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


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Abstract

The COVID-19 pandemic resulted in major declines in employment of women. We provide projections of impacts of this reduction on less-educated women's future human capital framed within the traditional Mincerian model. We find that wage losses one year out from 2020 are relatively modest on average, generally less than one percent, with the largest for married women without children in the home. But losses are greater for young married women, mothers with very young children, and those working in COVID-impacted industries. School and child care closures increase negative wage impacts for married mothers by an additional 50 percent.

The 2020 COVID-19 pandemic recession reduced employment by 22 million workers, a 14 percent drop, and increased the unemployment rate to 15 percent, all within two months. Yet these indicators recovered within 10 months: employment reductions fell to 7 percent relative to the initial point and the unemployment rate fell to 7 percent, creating the so-called "V-shaped" recession. The magnitude of the losses and the sharp rebound made the recession unlike any other recession in the last 80 years.

There was much discussion on the impact of the recession on women. That discussion was 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, larger than the 18 percent drop for 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), Albanesi and Kim (2021), Alon et al. (2021)). However, women's employment recovered faster than that of 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),Fairlie et al. (2021), Mongey 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

[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.

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 women with less than a college degree because college 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 projection, including (i) allowing the impact of a recession to differ if the woman was in an industry that was especially (later) impacted by COVID 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 in 2020, 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 these are only projections, not forecasts, made under the assumption that the estimated model is correctly specified and 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. The largest effects are for married women without children in the home, who have high returns to working and who therefore lose the most human capital in a recession. Losses are also greater for married women at young ages, mothers with very young children, and for those working in COVID-impacted industries. School and child care closures increase projected negative wage impacts for married mothers by an additional 50 percent. We also find some suggestive evidence that an increase in part-year work projected to occur during the pandemic could increase the size of human capital losses for some women, although the estimates are imprecisely determined.

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. In addition to focusing on mass layoffs rather than general unemployment as our work does, this literature usually studies men and mostly uses earnings as the outcome variable, while our work examines women and utilizes the hourly wage–a better proxy for human capital–as the outcome. 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.

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 a 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 occur 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 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 $t \quad 1$, 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 that $k_t$ declines over time because of rising opportunity costs and decreasing remaining years of work life, and that it declines linearly:

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

Then

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

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

$$rk_t = \quad \eta_t + \quad {}_tE_t \tag{5}$$

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 $\eta_t$. The human capital accumulation function is consequently

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

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 $_t$, 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 $_t$ in the same spirit as the original Mincer model. A quadratic profile is consistent with a high $_t$ at early ages, before children arrive and women are working and investing in their human capital, a low $_t$ in the middle years when young children are present and women are either not working or working at part-time jobs with smaller human capital content, and a higher $_t$ at later ages after the children have grown older or left the home and women have returned to work and to investing in skills.[3]

With a quadratic profile for $_t$ and an assumption that depreciation is constant, we have

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

$$_\tau = \quad + \gamma\tau\psi + \psi\tau^2 \tag{8}$$

which generates the wage equation

$$lnW_t = \alpha \quad \eta(t \quad 1) + \sum_{\tau=1}^{\tau=t \quad 1} E_\tau + \sum_{\tau=1}^{\tau=t \quad 1} \tau E_\tau + \sum_{\tau=1}^{\tau=t \quad 1} \tau^2 E_\tau \tag{9}$$

Wages are a function of total work experience only if $E_\tau = 1$ for all $\tau$, which does not hold for women. The impact of past work on current human capital and wages depends, instead,

[3] 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 below for whether investments are greater for full-year and part-year work to allow both hours and employment to affect wages. In an important early paper, Light and Ureta (1995) estimated a log wage equation for women which had lags in the amount of work prior to the current period, allowing separate coefficients on each lag.

on when the work occurred during the lifetime since a different quantity of investment is made at different ages, which implies that total investment will be different if total years of work experience are held constant but the investment takes place at different times. With the quadratic assumption on the investment profile in eqn(8), wages will be cubic in $t$ if $\neq 0$.

## 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

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

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 $\epsilon_{it}$ have been added.[4] The variable $t$ is years since the beginning of work and, as we describe below, we will define it as age minus 25, meaning that age is controlled in the regression (it picks up $\eta$, the rate of depreciation when not working).

We wish to estimate the experience parameters, , , and using variation in experience induced only by business cycle variation.[5] We start by specifying a reduced-form first-stage equation

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

[4] Both $\alpha$ and $\eta$ are slightly redefined.

[5] Work experience can be endogenous for many reasons, with perhaps the oldest argument that unobserved ability is in the error term and is correlated with past work. Eckstein and Wolpin (1989) was perhaps the first to note this in a formal econometric model, although using a quadratic in total experience. Even though endogeneity is not the immediate motivation for our use of business cycle variation to induce variation in experience, it can be viewed as addressing that issue.

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

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

Using only the business cycle variables $B_{i\tau}$ to identify the parameters , , and  requires that all $X_{i\tau}$ for $\tau \neq 1, ..., t$  1 be included in $X_{it}$ in eqn(10). 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 lifetime historical variables in eqn(10) is infeasible.[6] We shall instead, when predicting employment, hold the variables in $X_{i\tau}$ constant at their cohort means for each observation, allowing only the individual-specific history of business cycles to identify the coe cients on experience. We provide exact details on our IV procedure in the next section.

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 in 2020 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 in projected wages represents the estimated 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.

[6]Education and race are time-invariant in our $X$ vector, but the rest are time-varying.

## 2. Data and Equation Specification

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 industry, marital status, etc.) of almost all women in our sample back to age 25 (1968 is 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.[7] Importantly, we select women with less than a college degree for our analysis for, consistent with past work, preliminary analysis indicated little if any impact for college-educated women. 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 13,315 pooled over women and years, with 1,832 unique women and an average of 7.27 years per woman.[8] Potential years of experience, $t$, is measured as age minus 25. Data construction details appear in Online Appendix B.

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. Reductions in employment at the beginning of the pandemic were greatest for unmarried mothers and unmarried women without children present, for example. We therefore stratify by marital status and the presence of children in the model and estimate the model, both eqns(10) and (11)-(12), separately for four groups (married and unmarried women, with and without children).[9] We measure these family structure variables as of the interview date (and hence represent women by their current marital status and presence of

[7] We could start the lifetime at the first year after school completion, but we would need to go further back beyond 1968 in that case.

[8] 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.

[9] This requires the assumption that those family structure variables are exogenous, but a similar assumption is made in much of the literature on women's labor supply, and treating those variables as endogenous is beyond the scope of the exercise.

children, not by their status at previous ages, which is often different).

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-year work, we 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. We have 35,981 observations on employment pooled over years and women. Our annualized employment rate averages a little over 50 percent.

For the key business cycle variables in the first-stage employment 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 (which we henceforth call "COVID employment"), 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, namely, 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 and the fourth (the recession indicator) is identified from deviations from trend.

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.[10] Of particular interest are the COVID and total employment variables which, while highly positively correlated, vary in their relationship over time.[11] This variation (albeit at the state level in our regressions) allows us to estimate the separate impacts of COVID 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, which will allow us to project differential impacts of the pandemic along those dimensions.

For the rest of the $X_{it}$ vector in the employment equation, 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.[12] We interact the child age variables with the business cycle variables to determine if womens' employment response to a downturn depends on the age of her children (we will also use this interaction variable in a later analysis of school and child care provider closures). We also add a race variable for nonwhite women and test for interactions between it and the business cycle variables as well.

With the employment equation estimated, we estimate the second-stage log wage

[10] Also, variation in the unemployment rate holding employment fixed will pick up labor force effects.

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

[12] 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.

equation in eqn(10) using predicted experience variables over each woman's history back to age 25. To ensure that the experience coe cients in the wage equation are identified solely from variation in each women's history of business cycle variables and when they occurred in her lifetime, we predict employment at each past age using means of the $X$ variables for each woman taken over all observations for her cohort and family structure (i.e., marital status and presence of children category). Because age, year, and family structure are in the wage equation, then, conditional on those variables, predicted employment will vary only because women in that cohort in that year had differing business cycle histories. We term this procedure Cohort IV.[13]

We test for selection into the wage sample by entering a traditional selection bias term in the wage equation. The selection bias term is constructed from the first stage employment equation but it uses current values of the business cycle variables (which affect the current probability of working) and not the past variables which enter the prediction of the experience variables, and hence is identified from that difference. 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, eqns(11)-(12), are shown in Table 1.[14] 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

[13]To give an example, for a wage observation for a woman who is age 35 in 1980 and married with children at that time, her past employment history is predicted using the actual business cycle variables at each past age and year going back to age 25, but using the means of the $X$ variables at each past age and year (including family structure at each) taken over all women who were married with children in 1980 and age 35 in that year. Thus all women in the wage equation of a particular age, year, and family structure will have predicted experience variables that vary only from their business cycle variation.

