

Daddy's Girl: Daughters, Managerial Decisions, and Gender Inequality

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Abstract

Using Danish registry data, we study how managers' gender attitudes shape gender inequality in the workplace by exploiting the birth of a daughter - as opposed to a son - as a plausibly exogenous shock to male managers' gender attitudes. Comparing within-firm changes in women's labor outcomes depending on the gender of the manager's newborn child, we find that women's relative earnings and employment increase by 4.4% and 2.9% respectively following the birth of the manager's first daughter. These effects are driven by an increase in managers' propensity to substitute male hires with female hires that have comparable education, hours worked, and earnings. Consistent with this substitution channel, we do not detect any significant effect on firm performance. Because these effects emerge rapidly and persist over time, we can rule out the need for prolonged exposure to gender issues or for personal incentives tied to improving conditions for their own daughters as prerequisites for managers to promote gender equality within their firms.

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1 Introduction

Managers are a key determinant of firm outcomes, and a large body of work shows that their attitudes and traits strongly influence company growth and financial policies (see Malmendier, 2018, for a recent review). Managers’ attitudes and values may also have far-reaching consequences for gender equality in the workplace. By wielding significant influence over the careers of both male and female employees, managers are in a unique position to either perpetuate or reduce the large gender disparities that remain in the corporate world.¹ For example, if managers’ attitudes toward women’s role in the labor market affect their hiring, retention, or promotion decisions, we may observe differences in women’s relative earnings and employment across otherwise comparable firms.

Despite managers’ central role in affecting labor outcomes, and the growing emphasis on gender attitudes in the study of gender gaps (Bertrand, 2011, 2020), it remains unclear whether managers’ gender attitudes causally shape gender inequalities in firms. This reflects two fundamental challenges. First, identifying and isolating the role of attitudes is difficult, as they are generally unobservable and endogenous. Second, estimating the causal effect of managers on corporate outcomes is complicated by the fact that managers are not randomly allocated to firms. To overcome these challenges one would ideally need to randomly assign attitudes to managers, holding all else fixed, including managers’ background characteristics and the match between a manager and a firm.² Such an experiment, however, is rarely feasible in practice.

In this paper we overcome these challenges by leveraging a natural experiment that comes as close as possible to the ideal one, as it generates an exogenous shift in managers’ attitudes while allowing us to estimate changes in gender inequality within a manager-firm match. Specifically, motivated by sociological work showing that fathering daughters makes men more sensitive to themes of gender equality (Warner, 1991; Warner and Steel, 1999), we exploit the birth of a daughter, as opposed to a son, as a plausibly exogenous shock to male managers’ unobservable gender attitudes. To account for the non-random allocation of managers across firms, we compare changes in gender inequality along the hiring, retention, and promotion margins *within* a manager-firm match, depending on the gender of the manager’s newborn.

We implement this design using Danish administrative data that link matched employer–employee records with longitudinal information on managers’ family composition. These data allow us to test for a *daughter effect*, i.e., whether women’s relative outcomes improve following the birth of a daughter rather than a son to a male manager. We focus on single-manager establishments, defined as those with only one individual in a managerial position in a given year. This increases the likelihood that the managers we study are directly involved in personnel decisions, such as hiring and promotions, while also supervising and interacting with employees

¹Despite decades of progress, women remain underrepresented in high-pay occupations and leadership positions (e.g. Bertrand, 2018), are promoted at lower rates than men (e.g. Azmat and Ferrer, 2017; Benson et al., 2021), receive less recognition for their work (e.g. Sarsons, 2017a,b), and face harsher penalties for misconduct (e.g. Egan et al., 2022). Many of these outcomes are directly or indirectly shaped by managerial decisions.

²Even if attitudes could be observed and we could randomize managers to firms, this would still not isolate the effect of interest, since gender attitudes likely correlate with other personal characteristics.

on a day-to-day basis.³

We present four sets of results. First, we find evidence of a daughter effect, as women’s relative earnings and employment increase in establishments where male managers experience the birth of an additional daughter, as opposed to a son. This result is driven by managers who experience the birth of their first daughter, while we do not detect similar effects from a newborn’s gender at higher parities. Quantitatively, female relative earnings and employment increase by 4.4% and 2.9% respectively following the birth of a manager’s first daughter. To contextualize these magnitudes, we draw on previous research that has identified a positive relation between female representation in managerial positions and gender equality in firms (e.g. Kunze and Miller, 2017; Flabbi et al., 2019) and benchmark our estimates to the effect of having a female manager. This analysis reveals that the size of the daughter effect is between 20% and 50% of the positive association observed between female outcomes and female-led firms. We rule out alternative explanations by showing that a child’s gender does not affect other aspects of managers’ behavior that could plausibly influence firm outcomes, such as paternity leave take-up or the duration of the manager–establishment match. We also find that managers with daughters subsequently sort into firms with better outcomes for female employees, reinforcing the importance of accounting for managerial sorting.

Second, we investigate the margins of adjustment behind the estimated effects and find that female employment and earnings are mostly impacted by the hiring margin. In particular, the birth of a manager’s first daughter steers the composition of new hires towards women, and especially women who have higher levels of education, work full-time, and earn high salaries in the firm. Importantly, although the characteristics of the female workforce change, we find no corresponding change in the characteristics or average labor costs of the workforce as a whole. Thus, within the range of our estimated effects, managers appear able to substitute between male and female hires who are close equivalents in terms of education, hours worked, and pay, while leaving the broader composition of the workforce unchanged. Consistent with our findings on hiring, we also find evidence that promotion decisions shift in favor of women. Earnings increase among female incumbents who are more likely to be considered for promotion, with no corresponding effect for men.

Third, we examine whether the improvement in women’s relative earnings and employment affects firm performance, using sales, employment, and value added per employee as outcomes. We find small and insignificant effects, consistent with our earlier result that managers substitute male workers with similarly qualified female workers. This indicates that managers who shift toward hiring women after the birth of a daughter can do so without compromising match quality - implying no equity–efficiency trade-off. At the same time, it suggests that when these same managers previously favored men, their bias did not impose a performance penalty on the firm. In other words, managers’ gender bias appears to operate “at the margin”: it influences the gender of the chosen candidate in personnel decisions with little consequence for output in the short run. Importantly, the absence of short-run performance effects does not mean that

³We focus on male managers for two main reasons. First, prior studies find that daughters influence the gender attitudes of fathers, but not mothers. Second, in our sample, 80% of managers are male, leaving too few observations to obtain precise estimates when replicating the analysis for female managers.

such *subtle bias* is inconsequential for aggregate productivity. Recent theoretical work shows that even subtle biases in marginal evaluations can reduce aggregate productivity by distorting expectations and investment incentives among the disfavored group (Pikulina and Ferreira, 2024; Siniscalchi and Veronesi, 2021).⁴

Finally, we explore the mechanisms through which daughters can affect male managers' behavior towards women. One possibility is gradual learning: by raising their daughters, managers may progressively learn about women's abilities and the structural or cultural barriers they face. Under this channel, changes in personnel decisions should emerge only after some time and intensify as daughters grow older. Alternatively, the birth of a daughter may discontinuously heighten the salience of gender issues, either by increasing fathers' awareness of gender inequality (Shafer and Malhotra, 2011; Borrell-Porta et al., 2018) or by leading them to identify more closely with women when forming their preferences (Oswald and Powdthavee, 2010). Under this channel, managers' behavior could change rapidly following the birth.

We leverage the longitudinal nature of our data and examine the timing of the effects to gain indirect insights into these mechanisms. An event-study analysis shows that the effects emerge immediately after the birth and persist for the following years the manager remains in the firm. This pattern suggests that prolonged exposure to daughters is not required for managers to change their behavior toward female employees and is consistent with the birth of a first daughter triggering a discontinuous increase in the salience of gender issues. We also exploit cross-sectional variation to examine longer-run exposure and find that the daughter effect strengthens as daughters grow older, consistent with learning through exposure contributing to its persistence over time. Finally, the rapid onset of the effect also argues against a self-interest mechanism - namely, that managers with daughters act to reduce gender gaps because their own children bear some of the costs of gender inequality. Under such motives, we would expect managers to change their behavior only *if* or *when* their actions could plausibly benefit their daughters. Instead, the evidence suggests that the birth of a daughter acts as a catalyst for a lasting shift in how men perceive the broader costs of gender inequality.⁵

Our work relates to several strands of literature. First, it contributes to the literature investigating the causes of remaining differentials in men's and women's labor market outcomes. Since traditional human capital factors such as education and experience no longer account for differences in earnings or employment, economists have increasingly examined gender differences in opportunities and constraints, including attitudes toward women's role in the labor market. Our paper brings a personnel perspective to the growing literature showing that gender attitudes feed into gender gaps by shaping women's choices in the labor and marriage markets (Bertrand et al., 2015; Fortin, 2015; Bursztyn et al., 2017; Ichino et al., 2024). By focusing on managers, we extend this perspective beyond individual and household outcomes,

⁴In Section 6.4, we also discuss other explanations that may reconcile the lack of short-run performance effects with potential improvements in firm performance over a longer time horizon.

⁵In this sense, our results contrast with those of McGuirk et al. (2023), who show that self-interest explains politicians' responses to child gender in the context of conscription: having a draft-age son reduces support for conscription, but this opposition fades once the son is no longer draft-eligible, suggesting that politicians' behavior is shaped by private incentives, and that having a son does not lead to a reconsideration of the social costs of war.

demonstrating that gender attitudes can also drive inequality at the corporate level through their influence on managerial decision-making. Furthermore, by examining the timing of the daughter effect, we contribute to the limited body of work on the origins of attitudes, providing evidence that such attitudes are not necessarily innate but can evolve over the life cycle and be shaped by personal experiences (e.g. Kuziemko et al., 2018; Dahl et al., 2021; Bursztyn et al., 2024; Andries et al., 2025; McGuirk et al., 2023).

This paper also relates to the literature on managerial traits and firm outcomes. Since the seminal work of Bertrand and Schoar (2003), much of this research has highlighted the role of managers’ attitudes - particularly overconfidence and risk aversion - in shaping corporate performance and financial decisions (see Baker and Wurgler, 2013; Malmendier, 2018, for recent reviews). While this literature provides ample evidence of such links, its ability to establish causality is limited by the scarcity of exogenous variation in both managers’ attitudes and firms’ exposure to them (Cronqvist et al., 2012; Fee et al., 2013; Malmendier, 2018). Our paper makes two contributions. First, by exploiting changes within a manager–firm spell, we address endogenous sorting and provide causal evidence on the impact of managerial attitudes on organizations. Second, we contribute to the emerging literature extending management-style research to workers’ labor market outcomes (e.g. Duchin et al., 2021; Acemoglu et al., 2022; He and le Maire, 2024). Within this literature, our analysis moves beyond existing work documenting how managers’ own gender relates to gender gaps in firms (e.g. Kunze and Miller, 2017; Flabbi et al., 2019; Cullen and Perez-Truglia, 2023), showing that male managers too may internalize the social costs of gender inequality - an important insight given the persistent over-representation of men in corporate leadership.

Of particular relevance to our study is the work documenting a relationship between the gender mix of CEOs’ children and firm outcomes (Dahl et al., 2012; Cronqvist and Yu, 2017; Calder-Wang and Gompers, 2021; Wu et al., 2023).⁶ The longitudinal and rich nature of our data allows us to extend this literature in three key ways. First, by examining the timing of the daughter effect, we shed new light on the mechanisms through which daughters shape managers’ behavior. The rapid and persistent nature of our effects allows us to rule out two channels: (i) that managers need prolonged exposure to gender-related issues before promoting gender equality within firms, and (ii) that changes are driven solely by personal incentives to improve conditions for one’s own daughter. These findings are policy-relevant because they show that managers’ gender attitudes can change rapidly, even in the absence of prolonged exposure or self-interested motives, suggesting that a broad range of managers may be receptive to shifts in attitudes that promote gender equality. Second, by exploiting birth events and controlling for manager–establishment spells, our paper provides the first causal evidence that the daughter effect operates through changes in male managers’ personnel decisions, rather than sorting

⁶Dahl et al. (2012) show that when managers endogenously transition into fatherhood, they increase their own salary at the expense of employees’ wages, with smaller effects when the child is a daughter. Using cross-sectional variation in the gender mix of children of venture capitalists and founders, Calder-Wang and Gompers (2021) and Wu et al. (2023) document that having daughters correlates with higher female employment, while Cronqvist and Yu (2017) find that S&P 500 companies led by CEOs with daughters have better corporate social responsibility ratings. A separate group of studies focus instead on the effect of fathering daughters on men’s political views (e.g. Washington, 2008; Oswald and Powdthavee, 2010), finding that men with daughters tend to vote more liberally.

alone. This distinction matters because we also find that managers with daughters subsequently sort into firms with better outcomes for female employees. Our within-spell design therefore shows that the daughter effect reflects changes in managers’ behavior within firms, not only differences in where they choose to work. Finally, access to firm-level balance sheet data allows us to examine whether and how changes in gender equality following the birth of managers’ daughters relate to firm performance, enabling us to assess both the equity and efficiency implications of the daughter effect within a causal design.

