

# Expansions in Paid Parental Leave and Mothers' Economic Progress

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# Expansions in Paid Parental Leave and Mothers' Economic Progress\*

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## Abstract

We examine the impact of government-funded universal paid parental leave extensions on the likelihood that mothers reach top-pay jobs and executive positions, using eight Norwegian reforms. Up to a quarter of a century after childbirth, such reforms neither helped nor hurt mothers' chances to be at the top of their companies' pay ranking or in leadership positions. We detect no differential effect across many characteristics, and no impact on other outcomes, such as hours worked and promotions. No reform affected fathers' pay or the gender pay gaps between mothers and their male colleagues and between mothers and their partners.

*JEL Classification:* H42, J13, J16, J18, M12, M14

*Keywords:* Gender inequality; Within-firm pay ranking; Glass ceiling; Leadership; Top executives

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# 1 Introduction

*Motivation* — This paper examines the impact of government-funded parental leave extensions on the likelihood that mothers are in top-pay jobs within their own organizations or in executive positions over a long time horizon since childbirth. It also explores how the same leave extensions affect the pay gap between mothers and their male coworkers or between mothers and their partners. We perform our analysis using unique register data on the entire population of workers and firms in Norway, while taking advantage of eight policy interventions that prolonged paid parental leave through seven successive reforms introduced every year between 1987 and 1993 and another in 2005. We find robust evidence that the reforms had no effect on mothers’ economic status in the short, medium or long term, both at the top of their companies and within their households.

There are at least three important reasons to focus attention on top earners and top executives. First, they are powerful economic players. In Norway, individuals in the top 1% of the income distribution on average have held around 18% of the aggregate before-tax income since 2000 (Alstadsæter et al., 2017; Aaberge et al., 2021). Second, a large share of the best talents in any economy is likely to be reflected in top earnings (Hsieh et al., 2018; Guvenen et al., 2020). Third, top earners are key political actors, as they can wield the political landscape by influencing policy agenda setting (Barber, 2016). Understanding whether and how parental leave policies affect mothers’ chances to reach the top of the earnings distribution, even many years after childbirth, can give us new insights into the allocation of talent in the economy. Moreover, if parental leave reforms change mothers’ representation among top earners and in top management, this may impact firm performance (Dezsö and Ross, 2012; Post and Byron, 2014), while further female empowerment may be set in place as we have seen occurring across other domains in several countries (e.g., Besley et al., 2017; Bhalotra et al., 2018).

The focus on top earners and executives is crucial also for another economically salient reason. A large literature documents pronounced gender differentials in wages and earnings among top earners (Albrecht et al., 2003 and 2018; Arulampalam et al., 2007; Goldin, 2014; Keller et al., 2020) as well as female under-representation in leadership positions (Miller, 2018; World Economic Forum, 2020). The two main factors that account for a considerable share of such gaps are greater career discontinuities and shorter work hours, both of which are strongly associated with motherhood (Bertrand et al., 2010; Angelov et al., 2016; Antecol et al., 2018; Bütikofer et al., 2018; Kleven et al., 2019; Keloharju et al., 2019; Iversen et al., 2020).<sup>1</sup>

Women’s remarkable labor market progress over the last half century (Blau and Kahn,

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<sup>1</sup>Azmat and Ferrer (2017), however, find no evidence of a motherhood penalty among associate lawyers in the United States. An alternative approach to interpret this penalty emphasizes the importance of discrimination. Lang and Spitzer (2020) provide an up-to-date review of this literature. Testing one interpretation against the other is beyond the scope of our paper.

2003 and 2008; Bertrand, 2011; Goldin, 2014) — even though considerable gender differences persist (Bertrand, 2020) — has in fact not been matched by mothers. In the United States, for example, the difference in median hourly earnings in 2019 was about 22% among men and women with children, almost double the 12% gap for men and women without children (OECD, 2021). Similar patterns emerge in many other advanced economies.

At the same time, most industrialized countries — with the notable exception of the United States — have introduced and expanded nationwide government-funded parental leave programs with the explicit intent of leveling the playing field between men and women in the labor market and enabling women to combine careers and motherhood (Olivetti and Petrongolo, 2017; Rossin-Slater, 2018).

Because of our focus on top earners and workers in leadership positions, we emphasize the importance of a long-term perspective, as we follow mothers' careers since childbirth. To fix ideas on how parental leave extensions can affect mothers' wages in the short and long term, it is useful to set the stage in an environment that mimics the Norwegian institutional settings under analysis.<sup>2</sup> After exhausting her leave, a mother can return to work for the same employer in the same (or similar) job and at the same pay as she had before childbirth. Parental leave is paid out of general tax revenues. In the short run, if firms do not bear any costs and the policy is financed by nondistortionary taxes, both labor supply and labor demand remain unaltered and we expect to observe no change in wages.

Over time, however, paid leave extensions may lead to a different outcome. On the one hand, were women to place a higher value on prolonged benefits, new mothers' labor supply is expected to increase relative to that of other workers unaffected by the leave policy (Klerman and Leibowitz, 1997; Ruhm, 1998).<sup>3</sup> On the other hand, firms are likely to bear some of the costs of mothers' absence, e.g., hiring and training temporary replacements for mothers on leave. Leave extensions could then reduce labor demand for new mothers. These shifts yield a reduction in wages and an ambiguous impact on employment.<sup>4</sup>

An alternative argument is based on the notion that extended leaves may lead to a stronger labor market attachment raising mothers' level of firm-specific human capital

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<sup>2</sup>The following discussion focuses on incumbent workers. As leave eligibility requires a minimum number of months of employment before childbirth, new hires can also be affected as long as they have worked six out of the 10 months prior to birth. Section 2 will describe the institutional environment in finer detail.

<sup>3</sup>Increased financial incentives for longer absences could reduce hours worked among mothers soon after returning to work. This will put a downward pressure on their labor supply moving it back towards the pre-reform equilibrium. We expect this intensive margin effect to be relatively modest among high-skill workers and potential top earners.

