The Effect of the COVID-19 Pandemic Recession on Less Educated Women's Human Capital: Some Projections *

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November 6, 2022

Keywords: COVID-19, human capital, unemployment, female labor supply
JEL Classification: J16, J22, J24, J64

*This is a revised version of the Presidential Address to the Society of Labor Economists annual conference in May, 2021. The authors thank Francine Blau for help with PSID codes and the Smith Richardson Foundation for research support. We wish to thank Stefania Albanesi, Joseph Altonji, Benjamin Cowan, James Heckman, V. Joseph Hotz, Michael Keane, Marjorie McElroy, and Claudia Olivetti for comments. Comments from participants of seminars at Rutgers University and the University of Chicago are also appreciated.

Abstract

The recession induced by the COVID-19 pandemic resulted in major declines in employment of women, both from the demand side as firms reduced employment and from the supply side resulting from school closures and the closing of many child care facilities. We provide projections of possible impacts of this reduction on less-educated women's future human capital framed within the traditional Mincerian model that implies that wage growth falls if a recession reduces the growth of work experience. We develop a new and modified form of the Mincerian log wage equation which we argue captures the effect of women's work experience on their human capital in a way superior to the traditional form of that equation. Using that modified form, we estimate the impact of recession-induced loss of work experience on wages using what we term Cohort IV. Our model, estimated on pre-COVID data, incorporates special features anticipated to be of importance in the pandemic, including the degree to which negative aggregate shocks occur to pandemic-specific industries, whether the impact of shocks varies by telecommuting occupation, and how the impact varies with the presence of preschool and school-age children who are affected by school and child-care facility closures. We find that wage losses one year out from 2020 are relatively modest on average, generally less than one percent, but larger for married women than for unmarried women and for those working in COVID-impacted industries. For married women, it is more severe for younger married mothers, for younger and older married childless women, and for married mothers with older children. School closures are also important for married women with school-age children and increase negative wage impacts by 50 percent. An increase in part-year work projected to occur during the pandemic increases the size of human capital losses.

The impact of the 2020-2021 COVID-19 pandemic recession on the U.S. labor market was dramatically sharper than that in all prior recessions since World War II in the short-run, by reducing employment by 22 million workers in 2 months, a 14 percent drop. Employment was depressed for many months and had still not risen back to its February 2020 level by the end of 2021. The unemployment rate jumped to almost 15 percent over that same initial period, a level not seen in any prior recession as well. On the other hand, the recovery in both employment and unemployment has also been sharp, with employment reductions from the initial point falling to 7 percent and the unemployment rate falling down to 7 percent, both after 10 months into the recession (the so-called "V-shaped" recession). While the unemployment rate at that point was well within the range of prior recessions, the employment reduction was still below that of all prior recessions, except possibly that of the Great Recession at its peak. The labor market recovered to its February 2020 level in June 2022, a recovery period far shorter than the 47 months taken for the 2001 recession or the 76 months taken for the Great Recession.

There has been much discussion on the impact of the recession on women. That discussion has been particularly extensive in the popular media, where it has been coined a "she-cession."[1] The pandemic began in March 2020 and, by April 2020, the employment-population ratio for women 25-54 had fallen by 23 percent from its level one year earlier, 5 percent more than that of men (Albanesi (2022)).[2] The greater impact on women has been ascribed partly to their heavier representation in sectors especially hit by the recession (leisure and hospitality, trade, services) and, for mothers, by reductions in the availability of child care and school closures (Alon et al. (2020a,b), Alon et al. (2021), Albanesi and Kim (2021)). However, women's employment recovered faster than that of

[1] https://www.nytimes.com/2021/03/04/upshot/mothers-jobs-pandemic.html?searchResultPosition=1.

[2] As emphasized by Goldin (2022), the decline for women was greater than this compared to February 2020 and lower compared to 2018 because women's employment was gradually increasing from 2018 to February 2020.

men's and their employment declines had reached parity by January 2021 (Albanesi (2022)). The decline was greater for women in what Albanesi and Kim (2021) call "inflexible" occupations–occupations which cannot be performed remotely–and high-contact occupations (Albanesi and Kim (2021), Heggeness and Suri (2021), Mongey et al. (2021), Fairlie et al. (2021)); greater for single women than married women (Albanesi (2022)); and was much more pronounced for less educated women (Aaronson et al. (2021), Furman et al. (2021), Goldin (2022)). Aaronson et al. (2021) also found that, despite a rapid recovery, negative impacts were particularly severe for particular subgroups of mothers–Black, single, and less educated mothers–but Lee et al. (2021) showed that most of the unequal patterns of decline by demographic characteristic had disappeared by the end of 2021.

This paper reports the results of an exercise to project possible effects of the pandemic recession on women's human capital. The conceptual approach is squarely in the Mincerian tradition framing the human capital stock as reflecting lifetime investments in skills, and that those investments can be proxied by years of work experience. In its most basic terms, a loss of employment in the Mincer model results in a smaller human capital stock both because on-the-job investments do not take place and because of skill depreciation when not working. A recession-induced loss of employment therefore reduces the stock of future human capital relative to what it would have been in the absence of the recession.

Our approach is to use historical experience to estimate the effect of recessions on women's employment loss and subsequent reductions in human capital, and then to use those historical relationships to make projections of the effect of the pandemic recession on women's later market wage rates. We use pre-pandemic data from 1968 to 2017 from the Michigan Panel Study of Income Dynamics (PSID), focusing on less-educated women because more educated women had modest impacts of the pandemic on employment (Goldin (2022)). In estimating the impact of past recessions on employment and on work experience, we also allow the impact to differ in ways specifically designed for the pandemic, including (i) allowing the impact of a recession to differ if the woman was in a (later) COVID-impacted industry and (ii) allowing the impact of recessions to be different for women who were in occupations that are likely to be telecommutable. We also pay close attention to the importance of recessionary impacts on women's employment by the age of the children, whether preschool age (and hence dependent on child care for the mother to work) or school age (and hence affected by school closures). Using the estimated model, the impact of the pandemic recession is then projected by first assuming the recession had not occurred and that the business cycle had stayed at its 2019 level, and then using the actual 2020 business cycle level. The difference in projected work experience and wage rates is our estimate of the impact of the recession on women's human capital. Although obvious, it is worth emphasizing that, keeping in mind the familiar distinction between forecasts and projections, these are only projections, not forecasts. They are counterfactual projections made under the assumption that the estimated model is not only correctly specified but that it would still hold beyond the observation period.

We project that wage losses one year out from 2020 to be relatively modest on average, generally less than one percent. Larger impacts are projected for married women than for unmarried women and for those working in COVID-impacted industries. For married women, it is more severe for younger married mothers, for younger and older married childless women, and for married mothers with older children. School closures are also important for married women with school-age children and increase negative wage impacts by 50 percent. We also find that an increase in part-year work projected to occur during the pandemic increases the size of human capital losses.

To our knowledge, the specific impact of recessions on women's human capital has not been examined in the literature within the framework of the Mincer model. There is a significant literature on the impact of mass layoffs in recessions on future earnings (the "scarring effect" of recessions), which shows those impacts to be large and long-lasting, starting with Topel (1990), Ruhm (1991), Jakubson et al. (1993) and continuing with Davis and vonWachter (2011) and many more.[3] In addition to focusing on mass layoffs rather than general unemployment as our work does, this literature usually focuses on men and mostly uses earnings as the outcome variable, while our work examines women and focuses on the hourly wage, which is a better proxy for human capital. And, as just implied, the reduced form nature of this literature is different than our work, which specifies the Mincer model framework as the mechanism through which recessions affect human capital.[4]

Our paper builds on a vast history of work on women's employment and human capital. The largest literature relative to this paper is that on the impact on women's future earnings of time spent away from work to take care of children in the home, including the literature on the contribution of that effect to the gender wage gap (Waldfogel (1997), Altonji and Blank (1999), Bertrand (2011), Goldin (2014), Olivetti and Petrongolo (2016), Blau and Kahn (2017), Cortes and Pan (2022), and many more). Whether the impact of nonemployment to engage in child-rearing has the same impact on human capital as the involuntary job loss associated with recessions is an interesting question but one beyond the scope of this paper. On the one hand, the reasons for nonemployment are very different but, on the other hand, it may be that time out of the labor force has the same impact, regardless of the cause.

The first section of the paper lays out the basics of Mincer human capital models for women and discusses how we make an important modification of that model that is more in line with the original Mincer and Polachek (1974) work. Our modified model implies that the impact of recessions on women's human capital depends on the ages at which those recessions occurred in her life history. We then describe our econometric model for capturing human capital effects solely through business cycle forces, showing that identifying the impact of business cycles on human capital is not quite as straightforward as it might seem, leading us to use a method we call Cohort IV for identification and

[3] Heathcote et al. (2020) argue that this has driven the long run trend in earnings inequality.

[4] Adda et al. (2013) estimate a structural model of how German apprenticeship programs provide protection against recessions for men by reducing human capital losses.

estimation. The next section describes the PSID sample used for the exercise and the subsequent section presents estimates of the baseline model and its projections for the impact of the pandemic recession on women's human capital. We then conduct a number of sensitivity tests and extensions to the baseline model. The final section summarizes the results and discusses its limitations.

1. Mincerian Human Capital Models

The classic model developed in Mincer (1974) derived the now-standard quadratic experience specification from an assumed human capital accumulation equation that was log-linear in investment and which assumed investment to decline linearly over time. Letting the stock of human capital be H_t at experience year t, k_{t-1} the fraction devoted to investment at time $t-1$, and r the rate of return to investment, human capital accumulates according to the process

$$lnH_t = rk_{t-1} + lnH_{t-1} \tag{1}$$

which implies that

$$lnH_t = r \sum_{\tau=1}^{\tau=t-1} k_\tau + ln(H_1) \tag{2}$$

where H_1 is the initial level of human capital when entering the labor market. Mincer assumed k_t declines over time because of rising opportunity costs and decreasing remaining years of work life, and that it declines linearly:

$$k_t = \zeta - \xi t \tag{3}$$

Then

$$lnH_t = r \sum_{\tau=1}^{\tau=t-1} (\zeta - \xi\tau) + ln(H_1) \tag{4}$$

Multiplying out the two terms in the sum gives a quadratic in t (experience).

But Mincer and Polachek (1974) immediately noted that the assumption of continuous work was inappropriate for women and replaced it with the assumption (using notation different than theirs and simplifying) that

$$rk_t = -\eta_t + \phi_t E_t \tag{5}$$

where E_t is a binary employment indicator and where t is now an indicator of potential, not actual, experience (i.e., age minus the school-leaving age). Depreciation occurs during nonwork periods at the rate η_t. The human capital accumulation function is consequently

$$lnH_t = \sum_{\tau=1}^{\tau=t-1} (-\eta_\tau + \phi_\tau E_\tau) + lnH_1 \tag{6}$$

With the market wage rate equal to H_t times the rental rate on human capital, this is in principle an estimable equation, requiring a full set of potential experience dummies and a separate coefficient on employment at each level of potential experience. While Mincer and Polachek emphasized that the parameter ϕ_t, representing the amount of human capital investment at each t, should depend on the types of jobs that women take, particularly during the child-bearing years, and on the human capital investment content of those jobs, we shall simplify and just assume a parametric quadratic profile for ϕ_t in the same spirit as the original Mincer model. A quadratic profile is consistent with a high ϕ_t at early ages, before children arrive, a low ϕ_t in the middle years when young children are present, and a higher ϕ_t at later ages after the children have grown older or left the home.[5]

With a quadratic profile for ϕ_t and an assumption that depreciation is constant, we

[5] Attanasio et al. (2008) allows human capital investment to decline linearly with age while Olivetti (2006) allows it to be quadratic in age. Olivetti also allows it to be a function of hours of work, not just whether working or not (as does Altug and Miller (1998)). We will test for whether investments are greater for full-year and part-year work to allow both hours and employment to affect wages.

have

$$\eta_\tau = \eta \tag{7}$$

$$\phi_\tau = \beta + \gamma\tau + \psi\tau^2 \tag{8}$$

which generates the wage equation

$$lnW_t = \alpha - \eta(t-1) + \beta \sum_{\tau=1}^{\tau=t-1} E_t + \gamma \sum_{\tau=1}^{\tau=t-1} \tau E_\tau + \psi \sum_{\tau=1}^{\tau=t-1} \tau^2 E_\tau \tag{9}$$

This equation yields a polynomial in total work experience only if $E_t = 1$ for all t, which does not hold for women. The impact of past work on current human capital and wages depends on when the work occurred, holding constant total years of work experience.

1.1 Identification of Experience Effects with Business Cycles

Our empirical model of the effect of experience on wages can be written in econometric terms as

$$lnW_{it} = \alpha + \eta t + \beta \sum_{\tau=1}^{\tau=t-1} EXP_{it} + \gamma \sum_{\tau=1}^{\tau=t-1} E\tilde{X}P_{i\tau} + \psi \sum_{\tau=1}^{\tau=t-1} E\tilde{\tilde{X}}P_{i\tau} + X_{it}\theta + \epsilon_{it} \tag{10}$$

for a sample $i = 1, ..., N$ observed at potential experience periods $t = 1, ..., T$ and where EXP, $E\tilde{X}P$, and $E\tilde{\tilde{X}}P$ are total, t-weighted, and t^2-weighted experience, respectively, as defined in eqn(9), and where an additional vector of conditioning characteristics X_{it} and a disturbance ϵ_{it} have been added.[6]

We wish to estimate the experience parameters, β, γ, and ψ using variation in experience induced only by business cycle variation.[7] We start by specifying a

[6]Both α and η are slightly redefined.

[7]Work experience can be endogenous for many reasons. Business cycle variation can also be viewed as an instrument to induce exogenous variation. Eckstein and Wolpin (1989) was perhaps the first to note that experience is endogenous in Mincerian wage equations for women, although they used a quadratic in total experience.

reduced-form first-stage equation

$$E^*_{i\tau} = \mu + \pi B_{i\tau} + X_{i\tau}\lambda + \nu_{i\tau} \tag{11}$$

$$E_{i\tau} = 1(E^*_{i\tau} > 0) \tag{12}$$

where $B_{i\tau}$ is a measure of the business cycle for individual i at potential experience period τ. This first-stage equation can be estimated for all τ but, for two-stage estimation of (10) at any given t, only the equations for $\tau = 1, ..., t-1$ are needed. As in textbook versions of these models, ϵ_{it} and $\nu_{i\tau}$ may be freely correlated for all t and τ but $B_{i\tau}$ is assumed to be distributed independently of all ϵ_{it}. Consistent estimates of the parameters β, γ, and ψ can be estimated by a conventional two-stage procedure.

Using only the business cycle variables $B_{i\tau}$ to identify the parameters β, γ, and ψ requires that all $X_{i\tau}$ for $\tau = 1, ..., t-1$ be included in X_{it}. But $X_{i\tau}$ in eqn(11) must include the education level, industry, occupation, marital status, numbers of ages of children, and similar variables measured at the same time as the employment decision, not at the later age when wages are measured. Including all such historical variables in eqn(10) is infeasible.[8] We shall instead hold $X_{i\tau}$ constant at their cohort values for the cohort present in each t observation in eqn(10) when predicting those lagged experience variables. We call this procedure Cohort IV and provide details later in this section.

For the X vector (both at t and all past τ), we shall first stratify by education level, taking only less-educated women, thereby incorporating schooling effects as in the conventional Mincer model (we will begin the life cycle at age 25 to assure that most women have completed their schooling). However, we shall also stratify by additional variables that are particularly important to what is known about the pandemic recession. As shown by Albanesi and Kim (2021) and Albanesi (2022), the impact of the recession on women's employment varied dramatically by marital status and the presence of children.

[8]Only education and race are time-invariant in our X vector.

Reductions in employment at the beginning of the pandemic were greatest for unmarried mothers and unmarried women without children, for example. We shall therefore completely stratify by marital status and the presence of children in the model as a whole, meaning both eqns(10) and (11)-(12). This requires the assumption that those family structure variables are exogenous, but a similar assumption is made in most of the literature on women's labor supply. Similarly we shall include the ages of children in X as those are well-known determinants of mother's labor supply and had a special role in the pandemic as well because of the closures of schools for school-age children and of child care facilities for pre-school children. In addition, an important feature of the pandemic is that the impact of the recession varied with the industrial affiliation and occupations held by women. Unlike most prior recessions, the industries experiencing the greatest declines in employment were those in entertainment and restaurants, hospitality, education, travel, etc. Women in telecommutable occupations also were less affected than women in non-telecommutable occupations. We shall therefore represent measures of COVID-affected industry and occupation in X as well.

The nature of our projection exercise is to use estimates of the model from pre-COVID data to project impacts of the 2020 pandemic recession on 2021 hourly wages. We first use the actual business cycle variables in 2020 and project the impact of those variables on women's work experience and then on their 2021 wage rates. We then repeat the exercise assuming that the 2020 business cycle had instead remained at its 2019 level. The difference represents the impact of the pandemic on women's human capital one year out. We extend the exercise to impacts on 2022 wage rates in an additional exercise.

