Occupational Heterogeneity of Child Penalty in the United States

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Most recent draft here.

Abstract

I investigate how parenthood reshapes employment patterns across occupations and how this occupational heterogeneity contributes to earning disparities. Using a novel rotating panel approach to estimating child penalties, I document that both men and women change occupations. The well-established null effect of fatherhood hides that men's employment rate decreases in some occupations like finance and increases in others like construction. Women leave most occupations but select into occupations with part-time options. These occupational changes explain 40% of the income penalty for women, most of the income penalty for men, and most of the wage penalty for both genders.

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1 Introduction

Since the 1970s, the gender gap in earnings and employment in the United States has narrowed substantially (Goldin, 2024). Nevertheless, significant disparities persist: women participate less in the labor force than men and, when employed, earn lower wages. An extensive literature examines the causes of these persistent gaps. One prominent strand emphasizes occupational segregation, the single largest factor accounting for the gender pay gap (Blau and Kahn, 2017). Concurrently, a growing literature highlights the importance of child penalties: the differential impact of parenthood on labor market outcomes between women and men. In developed countries, these penalties explain a substantial portion of gender inequality in the labor markets (Kleven et al., 2019a,b, 2021; Cortés and Pan, 2023). However, the intersection between parenthood and occupational choices remains underexplored.

This paper addresses this critical gap by examining how parenthood reshapes employment patterns across occupations and how this occupational heterogeneity contributes to earnings disparities. I document that both men and women change occupations. The well-established null effect of fatherhood hides that men's employment rate decreases in some occupations such as finance and increases in others like construction. Women leave most occupations but select into occupations with part-time options. These occupational changes explain 40% of the income penalty for women, most of the income penalty for men, and most of the wage penalty for both genders.

Investigating the occupational heterogeneity of child penalties presents an empirical challenge. Most existing work relies on panel data, which typically lack sufficient observations to precisely estimate child penalties by occupation. I take advantage of the rotating panels in the Current Population Survey (CPS)—which are two orders of magnitude larger than panel datasets usually used to study the child penalty, such as the National Longitudinal Survey of Youth (NLSY) and the Panel Study of Income Dynamics (PSID) (Kleven et al., 2019b; Bang, 2022; Cortés and Pan, 2023; Kleven et al., 2024b)—to estimate the effects of having children on the probability of being employed in different occupations. My methodological contribution is showing how to use rotating panel datasets, which are both more prevalent and often substantially larger than panel datasets (Donovan et al., 2023), to estimate child penalties with greater precision and without additional assumptions.

I document that parenthood's effect on employment probability differs widely across occupations for both genders. First, the almost zero effect of fatherhood on men, which has been well established in the literature, hides considerable heterogeneity. While men's employment probability decreases by 1.9 percentage points (pp) (32%) in computer & mathematics and by 1.1 pp (20%) in finance, it increases by 2.3 pp (36%) in construction. The positive and negative effects of fatherhood across occupations largely offset each other; however, the greater precision of my method reveals a statistically significant negative aggregate effect for men. Second, women leave most occupations but select into others. For example, women's employment probability decreases by 4.2 pp (36%) in management while it increases by 0.3 pp (13%) in personal care and services.[1]

[1]The heterogeneous impact of parenthood across occupations stems from three distinct mechanisms: (1) in-

Occupational heterogeneity in employment penalties significantly drives child income penalties for both women and men, especially in recent years. Mothers faced income penalties of approximately 20% throughout 1990–2019, with parenthood-induced occupational change accounting for about 30% of this penalty in the 1990s, rising to 40% in the 2010s as within-occupation penalties declined. In contrast, men experienced a modest 4% income penalty in the 1990s that gradually increased to 8.5% in the 2010s, almost entirely attributable to fatherhood-induced occupational changes. Both the increasing income penalty for men and its occupational change mechanism represent novel findings, likely because this is a recent phenomenon undetectable in NLSY and PSID. Notably, the occupational change component affects both genders similarly and does not contribute to the child-induced income gap between men and women, which is primarily driven by within-occupation differences.

Income penalties stem from changes in both hours worked and hourly wages. To distinguish between these mechanisms, I analyze occupational change's role in wage penalties and discover patterns that largely parallel those of income penalties. Women's wage penalties increased from 8% in the 1990s to 10% in the 2010s, with approximately three-fourths attributable to occupational changes. The wage penalty being half as large as the income penalty implies that the other half comes from reduced hours. In contrast, men's wage penalties mirror their income penalties: a 4% penalty in the 1990s increasing to 8.4% in the 2010s. The wage and income penalties being equal implies that men do not reduce their work hours conditional on working. Remarkably, the differential wage penalty between mothers and fathers decreased from 4% to 2%. This is notable for two reasons. First, the raw hourly wage gap in my sample was 20% in the 1990s and 13% in the 2010s. Parenthood thus accounted for roughly 20% of the wage gap in the 1990s (4 out of 20 percentage points) and 15% in the 2010s (2 out of 13 percentage points)—a meaningful but not dominant share. Second, even this modest contribution decreased by half over time. These results highlight an important aspect of child penalties in the US: parenthood causes significant disparities in employment probability, hours worked, and therefore income, but does not cause a major disparity in hourly wages conditional on working.

Lastly, I analyze which occupational attributes explain the heterogeneity in child penalties. I find that women's employment rate declines more in occupations that do not allow for part-time work (e.g., engineering) and actually increases in occupations that allow for most part-time work (e.g., personal care and food preparation). In contrast, part-time work is not correlated with men's employment penalties. Interestingly, occupations with greater part-time availability exhibit larger reductions in women's income and hours worked, highlighting part-time work's paradoxical role: facilitating continued employment post-childbirth while simultaneously constraining income potential. Neither hour flexibility—the ability to alter start and end times—nor women's occupational representation explains gendered heterogeneity in child employment penalties. This evidence suggests that, for mothers, parenthood-induced occupational changes are likely driven by preferences

dividuals exiting specific occupations for non-employment, (2) workers transitioning between occupations, and (3) previously non-employed individuals entering particular occupations. The lack of a large panel data limits isolating these forces, which remains an open question for future work.

for reduced working hours, and that the relevant dimension of flexibility is the ability to reduce hours rather than to determine when to work.[2] These results contribute to the literature studying the role of temporal flexibility in gender inequality (Bertrand et al., 2010; Goldin, 2014; Goldin and Katz, 2016; Ciasullo and Uccioli, 2023).[3] Mothers tend to sort into occupations with part-time options, which ultimately contributes to the pay gap, as these positions typically pay lower wages (Hirsch, 2005) and may reduce productivity (Gallen, 2024).

My results on the occupational heterogeneity of child penalties complement a growing body of work investigating how the effects of parenthood differ across occupations, industries, and firms. On the intensive margin, income penalties differ across occupations with linear and non-linear wage structures (Bütikofer et al., 2018). On the extensive margin, women become less likely to work in skilled occupations (Gallen et al., 2024) or high-paying firms (Jack et al., 2025). I contribute to this literature in several ways. I show that the gendered differences in employment penalties differ widely across occupations, which implies that occupational segregation across genders, the single largest driver of wage disparities, is partially driven by parenthood. Moreover, these differences significantly drive both income and wage penalties. An advantage of using large survey data like CPS, as opposed to census data used in earlier studies, is the ability to track income and wages separately, as information on work hours is often not collected in tax records. For instance, to the best of my knowledge, I am the first to show that half of the income penalty for women comes from reduced working hours and that men and women have incurred similar wage penalties in the United States since 2010.

This paper makes a methodological contribution to the literature on child penalties (Angrist and Evans, 1998; Angelov et al., 2016; Lundborg et al., 2017; Kleven et al., 2019a,b). Most related to my paper, Kleven (2025) develops a new approach for estimating child penalties using cross-sectional data. His method employs exact matching to predict who will eventually have a child among those without children and uses them as a control group. This *pseudo-panel* method enables studying child penalties with precision using widely available cross-sectional data, though the matching step introduces additional assumptions about the comparability of matched non-parents to eventual parents.[4] In contrast, rotating panels directly observe the control group—individuals before they become parents—relying only on the standard random timing assumption. Rotating panels are less

[2]An alternative explanation that I cannot rule out is that firms discriminate against mothers in full-time jobs, which forces women to transition into part-time occupations.

[3]A similar connection between work hours and flexibility was first made by Flabbi and Moro (2012), who defined flexibility as having a part-time job in the context of a search model. Using Swedish matched employer-employee data, Hotz et al. (2017) document that mothers move to workplaces with higher shares of part-time workers and female co-workers with young children after childbirth. Bang (2022) also studies how occupational flexibility affects child penalties, finding that wives with more flexible occupations restore pre-birth hours faster and that wives with husbands in less flexible occupations reduce labor supply more. However, the NLSY's limited sample size restricts her analysis to above/below median flexibility comparisons, with differences in child penalty estimates being statistically insignificant in some specifications. The greater precision of my rotating-panel method allows me to examine how child penalties vary across multiple flexibility dimensions, revealing that part-time availability—not hour flexibility—explains gendered heterogeneity in employment penalties.

[4]Using this method, Kleven (2025) studies the heterogeneity in child penalties across US states, and Kleven et al. (2024a) study heterogeneity across the globe.

common than cross-sectional datasets but more prevalent and substantially larger than traditional panel datasets. Our methods can thus be seen as alternatives with different use cases. When rotating panel data are available, researchers can use my *rotating-panel* approach to explore child penalties across demographics and job characteristics without using more assumptions. Conversely, when only cross-sectional data are available or when existing panel or rotating panel datasets are too small, researchers can use Kleven (2025)'s pseudo-panel approach to study child penalties.

2 Data

My primary dataset is the basic monthly files of CPS downloaded from IPUMS between the years 1977–2019. The main outcomes of interest are employment, weekly income, and usual hours worked, all of which are directly observed in the data, and hourly wages, which I calculate by dividing weekly income by usual hours worked. Parental status is determined using the age of the oldest child in the household, with event time assigned based on the child's age. Following convention, I restrict the sample to eventual parents who are between ages 20 and 55, had their first child between ages 25–45, are always either a household head or spouse, and have an eldest child of at most 10 years old. Since the control group—people who are about to become parents within the next year—can only be identified if they appear in both interview rounds, I further restrict the treatment group to individuals appearing in both rounds to ensure comparable treatment and control groups.[5] I drop observations where the eldest child's age jumps across consecutive observations, which excludes parents who adopted older children or began living with a partner who already had children. The final dataset includes 356,850 unique parents and 2,648,089 person-month observations.

I validate my rotating-sample design by comparing CPS results with identical specifications using NLSY and PSID data (3,649 and 3,443 unique parents, respectively). These comparisons focus solely on weekly employment, as income measures differ across datasets (CPS collects weekly income in basic monthly files, while NLSY and PSID collect annual income).

For occupational analysis, I examine employment effects across 22 main occupation groups following Standard Occupational Classification (SOC) guidelines (excluding military occupations). I measure temporal flexibility in the main text using part-time availability, which I define as the proportion of workers employed part-time in each occupation. In the Online Appendix, I also explore hour-flexibility, the proportion of workers who can vary their start/end times, calculated from the CPS Work Schedules Supplement (1.6 million observations across 12 years), and the ratio of women in each occupation. All metrics are calculated using individuals without children, though robustness checks (shown in the Online Appendix) confirm similar results when using all workers or pre-child observations of eventual parents.

[5] CPS follows a 4-8-4 rotation pattern: households are interviewed for four consecutive months, rotate out of the sample for the next eight months, and are then interviewed again for the next four months before rotating out of the sample for good. Restricting to individuals appearing in both rounds reduces the sample by approximately 30%. This ratio is stable across time with one exception: the 1994 CPS redesign changed household identifiers, lowering the share of individuals I can track across rounds in 1994–1995. Since 1996, the restriction binds at a constant rate as shown in Appendix Figure A.8.

3 Identification

3.1 Event study approach

The event-study approach of estimating child penalty uses panel data on men and women who become parents. I run the following specification separately for men and women,

$$Y^g_{iat} = \sum_{j \neq -1} \beta^g_j \Delta D_{i,t-j} + \mu^g_a + \lambda^g_t + \epsilon^g_{it}, \tag{1}$$

where Y^g_{iat} is the outcome for individual i of age a and gender $g \in \{m, w\}$ at event time t, $\Delta D_{i,t} = 1$ if individual i had first child in time t, μ_a and λ_t are age and year fixed effects that non-parametrically control for lifecycle and time trends. The identification assumption is that, after controlling for age and calendar time fixed effects, the timing of having children is exogenous to potential labor market outcomes of parents. Consistent with this assumption, the event study approach shows little to no pre-trends in the five years before having a child for both men and women (Kleven et al., 2019a). This approach has been widely used to study the effect of the first child on parents' labor market outcomes (Kleven et al., 2019b, 2021; Cortés and Pan, 2023; Kleven et al., 2024b).

