

# **LABOR MARKET INEQUALITY AND THE CHANGING LIFE CYCLE PROFILE OF MALE AND FEMALE WAGES\***

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**Abstract:** We estimate the distribution of life cycle wages for cohorts of prime-age men and women in the US. A quantile selection model is used to consistently recover the full distribution of wages accounting for systematic differences in employment, permitting us to construct gender- and education-specific age-wage profiles, as well as measures of life cycle gender wage gaps. Although common within-group time effects are shown to be a key driver of labor-market inequalities across gender, important additional differences by birth cohort emerge with more recent cohorts of women delaying child rearing, and by implication the onset of child penalties in wages. These important cross-cohort differences help account for the stalling of progress in gender wage gaps over the past quarter century.

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Women’s relative economic progress in the second half of the 20<sup>th</sup> Century United States was no less than “revolutionary” (Goldin 2006). This is exemplified in Figure 1 that shows gender wage gaps were cut in half across the distribution from the mid 1970s to the mid 1990s.<sup>1</sup> However, in a sharp reversal, progress stalled in the subsequent quarter century, especially in the top half of the wage distribution. These time-series patterns hold among full-time workers, and by education group (see Appendix Figure A1). Most of the extant literature on gender wage gaps has focused on levels and determinants in the cross-section over time (Blau and Beller 1988; Blau and Kahn 1997; Olivetti and Petrongolo 2008, 2016; Mulligan and Rubinstein 2008; Klevin et al. 2019; Maasoumi and Wang 2019; Fernández-Val et al. 2023; Blau et al. 2023), even though the sources and patterns of lifetime gender inequality may differ considerably both within and between birth cohorts. The latter includes life cycle and cohort changes in selection into employment, in education attainment, in fertility and family formation, in promotion opportunities, and exposure to macroeconomic shocks, among others (Deaton and Paxson 1994; Blundell et al. 2007; Bertrand 2011; Huggett et al. 2011; Goldin 2014; Goldin and Mitchell 2016; Juhn and McCue 2017; Klevin et al. 2019; Borella et al. 2020; Sloane et al. 2021).

[Figure 1 here]

In this paper we provide new estimates of the life cycle gender wage gap across birth cohorts, education group, and the wage distribution in the presence of nonrandom selection into work. We develop a quantile model for wages that combines the long literature on estimating age, cohort, and time effects (Weiss and Lillard 1978; Welch 1979; Berger 1985; Heckman and Robb 1985; Deaton and Paxson 1994; MaCurdy and Mroz 1995; Beaudry and Green 2000; Gosling et al. 2000; Card and Lemieux 2001; Fitzenberger and Wunderlich 2002; Kambourov

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<sup>1</sup> The sample is workers aged 25-55 in the Current Population Survey Annual Social and Economic Supplement. See Appendix A for details.

and Manovskii 2009; Kong et al. 2018; Lagakos et al. 2018) with the equally long literature of estimating wages in the presence of nonrandom sample selection (Heckman 1979; Lee 1982; Buchinsky 1998; Neal 2004; Blundell et al. 2007; Olivetti and Petrongolo 2008; Mulligan and Rubinstein 2008; Bollinger, et al. 2011; Arellano and Bonhomme 2017; Bayer and Charles 2018; Maasoumi and Wang 2019; Ashworth et al. 2021; Fernández-Val et al. 2023; Blau et al. 2023). Some in the cohort literature have estimated quantiles, and some in the quantile literature have estimated models with nonrandom selection. We bring these literatures together in a unified framework to study wage gaps over the working life of cohorts of men and women.

A well-known identification problem in models with age, birth cohort, and period effects is that any period can be written as the sum of age and cohort, and thus restrictions on functional form are necessary (Heckman and Robb 1985). We expand upon the cohort wage specification of MaCurdy and Mroz (1995) and Fitzenberger and Wunderlich (2002) that includes a highly nonlinear parameterization of age, cohort, and time to flexibly capture changes in wages over the life cycle as well as macroeconomic trends, while controlling for gender and education-group time shocks. We also relax separability in age and time effects—meaning we do not assume age-wage profiles are parallel across cohorts (Mincer 1974)—but the specification embeds a direct test of whether life cycle age-wage profiles are parallel.

A similarly challenging identification issue is found in models with nonrandom selection that requires separating the extensive margin of employment from the intensive margin of wages. The issues include whether to model selection based on observables or unobservables, whether to identify the selection rule with exclusion restrictions, or whether to impose monotonicity in the selection rule such as positive selection (Vella 1998). While the gender gap literature has long been concerned about selection of women on unobservables into work (Blau and Kahn 2017),

with some recent exceptions (e.g. Maasoumi and Wang 2019; Fernández-Val et al. 2023; Blau et al. 2023), these issues were often eschewed for men on the assumption that their high labor force attachment rendered selection exogenous (and thus implicitly imposed a so-called identification at infinity assumption, c.f. Chamberlain 1986; Heckman 1990). However, there has been a retreat from work among men, especially among those at lower education levels (Blundell et al. 2018; Abraham and Kearney 2020; Aguiar et al. 2021), suggesting that the assumption of exogenous selection of men on unobservables may no longer be tenable.

We estimate the cohort wage model using the recently developed quantile with selection estimator proposed by Arellano and Bonhomme (2017). This estimator extends the canonical conditional mean selection on unobservables model from Heckman (1979) to the full distribution of wages, generalizing earlier efforts (Buchinsky 1998). Like the Heckman model, the power of the Arellano and Bonhomme estimator is strengthened by exclusion restrictions to identify the extensive margin of employment from the intensive margin of wages. Recent gender wage gap papers have used the presence and age composition of children to identify the selection rule (Mulligan and Rubinstein 2008; Maasoumi and Wang 2019; Fernández-Val et al. 2023). Instead of using potentially correlated family structure variables like the age composition of children to identify the selection rule, our approach to identification of the selection process is to exploit the dramatic changes in the tax and transfer system since the 1970s to create simulated disposable income instruments—one capturing disposable income if one or both partners (in married couples) are out of work, and a second a weighted sum of disposable income across 8 categories of no work, part-time work, and full-time work for the partners—as well as changes in the tightness of local labor markets as proxied by state unemployment rates. The use of tax and transfer policy reforms to construct simulated instruments is well established, and has been used

to study such diverse topics as the effect of health insurance on birth outcomes (Currie and Gruber 2006), the effect tax credits on labor supply (Meyer and Rosenbaum 2001; Blundell et al. 2016; Hoynes and Patel 2018), the effect of marginal tax rates on taxable income (Gruber and Saez 2002; Weber 2014; Burns and Ziliak 2017), and the effect of the safety net on food insecurity (Schmidt, Shore-Sheppard 2016), among many others. To our knowledge we are the first to use this approach in the gender wage gap literature.

Using a sample of prime-aged men and women from the Annual Social and Economic Supplement of the Current Population Survey for calendar years 1976-2018, the model estimates show striking convergence in gender wage gaps across cohorts at a given age, especially for those born before the 1960s, which helps account for the time-series decline before the mid 1990s depicted in Figure 1. At the same time, we find sharply increasing life cycle gender wage gaps until around age 45, across both the wage distribution and education level. Whether these life cycle gaps fall at older ages varies by cohort, education level, and location in the wage distribution. The gender wage gaps tend to be more quadratic in age among workers without a college degree, but there is little life cycle convergence among college educated women born after 1960 in the upper half of the wage distribution, and indeed some divergence among high-educated, high-wage Millennials. This helps account for the observed stagnation of gender wage gaps in the time series over the last quarter century. These cohort and life cycle patterns are robust to alternative approaches to specification of both the selection and wage equations.

We explore three possible mechanisms for the estimated cohort patterns of life cycle gender age gaps, including the role of gender and education group-specific shocks common to cohorts, changes in the life cycle timing of child rearing across cohorts, and the rise of full-time work among women. These time shocks could include technological change affecting the returns

to skill (Bound and Johnson 1992; Katz and Murphy 1992; Juhn, Murphy, and Pierce 1993; Card and DiNardo 2002), legislative and judicial changes affecting access to employment (Goldin and Katz 2002; Berger and Waldfogel 2004; Bailey 2006), and shifting cultural norms and expectations (Bertrand 2011). Many of these developments, including the rise of full-time work among women, would be expected to lead to reductions in gender gaps. At the same time, delays in fertility could result in postponement in the onset of large child penalties in wages (Kleven et al. 2019; Cortes and Pan 2023), and thus exacerbating gender gaps at older ages.

Our estimates indicate that common group-specific time effects eliminate much of the cross-cohort differences in gender wage gaps, both in levels and life cycle profiles, suggesting that common shocks differentially favored women over men. These time effects were more neutral across gender starting with the 1960s birth cohort. However, just as the cohorts of women less likely to differentially benefit from common shocks were entering the labor force, they also started to delay fertility, with peak age of raising a child increasing by at least 5 years between the 1940s and 1960s birth cohorts. This suggests that delayed child rearing likely has resulted in the child wage penalties kicking in later in the life cycle, which helps account for the stalling of the gender gap in recent cohorts. Importantly, our approach allows children to have a direct impact on wages, as well as an impact through selection in to work. Both avenues are important. Indeed, excluding the number and age distribution of children from the wage equation has a systematic impact on our estimates of selection, suggesting that positive selection for college educated women appears to be largely driven by children, most likely through the impact on past human capital investments.

The paper proceeds as follows. In the next section we present stylized facts on the evolution of employment and wages across life cycle cohorts, highlighting the potential concerns

of nonrandom selection in work. Section III then develops our life cycle model of wage determination, including a discussion of identification and estimation of distributional wage profiles. Section IV presents the results of wage profiles of men and women in the presence of nonrandom selection. The fifth section then discusses the implications of the distributional profiles for gender wage gaps across the life course. The final section concludes.

## **II. Trends in Employment and Wages of Cohorts**

We begin by presenting stylized facts on life cycle employment and hourly wages across cohorts. The aim is to highlight the changing selection into the labor force among men and women across cohorts. As described in Appendix A, the data come from repeated cross sections of the Annual Social and Economic Supplement (ASEC) of the Current Population Survey (CPS) spanning the 1976 to 2018 calendar years. The sample consists of men and women born between the years 1921 and 1993 who are ages 25 to 55, capturing the prime working years for most after formal schooling is completed and prior to retirement decisions. Cohorts are defined as single birth years, but to ease presentation we take averages within decades at each age for the figures. We include men and women of all education levels, but we group them into those with at least four years of college (“college or more”) and those with three or fewer years of college (“some college or less”).<sup>2</sup> Employment is defined as any paid work in the calendar year, and hourly wages are defined as the ratio of annual earnings to annual hours, deflated to 2010 base year with the Personal Consumption Expenditure Deflator. Additional details are in Appendix A.

[Figure II here]

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<sup>2</sup> Because of secular trends in education attainment, Bailey, Guldi, and Hershbein (2014) split by quartiles of the education attainment distribution. In Appendix E and discussed in the robustness section below, we split the sample into the top quartile for the high education group and pool the bottom three quartiles for the lower education group. Because the top quartile coincides with the college and more group for over half the sample period, this has no substantive effects on the estimated parameters and gaps.

Figure II depicts employment rates for men and women by decadal birth cohort and education level. The figure shows that employment among men with less than a college degree has fallen over time at any given age starting with the 1950s cohort, and that employment rates progressively peak earlier in the working life. For example, employment among non-college men in the 1930s cohort peaked just before age 40, but that peak occurred a decade earlier among those born in the 1970s.<sup>3</sup> For men with at least a college degree, employment remains high over much of the working life, though it has fallen at each age and there is some evidence that it peaks at younger ages starting with the 1960s cohort.

The employment patterns across cohorts of women are striking. For example, employment rates among non-college women born in the 1940s were stable at around 65 percent from ages 25-35, before accelerating and taking on the familiar hump-shaped life cycle profile. Women born in the 1950s began the upward climb five years younger and thus sustained higher employment rates across more of their working life. Women born in the 1960s and 1970s had even higher employment rates at young ages, but in an important departure from earlier cohorts, employment rates trended downward over the entire working life. Perhaps most striking, employment rates among non-college women under age 35 born in the 1980s and 1990s have fully reverted to levels last observed with the 1940s cohort.

Employment rates of college educated women born in the 1940s were U-shaped between the ages of 25 and 50, declining for the first decade, then increasing for the next 15 years, before turning down after age 50 (the 1930s cohort has a similar pattern over ages 35-55). This U-shape was replaced with more of an L-shaped profile for cohorts born in the 1950s and 60s, but starting with the 1970s most of this life cycle curvature was eliminated and with employment rates lower

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<sup>3</sup> The sharp decline in employment of the 1990s cohort of less-skilled men reflects poor labor-market opportunities for those entering work during the Great Recession.



after age 30. Appendix Figure A2 shows that among those employed, the aggregate share working full time (defined as working at least 35 hours per week for 50 weeks) has increased over time; that is, aggregate employment declines for men and women are coming from marginally attached workers. Appendix Figure A3 decomposes these trends into decadal cohorts, revealing striking changes across cohorts in the share of workers employed full time. This is especially notable for women where the share of workers at full time is increasing across the working life, and is higher at each age among younger cohorts. This is also true for non-college men starting with the 1960s cohort.

