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Motherhood Penalties and Fatherhood Premiums: Effects of Parenthood on Earnings Growth Within and Across Firms

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ABSTRACT Despite much interest in how parenthood contributes to the gender pay gap, prior research has rarely explored firms' roles in shaping the parenthood pay penalty or premium. The handful of studies that investigated parenthood's effects within and across firms generally compared parents and their childless peers at a given time and failed to account for unobserved heterogeneity between the two groups. Such comparisons also cannot inform how having children may alter individuals' earnings trajectories within and across firms. Using 26 rounds of the National Longitudinal Survey of Youth 1979 and fixed-effects models, we examine how being a mother or father is linked to earnings growth within and across firms. We find that women's pay decreases as they become mothers and that the across-employer motherhood penalty is larger than the within-employer penalty. By contrast, fatherhood is associated with a pay premium, and the within-employer fatherhood premium is considerably greater than the across-employer one. We argue that these results are consistent with the discrimination explanation of the motherhood penalty and fatherhood premium. Because employers are likely to trust women who become mothers while working for them more than new recruits who are mothers, their negative bias against mothers would be more salient when evaluating the latter, which could result in a larger between-organizational motherhood penalty. Conversely, employers' likely greater trust in existing workers who become fathers than fathers they hire from elsewhere may amplify their positive bias favoring fathers in assessing the former, which could explain the greater within-firm fatherhood premium.

KEYWORDS Motherhood penalty • Fatherhood premium • Earnings growth • Parenthood effects between and across firms

Introduction

Research on gender and family has long shown that parenthood has divergent effects on women's and men's earnings. Whereas women typically undergo a wage decrease with the arrival of each child (Budig and England 2001; Gangl and Ziefle 2009; Gough and Noonan 2013; Yu and Kuo 2017), men's earnings tend to increase with their transition to fatherhood (Glauber 2008; Hodges and Budig 2010; Killewald

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2013). As a result of the wage penalty associated with motherhood and the wage premium tied to fatherhood, the gender pay gap widens as men and women move through the life course. Parenthood is therefore a key contributor to gender inequality (Angelov et al. 2016; England 2005).

Corresponding to the important role of parenthood in shaping earnings inequality, much research has been devoted to explaining the motherhood pay penalty (Budig and England 2001; Correll et al. 2007; Gough and Noonan 2013; Staff and Mortimer 2012) and exploring factors that mitigate or amplify this penalty (Anderson et al. 2002; England et al. 2016; Gangl and Ziefle 2009; Glauber 2012; Yu and Kuo 2017). Because employing organizations are potential driving forces for wage disparities (Baron and Bielby 1980; Petersen and Morgan 1995), recent studies called attention to the organizations in which mothers work (Fuller 2018; Fuller and Hirsh 2019; Petersen et al. 2010). Using linked employer-employee data from Norway and Canada, respectively, two studies showed that mothers' lower earnings largely resulted from their greater concentration in firms that pay less. Wage differences between mothers and nonmothers within firms accounted for a relatively small portion of the motherhood pay penalty in these countries (Fuller 2018; Petersen et al. 2010).

Despite existing evidence on the within- and across-firm motherhood penalties, it remains unclear how women's earnings change with motherhood and firm settings. Because the relevant research has generally relied on cross-sectional observations (Fuller 2018; Petersen et al. 2010),[1] we do not know whether the wage differences between mothers' and nonmothers' firms indicate that the firms for which women work before and after childbearing pay differently or that women in lower-paying organizations more likely become mothers. It is possible that women prioritizing family over careers choose a particular type of employer to begin with and that such women tend to become mothers soon after entering their firms. Similarly, with cross-sectional comparisons between mothers and their childless peers within firms, prior research cannot tell whether the transition into motherhood indeed hampers a woman's earnings growth within her firm or, instead, certain unobserved personal traits, such as relative commitment to work, lead those who chose to become mothers to receive lower wages than their coworkers who chose otherwise.

Precisely because of the difficulty of ruling out alternative explanations with cross-sectional comparisons, most research on the motherhood penalty has used longitudinal data and conceptualized this penalty as the pay decrease a woman experiences after childbearing rather than as the wage gap between mothers and their childless counterparts (Anderson et al. 2002; Budig and England 2001; Budig and Hodges 2010; England et al. 2016; Gangl and Ziefle 2009; Yu and Kuo 2017). To be consistent with this conceptualization, researchers interested in motherhood's effects within and across firms should ask how much post-birth shifts in women's earnings within organizations, compared with pay differences between women's organizations before and after childbearing, contribute to the overall earnings disadvantage of mothers.

This study specifically addresses this question using 36 years of data from the National Longitudinal Survey of Youth 1979 (NLSY79). In the paper, we refer to

[1] Although Petersen and colleagues' (2010) study included a subanalysis using repeated observations of the same women, their main findings—related to the magnitudes of within- and across-organizational motherhood wage penalties—were based on the comparison of different women within and across organizations.

a post-birth decrease in earnings within a firm as the *within-firm* motherhood penalty; we refer to a pay reduction between a woman's pre- and post-birth employers as the *across-firm* motherhood penalty. Unlike previous studies focusing only on mothers (Fuller 2018; Petersen et al. 2010), we also ask how fatherhood shapes men's earnings growth within and across employing organizations, and we compare their experiences with women's. Prior research has rarely examined the within- and across-firm fatherhood premiums, with just one Canadian study showing how the differing employers between fathers and childless men explain the former's higher wages (Cooke and Fuller 2018). Because that study relied on cross-sectional data, however, the question of how fatherhood alters earnings growth within and across firms remains unanswered.

Besides showing the within- and across-employer parenthood penalties or premiums, this study helps disentangle the various mechanisms proposed to explain how having children alters women's and men's earnings. For example, one explanation of why becoming a parent increases men's and decreases women's pay is that it enhances men's motivation to provide for the family but reduces women's available time and energy for their jobs (Budig and England 2001; Gough and Noonan 2013; Killewald 2013). The corresponding changes in effort and productivity lead mothers to earn less and lead fathers to earn more. If this is the case, then parenthood should hamper women's earnings growth while amplifying men's both within and across firms because men and women would modify their effort regardless of their organizational settings. Conversely, because discrimination based on the assumption that motherhood reduces women's work effort is likely weaker when an employer is already familiar with the woman's job performance (Fuller 2018), a discrimination-based explanation would lead us to expect the impact of parenthood to be weaker when women remain with the same employers than when they move across firms. Our analysis of within- and across-firm pay penalties and premiums thus has theoretical implications for explanations of gendered relationships between parenthood and earnings.

Parenthood and Pay Within and Across Firms

Researchers have studied parenthood's effects on earnings extensively. Using data from various cohorts and across industrial countries, several studies have shown that having a child leads to a wage penalty for women, even after many observable and unobservable personal traits were taken into account (Anderson et al. 2002; Budig and England 2001; Fuller 2018; Gangl and Ziefle 2009; Gough and Noonan 2013; Petersen et al. 2010; Staff and Mortimer 2012; Yu and Kuo 2017). Conversely, fatherhood is associated with a 3% to 10% pay premium (Glauber 2008; Hodges and Budig 2010; Killewald 2013; Lundberg and Rose 2000, 2002).