2 Institutional Background and Data

2.1 Institutional Background

One might wonder whether Scandinavia - often perceived as highly gender-equal due to its high female employment rates and generous family policies - is an appropriate setting to study the role of managers’ gender attitudes in explaining gender gaps. Indeed, Denmark and other Nordic countries have historically differed from countries like the United States, the United Kingdom, or much of continental Europe in terms of gender outcomes.

Two figures in the Appendix help address this concern. First, Figure A1, based on OECD data (OECD, 2025b,c), shows that although gender gaps were historically smaller in Scandinavia, countries such as the U.S., the U.K., and France have gradually closed their gaps, converging toward the Scandinavian levels over time. Crucially, this convergence occurs during the period covered by our data (1992–2017), suggesting that studying the daughter effect in Denmark during this time-frame can yield insights relevant beyond the Scandinavian context.⁷ Second, Figure A2 illustrates cross-country trends in gender norms, drawing on data from the European Values Survey (EVS, 2022). At the start of our sample period in 1990, Denmark and Sweden already demonstrated more progressive gender attitudes, as reflected in higher levels of disagreement with statements such as “Men should have more right to a job than women” or “A pre-school child is likely to suffer if his or her mother works”. However, by 2017 - the end of our sample period - countries like the U.K. and France had largely caught up.⁸

Two key points emerge from this analysis. First, although Denmark began from a position of greater gender equality in both labor market outcomes and gender norms, cross-country differences narrowed considerably over the time period considered in our study. This suggests that studying the daughter effect in Denmark during these years can provide insights that are relevant beyond the Scandinavian context. Second, Denmark itself experienced substantial improvements in both dimensions during our sample period. In Section 7, we exploit this evolution in gender norms and inequality in Denmark to examine how the daughter effect

⁷In addition, recent research using Danish data highlights the persistence of sizable gender gaps, reflected in persistent glass-ceiling effects (Smith et al., 2011, 2013) and in women continuing to bear nearly the full earnings penalty of childbirth (Kleven et al., 2019).

⁸We rely on these two questions from the European Values Survey because they offer consistent measurement of gender norms across all four countries (Denmark, Sweden, the U.K., and France) at both the beginning and end of our sample period. Unfortunately, U.S. data are only available for the 1990 wave and are therefore not well-suited to examine changes in norms over our sample period.

changes over time.

2.2 Data and Sample

We use Danish registry data covering employer-employee matched information on the universe of Danish firms and individuals between 1992 and 2017 (Statistics Denmark, 2020). We link individual career histories with longitudinal data on household structure so that we can identify, for each manager in each year, the establishment in which the manager works, the characteristics of the employees the manager supervises, and the number, gender, and age of the manager’s children. By observing variation in the family composition of managers due to birth events, we can relate changes in women’s relative outcomes within a firm to the gender of the manager’s newborn child.

Following previous work, we identify managers in the data as individuals whose occupational code begins with the digit corresponding to managerial positions. Since our goal is to identify managers who are directly involved in personnel decisions, our sample focuses on single-manager establishments - i.e., establishments where in a given year we observe only one individual holding a managerial position.⁹ This sample choice increases the likelihood that we can identify the manager responsible for personnel decisions, such as hiring and promotions, and that this manager also supervises and interacts with employees on a day-to-day basis.¹⁰ While single-manager establishments are relatively small - employing on average between 12 and 13 workers - they account for about one-third of total employment and more than 90% of all establishments, making them an important source of jobs and the most representative type of establishment in the Danish economy. Appendix Table A1 documents this representativeness. It also shows that managers in single-manager establishments are broadly similar to those in the overall population in terms of age, gender, and fertility, though they earn less. The same pattern holds for employees of both genders. Finally, single-manager establishments report lower sales and value added per employee but closely match the overall economy in sectoral composition. While single-manager establishments are broadly representative along these dimensions, our estimates speak to a specific organizational setting, and their generalizability to larger, multi-manager firms depends on the extent of managerial discretion in those organizations and on whether managers sort across firms according to their gender attitudes.¹¹

As we outline in Section 3, our empirical strategy leverages birth events for identification.

⁹In this paper, “establishment” refers to a firm’s individual plant and, except in Section 6.4, all analysis is conducted at the establishment level. For simplicity, however, we sometimes refer to establishments as firms.

¹⁰Our data do not allow us to identify which employees report to which manager in multi-manager firms. We therefore exclude these firms from the analysis, since any absence of a daughter effect would be difficult to interpret because we cannot delineate the sphere of influence of individual managers. Moreover, while one might expect many of the single-manager establishments in our sample to be family firms, we lack ownership data to verify this. In particular, we do not observe the full set of owners or their familial relationships, which are necessary to classify family firms according to standard definitions in the literature (Burkart et al., 2003).

¹¹If managers in larger firms hold similar gender attitudes and retain comparable discretion over personnel decisions within their own domain - a team, unit, or division - the mechanism we document should operate there as well. In more formalized organizations, however, centralized human-resource policies may constrain individual managers’ discretion. Whether a daughter effect emerges will then depend on how binding these policies are and on whether they themselves embed gender bias or, conversely, make changes in personnel practices more durable, as Miller (2017) shows for affirmative action.

Thus, from the overall sample of single-manager establishments, we construct the *birth sample*, defined as all manager–establishment spells in which the manager experiences at least one birth event during the observation period. We define a manager-establishment spell as a continuous period during which a manager serves as the sole decision-maker at a given establishment, thereby capturing a stable match between the manager and the firm. Because managers are typically in later stages of their careers, they are on average older than the typical employee (see Table A1) and, in most cases, past their reproductive years. As a result, birth events are relatively rare: only about 4% of all spells in the overall sample include a birth. Consistent with prior evidence that the impact of daughters on parental attitudes is concentrated among fathers (Warner and Steel, 1999; Shafer and Malhotra, 2011; Borrell-Porta et al., 2018), and since over 80% of the managers in our birth sample are men, we restrict the analysis to male managers. This yields a final birth sample of 6,701 unique manager–establishment spells.¹²

2.3 Descriptive Statistics and Outcome Variables

Table 1 reports descriptive characteristics for the managers, employees, and establishments included in the birth sample. On average managers who experience a birth event are about 38 years old, have two children - one boy and one girl - and their youngest child is just below 4 years old.¹³ Columns (2) and (3) report demographic characteristics for male and female employees respectively, and indicate that, on average, female workers are statistically more likely to be younger, to work part-time, and to earn less. These gender differences also carry over to the establishment level. In particular, column (4) shows our main outcome variables, namely the Share of Female Employment and the Female Earnings Ratio, which, for each establishment-year observation, are defined in the following way:

$$\text{Share } F \text{ Employment} = \frac{N_f}{N_m + N_f} \quad \quad F \text{ Earnings Ratio} = \frac{\bar{w}_F}{\bar{w}_F + \bar{w}_M} \quad (1)$$

N_f and N_m represent respectively the total number of female and male workers, and $\bar{w}_f$ and $\bar{w}_m$ represent their average annualized labor earnings.¹⁴ All variables are set to zero when a gender is not present in a given establishment-year.

Column (4) shows that female workers represent only 34% of the total workforce and that 26% (7%) of all establishment-year observations are characterized by zero female (male) employees. Because not all establishments employ both genders in every year, and to avoid conditioning the sample on an outcome variable - namely, whether a firm employs women - we define the Female Earnings Ratio as in Equation (1).¹⁵ This measure is a share bounded be-

¹²While the birth sample forms the core of our analysis, we also draw on the broader cross-section of manager–establishment spells from the full universe of single-(male)-manager firms to examine how effects evolve over longer horizons, such as when daughters reach adolescence or adulthood (see Section 5).

¹³Thus, unsurprisingly, managers in the birth sample are younger than those in the full sample of single-manager establishments (see Table A1).

¹⁴Labor earnings of individuals employed for only part of the year are scaled to a full-year equivalent.

¹⁵We avoid inequality measures based on differences in average earnings between men and women - whether overall or within categories such as occupation - since these require observing both male and female employees

tween zero and one, where a value of 0.5 indicates gender equality in average earnings. Column (4) of Table 1 shows that the Female Earnings Ratio averages 0.39, reflecting lower average earnings for women relative to men, consistent with the individual-level statistics. A key advantage of this measure is that it remains well-defined even when one gender is entirely absent, allowing us to compute it for the full sample of establishment-year observations. However, at the extensive margin - when a firm transitions from zero to at least one female employee - changes in this measure reflect both quantity and earnings effects.¹⁶ To enable a more precise interpretation of the overall treatment effect on the Female Earnings Ratio, in Section 4 we formally decompose the effect into contributions from the extensive margin (changes in the presence or absence of women) and the intensive margin (changes in relative earnings conditional on female employment). In addition, in Section 6 we examine the role of the hiring and promotion margins in shaping women’s access to higher-paying roles.

3 Empirical Strategy

Building on the work of Warner (1991) and Warner and Steel (1999), and the subsequent studies inspired by their findings, we know that a child’s gender affects the likelihood that men adopt more egalitarian views and support policies addressing gender inequality. To extend this insight to the corporate setting, we exploit birth events to test whether child gender shapes managers’ personnel decisions and within-firm gender inequality through the following regression:

$$Y_{et} = \alpha + \beta N \text{ Daughters}_{et} + C_{et} + \chi_{me} + \phi_t + \epsilon_{et} \quad (2)$$

where Y is the outcome of interest in establishment e and year t , $N \text{ Daughters}$ is the number of daughters parented by the manager of the establishment e at time t , and C is a set of fixed effects for the total number of the manager’s children.¹⁷ Conditioning on the total number of a manager’s children is key, as it allows us to separate the effect of parenting an additional child, an endogenous decision, from the impact of parenting an additional daughter, an exogenous event once we condition on the decision to have a child.¹⁸ ϕ_t and χ_{me} are respectively year fixed-effects and fixed-effects for manager-establishment spells. Standard errors are clustered at the establishment level. Including manager-establishment fixed effects ensures that we compare outcomes for female employees within the same establishment and under the same manager

in each category. Specifically, because our results show that the birth of a daughter affects both whether women are hired and the type of positions into which they are hired, such measures would introduce selection bias.

¹⁶Specifically, when a firm transitions from having no female employees to hiring its first woman, the Female Earnings Ratio jumps from 0 to a positive value determined by her earnings relative to the average male earnings. For example, if she earns exactly the male average, the ratio increases to 0.5; if she earns less (more), the ratio rises to a value below (above) 0.5.

¹⁷Because we restrict to single-manager establishments, there is a one-to-one correspondence between managers and establishments. Accordingly, we use the subscript e to also refer to managers’ characteristics, such as their number of daughters and children.

¹⁸Section 3.1 presents evidence supporting the assumption that, conditional on the decision to have a child, the newborn’s gender is exogenous. Note that once we condition on family size, the number of daughters and the number of sons are mechanically dependent. Consequently, this specification does not allow us to distinguish whether changes in managers’ behavior are driven by the presence of an extra daughter or by the absence of an extra son.

before and after the birth of the manager’s child. This implies that we can isolate the impact of daughters on managers’ personnel decisions from any influence on sorting into different firms by child gender. For instance, given evidence that daughters can affect men’s values and behavior toward women, male managers might be more likely to join female-friendly workplaces after having a daughter rather than a son. Section 7.2 provides direct evidence on this possibility.

Given the inclusion of fixed effects for both managers’ total number of children and manager-establishment spells, our identification strategy can be viewed as the empirical analogue of the following experiment. Consider two single-manager establishments where managers have the same number of children and each experiences the birth of an additional child while employed at the firm. Nature assigns one manager a daughter (*treated*) and the other a son (*control*). The treatment effect is the difference in changes in female employees’ labor market outcomes between treated and control establishments, captured by β in equation (2). In other words, β identifies the relative daughter effect while controlling for unobservable characteristics of both managers and firms.

Finally, we extend equation (2) to include controls for time-varying establishment characteristics, such as the workforce’s average education, the share of full-time workers, and establishment size.¹⁹ Since the regression includes manager-establishment fixed effects, coefficients on these controls are identified from within-establishment changes before and after the birth event. While some of these controls may themselves be endogenous, including them allows us to study changes in female employees’ outcomes conditional on broader shifts in workforce composition. For robustness, we always report results both with and without these controls.