2. Data and Procedures

We select women in the Panel Study on Income Dynamics (PSID) who were 25-54 in 1990 or after. The latest release we use is 2017, and is therefore the final year of data. We begin at 1990 because that allows us to trace the histories of employment and family structure of almost all women in our sample back to age 25 (1968 is the first year of the PSID), the first year of the PSID. We begin at age 25 because most women had completed school by that age, and stop at age 54, before retirement has a major impact on employment. We construct a time-consistent measure of years of education across all interviews by reconciling small reporting discrepancies and we select women with less than a college degree for our analysis. Hourly wages are computed as last year's annual earnings divided by last year's annual hours worked, which we put into real 2010 PCE dollars. Our sample size of hourly wage observations is 13,341 pooled over women and years, with 1,839 unique women and an average of 7.25 years per woman.[9] Potential years of experience, t, is measured as age minus 25. We describe the construction of the three experience variables needed to estimate eqn(10) below.

As noted previously, following the work of Albanesi and Kim (2021) on COVID and much prior work on women's labor supply, we stratify the sample by marital status and the presence of children, leading to four separate estimation groups (married and unmarried, with children in the household and without). We therefore assume family structure to be driven by exogenous forces. We measure these family structure variables as of the interview date. We construct variables for the age of the youngest child for women with children and also a variable for the husband's annual earnings for married women, which we allow to affect the wife's employment.[10] We add a race variable for nonwhite women in our regressions.

For the first-stage employment equation in eqns(11)-(12), we define an employment indicator for whether the woman worked more than 1600 hours per year, chosen to proxy full-year work which, based on past work showing low wage payoffs to part-time work, we

[9] After 1997, the PSID went to every-other-year interviewing. Our sample of wage observations includes only every other year after 1997. The PSID interview does ask earnings questions two years back but, after an inspection of those reported values and discovering anomalous values inconsistent with the values at the prior interview, we chose not to use them.

[10] To address potential endogeneity of husbands' earnings, we estimate what we call a zeroth stage equation for that earnings variable which includes all variables in the women's employment equation–including the same business cycle variables–plus the husband's years of education, for identification.

expect to have more of a human capital impact. But we conduct sensitivity tests to this definition as well as also estimating models with part-year and full-year work distinguished. We estimate the equation on all observations with a valid employment variable in the year in question.[11] We have 36,271 observations on employment pooled over years and women. Our annualized employment rate averages a little over 50 percent.

For the business cycle variables in the first-stage equation, we use four variables, three of which are state-specific. The four are the state unemployment rate, the log of total state employment per capita, the log of state employment per capita in COVID-impacted industries, and a binary indicator for a national recession year, where the fourth is defined as equal to one if the majority of the year was in an NBER-defined recession. COVID-impacted industries are those shown in other work to have been heavily affected by COVID, including working in leisure and hospitality, transportation and utilities, other services, wholesale and retail trade, and education and health. While we do not enter year fixed effects for sample size reasons, we enter a year trend, implying that the effects of the first three variables will arise from cross-state variation (although also from nonlinear time-series trends).

Figure 1 shows the time series pattern of our business cycle variables, using national averages for the three state-specific variables and with recession years noted by shading (the two employment variables are measured as deviations from trend). All four variables are correlated in the expected way, but the exact patterns differ for each. The unemployment rate and employment variables have variation that does not exactly coincide with NBER recession years, and they are not perfectly aligned with each other. Using all four consequently may pick up additional variation. Of particular interest are the COVID and total employment variables which, while highly positive correlated, vary in their

[11] We include all valid observations back to 1968 for those included in our sample. We impute employment status and the regressor variables in the employment equation for the missing every-other-year after 1997, using the values on either side of the missing year (see prior footnote on the intervening year earnings data), and for random missing values, using the nearby years for the imputation. See Appendix B for details.

relationship over time.[12] This variation (albeit at the state level in our regressions) allows us to estimate the separate impacts of COVID industry employment and total employment on women's individual employment outcomes.

We use reported industry and occupation to construct three COVID-related variables for each woman for the employment equation. The first is whether she worked in a COVID-impacted industry, which we define using PSID codes for the same industries noted above. We also use PSID occupation codes to create an indicator for working in a telecommutable occupation and in a high-contact occupation, drawing on the occupations identified as with more than 25 percent commutable jobs and high-contact occupations by Alon et al. (2020a) and Albanesi and Kim (2021). We interact these industry and occupation variables with the business cycle variables to determine whether the effect of business cycles depends on whether a woman is in a COVID-related industry or occupation.

With the employment equation estimated, we estimate the second-stage log wage equation in eqn(10) using predicted experience variables over each woman's history back to age 25, using the values of each of the variables in the employment equation in each year of her lifetime for the prediction.[13] We apply what we call Cohort IV by predicting those variables using age-year-family-structure-specific cohort means of all variables in the employment equation at each previous age except the four business cycle variables, for which we use actual values at each past age for each woman. The predicted employment histories will consequently vary across women only from different histories of the business cycle variables conditional on year, age, and family structure, all of which are in the wage equation.[14]

We enter a traditional selection bias term in the wage equation because the sample

[12]Because COVID employment is necessarily smaller in magnitude than total employment, the magnitude of its cyclical variation is smaller than that of total employment.

[13]Because a few of the women in the sample have left-censored histories because they entered the sample after age 25, we impute all regressors in the employment equation back to 25 when missing (see Appendix B).

[14]The lifetime histories of women of a particular family structure at time age t include many periods when the woman experienced a different family structure.

only contains working women. The selection bias term is identified from current values of the business cycle variables, which affect the current probability of working and do not affect the experience variables.[15] We jointly bootstrap all three equations (zeroth stage, first stage, and second stage) to obtain standard errors.

Appendix Tables A1 and A2 show the means of the variables used in the analysis.

3. Results

The first stage estimates of the employment equation are shown in Table 1.[16] The business cycle variables generally have expected signs but vary in significance across family structure groups, with married women more sensitive to the total employment level and the recession indicator and with unmarried women more sensitive to the unemployment rate. Because an increase in unemployment conditional on total employment represents a decrease in the size of the labor force, these findings may indicate that unmarried women tend to decrease employment by withdrawing from the labor force compared to married women. The negative effect of total employment for unmarried mothers will be shown below to result from an increase in part-year employment and a decrease in full-year employment.

The effect of COVID employment is negative for three of the four family structure groups but, because total employment appears elsewhere in the regression, must be interpreted as the effect of increasing employment in COVID sectors but simultaneously decreasing it in non-COVID sectors. That the net effect is often negative implies that more women are affected by the latter than by the former. To differentiate these effects, we interact the COVID employment variable with whether the woman herself is in a COVID-impacted industry. The interacted coefficient is positive for three of the family structure groups (and negative but insignificant in the fourth), consistent with the

[15] Keane et al. (1988) showed that selection bias varies over the business cycle so it is important to address selection bias.

[16] The "zeroth stage" estimates for spousal annual earnings are shown in Appendix Table A3. The spousal earnings variable in Table 1 is predicted from that equation. We should also note that we use OLS rather than probit in this first stage for simplicity of interpretation of the estimates.

interpretation that an increase in aggregate COVID-industry employment in the state has a greater effect on women who are in affected industries. This necessarily means that a decrease in employment in those specific industries also has a more negative effect on those who are in the industries, which will affect our COVID projections.

We tested a number of additional interactions of the four business cycle variables with other variables in the regression in a variety of specifications. Table 1 shows only those which were consistently statistically significant at conventional levels for women of at least one family structure or which are of independent interest. Increases in total state employment has less of a positive impact on women in telecommutable occupations but at low levels of significance, less of a positive effect for nonwhite married women relative to white women (again at low levels of significance), and more likely a positive effect on unmarried mothers with older children (relative to the omitted category of having a child 0-5) but less of a positive effect on married mothers with older children. The lack of a significant impact of telecommutable occupations may simply be a result of those occupations not having been telecommutable historically, so we conduct a sensitivity test to this below. An increase in state COVID employment has less of an impact on mothers with older children. The unemployment rate has a greater negative effect on the employment of married mothers with older children.

Estimates of the second-stage log wage equation appear in Table 2.[17] The positive, negative, and positive signs on the first, second, and third experience variables for three of the family structure groups are consistent with the U-shaped profile hypothesized above. While the individual coefficients are generally insignificant at conventional levels, they are jointly highly significant. However, unmarried mothers have no significant curvature in their experience profiles.[18]

Figure 2 shows the implied estimated returns to one additional year of potential

[17]Estimates of COVID wage impacts without the inverse Mills ratio will be given below.
[18]Estimates of wage impacts without the third experience variable and hence assuming linear profiles are given below.

experience by family structure group. In the model specification used here, the return to an additional year does not depend on total years of prior experience but on t, potential years of experience (see eqn(9)). The figure shows that estimated rates of return are not too far from linearity but almost all exhibit at least some second-order curvature. Returns are highest for married mothers but decline relatively steeply with potential experience. The interpretation given by the Mincer model is that their human capital investments decline with age. Recalling that these effects are those induced by business cycle variation in job holding, this implies that job losses and reductions in experience (relative to a counterfactual of no losses) resulting from cyclical downturns will have the largest negative effects on married mothers at young ages and much smaller impacts on those at older ages, when investments are small. Married childless women have slightly downward sloping profiles but with average returns about the same as those for married mothers. Returns are also greater at older ages for childless married women than for married mothers, implying greater investments at those ages, suggesting that business cycle impacts will have greater negative effects for older childless married women than for older married mothers (the former are, in fact, older on average than the latter; see Appendix Table A2). Much lower returns are found for unmarried women, especially unmarried mothers, whose returns are particularly flat. Unmarried mothers often have jobs with very little human capital accumulation content at all ages and hence should have wage losses from cyclical downturns that are consequently small. Unmarried childless women, who also tend to be older than mothers, have low returns at older ages (i.e., smaller investments) and should be expected to have smaller wage impacts from recessions at those ages as well, but somewhat greater losses at younger ages.

We project the impact of the 2020 pandemic recession on 2021 wages by first predicting the employment impact of that recession from Table 1, using the 2020 values of the business cycle variables and then using those to estimate the impact on wages from the resulting changes in employment from Table 2. For the counterfactual impact, we repeat the exercise using the 2019 values of the business cycle values, to project wage levels that would have occurred if the business cycle had stayed at its 2019 level in 2020.

The results are shown in Table 3. The largest negative impacts on wages are those for the two groups of married women, but both are less than one percent. Those for unmarried women are much smaller. For unmarried mothers, this is a result of the very low rate of return to experience shown in Figure 2, and occurs despite unmarried mothers having the largest employment losses. Unmarried childless women have slightly greater rates of return than unmarried mothers but also smaller employment losses.[19] These effects are those at the mean of all the variables in the model (especially including mean ages–see Figure 2 again), so we next discuss heterogeneity in impacts.

Heterogeneity. The modest mean market wage impacts we project mask significant heterogeneity. Figure 3 shows the distribution of percent wage impacts across the sample for all four family structure groups. Married mothers have a wide spread of impacts, with a left tail of impacts between 1 and 2 percent. Married childless women have a non-trivial dispersion as well, with a left tail over 1 percent, but with less dispersion than that for married mothers. Unmarried women have the smallest heterogeneity, with effects relatively concentrated around the mean.

Much of this heterogeneity arises simply because women live in different states with different magnitudes of business cycle downturns. Figure 4 shows impacts for women with employment effects below the median in the sample (hence, more negative changes in employment) compared to those living in states with smaller magnitudes of downturns. For married women, wage declines are almost twice as large for those living in higher downturn states than those living in smaller downturn states. As expected from our results on the impact of downturns on market wages working through employment impacts, there is much less difference for unmarried women. Figure 5 shows impacts specifically for women living

[19]Appendix Tables A5 and A6 show estimated wage equations and wage rate impacts with a linear specification for experience. At the mean, the wage impacts are about the same as in Table 3. But away from the mean, there are larger differences.

in states with larger or smaller declines in COVID-industry employment, again showing larger negative impacts on wage rates for married women living in states with larger declines in those industries.

Living in a state with a large increase in the unemployment rate is also projected to have a large impact on market wage declines. Figure 6 shows projections by whether the state unemployment rate in 2020 rose more than the median or less. Wage losses are in many cases almost double those for women living in states with smaller increases in unemployment. We find for this projection that impacts also occur for unmarried childless women, not just married women.

Heterogeneity by age is shown in Figure 7. For married mothers, the projected negative impacts of the pandemic on married mothers' market wage rates are largest for married mothers at young ages, and quite small for older women. Human capital investment is highest at younger ages and this is responsible for their larger recession-induced losses. The effects for married childless women are quadratic, higher at younger and older ages than in the middle ages. As noted above in connection with Figure 2, investment is high at older ages for married childless women and not just at younger ages. The impacts for unmarried women are small at all ages.[20]

The negative impact of the pandemic on the market wage rates of women working in industries hard hit by COVID is projected to be larger than for those working in other industries, as shown in Figure 8. The difference is largest for married childless women, somewhat smaller for married childless women, quite small for unmarried childless women, and essentially zero for unmarried mothers. These results are consistent with the pattern of impacts already discussed. About 60 percent of women in all four family structure groups work in COVID-hit industries (Table A2), so the larger impacts for those working in such industries pushes up mean impacts.

Figure 9 shows projections of wage impacts of the pandemic by the age of the

[20]The positive impacts for older unmarried mothers are the result of negative rates of return at high ages, which we regard as extrapolations resulting from our functional form assumption.

youngest child for women with children. The negative impacts are greatest for married women with older children and near-zero for unmarried mothers. The result for married mothers with older children was suggested by the first-stage estimate of employment impacts for married women, and is likely a result of the higher initial employment rates for those women than for women with younger children. Married mothers with older children have employment rates 15 percentage points above those of women with the youngest children, and our projected negative impacts of the pandemic for those with older children are double those of women with the youngest children.

Child Care and School Closures. There has been much discussion of the impact of school closures and closing of child care facilities on mother's employment, as noted in the Introduction.[21] Neither is easily captured historically with the PSID. While the PSID does ask questions about the use of child care, showing that about one-quarter of mothers use it, a proper model capable of projecting the impact of pandemic child care closures would require modeling the historical availability of child care to PSID mothers in their locations (so that reductions in that availability could be estimated), which is beyond the scope of this project. The impact of school closures is even less capable of being captured historically, as the closing of schools has not occurred on any scale in the recent past.

To project the impact of child care closures for pre-school children and the impact of school closures, we draw on the recent literature on the causal impacts of those events on maternal employment in the pandemic (Heggeness (2020),Russell and Sun (2020),Garcia and Cowan (2022),Hansen et al. (2022)). For the impact of child care closures on mothers of children 0-5, we draw on Russell and Sun (2020) who found mandatory child care closures by September 2021 to have reduced mothers' employment by 2 percentage points.[22] The authors found no differential impacts by education but did not disaggregate

[21]See also Zamarro and Prados (2021) for a study of child care duties assumed by married mothers in the early months of the pandemic.

[22]The authors only estimated the impact of closures on unemployment but they found no impact on labor force participation. With an assumed fixed labor force participation rate of .75, the increase in the unemployment rate of .027 found by the authors corresponds to a .02 decrease in the employment rate.

by marital status, so we assume the impact to be the same for married and unmarried women. Child care closures were more widespread than mandatory closures, so we conduct sensitivity tests using larger impacts of closures in the next section.

For school closures, we draw on Hansen et al. (2022) who studied the impact of school reopenings from May 2020 to September 2021, finding them to increase married mothers' employment by 2.4 percentage points for low-educated mothers of children 6-11 and 4.5 percentage points for low-educated married mothers of children 12-17. They found no effects on unmarried mothers. The latter is not entirely plausible, so we again conduct sensitivity tests to this result in the next section. Heggeness (2020) found larger impacts but only for women pooled by education and marital status. We model only school closings and assume they occurred in 2020 and had the opposite effect as school reopenings.

Table 3(b) shows the incremental, additional projected impacts of child care and school closures on experience and on market wages. The experience impacts are weighted averages of impacts for women with children of different ages. The wage impacts for married mothers are only one-fifth of a percentage point, but this represents almost a 50 percent increase in the impact from the business cycle alone, as indicated in panel (a). In relative terms, the impact is therefore sizable and, assuming these effects apply to married mothers of all types, implies that the heterogeneous effects discussed above might be 50 percent higher. For unmarried women, however, the impact is negligible both because we assume no employment effects of school closures for them and because of the low impact of job losses on wages for those women already discussed. Figure 10 shows the impacts by age of the youngest child, showing greater impacts for mothers of older children, consistent with our discussion above of Figure 9.