[14]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.

the unemployment rate. We will show suggestive evidence below that the negative effect of total employment for unmarried mothers is a result of 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, it 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 coe cient is positive for all family structure groups (but of low levels of significance for three of them), consistent with the interpretation that an increase in aggregate COVID employment in the state has a greater effect on women who are in the 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 for more women, 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 for most women 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 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, eqn(10), appear in Table 2 for the four family structure groups separately (keeping in mind that the effects are for women currently in that group, even if having transitioned into it from a different group earlier).[15] While the individual coe cients on the three experience variables are often insignificant at conventional levels, they are jointly highly significant for all three family structure groups except for unmarried mothers, who have no significant curvature in their experience profiles and have constant returns. The positive, negative, and positive signs on the first, second, and third experience variables for three of the family structure groups are consistent, in sign, with the cubic in experience hypothesized above. Under the Mincer interpretation, these signs imply that investments are high at young ages, decline as women grow older, but that the rate of decline slows or even reverses at older ages.[16]

The derivative of the log wage equation with respect to potential experience (age minus 25) is of most interest. Figure 2 shows the implied estimated returns to one additional year of age by family structure group. The figure shows that, despite the joint significance of the three experience coe cients for three of the groups, estimated rates of return are not too far from linearity, especially for married mothers, whose returns fall essentially linearly with age. There is second-order curvature for both groups of childless women but it is mild, implying that investments continue to decline at older ages but at a slower rate (and, in fact, the curves essentially flatten out at the end).[17] Keeping in mind that these curves are identified by business cycle variation in past recessions, the curves imply that the greatest losses of human capital will occur when married women (both

[15]The selection bias term has a very high standard error in all wage specifications and is consequently not included in these and the other wage equation estimates reported below.

[16]We note that the coe cient on age is negative for three of the groups, although low in significance for two of them. A negative sign is consistent with depreciation of human capital during periods of nonwork in the Mincer model.

[17]Online Appendix Table 1 shows wage equation estimates when the third, cubic term is dropped. Online Appendix Table 2 shows the implied wage impacts, to be compared to those for Table 3 below.

mothers and childless) are young and when married childless women are older, for these are when investments appear to be the largest. The much lower and flatter effects for unmarried mothers may be because those women are often very unskilled and have jobs with very little human capital accumulation content at all ages. This implies that their wage losses from cyclical downturns are likely to be small, with very little loss of human capital.[18]

While we are most interested in these short-run impacts of business cycles on women of different family structures at the time of the cycle–that is, one or two years out from the recession, holding their family structure fixed–it should be noted that the effects shown in Figure 2 are not life cycle profiles because women change their family structure as they age, as noted earlier. Mean life cycle returns can instead be approximated by weighting the effects at each age in Figure 2 by the fraction of women in each family structure at each. Appendix Figure A1 shows the result of that calculation. Returns decline over time but flatten out at older ages, which is mostly a result of a gradual movement to the married childless category with its relatively high level of investments.

We project the short-run impact of the 2020 pandemic recession on 2021 wages for each separate family structure group by first predicting the employment impact for the 2017 sample 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 3A. All net wage impacts are negative but modest in

[18] A useful comparison to our estimated returns is provided by Light and Ureta (1995), who estimate women's log wage equations with cumulative experience and experience squared, as in the standard specification for men, but also for a specification which allows past work to have different effects on current wages, depending on the lag length (but ignoring the specific cause of past work levels and hence not specific to business cycle effects). They only examined the first few years of work but found returns to the first three years of work experience somewhat larger than those in our Figure 2 (but they do not separate by family structure) and also higher than obtained using the usual male specification with total years of experience and experience squared.

magnitude, always less than one percent. The largest impacts are those for the two groups of married women, while those for unmarried women are much smaller and those for unmarried mothers are effectively zero (as a result of the very low rate of investments shown in Figure 2 and despite their having the largest employment losses). Unmarried childless women have slightly greater rates of investment than unmarried mothers but also smaller employment losses. We emphasize that these are mean impacts across women of different ages and hence are weighted averages of the effects shown in Figure 2. Women of different family structures have different age distributions, which will affect these means. In our heterogeneity section below, we will show wage impacts by age, which will line up more closely to the curves in Figure 2.[19]

While our main interest is in the pandemic, we also estimate projected wage impacts for the Great Recession and the 1991 Recession. We use the same method, projecting wage losses one year into the recession with the counterfactual calculation holding the business cycle variables fixed at their values the year before the recession. As shown in Online Appendix Table 4, the negative wage impacts are generally somewhat smaller than in the pandemic. While there were some differences in the characteristics of the women in those years (state distribution, age, industry and occupation, and so on), the main reason for the difference is that the business cycle one year into those recessions was less severe than in the pandemic.

Heterogeneity. These projected impacts are at the mean of all the variables in the model–age, state of the business cycle in the state of residence, industry and occupation a liation, ages of children, and the other variables in the model. As we will show in this section, the modest mean market wage impacts we project mask significant heterogeneity.

[19] But because the impact of the recession on employment is different for the different family structures, as also shown in Table 3, the projected impact of the pandemic could differ across family structure groups for that reason as well, even holding age fixed. We conducted a counterfactual exercise to isolate the contribution of differing employment impacts to the differences in wage impacts across the four family structure groups by using the same business cycle coe cients in Table 1 for all (setting them equal to the average) and reprojecting the wage impact. Online Appendix Table 3 shows that this has very little effect on the variance of wage impacts across the groups, implying that it is more the differing investment profiles in Figure 2 that are generating the group differences.

An overall sense of heterogeneity is shown in Figure 3, which shows the distribution of percent wage impacts across the sample for all four family structure groups.[20] Married women have a wide spread of impacts, with a left tail including impacts between 1 and 2 percent, much greater than those at the mean shown in Table 3. But unmarried women have the smallest heterogeneity, with effects relatively concentrated around the mean.

The sources of this heterogeneity can be seen by isolating its several dimensions. Figure 4 illustrates heterogeneity of impact by the differing business cycle impacts in the state of residence, showing differences by the level of the state unemployment rate, the level of total employment per capita, and the level of COVID industry employment per capita, in each case showing the net impact of the pandemic for women with values below and above the median. Figure 4(a) shows that living in a state with an above-median increase in the unemployment rate is projected to have a large impact on market wage declines, with wage losses for married women almost double those for women living in states with below-median increases in unemployment and about 50 percent larger for unmarried childless women (but no effects for unmarried mothers in any of these figures). For total employment (Figure 4(b)) wage declines are again much larger for the same groups of women living in states with below-median growth in employment, though the magnitudes are smaller for unmarried childless women. Figure 4(c) shows impacts specifically for women living in states with larger or smaller declines in COVID employment, showing larger negative impacts on wage rates for married women living in states with below median growth in employment in those industries. But the impact for unmarried childless women does not appear and is slightly positive, which we interpret as near-zero.

Possibly more interesting is heterogeneity by age, employment in a COVID industry, and age of the youngest child, shown in Figures 5(a)-5(c). For married mothers, the projected negative impacts of the pandemic on market wage rates are largest for mothers at

[20] This distribution is not a result of sampling error or imprecision of the parameter estimates, but entirely the result of variation in the observables in the model.

young ages, and fall to zero for older women (Figure 5(a)).[21] 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. The negative impact of the pandemic on the wage rates of married women working in industries impacted by COVID is projected to be slightly larger than for those working in other industries (Figure 5(b)) but the difference is 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-impacted industries (Table A2), so the larger impacts for those working in such industries pushes up mean impacts non-trivially. Figure 5(c) shows projections of wage impacts of the pandemic by the age of the youngest child for women with children. The negative impacts decline monotonically with the age of the youngest child for married mothers but are near-zero for unmarried mothers.

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.[22] 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 during recessions has not occurred on any scale in the

[21] A few of the impacts for older women are positive, which is a result of extrapolation of the marginal returns relationship to regions where that return is slightly negative. We interpret these impacts as effectively zero.

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

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. The authors found no differential impacts by education but did not disaggregate by marital status, so we assume the impact to be the same for married and unmarried women.[23] 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 3B shows the resulting 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 about half the size of the baseline wage impacts in Table 3A, implying a 50 percent increase in those impacts from school and childcare closures. For

[23]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. A revised version of this work (Russell and Sun (2022)) found larger effects for unmarried women and low income mothers, although imprecisely determined. Our sensitivity tests below gauge the impact of increasing the estimates of these effects.

unmarried mothers, however, the wage impact is negligible both because the study referenced above found no employment effects of school closures for them and because of the low impact of job losses on wages for those women, already discussed. For the former, see the sensitivity test below. Figure 5(d) shows effects broken out by age of the youngest child graphically, finding the largest negative wage impacts to have occurred for mothers with the youngest children.

# 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. The results of all 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 test a cutoff of 1800 hours as a rough way to test this possibility (but the employment rate only drops from about 50 percent to 47 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.[24]

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 slightly greater for the 1800 hours cutoff. These results support the hypothesis that women working longer hours in the year, at least at the top of that

[24] Online Appendix Figure 1 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.

distribution, are making larger human capital investments and hence suffer slightly greater losses of human capital from recessions. Interestingly, for three of the four family structure groups, using a 1400-hour cutoff also has a greater effect than those for the baseline. The lower hours cutoff begins to approach what many would 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 (Table 1). 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.[25] With 57 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 28 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 there is not discernible impact. This suggests that the telecommutable effect for women in those occupations, which reduces the negative wage impact, outweighs the effect of being in a high-contact occupation. For married mothers, the high contact effect equals the telecommutable effect.

Child Care and School Closures. We previously noted two limitations in the past

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

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 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 Child Care/School 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.

### 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 tests 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.

We briefly examine this issue by including a category for part-year work, estimating how recessions affect it, how those part-year employment effects impact wages, and whether this changes our projected COVID wage impacts. We define part-year work as having annual hours between 300 and 1600, with nonwork now defined as less than 300 hours. 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 and then reestimate our wage equations with one additional variable, total years of part-year work over the lifetime as of time $t - 1$ (year before the wage observation), not allowing for effects at different ages for simplicity. The first stage part-year and full-year employment equations (not shown) reveal that part-time work often increases in recessions, which would not be surprising if many workers move from full-time work to part-time work. Many of the positive effects of employment on part-year work are also smaller in magnitude than for full-year work.