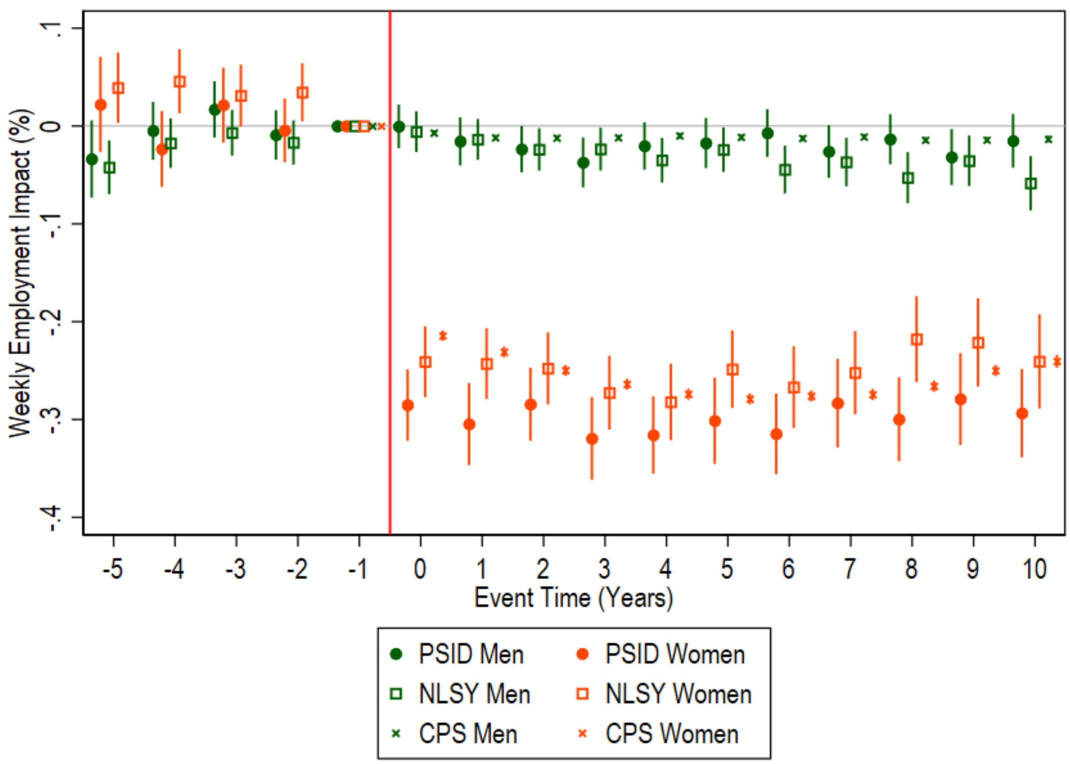
My methodological innovation is predicated on the already established absence of pre-trends in the data, which simplifies the data requirements to only the year before the first child ($t = -1$). Rotating panels like the CPS, where individuals are interviewed in two rounds with a significant time interval between the rounds (eight months in the case of the CPS), are then sufficient to estimate equation 1. Consider an individual who is not a parent during the first round but becomes a parent during the second round of interviews. In the first round, we observe this person at least eight months before having a child, which is enough to index them as $t = -1$.[6]

To validate this approach, I compare the CPS estimates with the NLSY and PSID estimates, the two panel datasets available in the US that have been used to study child penalties. Figure 1 displays the results. The point estimates using CPS are highly comparable to those using PSID and NLSY, providing strong credibility for this method. On average, I find that women lose more jobs than men by 28% using PSID and 22% using NLSY. Using CPS reveals an estimate between the two, a child penalty of 24%. The main difference is that the estimates using CPS are much more precise than those using NLSY and PSID. In fact, the 95% confidence intervals of the CPS estimates are barely visible in the figure.

I use the event study approach only to validate the CPS as an applicable dataset to study the child penalty. After validation, I continue by estimating occupation-specific child penalties. Note that, while the standard child penalty estimation assumes random timing in the aggregate,

[6] Using the "panel" nature of CPS is not novel in the Economics literature, going as far back as Poterba and Summers (1986). However, how the CPS can be used to estimate child penalties has not been shown before. This is likely because the literature on child penalty focuses on long-term effects (as far as ten years after the first child), while the same individual is only observed for sixteen months in CPS. My method enables me to study the long-term effects precisely because it does not exploit changes in the outcome within a person. To estimate the child penalty, I only need to observe $t = -1$ for *some*, not all people in the data.

Figure 1: CPS vs NLSY and PSID



Estimates come from the regression equation $Y^g_{iat} = \sum_{j\neq -1} \beta^g_j \Delta D_{i,t-j} + \mu^g_a + \lambda^g_t + \epsilon^g_{it}$, where Y^g_{iat} is the outcome for individual i of age a and gender $g = w, m$ at event time t, $\Delta D_{i,t} = 1$ if individual i had first child in time t, μ_a and λ_t are age and year fixed effects that control non-parametrically for lifecycle trends and time trends. Percentage estimates are obtained by dividing the level estimates β^g_j with predicted outcome absent child effects. The difference in child penalties across men and women (which is often referred to as *the* child penalty in the literature) using the estimates between t=-1 and t=10 is estimated as 22% using NLSY, 28% using PSID, and 24% using CPS.

my occupation-specific estimates require this assumption to hold at the occupation level. While I cannot directly test for pre-trends at the occupation level given the CPS structure, there is no theoretical reason to expect the random timing assumption to hold in aggregate but fail for specific occupations.

3.2 Differences in means design

To understand how people's occupational choice is impacted by having children, I employ the following design:

$$Employment_{iat}^{o,g} = \beta^{o,g} D_{it} + \mu_a^{o,g} + \lambda_t^{o,g} + \epsilon_{it}^{o,g}, \tag{2}$$

where $Employment_{iat}^{o,g}$ is a dummy equal to one if individual i of gender g is employed in occupation o at time t, and D_{it} is an indicator of parenthood.[7] For each gender $g \in \{m, w\}$, I run different regressions using the same sample, where I change only the outcome (if the person i is working as a manager, $Employment_{iat}^{o,g}$ equals one only for the occupation $o = Manager$, and zero otherwise). To obtain percentage estimates, I divide the level estimates $\hat{\beta}^{o,g}$ with the predicted outcome absent child effects:

$$\hat{P}^{o,g} = \frac{\hat{\beta}^{o,g}}{E\left[\widetilde{Y}_{iat}^{o,g}\right]}, \tag{3}$$

where $\widetilde{Y}_{iat}^{o}$ is the predicted employment probability of individual i in occupation o when omitting the contribution of the child effect.[8] The coefficient estimates $\hat{\beta}^{o,g}$ and $\hat{P}^{o,g}$ should be interpreted as the effects of having children on the probability of being employed in occupation o for gender g. For example, I find that the probability of working as an engineer decreases by around 50% for women. This is the net effect of three separate forces: (i) engineers leaving employment after having children, (ii) engineers transitioning into different occupations, and (iii) people from other occupations and unemployment transitioning into engineering. The lack of a large panel data limits isolating these forces, which remains an open question for future work.

Throughout the rest of the paper, I refer to these effects interchangeably as occupational heterogeneity and occupational change. This should not be confused with heterogeneous treatment effects, which would apply only to the first of the three forces I described.

I use the occupation-gender specific child penalty estimates $\hat{\beta}^{o,g}$ in two ways. First, I compare their magnitudes to document how the effects of having children differ across occupations. Second, I regress these estimates on occupational characteristics, such as part-time availability, to analyze what attributes can explain this heterogeneity. To obtain inference that is robust to multiple

[7] I utilize a simple dummy treatment instead of the more flexible dynamic event-study design because, for certain small occupations, particular event-time dummies are imprecisely estimated due to having very few observations (e.g., women with 5 year olds who work in construction). The simple average across event-study estimates, therefore, becomes noisy. The dummy treatment prevents this problem by taking a weighted average across all post-treatment periods. Nevertheless, I show how the results differ across short, medium, and long term in Figure A.3.

[8] An alternative would be to estimate a multinomial logit over occupations plus non-employment. However, the sum of occupation-specific employment indicators $\sum_o Employment_{iat}^{o,g}$ equals one if the individual is employed and zero otherwise, i.e., there is no constraint that this sum equals one across all observations. Because overall employment is free to change, estimating separate linear probability models for each occupation should be innocuous.

hypothesis testing, I employ two additional checks in the Online Appendix. First, I employ the Bonferroni correction to adjust the standard errors of the $\hat{\beta}^{o,g}$ estimates in a conservative way. Second, I employ the Empirical Bayes (EB) shrinkage, which takes into account that occupation-specific child penalties are noisily estimated. All the results presented in the paper remain robust to these adjustments. For simplicity, I present the OLS estimates in the main text and present the robustness checks in the Online Appendix.

4 Results

4.1 Child penalties across occupations

Figure 2a displays child penalty estimates for men and women across the 22 major occupational groups, excluding military service, sorted by the penalty's impact on women. This figure reveals three key findings. First, the motherhood penalty varies substantially across occupations. Women's employment probability decreases significantly in 14 out of 22 occupations and increases significantly in 3 occupations. The largest effects come from management, where women become 4.2 pp less likely to work, and personal care and services, where women become 0.3 pp more likely to work.

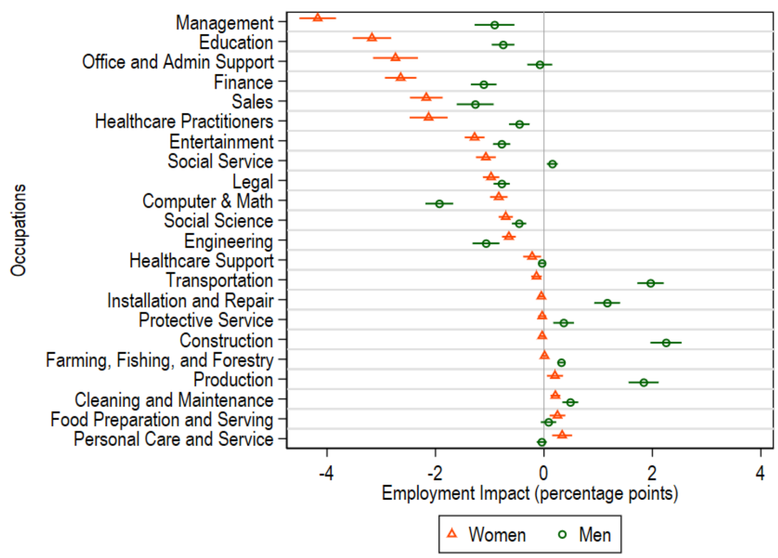
Second, the almost zero employment penalty of fatherhood, which has been well documented in the literature, masks significant heterogeneity across occupations. Men's employment probability decreases significantly in 10 occupations and increases significantly in 8 occupations. For instance, fathers are 1.9 pp less likely to work in computer and mathematics, whereas they are 2.2 pp more likely to work in construction and transportation. The positive and negative effects of fatherhood largely offset each other, leading to the near-zero aggregate estimate in the literature. This null is often interpreted as parenthood not impacting men's labor market outcomes. My findings reject this interpretation and show that men change occupations after becoming parents.[9]

Third, child penalties vary more between occupations than between genders. The most notable within-occupation disparity in levels occurs in management roles, where the likelihood of women holding a management position declines by 4.2 pp, compared to a mere 0.9 pp decline for men, resulting in a 3.3 pp difference in the within-occupation penalties between genders. In comparison, the largest difference in penalties for women across occupations is between personal care and management. In the former, women's employment probability increases by 0.3 pp, leading to a 4.5 pp difference in treatment effects, which is larger than the 3.3 pp maximum difference across genders. Similarly, men's employment probability decreases by 1.9 pp in computer & mathematics and increases by 2.2 pp in construction, creating a 4.1 pp maximum difference across occupations, which is also larger from the maximum difference between genders. These results indicate that

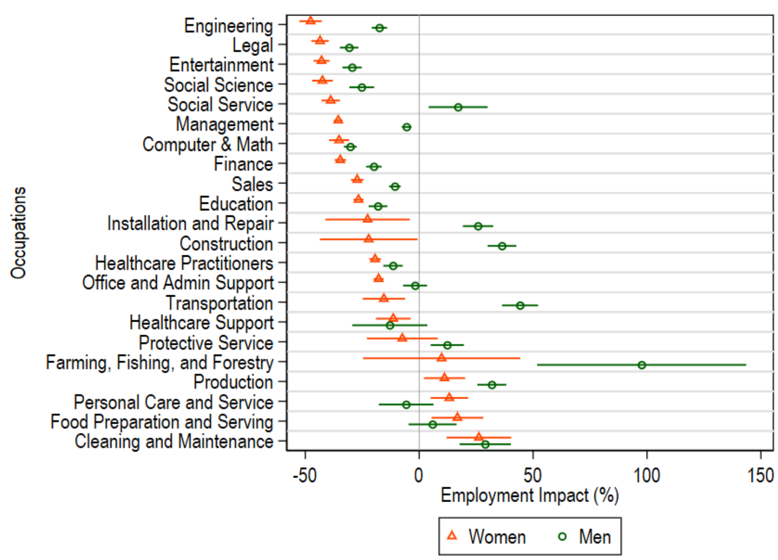
[9]The employment rate for men about to become fathers is 95%, meaning transitions from non-employment to employment can account for at most 5 pp of increased employment probability across all occupations combined. However, the sum of positive employment effects across the 22 major occupations for men is 8.6 pp, nearly double what unemployment-to-employment transitions alone could explain. This implies that occupational transitions must occur. For women, I cannot reject the null that the increases in employment probabilities come solely from non-employment to employment transitions.

occupational differences in child penalties are economically significant.

Figure 2: Occupational Heterogeneity in Child Employment Penalty



(a) Employment Penalties (in levels)



(b) Employment Penalties (in percentages)

Note: Results on employment come from the regression $Employment^{o,g}_{iat} = \beta^{o,g} D_{it} + \mu^{o,g}_a + \lambda^{o,g}_t + \epsilon^{o,g}_{it}$, where $Employment^{o,g}_{iat}$ is a dummy equaling to one if individual i of gender g is employed in occupation o at time t, μ_a and λ_t are age and year fixed effects that control non-parametrically for lifecycle trends and time trends. Superscripts mean sample restrictions. For each gender $g \in \{m, w\}$, 22 separate regressions are run for each occupation-specific outcome. Robust standard errors are used to calculate the 95% confidence intervals. To obtain percentage estimates, I divide the level estimates $\beta^{o,g}$ with predicted outcome absent child effects: $\hat{P}^{o,g} = \frac{\hat{\beta}^{o,g}}{E[\widetilde{Y}^{o,g}_{iat}]}$, where $\widetilde{Y}^{o}_{iat}$ is the predicted employment rate when omitting the contribution of the child effect. Standard errors for percentage effects are calculated using the Delta method. 95% confidence intervals are plotted.

Figure 2b presents the employment penalties in percentages to account for the level differences at baseline. The results remain similar. There is significant heterogeneity in child penalties across occupations for both genders. In percentages, women's employment probability decreases most in engineering (48%), legal (45%), and entertainment (43%) while increasing most in cleaning (26%), food preparation (17%), and personal care (13%).

These results remain robust to using more conservative confidence intervals or shrinkage estimators. Appendix Figure B.1 plots the updated confidence intervals after applying the Bonferroni correction. Since the parameters are precisely estimated, this conservative correction does not alter the inference. Moreover, Figure C.1 displays the estimates based on EB shrinkage. OLS and EB estimates are highly similar: I document economically meaningful differences in child penalties across occupations for both men and women.[10] Overall, the heterogeneous effects of children on employment probabilities in different occupations are not driven by wrong inference due to multiple hypothesis testing or small samples.

In addition, I study the heterogeneity of child penalties in income and hours, conditional on working. Figure A.1 in the Online Appendix shows that women lose income and hours in nearly all occupations, while men lose income and hours only in a few occupations, if any. Women's penalties differ largely across occupations, while men's penalties do not vary in economically meaningful amounts. Based on EB estimates shown in Figure C.2, women lose 34% income in personal care while losing only 8% income in computer & mathematics.[11] As I show in the next section, this disparity can be explained by personal care occupations allowing mothers to reduce their working hours by working part-time, while computer & mathematics related occupations not providing this flexibility.