Despite much evidence on mothers' earnings disadvantage and fathers' earnings advantage, relatively few studies have distinguished between within- and across-firm motherhood penalties or fatherhood premiums and compared these penalties or premiums. Among a handful of studies that made this distinction are studies by Petersen and colleagues (2010) and Fuller (2018), who used linked employer-employee data from Norway and Canada, respectively. Both studies compared mothers with childless women who were otherwise similar and found the two groups less likely to

receive unequal pay when they worked in the same establishment than in different ones. The authors therefore argued that the motherhood wage penalty largely arises from mothers' concentration in lower-paying firms. Evidence from Canada also indicates that the uneven distribution of fathers and childless men across establishments explains the majority of the fatherhood wage premium for men with lower education but not for the highly educated. Among high-skilled Canadian men, fathers earn 5% to 6% more than their childless counterparts within the same firms (Cooke and Fuller 2018).

The findings from the studies in Canada and Norway suggest that for women and at least some men, parenthood's effect on earnings may be greater across than within firms. Nearly all these studies, however, compared parents and nonparents who were otherwise similar (Cooke and Fuller 2018; Fuller 2018; Fuller and Cooke 2018), making it possible that the results were confounded by unmeasured personal traits that both distinguish parents from nonparents and explain the two groups' different earnings (e.g., values placed on jobs *vis-à-vis* family). More important, cross-sectional comparisons cannot inform whether having a child actually alters an individual's earnings within and across firms. Some evidence suggests that the within-firm earnings shifts following childbirth may contribute to a relatively small part of the overall motherhood penalty. Two studies, for example, found that mothers with more employer changes since childbirth receive lower wages (Gangl and Ziefle 2009; Glass 2004). Cooke and Fuller (2018) also showed that Canadian men who moved to their jobs recently experienced smaller fatherhood premiums, implying that fathers gain more by remaining with the same employer over time. None of these studies, however, offered direct estimates on the parenthood penalty or premium attributable to post-birth changes in earnings within firms *vis-à-vis* that resulting from the pay discrepancies between individuals' organizations before and after having a child.

Rationales for Within- and Cross-Firm Parenthood Effects

Answering the question of how parenthood affects pay differently within and across firms requires an understanding of why mothers face a pay penalty while fathers receive a pay premium in the first place. Prior research has proposed three major explanations, which have distinctive implications for whether having children may affect earnings growth more within or across firms. We discuss these explanations and their corresponding hypotheses regarding the within- and across-firm parenthood effects.

Work Effort and Productivity

One main reason why parenthood may alter women's and men's earnings is that it strengthens traditional gender roles, making men feel a greater responsibility to provide for the family and compelling women to spend more time on domestic work (Sanchez and Thomson 1997; Sayer 2005). These changes are thought to lead fathers to allocate more effort to their jobs and mothers to allocate less (Becker 1985). Because fathers' increased work effort enhances productivity, fatherhood may augment men's earnings even if they spend the same amount of time at work as they did before having a child

Table 1 Explanatory frameworks and hypotheses about within- and across-firm earnings growth

Explanations	Hypotheses
A. Effort and Productivity	Motherhood will reduce, and fatherhood will increase, pay growth within and across firms to a similar extent.
B. Compensating Differentials	Motherhood will decrease, and fatherhood will increase, earnings growth across firms but not within firms.
C. Discrimination	
C1. Differential trust	The motherhood penalty will be smaller within firms than across firms, whereas the fatherhood premium will be larger within firms than across firms.
C2. Differing information availability	Both the motherhood penalty and fatherhood premium will be smaller within than across firms.

(Killewald 2013). Conversely, because lower effort reduces productivity, women experience a wage decrease with each additional child (Gough and Noonan 2013).

If parenthood does amplify men's work effort and productivity and if, as the productivity-based explanation assumes, men are able to bargain for higher wages or move to organizations that properly compensate their increased productivity, then we should find that a man's earnings increase equally whether he remains in the same firm or moves to a different firm. We should therefore find comparable fatherhood pay premiums within and across employing organizations. Likewise, a mother with reduced work effort would experience decreases in productivity and pay after having a child regardless of whether she was in the same or a different organization, as long as her reduced effort is entirely due to motherhood. Thus, the within- and between-firm motherhood penalties should also be similar in magnitude. We summarize these expectations and present them as the *effort and productivity hypothesis* in Table 1.

Because the effort and productivity perspective assumes that mothers' diminished work effort is rooted in their increased care obligations at home and fathers' enhanced effort reflects the extra financial burden an additional child brings, this perspective has another implication: as mothers' care responsibilities and fathers' financial burden both grow with the number of children, the changes in their work effort should correspond to rises in their family size. Therefore, we can further expect women's earnings to decrease and men's to increase proportionally, both within and across firms, with increases in the number of children.

Compensating Differentials and Selection Into Workplaces

An alternative explanation for the motherhood pay penalty is rooted in the compensating differentials theory, which focuses on workers' selection into jobs and workplaces. This theory maintains that worker compensations encompass both wages and nonpecuniary amenities, such as paid leaves and schedule flexibility, and that individuals may trade part of their wages for preferred amenities in choosing jobs (Glauber 2012; Heywood et al. 2007). Having a child is thought to affect workers'

preferences and, therefore, their decisions about the trade between pay and job amenities. Because motherhood intensifies women's preferences for jobs compatible with their family obligations, mothers may more likely choose family-friendly workplaces at the expense of earnings (Fuller 2018; Petersen et al. 2010). Conversely, because parenthood amplifies men's responsibility to provide financially for their family, fathers may be more willing to sacrifice nonpecuniary amenities, such as a short commute, for jobs that pay more.

A few prior studies have cast doubt on the argument that mothers' work settings are more family-friendly than nonmothers' (e.g., Glass and Camarigg 1992; Glauber 2012). However, because such studies did not account for *all* differences between workplaces, it remains possible that parenthood leads people to select workplaces with unmeasured amenities or disamenities, resulting in decreased or increased pay. Although we cannot tell whether people actually trade wages for job amenities or vice versa when changing employers, we should find a sizable fatherhood premium across firms if the compensating differentials argument is valid. Compared with childless men, fathers may more actively seek to move to high-paying workplaces and bargain harder for wages (instead of other amenities) when they move. Men therefore are likely to earn more at the firms they shift to after becoming fathers. At the same time, if mothers indeed prioritize family-friendly working conditions over pay in choosing workplaces, women can be expected to be compensated less at the firms they enter after childbirth, resulting in a negative association between motherhood and earnings across firms.

According to this framework, parenthood should not significantly affect earnings growth within firms because people who change parenthood status while with the same employer likely made decisions about trade-offs between pay and nonpecuniary job amenities before the change. Because most nonpecuniary benefits and family-responsive policies are workplace-specific (Fuller 2018; Heywood et al. 2007), workers are typically unable to trade job amenities for pay (or vice versa) upon entering parenthood without changing employers.[2] Of course, the decision to stay with a given employer upon having a child may not be random. It is possible that women in relatively family-friendly workplaces, which may have a lower potential for long-term wage growth (Glass 2004; Heywood et al. 2007), are more likely to make the choice to have a child without leaving their employer. Similarly, men may elect to have a child when they work for employers that offer better wage prospects over the long run. Nevertheless, neither selection would lead a given worker to have different earnings trajectories before and after parenthood within the same firm, as long as the employer does not discriminate based on parenthood status. Because our analysis focuses on within-person, within-firm earnings changes by parenthood status, workers' decisions to stay with an employer would not alter the *compensating differentials-based hypothesis*, which suggests that motherhood will decrease earnings and fatherhood will increase earnings across firms but not within firms. We also list this hypothesis in Table 1.

[2] In less common cases, an employee may be able to switch to a position with different amenities or a part-time schedule within the same firm upon entering parenthood, but controlling for changes in detailed occupations and work schedules should largely account for these scenarios.