[Figures III and IV here]

Figures III and IV present the corresponding age profiles of log real wages for men and women, respectively, at the 10<sup>th</sup>, 50<sup>th</sup>, and 90<sup>th</sup> percentiles by education and cohort. Non-college men born in the 1940s had notably higher median log wages than more recent cohorts until at least age 40, suggesting cumulative lifetime wages have declined for less-skilled men among younger cohorts. The exact opposite occurs among college educated men, where younger cohorts have substantially higher hourly wages at all percentiles and most ages across the life cycle. This is consistent with the rising return to skill underlying the secular rise in wage inequality in the cross section (Lemieux 2006; Autor, Katz, and Kearney 2008). This secular rise for men with college or more education is particularly strong for the higher quantiles. Importantly, the profile of college men is steeper at younger ages among more recent cohorts, suggesting greater lifetime inequality across education groups, and while flattening out around age 45, wages of higher-educated men do not turn down at older ages. Appendix Figure A4 shows a similar pattern obtains when restricting the sample to full-time workers.

The wage profile of women in Figure IV reveals substantial fanning out across cohorts from the 1920s to the 1960s over the distribution, but then little cohort differences thereafter. Wages increase linearly with age, especially with the 1940s and 1950s cohorts, but then after rapid growth up to age 35 in the 1960s cohorts and beyond, there is a noticeable slowdown (curvature) in life cycle wage growth in more recent cohorts of women, especially those with at least college and in the upper half of the wage distribution. As with men, these patterns persist when restricting the sample to full-time working women in Appendix Figure A5.

These stylized facts point to the potential importance of three key features that guide our choice of specification for modeling wage profiles underlying life cycle gender wage gaps. First, the employment patterns suggest that differential selection into work of both women and men could affect the labor-market fortunes of birth cohorts. Second, the descriptive wage profiles uncover important differences in the pattern of cohort age profiles across quantiles. Third, changes in profiles across cohorts point to large secular changes in wages that differ by gender and education group. This is exemplified in Appendix Figure A6 which depicts the gender gap in log wages for all workers across the working life and cohorts (Appendix Figure A7 restricted to full-time workers). There we see very large gender wage gaps in older cohorts of workers that declined with each successive birth cohort from the 1920s to the 1950s, which helps account for the trend decline from the mid 1970s to mid 1990s depicted in Figure 1. Starting in the 1960s there is not only no additional gains in younger cohorts at any given age, but unlike older cohorts of women who narrowed the wage gap after age 40, the gender wage gap continues to increase across the working life.

In the next section, we develop an empirical framework of wages across the distribution that accounts for these key features. Because of potential differences between those who choose

part-time and full-time work, we estimate our empirical models separately for selection into any work and into full-time work.

### III. Quantile Model of Cohort Wages in the Presence of Selection

We are interested in how the natural log of hourly wages  $\ln w$  vary over time  $t$  and working ages  $a$  across different birth cohorts  $c$ . Holding cohort constant, growth in wages can be a result of both time and aging. On the other hand, holding age constant, wages differ both because of cohort effects and time effects. This results in a well-known identification problem because any given time period is comprised of individuals from different cohorts at different ages, i.e.,  $t = c + a$ , and thus it is necessary to impose restrictions in order to separately identify age from cohort from time (Heckman and Robb 1985). Notably, in the event that growth in wages over the life cycle is independent of time, then it is possible to identify the pure age-wage profile, implying that wages are parallel across cohorts (Mincer 1974). This suggests that we want to adopt a wage specification that has lots of flexibility, but also nests the pure life cycle model. This is exactly the approach of MaCurdy and Mroz (1995) and Fitzenberger and Wunderlich (2002), who used different parametric functional forms in age, cohort, and time to identify the separate factors. At the same time, we are interested not just in mean wages, but wages across the distribution and how that distribution changes when workers select nonrandomly into the labor force. This leads us to a framework that extends the standard cohort models by incorporating nonrandom selection into work across the wage distribution as proposed in Arellano and Bonhomme (2017). A more complete description of the model, estimation, and identification is found in online Appendix B. Here we sketch out the key details.

Specifically, let the natural log of the latent log wage ( $\ln w^*$ ) of an individual of gender  $j$  with schooling level  $s$  be given as

$$(1) \quad \ln w_j^{s*} = X_j^s(a, c, t; l)' \beta_j^s(U_j^s),$$

where  $X$  is a flexible function in age, cohort, time, and demographics  $l$ , all terms found in the prototypical Mincer wage equation.  $\beta$  is a vector of unknown parameters that depend on unobserved heterogeneity  $U$  that varies across gender  $j$  and education group  $s$ . The unobserved heterogeneity is assumed to be independent of  $X$  and distributed uniformly on the  $(0,1)$  interval reflecting the rank of the individual in the distribution of latent wages conditional on covariates  $X$  for gender  $j$  of schooling level  $s$ .

Wages are observed,  $\ln w_j^s$ , if the individual of gender  $j$  and education level  $s$  participates in the labor market according to the participation decision

$$(2) \quad E_j^s = \mathbf{1} \left\{ V_j^s \leq p_j^s \left( D_j^s(a, c, t, l; z) \right) \right\},$$

where the indicator variable takes a value of 1 if the rank of the uniformly distributed unobserved heterogeneity  $V$  is less than the propensity score  $p(D)$ , with index  $D$  that is a flexible function of age, cohort, time, and demographics, as well as additional identifying excluded covariates of the decision to work  $z$  beyond the variables in  $l$  from the wage equation (1). As discussed in detail below, the unobservables in the log wage equation are assumed to be independent of these excluded ‘instruments’ conditional on the flexible function of the age, cohort, time, and demographic variables included in the regression. That is, observed log wages are the product of latent wages and the employment indicator, i.e.  $\ln w_j^s = E_j^s * \ln w_j^{s*}$ . The propensity score is assumed to be independent of  $V$ .

#### A. *Specification of Wages*

To parameterize the model of observed wages, we expand upon the specification of Fitzenberger and Wunderlich (2002) where different polynomial orders are selected in order to permit separate identification of age, cohort, and time effects. Each individual is allocated to a

birth cohort  $c$  based on the calendar year  $t$ , normalized both with respect to the first year of the sample (1976), and on their age normalized at labor-market entry age 25; namely,  $c = t - e$ , where  $t = (year - 1976)/10$  and  $e$  is the entry age defined as  $e = (age - 25)/10$ . This means that cohort 0 consists of those individuals whose age is 25 in 1976, persons older than age 25 in 1976 are assigned negative cohort values, and those that reach age 25 after 1976 are assigned positive cohort values.<sup>4</sup>

The corresponding wage equation for gender  $j$  and schooling level  $s$  is given as

$$(3) \quad \ln w_j^s = \beta_{0j}^s(U_j^s) + \sum_{f=1}^3 \beta_{a,fj}^s(U_j^s)e_j^f + \sum_{g=1}^5 \beta_{t,gj}^s(U_j^s)t^g + \sum_{h=1}^3 \beta_{c,hj}^s(U_j^s)((1 - \theta)c_j^h + \theta c_j^{h-1}) + \sum_{m=1}^4 \beta_{R,mj}^s(U_j^s)R_j^m + l_j^s \beta_{l,j}^s(U_j^s) + \delta_j^s(U_j^s) + \eta_j^s(U_j^s),$$

which includes a cubic in labor-market entry age, a quintic in time, a cubic in cohort, a quartic in interactions between entry age and time, a vector of sociodemographic controls, a normalized set of time fixed effects ( $\delta_j^s(U)$ ), and a set of state fixed effects ( $\eta_j^s(U)$ ). The cubic in entry age provides curvature for capturing pure life cycle age effects, assuming strong separability with time, i.e. that  $\beta_{R,mj}^s = 0$ . The quintic in time is a flexible parameterization for capturing macroeconomic trends in wages, while the normalized time dummies control for common shocks affecting all cohorts the same, but differently across gender and education.<sup>5</sup> The state fixed effects control for permanent differences in state labor markets that may have differential effects across gender and education. We set the parameter  $\theta=1$  for cohorts entering in 1976 and later, and zero otherwise, and thus a cubic in cohort is admitted for the pre-1976 labor-market entrants

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<sup>4</sup> The literature on labor-market scarring (e.g. Kahn 2010; Altonji, Kahn, and Speer 2016; Rothstein 2020) tends to focus on college-educated workers and to drop those older cohorts with negative cohort values. This implies that they follow entry cohorts (see also Kambourov and Manovskii 2009). Admitting older cohorts and the less educated has the advantage of larger samples and a longer look at cohort changes over the life cycle, and is more akin to the earlier research on cohort earnings (Welch 1979; Berger 1985; Gosling, Machin, and Meghir 2000).

<sup>5</sup> With a fifth-order polynomial in time and a constant term, the minimum number of time dummies that must be omitted is 6. However, with the linear age effect, and age and time interactions, we had to omit 8 time effects, four at the beginning of the sample period, and four at the end.

and a quadratic in cohort for the 1976 and later cohorts. The identifying assumption for pinning down the cohort effects is to normalize around the linear cohort term and set  $\beta_{c,0j}^s = 0$ .

Nonseparability between entry age and time is admitted into the model by including four entry age-time interaction terms in  $R_j^m$  that are found by integrating over entry age— $et, et^2, e^2t, e^2t^2$  (see Appendix B for details). Again, using the relation that  $t = c + e$ , plugging this into the interactions prior to integrating, and solving yields the four regressors of  $(\frac{ce^2}{2} + \frac{e^3}{3})$ ,  $(\frac{c^2e^2}{2} + \frac{2ce^3}{3} + \frac{e^4}{4})$ ,  $(\frac{ce^3}{3} + \frac{e^4}{4})$ , and  $(\frac{c^2e^3}{3} + \frac{2ce^4}{4} + \frac{e^5}{5})$ .<sup>6</sup> If these interaction terms are found to be jointly zero, then it is possible to interpret the coefficients on the cubic in entry age as a pure life cycle aging effect, and that wage-age profiles across cohorts are parallel. However, rejecting the null hypothesis implies that cohort age profiles are not parallel and thus the entry age coefficients are a convolution of age and trend effects, yielding what we refer to as *pseudo* life cycle age-wage profiles. This is a key test in our empirical results.

Beyond the age, cohort, time (trends and common shocks), and state controls, the employment and wage equations within each gender-education group include indicators for race (white is omitted), Hispanic ethnicity, whether married, and whether reside in a metropolitan area, as well as the numbers of children ages 0-5 and 6-18. We discuss identification of the employment equation from the selection equation in the next section. In Appendix E we include a comprehensive set of robustness tests on the specification of the wage and selection equations.

### *B. Estimation and Identification*

A number of approaches have been adopted in the literature to address selection into employment. In studying the median gender wage gap, Neal (2004), Olivetti and Petrongolo

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<sup>6</sup> The constants of integration are set equal to 0.

(2008), and Blau et al. (2023) filled in missing wages of nonworkers by using actual wages from adjacent periods, and predicted wages when actual wages were not available. This is attractive in that it only requires assumptions on the position of imputed wages vis-à-vis the median and not the level. However, it relies on selection on the observed wages in the panel and is not consistent in the presence of nonrandom selection on unobservables. Blundell et al. (2007) adopt a nonparametric approach and calculate bounds on the distribution of wages that relax the reliance on exclusion restrictions but strengthen the restrictions underlying worse case bounds. The advantage is greater flexibility in the selection rule, though at the cost of losing point identification. Neal and Johnson (1996) and Bayer and Charles (2018) impose monotonicity and a median selection rule in examining black-white wage gaps. The assumption is that nonworkers, were they to work, would be drawn from the bottom half of the distribution, and thus it remains possible to recover the median and upper quantiles of the wage distribution. The cost is wages in the bottom half of the distribution are not identified, and not likely a credible assumption for high-skilled women who have periods of nonwork during child-rearing years.