Online Appendix Table 5 shows estimates of the wage equation with total years of part-year work experience added. For all family structure groups except married mothers, increases in part-year work decrease, not increase, wage rates, but at very low levels of precision. Even though we cannot rule out the possibility that the data do not have the power to detect effects at a high enough significance level, the negative effects of part-year experience are consistent with the widespread view in the literature that part-year work has little or no human capital content. For the sake of completeness, we also conduct wage projections with the results shown in Online Appendix Table 6. For all groups, recessions have a positive impact on part-year work and, for both groups of unmarried women, the negative wage impacts are quite a bit higher than in our baseline, especially for the unmarried mothers whose effects have been effectively zero heretofore. The larger wage impacts arise from the movement to part-time work, which has negative effects on wages. These findings are only suggestive and deserve attention using data sets with more power or more precision.

## 4.3 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 pursue a far less ambitious exercise by simply estimating the rate of industry and occupational mobility that has occurred over past recessions, and relating that to the same business cycle indicators 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.

Online Appendix Tables 7 to 9 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 employment, on the other hand, generally has positive effects on mobility. COVID-impacted employment (when occurring without a reduction in non-COVID-impacted employment) usually has a positive impact on mobility but smaller than that for total employment. Women working in COVID-impacted industries usually have much smaller probabilities of occupation or industry change although sometimes insignificant or the opposite sign. Having children

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

reduces the probability of change for married mothers and increases it for unmarried mothers.

Table 5 shows the results of our usual projection exercise plugging in actual 2020 business cycle variables and comparing to results using 2019 variables. We project declines in occupational mobility and increases in industry mobility for married mothers, while the reverse holds true for married women without children. These mixed results are a result of the mixed impacts of the business cycle variables discussed in the prior paragraph. For unmarried women, occupational mobility is projected to increase but mixed results are obtained for industry mobility. The percentage effects are fairly small and we conclude that there is no indication from this analysis of a large mobility impact of past economic downturns and therefore projected mobility in COVID.

## 4.4 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 6. 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 relative to what is projected to have actually happened. 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 6, are slightly greater than those projected for 2021 and shown in Table 3A. 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 with less than a college degree 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, although somewhat larger for married women than for unmarried women and for those working in COVID-impacted industries. For married women, losses are somewhat larger for younger married mothers, for younger and older married childless women, and for married mothers with older children. But school closures are also important for married women with school-age children and increase negative wage impacts by 50 percent. We have also found suggestive but imprecise evidence that an increase in part-year work projected to occur during the pandemic increases the size of human capital losses for some women. 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 are factors 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 and hence their impacts on wages can also not be captured historically. This factor should have resulted in greater losses relative to the assumed counterfactual estimates used here, but whether they would affect our main results on the modest size of the wage losses is not clear. 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 for the magnitude of effects stemming 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. These two missing factors may consequently not have large impacts on our estimates of rather modest human capital losses for women from the Pandemic.

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 di cult. The impact on future wages of the 2020 downturn by itself, for example, will have to address the continued labor market 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 di cult 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

SOURCE.— US Bureau of Labor Statistics
NOTE.— COVID Employment is employment in COVID-impacted industries. The left vertical axis represents deviations from a linear trend for both employment variables.

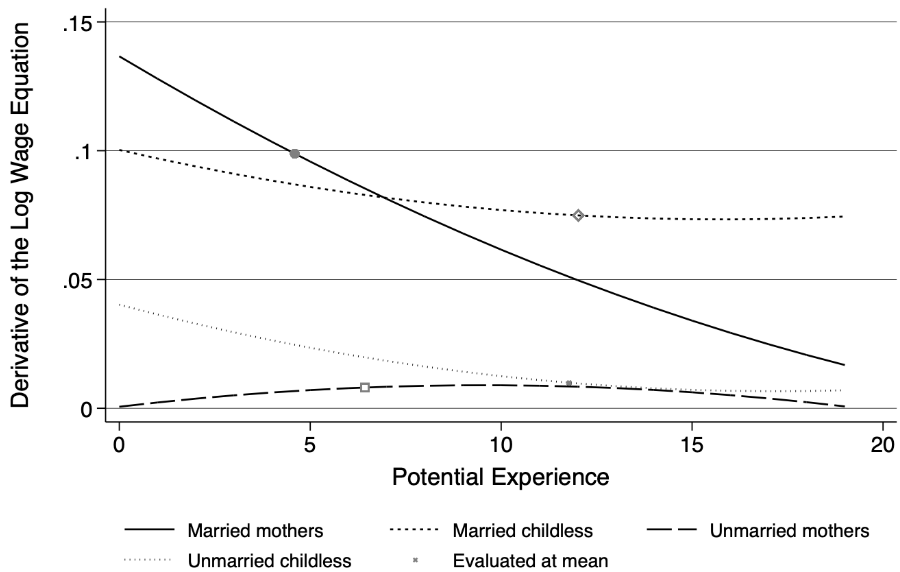



Figure 2: Derivative of the Log Wage Equation with respect to Potential Experience (t) by Family Structure

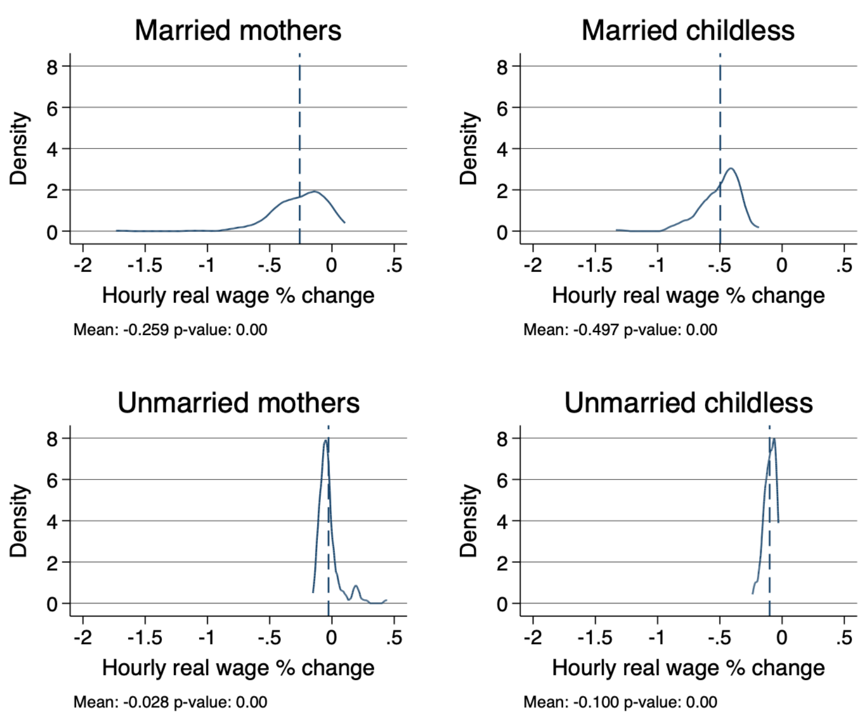



Figure 3: Distribution of projected COVID Wage Effects by Family Structure

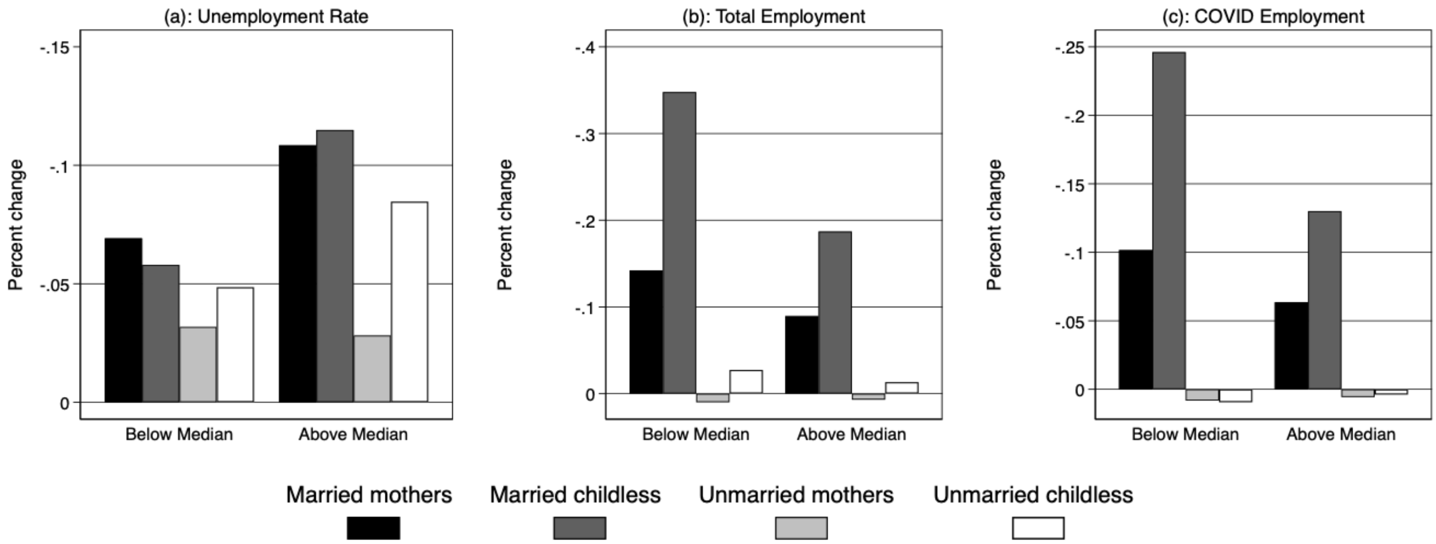



Figure 4: COVID Wage Effects for Alternative State Business Cycle Variables by Family Structure