<sup>4</sup>If benefits are financed through higher corporate income taxes, labor demand will decrease even further, leading to an additional fall in wages all else equal. If instead financing comes through higher personal income taxes, labor supply may decrease, mitigating the extent of the expansion mentioned above, and *ceteris paribus* implying a smaller wage reduction.

(Blundell et al., 2016). This increases the marginal revenue product of workers, causing a rightward shift in the demand curve and an ambiguous effect on wages. In imperfectly competitive labor markets where skills are not easily substitutable and workers have limited mobility (Manning, 2003; Le Barbanchon et al., 2021), this effect is likely to be particularly important for high-achieving mothers, who could otherwise fall behind on the career ladder in the absence of an extended job-protected leave, as they tend to be more vulnerable to career interruptions with greater human capital depreciation and atrophy of skills while out of work (Demougin and Siow, 1994; Anderson et al., 2002; Ejrnæs and Kunze, 2013; Adda et al., 2017). If employers value continuity in employment relationships with their high-skill workers, such mothers may therefore face a steep pay profile after reentry and, in the longer run, climb the job ladder to the top of their firms' pay distribution.<sup>5</sup>

With ambiguous theoretical predictions about the impact of the extension of government-provided paid maternity leave on high achieving mothers' careers, the question remains essentially empirical.

*Relevant Literature and Our Contribution* — Much of the existing empirical research focuses on mothers' short- to medium-term *average* outcomes and not on top-pay jobs or leadership positions over the long run. The available evidence yields mixed results. For instance, for the United States and Britain, Waldfogel (1998) finds that women who have leave coverage and return to work after childbirth receive a wage premium which offsets the negative wage effects of children compared to other women.<sup>6</sup> Ruhm (1998) instead shows that paid leaves across nine European countries are associated with reductions in women's hourly wages at extended durations relative to men's. Leveraging five large reforms in Germany, Schönberg and Ludsteck (2014) find evidence of an overall small, statistically insignificant, impact on maternal labor income up to the child's sixth birthday.

Lalive et al. (2014) also detect no detrimental effect on mothers' earnings in the first five years after childbirth using three major reforms in Austria. Analyzing the same three reforms, Kleven et al. (2021) instead find sizable negative (positive) impacts of leave extensions (reductions) on the average annual earnings of all mothers. These impacts, however, are short-lived and disappear three years after childbirth, confirming the null results found by Lalive and Zweimüller (2009). Although not at the core of their analysis, Kleven et al. (2021) also show that the Austrian reforms had no long term effects among mothers in the top quartile of the pre-birth earnings distribution.<sup>7</sup>

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<sup>5</sup>In anticipation of future female hires being on leave for an extended period of time, employers could respond to extended leave entitlements by statistically discriminating in their hiring strategy of, or wage offers to, childless young women. This is an important margin, which is left for future analysis.

<sup>6</sup>Examining California's Paid Family Leave Program, Rossin-Slater et al., (2013) and Baum and Ruhm (2016) find evidence that the policy increased mothers' wage income up to three years after birth.

<sup>7</sup>Other studies on the effect of parental leave on mother's earnings include Baum (2003), Ejrnæs and Kunze (2013), and Bailey et al. (2019). The first study finds a small and statistically insignificant effect of the 1993 US Family and Medical Leave Act on wages. The latter two papers identify significantly

In the case of Norway, Dahl et al. (2016) find that the six expansions in maternity leave occurring between 1987 and 1992 had small positive, but statistically insignificant, effects on mother’s annuity income up to 14 years after childbirth. They also find that the reforms led mothers to spend more time at home without a reduction in family income, and did not affect completed fertility. We broaden the analysis to two additional reforms after 1992, which extended paid parental leave for mothers and introduced a paternity quota. We confirm their results that none of the reforms (including the 1993 and 2005 interventions) affected fertility, regardless of whether we consider birth timing (i.e., age at first birth), birth spacing, or total number of children. Moreover, we find that take-up was virtually universal, even among women at the higher end of the earnings distribution.<sup>8</sup>

Importantly, our study takes a new perspective and investigates whether prolonged parental leave changes the representation of mothers in the upper echelons of their companies, defined by the likelihood of being in the top decile of the companies’ income distribution. We cover the full range of effects, immediately after birth and up to the next 26 years, which allows us to see women progressing in their careers as their children grow up. We consider both the impact of each 2–4 week expansion in isolation and the cumulative impact of the first six reforms between 1987 and 1992, as if maternity leave were to be extended in one intervention from 18 to 35 weeks with full income replacement. Analyzing each reform separately provides us with a full evaluation of the impact of different leave regimes and avoids cherry-picking of convenient results. It does, however, generate a plenitude of estimates from smaller interventions with the risk of lower statistical power. This is why we also analyze cumulative impacts.

Looking at within-firm pay with data that are not top censored eliminates most of the issues related to differential career tracks and job definitions across firms, which could undermine the comparison of mothers with women and men in other organizations. This intra-firm perspective also reiterates the importance of the role played by firms in explaining the gender pay gap underlined by recent related contributions (e.g., Heinze and Wolfe 2010; Card et al., 2016; Bertrand et al., 2019; Casarico and Lattanzio, 2019; Palladino et al., 2020).

We bolster this analysis by exploring, for the first time, the impact of the leave reforms on the mothers’ probability of reaching top corporate positions. Focusing on top executives, namely, chief executive officers (CEOs), chief financial officers (CFOs), board directors, and board chairs in all Norwegian firms, gives us a measure for being in the “C-suite” of a company. Isolating the people at the very top allows us to overcome the possibility that pay differentials are driven by men and women working in different jobs

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negative impacts on wages of mothers in Germany and California, respectively.

<sup>8</sup>Full compliance could be driven by the fact that, when making their leave decisions, mothers internalize the long term consequences of their choice on their careers, as suggested by Kleven et al. (2021). It could also be driven by gender identity norms and social expectations. Assessing the distinctive contribution of these two explanations is interesting, but goes beyond the scope of this paper and is left for future research.

within the same organization, as postulated by Lazear and Rosen (1990). We finally complement this perspective by examining the likelihood that mothers reach managerial positions across all ranks within a firm.

