Child Penalty and Occupational Sorting

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Most recent draft here.

Abstract

I investigate the extent to which the child penalty varies by occupation, the role of parenthood-induced occupational sorting in driving gender inequality in earnings, and the correlates of occupation-specific child penalties. I document that both women and men lose jobs in some occupations and gain jobs in others after parenthood. Occupational change post-parenthood explains one-third of the income penalties for women and almost all for men. The availability of part-time work, not the flexibility to determine when to work, is associated with a lesser inequality in child employment penalties.

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1 Introduction

Recent literature on gender inequality underscores the significance of child penalties: the disparate impact of parenthood on the labor market outcomes of women compared to men. In developed countries, these penalties explain a substantial portion of the gender inequality observed in the labor market (Kleven et al., 2019a,b, 2021a; Cortés and Pan, 2023). Another large literature has emphasized the role of occupations, and particularly the extent of temporal flexibility in occupations, in determining gender inequality in earnings (Bertrand et al., 2010; Goldin, 2014; Goldin and Katz, 2016). This paper bridges these two strands. I investigate the extent to which the child penalty varies by occupation, the role of occupational sorting in driving gender inequality in earnings, and the correlates of occupation-specific child penalties. I document that both women and men lose jobs in some occupations and gain jobs in others after parenthood. Occupational change post-parenthood explains one-third of the child income penalties for women and almost all for men. The availability of part-time work, not the flexibility to determine when to work, is associated with a lesser inequality in child employment penalties.

One challenge to investigating occupational sorting after childbirth is that the vast majority of work on the child penalty uses panel data, which typically do not have sufficient sample size to recover precise estimates of child penalties by sub-groups like occupation. I take advantage of the rotating panels in the Current Population Survey - which offer about a 100 times larger sample size than prior panel data sets that have been 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; Cortés and Pan, 2023; Kleven, 2023; Bang, 2022) - to estimate the effects of having children on the probability of being employed in different occupations. One of the contributions of this paper is showing that datasets with rotating panels, which are both more prevalent and often substantially larger than panel datasets (Donovan et al., 2023), can be readily used to estimate child penalties with greater precision and without additional assumptions.

I document two new findings on the incidence of child employment penalties. First, the almost zero effect of fatherhood on men, which has been well established in the literature, hides that fatherhood causes men to *change* occupations. While men's employment probability decreases by 1.36 percentage points (pp) or by 27% in Computer & Mathematics, it increases by 1.91 pp (29%) in Construction. The positive and negative fatherhood effects offset each other, leading to the almost zero estimate in the literature. Second, women experience employment declines in most occupations, but also gain jobs in others. For example, women become 4.1 pp (36%) less likely to work in Management, whereas their probability of working in Personal Care and Services goes up by 0.5 pp (22%). Overall, parenthood causes both genders to change occupations.

I further demonstrate that this occupational change is a significant and consistent component of the child income penalty. For example, between 1990 and 1994, motherhood reduced women's income by approximately 29% conditional on working, with 6% attributable to occupational changes. By 2015–2019, the income penalty decreased to 21%, but the occupational change continued causing a 6% income loss. In other words, there has been no change in the past 30 years that has affected the compositional component of the child income penalty for women. Similarly, between 2015 and 2019, fatherhood reduced men's income by around 12%, with 8% attributable to occupational changes. Perhaps surprisingly, both men and women experience similar income losses from changing occupations after becoming parents. Therefore, child-induced occupational change does not contribute to the gender income gap in the United States.

Lastly, I analyze which occupational attributes can explain the occupational heterogeneity in child penalties. I show that the availability of part-time work enables women to remain employed after giving birth, does not impact men, and consequently leads to less disparate impacts of parenthood on the employment rate of women compared to men. In contrast, occupations in which workers have flexibility to determine when to work incur higher employment penalties for both men and women. Therefore, this type of flexibility does not lead to more equal outcomes in child employment penalties. This finding builds on an extensive literature, pioneered by Goldin (2014), showing that the organization of the workplace is a pivotal factor in driving the gender earnings gap (Goldin and Katz, 2016; Bütikofer et al., 2018; Ciasullo and Uccioli, 2023; Goldin, 2024). Women, especially mothers, are more likely to need temporal flexibility, the ability to control when and where they work, to balance caregiving responsibilities. Since high-paying jobs are less flexible on average, this leads to a large and persistent wage gap between genders. I contribute to this literature by showing that the differences in occupational choice between men and women are partly determined by the availability of part-time work. While the flexibility to determine *when* and *where* to work explains the intensive margin differences between men and women, the flexibility to determine *how much* to work explains the extensive margin differences.

This paper also contributes methodologically to research investigating the impact of parenthood on parents' labor market outcomes (Angrist and Evans, 1998; Angelov et al., 2016; Kleven et al., 2019a,b; Cortés and Pan, 2023). Most related to the present paper, Kleven (2023) develops a new approach to estimating child penalties using cross-sectional data. His method employs matching techniques to predict who will eventually have a child among those without children and uses them as a control group. This pseudo-panel method needs stronger assumptions for identification due to the matching step. However, it enables studying child penalties across demographics and space, as large cross-sectional data are widely available.[1] In contrast, the actual control group is observed in rotating panels. Hence, my method does not need any more identifying assumptions other than the standard *random timing of first child* assumption. My method enables heterogeneity analyses not only across demographics and space similar to Kleven (2023) but also across job characteristics such as occupations and industries, which is not doable using his approach. Our methods can thus be seen as strategic substitutes: when rotating panel data are available, researchers can use my approach to explore child penalties across demographics and job characteristics.[2] Conversely, when only cross-sectional data are available or when existing panel or rotating panel datasets are too

[1]Using this method, Kleven (2023) studies heterogeneity in child penalties across the US states, and Kleven et al. (2024) study heterogeneity across the globe.

[2]Rotating panel labor force surveys are also available across various countries, albeit not as frequently as cross-sectional datasets (Donovan et al., 2023).

small, researchers can use Kleven (2023)'s methodology to study child penalties.

2 Data

The primary dataset used in this paper is the basic monthly files of the Current Population Survey (CPS) downloaded from IPUMS between the years 1977–2019. The main outcomes of interest are employment and income. The event time is determined using information on the age of the oldest child living in the household. People who had their first child in the second round of interviews are assigned the event time t=-1 for the observations during the first round, which occurred at least eight months prior. Following the literature, the sample is restricted to parents who had their first child between the ages of 25 and 45. In addition, the data are restricted to people who appear in both rounds to keep the treatment and control groups (those with and without children) comparable. The final dataset consists of 474,034 unique parents and 3,078,598 person-month observations.

The event study specification using CPS is validated against the same specification using the National Longitudinal Survey of Youth (NLSY) and the Panel Study of Income Dynamics (PSID). The working samples from these datasets have 3,649 and 3,443 unique parents, respectively. The comparison is performed only for weekly employment because the income-related questions in monthly CPS do not match those in NLSY and PSID.[3]

The effects of having children on the employment probability across occupations are analyzed using the 22 main occupation groups following the SOC guidelines. Military occupations are excluded from the investigation.

The occupation-level flexibility to determine when to work is calculated using the Work Schedules Supplement of the CPS as the ratio of people who state that they can vary when they begin and end the work day. These surveys were conducted over 12 years between 1976 and 2004 and include about 1.6 million observations in total. The availability of part-time work is calculated using monthly CPS as the ratio of people who work part-time in a given occupation. These attributes are calculated using individuals without kids. The results reported in the paper are robust to calculating these measures using all workers or eventual parents the year before they have a child. These are reported in the Online Appendix.

[3]Specifically, basic monthly CPS collects information on weekly income, whereas NLSY and PSID collect information on annual income.

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. The following specification is run separately for men and women:

$$Y_{iat}^{g} = \sum_{j \neq -1} \beta_j^g \Delta D_{i,t-j} + \mu_a^g + \lambda_t^g + \epsilon_{it}^g \tag{1}$$

where Y_{iat}^{g} 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. The identification assumption is that 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, 2021b,a; Cortés and Pan, 2023).

