Selection and Specialization in the Evolution of Marriage Earnings Gaps

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Abstract

We examine changes in marriage and earnings patterns across four cohorts born between 1936 and 1975, using data from a series of Survey of Income and Program Participation panels linked to administrative data on earnings. We find that for both men and women, marriage has become increasingly positively associated with education and earnings potential. We compare ordinary least squares (OLS) and fixed effect (FE) estimates of the earnings differential associated with marriage. We find that the marriage earnings gap fell for women in fixed-effect estimates implying that the impact of specialization has diminished over time. We also find that increasingly positive selection into marriage means that OLS estimates overstate the reduction in the marriage earnings gap. While our findings imply that marriage is no longer associated with lower earnings among women without minor children in our most recent cohort, the motherhood gap remains large. Among men, we find that the marriage premium actually increases for more recent birth cohorts in fixed-effects regressions.

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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] In contrast, married women earn significantly less than unmarried women with similar human capital characteristics, and this is particularly true for those with children.[2] There is evidence that two patterns contribute to these differentials: (i) selection, by which we mean that characteristics that are related to earnings differ between those who are married and those who are not and (ii) specialization, in which spouses increase total family consumption by one spouse investing more heavily in skills rewarded in the labor market while the other spouse takes a primary role in home production.[3] We expect specialization to lead 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 is consistent with marriage allowing men to shift towards more market work and less home production.[4] 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.[5]

How have earnings differentials associated with marriage—which we will refer to as "marriage gaps"—evolved over time? Figure 1 presents estimates of these gaps for cohorts born between 1936 and

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[1] For example, Korenman and Neumark, 1991; Antonovics and Town, 2004.
[2] Waldfogel, 1997, 1998.
[3] We focus on changing economic incentives as an important determinant of the direction of changes in behavior, though gender norms may also play a role in determining the rate at which behavior adjusts.
[4] A recent paper by Killewald (2014) finds that among men in the NLSY79, wages rise at least 5 years prior to marriage. This is still consistent with specialization if men invest in steady work and higher paying jobs in anticipation of marriage.
[5] Our data provide marital histories only for heterosexual couples, and do not provide cohabitation histories, so our findings are based on a sample of only heterosexual marriages.

1975 based on a series of cross-sectional surveys.[6] In the cross-section, married men enjoy a substantially positive earnings gap (on the order of 30 to 35 percentage points) which has remained fairly stable across these cohorts. Married women, on the other hand, began with a substantially negative earnings gap (-24 percentage points for the 1936-40 cohort) but across cohorts have steadily gained relative to their single counterparts. In the most recent birth cohorts (1971-1975) married women enjoy a positive earnings gap of 11 percentage points. There are similar trends across racial groups, although the change is smaller or black women, with the gap changing from roughly -3 percentage points for the 1936-1940 cohort to positive 15 percentage points for the most recent cohort.

Stevenson and Wolfers (2007) hypothesize that the returns to marriage from specialization by gender in different types of production have diminished over time. Technological progress in household production, in the form of 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 more costly for women to stay home. In addition, unilateral divorce laws also increased the risk 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 to find that the marriage gaps for both men and women have narrowed over time.[7]

The factors that led to declining marriage rates, however, did not affect all men and women equally. While wages of women in general rose relative to those of men , their relative gains were larger at the bottom than at the top of the skill distribution (Blau and Kahn (1997)). Among less educated

[6] The figure is based on 1960-2000 decennial census and 2010 American Community Survey IPUMS data. The figure reports coefficients on 5-year birth cohort dummies interacted with a currently married dummy from a regression of log annual wage and salary earnings on year dummies, a quartic in age, education dummies, number of children, and presence of children<5. The sample consists of men and women who have 1-35 years of potential work experience.

[7] There appears to be some evidence that the male marriage premium narrowed in the 1970s and 1980s (Gray (1997), Blackburn and Korenman (1994)). Gray (1997) finds that the marriage premium fell, particularly for men whose wives work.

couples especially, the incentive to marry based on household specialization fell as male earnings prospects fell. There is considerable evidence that these changes resulted in shifts in selection into marriage as well. Among women, the cross-sectional correlation between marriage and education has reversed sign. In earlier cohorts, marriage rates were lowest among the most educated women, whereas now the most educated women are the most likely to be married (Isen and Stevenson (2010), Goldstein and Kenny (2001)). Among men as well, marriage rates have fallen most dramatically for the less educated, reinforcing the positive relationship between marriage and earnings potential that existed in earlier cohorts.

In this study we examine how these shifts have affected the evolution of earnings differentials associated with marriage, and the importance of selection versus specialization in understanding these changes. Our basic empirical strategy is to estimate marriage gaps in earnings using both cross-sectional and fixed-effect models. Cross-sectional estimates combine the effects of changes in earnings associated with a change in marital status (specialization) with any persistent pre-existing differences in mean earnings between those who are married and those who are not (selection effects). When we include fixed effects, the coefficient on marriage isolates changes in earnings associated with a change in marital status. Our specifications include controls for age, so where our fixed-effects estimates show negative marriage gaps for women, this reflects slower wage growth for women relative to their peers during periods when they are married. The difference between the cross-sectional and fixed-effects estimates then provides us with information on the contribution of selection into marriage in explaining the overall gap.

We use data from Survey of Income and Program Participation (SIPP) panels matched to Social Security Administration earnings records from 1954-2011. These data have many advantages for our purposes: they provide detailed earnings histories that allow us to estimate both cross-sectional and fixed-effect models of the marriage premium; the samples of individuals from the pooled SIPP panels are considerably larger than those available from other long panels such as the PSID or the National Longitudinal Study of Youth 1979 (NLSY79); and the earnings data span a period long enough to allow us to meaningfully compare across birth cohorts. To the best of our knowledge, our paper is the first to compare cross-sectional and fixed-effect models for birth cohorts spanning four decades. An important caveat is that we observe annual earnings but not hours worked in our data, so we cannot separately examine the relative contributions of wages and hours.[8]

To preview our findings, we find that selection into marriage has become increasingly positive: that is, higher levels of education and earnings potential are both associated with a greater likelihood of being married. Like Isen and Stevenson (2010), we find that the most educated women are the most likely to marry among the most recent birth cohorts. Pooling our sample of women born between 1936 and 1975, we find an average cross-sectional earnings gap of approximately -26 percent[9] associated with marriage. The marriage gap is roughly half as large (-12 percent) when we control for the number of children and the presence of young children. Our estimates are somewhat more negative in fixed-effect specifications (-29 percent), indicating that positive selection into marriage leads to some understatement of the average marriage gap for women in our sample.

Comparing across cohorts, we find that the marriage earnings gap became less negative for women both in the cross-section and in fixed-effect estimates. In both sets of estimates, married women who remain childless actually enjoy an earnings *premium* in the most recent birth cohort (born in 1966-75). Among women with children, the difference between the earnings of married and single women also narrows. Comparing cross-sectional and fixed-effect estimates, we find that increasingly positive selection into marriage contributes to the reduction in the marriage earnings gap in cross sectional data. While our findings imply that marriage is no longer associated with lower earnings among women in the most recent cohort, we find that the motherhood gap remains large.

We find quite different results for men. While educated men with the highest earnings potential have always been the most likely to marry, the relationship has become more pronounced among recent birth cohorts. Pooling across all cohorts of men, we find a positive cross-sectional marriage earnings gap equaling approximately 34 percent . In fixed-effect specifications, however, the estimate is reduced by

[8] We do however examine the extent to which women leaving the labor force and have zero earnings.
[9] Our coefficient estimates essentially give log differences, but in the text we use the common (but rough) approximation of referring to 100 times the log difference as a percent change.

more than half (to about 14 percent) suggesting that selection plays an important role in accounting for the marriage premium for men. When we allow the premium to vary across cohorts, we find successively larger positive marriage gaps in the fixed-effect regressions. Taken literally, this would suggest an increase in specialization across successive birth cohorts. But this interpretation assumes that there is not selection into marriage on wage growth. We suspect that this empirical regularity instead represents either selection on individual-specific earnings growth rates, or an interaction between changes in age-earnings profiles and changes in patterns of 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 rates. Section 4 describes our basic empirical methodology and regression results. Section 5 concludes.

II. Data

Our sample of individuals is drawn from respondents in the 1984, 1990-1993, 1996, 2001, 2004 and 2008 SIPP panels who provided the information needed to validate matches to Social Security Administration (SSA) earnings records.[10] 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.[11] For matched individuals, we use annual earnings for 1954-2011 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

[10]The results presented here are based on confidential data from Version 6.0 of the SIPP Gold Standard File. External researchers can access related data through the public-use SIPP Synthetic Beta (SSB) files, and Census will validate results obtained from the SSB on the internal, confidential version of these data (the Completed Gold Standard Files). For more information, please visit http://www.census.gov/programs-surveys/sipp/methodology/sipp-synthetic-beta-data-product.html. The U.S. Census Bureau also supports external researchers use of some of these data through the Research Data Center network (www.census.gov/ces/rdcresearch).

[11] 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. Since our sample pools data from several SIPP panel samples, we do not use SIPP survey weights in our analysis, so the results cannot be assumed to be nationally representative.

earnings are also included. We include employees' contributions to deferred compensation plans as part of our earnings measure. For the years prior to 1978, earnings measures are truncated at the maximum earnings subject to FICA taxes. A significant number of men in our sample have earnings that exceed the taxable maximum, and so are understated in our data.[12] But the cap affects a very small share of our sample of women, in part because our sample is young in those years; for example, they are at most aged 29 in 1965, the year in which the share of all covered workers who had earnings that exceeded the taxable max reached its peak at 36%).[13] For both men and women we used the full distribution of earnings for 1978-1990 to estimate the appropriate adjustment to log earnings based on the assumption that the upper tail of the earnings distribution resembles a Pareto distribution, and then used the parameter estimate to make a mean adjustment to log earnings for those affected by the cap.[14] The very small adjustment to earnings for women made little difference for any of our estimates, so we present those with the adjustment here. As expected, the adjustment had more important effects on the results for men, particularly for estimates of changes across cohorts, given that the earliest cohort was much more affected than later ones. So for men, the regression results we present here are based only on 1978-2011 earnings. Marital histories, educational attainment, and women's fertility histories are based on data collected in the SIPP. Age is based on SSA sources.

[12] In the late 1950s and early 1960s, there were several years in which over 25% of covered workers had earnings that exceeded the taxable maximum. See http://www.ssa.gov/policy/docs/policybriefs/pb2011-02.html.
[13] Women may be more affected by the pre-1978 absence of earnings records in these data for workers who were not subject to FICA taxes, given that the excluded group included many public-school employees. This will bias our estimates for the earliest cohorts if the relationship between earnings and marital status for this group differs from that among the women we do observe.
[14] We estimated separate Pareto parameters for men and women, by year and by education group (<=high school versus >=some college). For women, the estimates were similar across education groups, so we used an adjustment on estimates that pooled the two groups. For men, the implied adjustment for the more educated group was substantially larger, so we applied it separately by education group. For both men and women, the estimated adjustment parameter did not vary much across years. The OLS estimates for men are not very sensitive to which way we handle the capped data—i.e. use all years and ignore the cap, use all years and make Pareto adjustment to capped data, or only use years with uncapped data. But the fixed-effects estimates are more sensitive to this issue. Using all years results in a larger marriage gap for the first cohort, and smaller changes across cohorts, with the Pareto adjusted version resulting in an inconsistent trend across cohorts with fixed effects.

We use these data to look at cohorts born between 1936 and 1975, following their earnings over years in which we have earnings data and the individual had 1 to 35 years of potential experience.[15] 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 drop any earnings records from years after the individual is last observed in the panel. 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 through age 35 for everyone in the sample. Thus for a 50 year old woman who did not start college, interviewed in the 1990 SIPP panel, we use earnings for 1958-1992 (ages 18-52), while for a 35 year old college graduate interviewed in the 1996 panel we use earnings for 1984-1999 (ages 25-38).

Table 1 presents descriptive statistics by birth cohort for the men and women in our sample. The first pair of panels (labeled "Person-weighted") consists of variables that do not vary year by year, so each individual contributes a single observation. The second pair of panels is based on the full set of person/year observations we use in our regressions, so individuals who we follow for more years contribute more observations to the sample means.[16] In general, the differences across cohorts in the top panels reflect well established trends in the population: for example, the earliest cohort has less education and more children. But it is also worth noting that we can follow members of the older cohorts to later ages. This is why the sample mean of age is larger for the older cohorts. On the other hand, despite their higher fertility, women in the first cohort have lower means for time-varying child variables such as having a child aged less than 6 in the current year as shown in the bottom panel.