To identify the short- and long-term effects of each parental leave expansion we use a regression discontinuity design that compares mothers giving birth within a narrow time window before and after each of the eight reforms. For estimation, we combine the policy changes with rich employer-employee-partner matched register data. Our analysis reveals that each of the eight expansions in parental leave had a quantitatively small and statistically insignificant impact on the probability that mothers reach the upper decile of the pay distribution within their companies in the short, medium, or long run. Similarly, the expansions had no effect on the likelihood that mothers climb to the C-suite or occupy managerial positions in their firms. These null results hold even if we cumulate the estimates across the first six expansions, which did not set aside any explicit paternity leave. They also hold when we combine each reform-specific sample into one larger sample, which gives us further statistical power. Such results are in line with the estimates found by Lalive and Zweimüller (2009), Lalive et al. (2014), Schönberg and Ludsteck (2014), Dahl et al. (2016), and Kleven et al. (2021) for average earnings.

The increased maternity leave had no heterogeneous effects along several dimensions, including education, parity, firm size, and industry. They also had no significant impact on other channels that could have led to a change in within-firm pay ranking, such as hours of work, internal promotions, and mobility across firms.

Although our focus is on mothers, we also exploit the fact that part of the package of the 1993 reform was the introduction of a paternity leave quota (known as “daddy’s quota”), reserving four of the 42 weeks exclusively for the father. Before then, fewer than 3% of fathers took any leave. The 2005 reform added one extra week to the paternal leave entitlement while keeping the maternal leave provision unchanged at 38 weeks as in 1993. The existing evidence of the effect of the 1993 parental leave on fathers’ labor market income is limited to short-run effects and mixed. Rege and Solli (2013) find a negative impact up to the child’s fifth birthday, whereas Cools et al. (2015) report a null effect. Furthermore, there exists no study testing whether the leave taken by the father helps the mother to advance to a top position. We find a null effect of the 1993 paternal quota on the fathers’ likelihood to be in the upper earnings decile of their companies, and we also detect no effect after the last extension in 2005. This is consistent with the evidence found for Sweden by Ekberg, Eriksson, and Friebel (2013) and for Quebec by Patnaik (2019). Moreover, and for the first time, we show there is no spillover impact of the remaining six pre-1993 reforms on fathers’ pay, and on the mothers’ likelihood to be in a top-pay position or a C-suite job in response to the paternity leave reforms.

A large literature documents the impact of parental leave policies on the gender pay

gap.<sup>9</sup> For example, using data from 22 high-income countries, Olivetti and Petrongolo (2017) show that the gender gap in wages shrinks with parental leave rights for entitlements up to about one year. But longer parental leaves are typically associated with wider earnings gaps for college-educated women. For Norway, the results in Dahl et al. (2016) reveal that the ratio of male to female annuity income did not change in response to the reforms. Similarly, for Austria, Kleven et al. (2021) find that the expansions of parental leave had a small, possibly negative, impact on gender equality.

Much less is known about the impact of parental leave mandates on the pay gap between mothers and their male co-workers or between mothers and their partners.<sup>10</sup> We find evidence that none of the eight reforms contributed to the secular decline in working-hour-adjusted wage gaps that we document within firms between mothers and their male coworkers. This null result holds true whether we consider women in the upper decile of their companies' earnings distribution or mothers across the rest of the income distribution. The reforms also did not induce any significant change in gender pay gaps between mothers holding a top position and their partners.

In sum, protected career breaks due to childbirth of up to nearly one year covered by government-mandated paid parental leave programs do not hurt (but also do not help) mothers' economic progress, both in the short and the long term. In the case of Norway, this could be due in part to the staggered approach implemented more than 30 years ago to deliver the parental leave program, which might have allowed firms to adjust gradually to progressively longer leave extensions. It cannot be explained, instead, by a low take-up rate, which we document to be close to 100% even among high earners. Nor can it be driven by low returns to experience faced by high-achieving mothers, as shown by Bütikofer et al. (2018) and Eika et al. (2019), among others.

Some may view our results as suggesting that prolonged paid leave be an inadequate tool to power female progress. Others may argue that extended leave was never intended to push women at the top and that our results provide evidence of a successful attempt to level the playing field for mothers. Regardless of one's interpretation of our findings, gender imbalances persist in Norway and elsewhere across the globe, while they feature prominently at the core of the UN policy agenda, which has declared gender equality among its key sustainable development goals for 2030. Our findings also gain further importance in light of renewed interest in paid parental leave reform in countries like the United States (see AEI-Brookings, 2020) and in the context of the post-Covid-19 pandemic, which seems to have created further divides between mothers and other members of society worldwide (e.g., Alon et al., 2020; Andrew et al., 2020; Farré et al., 2020).

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<sup>9</sup>For comprehensive reviews, with a broad span of interventions including parental leave provisions, see Olivetti and Petrongolo (2016 and 2017), Blau and Kahn (2017), and Rossin-Slater (2018).

<sup>10</sup>Beblo et al. (2009) is the only related study that compares a small sample of first-time mothers' wages upon return to work to those of their female colleagues within the same establishment in Germany. They find that job interruptions reduce mothers' wages by 19–26%.



## 2 Institutional Background

Norway has a long history in supporting working women around motherhood, which dates back to the late nineteenth century and has continued through to the post-WWII period (Vollset, 2011; Ellingsæter et al., 2020).<sup>11</sup> In 1956, women became eligible to maternity compensation through the sickness benefit scheme that replaced part of pre-birth earnings and provided protected leave for up to 12 weeks (NOU 1996:13, p.214). With the 1978 Social Insurance Act reform, paid parental leave was granted for 18 weeks. Eligibility required mothers to have worked six of the 10 months before birth and earn more than the basic income. Mothers were entitled to a minimum of six weeks after birth. Although there was no explicit formal paternal quota, both parents could share the remaining 12 weeks, but mothers typically took the whole leave available.<sup>12</sup> The mandates provided 100% income replacement up to a generous earnings threshold (equivalent to six times the basic income).<sup>13</sup> Employers could not dismiss workers for taking leave, and parents had the right to return to the same (or comparable) job. These features remain in place over the entire observation period that we study.

