Selection and Specialization in the Evolution of Couples' Earnings

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Abstract: We examine changes across birth cohorts in marriage patterns and the earnings differentials associated with marriage using data from a series of Survey of Income and Program Participation panels linked to administrative data on earnings. We find that marriage has become increasingly positively selected on education and earnings potential. Among women, selection into marriage has reversed sign, with the most educated women switching from being the least likely to be married to being the most likely. While men with the highest earnings potential have always been more likely to be married, this relationship has become even more pronounced. Changing selection into marriage is entirely responsible for the observed decline in marriage penalty for women in the cross section. In fixed-effects regressions, the earnings penalty continues to exist even for the most recent cohorts, consistent with specialization after marriage. For men, we find that the marriage premium actually increases for more recent birth cohorts in fixed-effects regressions. Taken literally, this suggests that specialization has become more important. We plan to explore further the robustness of this result by allowing selection to affect not only the level but the growth rate of male earnings.

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I. Introduction

There are well documented differences in work behavior and earnings associated with marital status and the presence of children. In the cross-section, wage regressions typically find that married men earn from 10% to 40% more than single men.[1] For women, in contrast, married women with children work and earn significantly less than childless women with similar human capital characteristics.[2] There is evidence that two patterns contribute to these differentials: (i) selection, in which the characteristics of those who marry differ on average from the characteristics of those who do not and (ii) specialization, in which spouses increase total family consumption by one spouse investing in skills rewarded in the market and one spouse taking a primary role in home production, leading to faster wage growth relative to single peers for one spouse, but a decline relative to peers for the other. For example, Korenman and Neumark (1991) find evidence of positive selection of men into marriage based on earnings—that is, men with higher earnings are more likely to marry. But they also find that much of the marriage premium accrues from faster wage growth for men after marriage, which would be consistent with marriage allowing men to shift towards more market work and less home production. Comparing married and co-habitating couples in Sweden, Ginther, Sundstrom and Bjorklund (2008) find that most of the marriage premium among men can be attributed to positive selection while increased specialization after marriage accounts for the marriage penalty for women.

How has the marriage premium/penalty evolved over time? Stevenson and Wolfers (2007) hypothesize that the returns to marriage based on production complementarities have diminished over time. The introduction of technology in household production, such as washing machines, microwave ovens and vacuum cleaners, has reduced incentives to marry based on household specialization (Greenwood and Guner (2008)). Women's market opportunities increased for a variety of reasons making it costly for women to stay home. In addition, unilateral divorce laws also increased the risk

[1] For example, Korenman and Neumark, 1991; Antonovics and Town, 2004.
[2] Waldfogel, 1997, 1998.

associated with specializing in the household sector for women (Stevenson (2007)). While the returns to specialization may have declined, the benefits of marriage based on consumption and leisure complementarities may have increased due to increased longevity and leisure (Aguiar & Hurst, 2007). Based on these developments, we would expect the marriage premium for men and marriage penalty for women to decline among couples who do marry.[3]

The factors that led to the decline in marriage rates, however, did not affect all men and women equally. There is considerable evidence that selection into marriage has shifted as well. While women gained relative to men in terms of wages, women's relative gains were larger at the bottom than at the top of the skill distribution (Blau and Kahn (1997)). Among less educated couples especially, the incentive to marry based on household specialization fell as male earnings prospects fell. Among women, the cross sectional correlation between marriage and education has reversed sign. Marriage rates were lowest among the most educated women in the earlier cohorts whereas the most educated women are now the most likely to be married (Isen and Stevenson (2010), Goldstein and Kenny (2001)). Likewise, marriage rates have fallen most dramatically among less educated men. In this study we examine the contribution of changing selection and changing specialization on the evolution of earnings differentials associated with marriage. Our basic empirical strategy is to estimate the marriage effect on earnings both in the cross-section and in fixed effects models. The difference between the two estimates then provides us with the net effect of selection into marriage.

We employ data from Survey of Income and Program Participation (SIPP) panels matched to Social Security Administration earnings records from 1978-2006. The data are ideal for our purposes in that the earnings histories allow us to estimate both cross sectional and fixed effects models of the marriage premium while the sample sizes are considerably larger than in the PSID. Additionally, the data span a long enough period so that we can meaningfully compare across birth cohorts.

[3] There appears to be some evidence that male marriage premium narrowed in recent decades (Gray (1997), Blackburn and Korenman (1994)). Gray (1997) finds that the marriage premium fell, particularly for men whose wives work.