4. Sensitivity Tests and Extensions

4.1 Sensitivity Tests

Hours Cutoff. We conduct a number of sensitivity tests to the baseline specification reported in the previous section. All results of the tests are reported in Table 4. First, we test the sensitivity of the results to the 1600-hour annual cutoff for full-year employment. A woman working 40 hours a week would be at that cutoff if she worked 40 weeks in the year and spent 12 weeks not working. It is possible that women not working for 12 weeks in the year could have smaller reductions in employment and hence wage impacts from a recession. We shall test a cutoff of 1800 hours as a rough way to test this possibility (the employment rate drops from about 50 percent to 30 percent when we use this higher cutoff). On the other hand, counting only 50 percent of women as employed leaves out a number of women who have substantial commitment to the labor force, whose employment could easily also be affected by downturns. We therefore also test a threshold of 1400 hours to assess the sensitivity of our results to this lower cutoff as well.[23]

The first row of Table 4 shows the baseline estimates using the 1600-hours cutoff and the second and third rows show estimates using 1800 and 1400, respectively. The negative wage impacts are substantively greater for the 1800 hours cutoff for women of all family structures, even the group of unmarried mothers whose effects have been effectively zero thus far (they have greater returns to work experience at the higher level). This supports the hypothesis that women working longer hours in the year, at least at the top of that distribution, are making larger human capital investments and hence suffer greater losses of human capital from recessions. Interestingly, for three of the four family structure groups, using a 1400-hour cutoff also increases negative wage impacts, although by only a small amount for married women. The lower hours cutoff begins to approach what many would

[23] Appendix Figure A1 shows the distribution of annual hours for women of different family structure. Depending on the family structure category, about 10 to 15 percent of women fall into the 1400-to-1800 interval.

characterize as the part-year range, so we revisit this topic again below in a more direct examination of part-year work and offer an explanation for this finding.

Occupation Effects. Our baseline results show that increases (decreases) in state employment have less of a positive (negative) impact on employment for women in telecommutable occupations but at low levels of significance. As a sensitivity test based on fairly arbitrary grounds given the lack of evidence, we increase the size of the interaction coefficients between employment and telecommutable occupation by 10 percent to gauge the sensitivity of mean wage impacts to this factor.[24] With 55 percent of the sample in telecommutable occupations (Appendix Table A1), this is a reasonable test of whether major changes in wage rate effects would occur. However, we also arbitrarily set the coefficient on an interaction between state employment and being in a high contact occupation to +.10, in the same range as those for the telecommutable occupations. This will increase negative impacts of a recession on women who are in such occupations. However, only 19 percent of women are in these occupations, so the impact should be smaller overall.

The fourth row of Table 4 shows the results. The net effect of the telecommutable and high contact adjustments is to make the negative wage effects smaller than in the baseline for all women except married mothers, for whom the impacts actually increase (because, for them, the high contact effect outweighs the telecommutable effect). The reductions in wage effects are small for all except married childless women, who would see a halving of their negative wage effects from this change.

Child Care and School Closures. We noted two limitations in the past work on child care closures and school closures we used to estimate their impacts on employment of mothers during COVID. One was that the study of child care closures only used information on government mandatory closures, which likely underestimates the impact since many child care closures were voluntary. To gauge the sensitivity of the estimates in

[24] We set the interaction coefficient for unmarried childless women at -.10, about the mean for the other three groups.

this respect, we increase the impact of child care closures for mothers whose youngest child is 0-to-5 by 50 percent. The other limitation was the finding that unmarried mothers were not affected by school closures at all. As a sensitivity test, we use estimates for single mothers' employment response to school closures from Garcia and Cowan (2022), who found a negative and significant impact for that group.

Table 4 shows the baseline impacts of child care and school closures in row 5 and the new set of estimates for the row entitled "More Childcare Closures." Negative impacts on mothers' wage rates increase but by only a small amount. In the rows thereafter, we break the impacts down by age of child. Here we see that the only detectable changes are those for children 0-5. Based on these results, we conclude that reasonably modest deviations of our baseline projections of the impact of child care and school closures would not significantly affect our baseline estimates of human capital losses for mothers.

Selection Bias Adjustments. We conduct three sensitivity tests to our estimates of selection bias adjustments. First, we simply drop the selection bias adjustment completely and recompute all projected employment and wage impacts. Appendix Table A7 shows the estimated wage impacts. The negative wage impacts are not very different except for married mothers, which is consistent with their larger impacts of selection bias in the wage equation (see Table 2). For married mothers, the strong evidence of selection bias makes our baseline results more credible. Second, given the well-known dependence of the canonical inverse Mills ratio on the assumption of bivariate normality, we test instead the nonparametric version suggested by Newey (2009) which simply uses a polynomial in the predicted employment probability for the selection bias adjustment. As the penultimate row in Table 4 reveals, wage projections from the pandemic are only slightly lowered. Finally, we test whether selection bias has changed over time by interacting the inverse Mills ratio in the wage equation with calendar year.[25] The interaction coefficient (not shown) indicates that selection among married women has indeed changed over time, as

[25]We thank Stefania Albanesi for this suggestion.

selection into work has been increasingly positively correlated with the wage residual. However, as the last row in Table 4 shows, this change has little impact on our wage projections.

4.2 Part-Year Work

Our baseline analysis attempted to restrict the analysis to the impact of COVID on wages of near-full-year workers. Our sensitivity reported in the last section indicated that an even tighter definition of full-year work produces somewhat larger negative wage impacts, supporting the presumption of greater impacts. But the impact of recessions on part-year work, and consequent projections of the impact of COVID which includes such effects, is of independent and important interest.

As a way to determine whether including part-year work in the analysis makes any difference, we define part-year work as having annual hours between 300 and 1600. We keep our definition of full-year work as 1600 or over and redefine nonwork as working less than 300 hours. Following the same method as in the baseline analysis, we estimate first stage equations for part-year and full-year work separately, as a function of the same business cycle and other variables as in the baseline. We then reestimate our wage equations with one additional variable, which is total years of part-year work over the lifetime as of time $t-1$ (year before the wage observation). We assume that, if part-year work has any impact at all, it will show up in that form, which assumes constant investment per year.

The first stage part-year and full-year employment equations are shown in Appendix Tables A8 and A9. The full-year equations in Table A8 show impacts that are approximately the same as those in the baseline but, interesting, with coefficients usually larger in magnitude and statistical significance. The coefficients on the second experience term are particularly large, suggesting a faster decline in returns with age than before. This by itself suggests that nonworkers and part-year workers must be responding differently to recessions; if they were responding identically, it would not matter for full-year effects whether those other two categories were pooled. Table A9 shows that part-year employment responds differently to the business cycle than full-year work, and sometimes in the opposite direction. For example, increases in the unemployment rate typically increase the probability of part-year work, and even recessions tend to increase it as well (although not with high degrees of statistical precision). Many of the positive effects of employment on part-year work are also smaller in magnitude than for full-year work. This suggests that recessions may result in movements from full-year work to part-year work, which would not be terribly surprising.

Table 5 shows estimates of the wage equation with total years of part-year work experience added. For all family structure groups except unmarried childless women, increases in part-year work decrease, not increase, wage rates. The effects of full-year experience are stronger than in the baseline. Figure 11 shows the rates of return for part-year work along with those of full-year work and makes the magnitude of the difference clear visually. Table 6 shows projected wage losses from the pandemic for our model including part-year work. Part-year employment increases and full-year work decreases. While married women experience smaller wage losses than before, unmarried women now experience much larger losses, in the same or nearby range to those of married women. Even unmarried women experience nonzero wage losses as a result of their large negative returns to part-year work.

4.3 Industry and Occupation Experience

The traditional Mincerian framework which we employ is a model of general labor market experience. Since the development of the Mincer model, the importance of industry-specific and occupation-specific experience has been recognized (Shaw (1984),Neal (1995),Kambourov and Manovskii (2009)). While a construction of industry and occupation experience over all years of the PSID is beyond the scope of our analysis, we can test whether returns to experience in the wage equation are affected by working in

COVID-impacted industries or occupations. We report the results of such a limited examination in this subsection.

Table 7 reports the results of reestimating the wage equation allowing interactions between the first experience term (total years of experience) and working in a hard-hit COVID industry, being in a telecommutable occupation, and being in a high-contact occupation (Appendix Table A10 shows estimates of the wage equation). To the extent that the experience variable mostly reflects experience in the industry or occupation in which they are currently employed, the interactions will capture industry and occupation experience. The coefficient estimates for unmarried mothers are near-zero, indicating no difference in experience impacts by industry or occupation, while some of those for married women were significant in magnitude and statistical strength. The projected pandemic losses in human capital for those interactions which are significant in both senses are shown in Table 7. Interestingly, impacts both for those in COVID-impacted and COVID-non-impacted industries, for those in both telecommutable and non-telecommutable occupations, and for those in high-contact and low-contact occupations, are larger than the averages previously reported in Table 3. The impacts for married childless women are particularly large. We take these findings to suggest that industry- and occupation-specific experience are likely to be important and result in larger losses in human capital from recessions than general labor market experience.

4.4 Industry and Occupational Mobility

The COVID pandemic recession has been characterized by relatively high rates of industry and occupational mobility. The role of that mobility in presumably moderating wage and human capital losses is a question of interest.[26] A complete analysis of this question would require estimating a model of the joint movements of wages, industry, and occupation, with all three endogenously and jointly chosen from the offer distributions of each type. We

[26] The authors thank Joseph Altonji for suggesting this topic.

pursue a far less ambitious exercise by simply estimating the rate of industry and occupational mobility that has occurred over last recessions, and relating that to the same business cycle indicators, COVID industry impacts, and COVID-related individual industry and occupation status of the individual. We then follow the same counterfactual exercise as we have conducted for wages to project the impact of the pandemic recession on occupation and industry mobility. We leave the role of this mobility in moderating wage declines to future work.

To this end we use 3-digit PSID occupations to create 25 unique occupations which exhaust the space of possibilities and allow us to place all women into one group. We work from 2-digit industries to create, similarly, 13 unique industries. We then use our panel of PSID women to code occupation and industry changes as a binary indicator for change from each $t-1$ to $t+1$ as a function of business cycle variables at time t. In addition to business cycle variables, we include all the variables we include in the first-stage employment equations we have estimated for our baseline and extended specifications.

Appendix Tables A11 to A13 show the estimates of equations for occupational change, industry change, or both simultaneously. The results show mixed evidence of the role of business cycles. The unemployment rate usually (but not always) increases the probability of a change for most family structure groups. A recession often has no strong effect and, when it is strong, it is just as often negative as positive. Total state employment, on the other hand, generally has positive effects on mobility. COVID state employment (when occurring without a reduction in non-COVID employment) usually has a positive impact on mobility but smaller than that for total employment. Women working in COVID-impact industries usually have much smaller probabilities of occupation or industry change although sometimes insignificant or the opposite sign. Having children reduces the probability of change for married mothers and increases it for unmarried mothers.

Table 8 shows the results of our usual projection exercise plugging in actual 2020 business cycle variables and comparing to results using 2019 variables. Interestingly, for married women, we project declines, not increases in occupational and industry mobility. The net impact of the mixed results in the change equations just described is that mobility is higher during expansionary periods of the economy and lower during contractionary periods. However, the results are different for unmarried women, where mobility is projected to increase slightly. Nevertheless, all effects are small and there is no indication from this analysis of a large mobility impact of past economic downturns.

4.5 2022 Wage Impacts

Making long-run projections of wage impacts from our model would not be particularly interesting because we would have to make assumptions about the course of the business cycle into the future. However, we do have actual 2021 business cycle values and can make projections to 2022 with our current model. The 2021 period was an expansionary period of the economy but the labor market had still not recovered to its 2019 levels. The nature of the Mincer model implies that, if the counterfactual remains what would have happened to women's employment and wages if the recession had never occurred, and if 2019 business cycle levels had persisted into 2021 as well as 2020, then the wage and human capital losses from the pandemic recession have to grow larger. However, we should expect the additional decline of wages to be smaller than that which resulted from the 2020 downturn since the labor market had partially recovered.

The results of this simple exercise are shown in Table 9. As just described, we first project women's employment in 2020 assuming business cycle variables had remained at their 2019 values, and the consequent increases in work experience. We then increment work experience by the estimated additional work experience that would have occurred if 2019 business cycle levels had persisted into 2021. We project 2022 hourly wage rates for those levels of experience. We then first use actual 2020 business cycle levels and then actual 2021 levels and similarly calculate implied 2022 wage rates. The differences, reported in Table 9, are slightly greater than those projected for 2021 and shown in Table

3. For married women, wage losses are about one-third greater while, for unmarried women, with their lower returns to experience, the increase is very small. Continued recovery of the labor market should consequently be expected to stabilize these losses.

5. Summary and Conclusions

We have reported the results of an exercise to project human capital losses for women from the pandemic recession one year out from 2020, the worst year of the downturn, as well as extended estimates for 2022. We use historical variation in business cycles to estimate employment impacts and we use a modified form of the Mincer model to translate those losses of experience into losses of human capital. We also use outside estimates to project additional losses of employment and human capital from school and child-care facility closures. We find that wage losses one year out from 2020 are relatively modest on average, generally less than one percent, but larger for married women than for unmarried women and for those working in COVID-impacted industries. For married women, it is more severe for younger married mothers, for younger and older married childless women, and for married mothers with older children. School closures are also important for married women with school-age children and increase negative wage impacts by 50 percent. An increase in part-year work projected to occur during the pandemic increases the size of human capital losses. Projections to 2022 show small increases in wage losses for married women.

While much of the response to the pandemic can be captured historically or from studies of specific pandemic factors (like school closures and child care closures), there is much that cannot be captured. Perhaps the most important are the declines in employment resulting from the risk of contracting COVID, for such health factors in employment decline have not occurred in recent history. This factor is likely to have resulted in greater losses relative to the assumed counterfactual estimates used here. In this sense the human capital losses estimated here may constitute a lower bound. The increase in working from home is another factor not easily captured by historical patterns, but that factor has been shown to be much less important for the less educated women studied here than for more educated women. In addition, the sensitivity tests we conduct to the magnitude of effects from an increase in ability to work from home suggest that it may not reduce human capital losses by a large amount. It is consequently arguable that it would not have a major impact on our projections. In addition, as we note in our paper, the course of future human capital losses will depend on the future course of the labor market for women, which cannot be known at this time.

Looking ahead, studies of wage losses using actual data from the pandemic and in its succeeding years, when those data come available, are likely to be difficult. The impact on future wages of the 2020 downturn by itself, for example, will have to address the continued recovery in 2021 and beyond, and the ever-shifting labor market landscape as the market changes from a labor surplus market to an excess demand market. The impact of the pandemic on work from home may also have independent effects difficult to separate from the pure effects of the 2020 downturn by itself. Any causal analysis will necessarily require valuing counterfactuals whose estimation is likely to pose significant challenges.