[15] More specifically, for those who finished high school or less, the age range is 18-53. For those with some college, the age range is 21-56. For college graduates, the age range is 23-58. For those with post-college education, the age range is 25-60. We drop the very small share of men that have a match key, but for whom we never observe positive earnings while meeting our sample exclusion restrictions. But we keep women who similarly have no observed earnings because we estimate participation regressions for women but not men.

[16] Sample counts are rounded to the nearest 100 here to help maintain respondent confidentiality. Person-year counts include all years in the window over which we have data for the individual, whether or not earnings are observed in a particular year.

III. Trends in marriage rates

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.

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.[17] For the small portion of women in the sample that have a match key but not enough years of earnings in sample to provide us with an estimated fixed effect, we assign a random draw from the distribution of this measure. 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.[18] We then assign each person to a potential earnings quartile based on their ranking among those of the same gender in their ten-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.

[17] 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.
[18] More specifically, we assign women with estimated fixed effects to deciles within their cohort/education group based on the share of years in sample for which they have earnings, and then use the bottom decile as our imputation deck.

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 1936-1945 and 1956-1965 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 college graduates, but the share married among those with 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 over time marriage rates dropped more among the less educated, widening the gap across education groups. 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. In the 1936-1945 birth cohort, women who were single at age 35 were slightly more likely to be college graduates than married women (20% versus 19%), but by the 1966-1975 birth cohort, that pattern had reversed: 38% of 35 married women were college graduates, while only 26% of single women that age were.[19] For men, the change is less dramatic but still substantial: the share of college graduates grew 8 percentage points among married men, but did not grow among single men.[20]

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

[19] These statistics are derived by combining information on the cohort-specific education distribution in Table 1 with the information on marital status by education/cohort in Table 2. E.g. Pr(college grad | single)=Pr(single|college grad)*Pr(college grad)/Pr(single).

[20] For men, the share of college graduates does not consistently grow across each of these birth cohorts. The sharp increase and then decline over the first three cohorts of men likely reflects the effect of Vietnam-era draft deferrals on men's college attendance documented in Card and Lemieux (2000), which was greatest for men born in the late 1940s. We focus on the increase from the first to the last cohort as reflecting the longer term trend increase in college attendance.

distribution. Comparing across the 1936-45 and 1966-75 birth cohorts of women, the share married actually increased slightly in the top quartile while it fell 20 percentage points from .80 to .60 in the bottom quartile. For men the marriage declined 6 percentage points in the top quartile, while declining 16 percentage points in the bottom quartile. A striking finding is that, in the most recent cohort, only 55 percent 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 were 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 has increased 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.

IV. Effect of marriage on earnings

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

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

where *i* indexes an 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 gap 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 specified above via ordinary least squares (OLS) 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.[21] The fixed-effect marriage earnings gap captures the average difference in an individual's earnings between periods in which they are married and those in which they are not, relative to the average age-earnings profile. These are our estimates of the effects of specialization, by which we mean changes in earnings that arise from changes in work behavior that result from marriage. The difference between the OLS and fixed-effect estimates provides us with an estimate of selection component of these differentials. That is, the difference gives us an estimate of the extent to which the earnings of those who are currently married are persistently different from the earnings of those who are currently single.[22]

1. Effect of marriage on women's earnings

Table 4 presents coefficient estimates from the earnings regressions for women. In column (1) we report the coefficient on the "married" dummy without controlling for children while in column (2) we

[21] 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.

[22] Note that our specifications implicitly assume that divorce is associated with the same size change in earnings as marriage, but with the opposite sign. We examined estimates of specifications that allowed these effects to differ, but found that this simplification was a reasonably good description of the patterns in our data.

control for the number of children and whether any of those children are under age 6.[23] In column (3) we interact both married and children dummies with birth cohort to estimate the evolution of marriage and child effects on earnings. Finally, in column (4), we additionally interact the married and children variables with each other and with birth cohort dummies, thereby allowing the impact of children on earnings to differ between married and single women, and for that interaction effect to vary across cohorts.

We find a substantial negative earnings differential for married women in the first specification for both OLS and fixed-effect estimates. The larger absolute size of the fixed-effect estimate implies that, for this set of cohorts overall, positive selection into marriage offsets a modest share of the differential. That is, pooling all of our data, the negative changes in earnings profiles associated with marriage are slightly larger than the average difference in earnings between married and single women because women with higher earnings are slightly more likely to be married. Consistent with others' findings, adding controls for children in column (2) reduces the marriage differential substantially. In the OLS regression, the marriage earnings differential falls by more than half, from -.256 to -.118. In the fixed-effect regression, the coefficient falls by about one-third from -.292 to -.197.

In column (3) we add interactions between the married and children variables and ten-year birth cohort dummies to examine changes in these earnings differentials across these cohorts of women. In both the OLS and fixed-effect regressions, the earnings differential associated with marriage becomes less

[23] In our specification we cap the number of children at 3, because in specifications in which we use a series of dummy variables to control for children, additions beyond 3 had little additional effect on our estimates. The count of children less than 18 includes those less than 6, so the coefficient on the count of children less than 18 represents the effect of 1 school-aged child while the coefficient on the dummy for having a child less than 6 gives the additional effect of having that child be pre-school aged. 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 were 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.

negative across birth cohorts. The decline in the earnings differential in fixed-effect estimates suggests a much reduced role for specialization. Comparing the OLS and fixed-effect estimates, we find a larger increase between the earliest and the most recent birth cohorts in the OLS estimate (.331) than in the fixed-effect estimates (.277). This implies that selection into marriage based on potential earnings became increasingly positive across these cohorts—i.e. women with higher potential earnings became more likely to marry. 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 contrast to the consistent shrinkage of the marriage differential across these cohorts, the coefficients on the children/cohort interaction terms do not show a notable decline in the earnings differentials associated with children. The differential associated with young children has declined across cohorts but the differential associated with school-aged children has actually increased.[24]

In the final specification in (4), we also include three way interactions that allow the earnings differentials associated with children to differ between married and single women, and for that to vary across cohorts as well. The three-way interactions make it much more difficult to interpret individual coefficients, so we present a series of earnings differentials in Figures 2 and 3 that describe the patterns of interest: Figure 2 based on the OLS results, and Figure 3 based on our fixed-effect estimates. Each estimate in the figures gives an earnings differential for a particular group based on specification (4) relative to single, childless women in the same cohort. As a way of illustrating how changes across cohorts in the marriage differential differ between women with children and those without, we include implied differentials for two child scenarios—having one school-aged child, and having one pre-school aged child.

[24] Our results are somewhat at odds with Pal and Waldfogel (2015) who find a decline in the motherhood wage gap in the more recent period, 1993-2013. One potential explanation for the differences in our findings is that they focus on hourly wages instead of annual earnings. They also include more recent birth cohorts at younger ages, which may put more weight on the motherhood gap associated with having young children. We also find that the motherhood gap associated with young children has decreased.

The more heavily shaded bars in the graphs represent differentials for married women, while the lighter bars give those for single women. The top panel gives the marriage differential for women without children, and in both figures this differential is positive for the most recent cohort. In other words, among the most recent birth cohort of women who remain childless, marriage actually *increases* earnings relative to single women without children.[25] The second and third panels give estimated differentials for married and single women with one preschool aged child and one school-aged child respectively. In both the OLS and fixed-effect results, we see little change in the earnings of single mothers relative to single, childless women, but large decreases for married women with children relative to that group.

The marriage differential in these figures, conditional on children, is given by the difference between the married and single bars and we present this differential in Figures 4 and 5. Both the OLS and fixed-effect estimates imply that the gap in earnings between married and single women with children has declined dramatically. The OLS results (Figure 4) suggest that married women with children now earn more than single women with children, but comparison with the fixed-effect results (Figure 5) suggests that this is due to married women with children being increasingly positively selected relative to single women with children. While our findings imply that marriage is no longer associated with lower earnings among women in the most recent cohort (with the exception of married women with young children who are still slightly behind their single counterparts), it is important to keep in mind that the motherhood gap (as shown in Figure 3) remains substantial. Even among the most recent birth cohort, married women with pre-school age children have approximately 40 percent lower earnings compared to married women without children, while married women with school-aged children have 20 percent lower earnings.

2. Effect of marriage on women's participation

[25] The fact that this is also true for the fixed-effect estimates suggests that this is not due to selection on earnings levels. One possibility is that women are increasingly likely to marry upon finishing school so that we observe their earnings rise along with change in marital status.

These log earnings results condition on having positive earnings. While we cannot separately examine wages and hours worked in our data, we can examine to what extent the earnings changes are driven by women entering or leaving the labor force and having positive versus zero earnings. To look at the extensive margin for women, we estimate specifications similar to (1) but with an indicator for having positive earnings in a calendar year as the dependent variable.[26]

The results of the regressions are reported in Table A1 of the appendix but we illustrate changes in the participation gap associated with marriage and motherhood in Figures 6 and 7 (showing OLS and fixed-effect estimates respectively). Like Figures 4 and 5, these are based on specifications in which we allow for interactions between marriage and children that change across cohorts. In both figures, we see that, among women without children, the participation differential between married and single women declined so that married women are in fact slightly more likely to participate than single women in the most recent birth cohort. In contrast, the participation differential between married mothers and single mothers remains negative and shows no clear pattern of decline. Mothers with young children in particular continue to participate substantially less than single and married women without children. Our results on the participation effect of young children are broadly consistent with Byker (2015) who also does not find much trend across cohorts in changes in mother's labor force participation around the time of birth.

3. Effect of marriage on women's earnings by education and race

We further explore the effects of marriage on women's earnings by examining whether these patterns differ by education or race. We report the regression results in appendix Table A2 but highlight trends based on the fully interacted model in Figures 8 and 9. We find patterns similar to those in our main results when we look separately at the evidence for specific education groups and for black women:

[26] Note that because we use the absence of earnings as non-employment, the estimates for the first two cohorts are likely to be affected by the exclusion of those not subject to FICA taxes in years prior to 1978. In our next draft, we will try to gauge the potential magnitude of this bias.

the earnings differentials associated with marriage becomes less negative across cohorts, but part of that decline reflects increasingly positive selection into marriage. The selection effect is more pronounced among less educated women—those with a high school education or less and those with some college—and among black women, so we find that positive selection into marriage has become increasingly important for these groups.

4. Effect of marriage on men's earnings

Table 5 and Figure 10 present estimates of the marriage gap for men based on the same log earnings regressions we estimated for women except that we do not control for children.[27] We find a very large positive marriage premium for men in the cross-sectional regressions (34%), but the fixed effect estimates are much smaller (14%). This is consistent with our findings in Tables 2 and 3 that selection on labor market skills into marriage are quite positive for men. But the estimated interaction effects with birth cohort dummies are somewhat puzzling. They indicate that the fixed-effects marriage gap has risen steadily across cohorts. The cross-sectional estimates have risen as well, but have done so more slowly and less consistently. The larger increase in the marriage premium over time in the fixed-effect results suggests that selection into marriage has declined somewhat in importance, while the effects of specialization after marriage have increased. That seems inconsistent with both the increase in the selectivity of marriage for men—documented in Tables 2 and 3—and with evidence of increased labor market skills and work among married women across these cohorts. When we allow the marriage premium to vary with education level (Figure 11 and Table A4 in the appendix), the OLS and fixed-effect results similarly imply a reduction in selection but growth in specialization for each group of men we examine.

[27] We omit controls for children 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 treat all fathers interviewed after their children were grown as if they were childless.

One possibility is that the fixed-effect estimates for early cohorts of men are downward biased because we do not use pre-1978 earnings for men, and so have earnings for them only at older ages. For example, we observe earnings beginning at age 42 for those born in 1936, long after most would have started a first marriage. That means that changes in marital status for this group are likely to primarily involve divorce and remarriage, and these might have smaller effects on earnings than first marriages. But note that our cross-sectional estimates for men based on public-use data in Figure 1 are reasonably consistent with our estimates based on the SIPP-SSA panel in that they also show an inconsistent rise in the marriage gap for men. 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 cross-sectional 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-effect 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.

V. Conclusion

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 1936 to 1975.

How has this increased selection affected male and female marriage gaps? Pooling our sample of women born between 1936 and 1975, we find an average earnings gap of approximately 26 percent associated with marriage, but it falls to 12 percent when we control for children. Our fixed-effect estimates are somewhat larger at 29 percent, indicating to a slight positive selection into marriage overall. Comparing across cohorts, we find that the marriage earnings gap fell for women both in the cross-section and in fixed-effect estimates. Both sets of estimates imply that married women with no minor children actually enjoy an earnings premium in the most recent birth cohort (women born in 1966-75). The difference between the earnings of married and single women also narrows among women with children. Comparing cross-sectional and fixed-effect estimates, we find that increasingly positive selection into marriage contributes to the reduction in the cross-sectional marriage earnings gap. The result is that the earnings advantage observed for our most recent cohort in cross-sectional comparisons is somewhat misleading. Particularly for mothers of preschool-aged children, marriage continues to be associated a reduction in earnings growth relative to peers that we associate with household specialization. While our findings imply that marriage is no longer associated with lower earnings among childless, we find that the motherhood gap remains substantial.