3.1 Discussion on the Validity of the Identification Strategy

Our empirical strategy relies on two identifying assumptions. The first is that, conditional on a manager’s decision to have a child, the child’s gender is exogenous. This assumption holds in Denmark, where sex-selective abortion is illegal and parents cannot systematically influence the gender of their child. By contrast, prior studies exploiting cross-sectional variation in the gender mix of children - rather than within-individual variation from birth events, as in our design - require a stronger assumption: namely that, conditional on family size, the gender mix of children is random, i.e., parents do not follow a gender-based stopping rule for fertility. For consistency with this literature, and to further characterize our sample of managers, we implement the standard test for such a rule by examining whether having a first-born daughter predicts the total number of managers’ children (e.g., Washington 2008). Results, reported in Table A2 of the Appendix, show that while having a first-born daughter strongly predicts the total number of daughters, it does not affect the total number of children or the probability of having more than one or two children. This evidence indicates that managers are not following a gender-biased stopping rule, consistent with treating the number of daughters as random

¹⁹The full set of controls includes employees’ average age, average years of education, average tenure, the share working full-time, the share married, the share with children, and establishment size. We do not explicitly control for characteristics of the manager that are time-invariant (e.g., age at the birth event) or that evolve linearly over time, as these are absorbed by year and manager-establishment fixed effects.

once we condition on total family size.²⁰

To interpret our estimates as the effects of child’s gender on managers’ personnel decisions and within-firm gender inequality, we must also assume that the treatment does not directly affect managers’ capacity to influence personnel policies. This assumption could be violated if child’s gender affects the time managers spend in the establishment where the birth occurred, thereby limiting their involvement in the firm. To rule this out, we first test whether newborn gender influences the propensity to take paternity leave, as this could reduce managers’ presence and involvement in the establishment after a birth event. Table A3 shows that child’s gender has no effect on either the probability of taking leave (columns 1-4) or, among those who do take leave, the duration of that leave (columns 5-6).

We next test whether newborn gender affects the probability of a break in the manager–establishment spell in the years following the birth. For instance, if managers with sons were more likely to exit the firm where the birth occurred, this would reduce their scope to affect gender equality and other firm outcomes, thereby challenging our proposed interpretation. Figure 2 plots the “survival rate” of manager–establishment spells by newborn gender. The figure shows that the probability and timing of spell breaks do not differ by child’s gender.²¹ Because the differences between the two groups are never statistically significant, we can rule out differential attrition between treated and control managers as a driver of our results.²² Note that the absence of differences in the years preceding the birth further reassures us that spell formation is unrelated to the gender of the future child, consistent with parents’ inability to predict or select child gender.²³

4 Women’s Relative Earnings and Employment

Women’s relative labor outcomes improve when managers experience the birth of a daughter as opposed to a son. This is shown in Panel A of Table 2, which presents estimates of equation (2) for our two main outcomes: the Female Earnings Ratio (first two columns) and the Share of Female Employment (last two columns). All specifications include manager–establishment fixed effects, fixed-effects for the total number of managers’ children, and year fixed effects. We find that following the birth of a daughter the Female Earnings Ratio rises by almost 1 percentage point, or about 2.5% of the mean. Similarly, female employment increases by 2.3% in establishments where managers become fathers to a daughter. Controls vary between even and odd columns but, as the table shows, their inclusion does not affect the estimates.

²⁰Note that since our identifying assumption is that, conditional on a birth event, a child’s gender is exogenous, even fertility decisions based on the gender mix of previous children would not undermine causal identification. However, ruling out gender-biased stopping rules remains important for our analysis of mechanisms in Section 5, which partly relies on cross-sectional variation.

²¹Since we rely on the birth sample, by construction all spells last at least two years - the birth year and the preceding year. Thus, survival rates are mechanically equal to one in these periods.

²²We have also verified that this result holds across managers with different baseline characteristics, including education, age, number of existing children, experience, and whether they work in a goods-producing versus service industry.

²³Despite the exogeneity of child’s gender, we formally inspect for the absence of differential pre-trends between treated and control managers in Section 4.1, where we also discuss how we address the pitfalls of two-way fixed effects estimators.

We characterize these results along two dimensions. First, although we have shown that managers in our sample do not follow a gender-based fertility stopping rule - and our empirical strategy does not depend on this assumption - we replicate the analysis for the subsample of managers whose fertility decision by definition cannot be influenced by the gender of previous children, i.e., those experiencing the birth of their first child.²⁴ Restricting to first births also means that family size is automatically controlled for, since all managers in this group had no children prior to the birth event. This allows us to estimate effects for daughters and sons separately and to verify whether the improvement in women’s relative outcomes is driven by a positive effect of daughters rather than a negative effect of sons. Figure 1 shows that women’s relative earnings and employment rise significantly when the manager’s first-born is female, but remain unchanged when the first-born is male. This finding is consistent with Warner (1991)’s original notion of the daughter effect: daughters make men more sensitive to gender issues, whereas sons do not make men “antifeminist,” but rather render concerns about gender fairness less salient.

Second, while the estimates in Panel A pool all daughter births, we next separate the impact of a first daughter from that of an additional daughter, conditional on already having one. If, as argued by sociologists, daughters heighten men’s awareness of gender-related issues, the birth of a first daughter might affect fathers’ behavior more strongly than the birth of a subsequent one. To assess whether effects vary with parity, we replace the continuous variable *N Daughters* in Equation (2) with two dummies: *First Daughter*, which switches to one when the total number of daughters becomes positive, and *Additional Daughters*, which switches to one when it becomes greater than one.²⁵ This specification also ensures that managers who have their first daughter are matched to control managers not only on total fertility (through fixed effects for the number of children) but also on the gender composition of existing children up to that birth.²⁶ The results are shown in Panel B of Table 2 and indicate that the aggregate effects of Panel A are driven by managers who experienced the birth of their first daughter. Specifically, following the birth of the manager’s first daughter, female relative earnings and employment rise by 4.4% and 2.9% respectively. By contrast, while still positive, the coefficient of *Additional Daughters* is smaller and statistically insignificant. These findings suggest that the first female child is especially important in shifting managers’ behavior, consistent with the idea that saliency and awareness of gender issues may increase discontinuously after men experience the birth of their first daughter - a hypothesis we explore further in Section 5. Given this result, the remainder of the analysis focuses on the effects of a first daughter by relying on the specification reported in Panel B.

To help interpret the magnitudes of the first daughter effect, we benchmark them against evidence that female executives improve gender equality within firms. Prior studies document

²⁴We observe first births in 2,340 distinct manager–establishment spells, corresponding to 35% of the distinct spells in our birth sample.

²⁵The sample used to estimate the *First Daughter* coefficient is not identical to that in Figure 1, as a first daughter may be born later than the first child. In total, we observe 2,239 distinct manager–establishment spells in which a first daughter is born.

²⁶Specifically, the coefficient on *First Daughter* is identified by comparing two managers with the same number of children - all sons if the number is positive - who both experience the birth of an additional child: by chance, one has a daughter for the first time, while the other has (another) son.

a positive relationship between female-led companies and gender equality in earnings and promotions (e.g. Kunze and Miller 2017; Flabbi et al. 2019; Cullen and Perez-Truglia 2023). Therefore, using the full sample of single-manager establishments, we exploit cross-sectional variation in managers' gender to estimate the effect of having a female manager and compare it to the magnitude of the results in Panel B of Table 2. Table A4 shows that, consistent with the literature, female-led establishments exhibit better relative outcomes for women. Even after accounting for managers' sorting across narrowly defined sectors and controlling for average workforce characteristics, establishments with a female manager display significantly higher female relative earnings and employment.²⁷ Specifically, columns (2) and (4) indicate that the Female Earnings Ratio and the Share of Female Employment are about 4 and 5 percentage points higher in female-led than in comparable male-led establishments. Comparing these estimates with those in Panel B of Table 2, the effects of first daughters on male managers amount to between 20% and 50% of the impact of having a female manager.

Finally, as discussed in Section 2.3, when an establishment transitions from zero to at least one female employee, changes in the Female Earnings Ratio reflect both quantity and earnings effects. We therefore decompose the overall effect into an extensive margin, which captures women entering previously all-male establishments, and an intensive margin, which captures changes in relative female earnings among establishments that already employed at least one woman before the birth event. For the extensive margin, we re-estimate the specification in column (1) of Panel B of Table 2 on the full sample, replacing the dependent variable with an indicator for whether the establishment employs at least one woman: the coefficient on *First Daughter* gives $\Delta P_{\text{entry}} = 0.0127$. We weight it by $\bar{R}_{\text{entry}} = 0.398$, the average Female Earnings Ratio at the establishments that transition from all-male to mixed-gender. For the intensive margin, we return to the Female Earnings Ratio as the dependent variable but restrict the sample to establishments that already employed at least one woman before the birth event: the coefficient on *First Daughter* gives $\Delta R_{\text{intensive}} = 0.0158$. We weight it by $P_{\text{Female Pre}} = 0.751$, the share of establishment-year observations accounted for by such establishments. Putting these components together, the overall effect of the birth of a manager's first daughter on the Female Earnings Ratio can be expressed as:

$$\Delta R_{\text{overall}} \approx \Delta P_{\text{entry}} \times \bar{R}_{\text{entry}} + P_{\text{Female Pre}} \times \Delta R_{\text{intensive}}.$$

We obtain an extensive margin equal to 0.0051, and an intensive margin equal to 0.0119, summing to 0.017 - matching the overall coefficient of *First Daughter* reported in Panel B of Table 2. This decomposition suggests that approximately 70% of the effect of the birth of a manager's first daughter on the Female Earnings Ratio is driven by intensive-margin changes in relative female earnings, while the remaining 30% reflects entry effects at the extensive margin.

²⁷While controlling for workforce composition may be problematic given potential endogeneity, omitting these controls could bias the female manager coefficient upward given the cross-sectional nature of this comparison. This can be seen by contrasting odd and even columns in Table A4.

4.1 Event Study Analysis

In this section we present an event-study analysis, which serves several purposes. First, it allows us to test for differential pre-trends between treated and control units. Since child gender is exogenous, the birth of a daughter - rather than a son - should be unrelated to pre-birth outcome dynamics within the establishment, a hypothesis we can assess directly with the event study.²⁸ Second, the event-study allows us to trace when the daughter effect emerges and how it evolves following the birth; we use these dynamics in Section 5 to shed light on the underlying mechanisms. Third, it helps address limitations of two-way fixed effects estimators (Borusyak et al. 2024; De Chaisemartin and d’Haultfoeuille 2020; Goodman-Bacon 2021), which are especially pronounced in staggered treatment settings where “already-treated” units may serve as controls.²⁹ To address this, we define treated spells as those in which a manager experiences the birth of his first daughter, and control spells as those in which a manager - with the same number of children prior to the birth event - experiences the birth of a son but has no daughters, thereby isolating the “never-treated.”³⁰

Our event-study specification is given by:

$$Y_{me,jt} = \sum_{j \neq -1} [\alpha_j \text{event}_{me,j} + \beta_j (\text{event}_{me,j} \times \text{treated}_{me})] + C_{me,jt} + \chi_{me} + \phi_t + \epsilon_{me,jt} \quad (3)$$

where we set the year of the manager’s child’s birth as event time $j = 0$ and index all other years relative to it. The dependent variable $Y_{me,jt}$ denotes the outcome of interest for manager-establishment spell me in year t at event time j . The specification includes a full set of event-time indicators $\text{event}_{me,j}$ and their interactions with the treatment indicator treated_{me} , as well as calendar year fixed effects ϕ_t , fixed effects for the manager’s total number of children $C_{me,jt}$, and manager-establishment spell fixed effects χ_{me} . We let the event time j run from 3 years before to 5 years after the birth, omitting the year immediately preceding it ($j = -1$). This window was chosen to balance two objectives: ensuring sufficient observations in each event-time period to estimate dynamic treatment effects precisely - as the number of manager-establishment spells declines the further we move from the birth event (see Figure 2) - and covering a long enough horizon to capture post-birth adjustments in managers’ behavior.

Figure 3 plots the coefficients β_j , which capture the evolution of the difference in outcomes between treated and control establishments relative to the year before the birth, for both the Female Earnings Ratio and the Share of Female Employment. We find no evidence of differential pre-trends, as the coefficients in the pre-birth years are statistically indistinguishable from zero for both outcomes. The figure also shows that the positive effects of daughters emerge immediately after birth and persist over the subsequent years that the manager remains in the

²⁸Consistent with this, Figure 2 shows that the timing of job-spell creation is orthogonal to newborn gender.

²⁹In particular, while our setting offers a natural control group - managers who experience the birth of a son - some of these managers may already have daughters and can therefore be considered already treated. In the presence of dynamic treatment effects, this could bias, likely downward, our estimates.

³⁰Note that the identification of the *First Daughter* coefficient in the baseline specification also relies on comparisons with a control group of never-treated managers. Thus, as in the estimation of the *First Daughter* effect, our event-study design ensures comparability between treated and control managers in both the number and gender composition of their children prior to the analyzed birth event.

firm.³¹

We assess the robustness of these results along three dimensions. First, we extend the event-study window from $j = -4$ to $j = +8$ around the birth event. Although widening the window can reduce precision - since fewer manager-establishment spells are observed in the earliest and latest periods - it provides a stronger test of the parallel-trends assumption and allows us to examine whether the daughter effect persists over a longer horizon.³² Appendix Figure A5 confirms both parallel pre-trends and the persistence of the daughter effect throughout the extended post-birth horizon. Second, we re-estimate the event study using a specification that more closely follows that of Panel B of Table 2. As before, we define as treated those manager-establishment spells in which a manager experiences the birth of a first daughter, but we broaden the control group to include not only managers with no daughters who experience the birth of a son (the “never treated”) but also managers who already have at least one daughter and experience an additional birth (the “already treated”). The results, shown in Figure A3, are qualitatively similar but generally smaller in magnitude than those in Figure 3, where the control group is restricted to the never treated. This pattern suggests that concerns about dynamic treatment effects are limited and, if anything, bias our results downward. Third, to further validate this interpretation and address any residual concern related to the presence of staggered treatment timing, we re-estimate the event-study using the estimator proposed by De Chaisemartin and d’Haultfoeuille (2020). Results, shown in Figure A4, are qualitatively and quantitatively similar to the event-study estimates obtained in Figure 3.