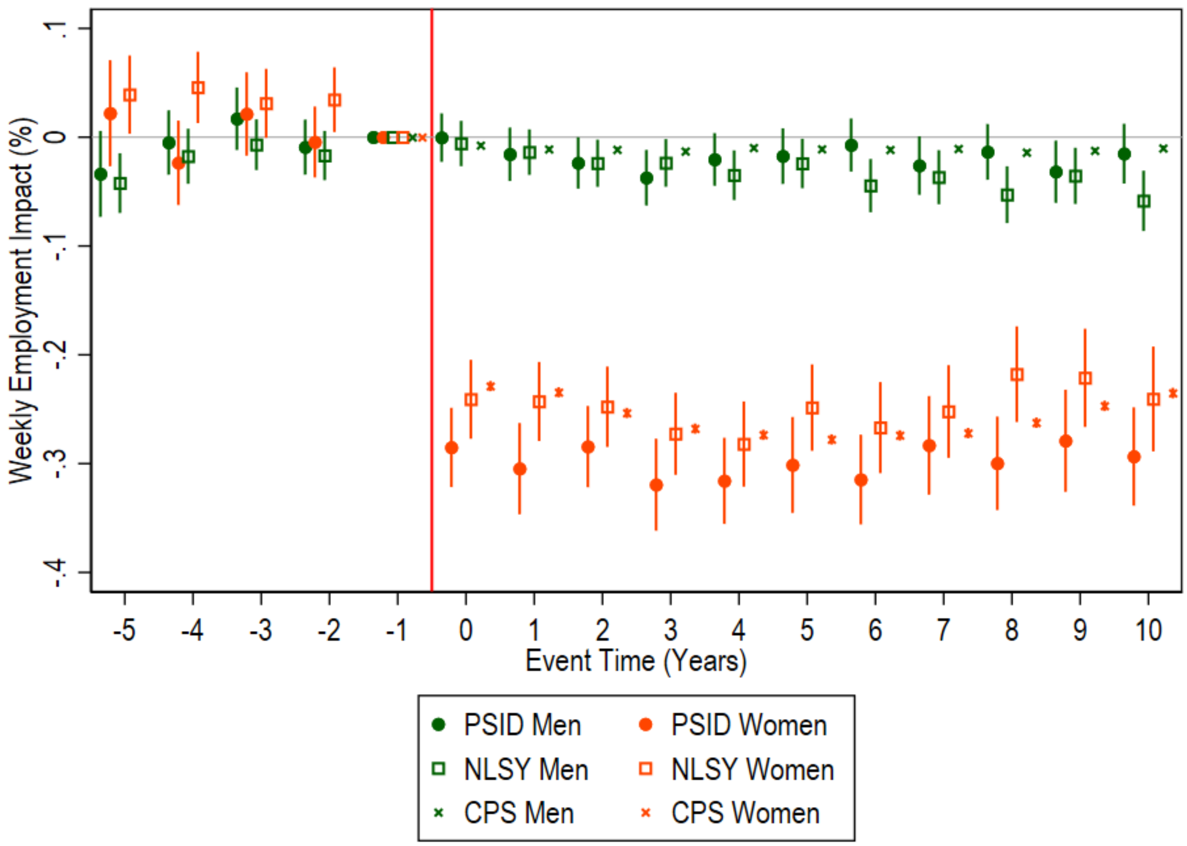
The methodological innovation introduced in this paper is predicated on the already established absence of pre-trends in the data, which simplifies the data requirements to only the year before the first child (t=-1). Rotating panels like the CPS, where individuals are interviewed in two rounds with 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 $t = -1$. This setup enables the implementation of the event study specification as outlined by Kleven et al. (2019a).[4]

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 (Kleven et al., 2019b; Cortés and Pan, 2023; Kleven, 2023; Bang, 2022). I estimate equation 1 using CPS, NLSY, and PSID separately. Robust standard errors are used following the literature standards. 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, we find that women lose more jobs than men by 29% using PSID and 24% using NLSY. Using CPS reveals an estimate between the two, a child penalty of 25%. 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

[4] Using the "panel" nature of CPS is not novel in the Economics literature, going as far back to 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 observed for up to only 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 child penalty, we only need to observe $t = -1$ for *some*, not all people in the data.

CPS estimates are practically invisible in the figure.

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) is estimated as 24% using NLSY, 29% using PSID, and 25% using CPS.

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.

3.2 Differences in means design

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

$$Employment^g_{o,iat} = \beta^g_o D_{it} + \mu^g_{o,a} + \lambda^g_{o,t} + \epsilon^g_{o,it} \tag{2}$$

where $Employment^g_{o,iat}$ is a dummy equaling to one if individual i of gender g is employed in occupation o at time t. For each gender $g = m, w$, I run different regressions using the same sample, where I change only the outcome (if the person i is working as a manager, $Employment^g_{o,iat}$ equals one only for the occupation $o = Manager$, and zero otherwise). To obtain percentage estimates, I divide the level estimates β^g_o with the predicted outcome absent child effects:

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

where $\widetilde{Y}_{o,iat}$ is the predicted employment rate when omitting the contribution of the child effect.[5] The coefficient estimates $\hat{\beta}^g_o$ and $\hat{P}^g_o$ should be interpreted as the effects of having children on the probability of being employed in occupation o for gender g. These effects can incorporate both the heterogeneous treatment effects of having children conditional on working in different occupations and sorting. For example, suppose that the probability of working as an engineer goes down by 50% for women. This is a net effect of three separate forces: (1) (pre-period) engineers may decide to leave employment after having children (which the literature refers to as the child penalty), (2) engineers may decide to transition into different occupations, and (3) people from other occupations may transition into engineering. What this design estimates is the net effect of these separate forces. In theory, these forces can be estimated separately with a large panel dataset. One limitation of CPS is that the inability to observe people over two years makes it impossible to condition on occupation in the pre-period and estimate any effect after event time $t = 0$. Therefore, I can only estimate the net effect of these forces.

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

I use the occupation-gender specific child penalty estimates $\hat{\beta}^g_o$ 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 the availability of part-time and flexibility of hours, to analyze what attribute can explain this heterogeneity. To obtain inference that is robust to multiple hypothesis testing, I employ two additional checks in the Online Appendix. First, I employ the Bonferroni correction to adjust the standard errors in a conservative way. Second, I employ the Empirical Bayes shrinkage, which takes into account that occupation-specific child penalties are noisely 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.

[5] An alternative to this linear model would be to use a conditional logit model. Given that there is no integration constraint in the design as the sum of occupations equals the employment probability, which is free to change, assuming a linear probability model should be innocuous.

4 Results

4.1 Child penalties across occupations

Figure 2 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. Figure 2a illustrates the effect of children on employment probabilities within each occupation, revealing considerable heterogeneity in child penalties for both genders. There are three main takeaways from this figure.

First, women experience statistically significant employment declines in 14 out of 22 occupations, no significant change in employment probability in 5 occupations, and significant increases in 3 occupations. For example, women become 4.1 percentage points (pp) less likely to work in Management, whereas their probability of working in Personal Care and Services increases by 0.5 percentage points.

Second, the almost zero employment penalty on men for having children, which has been well documented in the literature, masks a significant heterogeneity across occupations. Men experience statistically significant decreases in employment probability in 10 occupations, no significant change in 5 occupations, and significant increases in 7 out of 22 major occupations. For instance, fathers are 1.4 pp less likely to work in Computer and Mathematics, whereas they are 1.9 pp more likely to work in Construction and Transportation compared to similar men without children.

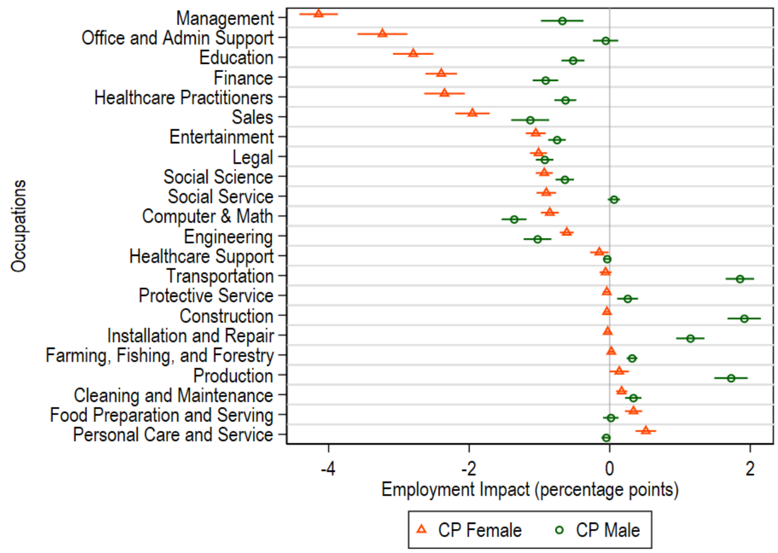
Third, the heterogeneity across occupations in child penalties for women is greater than the largest within-occupation difference between genders, highlighting the magnitude of the occupational heterogeneity in child penalties. The most notable within-occupation disparity in percentage points occurs in Management roles, where the likelihood of women holding a management position declines by 4.1 pp, compared to a mere 0.7 pp decline for men, resulting in a 3.4 pp difference in the within-occupation penalties between men and women. 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 0.5 pp, leading to a 4.6 pp difference in treatment effects.[6]

One shortcoming of studying the employment effects in levels is that the baseline employment rates can skew the results. To address this, Figure 2b presents the employment penalties in percentages. Results remain robust. There is a significant heterogeneity in child penalties across major occupations for both sexes. In percentage terms, women lose most jobs in Engineering, Legal, and Social Science Occupations while seeing job gains in Personal Care, Food Preparation, and Cleaning occupations.