We find quite different results for men. Pooling all of our cohorts, we find an average marriage earnings premium equaling 34 percent in cross-sectional data for men. In fixed-effect results, however, the estimate is reduced to less than 14 percent, suggesting that selection plays a much more important role in accounting for the positive marriage gap for men. In specifications that allow marriage to affect only the level of male earnings, we find successively larger marriage premiums in the fixed-effect 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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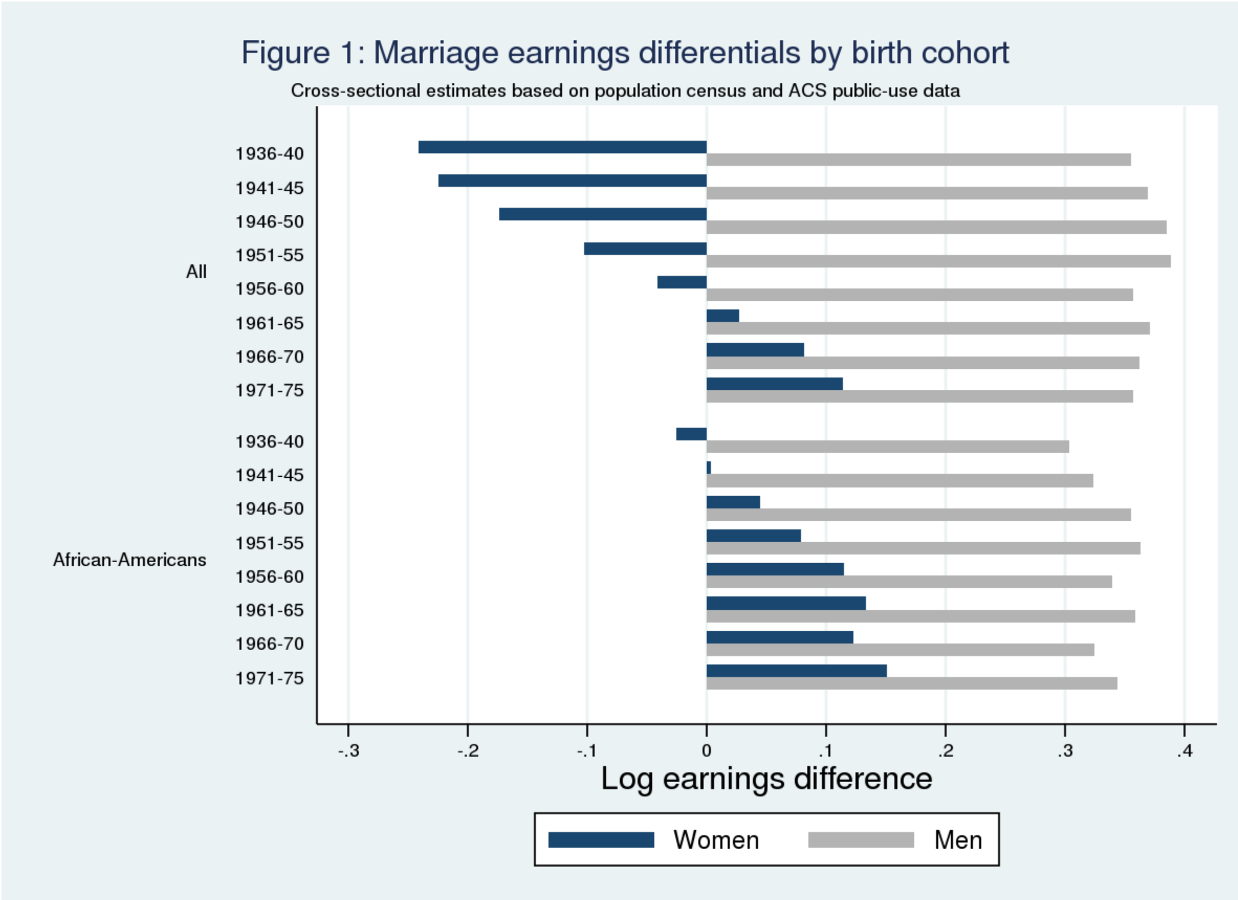
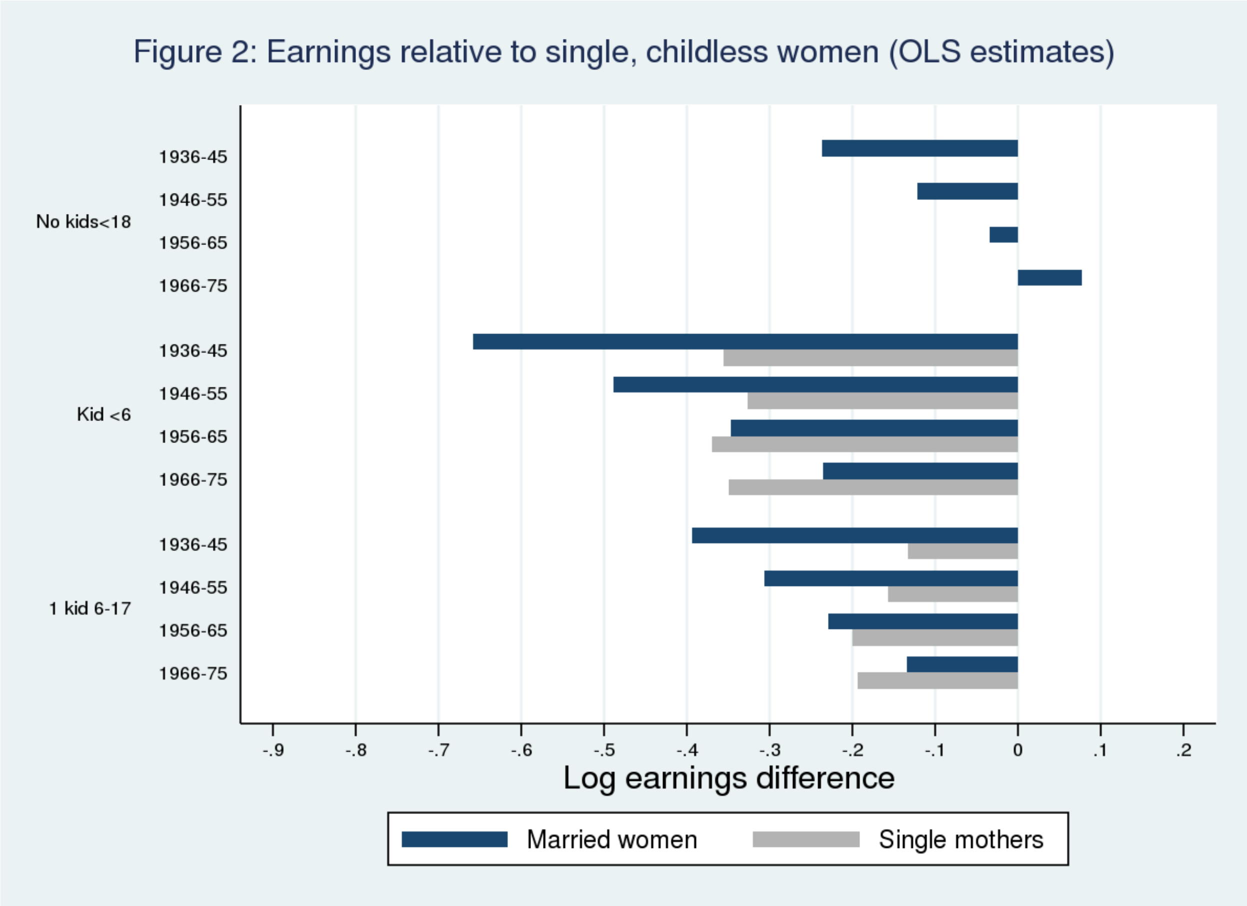


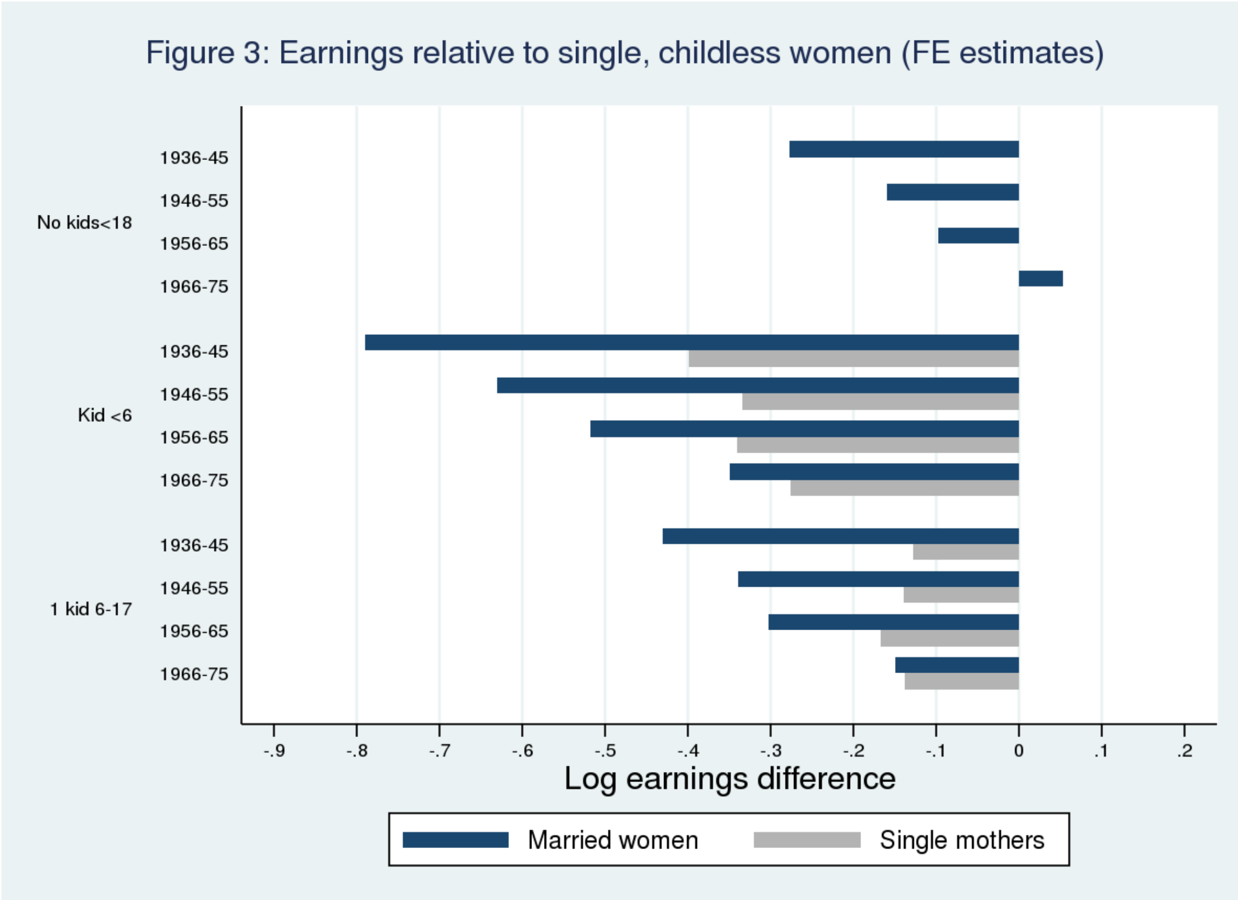
Source: Author's calculations based on 1960-2000 decennial census and 2010 American Community Survey IPUMS data.

Notes: The figure reports coefficients on 5-year birth cohort dummies interacted with a dummy for being currently married from regressions with log annual wage and salary earnings as the dependent variable. These regressions also include year dummies, a quartic in age, education dummies, and (in results for women) number of children, and presence of children<6. The sample consists of men and women who have 1-35 years of potential experience.