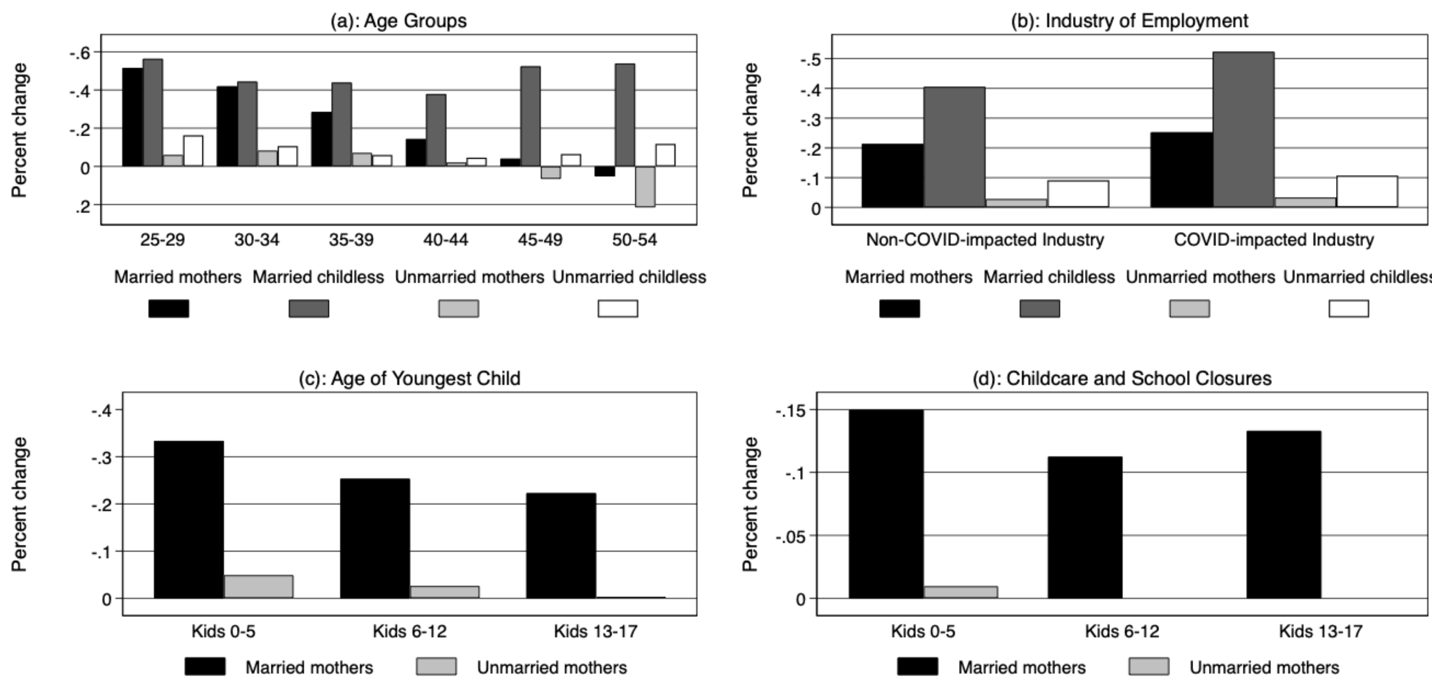



Figure 5: COVID Wage Effects by Age of the Woman, Industry of Employment, Age of the Youngest Child, and Incremental Effects from Child Care and School Closures by the Age of the Youngest Child

Table 1: Employment Equation

| | Married | | Unmarried | |
|---|---|---|---|---|
| | Mothers | Childless | Mothers | Childless |
| Unemployment Rate | 0.004 | 0.002 | 0.029*** | 0.009** |
| | (0.003) | (0.005) | (0.006) | (0.004) |
| Log per-capita COVID Employment | 0.292 | 0.450* | 0.004 | 0.561*** |
| | (0.186) | (0.234) | (0.251) | (0.200) |
| Log per-capita Total Employment | 0.500** | 0.592* | 0.454 | 0.272 |
| | (0.230) | (0.333) | (0.334) | (0.262) |
| Recession Indicator | 0.016** | 0.031** | 0.023* | 0.030*** |
| | (0.007) | (0.013) | (0.013) | (0.011) |
| Emp. COVID × COVID-impacted Industry | 0.195 | 0.363 | 0.008 | 0.399* |
| | (0.200) | (0.251) | (0.257) | (0.216) |
| Emp. Total × COVID-impacted Industry | 0.073 | 0.142 | 0.244 | 0.267 |
| | (0.266) | (0.316) | (0.320) | (0.297) |
| Emp. Total × Occupation can telecommute > 25% | 0.036 | 0.019 | 0.289** | 0.188 |
| | (0.116) | (0.176) | (0.142) | (0.129) |
| Emp. Total × Nonwhite | 0.265 | 0.352 | 0.122 | 0.031 |
| | (0.189) | (0.392) | (0.164) | (0.199) |
| Emp. COVID × Youngest Child ages 6-12 | 0.313** | | 0.320 | |
| | (0.144) | | (0.200) | |
| Emp. COVID × Youngest Child ages 13-17 | 0.141 | | 0.511** | |
| | (0.203) | | (0.248) | |
| Emp. Total × Youngest Child ages 6-12 | 0.178 | | 0.578** | |
| | (0.197) | | (0.249) | |
| Emp. Total × Youngest Child ages 13-17 | 0.512* | | 0.722** | |
| | (0.290) | | (0.324) | |
| Unemployment × Youngest Child ages 6-12 | 0.007 | | 0.010 | |
| | (0.005) | | (0.008) | |
| Unemployment × Youngest Child ages 13-17 | 0.025*** | | 0.012 | |
| | (0.007) | | (0.010) | |
| COVID-impacted Industry | 0.163 | 0.375 | 0.180 | 0.381** |
| | (0.157) | (0.239) | (0.225) | (0.189) |
| Occupation can telecommute > 25% | 0.043 | 0.097 | 0.141 | 0.202* |
| | (0.096) | (0.140) | (0.118) | (0.108) |
| High Contact Occupation | 0.005 | 0.002 | 0.00001 | 0.003 |
| | (0.018) | (0.025) | (0.026) | (0.026) |
| Nonwhite | 0.199 | 0.294 | 0.088 | 0.027 |
| | (0.164) | (0.321) | (0.134) | (0.165) |
| Age - 25 | 0.001 | 0.008*** | 0.008*** | 0.029*** |
| | (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 | 0.236** | | 0.066 | |
| | (0.114) | | (0.196) | |
| Youngest Child ages 13-17 | 0.366** | | 0.233 | |
| | (0.175) | | (0.233) | |
| Predicted Log(1000 + Spouse Earnings) | 0.124*** | 0.047 | | |
| | (0.045) | (0.064) | | |
| Constant | 9.271*** | 4.021 | 0.371 | 5.776** |
| | (1.713) | (2.521) | (2.313) | (2.350) |
| Observations | 17140 | 7433 | 5727 | 5681 |
| Mean of Dependent Variable | 0.42 | 0.60 | 0.63 | 0.70 |
| Median F-Statistic | 17.12 | 11.01 | 10.50 | 9.09 |

NOTE.— *p<0.1; **p<0.05; ***p<0.01. OLS. Standard errors calculated by bootstrapping with 500 replications. "Emp. Total" is Log per-capita Total Employment. "Emp. COVID" is Log per-capita COVID employment, and "Unemployment" is the unemployment rate. The Median F-Statistic is the median of F-statistics for the coefficients involving business cycle variables over the bootstrapped draws. Predicted spousal earnings come from the Zero-th Stage estimates where the dependent variable is log(1000 + Spouse Earnings) in real terms.

Table 2: Wage Equation

| | Married | | Unmarried | |
|---|---|---|---|---|
| | Mothers | Childless | Mothers | Childless |
| Total Experience | 0.137*** | 0.100** | 0.001 | 0.040 |
| | (0.043) | (0.042) | (0.045) | (0.038) |
| $t$-weighted Experience | −0.009 | −0.003 | 0.002 | −0.004 |
| | (0.009) | (0.004) | (0.005) | (0.003) |
| $t^2$-weighted Experience | 0.0001 | 0.0001 | −0.0001 | 0.0001 |
| | (0.0004) | (0.0001) | (0.0002) | (0.0001) |
| Age - 25 | −0.002 | −0.043** | 0.007 | −0.005 |
| | (0.005) | (0.020) | (0.019) | (0.029) |
| Year | 0.005*** | 0.004 | 0.003* | 0.005** |
| | (0.002) | (0.003) | (0.002) | (0.002) |
| Nonwhite | −0.149*** | −0.246** | −0.140*** | −0.128** |
| | (0.050) | (0.107) | (0.041) | (0.060) |
| Youngest Child ages 6-12 | −0.040* | | 0.046* | |
| | (0.023) | | (0.027) | |
| Youngest Child ages 13-17 | 0.028 | | 0.062* | |
| | (0.035) | | (0.037) | |
| Constant | −7.811** | −5.796 | −3.506 | −7.861* |
| | (3.596) | (5.758) | (3.064) | (4.417) |
| Observations | 5230 | 2998 | 2472 | 2615 |
| Mean of Dependent Variable | 2.59 | 2.74 | 2.60 | 2.75 |