To preview our findings, we find that marriage has become increasingly selected in terms of both education and earnings potential. We also find, as others have, that the most educated women are now the most likely to marry among the most recent birth cohorts. While educated men with the highest earnings potential have always been the most likely to marry, the relationship has become even more pronounced among recent birth cohorts. What is the impact of this changing selection on the marriage premium and marriage penalty? Among women, we find an earnings penalty of approximately 25 percent associated with marriage. The marriage penalty is roughly half as large when we control for presence and number of children. The estimates are similar in the cross-section and fixed-effects specifications, suggesting that selection accounts for little of the marriage penalty for women. Interestingly, selection plays a larger role in explaining the declining marriage penalty observed in the cross-sectional estimates. In the cross-section, the marriage penalty disappears across cohorts. This appears to be largely due to the fact that more educated women and women with higher earnings potential are increasingly likely to marry. In fixed-effects estimates, we see little systematic trend with the marriage penalty first increasing and then decreasing across cohorts.

We find dramatically different results for men. We find a marriage earnings premium equaling 46 percent in cross sectional data for men. In fixed-effects specifications, however, the estimate is reduced to less than 20 percent, suggesting that selection plays a much more important role in accounting for the marriage premium for men. When we allow selection to affect only the level of male earnings, we find successively larger marriage premiums in the fixed-effects regressions. Taken literally, this would suggest an increase in specialization across successive birth cohorts. We suspect that instead this represents either selection on individual-specific earnings growth rates, or an interaction between changes in age-earnings profiles and changes in selection into marriage. We have begun to investigate these possibilities, but have not yet resolved this puzzle.

Our paper is structured as follows. Section 2 describes our data. Section 3 describes trends in marriage and marital sorting. Section 4 describes our basic empirical methodology and regression results. Section 5 concludes.

II. Data

Our sample of individuals is drawn from respondents to the 1990-1993, 1996, 2001, and 2004 SIPP panels who provided the information needed to validate matches to Social Security Administration (SSA) earnings records. Individuals had to be at least 15 years old at the time of their second SIPP interview to be eligible for inclusion in the matched data.[4] For matched individuals, we have annual earnings for 1978-2006 based on annual summaries of earnings on jobs recorded in SSA's Master Earnings File. The primary source of the earnings information is W-2 records, but self-employment earnings are also included. We include employees' contributions to deferred compensation plans as part of our earnings measure. We obtain marital histories, educational attainment, and women's fertility histories from the SIPP. Age and gender are based on combined information from the SIPP and SSA sources, with the administrative data used to fill in missing values.

We use these data to look at cohorts born between 1931 and 1970, following their earnings over years in the 1978-2006 window during which they were aged 25-59. To determine marital status at a point in time, we use the marital history information collected in the relevant SIPP panel with some additional updates from changes in later waves of that panel. This largely gives us the information we need for years leading up to or during the SIPP panel, but not for the years after the panel is over. For this reason, we use earnings data only through that date. Since our focus here is on marital status, we further restrict the sample to men and women who are interviewed at age 35 or older, so that at a minimum we know marital status at age 35 for everyone in the sample. Thus for a 50 year old interviewed in the 1990 SIPP panel, we use earnings for 1978-1992 (ages 38-52), while for a 35 year old interviewed in the 1996 panel we use earnings for 1986-1999 (ages 25-38).

One further complication in examining the earnings of married couples is that we only have information on both members of couples identified during their SIPP panel. For a sample member who

[4] The SIPP is a series of short panel surveys in which respondents are surveyed every 4 months to collected detailed information on household members' income, employment and program participation over the previous months. The surveys also periodically collect detailed information on the demographic characteristics and relationships of household members. Panels have ranged in length from about 2 to 4 years. More detail on the SIPP is available at: http://www.census.gov/hhes/www/sippdesc.html.

divorced before the start of the SIPP panel, we have information on that sample member and know in which prior years they were married, but we cannot, for example, look at spousal characteristics in those earlier marriages because their previous spouse is not in the sample.

To help inform our analysis of changing marriage patterns, we construct a measure of potential earnings based on predicted earnings from a fixed-effect regression of log earnings on year dummies, main effects for education, a quartic in age, interactions between the age terms and education dummies, marital status, and for women, age and presence of children. We use the results to predict earnings for a single, childless person at age 40, and then add the estimated person-specific fixed-effect to that prediction to get potential earnings.[5] We assign a random draw from the distribution of this measure for the small portion of the sample that matched to the SSA earnings database but had zero earnings in all the years that they were observed. The random draw is taken from among other members of their birth cohort with the same education level who had relatively large numbers of years with zero earnings. We then assign each person to a potential earnings quartile based on their ranking among those of the same gender in their five-year birth cohort. While this measure will capture potential earnings imperfectly, it incorporates information drawn from the earnings data in addition to education level, and also has the advantage that we can use it to divide men and women into equal size groups over time.

III. Trends in marriage and marital sorting

Table 1 presents sample means for men and women. One concern in using this sample is that the use of retrospective information on marital status introduces some bias. To gauge whether this is likely to be an issue, in each panel, we split the sample between those who have reported marital status and other variables concurrently (e.g. in 1990 in the first panel) and those who have reported marital status in later panels. In the 1990 panel, we know marital status through at most September of 1992, and so 1990 panel

[5] This measure is essentially based on average earnings that have been adjusted for differences in age, calendar years observed, marital status and, for women, presence and age of children using the regression coefficients.

members are at least age 33 in 1990. We try to make the age ranges for the two groups comparable by including only those at least age 33 in 1990 for both groups. The concurrent panel members tend to be slightly younger than those interviewed in later panels, likely because this age adjustment is imperfect. Marital tenure is slightly higher for the older group, but in general, differences between the two panel groups are relatively small.

