first quarter birth, a fourth quarter birth is associated with (weakly) higher education levels across the entire support of the education distribution. This is consistent with a monotonic effect (i.e. a fourth quarter birth doesn't increase secondary school attendance yet decrease college attendance). Of course, the underlying assumption, which we cannot prove, is that *no* woman responds to the instrument by *lowering* her education level. To the extent that this occurs, and we find it unlikely to be widespread, the resulting estimates are inconsistent.

Before proceeding to robustness checks, several pieces of existing evidence suggest that quarter of birth is likely to satisfy the exclusion restriction necessary for satisfactory identification. First, Acemoglu and Angrist (1999) find that estimates of the labor market return to education are similar whether one uses quarter of birth instruments or variation over time in compulsory schooling laws. Given that these instruments affect the same types of individuals, this evidence supports the notion that the quarter of birth instruments are valid. Second, if the quarter of birth instruments are invalid, one would expect that the tests of over-identifying restrictions would be rejected.[28] We fail to reject the null hypothesis that the instruments are jointly valid.

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[27] Angrist and Krueger (1995) assert that when examining the effect of education on male earnings, using thirty quarter of birth instruments yields estimates that are approximately unbiased.

[28] One may be concerned that a test of over-identifying restrictions has no content when all of the instruments are so closely related. For example, if quarter of birth generates variation only through its correlation with family background, each instrument might yield a second stage coefficient estimate that was biased in a similar way. In this case, a test of over-identifying restrictions would fail to reject the null

Though the evidence discussed above does not suggest that the IV estimates are biased, it is still worthwhile to examine several specific concerns. Table 6 shows the results from a number of robustness checks designed to ascertain the validity of our instruments. One potential concern is that there exists a mechanical correlation between quarter of birth and age. In Row 2 of Table 6, we show the estimates when we control for a fourth order polynomial in women's age. This has no substantial effect on the estimated impact of education on the probability of marriage but dramatically increases the estimated impact on husband earnings. The standard errors are larger, however, and the new estimate is not statistically significantly different than the baseline estimate. The large standard errors suggest that a fourth order polynomial may induce too much flexibility into the age equation—especially given that we use only a sixteen year age interval. In Row 3, we use 3 age category dummies along with our second order polynomial.[29] This specification yields results very similar to the baseline estimates. In Row 4, we control for the age category dummies and instrument using quarter of birth interacted with these dummies. This allows quarter of birth to have a different effect on older and younger cohorts. This specification again yields results that are quite similar to the baseline estimates.

A further concern is that quarter of birth is correlated to parental characteristics. We earlier showed evidence that a second quarter birth was correlated to higher family income. We attempt to address this concern by excluding variation in education attributable to a second quarter birth. In Row 5, we instrument using only third and fourth quarter birth (second quarter birth is a control variable). Doing so yields estimates that are quite close to our baseline estimates. Another way to address the concern that quarter of birth is correlated to family income is to control for a proxy of a woman's parents' income. We do so by constructing a measure of average childhood family

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hypothesis that the instruments are jointly valid. On the other hand, to the extent that only a second quarter birth is associated with high family incomes, the variation in second quarter birth (relative to first) could yield a biased IV estimate of the return to education that was substantially different than the estimate generated by a third or fourth quarter birth. In this case, our test of over-identifying restrictions, if sufficiently powerful, would reject the null hypothesis that the instruments are jointly valid.

[29] The age categories are 30-34, 35-39, and 40-45.

income that varies by state and quarter of birth.[30] Row 6 shows that controlling for this measure, the results do not change substantially.

Row 7 limits the sample to women married after the age of 25, when schooling decisions are largely complete. If this estimate is very different, it would suggest that timing bias may be important. Although imprecise due to the smaller sample size, the estimate changes very little from the original. Finally, in our primary specification we treat each quarter of birth as a separate instrument. If quarter of birth affects educational attainment primarily through the proximity of a woman's birthday with a December 31st enrollment cutoff, instrumenting education with a linear measure of birth quarter should yield similar results. In Row 8, we show that doing so has no substantive effect.

Overall, the robustness checks do not suggest that quarter of birth instruments are invalid. To the extent that they are valid, it would appear that education has a strong positive causal effect on husband quality, at least for women affected by compulsory schooling. Since OLS estimates suggest that there may be substantial heterogeneity in treatment effects, the identified parameter probably does not readily generalize to other women. Furthermore, sufficient remains unknown about how birth quarter affects life outcomes that additional evidence on the causal effect of women's education on marriage outcomes would be very useful.

7. Conclusion

The evidence in this paper suggests that one of the primary avenues through which education is correlated with women's well-being is through the marriage market. Back of the envelope calculations suggest that for women in the 2000 Census about half of the increase in a woman's available income predicted by higher education is attributable to the marriage market. Inasmuch as marriage generates non-pecuniary benefits as well, the marriage market could be an even more important avenue through which education increases women's welfare. Given the importance of the relationship between education and marriage market outcomes, we believe that additional theoretical

[30] We construct this measure using data on children who were between the ages of 5 and 15 in the 1960 1 percent Census Public Use Sample.

and empirical research should be done in the area. Useful stylized facts that should be further investigated include the following:

- Between the end of secondary school and college completion, increases in education are associated with higher probabilities of being married. Most of this is driven through marriage stability (or the rate of remarriage) as opposed to the probability that a woman ever marries.
- Education is generally associated with large increases in husband earnings. College completion appears to be a particularly important step—it is associated with an increase in husband earnings that exceeds $22,000.
- In levels, women's secondary school education is associated with roughly constant increases in husband income across the distribution of husband earnings. As a percentage of the husband's income, however, the effects are much larger at the bottom of the distribution. College and graduate education, on the other hand, are associated with enormous gains at the top of the husband earnings distribution. It would appear achieving these high levels of education dramatically increases access to very high earning husbands.