As detailed in Appendix B, our approach to consistently estimate the quantile model in the presence of nonrandom selection is to point identify the parameters of the wage and selection process using the three-step method of Arelleno and Bonhomme (2017). Assuming that the joint distribution of  $(U_j^s, V_j^s)$  in equations (1) and (2) follows a bivariate Gaussian copula with dependence parameter  $\rho_j^s$  that is independent of the propensity score index  $D_j^s$ , the first step involves estimating the probability of employment (or probability of full-time work when examining wages of full-time workers) in equation (2) via probit maximum likelihood, much like in a standard Heckman-selection model. Given estimates of the selection model parameters, the second step involves estimating the copula dependence parameter  $\rho_j^s$  via generalized method of

moments using functions of the fitted propensity score index  $\widehat{D}_j^s$  from the first-stage probit estimates as “instruments”. The copula parameter captures the correlation between the unobserved heterogeneity in the wage ( $U$ ) and participation ( $V$ ) equations. If this correlation is negative, then selection on unobservables into work is positive, i.e. those with higher wages are more likely to work, and likewise if the correlation is positive then selection into work is negative. We use the Frank copula because it is comprehensive in its dependence structure allowing for both negative and positive selection, as well as independence. The third step involves estimating the parameters from the wage equation (3) at selected quantiles, using rotated quantile regression, where the rotation is a function of the degree of selection and is person-specific as determined by the estimated propensity score index in each gender-education group,  $\widehat{D}_j^s$ , conditional on the estimated dependence parameter  $\widehat{\rho}_j^s$ . Note that the flexible parametric specifications for the quantile wage equation and the propensity score reduce the restrictiveness of the probit assumption in the first stage. In order to retain the dependence structure of the model, we conduct inference via the bootstrap across all three stages of estimation using the full sample of observations. Our sample sizes for the four groups of men and women range from over 300,000 to just under 900,000, and because we have 110 parameters to estimate, we set the number of bootstraps at 100.<sup>7</sup>

A common approach in the literature is to use the ages of children as exclusion restrictions to identify the system in equations (1)-(3) under the assumption that children affect the decision to work, but not the wage conditional on working (Mulligan and Rubinstein 2008; Maasoumi and Wang 2019; Fernandez-Val et al. 2023; Blau et al. 2023). This is consistent with

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<sup>7</sup> Estimation and bootstrap inference is conducted in Matlab, modifying the programs made available with the published version of Arellano and Bonhomme (2017). The bootstraps were conducted on the University of Kentucky supercomputing cluster.



a simple formulation of the wage determination process for spot-market hourly wages. However, children may affect accumulated labor-market experience and the timing of promotion opportunities, which could have a direct effect on the wage rate. Thus, we include the age composition of children in both the selection and wage equation, though in Appendix E we present estimates of the gender wage gap under this typical identification strategy.

Instead, our approach to identification of the selection process is to exploit changes in the tax and transfer system to create simulated disposable income instruments—one capturing disposable income if one or both partners (in married couples) are out of work, and a second a weighted sum of disposable income across 8 categories of no work, part-time work, and full-time work for the partners—as well as changes in the tightness of local labor markets as proxied by state unemployment rates. Over the span of our sample period there were numerous changes to the U.S. tax and transfer system. On the tax side, major federal legislation was passed in 1981, 1986, 1990, 1993, 1997, 2001, and 2017. The 1980s reforms included reductions in the number of marginal tax brackets from 16 to 4, along with reductions in the top marginal tax rate from 70 percent to 50 percent. Subsequent changes in the 1990s increased the number of brackets to 7 and top marginal rates to 39 percent, with incremental changes in rates (both up and down) in the 2000s. These reforms also included substantial expansions of the refundable Earned Income Tax Credit (EITC) for low-wage workers in 1986 and 1993, and the introduction of a partially refundable Child Tax Credit (CTC) in 1997 followed with substantial expansions in 2001 and 2017. On the welfare side, federal provision of cash assistance was fundamentally altered with the 1996 Welfare Reform Act that created the Temporary Assistance for Needy Families (TANF) program. Among other changes, this reform also had significant implications for the eligibility of food assistance from the near-cash Food Stamp Program, later renamed the

Supplemental Nutrition Assistance Program (SNAP) in 2008. See Auerbach and Slemrod (1997) and Piketty and Saez (2007) for references on the tax changes, and Grogger and Karoly (2005) and Moffitt and Ziliak (2019) for summaries of changes to the transfer system.

Appendix B contains extensive details on the construction of the simulated disposable income instruments, as well as evidence on the variation and support to identify the employment equation. Some of these tax and transfer changes were adopted at the federal level, and some at the state level, and our simulated instruments aim to capture variation at both levels. The key features are that the instrument for no-work captures gender-education specific variation across states and year in rent, interest, and dividend income as well as the generosity of welfare payments from SNAP and TANF, while the simulated instrument for in-work income captures changes in federal and state taxation of labor and nonlabor income, including the refundable EITC and partially refundable CTC. We then identify the selection equation from the wage equation by including in the selection model the two simulated disposable income instruments and the state unemployment rate described above. The unobservables in the log wage equation are assumed to be independent of these excluded ‘instruments’ conditional on the flexible function of the age, cohort, time, and demographic variables included in the regression, along with the year and state fixed effects. That is, identification of wages is based on the independence of  $U_j^s$  and  $z_j^s$  conditional on  $a, c, t, l, \delta, \eta$ . This means that the selection model is identified via the residual variation in potential disposable income derived from the interaction of federal-state-time policy changes in taxes and transfers and the wage and nonwage incomes across states and demographic groups. In section V we consider a number of robustness checks of this specification including a specification of the baseline model to include state-specific linear time trends.

Appendix Figures B1 and B2 show box and whisker plots of the two simulated income instruments for select years. Figure B1 shows a real decline in the out-of-work instrument from 1976 to 1990, reflecting real declines in maximum benefit guarantees in TANF noted by others (see Ziliak 2016), and then relative stability thereafter. Real median incomes hover around \$10,000 in a typical year with an interquartile range of about \$5,000. Appendix Figure B2 depicts much more variation in the weighted simulated income instrument across education group, reflecting the differences in both average wages and private nonlabor incomes, as well as tax liabilities. Again we see a decline in real simulated median incomes among the some college or less group, where in this case it reflects the decline in real wages in the 1980s. At the same time, we see substantial increases in median incomes among those with at least college after 1990, owing to rising real wages. In Appendix Figure B3 we present kernel density estimates by employment status of the predicted probability from the first-stage probit equation for each gender and education group used in estimation. There we see substantial overlap in the underlying support in the first stage, which is fundamental to identification of the selection model. The key takeaway is that the simulated instruments offer substantial variation to aid in identification of the selection equation.

#### **IV. Estimates of Cohort Wage Profiles**

We begin the empirical results with estimates of wage profiles from the conditional quantile models with selection at the 10<sup>th</sup>, 50<sup>th</sup>, and 90<sup>th</sup> percentiles. We focus on all workers, but also present a parallel set of estimates in Appendix D for those working full time. Tables A1-A3 contain summary statistics of the model covariates for all workers and nonworkers, full-time workers only, and nonworkers, respectively. Table A3 shows that nonworking men compared to men overall in Table A1 are less likely to be married, less likely to be white, and reside in

smaller households with fewer children. Nonworking women, on the contrary, are more likely to be married and to reside in larger households with children.

[Tables I-IV here]

Tables I-IV present the point estimates and associated bootstrap standard errors from the first-stage employment selection model as well as the quantile with selection log wage models. Tables I-II are for men with some college or less and those with college or more, respectively, while Tables III-IV contain the corresponding estimates for women. All models control for fixed state effects and normalized common time fixed effects.

The first stage probit estimates for male and female employment suggest that the demographic factors generally operate in expected ways, with married men and men residing in metro areas more likely to work, while non-White men and men with more children less likely to work. Among women, some of these patterns reverse. For example, college-educated Black women are more likely to work, as are college-educated women residing in non-metro areas compared to metro areas. The p-value on the joint significance of the three extra regressors in the employment equation is  $< 0.00$  in all four models, and with one exception (simulated disposable income from work among non-college educated women) they are all individually statistically significant, suggesting that they are predictive of the first stage. Employment is procyclical with respect to state business cycles. Moreover, employment declines with increases in simulated disposable income from no-work, which is akin to a nonlabor income effect on the extensive margin from a canonical labor supply model. On the other hand, increases in the amount of simulated income earned from work serves as an inducement to employment for men of both education groups, and a deterrent to work among college-educated women, suggesting some household substitution in work among partners.

The first column of each table also reports estimates of the copula dependence parameter,  $\hat{\rho}_j^s$ . There we see evidence of negative selection on unobservables into work for all four groups. The negative selection into work among less-skilled men is consistent with recent work of Aguiar et al. (2021), who highlight the shift toward leisure particularly among younger men. Negative selection among college-educated women is consistent with earlier work by Mulligan and Rubinstein (2008), although they found it became less negative over time. Maasoumi and Wang (2019) estimate the copula dependence parameter year-by-year in the cross section, finding a trend from negative selection to positive selection over time among women, though their sample is a full-time working sample. In Appendix Table D4 we find no evidence of nonrandom selection among full-time college educated women. The finding of negative selection among college-educated men is perhaps surprising, though there are few estimates to compare to in the literature on this demographic as selection is almost always assumed random for men. As shown in Figure II, employment rates are consistently higher for college educated men, reducing the importance of selection bias for this group. Maasoumi and Wang (2019) estimate selection among full-time men, where in some years they find negative selection though the modal estimate is positive selection. Ashworth et al. (2021) find that men are negatively selected on unobservables into part-time work. Blau et al. (2023) find positive selection on *observables* for both women and men, but little evidence on selection on unobservables. Fernandez-Val et al. (2023), like Maasoumi and Wang (2019), provide annual cross-sectional estimates of gender gaps and find that selection matters for women only in lower quantiles, where it is U-shaped, becoming less positive from the mid 1970s to 2000 and then more positive.

As described in Appendix E, and discussed in the next section, we estimate a number of alternative models of selection, including using the age composition of children as the

identifying instruments. Excluding the number and age distribution of children from the wage equation has a systematic impact on our estimates of selection. Our results suggest that positive selection for college educated women appears to be largely driven by children. Most likely through the impact on past human capital investments. For college educated men employment rates are sufficiently high to make selection less of an issue anyway. For ‘some college or less’ women and men it looks like there is a different story where negative selection is common. While the selection parameter is sensitive for some groups based on exclusion restrictions, the implied pattern of life cycle gender wage gaps presented in Appendix E is quite robust.

Turning to the parameter estimates in the wage equation, we see that wage gaps of race-ethnicity minority groups tend to be most pronounced in the bottom half of the distribution for both men and women, while the premium to marriage tends to be higher in the bottom half for men, and (slightly) higher in the top half of wages of women. The effects of children on wages are near zero for men without a college education, while they are a small positive for college-educated men, a result consistent with Lundberg and Rose (2002). These child effects on wages tend to be negative only in the bottom half of the female wage distribution, though as discussed previously, the negative impact is quite sizable on the extensive margin of employment.

At the bottom of Tables I-IV we present Wald tests of the joint significance of the cohort terms, the interaction terms between time and entry age (R1-R4 in the tables), and both cohort and the R1-R4 terms. The rejection of the null that time and entry age interactions are zero implies that we cannot construct pure life cycle age-wage profiles, i.e. cohorts do not have common wage growth with age. Additionally, rejecting both cohort and the interaction terms are zero means we do not have uniform wage growth across cohorts, and that the cross-section age-wage profile is not shifted over time by a common amount given by macroeconomic wage

growth. We interpret the results as suggesting that cohort age-wage profiles are not parallel, and thus refer to them as *pseudo* life cycle age-wage profiles.

## **V. The Evolution of Gender Gaps in the Pseudo Life Cycle distribution of Offer Wages**

With the estimated coefficients from Tables I-IV, in this section we examine the implications for the evolution of gender wage gaps across the life cycle. As detailed in Appendix C, with the quantile coefficients we construct counterfactual offer wage distributions for both working and nonworking men and women in each education group using the conditional quantile decomposition method of Machado and Mata (2005). The idea is for each gender-education group we randomly assign a set of quantile coefficients from the  $q$ th quantile,  $q = 0.1, 0.5, 0.9$ , to produce a prediction of the  $q$ th quantile offer wage distribution; that is, the offer wage is that wage the individual in a particular gender-education group is predicted to earn at a particular quantile based on their demographics and other determinants of wages in equation (3) along with parameters estimated in Tables I-IV, regardless of whether they are in actual work or not. Because Heathcote et al (2005) found that common within group time effects were a primary channel affecting the age profile of inequality, we net out additive within-group time effects on offer wages by regressing the predicted gender-education specific wage at each quantile on a full set of time dummies, saving the residual, and adding back the group- and quantile-specific mean prediction. To reduce sampling variation associated with any given draw, we repeat this process 30 times and then take the mean across the simulated samples. Finally, the life cycle gender gap in offer wages is found by taking the difference in predicted log wages of men and women at each age in a given education group at the 10<sup>th</sup>, 50<sup>th</sup>, and 90<sup>th</sup> percentiles, and we plot the gender-age-quantile-specific ten-year birth cohort mean at each age and cohort.