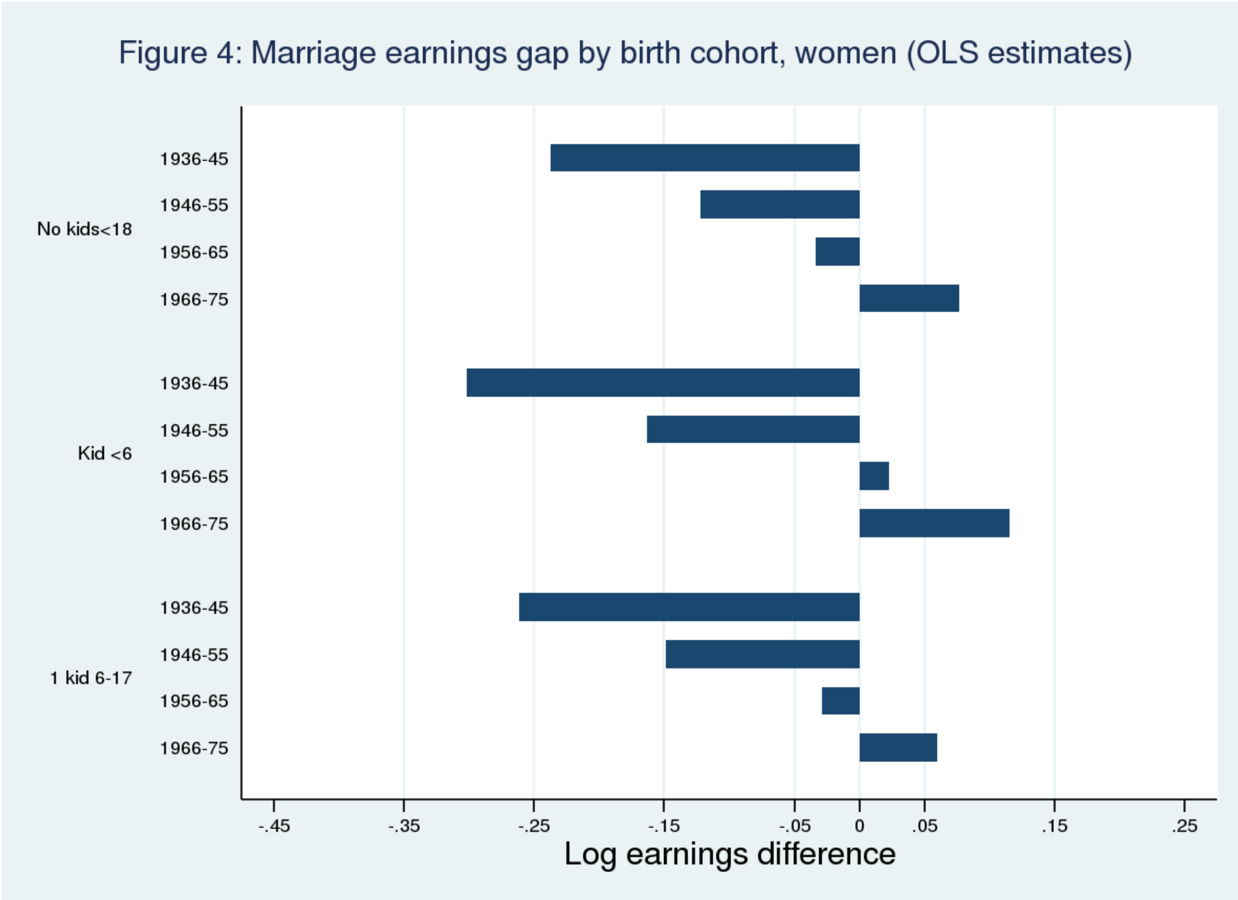
Source: Authors' calculationss based on SIPP-SSA data.

Note: Estimates are based on the fully interacted model reported in Table 4 (column 4, OLS). Each bar corresponds to the earnings differential for the specified group relative to single, childless women in the same cohort. We report implied differentials for two child scenarios—having one school aged child, and having one pre-school aged child.

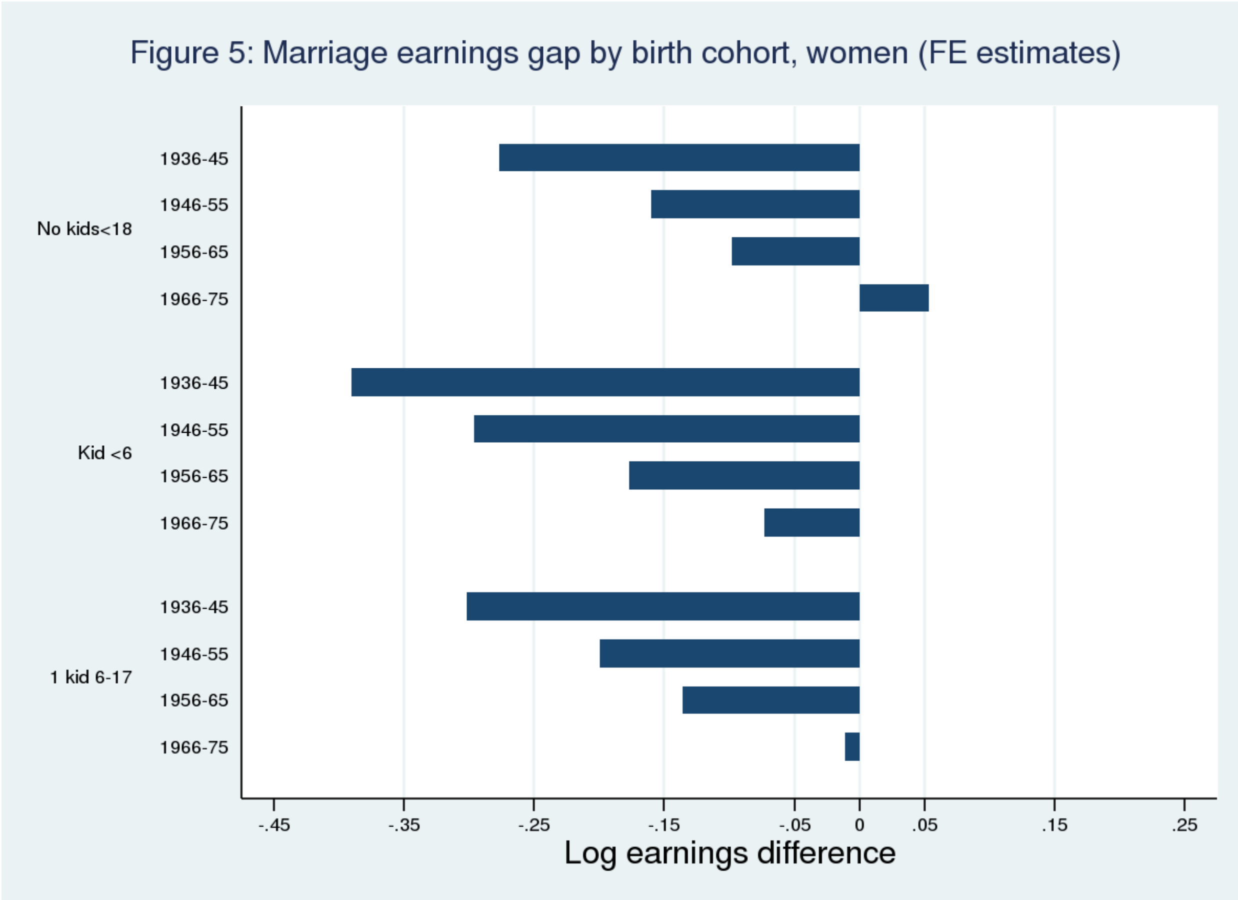


Source: Authors' calculations based on SIPP-SSA data.

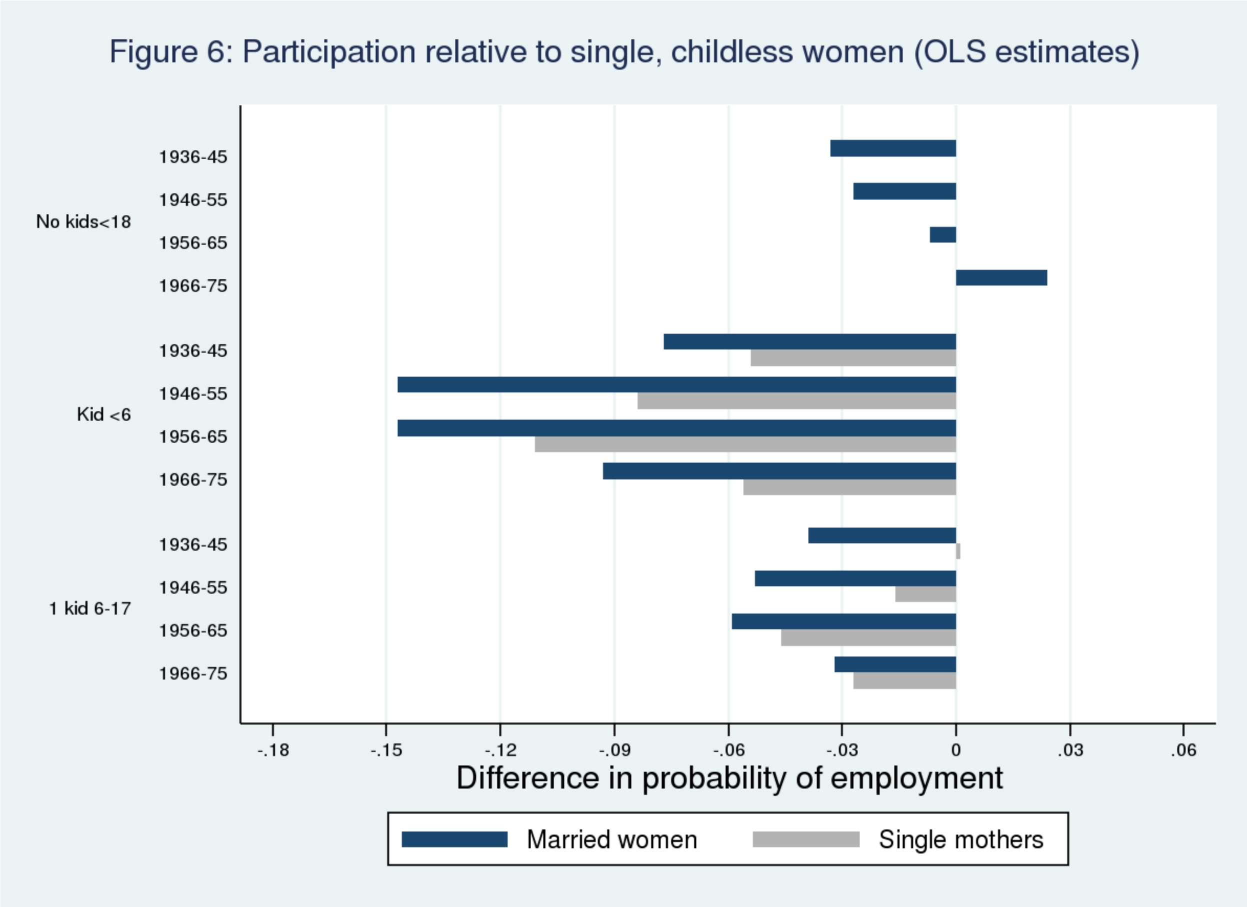
Note: The estimates are based on the fully interacted model reported in Table 4 (column 4, FE). Each bar corresponds to the earnings differential for the specified group relative to single, childless women in the same cohort. We report implied differentials for two child scenarios—having one school aged child, and having one pre-school aged child.



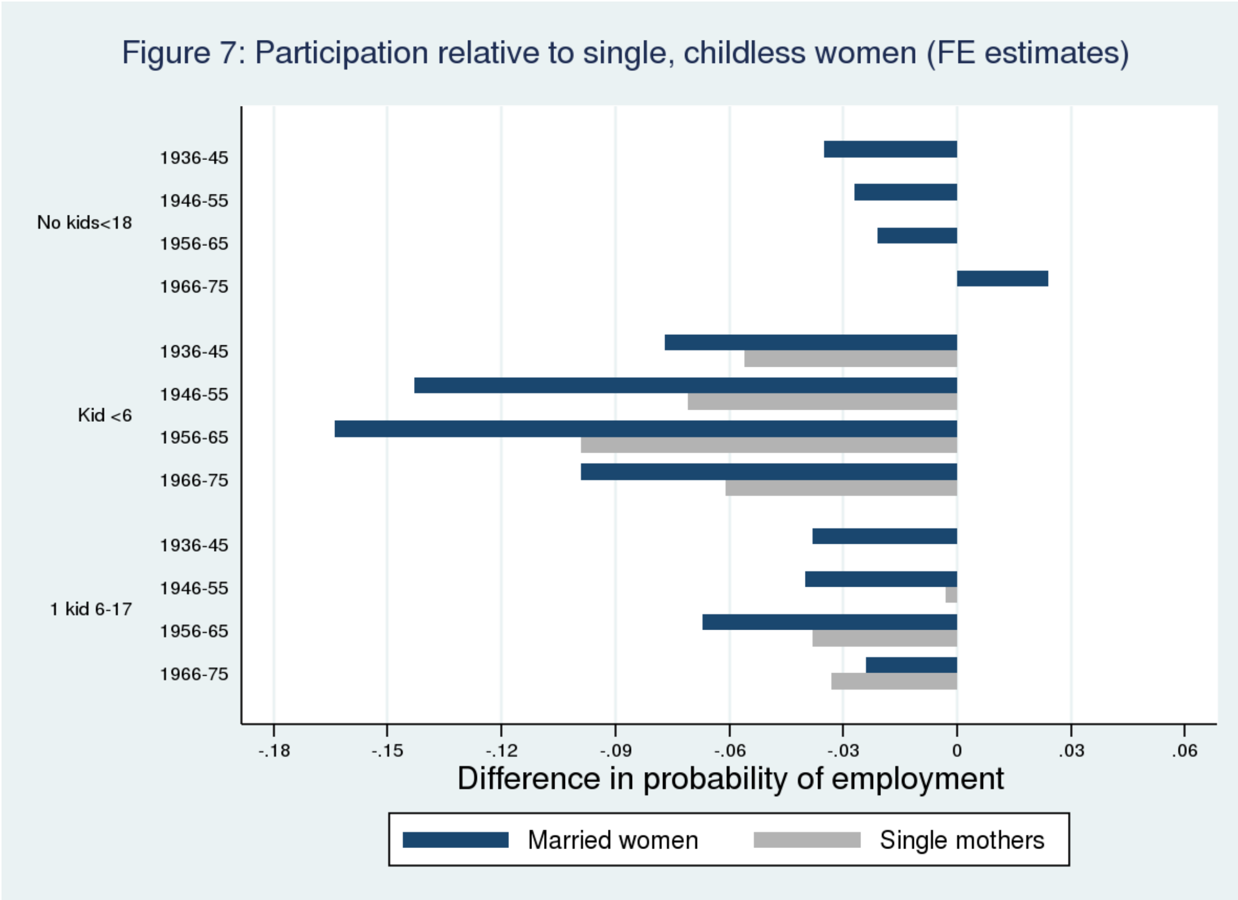
Note: The estimates are based on the fully interacted model reported in Table 4 (column 4, OLS). The marriage differential in these figures, conditional on children, is given by the difference between the married and single bars in Figure 2.