We first examine who is married among men and women. In particular, we are interested in whether those with relatively high stocks of human capital are more or less likely to marry than the average person. A pattern of positive selection into marriage based on labor market characteristics will tend to widen the gap in earnings of married couples relative to singles. We characterize labor market skill in two ways—using education levels, and using estimated potential earnings. While education level is a relatively simple, clean measure, its distribution has shifted significantly over time and in different ways for men and women, making it more complicated to parcel out what represents a change in selection patterns and what is simply the result of shifting education distributions.

Table 2 presents the share of men and women who are married at age 35 by level of education and by ten-year birth cohort. Overall, the probability of marriage fell between the 1931-1940 and 1951-1960 birth cohorts for all education groups, and for both men and women. There is some evidence of a rise in the share married at 35 for the last birth cohort among more educated groups, but the share married among those with a high school degree or less schooling continued to fall. Among men, the general pattern is that with few exceptions, being married is positively associated with higher levels of education, but a larger drop in share married among the less educated widened the gap across education groups over time. For women, marriage was modestly negatively associated with education in the first birth cohort, but a larger drop in marriage rates for the less educated resulted in a substantially positive relationship in the most recent birth cohort.

It is worth noting that these changes in the relationship between the probability of marriage and education level resulted in a substantial decline in the education levels of single relative to married people, particularly for women. Reconfiguring the information in Table 2, in the 1931-1940 birth cohort,

single women were more likely to be college graduates than married women (18% versus 16%), but by the 1961-1970 birth cohort, that pattern was reversed: 32% of married women were college graduates, while only 22 % of single women were. For men, the change is less dramatic but still substantial: the share of college graduates grew 7 percentage points among married men, but only 2 percentage points among single men.[6]

This pattern of a shift toward those who are married being those with greater labor market skills also appears in the statistics on marriage rates by quartiles of the potential earnings distribution, as illustrated in Table 3. Again, overall there is a decline in the share of men and of women who are married at age 35, but the decline in marriage is particularly large among those in the bottom part of the distribution, while relatively modest at the upper end of the distribution. While in the top quartile the share married fell about 4 percentage points for women and 10 percentage points for men, in the bottom quartile, the share fell 24 percentage points for women and 25 percentage points for men. A striking finding is that, in the most recent cohort, only half of the men in the lowest earnings category are married at age 35. The overall shares in these quartiles are fixed over time, so these changes quite directly imply that marriage is becoming increasingly associated with better labor market prospects. For men, this is a change in degree—married men are more educated and more likely to be in the upper part of the earnings distribution even in our earliest birth cohort, but the gap between married and single men increases over time. For women, there was modest negative selection into marriage on labor market prospects in the earliest cohort, but in our two most recent birth cohorts that selection has been positive. Isen and Stevenson (2010) report similar changes in marriage patterns by education. Our analysis here using earnings percentiles confirm that the patterns reflect real changes in the selection into marriage, rather than shifting composition of education groups.

[6] For men, the share of college graduates does not consistently grow across each of these birth cohorts. The overall share peaks at 32% for the 1941-1950 birth cohort, falls to 28% for the next cohort, and then rises slightly for the last cohort. The sharp increase for the 1941-1950 cohort likely reflects the effect of Vietnam-era draft deferrals on men's college attendance documented in Card and Lemieux (2000). We focus on the increase from the first to the last cohort as reflecting the longer term trend increase in college attendance.

How these shifts in marriage patterns affect the earnings of couples depends not only on the average characteristics of married men and women, but also on who marries whom. With this in mind, Tables 4 and 5 describe the relationship between spousal characteristics for married couples. Here the sample is further restricted to couples who were married to each other during the SIPP panel in which they were sampled. We know starting and ending dates for marriages that ended before the SIPP panel began, but we cannot measure spouse characteristics for such couples.

Panel A of Table 4 gives the joint distribution of couples across husband and wife education categories for our four 10-year birth cohorts. The cell probabilities sum to one for each birth cohort. Interpreting changes in joint probabilities over time is complicated by changes in the marginal distribution of education for both husbands and wives. Even if couples were randomly matched from a pool of men and women with marginal probabilities as given, the share of college graduate couples would rise over time while the share of high school or less couples would fall simply because of the shift towards higher levels of education for both men and women.