[Figure V here]

Figure V presents the within-education group gender offer wage gaps over the life cycle and wage distribution.<sup>8</sup> The patterns across cohorts, and across the life cycle within cohorts, are striking. Among men and women with some college or less, the life cycle gender offer wage gap is quadratic at the 10<sup>th</sup> percentile, nearly doubling from 20 percent at age 25 to 40 percent a decade later, and then tapering back to about 30 percent in the latter half of the working life. There is little difference across cohorts. At the median, however, there are substantial differences across the cohorts born before the 1960s and those born after, particularly from age 35 onward. In the pre-1960s cohorts the level gap in the offer wages at older ages was quite substantial, but it declined with successive cohorts and with age within a cohort. Starting with the 1960s, however, there are few differences across cohorts in median life cycle profiles, with gaps increasing sharply early in the working life, and also not tapering back as much later in the working life as in older cohorts (or in the 10<sup>th</sup> percentile). At the 90<sup>th</sup> percentile of the some college or less group the gender gaps tend to increase until age 45 before tapering off. There is substantial convergence across cohorts at the top of the wage distribution at older working ages that continued to the 1960s birth cohort, as well as some narrowing of the gender gap with age, but never a return to levels seen early in the working life.

In the bottom panel of Figure V we depict the corresponding pseudo life cycle gender wage gaps for those women and men with at least four years of college education. At low wages there is evidence of very sharply increasing wage penalties against women over the first two decades of the working life. These wage gaps reached levels of about 50 percent for cohorts of women born in the 1920s and 1930s, but there was substantial narrowing for the 1940s and

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<sup>8</sup> Note that all graphs in Figure V are net of within education and gender time effects. We return to examine the role of time effects in subsection B below.



1950s cohorts that started around age 40 and continued through age 55. For the 1960s cohort, however, there is no narrowing of the gender wage gap after age 45, and it appears the 1970s cohort is on that same trajectory. A similar pattern is observed at the median of the college educated group where for the 1960s cohort the gender offer wage gap is increasing linearly through age 55 and reaching levels found in the 1920s cohort. At the 90<sup>th</sup> percentile, where we might expect substantial improvements in life cycle gender gaps among more recent cohorts given these are high-wage workers with similar levels of human capital, we do in fact observe some modest improvements in the 1960s cohort relative to earlier ones, but the 1970s cohort is no better, and in fact there appears to be retrenchment in both the 1980s and 1990s cohorts of women at young ages.

The life cycle cohort gender wage gap profiles in Figure V help shed light on the time-series of the raw gender wage gap at the bottom, middle, and top of the wage distribution in Figure I. There we saw a decline in the gender gap from the mid 1970s to mid 1990s across the distribution, followed by a stalling that was more pronounced at the top of the distribution. The time series decline is largely driven by the relative wage gains in the cohorts of women in the 1940s and 1950s that reduced the level gap at middle working ages and with further progress in later working ages, while the plateauing of the time-series gap after the mid 1990s stems from a stalling of progress across the life cycle in cohorts after the 1950s. Gender progress at the top of the wage distribution among the highly educated in recent cohorts has actually retreated more than at the middle and bottom.

#### *A. Robustness*

In Appendix E we provide a detailed examination of the robustness of the life cycle gender wage gaps presented in Figure V to assumptions on identification and specification of the

selection model, on functional form of the wage determination process in equation (3), on how we split the sample based on education attainment, and on the potential influence of very old and very young cohorts.

More specifically, instead of the three excluded instruments used in the baseline model of Figure V (i.e. state unemployment rates, simulated disposable income from no work, and weighted simulated disposable income from work), we drop the simulated in-work instrument so the model is more akin to a standard labor supply model identification based on nonlabor income (Appendix Figure E1). We then drop both simulated disposable income instruments and replace them with the maximum benefit guarantee from the SNAP and TANF welfare programs as they are based solely on policy decisions and not a mix of policy decisions and household demographics as with the simulated instruments (Figure E2). This is then followed by a model where we drop all three extra regressors and thus rely on the nonlinear restrictions in our flexible parametric framework to identify the selection from wage model (Figure E3). The fourth check is to implement the so-called median selection rule frequently used in the racial wage gap literature whereby anyone out of work is assumed to pull from the bottom half of the wage distribution, and after adding nonworkers back to the sample with a log wage of 0, we re-estimate wages at the median and above (Figure E4). The next test uses the age composition of children to identify the selection model as in Mulligan and Rubinstein (2008), Maasoumi and Wang (2019), Blau et al. (2023), and Fernandez-Val et al. (2023) instead of the simulated disposable income instruments (Figures E5 and E6). The next test drops the selection model altogether and assumes that men and women in work are randomly selected from the population (Figure E7). We then return to the baseline specification of the selection model with the three exclusion restrictions and instead split the sample based on quartiles of the education attainment

distribution rather than a sheepskin effect of a college degree (Figure E8). We next take a much more parsimonious specification of wage determination in equation (3) by assuming a quadratic in cohort, entry age, and time (Figure E9). This is then followed by an expanded specification of the baseline model to include state-specific linear time trends (Figure E10). The final two specification checks examine whether there was undue influence on the parameter estimates from the oldest (1920s) and youngest (1990s) birth cohorts given their relatively smaller sample sizes (Figures E11 and E12).

The takeaway from these various specification tests is that the key patterns depicted in Figure V—reduced levels of gender wage gaps at older ages across cohorts born before the 1960s, along with a stalled progress among more recent cohorts across the working life—remain robust. However, there are a few differences worth highlighting in these specifications. First, when we use the median selection rule to identify the employment from the wage equation we find substantive differences among older cohorts, especially those with some college or less. The reason is that many older women were not in the labor force and thus inclusion of zeros pulls the median substantially lower, and inflates the gender gap. This is particularly pronounced among the 1920s and 1930s cohorts. However, by the 1950s cohort, the life cycle profiles of the gender gap, particularly among the college educated, are much more similar to our baseline estimates, suggesting our results are robust to less parametric alternatives, at least starting with the 1950s cohorts. Second, when identification is based on the standard approach of the age composition of children in the employment equation but not the wage equation, we find more of a narrowing of the gender wage gap than in our baseline estimates. This is more pronounced among the college educated. That is, omitting children from the wage equation results in too low of a gender wage gap. Indeed, our results suggest that positive selection for college educated women is largely

driven by children, most likely through the impact of accumulated work experience.<sup>9</sup> For college educated women, experience capital is likely to be particularly important determinant of wages, see Blundell, Costa-Dias, Meghir and Shaw (2016). For college educated men employment rates are sufficiently high to make selection less of an issue anyway. For both women and men with ‘some college or less’ it appears there is a different story in which negative selection is common. Third, when we assume selection into work on unobservables is random, we find that for most cohorts the gender wage gap is attenuated at most ages and that the life cycle gender wage gaps among the college educated have much less curvature later in the working life than we found when selection is modeled in Figure V, meaning less catch-up of women relative to men.

#### *B. Potential Mechanisms*

To further understand the evolution of the life cycle gender gaps across cohorts in Figure V, in this section we explore three potential mechanisms—the role of common (group-specific) shocks, the life cycle timing of child rearing, and the rise of full-time work.

[Figure VI here]

Heathcote et al. (2005) emphasized the importance of time effects in accounting for the life cycle inequality of wages, and thus in our baseline counterfactuals we first filtered the predicted offer wage for each working and non-working individual through a set of gender and education-group specific unrestricted time dummies prior to constructing the gender gaps at each age in a cohort. In Figure VI we present the corresponding life cycle gender gaps without netting out the common group-specific time effect. There we see much more fanning out of gender gaps across cohorts at each age akin to that observed in the actual data in Appendix Figure A6; that is,

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<sup>9</sup> If we retain our instruments but just remove age composition of kids in the wage equation we still get positive selection for college women.

there are sharp reductions in gender gaps from the 1920s to the 1950s birth cohorts. Moreover, in Figure VI we see gender wage gap peak earlier in the life cycle through the 1950s cohort and then a sharp reversal to later in the life cycle starting with the 1960s cohorts (except among the college-educated at the 90<sup>th</sup> percentile where the reversal started with the 1950s cohort).

This reduction in peak life cycle gender gap is less in evidence in Figure V with time effects netted out. To explore this further in Appendix C we present the pseudo life cycle offer wage profiles for men with and without time effects netted out (Appendix Figures C1 and C2) separately from the profiles of women (Figures C3 and C4). Those figures show that the common group-specific time effects eliminate most cohort differences in life cycle offer wages for both men and women, and in fact for the case of college-educated men re-order which cohorts had the highest wages (1920s and 1930s cohorts) compared to actual data and the model predictions with time effects. Moreover, for women, taking out the time effects results in a more uniform life cycle peak age of wages, which helps explain why in Figure V we do not observe a reduction in peak age of the gender gap as we do in Figure VI.

[Figure VII here]

This becomes more readily apparent in Figure VII where we plot the coefficients on the time effects for men and women of each education group from the 50<sup>th</sup> quantile regressions of the predicted offer wages on the time effects, averaged across the 30 replications. Within each education group there is clear evidence that common within group time effects favored the wages of women over men from the mid 1970s to the mid 1990s, with the time effects of women converging towards those of men. In the subsequent two and a half decades the time effects followed parallel trends. The gender wage gap with time effects included shows substantially elevated gaps at each age among cohorts born before 1960, but because common time effects

disproportionately favored female workers in the labor force before the mid 1990s, once we net these time effects out, the cross-cohort gender wage gap is substantially attenuated. The parallel trends in time effects in the last 25 years implies a stalling of progress favoring women in labor markets and thus little improvement in gender gaps in the second half of the working life.

[Figure VIII here]

Part of the stall in the gender gap is likely attributable to changes in life cycle fertility and child rearing, which is the second mechanism we explore. Evidence from Juhn and McCue (2017), Kleven et al. (2019), and Cortes and Pan (2023) suggest that these “child penalties” to womens’ wages from childbirth are on the order of 30 percent in the United States.<sup>10</sup> In Figure VIII we present life cycle profiles of the share of women by education and cohort with children aged 0-5 in the top panel and aged 6-18 in the bottom panel. Across both education groups there has been a rightward shift in the distribution of child rearing to older ages among more recent cohorts, and this is especially pronounced among those women with at least a college education. In other words, delayed child rearing likely has resulted in these child penalties kicking in later in the life cycle, which helps account for the stalling of the life cycle gender gap in recent cohorts. Importantly, our approach allows children to have a direct impact on wages, most likely through their impact on past work experience, as well as an impact through selection in to work. Based on the coefficients in Tables I-IV, both avenues are important and much of the penalty appears to operate through the extensive margin of employment.

[Figure IX here]

The third mechanism we explore is whether much of the patterns observed in life cycle gender wage gaps fall upon those women in recent cohorts with weaker attachment to the labor

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<sup>10</sup> Note in our model we have numbers of children aged 0-5 and aged 6-18, not indicators for the presence of children or arrival of “first-birth” as used in some of the child penalty literature.

force, e.g. through part-time work. Appendix Figure A2 depicts a secular rise in the share of working women who are employed full time over our sample period, increasing about 25 percentage points for both education groups. Figure IX presents the life cycle gender offer wage gaps for workers who select into full-time work. That is, the model is specified the same as our baseline model in equations (1)-(3), but instead of selecting into any work we redefine it into selection into full-time work, defined as working at least 35 hours per week for 50 weeks per year. The parameter estimates along with the associated gender and education specific pseudo life cycle wage profiles are presented in Appendix D. Figure IX shows that the life cycle gender offer wage gaps are substantively unchanged from Figure V for all workers. This was perhaps foreshadowed in the time-series Appendix Figure A1 of full-time workers, where similar to the sample overall, there is a sharp decline in the gender wage gap from the 1970s to the mid 1990s, followed by a stalling in the gap, which is more pronounced in the top half of the wage distribution. However, current full-time work is unlikely to be enough to draw definitive conclusions about the impact of part-time work and labor market attachment. The history of labor market attachment will impact the current wage of women through investment in experience capital.<sup>11</sup> This history will be driven largely by the numbers and ages of children which we account for directly in our approach to modeling wages and selection into work.

## **VI. Conclusion**

We estimated the distribution of life cycle gender wage gaps for cohorts of prime-age men and women in the presence of nonrandom sample selection using data from the Current Population Survey for calendar years 1976-2018. We found that the evolution of gender wage gaps varied dramatically across cohorts, the working life, education groups, and the wage

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<sup>11</sup> See Blundell, Costa Dias, Meghir, and Shaw (2016).

distribution. Moreover, controlling for both nonrandom selection into work and for within group common time effects were important for our understanding of those life cycle gender wage gaps. Most of the gains in women's relative wages across the lifecycle and distribution occurred among the cohorts born in the 1940s and 1950s relative to those born before 1940 and those born after 1960. This led to a substantial convergence in the time series of women's wages relative to men's until the mid 1990s when progress stalled. This lack of progress for women born after the 1950s occurred across the working life and distribution. Indeed, this stalling of convergence coincided with an increase in the age of the peak gender gap, likely a result of delayed fertility and child rearing among recent cohorts of women.