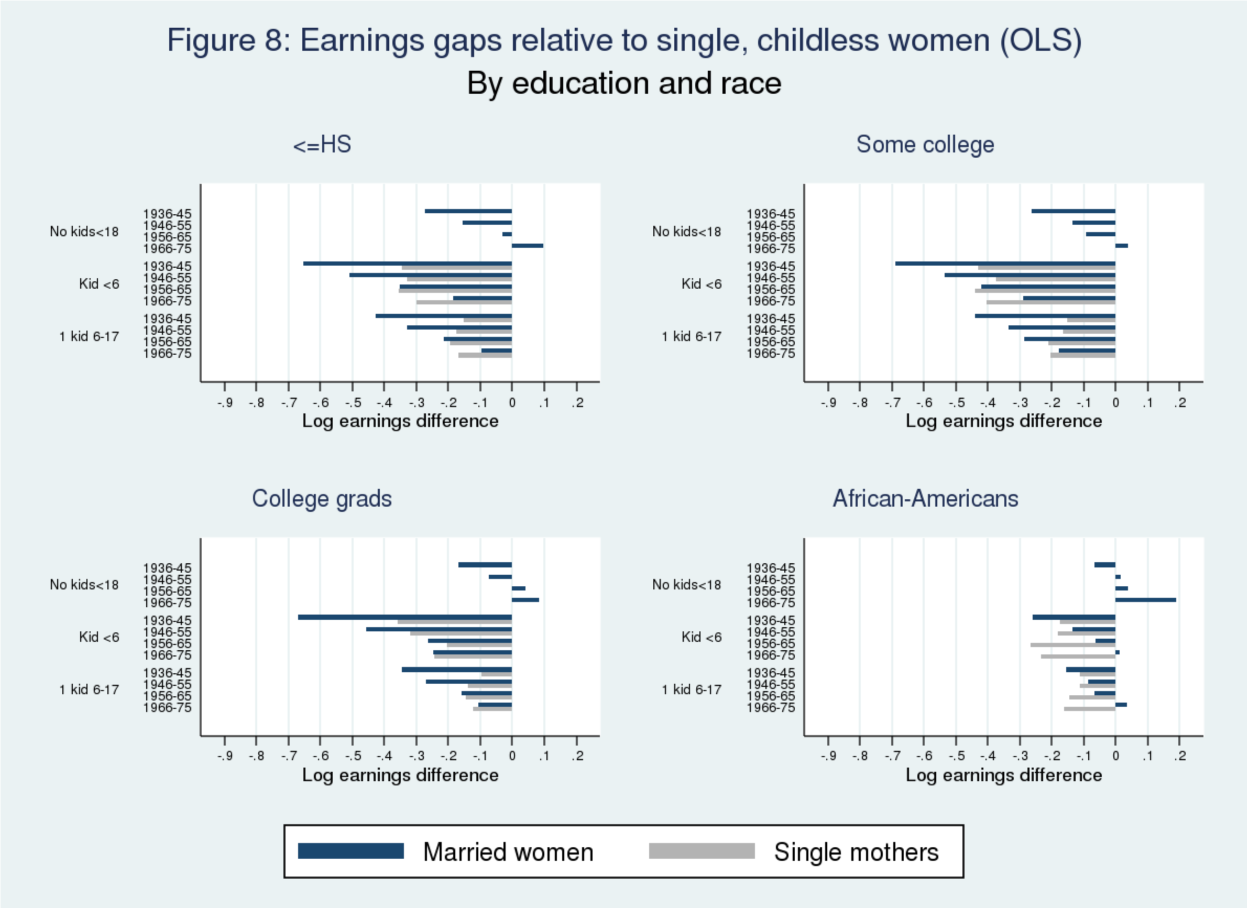
Note: The estimates are based on the fully interacted model reported in Table 4 (column 4, FE). The marriage differential in these figures, conditional on children, is given by the difference between the married and single bars in Figure 3.



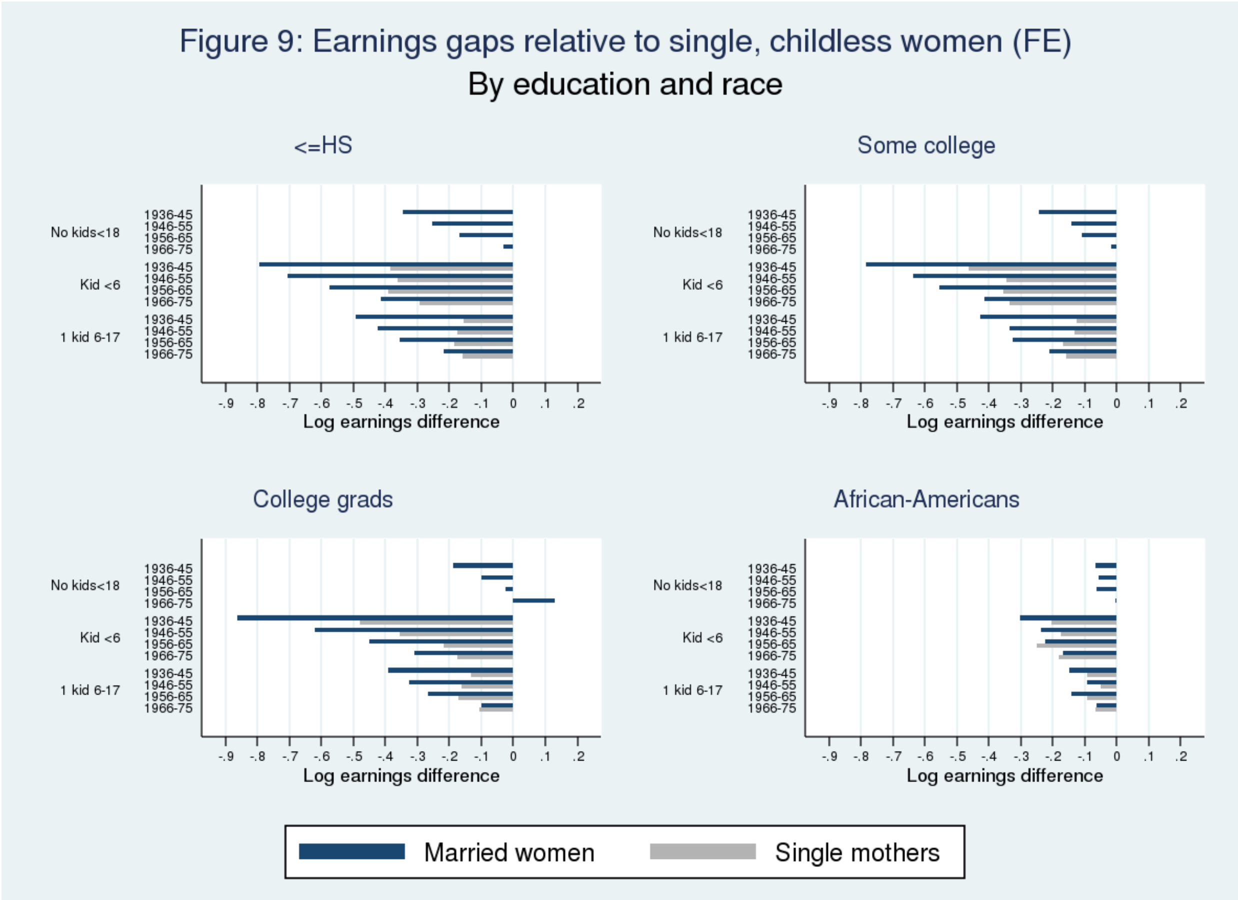
Note: The estimates are based on the fully interacted model reported in Table A1 (column 4, OLS). Each bar corresponds to the participation differential for the specified group relative to single, childless women in the same cohort. We report implied differentials for two child scenarios—having one school aged child, and having one pre-school aged child.



Note: The estimates are based on the fully interacted model reported in Table A2 (column 4, FE). Each bar corresponds to the participation differential for the specified group relative to single, childless women in the same cohort. We report implied differentials for two child scenarios—having one school aged child, and having one pre-school aged child.

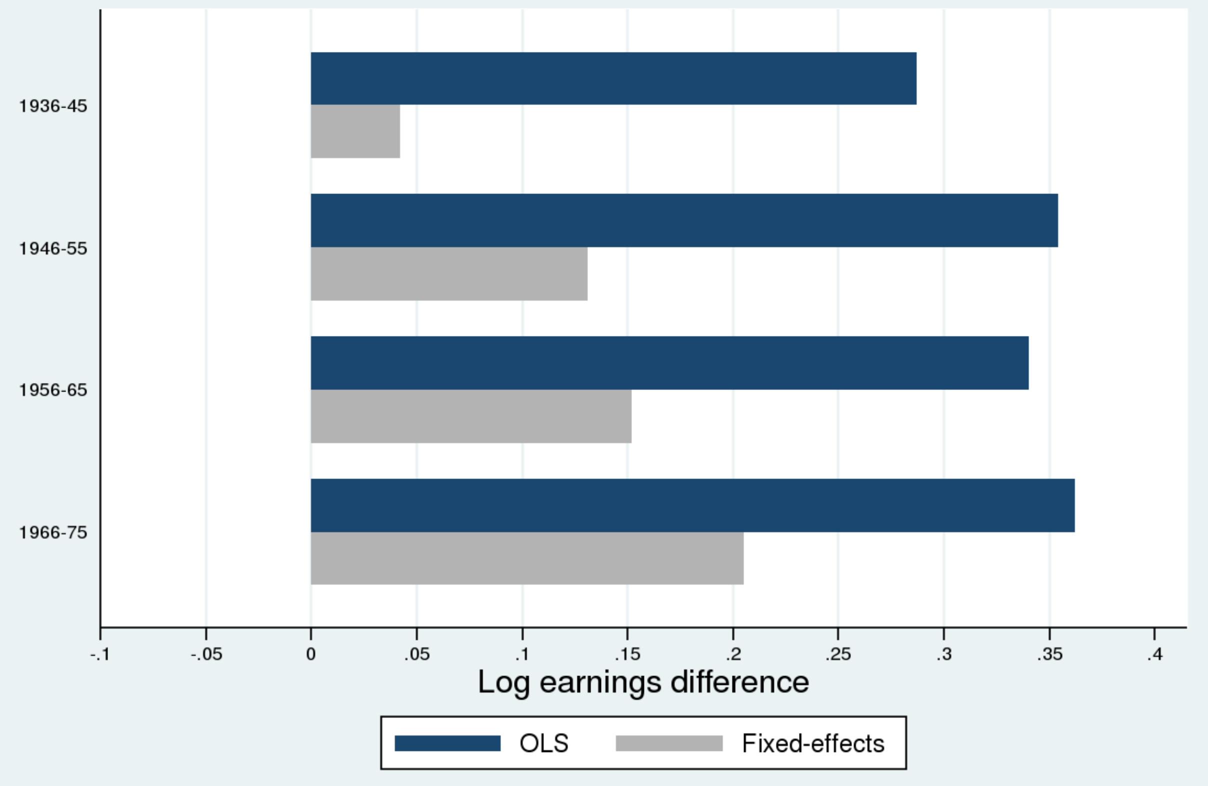


Note: The estimates are based on the fully interacted model reported in Table A2 (OLS). Each bar corresponds to the earnings differential for the specified group relative to single, childless women in the same cohort. We report implied differentials for two child scenarios—having one school aged child, and having one pre-school aged child.