Discrimination on the Basis of Parenthood Status

Another perspective commonly proposed to explain the motherhood wage penalty and fatherhood wage premium centers on employer biases, which tend to work against mothers but favor fathers (Budig and England 2001; Fuller 2018; Hodges and Budig 2010; Killewald 2013).[3] This perspective contends that employers' ability to measure workers' productivity is limited, which prompts them to use parenthood status as a proxy for workers' productivity levels. Being a mother, as a status characteristic, signals to employers that the woman must divide her devotion between family and work, making her the opposite of the "ideal worker" that employers have in mind (Ridgeway and Correll 2004; Williams 2001). Upon being convinced that mothers are suboptimal workers, employers may hold mothers to a higher standard, devaluate their job performance, and reward them less financially (Correll et al. 2007). By contrast, the status of fathers is associated with a greater financial responsibility and a higher level of motivation to earn. Employers are therefore likely to view fathers as more devoted to their jobs than childless men. In turn, employers may apply a more lenient standard to fathers and overpay fathers for a given level of productivity. Consistent with this argument, experiments have shown that fathers have an advantage in obtaining a job over equivalently qualified men who are not fathers (Correll et al. 2007). Fuller and Cooke (2018) also found that the fatherhood wage premium is greater in firms that lack formalized procedures to evaluate job performance, supporting the argument about employers' tendency to overestimate fathers' productivity.

Although discrimination or favoritism based on parenthood status could affect parents' earnings growth both within and across firms, the magnitudes of the parenthood effects may not be the same. Differences in magnitude likely arise from employers' differential trust in their existing employees *vis-à-vis* new recruits, who are virtually strangers to them. Research has shown that individuals' trust in others depends on their social distance from the people they are judging and whether they consider those people in-group members (Brewer 1999; Buchan et al. 2002; Foddy et al. 2009; Platow et al. 2012). People are likely to trust acquaintances more than strangers, especially if the acquaintances share certain identities with them or belong to the same loosely defined group (e.g., same workplaces) as they do. Employers are generally more familiar with their existing employees than with new job applicants, and they are more likely to see the former as in-group members. Because individuals tend to judge those they trust less harshly than others (Taylor and Koivumaki 1976), employers' greater trust in existing employees compared with unfamiliar new recruits should lead them to apply a more lenient standard to the former. This leniency may strengthen employers' belief that fatherhood enhances productivity when they evaluate those who became fathers within their organizations, and this combination would lead to a larger fatherhood bonus for these fathers than for fathers hired from elsewhere. Conversely, employers may discount the productivity of mothers who are

[3] Although we generally use "employers" to refer to the ones who evaluate workers' performance and determine the latter's pay, sometimes it is actually the workers' supervisors, managers, or even the people hiring the workers for contract work who are responsible for pay discrimination. For simplicity, however, we use "employers" to represent all agents who may evaluate workers and determine workers' earnings.

job seekers more than that of women who became mothers while working for them because employers are more lenient toward the latter. Being a mother may therefore be more detrimental to earnings when women move across firms than when they stay with the same employers. This argument—*differential discrimination by trust*—thus leads to the hypothesis that the within-firm motherhood penalty will be smaller than the across-firm one, whereas the within-firm fatherhood premium will be larger than the across-firm one (see Table 1).

There is an alternative reason why employer discrimination or favoritism may lead the wage effects of parenthood to differ within and across organizations. Based on the discrimination account, employers use stereotypes associated with mothers and fathers to estimate their worth partly because an accurate measure of productivity is rarely available. Although psychological research has yielded mixed results regarding whether familiarity reduces the use of stereotypes in judging people (Funder and Colvin 1988; Garcia-Marques et al. 2016; Smith et al. 2006), it is possible that having more direct information enables employers to gauge workers' performance more objectively. Because employers typically know more about their existing employees than about new recruits, they may let the former's parenthood status affect their assessment less than they do new recruits. Thus, fathers and mothers alike would encounter a greater parenthood-based bias when changing employers than when staying with the same employers. This argument leads to the *differential discrimination by information availability hypothesis*, which contends that both the motherhood penalty and the fatherhood premium will be smaller within than across firms (as shown in Table 1).[4]

Data and Methods

The data for the study come from 26 rounds of the NLSY79, conducted from 1979 to 2014. The survey has followed a nationally representative cohort of individuals who were 14–22 years old in 1979, collecting information annually through 1994 and biannually thereafter. At the last round included in our sample, fielded in 2014–2015, virtually all respondents were in their 50s. Because childbirth over age 50 is rare even among men, the 26 rounds of the NLSY79 have essentially captured the respondents' complete fertility histories. In addition to including comprehensive fertility histories, the NLSY79 data have the advantage of containing long and detailed work histories. At each round, the survey asks respondents to report jobs they have held each week since the last interview and to identify the employer for each job. Over the 36 years covered by the 26 rounds of the NLSY79, nearly all respondents have worked for multiple employers, and many of their employer spells are long. Such data make it

[4] The lack of information on prospective employees may also lead employers to be uncertain about the former's parenthood status, hence penalizing or rewarding them less for being a parent. If this is the case, both the motherhood penalty and fatherhood premium will be greater within than across firms. We do not formally propose this hypothesis, however, because research has shown that employers often pick up non–job-related information of prospective employees from subtle cues in resumes or interviews (Rivera 2012; Rivera and Tilcsik 2016). Employers may even actively seek information that is illegal to use in recruitment (Rivera 2017). In addition, employers may obtain ideas about prospective employees' parenthood status when checking the latter's references.

possible to examine how individuals' earnings growth within and across firms corresponds to changes in their parenthood status.

Although the NLSY79 does not contain employer-employee linked data, which are typically used to assess within- and across-firm wage inequality (Fuller 2018; Petersen and Morgan 1995; Petersen et al. 2010), it has the advantage of having repeated observations of a national sample. To our knowledge, no large-scale, longitudinal employer-employee linked data are available in the United States. Without a random sample of workers from each firm, the NSLY79 data cannot tell us whether firms in which parents concentrate pay differently from other firms. We can nevertheless address the extent to which pay differences between one's firms before and after childbearing contribute to the overall penalty or premium one experiences with parenthood, which is a central question in studies comparing the effects of parenthood within and across firms (Cooke and Fuller 2018; Fuller 2018). The longitudinal nature of the NLSY79 also enables us to estimate how much parenthood alters earnings growth within organizations. Because the vast majority of research measures the motherhood penalty and fatherhood premium as an individual's gain or loss in earnings with the transition to parenthood (Budig and England 2001; Gangl and Ziefle 2009; Glauber 2008; Killewald 2013; Yu and Kuo 2017), rather than as pay differences between parents and their childless peers, our estimates of how having a child changes people's earnings within and across organizations are more consistent with previous research than those based on pay gaps between different groups within and across firms.

To conduct the statistical analysis, we pool all rounds of the NLSY79 to create a person-month sample. Although the NLSY79 is widely used, researchers rarely take advantage of its weekly job records, which can better capture employer changes and variations within each employer spell than annual or biannual records. The typical approach, which uses respondents' jobs held at the interview time to generate a person-year sample (e.g., Budig and England 2001; England et al. 2016; Killewald 2013), would exclude employer spells that respondents experienced between rounds. As a result, firms with high turnover rates would likely be underrepresented. Moreover, when using the person-year approach, researchers can observe earnings changes within employer spells only if respondents reported the same employers at two or more interview times, making the observed within-employer earnings growth selective.[5] Using person-month data provides more accurate information on earnings changes over time. We convert respondents' weekly reports of jobs and employment status (i.e., having a job, unemployed, or voluntarily away from the labor force) to monthly observations. When a respondent held multiple jobs or statuses within the same month, we use the job or employment status with the longest duration to represent the month's status.