These relationships may not reflect a causal effect of women's education on marriage outcomes. Not only are marriage and education jointly determined, but the types of women who choose to invest heavily in education are likely to be systematically different than those who invest less. Selection and timing bias also complicate identification.

We provide evidence regarding the causal impact of education on marriage outcomes using a quarter of birth identification strategy. Focusing on women from the 1980 Census we confirm that quarter of birth is correlated with the educational attainment of these women—primarily in the places one would expect given compulsory schooling. The IV estimates suggest that women's education does not have a strong effect on the probability of being married but dramatically increases husband income. For women whose educational attainment was affected by quarter of birth, an extra year of schooling increases husband earnings by over $4,000. Examining a number of robustness checks, we do not find systematic evidence that the IV strategy yields biased

estimates. While this evidence applies only to a small subset of women in our sample, it lends credence to the hypothesis that education may have a causal impact on the marriage prospects of many women.

While this paper presents important evidence regarding the importance of education for women's marriage outcomes, the relationship between education and marriage outcomes begs for additional research. Due to concerns regarding the validity of quarter of birth instruments, and the difficulty in generalizing local treatment effects, further attempts to address the causality of the relationship would be welcome. Additionally, our paper only considers one side of the marriage market, ignoring the potential consequences of educational investment for men's marriage outcomes. Examining the male side of the market is likely to be substantially more complicated than the current analysis. Due to the often intermittent nature of female labor force participation, wife income is likely to be a poor proxy for the quality of a man's marriage outcome. Despite the challenges, however, it is likely that the marriage market is also an important avenue through which education can positively impact men's wellbeing.

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Appendix

In this Appendix we provide a simple model to motivate the empirical work.[31] The model could be expanded to include dynamic issues, such as the timing bias discussed in the text. As it stands, it provides a justification for the need for instrumental variables and the first–stage equation (7), specified in the text. It also models the potential for selection bias.

The model has two periods. In the first period, a woman chooses an education level, E, at cost, $C(E,\theta)$, where θ is a vector of cost shifters. In the second period she earns $I(E,\alpha)$ from the labor market; α is a set of variables that affect a woman's own income. In the second period, each woman also receives one draw from the distribution of husbands,[32] which she may accept or reject. In this simple framework, husband income, $H(E,\gamma,\eta)$, is an increasing function of education. γ and η are vectors of variables that affect the quality of the husband draw.[33] γ is known to the woman prior to the educational investment and may include factors such as physical attractiveness, personality, intelligence, and family background. η is not observed by the woman at the time of her educational investment and captures random factors in the matching process.

If a woman chooses not to marry, her lifetime utility as a single woman (we abstract from discounting) is a concave function of her income, $U(I)$, minus her cost of education, C:

$$U\left[I(E,\alpha)\right]-C(E,\theta) \tag{A1}$$

Married utility is family income with discount $\omega \in (0,1)$ that combines a sharing rule with economies of scale:

$$U\left[\left(I(E,\alpha)+H(E,\gamma,\eta)\right)\omega\right]-C(E,\theta). \tag{A2}$$

Given her first period choice of education and draw of husband quality, a woman chooses to marry if the utility associated with marriage exceeds that of remaining single. This will hold if

$$H(E,\gamma,\eta) > \frac{1-\omega}{\omega} I(E,\alpha). \tag{A3}$$

Intuitively, a woman will only choose marriage if her husband draw exceeds a cutoff that is related to the woman's own earning potential. High earning women need to share a

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[31] We readily acknowledge that our model is far too simple to capture all of the emotional, financial, and idiosyncratic factors underlying peoples' marriage decisions. The model does highlight, however, what we view as the principal challenges to identification.

32 One draw could be reinterpreted as the best of multiple draws.

[33] This should be viewed as a reduced form function resulting from the optimization problem of the potential husbands.

larger sum of money with their husbands and thus find marriage to a husband of a given quality less attractive.

Given this second period marriage decision rule, a woman wishing to maximize expected utility chooses education in the first period to solve the following problem.

$$\max_E E_\eta \left\{ \max \left\{ U\left[(I(E,\alpha) + H(E,\gamma,\eta))\omega \right] \right\}, U\left[I(E,\alpha) \right] \right\} - C(E,\theta) \right\}. \tag{A4}$$

Without imposing additional restrictions, the optimal choice of education is a function of the whole set of known parameters and the distribution F_η of the unknown parameter, η, which may differ across individuals. In other words, women base educational investments on the financial and psychic costs of investment and expectations about labor market and marriage prospects. The equilibrium level of education can be denoted:

$$E = f(\omega, \alpha, \theta, \gamma, F_\eta). \tag{A5}$$

For empirical analysis it is helpful to add additional structure to the problem. We assume that both *H* and *I* can be well approximated by strongly separable and linear functions, so that $H = \gamma + \delta_H E + \eta$ and $I = \delta_I E + \alpha$.[34] We then rewrite equation (A3) as:

$$\eta > -\gamma + \frac{1-\omega}{\omega}\alpha + \left(\frac{1-\omega}{\omega}\delta_I - \delta_H \right) E(\omega, \alpha, \theta, \gamma, F_\eta), \tag{A6}$$

where δ_H and δ_I are the linear effects of education on husband and own income respectively. If F_η is symmetric, the probability of marriage is simply:

$$\Pr(married) = F_\eta \left[\gamma - \frac{1-\omega}{\omega}\alpha + \left(\delta_H - \frac{1-\omega}{\omega}\delta_I \right) E(\omega, \alpha, \theta, \gamma, F_\eta) \right], \tag{A7}$$

which can in principle be estimated. This equation is helpful for two reasons. First, it shows that additional education does not necessarily increase the probability of marriage—even if husband quality is a strictly increasing function of education. Taking the derivative of the above with respect to education, one finds that $\frac{\partial \Pr(married)}{\partial E} = f_\eta * \left(\delta_H - \frac{1-\omega}{\omega}\delta_I \right)$. Thus, whether education increases or decreases the marriage rate depends on whether education has a stronger effect on husband quality, δ_H, or own income, δ_I, after discounting for the sharing rule ω. If education has a