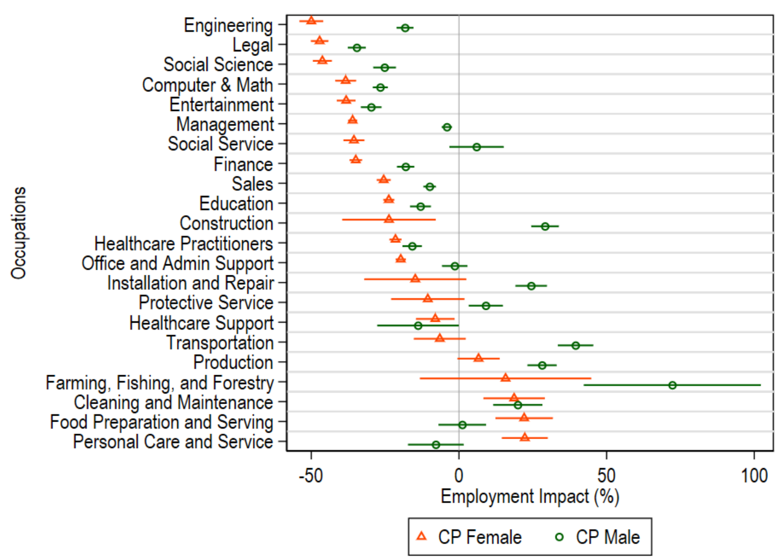
As noted in the identification section, inference from these many regressions is potentially subject to problems related to testing multiple hypotheses. To address this issue, Appendix Figure A.2 plots the 95% confidence intervals after applying the Bonferroni correction. Since the parameters are precisely estimated, this conservative correction does not alter the inference. For example,

[6]The heterogeneity across occupations in child penalties for men is almost equal to the largest within-occupation difference between genders. While men's employment probability decreases by 1.4 pp in Computer & Mathematics, it increases by 1.9 pp in Construction, creating a 3.3 pp difference across occupations.

Figure 2: Occupational Heterogeneity in Child Penalty



(a) Employment Penalties (in pp)



(b) Employment Penalties (in %)

Note: Results on employment come from the regression $Emp^g_{o,iat} = \beta^g_o D_{it} + \mu^g_{o,a} + \lambda^g_{o,t} + \epsilon^g_{o,it}$, where $Emp^g_{o,iat}$ 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 $g = m, w$, 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 β^g_o with predicted outcome absent child effects: $P^g_o = \frac{\hat{\beta}^g_o}{E[\widetilde{Y}^g_{o,iat}]}$, 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.

men's null effect in the aggregate still hides a statistically significant negative and positive penalties for different occupations.

Notably, both men and women show substantial employment gains in Farming-related occupations. However, these results are not precise because only a few men and women work in this occupation. This is a common problem in heterogeneity analyses. As the number of observations gets smaller in each subgroup, extreme observations become more likely due to increased variance. To address these concerns, in the Online Appendix, I adjust these estimates using Empirical Bayes. Figure B.3 displays the 95% confidence intervals of the posterior distribution of the occupation-specific child penalties. The results remain robust: I document economically meaningful differences in child penalties across occupations for both men and women. The reason why Empirical Bayes shrinkage does not move the OLS estimates by much (except for Farming-related occupations) is explained in more detail in Section B of the Online Appendix. The main intuition is that 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.

I further study the heterogeneity of child penalties in income and hours (conditional on working). These are reported in the Online Appendix. Figure A.1 shows that women lose income and hours in nearly all occupations, whereas men lose income and hours only in a few occupations, if any.

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 percentages matters in understanding which occupational attributes can explain the magnitude of penalties. For example, women become 50% less likely to work in Engineering and 20% more likely to work in Food Preparation and Serving related occupations. Among eventual parents the year before having a child, only 2% of Engineering jobs were part-time compared to 29% for Food Preparation and Serving, indicating that the availability of part-time jobs can explain the heterogeneity in child penalties. I explore this mechanism in Section 4.2. The employment penalty in levels impacts occupations' role in the gender gap in earnings. For example, Management is the third highest paid occupation on average throughout the sample period. As women lose more Management jobs than men after becoming parents, women end up losing more income, increasing the raw gender gap in earnings. I explore this mechanism in Section 4.3.

4.2 What explains the heterogeneity across occupations?

This section examines which attributes of occupations can explain the heterogeneity of child penalties. This is an underpowered analysis as I only have 22 data points from 22 occupations. For example, any linearly independent 22 attributes would fully explain the variation in child penalties, yet we would learn nothing from such an exercise. To make progress, in the main text, I show the bivariate relationship between child penalties and only two measures of flexibility: flexibility to alter the start and end times of a work day and flexibility to reduce work hours. The former is measured as the ratio of workers in a given occupation who have the ability to vary when they begin and end the work day, and the latter is measured as the ratio of part-time workers. For notational ease, I refer to these variables as *hour flexibility* and *part-time availability*, respectively.

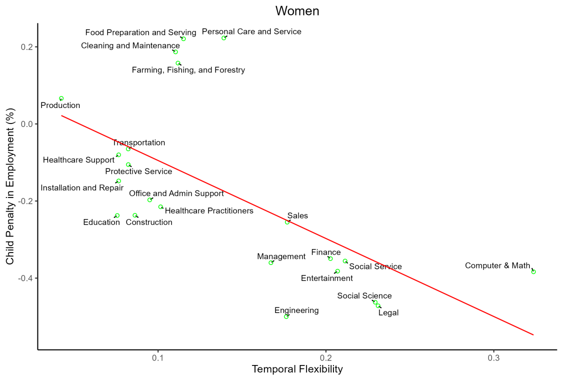
Figure 3 shows the six scatterplots of child penalties (on women, men, and the difference between men and women) and hour flexibility and part-time availability. Figure 3a shows that women, perhaps surprisingly, lose more jobs after motherhood in occupations with more hour flexibility. In contrast, Figure 3b shows that women lose most jobs in occupations with less part-time availability and gain jobs in occupations with more part-time availability.

Figures 3c and 3d replicate this analysis on the employment penalties on men. We see that men, like women, lose more jobs in occupations that allow for more hour flexibility, whereas there is no relationship between male employment penalty and availability of part-time work. Figures 3e and 3f replicate this analysis for the difference in penalties between men and women. We see that hour flexibility is not correlated with the inequality-inducing part of the child penalty: the slope of the linear line between child penalties and hour flexibility is similar for both men and women, leading to a null relationship between the differences in penalties between the two sexes and the hour flexibility of such occupations. In contrast, I document that part-time availability is negatively correlated with the inequality-inducing part of child penalties. This is expected as women incur lesser 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.

Online Appendix Figure B.6 replicates this analysis using Empirical Bayes estimates of child penalties instead of OLS estimates. The results remain robust. Both men and women experience greater job loss after becoming parents in occupations that offer more temporal flexibility. Consequently, the inequality-inducing aspect of the child penalty (i.e., the relative impact on women compared to men) remains unchanged. Conversely, women lose fewer jobs in occupations with greater part-time availability, while men are largely unaffected. As a result, there is a smaller gender difference in job loss in occupations with more part-time availability.