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Figures and Tables

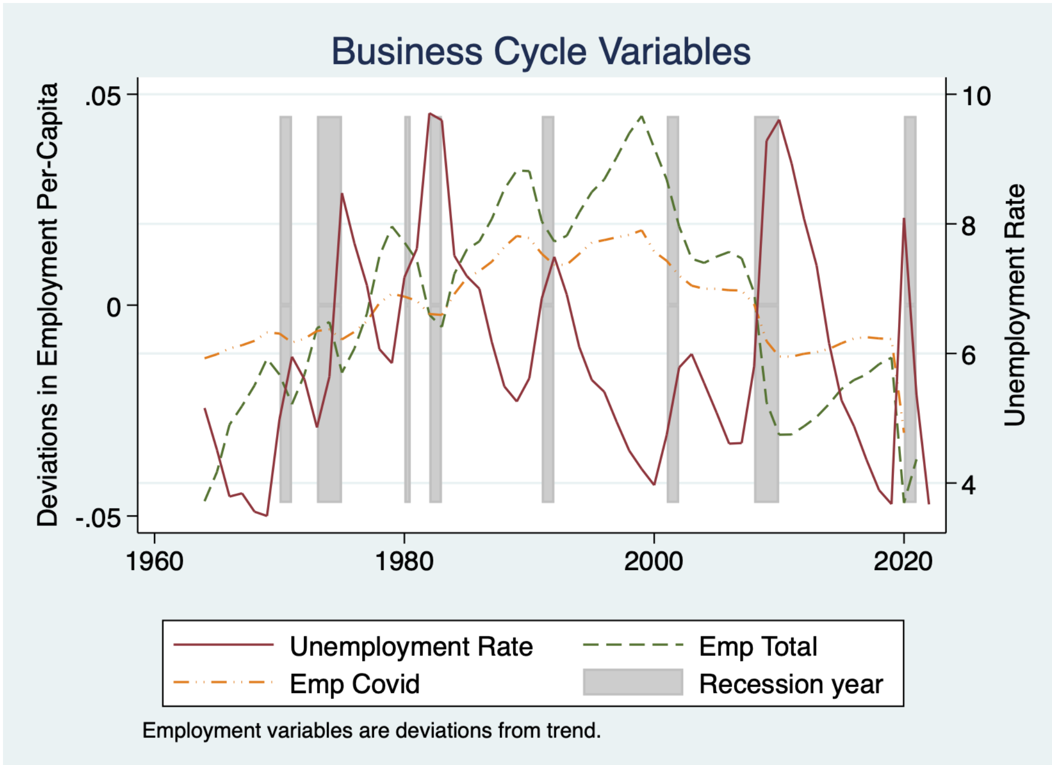

Figure 1: National Business Cycle Variables

Data Source: US Bureau of Labor Statistics

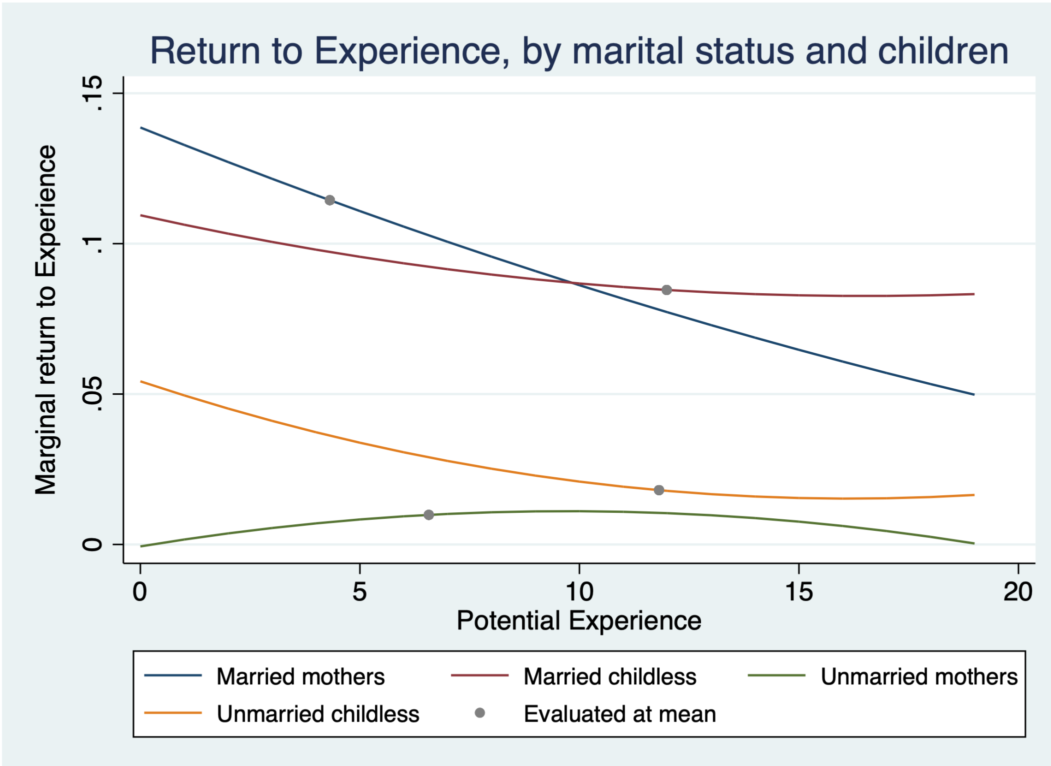

Figure 2: Rate of Return to 1 additional year of Experience

Notes: The return of return is calculated as the additional wage that would be received by choosing to work last period, all else equal.

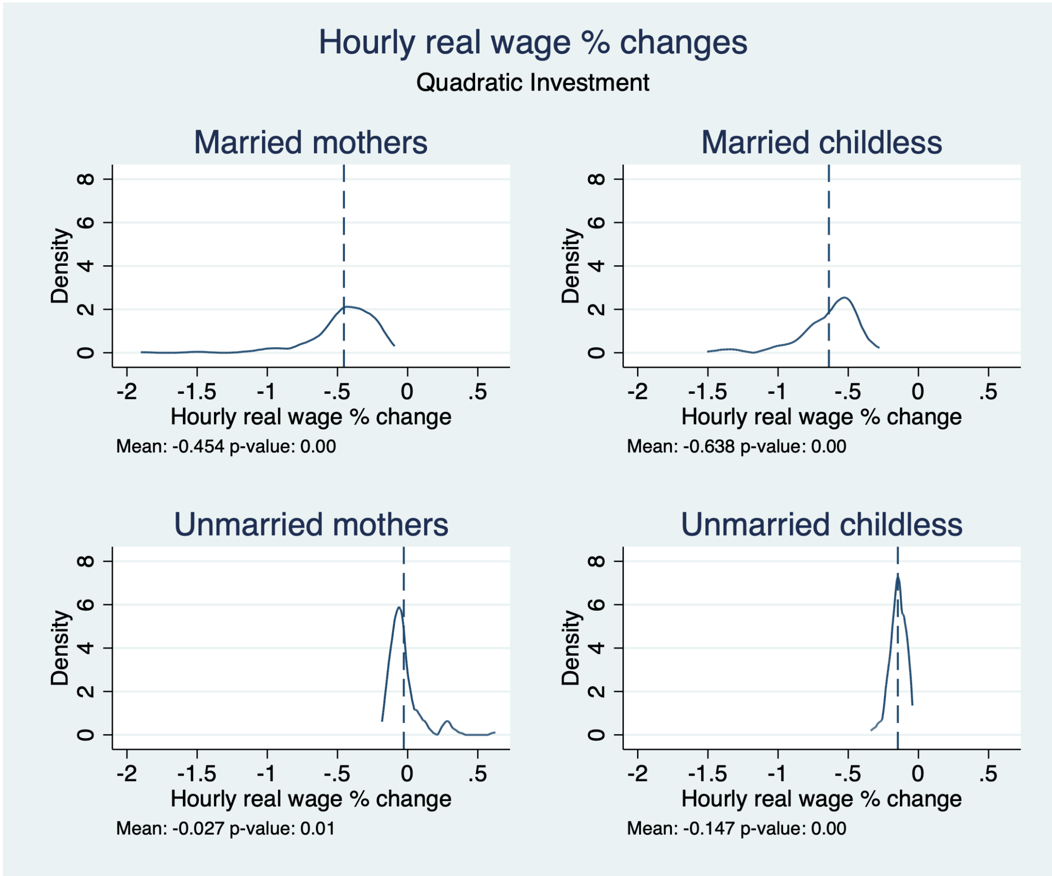

Figure 3: Distributions of Wage Effects

Notes: Vertical dotted lines denote the mean of the distribution.

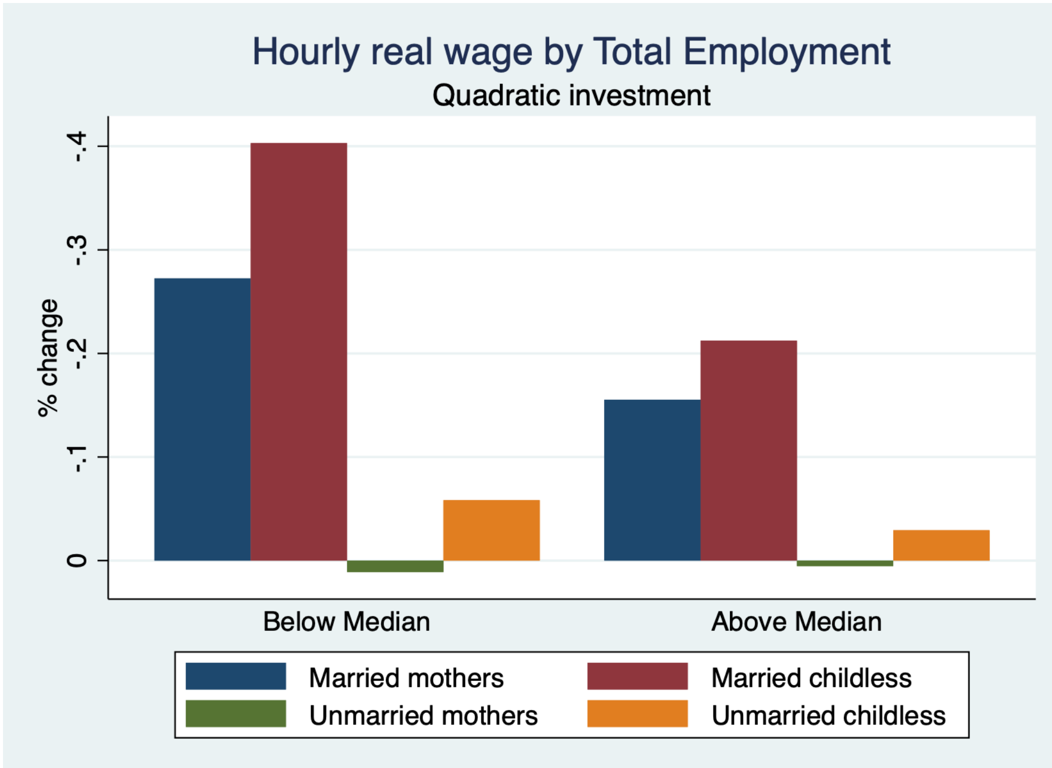

Figure 4: Pure Effects: Total Employment

Notes: Since employment in industries hard-hit by COVID is held fixed, the decrease in total employment comes only through industries that were not hard it by COVID.

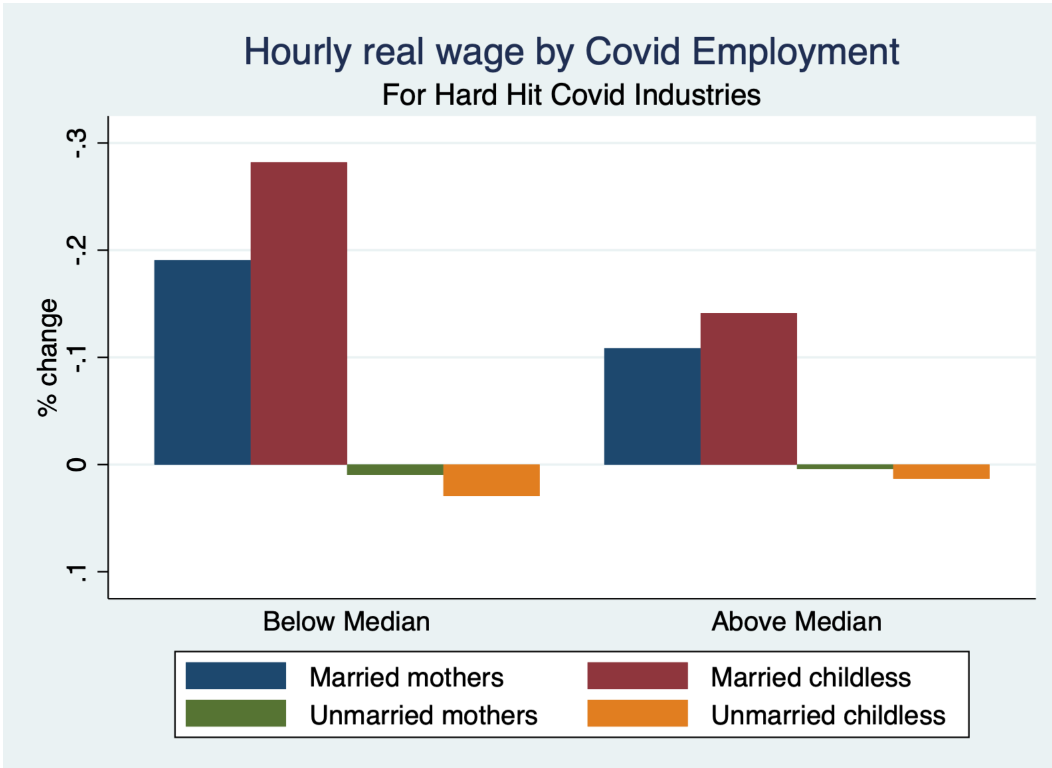

Figure 5: Pure Effects: Employment in COVID Industries

Notes: In order to hold fixed the employment in industries that were not hard hit by COVID, total employment must decrease by the same amount as the employment in the hard-hit COVID industries.