To illustrate which combinations are more likely than would be expected, panel B gives the ratio of the joint probability to the product of the marginal probabilities for married men and women. In our first birth cohort the probability that both spouses have a high school degree or less (for example) is about 40% greater than one would predict based on random matching. It is easy to see in scanning the table that the diagonal elements are consistently above one, while the only off diagonal combination that is more likely than it would be under random assignment is the combination of women with some college education married to men with at least a bachelor's degree. The excess probability for that cell falls over time, and by the 1951-1960 birth cohort, the ratio of probabilities falls below one. College graduates are particularly likely to be married to each other, though the ratio of actual to predicted probabilities falls somewhat over time. The share of college graduate men who have a college graduate wife rose substantially across these birth cohorts, from 50% in the first cohort to 71% in the last birth cohort.

Table 5 presents a similar set of results based on classifying husbands and wives using our measure of potential earnings. We again find that the diagonal ratios are most likely to be greater than

one, though the differences between the diagonal and off-diagonal elements are less striking using this measure. It is clear looking across birth cohorts that the diagonal ratios have grown across birth cohorts, with the increases particularly large for the top and bottom quartiles.

One way of characterizing such patterns is to measure the extent to which people marry spouses who are like them (positive assortative matching) versus those who have different characteristics. For both education and potential earnings, we can calculate the share of couples in which both spouses fall in the same category and compare that to the expected share based on the marginal probabilities. We find substantial increases using either measure of skill. For education, the share of couples from the same education group is about 60% higher than the expected share for the 1931-1940 birth cohort, and rises to 80% higher than expected in the 1961-1970 cohort. For potential earnings, the ratio of actual to expected share in the same quartile is quite close to 1 for the 1931-1940 cohort, but rises to 1.30 for the 1961-1970 birth cohort.

Earnings premiums or penalties associated with marriage are likely influenced by characteristics of the spouse as well as own characteristics. Our examination of the patterns of marital sorting suggest an increase in positive assortative matching, even accounting for educational patterns that would lead to a greater propensity of finding college educated couples (Schwartz and Mare (2005)). We also note, however, that the propensity for college educated women to marry men with education levels lower than their own has increased, leaving open the possibility that specialization may occur in non-traditional ways, with women entering the market and men taking on more household production. Lundberg and Rose (2000) find evidence that in households where wives remain continuously employed through childbirth, husbands reduce hours worked substantially. On the other hand, Bertrand, Goldin and Katz (2009) find that female MBAs who are married to high earnings spouses have sharp declines in earnings and hours following marriage and childbirth, suggesting "who" you marry matters for subsequent specialization decisions. We plan to address this important heterogeneity based on spouses' characteristics in our future analysis.

IV. Regression methodology and results

Comparison of cross sectional earnings regressions to fixed effects models forms the basis of much of our regression analysis. To fix ideas, we start with the following stylized statistical model of earnings:

$$(1) \qquad \ln Y_{it}^{j} = \beta^{jC} X_{it}^{j} + \gamma^{jC} M_{it} + \pi^{jC} K_{it} + \varepsilon_{it}^{jC}, \quad \varepsilon_{it}^{jC} = \alpha_{i}^{jC} + \nu_{it}^{jC}$$

where *i* indexes a couple or an unmarried individual, *C* indexes birth cohort, $j = m$ (male) or *f* (female), and X = observable characteristics such as education and age, M = marital status indicators, and K = indicators for the presence and age of children. In the above specification, $E(\alpha_i^{jC}) = E(\nu_{it}^{jC}) = 0$, α_i^{jC} = permanent (unobserved) skill component of earnings and ν_{it}^{jC} =transitory shocks. Adding interaction terms between M and characteristics of the individual allows us to examine how the marriage premium/penalty varies with these characteristics. We also include analogous interactions between K and individual characteristics in this part of the analysis.

Our first step is to examine changes in average differences in earnings associated with marital status, which are measured by γ^{jC}. We run the above regressions for men and women allowing for differences across birth cohorts. We first estimate the earnings regressions in levels, as specified above, in which case the marital status and parenthood coefficients include selection effects—that is they confound changes in earnings with marriage/children with average differences in the permanent skill component (α_i^{jC}) associated with marriage and children. We then estimate the regressions using fixed person effects in an attempt to remove effects of selection on earnings levels.[7] The difference between

[7] This method interprets steeper wage growth among married men as an effect of marriage, but it is difficult to entirely rule out selection since men with higher expected wage growth may be more likely to marry.

the OLS and fixed-effects estimates then provides us with an estimate of the net effects of selection on these differentials.

We turn now to examining the relationship between marital status and earnings. We run a series of regressions based on (1) to estimate earnings differentials associated with marriage. For each specification, we use the pooled person/year data to estimate one version based primarily on cross-sectional comparisons and a second version that allows for person-specific fixed effects. The first version assumes that the cross-sectional and within-person marriage earnings differentials are the same, while in the fixed-effects results, the marriage earnings differential captures the average difference in an individual's earnings between periods in which they are married and those in which they are not. We then examine the difference between the two versions in the earnings differential associated with marriage, interpreting the difference across specifications as evidence on the role that selection into marriage plays in generating the cross-sectional earnings differences.