[34] The linear form may be an acceptable approximation but still problematic in the context of an economic model. The *H* function maps a wife's ability to find a husband into the distribution of available husbands. Since these draws at least partially reflect the distribution of husbands' income, the curvature of the husband distribution should be reflected in the *H* function; which would likely violate both the linearity and separability we assume.

sufficiently strong effect on a woman's own labor market prospects, she may no longer find it optimal to enter marriage and share her income.[35]

Second, equation (A7) specifies what is required to identify the impact of education on the probability of marriage. Education is endogenous to marriage and labor market expectations. Without sufficient controls, education will be correlated to a composite error term that includes not only η but also elements of γ and α—biasing the estimated impact of education. More intuitively, women who find it optimal to invest heavily in education are likely to differ systematically in terms of marriage prospects and labor market productivity from those who invest little. This unobserved heterogeneity is an important source of bias that can be difficult to address.

Our approach is to instrument education using variation in the cost of education, θ.[36] To do so correctly, we need an element of θ that is not also an element of γ or α. Many factors that affect the cost of education, such as intelligence, are likely to have independent effects on marriage and labor market prospects and are thus elements of α and/or γ—rendering them unfit for use as instruments. As noted in the text, distance to college and compulsory schooling laws also fail the needed exclusion restriction, since both sources of variation affect potential husbands' education choice, and thus are likely to be elements of γ.

Estimating the causal impact of education on husband income presents similar challenges. Since education is itself a function of expected husband quality, the correlation between a woman's education and her husband's income cannot be interpreted as causal. Given the assumptions above, the equation we would like to estimate has the following form:

(A8) $$H = \gamma + \delta_H E(\omega, \alpha, \theta, \gamma, F_\varepsilon) + \eta \, .$$

If we could observe *H* for all women, we could identify δ_H either by controlling adequately for γ or instrumenting education using an element of α or θ that is not also included in γ.

Unfortunately, husband income is observed only for those women who choose to marry. For women who choose to marry, the error term will satisfy equation (A6). Thus the expected residual for those choosing marriage will itself be a function of *E*. *Any* change in E induces a compositional change that alters the expectation of observed residuals. This generates a downward bias that instrumenting cannot solve.

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[35] In a more general model, the sign of the education effect depends on whether education has a larger effect on utility in the married or single state.

[36] One could also attempt to control for marriage and labor market expectations. This is likely to be quite difficult. Doing so implicitly takes advantage only of variation in education driven by θ.

Table 1: Summary Statistics for 2000 Census.

Variable	Mean	Variable	Mean
Age	37.86 (4.52)	Husband in Labor Force	0.92 (0.27)
Currently Married	0.69 (0.46)	Husband Total Income	57,350 (59,542)
Never Married	0.12 (0.33)	Husband Total Earnings	54,565 (56,541)
In Labor Force	0.74 (0.44)	*Husband Education*	
Total Income	24,921 (29,982)	5 to 8 Years	0.01 (0.11)
Total Earnings	23,179 (28,513)	9 Years	0.01 (0.11)
Total Earnings for Workers	29,371 (29,290)	10 Years	0.02 (0.13)
Education		11 Years	0.02 (0.14)
5 to 8 Years	0.01 (0.09)	12 Years, no Diploma	0.02 (0.15)
9 Years	0.01 (0.10)	12 Years, Diploma	0.28 (0.45)
10 Years	0.02 (0.13)	Some College	0.31 (0.46)
11 Years	0.02 (0.13)	College Graduate	0.21 (0.41)
12 Years, no Diploma	0.02 (0.14)	Master's or Professional Degree	0.10 (0.30)
12 Years, Diploma	0.28 (0.45)	PhD	0.01 (0.11)
Some College	0.34 (0.47)		
College Graduate	0.21 (0.41)		
Master's or Professional Degree	0.09 (0.28)		
PhD	0.01 (0.07)		
Observations	1,170,626		

Notes for Table 1: The table contains summary statistics for women who were between the ages of 30 and 45 in 2000. Standard deviations are in parentheses.

Table 2: Summary Statistics for 1980 Census.

Variable	Mean	Variable	Mean
Age	36.74 (4.31)	*Quarter of Birth*	
Currently Married	0.79 (0.41)	First	0.24 (0.42)
Never Married	0.07 (0.25)	Second	0.26 (0.44)
In Labor Force	0.64 (0.48)	Third	0.25 (0.43)
Total Income	6,310 (7,612)	Fourth	0.25 (0.43)
Total Earnings	5,718 (7,163)	Husband in Labor Force	0.97 (0.18)
Total Earnings for Workers	8,586 (7,321)	Husband Total Income	22,579 (16,875
Education		Husband Total Earnings	21,620 (16,926
Total Years	12.77 (2.57)	*Husband Education*	
Less than High School	0.16 (0.37)	Total Years	13.18 (3.23)
High School Graduate	0.46 (0.50)	Less than High School	0.19 (0.39)
Some College	0.19 (0.39)	High School Graduate	0.36 (0.48)
College Graduate	0.10 (0.31)	Some College	0.18 (0.38)
Graduate Education	0.08 (0.27)	College Graduate	0.12 (0.33)
		Graduate Education	0.15 (0.36)
Observations	835,934		

Notes for Table 2: The table contains summary statistics for women who were between the ages of 30 and 45 in 1980. Standard deviations are in parentheses.