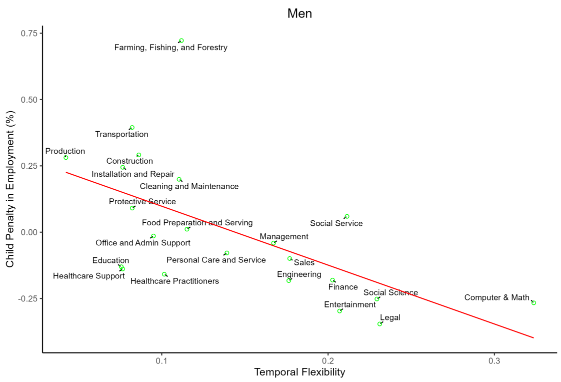
Appendix Table A.1 extends this analysis in three ways. First, it reports the coefficient estimates from bivariate regressions (at the occupation level) of child penalties on occupation attributes. Put differently, it reports the estimated slopes of the linear lines plotted in each panel of Figure 3. Regressions enable the reader to quantify the magnitude and statistical significance of these slopes. Second, it expands the list of dependent variables by including the child penalties in income and hours. Third, it expands the list of explanatory variables by including the ratio of women working in each occupation. The notable finding from this analysis is that occupations with more part-time availability experience more significant reductions in income and hours for women post-birth, although the results on income are less precise. The higher availability of part-time positions likely enables women to remain employed while transitioning to fewer hours. Although this shift helps maintain employment, it naturally reduces income due to fewer hours worked. The significance of part-time work in this context underscores its dual role: it acts as a facilitator for continued employment among women post-childbirth and as a factor in the decrease in overall income and

Figure 3: Correlates with Occupation-level Child Penalties

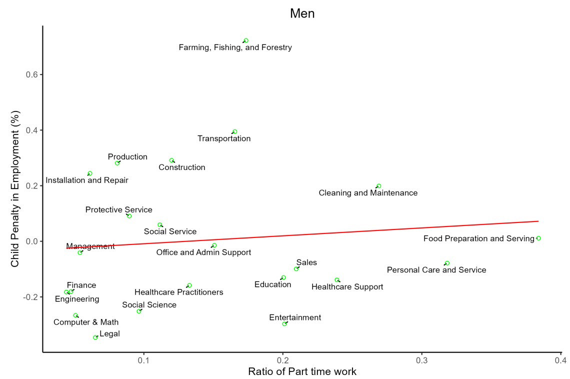


(a) Female penalty: hour flexibility

(b) Female penalty: part-time availability

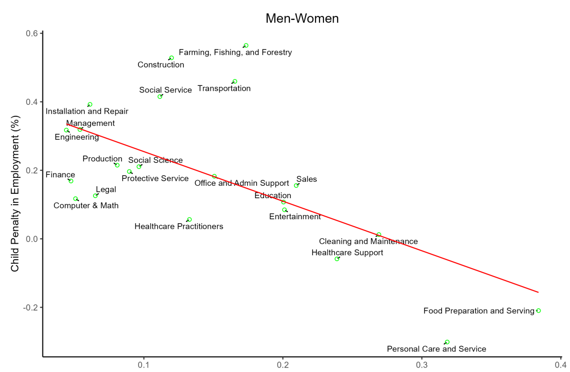


(c) Male penalty: hour flexibility



(d) Male penalty: part-time availability

(e) Child penalty: hour flexibility



(f) Child penalty: part-time availability

Notes: The occupational correlates are (1) the ratio of people who state that their job provides hour flexibility and (2) the ratio of part-time workers. These attributes are calculated using the sample of all workers without kids in the CPS.

hours worked conditional on being employed.[7]

This evidence builds on Goldin's pioneering work showing that the concentration of inflexible

[7] Online Appendix Tables A.2 and A.3 show that these results remain robust to defining the explanatory variables using all workers and eventual parents the year before they had a child.

schedules in higher-paying positions, in which workers cannot alter when and where to work, contributes to persistent gender wage disparities (Goldin, 2014; Goldin and Katz, 2016). This body of work focuses on workers and thus abstracts away from the extensive margin differences between genders. My results indicate that women sort into occupations where part-time work is available, which indicates a revealed preference toward lowering hours after parenthood. Therefore, I argue that flexibility to *reduce* hours is also an important dimension of occupational attributes that affects gender inequality in the labor market.

Taking the results at face value, the policy implication of this finding is not clear. 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 giving birth. However, in terms of gender inequality in the labor market, availability of part-time work can be increasing or decreasing the gender income gap. To see this, 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, if part-time wasn't available, whether women would have remained outside of the labor force or worked full time. If the former, then part-time availability would reduce income gaps, if the latter, it would increase it. Since I don't have quasi-experimental variation in part-time availability, I cannot answer this question, and therefore I leave it open for future work.

4.3 Effect of occupational sorting on the income penalty

This section studies the effects of child-induced occupational change on the child income penalty. Specifically, I estimate the child income penalty on men and women, with and without controlling for 22 major occupation groups, as outlined in equation 4. β_1^g captures the average effect of having the first child on income, conditional on employment. β_2^g captures the *within occupation* child income penalty, accounting for the occupational change that both men and women undergo after having children. The differential $\beta_1^g - \beta_2^g$ highlights the influence of these occupational changes on the overall income penalty, indicating how much of the penalty is due to changes in occupations versus income losses within the same occupation.

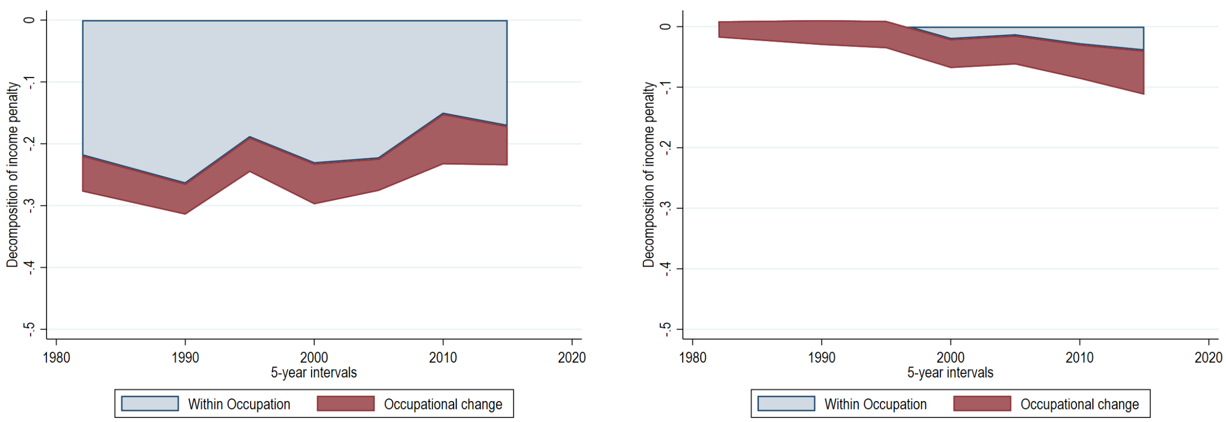
$$\begin{aligned} ln(Income_{iat}^g) &= \beta_1^g D_{it} + \mu_a^g + \lambda_t^g + \epsilon_{it}^g \\ ln(Income_{iat}^g) &= \beta_2^g D_{it} + \mu_a^g + \lambda_t^g + \theta^g Occ22_i + \epsilon_{it}^g \end{aligned} \tag{4}$$

To assess the evolution of these dynamics over time, I calculate these penalties separately for men and women from 1982 to 1990 and for each 5-year interval from 1990 to 2019. This longitudinal approach allows me to observe how child income penalties and the role of occupational adjustments have shifted over the past two and a half decades.

Figure 4 illustrates the evolving dynamics of the income penalty associated with parenthood, segmented by gender, and factoring in the impact of occupational change. The analysis reveals several key trends. Historically, from 1980 to 2000, women faced an income penalty of approximately

29% following the birth of a child. This penalty has since decreased to around 21%. Notably, occupational change post-childbirth has contributed around 6% throughout this period. This has two implications. First, approximately one-third of the child income penalty for women since 2010 can be explained by the occupational change induced by motherhood. Second, all the reductions in the child income penalty have come from the within-occupation component. There has been no change in the impact of child-induced occupational change on women's income.

Figure 4: Decomposition of Income Penalty



(a) Women

(b) Men

Note: Within occupation estimates come from the regression: $ln(Income^g_{iat}) = \beta^g_1 D_{it} + \mu^g_a + \lambda^g_t + f^g_{occ} + \epsilon^g_{it}$, where $ln(Income^g_{iat})$ is the log-income 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, f_{occ} is an occupation fixed effect. To obtain the occupational change estimate, I first estimate the income penalty without controlling for occupations from the regression: $ln(Income^g_{iat}) = \beta^g_2 D_{it} + \mu^g_a + \lambda^g_t + \epsilon^g_{it}$. The income penalty that comes from occupational change is calculated by the difference between the two regression estimates: $\hat{\beta}^g_2 - \hat{\beta}^g_1$.