In our person-month sample, we fill in respondents' time-varying marital and parenthood status using the NLSY79's reports of the years and months during which respondents' marital status changed and their children were born, respectively. For variables that are recorded on only a yearly basis, such as respondents' geographic location, we assume that respondents' information did not change between interviews

[5] Our exploratory analysis showed that using a person-year, instead of person-month, sample would reduce the number of employers included in the analysis by more than one-half.

if they reported the same conditions for two adjacent rounds. When there was a change between interviews, we assume that the change occurred in the month in the middle of the two adjacent rounds.[6]

Because the NLSY79 provides limited job information for those on active military duty, we exclude such person-month observations from the sample. Given our focus on earnings and firms, we also limit the sample to months during which respondents reported having a job and provided valid information about their employers and pay. Because parents' obligations considerably decline when all their children reach adulthood, we further eliminate person-months when respondents' youngest child was age 20 or older. When we used different ages of the youngest child to restrict the sample (e.g., 18 or 25 years old) or imposed no such restriction at all, the main results were similar. To show how variation in parenthood status corresponds to changes in respondents' earnings between and within their employing organizations, we also eliminate cases where no changes are possible. Specifically, we exclude 6.4% of NLSY79 respondents for having reported only one employer or only employer spells that lasted one month or less throughout the observation period. Finally, we restrict the sample to those whose job information was provided within three years from the time the job was held because information such as earnings provided many years later—as a result of respondents missing certain rounds—may suffer from recall errors. After all these selections and elimination of cases missing information on key variables, our analytic sample contains 1,176,234 monthly observations from 5,933 men and 1,039,476 monthly observations from 5,749 women.

Models and Measurement

The outcome of interest for our study is the reported hourly pay of respondents' jobs (in cents). Because of the skewedness of earnings distribution, we take the natural log of the hourly earnings. Like most studies of the motherhood pay penalty and fatherhood pay premium, we use fixed-effects models to predict log hourly earnings (Budig and England 2001; Gangl and Ziefle 2009; Glauber 2008; Killewald 2013; Yu and Kuo 2017). Because the selection into parenthood is unlikely to be random, regression models that compare earnings between parents and their otherwise similar peers face the problem that other unobserved factors that contribute to the two groups' childbearing decisions, such as devotion to employment careers, may also account for differences in pay. Fixed-effects models, by contrast, enable us to take into account all unmeasured characteristics that do not vary across observations for a given subject, be it a person, an occupation, or an employing organization (Allison 2009). Such models can better address the problem of unobserved heterogeneity.

[6] When respondents missed a round, we use the information provided by the next available interview to code the 12 months before the interview for the years through 1994 (when the survey conducted annual interviews) and to code the 24 months before the interview for years after 1994 (when biennial interviews were conducted).

We begin with a model of the following form:

$$\ln(pay_{it}) = \gamma_0 + \gamma_1 parent_{it} + \Sigma a_j \mathbf{X}_{jit} + \mu_i + year_k + \varepsilon_{it}, \quad (1)$$

where the outcome is the log hourly pay of person i ($i = 1, 2, 3, \ldots, n$) at month t; γ_0 is the intercept; γ_1 is the coefficient for being a parent; $\mathbf{X}_{jit}$ denotes j time-varying variables that may also affect earnings (e.g., education, work experience); μ_i and $year_k$ are fixed effects for i individuals and k calendar years in the data set, respectively; and ε_{it} is the error term. Equivalent to a dummy variable for each person, μ_i captures all time-constant characteristics for the individuals, even when the characteristics are unobservable. With $year_k$, the equivalent of a dummy variable for each calendar year, the model further takes into account any year-to-year shifts that influence workers' pay (e.g., economic downturns).[7]

Because occupational characteristics are important to earnings (Kilbourne et al. 1994), especially mothers' earnings (Yu and Kuo 2017), we also fit the following model:

$$\ln(pay_{it}) = \gamma_0 + \ddot{\gamma}_1 parent_{it} + \Sigma a_j \mathbf{X}_{jit} + \mu_i + year_k + occp_o + \varepsilon_{it}, \quad (2)$$

where $occp_o$ represents fixed effects for the o occupations. Adding occupation fixed effects enables us to account for all stable occupational differences that may shape earnings, such as occupational training and qualifications, occupational status, and the gender dominance of the occupation. The difference between γ_1, the estimated parenthood effect from Eq. (1), and $\ddot{\gamma}_1$, the estimated effect from Eq. (2), thus indicates the extent to which occupational shifts before and since entering parenthood explain the associations between parenthood and earnings.

We fit models with broad and fine occupation fixed effects, respectively. The NLSY79 used the three-digit 1970 census occupational codes through 2000 and switched to the 2000 census codes thereafter. We follow Meyer and Osborne's (2005) guidelines to create standard three-digit occupational codes across all the years, resulting in 372 fine occupations in the analytic sample. We then group various fine occupations under the same general occupational category to create 22 broad occupational categories (e.g., executive and managerial occupations, technician and related support occupations, mechanics and repairers, and machine operators and assemblers). We use broad occupational categories as an alternative measure to assess how occupational differences account for parenthood's association with pay.

Next, we investigate how beyond occupations, employing organizations may further contribute to the parenthood premium or penalty. The specific model can be expressed as follows:

$$\ln(pay_{it}) = \gamma_0 + \widetilde{\gamma}_1 parent_{it} + \Sigma a_j \mathbf{X}_{jit} + \mu_i + year_k + occp_o + employer_f + \varepsilon_{it}, \quad (3)$$

where $employer_f$ denotes fixed effects for f employers. The NLSY79 provides a unique identification number for each employer a respondent has had throughout his or her career. The availability of multiple monthly observations with each

[7] We use *reghdfe*, a user-written program for Stata to estimate models with multiple fixed effects (Correia 2016). The program uses an algorithm to achieve the equivalent of including dummy variables for every fixed-effects category, given that the latter is computationally difficult.

employer enables us to estimate models with employer fixed effects. Because all employer identification numbers in the survey are respondent-specific, our measure of employers is fully nested in the observations of the same individuals: each individual's months in the sample can be divided into various employer spells. Thus, the employer fixed effects included here are, in fact, person-employer fixed effects, which capture all stable between-firm differences for given individuals. Because the person-employer fixed effects would capture any time-constant between-individual differences, the results would be identical regardless of whether we include individual fixed effects in Eq. (3).

With $employer_f$ taking into account differences between employer spells, Eq. (3) ultimately estimates how alterations in individual attributes correlate with changes in earnings *within* employer spells. Thus, $\widetilde{\gamma_1}$ represents how becoming a parent affects an individual's earnings while the individual works for the same firm—that is, the within-employer parenthood premium or penalty. Following Fuller (2018), we estimate the between-employer parenthood penalty or premium by comparing the models before and after adding employer fixed effects (i.e., Eqs. (2) and (3)). Specifically, because Eq. (3) further accounts for average pay differences between employers, the change in the parenthood coefficient from Eqs. (2) to (3) $(\ddot{\gamma}_1 - \widetilde{\gamma_1})$ can be interpreted as the extent to which the parenthood penalty or premium is attributable to pay differences across employing organizations. Fuller (2018:1450–1451) referred to this change as the "between-firm parenthood penalty" (or premium). We similarly consider $(\ddot{\gamma}_1 - \widetilde{\gamma_1})$ as the between-employer contribution to the parenthood effect on earnings. We test the statistical significance of this contribution following Clogg and colleagues' (1995) procedures to compare coefficients for the same variable from two nested models.