Between 1987 and 1993, Norway introduced a series of seven reforms that expanded paid parental leave from 18 weeks to 42 weeks at 100% income replacement. In 1985, the Labour Party led by Gro Harlem Brundtland won the general election with a strong mandate to reform. One of the main goals of Brundtland's government in power since 1986 was to achieve greater gender equality, starting from her own cabinet in which eight of the 18 ministers were women. The extension of the mandated paid parental leave was a key part of this program. Besides political feasibility, the staggered introduction of the annual extensions might have been due to the 1985 oil price shock which led to an unexpected public deficit and a significant devaluation of the Norwegian krone. This was then compounded by the banking crisis, which began biting in 1988 and continued through to 1992.

Starting in 1989, parents could also choose between a package comprising shorter leave with full income replacement and a package with longer leave at 80% replacement. The 1993 reform set aside a four week quota of paternity leave for the first time worldwide. This added to the 48 weeks at 80% replacement taken by the mother, leading to a total of 52 weeks at the household level. If a couple opted instead for full income replacement, they would have enjoyed a total of 42 weeks of leave, of which (at least) four had to

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<sup>11</sup>Most of these early regulations were primarily intended to protect the child and mother's health, especially women in factory jobs. Since 1977, parliamentary debates have regularly given prominence to the goal of gender equality, and in particular in relation to mothers' and fathers' rights.

<sup>12</sup>Working fathers had the right to two weeks of unpaid leave after birth. Their income would have been typically replaced by their employers through bilateral agreements (Work Environment Act, 1 July 1977).

<sup>13</sup>This and all the thresholds related to the subsequent reforms were nonbinding for most mothers. When they were exceeded, employers topped up benefits so that forgone earnings were fully replaced, even for women at the top of the pay distribution (NOU 1996:13, p.218).

be used by the father. The last reform we consider was introduced in 2005, in which the father's quota was extended to five weeks, without changing the mandated length of maternity leave.

Table 1 summarizes the timeline of all the reforms over our sample period, including the pre-existing default, with information on the leave duration for mothers and fathers and the income replacement rates. Other reforms were implemented after 2005 that expanded the quota for fathers. We do not analyze them, however, because our aim is to emphasize the impact of the interventions on women's ability to reach top-pay positions, arguably a long run outcome for which we need a long time horizon after childbirth. By including the 1993 and 2005 reforms, we can evaluate the role played by paternity leave on mothers' outcomes (as well as on fathers'), while giving us a total increase of 20 weeks of paid maternity leave, a natural comparison to the initial 18 weeks introduced in 1978.

Given the long term perspective of our analysis, there might be a concern that other policies could interact with our treatment. We draw attention to three other public policies that were enacted during the period we analyze. Introduced in August 1998, the first policy is the Cash-for-Care program which awarded tax-free cash allowances to parents who did not use publicly subsidized child care schemes and had a one- or two-year-old toddler. There is evidence that the program reduced full-time employment and earnings of mothers without a university degree or with pre-reform earnings below the median, even when the child was no longer eligible at ages four and five (Drange and Rege, 2013). The effect, however, disappears when children enter school, from age six onwards. We therefore expect the Cash-for-Care program to have no impact on our estimates, because our focus is on the top of the earnings distribution among women who typically had children before the introduction of this program (apart those who gave birth around the 2005 leave reform), and because its fertility effect appears to be negligible (Bettinger et al., 2014).

The second policy relates to public child care provision. Havnes and Mogstad (2011) show that the roll out of the 1975 Kindergarten Act, which introduced subsidized and universally accessible child care for three- to six-year-old children throughout the second half of the 1970s, had no effect on maternal employment, while crowding out pre-existing informal child care arrangements. Most of the mothers exposed to the leave reforms in our study faced this sort of child care provision uniformly. In 2002, the Norwegian government expanded child care provision to include children aged one to three, essentially offering full child care coverage from birth to the start of primary school. Andresen and Havnes (2019) and Kunze and Liu (2019) show that this reform led to an increase in mothers' labor supply. Its impact on wages, however, are unknown. Moreover, this intervention overlaps only with the last leave reform in our study, which extended the daddy's quota while keeping the amount of leave available to mothers unchanged. As before, we expect little impact of this policy on our estimates, since seven of the eight reforms we focus on

did not overlap with it. Issues of interpretation might arise if we detected strong positive impacts of the 2005 reform on mothers' or fathers' earnings. As we find no effect of the 2005 reform for either parent, the interaction between the two programs is likely to be negligible.

The last policy is the gender board quota reform, introduced in 2005 but not fully implemented until 2008, which mandated 40% representation of each gender on the board of public limited liability firms. We expect that our estimates be largely unaffected by this policy for at least three reasons: the quota involved fewer than 0.3% of nearly 250,000 firms; it came fully into force three years after the last leave reform in our study, at the end of our sample period; and, according to estimates found by Bertrand et al. (2019), it did not have any spillover effect on women employed in managerial positions in the companies subject to the quota who were not appointed to boards.<sup>14</sup>

### 3 Data

Our analysis uses employer-employee matched panel data extracted from several registers on the entire population of employees and the universe of private- and public-sector firms in Norway. This enables us to have detailed workers' employment and earnings information from 1967 to 2013, and to follow them with their employers from 1983 to 2013. Earnings information is recorded without any top- or bottom-coding. For each firm, we observe the entire population of workers, including those at the very top of the organization, so that we can determine the exact rank occupied by each worker in the earnings distribution within her/his company. With unique identifiers for individuals and firms, we can match workers to their employers and partners to one another, and follow them over time without attrition (except death, a rare event in the group of workers under analysis, and out-migration, which is negligible over the sample period).