The preceding estimates average effects across all eleven post-treatment years ($t = 0$ to $t = 10$), potentially masking differences between short and long-term responses. To examine how occupational penalties evolve, I estimate treatment effects separately for three periods: the short run ($t \in \{0, 1, 2\}$), medium run ($t \in \{3, 4, 5, 6\}$), and long run ($t \in \{7, 8, 9, 10\}$). Appendix Figure A.3 presents the results. For women, employment probability decreases across all occupations in the short run, but positive effects emerge in specific occupations over the medium and long run. For men, short and long-term effects are qualitatively similar, though larger in absolute value over time.

What should we infer from the differences in child penalties in levels and percentages, as depicted in Figures 2a and 2b? The employment penalty in levels impacts occupations' role in the gender gap in earnings. For example, management is the third highest-paid occupation during

[10] EB shrinkage does not move the OLS estimates by much (except for farming) because the data have high signal-to-noise ratio: the standard deviation in the OLS estimates across occupations is substantially higher than the standard errors of the OLS estimates for each occupation. The Online Appendix Section C provides an in-depth explanation for the interested reader.

[11] Income and hour penalties come from a log-linear regression: $ln(y^{o,g}_{iat}) = \beta^{o,g} D_{it} + \mu^{o,g}_a + \lambda^{o,g}_t + \epsilon^{o,g}_{it}$, where the outcome is log income or log hours for individuals in a given occupation–gender cell, with separate regressions run for each cell, the treatment and the fixed effects are as described in the identification section. The percentage estimates are acquired by the transformation $\gamma = exp(\beta) - 1$. EB estimates are more reliable than OLS estimates for income and hour penalties because the OLS estimates are less precise for these outcomes.

the sample period. Since women lose more management jobs than men after becoming parents, women end up losing more income, increasing the gender gap in earnings. I explore this mechanism in Section 4.2. The percentage penalty helps uncover which occupational attributes can explain employment penalties. For example, women are over-represented in education and under-represented in construction, leading to vastly different employment penalties in levels. However, women leave these occupations at similar rates after becoming mothers, which is informative about which occupational attributes can explain these penalties. I explore this in Section 4.3.

4.2 Effect of occupational change on the income penalty

This section studies how occupational changes induced by parenthood affect income and wage penalties. Specifically, I estimate the effect of parenthood on men and women, with and without controlling for 477 occupation dummies, as outlined in equation 4.[12] $exp(\beta_1^g) - 1$ captures the average effect in percentages of having the first child on income or wage, conditional on employment.[13] $exp(\beta_2^g) - 1$ captures the *within occupation* penalty, accounting for the occupational change that both men and women undergo after having children. The differential $exp(\beta_1^g) - exp(\beta_2^g)$ highlights the influence of these occupational changes on the overall income and wage penalties, indicating how much of the penalty is due to changes in occupations versus income and wage losses within the same occupation,

$$\begin{aligned} ln(Y_{iat}^g) &= \beta_1^g D_{it} + \mu_a^g + \lambda_t^g + \epsilon_{it}^g, \\ ln(Y_{iat}^g) &= \beta_2^g D_{it} + \mu_a^g + \lambda_t^g + \theta^g Occ_{it} + \eta_{it}^g. \end{aligned} \tag{4}$$

To assess the evolution of these dynamics over time, I calculate these penalties in 10-year intervals from 1990 to 2019. This longitudinal approach allows me to observe how child penalties and the role of occupational adjustments have shifted over the past three decades.

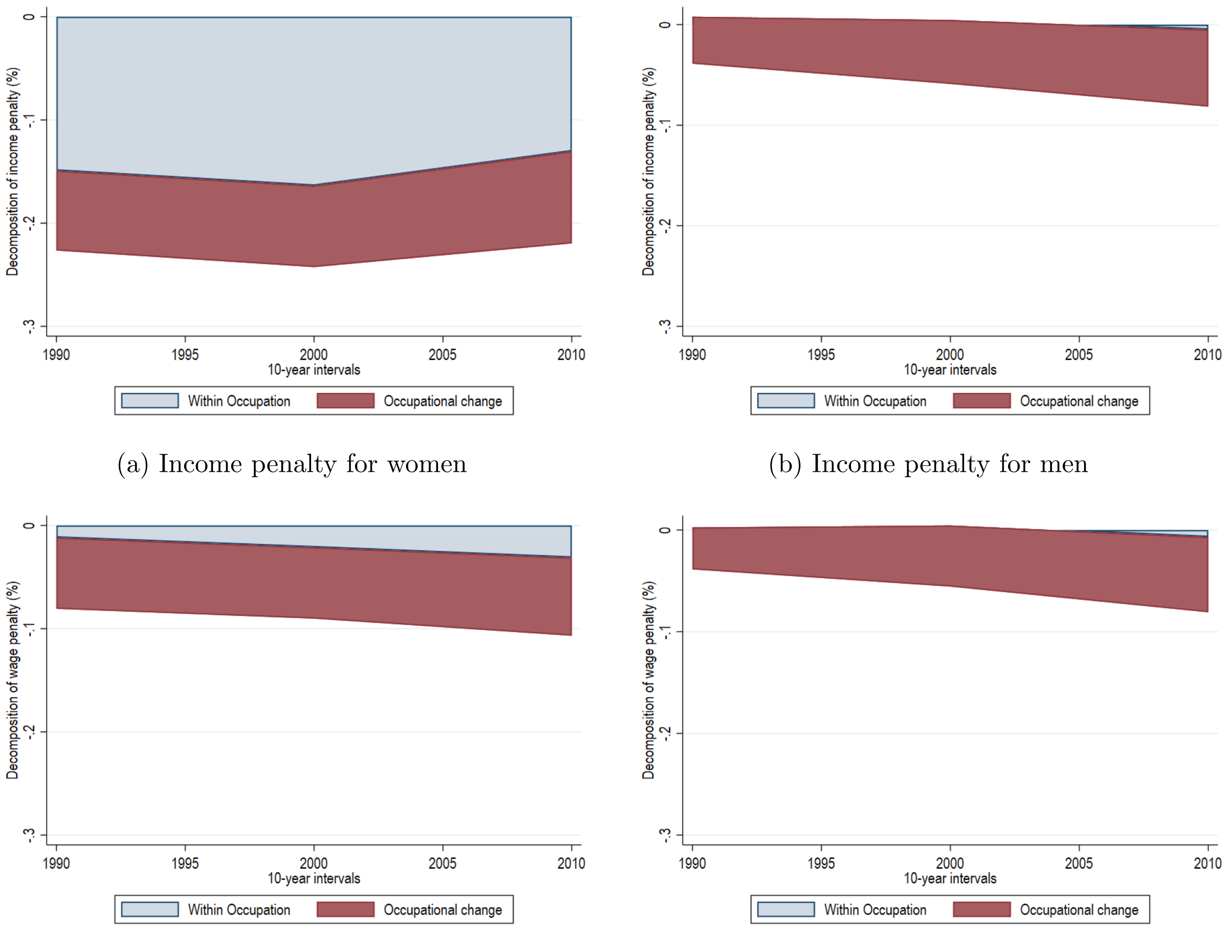
Figure 3a illustrates the evolving dynamics of the income penalty associated with motherhood. Mothers faced an income penalty of 20.3% in 1990s, which increased to 21.5% in 2000s, before decreasing back to 19.7%. Notably, parenthood-induced occupational change contributed around 6.4% in the 1990s and 2000s, or about 30% of the income penalty. However, while the within-occupation component of the income penalty has decreased significantly, the occupational change component has increased to around 8%. Today, 40% of the income penalty for women comes from occupational change.

The pattern for men reveals a strikingly different trend. As Figure 3b shows, while women's income penalties decreased over time, men's income penalties increased, from 4% in the 1990s to 6%

[12] I use all 477 occupation codes rather than the 22 major occupation groups to fully capture the role of occupational change. Results are qualitatively similar when controlling only for major occupation groups (Appendix Figure A.4).

[13] While this design faces potential bias from non-uniform employment exits across income and wage distributions, several factors support the reliability of my estimates. First, the minimal reduction in employment rates after fatherhood suggests that compositional biases are negligible at least for men. Second, the stability of women's employment penalty since the 1990s (Appendix Figure A.9) indicates that such biases would not significantly affect the temporal evolution of my estimates.

Figure 3: Decomposition of Income and Wage Penalties



(a) Income penalty for women

(b) Income penalty for men

(c) Wage penalty for women

(d) Wage penalty for men

Note: Within occupation estimates come from the regression: $ln(Y_{iat}^{g}) = \beta_2^g D_{it} + \mu_a^g + \lambda_t^g + \theta^g Occ_{it} + \eta_{it}^g$, where $ln(Y_{iat}^{g})$ is the log-income or log-wage of individual i of age a at time t from gender g, μ_a and λ_t are age and year fixed effects that control non-parametrically for lifecycle trends and time trends, Occ_{it} is an occupation fixed effect. The "within-occupation" component of child penalty in percentage terms is then defined by $exp(\hat{\beta}_2) - 1$. To obtain the occupational change estimate, I first estimate the child penalty without controlling for occupations from the regression: $ln(Y_{iat}^{g}) = \beta_1^g D_{it} + \mu_a^g + \lambda_t^g + \epsilon_{it}^g$. The child penalty that comes from occupational change in percentage terms is calculated by the difference between the two regression estimates: $\exp(\hat{\beta}_1^g) - exp(\hat{\beta}_2^g)$.

in the 2000s and 8.5% in the 2010s.[14] Virtually all of this income penalty can be attributed to the occupational change induced by parenthood.[15] Notably, the occupational change component does not lead to an income gap between men and women, as it affects both similarly. In other words, the child-induced income gap between men and women is primarily driven by within-occupation

[14] These trends are unlikely to be driven by compositional changes in who becomes a parent. While the mean age at first birth increased substantially from the 1980s to 2000, it has remained relatively stable since then (Appendix Figure A.7). Therefore, comparisons between the 2000s and 2010s involve parents with similar demographic characteristics.

[15] One explanation of this increase is given by Appendix Figure A.2, which shows that the fatherhood employment penalty on computer & mathematics-related occupations has been increasing over the last three decades.

differences.

Income penalties stem from changes in both hours worked and wages. To distinguish between these mechanisms, I analyze occupational change's role in wage penalties. Figures 3c and 3d present the results. Women's wage penalties increased from 8% in 1990s to 10% in 2010s. Approximately, three-fourths of this wage penalty is attributable to parenthood-induced occupational change. In contrast, men's wage penalties mirror their income penalties: from a 4% wage penalty in the 1990s to an 8.4% penalty in the 2010s.[16] The similar magnitudes of wage and income penalties are consistent with men not incurring hour penalties in most occupations, as shown in Figure A.1 in the Online Appendix.

This decomposition demonstrates that occupational heterogeneity in child penalties constitutes the primary driver of wage penalties across genders, as both men and women disproportionately exit higher-wage occupations after becoming parents. Remarkably, the differential wage penalty between mothers and fathers decreased from 4% to 2%. This is notable for two reasons. First, the raw hourly wage gap in my sample was 20% in the 1990s and 13% in the 2010s. Parenthood thus accounted for roughly 20% of the wage gap in the 1990s (4 out of 20 percentage points) and 15% in the 2010s (2 out of 13 percentage points)—a meaningful but not dominant share. Second, even this modest contribution decreased by half over time. These results highlight an important aspect of child penalties in the US: parenthood causes significant disparities between men and women in employment probability, hours worked, and therefore income, but does not cause a major disparity in hourly wages conditional on working.[17]

These findings do not indicate the absence of a gender wage gap in the 2010s, but rather that parenthood does not contribute significantly to this disparity. Appendix Figure A.6 illustrates this shift by tracking the lifecycle evolution of gender wage gaps among both parents and nonparents within the sample of eventual parents. During the 1990s, the wage gap was consistently larger among parents than nonparents across all age cohorts, demonstrating parenthood's substantial role in gender wage inequality. However, in the 2010s, this pattern disappeared for individuals aged 24–34, with parents and nonparents exhibiting statistically identical wage gaps, which explains why parenthood's contribution to the wage disparities decreased during this period.

[16] Appendix Figure B.3 shows that these results are robust to dropping observations with hourly wages below the federal minimum wage and using a linear design as in Kleven et al. (2019a).

[17] Appendix Figure B.2 replicates the results on income using a linear design that does not condition on participation, as in Kleven et al. (2019a). My results remain robust: men's income penalties increased overtime while women's decreased. The conditional wage and income effects, as shown in the main text, need not be causal, as selective exit from employment may bias the estimates. If higher-wage women disproportionately exit employment after motherhood, the wage penalty would be amplified; if lower-wage women exit, it would be attenuated. I condition on employment for two reasons. First, the literature on the gender pay gap predominantly conditions on employment, and my goal is to understand how parenthood contributes to this widely-studied statistic. Second, including zeros would mechanically inflate the role of occupational change in my decomposition, as transitions to non-employment could dominate the occupational heterogeneity I seek to characterize. While selection bias remains a concern, the temporal patterns in my results are unlikely to be driven by changing selection: employment penalties have been stable for both men and women since 1990 (Appendix Figure A.9), suggesting that any selection bias would be roughly constant across decades. Despite these limitations, the exercise is substantively important: parenthood causes significant disparities in employment, and in hours conditional on working, but does not cause a major disparity in hourly wages conditional on working.

A common concern about the event-study design for child penalties is that while short-term effects are well identified, long-term effects may be confounded by selection into the timing of parenthood. Parents who have children at ages 28 and 30 are likely more comparable than those who have children at ages 28 and 38. If selection bias from endogenous timing increased over time, it could explain my results. To address this concern, I replicate the analysis restricting the sample to the first five years after childbirth. Appendix Figure A.5 presents the results. The same pattern emerges: men's wage penalties increased over time, from an insignificant 2.9% in the 1990s to a statistically significant 6.4% in the 2010s, and the differential wage penalty between men and women decreased. This restricted sample has more plausible identification assumptions, though the estimated effects are smaller since penalties accumulate over time.

In summary, Figure 3 reveals that occupational heterogeneity in child employment penalties is a major component of income and wage penalties for both genders. Both women and men experience higher exit rates from high-paying occupations compared to low-paying ones, resulting in substantial income and wage reductions. Additionally, my results indicate a structural change in the labor market: while women's income penalties have decreased from the 1990s to the 2010s, men's income and wage penalties have increased. Since 2010, parenthood no longer functions as a key determinant of the gender wage gap.