In contrast, during the 1980s and 1990s, men experienced only negligible income penalties after becoming fathers. However, this trend shifted in the 2000s, with the penalty rising to about 8% between 2000 and 2015 and further increasing to 12% in the period from 2016 to 2019. A substantial portion of this increase can be attributed to men changing occupations after becoming fathers. The within-occupation component of the income penalty began to manifest in the early 2000s and has only slightly intensified since then.

Notice that in the 1980s and 1990s, the occupational change component in income penalties was larger for women than for men, which contributed to gender inequality in earnings. More interestingly, while this component remained constant for women, it increased for men starting from the 2000s. Why the occupational change component of the income penalty for women remained constant for almost 40 years while it increased for men is another question this paper raises.

5 Conclusion

This paper explores how the child penalty differs across occupations, the impact of children-induced occupational sorting on gender inequality, and the correlates of occupation-specific gender penalties. The average zero effect of fatherhood conceals significant variations, with some occupations experiencing substantial negative penalties and others showing large positive ones. Similarly, the overall negative effect of motherhood hides the fact that in certain occupations, penalties are minimal or even positive. This occupational change accounts for one third of income penalties for women and nearly all for men. Notably, the flexibility to reduce hours rather than to determine when to work is linked to reduced inequality in child employment penalties.

To obtain these results, I demonstrate how datasets with rotating panels, like the Current Population Survey, can readily be used to estimate child penalties with precision and without additional assumptions. This approach allows researchers to explore child penalties across individual and job characteristics while relying on the same identification assumptions as the existing literature based on panel data. Future work can use this method to investigate the sources of child penalties and help uncover the mechanisms behind gender inequalities in labor markets.

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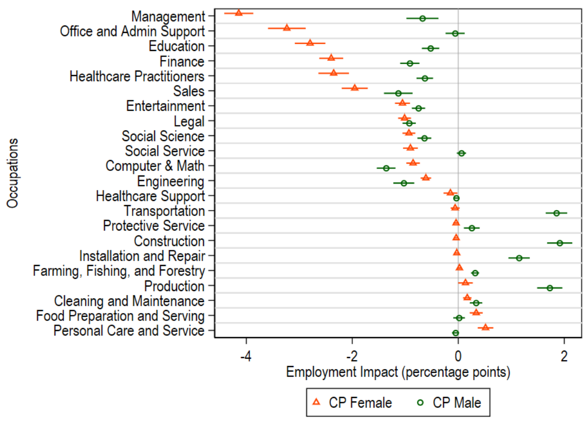
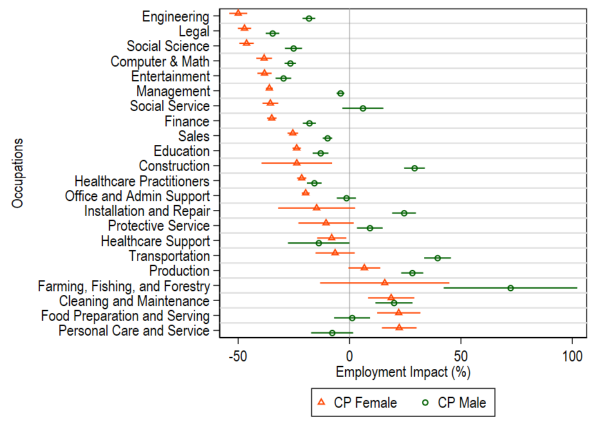
A Online Appendix

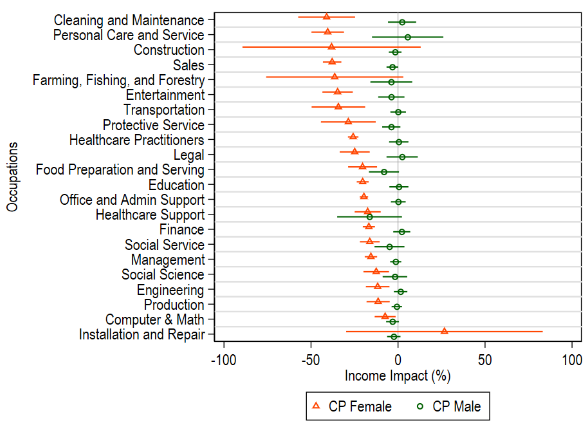
Figure A.1: Occupational Heterogeneity in Child Penalty



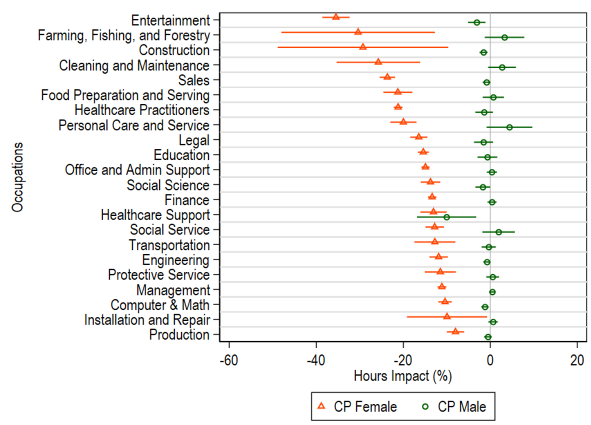
(a) Employment (in pp)



(b) Employment (in %)



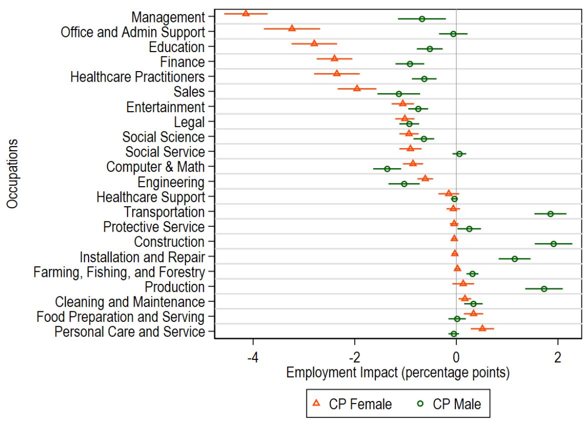
(c) Income (in %)



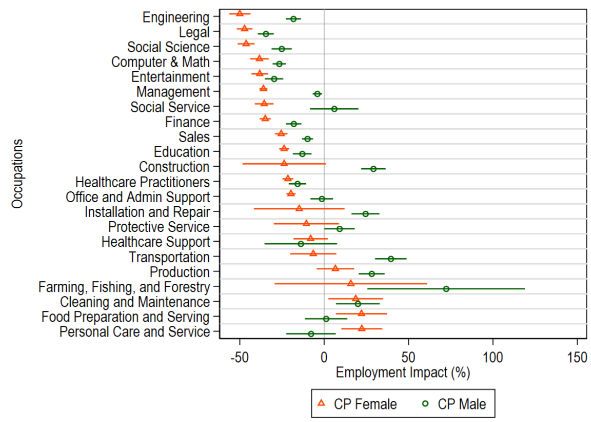
(d) Hours (in %)

Note: Results on employment come from the regression $Emp^g_{o,iat} = \beta^g_o D_{it} + \mu^g_{o,a} + \lambda^g_{o,t} + \epsilon^g_{o,it}$, where $Emp^g_{o,iat}$ 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 $g = m, w$, 22 separate regressions are run for each occupation-specific outcome. To obtain percentage estimates, I divide the level estimates β^g_o with predicted outcome absent child effects: $P^g_o = \frac{\hat{\beta}^g_o}{E[\widetilde{Y}^g_{o,iat}]}$, 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.