5 The Timing of the Effects and Plausible Mechanisms

This study builds on research showing that daughters positively affect men’s gender attitudes (Warner, 1991; Warner and Steel, 1999; Shafer and Malhotra, 2011; Borrell-Porta et al., 2018). Yet the mechanisms through which these shifts translate into the behavioral changes documented in the previous section remain unclear. One possible channel is gradual learning: exposure to daughters may reduce men’s knowledge gaps, if any, about women’s abilities and the constraints they face. For example, male managers could learn about women’s ability through their daughters’ accomplishments at school or gain insights into gender differences in opportunities and barriers in the labor market from their daughters’ professional experiences.³³ If effects operate mainly through gradual, exposure-driven updating of beliefs, changes in managers’ personnel decisions should emerge only some time after the birth and strengthen as daughters grow older.

³¹This result differs from Wu et al. (2023), whose limited sample of birth events yields imprecise within-manager estimates in their event-study design. Specifically, their analysis relies on a subsample of only about 100 births - roughly 50 daughters and 50 sons - naturally producing statistically insignificant estimates with very wide confidence intervals.

³²Importantly, Section 3.1 shows that even under this extended window there is no differential attrition between managers who had a daughter and those who had a son.

³³In line with this idea, Dahl et al. (2021) show that men serving in military squads with female recruits revise their views about mixed-gender teams and adopt more egalitarian attitudes. Similarly, Bursztyn et al. (2024) find that prolonged exposure to Arab Muslims improves natives’ attitudes and knowledge about this immigrant group.

By contrast, finding that firm-level gender equality improves shortly after the birth of the manager’s first daughter would suggest that our results cannot be explained solely by gradual learning. Such rapid responses would instead be consistent with a discontinuous increase in the salience of gender issues triggered by the birth event, accompanied by a shift in attitudes toward women.³⁴ The literature points to two main channels through which this may occur. First, the birth of a daughter may heighten fathers’ awareness of gender inequality, thereby shaping their *beliefs*. This view aligns with models of selective attention and learning through noticing (e.g. Schwartzstein, 2014; Hanna et al., 2014; Bordalo et al., 2017) and with evidence by Shafer and Malhotra (2011) and Borrell-Porta et al. (2018), which show that men revise their perceptions of gender roles shortly after their daughter’s birth. Second, the birth of a daughter can make men feel closer to women, thus affecting their *preferences*. This interpretation is consistent with Oswald and Powdthavee (2010) and Cronqvist and Yu (2017), who find that men with daughters identify more closely with women when forming preferences, in line with models of group identity and social preferences (e.g. Chen and Li, 2009; Klor and Shayo, 2010).

While registry data cannot reveal changes in managers’ beliefs or preferences, their longitudinal nature lets us examine the timing of managers’ behavioral responses. Specifically, by examining *when* the daughter effect emerges - how soon after the birth - and *how it evolves* over time - particularly in relation to the daughter’s age - we can assess whether our findings are more consistent with a rapid salience effect or with gradual learning from daughters’ life experiences.

The event-study evidence in Section 4.1 provides a first indication of the underlying mechanism. Because the daughter effect emerges immediately after birth and persists for at least eight years, prolonged exposure to daughters’ life experiences is not necessary for managers to change their behavior toward female employees.³⁵ Instead, the evidence is consistent with the birth of a first daughter triggering a discontinuous increase in the salience of gender issues. However, the event study covers a relatively limited horizon and therefore cannot tell us whether sustained exposure to daughters subsequently reinforces this initial shift. To examine how the daughter effect evolves over a longer horizon, we turn to cross-sectional evidence from the full sample of single-manager establishments. Specifically, we compare managers whose oldest daughters fall into different age groups, while controlling for both manager and firm characteristics by estimating:

$$Y_{et} = \alpha + \beta_1 D_{et}^{0-5} + \beta_2 D_{et}^{6-15} + \beta_3 D_{et}^{16-21} + \beta_4 D_{et}^{22+} + C_{et} + MGR'_{et} \delta + WORK'_{et} \gamma + \eta_j + \lambda_l + \phi_t + \epsilon_{et} \quad (4)$$

where D^{a-b} are dummies equal to one if the manager’s oldest daughter is between a and b years old, C_{et} are fixed effects for the manager’s total number of children, $WORK$ is a vector of workforce characteristics at the establishment–year level, and MGR is a vector of manager characteristics.³⁶ Importantly, to account for the correlation between daughters’ age

³⁴Andries et al. (2025) provide related evidence, showing that salient events can heighten perceived similarity between groups and elicit greater empathetic responses.

³⁵Note that this result does not rule out belief-based mechanisms entirely - only those in which a change in beliefs depends on extended exposure to daughters’ life experiences.

³⁶Daughters’ age groups are defined to match the Danish education system: 0–5 (pre-school), 6–15 (compul-

and managers' age, we include manager age decile dummies in the vector of controls MGR .³⁷ Finally, we include fixed effects for industry (5-digit level), municipality, and year.

Figure 4 plots the β coefficients from equation (4), which allow us to identify the relationship between the age of the manager's oldest daughter and female employees' outcomes at the firm-level. The estimates remain positive at all ages and grow stronger as daughters reach late adolescence and adulthood, suggesting that the initial shift in managers' behavior triggered by the birth of a daughter may be reinforced over time as fathers gain further exposure to their daughters' experiences.³⁸ In particular, the fact that effects increase around critical junctures in daughters' lives (e.g. transitions across education levels or entry into the labor market) is consistent with the hypothesis that gradual exposure helps reduce managers' informational asymmetries about women's abilities and constraints.

The immediate and persistent pattern of effects we document provides two novel insights into the literature on the daughter effect. First, and of particular relevance for policy, the rapid onset of the effects shows that managers' behavior toward gender equality within firms can shift quickly, without requiring prolonged exposure to gender-related issues. Second, the timing also argues against a self-interest mechanism - namely, that managers with daughters improve women's outcomes because their own daughters bear a personal cost associated with gender inequality. Under such a mechanism, we would expect managers' behavior to change only if or when their actions could directly benefit their daughters. Evidence of this self-interest channel has been documented by McGuirk et al. (2023) in the context of voting on war and peace in the U.S. Congress. They show that legislators with draft-age sons become less supportive of conscription, but that this effect vanishes once their sons age out of eligibility - consistent with child gender affecting fathers' behavior through personal motives rather than by triggering an ideological shift. Two factors make such an explanation unlikely in our setting. First, the daughter effect emerges when daughters are still infants - when self-interest motives are least plausible - and persists thereafter. Second, we study a context where the actions of a single manager have little influence on aggregate levels of gender inequality, further reducing the likelihood that private incentives explain the observed behavioral change. Instead, the rapid and persistent nature of the effects suggests that the birth of a daughter acts as a catalyst for a lasting change in how managers perceive the social costs associated with gender inequality, over and above any personal interest. Importantly from a policy perspective, this implies that changes in managerial behavior toward women need not depend on private incentives and could therefore arise across a broad set of managers.

sory schooling), 16–21 (upper secondary/university), and 22+ (post-education/working age).

³⁷Specifically, we include dummies for the following age deciles: younger than 34, 34–38, 39–42, 43–45, 46–48, 49–51, 52–53, 54–57, 58–61, older than 61. Results are unchanged if instead we control for the age of the oldest child using the same bins as in equation 4 (see Appendix Figure A6).

³⁸Our finding of significant effects even for young daughters contrasts with the cross-sectional results in Wu et al. (2023), who detect effects only when daughters reach late adolescence. Two key differences of their design could explain this divergence. First, they restrict the analysis to founders with only one child (about 15% of their sample), raising concerns about selection bias due to the endogeneity of fertility choices. Second, they do not control for managers' age: as a result, managers with young daughters (e.g. under six) are systematically compared to much older managers with sons (average age around 50). This confounds child gender with parental age, making their null effects for young daughters difficult to interpret.

6 Margins of Adjustment and Firm Performance

We now turn to the specific dimensions of impact. Ex ante, the birth of a daughter could influence managers' personnel decisions in different ways. On one hand, treated managers may become more supportive of family-friendly policies that improve work-life balance, leading to broader changes in workforce composition - for example, an increase in the share of women (and possibly men) working part-time or with young children. On the other hand, the birth of a first daughter may prompt managers to raise female representation in the workplace without restructuring the overall labor force. This would require substituting men with women of comparable characteristics, thereby increasing women's presence among full-time employees or in high-paying positions. To distinguish between these hypotheses, Section 6.1 examines how the birth of a manager's first daughter affects the average characteristics of the female workforce and, in turn, the composition of the overall labor force. Sections 6.2 and 6.3 then assess the relative role of hiring, retention, and promotion in driving these changes. Finally, Section 6.4 evaluates whether the observed shifts in managers' hiring and promotion decisions translate into differences in firm performance.

6.1 Changes in Workforce Composition

Table 3 examines whether daughters affect the average characteristics of the female workforce within the firm. Columns (1)–(3) show that women's representation among high earners increases. In particular, column (1) indicates that, following the birth of the manager's first daughter, the probability that the highest-paid employee in the establishment is a woman rises by 8%. Columns (2) and (3) show similar increases in the share of women with earnings above the firm-level 90th percentile and among the top three earners, respectively.

To test whether daughters induce broader family-friendly adjustments, we next examine the daughter effect on the representation of women working full-time versus part-time or having pre-school aged children. Column (4) shows a 2.2% increase in the share of female employees working full-time, but columns (5) and (6) indicate no effect on the share of women with young children or employed part-time. Finally, column (7) shows a 5.9% increase in the share of women with post-secondary education, while column (8) shows no change in the proportion with lower levels of education.³⁹ Overall, these results suggest that establishments led by treated managers see greater representation of women in higher-paying full-time positions, where they are typically underrepresented (see Table 1). By contrast, we find no support for the hypothesis that daughters lead managers to implement family-work balance policies such as more flexible work arrangements or additional amenities for women with very young children.

We then ask whether these adjustments translate into broader changes in overall workforce composition. Table 4 considers whether the birth of a manager's first daughter affects average worker age or tenure, the share employed full-time, or the shares with young children or with

³⁹We define post-secondary education as either post-secondary vocational education or university, as this classification most closely corresponds to the broad 'college' category in the U.S., which typically includes both community college and four-year college degrees.

post-secondary education.⁴⁰ In contrast to Table 3, columns (1)–(5) show no effect of newborn gender on the composition of the overall workforce. Importantly, column (6) indicates no changes also in average labor costs per employee, suggesting that managers are not substituting male workers with comparable female workers who are paid less. Taken together, the results of Tables 3 and 4 indicate that improvements in women’s relative outcomes are not driven by a general restructuring of the workforce. Instead, treated managers appear to *substitute* male employees with observably similar women who work full-time and occupy higher-paying positions within the firm. Consequently, the only workforce characteristic that systematically changes following the birth of a daughter is its gender composition.

The substitution between male and female workers - and the resulting rise in women’s representation among full-time and high-pay positions - can operate through two main margins. First, it may reflect changes in the promotion of *incumbents*, i.e., employees already at the firm before the birth event. For example, following the birth of a first daughter, treated managers may reassess male and female incumbents and become more likely to award higher pay to women, increasing their promotion rates relative to men. Second, the effect may run through adjustments in hiring and separations. Treated managers may increase their propensity to hire women into full-time, high-paying jobs, or newborn gender may differentially affect the likelihood that male versus female employees leave the firm. We examine these margins of adjustment in the next sections.

6.2 Hiring and Separations

Panel A of Table 5 examines whether newborn gender affects managers’ hiring and separation practices. We find that the birth of a first daughter has a positive effect on a manager’s propensity to hire women, as the proportion of female hires over total hires increases by 4.5%, while the effect on female separations is negative but imprecisely estimated.⁴¹

The observed increase in the share of women hired can come from two margins. On the one hand, treated managers might want to increase women’s representation among hires but, at the same time, keep constant the number of men hired. If this is the case, then we should observe treated managers hiring more workers overall. On the other hand, daughters might lead managers to substitute male hires with female candidates, without any effect on the total number of hires. Panel B of Table 5 supports the substitution mechanism, as treated establishments do not exhibit higher overall hiring (or separation) rates than control establishments. Taken together, these results suggest that the increase in female employment documented in our baseline results shown in Table 2 is primarily driven by a shift in the gender mix of new hires, with separation patterns largely unaffected.

We next investigate to what extent the change in hiring patterns can account for the

⁴⁰In Table 3, outcomes are defined as the number of female workers with a given characteristic divided by the total number of employees in the firm. For example, we can compute effects on both the share of full-time and part-time female workers, since the two do not sum to one. By contrast, in Table 4 these two shares sum to one, so we report results only for the share of full-time workers.