There are hints that economic progress of women relative to men reversed among Millennials, and if it persists then this could have long-term implications for gender equality. The recent shock of the Covid-19 pandemic has unraveled child-care markets, disproportionately affecting women's employment decisions with potential negative consequences for life cycle wage progression. This development underscores the importance of continued research on wage differences within- and between-genders both over time and the life course.



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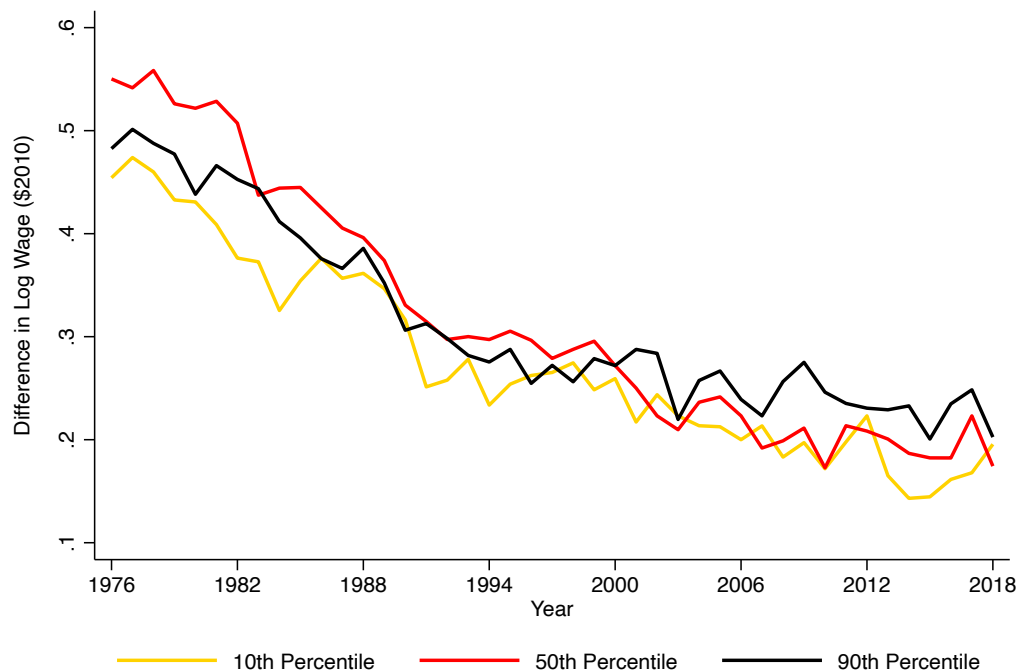
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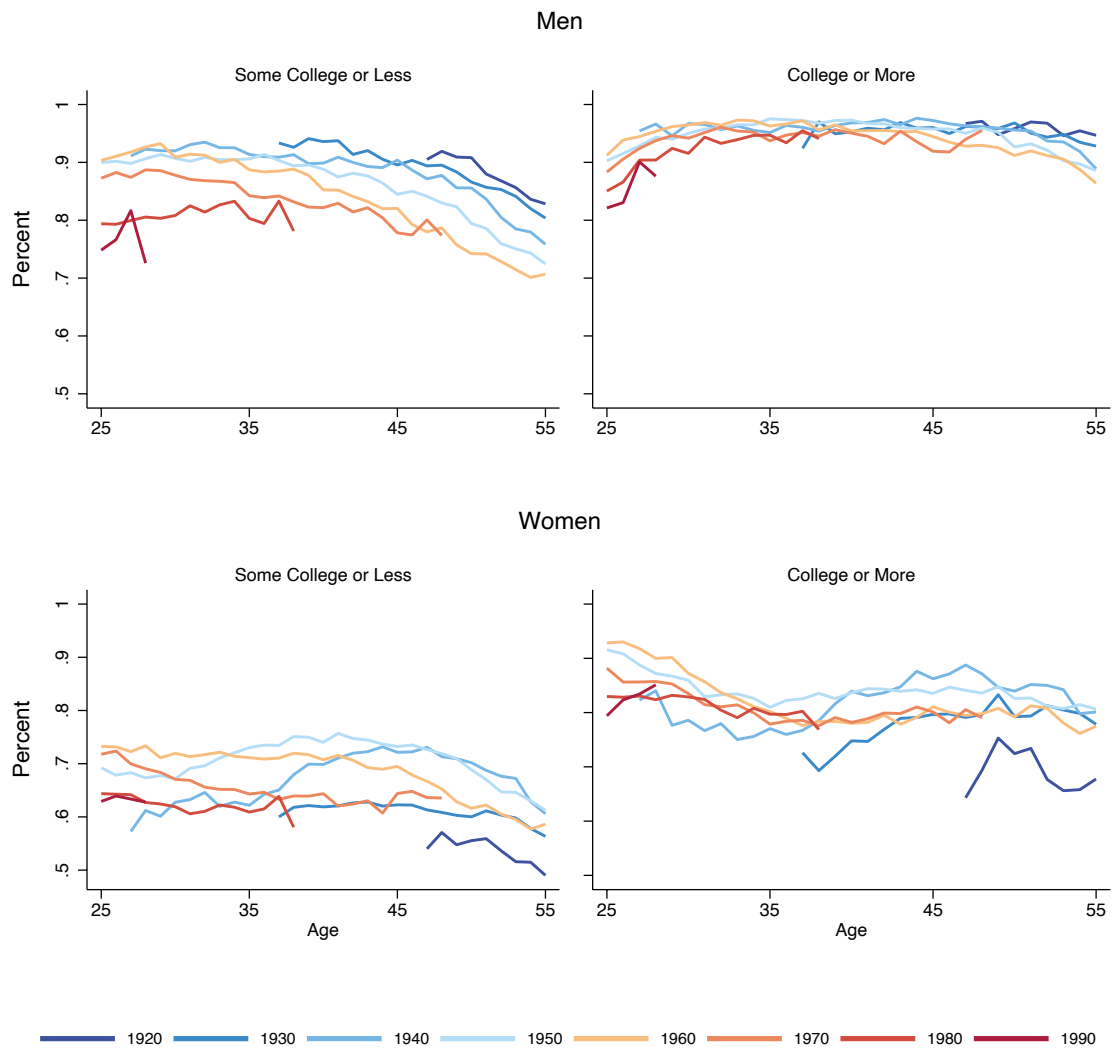
Figure I. Time Series of Gender Gap in Log Hourly Wages of Workers



Source: Current Population Survey Annual Social and Economic Supplement, Survey Years 1977-2019

Note: The figure depicts the difference in log wages of men and women at the 10th, 50th, and 90th percentiles of the gender-specific wage distributions. Wages are defined as the ratio of annual earnings to annual hours of work, and are in real 2010 dollars using the Personal Consumption Expenditure Deflator. Sample consists of employed men and women aged 25-55. Workers with imputed earnings or hours are dropped, as are those with wages below the 1<sup>st</sup> percentile or above the 0.1 percentile of the real gender- and year-specific wage distributions.

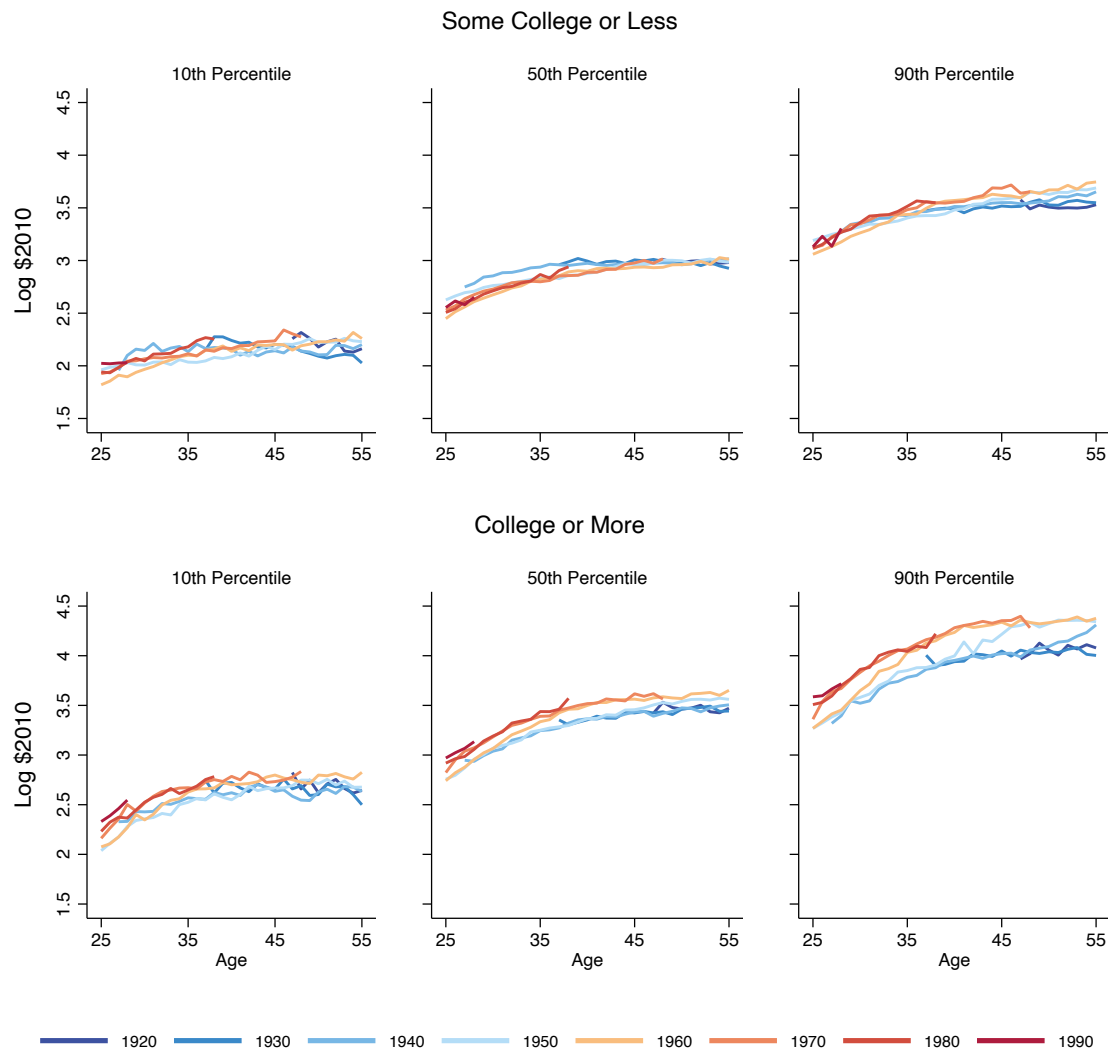
Figure II. Life Cycle Employment Rates across Cohorts



Source: Current Population Survey Annual Social and Economic Supplement, Survey Years 1977-2019  
 Note: Employment refers to any paid work in the calendar year. Sample consists of men and women aged 25-55 who do not have imputed earnings or hours of work.