To provide more details about employer spells in the analytic sample, Table 2 shows the number of employers and durations with reported employers by gender. Both men and women experienced more than seven employers during the months observed. Altogether, these experiences amount to 43,505 person-employer spells for men and 38,744 person-employer spells for women (i.e., the average number of employers multiplied by the total number of respondents). Because the average number of employers is somewhat large, virtually all the respondents who have ever had a child changed employers after they became parents.[8] Hence, our estimate of how much pay differences between respondents' pre- and post-birth firms contribute to the overall parenthood penalty or premium is not just based on the experiences of a small, selective group of parents. On average, men and women both spent more than 3 years with any given employer, and the mean of the longest duration with an employer is nearly 10 years for men and nearly 9 years for women. The considerable length of time most respondents spent with a firm enables us to observe ample earnings variation within their employer spells.

We measure the main predictor of interest, parenthood, as a binary variable based on the presence of any biological children. We focus on biological children because research has shown that the fatherhood wage premium applies to only fathers with biological children (Killewald 2013) and because the NLSY79 lacks information on nonbiological children's birth months or the exact month when they entered respondents' lives. For an additional analysis examining how men's and women's earnings

[8] Less than 4% of the respondents never had a new employer since they became parents.

Table 2 Statistics for employers and employer durations in the analytic sample

	Men		Women	
Total Number of Employers Experienced	7.6	(4.7)	7.3	(4.2)
Average Employer Duration (month)[a]	42.4	(38.7)	38.7	(40.8)
Maximum Duration With an Employer (month)	119.2	(87.5)	105.0	(80.5)
N	5,933		5,749	

Notes: Values in parentheses are standard deviations. All numbers are weighted by the NLSY79 longitudinal weights. The unit of analysis for the descriptive statistics is individuals.

[a] This refers to the average employer-spell duration across individuals. We calculate this average by first estimating the average employer-spell duration for each individual and then taking the average of the individual-specific values.

vary by their number of children, we also construct time-varying dummy variables indicating whether respondents have (1) no child, (2) one child, (3) two children, or (4) three or more children during a given month.

All the fixed-effects models also control for a series of time-varying individual characteristics that may affect earnings. First, we introduce a set of human capital indicators, including the level of education completed (less than high school, high school, some college, and university or more), work experience, and job tenure. We create a monthly measure of work experience based on the NLSY79's weekly records of the total amount of time respondents have held jobs. Job tenure is measured as the number of months respondents have worked for the same employer.[9] We also include the square terms of work experience and job tenure to capture potentially nonlinear relationships. To account for the possibility that mothers' frequent job turnover and career interruptions obstruct their earnings growth (Gangl and Ziefle 2009), we introduce the number of employer changes and major employment breaks into the models. We define an employer *change* as a transition from one employer to another with no more than six months without a job between the two spells. By contrast, an employment *break* is a job separation followed by a jobless period of six months or longer.[10] Because the NLSY79 has limited information on work histories before respondents entered the survey, which was as late as age 22 for some, we count only the number of employer changes and employment breaks from age 22 onward. Our other reason for doing so is that workers are unlikely penalized for high job turnover rates at a very young age, when job instability is common and expected. Starting counting employer changes and employment breaks at age 22 also requires us to include a dummy variable to capture the person-months before that age in the sample. Our

[9] Had everyone moved strictly from one firm to the next and never returned to a previous employer, the increases in job tenure and work experience with time would be identical within each employer spell. Nevertheless, a number of respondents reported working for the same employer for separate periods of time, and when they returned to an employer, they generally experienced a greater gain in work experience than tenure with the employer. It is therefore possible to include both job tenure and work experience even in models focusing on within-employer pay variation (i.e., those with employer fixed effects).

[10] We do not consider a jobless spell of six months or longer as an employment break if respondents returned to the employer they were working for immediately before the jobless period: such breaks may represent parental or other family leaves obtained from the employer.

exploratory analysis nonetheless indicates that measuring employer changes and employment breaks from the time respondents entered the survey or not including the dummy variable for age under 22 years would not cause meaningful changes in the results.

In addition to human capital variables, we introduce a binary variable indicating the job's full-time status (more than 35 weekly working hours) because part-time jobs may impose an additional pay penalty. We also take into account marital status, which is thought to be relevant to fathers' pay premiums and mothers' pay penalties (Killewald 2013; Petersen et al. 2010). Because a spouse's working hours may affect the amount of time and effort individuals can put into their jobs, we create a series of mutually exclusive dummy variables containing information about respondents' marital status and spousal working hours: (1) never-married, not cohabiting; (2) never-married, cohabiting; (3) married with the spouse working fewer than 20 hours per week; (3) married with the spouse working 20–34 hours per week; (4) married with the spouse working full-time; and (5) separated, divorced, or widowed.[11] Finally, we control for region (Northeast, North Central, South, and West, according to the census definitions) and whether respondents lived in urban areas. Table A1 in the online appendix presents detailed information about the predictors in the models.

We estimate all the statistical models separately by gender. To adjust for the NLSY79's oversampling of certain minority groups and for attrition over time, we apply the survey's longitudinal weights to all models. Along with the use of weights, we also estimate robust standard errors throughout the analysis.

Results

Table 3 presents a series of fixed-effects models predicting log hourly earnings for men and women. Model 1, featuring no controls except individual and calendar-year fixed effects, shows that becoming a father is associated with a 13% increase in earnings (exp(.124) – 1 = .132). The equivalent model for women (Model 5), by contrast, indicates a 9% gross motherhood penalty (exp(–.092) – 1 = –.088). Adding human capital indicators, marital status, and geographical locations reduces men's earnings premium by 60% ([.124 – .049] / .124 = .60), according to Model 2. Nevertheless, being a father is still associated with a 5% pay increase (exp(.049) – 1 = .050). Human capital differences also explain a large part of the motherhood penalty, reducing the penalty to 5% of the hourly pay in Model 6 (exp(–.050) – 1 = –.049).

Models 3 and 4 further take into account potential differences in occupations before and after men's entry into fatherhood. The inclusion of broad-occupation fixed effects, which controls for fathers' and nonfathers' distributions across broad occupational categories, changes the coefficient of being a parent very little. Accounting for the pay differences across fine occupational categories explains a slightly larger share

[11] Our exploratory analysis indicated that differentiating those who cohabited based on their partner's working hours would not affect the main results. Similarly, delineating those who were separated from those who were divorced or widowed, or using somewhat different cutoff points for the spousal working hours, made no meaningful difference to the results.