⁴¹Figure A7 in the Appendix plots the coefficients obtained from an event study analysis of these two outcomes and shows that the increase in female hires is not a one-off adjustment, as the coefficients remain consistently positive over time.

observed increase in the share of female workers who are high earners, work full-time, and have post-secondary education. We do so in two steps. First, in Table 6 we replicate the analysis of Table 3, but focus on female hires rather than the total stock of female workers. This allows us to compare the coefficients across the two tables and assess, for instance, how much of the overall rise in the share of full-time female workers is driven by an increase in the share of new hires who are both female and employed full-time. The results show that the hiring margin is central to explaining the increase in women’s representation among top earners, full-time employees, and workers with post-secondary education. In fact, the estimated magnitudes in Table 6 account for roughly 60–80% of the corresponding effects in Table 3.⁴²

6.3 Promotions

We capture the effect of daughters on managers’ promotion decisions by comparing within-firm changes in salary growth around a birth event, separately for male and female workers, and depending on the newborn gender. The first two columns of Table 7 show no significant effect of a manager’s first daughter on the earnings of either female or male incumbents. However, these estimates reflect average effects across all incumbents and may mask substantial heterogeneity. For instance, treated managers may only act on the promotion margin when the pool of female incumbents includes candidates suited for higher-responsibility roles - such as women working full-time, with higher levels of education, or employed in more skilled occupations. In this case, averaging across all incumbents could attenuate the estimated impact if the effects are concentrated among a subset of “promotable” employees.

To examine this possibility, we replicate the analysis restricting attention to incumbents more likely to be considered for promotion. Specifically, we define as “promotable” those employees who, at baseline, were under 40 - that is, typically younger than their managers and thus plausibly viewed as having stronger career prospects - and who met at least one of the following criteria: they were employed full-time (columns 3–4), had higher educational attainment (columns 5–6), or were not employed in low-skill occupations (columns 7–8).

All columns paint a consistent picture: the estimated effects for subgroups of women more likely to be considered for promotion are larger and statistically significant. In contrast, the results for men remain small and insignificant across all subsamples. Taken together, these findings suggest that when the pool of female employees includes candidates viewed as promotable, treated managers are more likely than control managers to promote them - consistent with the broader evidence on adjustment margins presented in previous sections.

6.4 Firm Performance

We now turn to examine whether the improvement in women’s relative outcomes translates into changes in firm performance, as measured by sales, employment, and value added per employee. Since registry data on worker characteristics are available at the establishment level but balance sheet data only at the firm level, we restrict the analysis to single-establishment

⁴²Appendix Table A5 conducts a similar exercise for female separations and shows no significant effect of child’s gender.

firms with non-missing information on our performance measures. This subsample covers about 75% of the observations used in the main analysis.

Despite finding that the birth of a manager’s first daughter improves women’s relative outcomes, we detect no significant effects on firm performance, as shown in Table 8. While the coefficients are imprecisely estimated, they are close to zero for both total employment and sales, while for value added per employee, the point estimate corresponds to a small increase of about 3,400 Danish kroner ($\approx$ 455 euros or 517 U.S. dollars at July 2026 exchange rates).⁴³ For comparison, Sauvagnat and Schivardi (2024) report that value added per employee changes by about 10,000 euros following a CEO turnover - an effect several times larger than ours.

The lack of an effect on firm performance is consistent with our evidence on the margins of adjustment, which shows that, within the range of our estimated effects, treated managers can alter the gender mix of the workforce without changing other workforce characteristics. This indicates that managers who begin hiring or promoting more women after the birth of a daughter can do so without compromising workers’ quality, implying no equity–efficiency trade-off. It also suggests that when these same managers previously favored men, their bias did not impose a performance penalty on the firm. In other words, pre-existing gender preferences were not substantially distorting the quality of personnel decisions. Intuitively, in such cases, one would not expect large performance losses from biased decisions - nor large gains from correcting them. Our results are therefore consistent with managers’ gender bias operating “at the margin”, influencing evaluations of equally qualified male and female candidates, with little consequence for short-run firm output.

Importantly, the absence of an effect on firm performance does not mean that the presence of such “subtle bias” is inconsequential. On the contrary, two recent theoretical papers show that even small biases in marginal decisions - such as tie-breaking between equally qualified candidates - can have important implications for aggregate productivity by distorting incentives and investment decisions. Pikulina and Ferreira (2024) develop a model of *subtle discrimination*, where bias in favor of one group over another operates only when it does not compromise the quality of the selected candidate. Correcting such bias therefore has no direct *ex-post* effect on firm performance, but can have important *ex-ante* consequences by shaping career incentives and investment decisions among the disfavored group. Anticipating they are less likely to advance, disfavored individuals may underinvest in skills, leading to persistent achievement gaps even when groups are ex ante identical in ability. Similarly, Siniscalchi and Veronesi (2021) show that evaluators’ tendency to favor candidates who resemble themselves over equally qualified others discourages participation by the disfavored group, resulting in lost talent. These models align with our empirical results and help clarify why subtle forms of managerial bias may leave short-run performance unaffected, yet still carry significant implications for aggregate productivity.⁴⁴

⁴³Once we consider the confidence intervals around this estimate, we can rule out with 90% probability an economically meaningful effect, either negative (larger than -8,617 kroner, or approximately -1,152 euros / -1,311 U.S. dollars) or positive (larger than 15,499 kroner, or 2,072 euros / 2,357 U.S. dollars).

⁴⁴Specifically, these models predict that reducing subtle bias can generate aggregate gains in the long-run, as women adjust their expectations and investment decisions once they no longer anticipate being disadvantaged. The shock to gender attitudes we study is not designed to capture such broader, longer-term consequences of

Beyond complementing recent theoretical work, our findings also resonate with the empirical evidence of Huber et al. (2021), which provides a useful benchmark for interpreting our results. Studying discrimination against Jewish managers in Nazi Germany, they show that while the expulsion of Jewish top managers reduced firm performance on average, this effect was entirely driven by firms where managers had hard-to-replace characteristics, such as advanced degrees or key business connections. Where such traits were absent, performance was unaffected. In other words, discrimination can appear payoff-irrelevant even when directed at CEOs - the single most important employee for firm outcomes - and when exclusionary practices are clearly unrelated to productivity. In our setting, the bias we study is not state-mandated, is less overt, and affects lower-level employees who are arguably easier to replace than CEOs. Against this backdrop, the absence of short-run performance effects in our case is perhaps not surprising - even though the behavioral shift we document leads to meaningful improvements in women’s relative outcomes and, as discussed above, may have important implications for aggregate productivity.

Other explanations can also rationalize the lack of an effect on firm performance. One possibility is that the effects of correcting bias may involve opposing forces: undoing discrimination could lead to hiring women who, despite being similar in observable characteristics, are more qualified along unobserved dimensions, marginally improving workforce quality. At the same time, these gains may be offset by short-term frictions from changes in team composition. Indeed, prior work shows that even qualified new hires can disrupt coordination, particularly in teams selected under different preferences (Jaravel et al., 2018; Chen, 2021; Bartel et al., 2014). Such frictions may be especially relevant in our setting, where hiring women into roles previously held by men increases workforce diversity which, in turn, could reduce performance by raising coordination costs (Lazear, 1999; Prat, 2002; Ronchi and Salvestrini, 2026). These dynamics can dampen the gains from reducing bias in the short-run, but they may ease over a longer horizon than the one we study, eventually allowing improvements in gender equality to translate into positive effects on firm performance.

A second possibility is that changes in managerial gender attitudes do not extend to the broader workforce. Most firms in our data are male-dominated, and existing employees may not share the manager’s updated views and retain biased attitudes. In such a case, newly hired or promoted women may face constraints that limit their full contribution to firm performance (Gagliarducci and Paserman, 2012; Karpowitz et al., 2024; Gagnon et al., 2025). These organizational dynamics may attenuate any productivity gains from improved hiring or promotion decisions and help explain the absence of measurable effects on firm performance in the short-run. Over a longer horizon, however, such cultural changes may gradually take hold, ultimately allowing improvements in gender equality to translate into positive effects on firm performance. Overall, while we cannot fully distinguish among alternative explanations by tracing all potential offsetting forces or longer-run adjustments, our findings provide evidence that in the short-run shifts in managers’ gender attitudes improve women’s relative outcomes without harming firm performance.

correcting these biases.

7 Evolution and Persistence of the Daughter Effect

This section is organized in two parts. Section 7.1 examines how the daughter effect evolves over time, exploring the potential influence of broader social and institutional changes. Section 7.2 tests whether child gender influences the types of firms managers are employed at over the medium to long run - especially with respect to their female workforce composition - and discusses the implications for managers' endogenous sorting across firms.

7.1 Evolution of the Daughter Effect Over Time

As discussed in Section 2.1, our sample period, which spans from the early 1990s to the late 2010s, was marked by changes in gender norms and in the labor market frictions faced by women. In Denmark, gender gaps in labor force participation and wages declined steadily over this period (Appendix Figure A9). At the same time, data from the European Values Survey (Appendix Figure A10) show a marked decline in the share of the Danish population endorsing traditional views about women's roles in the household and the labor market.⁴⁵

We exploit the long time span of our data to examine whether these broader social and labor market trends shape the evolution of the daughter effect. We split the data in two ways. First, we capture cohort differences by dividing manager-establishment spells according to whether the manager was born before or after 1965, the median birth year in our sample. This allows us to test whether male managers born in the latter half of the century - exposed to more progressive gender norms and lower labor market barriers for women - are more likely to adjust their personnel decisions following the birth of a daughter than those born earlier. Second, we compare birth events in the 1990s with those taking place later in the sample period, using 2002 as a cutoff year to align with a major parental leave reform introduced in January of that year. As paternity leave gradually expanded from that year onward, this split provides a relevant institutional backdrop for interpreting our findings, particularly in light of recent work showing that even modest extensions of paternity leave can foster more progressive gender norms and reduce gender inequality within households (González and Zoabi, 2021; Farré et al., 2023).⁴⁶

Results, reported in Table 9, suggest that the daughter effect has strengthened over time. Panels A and B show that estimates are larger - often about twice as large - for managers born after 1964 and for birth events after 2001. While not all coefficients are statistically distinguishable across groups, the overall pattern across outcomes and subsamples points to a strengthening of the effect over time. Two potential, and likely interconnected, explanations

⁴⁵Specifically, the share of respondents rejecting four traditional gender statements - (i) Men should have more right to a job than women; (ii) A job is the best way for a woman to be independent; (iii) What most women really want is a home and children; and (iv) A pre-school child is likely to suffer if his or her mother works - increased from just above 40% in 1990 to more than 80% by 2017.

⁴⁶The 2002 reform extended the duration of well-paid parental leave by 22 weeks and gave couples more flexibility in how it could be allocated. While evidence shows that the immediate effects were concentrated among mothers (Lassen, 2021; Gallen, 2019; Beuchert et al., 2016), paternity leave uptake also increased more gradually, going from about 1.8 weeks in 2002 to nearly 4.5 weeks by 2017, as shown in Appendix Figure A8. Correspondingly, the average duration of paternity leave taken after 2002 was substantially longer than in the pre-reform period. For comparison, the Spanish reform studied by Farré et al. (2023), which is shown to foster more progressive gender norms, granted fathers 13 fully paid days (in addition to the 2 days already available).

may underlie these patterns. First, they may reflect the evolution in gender norms documented in Appendix Figure A10: as societal support for gender equality expanded - and as fathers' attitudes might have been shaped by broader policy shifts such as the 2002 parental leave reform - managers born in the latter half of the 20th century, or those experiencing a birth in more recent years, may have developed different views on gender roles and different aspirations for their daughters than earlier cohorts. This could make their personnel policies toward female employees more responsive to the birth of a daughter. Second, the larger effects observed among younger managers and in more recent years may reflect declining barriers for women in the labor market, as documented in Appendix Figure A9. In particular, a labor market characterized by greater gender equality may better enable managers to translate shifts in attitudes into changes in hiring and promotion practices, including in higher-paying positions.

Warner and Steel (1999) already highlighted these two forces - shifting gender norms and changing labor market conditions - as central to understanding how the influence of daughters may evolve over time. They argue that while parents have always wanted the best for their children, what is considered "best" is socially constructed and shaped by prevailing norms. Several decades ago, parents might have hoped their daughters would secure stability through marriage to a financially reliable spouse. In today's context - marked by stronger support for women's employment and gender equity - parents increasingly view educational attainment and professional fulfillment as aspirations for daughters as well as sons. Against this backdrop, the birth of a daughter may prompt male managers - particularly those raised or becoming fathers in this new environment - to adopt workplace practices that promote greater gender equality. While we cannot fully disentangle the relative importance of shifting norms and reduced supply-side constraints - especially as these forces evolve together - the results in Table 9 can offer insights into how broader social and institutional changes can shape the daughter effect.