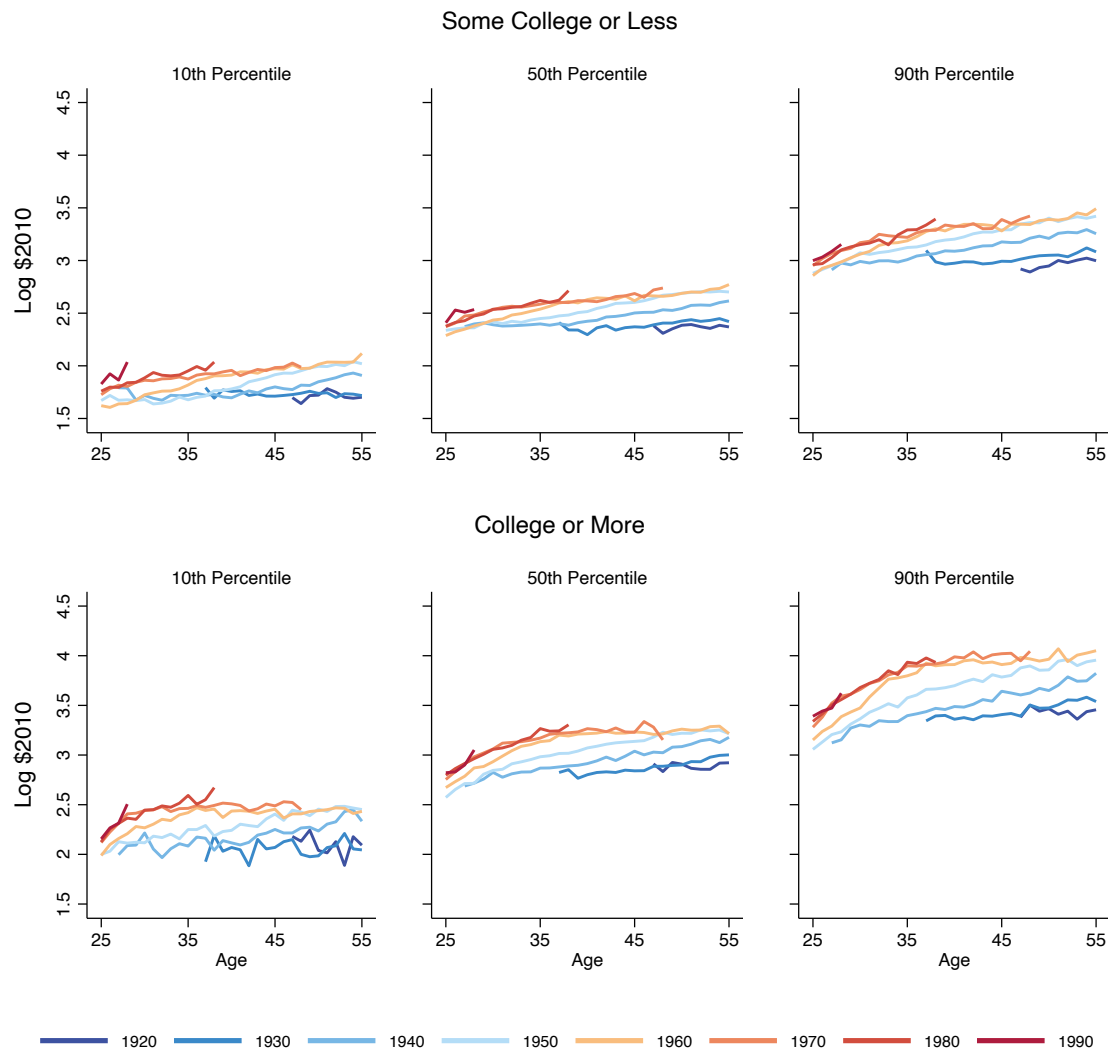


Figure III. Distribution of Life Cycle Real Hourly Wages of Working Men across Cohorts



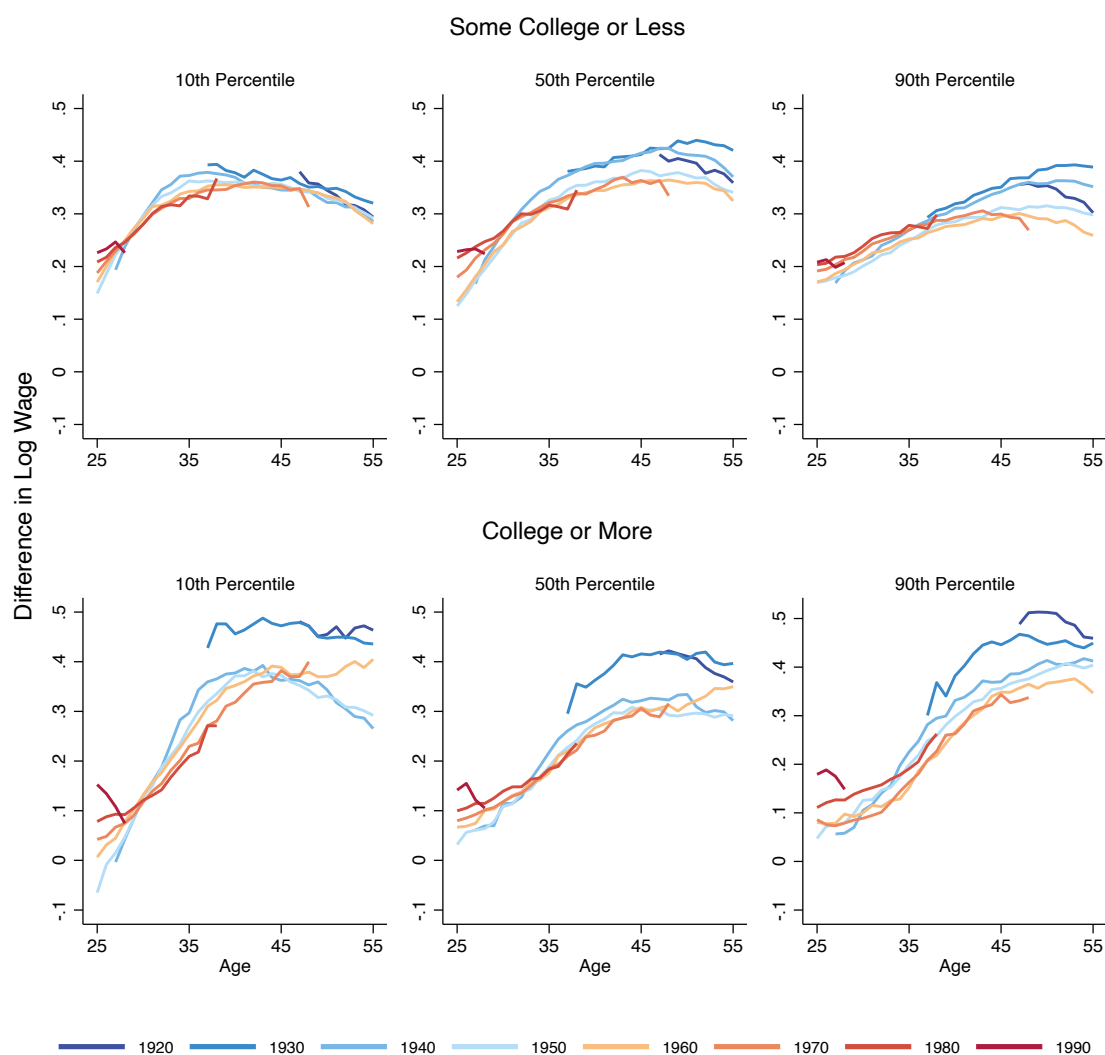
Source: Current Population Survey Annual Social and Economic Supplement, Survey Years 1977-2019  
 Note: Wages are defined as the ratio of annual earnings to annual hours of work, and are in real 2010 dollars using the Personal Consumption Expenditure Deflator. Sample consists of employed men aged 25-55. Workers with imputed earnings or hours are dropped, as are those with wages below the 1<sup>st</sup> percentile or above the 0.1 percentile of the real male- and year-specific wage distributions.

Figure IV. Distribution of Life Cycle Real Hourly Wages of Working Women across Cohorts



Source: Current Population Survey Annual Social and Economic Supplement, Survey Years 1977-2019  
 Note: Wages are defined as the ratio of annual earnings to annual hours of work, and are in real 2010 dollars using the Personal Consumption Expenditure Deflator. Sample consists of employed men aged 25-55. Workers with imputed earnings or hours are dropped, as are those with wages below the 1<sup>st</sup> percentile or above the 0.1 percentile of the real male- and year-specific wage distributions.

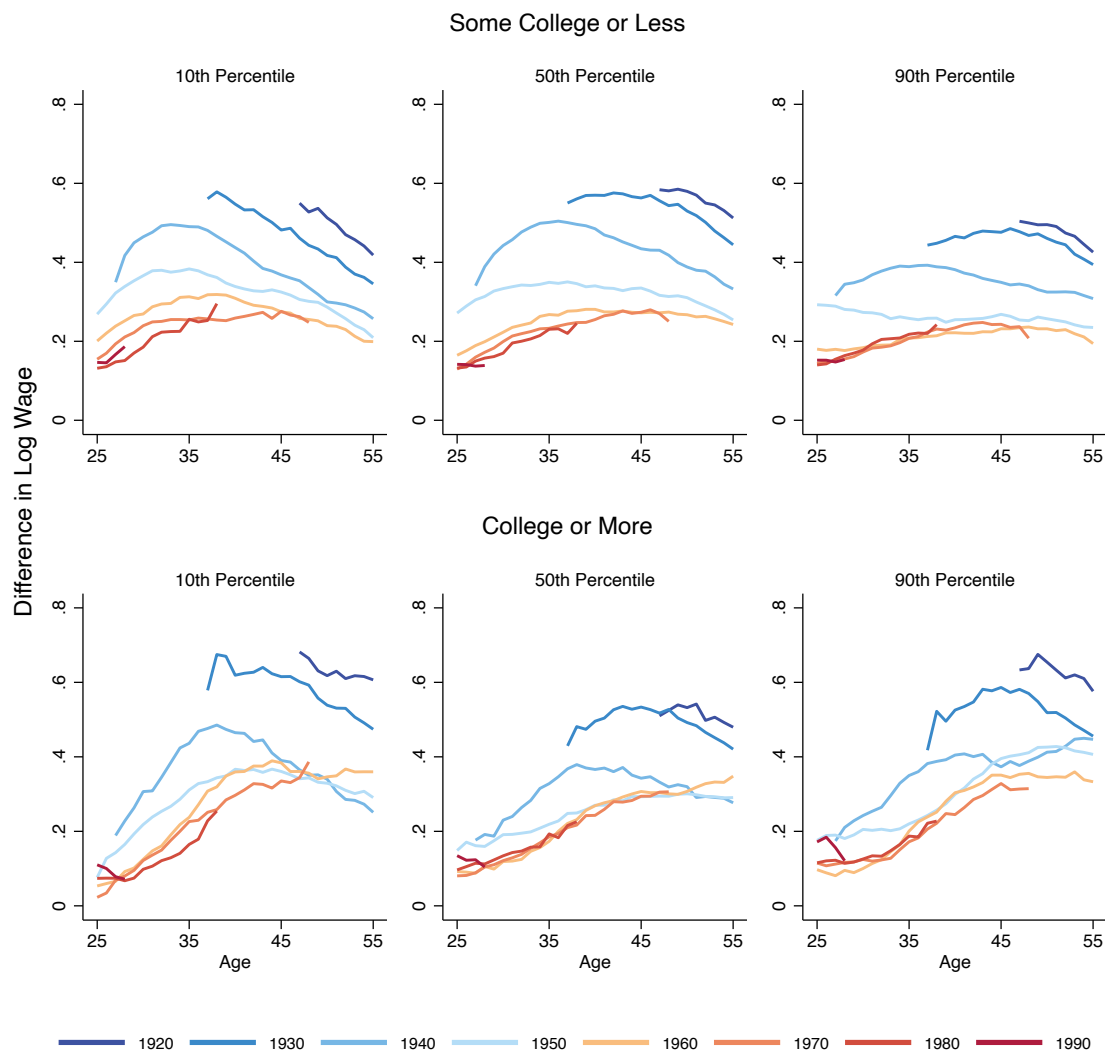
Figure V. Within-Education Group Gender Offer Wage Gaps over the Life Cycle



Source: Current Population Survey Annual Social and Economic Supplement, Survey Years 1977-2019

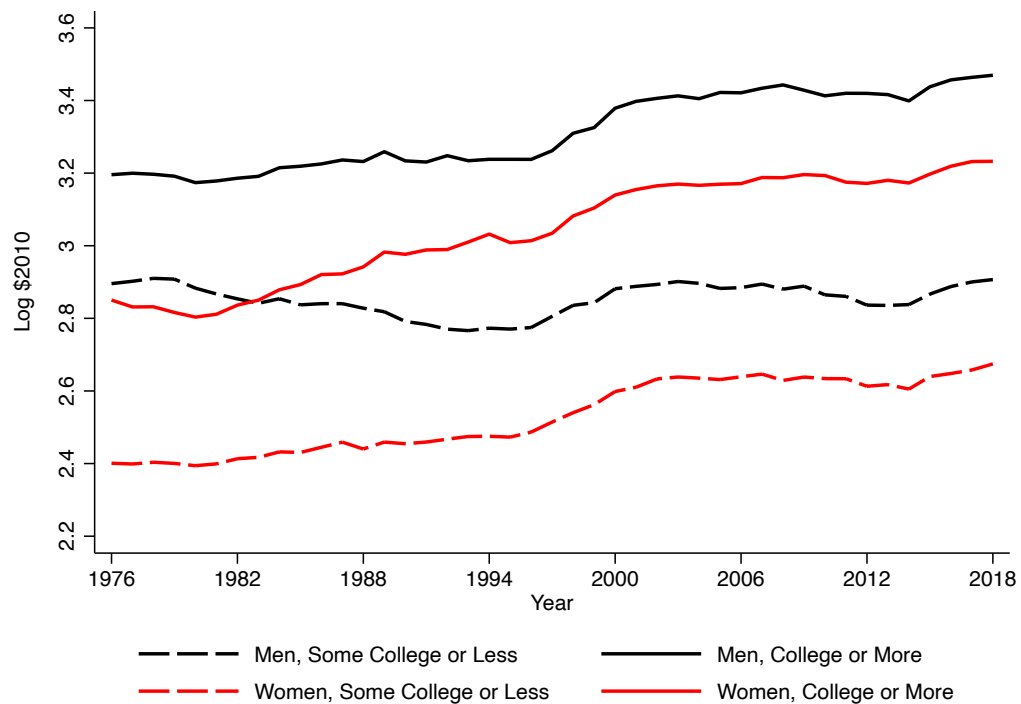
Note: Gender gaps are based on counterfactual offer wage distributions based on coefficients from the quantile selection model net of gender- and education-specific time effects. See text for additional details. Sample consists of working and nonworking men and women aged 25-55. Workers with imputed earnings or hours are dropped from estimation of quantile coefficients, as are those with wages below the 1<sup>st</sup> percentile or above the 0.1 percentile of the real gender- and year-specific wage distributions.