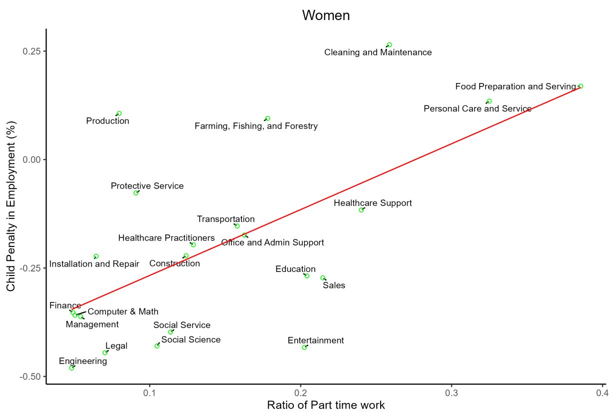
4.3 What explains the heterogeneity across occupations?

This section examines which occupational attributes explain the heterogeneous child penalties across occupations. I focus on part-time availability in the main text because I find it to be the best explanatory factor. In the Online Appendix I show that other factors, such as the flexibility to determine when to begin and end the work day (hour flexibility hereafter) or the representation on women does not explain the differences in child penalties between genders.

I focus on the bivariate relationship between child penalties and part-time availability. Figure 4 presents three scatterplots examining the relationship between part-time availability and child penalties for women, men, and the gender difference. Figure 4a shows that women lose most jobs in occupations with fewer part-time positions and gain jobs in occupations with more part-time positions. For example, less than 5% of workers in management, finance, engineering, and legal are part-time, and women lose most jobs relative to their baseline employment rates in these occupations. In contrast, personal care, cleaning, and food preparation are occupations with the highest rates of part-time workers, and women transition into these occupations after becoming mothers.

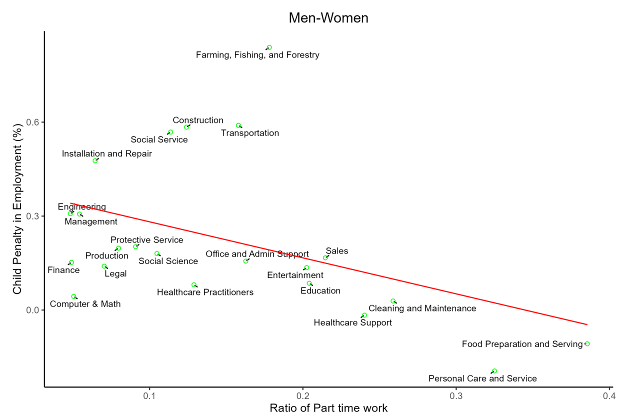
Figure 4b replicates this analysis on the employment penalties for men. I find no relationship between the fatherhood employment penalty and the availability of part-time work. Figure 4c replicates this analysis on the difference in penalties between men and women and documents that part-time availability is negatively correlated with the inequality-inducing part of child penalties. This is expected since women incur lower penalties in occupations that allow for part-time work, and men are unimpacted. Therefore, there is less inequality between men and women in child employment penalties in occupations with more part-time availability.

Figure 4: Occupation-level Child Penalties and Part-time Availability



(a) Motherhood penalty

(b) Fatherhood penalty



(c) Gender gap in child penalty

Notes: Part-time availability is calculated using the sample of all workers without kids in the CPS.

Appendix Table A.1 extends this analysis through three dimensions: (1) reporting coefficient estimates from bivariate regressions of child penalties on occupational attributes, (2) expanding dependent variables to include both income and hours penalties, and (3) incorporating additional explanatory variables, specifically hour flexibility proxied by the self-declared ability of workers to alter the start and end times of their work days, and women's representation. This expanded analysis yields two significant findings.

First, occupations with greater part-time availability exhibit larger reductions in women's income and hours worked. This pattern suggests that part-time availability enables women to maintain employment while reducing hours. This dual effect highlights part-time work's paradoxical role: facilitating continued employment post-childbirth while simultaneously constraining income potential.[18]

[18]Online Appendix Tables B.1 and B.2 show that these results remain robust to defining the explanatory variables using all workers and eventual parents the year before having children.

Second, neither hour flexibility nor women's occupational representation explains gendered heterogeneity in child employment penalties across occupations. Both genders experience higher job losses in occupations offering greater hour flexibility, resulting in no significant relationship between flexibility and differences in penalties between men and women. Similarly, the proportion of women in an occupation is not correlated with the employment penalty from motherhood.

Appendix Table B.3 further extends this analysis by using child penalty estimates across all occupations, instead of the 22 major groups. My results remain robust: availability of part-time work is associated with smaller employment penalties but larger income and hours penalties for women.

Mothers sorting into occupations with part-time options can be driven by two mechanisms: an increased preference toward reducing hours or firms discriminating against mothers in full-time positions. While I argue that the former is more likely in the present setting, I cannot rule out the discrimination channel with the available data at hand. Future work can distinguish between these mechanisms by collecting data on women's preferences for part-time positions.

Notably, if the results are driven by women's changing preferences, these findings would have ambiguous policy implications. A revealed preference approach would conclude that women are better off in terms of welfare when there are part-time options. Otherwise, they would not sort into occupations with part-time positions after childbirth. However, in terms of gender inequality in the labor market, part-time work can increase or decrease the gender income gap. Imagine a model where workers choose between three options: no work, part-time work, and full-time work. My results indicate that women's preference towards part-time work increases, but my results cannot say whether women would have remained outside of the labor force or worked full-time if part-time wasn't available. If women would have chosen full-time employment, part-time availability would actually *increase* income gaps.

5 Conclusion

This paper makes two contributions. First, I demonstrate how rotating panel datasets can be leveraged to estimate child penalties with greater precision and without requiring additional assumptions beyond the standard random timing of first child. This approach complements Kleven (2025)'s pseudo-panel method, which enables child penalty estimation using cross-sectional data through matching. When rotating panel data are available, my method offers an alternative that avoids the matching step; when only cross-sectional data are available or rotating panels are not large enough, researchers can use the pseudo-panel approach. Together, these methods expand the toolkit for studying child penalties across contexts where traditional panel data are unavailable or insufficient.

Second, using this method, I document that occupational segregation between genders, the single largest factor in the gender pay gap, is partly driven by parenthood. Both women and men lose jobs in some occupations and gain jobs in others after becoming parents. These occupational changes explain 40% of the income penalty for women, most of the income penalty for men, and most of the wage penalty for both genders. For mothers, these occupational changes are partly driven by preferences for reduced working hours, as evidenced by their tendency to transition into occupations with greater part-time availability. Notably, parenthood's contribution to the gender wage gap decreased by half from 1990s to 2010s, not because women incur lower wage penalties but because men incur higher penalties.

Several important questions emerge from these findings, which I leave for future work. First, occupational heterogeneity in child penalties incorporates three distinct mechanisms: differential employment exit rates across occupations, direct occupational changes, and transitions from non-employment into specific occupations. Future research with larger panel datasets could disentangle these mechanisms to provide a more nuanced understanding of parenthood's impact on occupational sorting.

Second, the contribution of occupational change to income and wage penalties has increased for both genders over time, even as women's overall income penalties have decreased. This suggests that while improvements have occurred within occupations, the negative impact of occupational re-sorting has intensified. Understanding the causes behind these opposing trends requires further investigation.

Third, the significant increase in men's income and wage penalties over the last twenty years represents a fundamental shift in how parenthood affects labor market outcomes. The causes of this shift, whether changing social norms, economic pressures, or policy environments, merit dedicated study.

Future research might also examine how emerging workplace trends, particularly remote work, affect child penalties. Since motherhood seems to increase the preference for reduced working hours, likely due to women's caregiving roles, remote work flexibility might substitute for part-time arrangements and potentially reduce gender pay gaps by enabling mothers to maintain higher working hours. How this impacts the gender gap in the labor market and women's welfare are interesting avenues for future research.

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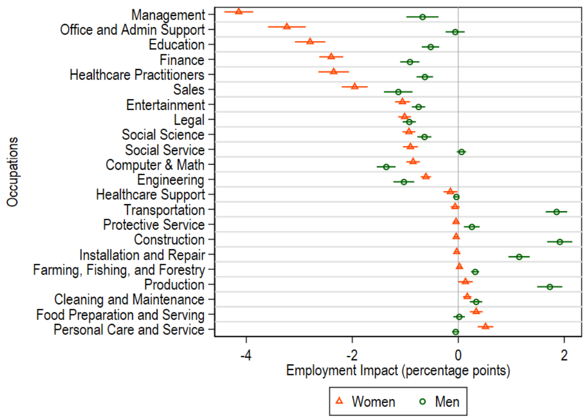
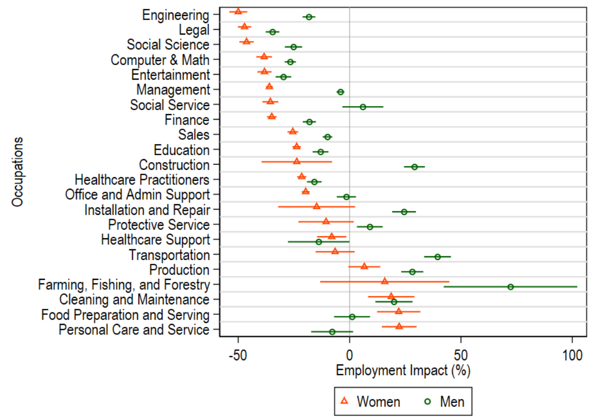
Online Appendix

A Additional Results

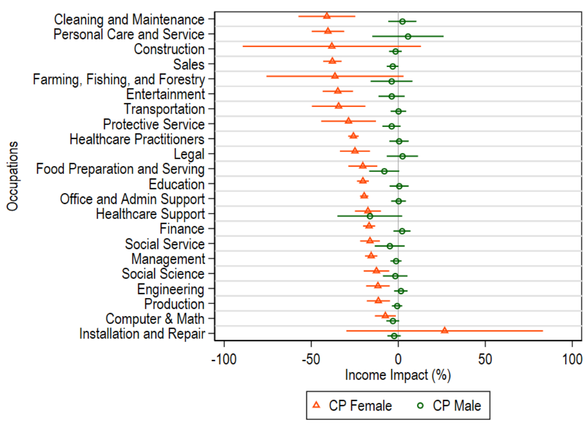
Figure A.1: Occupational Heterogeneity in Child Penalty



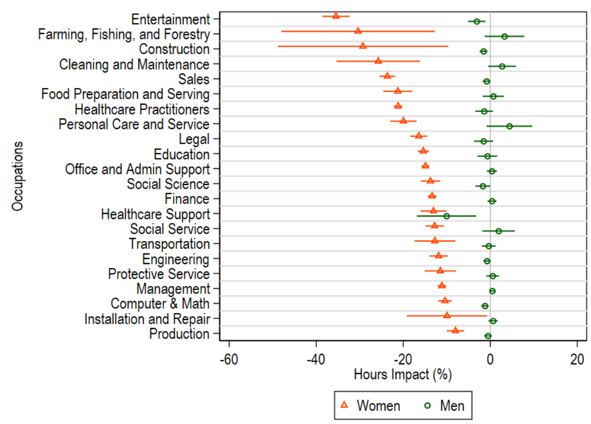
(a) Employment (in pp)



(b) Employment (in %)



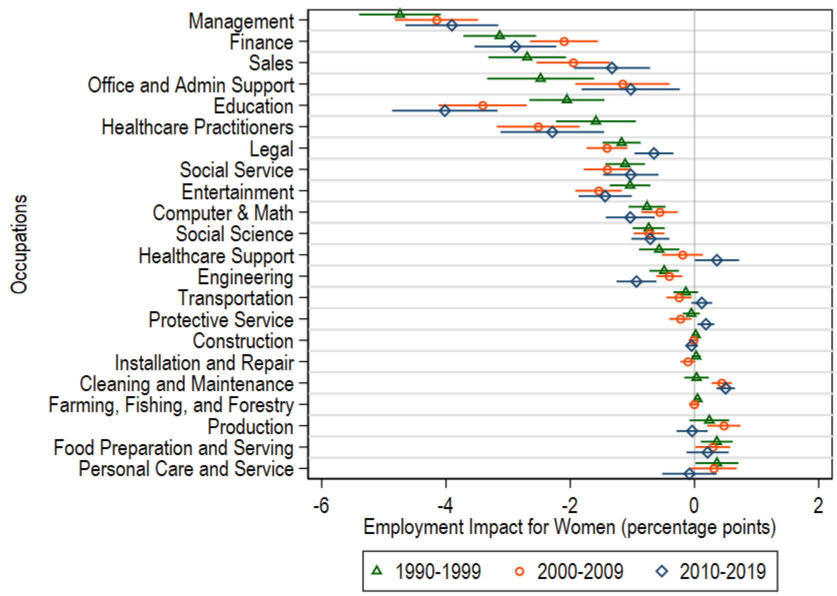
(c) Income (in %)



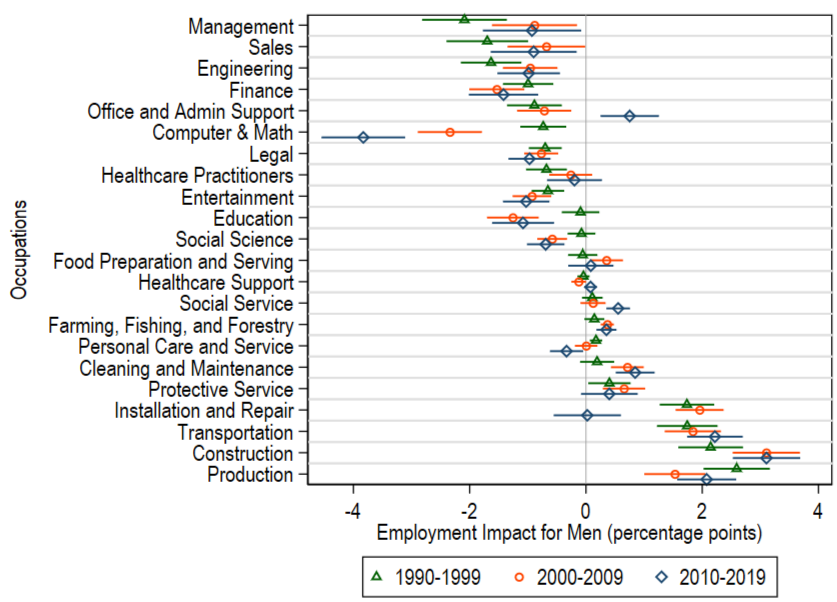
(d) Hours (in %)

Note: Results on employment come from the regression $Employment^{o,g}_{iat} = \beta^{o,g} D_{it} + \mu^{o,g}_a + \lambda^{o,g}_t + \epsilon^{o,g}_{it}$, where $Employment^{o,g}_{iat}$ is a dummy equaling to one if individual i of gender g is employed in occupation o at time t, μ_a and λ_t are age and year fixed effects that control non-parametrically for lifecycle trends and time trends. Superscripts mean sample restrictions. For each gender $g \in \{m, w\}$, 22 separate regressions are run for each occupation-specific outcome. To obtain percentage estimates, I divide the level estimates $\beta^{o,g}$ with predicted outcome absent child effects: $P^{o,g} = \frac{\hat{\beta}^{o,g}}{E[\widetilde{Y}^{o,g}_{iat}]}$, where $\widetilde{Y}^{o}_{iat}$ is the predicted employment rate when omitting the contribution of the child effect. Results on income and hours come from the regression $ln(Y^{o,g}_{iat}) = \gamma^{o,g} D_{it} + \mu^{o,g}_a + \lambda^{o,g}_t + \epsilon^{o,g}_{it}$, where the outcome is either the log income or the log hours worked of an individual i at time t. $\hat{\gamma}^{o,g}$ estimates come from 44 different samples for each occupation-gender combination. 95% Confidence intervals are plotted.