Table 6 presents coefficient estimates from the earnings regressions for women. In columns (1) and (2) for both sets of estimates we include detailed controls for marital status. In columns (3) and (4) we allow for interactions between marital status and other variables, and we collapse the detailed categories into a single married all year/not married at some point in the year categorization to keep the interactions manageable. We find a substantial negative earnings differential for women in the first specification for both the pooled estimates and the fixed effects estimates. The similarity of these estimates implies that selection into marriage is not an important explanation for this differential. As others have found, adding controls for the presence and age of children in column (2) reduces this differential substantially. In the pooled regression the marriage earnings differential falls by more than half, but the reduction is only about 30% in the fixed-effects results. The difference between the pooled and fixed-effect coefficients on the controls for number of children suggest that the large negative differential associated with having three or more children in the pooled regression is largely due to selection.

In column (3) we add interactions between married and five-year birth cohort to examine changes in the earnings differential across cohorts of women. In the pooled regression, the earnings differential associated with marriage falls across birth cohorts, becoming essentially zero for the more recent cohorts. The fixed-effect estimates of the earnings differential become more negative moving from the 1931-1935 birth cohort through the 1946-1950 cohort, and then become less negative across subsequent cohorts. A comparison of the fixed-effect and pooled coefficients implies that in the early cohorts there was substantial negative selection into marriage based on potential earnings—i.e. women with higher potential earnings were less likely to marry. For more recent cohorts, the comparison implies that selection into marriage on earnings characteristics is now positive, and that the essentially zero earnings differential for the most recent cohort in the pooled regressions comes about because the negative affect of marriage on earnings (specialization effect) is offset by this positive selection into marriage. The finding of positive selection into marriage on earnings characteristics is quite consistent with the evidence we present in Table 2, and also with the findings of Isen and Stevenson (2010) on selection based on education.

In the final specification in (4), we interact the married dummy with education categories. The differences across education groups have consistent patterns in the pooled and fixed-effects specifications, though the differences are somewhat larger in the pooled specifications, suggesting that part of this effect is due to selection. We find that the marriage earnings differential is most negative for women in the middle of the education distribution, particularly for those who are college graduates without an advanced degree. It is smallest for high school drop-outs, for whom the fixed effects estimates are consistently positive though generally small. The difference between the fixed-effects and pooled estimates implies that positive selection into marriage is particularly large for high school drop-outs (who have the lowest marriage rates) and for advanced degree holders.

These log earnings results condition on having positive earnings. To look at the extensive margin for women, we run an analogous set of regressions with an indicator for having zero earnings in a calendar year as the dependent variable. The estimates are presented in Table 7. Unsurprisingly, the results show a significant positive relationship between marriage and non-employment, with married

women 7% less likely to not work over the course of a calendar year than single women. The pooled and fixed-effect results look quite similar.

As in the log earnings regressions, the effect attributed to marriage is reduced by more than half when we add controls for children to the pooled results, but only by about one-third in the fixed-effect results.[8] Unsurprisingly, women with more children and/or with young children are more likely than others to not work. The coefficients on interactions between controls for children and the married dummy indicate that the effects of children on the probability of work are larger for married women than for single women. This work effect falls across birth cohorts, with the most recent few cohorts roughly half as likely as the earliest cohorts to have no earnings in a year. Effects are largest for college graduates, and smallest for those who did not graduate from high school, but the probability of not working does not rise consistently with education—graduate degree holders are less likely than all but drop-outs to not work. Overall, the comparison of the marriage penalty in earnings reported in Table 6 and the marriage penalty in labor force participation reported in Table 7 suggests that there has been a genuine decline in marriage penalty in terms of the continuity of market work but the penalty is still substantial in terms of wages and earnings. This would be consistent with the younger cohorts of women staying the labor force and yet choosing a less ambitious career track upon marriage and motherhood.

Table 8 presents estimates of the marriage premium for men based on the same log earnings regressions we estimated for women except that we do not control for the age or presence of children.[9] We find a very large positive marriage premium for men in the pooled regressions, but the fixed effect estimates are much smaller. This is consistent with our findings in Tables 2 and 3 that selection on labor

[8] We use information from fertility histories to measure the number and ages of children. These questions apply only to biological children, and we know the year of birth for only the oldest and youngest children. This means we miss the presence of all step and adopted children. To create controls for children, if there are one or two biological children, we assume that both live with their mother between birth and the year they turn 18, and set the control for the presence of young children based on the years in which one or both children was less than 6. For mothers with three or more children, we assume that a child less than 6 was present between the 6th birthday of the first child and the birth of the last child. We count the number of children present between the birth of the first and of the last by assuming that the intervening child or children are evenly spaced. These measures are clearly approximations, with errors in both directions—not all children are counted, but some of those who are counted do not live at home

[9] We do so mostly because in the fertility history SIPP only collects a count of total number of biological children for men. While we could put together information on the age and presence of children during the SIPP panel, this would miss the grown children of older respondents.

market skills into marriage are quite positive for men. The estimated interaction effects with birth cohort dummies are somewhat puzzling. They indicate that the marriage premium is larger for more recent cohorts than for the 1931-1945 cohorts, though there is not a consistent positive trend across later cohorts. This increase in the marriage premium over time shows up in the fixed-effects results as well as the pooled results, suggesting that the effects of specialization after marriage are larger for more recent cohorts. That seems inconsistent with evidence of increased labor market skills and work among married women for these cohorts.