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

Table 3: COVID Wage Projection

| Panel A: Effect through Business Cycle Variables | | | | |
|---|---|---|---|---|
| | Married | | Unmarried | |
| | Mothers | Childless | Mothers | Childless |
| Hourly Real Wage | | | | |
| Actual Mean Wage | 20.94 | 16.73 | 14.90 | 17.22 |
| Counterfactual Mean Wage | 20.99 | 16.81 | 14.90 | 17.23 |
| Mean Percent Change | -0.259 | -0.497 | -0.028 | -0.100 |
| Predicted Experience | | | | |
| Actual Mean | 7.05 | 10.93 | 6.83 | 10.50 |
| Counterfactual Mean | 7.11 | 10.99 | 6.93 | 10.56 |
| Mean Percent Change | -1.346 | -0.919 | -2.661 | -1.291 |

Panel B: Additional Effects of Child Care/School Closures

| | Mothers | |
|---|---|---|
| | Married | Unmarried |
| Hourly Real Wage | | |
| Mean Wage under Child Care/School Closures | 20.963 | 14.905 |
| Counterfactual Mean Wage | 20.989 | 14.905 |
| Mean Percent Change | -0.132 | -0.003 |
| Predicted Experience | | |
| Mean under Child Care/School Closures | 7.088 | 6.921 |
| Counterfactual Mean | 7.114 | 6.928 |
| Mean Percent Change | -0.544 | -0.241 |

NOTE.— "Actual Mean Wage" is the predicted 2021 wage using actual 2020 business cycle variables and "Counterfactual Mean Wage" is the predicted 2021 wage assuming 2019 business cycle variables in 2020.

Table 4: Mean Percent Change in Hourly Wages under Alternative Specifications

| | Married | | Unmarried | |
|---|---|---|---|---|
| | Mothers | Childless | Mothers | Childless |
| Employment Definition | | | | |
| Projection for 1600 Hours (baseline) | -0.26 | -0.50 | -0.03 | -0.10 |
| Projection for 1800 Hours | -0.28 | -0.65 | -0.04 | -0.25 |
| Projection for 1400 Hours | -0.29 | -0.61 | -0.11 | -0.06 |
| Occupation Effects | -0.26 | -0.50 | -0.03 | -0.10 |
| Child Care and School Closures | | | | |
| Baseline Closures | -0.39 | | -0.03 | |
| More Child Care/School Closures | -0.42 | | -0.04 | |
| Child Care/School Closures by Age of Children | | | | |
| Baseline Closures | | | | |
| Youngest Child ages 0-5 | -0.46 | | -0.06 | |
| Youngest Child ages 6-12 | -0.34 | | -0.02 | |
| Youngest Child ages 13-17 | -0.34 | | -0.00 | |
| More Child Care/School Closures | | | | |
| Youngest Child ages 0-5 | -0.54 | | -0.07 | |
| Youngest Child ages 6-12 | -0.34 | | -0.03 | |
| Youngest Child ages 13-17 | -0.34 | | -0.01 | |

NOTE.— “Employment Definition” tests different cutoffs for defining employment in the year. “Occupation Effects” test the impact of larger telecommutable effects.

Table 5: Job Change Effects under Counterfactual Analysis

| | Married | | Unmarried | |
|---|---|---|---|---|
| | Mothers | Childless | Mothers | Childless |
| Occupation change | | | | |
| Actual Median | 0.259 | 0.201 | 0.372 | 0.257 |
| Counterfactual Median | 0.271 | 0.192 | 0.325 | 0.238 |
| Median Percent Change | -1.124 | 3.125 | 12.529 | 7.766 |
| Industry change | | | | |
| Actual Median | 0.188 | 0.153 | 0.276 | 0.191 |
| Counterfactual Median | 0.187 | 0.159 | 0.253 | 0.193 |
| Median Percent Change | 3.084 | -2.457 | 8.191 | -1.112 |
| Either change | | | | |
| Actual Median | 0.329 | 0.258 | 0.450 | 0.323 |
| Counterfactual Median | 0.321 | 0.248 | 0.397 | 0.299 |
| Median Percent Change | 3.616 | 3.029 | 10.906 | 8.218 |

NOTE.— Cell entries are the probabilities of occupation and industry change under the actual 2020 business cycle variables and the 2019 business cycle variables.

Table 6: Wage Projections in 2022

| | Married | | Unmarried | |
|---|---|---|---|---|
| | Mothers | Childless | Mothers | Childless |
| Hourly Real Wage | | | | |
| Actual Mean | 21.42 | 16.96 | 15.06 | 17.41 |
| Counterfactual Mean | 21.48 | 17.06 | 15.07 | 17.43 |
| Mean Percent Change | -0.30 | -0.59 | -0.03 | -0.12 |
| Predicted Experience | | | | |
| Actual Mean | 7.61 | 11.56 | 7.47 | 11.12 |
| Counterfactual Mean | 7.69 | 11.63 | 7.58 | 11.20 |
| Mean Percent Change | -1.33 | -0.93 | -2.32 | -1.15 |

NOTE.— See footnotes to prior tables.

# Appendix Figures and Tables

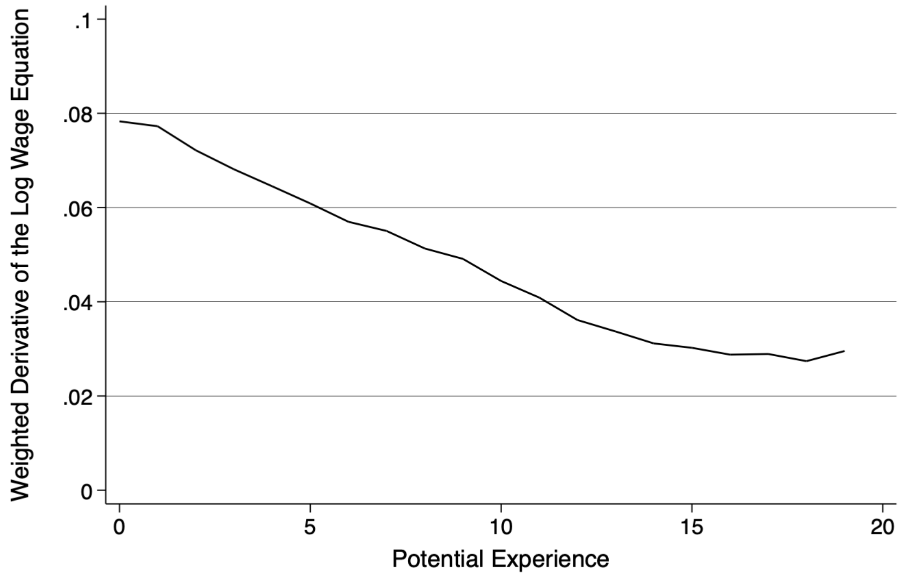



Figure A1: Weighted Effect of an Additional Year of Potential Experience

NOTE.— Weighted value of Figure 2 using the fractions of the sample in the different family structure categories at each year of potential experience.

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 COVID Employment | -1.65 | 0.15 | -2.37 | -0.80 |
| Log per-capita Total Employment | -0.84 | 0.12 | -1.39 | 0.19 |
| Recession 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 | 38.02 | 8.22 | 25 | 54 |
| COVID-impacted Industry | 0.63 | 0.47 | 0 | 1 |
| Occupation can telecommute >25% | 0.57 | 0.48 | 0 | 1 |
| High Contact Occupation | 0.28 | 0.45 | 0 | 1 |
| Youngest Child ages 0-5 | 0.27 | 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) | 10.59 | 0.80 | 6.91 | 11.8 |
| Wage Observations | 13315 | | | |
| Observed Work Observations | 24967 | | | |
| Total Observations | 35981 | | | |

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

| | Married | | Unmarried | |
|---|---|---|---|---|
| | Mothers | Childless | Mothers | Childless |
| Employment Variables | | | | |
| Full-Year Log(Hourly Wage) | 2.591 | 2.737 | 2.602 | 2.747 |
| | (0.493) | (0.478) | (0.468) | (0.487) |
| Work† | 0.414 | 0.620 | 0.641 | 0.737 |
| | (0.493) | (0.485) | (0.480) | (0.440) |
| Employment Breakdown by Youngest Child | | | | |
| Youngest Child ages 0-5 | 0.335 | | 0.571 | |
| | (0.472) | | (0.495) | |
| Youngest Child ages 6-12 | 0.453 | | 0.652 | |
| | (0.498) | | (0.477) | |
| Youngest Child ages 13-17 | 0.542 | | 0.738 | |
| | (0.498) | | (0.440) | |
| Business Cycle Variables | | | | |
| Unemployment Rate | 6.149 | 5.989 | 6.247 | 6.192 |
| | (2.082) | (1.969) | (2.048) | (1.994) |
| Log per-capita Total Employment | -0.863 | -0.822 | -0.840 | -0.825 |
| | (0.119) | (0.096) | (0.126) | (0.109) |
| Log per-capita COVID Employment | -1.672 | -1.617 | -1.644 | -1.617 |
| | (0.157) | (0.132) | (0.145) | (0.132) |
| Recession Indicator | 0.174 | 0.149 | 0.150 | 0.146 |
| | (0.379) | (0.356) | (0.357) | (0.353) |
| Covariates | | | | |
| Age | 35.458 | 43.316 | 36.020 | 40.799 |
| | (6.635) | (8.665) | (6.909) | (8.989) |
| COVID-impacted Industry | 0.647 | 0.614 | 0.624 | 0.605 |
| | (0.462) | (0.479) | (0.473) | (0.479) |
| Occupation can telecommute >25% | 0.567 | 0.624 | 0.497 | 0.560 |
| | (0.480) | (0.477) | (0.489) | (0.486) |
| High Contact Occupation | 0.304 | 0.240 | 0.310 | 0.256 |
| | (0.460) | (0.427) | (0.463) | (0.437) |
| Youngest Child ages 0-5 | 0.441 | | 0.347 | |
| | (0.497) | | (0.476) | |
| Youngest Child ages 6-12 | 0.371 | | 0.432 | |
| | (0.483) | | (0.495) | |
| Youngest Child ages 13-17 | 0.187 | | 0.221 | |
| | (0.390) | | (0.415) | |
| Log(1000 + Spouse Earnings) | 10.608 | 10.545 | | |
| | (0.755) | (0.889) | | |
| Observations | 17140 | 7433 | 5727 | 5681 |