Table 3 Results from fixed-effects models predicting log hourly earnings

	Men				Women			
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Parent	0.124**	0.049**	0.047**	0.042**	–0.092**	–0.050**	–0.051**	–0.027**
	(0.002)	(0.002)	(0.002)	(0.002)	(.003)	(0.003)	(0.003)	(0.003)
Education (ref. = less than high school)								
High school		–0.0102**	–0.102**	–0.101**		0.065**	0.043**	–0.011
		(0.014)	(0.014)	(0.013)		(0.016)	(0.016)	(0.013)
Some college		0.027†	0.024†	0.013		0.183**	0.148**	0.064**
		(0.014)	(0.014)	(0.014)		(0.016)	(0.016)	(0.014)
University and more		0.412**	0.393**	0.369**		0.405**	0.360**	0.302**
		(0.015)	(0.015)	(0.015)		(0.017)	(0.016)	(0.014)
Work Experience		0.006**	0.006**	0.006**		0.005**	0.005**	0.005**
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Work Experience Squared		–0.000006**	–0.000006**	–0.000006**		–0.000005**	–0.00005**	–0.000005**
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Job Tenure		0.001**	0.001**	0.001**		0.002**	0.002**	0.002**
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Job Tenure Squared		–0.000004**	–0.000004**	–0.000004**		–0.000005**	–0.000005**	–0.000005**
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Number of Major Breaks Since Age 22		–0.044**	–0.043**	–0.046**		–0.013**	–0.011**	–.013**
		(0.002)	(0.002)	(0.002)		(0.002)	(0.002)	(0.002)
Number of Employer Changes Since Age 22		–0.001*	–0.002**	–0.001**		0.002**	0.001†	0.002**
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Age Under 22		–0.110**	–0.103**	–0.098**		–0.135**	–.123**	–0.101**
		(0.003)	(0.003)	(0.003)		(0.003)	(0.003)	(0.003)
Full-time Job		0.059**	0.036**	0.007*		0.049**	0.019**	0.007**
		(0.003)	(0.003)	(0.003)		(0.002)	(0.002)	(0.002)

Table 3 (continued)

	Men				Women			
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Marital status (ref. = never-married, not cohabiting)								
Never-married, cohabiting		0.063**	0.060**	0.059**		−0.008†	−0.009†	−0.015**
		(0.005)	(0.005)	(0.005)		(0.005)	(0.005)	(0.005)
Married, spouse <20 work hours		0.134**	0.131**	0.123**		0.059**	0.061**	0.061**
		(0.003)	(0.003)	(0.003)		(0.004)	(0.004)	(0.004)
Married, spouse 20–34 work hours		0.093**	0.092**	0.084**		0.043**	0.036**	0.037**
		(0.003)	(0.003)	(0.003)		(0.006)	(0.006)	(0.006)
Married, spouse working full-time		0.050**	0.048**	0.044**		0.024**	0.023**	0.031**
		(0.002)	(0.002)	(0.002)		(0.003)	(0.003)	(0.003)
Separated/divorced/widowed		0.001	−0.001	−0.004		0.040**	0.038**	0.032**
		(0.003)	(0.003)	(0.003)		(0.003)	(0.003)	(0.003)
Region (ref. = Northeast)								
North central		−0.120**	−0.120**	−0.113**		−0.095**	−0.089**	−0.078**
		(0.007)	(0.007)	(0.007)		(0.008)	(0.008)	(0.008)
South		−0.043**	−0.049**	−0.048**		−0.065**	−0.062**	−0.063**
		(0.006)	(0.006)	(0.006)		(0.006)	(0.006)	(0.006)
West		0.016*	0.012	−0.004		0.057**	0.063**	0.063**
		(0.008)	(0.008)	(0.008)		(0.008)	(0.008)	(0.008)
Urban Area		0.037**	0.034**	0.034**		0.016**	0.016**	0.018**
		(0.002)	(0.002)	(0.002)		(0.002)	(0.002)	(0.002)
Fixed Effects								
Individual	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Broad occupation			Yes				Yes	
Fine occupation				Yes				Yes
Constant	7.040**	6.061**	6.104**	6.160**	6.889**	5.986**	6.054**	6.163**
	(0.001)	(0.024)	(0.024)	(0.023)	(0.002)	(0.020)	(0.019)	(0.017)

Notes: The analytic sample contains 1,176,234 person-months from 5,933 men and 1,039,476 person-months from 5,749 women. Values in parentheses are robust standard errors. The NLSY79 longitudinal weights are applied in estimating models.

†$p < .10$; *$p < .05$; **$p < .01$

of the fatherhood pay premium, but men's hourly earnings still rise by slightly more than 4% as they become fathers.

Similar to the results for men, including fixed effects of broad occupational categories, hardly alters the extent of mothers' pay disadvantage. Women continue to receive about 5% less pay as they become mothers (Model 6). Adding fine occupational fixed effects, however, reduces the motherhood earnings penalty considerably, by 47%. This change indicates that how women are distributed across fine occupations before and after they become mothers explains a large part of the motherhood penalty. This finding is important because most research on the motherhood penalty has taken into account just a handful of occupational characteristics (e.g., occupational female concentration, occupational training and skill requirements) and found them to be of little relevance (Budig and England 2001; Budig and Hodges 2010). We show that when using a fixed-effects approach, which accounts for *all* stable differences across detailed occupational categories, occupational differences do contribute to the motherhood penalty substantially.

The control variables in the models in Table 3 are generally consistent with what would typically be expected, boosting our confidence in the results. For example, increases in education, work experience, and job tenure all raise hourly earnings, whereas a higher number of employment breaks is associated with lower pay. Moreover, marriage, especially to spouses who work relatively few hours, tends to be associated with higher earnings for men.

To examine earnings changes within firms, we further add employer fixed effects to the models. Table 4 contrasts the parenthood effects between the models without employer fixed effects (identical to Models 4 and 8 in Table 3) and the models with such effects. Model 2 indicates that even after we control for differences across employers, fatherhood is linked to a near 4% increase in pay. Entering fatherhood clearly boosts men's pay growth within employer spells. Conversely, once we account for employer differences, being a mother is hardly associated with any decrease in women's hourly earnings (Model 4). Thus, women are rarely penalized for becoming a parent while they work for the same firms.

Table 4 also shows the differences in parenthood's effects between the models without and with employer fixed effects, which (as discussed earlier) can be interpreted as the between-employer contributions to the motherhood penalty and fatherhood premium, or the across-firm penalty and premium. A comparison of the differences with the coefficients for being a parent within firms (i.e., the coefficients in Models 2 and 4 indicates that the within-employer fatherhood pay premium is larger than the across-employer one, whereas the across-employer contribution to the motherhood penalty is greater than the within-employer contribution ($p < .05$ for the tests of the differences). This result is inconsistent with the selection perspective, whose argument that fatherhood increases men's tendency to trade job amenities for higher-paying positions elsewhere implies that most of the fatherhood premium comes from pay differences between men's firms before and after childbirth. The finding similarly casts doubt on the explanation focusing on effort and productivity, which would expect the parenthood penalty and premium to be similar within and across employers. The uneven motherhood penalties and fatherhood premiums within and across firms are instead consistent with the discrimination perspective that emphasizes employers' uneven trust in, but not uneven information about, their employees.

Table 4 Results from regressions of log hourly earnings with and without employer fixed effects

	Men			Women		
	Model 1	Model 2 (within-firm effect)	Model 1 – Model 2 (across-firm effect)	Model 3	Model 4 (within-firm effect)	Model 3 – Model 4 (across-firm effect)
Parent	0.042** (0.002)	0.036** (0.003)	0.006** (0.001)	–0.027** (0.003)	–0.006† (0.003)	–0.021** (0.003)
Fixed Effects						
Individual	Yes	Yes		Yes	Yes	
Calendar year	Yes	Yes		Yes	Yes	
Fine occupation	Yes	Yes		Yes	Yes	
Employer	No	Yes		No	Yes	
Sociodemographic Controls	Yes	Yes		Yes	Yes	

Notes: The analytic sample contains 1,176,234 person-months from 5,933 men and 1,039,476 person-months from 5,749 women. Values in parentheses are robust standard errors for Models 1–4 and are standard errors for the columns showing differences between models. The sociodemographic controls include various human capital indicators, full-time job status, marital status, region, and being in an urban area (same as those in the models in Table 3). The NLSY79 longitudinal weights are applied in all models.