Figure VI. Within-Education Group Gender Offer Wage Gaps over the Life Cycle with Time Effects



Source: Current Population Survey Annual Social and Economic Supplement, Survey Years 1977-2019  
 Note: Gender gaps are based on counterfactual offer wage distributions based on coefficients from the quantile selection model inclusive of gender- and education-specific time effects. See text for additional details. Sample consists of working and nonworking men and women aged 25-55. Workers with imputed earnings or hours are dropped from estimation of quantile coefficients, as are those with wages below the 1<sup>st</sup> percentile or above the 0.1 percentile of the real gender- and year-specific wage distributions.

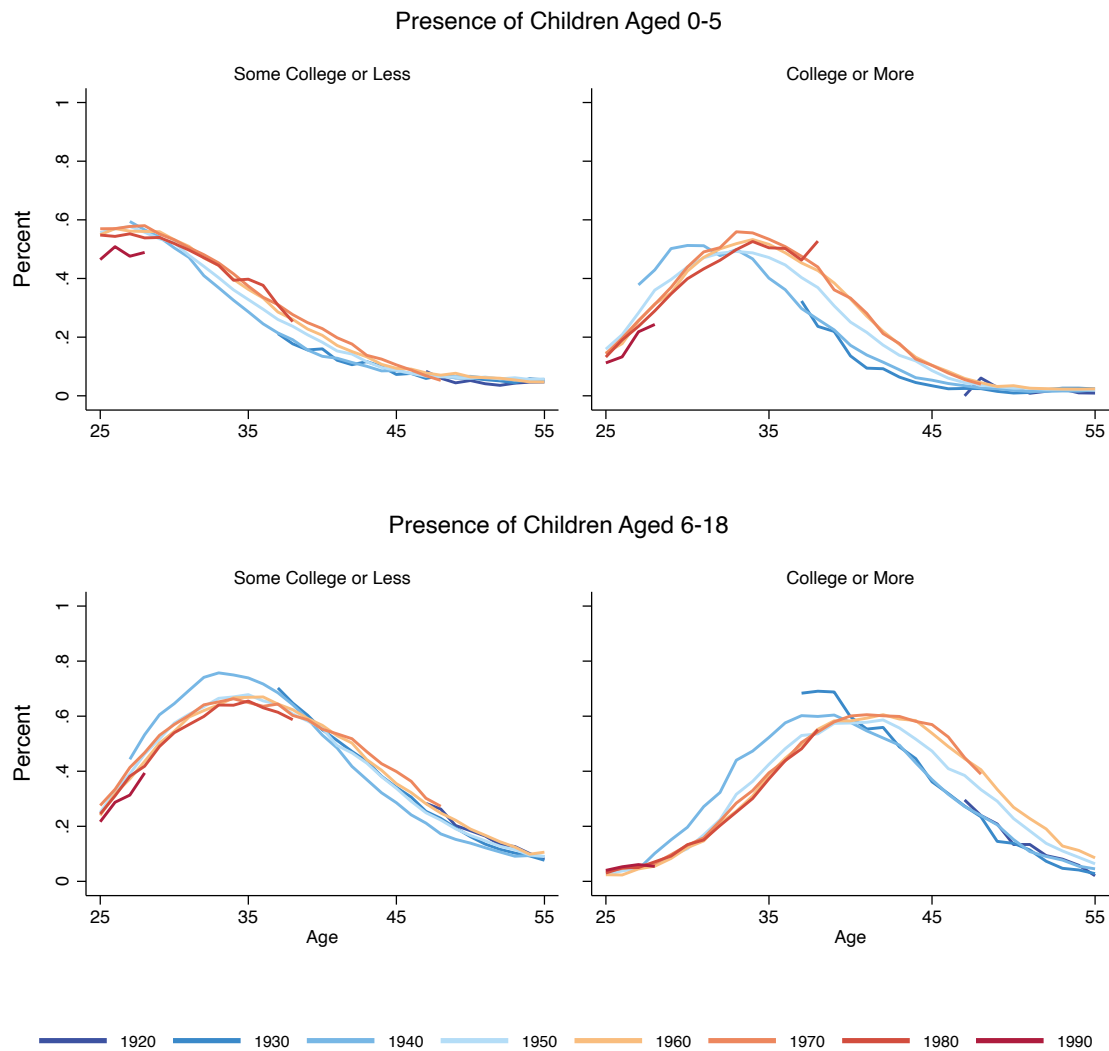
Figure VII. Aggregate Time Effects from Median Selection Offer Wages of Men and Women



Source: Current Population Survey Annual Social and Economic Supplement, Survey Years 1977-2019

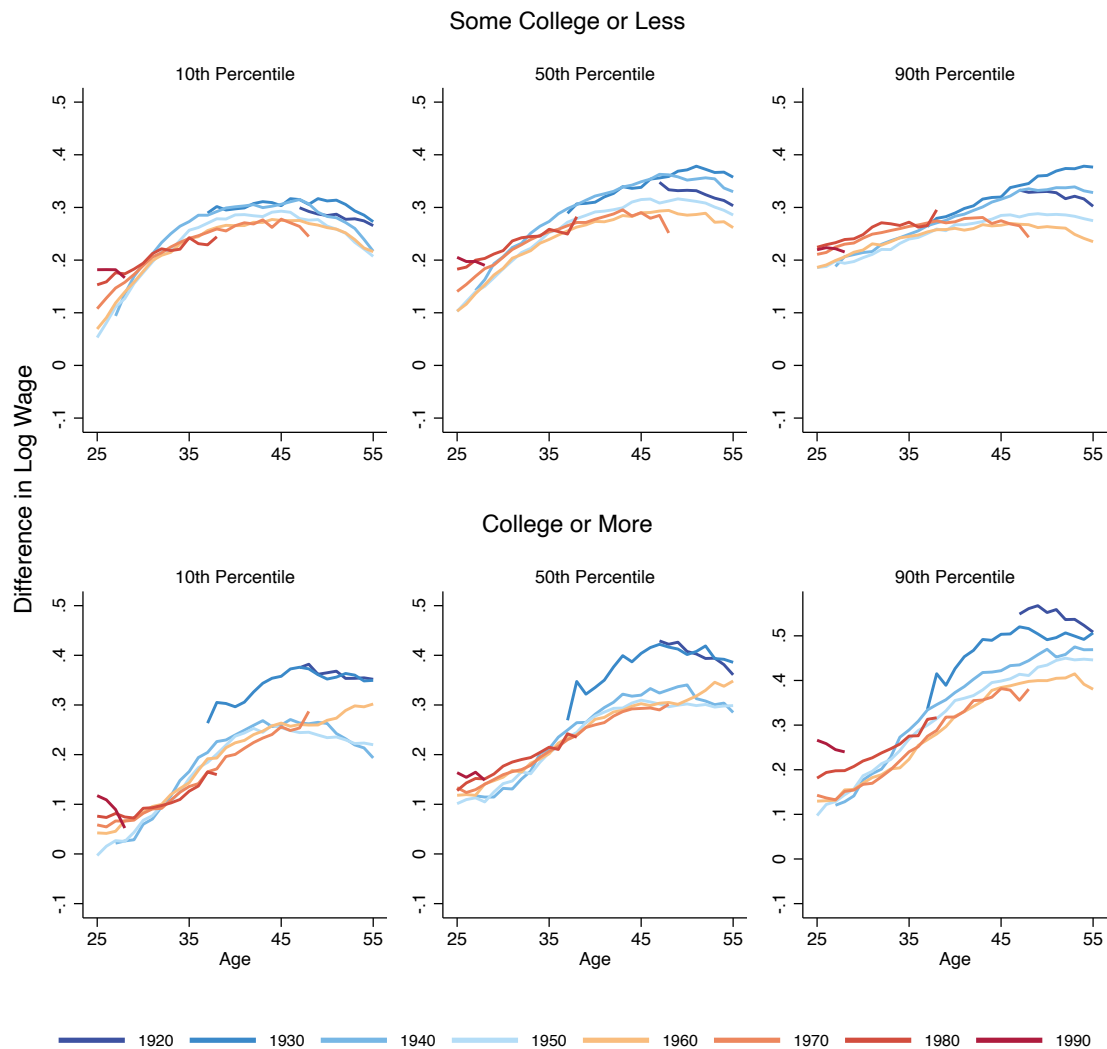
Note: Aggregate time effects are the coefficients on year dummies in a regression of log offer wages at the median. Median wages are counterfactual offer wage distributions based on median coefficients from the quantile selection model. See text for additional details. Sample consists of working and nonworking men and women aged 25-55. Workers with imputed earnings or hours are dropped from estimation of quantile coefficients, as are those with wages below the 1<sup>st</sup> percentile or above the 0.1 percentile of the real gender- and year-specific wage distributions.

Figure VIII. Share of Women with Children over the Life Cycle by Age of Child



Source: Current Population Survey Annual Social and Economic Supplement, Survey Years 1977-2019  
 Note: The figure shows the fraction of women at a given age by education who have any children in the household between the ages of 0-5 (top panel) and ages 6-18 (bottom panel). Sample consists of working and nonworking women aged 25-55.

Figure IX. Within-Education Group Gender Offer Wage Gaps over the Life Cycle: Full-Time Employment



Source: Current Population Survey Annual Social and Economic Supplement, Survey Years 1977-2019  
 Note: Gender gaps are based on counterfactual offer wage distributions based on coefficients from the quantile selection model for full-time employment net of gender- and education-specific time effects. See text for additional details. Sample consists of full-time working and nonworking (and less than full time working) men and women aged 25-55. Workers with imputed earnings or hours are dropped from estimation of quantile coefficients, as are those with wages below the 1<sup>st</sup> percentile or above the 0.1 percentile of the real gender- and year-specific wage distributions.

Table I. Quantile Selection Estimates of Log Wages for Men with Some College or Less

	Employment	10th Quantile	50th Quantile	90th Quantile
Constant	1.115 (0.036)	1.799 (0.015)	2.438 (0.011)	3.005 (0.011)
Entryage	-0.113 (0.035)	0.313 (0.020)	0.391 (0.012)	0.311 (0.016)
Entryage <sup>2</sup>	0.022 (0.034)	-0.125 (0.021)	-0.091 (0.012)	-0.042 (0.016)
Entryage <sup>3</sup>	-0.033 (0.008)	0.016 (0.005)	0.014 (0.003)	0.004 (0.004)
Time	0.564 (0.256)	0.134 (0.121)	-0.020 (0.082)	0.043 (0.103)
Time <sup>2</sup>	-1.198 (0.945)	-0.490 (0.481)	0.037 (0.328)	0.066 (0.411)
Time <sup>3</sup>	0.329 (0.627)	0.375 (0.348)	-0.130 (0.235)	-0.111 (0.326)
Time <sup>4</sup>	0.022 (0.161)	-0.107 (0.098)	0.056 (0.066)	0.043 (0.102)
Time <sup>5</sup>	-0.009 (0.015)	0.010 (0.010)	-0.007 (0.006)	-0.005 (0.011)
Cohort <sup>2</sup>	-0.030 (0.008)	0.011 (0.005)	0.009 (0.003)	-0.015 (0.004)
Cohort <sup>2</sup> *delta	0.142 (0.024)	-0.013 (0.016)	-0.100 (0.009)	-0.092 (0.012)
Cohort <sup>3</sup>	0.039 (0.006)	0.001 (0.004)	0.003 (0.002)	0.000 (0.003)
R1	-98.206 (37.238)	16.120 (26.824)	-81.678 (14.904)	-94.268 (21.313)
R2	9.049 (7.845)	0.065 (5.556)	18.901 (3.562)	11.294 (4.861)
R3	43.776 (16.231)	-7.401 (11.705)	3.968 (6.352)	15.811 (9.315)
R4	-1.986 (3.725)	1.190 (2.514)	-0.633 (1.607)	-1.191 (2.330)
Black	-0.455 (0.005)	-0.203 (0.005)	-0.198 (0.003)	-0.172 (0.004)
Other Race	-0.363 (0.008)	-0.267 (0.007)	-0.218 (0.004)	-0.141 (0.007)
Hispanic	-0.037 (0.006)	-0.339 (0.004)	-0.361 (0.002)	-0.268 (0.003)
Married	0.492 (0.006)	0.209 (0.003)	0.158 (0.002)	0.115 (0.003)
Live in Metro Area	0.111 (0.005)	0.164 (0.003)	0.139 (0.002)	0.111 (0.002)
Number of Children Ages 0-5	-0.020 (0.004)	-0.016 (0.002)	-0.008 (0.001)	0.004 (0.002)
Number of Children Ages 6-18	-0.038 (0.004)	-0.000 (0.001)	-0.001 (0.001)	-0.001 (0.001)
State Unemployment Rate	-0.027 (0.002)			
Simulated Disposable Income at No Work	-0.018 (0.001)			
Simulated Weighted Disposable	0.006			



Income at Work	(0.000)			
Rho	0.98			
	(0.05)			
P-value on Excluded Variables	0.00			
P-value on Cohort terms		0.00	0.00	0.00
P-value on R terms		0.02	0.00	0.00
P-value on R and Cohort terms		0.00	0.00	0.00

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Source: Current Population Survey Annual Social and Economic Supplement, Survey Years 1977-2019

Note: The table contains estimates from the quantile with selection model as described in the text. The models include indicators for state fixed effects and normalized aggregate time effects. Bootstrap standard errors are in parentheses.