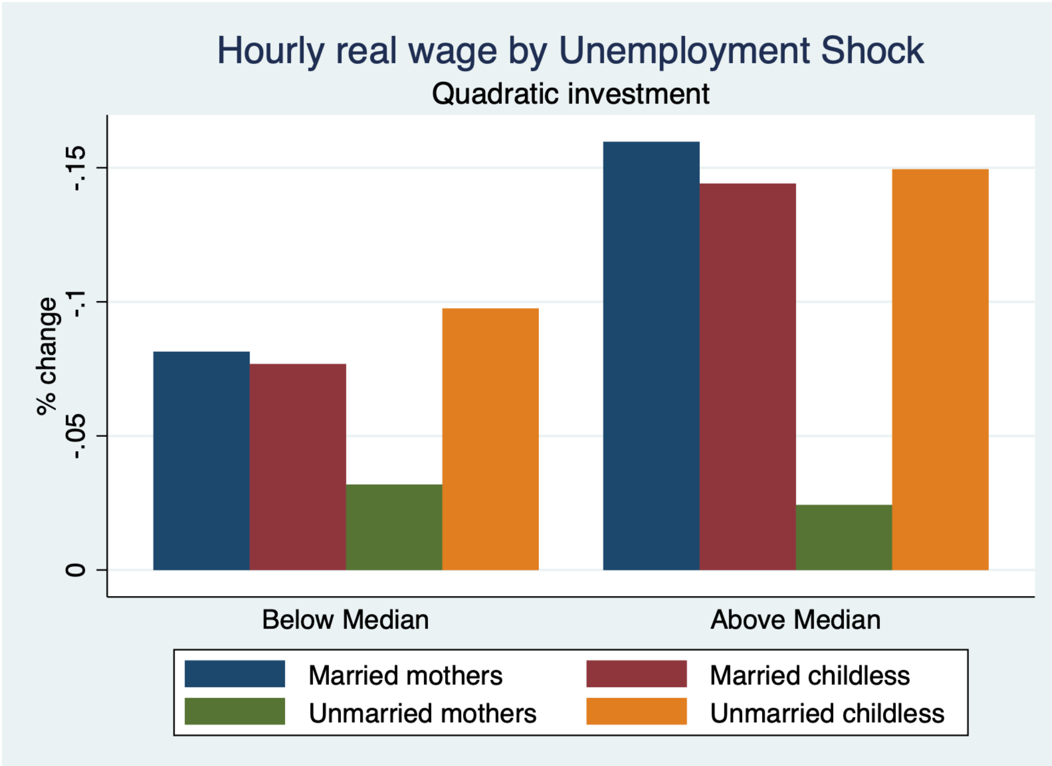

Figure 6: Pure Effects: Unemployment Rate

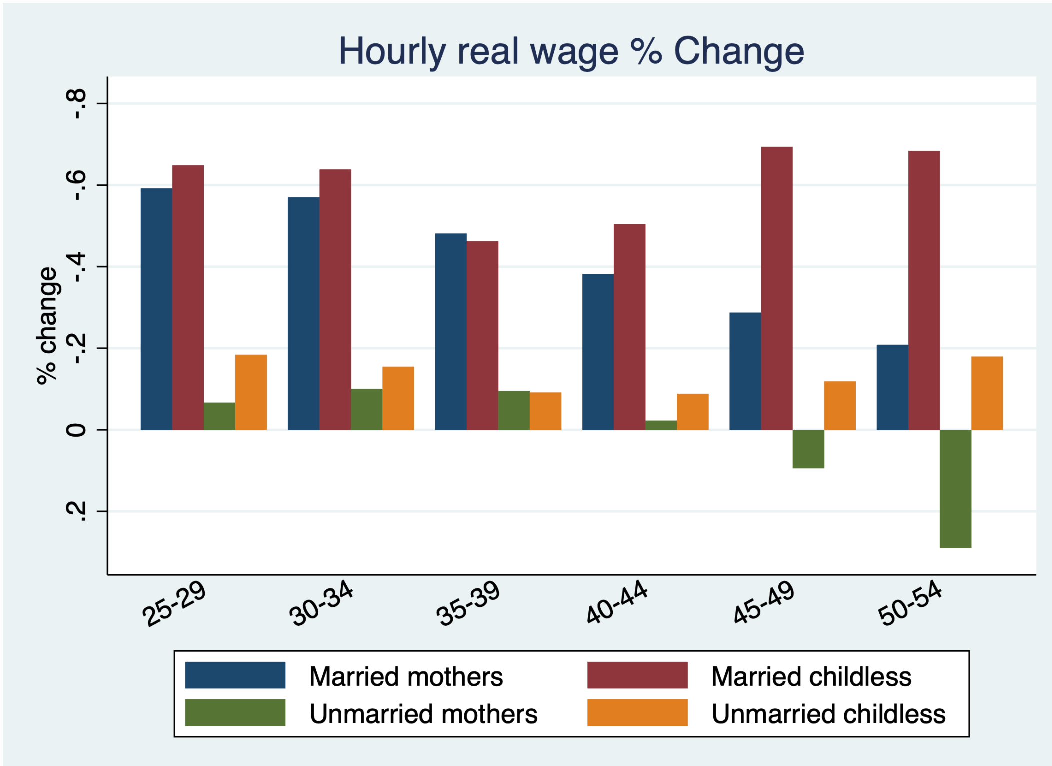

Figure 7: Wage Effects by Age Group

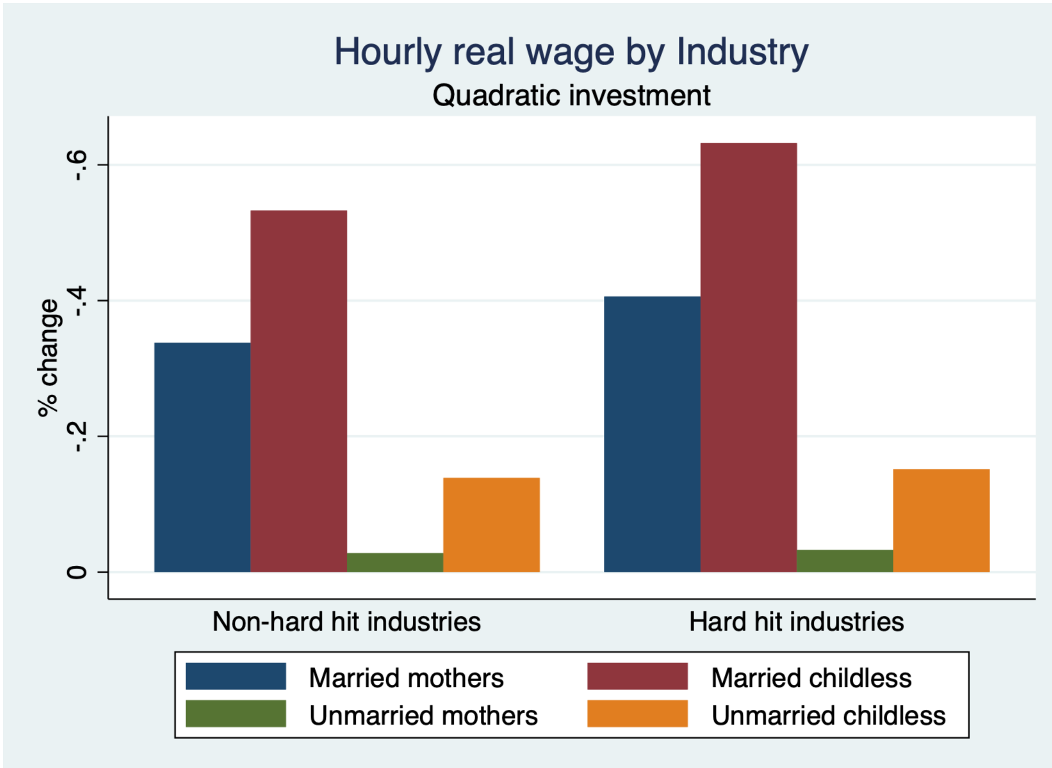

Figure 8: Wage Effects by Industries Hard Hit by COVID

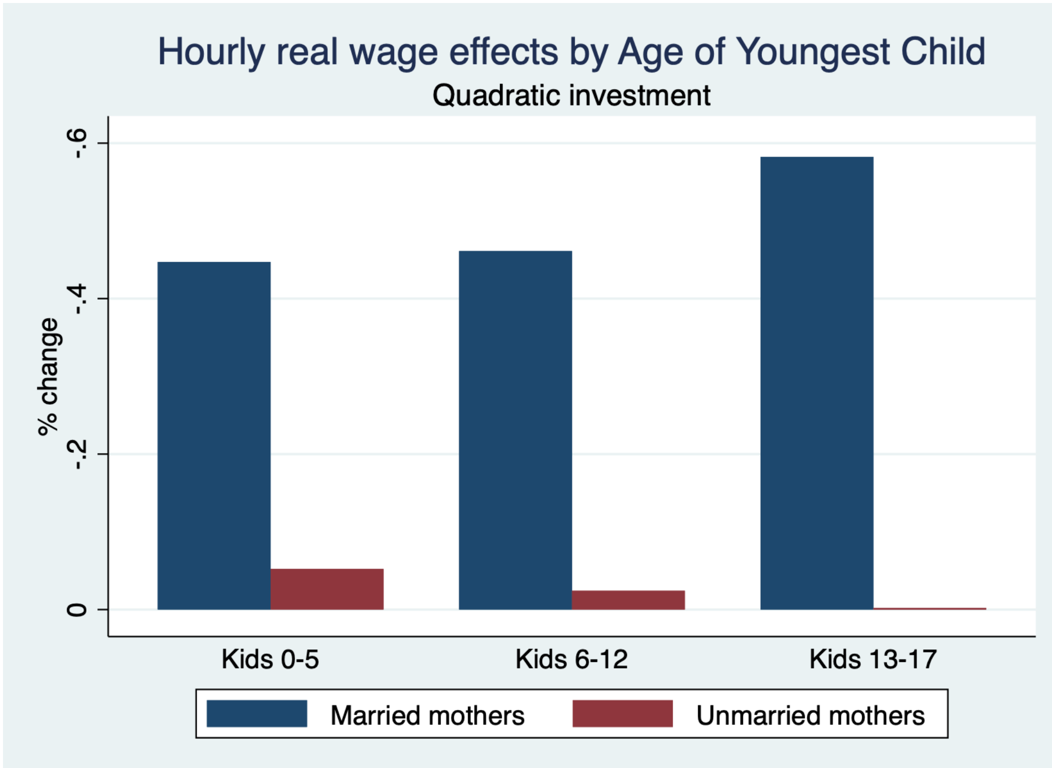

Figure 9: Wage Effects by Age of the Youngest Child

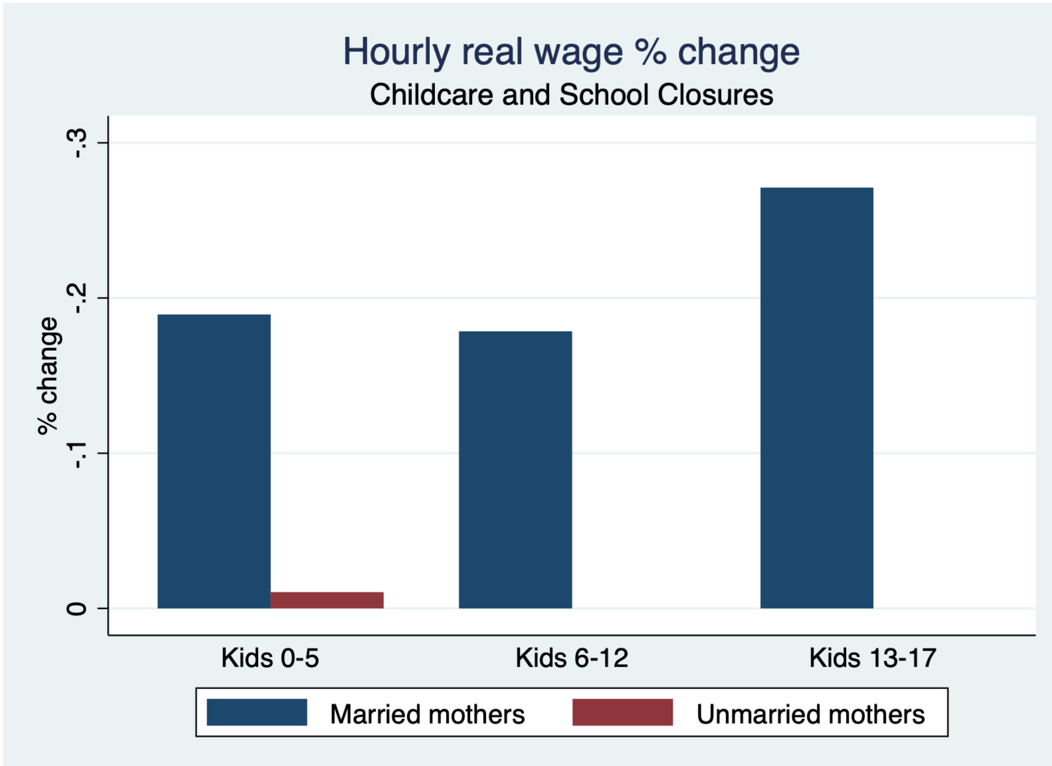

Figure 10: Mean Wage Effects of Childcare/School Closures

Notes: Estimates of the work effects from the closures of childcare services are from Russell and Sun (2020). Estimates of the work effects from the closures of schools are from Hansen et al. (2022).

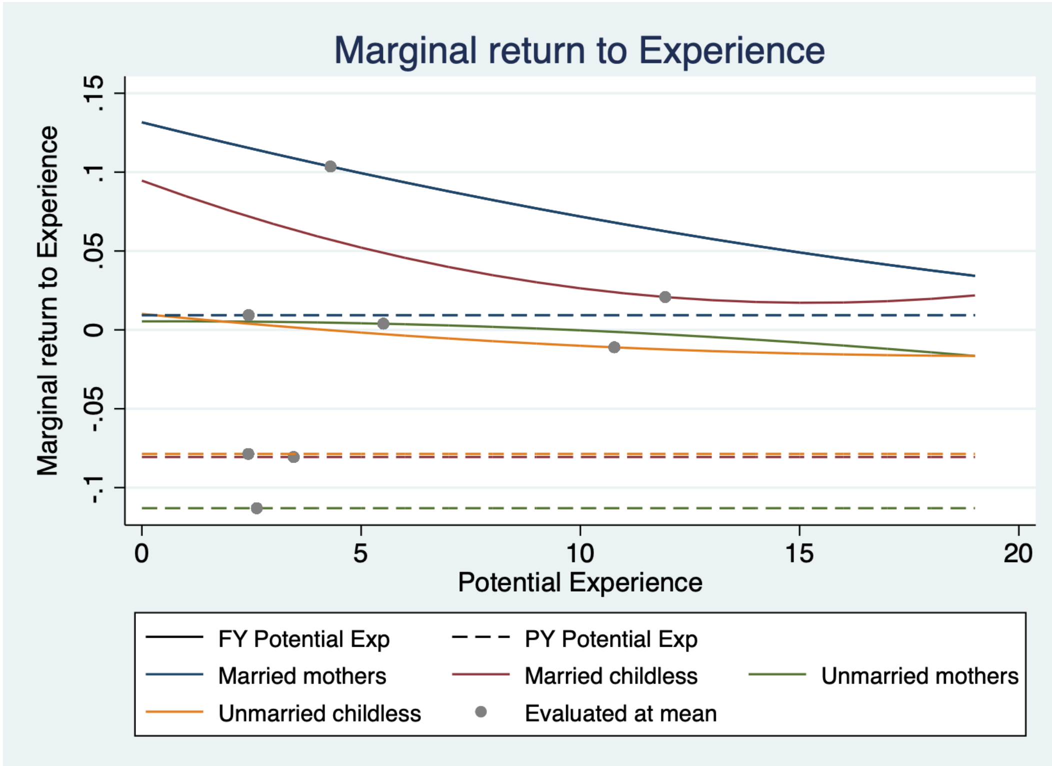

Figure 11: Rate of Return for Full-Year and Part-Year Experience

Notes: FY=Full-Year. PY=Part-Year. Full-year is defined as at least 1600 annual working hours. Part-year is defined as at least 300 hours but fewer than 1600 hours.

Table 1: First Stage – Employment equation

	Married		Unmarried	
	Mothers	Childless	Mothers	Childless
Unemployment Rate	−0.003	−0.002	−0.029***	−0.009**
	(0.003)	(0.005)	(0.006)	(0.004)
Log per-capita COVID Employment	−0.275	−0.470**	0.046	−0.597***
	(0.198)	(0.233)	(0.264)	(0.188)
Log per-capita Total Employment	0.527**	0.715**	−0.581*	0.331
	(0.231)	(0.340)	(0.327)	(0.252)
Recession Indicator	−0.018**	−0.038***	−0.024*	−0.017
	(0.007)	(0.014)	(0.014)	(0.011)
Emp. COVID × COVID Industry	0.138	0.410	−0.061	0.346*
	(0.194)	(0.254)	(0.265)	(0.201)
Emp. Total × COVID Industry	−0.003	−0.238	0.309	−0.295
	(0.244)	(0.348)	(0.306)	(0.271)
Emp. Total × Occupation can telecommute > 25%	−0.096	−0.056	−0.130	0.195
	(0.104)	(0.172)	(0.130)	(0.133)
Emp Total × Nonwhite	−0.249	−0.341	0.145	0.034
	(0.185)	(0.415)	(0.166)	(0.200)
Emp. Covid × Youngest Child ages 6-12	−0.373**		−0.328	
	(0.149)		(0.212)	
Emp. Covid × Youngest Child ages 13-17	−0.184		−0.611**	
	(0.205)		(0.243)	
Emp. Total × Youngest Child ages 6-12	0.261		0.574**	
	(0.197)		(0.264)	
Emp. Total × Youngest Child ages 13-17	−0.481		0.842***	
	(0.300)		(0.305)	
Unemployment × Youngest Child ages 6-12	−0.006		0.008	
	(0.005)		(0.009)	
Unemployment × Youngest Child ages 13-17	−0.026***		0.012	
	(0.007)		(0.009)	
Industry hard hit by COVID	0.118	0.365	0.105	0.270
	(0.163)	(0.239)	(0.237)	(0.187)
Occupation can telecommute > 25%	−0.008	0.038	−0.047	0.182*
	(0.087)	(0.138)	(0.108)	(0.109)
High Contact Occupation	0.005	0.002	−0.0001	−0.002
	(0.017)	(0.021)	(0.022)	(0.020)
Nonwhite	−0.182	−0.290	0.096	0.020
	(0.163)	(0.338)	(0.135)	(0.166)
Age -25	0.051***	0.042***	0.032***	−0.005***
	(0.001)	(0.001)	(0.001)	(0.001)
Year	−0.002**	−0.002**	0.003***	−0.003***
	(0.001)	(0.001)	(0.001)	(0.001)
Youngest Child ages 6-12	−0.271**		−0.068	
	(0.124)		(0.221)	
Youngest Child ages 13-17	−0.408**		−0.287	
	(0.170)		(0.246)	
Predicted Log(1000 + Spouse Earnings)	−0.101**	0.048		
	(0.044)	(0.067)		
Constant	−8.612***	−3.305	0.621	5.041**
	(1.793)	(2.609)	(2.372)	(2.465)
Observations	17187	7461	5730	5676
Business Cycle F-Stat	16.45	9.83	8.48	6.83

Note: *p<0.1; **p<0.05; ***p<0.01. OLS. Predicted Spousal Earnings come from the Zero-th Stage estimates where the dependent variable is log(1000 + Spousal Earnings) in real terms. Standard errors calculated by bootstrapping with 500 replications.

Table 2: Selection Corrected Wage Equation with Quadratic Experience Specification

	Married		Unmarried	
	Mothers	Childless	Mothers	Childless
Exp Hat	0.139***	0.109***	−0.001	0.054
	(0.045)	(0.041)	(0.044)	(0.037)
Linear Weighted Exp Hat	−0.006	−0.003	0.002	−0.005
	(0.010)	(0.004)	(0.005)	(0.003)
Quadratic Weighted Exp Hat	0.0001	0.0001	−0.0001	0.0001
	(0.0005)	(0.0001)	(0.0002)	(0.0001)
Age -25	−0.005	−0.047**	0.007	−0.012
	(0.006)	(0.020)	(0.020)	(0.028)
Year	0.005***	0.005	0.001	0.007
	(0.002)	(0.003)	(0.005)	(0.004)
Nonwhite	−0.182***	−0.245**	−0.131***	−0.126**
	(0.061)	(0.120)	(0.046)	(0.059)
Youngest Child ages 6-12	−0.062*		0.044	
	(0.033)		(0.033)	
Youngest Child ages 13-17	−0.014		0.074	
	(0.053)		(0.055)	
Inverse Mills Ratio	-0.17	-0.10	0.084	-0.050
	(0.151)	(0.137)	(0.189)	(0.165)
Constant	−7.364**	−7.173	0.358	−11.129
	(3.444)	(5.978)	(8.936)	(8.588)
Observations	5254	3023	2456	2617

*p<0.1; **p<0.05; ***p<0.01. Standard errors calculated by bootstrapping with 500 replications.

Table 3: Wage Projection Table

Panel A: Effect through Business Cycle Variables

		Married		Unmarried	
		Mothers	Childless	Mothers	Childless
Hourly real wage	Actual Mean Wage	28.11	18.58	13.15	18.02
	Counterfactual Mean Wage	28.23	18.70	13.16	18.05
	Avg % change	-0.454%	-0.638%	-0.027%	-0.147%
Predicted Experience	Actual Mean	6.90	10.76	6.95	10.51
	Counterfactual Mean	6.97	10.83	7.05	10.56
	Avg % change	-1.385%	-1.069%	-2.717%	-1.016%

Panel B: Additional Effects of Childcare/School Closures

		Mothers	
		Married	Unmarried
Hourly real wage	Mean Wage under Childcare/School Closures	28.173	13.156
	Counterfactual Mean Wage	28.227	13.156
	Avg % change	-0.200%	-0.004%
Predicted Experience	Mean under Childcare/School Closures	6.941	7.038
	Counterfactual Mean	6.967	7.045
	Avg % change	-0.544%	-0.263%

Notes: "Actual" wages are predicted using the 2020 business cycle variables. Counterfactual wages are predicted using the 2019 business cycle variables.