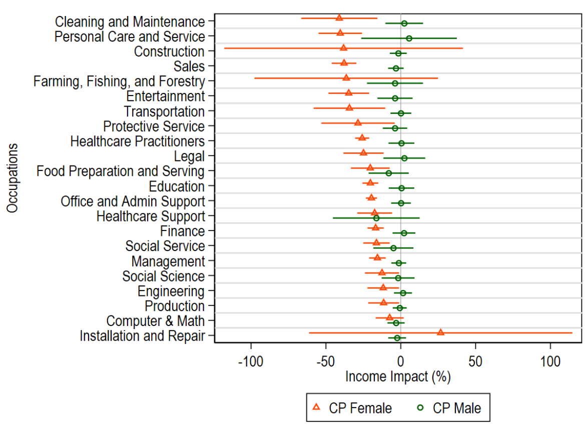
Figure A.2: Occupational Heterogeneity in Child Penalty (Bonferroni corrected confidence intervals)



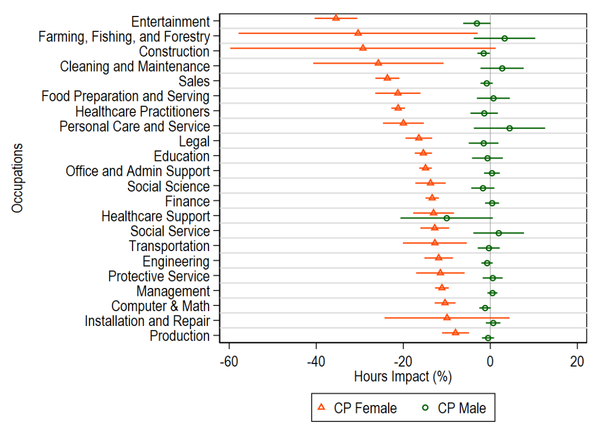
(a) Employment (in pp)



(b) Employment (in %)



(c) Income (in %)



(d) Hours (in %)

Note: Results on employment come from the regression $Emp^g_{o,iat} = \beta^g_o D_{it} + \mu^g_{o,a} + \lambda^g_{o,t} + \epsilon^g_{o,it}$, where $Emp^g_{o,iat}$ 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 $g = m, w$, 22 separate regressions are run for each occupation-specific outcome. To obtain percentage estimates, I divide the level estimates β^g_o with predicted outcome absent child effects: $P^g_o = \frac{\hat{\beta}^g_o}{E[\widetilde{Y}^g_{o,iat}]}$, 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 after adjusting the critical values using Bonferroni correction.

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 (in %)									
Hour flexibility	-2.024***			-2.220***			-0.196		
	(0.430)			(0.531)			(0.427)		
Share of part time		1.732***			0.285			-1.447***	
		(0.294)			(0.407)			(0.322)	
Share of women			0.005			-0.586***			-0.591***
			(0.171)			(0.179)			(0.142)
Panel B: Income									
Hour flexibility	0.148			0.070			-0.078		
	(0.460)			(0.123)			(0.502)		
Share of part time		-0.808**			-0.106			0.701	
		(0.373)			(0.133)			(0.458)	
Share of women			-0.092			-0.024			0.068
			(0.183)			(0.056)			(0.204)
Panel C: Hours									
Hour flexibility	0.018			0.001			-0.016		
	(0.234)			(0.074)			(0.250)		
Share of part time		-0.399***			0.023			0.423***	
		(0.121)			(0.071)			(0.138)	
Share of women			0.005			-0.028			-0.032
			(0.065)			(0.036)			(0.080)

*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 people who state that their job provides hour flexibility, (2) the ratio of women, and (3) the ratio of part time workers. 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 $Emp^g_{o,iat} = \beta^g_o D_{it} + \mu^g_{o,a} + \lambda^g_{o,t} + \epsilon^g_{o,it}$, where $Emp^g_{o,iat}$ 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 $\hat{\beta}^g_o$ by the predicted outcome absent child effects: $P^g_o = \frac{\hat{\beta}^g_o}{E[\tilde{Y}^g_{o,iat}]}$ In Panels B and C, the outcome variable is the income and hour penalty estimate coming from the regression $ln(Y^{o,g}_{iat}) = \gamma^{o,g} D_{it} + \mu^{o,g}_a + \lambda^{o,g}_t + \epsilon^{o,g}_{it}$. Robust standard errors are shown in parenthesis. * p<0.10, ** p<0.05, *** p<0.01*

Table A.2: 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 (in %)									
Hour flexibility	-1.884***			-2.009***			-0.126		
	(0.391)			(0.491)			(0.417)		
Share of part time		1.516***			0.022			-1.493***	
		(0.318)			(0.383)			(0.280)	
Share of women			0.124			-0.465***			-0.590***
			(0.159)			(0.159)			(0.130)
Panel B: Income									
Hour flexibility	0.100			0.067			-0.032		
	(0.422)			(0.108)			(0.457)		
Share of part time		-0.748**			-0.092			0.657	
		(0.347)			(0.130)			(0.429)	
Share of women			-0.117			-0.024			0.093
			(0.170)			(0.054)			(0.192)
Panel C: Hours									
Hour flexibility	0.002			-0.002			-0.003		
	(0.215)			(0.063)			(0.224)		
Share of part time		-0.382***			0.012			0.395***	
		(0.119)			(0.073)			(0.137)	
Share of women			-0.013			-0.021			-0.008
			(0.059)			(0.035)			(0.075)

*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 people who state that their job provides hour flexibility, (2) the ratio of women, and (3) the ratio of part time workers. 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 $Emp^g_{o,iat} = \beta^g_o D_{it} + \mu^g_{o,a} + \lambda^g_{o,t} + \epsilon^g_{o,it}$, where $Emp^g_{o,iat}$ 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 $\hat{\beta}^g_o$ by the predicted outcome absent child effects: $P^g_o = \frac{\hat{\beta}^g_o}{E[\tilde{Y}^g_{o,iat}]}$ In Panels B and C, the outcome variable is the income and hour penalty estimate coming from the regression $ln(Y^{o,g}_{iat}) = \gamma^{o,g} D_{it} + \mu^{o,g}_a + \lambda^{o,g}_t + \epsilon^{o,g}_{it}$. Robust standard errors are shown in parenthesis. * p<0.10, ** p<0.05, *** p<0.01*

Table A.3: 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 (in %)									
Hour flexibility	-0.638			-0.485			0.153		
	(0.414)			(0.577)			(0.406)		
Share of part time		1.903***			0.001			-1.902***	
		(0.374)			(0.418)			(0.296)	
Share of women			-0.003			-0.495***			-0.492***
			(0.157)			(0.152)			(0.139)
Panel B: Income									
Hour flexibility	-0.081			0.091			0.172		
	(0.268)			(0.089)			(0.312)		
Share of part time		-0.740*			-0.158			0.582	
		(0.419)			(0.198)			(0.558)	
Share of women			-0.068			-0.022			0.046
			(0.151)			(0.051)			(0.172)
Panel C: Hours									
Hour flexibility	-0.085			0.047			0.132		
	(0.148)			(0.055)			(0.184)		
Share of part time		-0.359***			-0.016			0.343	
		(0.147)			(0.125)			(0.226)	
Share of women			-0.001			-0.023			-0.022
			(0.056)			(0.033)			(0.070)

*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 people who state that their job provides hour flexibility, (2) the ratio of women, and (3) the ratio of part time workers. 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 $Emp^g_{o,iat} = \beta^g_o D_{it} + \mu^g_{o,a} + \lambda^g_{o,t} + \epsilon^g_{o,it}$, where $Emp^g_{o,iat}$ 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 $\hat{\beta}^g_o$ by the predicted outcome absent child effects: $P^g_o = \frac{\hat{\beta}^g_o}{E[\tilde{Y}^g_{o,iat}]}$ In Panels B and C, the outcome variable is the income and hour penalty estimate coming from the regression $ln(Y^{o,g}_{iat}) = \gamma^{o,g} D_{it} + \mu^{o,g}_a + \lambda^{o,g}_t + \epsilon^{o,g}_{it}$. Robust standard errors are shown in parenthesis. * p<0.10, ** p<0.05, *** p<0.01*

B 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:

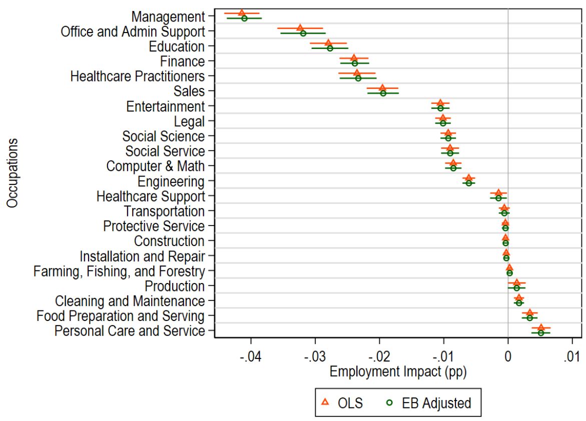
$$\begin{aligned}
\hat{\beta}_j^* &= \left(\frac{\hat{\sigma}_\beta^2}{\hat{\sigma}_\beta^2 + s_j^2}\right)\hat{\beta}_j + \left(\frac{s_j^2}{\hat{\sigma}_\beta^2 + s_j^2}\right)\hat{\mu}_\beta \\
\hat{s}_j^{2*} &= \frac{\hat{s}_j^2\hat{\sigma}_\beta^2}{\hat{s}_j^2 + \hat{\sigma}_\beta^2}
\end{aligned}$$

Using the posterior distribution of occupation child penalties, I replicate Figures 2 and 3 of the main text. Figure B.3 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 B.4. 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.