Figure A.2: Evolution of the Occupational Heterogeneity in Child Penalty



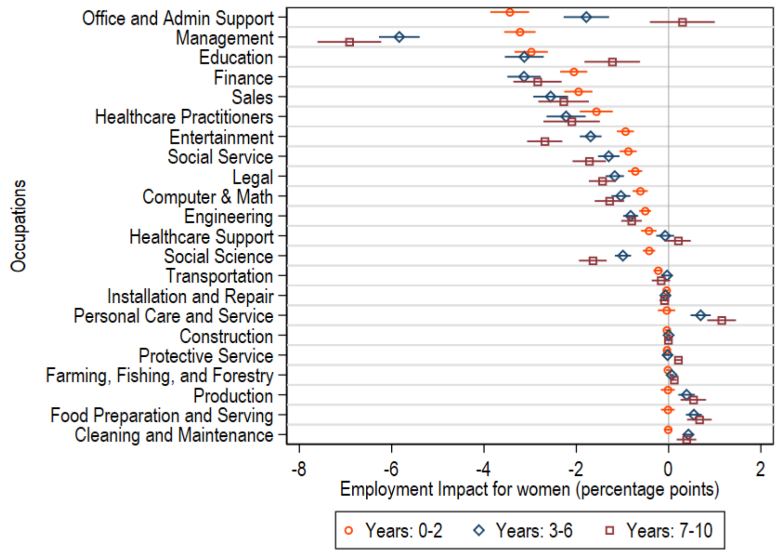
(a) Women



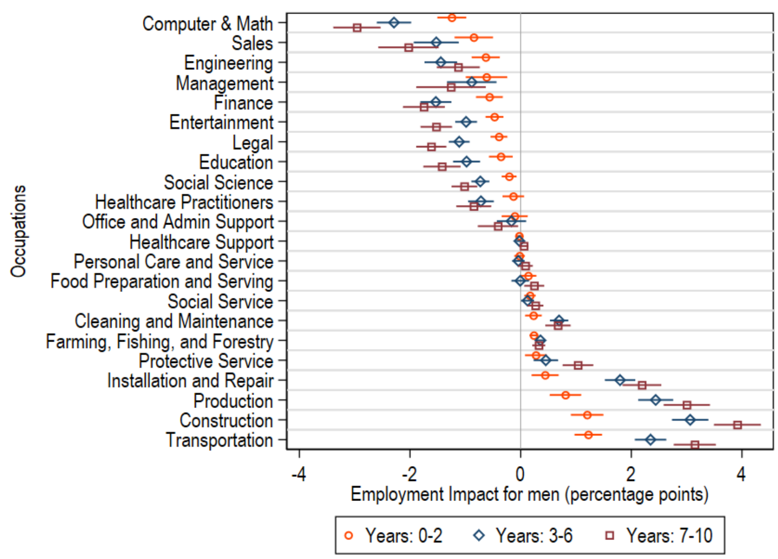
(b) Men

Note: Results on employment come from the regression $Employment_{iat}^{o,g,Y} = \beta^{o,g,Y} D_{it} + \mu_a^{o,g,Y} + \lambda_t^{o,g,Y} + \epsilon_{it}^{o,g,Y}$, where $Employment_{iat}^{o,g,Y}$ is a dummy equaling to one if individual i of gender g and time period Y is employed in occupation o at time t, μ_a and λ_t are age and year fixed effects that control non-parametrically for lifecycle trends and time trends. Superscripts mean sample restrictions. For each gender $g \in \{m, w\}$ and decade $Y \in \{1990\text{-}1999, 2000\text{-}2009, 2010\text{-}2019\}$, 22 separate regressions are run for each occupation-specific outcome.

Figure A.3: Occupational Heterogeneity in Child Employment Penalty in Short, Medium, and Long-run



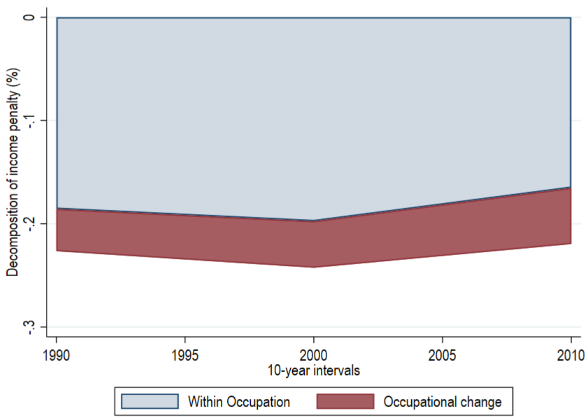
(a) Employment Penalties for Women



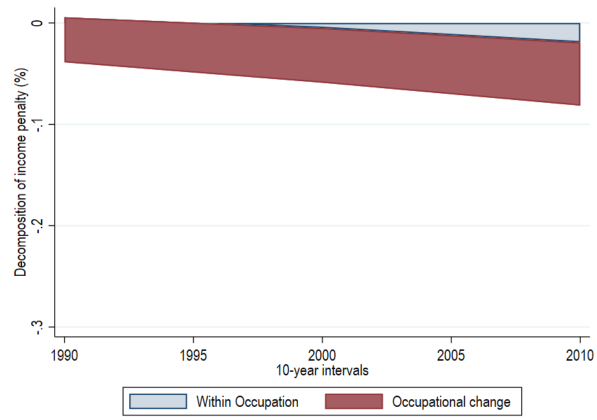
(b) Employment Penalties for Men

Note: Results on employment come from the regression $Employment^{o,g}_{iat} = \beta^{g,S}_o D^S_{it} + \beta^{g,M}_o D^M_{it} + \beta^{g,L}_o D^L_{it} + \mu^{o,g}_a + \lambda^{o,g}_t + \epsilon^{o,g}_{it}$, where D^S, D^M, D^L denote the short-run, medium-run and long-run treatments as indicators for person having an eldest child of ages between 0 and 2, 3 and 6, and 7 and 10, respectively, $Employment^{o,g}_{iat}$ is a dummy equaling to one if individual i of gender g is employed in occupation o at time t, μ_a and λ_t are age and year fixed effects that control non-parametrically for lifecycle trends and time trends. Superscripts mean sample restrictions. For each gender $g \in \{m, w\}$, 22 separate regressions are run for each occupation-specific outcome. Robust standard errors are used to calculate the 95% confidence intervals.

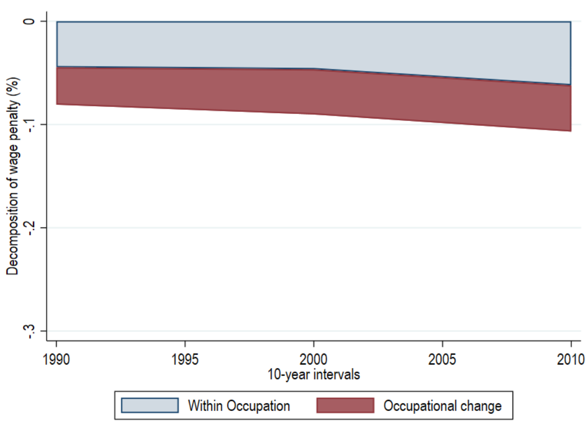
Figure A.4: Decomposition of Income and Wage Penalties



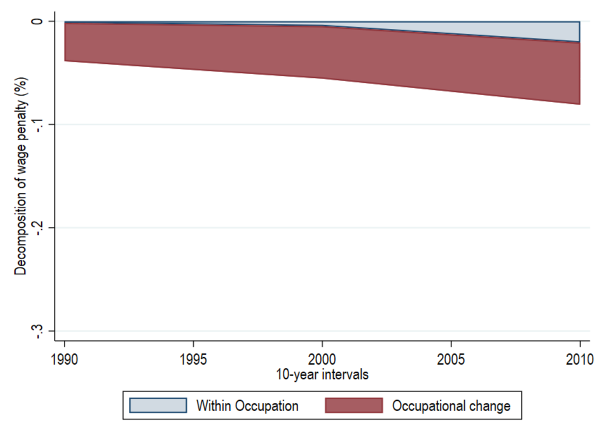
(a) Income penalty for women



(b) Income penalty for men



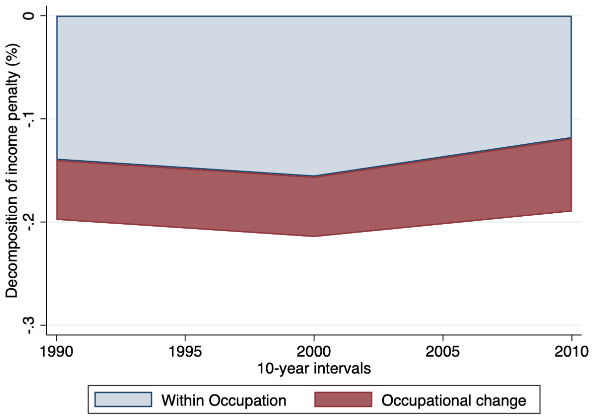
(c) Wage penalty for women



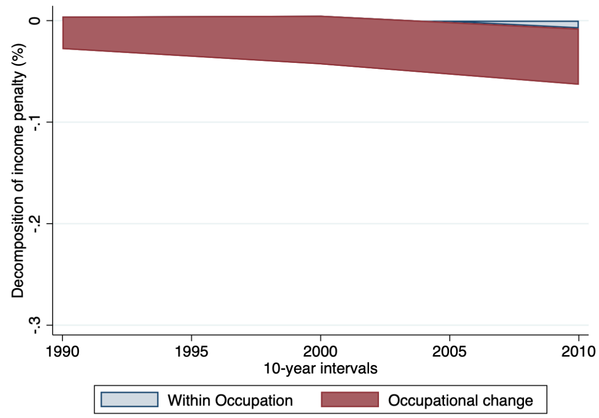
(d) Wage penalty for men

Note: Within occupation estimates come from the regression: $ln(Y^g_{iat}) = \beta^g_2 D_{it} + \mu^g_a + \lambda^g_t + \theta^g Occ22_{it} + \eta^g_{it}$, where $ln(Y^g_{iat})$ is the log-income or log-wage of individual i of age a at time t from gender g, μ_a and λ_t are age and year fixed effects that control non-parametrically for lifecycle trends and time trends, $Occ22_{it}$ is an occupation fixed effect for the 22 major occupation groups excluding military service. The "within-occupation" component of child penalty in percentage terms is then defined by $exp(\hat{\beta}_2) - 1$. To obtain the occupational change estimate, I first estimate the child penalty without controlling for occupations from the regression: $ln(Y^g_{iat}) = \beta^g_1 D_{it} + \mu^g_a + \lambda^g_t + \epsilon^g_{it}$. The child penalty that comes from occupational change in percentage terms is calculated by the difference between the two regression estimates: $\exp(\hat{\beta}^g_1) - exp(\hat{\beta}^g_2)$.

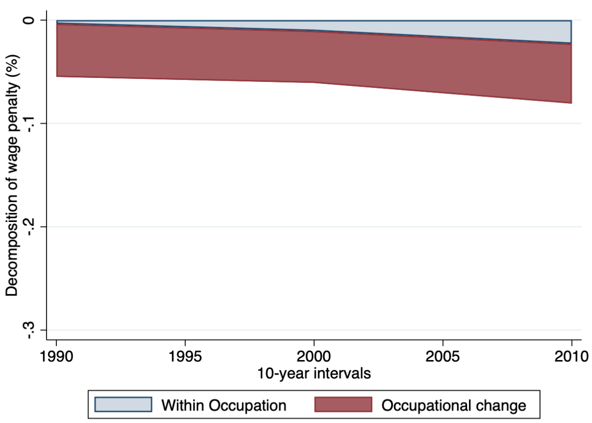
Figure A.5: Decomposition of Income and Wage Penalties using Parents with Children younger than 5 years old



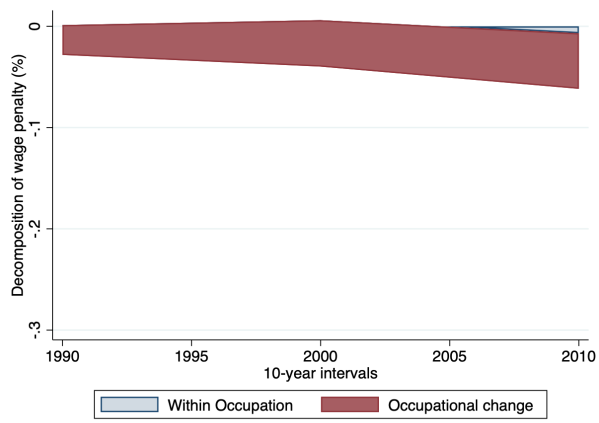
(a) Income penalty for women



(b) Income penalty for men



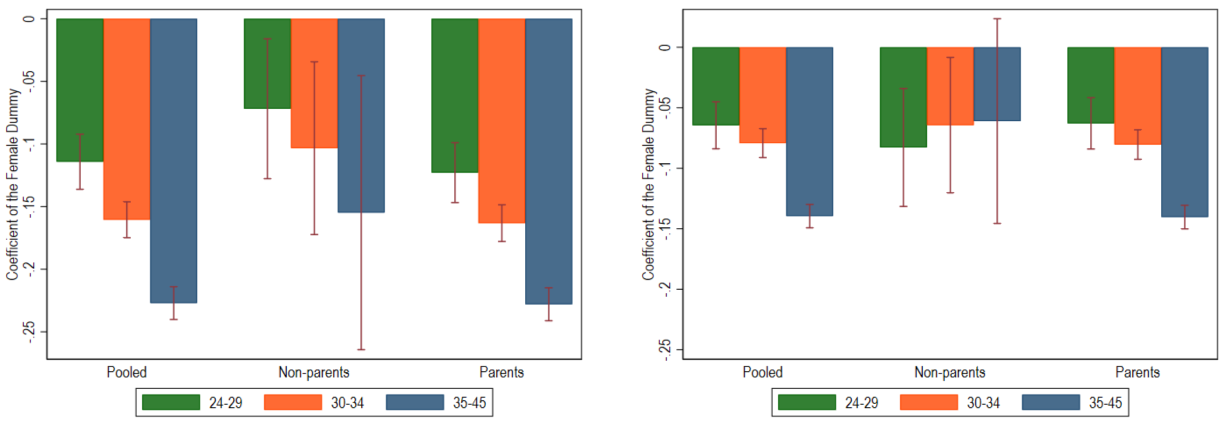
(c) Wage penalty for women



(d) Wage penalty for men

Note: Sample restricted to parents with children younger than 5 years old. Within occupation estimates come from the regression: $ln(Y^g_{iat}) = \beta^g_2 D_{it} + \mu^g_a + \lambda^g_t + \theta^g Occ_{it} + \eta^g_{it}$, where $ln(Y^g_{iat})$ is the log-income or log-wage of individual i of age a at time t from gender g, μ_a and λ_t are age and year fixed effects that control non-parametrically for lifecycle trends and time trends, Occ_{it} is an occupation fixed effect. The "within-occupation" component of child penalty in percentage terms is then defined by $exp(\hat{\beta}_2) - 1$. To obtain the occupational change estimate, I first estimate the child penalty without controlling for occupations from the regression: $ln(Y^g_{iat}) = \beta^g_1 D_{it} + \mu^g_a + \lambda^g_t + \epsilon^g_{it}$. The child penalty that comes from occupational change in percentage terms is calculated by the difference between the two regression estimates: $\exp(\hat{\beta}^g_1) - exp(\hat{\beta}^g_2)$.