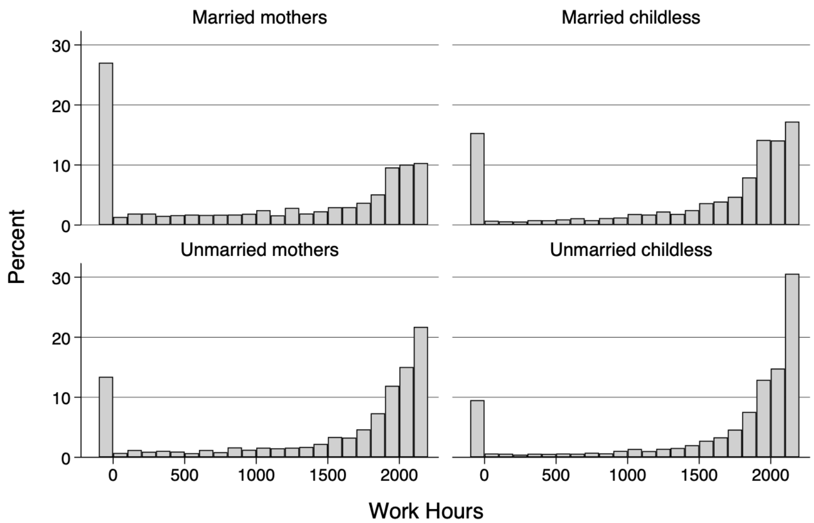
NOTE.— This table reports the means and standard deviations for women ages 25-54 that have not attained a college degree by marital status/children category. Per-capita variables use state-level population estimates from the US Census Bureau's Intercensal Tables. Employment data come from the Bureau of Labor Statistics. †Summary statistics for Work are calculated without any imputed values. 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 | 0.071*** | 0.069*** |
| | (0.007) | (0.012) |
| Unemployment Rate | 0.007 | 0.004 |
| | (0.006) | (0.010) |
| Log per-capita COVID Employment | 0.293 | 0.019 |
| | (0.287) | (0.576) |
| Log per-capita Total Employment | 0.400 | 1.163 |
| | (0.433) | (0.763) |
| Recession Indicator | 0.025** | 0.098*** |
| | (0.011) | (0.023) |
| Emp. COVID × COVID-impacted Industry | 0.152 | 0.221 |
| | (0.320) | (0.604) |
| Emp. Total × COVID-impacted Industry | 0.185 | 0.082 |
| | (0.446) | (0.801) |
| Emp. Total × Occupation can telecommute > 25% | 0.185 | 0.655* |
| | (0.221) | (0.341) |
| Emp. Total × Nonwhite | 0.501 | 0.452 |
| | (0.312) | (0.676) |
| Emp. COVID × Youngest Child ages 6-12 | 0.010 | |
| | (0.274) | |
| Emp. COVID × Youngest Child ages 13-17 | 0.169 | |
| | (0.363) | |
| Emp. Total × Youngest Child ages 6-12 | 0.205 | |
| | (0.342) | |
| Emp. Total × Youngest Child ages 13-17 | 0.123 | |
| | (0.536) | |
| Unemployment × Youngest Child ages 6-12 | 0.0004 | |
| | (0.007) | |
| Unemployment × Youngest Child ages 13-17 | 0.023 | |
| | (0.014) | |
| COVID-impacted Industry | 0.443 | 0.393 |
| | (0.297) | (0.501) |
| Occupation can telecommute > 25% | 0.048 | 0.417 |
| | (0.194) | (0.280) |
| High Contact Occupation | 0.002 | 0.0002 |
| | (0.031) | (0.058) |
| Nonwhite | 0.591** | 0.317 |
| | (0.287) | (0.550) |
| Age - 25 | 0.021*** | 0.036*** |
| | (0.003) | (0.002) |
| Year | 0.004*** | 0.002 |
| | (0.001) | (0.003) |
| Youngest Child ages 6-12 | 0.067 | |
| | (0.215) | |
| Youngest Child ages 13-17 | 0.143 | |
| | (0.288) | |
| Constant | 15.150*** | 10.905** |
| | (3.060) | (5.437) |
| Observations | 17140 | 7433 |
| F-Statistic | 714.59 | 235.18 |
| Mean of Dependent Variable | 10.61 | 10.54 |

NOTE.— *p<0.1; **p<0.05; ***p<0.01. OLS. Dependent variable is Log(1000 + Spouse Earnings), which is the spouse's annual earnings converted to real terms. Standard errors calculated by bootstrapping with 500 replications.

# Online Appendix Figures and Tables




Online Appendix Figure 1: Annual Hours by Marital Status and Children

NOTE.— Each bin captures 100 hours.

Online Appendix Table 1: Wage Equation - Linear Investment in Human Capital Specification

| | Married | | Unmarried | |
|---|---|---|---|---|
| | Mothers | Childless | Mothers | Childless |
| Total Experience | 0.123*** | 0.084** | 0.012 | 0.024 |
| | (0.031) | (0.035) | (0.035) | (0.036) |
| $t$-weighted Experience | 0.006** | 0.0001 | 0.001 | 0.001 |
| | (0.002) | (0.001) | (0.001) | (0.001) |
| Age - 25 | 0.003 | 0.046** | 0.008 | 0.006 |
| | (0.005) | (0.020) | (0.020) | (0.029) |
| Year | 0.005*** | 0.004 | 0.003** | 0.005** |
| | (0.002) | (0.003) | (0.002) | (0.002) |
| Nonwhite | 0.149*** | 0.243** | 0.140*** | 0.130** |
| | (0.050) | (0.106) | (0.041) | (0.060) |
| Youngest Child ages 6-12 | 0.041* | | 0.045 | |
| | (0.023) | | (0.027) | |
| Youngest Child ages 13-17 | 0.027 | | 0.063* | |
| | (0.035) | | (0.037) | |
| Constant | 7.963** | 6.018 | 3.544 | 8.094* |
| | (3.584) | (5.648) | (3.034) | (4.437) |
| Observations | 5230 | 2998 | 2472 | 2615 |
| Mean of Dependent Variable | 2.59 | 2.74 | 2.60 | 2.75 |

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

Online Appendix Table 2: COVID Wage Projections under Restricted Experience Specification

| Panel A: Effect through Business Cycle Variables | | | | |
|---|---|---|---|---|
| | Married | | Unmarried | |
| | Mothers | Childless | Mothers | Childless |
| Hourly Real Wage | | | | |
| Actual Mean Wage | 20.96 | 16.68 | 14.90 | 17.22 |
| Counterfactual Mean Wage | 21.01 | 16.76 | 14.90 | 17.23 |
| Mean Percent Change | -0.248 | -0.489 | -0.021 | -0.076 |
| Predicted Experience | | | | |
| Actual Mean | 7.05 | 10.93 | 6.83 | 10.50 |
| Counterfactual Mean | 7.11 | 10.99 | 6.93 | 10.56 |
| Mean Percent Change | -1.346 | -0.919 | -2.661 | -1.291 |

Panel B: Additional Effects of Child Care/School Closures

| | Mothers | |
|---|---|---|
| | Married | Unmarried |
| Hourly Real Wage | | |
| Mean Wage under Child Care/School Closures | 20.981 | 14.903 |
| Counterfactual Mean Wage | 21.007 | 14.903 |
| Mean Percent Change | -0.131 | -0.003 |
| Predicted Experience | | |
| Mean under Child Care/School Closures | 7.088 | 6.921 |
| Counterfactual Mean | 7.114 | 6.928 |
| Mean Percent Change | -0.544 | -0.241 |

NOTE.— Wage estimates drawn from Table A4. See notes to Table 3 for definitions.

Online Appendix Table 3: COVID Wage Projection Decomposition — Uniform Effect of Business Cycle

| Effect through Business Cycle Variables | | | | |
|---|---|---|---|---|
| | Married | | Unmarried | |
| | Mothers | Childless | Mothers | Childless |
| Hourly Real Wage | | | | |
| Actual Mean Wage | 22.83 | 19.08 | 14.94 | 16.99 |
| Counterfactual Mean Wage | 22.89 | 19.19 | 14.94 | 17.01 |
| Mean Percent Change | -0.293 | -0.585 | -0.018 | -0.117 |
| Predicted Experience | | | | |
| Actual Mean | 8.14 | 12.50 | 7.26 | 9.57 |
| Counterfactual Mean | 8.21 | 12.57 | 7.33 | 9.64 |
| Mean Percent Change | -1.305 | -1.056 | -1.964 | -2.316 |

NOTE.— "Actual Mean Wage" is the predicted 2021 wage using actual 2020 business cycle variables and "Counterfactual Mean Wage" is the predicted 2021 wage assuming 2019 business cycle variables in 2020. Business cycles have uniform effect by imposing same coefficients on business cycle variables from first stage equation across marital status and children categories.