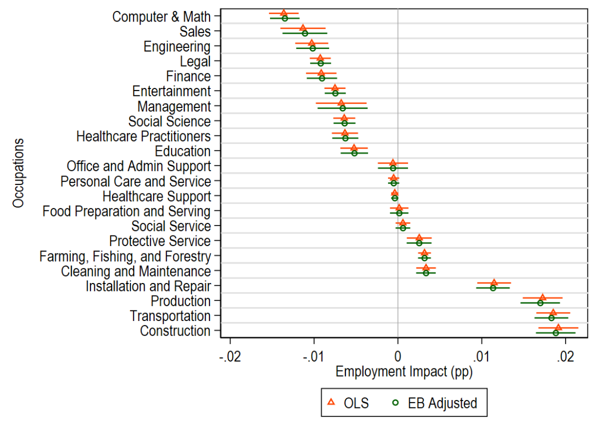
How much the EB adjustment moves the OLS estimates can also be seen in Figure B.5, which displays the scatter plot of OLS and EB estimates of child penalties. As employment effects are precisely estimated in OLS, EB and OLS estimates mostly align on the 45 degree line. However, as the hour and income penalty estimates are less precise, EB estimates are visibly different from OLS estimates.

Figure B.6 replicates Figure 3 using EB adjusted child penalty estimates. Results remain robust. Both men and women lose more jobs after becoming parents in occupations with more temporal flexibility. Consequently, the inequality-inducing part of the child penalty (i.e., the relative impact on women compared to men) remains the same. In contrast, women lose fewer jobs in occupations with more part-time availability, and men are largely unaffected. Therefore, there is a smaller difference across genders in occupations with more part-time availability.

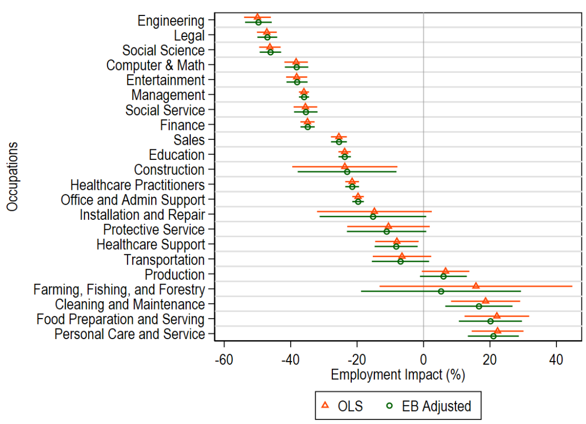
Figure B.3: Occupational Heterogeneity in Child Employment Penalty: OLS vs EB estimates



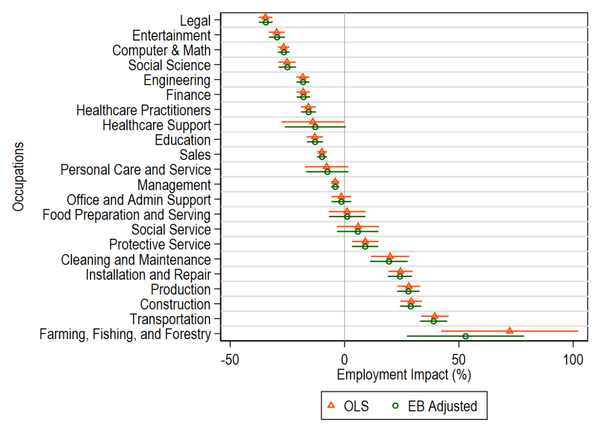
(a) Employment penalties for women (in pp)



(b) Employment penalties for men (in pp)



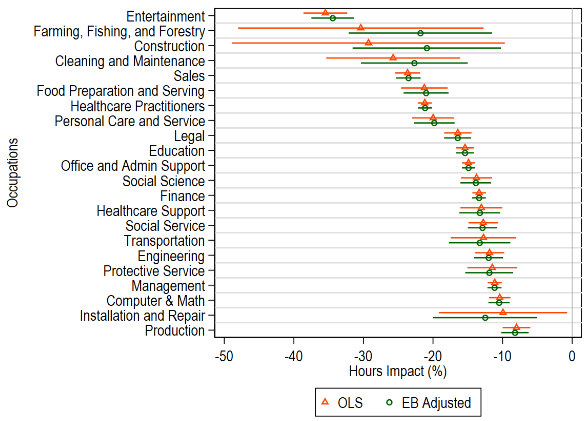
(c) Employment penalties for women (in %)



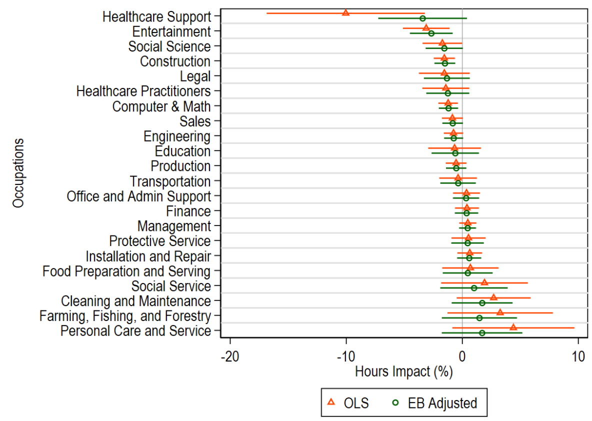
(d) Employment penalties for men (in %)

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.

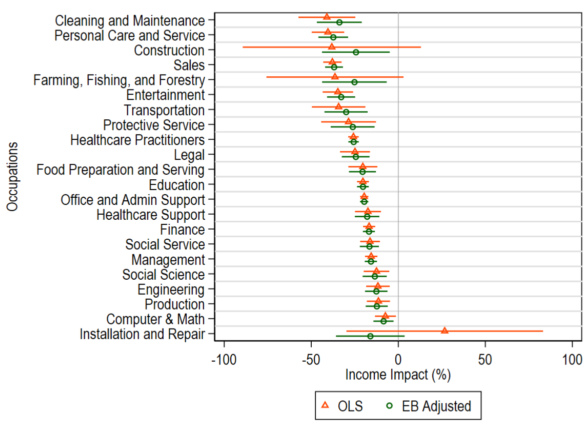
Figure B.4: Occupational Heterogeneity in Child Income and Hour Penalties: OLS vs EB estimates



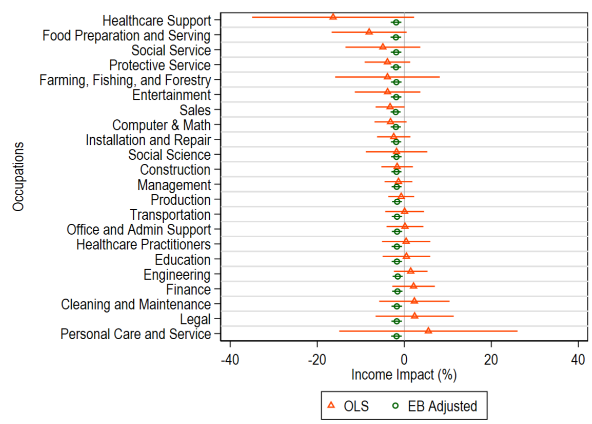
(a) Hours penalties for women (in %)



(b) Hours penalties for men (in %)



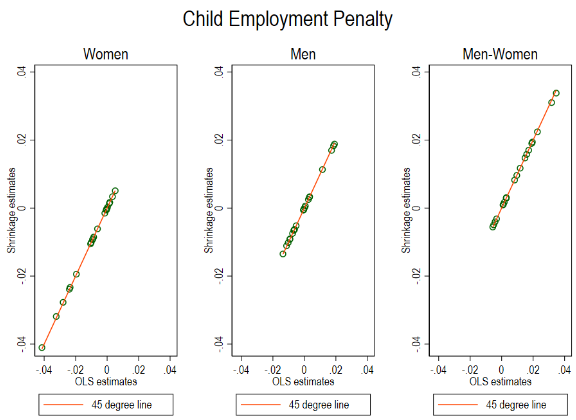
(c) Income penalties for women (in %)



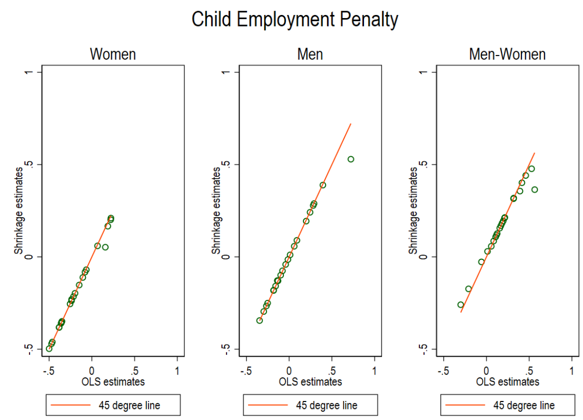
(d) Income penalties for men (in %)

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.