7.2 Persistence Through Managers' Sorting Across Firms

So far, our analysis has focused on changes within the establishment where the manager experiences the birth. We now ask whether the daughter effect persists beyond that manager-establishment spell by shaping the types of firms managers subsequently choose to work for. In particular, we examine whether managers who have their first daughter differ from those who have an additional son but no daughters in the gender equality of the firms they subsequently join. If, as suggested in Section 5, daughters permanently shift fathers' perceptions of the social costs associated with gender inequality, then treated managers may be more likely to select into firms offering better opportunities for women after the break of the spell in which the birth event occurred.

To isolate this mechanism, we analyze job transitions following the spell break. We follow managers into their next establishment and compare gender equality at the destination firm measured the year before the manager's arrival, depending on treatment status. By examining firm characteristics measured before the arrival of treated and control managers, we can net out any influence they may have had on gender equality at the destination firm and test whether managers who experienced the birth of their first daughter are more likely - after the break of

the job spell - to sort into firms that were already more female-friendly prior to their arrival.

Figure 5 shows that treated managers sort into firms with different outcomes for female employees. Specifically, these firms employ a higher share of women working part-time and exhibit higher female relative earnings - particularly in terms of women’s representation among the top decile of the earnings distribution. Since such outcomes have been used in prior work as proxies for a workplace’s female-friendliness (e.g. Kleven et al. 2019), this evidence suggests that treated managers are more likely to move into firms offering better opportunities for women. By highlighting that daughters can shape the type of workplace fathers subsequently choose, these results caution against interpreting cross-sectional correlations between managers’ family composition and firm outcomes as purely reflecting changes in personnel practices. They also reinforce our interpretation that the daughter effect captures a persistent shift in managers’ perceptions of the social costs of gender inequality, which can influence both how they act within firms and where they choose to work.

8 Conclusion

Despite decades of progress, large gender gaps in career progression persist in the corporate world. This paper highlights one mechanism that can help reduce such gaps: shifts in male managers’ gender attitudes. Building on evidence that fathering daughters shapes men’s views toward women, we extend this idea to the workplace. Using unique features of Danish registry data, we exploit the birth of a daughter - rather than a son - as a plausibly exogenous shock to male managers’ gender attitudes, and compare within-firm changes in women’s relative earnings and employment by newborn gender. By identifying changes within manager–firm spells, we overcome the key empirical challenge of the endogenous sorting of managers across firms.

We find that women’s relative outcomes improve in establishments where male managers experience the birth of an additional daughter, compared to a son. The effect is concentrated among managers experiencing the birth of their first daughter: following the birth of a first daughter, the Female Earnings Ratio rises by 4.4% and the Share of Female Employment by 2.9%. Examining the underlying personnel decisions, we show that these changes are driven primarily by hiring: after the birth of a first daughter, managers are more likely to recruit women, especially those who are highly educated, work full-time, and hold higher-paying positions. In addition, managers become more likely to promote women, provided the pool of incumbents includes female employees who are plausibly better suited for higher-responsibility roles. Importantly, these changes do not alter other workforce characteristics or average labor costs per employee. Taken together, the evidence indicates that managers substitute male workers with comparably qualified women, leaving the gender mix as the only observable workforce characteristic affected by the daughter effect. Finally, we find small and insignificant effects on firm performance, indicating that, within the range of our estimated effects, managers can promote gender equality at no additional cost to the firm.

We analyze the timing of the effects to identify the mechanisms through which daughters influence men’s behavior. Using an event study analysis, we show that the positive effects on female earnings and employment appear immediately after the birth and persist for the

full duration of the job spell. This pattern suggests that neither self-interest motives - such as managers with daughters acting to reduce gender gaps because their own children bear the cost of inequality - nor prolonged exposure to gender issues are required for managers to change their behavior toward female employees. Instead, the evidence points to a rapid and lasting increase in the salience of gender inequality following the birth of a daughter, accompanied by a shift in attitudes toward women. Looking beyond the birth spell, managers with daughters are also more likely to move into female-friendly firms, consistent with a persistent change in their perception of the social costs of gender inequality.

Given the central role of managers in the labor market and the over-representation of men in managerial positions, showing that male managers' gender attitudes shape personnel outcomes is of significant policy relevance. Prior research on gender board quotas (Bertrand et al., 2019; Maida and Weber, 2022) shows that increasing women's representation alone may not suffice to close gender gaps. Policies aimed at shifting managers' perceptions of the social costs of gender inequality may therefore be necessary. Our findings support this view: personnel practices and firm-level gender inequality respond to shifts in managers' gender attitudes. Importantly, these attitudes can change rapidly in response to personal experiences, even without self-interested motives or prolonged exposure to gender-related issues, suggesting that a broad set of managers may be receptive to such shifts. Finally, our design rules out the possibility that improved outcomes for female employees are driven solely by managers sorting into more gender-equal firms. This matters for policy: if the effect operated only through sorting, shifts in managerial attitudes would have limited potential to drive change from within firms. Instead, by showing that managers with daughters actively make different personnel choices, we highlight the potential for shifts in decision-makers' attitudes to directly foster workplace gender equality. Given the substantial resources firms spend - often ineffectively - on diversity training (Bohnet, 2016), future research could explore which interventions best replicate the daughter effect on managerial decision-making and the implications for workplace inequality and firm performance.

Data Availability Statement

The data and code underlying this article are available in the replication package at <https://doi.org/10.5281/zenodo.21562174>. The analysis uses confidential administrative data from Statistics Denmark (Statistics Denmark, 2020), which the authors are not permitted to redistribute. Researchers may apply for access through Statistics Denmark’s Research Services (<https://www.dst.dk/en/TilSalg/Forskningsservice>). The replication package contains all programs used to construct the analysis data and to produce the results reported in the paper, together with the publicly available data used for selected appendix figures (OECD, 2025a,b,c; EVS, 2022; Statistics Denmark, 2025, various years).

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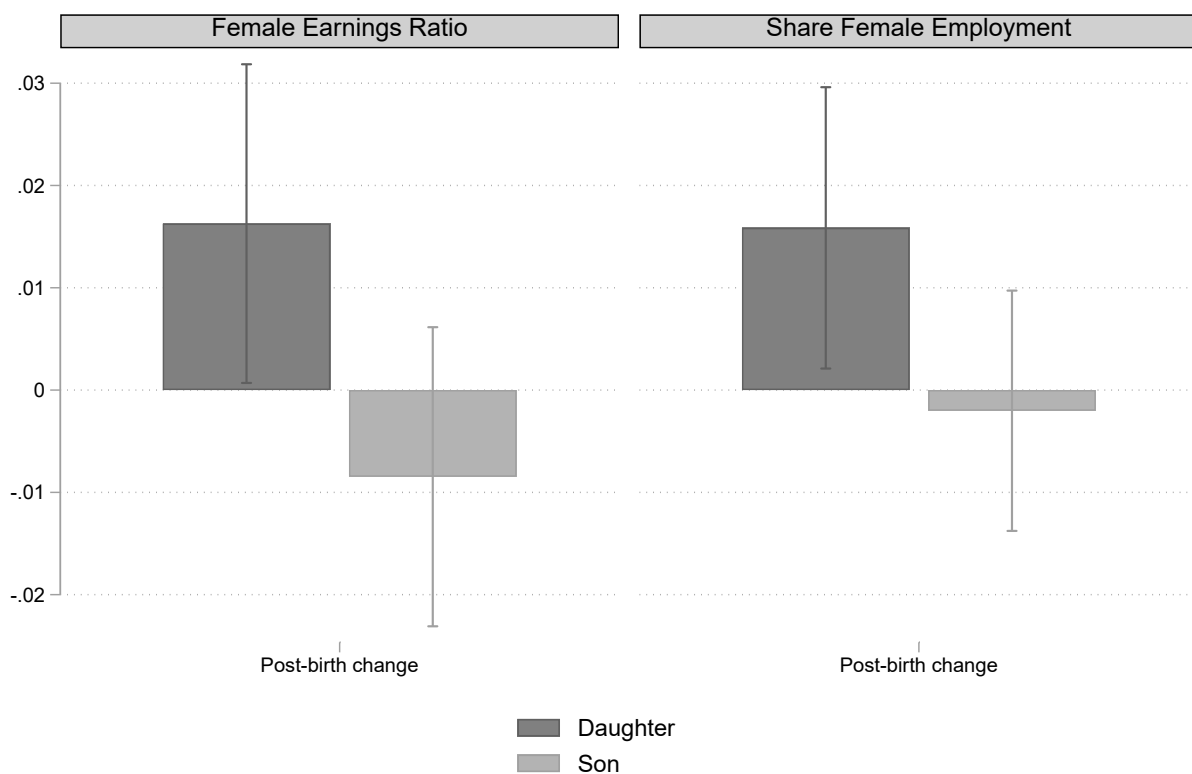
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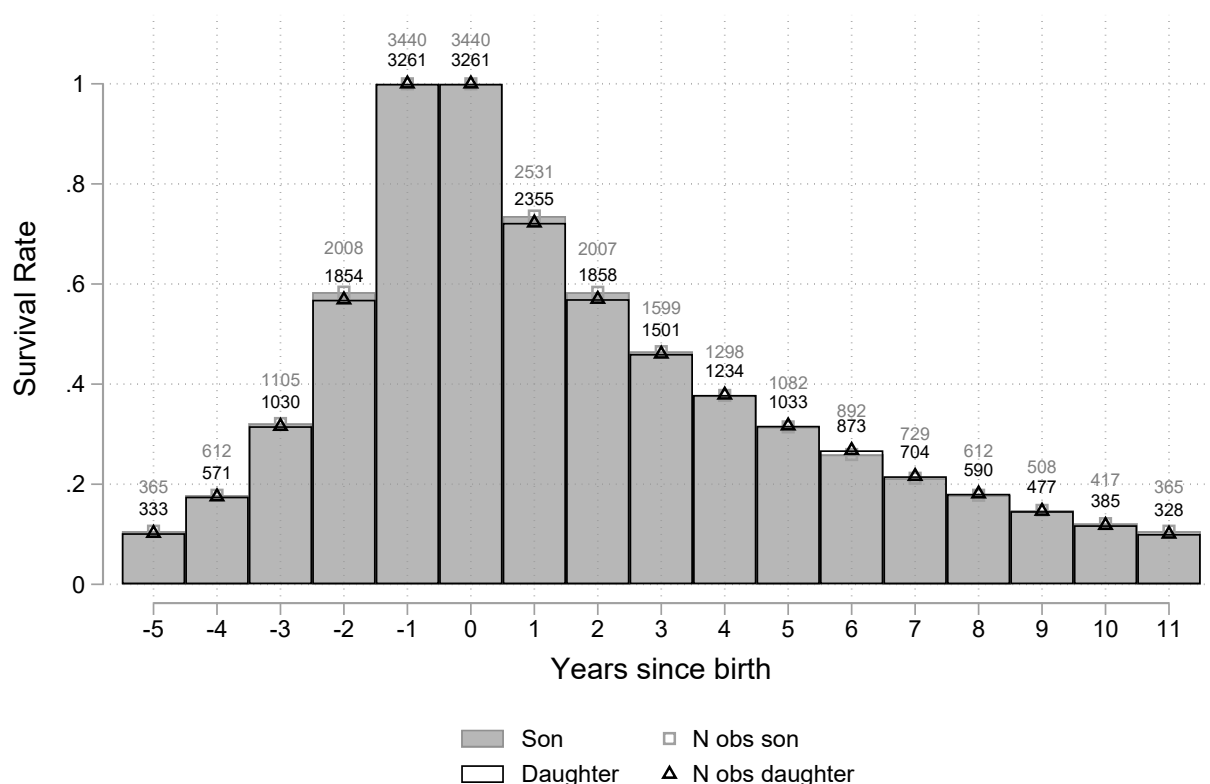
Figures