Table 4: Average Percentage Changes for Hourly Wage Projections with Sensitivity Tests

	Married		Unmarried	
	Mothers	Childless	Mothers	Childless
Employment Definition				
Projection for 1600 Hours (baseline)	-0.45%	-0.64%	-0.03%	-0.15%
Projeciton for 1800 Hours	-0.57%	-0.70%	-0.15%	-0.17%
Projection for 1400 Hours	-0.49%	-0.68%	-0.16%	-0.02%
Occupation Effects	-0.48%	-0.31%	-0.00%	-0.05%
Childcare and School Closures				
Basline Closures	-0.65%		-0.03%	
More Childcare/School Closures	-0.69%		-0.04%	
Childcare/School Closures by Age of Kids				
Baseline Closures				
Youngest Child ages 0-5	-0.64%		-0.07%	
Youngest Child ages 6-12	-0.60%		-0.02%	
Youngest Child ages 13-17	-0.81%		-0.00%	
More Childcare/School Closures				
Youngest Child ages 0-5	-0.74%		-0.08%	
Youngest Child ages 6-12	-0.60%		-0.03%	
Youngest Child ages 13-17	-0.81%		-0.01%	
Heckman Adjustment				
Relaxing Bivariate Normality	-0.41%	-0.62%	0.05%	-0.11%
Trending Selection	-0.45%	-0.62%	0.01%	-0.11%

Notes for table: More Childcare/School Closures uses employment effect estimates from Garcia and Cowan (2022).

Table 5: Selection Corrected Wage Equation with Full-Year and Part-Year

	Married		Unmarried	
	Mothers	Childless	Mothers	Childless
FY Exp Hat	0.13***	0.095***	0.0054	0.010
	(0.025)	(0.017)	(0.020)	(0.019)
Linear Weighted FY Exp Hat	-0.0069	-0.010***	0.000086	-0.0027
	(0.0045)	(0.0024)	(0.0033)	(0.0021)
Quadratic Weighted FY Exp Hat	0.000094	0.00033***	-0.000065	0.000069
	(0.00017)	(0.000078)	(0.00012)	(0.000070)
PY Exp Hat	0.0093	-0.081***	-0.11**	-0.079***
	(0.022)	(0.022)	(0.055)	(0.028)
Observations	8949	4198	3256	3170

Notes: FY=Full-year. PY=Part-year. Full-year employment is defined as at least 1600 annual employment hours, while part-year employment is defined as at least 300 hours but fewer than 1600 hours. Regressions also include controls for age of the youngest child, race, potential experience (i.e. age-25), and a linear year trend.

Table 6: Wage Projection Table including Part-Year and Full-Year Experience

		Married		Unmarried	
		Mothers	Childless	Mothers	Childless
Hourly real wage	Actual Mean Wage	21.05	20.00	13.91	19.62
	Counterfactual Mean Wage	21.12	20.08	13.96	19.65
	Avg % change	-0.331%	-0.387%	-0.318%	-0.201%
Predicted FY Experience	Actual Mean	6.38	10.40	6.36	9.64
	Counterfactual Mean	6.45	10.46	6.45	9.68
	Avg % change	-1.42%	-0.80%	-3.10%	-1.06%
Predicted PY Experience	Actual Mean	3.03	4.25	2.46	3.03
	Counterfactual Mean	3.01	4.24	2.43	3.00
	Avg % change	0.88%	0.66%	2.43%	2.64%

Notes: FY=Full-year. PY=Part-year. Full-year employment is at least 1600 annual working hours. Part-year employment is at least 300 annual working hours but fewer than 1600 hours. "Actual" wages are predicted using the 2020 business cycle variables. Counterfactual wages are predicted using the 2019 business cycle variables.

Table 7: Occupation/Industry Experience Mean Projections for Married Women

	Married Mothers	Married Childless
Non-hard Hit COVID Industry	-0.58%	(Insig.)
Hard Hit COVID Industry	-0.37%	(Insig.)
Occupation not able to telecommute	(Insig.)	-0.62%
Occupation can telecommute	(Insig.)	-0.59%
Low Contact Occupation	(Insig.)	-0.56%
High Contact Occupation	(Insig.)	-0.70%

Notes for table: Results insignificant for unmarried women.

Table 8: Job Change Effects under Counterfactual Analysis

		Occupation Change	Industry Change	Either Change
Married mothers	Actual Median	0.294	0.199	0.354
	Counterfactual Median	0.332	0.194	0.374
	Median Pct Change	-4.817%	4.545%	0.616%
Married childless	Actual Median	0.201	0.145	0.250
	Counterfactual Median	0.224	0.150	0.271
	Median Pct Change	-11.044%	0.333%	-5.751%
Unmarried mothers	Actual Median	0.384	0.276	0.455
	Counterfactual Median	0.363	0.241	0.424
	Median Pct Change	6.334%	9.747%	8.794%
Unmarried childless	Actual Median	0.265	0.188	0.336
	Counterfactual Median	0.261	0.190	0.308
	Median Pct Change	3.097%	-0.810%	8.516%

Notes: “Actual” wages are predicted using the 2020 business cycle variables. Counterfactual wages are predicted using the 2019 business cycle variables.

Table 9: Wage Projections in 2022

		Married		Unmarried	
		Mothers	Childless	Mothers	Childless
Hourly real wage	Actual Mean	29.09	18.88	13.27	18.26
	Counterfactual Mean	29.24	19.03	13.27	18.29
	Average % Change	-0.55%	-0.79%	-0.03%	-0.18%
Experience	Actual Mean	7.46	11.39	7.58	11.13
	Counterfactual Mean	7.54	11.47	7.69	11.19
	Average % Change	-1.41%	-1.09%	-2.32%	-0.91%

Note: "Actual" wages are predicted using the 2020 business cycle variables. Counterfactual wages are predicted using the 2019 business cycle variables.

A Appendix: Figures and Tables

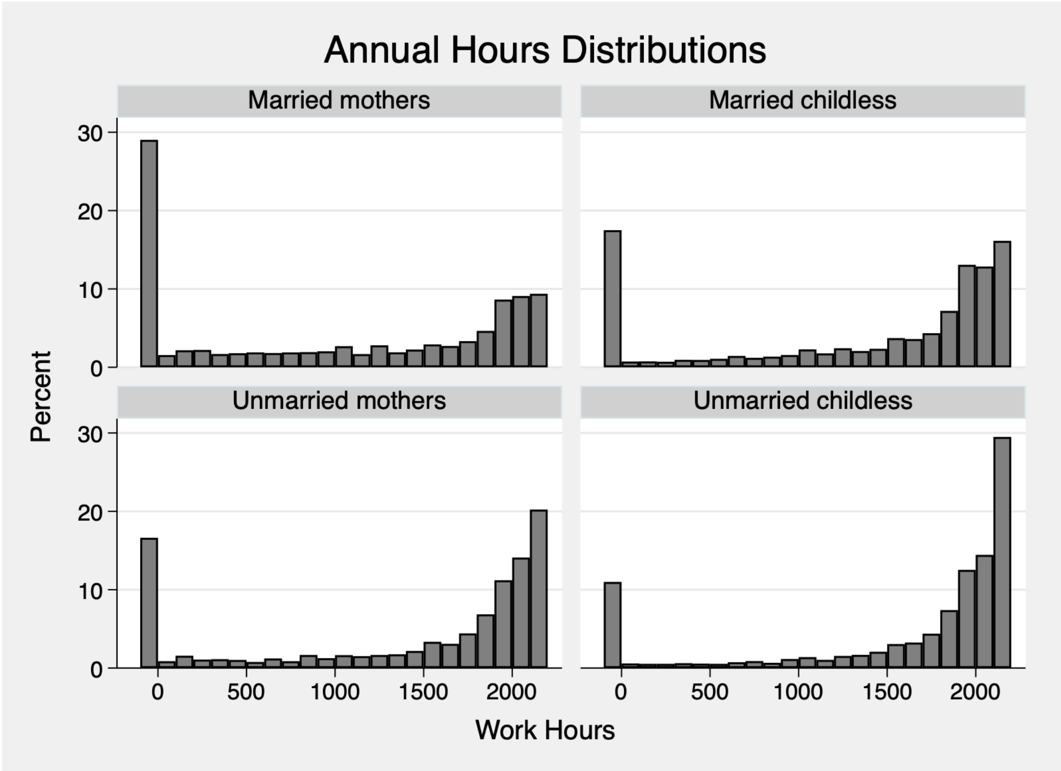

Figure A1: Annual Hours by Marital Status and Kids

Notes: Each bin captures 100 hours.

Table A1: Summary Statistics

	Mean	SD	Min	Max
Employment Variables				
Full-Year Log(Hourly Wage)	2.66	0.49	1.27	4.32
Work†	0.54	0.50	0	1
Business Cycle Variables				
Unemployment Rate	6.13	2.04	2	17.8
Log per-capita Employment, COVID	-1.65	0.15	-2.37	-0.80
Log per-capita Employment, Total	-0.84	0.12	-1.39	0.19
Recession year indicator	0.16	0.37	0	1
Marital Status and Children				
Married mothers	0.48	0.50	0	1
Married childless	0.21	0.41	0	1
Unmarried mothers	0.16	0.36	0	1
Unmarried childless	0.16	0.36	0	1
Covariates				
Age	38.01	8.22	25	54
Industry - hit by COVID	0.63	0.47	0	1
Occupation can telecommute $> 25\%$	0.55	0.48	0	1
High Contact Occupation	0.19	0.39	0	1
Youngest Child ages 0-5	0.26	0.44	0	1
Youngest Child ages 6-12	0.25	0.43	0	1
Youngest Child ages 13-17	0.12	0.33	0	1
Log(1000 + Spouse Earnings)	10.58	0.81	6.91	11.8
Wage Observations	13341			
Observed Work Observations	25029			
Total Observations	36271			

†Summary statistics for Work are calculated without any imputed values.

This table reports the summary statistics for women ages 25-54 that have not attained a college degree. Per-capita variables use state-level population estimates from the US Census Bureau's Intercensal Tables. Employment data come from the Bureau of Labor Statistics. Missing Work values are imputed per the procedure outlined in Appendix B. Log wages are in real terms and trimmed at the 5th and 95th percentiles.

Table A2: Summary Statistics by Marital Status and Kids

	Married		Unmarried	
	Mothers	Childless	Mothers	Childless
Employment Variables				
Full-Year Log(Hourly Wage)	2.592	2.738	2.601	2.745
	(0.493)	(0.479)	(0.467)	(0.485)
Work†	0.415	0.622	0.638	0.738
	(0.493)	(0.485)	(0.481)	(0.440)
Employment Breakdown by Youngest Child				
Youngest Child ages 0-5	0.337		0.565	
	(0.473)		(0.496)	
Youngest Child ages 6-12	0.453		0.650	
	(0.498)		(0.477)	
Youngest Child ages 13-17	0.541		0.736	
	(0.498)		(0.441)	
Business Cycle Variables				
Unemployment Rate	6.149	5.978	6.235	6.201
	(2.083)	(1.962)	(2.035)	(2.009)
Log per-capita Employment, Total	-0.863	-0.822	-0.839	-0.825
	(0.119)	(0.0956)	(0.126)	(0.109)
Log per-capita Employment, COVID	-1.672	-1.616	-1.644	-1.617
	(0.157)	(0.131)	(0.145)	(0.132)
Recession year indicator	0.174	0.149	0.149	0.144
	(0.379)	(0.356)	(0.356)	(0.351)
Covariates				
Age	35.44	43.24	36.04	40.82
	(6.619)	(8.705)	(6.906)	(8.993)
Industry - hit by COVID	0.646	0.614	0.626	0.606
	(0.463)	(0.480)	(0.473)	(0.479)
Occupation can telecommute >25%	0.546	0.590	0.528	0.563
	(0.482)	(0.485)	(0.488)	(0.486)
High Contact Occupation	0.180	0.200	0.199	0.202
	(0.384)	(0.400)	(0.400)	(0.402)
Youngest Child ages 0-5	0.441	0	0.345	0
	(0.496)	(0)	(0.475)	(0)
Youngest Child ages 6-12	0.373	0	0.433	0
	(0.484)	(0)	(0.496)	(0)
Youngest Child ages 13-17	0.187	0	0.223	0
	(0.390)	(0)	(0.416)	(0)
Log(1000 + Spouse Earnings)	10.60	10.54		
	(0.766)	(0.892)		
Observations	17284	7581	5730	5676

†Summary statistics for Work are calculated without any imputed values.
This table reports the means and standard deviations for women ages 25-54 that have not attained a college degree by martial status/children category. Per-capita variables use state-level population estimates from the US Census Bureau's Intercensal Tables. Employment data come from the Bureau of Labor Statistics. Missing Work values are imputed per the procedure outlined in Appendix B. Log wages are in real terms and trimmed at the 5th and 95th percentiles.

Table A3: Zeroth Stage – Spousal Earnings equation

	Married	
	Mothers	Childless
Spouse's Years of Education	0.076***	0.072***
	(0.007)	(0.012)
Unemployment Rate	−0.006	0.005
	(0.006)	(0.009)
Log per-capita COVID Employment	0.256	−0.126
	(0.296)	(0.546)
Log per-capita Total Employment	0.433	1.247*
	(0.448)	(0.727)
Recession Indicator	0.034***	0.095***
	(0.011)	(0.022)
Emp. COVID × COVID Industry	−0.195	−0.229
	(0.322)	(0.585)
Emp. Total × COVID Industry	−0.143	−0.034
	(0.441)	(0.796)
Emp. Total × Occupation can telecommute > 25%	−0.054	−0.530
	(0.203)	(0.346)
Emp Total × Nonwhite	−0.493	0.578
	(0.346)	(0.708)
Emp. Covid × Youngest Child ages 6-12	−0.014	
	(0.263)	
Emp. Covid × Youngest Child ages 13-17	0.227	
	(0.374)	
Emp. Total × Youngest Child ages 6-12	−0.188	
	(0.333)	
Emp. Total × Youngest Child ages 13-17	−0.344	
	(0.578)	
Unemployment × Youngest Child ages 6-12	0.001	
	(0.007)	
Unemployment × Youngest Child ages 13-17	0.019	
	(0.014)	
Industry hard hit by COVID	−0.487*	−0.363
	(0.295)	(0.492)
Occupation can telecommute > 25%	−0.012	−0.334
	(0.173)	(0.280)
High Contact Occupation	−0.002	0.0004
	(0.030)	(0.047)
Nonwhite	−0.604*	0.401
	(0.321)	(0.579)
Age -25	0.008**	0.013***
	(0.003)	(0.002)
Year	−0.004***	−0.002
	(0.001)	(0.002)
Youngest Child ages 6-12	−0.094	
	(0.205)	
Youngest Child ages 13-17	0.074	
	(0.287)	
Constant	15.442***	9.504*
	(3.086)	(5.295)
Observations	17172	7453
0th Stage F-Stat	838.73	268.29

Note: *p<0.1; **p<0.05; ***p<0.01. OLS. Dependent variable is log(1000 + Real Annual Spousal Earnings). Standard errors calculated by bootstrapping with 500 replications.

Table A4: Probit Selection Equation with Quadratic Experience Specification

	Married		Unmarried	
	Mothers	Childless	Mothers	Childless
Unemployment Rate	0.008	0.048***	−0.017	0.022
	(0.010)	(0.017)	(0.013)	(0.014)
Log per-capita COVID Employment	−1.449***	−0.801	−0.798*	−0.574
	(0.522)	(0.581)	(0.448)	(0.452)
Log per-capita Total Employment	1.964***	1.974***	0.335	0.218
	(0.542)	(0.749)	(0.462)	(0.614)
Recession indicator	−0.162***	−0.294***	−0.246***	−0.307***
	(0.023)	(0.036)	(0.044)	(0.043)
Exp Hat	−0.107	−0.114*	0.063	−0.027
	(0.097)	(0.061)	(0.076)	(0.056)
Linear Weighted Exp Hat	−0.005	−0.001	−0.0003	0.014***
	(0.021)	(0.006)	(0.009)	(0.005)
Quadratic Weighted Exp Hat	−0.0002	0.0001	−0.0001	−0.0003**
	(0.001)	(0.0002)	(0.0003)	(0.0002)
Age -25	0.025*	0.047	−0.023	−0.083**
	(0.014)	(0.029)	(0.038)	(0.037)
Year	0.006	−0.018***	−0.029***	−0.038***
	(0.004)	(0.005)	(0.003)	(0.003)
Nonwhite	0.224*	−0.042	−0.087	−0.016
	(0.116)	(0.184)	(0.064)	(0.055)
Youngest Child ages 6-12	0.221***		0.084*	
	(0.038)		(0.048)	
Youngest Child ages 13-17	0.368***		0.235***	
	(0.058)		(0.068)	
Constant	−12.550	36.940***	57.168***	76.482***
	(8.254)	(10.674)	(6.187)	(6.709)
Observations	17291	7583	5730	5676

Note: *p<0.1; **p<0.05; ***p<0.01.
Standard errors calculated by bootstrapping with 500 replications.