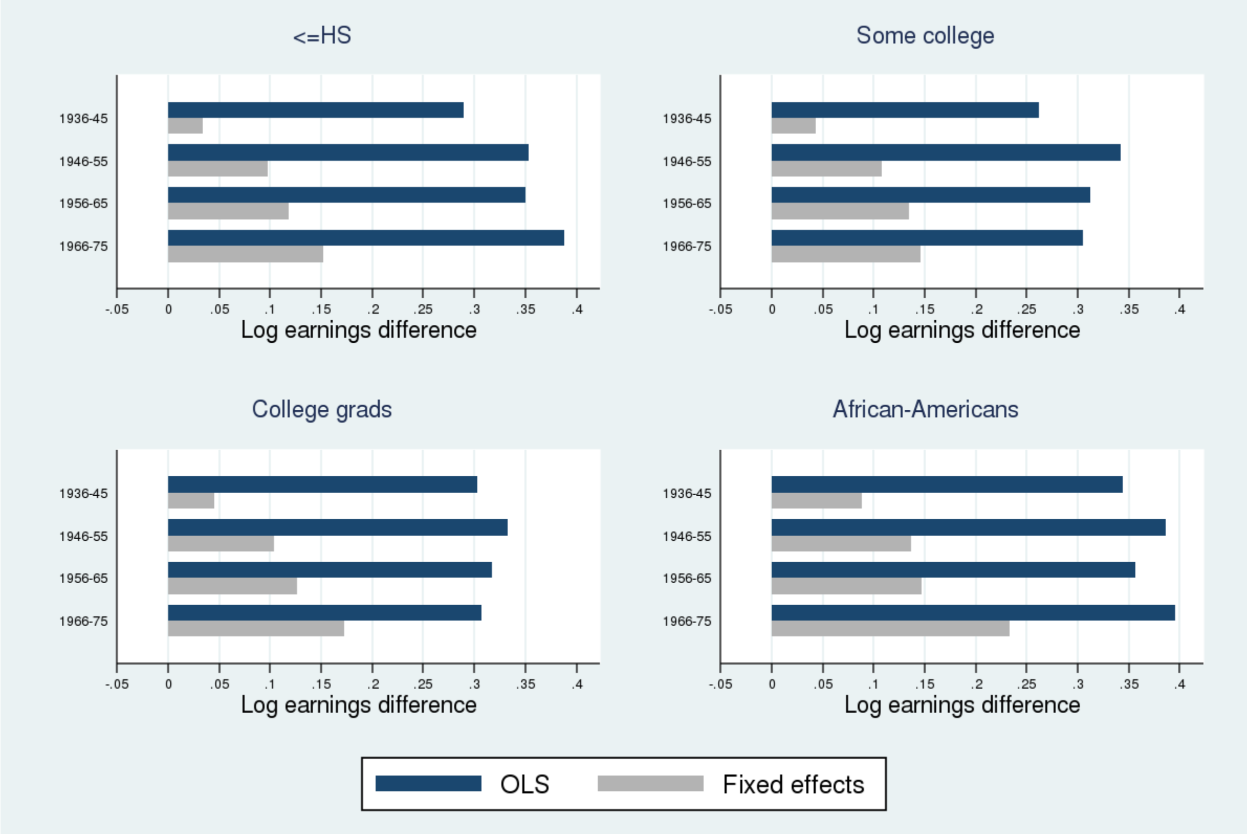
Note: The estimates are based on the fully interacted model reported in Table A2 (column 4, FE). Each bar corresponds to the earnings differential for the specified group relative to single, childless women in the same cohort. We report implied differentials for two child scenarios—having one school aged child, and having one pre-school aged child.

Figure 10: Marriage earnings gap by birth cohort, men (OLS and FE estimates)



Note: The estimates are based on the model reported in Table 5 (column2 OLS and FE). Each bar corresponds to the earnings differential for married relative to single men in the same cohort.

Figure 11: Earnings gaps for men, by education and race



Note: The estimates are based on the model reported in Table 5 (column2 OLS and FE). Each bar corresponds to the earnings differential for married relative to single men in the same cohort and with the same education or race..

Table 1: Sample means

Birth cohorts:	1936-45	1946-55	1956-65	1966-75
	Averages across individuals			
Women				
Ever married	0.942	0.909	0.868	0.833
Any children	0.864	0.824	0.821	0.828
Number of Children Ever Born	2.5	2.0	1.9	2.0
Number of years with earnings	19.1	19.5	17.0	14.1
Number of years in sample	33.5	28.4	23.6	18.8
<=High school grad	0.555	0.421	0.386	0.285
Some college	0.256	0.320	0.352	0.374
College grad	0.189	0.259	0.262	0.341
N (rounded)	19,300	35,700	28,300	10,800
Men				
Ever married	0.946	0.906	0.849	0.818
Number of years with earnings	31.3	27.1	23.3	19.0
Number of years in sample	33.7	28.2	23.6	19.0
<=High school grad	0.472	0.371	0.409	0.331
Some college	0.253	0.313	0.319	0.347
College grad	0.275	0.316	0.272	0.322
N (rounded)	21,000	32,100	25,200	9,700
	Averages across person-years			
Women				
Log earnings, $2014, adjusted for cap in years<1978	9.685	9.820	9.799	9.772
Has positive earnings in current year	0.611	0.719	0.761	0.789
Age	36.3	34.8	32.5	30.1
Married	0.716	0.648	0.578	0.525
Number of children <18	1.396	1.281	1.306	1.336
Has child who is <6	0.149	0.166	0.193	0.228
N (rounded)	646,800	1,014,900	666,700	202,800
Men				
Log earnings, $2014, adjusted for cap in years<1978	10.590	10.515	10.363	10.272
Has positive earnings in current year	0.887	0.893	0.887	0.895
Age	36.9	35.0	32.4	30.0
Married	0.710	0.633	0.530	0.468
N (rounded)	708,500	905,200	595,500	183,500

Source: Survey of Income and Program Participation (SIPP) panels matched to Social Security Administration earnings records from 1954-2011. See text for details.

Table 2: Marriage rates by education group

Birth cohort	Share married at age 35 <=HS	Some college	College grads	Overall
Women				
1936-45	0.796	0.790	0.783	0.792
1946-55	0.703	0.711	0.716	0.709
1956-65	0.671	0.667	0.721	0.683
1966-75	0.628	0.643	0.751	0.676
Men				
1936-45	0.799	0.804	0.825	0.807
1946-55	0.706	0.722	0.750	0.725
1956-65	0.653	0.677	0.724	0.680
1966-75	0.636	0.672	0.761	0.688

Notes: N= 94,100 women and 88,000 men.

Note: The marriages rates are based on the data described in Table 1. We report the share of men and women who are married at age 35.

Table 3: Marriage rates by quartiles of potential earnings distribution

Birth cohort	Share married at age 35 (1)	(2)	(3)	(4)	Total
Women					
1936-45	0.795	0.811	0.795	0.767	0.792
1946-55	0.700	0.723	0.692	0.722	0.709
1956-65	0.623	0.677	0.679	0.751	0.683
1966-75	0.604	0.654	0.666	0.779	0.676
Men					
1936-45	0.712	0.810	0.844	0.863	0.807
1946-55	0.590	0.736	0.776	0.799	0.725
1956-65	0.536	0.680	0.726	0.778	0.680
1966-75	0.548	0.682	0.725	0.799	0.688

Notes: N= 94,100 women and 88,000 men.

Note: The marriages rates are based on the data described in Table 1. 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. We report the share of men and women who are 35 and in the potential earnings category who report they are married.

Table 4: Log earnings regressions, Women

	OLS coefficient estimates				Fixed-effect coefficient estimates			
Controls	**1**	**2**	**3**	**4**	**1**	**2**	**3**	**4**
Married	-.256***	-.118***	-.266***	-.237***	-.292***	-.197***	-.312***	-.277***
Married*Cohort 1946-55			.116***	.115***			.101***	.117***
Married*Cohort 1956-65			.245***	.203***			.166***	.179***
Married*Cohort 1966-75			.331***	.314***			.277***	.330***
Number of children <18		-.177***	-.149***	-.133***		-.167***	-.145***	-.128***
# children*Cohort 1946-55			-.025***	-.024**			-.022***	-.012**
# children*Cohort 1956-65			-.048***	-.067***			-.046***	-.039***
# children*Cohort 1966-75			-.054***	-.061***			-.029***	-.010
Child <6 yrs old		-.167***	-.257***	-.223***		-.250***	-.344***	-.271***
Child <6*Cohort 1946-55			.076***	.054*			.073***	.077***
Child <6*Cohort 1956-65			.126***	.053*			.139***	.098***
Child <6*Cohort 1966-75			.138***	.067*			.160***	.133***
Married*# children				-.024***				-.025***
Married*# children*Cohort 1946-55				-.003				-.014***
Married*# children*Cohort 1956-65				.029**				-.013**
Married*# children*Cohort 1966-75				.007				-.039***
Married*Child <6				-.041				-.088***
Married*Child <6*Cohort 1946-55				.027				-.009
Married*Child <6*Cohort 1956-65				.093**				.047**
Married*Child <6*Cohort 1966-75				.096**				.026

Notes: N=1,696,700 (to nearest 100). Dependent variable is log annual earnings from SSA records. Regressions also include controls for year, education, dummies indicating if race is African-American, and indicating if ethnicity is Hispanic, main effects for birth cohort, and a quartic in age. Standard errors for each cell appear in appendix Table A5.

Table 5: Log earnings regressions, Men

	OLS coefficient estimates		Fixed-effect coefficient estimates	
Controls	**1**	**2**	**1**	**2**
Married	.339***	.287***	.139***	.042***
	(.004)	(.011)	(.002)	(.005)
Married*Cohort 1946-55		.067***		.089***
		(.014)		(.006)
Married*Cohort 1956-65		.053***		.110***
		(.013)		(.006)
Married*Cohort 1966-75		.075***		.163***
		(.016)		(.007)

Notes: N=1,617,700 (to nearest 100). Dependent variable is log annual earnings from SSA records for years 1978-2011. Regressions also include controls for year, education, dummies indicating if race is African-American, and indicating if ethnicity is Hispanic, main effects for birth cohort, and a quartic in age.

APPENDIX TABLES

Table A1: Participation regressions, Women

	OLS coefficient estimates				Fixed-effect coefficient estimates			
Controls	**1**	**2**	**3**	**4**	**1**	**2**	**3**	**4**
Married	-.063***	-.034***	-.039***	-.033***	-.057***	-.038***	-.037***	-.035***
Married*Cohort 1946-55			-.002	.006			-.006***	.008***
Married*Cohort 1956-65			.021***	.026***			.002	.014***
Married*Cohort 1966-75			.017***	.057***			.034***	.059***
Number of children <18		-.025***	-.003***	.001		-.016***	-.002***	.000
# children*Cohort 1946-55			-.018***	-.017***			-.008***	-.003***
# children*Cohort 1956-65			-.045***	-.047***			-.040***	-.038***
# children*Cohort 1966-75			-.036***	-.028***			-.039***	-.033***
Child <6 yrs old		-.076***	-.041***	-.055***		-.080***	-.041***	-.056***
Child <6*Cohort 1946-55			-.049***	-.013*			-.055***	-.012**
Child <6*Cohort 1956-65			-.041***	-.010			-.047***	-.005
Child <6*Cohort 1966-75			-.011**	.026***			-.021***	.028***
Married*# children				-.007***				-.003***
Married*# children*Cohort 1946-55				-.003				-.007***
Married*# children*Cohort 1956-65				.001				-.005***
Married*# children*Cohort 1966-75				-.022***				-.012***
Married*Child <6				.017***				.017***
Married*Child <6*Cohort 1946-55				-.043***				-.052***
Married*Child <6*Cohort 1956-65				-.040***				-.053***
Married*Child <6*Cohort 1966-75				-.049***				-.064***

Notes: N=2,382,700 (to nearest 100). Dependent variable =1 if the individual has any SSA earnings in the current year, 0 otherwise. Regressions also include controls for year, education, dummies indicating if race is African-American, and indicating if ethnicity is Hispanic, main effects for birth cohort, and a quartic in age. Standard errors for each cell appear in appendix Table A6.