Figure 1: Relative daughter effect: sub-sample of first births



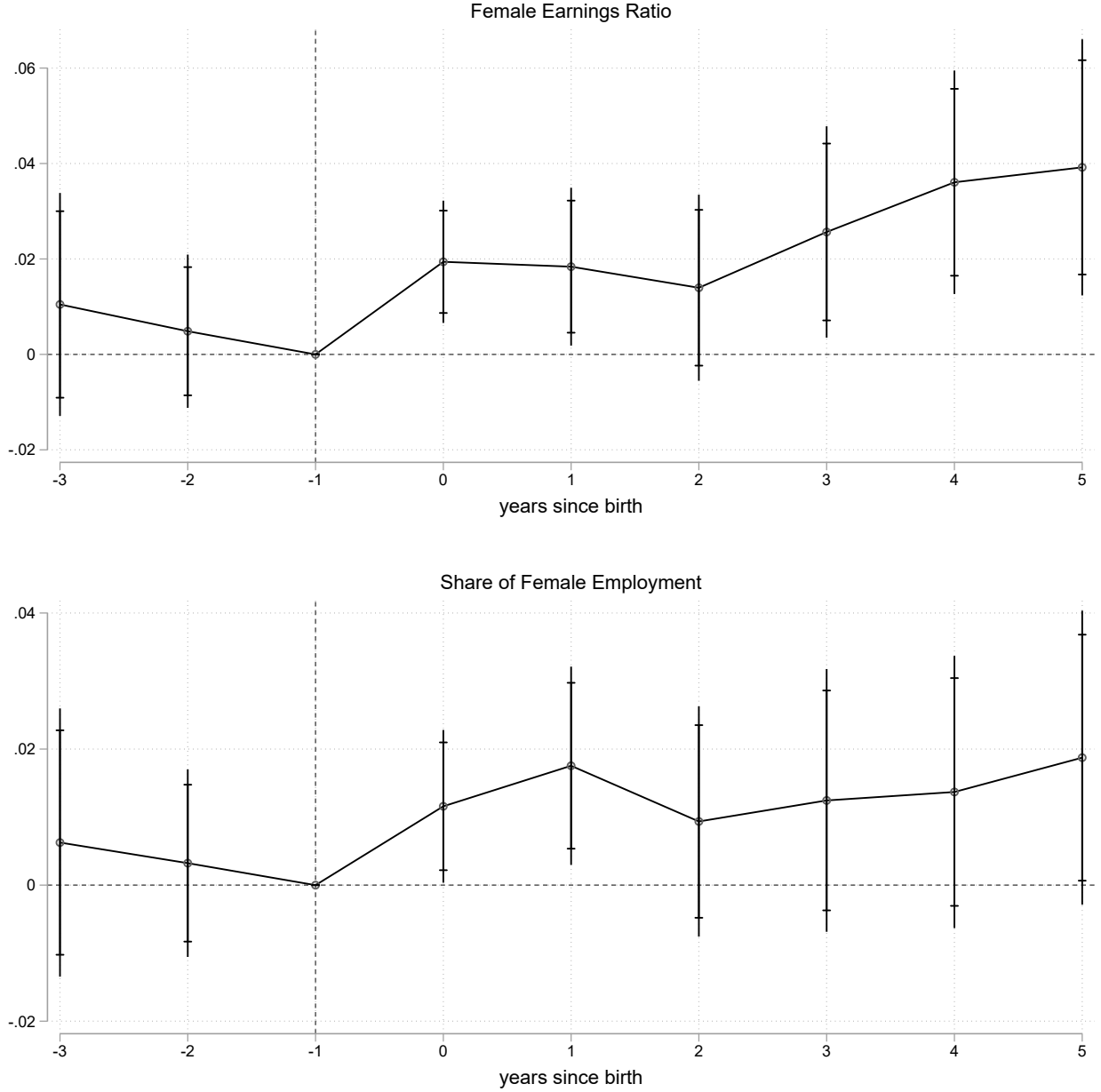
Notes: This Figure shows the magnitudes and the 95% confidence intervals of the coefficients obtained from two regressions estimated on the sub-sample of managers who have zero children and experience the birth of their first child. In particular, the dark and light gray bars show changes in the Female Earnings Ratio and the Share of Female Employment depending on the gender of the manager's first-born. The relative daughter effect, captured by the coefficient β in equation (2), corresponds to the difference between the two coefficients represented by the bars. Standard errors are clustered at the establishment level.

Figure 2: Effect of newborn child's gender on the duration of manager-establishment spells



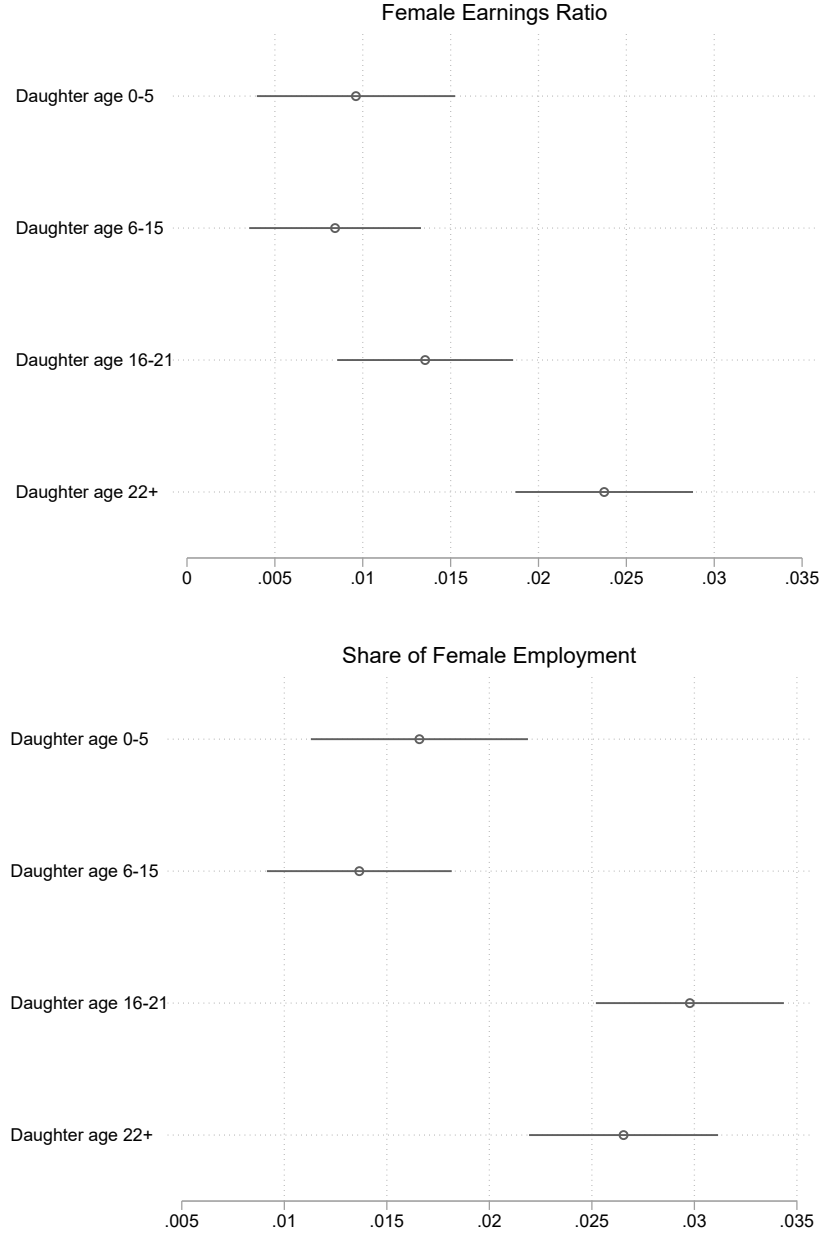
Notes: This figure shows the inverse of the probability of observing a break in manager-establishment spells over time. The y-axis reports, for each year before and after the birth, the share of manager-establishment spells that remain in the sample relative to the total number of spells used for identification. By construction, this share equals one in the year of the birth and in the preceding year, since all spells are necessarily observed during this period. The black (gray) bars represent the survival rate of spells if the manager experienced the birth of a daughter (son). Similarly, the numbers on top of the bars represent the overall number of spells observed at each point in time. We truncate the figure once survival falls below 10% of the total sample of spells, which occurs five years before and eleven years after the birth. The differences between the two bars indicate whether the likelihood of a spell being observed at a given point in time varies with the gender of the newborn child. Across all periods, these differences are small and never statistically significant.

Figure 3: Event study estimates: baseline outcomes



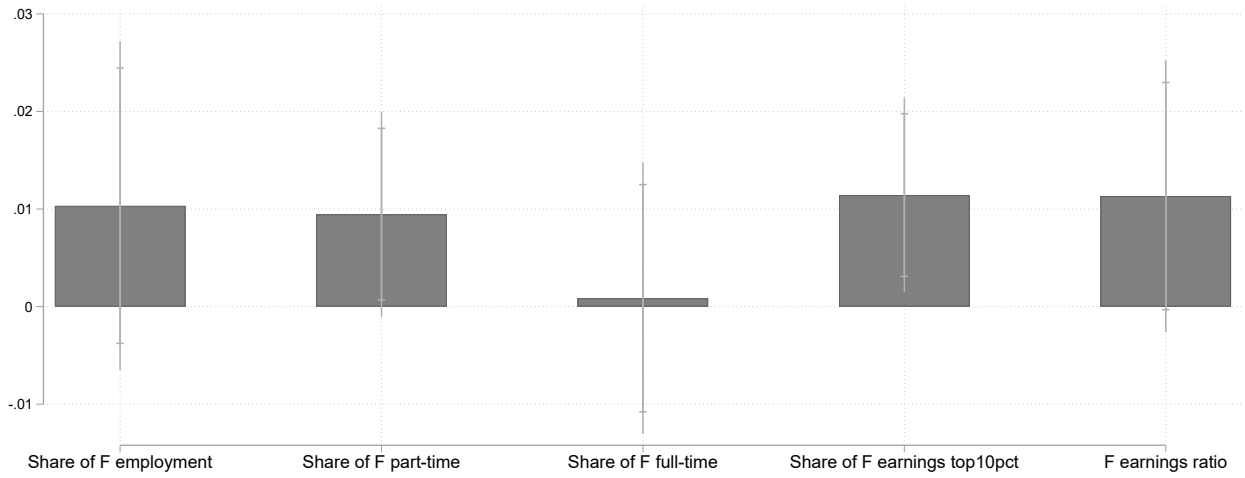
Notes: This figure plots difference-in-difference coefficients and 90% and 95% confidence intervals from estimating equation (3) using the sample of establishments in which managers experienced the birth of their first daughter (treated group) and in which managers experience the birth of an extra son and do not have any daughter (control group). If, within a manager-establishment spell, the manager experiences both the birth of his first daughter and the birth of an extra son, we consider him treated. The coefficients are plotted relative to the difference between the treated and control group the year before the birth (time $j = -1$), which is normalized to zero. Event times beyond the time window - i.e. $j < -3$ and $j > 5$ - are binned into the first and last event-time indicators, respectively. Standard errors are clustered at the establishment level.

Figure 4: Effects by age of oldest daughter



Notes: This figure plots the coefficients β_1 , β_2 , β_3 , and β_4 from equation (4) and their 95% confidence intervals. The sample consists of all single-manager establishment-year observations. Controls include 10 dummies for managers' age groups (younger than 34, 34-38, 39-42, 43-45, 46-48, 49-51, 52-53, 54-57, 58-61, older than 61), managers' years of education and years of tenure in the establishment. The establishment-level controls include employees' average age, employees' average years of education, employees' average tenure, the share of employees working full-time, the share of employees who are married, the share of employees who have at least one child, and establishment size. Finally, we include fixed effects for managers' total number of children, municipalities, five-digit industries, and year. Standard errors are clustered at the establishment level.

Figure 5: Sorting of Managers Across Firms After Spell Break



Notes: The figure is estimated using observations on managers' job transitions occurring after the end of the spell during which they experienced the birth event. We compare managers who had their first daughter with those who had an additional son and no daughters, and report 90% and 95% confidence intervals. The outcomes are: (i) the Share of Female Employment, defined as in equation (1); (ii) the share of female employees working part-time; (iii) the share of female employees working full-time; (iv) the share of women in the top decile of the within-firm earnings distribution; and (v) the Female Earnings Ratio, defined as in equation (1). All outcomes are measured in the year before the manager's arrival. All regressions use robust standard errors and include year and number-of-children fixed effects.

Tables

Table 1: Managers, Employees, and Establishment Characteristics in the Birth Sample

	Managers	Male Employees	Female Employees	Establishments
	(1)	(2)	(3)	(4)
Age	38.30 (7.42)			
N of children	1.97 (1.26)			
N of daughters	0.95 (0.94)			
N of sons	1.02 (0.96)			
Age of oldest child	9.26 (7.73)			
Age of youngest child	3.91 (4.46)			
Observations	48766			
Age		33.32 (13.69)	30.79 (13.45)	
Earnings (DKK)		234344.14 (173537.93)	157523.03 (135978.34)	
Full-time		0.75 (0.44)	0.57 (0.49)	
Tenure		3.56 (3.71)	3.16 (3.27)	
Years of education		11.33 (3.47)	11.13 (3.16)	
Observations		356023	201791	
Share of female employment				0.34 (0.32)
Share of observations only male employees				0.26 (0.44)
Share of observations only female employees				0.07 (0.26)
Female earnings ratio				0.39 (0.29)
Observations				48766

Notes: This table reports summary statistics for the birth sample, covering characteristics of managers, employees, and establishments. Each column shows means, with standard deviations reported in parentheses below. Employees' earnings are expressed in real Danish kroner. All differences between male and female employees (columns 2 and 3) are statistically significant at the 1% level. The establishment-level variables *Share of Female Employment* and *Female Earnings Ratio* are defined as in equation (1).