Table II. Quantile Selection Estimates of Log Wages for Men with College or More

	Employment	10th Quantile	50th Quantile	90th Quantile
Constant	1.122 (0.071)	1.806 (0.032)	2.487 (0.017)	2.958 (0.021)
Entryage	0.389 (0.080)	0.569 (0.048)	0.532 (0.019)	0.555 (0.031)
Entryage <sup>2</sup>	-0.089 (0.080)	-0.232 (0.044)	-0.210 (0.020)	-0.208 (0.033)
Entryage <sup>3</sup>	-0.003 (0.020)	0.028 (0.011)	0.032 (0.005)	0.019 (0.008)
Time	0.405 (0.548)	-0.056 (0.282)	0.047 (0.120)	0.481 (0.228)
Time <sup>2</sup>	-0.215 (2.147)	-0.503 (1.128)	-0.019 (0.476)	-1.054 (0.876)
Time <sup>3</sup>	-0.523 (1.440)	0.371 (0.785)	-0.028 (0.330)	0.561 (0.615)
Time <sup>4</sup>	0.284 (0.364)	-0.089 (0.206)	0.017 (0.093)	-0.108 (0.165)
Time <sup>5</sup>	-0.036 (0.032)	0.007 (0.019)	-0.002 (0.009)	0.007 (0.015)
Cohort <sup>2</sup>	-0.074 (0.017)	0.011 (0.010)	0.002 (0.005)	-0.003 (0.008)
Cohort <sup>2</sup> *delta	-0.112 (0.054)	0.061 (0.026)	0.103 (0.016)	0.135 (0.027)
Cohort <sup>3</sup>	-0.004 (0.015)	0.015 (0.007)	0.029 (0.004)	0.003 (0.006)
R1	-179.660 (82.396)	10.979 (43.881)	82.872 (25.431)	148.340 (48.727)
R2	-26.792 (17.512)	-1.028 (9.125)	-17.892 (5.324)	-31.780 (10.278)
R3	-12.658 (36.417)	-5.695 (20.006)	-40.890 (11.210)	-22.255 (21.183)
R4	26.577 (8.366)	3.096 (4.327)	9.267 (2.446)	4.291 (4.731)
Black	-0.321 (0.016)	-0.226 (0.011)	-0.222 (0.006)	-0.238 (0.010)
Other Race	-0.353 (0.014)	-0.209 (0.009)	-0.027 (0.006)	-0.021 (0.008)
Hispanic	-0.141 (0.015)	-0.334 (0.010)	-0.194 (0.006)	-0.168 (0.007)
Married	0.209 (0.013)	0.212 (0.007)	0.132 (0.005)	0.084 (0.006)
Live in Metro Area	0.082 (0.014)	0.221 (0.006)	0.207 (0.003)	0.188 (0.006)
Number of Children Ages 0-5	-0.026 (0.011)	0.030 (0.003)	0.031 (0.001)	0.049 (0.003)
Number of Children Ages 6-18	-0.037 (0.010)	0.031 (0.002)	0.029 (0.001)	0.045 (0.003)
State Unemployment Rate	-0.016 (0.004)			
Simulated Disposable Income at No Work	-0.031 (0.002)			
Simulated Weighted Disposable Income at Work	0.009 (0.000)			

Rho	0.92			
	(0.48)			
P-value on Excluded Variables	0.00			
P-value on Cohort terms		0.10	0.00	0.00
P-value on R terms		0.00	0.00	0.00
P-value on R and Cohort terms		0.00	0.00	0.00

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Source: Current Population Survey Annual Social and Economic Supplement, Survey Years 1977-2019

Note: The table contains estimates from the quantile with selection model as described in the text. The models include indicators for state fixed effects and normalized aggregate time effects. Bootstrap standard errors are in parentheses.

Table III. Quantile Selection Estimates of Log Wages for Women with Some College or Less

	Employment	10th Quantile	50th Quantile	90th Quantile
Constant	0.857 (0.023)	1.629 (0.019)	2.152 (0.012)	2.624 (0.018)
Entryage	-0.151 (0.025)	0.099 (0.026)	0.253 (0.015)	0.378 (0.022)
Entryage <sup>2</sup>	0.053 (0.025)	-0.003 (0.023)	-0.118 (0.015)	-0.177 (0.023)
Entryage <sup>3</sup>	-0.028 (0.006)	0.001 (0.005)	0.025 (0.003)	0.034 (0.006)
Time	-0.251 (0.182)	-0.284 (0.187)	-0.135 (0.082)	0.150 (0.139)
Time <sup>2</sup>	2.909 (0.700)	1.222 (0.713)	0.300 (0.342)	-0.434 (0.536)
Time <sup>3</sup>	-2.357 (0.504)	-0.980 (0.486)	-0.134 (0.252)	0.286 (0.384)
Time <sup>4</sup>	0.652 (0.140)	0.284 (0.127)	0.025 (0.071)	-0.066 (0.109)
Time <sup>5</sup>	-0.060 (0.014)	-0.027 (0.012)	-0.001 (0.007)	0.005 (0.011)
Cohort <sup>2</sup>	-0.041 (0.006)	-0.031 (0.006)	-0.025 (0.003)	-0.012 (0.006)
Cohort <sup>2</sup> *delta	-0.075 (0.019)	-0.109 (0.015)	-0.110 (0.010)	-0.087 (0.017)
Cohort <sup>3</sup>	-0.029 (0.004)	-0.007 (0.004)	-0.013 (0.002)	-0.016 (0.004)
R1	39.124 (35.603)	-127.680 (24.758)	-76.342 (17.700)	14.125 (27.253)
R2	-35.282 (7.682)	6.793 (6.229)	5.117 (3.992)	-7.924 (6.102)
R3	-18.957 (14.320)	27.736 (10.708)	10.008 (7.417)	-21.734 (12.075)
R4	13.142 (3.243)	0.420 (2.871)	0.706 (1.853)	6.018 (2.946)
Black	-0.088 (0.005)	-0.051 (0.005)	-0.083 (0.003)	-0.111 (0.004)
Other Race	-0.231 (0.007)	-0.105 (0.007)	-0.105 (0.006)	-0.094 (0.008)
Hispanic	-0.257 (0.004)	-0.168 (0.005)	-0.234 (0.005)	-0.206 (0.005)
Married	-0.121 (0.004)	0.030 (0.003)	0.032 (0.002)	0.042 (0.003)
Live in Metro Area	0.038 (0.004)	0.155 (0.004)	0.164 (0.002)	0.171 (0.004)
Number of Children Ages 0-5	-0.299 (0.003)	-0.052 (0.006)	-0.010 (0.006)	0.034 (0.007)
Number of Children Ages 6-18	-0.112 (0.002)	-0.065 (0.003)	-0.058 (0.002)	-0.029 (0.003)
State Unemployment Rate	-0.018 (0.001)			
Simulated Disposable Income at No Work	-0.011 (0.001)			
Simulated Weighted Disposable	0.000			

Income at Work	(0.000)			
Rho	0.92			
	(0.29)			
P-value on Excluded Variables	0.00			
P-value on Cohort terms		0.00	0.00	0.00
P-value on R terms		0.00	0.00	0.04
P-value on R and Cohort terms		0.00	0.00	0.00

Source: Current Population Survey Annual Social and Economic Supplement, Survey Years 1977-2019

Note: The table contains estimates from the quantile with selection model as described in the text. The models include indicators for state fixed effects and normalized aggregate time effects. Bootstrap standard errors are in parentheses.

Table IV. Quantile Selection Estimates of Log Wages for Women with College or More

	Employment	10th Quantile	50th Quantile	90th Quantile
Constant	1.858 (0.051)	1.889 (0.040)	2.395 (0.015)	2.746 (0.023)
Entryage	-0.785 (0.055)	0.393 (0.052)	0.505 (0.021)	0.466 (0.033)
Entryage <sup>2</sup>	0.757 (0.049)	-0.117 (0.055)	-0.250 (0.023)	-0.249 (0.035)
Entryage <sup>3</sup>	-0.187 (0.012)	0.005 (0.014)	0.044 (0.006)	0.045 (0.009)
Time	-0.190 (0.397)	-0.353 (0.419)	-0.165 (0.149)	-0.382 (0.275)
Time <sup>2</sup>	2.726 (1.609)	0.941 (1.622)	0.275 (0.574)	2.099 (1.067)
Time <sup>3</sup>	-1.882 (1.107)	-0.705 (1.062)	-0.276 (0.393)	-1.439 (0.736)
Time <sup>4</sup>	0.475 (0.282)	0.210 (0.258)	0.101 (0.103)	0.358 (0.191)
Time <sup>5</sup>	-0.040 (0.025)	-0.021 (0.022)	-0.011 (0.009)	-0.030 (0.017)
Cohort <sup>2</sup>	-0.238 (0.011)	-0.056 (0.011)	-0.011 (0.005)	-0.022 (0.008)
Cohort <sup>2</sup> *delta	-0.447 (0.033)	-0.208 (0.038)	-0.044 (0.017)	0.064 (0.028)
Cohort <sup>3</sup>	-0.055 (0.009)	-0.037 (0.009)	-0.013 (0.005)	0.012 (0.007)
R1	-845.320 (52.203)	-309.910 (63.591)	14.074 (26.531)	110.820 (43.438)
R2	59.455 (11.791)	44.570 (11.196)	2.750 (5.323)	-24.042 (10.292)
R3	310.090 (25.388)	117.440 (27.040)	-9.722 (12.244)	-52.472 (19.501)
R4	-32.637 (5.880)	-19.897 (5.108)	-1.515 (2.656)	10.363 (4.618)
Black	0.124 (0.012)	0.001 (0.008)	-0.066 (0.004)	-0.103 (0.006)
Other Race	-0.397 (0.009)	-0.115 (0.010)	0.005 (0.008)	0.025 (0.009)
Hispanic	-0.143 (0.010)	-0.231 (0.010)	-0.125 (0.006)	-0.124 (0.007)
Married	-0.195 (0.008)	0.017 (0.006)	0.028 (0.004)	0.032 (0.004)
Live in Metro Area	-0.126 (0.009)	0.118 (0.007)	0.133 (0.003)	0.179 (0.005)
Number of Children Ages 0-5	-0.315 (0.004)	-0.017 (0.008)	0.054 (0.005)	0.107 (0.007)
Number of Children Ages 6-18	-0.116 (0.005)	-0.100 (0.003)	-0.031 (0.003)	0.002 (0.004)
State Unemployment Rate	-0.007 (0.002)			
Simulated Disposable Income at No Work	-0.020 (0.001)			
Simulated Weighted Disposable Income at Work	-0.003 (0.000)			

Rho	0.78			
	(0.28)			
P-value on Excluded Variables	0.00			
P-value on Cohort terms		0.00	0.01	0.00
P-value on R terms		0.00	0.00	0.04
P-value on R and Cohort terms		0.00	0.00	0.00

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Source: Current Population Survey Annual Social and Economic Supplement, Survey Years 1977-2019

Note: The table contains estimates from the quantile with selection model as described in the text. The models include indicators for state fixed effects and normalized aggregate time effects. Bootstrap standard errors are in parentheses.