Table 3: Examining the Fraction of the Correlation between Women's Financial Wellbeing and Education that Operates through Marriage Market.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	I_m	$\frac{dI_m}{dE}$	I_s	$\frac{dI_s}{dE}$	H	$\frac{dH}{dE}$	p	$\frac{dp}{dE}$	$\frac{\frac{dH}{dE}}{\frac{dI_m}{dE}+\frac{dH}{dE}}$	$\frac{\frac{dp}{dE}\left[\frac{1}{2}H+\frac{1}{2}I_m-I_s\right]+\frac{1}{2}p\frac{dH}{dE}}{\frac{dp}{dE}\left[\frac{1}{2}H+\frac{1}{2}I_m-I_s\right]+\frac{1}{2}p\frac{dH}{dE}+\frac{1}{2}p\frac{dI_m}{dE}+(1-p)\frac{dI_s}{dE}}$
Education Category	Married Women Income	Change Married Women Income	Single Women Income	Change Single Women Income	Husband Income	Change Husband Income	Probability Married	Change Probability Married	Fraction Change Married Family Income Through Husband	Fraction Change Net Income Through Marriage
1 to 4 Years	$8,630	-$2,130	$10,396	$1,394	$29,051	-$6,358	0.212	0.020	0.749	-1.370
5 to 8 Years	$8,456	-$175	$11,297	$901	$32,129	$3,078	0.534	0.322	1.060	0.909
9 Years	$10,191	$1,735	$12,045	$748	$33,389	$1,260	0.566	0.032	0.421	0.451
10 Years	$11,247	$1,057	$13,759	$1,714	$34,003	$614	0.587	0.021	0.368	0.263
11 Years	$11,128	-$119	$14,252	$493	$35,336	$1,333	0.583	-0.004	1.098	0.676
12 Years (No Diploma)	$13,622	$2,494	$16,559	$2,307	$40,446	$5,109	0.612	0.029	0.672	0.529
HS Graduate	$16,131	$2,509	$20,831	$4,272	$44,661	$4,215	0.687	0.075	0.627	0.496
Some College	$20,867	$4,737	$27,811	$6,980	$53,758	$9,097	0.694	0.007	0.658	0.461
College Graduate	$30,046	$9,178	$42,895	$15,084	$76,041	$22,283	0.734	0.039	0.708	0.537
Master's Degree	$42,958	$12,913	$52,010	$9,115	$80,258	$4,217	0.705	-0.029	0.246	0.143
PhD	$50,556	$7,597	$51,482	-$528	$80,326	$69	0.643	-0.062	0.009	-0.606

Notes for Table 3: This table shows the fraction of the correlation between women's education and family or net income that operates through the marriage market. The incomes reported are the predicted incomes for women of the given education category with covariates set to the sample average. The change in incomes are the marginal increase in income associated with increasing one educational category and are obtained from a regression that includes age and state fixed effects. The mathematical expressions correspond to equations (2) and (3) in the text.

Table 4: The Relationship between Quarter of Birth and Educational Outcomes Conditional on Age.

Educational Outcome	Quarter of Birth			F-Statistic
	Second	*Third*	*Fourth*	
Years of Education	0.041 (5.02)	0.062 (7.83)	0.078 (9.68)	35.30 [0.00]
Completed High School	-0.000 (0.42)	0.014 (12.07)	0.018 (15.52)	133.71 [0.00]
Completed College	0.007 (5.56)	-0.001 (0.51)	0.001 (0.64)	15.73 [0.00]
Attended Graduate School	0.005 (5.37)	0.001 (0.67)	0.001 (1.21)	11.78 [0.00]

Notes for Table 4: This table shows the coefficients of OLS regressions of educational outcomes on year of birth. The education variables are detrended by subtracting the mean educational outcomes of the birth cohorts two quarters prior and subsequent to the reference individual. Sample size is 822,507. T-statistics are in parentheses. The p-values of the F-statistics are in square brackets.

Table 5: Quarter of Birth Evidence regarding the Causal Effect of Women's Educational Attainment on 1980 Marriage Outcomes.

	Dependent Variable			
	Married in 1980		*Husband Income*	
Independent Variable	(1) OLS	(2) IV	(3) OLS	(4) IV
Years of Education	-0.001 (5.30)	-0.015 (0.81)	1,665 (173.05)	4,129 (3.84)
Age	0.030 (17.54)	0.030 (13.90)	3,063 (39.46)	3,137 (35.48)
Age-Squared	-0.0004 (15.83)	-0.0004 (15.78)	-35.10 (33.88)	-34.02 (28.40)
State of Birth Dummies	Yes	Yes	Yes	Yes
F-Statistic of Instruments	--	24.09 [0.00]	--	15.46 [0.00]
Chi-Squared Test of Over-Identifying Restrictions	--	2.52 [0.28]	--	0.52 [0.77]
Observations	879,418	879,418	687,060	687,060

Notes for Table 5: This table contains the OLS and IV estimates of the effect of wife's education on marriage market outcomes. The sample includes white women between the ages of 30 and 45 from the 1980 PUMS. The instruments include quarter of birth. T-statistics are included in parentheses. The p-values of the F-statistics are in square brackets.

Table 6: Robustness Checks.

		Dependent Variable	
Row	**Specification**	*Probability of Being Married*	*Husband Income*
1	Baseline Specification	-0.015 (0.81)	4,129 (3.84)
2	Controlling for third and fourth order polynomials of age	-0.026 (1.30)	7,068 (4.06)
3	Controlling for 3 age category dummies	-0.023 (1.15)	4,178 (3.78)
4	Controlling for 3 age category—instrumenting with age category and quarter of birth interactions	-0.019 (1.12)	3,987 (4.19)
5	Instrumenting using only the third and fourth quarters of birth	-0.015 (0.78)	4,036 (3.61)
6	Controlling for differences in average family characteristics by state and quarter of birth	-0.016 (0.85)	4,265 (3.99)
7	Limit Sample to those married over the age of 25.	-	3,777 (1.63)
8	Restrict quarter of birth to a linear effect.	-0.018 (0.91)	4,189 (3.87)

Notes for Table 6: This table contains the IV estimates of the effect of wife's education on marriage market outcomes. The specification is identical to those in Table 5 except for the differences noted. The age categories mentioned in rows 3 and 4 are 30-34, 35-39, and 40-45. T-statistics are included in parentheses.