One possibility is that the fixed-effects estimates for early cohorts are downward biased because we have earnings for them only at older ages. For example, for those born in 1931, we observe earnings while they are aged 47 to 59, long after most would have married. Thus changes in marital status for this group are likely to involve primarily divorce and remarriage which might have smaller effects on earnings than first marriages. It is also possible that our implicit assumption of constant returns to experience over time is leading to a rising estimate of the marriage premium in both the fixed-effect and pooled results because married men on average have greater experience than unmarried men. However, examination of alternative specifications in which we allow returns to experience and education to change over time leads to essentially the same pattern as in the results presented here. A third possibility is that there is selection into marriage based on individual-specific earnings growth, in which case the fixed-effects estimates are also subject to bias from selection. Given such a misspecification, changes in the distribution of the individual-specific growth component, or in selection based on that component, could result in the pattern we find here.

When we allow the marriage premium to vary with education level, the pooled and fixed-effects results suggest quite different premia for everyone except for those with advanced degrees. Fixed-effects estimates of the marriage premium are negligible for everyone except those with at least a college degree, while the pooled results suggest that the marriage premium is large for all education groups, but largest for those who did not complete high school. These differences are consistent with positive selection into marriage that is strongest for the least educated, weakest for the most educated.

V. Conclusions

We find that those who are married have become increasingly positively selected from the population at large in terms of both education and earnings potential. Consistent with others' findings, we also find that the most educated women are the most likely to be married among recent birth cohorts. While educated men with the highest earnings potential have always been the most likely to be married, the relationship has become more pronounced across birth cohorts spanning 1931 to 1970.

What is the impact of this changing selection on the marriage premium and marriage penalty? Among women, we find an average earnings penalty of approximately 25 percent associated with marriage, but it is roughly half as large when we control for presence and number of children. The similarity of cross-sectional and fixed-effects estimates suggests that selection accounts for little of the average marriage penalty for women. Interestingly, selection plays a larger role in explaining the declining marriage penalty we estimate in the pooled regressions. The decline in the penalty appears largely due to the fact that more educated women and women with higher earnings potential are increasingly likely to marry. In fixed-effects estimates, we see little systematic trend in the marriage penalty.

We find dramatically different results for men. We find a marriage earnings premium equaling 46 percent in cross sectional data for men. In fixed-effects, however, the estimate is reduced to less than 20 percent, suggesting that selection plays a much more important role in accounting for the marriage premium for men. In our specifications which allow marriage to affect only the level of male earnings, we find successively larger marriage premiums in the fixed-effects regressions. Taken literally, this would suggest an increase in specialization across successive birth cohorts. We suspect that instead this represents some form of misspecification. We have begun to investigate these possibilities, but have not yet resolved this puzzle.

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Table 1: Sample means

Year characteristics measured	SIPP Panel	Age	Marital tenure	Share married	Log earnings	Share with zero earnings	Share college grad	N
Women								
1990	1991 and later panels	43.5	18.6	0.702	9.34	0.217	0.229	50,135
1990	1990	44.1	18.8	0.654	9.39	0.214	0.211	8,204
1996	2001,2004	43.1	17.1	0.680	9.57	0.203	0.261	25,004
1996	1996	43.6	17.3	0.685	9.56	0.204	0.235	14,740
Men								
1990	1991 and later	43.6	17.0	0.757	10.09	0.099	0.287	46,952
1990	1990	44.1	17.1	0.752	10.13	0.091	0.277	7,603
1996	2001,2004	43.1	15.9	0.713	10.21	0.104	0.295	22,878
1996	1996	43.7	16.1	0.741	10.22	0.106	0.275	13,701

Table 2: Share married at age 35 by education group

Birth cohort	<=High school grad	Some college	College grad
Women			
1931-1940	0.830	0.829	0.809
1941-1950	0.731	0.733	0.732
1951-1960	0.666	0.679	0.698
1961-1970	0.620	0.644	0.738
Men			
1931-1940	0.819	0.846	0.842
1941-1950	0.736	0.753	0.767
1951-1960	0.661	0.687	0.715
1961-1970	0.613	0.693	0.729

Notes: N=77,227 women and 71,914 men.