†$p < .10$; **$p < .01$

Employers' greater trust in those who become fathers while working for them compared with fathers who come from other organizations could explain a larger fatherhood premium for the former. Similarly, the greater trust in existing workers could lead women who become mothers not to be penalized while working for the same employers but to face a significant penalty when switching firms.

Although the results presented so far suggest that mothers' and fathers' productivity levels cannot fully account for their respective earnings penalties and premiums, it is possible that changes in work effort resulting from parenthood contribute to some of the motherhood penalty and fatherhood premium. Because such changes should be sensitive to the number of children added to the household, we fit additional fixed-effects models in which we distinguish parents according to their number of children at the time of observation. Using the same methods as in Table 4 and regression models with and without employer fixed effects (the models also include individual, year, and occupation fixed effects, along with sociodemographic controls), we calculate the within- and across-employer contributions to the earnings premiums or penalties of having one, two, or three or more children. Figure 1 illustrates the results.

The figure shows that compared with being childless, having any number of children enhances men's earnings. Nevertheless, each additional child does not proportionally increase the fatherhood premium. Overall, men experience the largest gain when transitioning from having no children to having one child—that is, when they begin to be subjected to positive stereotypes associated with fathers. The additional gain from having a second or third child is comparatively small. This pattern further

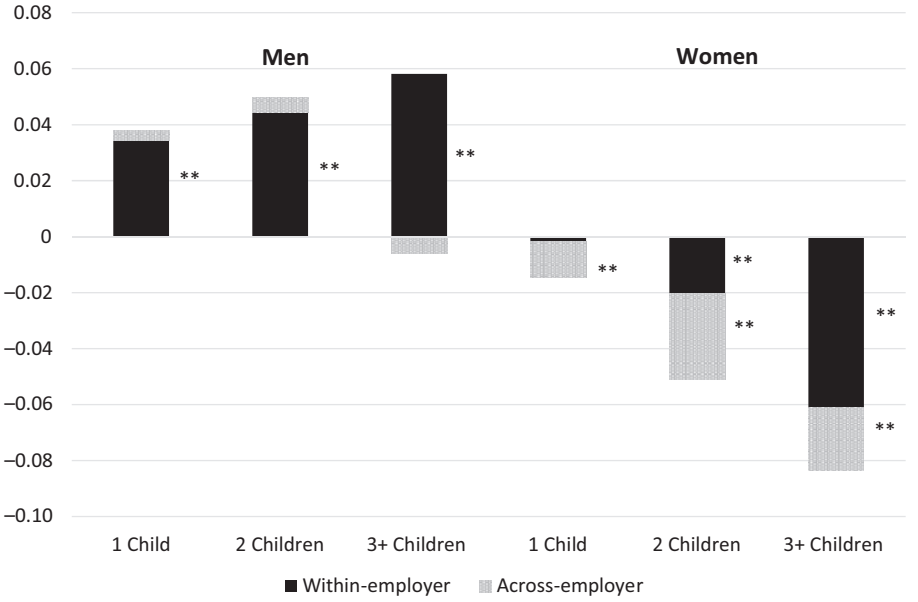

Fig. 1 Within- and across-employer parenthood effects by parity. The effects depicted here are derived from regression models identical to Models 4 and 8 in Table 3 and Models 2 and 4 in Table 4, but replacing the binary indicator of being a parent with time-varying dummy variables for the number of biological children present. All the penalties/premiums are in relation to not having a child. The asterisks indicate the statistical significance of the effect. $^{**}p < .01$

suggests that increases in men's work effort with their increasing financial obligations is not sufficient to explain the fatherhood premium.

More important, distinguishing fathers according to their number of children does not alter the fact that the within-employer fatherhood premium is greater than the across-employer premium. As Figure 1 indicates, the differences between employers hardly contribute to the fatherhood premium: none of the across-employer effects are statistically significant. Conversely, the within-employer pay premiums are significant and sizable for men with one, two, or three or more children. Once again, the uneven fatherhood premiums within and across employers is congruent with the hypothesis that employers especially favor the men who become fathers while working for them. The fact that the within-employer fatherhood bonus increases slightly with the number of children ($p<.05$ for the differences in the effects) while the across-employer bonus does not also suggests that employer favoritism, rather than work effort, more likely explains the fatherhood premium. Specifically, the pattern that men's earnings rise only with the number of children within employer spells could reflect employers' uneven knowledge; employers are likely to know when their existing employees have an additional child but are perhaps unaware of such information about their new recruits.

Figure 1 shows that mothers with any number of children experience reductions in earnings. The size of the motherhood penalty generally corresponds to the number of children both within and across employing organizations, suggesting that mothers' greater caring responsibilities and hampered work effort at least partly contribute to

their earnings disadvantage. The within-employer motherhood penalty, however, remains smaller than the between-employer penalty when women have just one or two children, which is consistent with the argument emphasizing employers' discrimination and greater trust in workers they know more.

When women have three or more children, the relative magnitudes of the within- and across-employer motherhood penalties nevertheless change. Compared with having no children, having three or more children is associated with a considerable earnings reduction within firms (about 6%) but a smaller pay reduction across firms (about 2%). The especially large increase in the within-employer motherhood penalty when women transition from having two to more children suggests a potential limit to employers' trust in the mothers who have been working for them. Although trust could reduce employers' bias against women in their organizations who are having their first or second child, employers might draw a line for female employees they perceive as having too many children. Employers may view women having three or more children as having greater-than-average devotion to their family and may no longer be willing to give them the benefit of doubt as they do for women with fewer children. Perhaps because employers have the chance to learn about each childbirth of their existing workers but typically could not differentiate new recruits based on their number of children, we find that women's earnings are disproportionately reduced when they transition from having two to three children within the same firms but not between firms. In this sense, the finding that the within-employer motherhood penalty is greater than the across-employer penalty for mothers with three or more children is still consistent with the argument about employer bias. The perspectives about mothers' lower work effort or selection into lower-paying firms, by contrast, cannot explain the greater within-firm pay penalty for women with three or more children.

So far we have argued that our results suggest that employer discrimination explains a substantial part, if not all, of mothers' earnings disadvantage and fathers' earnings advantage. There is, however, an alternative explanation for our findings that the motherhood penalty is larger across than within firms but the fatherhood premium is larger within than across firms: mothers and fathers may stay longer in firms where they experience smaller penalties and larger premiums. If so, our estimates of within-firm fatherhood premium and motherhood penalty would be disproportionately based on experiences from firms that treat parents especially well, explaining the relatively large fatherhood premium and relatively small motherhood penalty within firms. To test whether the extent of parenthood penalty or premium is indeed related to how long one stays in a firm, we fit additional models dividing employer spells based on whether they ended soon after respondents entered parenthood or had a childbirth. We argue that only upon parenthood are workers likely to know how much their organizations penalize or reward parents. Those who feel that their employers overpenalize mothers or undercompensate fathers likely leave their workplaces soon after they find out. Following this logic, parenthood would be associated with a greater penalty for women and a smaller premium for men in the organizations that individuals leave shortly after having a child than in other organizations.