Figure 1: The Relationship between Education and the Probability of Marriage in 2000

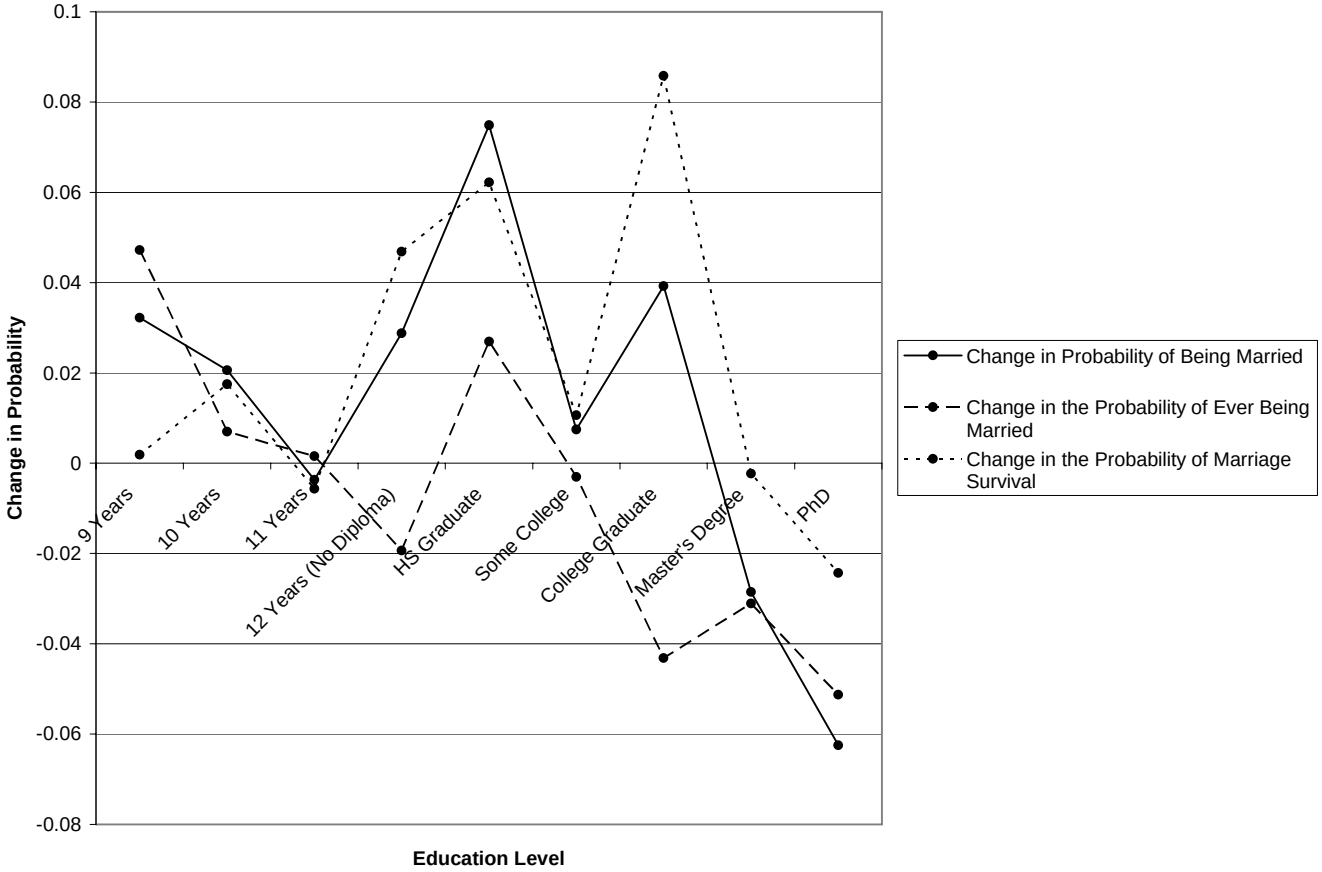

Figure 2: Women's Education and Husband Income in 2000.