Online Appendix Table 4: COVID Wage Projections for Other Recessions

Panel A: Effect of Great Recession

| | Married | | Unmarried | |
|---|---|---|---|---|
| | Mothers | Childless | Mothers | Childless |
| Hourly Real Wage | | | | |
| Actual Mean Wage | 20.65 | 16.72 | 14.80 | 17.15 |
| Counterfactual Mean Wage | 20.68 | 16.77 | 14.81 | 17.16 |
| Mean Percent Change | -0.143 | -0.297 | -0.019 | -0.059 |
| Predicted Experience | | | | |
| Actual Mean | 6.60 | 11.24 | 6.26 | 10.73 |
| Counterfactual Mean | 6.63 | 11.28 | 6.31 | 10.77 |
| Mean Percent Change | -0.686 | -0.538 | -1.533 | -0.649 |

Panel B: Effect of 1991 Recession

| | Married | | Unmarried | |
|---|---|---|---|---|
| | Mothers | Childless | Mothers | Childless |
| Hourly Real Wage | | | | |
| Actual Mean Wage | 18.037 | 14.691 | 14.445 | 17.099 |
| Counterfactual Mean Wage | 18.069 | 14.736 | 14.448 | 17.110 |
| Mean Percent Change | -0.185 | -0.306 | -0.021 | -0.062 |
| Predicted Experience | | | | |
| Actual Mean | 4.471 | 8.222 | 5.161 | 8.650 |
| Counterfactual Mean | 4.503 | 8.260 | 5.205 | 8.692 |
| Mean Percent Change | -1.132 | -0.865 | -1.597 | -0.796 |

NOTE.— "Actual Mean Wage" is the predicted hourly wage for the year after the first year of the recession using business cycle variables from the first year of the recession. "Counterfactual Mean Wage" is the predicted hourly wage using the business cycle variables from the year just prior to recession.

Online Appendix Table 5: Wage Equation with Full-Year and Part-Year Experience Terms

| | Married | | Unmarried | |
|---|---|---|---|---|
| | Mothers | Childless | Mothers | Childless |
| Total FY Experience | 0.121*** | 0.076* | 0.021 | 0.038 |
| | (0.032) | (0.046) | (0.047) | (0.036) |
| $t$-weighted FY Experience | 0.009** | 0.004 | 0.001 | 0.004 |
| | (0.005) | (0.004) | (0.004) | (0.003) |
| $t^2$-weighted FY Experience | 0.0002 | 0.0001 | 0.00005 | 0.0001 |
| | (0.0002) | (0.0001) | (0.0002) | (0.0001) |
| Total PY Experience | 0.058 | 0.033 | 0.120 | 0.065 |
| | (0.043) | (0.057) | (0.089) | (0.051) |
| Observations | 5230 | 2998 | 2472 | 2615 |
| Mean of Dependent Variable | 2.59 | 2.74 | 2.60 | 2.75 |

NOTE.— *p<0.1; **p<0.05; ***p<0.01. OLS. 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.

Online Appendix Table 6: COVID Wage Projections with Part-Year and Full-Year Experience

| | Married | | Unmarried | |
|---|---|---|---|---|
| | Mothers | Childless | Mothers | Childless |
| Hourly Real Wage | | | | |
| Actual Mean Wage | 20.10 | 17.66 | 17.25 | 16.71 |
| Counterfactual Mean Wage | 20.10 | 17.69 | 17.33 | 16.78 |
| Mean Percent Change | -0.041 | -0.197 | -0.469 | -0.381 |
| Predicted FY Experience | | | | |
| Actual Mean | 7.99 | 12.26 | 7.14 | 11.15 |
| Counterfactual Mean | 8.04 | 12.29 | 7.24 | 11.19 |
| Mean Percent Change | -0.84 | -0.43 | -2.86 | -0.86 |
| Predicted PY Experience | | | | |
| Actual Mean | 2.65 | 3.45 | 2.48 | 2.84 |
| Counterfactual Mean | 2.63 | 3.45 | 2.42 | 2.79 |
| Mean Percent Change | 1.41 | 0.73 | 4.21 | 4.79 |

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.

Online Appendix Table 7: Change of Occupation

| | Married | | Unmarried | |
|---|---|---|---|---|
| | Mothers | Childless | Mothers | Childless |
| Unemployment Rate | 0.017* | 0.021 | 0.001 | 0.013 |
| | (0.010) | (0.014) | (0.021) | (0.016) |
| Log per-capita COVID Employment | 1.061** | 0.459 | 0.554 | 0.717 |
| | (0.495) | (0.679) | (0.764) | (0.609) |
| Log per-capita Total Employment | 0.462 | 0.793 | 0.303 | 1.154 |
| | (0.612) | (0.939) | (0.940) | (0.925) |
| Recession Indicator | 0.016 | 0.017 | 0.130** | 0.034 |
| | (0.029) | (0.055) | (0.057) | (0.056) |
| Emp. COVID × COVID-impacted Industry | 2.152*** | 0.350 | 0.934 | 0.091 |
| | (0.473) | (0.710) | (0.653) | (0.703) |
| Emp. Total × COVID-impacted Industry | 1.839*** | 0.209 | 0.818 | 0.270 |
| | (0.579) | (0.948) | (0.795) | (0.946) |
| Emp. Total × Occupation can telecommute > 25% | 0.183 | 0.099 | 0.300 | 0.473 |
| | (0.241) | (0.542) | (0.324) | (0.488) |
| Emp. Total × Nonwhite | 0.516 | 0.393 | 0.056 | 0.291 |
| | (0.473) | (0.974) | (0.525) | (0.573) |
| Emp. COVID × Youngest Child ages 6-12 | 0.119 | | 0.573 | |
| | (0.451) | | (0.663) | |
| Emp. COVID × Youngest Child ages 13-17 | 0.252 | | 0.945 | |
| | (0.576) | | (0.789) | |
| Emp. Total × Youngest Child ages 6-12 | 0.267 | | 0.206 | |
| | (0.617) | | (0.825) | |
| Emp. Total × Youngest Child ages 13-17 | 1.655** | | 0.841 | |
| | (0.836) | | (0.976) | |
| Unemployment × Youngest Child ages 6-12 | 0.015 | | 0.013 | |
| | (0.015) | | (0.025) | |
| Unemployment × Youngest Child ages 13-17 | 0.084*** | | 0.007 | |
| | (0.022) | | (0.030) | |
| Predicted Log(1000 + Spouse Earnings) | 0.075 | 0.316 | | |
| | (0.115) | (0.197) | | |
| COVID-impacted Industry | 2.037*** | 0.458 | 0.755 | 0.255 |
| | (0.438) | (0.653) | (0.614) | (0.671) |
| Occupation can telecommute > 25% | 0.085 | 0.059 | 0.330 | 0.501 |
| | (0.211) | (0.435) | (0.287) | (0.407) |
| High Contact Occupation | 0.040 | 0.084 | 0.157** | 0.120 |
| | (0.046) | (0.081) | (0.074) | (0.084) |
| Year | 0.003 | 0.008** | 0.006* | 0.001 |
| | (0.002) | (0.003) | (0.004) | (0.004) |
| Nonwhite | 0.469 | 0.499 | 0.209 | 0.276 |
| | (0.438) | (0.840) | (0.452) | (0.477) |
| Age - 25 | 0.007** | 0.014*** | 0.014*** | 0.009*** |
| | (0.004) | (0.003) | (0.005) | (0.003) |
| Youngest Child ages 6-12 | 0.402 | | 0.734 | |
| | (0.354) | | (0.655) | |
| Youngest Child ages 13-17 | 0.390 | | 0.764 | |
| | (0.534) | | (0.777) | |
| Constant | 4.714 | 14.123* | 12.741* | 3.690 |
| | (4.716) | (7.361) | (7.542) | (7.593) |
| Observations | 17140 | 7433 | 5727 | 5681 |

Notes: *p<0.1; **p<0.05; ***p<0.01. Standard errors calculated by bootstrapping with 500 replications. These results are probit regressions. The dependent variable is an indicator for whether the occupation group changed between time (t-1) and (t+1).See notes to Table 1.