Table 5 presents models in which we introduce an interaction between being a parent and a binary indicator for employer spells that ended less than three years after individuals' entry into parenthood or after any childbirth, along with all other variables included in the most comprehensive models in Tables 3 and 4. Because

Table 5 Estimates of parenthood effects by employer-spell characteristics from fixed-effects models predicting log hourly earnings

	Men		Women	
	Model 1	Model 2	Model 3	Model 4
Parent	0.056**	0.055**	–0.013**	–0.021**
	(0.003)	(0.003)	(0.004)	(0.004)
Parent × Employer Spell Ended Early in Parenthood	–0.090**		0.031**	
	(0.005)		(0.007)	
Parent × Employer Spell Ended While Having a Young Child		–0.055**		0.045**
		(0.005)		(0.006)
N	1,176,234	1,176,234	1,039,476	1,039,476

Notes: Values in parentheses are robust standard errors. All models include the time-varying sociodemographic variables included in the long models in Tables 3 and 4, as well as employer, individual, calendar year, and detailed-occupation fixed effects. The NLSY79 longitudinal weights are applied in estimating the models.

**$p < .01$

the models include employer fixed effects, the main effects for the employer-spell characteristics are dropped from the models, given that they do not vary within an employer spell. Models 1 and 2 for men indicate that employer spells that ended early in parenthood or shortly after a childbirth indeed reward fathers less; there is virtually no fatherhood premium within such spells. For mothers, however, the results contradict the argument that women experiencing greater within-firm motherhood penalties are likely to leave their employers soon after childbearing. Women actually encounter a pay increase with motherhood during employer spells that end early in parenthood or soon after a childbirth ($p<.05$ for the parenthood plus interaction effects).

Although men's results in Table 5 are consistent with the alternative explanation that they tend to remain in firms that compensate fatherhood more, it is noteworthy that men receive a very small fatherhood premium across firms (0.6%, as shown in Table 4). If men indeed leave their employers because their fatherhood status is not rewarded, they are likely to move to firms that pay a sizable premium for the status, which should lead the across-firm fatherhood premium to be somewhat comparable with the within-firm premium. Given the minimal between-organizational fatherhood premium and the lack of consistent results for women, we suggest that the argument that emphasizes the selection to stay based on the extent of the motherhood penalty and fatherhood premium is unlikely to explain our findings.

Conclusions

Despite much scholarly interest in pay premiums and penalties for parents, research on how parenthood alters individuals' earnings trajectories within and across employing organizations has been limited. To our knowledge, this study is the first to com-

pare within- and between-firm fatherhood premiums and motherhood penalties in the United States. With fixed-effects models, we show that the fatherhood premium primarily results from the additional pay raises fathers receive within firms; being a father brings a rather small bonus when men move to another employer. Conversely, women experience a greater across-employer than within-employer motherhood penalty except when they have a comparatively large number of children. Until women have three or more children, they tend to encounter a smaller pay reduction with their childbearing transition if they stick with the same firm rather than moving to a different firm.

Earlier in this paper, we described the three major explanations for the motherhood pay penalty and fatherhood pay premium: workers' effort and productivity, workers' trade-offs between monetary and nonpecuniary compensations, and employer discrimination. We proposed that these explanations predict differences in the relative magnitude of within- and across-firm parenthood premiums or penalties. Results from our analysis are most consistent with the theory that employers favor fathers but discriminate against mothers and that the extent of their favoritism and discrimination corresponds to how well they know and trust their workers. Employers appear to offer a larger fatherhood bonus to men who become fathers while working for them than to fathers they hire from elsewhere, contributing to a greater within- than across-firm fatherhood premium. Even though we do not have direct evidence on employers' intentions, their likely greater trust in women who become mothers while working for them compared with women who are mothers at the point of hire could also explain the smaller within-firm motherhood penalty.

Although our findings are consistent with the account emphasizing employer favoritism and discrimination, they do not entirely rule out the possibility that heightened work-family conflict obstructs mothers' work effort, leading to their earnings disadvantage. We show that the motherhood penalty is amplified with the number of children, both within and across organizations. This result suggests that the increased family burden with each additional child may hamper mothers' job performance somewhat, even though the effort-based account cannot fully explain why the motherhood penalty is generally greater across organizations than within them. Compared with the findings on women, the evidence for fathers' enhanced work effort with the addition of each child is weaker. Men experience only a modest pay increase with their second or third child, and the pattern of earnings growth with each additional child only appears within, not across, employer spells. This lack of symmetrical gains across firms suggests that employers' beliefs about what adding a child does to men's productivity, rather than men's actual changes in work effort, more likely account for men's within-firm pay raises with each additional child.

Our results provide little support for the account concerning mothers' and fathers' selection into jobs that involve different trade-offs between nonpecuniary amenities and pay. For men, the between-firm fatherhood premium is relatively small, indicating that being a father does not lead men to move to workplaces that offer higher wages at the expense of other amenities. That the fatherhood premium mostly results from fathers' greater earnings growth within firms also shows that fathers do not have to choose different trade-offs between job amenities and pay to gain financially. As for women, although we show that they receive lower pay in the firms they shift to after childbearing, this pattern does not necessarily support the

argument that having a child leads women to choose firms that are more family-friendly but pay less. Instead, the finding could reflect a heightened penalty that employers apply to prospective employees who are mothers. Because our data do not include multiple workers for each employer, we unfortunately cannot determine whether the firms women move to after entering motherhood pay all workers less or just the women less. It is, however, noteworthy that women encounter a considerable within-firm motherhood penalty—more than the between-firm one—when they have three or more children. This evidence directly contradicts the claim that selection into family-friendly workplaces is the root of mothers' pay disadvantage. Taken all together, our results suggest that the compensating differentials account is unlikely to be valid.

Beyond offering evidence for the different perspectives, this study contributes to the gender and work literature by examining parenthood and earnings growth within firms. As far as we know, no prior research has systematically examined how parenthood shapes the extent to which individuals' earnings grow within their workplaces over time. Our study demonstrates that having a child is linked with a steeper earnings growth for men within firms, but it hampers women's earnings growth within firms when they have more than one child. Thus, even if women can avoid the extra pay penalty for job seekers who are mothers by remaining with the same employer for a long period, they still face an increasing gender pay gap as they and their male colleagues have additional children. Despite prior research noting that mothers' frequent job turnover rates diminish their earnings prospects (e.g., Gangl and Ziefle 2009; Glass 2004), our study shows that a thorough understanding of gender inequality at work also requires attention to how fathers' earnings advantage extends with their tenure within a firm.

More generally, our study illustrates an innovative way to examine how workplaces contribute to pay disparities in the absence of employer-employee linked data. As discussed earlier, our approach of using unique employer identification information from individual-level data means that we lack data from multiple employees in each firm, making it impossible to tell how firms pay other workers. Despite this limitation, this approach has the important advantage of enabling longitudinal models that account for unobserved heterogeneity. Because long-term longitudinal data are more readily available than employer-employee linked data, researchers can easily use our approach to shed light on how other time-varying conditions, such as individuals' marital status, health, and receipt of special training or certificates, may affect earnings differently within and across employing organizations.

Finally, this study has a general theoretical implication. Our results demonstrate the important roles of employing organizations in shaping gender inequality. We find, for example, that women's shifts across employers contribute to nearly as much of the motherhood pay penalty as do their shifts across detailed occupations. This finding suggests that firms are just as important as occupations in determining women's earnings trajectories throughout the life course, even though much more research focuses on the influences of occupational characteristics on wages (e.g., Glauber 2012; Kilbourne et al. 1994; Levanon et al. 2009; Yu and Kuo 2017). Our results about the uneven effects of parenthood within and across firms further suggest that much of the earnings disadvantage or advantage parents face occurs at the firm level. This study thus echoes sociologists' long-standing call to regard employing organizations as the driving force for social stratification (Baron and Bielby 1980; Stolzenberg

1978). More research on firm settings would help enrich our understanding of earnings inequality between women and men. ■

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