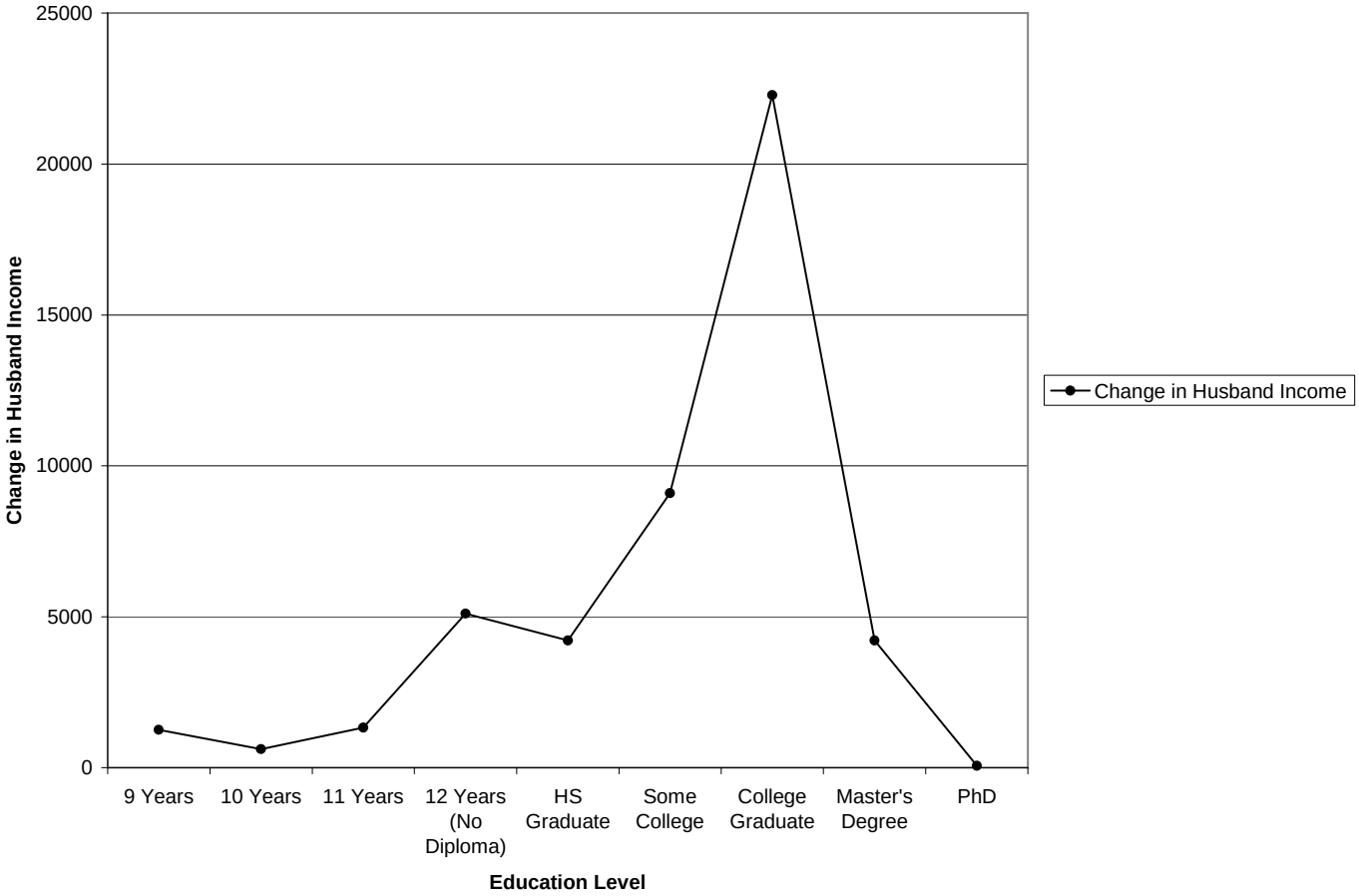

Figure 3: Women's Education and Distribution of Husband Income (Levels) in 2000

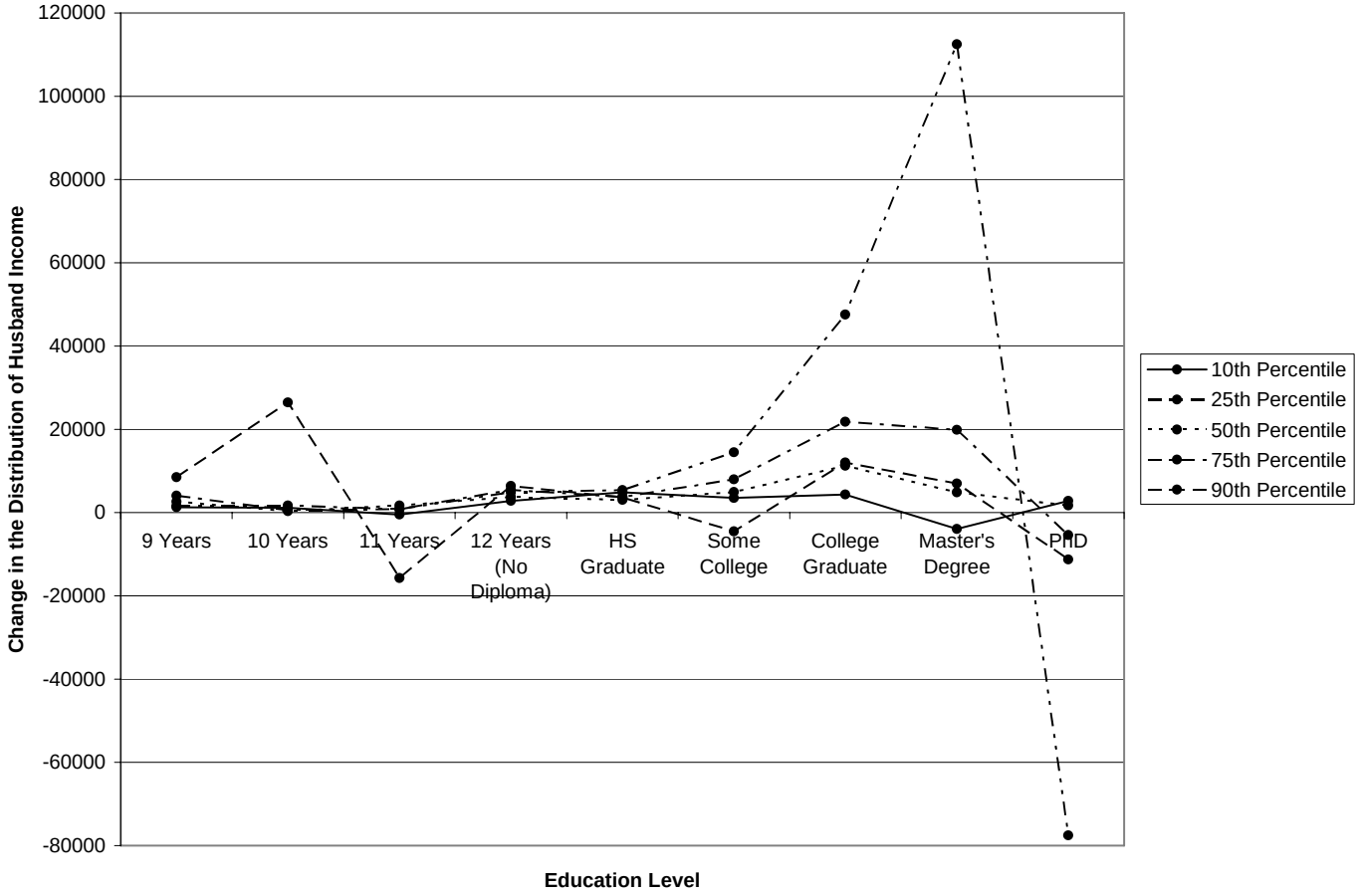

Figure 4: Women's Education and Distribution of Husband Income (Logs) in 2000