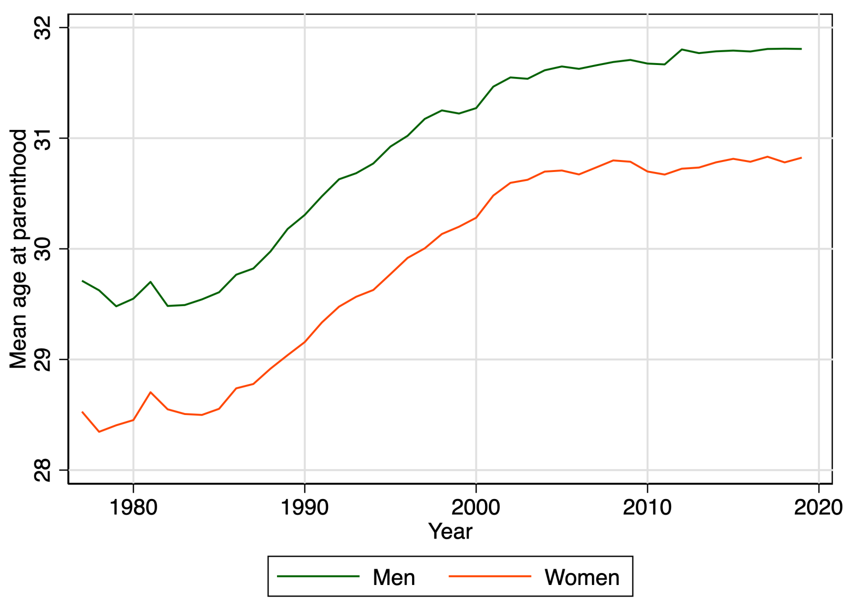
Figure A.6: Decomposition of the Gender Inequality in Wages



(a) 1990–1999 (b) 2010–2019

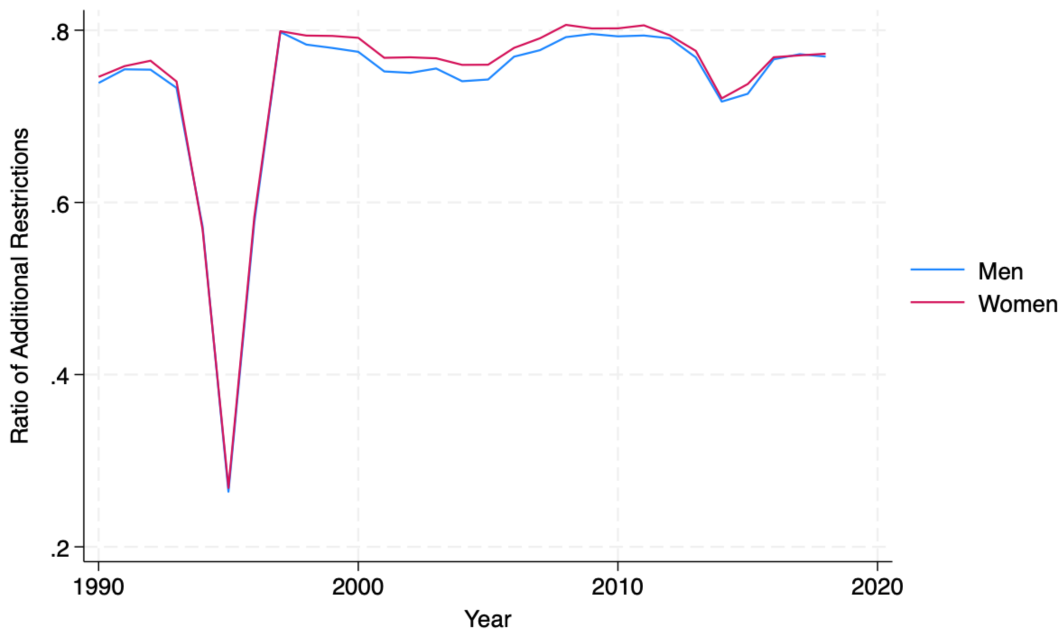
Note: This figure plots the coefficient estimate from the regression: $ln(wage)^s_{iat} = \beta^s Female^s_i + \mu^s_a + \lambda^s_t + \epsilon^s_{it}$, where *Female* is a dummy variable, μ_a and λ_t are age and calendar year fixed effects, and superscript s denotes different samples. In particular, I separately estimate β in two decades, three age groups, and using parents and non-parents *within* the sample of eventual parents.

Figure A.7: Age at birth across years



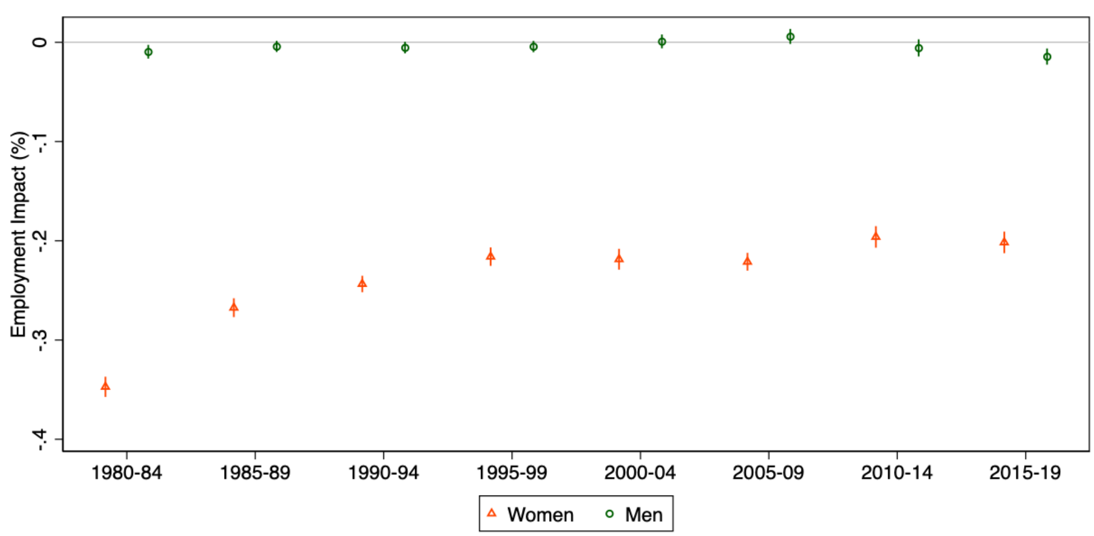
Note: Sample restricted to eventual parents aged 20–55 who had their first child between ages 25–45, appear in both CPS interview rounds, and have an eldest child aged 10 or younger. Parents whose eldest child's age evolves unexpectedly are excluded.

Figure A.8: Share of Sample Retained After Additional Restrictions



Notes: This figure shows the share of eventual parents retained after imposing two additional restrictions: (1) individuals must appear in both CPS interview rounds, and (2) the eldest child's age must evolve as expected across observations (changing by at most two years). The drop around 1994–1995 reflects the CPS redesign, which changed household identifiers and reduced the share of individuals trackable across rounds. Since 1996, the restrictions bind at a stable rate.

Figure A.9: Child Employment Penalties Across Decades



Note: Estimates come from the regression equation $Y^g_{iat} = D_{it} + \mu^g_a + \lambda^g_t + \epsilon^g_{it}$, where Y^g_{iat} is the outcome for individual i of age a and gender $g = w, m$ at event time t, $D_{it} = 1$ is an indicator of parenthood, μ_a and λ_t are age and year fixed effects that control non-parametrically for lifecycle trends and time trends. Percentage estimates are obtained by dividing the level estimates β^g_j with predicted outcome absent child effects.

Table A.1: Correlates with Occupation-level Child Penalties
Sample: Workers without children

	Women			Men			Inequality: Men - Women		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Employment									
Share of part time	0.142***			0.034			-0.107***		
	(0.030)			(0.043)			(0.034)		
Hour flexibility		-0.137***			-0.176***			-0.039	
		(0.032)			(0.043)			(0.040)	
Share of women			-0.010			-0.163***			-0.152***
			(0.038)			(0.052)			(0.041)
Panel B: Income									
Share of part time	-0.085**			-0.018			0.067		
	(0.043)			(0.014)			(0.051)		
Hour flexibility		-0.006			0.020			0.025	
		(0.043)			(0.013)			(0.047)	
Share of women			-0.048			0.000			0.049
			(0.060)			(0.016)			(0.064)
Panel C: Hours									
Share of part time	-0.041***			0.001			0.042***		
	(0.010)			(0.009)			(0.014)		
Hour flexibility		-0.011			0.003			0.014	
		(0.017)			(0.007)			(0.019)	
Share of women			-0.024**			-0.007			0.017
			(0.012)			(0.011)			(0.020)

Notes: Each column shows the estimates of a regression $\hat{\beta}_o = \gamma_0 + \delta W_o + \eta_o$, where $\hat{\beta}_o$ represents the estimated occupation-specific child penalty, and W_o is a vector of occupation attributes, which are (1) the ratio of part time workers, (2) the ratio of people who state that their job provides hour flexibility, and (3) the ratio of women. These attributes are calculated using the sample of all workers without kids. In Panel A, the outcome is the employment penalty estimate in percentages coming from the regression $Employment^{o,g}_{iat} = \beta^{o,g} D_{it} + \mu^{o,g}_a + \lambda^{o,g}_t + \epsilon^{o,g}_{it}$, where $Employment^{o,g}_{iat}$ is a dummy equaling to one if individual i of gender g is employed in occupation o at time t, μ_a and λ_t are age and year fixed effects that control non-parametrically for lifecycle trends and time trends. Percentage effects are calculated by dividing $\hat{\beta}^{o,g}$ by the predicted outcome absent child effects: $P^{o,g} = \frac{\hat{\beta}^{o,g}}{E[\tilde{Y}^{o,g}_{iat}]}$ In Panels B and C, the outcome variable is the income and hour penalty estimate $\hat{\gamma}^{o,g}$ coming from the regression $ln(Y^{o,g}_{iat}) = \gamma^{o,g} D_{it} + \mu^{o,g}_a + \lambda^{o,g}_t + \epsilon^{o,g}_{it}$. Unit of observation is major occupation group, so N=22. Robust standard errors are shown in parenthesis.
** p<0.10, ** p<0.05, *** p<0.01*

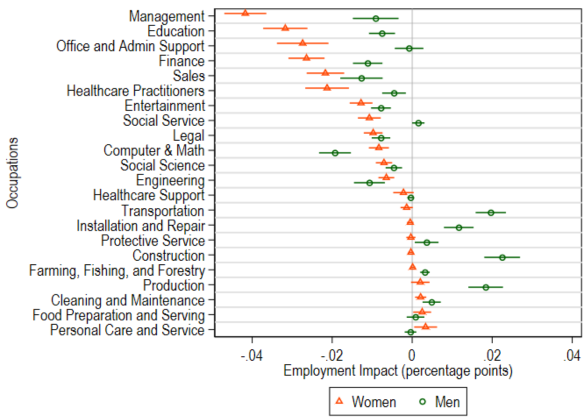
Table A.2: Predicted Employment Rate absent Child Effects

	Men	Women
Management	0.165	0.117
Finance	0.056	0.076
Computer & Math	0.064	0.024
Engineering	0.061	0.014
Social Science	0.018	0.017
Social Service	0.009	0.028
Legal	0.025	0.022
Education	0.042	0.119
Entertainment	0.026	0.030
Healthcare Practitioners	0.040	0.110
Healthcare Support	0.003	0.019
Protective Service	0.029	0.004
Food Preparation and Serving	0.014	0.015
Cleaning and Maintenance	0.017	0.008
Personal Care and Service	0.007	0.025
Sales	0.119	0.080
Office and Admin Support	0.043	0.154
Farming, Fishing, and Forestry	0.003	0.001
Construction	0.062	0.001
Installation and Repair	0.045	0.002
Production	0.058	0.018
Transportation	0.044	0.009

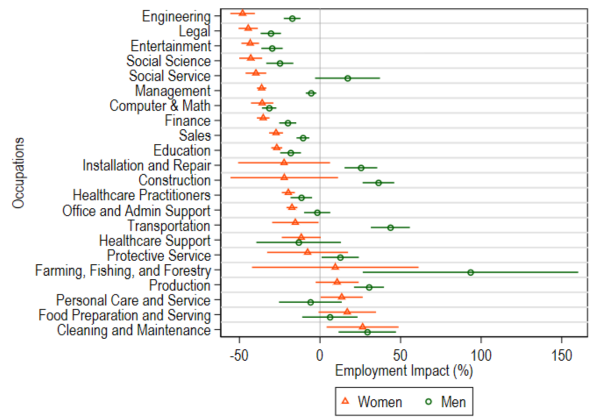
Notes: This table reports the predicted employment rate in each occupation × gender cell in the absence of child-related effects. The estimates in Figure 2a are in percentage points. The estimates in Figure 2b express the same effects as percentages relative to the baseline employment rate in this table, i.e., the percentage-point child effect divided by the corresponding occupation–gender predicted employment rate.