Besides earnings and employment, the registers provide information on demographics, education, and broad categories of hours worked. Accurate information on hours is available only from 1997, which we will explicitly analyze in subsection 5.3. Data on all births by month and year over the entire period are obtained from birth registers and merged with detailed individual receipt of parental leave benefits and duration obtained from the social security registers, which contain complete records for all mothers and fathers from 1992 onwards.

To analyse individual earnings positions within firms, we focus on annual total earnings, reported in the tax registry and deflated using the consumer price index with 1998 as base year. This measure comprises all types of formal remuneration, including overtime

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<sup>14</sup>In general, Bertrand et al. (2019) find that the board quota policy had little impact on women, apart from its direct impact on the women who made it into boardrooms. Other studies on the same reform include Ahern and Dittmar (2012), who report a decline in firm value as a result of the quota, and Eckbo et al. (2022), who find a value-neutral impact.

pay, performance-related pay and bonuses, financial income and benefit income. We also analyze income adjusted for hours of work, using the full-time equivalent monthly wage measure constructed by Statistics Norway with hours data from wage statistics information.

Our final sample contains all individuals employed and with positive earnings from the first time they entered the labor market after completing education (defined at age 18 for those without a university qualification, and age 24 for those with a college degree or more) up to age 60. To construct a meaningful measure of intra-firm gender composition, we exclude firms with fewer than 4 employees and the self-employed.<sup>15</sup>

We use three different measures to identify gender diversity in high-pay jobs and at the top end of the corporate world. The first is an indicator of whether a female employee is in the top decile of her firm's earnings distribution. Different companies could use different pay structures, and women may be at the top in low-paying firms.<sup>16</sup> In spite of this, appearing among the best paid workers in any given organisation can be taken as a clear signal of high performance and value to the firm, irrespective of job titles or career tracks. To construct this measure, we need to observe the universe of employees in all firms in the economy over the sample period.

A slight variant of this measure is redefined with an indicator variable taking value one if a mother is in the top decile of the salary distribution among all women and men in the same age group, and zero otherwise. We distinguish eight age categories, seven of which are defined in 5-year bands and the first is slightly broader (i.e., 18–24, 25–29, 30–34, 35–39, 40–44, 45–49, 50–54, 55–60). This outcome accounts directly for the fact that it might be difficult for women to be at the top of their organization at the time of childbirth when they are typically young or at the early stages of their careers.

The second measure is given by specific leadership positions within each company, a proxy for being in the C-suite of the organization. Using data from the job title register, which are available from 2003 onwards, we can identify the chief executive officer, the chief financial officer, all the board directors and the chair of the board for each firm in the data. Our measure takes value one if a woman holds one of these four positions, and zero otherwise. Finally, our third measure, which is based on register-level occupational data, singles out all the employees with managerial responsibilities within an organization from 2010 to the end of the sample period. This variable takes value one if a woman has managerial duties, and zero otherwise. Because this outcome identifies managers across all hierarchical levels and covers a short time horizon over the sample, we focus more on the previous two measures.

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<sup>15</sup>The results in Section 5 are not sensitive even if we select companies with 10 or more employees. Furthermore, to comply with the eligibility criteria imposed by the Norwegian regulations, the sample used in the regression discontinuity analysis is restricted to women who received labor income in the year prior to childbirth.

<sup>16</sup>Our measure, therefore, is robust to differential sorting of mothers into firms both pre- and post-birth based, for instance, on expectations of maternity leave.

Figure 1 displays the trends between 1983 and 2013 in the first measure of gender diversity just described, i.e., the proportion of mothers whose annual pay is in top decile of the earnings distribution of their companies. Equality is attained at 50%, shown by the horizontal line. To have a more complete picture of the evolution of mothers' labor income across the entire distribution, the figure also presents the proportions of those in the middle of the distribution (40th to 60th percentiles) and in the bottom decile within their firms. For comparison, it also displays the trends for all women.

At the start of the period, mothers (and women in general) were severely under-represented at the top and over-represented at the bottom of the within-firm pay distribution. Over the 31-year period under analysis, however, the fraction of mothers in the top earnings decile has more than doubled, from about 13% in 1983 to nearly 30% in 2013. The share at the bottom decile instead has substantially declined, from 60 to 35%. Mothers' representation in the middle of the distribution has gone up, but more modestly than at the top, from one-quarter to one-third over the sample period. The trends for all women are essentially identical to mothers', although the fraction of low-pay jobs held by women declined less sharply and was still 60% in 2013. Similar trends emerge for the proportion of mothers in the top decile of their age-salary distribution.

Figure 2 displays the shares of mothers and women in top executive posts. In 2003, only 20% of such posts were occupied by mothers, and 23% by all women. By 2013, those figures increased to almost 30 and 37%, respectively. Part of this upward trend is likely to reflect the 2005 gender quota law. If we use the broader measure of management based on the occupational code, we observe 35% of leadership posts occupied by mothers, and slight more by all women, from 2010 onwards. Although both Figures 1 and 2 show an ascent of mothers to their firms' top echelon, we do not know if the parental leave reforms played any role, facilitating mothers' return to top-flying careers after childbirth.

In addition to studying how extensions of parental leave have affected women at the top, we also investigate if they improved pay differentials between mothers and men in the same firm or in the same household. The within-firm gender pay gap is defined by  $\Delta_{jt} = \log(w_{jt}^m) - \log(w_{jt}^f)$ , where  $w_{jt}^s$  is the pay (either annual earnings or full-time equivalent monthly wages) of gender  $s = f, m$  (female and male, respectively) in firm  $j$  at time  $t$ . The differential between partners is given by  $G_{ht} = \log(w_{ht}^m) - \log(w_{ht}^f)$ , where  $w_{ht}^s$  is the pay measure of the female ( $s = f$ ) or male ( $s = m$ ) partner in household  $h$  for year  $t$ . The evidence for Norway on both  $\Delta$  and  $G$  is surprisingly scant, especially if one takes a long run perspective.