Table A5: Selection Corrected Wage Equation with Linear Experience Specification

	Married		Unmarried	
	Mothers	Childless	Mothers	Childless
Exp Hat	0.132***	0.094***	0.014	0.033
	(0.032)	(0.034)	(0.035)	(0.033)
Linear Weighted Exp Hat	−0.004	−0.0002	−0.001	−0.001
	(0.004)	(0.001)	(0.001)	(0.001)
Age -25	−0.006	−0.049**	0.009	−0.015
	(0.006)	(0.020)	(0.020)	(0.028)
Year	0.005***	0.005*	0.001	0.007
	(0.002)	(0.003)	(0.005)	(0.004)
Nonwhite	−0.182***	−0.243**	−0.131***	−0.129**
	(0.061)	(0.119)	(0.046)	(0.059)
Youngest Child ages 6-12	−0.062*		0.043	
	(0.033)		(0.033)	
Youngest Child ages 13-17	−0.014		0.076	
	(0.053)		(0.055)	
Inverse Mills Ratio	-0.17	-0.10	0.084	-0.044
	(0.1523)	(0.136)	(0.189)	(0.164)
Constant	−7.445**	−7.387	0.335	−11.211
	(3.407)	(5.883)	(8.929)	(8.638)
Observations	5254	3023	2456	2617

*p<0.1; **p<0.05; ***p<0.01. Standard errors calculated by bootstrapping with 500 replications.

Table A6: Selection Corrected Wage Projections with Linear Experience Specification

Panel A: Effect through Business Cycle Variables

		Married		Unmarried	
		Mothers	Childless	Mothers	Childless
Hourly real wage	Actual Mean Wage	28.10	18.51	13.13	17.86
	Counterfactual Mean Wage	28.22	18.62	13.13	17.88
	Avg % change	-0.448%	-0.625%	-0.014%	-0.124%
Predicted Experience	Actual Mean	6.90	10.76	6.95	10.51
	Counterfactual Mean	6.97	10.83	7.05	10.56
	Avg % change	-1.385%	-1.069%	-2.717%	-1.016%

Panel B: Additional Effects of Childcare/School Closures

		Mothers	
		Married	Unmarried
Hourly real wage	Mean Wage under Childcare/School Closures	28.163	13.133
	Counterfactual Mean Wage	28.217	13.134
	Avg % change	-0.199%	-0.004%
Predicted Experience	Mean under Childcare/School Closures	6.941	7.038
	Counterfactual Mean	6.967	7.045
	Avg % change	-0.544%	-0.263%

Notes: "Actual" wages are predicted using the 2020 business cycle variables. Counterfactual wages are predicted using the 2019 business cycle variables.

Table A7: Wage Projection Table under OLS Wage Equation with Quadratic Experience Specification

Panel A: Effect through Business Cycle Variables

		Married		Unmarried	
		Mothers	Childless	Mothers	Childless
Hourly real wage	Actual Mean Wage	21.20	16.42	14.83	16.63
	Counterfactual Mean Wage	21.26	16.52	14.83	16.66
	Avg % change	-0.269%	-0.603%	-0.007%	-0.174%
Predicted Experience	Actual Mean	6.90	10.76	6.95	10.51
	Counterfactual Mean	6.97	10.83	7.05	10.56
	Avg % change	-1.385%	-1.069%	-2.717%	-1.016%

Panel B: Additional Effects of Childcare/School Closures

		Mothers	
		Married	Unmarried
Hourly real wage	Mean Wage under Childcare/School Closures	21.229	14.826
	Counterfactual Mean Wage	21.256	14.826
	Avg % change	-0.135%	-0.002%
Predicted Experience	Mean under Childcare/School Closures	6.941	7.038
	Counterfactual Mean	6.967	7.045
	Avg % change	-0.544%	-0.263%

Notes: "Actual" wages are predicted using the 2020 business cycle variables. Counterfactual wages are predicted using the 2019 business cycle variables.

Table A8: Full-Year Employment equation

	Married		Unmarried	
	Mothers	Childless	Mothers	Childless
Unemployment Rate	-0.004*	-0.002	-0.035***	-0.009***
	(0.002)	(0.003)	(0.004)	(0.003)
Log per-capita Employment, COVID	-0.392***	-0.623***	0.100	-0.480***
	(0.091)	(0.116)	(0.153)	(0.114)
Log per-capita Employment, Total	0.559***	0.851***	-0.620***	0.217
	(0.122)	(0.169)	(0.197)	(0.162)
Recession year indicator	-0.015**	-0.021*	-0.011	-0.012
	(0.008)	(0.013)	(0.014)	(0.013)
Emp. COVID × COVID Industry	0.139	0.550***	-0.146	0.248*
	(0.093)	(0.133)	(0.143)	(0.128)
Emp. Total × COVID Industry	0.045	-0.339*	0.349*	-0.148
	(0.123)	(0.180)	(0.181)	(0.172)
Emp. Total × Nonwhite	-0.268***	-0.294*	0.262***	0.026
	(0.081)	(0.177)	(0.095)	(0.101)
Emp. COVID × Youngest Child ages 6-12	-0.269***		-0.355**	
	(0.093)		(0.144)	
Emp. COVID × Youngest Child ages 13-17	-0.053		-0.589***	
	(0.118)		(0.181)	
Emp. Total × Youngest Child ages 6-12	0.208*		0.553***	
	(0.126)		(0.171)	
Emp. Total × Youngest Child ages 13-17	-0.510***		0.809***	
	(0.173)		(0.232)	
Unemployment × Youngest Child ages 6-12	-0.005		0.012**	
	(0.003)		(0.006)	
Unemployment × Youngest Child ages 13-17	-0.025***		0.021***	
	(0.005)		(0.008)	
Predicted Log(1000 + Spousal Earnings)	-0.120***	0.030		
	(0.018)	(0.026)		
Industry - hit by COVID	0.156**	0.501***	-0.026	0.232*
	(0.078)	(0.122)	(0.134)	(0.128)
Occupation can telecommute > 25%	0.056	0.059	-0.095	0.188**
	(0.046)	(0.079)	(0.078)	(0.075)
High Contact Occupation	0.049***	0.044***	0.059***	0.011
	(0.009)	(0.013)	(0.015)	(0.013)
Year	0.005***	0.004***	0.000	-0.003***
	(0.000)	(0.001)	(0.001)	(0.001)
Non-white	-0.202***	-0.240	0.178**	-0.002
	(0.072)	(0.151)	(0.081)	(0.084)
Years since age 25	-0.001	-0.005***	0.004***	-0.003***
	(0.001)	(0.001)	(0.001)	(0.001)
Youngest Child ages 6-12	-0.154**		-0.151	
	(0.076)		(0.141)	
Youngest Child ages 13-17	-0.238**		-0.340*	
	(0.099)		(0.177)	
Constant	-8.629***	-7.331***	-0.540	6.499***
	(0.812)	(1.225)	(1.293)	(1.236)
Observations	19085	8038	6167	5899

Note: OLS. Predicted Spousal Earnings come from the Zero-th Stage estimates where the dependent variable is log(1000 + Spousal Earnings) in real terms. Full-year is defined as at least 1600 annual working hours.

Table A9: Part-Year Employment equation

	Married		Unmarried	
	Mothers	Childless	Mothers	Childless
Unemployment Rate	0.007***	-0.002	0.005	0.007***
	(0.002)	(0.002)	(0.004)	(0.002)
Log per-capita Employment, COVID	0.237***	0.555***	0.197	0.486***
	(0.086)	(0.099)	(0.126)	(0.093)
Log per-capita Employment, Total	0.032	-0.525***	-0.131	-0.410***
	(0.115)	(0.145)	(0.163)	(0.132)
Recession year indicator	0.001	0.009	0.020*	0.005
	(0.007)	(0.011)	(0.012)	(0.011)
Emp. COVID × COVID Industry	-0.201**	-0.400***	0.014	-0.312***
	(0.087)	(0.114)	(0.119)	(0.105)
Emp. Total × COVID Industry	0.203*	0.354**	-0.005	0.346**
	(0.116)	(0.154)	(0.150)	(0.140)
Emp. Total × Nonwhite	-0.196***	-0.028	-0.037	-0.143*
	(0.076)	(0.151)	(0.078)	(0.083)
Emp. COVID × Youngest Child ages 6-12	-0.005		0.047	
	(0.088)		(0.119)	
Emp. COVID × Youngest Child ages 13-17	-0.240**		0.257*	
	(0.111)		(0.150)	
Emp. Total × Youngest Child ages 6-12	-0.221*		-0.030	
	(0.119)		(0.141)	
Emp. Total × Youngest Child ages 13-17	0.238		-0.274	
	(0.164)		(0.193)	
Unemployment × Youngest Child ages 6-12	-0.002		0.001	
	(0.003)		(0.005)	
Unemployment × Youngest Child ages 13-17	0.005		0.000	
	(0.005)		(0.006)	
Predicted Log(1000 + Spousal Earnings)	0.101***	0.071***		
	(0.017)	(0.023)		
Industry - hit by COVID	-0.099	-0.297***	0.047	-0.201*
	(0.074)	(0.104)	(0.111)	(0.104)
Occupation can telecommute > 25%	-0.003	-0.140**	0.058	-0.013
	(0.044)	(0.068)	(0.065)	(0.061)
High Contact Occupation	0.031***	0.038***	0.029**	0.029***
	(0.009)	(0.011)	(0.013)	(0.011)
Year	-0.001***	-0.005***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.001)	(0.000)
Non-white	-0.171**	-0.032	-0.019	-0.103
	(0.068)	(0.129)	(0.067)	(0.069)
Years since age 25	-0.000	0.002***	-0.003***	0.000
	(0.001)	(0.000)	(0.001)	(0.000)
Youngest Child ages 6-12	-0.172**		0.044	
	(0.072)		(0.117)	
Youngest Child ages 13-17	-0.222**		0.170	
	(0.093)		(0.146)	
Constant	1.947**	9.077***	3.178***	3.255***
	(0.767)	(1.048)	(1.072)	(1.008)
Observations	19085	8038	6167	5899

Note: OLS. Predicted Spousal Earnings come from the Zero-th Stage estimates where the dependent variable is log(1000 + Spousal Earnings) in real terms. Part-year is defined as at least 300 annual working hours but fewer than 1600 hours.

Table A10: Selection Corrected Wage Equation with Occupation and Industry Variables

	Married	
	Mothers	Childless
Exp Hat	0.15***	0.097***
	(0.031)	(0.019)
Linear Weighted Exp Hat	-0.0070	-0.0031
	(0.0063)	(0.0025)
Quadratic Weighted Exp Hat	0.00013	0.000093
	(0.00026)	(0.000079)
Years since age 25	-0.0052*	-0.045***
	(0.0027)	(0.0076)
Year	0.0054***	0.0052***
	(0.00091)	(0.0016)
Non-white	-0.16***	-0.22***
	(0.026)	(0.035)
Industry - hit by COVID	-0.061*	-0.072*
	(0.035)	(0.038)
Occupation can telecommute > 25%	0.10***	0.063
	(0.028)	(0.042)
High Contact Occupation	0.034	0.073
	(0.045)	(0.053)
Youngest Child ages 6-12	-0.058***	
	(0.021)	
Youngest Child ages 13-17	-0.014	
	(0.030)	
Exp Hat × Industry - hit by COVID	-0.035***	-0.0036
	(0.0094)	(0.0032)
Exp Hat × Occupation can telecommute > 25%	0.013	0.011***
	(0.0098)	(0.0037)
Exp Hat × High Contact Occupation	0.0095	0.011**
	(0.013)	(0.0046)
Inverse Mills Ratio	-0.18**	-0.13
	(0.079)	(0.087)
Constant	-8.11***	-7.52**
	(1.82)	(3.06)
Observations	5254	3023

Notes: *p<0.1; **p<0.05; ***p<0.01.

Table A11: Change of Occupation

	Married		Unmarried	
	Mothers	Childless	Mothers	Childless
Unemployment Rate	0.037***	0.027***	-0.028*	0.022**
	(0.0079)	(0.010)	(0.016)	(0.011)
Log per-capita Employment, COVID	0.17	-0.98**	0.23	-1.72***
	(0.32)	(0.39)	(0.52)	(0.43)
Log per-capita Employment, Total	1.76***	2.95***	0.31	2.42***
	(0.43)	(0.58)	(0.66)	(0.61)
Recession year indicator	-0.058**	-0.057	0.080*	-0.0097
	(0.027)	(0.044)	(0.048)	(0.050)
Emp. COVID × COVID Industry	-1.10***	0.77*	-0.49	1.04**
	(0.32)	(0.46)	(0.49)	(0.49)
Emp. Total × COVID Industry	1.36***	-1.42**	0.45	-1.19*
	(0.42)	(0.63)	(0.61)	(0.65)
Emp. Total × Nonwhite	-0.56**	0.070	-0.49	-0.49
	(0.27)	(0.60)	(0.32)	(0.38)
Emp. COVID × Youngest Child ages 6-12	-0.18		-0.15	
	(0.33)		(0.50)	
Emp. COVID × Youngest Child ages 13-17	0.24		-0.97	
	(0.41)		(0.61)	
Emp. Total × Youngest Child ages 6-12	-0.39		0.95	
	(0.45)		(0.58)	
Emp. Total × Youngest Child ages 13-17	-1.44**		1.42*	
	(0.60)		(0.77)	
Unemployment × Youngest Child ages 6-12	-0.0017		0.050**	
	(0.012)		(0.021)	
Unemployment × Youngest Child ages 13-17	-0.098***		0.044*	
	(0.017)		(0.026)	
Predicted Log(1000 + Spouse Earnings)	0.17**	0.31***		
	(0.068)	(0.098)		
Industry - hit by COVID	-0.79***	-0.11	-0.54	0.58
	(0.27)	(0.42)	(0.46)	(0.48)
Occupation can telecommute > 25%	-0.27*	0.42	0.11	0.57**
	(0.16)	(0.28)	(0.26)	(0.28)
High Contact Occupation	0.056*	-0.0056	0.046	0.089*
	(0.031)	(0.046)	(0.051)	(0.051)
Year	-0.0033***	-0.016***	-0.0019	-0.0091***
	(0.0013)	(0.0020)	(0.0021)	(0.0023)
Non-white	-0.44*	0.16	-0.59**	-0.51
	(0.24)	(0.52)	(0.27)	(0.32)
Years since age 25	-0.0047**	-0.0087***	-0.013***	-0.0066***
	(0.0020)	(0.0019)	(0.0029)	(0.0021)
Youngest Child ages 6-12	-0.52*		0.29	
	(0.27)		(0.49)	
Youngest Child ages 13-17	-0.14		-0.52	
	(0.34)		(0.60)	
Constant	6.04**	28.8***	4.53	17.0***
	(2.86)	(4.40)	(4.38)	(4.73)
Observations	17187	7461	5730	5676

Notes: *p<0.1; **p<0.05; ***p<0.01. These results are probit regressions. The dependent variable is an indicator for whether the occupation group changed between time (t-1) and (t+1).