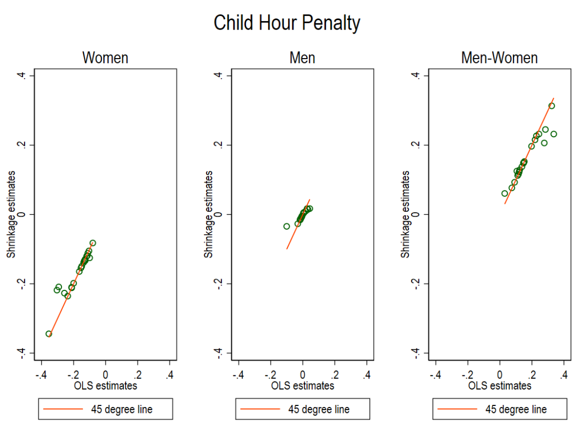
Figure B.5: Comparison of OLS and EB estimates



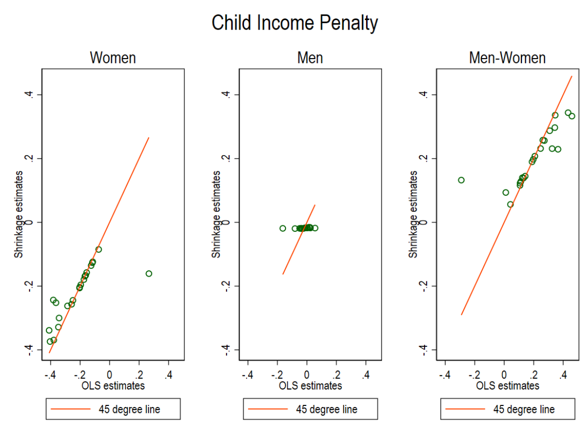
(a) Employment Penalties (in pp)



(b) Employment Penalties (in %)



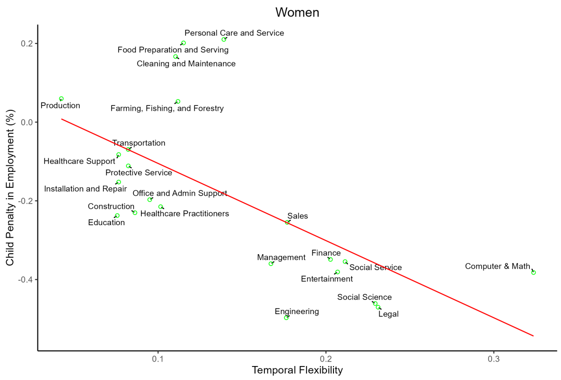
(c) Hour Penalties (in %)



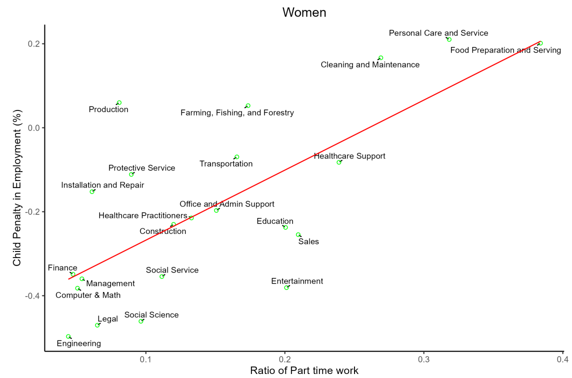
(d) Income Penalties (in %)

Note: OLS estimates on employment come from the regression $Emp^g_{o,iat} = \beta^g_o D_{it} + \mu^g_{o,a} + \lambda^g_{o,t} + \epsilon^g_{o,it}$, where $Emp^g_{o,iat}$ 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 $g = m, w$, 22 separate regressions are run for each occupation-specific outcome. To obtain percentage estimates, I divide the level estimates β^g_o with predicted outcome absent child effects: $P^g_o = \frac{\hat{\beta}^g_o}{E[\widetilde{Y}^g_{o,iat}]}$, 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. EB estimates update the OLS estimates using a normal prior with known mean and variance. The exact equations can be found in the Online Appendix.

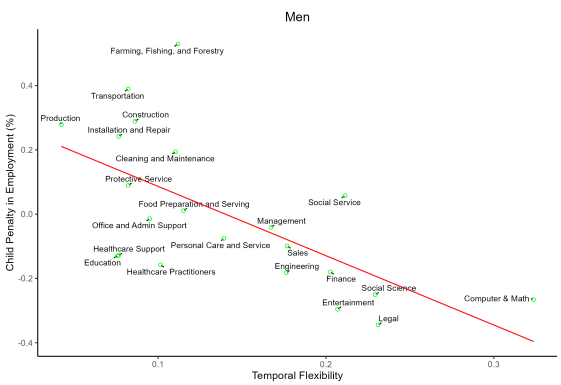
Figure B.6: Correlates with Occupation-level Child Penalties (with Empirical Bayes correction)



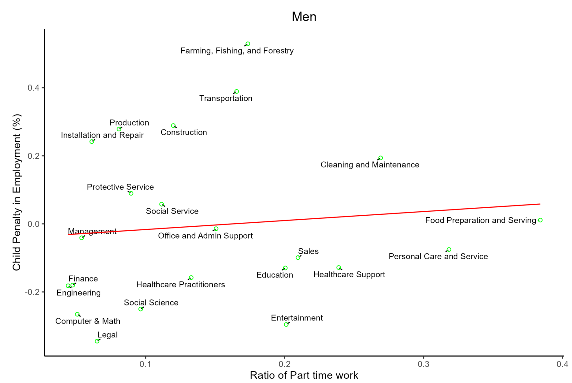
(a) Female penalty: hour flexibility



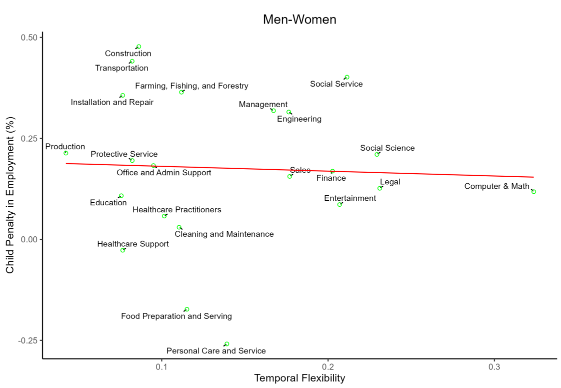
(b) Female penalty: availability of part-time



(c) Male penalty: hour flexibility



(d) Male penalty: availability of part-time



(e) Child penalty: hour flexibility

(f) Child penalty: availability of part-time

Notes: The occupational correlates are (1) the ratio of people who state that their job provides hour flexibility and (2) the ratio of part-time workers. These attributes are calculated using the sample of all workers without kids in the CPS. Empirical Bayes corrected child penalty estimates are used.

Table B.4: Correlates with Occupation-level Child Penalties

	Women			Men			Inequality: Men - Women		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Employment (in %)									
Hour flexibility	-1.961***			-2.156***			-0.120		
	(0.412)			(0.503)			(0.381)		
Share of part time		1.668***			0.264			-1.334***	
		(0.287)			(0.387)			(0.275)	
Share of women			0.025			-0.538***			-0.496***
			(0.159)			(0.155)			(0.123)
Panel B: Income									
Hour flexibility	0.246			-0.003			-0.221		
	(0.262)			(0.003)			(0.286)		
Share of part time		-0.519***			-0.004			0.371	
		(0.223)			(0.003)			(0.254)	
Share of women			-0.036			0.000			0.012
			(0.064)			(0.001)			(0.072)
Panel C: Hours									
Hour flexibility	-0.033			-0.031			0.026		
	(0.205)			(0.034)			(0.214)		
Share of part time		-0.358***			0.021			0.376***	
		(0.095)			(0.031)			(0.105)	
Share of women			-0.031			-0.008			0.014
			(0.039)			(0.014)			(0.054)

*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 people who state that their job provides hour flexibility, (2) the ratio of women, and (3) the ratio of part time workers. 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*