The evolution of  $\Delta$  over time is reported in Figure 3, where gender equality is achieved on the horizontal line at 0. Focusing on workers in the top decile of their companies' annual earnings, mothers' pay disadvantage with respect to their male coworkers is substantial even in 2013 at about 25%, although this has steadily declined from a staggering 55% in the early 1980s. Progress for all mothers (not just those in the top decile) has been

sharper, with the intra-firm gender pay gap declining from approximately 57% at the start of the sample period to about 17% at the end. Since the early 1990s,  $\Delta$  among top earners is greater than among all mothers, providing an indication of the presence of a glass ceiling. The patterns for all women are similar.

Looking at full-time equivalent monthly wages from 1997 onwards, we observe a smaller gap of about 18% between mothers and all their male coworkers in the top decile in 1997 and this halves to 9% by the end of the sample period. This suggests that hours worked play an important role in explaining earnings differences between men and mothers in the same firm even at the top echelons of pay.

Figure 4 displays the patterns of the two measures of  $G$  (one based on total annual earnings, the other on full-time equivalent monthly wages) for mothers in the top decile of their firm's pay distribution and for all mothers. For those in the top decile, the intra-household annual pay gap is close to 0 up to the late 1990s and becomes negative since then. From the start of the 21st century, therefore, mothers at the top of their firm's pay distribution have been earning more than their partners, and nearly 10% more by 2013. Adjusting for hours worked increases the pay penalty for mothers, indicating that this group of highly paid women work longer hours than their partners. By the end of the period, the gap is again negative reflecting a pay premium of about 5% for female top earners. The average intra-household pay disadvantage for all mothers, instead, is sizeable and still in excess of 40% in 2013, although declining from 65% since the late 1980s. The penalty in full-time equivalent monthly wages for all mothers has instead remained stable around 20% over the period.

Summary statistics of the main variables used in the analysis are presented in Table 2. They refer to all working women and men who had a child in the six months around the 1993 reform.<sup>17</sup> To give an idea of the change over time, we report figures for 1993, 2003, and 2013, that is, when children were 0, 10, and 20 years old respectively, provided individuals were in employment in those years. Selecting adjacent years delivers similar snapshots of the sample.

At the start of the period, mothers were nearly 30 years old, and fathers about two years older. By the end of the period, the average age of this cohort of parents was about 50 and 51, respectively. Mothers and fathers had similar schooling levels, with nearly 13.5 years of completed education. For most of them, mothers and fathers alike, the child born in 1993 was the second, and on average they had one additional child by 2013. The fraction of individuals in a marriage or a cohabitation was close to 60% in 1993, and this rose to about two-thirds 20 years later.

Worked hours were stable over time at about 33 and 37 per week for women and men, respectively. At the time of the 1993 birth, mothers had already accumulated

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<sup>17</sup>Summary statistics for parents who had a child around the other reforms provide comparable evidence are not presented for space concerns.

almost 10 years of work experience, and fathers nearly 13. By the end of the period, the corresponding figures were 30 and 32 years, respectively. Firm tenure also increased smoothly over time, from approximately four years in 1993 to 9 or 10 years at the end of the panel. This is reflected in the relatively stable mobility across employers, with an average of 3 jobs held between childbirth and 2013.

Over the 20 year period, real annual earnings more than doubled for mothers and rose by 95% among fathers. This is reflected in the increases in the proportion of mothers and fathers in the top earnings decile within their firms, confirming the patterns observed in Figure 1 for mothers. It is also reflected in the other outcome measures, including the proportion of mothers and fathers in the top earnings-by-age decile.

Finally, we also analyze internal promotions. To do this, we use comprehensive data on over half a million white collar worker-year observations across 4,000 plants from 1987 to 1997. The data are merged to the registers from plant-level job surveys compiled by the Confederation of Norwegian Enterprise, the primary employer association in Norway. The sample includes workers at private sector firms and over-represents manufacturing but retains broad coverage.<sup>18</sup> These data contain detailed job information that allows us to assign workers to one of seven hierarchical ranks defined consistently across plants and over time. We can track individual promotions within the same organization as well as promotions that involve a change of employer. Table 2 shows that the annual promotion rate for women was 4%, just half the rate experienced by men.<sup>19</sup>

## 4 Research Design

We use the same identification strategy in each of the eight reforms that took place between 1987 and 2005 and employ a regression discontinuity (RD) design as in Dahl et al. (2016). Let  $y_a$  denote the relevant labor market outcome measured at child age  $a$  ( $a=1, 2, \dots$  up to age 26 depending on the reform),  $x$  be the birthdate (year and month) of the child, and  $t$  the date of the reform's enactment. For every mother in the data and

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<sup>18</sup>For more details, see Kunze and Miller (2017).

<sup>19</sup>In the Online Appendix Table A.1, we report the same descriptive statistics for workers in the top decile of their firms earnings distribution and for executives and board members who had a child in the six months around the 1993 reform. Compared to the whole population of parents, top earners were on average slightly older, substantially more educated, more likely to be married or cohabit, had their first child later, accumulated more work experience, and worked in larger firms. Not only were their mean earnings much higher, which is true by construction, but their weekly hours worked were also greater, especially among mothers. While male top earners have similar promotion rates to the whole population of fathers, female top earners have a strikingly higher rate at about 18%. The average characteristics of executives and board members are typically in between those of the whole population and those of top earners, except for firm size, as they worked in smaller organizations on average.

for each reform, we fit the following regression model:<sup>20</sup>

$$y_a = \alpha_a + f_{\ell,a}(t-x)\mathbb{I}(t-x) + [\beta_a + f_{r,a}(x-t)]\mathbb{I}(x-t) + \varepsilon_a, \quad (1)$$

where  $\mathbb{I}(z)$  is an indicator that takes value one if the event  $z$  occurs and zero otherwise,  $f_{\ell,a}(\cdot)$  and  $f_{r,a}(\cdot)$  are unknown functions of the time distance to and from the reform, respectively, which vary with child age, and  $\varepsilon_a$  is an error term. The parameter of interest is  $\beta_a$ , which captures the intention to treat (ITT) of the reform offering additional weeks of paid leave on the outcome  $y_a$ .