Table A12: Change of Industry

	Married		Unmarried	
	Mothers	Childless	Mothers	Childless
Unemployment Rate	0.028***	0.0057	-0.0039	0.0031
	(0.0087)	(0.011)	(0.017)	(0.012)
Log per-capita Employment, COVID	0.31	1.70***	0.79	-0.32
	(0.34)	(0.43)	(0.56)	(0.47)
Log per-capita Employment, Total	1.41***	-0.40	-0.54	1.17*
	(0.46)	(0.64)	(0.70)	(0.66)
Recession year indicator	-0.0063	0.023	0.053	0.023
	(0.030)	(0.049)	(0.052)	(0.055)
Emp. COVID $\times$ COVID Industry	-0.81**	-0.64	-1.51***	0.44
	(0.34)	(0.51)	(0.52)	(0.53)
Emp. Total $\times$ COVID Industry	0.071	-0.35	1.02	-0.64
	(0.46)	(0.69)	(0.64)	(0.71)
Emp. Total $\times$ Nonwhite	-0.30	1.68**	0.69**	-0.45
	(0.30)	(0.69)	(0.35)	(0.43)
Emp. COVID $\times$ Youngest Child ages 6-12	-0.31		-0.14	
	(0.36)		(0.54)	
Emp. COVID $\times$ Youngest Child ages 13-17	0.42		-1.16*	
	(0.45)		(0.66)	
Emp. Total $\times$ Youngest Child ages 6-12	-0.18		0.021	
	(0.49)		(0.60)	
Emp. Total $\times$ Youngest Child ages 13-17	-1.12*		1.19	
	(0.67)		(0.81)	
Unemployment $\times$ Youngest Child ages 6-12	0.0015		0.012	
	(0.013)		(0.022)	
Unemployment $\times$ Youngest Child ages 13-17	-0.060***		0.036	
	(0.019)		(0.027)	
Predicted Log(1000 + Spouse Earnings)	0.033	0.39***		
	(0.075)	(0.11)		
Industry - hit by COVID	-1.42***	-1.51***	-1.73***	0.12
	(0.29)	(0.47)	(0.49)	(0.53)
Occupation can telecommute > 25%	-0.23	0.019	-0.035	-0.099
	(0.18)	(0.31)	(0.27)	(0.32)
High Contact Occupation	-0.038	-0.080	-0.012	0.014
	(0.034)	(0.051)	(0.055)	(0.056)
Year	0.0015	-0.011***	0.0080***	0.00090
	(0.0014)	(0.0023)	(0.0022)	(0.0025)
Non-white	-0.28	1.62***	0.37	-0.43
	(0.27)	(0.59)	(0.29)	(0.35)
Years since age 25	-0.0094***	-0.012***	-0.014***	-0.0098***
	(0.0022)	(0.0021)	(0.0031)	(0.0023)
Youngest Child ages 6-12	-0.56*		-0.32	
	(0.29)		(0.53)	
Youngest Child ages 13-17	0.17		-1.04	
	(0.38)		(0.65)	
Constant	-2.46	19.1***	-15.5***	-2.00
	(3.14)	(4.86)	(4.75)	(5.22)
Observations	17187	7461	5730	5676

Notes: *p<0.1; **p<0.05; ***p<0.01. These results are probit regressions. The dependent variable is an indicator for whether the industry group changed between time (t-1) and (t+1).

Table A13: Change of Either Occupation or Industry

	Married		Unmarried	
	Mothers	Childless	Mothers	Childless
Unemployment Rate	0.038***	0.026***	-0.033**	0.024**
	(0.0078)	(0.0100)	(0.016)	(0.010)
Log per-capita Employment, COVID	0.42	-0.68*	0.59	-1.67***
	(0.31)	(0.39)	(0.51)	(0.43)
Log per-capita Employment, Total	1.52***	3.16***	0.050	2.35***
	(0.42)	(0.58)	(0.65)	(0.60)
Recession year indicator	-0.024	-0.0058	0.088*	0.045
	(0.026)	(0.043)	(0.048)	(0.049)
Emp. COVID × COVID Industry	-1.21***	0.80*	-0.90*	0.98**
	(0.31)	(0.45)	(0.48)	(0.48)
Emp. Total × COVID Industry	1.30***	-1.83***	0.80	-1.06*
	(0.41)	(0.62)	(0.60)	(0.64)
Emp. Total × Nonwhite	-0.39	0.34	-0.26	-0.46
	(0.27)	(0.60)	(0.32)	(0.38)
Emp. COVID × Youngest Child ages 6-12	-0.50		-0.20	
	(0.32)		(0.49)	
Emp. COVID × Youngest Child ages 13-17	0.11		-1.19**	
	(0.40)		(0.61)	
Emp. Total × Youngest Child ages 6-12	-0.047		0.72	
	(0.44)		(0.57)	
Emp. Total × Youngest Child ages 13-17	-1.24**		1.77**	
	(0.59)		(0.78)	
Unemployment × Youngest Child ages 6-12	0.0043		0.056***	
	(0.012)		(0.021)	
Unemployment × Youngest Child ages 13-17	-0.087***		0.059**	
	(0.016)		(0.026)	
Predicted Log(1000 + Spouse Earnings)	0.16**	0.26***		
	(0.067)	(0.097)		
Industry - hit by COVID	-1.09***	-0.44	-0.97**	0.57
	(0.26)	(0.41)	(0.45)	(0.47)
Occupation can telecommute > 25%	-0.22	0.40	-0.089	0.72**
	(0.16)	(0.27)	(0.26)	(0.28)
High Contact Occupation	0.020	0.016	0.033	0.053
	(0.030)	(0.045)	(0.051)	(0.050)
Year	-0.0012	-0.017***	-0.00028	-0.0077***
	(0.0012)	(0.0020)	(0.0020)	(0.0022)
Non-white	-0.30	0.39	-0.39	-0.46
	(0.24)	(0.51)	(0.27)	(0.31)
Years since age 25	-0.0046**	-0.0095***	-0.012***	-0.0064***
	(0.0019)	(0.0019)	(0.0028)	(0.0020)
Youngest Child ages 6-12	-0.76***		-0.025	
	(0.26)		(0.48)	
Youngest Child ages 13-17	-0.21		-0.64	
	(0.34)		(0.59)	
Constant	2.21	32.4***	1.78	14.4***
	(2.82)	(4.32)	(4.33)	(4.64)
Observations	17187	7461	5730	5676

Notes: *p<0.1; **p<0.05; ***p<0.01. These results are probit regressions. The dependent variable is an indicator for whether the occupation group or the industry group changed between time (t-1) and (t+1).

Table A14: Breakdown of Imputed Observations

	Married		Unmarried	
	Mothers	Childless	Mothers	Childless
Total Observations including Imputed Histories	19304	8158	6254	6406
Total Observations excluding Imputed Histories	12692	4811	3882	3556
Difference	6612	3347	2372	2850
Before 1968	346	56	37	25
Left Censored	1026	334	334	524
Missing Information in Final Appearance	41	43	21	26
PSID Interviewing Gap	3797	2344	1601	1863
Other Gap	1402	570	379	412
Other Gap (2 or fewer years)	698	306	247	257

B Data Appendix

B.1 Construction of Dataset

Our sample uses prime-age women (25-54) in the Survey Research Center (SRC) sample from the 1968 to 2017 waves of the Panel Study of Income Dynamics who appear at least once in the 1990 to 2017 time frame. Observations from the PSID that are age 55 or above in 1990 are therefore excluded from the analysis. We drop individuals with longitudinal weight of zero and individuals with completely missing employment or education variables (i.e. no employment or education information across any appearances in the panel). Furthermore, in partnered households, we remove same-sex families identified by households where the head of household and the spouse of household were both female.

In order to construct the complete history, we construct a record of each individual's history of all variables used in the analysis from 25 to 54. Several reasons lead some observations for some ages to be missing. Individuals can be missing because of left censoring (either being age 25 before 1968 or entering the PSID at an age after 25), missing employment information in the final appearance, or having miscellaneous gaps (e.g., not being observed in consecutive surveys). Also, with the move to biannual interviewing, the PSID has gaps for all observations after 1997. Table A14 quantifies the degree of the missing observations by each marital status and kids categories. [1] How the missing observations are handled is described in the Imputations section of this Appendix.

The variables include in the analysis are as follows.

B.1.1 Education

Our focus is on women without a college degree. This educational group is identified as those with fewer than 16 years of education. Because of small discrepancies in reported education in different interviews, we take the modal values across interview years to identify the most common education status for women over the life cycle. As a result, any women who obtained a college degree after age 25 will only be classified as without a college degree if they obtained this degree earlier in the

[1] Imputations for marital status and children are conducted first (see below), so Table A14 characterizes the missing additional information given the already imputed marital status and kids category.

life cycle. For example, if a woman enters the PSID at age 25 with 15 years of schooling, and completes her college degree the following year, her modal value of education will indicate that she does indeed have a college degree and will *not* be included in our analysis.

B.1.2 Marital Status and Children

Marital status and the presence of children are available in every wave of the PSID. Given information on the age of the youngest child, we construct three dummy variables for the youngest child in the 0-5, 6-12, and 13-17 age ranges. For all regressions, we omit the indicator for the youngest child in the 0-5 age range as the reference category. Responses to the questions of marital status and age of the youngest child are recorded in the same year as the interview.

B.1.3 Employment and earnings

We observe employment status based on responses to questions on annual labor income and annual hours. The distributions for the annual working hours by the marital status and presence of children are presented in Figure A1.

Employment information is recorded for the year prior to the survey year. While the PSID does include employment questions for two years prior to the interview, we find that responses for this question tended to systematically differ with previous responses and therefore we choose not to use this information. We use responses to the total labor income to characterize annual earnings for the women, as well as for spouses for married women. Hourly wages are obtained by dividing the total annual earnings by annual reported working hours. All earnings and wage variables are put into real terms (2010 base year) using the Personal Consumption Expenditures deflator. We trim all earnings and wage variables used in the regressions at the 5th and 95th percentiles.

B.1.4 Occupation

The 25 occupation groups are management, business operations, financial specialist, computer and mathematical, architecture and engineering, life physical and social science, community and social services, legal, education training and library, arts design entertainment sports and media, healthcare practitioners and technical, healthcare support, protective service, food preparation and serving, building and grounds cleaning and maintenance, personal care and service, sales, office and administrative support, farming fishing and forestry, construction, extraction, installation maintenance and repair, production, transportation and material moving, military specific. The codes for specific occupations are different across years (three coding systems for 1968-2001, 2003-2015, and 2017 separately). The codes for 2003-2015 and 2017 can be easily matched to the 25 occupations groups above. The codes for 1968-2001 (which is the 1970 census code) cannot. To do the conversion, the 1970 code is first converted to the 1990 code using data from the IPUMS website, and then converted to the 2000 code using the Blau et al. (2013) crosswalk.

Furthermore, the percentages of the ability to telecommute are from the Alon et al. (2020) We classify occupations as "telecommutable" if they are able to telecommute at least 25% of the time. We chose this threshold since it roughly divided the sample in half, but we also conducted sensitivity tests to this definition. As we increased the threshold for the telecommutable classification, the statistical precision attenuated. The indicator for high contact occupations is created using the Albanesi and Kim (2021) classifications. Albanesi and Kim (2021) also provide classifications for "inflexible" occupations. However, these occupations ended up being the complement to the occupations that are able to telecommute.

B.1.5 Industry

The industry coding uses two-digit codes with the exception of the 2017 Wave. The industry groups are leisure and hospitality, mining, agriculture, construction, transportation and utilities, other services, information, professional and business services, wholesale and retail trade, manufacturing, education and health services, government workers, and financial activities. The codes for specific industries are different across years (three coding systems for 1968-2001, 2003-2015, and 2017 separately) but they can be easily matched into the 13 industry groups above.

The industry variable is then used to create the indicator for industries hard hit by COVID, which are classified as the following five industries: leisure and hospitality, transportation and utilities, wholesale and retail trade, education and health, and other services.

B.1.6 Race

We use the modal value of race in order to identify the race of the individual. In the early waves of the PSID, respondents could only choose between 3 race options: White, Black, or Other. In subsequent waves, the additional racial categories were included. Given the changing nature of this question, we elect to use the binary variable "Nonwhite" such that Nonwhite = $\mathbb{1}$ (Modal Race $\neq$ White), where $\mathbb{1}(\cdot)$ is the indicator function.

B.1.7 Business Cycle Variables

State Unemployment Rate

The state level unemployment rates are obtained from two sources: U.S. Bureau of Labor Statistics (1976 and after) and the Statistical Abstract (S.A., 1969-1975). [2] For state unemployment rates before 1969, we impute a value using the national unemployment rate. For each year y, we assume the unemployment rate in a state is the state's unemployment rate in 1969 times the ratio of the national unemployment rate in year y to the national unemployment rate in 1969.

State Employment Variables

State employment variables are available from the US. Bureau of Labor Statistics. We utilize the state non-seasonally adjusted total non-farm employment numbers for our measure of total employment. The employment in the hard-hit COVID industries obtains the employment numbers for the industries that were affected by COVID as identified earlier. While the total employment numbers are available for every year in our sample, the industry-specific state level employment numbers only go back to 1990. For observations before 1990, we assume that the ratio of employment in COVID industries to the total employment remained the same as that observed in 1990. Thus, for a year $y < 1990$, the employment in the hard-hit COVID industries is equal to the total employment in year y times the COVID-industry employment in 1990 divided by total employment in 1990. Per capita state employment variables are calculated using the intercensal population estimates from

[2] We use the following tables from the Statistical Abstract for state unemployment rate between 1969 and 1975:
1969: 1971 S.A., Part 3, Section 8, Table 337;
1970,1971: 1972 S.A., Part 4, Section 8, Table 353;
1972: 1974 S.A., Part 6, Section 12, Table 557;
1973, 1974, 1975: 1977 S.A., Part 6, Section 13, Table 643.

the US Census Bureau. Finally, we take the natural logarithm of both per-capita employment variables.

B.2 Imputations

A key feature of the Mincer-Polachek model is that human capital accumulation depends on the timing of such investments (see Equation 9 in the main text). We have a relatively small number of missing observations on employment and other variables as noted previously. Missing data for short gaps (between 1 and 2 years), which includes the 1-year gaps after 1997 and occasional short gaps prior to 1997–can be reasonably imputed by using information from before and after the gap. The methods are described below.[3]

B.2.1 Marital Status and Children

We use the following algorithm to impute values for marital status and children during the gap years. Denoting MSK as a four-valued variable for the four family structure categories, we first determine whether a missing MSK value at age a if surrounded by identical values of MSK between age $a-1$ and $a+1$, in which case we assume that the MSK remained the same during age a. For single year gap year where $\text{MSK}_{a-1} \neq \text{MSK}_{a+1}$, we use a multinomial logit to obtain MSK probabilities for age a. We use the dummy variables for the MSK at $a+1$ and MSK $a-1$, age, and interactions of age with both the lag and the lead MSK dummy variables, to obtain MSK probabilities for age a. These multinomial logits are pooled over all ages to increase the predictive power for these single year gaps. Furthermore, this multinomial logit only uses observed MSK status from the complete data. As a result, this multinomial logit is estimated on the pre-1997 data since the MSK at times three consecutive MSK variables cannot be observed under the biannual interviewing schedule.

For gaps larger than a year, we use a multinomial logit based on the closest observation. For each age, we again use complete data on adjacent values to estimate a probability model that a

[3]We do not need to impute wages because all observations included in the estimation of the wage equations are observed without imputed values.

current MSK is related to either the lead or the lag MSK. For example, if there were an observation with a two-year gap at ages a and $a+1$, we estimate a multinomial logit of MSK_{a+1} on MSK_{a+2} to provide MSK probabilities for age $a+1$, and similarly we use a multinomial logit of MSK_a on MSK_{a-1} to provide MSK probabilities for age a. As with the multinomial logits for the single year, they are also estimated only with observations with consecutive MSK values, (which means that these must be estimated once again on the pre-1997 sample). Because of sample size limitations, we pool the 46-50 and 51-55 age groups for estimation of these multinomial logits.

Given MSK probabilities for all observations, we take a random draw from a uniform distribution in order to assign an MSK for a given missing observation. With these MSK values, these observations can enter into the MSK-specific first stage employment equations.

B.2.2 Employment Variables

In our baseline specifications, we define an observation as employed if they record at least 1600 annual hours of work. Presented with the same missing data issues as with the MSK variables, we impute the employment status for individuals with short gaps. To do so, we estimate a probit for the employment status as a function of the business cycle variables and the probabilities of being in each MSK category. Given the discrepancy in the timing of the employment and earnings variables (reported for the year prior to the interview year) compared to the marital status and children variables (reported in the year of interview), the MSK probabilities must be included in order to allow employment information post-1997 to contribute to prediction of the work status. This procedure gives us a predicted employment status value for the observations with missing values due to gaps, which allow these observations to enter our first stage employment equation in the form of a predicted employment probability for the dependent variable in the OLS estimated employment equations.

The general procedure remains the same under the different definitions of employment status used in Section 4 of the main text. In the sensitivity analyses that adjust the hours requirements, an identical procedure is followed. The employment variable is redefined according to the hours requirement in question For the Part-Time/ Full-Time exercise, we utilize a categorical variable assigning 1 to Part-Time employment ($300 \leq$ Hours < 1600) and 2 to Full Time Work (Hours $\geq$ 1600). We use a multinomial logit in order to impute the missing employment variables. Doing so provides separate employment probabilities for the two employment statuses.

Versions that estimated our equations without any imputed employment histories were conducted and most results remained unchanged. These results are available upon request of the authors.

B.2.3 Non-Employment Variables

Missing non-employment variables are filled in using the age-MSK-specific means. For a given variable, we tag the the sample of women at each MSK-age combination for which this variable is observed. Using this tagged sample, we calculate the mean of the variable and use this value to replace the missing value for an individual of the same MSK and age as the tagged sample.

This procedure remains the same for the MSK-specific variables. For married observations with missing spousal annual earnings, this variable is imputed in the same fashion described above. However, a particular difficulty occurs for missing values for the age of youngest child indicators. In order to keep this information binary, we calculate the mean age of the youngest child for the tagged age-MSK sample. For observations filled in with this mean value, the appropriate indicator is then updated.

For the imputation of individual industry and occupation in years when the individual does not work, we use the most recent industry or occupation when available. Otherwise, these values are imputed according to the procedures described above.

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