Online Appendix Table 8: Change of Industry

| | Married | | Unmarried | |
|---|---|---|---|---|
| | Mothers | Childless | Mothers | Childless |
| Unemployment Rate | 0.024** | 0.007 | 0.003 | 0.003 |
| | (0.010) | (0.016) | (0.021) | (0.017) |
| Log per-capita COVID Employment | 0.334 | 1.443** | 0.656 | 0.396 |
| | (0.501) | (0.727) | (0.832) | (0.722) |
| Log per-capita Total Employment | 1.501** | 0.488 | 0.384 | 1.464 |
| | (0.663) | (1.009) | (1.104) | (1.029) |
| Recession Indicator | 0.009 | 0.019 | 0.054 | 0.022 |
| | (0.031) | (0.056) | (0.058) | (0.060) |
| Emp. COVID × COVID-impacted Industry | 0.848* | 0.494 | 1.306* | 0.467 |
| | (0.476) | (0.777) | (0.719) | (0.696) |
| Emp. Total × COVID-impacted Industry | 0.067 | 0.206 | 0.813 | 0.827 |
| | (0.605) | (1.009) | (0.867) | (0.973) |
| Emp. Total × Occupation can telecommute > 25% | 0.419 | 0.475 | 0.013 | 0.264 |
| | (0.280) | (0.551) | (0.458) | (0.547) |
| Emp. Total × Nonwhite | 0.438 | 1.728 | 0.597 | 0.373 |
| | (0.448) | (1.383) | (0.573) | (0.733) |
| Emp. COVID × Youngest Child ages 6-12 | 0.362 | | 0.010 | |
| | (0.461) | | (0.628) | |
| Emp. COVID × Youngest Child ages 13-17 | 0.444 | | 1.132 | |
| | (0.652) | | (0.949) | |
| Emp. Total × Youngest Child ages 6-12 | 0.012 | | 0.044 | |
| | (0.624) | | (0.807) | |
| Emp. Total × Youngest Child ages 13-17 | 0.968 | | 1.172 | |
| | (0.941) | | (1.232) | |
| Unemployment × Youngest Child ages 6-12 | 0.005 | | 0.008 | |
| | (0.015) | | (0.027) | |
| Unemployment × Youngest Child ages 13-17 | 0.053** | | 0.033 | |
| | (0.023) | | (0.035) | |
| Predicted Log(1000 + Spouse Earnings) | 0.036 | 0.323 | | |
| | (0.116) | (0.201) | | |
| COVID-impacted Industry | 1.466*** | 1.048 | 1.499** | 0.030 |
| | (0.419) | (0.698) | (0.702) | (0.696) |
| Occupation can telecommute > 25% | 0.281 | 0.398 | 0.010 | 0.143 |
| | (0.242) | (0.445) | (0.396) | (0.453) |
| High Contact Occupation | 0.025 | 0.247*** | 0.143* | 0.053 |
| | (0.048) | (0.083) | (0.077) | (0.082) |
| Year | 0.0001 | 0.010*** | 0.008** | 0.001 |
| | (0.002) | (0.004) | (0.004) | (0.004) |
| Nonwhite | 0.389 | 1.698 | 0.297 | 0.332 |
| | (0.399) | (1.156) | (0.491) | (0.609) |
| Age - 25 | 0.009** | 0.013*** | 0.016*** | 0.010*** |
| | (0.003) | (0.003) | (0.004) | (0.003) |
| Youngest Child ages 6-12 | 0.516 | | 0.115 | |
| | (0.370) | | (0.601) | |
| Youngest Child ages 13-17 | 0.301 | | 0.988 | |
| | (0.566) | | (0.948) | |
| Constant | 1.159 | 17.398** | 15.138** | 2.525 |
| | (4.891) | (8.015) | (7.669) | (8.302) |
| Observations | 17140 | 7433 | 5727 | 5681 |

NOTE.— *p<0.1; **p<0.05; ***p<0.01. Standard errors calculated by bootstrapping with 500 replications. These results are probit regressions. The dependent variable is an indicator for whether the industry group changed between time (t-1) and (t+1). See notes to Table 1.

Online Appendix Table 9: Change of Either Occupation or Industry

| | Married | | Unmarried | |
|---|---|---|---|---|
| | Mothers | Childless | Mothers | Childless |
| Unemployment Rate | 0.023** | 0.018 | 0.003 | 0.015 |
| | (0.010) | (0.014) | (0.020) | (0.015) |
| Log per-capita COVID Employment | 0.933** | 0.426 | 0.455 | 0.898 |
| | (0.467) | (0.671) | (0.775) | (0.603) |
| Log per-capita Total Employment | 1.072* | 1.234 | 0.016 | 1.798* |
| | (0.587) | (0.945) | (1.009) | (0.964) |
| Recession Indicator | 0.011 | 0.059 | 0.113** | 0.067 |
| | (0.028) | (0.050) | (0.054) | (0.052) |
| Emp. COVID × COVID-impacted Industry | 1.607*** | 0.094 | 0.957 | 0.206 |
| | (0.453) | (0.722) | (0.675) | (0.655) |
| Emp. Total × COVID-impacted Industry | 1.042* | 0.344 | 0.672 | 0.517 |
| | (0.578) | (0.958) | (0.855) | (0.931) |
| Emp. Total × Occupation can telecommute > 25% | 0.505** | 0.316 | 0.099 | 0.422 |
| | (0.248) | (0.555) | (0.364) | (0.439) |
| Emp. Total × Nonwhite | 0.424 | 0.379 | 0.066 | 0.575 |
| | (0.438) | (0.971) | (0.489) | (0.580) |
| Emp. COVID × Youngest Child ages 6-12 | 0.500 | | 0.357 | |
| | (0.419) | | (0.632) | |
| Emp. COVID × Youngest Child ages 13-17 | 0.121 | | 0.972 | |
| | (0.563) | | (0.841) | |
| Emp. Total × Youngest Child ages 6-12 | 0.037 | | 0.100 | |
| | (0.571) | | (0.772) | |
| Emp. Total × Youngest Child ages 13-17 | 1.274 | | 1.298 | |
| | (0.814) | | (1.104) | |
| Unemployment × Youngest Child ages 6-12 | 0.012 | | 0.014 | |
| | (0.015) | | (0.025) | |
| Unemployment × Youngest Child ages 13-17 | 0.071*** | | 0.034 | |
| | (0.021) | | (0.030) | |
| Predicted Log(1000 + Spouse Earnings) | 0.054 | 0.325* | | |
| | (0.112) | (0.195) | | |
| COVID-impacted Industry | 1.889*** | 0.538 | 0.997 | 0.052 |
| | (0.410) | (0.666) | (0.624) | (0.618) |
| Occupation can telecommute > 25% | 0.253 | 0.365 | 0.111 | 0.553 |
| | (0.216) | (0.443) | (0.320) | (0.360) |
| High Contact Occupation | 0.012 | 0.147* | 0.160** | 0.110 |
| | (0.048) | (0.084) | (0.077) | (0.080) |
| Year | 0.002 | 0.011*** | 0.006* | 0.0003 |
| | (0.002) | (0.003) | (0.003) | (0.004) |
| Nonwhite | 0.358 | 0.527 | 0.148 | 0.483 |
| | (0.396) | (0.828) | (0.426) | (0.481) |
| Age - 25 | 0.006* | 0.014*** | 0.012*** | 0.009*** |
| | (0.003) | (0.003) | (0.004) | (0.003) |
| Youngest Child ages 6-12 | 0.737** | | 0.474 | |
| | (0.334) | | (0.612) | |
| Youngest Child ages 13-17 | 0.340 | | 0.608 | |
| | (0.531) | | (0.802) | |
| Constant | 2.419 | 19.653*** | 11.956* | 1.121 |
| | (4.627) | (7.338) | (7.070) | (7.356) |
| Observations | 17140 | 7433 | 5727 | 5681 |

NOTE.— *p<0.1; **p<0.05; ***p<0.01. Standard errors calculated by bootstrapping with 500 replications. 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). See notes to Table 1.

# B Data Appendix

## B.1 Construction of Dataset

Our sample uses prime-age women (ages 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). In partnered households, we remove same-sex families identified by households where the head of household and the spouse of household were both female.

We construct a record of each individual's history of all variables used in the analysis from ages 25 to 54. However, some observations at some ages are missing for several reasons. 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 B1 quantifies the degree of the missing observations by each marital status and children categories. [1] The Imputations section of this Appendix (Section B.2) describes the handling of missing information.

The variables included 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. 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 life cycle.

[1] Imputations for marital status and children are conducted first (see below), so Table B1 characterizes the missing additional information given the already imputed family structure.

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.

For instrumenting the spouse's earnings in married households, we use the years of education of the spouse. Spouses are not restricted to have fewer than 16 years of education. In the event that there is missing information on the spouse's education, we assume that there was no change to the variable and use the most recently observed years of education to replace the missing value.

#### 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 A2.

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, 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 using the Personal Consumption Expenditures deflator with 2010 as the base year. 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; and 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.

The percentages for each occupation ability to telecommute are from 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 impacted 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 it is equal to unity if the respondent's modal race is *not* white, and zero otherwise.

### 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 1968 and before) 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 COVID-impacted industries obtains the employment numbers for the industries that were affected by COVID as identified earlier. While the total employment

[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.

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-impacted industries to the total employment remained the same as that observed in 1990. Thus, for a year $y < 1990$, the employment in the COVID-impacted 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 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 $a$ is 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

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

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 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 $\text{MSK}_{a+1}$ on $\text{MSK}_{a+2}$ to provide MSK probabilities for age $a+1$, and similarly we use a multinomial logit of $\text{MSK}_{a}$ on $\text{MSK}_{a-1}$ 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. [4] 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

[4] When the MSK is known, then we simply set the probability for that category to unity. When it is unknown, we use the probabilities obtained from the multinomial logits.

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.

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-year/ full-year employment exercise, we utilize a categorical variable assigning 1 to part-year employment ($300 \leq$ Hours $< 1600$) and 2 to full-year employment (Hours 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.

Online Appendix Table B10: Breakdown of Imputed Observations

| | Married | | Unmarried | |
|---|---|---|---|---|
| | Mothers | Childless | Mothers | Childless |
| Total Observations including Imputed Histories | 19,241 | 8,165 | 6,244 | 6,387 |
| Total Observations excluding Imputed Histories | 12,651 | 4,776 | 3,890 | 3,559 |
| Difference | 6,590 | 3,389 | 2,354 | 2,828 |
| Before 1968 | 339 | 71 | 34 | 19 |
| Left Censored | 1,026 | 353 | 337 | 509 |
| Missing Information in Final Appearance | 41 | 42 | 21 | 26 |
| PSID Interviewing Gap | 3,777 | 2,338 | 1,600 | 1,863 |
| Other Gap | 1,407 | 585 | 362 | 411 |
| Other Gap (2 or fewer years) | 712 | 319 | 237 | 259 |

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