To obtain an average treatment effect,  $\beta_a$  must be adjusted by the change in leave compliance around the cutoff  $t$  from another RD regression similar to (1). As discussed by Dahl et al. (2016), this is not possible for the reforms that were introduced before 1992, because the take-up information is not available. For the 1992, 1993, and 2005 reforms, for which we have information, we find that eligible mothers extended their leave durations by the additional number of days allowed by the law, irrespective of whether we consider all women or those in the top earnings decile (see Appendix Figures A.1 and A.2). This indicates a compliance close to 100% across all mothers as well as high-achieving mothers who reach the top decile 10 years after childbirth. The  $\beta_a$  estimate from (1), therefore, should be close to the average effect on the treated for those three reforms. There is no (anecdotal) evidence that take-up rates were lower for the earlier policy interventions. This is why we focus on the ITT estimates for the rest of the paper.<sup>21</sup>

Estimating (1) for every post-reform year separately allows us to trace out the life cycle pattern of the ITT effects from a minimum of eight years (after the 2005 reform) to a maximum of 26 years (after the 1987 reform) in the case of the probability of being in the top decile of the within-firm pay distribution. When our measure of gender diversity at the top is the likelihood of being in the C-suite of the organization, we will have estimates for 11 separate years (from 2003 to 2013) for all interventions, except for the 2005 reform for which we have again eight annual estimates from the year after the reform to the end of the sample period.

In the benchmark specification, the time window around every reform is defined to be six months, although we perform a number of sensitivity checks to test the robustness of the results to this assumption.<sup>22</sup> The discontinuity that model (1) exploits arises from the

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<sup>20</sup>Given time is the running variable with a treatment date as the threshold, our design reflects the regression discontinuity in time approach described by Hausman and Rapson (2018). Notice, however, that our application does not lack cross-sectional variation (as we have the universe of women having a child around the cutoff date) and does not ignore the time-series properties of the data (as we analyze both short- and long-run effects).

<sup>21</sup>For fathers, instead, the take-up rate is lower at the start, about 40% in 1993, but growing considerably over time to about 85% in 1998, and essentially universal after that year. Furthermore, as the difference in compliance rates between treatment and control groups on each side of the cutoff  $t$  is never large, scaling up the ITT estimates does not have much scope.

<sup>22</sup>Essentially, we allow for shorter time windows, from five months down to one month around the



reform being contingent on the birthdate of the child. As an example, let us consider the 1992 reform. Mothers of a child born on April 1, 1992 or after were entitled to 35 weeks of fully paid leave, while those with a child born on March 31, 1992 or before could only receive 32 weeks. When estimating the effect of the 1992 reform, it should then be kept in mind that each  $\beta_a$  picks up the impact on  $y_a$  of this *marginal* increase in leave. From Table 1, we can see that each intervention extended maternal leave from a minimum of two to a maximum of seven weeks, while the 2005 reform kept the same duration constant for mothers, while increasing the leave for fathers by one week.

Although it is unlikely that the same woman contributes to the treatment group one year and to the control group the subsequent year, or viceversa, this switch of treatment status could nonetheless occur during the sequence of the seven reforms from 1987 to 1993. For this reason, we performed sensitivity checks in which we drop individuals who switch treatment status. Since such an exclusion does not alter any of the benchmark estimates, the results from that analysis are not presented for the sake of brevity.

We perform a number of tests to validate the RD assumptions. First, we verify that there is no manipulation of the assignment variable  $x$ , the child’s birthdate. Appendix Figure A.3 shows there is no systematic effect of the reforms on the distribution of births around the cutoff,  $t$ .<sup>23</sup>

Another check is that parents cannot modify their eligibility status around the cutoff date. Eligibility to maternity leave is essentially driven by the mother’s annual earnings in the calendar year prior to the reform. With all the reforms being announced toward the end of the year, mothers have little scope to adjust their earnings before childbirth.<sup>24</sup> Finally, the distribution of parents’ predetermined characteristics may differ around the introduction of the reform if parents can manipulate their child’s date of birth or their own eligibility status. We find little evidence that this is the case. Only one of the 16 estimates shown in the first two columns of Table 3 for mother’s age at childbirth and education is statistically significant, and we cannot reject the hypothesis that all 16 coefficients are jointly equal to zero ( $p$ -value=0.359). Repeating the exercise for other different subsamples used in the analysis below leads to similar conclusions (see the remaining columns of Table 3). Despite this balance, all our RD regressions control for mother’s age and education, as well as municipality fixed effects. Excluding them does not alter the results.

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introduction of each reform. The estimates from all these different bandwidths confirm our main results. In the Online Appendix, we report some of the results obtained with a bandwidth of three months. Given the large number of results, we cannot show all of them. They can be obtained upon request.

<sup>23</sup>Dahl et al. (2016) provide additional evidence that  $x$  cannot be timed in response to the reform (e.g., randomness of announcement and implementation dates) and discuss birth practices up to the 1992 reform that made it hard for women to postpone induced births and cesarean sections. The same considerations apply to the two later reforms in our study.

<sup>24</sup>As mentioned in Section 3, we restrict the sample to women who have received labor income in the year prior to childbirth. Imposing a stronger restriction, such as having worked for two consecutive calendar years before birth, does not change our estimates.

Furthermore, the estimates in the third column of Table 3 are small and never statistically significantly different from zero, suggesting that none of the reforms had an impact on the timing of first births. Also, as shown in Appendix Table A.2, none of the reforms affected either the spacing between first and second births or the total number of children. This set of results provides strong evidence that fertility outcomes were unchanged by the leave extensions. Although exploring such outcomes in greater detail would be interesting, this is not the scope of the paper.