Table 2: Effects on baseline outcomes

	Female Earnings Ratio		Share of Female Employment	
	(1)	(2)	(3)	(4)
<i>Panel A</i>				
N of daughters	0.0094** (0.0046)	0.0098** (0.0046)	0.0075* (0.0039)	0.0079** (0.0039)
<i>Panel B</i>				
First Daughter	0.0168*** (0.0060)	0.0172*** (0.0059)	0.0102** (0.0050)	0.0100** (0.0050)
Additional Daughters	0.0048 (0.0064)	0.0050 (0.0063)	0.0023 (0.0053)	0.0031 (0.0053)
Observations	48,766	48,766	48,766	48,766
R-squared	0.73	0.73	0.84	0.84
N of Children FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Manager-Establishment FE	Yes	Yes	Yes	Yes
Time-varying controls		Yes		Yes
Mean	0.39	0.39	0.34	0.34

Notes: Panel A reports estimates from Equation 2, where the key regressor, *N of daughters*, is a continuous variable measuring the total number of daughters a manager has in a given year. Panel B replaces this regressor with two indicator variables: *First Daughter*, equal to one if a manager has at least one daughter, and *Additional Daughters*, equal to one if a manager has more than one daughter. The outcome variable in Columns (1) and (2) is the Female Earnings Ratio, while in Columns (3) and (4) it is the Share of Female Employment. The reported R-squared refers to the Panel B specification. Time-varying controls include employees' average age, employees' average years of education, employees' average years of tenure in the establishment, the share of employees working full-time, the share of employees who are married, the share of employees with children, and establishment size. Standard errors are clustered at the establishment level. *** p<0.01, ** p<0.05, *p<0.1.

Table 3: Effects on female workforce characteristics

	Top earner is a woman		Have earnings above the 90th pct		Are among the 3 highest paid		Share of female employees that:						Have post secondary education		Have lower education	
	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)	
First Daughter	0.0218** (0.0102)	0.0216** (0.0101)	0.0099** (0.0042)	0.0093** (0.0042)	0.0106** (0.0050)	0.0091* (0.0048)	0.0083* (0.0047)	0.0076* (0.0041)	0.0019 (0.0041)	0.0024 (0.0034)	-0.0001 (0.0035)	-0.0000 (0.0030)	0.0090** (0.0040)	0.0082** (0.0038)	0.0011 (0.0047)	0.0018 (0.0045)
Observations	48,766	48,766	48,766	48,766	48,766	48,766	48,766	48,766	48,766	48,766	48,766	48,766	48,766	48,766	48,766	48,766
R-squared	0.62	0.62	0.66	0.67	0.76	0.77	0.76	0.81	0.75	0.82	0.56	0.63	0.74	0.77	0.78	0.79
N of Children FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Manager-Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying controls		Yes		Yes		Yes		Yes		Yes		Yes		Yes		Yes
Mean	0.27	0.27	0.08	0.08	0.15	0.15	0.21	0.21	0.13	0.13	0.06	0.06	0.14	0.14	0.20	0.20

Notes: Outcome variables from columns (2) to (8) are all defined as the total number of female workers with a certain characteristic divided by the total number of employees. The outcome variable in column (1) is defined as a dummy equal to 1 if the employee with the highest earnings in the establishment is a woman. We define employees with post-secondary education as those who completed either vocational education or university. All regressions include a dummy for additional daughters, fixed effects for manager-establishment spells, fixed effects for the total number of children, and fixed effects for years. Time-varying controls include employees' average age, employees' average years of education, employees' average years of tenure in the establishment, the share of employees working full-time, the share of employees who are married, the share of employees with children, and establishment size. Standard errors are clustered at the establishment level. *** p<0.01, ** p<0.05, *p<0.1.

Table 4: Effects on overall workforce characteristics

	Share of employees that:						Average age		Average tenure		Log labor costs per employee	
	Are working full-time		Have youngest child age 5 or less		Have post secondary education							
	(1)		(2)		(3)		(4)		(5)		(6)	
First Daughter	0.0013 (0.0054)	-0.0001 (0.0051)	-0.0008 (0.0056)	-0.0004 (0.0037)	-0.0007 (0.0060)	-0.0014 (0.0056)	0.0097 (0.1476)	-0.0478 (0.1104)	0.0171 (0.0429)	0.0034 (0.0409)	0.0030 (0.0094)	0.0013 (0.0080)
Observations	48,766	48,766	48,766	48,766	48,766	48,766	48,766	48,766	48,766	48,766	48,766	48,766
R-squared	0.76	0.78	0.51	0.77	0.73	0.76	0.81	0.89	0.78	0.80	0.83	0.87
N of Children FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Manager-Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying controls		Yes		Yes		Yes		Yes		Yes		Yes
Mean	0.71	0.71	0.18	0.18	0.47	0.47	32.28	32.28	3.26	3.26	12.19	12.19

Notes: Outcome variables from column (1) to (3) are all defined as the total number of workers with a certain characteristic divided by the total number of employees. While the outcome variables in columns (4) and (5) are defined as the average age and tenure of the employees. We define employees with post-secondary education as those who completed either vocational education or university. All regressions include a dummy for additional daughters, fixed effects for manager-establishment spells, fixed effects for the total number of children, and fixed effects for years. The full list of time-varying controls includes employees' average age (omitted in column (4)), employees' average years of education (omitted in column (3)), employees' average years of tenure in the establishment (omitted in column (5)), the share of employees working full-time (omitted in column (1)), the share of employees who are married, the share of employees with children, and establishment size. Standard errors are clustered at the establishment level. *** p<0.01, ** p<0.05, *p<0.1.

Table 5: Effects on hiring and separation rates

	(1)	(2)	(3)	(4)
<i>Panel A</i>				
	Share of female hirings		Share of female separations	
First Daughter	0.0155** (0.0075)	0.0153** (0.0075)	-0.0105 (0.0081)	-0.0107 (0.0081)
<i>Panel B</i>				
	Share of total hirings		Share of total separations	
First Daughter	0.0062 (0.0060)	0.0082 (0.0058)	0.0014 (0.0058)	0.0030 (0.0057)
Observations Panel A	38,648	38,648	36,682	36,682
Observations Panel B	48,766	48,766	48,766	48,766
N of Children FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Manager-Establishment FE	Yes	Yes	Yes	Yes
Time-varying controls		Yes		Yes
Mean Panel A	0.34	0.34	0.35	0.35
Mean Panel B	0.32	0.32	0.29	0.29

Notes: The outcome variables in Panel A are defined as the total number of female hired over total hires - columns (1) and (2) - and as the total number of female separations over total separations - columns (3) and (4). The outcome variables in Panel B instead are defined as the total number of hires over the total number of workers - columns (1) and (2) - and as the total number of separations over the total number of workers - columns (3) and (4). An individual is considered hired if she receives remuneration from the establishment in a given year but not during the preceding 3 years. The separation variable is a dummy taking the value one if the worker leaves the firm in $t + 1$ and does not return within 3 years. All regressions include a dummy for additional daughters, fixed effects for manager-establishment spells, fixed effects for the total number of children, and fixed effects for years. Time-varying controls include employees' average age, employees' average years of education, employees' average years of tenure in the establishment, the share of employees working full-time, the share of employees who are married, the share of employees with children, and establishment size. Standard errors are clustered at the establishment level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6: Effects on the characteristics of female hires

	Share of female hires that:											
	Have earnings above the 90th pct		Are working full-time		Are working part-time		Have pre-school aged child		Have post secondary education		Have lower education	
	(1)		(2)		(3)		(4)		(5)		(6)	
First Daughter	0.0075*** (0.0024)	0.0074*** (0.0024)	0.0051* (0.0027)	0.0052* (0.0027)	0.0033 (0.0028)	0.0039 (0.0027)	0.0020 (0.0016)	0.0020 (0.0015)	0.0049** (0.0022)	0.0048** (0.0021)	0.0036 (0.0033)	0.0042 (0.0032)
Observations	48,766	48,766	48,766	48,766	48,766	48,766	48,766	48,766	48,766	48,766	48,766	48,766
R-squared	0.31	0.32	0.33	0.37	0.54	0.59	0.22	0.26	0.30	0.32	0.51	0.54
N of Children FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Manager-Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying controls		Yes		Yes		Yes		Yes		Yes		Yes
Mean	0.02	0.02	0.05	0.05	0.07	0.07	0.01	0.01	0.03	0.03	0.09	0.09

Notes: The outcome variables are defined as the ratio between the total number of female workers who are hired and have certain characteristics divided by the number of total workers. We define employees with post-secondary education as those who completed either vocational education or university. All regressions include a dummy for additional daughters, fixed effects for manager-establishment spells, fixed effects for the total number of children, and fixed effects for years. Time-varying controls include employees' average age, employees' average years of education, employees' average years of tenure in the establishment, the share of employees working full-time, the share of employees who are married, the share of employees with children, and establishment size. Standard errors are clustered at the establishment level. *** p<0.01, ** p<0.05, *p<0.1.

Table 7: Effects on promotions of incumbents

	Log Earnings							
	All Incumbents		Promotable incumbents					
			Full-time		More Educated		Mid/High-Skill	
	Female (1)	Male (2)	Female (3)	Male (4)	Female (5)	Male (6)	Female (7)	Male (8)
First Daughter	0.0160 (0.0163)	-0.0009 (0.0132)	0.0298** (0.0138)	-0.0026 (0.0100)	0.0407** (0.0173)	0.0005 (0.0117)	0.0396** (0.0171)	-0.0011 (0.0133)
Observations	84,775	156,037	36,410	80,379	24,773	52,862	39,485	71,742
R-squared	0.47	0.50	0.44	0.44	0.47	0.50	0.55	0.57
N of Children FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Manager-Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean	12.00	12.40	12.41	12.62	12.44	12.71	12.10	12.46

Notes: The sample used to estimate the coefficients consists of the population of female and male workers employed at the firm in the year prior to the birth event (shown in odd and even columns, respectively). In columns (3) to (8), we restrict the sample to the subset of incumbents who are more likely to be considered for promotion - namely, those who were, at baseline under 40 years old and who: (i) were employed full-time (columns 3–4); (ii) had post-secondary education, where we define employees with post-secondary education as those who completed either vocational education or university (columns 5–6); or (iii) were not employed in elementary occupations, as defined by the ISCO occupational classification (columns 7–8). Standard errors are clustered at the establishment level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 8: Effects on firm performance

	Log Employment		Log Sales		VA per Employee (1,000 DKK)	
	(1)	(2)	(3)	(4)	(5)	(6)
First Daughter	0.0072 (0.0192)	0.0086 (0.0187)	-0.0018 (0.0200)	-0.0012 (0.0199)	4.0413 (7.4660)	3.4413 (7.3298)
Observations	36,201	36,201	36,201	36,201	36,201	36,201
R-squared	0.86	0.87	0.90	0.90	0.65	0.67
N of Children FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Manager-Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying controls		Yes		Yes		Yes

Notes: Estimates are computed on the sample of single-establishment firms that have non-missing information for value-added and sales. To account for outliers, we winsorize the outcomes below the 1st and above the 99th percentile to value of the 1st and 99th percentile respectively. The mean of employment is 12 employees, the mean of total sales is 11,741,242 Danish Kroner, and the mean of value added per employee is 335,064 Danish Kroner. All regressions include a dummy for additional daughters, fixed effects for manager-establishment spells, manager's total number of children, and years. Time-varying controls include employees' average age, employees' average years of education, employees' average years of tenure in the establishment, the share of employees working full-time, the share of employees who are married, the share of employees with children. Standard errors are clustered at the establishment level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 9: Variation in the Daughter Effect Over Time and Across Manager Birth Cohorts

	Share of Female Employment				Female Earnings Ratio			
	(1)		(2)		(3)		(4)	
<i>Panel A: Managers' birth cohort</i>								
	Born Before 1965		Born from 1965		Born Before 1965		Born from 1965	
First Daughter	0.0087 (0.0097)	0.0107 (0.0098)	0.0221*** (0.0074)	0.0208*** (0.0073)	0.0004 (0.0080)	0.0019 (0.0081)	0.0172*** (0.0063)	0.0153** (0.0062)
<i>Panel B: Time period</i>								
	Pre 2002		After 2001		Pre 2002		After 2001	
First Daughter	0.0137* (0.0081)	0.0139* (0.0081)	0.0178** (0.0077)	0.0183** (0.0076)	0.0060 (0.0071)	0.0060 (0.0071)	0.0133** (0.0060)	0.0124** (0.0060)
Observations Panel A	24,866	24,866	23,899	23,899	24,866	24,866	23,899	23,899
Observations Panel B	23,951	23,951	23,716	23,716	23,951	23,951	23,716	23,716
N of Children FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Manager-Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying controls		Yes		Yes		Yes		Yes
Mean Panel A	0.38	0.38	0.40	0.40	0.33	0.33	0.35	0.35
Mean Panel B	0.39	0.39	0.39	0.39	0.36	0.36	0.33	0.33

Notes: Panel A presents results splitting observations by whether the manager was born before or after 1965, the median birth year in the sample. Panel B compares birth events that occurred before versus after 2002, the year of a major parental leave reform. All regressions include a dummy for additional daughters, fixed effects for manager-establishment spells, manager's number of total children, and years. Time-varying controls include employees' average age, employees' average years of education, employees' average years of tenure in the establishment, the share of employees working full-time, the share of employees who are married, the share of employees with children, and establishment's size. Standard errors are clustered at the establishment level.

*** p<0.01, ** p<0.05, *p<0.1.