Table 3: Share married at age 35 by potential earnings quartile

	Potential earnings quartile			
Birth cohort	**1**	**2**	**3**	**4**
Women				
1931-1940	0.848	0.836	0.820	0.802
1941-1950	0.762	0.758	0.720	0.687
1951-1960	0.665	0.694	0.653	0.702
1961-1970	0.604	0.629	0.653	0.764
Men				
1931-1940	0.754	0.830	0.855	0.884
1941-1950	0.622	0.760	0.808	0.813
1951-1960	0.526	0.694	0.742	0.774
1961-1970	0.500	0.690	0.721	0.778

Notes: N=77,227 women and 71,914 men.

Table 4: Relationship between education of spouses

A: Joint distribution

Husband's education	Husband's birth cohort	Wife's education <=HS grad	Some college	College grad
<=High school grad		0.423	0.081	0.016
Some college	1931-1940	0.110	0.091	0.023
College grad		0.052	0.076	0.127
<=High school grad		0.286	0.078	0.019
Some college	1941-1950	0.110	0.117	0.045
College grad		0.046	0.098	0.202
<=High school grad		0.258	0.088	0.027
Some college	1951-1960	0.097	0.153	0.056
College grad		0.039	0.085	0.196
<=High school grad		0.192	0.100	0.036
Some college	1961-1970	0.085	0.173	0.079
College grad		0.024	0.073	0.237

B: Probability relative to random matching

Husband's education	Husband's birth cohort	Wife's education <=HS grad	Some college	College grad
<=High school grad		1.4	0.6	0.2
Some college	1931-1940	0.8	1.6	0.6
College grad		0.4	1.2	3.0
<=High school grad		1.7	0.7	0.2
Some college	1941-1950	0.9	1.5	0.6
College grad		0.3	1.0	2.2
<=High school grad		1.8	0.7	0.3
Some college	1951-1960	0.8	1.5	0.7
College grad		0.3	0.8	2.2
<=High school grad		1.9	0.9	0.3
Some college	1961-1970	0.8	1.5	0.7
College grad		0.2	0.6	2.0

Notes: N=34,969 couples.

Table 5: Relationship between potential earnings of spouses

A: Joint distribution

Husband's potential earnings quartile	Husband's birth cohort	Wife's potential earnings quartile 1	2	3	4
1	1931-1940	0.061	0.057	0.046	0.036
2		0.061	0.066	0.065	0.056
3		0.071	0.065	0.065	0.070
4		0.091	0.065	0.061	0.063
1	1941-1950	0.059	0.050	0.040	0.034
2		0.061	0.068	0.062	0.056
3		0.068	0.069	0.067	0.081
4		0.090	0.073	0.055	0.068
1	1951-1960	0.053	0.044	0.036	0.032
2		0.056	0.066	0.064	0.057
3		0.061	0.064	0.071	0.084
4		0.072	0.072	0.069	0.099
1	1961-1970	0.050	0.042	0.029	0.030
2		0.056	0.066	0.064	0.057
3		0.046	0.070	0.080	0.084
4		0.061	0.063	0.073	0.129

B: Probability relative to random matching

Husband's potential earnings quartile	Husband's birth cohort	Wife's potential earnings quartile 1	2	3	4
1	1931-1940	1.08	1.12	0.97	0.80
2		0.86	1.06	1.10	1.00
3		0.92	0.94	1.01	1.15
4		1.14	0.92	0.92	0.99
1	1941-1950	1.16	1.05	0.98	0.78
2		0.89	1.06	1.12	0.95
3		0.86	0.93	1.05	1.19
4		1.13	0.98	0.86	1.00
1	1951-1960	1.32	1.08	0.92	0.71
2		0.96	1.10	1.09	0.86
3		0.90	0.93	1.06	1.10
4		0.95	0.94	0.92	1.17
1	1961-1970	1.55	1.17	0.77	0.66
2		1.08	1.12	1.07	0.78
3		0.77	1.04	1.17	1.00
4		0.88	0.80	0.91	1.32

Notes: N=34,969 couples.