B Robustness Checks

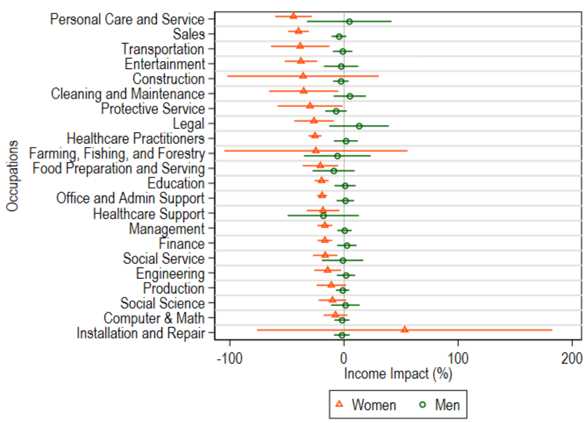
Figure B.1: Occupational Heterogeneity in Child Penalty (Bonferroni corrected confidence intervals)



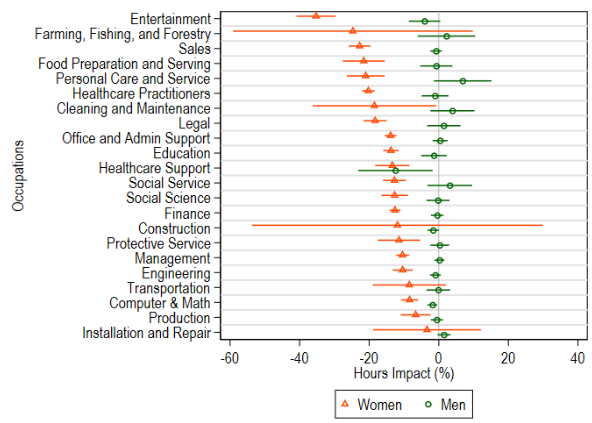
(a) Employment (in pp)



(b) Employment (in %)



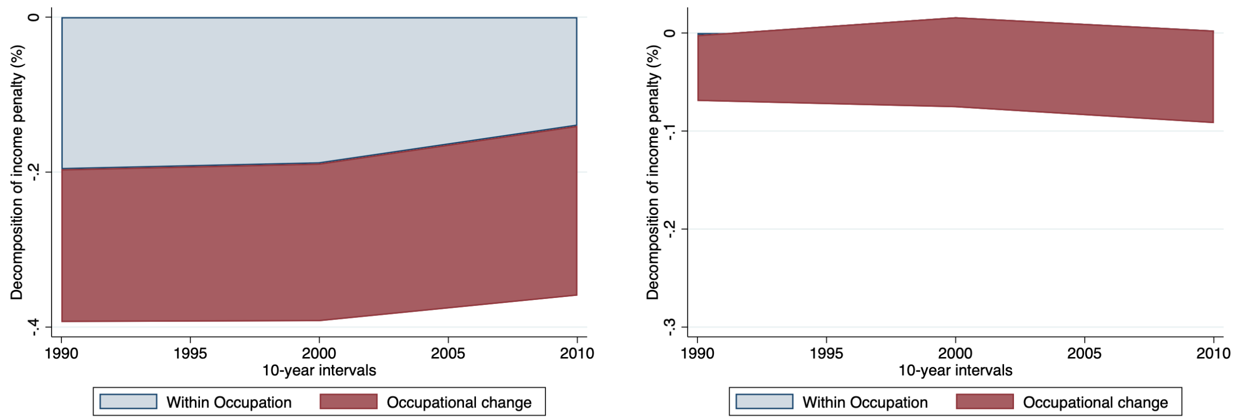
(c) Income (in %)



(d) Hours (in %)

Note: Results on employment come from the regression $Employment_{iat}^{o,g} = \beta^{o,g} D_{it} + \mu_a^{o,g} + \lambda_t^{o,g} + \epsilon_{it}^{o,g}$, where $Employment_{iat}^{o,g}$ is a dummy equaling to one if individual i of gender g is employed in occupation o at time t, μ_a and λ_t are age and year fixed effects that control non-parametrically for lifecycle trends and time trends. Superscripts mean sample restrictions. For each gender $g \in \{m, w\}$, 22 separate regressions are run for each occupation-specific outcome. To obtain percentage estimates, I divide the level estimates $\beta^{o,g}$ with predicted outcome absent child effects: $P^{o,g} = \frac{\hat{\beta}^{o,g}}{E[\widetilde{Y}_{iat}^{o,g}]}$, where $\widetilde{Y}_{iat}^{o}$ is the predicted employment rate when omitting the contribution of the child effect. Results on income and hours come from the regression $ln(Y_{iat}^{o,g}) = \gamma^{o,g} D_{it} + \mu_a^{o,g} + \lambda_t^{o,g} + \epsilon_{it}^{o,g}$, where the outcome is either the log income or the log hours worked of an individual i at time t. $\hat{\gamma}^{o,g}$ estimates come from 44 different samples for each occupation-gender combination. 95% Confidence intervals are plotted after adjusting the critical values using Bonferroni correction.

Figure B.2: Decomposition of Unconditional Income Penalties

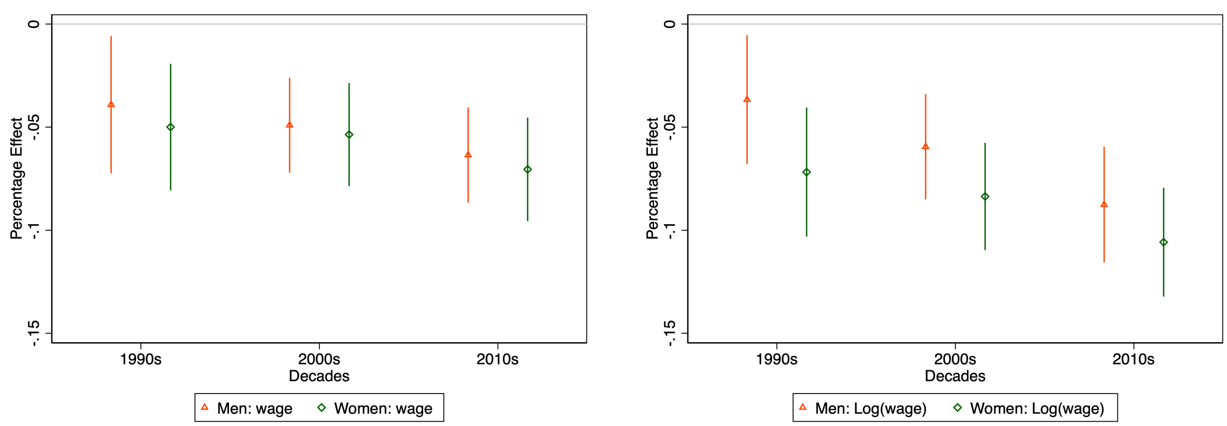


(a) Income penalty for women

(b) Income penalty for men

Notes: This figure replicates the decomposition in Figure 3 using unconditional income (assigning zero income to non-employed individuals) rather than income conditional on employment. Within-occupation estimates come from the regression $Y^g_{iat} = \sum_{j \neq -1} \beta^g_{2,j} \Delta D_{i,t-j} + \mu^g_a + \lambda^g_t + \theta^g Occ_{it} + \eta^g_{it}$, where Y^g_{iat} is unconditional income, μ_a and λ_t are age and year fixed effects, and Occ_{it} is an occupation fixed effect. Percentage estimates are obtained by dividing level estimates by the predicted outcome absent child effects. The occupational change component is calculated as the difference between total and within-occupation penalties.

Figure B.3: Wage Penalties Using Linear vs. Log-Linear Design



(a) Wage penalty (linear design)

(b) Wage penalty (log-linear design)

Notes: This figure compares wage penalties across decades using two specifications. Panel (a) uses a linear design: $Y^g_{iat} = \beta^g_1 D_{it} + \mu^g_a + \lambda^g_t + \eta^g_{it}$, where Y^g_{iat} is the hourly wage. Panel (b) uses the log-linear design from the main text: $\ln(Y^g_{iat}) = \beta^g_2 D_{it} + \mu^g_a + \lambda^g_t + \eta^g_{it}$. I omit 2009 due to increased wage variance in that year, which makes the 2000s estimate too noisy under the linear design. Observations with wages below the federal minimum wage ($4.25) are excluded. Both panels show that men and women incur similar wage penalties and that the differential between genders decreased over time.

Table B.1: Correlates with Occupation-level Child Penalties
Sample: All workers

	Women			Men			Inequality: Men - Women		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Employment									
Share of part time	0.132***			0.007			-0.125***		
	(0.033)			(0.043)			(0.031)		
Hour flexibility		-0.141***			-0.176***			-0.035	
		(0.032)			(0.045)			(0.043)	
Share of women			0.019			-0.139***			-0.158***
			(0.035)			(0.049)			(0.037)
Panel B: Income									
Share of part time	-0.087*			-0.015			0.072		
	(0.045)			(0.015)			(0.053)		
Hour flexibility		-0.009			0.021			0.030	
		(0.043)			(0.013)			(0.047)	
Share of women			-0.055			-0.002			0.053
			(0.060)			(0.016)			(0.065)
Panel C: Hours									
Share of part time	-0.044***			0.000			0.044***		
	(0.010)			(0.010)			(0.014)		
Hour flexibility		-0.012			0.003			0.015	
		(0.017)			(0.006)			(0.019)	
Share of women			-0.026**			-0.006			0.020
			(0.011)			(0.012)			(0.020)

*Notes: Each column shows the estimates of a regression $\hat{\beta}_o = \gamma_0 + \delta W_o + \eta_o$, where $\hat{\beta}_o$ represents the estimated occupation-specific child penalty, and W_o is a vector of occupation attributes, which are (1) the ratio of part time workers, (2) the ratio of people who state that their job provides hour flexibility, and (3) the ratio of women. These attributes are calculated using the sample of all workers. In Panel A, the outcome is the employment penalty estimate in percentages coming from the regression $Employment^{o,g}_{iat} = \beta^{o,g} D_{it} + \mu^{o,g}_a + \lambda^{o,g}_t + \epsilon^{o,g}_{it}$, where $Employment^{o,g}_{iat}$ is a dummy equaling to one if individual i of gender g is employed in occupation o at time t, μ_a and λ_t are age and year fixed effects that control non-parametrically for lifecycle trends and time trends. Percentage effects are calculated by dividing $\hat{\beta}^{o,g}$ by the predicted outcome absent child effects: $P^{o,g} = \frac{\hat{\beta}^{o,g}}{E[\tilde{Y}^{o,g}_{iat}]}$ In Panels B and C, the outcome variable is the income and hour penalty estimate $\hat{\gamma}^{o,g}$ coming from the regression $ln(Y^{o,g}_{iat}) = \gamma^{o,g} D_{it} + \mu^{o,g}_a + \lambda^{o,g}_t + \epsilon^{o,g}_{it}$. Unit of observation is major occupation group, so N=22. Robust standard errors are shown in parenthesis. * p<0.10, ** p<0.05, *** p<0.01*

Table B.2: Correlates with Occupation-level Child Penalties
Sample: Eventual parents at $t = -1$

	Women			Men			Inequality: Men - Women		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Employment									
Share of part time	0.139***			0.019			-0.120***		
	(0.031)			(0.040)			(0.029)		
Hour flexibility		-0.063			-0.077			-0.014	
		(0.042)			(0.068)			(0.058)	
Share of women			-0.014			-0.150***			-0.136***
			(0.040)			(0.050)			(0.046)
Panel B: Income									
Share of part time	-0.071*			-0.018			0.053		
	(0.040)			(0.018)			(0.051)		
Hour flexibility		-0.011			0.022			0.033	
		(0.038)			(0.015)			(0.045)	
Share of women			-0.043			-0.002			0.041
			(0.055)			(0.016)			(0.060)
Panel C: Hours									
Share of part time	-0.036***			-0.002			0.034*		
	(0.010)			(0.013)			(0.019)		
Hour flexibility		-0.010			0.011			0.021	
		(0.015)			(0.010)			(0.022)	
Share of women			-0.024**			-0.007			0.017
			(0.012)			(0.012)			(0.020)

*Notes: Each column shows the estimates of a regression $\hat{\beta}_o = \gamma_0 + \delta W_o + \eta_o$, where $\hat{\beta}_o$ represents the estimated occupation-specific child penalty, and W_o is a vector of occupation attributes, which are (1) the ratio of part time workers, (2) the ratio of people who state that their job provides hour flexibility, and (3) the ratio of women. These attributes are calculated using the sample of eventual parents before they had a child. In Panel A, the outcome is the employment penalty estimate in percentages coming from the regression $Employment^{o,g}_{iat} = \beta^{o,g} D_{it} + \mu^{o,g}_a + \lambda^{o,g}_t + \epsilon^{o,g}_{it}$, where $Employment^{o,g}_{iat}$ is a dummy equaling to one if individual i of gender g is employed in occupation o at time t, μ_a and λ_t are age and year fixed effects that control non-parametrically for lifecycle trends and time trends. Percentage effects are calculated by dividing $\hat{\beta}^{o,g}$ by the predicted outcome absent child effects: $P^{o,g} = \frac{\hat{\beta}^{o,g}}{E[\tilde{Y}^{o,g}_{iat}]}$ In Panels B and C, the outcome variable is the income and hour penalty estimate $\hat{\gamma}^{o,g}$ coming from the regression $ln(Y^{o,g}_{iat}) = \gamma^{o,g} D_{it} + \mu^{o,g}_a + \lambda^{o,g}_t + \epsilon^{o,g}_{it}$. Unit of observation is major occupation group, so N=22. Robust standard errors are shown in parenthesis. * p<0.10, ** p<0.05, *** p<0.01*

Table B.3: Correlates with Occupation-level Child Penalties Using All Occupations
Sample: Workers without children