Table A2: Log earnings regressions by education and race, women								
	<=High school		Some college		College grads		African-Americans	
Controls	OLS	FE	OLS	FE	OLS	FE	OLS	FE
Married	-.273***	-.345**	-.263**	-.243**	-.170**	-.188**	-.067	-.066**
Married*1946-55 cohort	.118***	.090**	.126**	.101**	.097**	.089**	.081	.009
Married*1956-65 cohort	.242***	.175**	.169**	.132**	.211**	.162**	.104*	.001
Married*1966-75 cohort	.369***	.315**	.302**	.224**	.255**	.319**	.255**	.060
Number of children<18	-.153***	-.156**	-.153**	-.127**	-.098**	-.133**	-.112**	-.093**
# Children * 1946-55 cohort	-.022*	-.019**	-.012	-.007	-.041*	-.028**	-.000	.042**
# Children * 1956-65 cohort	-.041***	-.029**	-.059**	-.043**	-.047*	-.039**	-.034	.001
# Children * 1966-75 cohort	-.016	-.004	-.053**	-.032**	-.024	.025	-.050*	.027
Has child <6 yrs old	-.191***	-.228**	-.278**	-.338**	-.260**	-.348**	-.062	-.110**
Child <6 * 1946-55 cohort	.038	.040	.069	.125**	.081	.155**	-.008	-.015
Child <6 * 1956-65 cohort	.030	.023	.048	.152**	.201**	.303**	-.059	-.050
Child <6 * 1966-75 cohort	.061	.095**	.079	.163**	.138	.280**	-.009	-.007
Married * Number of children<18	-.001	.008	-.023	-.056**	-.078**	-.069**	.024	.009
Married * # children * 1946-65 cohort	.001	-.003	-.010	-.003	.020	.004	-.013	.006
Married * # children * 1956-65 cohort	.011	-.007	.041*	.011	.024	-.001	.018	.006
Married * # children * 1966-75 cohort	-.024	-.037**	.013	.023*	.009	-.053**	-.016	-.002
Married*child <6	-.036	-.074**	.028	-.019	-.063	-.125**	-.043	-.044
Married * Child<6 * 1946-55 cohort	.011	-.018	-.019	-.071*	.056	.023	.063	.024
Married * Child<6 * 1956-65 cohort	.060	.058*	.069	-.023	.015	-.013	.167**	.123**
Married * Child<6 * 1966-75 cohort	.080	.013	.059	-.010	.045	-.018	.092	.057
N (rounded)	670,300	670,300	584,000	584,000	442,400	442,400	210,900	210,900

Notes: Dependent variable is log annual earnings from SSA records. Regressions also include controls for year, education (except in columns 3/4 where no additional detail is available), dummies indicating race is African-American (in regressions for education groups), and indicating that ethnicity is Hispanic, main effects for birth cohort, and a quartic in age. Standard errors for each cell appear in appendix Table A7.

Table A3: Participation regressions by education and race, women

	<=High school		Some college		College grads		African-Americans	
Controls	**OLS**	**FE**	**OLS**	**FE**	**OLS**	**FE**	**OLS**	**FE**
Married	-.034***	-.031**	-.033**	-.035**	-.034**	-.042**	-.010	.004
Married*1946-55 cohort	.012*	.001	.009	.005	.003	.021**	-.003	-.013*
Married*1956-65 cohort	.036***	.004	.020*	.005	.032**	.037**	.063**	.008
Married*1966-75 cohort	.076***	.043**	.054**	.046**	.044**	.074**	.043	.016
Number of children<18	-.004*	-.006**	.000	.001	.003	-.004	-.008*	-.010**
# Children * 1946-55 cohort	-.011***	.006**	-.012**	-.006**	-.022**	-.014**	-.015**	.013**
# Children * 1956-65 cohort	-.044***	-.026**	-.036**	-.040**	-.033**	-.040**	-.020**	-.000
# Children * 1966-75 cohort	-.020***	-.022**	-.020**	-.039**	-.019**	-.034**	.002	.006
Has child <6 yrs old	-.055***	-.049**	-.062**	-.073**	-.055**	-.058**	-.022**	-.022**
Child <6 * 1946-55 cohort	-.009	-.019**	-.016	.002	-.006	.006	-.018	-.026**
Child <6 * 1956-65 cohort	-.004	-.013*	-.016	.010	.013	.019	-.030**	-.037**
Child <6 * 1966-75 cohort	.035**	.027**	.014	.029**	.043*	.054**	.030*	.014
Married * Number of children<18	-.004*	-.000	-.010**	-.009**	-.012**	-.006**	.004	.001
Married * # children * 1946-65 cohort	-.001	-.006**	-.007	-.003	-.004	-.009**	.012*	.004
Married * # children * 1956-65 cohort	.003	.002	-.006	-.003	-.010	-.013**	-.004	.010**
Married * # children * 1966-75 cohort	-.026***	-.005	-.022**	-.005	-.029**	-.017**	-.018	.003
Married*child <6	.021***	.016**	.029**	.037**	.013	.014	-.015	-.012
Married * Child<6 * 1946-55 cohort	-.048***	-.044**	-.047**	-.073**	-.051**	-.064**	.025*	.018*
Married * Child<6 * 1956-65 cohort	-.046***	-.047**	-.038**	-.069**	-.062**	-.064**	.055**	.042**
Married * Child<6 * 1966-75 cohort	-.057***	-.061**	-.037**	-.064**	-.070**	-.086**	-.008	-.011
N (rounded)	1,070,500	1,070,500	761,700	761,700	550,600	550,600	290,000	290,000

Notes: Dependent variable is =1 if the individual has any SSA earnings in the current year, 0 otherwise. Regressions also include controls for year, education (except in columns 3/4 where no additional detail is available), dummies indicating race is African-American (in regressions for education groups), and indicating that ethnicity is Hispanic, main effects for birth cohort, and a quartic in age. Standard errors for each cell appear in appendix Table A8.

Table A4: Log earnings regressions by education and race, men

	<=High school		Some college		College grads		African-Americans	
Controls	**OLS**	**FE**	**OLS**	**FE**	**OLS**	**FE**	**OLS**	**FE**
Married	.290***	.034***	.262***	.043***	.303***	.045***	.344***	.088***
	(.007)	(.009)	(.008)	(.010)	(.008)	(.009)	(.036)	(.020)
Married*1946-55 cohort	.063***	.064***	.080***	.065***	.030***	.059***	.042	.049*
	(.008)	(.011)	(.009)	(.011)	(.009)	(.010)	(.044)	(.023)
Married*1956-65 cohort	.060***	.084***	.050***	.092***	.014	.081***	.012	.059*
	(.008)	(.010)	(.009)	(.011)	(.009)	(.011)	(.044)	(.023)
Married*1966-75 cohort	.098***	.118***	.043***	.103***	.004	.128***	.051	.145***
	(.011)	(.012)	(.011)	(.012)	(.012)	(.013)	(.053)	(.028)
N (rounded)	609,400	609,400	519,000	519,000	489,300	489,300	142,900	142,900

Notes: Dependent variable is log annual earnings. Regressions also include controls for year, education (except in columns 3/4 where no additional detail is available), dummies indicating race is African-American (in regressions for education groups), and indicating that ethnicity is Hispanic, main effects for birth cohort, and a quartic in age.

Table A5: Standard errors for Table 4 (Log earnings regressions, Women)

	Pooled coefficient estimates				Fixed-effect coefficient estimates			
Controls	**1**	**2**	**3**	**4**	**1**	**2**	**3**	**4**
Married	(.004)	(.004)	(.010)	(.012)	(.002)	(.002)	(.005)	(.006)
Married*Cohort 1946-55			(.012)	(.015)			(.006)	(.007)
Married*Cohort 1956-65			(.013)	(.016)			(.006)	(.008)
Married*Cohort 1966-75			(.016)	(.020)			(.009)	(.012)
Number of children <18		(.002)	(.004)	(.006)		(.001)	(.002)	(.003)
# children*Cohort 1946-55			(.005)	(.007)			(.002)	(.004)
# children*Cohort 1956-65			(.005)	(.008)			(.003)	(.004)
# children*Cohort 1966-75			(.007)	(.010)			(.004)	(.006)
Child <6 yrs old		(.004)	(.011)	(.023)		(.002)	(.006)	(.013)
Child <6*Cohort 1946-55			(.012)	(.026)			(.007)	(.015)
Child <6*Cohort 1956-65			(.013)	(.027)			(.007)	(.015)
Child <6*Cohort 1966-75			(.015)	(.030)			(.009)	(.018)
Married*# children				(.007)				(.003)
Married*# children*Cohort 1946-55				(.009)				(.004)
Married*# children*Cohort 1956-65				(.009)				(.005)
Married*# children*Cohort 1966-75				(.012)				(.007)
Married*Child <6				(.026)				(.015)
Married*Child <6*Cohort 1946-55				(.030)				(.017)
Married*Child <6*Cohort 1956-65				(.031)				(.017)
Married*Child <6*Cohort 1966-75				(.035)				(.021)

Table A6: Standard errors for Table A1 (Participation regressions, Women)

	Pooled coefficient estimates				Fixed-effect coefficient estimates			
Controls	**1**	**2**	**3**	**4**	**1**	**2**	**3**	**4**
Married	(.001)	(.001)	(.002)	(.003)	(.001)	(.001)	(.001)	(.002)
Married*Cohort 1946-55			(.003)	(.004)			(.002)	(.002)
Married*Cohort 1956-65			(.004)	(.005)			(.002)	(.002)
Married*Cohort 1966-75			(.005)	(.006)			(.002)	(.003)
Number of children <18		(.001)	(.001)	(.001)		(.000)	(.000)	(.001)
# children*Cohort 1946-55			(.001)	(.002)			(.001)	(.001)
# children*Cohort 1956-65			(.001)	(.002)			(.001)	(.001)
# children*Cohort 1966-75			(.002)	(.003)			(.001)	(.002)
Child <6 yrs old		(.001)	(.002)	(.004)		(.001)	(.001)	(.003)
Child <6*Cohort 1946-55			(.002)	(.005)			(.001)	(.004)
Child <6*Cohort 1956-65			(.003)	(.006)			(.002)	(.004)
Child <6*Cohort 1966-75			(.004)	(.008)			(.002)	(.005)
Married*# children				(.002)				(.001)
Married*# children*Cohort 1946-55				(.002)				(.001)
Married*# children*Cohort 1956-65				(.003)				(.001)
Married*# children*Cohort 1966-75				(.004)				(.002)
Married*Child <6				(.004)				(.003)
Married*Child <6*Cohort 1946-55				(.006)				(.004)
Married*Child <6*Cohort 1956-65				(.007)				(.004)
Married*Child <6*Cohort 1966-75				(.009)				(.005)

Notes: N=2,382,700 (to nearest 100). Dependent variable =1 if the individual has any SSA earnings in the current year, 0 otherwise. Regressions also include controls for year, education, dummies indicating if race is African-American, and indicating if ethnicity is Hispanic, main effects for birth cohort, and a quartic in age. Standard errors for each cell appear in appendix Table A6.