Table 6: Log earnings regressions, Women

		Pooled coefficient estimates				Fixed-effect coefficient estimates			
Controls		**1**	**2**	**3**	**4**	**1**	**2**	**3**	**4**
Married all yr		-0.255 *	-0.110 *	-0.252 *	-0.081 *	-0.252 *	-0.178 *	-0.117 *	0.026
Divorced all yr		-0.031 *	0.081 *			-0.041 *	0.022 *		
Married during year		-0.015	0.054 *			-0.041 *	-0.008		
Divorced during year		-0.148 *	-0.014			-0.123 *	-0.054 *		
1 child			-0.121 *	-0.116 *	-0.119 *		-0.129 *	-0.129 *	-0.087 *
2 children			-0.268 *	-0.264 *	-0.231 *		-0.257 *	-0.257 *	-0.177 *
3 or more children			-0.451 *	-0.446 *	-0.422 *		-0.207 *	-0.207 *	-0.138 *
Kids<6 years old			-0.014 *	-0.022 *	-0.058 *		-0.061 *	-0.060 *	-0.072 *
Married * birth cohort	1936-40			0.017	0.031 *			-0.034	-0.012
	1941-45			0.038 *	0.063 *			-0.054 *	-0.010
	1946-50			0.033 *	0.069 *			-0.086 *	-0.026
	1951-55			0.118 *	0.158 *			-0.080 *	-0.016
	1956-60			0.156 *	0.200 *			-0.072 *	-0.005
	1961-65			0.217 *	0.263 *			-0.065 *	0.006
	1966-70			0.256 *	0.302 *			0.025	0.092 *
Married * education	HS grad				-0.203 *				-0.152 *
	Some college				-0.232 *				-0.180 *
	College grad				-0.290 *				-0.205 *
	Adv degree				-0.058 *				-0.063 *
Married * children	Child<6 years old				0.048 *				0.011
	1 child				0.002				-0.072 *
	2 children				-0.046 *				-0.110 *
	3 or more children				-0.034 *				-0.097 *

Notes: Other controls are year dummies, main effects for the education and 5-year birth cohort dummies, and a quartic in age. N=1,109,848 person/years for 76,211 women. * indicates that the coefficient is statistically significant at at least the 5% level.

Table 7: Zero earnings regressions, Women

		Pooled coefficient estimates				Fixed-effects coefficient estimates			
Controls		**1**	**2**	**3**	**4**	**1**	**2**	**3**	**4**
Married all yr		0.070 *	0.033 *	0.097 *	0.027 *	0.068 *	0.046 *	0.077 *	0.015 *
Divorced all yr		-0.025 *	-0.053 *			-0.001	-0.022 *		
Married during year		-0.038 *	-0.055 *			-0.013 *	-0.025 *		
Divorced during year		-0.031 *	-0.063 *			-0.002	-0.023 *		
1 child			0.031 *	0.025 *	0.013 *		0.027 *	0.027 *	0.011 *
2 children			0.050 *	0.044 *	0.017 *		0.055 *	0.055 *	0.026 *
3 or more children			0.104 *	0.098 *	0.081 *		0.036 *	0.035 *	0.010 *
Kids<6 years old			0.064 *	0.065 *	0.061 *		0.040 *	0.039 *	0.040 *
Married * birth cohort	1936-40			-0.012 *	-0.020 *			-0.012	-0.021 *
	1941-45			-0.018 *	-0.035 *			-0.011	-0.031 *
	1946-50			-0.015 *	-0.038 *			-0.006	-0.033 *
	1951-55			-0.038 *	-0.065 *			-0.009	-0.038 *
	1956-60			-0.048 *	-0.076 *			-0.020 *	-0.050 *
	1961-65			-0.053 *	-0.083 *			-0.026 *	-0.057 *
	1966-70			-0.044 *	-0.074 *			-0.034 *	-0.066 *
Married * education	HS grad				0.088 *				0.072 *
	Some college				0.085 *				0.078 *
	College grad				0.113 *				0.094 *
	Adv degree				0.051 *				0.061 *
Married * children	Kids<6 years old				0.005				0.001
	1 child				0.020 *				0.026 *
	2 children				0.038 *				0.039 *
	3 or more children				0.026 *				0.034 *

Notes: The dependent variable is a dummy variable for having zero earnings in a calendar year. Estimates are based on a linear probability model. Other controls are year dummies, main effects for the education and 5-year birth cohort dummies, and a quartic in age. N=1,451,494 person/years for 77,227 women. * indicates that the coefficient is statistically significant at at least the 5% level.

Table 8: Log earnings regressions, Men

		Pooled coefficient estimates			Fixed-effect coefficients		
Controls		**1**	**2**	**3**	**1**	**2**	**3**
Married all yr		0.463 *	0.325 *	0.399 *	0.190 *	0.025	-0.004
Divorced all yr		0.159 *			0.084 *		
Married during year		0.308 *			0.125 *		
Divorced during year		0.290 *			0.143 *		
Married * birth cohort	1936-40		-0.012	-0.006		0.013	0.006
	1941-45		-0.007	0.003		0.019	0.000
	1946-50		0.037 *	0.054 *		0.072 *	0.052 *
	1951-55		0.056 *	0.076 *		0.117 *	0.103 *
	1956-60		0.065 *	0.084 *		0.121 *	0.113 *
	1961-65		0.059 *	0.079 *		0.111 *	0.102 *
	1966-70		0.029 *	0.050 *		0.159 *	0.143 *
Married * education	HS grad			-0.103 *			-0.017 *
	Some college			-0.124 *			0.003
	College grad			-0.105 *			0.077 *
	Adv degree			-0.023 *			0.273 *

Notes: Other controls are year dummies, main effects for the education and 5-year birth cohort dummies, and a quartic in age. N=1,213,406 person/years for 71,672 women. * indicates that the coefficient is statistically significant at at least the 5% level.