	Women			Men			Inequality: Men - Women		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Employment									
Share of part time	0.125***			0.002			-0.162***		
	(0.026)			(0.075)			(0.034)		
Hour flexibility		-0.149***			-0.023			0.098	
		(0.021)			(0.128)			(0.064)	
Share of women			0.032			-0.108			-0.149***
			(0.026)			(0.110)			(0.037)
Panel B: Income									
Share of part time	-0.066***			0.013			0.078*		
	(0.016)			(0.022)			(0.041)		
Hour flexibility		0.018			-0.013			-0.039	
		(0.020)			(0.014)			(0.025)	
Share of women			-0.018			-0.013			-0.015
			(0.015)			(0.014)			(0.030)
Panel C: Hours									
Share of part time	-0.083***			-0.001			0.091***		
	(0.016)			(0.009)			(0.021)		
Hour flexibility		0.008			0.001			0.002	
		(0.015)			(0.009)			(0.022)	
Share of women			-0.003			-0.001			0.004
			(0.023)			(0.006)			(0.028)

*Notes: Each column shows the estimates of a regression $\hat{\beta}_o = \gamma_0 + \delta W_o + \eta_o$, where $\hat{\beta}_o$ represents the estimated occupation-specific child penalty, and W_o is a vector of occupation attributes, which are (1) the ratio of part time workers, (2) the ratio of people who state that their job provides hour flexibility, and (3) the ratio of women. These attributes are calculated using the sample of all workers without kids. In Panel A, the outcome is the employment penalty estimate in percentages coming from the regression $Employment^{o,g}_{iat} = \beta^{o,g} D_{it} + \mu^{o,g}_a + \lambda^{o,g}_t + \epsilon^{o,g}_{it}$, where $Employment^{o,g}_{iat}$ is a dummy equaling to one if individual i of gender g is employed in occupation o at time t, μ_a and λ_t are age and year fixed effects that control non-parametrically for lifecycle trends and time trends. Percentage effects are calculated by dividing $\hat{\beta}^{o,g}$ by the predicted outcome absent child effects: $P^{o,g} = \frac{\hat{\beta}^{o,g}}{E[\tilde{Y}^{o,g}_{iat}]}$ In Panels B and C, the outcome variable is the income and hour penalty estimate $\hat{\gamma}^{o,g}$ coming from the regression $ln(Y^{o,g}_{iat}) = \gamma^{o,g} D_{it} + \mu^{o,g}_a + \lambda^{o,g}_t + \epsilon^{o,g}_{it}$. Note that, unlike the estimates in the earlier tables where regressions include only 22 major occupations, regressions here include all occupations: the child penalty in outcome y in an occupation o for gender g is estimated if there are 10 eventual parents (before children) of that gender in that occupation with nonmissing data in outcome y. Robust standard errors are shown in parenthesis. * p<0.10, ** p<0.05, *** p<0.01*

C Empirical Bayes Correction

This section replicates the main results of the paper using Empirical Bayes.

Let β_j be the child penalty in occupation j for gender g, where I suppress g for notational purposes. Let $\hat{\beta}_j$ be an estimate of β_j. For example, Figure 2 in the main text shows the OLS estimates of Child employment penalties across the 22 major occupations separately for both men and women. Assume that the identification strategy is correct, hence $\hat{\beta}_j$'s are unbiased estimators of unknown β_j's:

$$\hat{\beta}_j|\beta_j \sim N(\beta_j, s_j^2)$$

Let F denote the distribution of occupation-specific child penalties. Suppose F is a normal distribution and independent of s_j's. This gives the following hierarchical model:

$$\begin{aligned}
\hat{\beta}_j|\beta_j, s_j &\sim N(\beta_j, s_j^2) \\
\beta_j|s_j &\sim N(\mu_\beta, \sigma_\beta^2)
\end{aligned}$$

In this normal/normal model, the posterior mean and variance for β_j given $\hat{\beta}_j$ is given by

$$\begin{aligned}
\beta_j^* \equiv E[\beta_j|\hat{\beta}_j] &= \left(\frac{\sigma_\beta^2}{\sigma_\beta^2 + s_j^2}\right)\hat{\beta}_j + \left(\frac{s_j^2}{\sigma_\beta^2 + s_j^2}\right)\mu_\beta \\
s_j^{2*} \equiv E[s_j^2|\hat{s}_j^2] &= \frac{s_j^2 \sigma_\beta^2}{s_j^2 + \sigma_\beta^2}
\end{aligned}$$

I use the following estimators for the hyperparameters $\mu_\beta, \sigma_\beta^2$.

$$\begin{aligned}
\hat{\mu}_\beta &= \frac{1}{J}\sum_{j=1}^{J}\hat{\beta}_j \\
\hat{\sigma}_\theta^2 &= \frac{1}{J}\sum_{j=1}^{J}\left[(\hat{\beta}_j - \hat{\mu}_\beta)^2 - s_j^2\right]
\end{aligned}$$

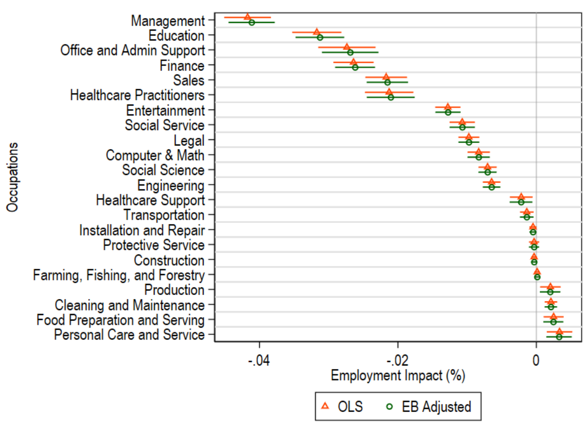
Replacing the unknown parameters by their estimates, I obtain the Empirical Bayes posterior mean and variance:

$$\begin{aligned}
\hat{\beta}_j^* &= \left(\frac{\hat{\sigma}_\beta^2}{\hat{\sigma}_\beta^2 + s_j^2}\right)\hat{\beta}_j + \left(\frac{s_j^2}{\hat{\sigma}_\beta^2 + s_j^2}\right)\hat{\mu}_\beta \\
\hat{s}_j^{2*} &= \frac{\hat{s}_j^2 \hat{\sigma}_\beta^2}{\hat{s}_j^2 + \hat{\sigma}_\beta^2}
\end{aligned}$$

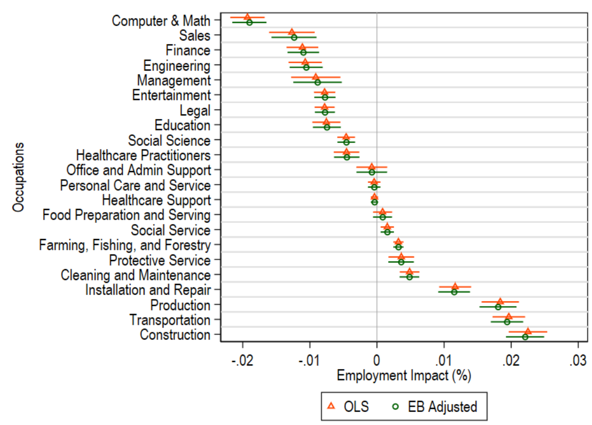
Using the posterior distribution of occupation child penalties, I replicate Figures 2 and 4 of the main text. Figure C.1 plots the 95% confidence intervals of the child employment penalties for the 22 major occupation groups. Notice that OLS and EB estimates are similar. This is because child penalties are precisely estimated compared to the observed variation in point estimates across occupations. Therefore, EB updating assigns most of the weight to the data and less of the weight to the prior. This is different for the income and hour penalties, which are plotted in Figure C.2. As the hour and income penalty estimates are less precise and the observed variation across occupations is less prevalent, EB and OLS estimates differ. For example, EB assigns practically all the weight to the prior for Men's income penalties.

Table C.1 replicates the correlational analysis in Table B.3 using EB estimates of child penalties. My results remain robust: availability of part-time work is associated with smaller employment penalties but larger income and hours penalties for women.

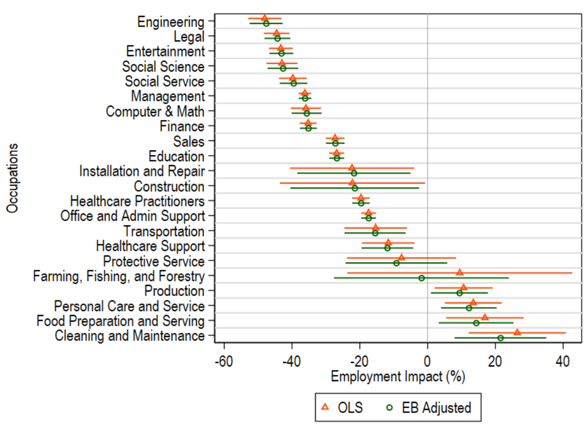
Figure C.1: Occupational Heterogeneity in Child Employment Penalty: OLS vs EB estimates



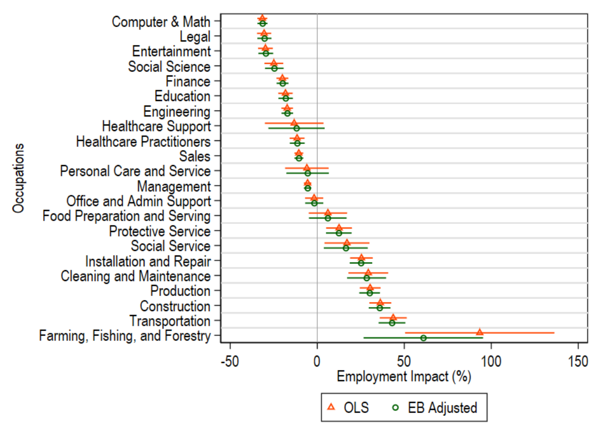
(a) Employment penalties for women (in pp)



(b) Employment penalties for men (in pp)



(c) Employment penalties for women (in %)

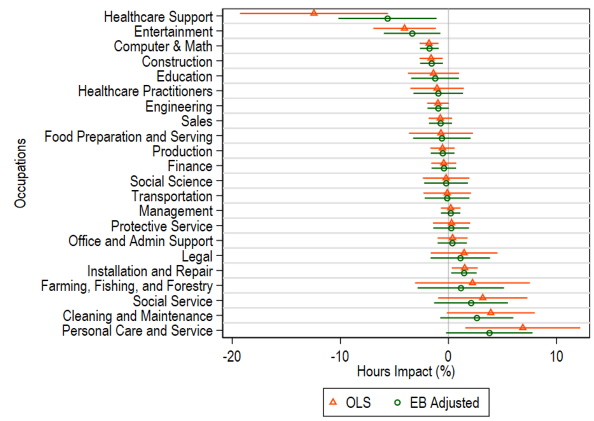


(d) Employment penalties for men (in %)

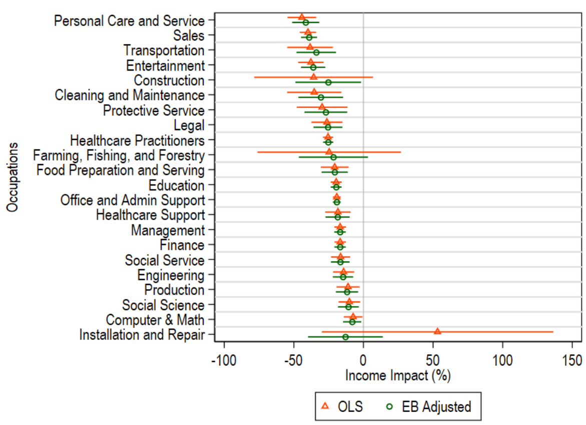
Note: This figure plots the OLS estimates alongside the estimated mean and the 95% confidence interval of the occupation-gender specific child penalties based on the Bayesian posterior, where the distibution for the occupation penalties (for each gender) is assumed to be normal with known mean and variance. Posterior is obtained using empirical bayes, separately for each gender.

Figure C.2: Occupational Heterogeneity in Child Income and Hour Penalties: OLS vs EB estimates

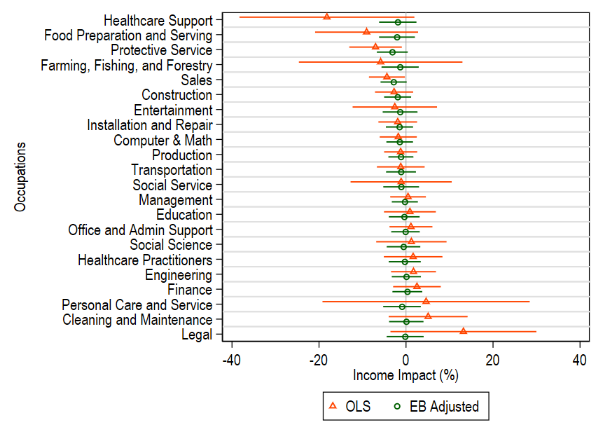
(a) Hours penalties for women (in %)



(b) Hours penalties for men (in %)



(c) Income penalties for women (in %)



(d) Income penalties for men (in %)

Note: This figure plots the OLS estimates alongside the estimated mean and the 95% confidence interval of the occupation-gender specific child penalties based on the Bayesian posterior, where the distibution for the occupation penalties (for each gender) is assumed to be normal with known mean and variance. Posterior is obtained using empirical bayes, separately for each gender.

Table C.1: Correlates with Occupation-level Child Penalties Using 22 Major Occupations

	Women			Men			Inequality: Men - Women		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Employment									
Share of part time	0.133***			0.030			-0.098***		
	(0.028)			(0.040)			(0.028)		
Hour flexibility		-0.130***			-0.167***			-0.028	
		(0.029)			(0.038)			(0.034)	
Share of women			-0.005			-0.144***			-0.120***
			(0.034)			(0.042)			(0.032)
Panel B: Income									
Share of part time	-0.051**			-0.002			0.030		
	(0.023)			(0.002)			(0.026)		
Hour flexibility		0.015			0.002			-0.004	
		(0.021)			(0.002)			(0.024)	
Share of women			-0.011			0.002			0.007
			(0.018)			(0.002)			(0.021)
Panel C: Hours									
Share of part time	-0.034***			0.001			0.033***		
	(0.008)			(0.005)			(0.011)		
Hour flexibility		-0.008			0.000			0.011	
		(0.015)			(0.004)			(0.017)	
Share of women			-0.020***			-0.003			0.016
			(0.008)			(0.006)			(0.014)

*Notes: Each column shows the estimates of a regression $\hat{\beta}_o = \gamma_0 + \delta W_o + \eta_o$, where $\hat{\beta}_o$ represents the estimated occupation-specific child penalty, and W_o is a vector of occupation attributes, which are (1) the ratio of part time workers, (2) the ratio of people who state that their job provides hour flexibility, and (3) the ratio of women. These attributes are calculated using the sample of all workers without kids. The outcome variables are the Empirical Bayes estimates of child penalties. Robust standard errors are shown in parenthesis. * p<0.10, ** p<0.05, *** p<0.01*