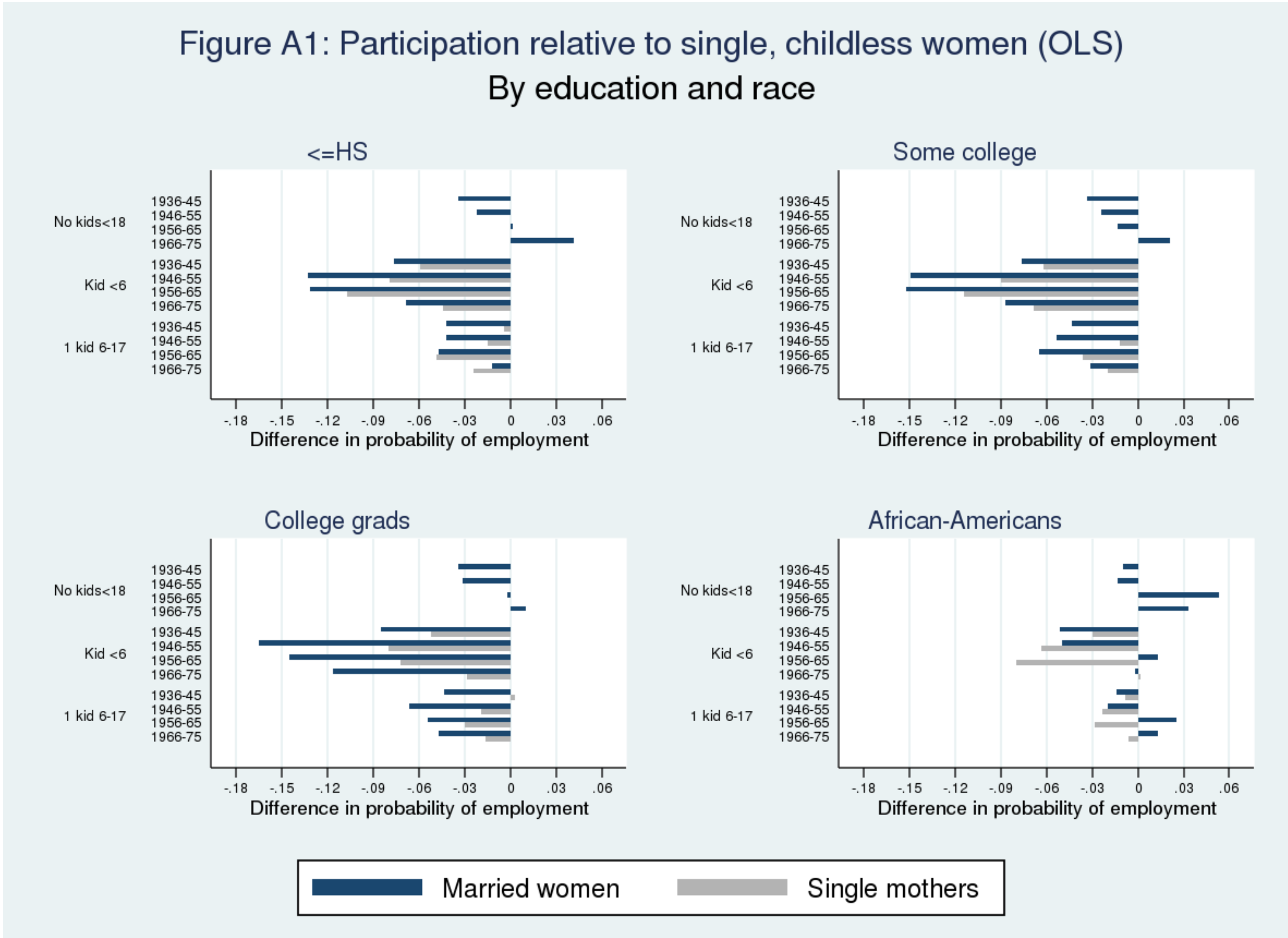
Table A7: Standard errors for Table A2 (Log earnings regressions for women, by education and race)

	<=High school		Some college		College grads		African-Americans	
Controls	**Pooled**	**FE**	**Pooled**	**FE**	**Pooled**	**FE**	**Pooled**	**FE**
Married	(.017)	(.009)	(.022)	(.011)	(.027)	(.013)	(.035)	(.018)
Married*1946-55 cohort	(.022)	(.011)	(.026)	(.013)	(.031)	(.015)	(.044)	(.022)
Married*1956-65 cohort	(.025)	(.013)	(.028)	(.014)	(.032)	(.016)	(.049)	(.025)
Married*1966-75 cohort	(.042)	(.024)	(.035)	(.020)	(.035)	(.019)	(.066)	(.041)
Number of children<18	(.008)	(.004)	(.011)	(.005)	(.016)	(.008)	(.014)	(.007)
# Children * 1946-55 cohort	(.010)	(.006)	(.013)	(.007)	(.020)	(.010)	(.018)	(.009)
# Children * 1956-65 cohort	(.012)	(.006)	(.014)	(.007)	(.021)	(.011)	(.018)	(.010)
# Children * 1966-75 cohort	(.016)	(.010)	(.017)	(.010)	(.025)	(.014)	(.023)	(.015)
Has child <6 yrs old	(.029)	(.018)	(.041)	(.024)	(.067)	(.035)	(.039)	(.024)
Child <6 * 1946-55 cohort	(.036)	(.022)	(.046)	(.027)	(.075)	(.039)	(.047)	(.028)
Child <6 * 1956-65 cohort	(.037)	(.022)	(.046)	(.027)	(.076)	(.041)	(.048)	(.028)
Child <6 * 1966-75 cohort	(.044)	(.027)	(.051)	(.029)	(.083)	(.047)	(.052)	(.032)
Married * Number of children<18	(.009)	(.005)	(.013)	(.006)	(.018)	(.008)	(.017)	(.008)
Married * # children * 1946-65 cohort	(.012)	(.006)	(.015)	(.007)	(.022)	(.010)	(.022)	(.011)
Married * # children * 1956-65 cohort	(.014)	(.007)	(.016)	(.008)	(.024)	(.011)	(.024)	(.012)
Married * # children * 1966-75 cohort	(.021)	(.012)	(.021)	(.011)	(.028)	(.015)	(.034)	(.019)
Married*child <6	(.033)	(.020)	(.047)	(.026)	(.074)	(.037)	(.049)	(.029)
Married * Child<6 * 1946-55 cohort	(.041)	(.025)	(.053)	(.030)	(.082)	(.042)	(.059)	(.035)
Married * Child<6 * 1956-65 cohort	(.043)	(.025)	(.053)	(.030)	(.083)	(.044)	(.061)	(.036)
Married * Child<6 * 1966-75 cohort	(.054)	(.033)	(.059)	(.034)	(.091)	(.050)	(.070)	(.045)
N (rounded)	670,300	670,300	584,000	584,000	442,400	442,400	210,900	210,900

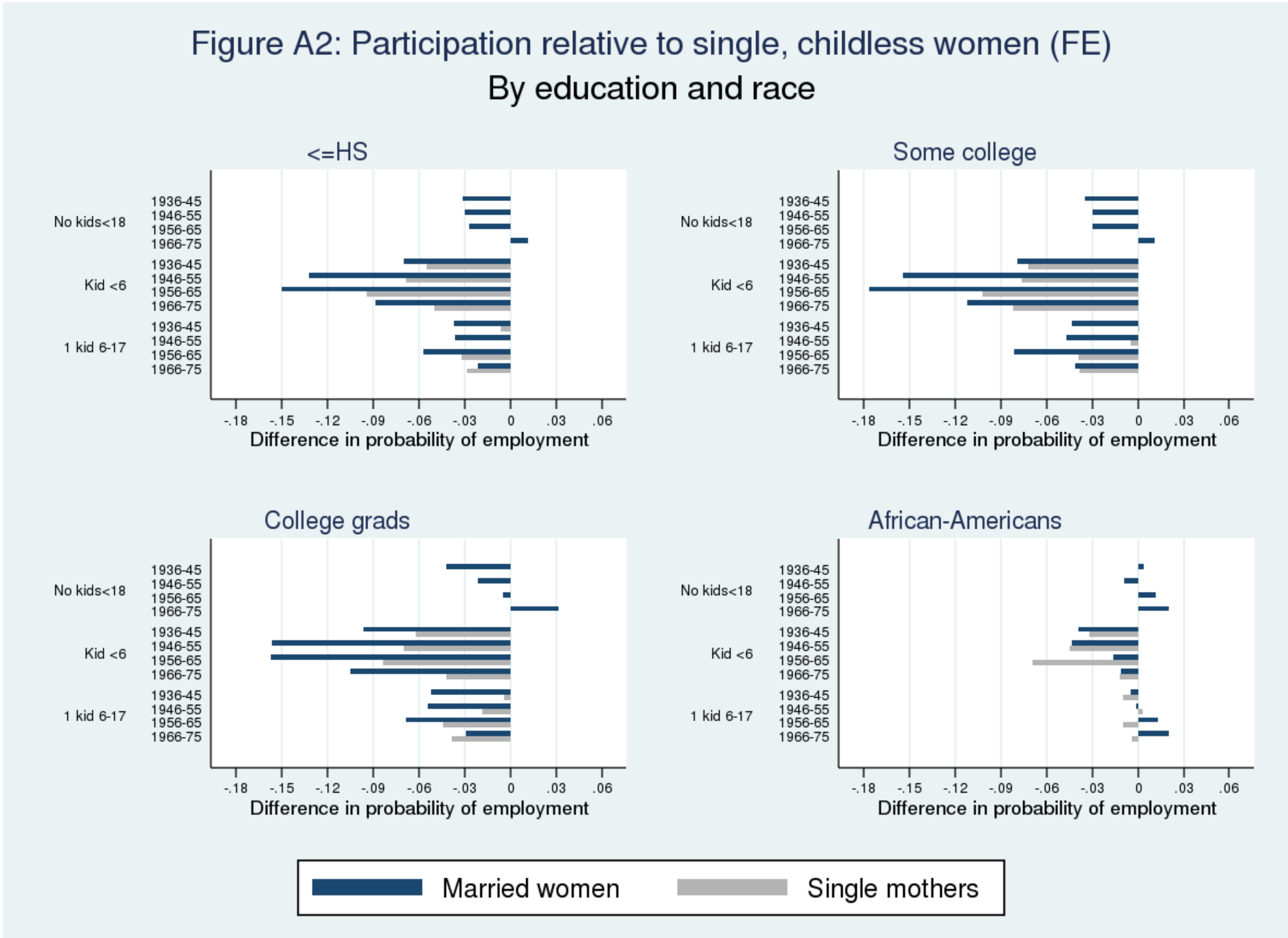
Notes: Dependent variable is log annual earnings from SSA records. Regressions also include controls for year, education (except in columns 3/4 where no additional detail is available), dummies indicating race is African-American (in regressions for education groups), and indicating that ethnicity is Hispanic, main effects for birth cohort, and a quartic in age. Standard errors for each cell appear in appendix Table A7.

Table A8: Standard errors for Table A3 (Participation regressions for women, by education and race)								
	<=High school		**Some college**		**College grads**		**African-Americans**	
Controls	**Pooled**	**FE**	**Pooled**	**FE**	**Pooled**	**FE**	**Pooled**	**FE**
Married	(.004)	(.002)	(.006)	(.003)	(.006)	(.003)	(.009)	(.005)
Married*1946-55 cohort	(.006)	(.003)	(.007)	(.004)	(.007)	(.004)	(.013)	(.006)
Married*1956-65 cohort	(.008)	(.003)	(.008)	(.004)	(.008)	(.004)	(.015)	(.007)
Married*1966-75 cohort	(.015)	(.006)	(.010)	(.006)	(.009)	(.005)	(.024)	(.011)
Number of children<18	(.002)	(.001)	(.002)	(.002)	(.003)	(.002)	(.003)	(.002)
# Children * 1946-55 cohort	(.002)	(.001)	(.003)	(.002)	(.004)	(.003)	(.004)	(.002)
# Children * 1956-65 cohort	(.003)	(.002)	(.004)	(.002)	(.005)	(.003)	(.005)	(.002)
# Children * 1966-75 cohort	(.005)	(.002)	(.004)	(.003)	(.006)	(.004)	(.007)	(.004)
Has child <6 yrs old	(.005)	(.004)	(.007)	(.006)	(.011)	(.009)	(.007)	(.006)
Child <6 * 1946-55 cohort	(.007)	(.005)	(.009)	(.007)	(.015)	(.010)	(.010)	(.007)
Child <6 * 1956-65 cohort	(.008)	(.005)	(.010)	(.007)	(.016)	(.010)	(.011)	(.007)
Child <6 * 1966-75 cohort	(.012)	(.007)	(.011)	(.008)	(.018)	(.012)	(.013)	(.008)
Married * Number of children<18	(.002)	(.001)	(.003)	(.002)	(.004)	(.002)	(.004)	(.002)
Married * # children * 1946-65 cohort	(.003)	(.002)	(.004)	(.002)	(.005)	(.003)	(.006)	(.003)
Married * # children * 1956-65 cohort	(.004)	(.002)	(.005)	(.002)	(.006)	(.003)	(.007)	(.003)
Married * # children * 1966-75 cohort	(.007)	(.003)	(.006)	(.003)	(.008)	(.004)	(.011)	(.005)
Married*child <6	(.006)	(.005)	(.008)	(.007)	(.012)	(.009)	(.009)	(.007)
Married * Child<6 * 1946-55 cohort	(.008)	(.006)	(.011)	(.008)	(.016)	(.010)	(.013)	(.009)
Married * Child<6 * 1956-65 cohort	(.010)	(.006)	(.011)	(.008)	(.017)	(.011)	(.015)	(.009)
Married * Child<6 * 1966-75 cohort	(.015)	(.008)	(.014)	(.009)	(.020)	(.013)	(.020)	(.012)

Notes: Dependent variable is =1 if the individual has any SSA earnings in the current year, 0 otherwise. Regressions also include controls for year, education (except in columns 3/4 where no additional detail is available), dummies indicating race is African-American (in regressions for education groups), and indicating that ethnicity is Hispanic, main effects for birth cohort, and a quartic in age. Standard errors for each cell appear in appendix Table A8.



Note: The estimates are based on the fully interacted model reported in Table A3 (OLS). Each bar corresponds to the participation differential for the specified group relative to single, childless women in the same cohort. We report implied differentials for two child scenarios—having one school aged child, and having one pre-school aged child.



Note: The estimates are based on the fully interacted model reported in Table A3 (FE). Each bar corresponds to the participation differential for the specified group relative to single, childless women in the same cohort. We report implied differentials for two child scenarios—having one school aged child, and having one pre-school aged child.