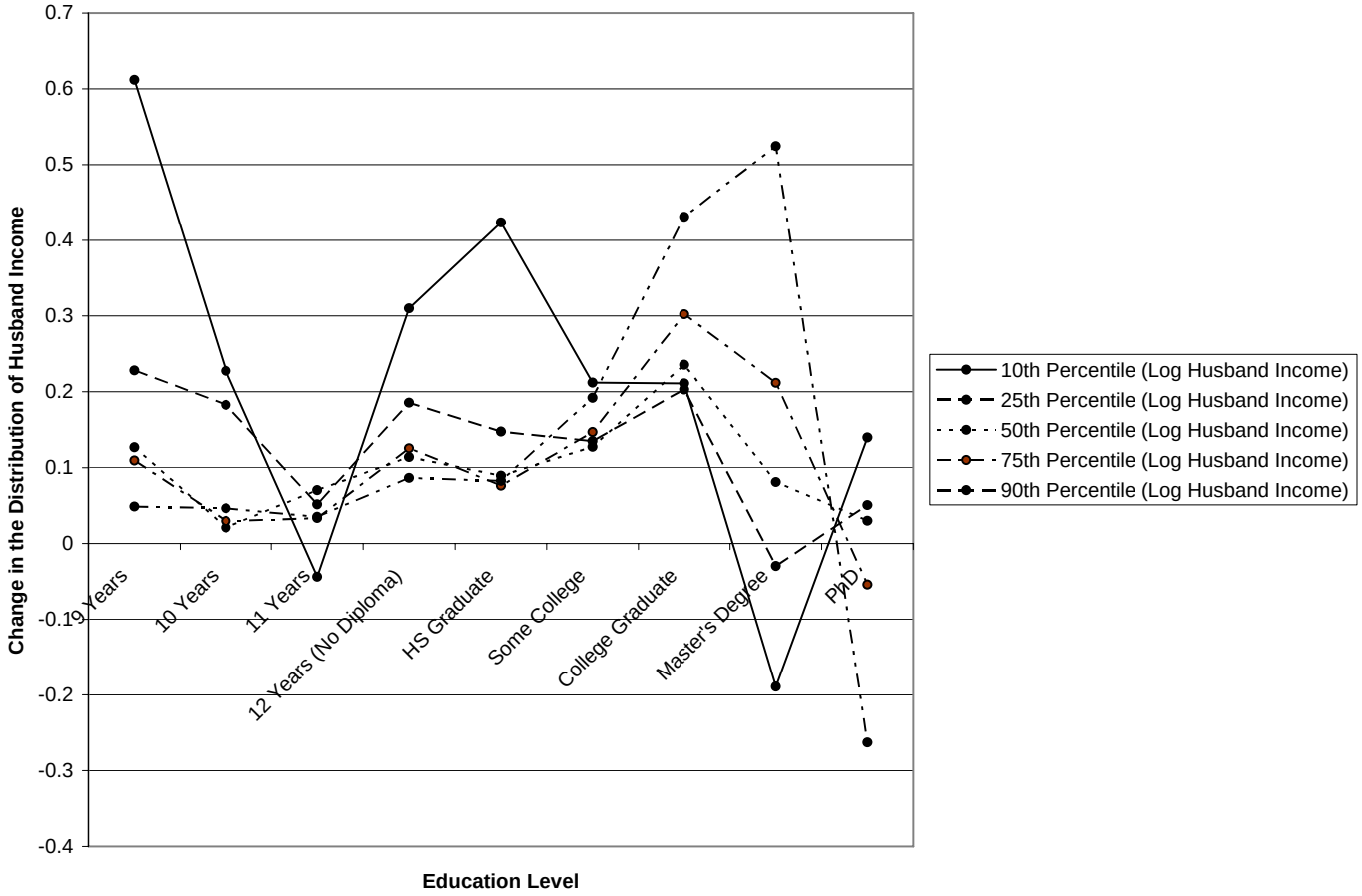

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Figure 5: Difference in CDF of Women's Education for Women Born in the Fourth and First Quarters in 1980.

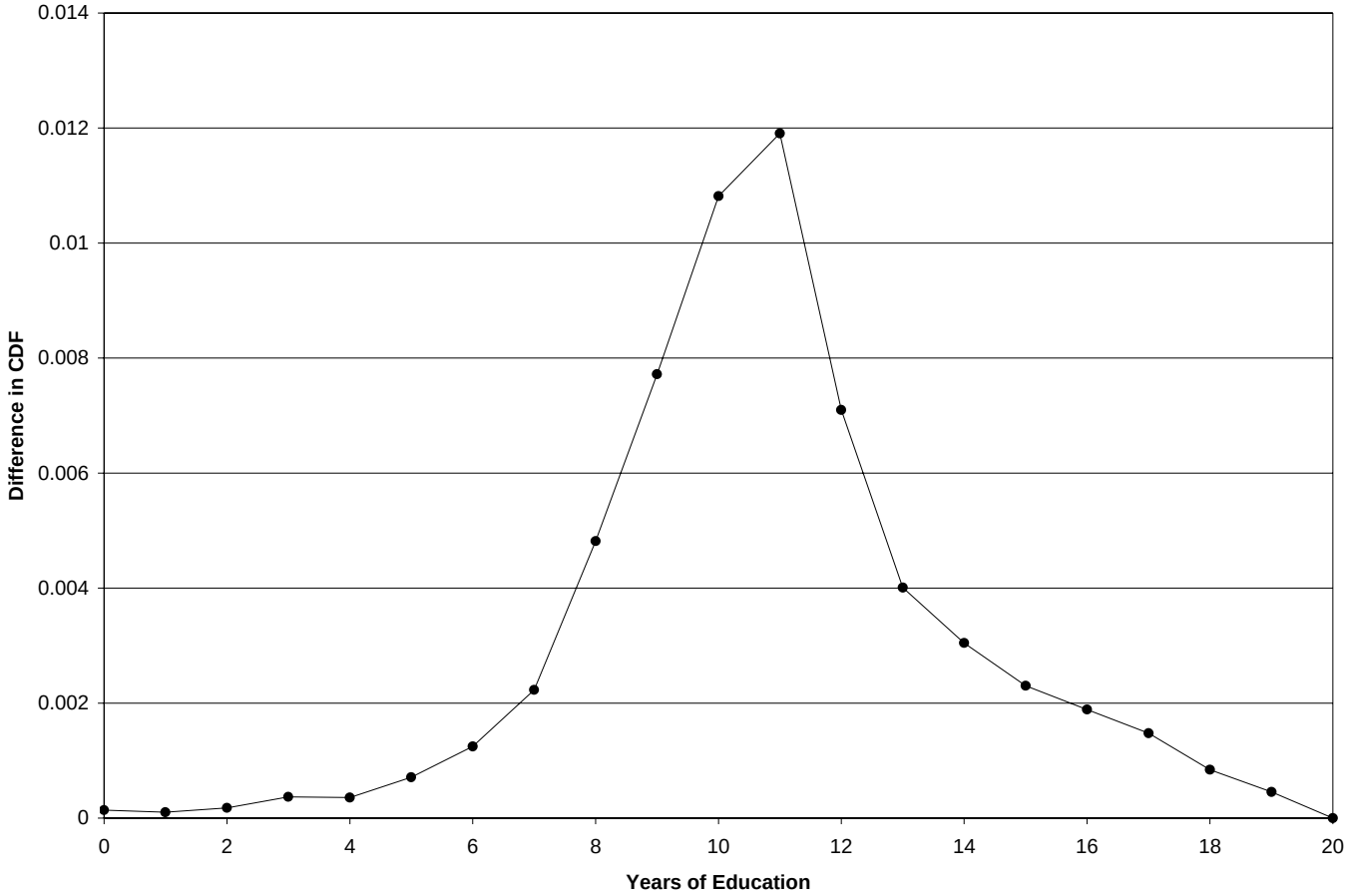

Figure 6: Women's Education and the Probability of Being Married in 1980

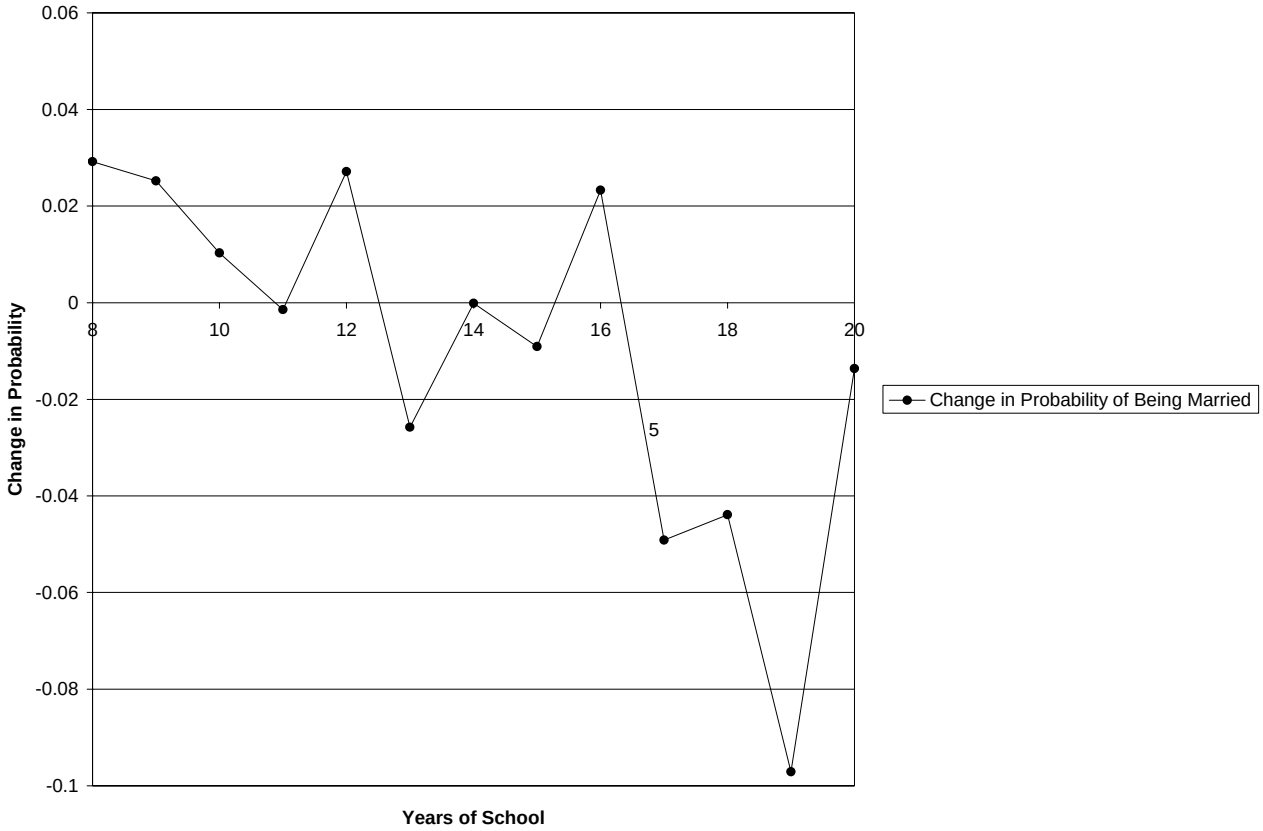

Figure 7: Women's Education and Husband Income in 1980.