## 5 Main Results

### 5.1 Top Pay and Leadership Jobs

We present our RD results graphically, separately for each reform. Figure 5 plots the year-specific (or child-age-specific) effect,  $\beta_a$  in (1), of each reform on the probability that a mother is in the top earnings decile within her firm. We track women from the year following the reform to the end of the sample period, with each dot in the figure representing a different year (or child age) and the dotted lines around the point estimates indicating the 95% confidence intervals. Following Gelman and Imbens (2019), the different  $f_a$  functions to the left and right of each reform are linear in the running variable  $x$  (month of childbirth) and estimated using triangular weights. Each regression controls for a cubic polynomial in mother’s age, years of schooling, and municipality fixed effects. At the bottom of every panel, we report the mean effect of each reform averaged over all the post-reform years,  $\sum_a \pi_a \beta_a$ , where  $\pi_a$  is a weight given by the number of observations in the year in which the child age is  $a$  divided by the total number of observations in the entire post-reform period for that specific reform. We also report the mean effect averaged over the period ten years after each reform (labelled “Average RD 10+”). In this way, we take away the years in which mothers are younger and less likely to be at the top of their organization’s pay.

Figure 5 provides little visual evidence that parental leave extensions affected mothers’ chances to be in the top earnings decile of their organization in both the short, medium, and long run. We do find that the 1992 reform increased such chances by about 2 percentage points between 2007 and 2012, when children were 15 to 20 years old. A similar, albeit shorter lived, positive impact emerges after the 1989 reform between 2006 and 2008 when children were in their late teens. The response to the 1990 reform instead was negative between 2003 and 2005. In general however, considering all the reforms together, there is no systematic pattern of results with a sustained (either positive or negative) impact over time. Put differently, none of the reforms contributed to the observed increased representation of mothers in the top echelons of their companies’ pay revealed by Figure 1. At the same time, none of the leave expansions had a negative impact on the mothers’

likelihood to reach the top pay decile in their firms.<sup>25</sup>

The same evidence emerges when we average out the impact of each extension, over either the whole post-reform period or starting 10 years after every intervention. They are statistically indistinguishable from zero and quantitatively small, with effect sizes ranging between  $-0.4$  and  $0.7$  percentage points (or, at most, 5% of the baseline probability for a mother to be in the top decile in 1987 and 2% of that in 2013).<sup>26</sup> Using the proportion of mothers in the top earnings-by-age decile as our outcome does not change the results.<sup>27</sup>

Another, more direct way of assessing whether the expansion in parental leave duration affected mothers' chances to break the glass ceiling is to re-estimate the model using the probability for being in the C-suite (CEO, CFO, board chair, or board director) as our new outcome. Figure 6 presents the results, which are available only from 2003 onwards. This clearly shows that none of the eight parental leave reforms contributed to the increasing trend reported in Figure 2. Nor did they significantly impact mothers' high-flying careers negatively. If we consider the fraction of women selected as chairs or directors of boards separately from the female share in top executive positions, we find the same null result. We reach the same conclusion also when we analyze the likelihood of mothers holding a managerial occupation (see Appendix Figures A.7 and A.8).

Summing up our benchmark results, we find little support for a short-term reduction in the probability of being a top earner among mothers who faced leave expansions. Similarly, there is no evidence of a long-term increase in the same probability. Even more clearly, mothers did not see their chances to be in the C-suite either compromised or enhanced, even two decades or more after childbirth. This evidence, therefore, convincingly shows that prolonged leave neither favors nor hinders mothers' career success.

Confirming that the leave extensions did not induce selection in/out of the labor force is important to the credibility of these null results. We thus ask whether the policy interventions changed human capital accumulation or not. Appendix Figure A.9 shows that none of the reforms affected work experience, enabling mothers to stay attached to the labor market despite the longer (temporary) interruptions of employment. Although this might have allowed mothers to retain part of their firm-specific human capital, it was not enough to support their careers to climb to the top rung of their companies.

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<sup>25</sup>Additional estimates displayed in Online Appendix Figure A.4 show that the results are not sensitive if we change the bandwidth around the reform cutoff date from six to three months.

<sup>26</sup>To trace out effects on other parts of the earnings distribution, we also look at the effect of the leave extensions on the probability that mothers are in the middle (from the 40th to the 60th percentiles) or in the bottom decile of the earnings distribution within their firm. The results in Online Appendix Figures A.5 and A.6 document that, even in these other segments of the distribution, there is no evidence of a systematic impact of the reforms. Extending maternity leave duration, therefore, had neither an immediate nor a delayed effect on mothers' relative intra-firm pay position, regardless of whether we look at the top, middle, or bottom of the earnings distribution.

<sup>27</sup>In that exercise, we restrict the analysis to women in firms with at least 10 coworkers within the same age group. As a robustness check, we repeated the analysis on mothers in firms with at least 30 coworkers. We also redefined the outcome using 10-year age categories. In all cases, the results are similar and are thus not reported for brevity.

While this result could be seen as a missed opportunity, it nonetheless indicates that long work interruptions due to maternity leave do not penalize mothers. Similar null estimates emerge also for firm tenure, as documented in Figure A.10 of the Online Appendix.

*Cumulative Impact* — One concern with the analysis so far is that we estimate each reform separately, and this may capture only marginal changes in maternity leave duration. In other words, each incremental change may be too small to detect any statistically significant effect of the increased leave generosity. There might be nonlinear negative effects that emerge only when work interruptions are long enough.

To address this statistical power issue, we look at the cumulative impact of all policy changes, using a difference in discontinuity (or diff-in-disc) approach (e.g., Grembi et al., 2016). To keep the comparison clean, we exclude the 1993 and 2005 reforms, since these introduced and extended the daddy’s quota.<sup>28</sup> This estimator combines the previous RD specification with a difference-in-difference design fitting linear regression functions to the observations distributed within a given distance (in months) on either side of the 1992 and 1987 reforms.<sup>29</sup> Formally, we restrict the sample to mothers who had a birth within the six-month interval around each of the two reforms and estimate the model

$$y_{it} = \delta_0 + \delta_1 x_{it} + m_i(\gamma_0 + \gamma_1 x_{it}) + \tau_t[\lambda_0 + \lambda_1 x_{it} + m_i(\theta_0 + \theta_1 x_{it})] + \xi_{it}, \quad (2)$$

where  $m_i$  is a dummy variable indicating eligibility for mother  $i$  to longer maternity leave,  $\tau_t$  is an indicator for the post-treatment period (i.e., taking value one for the post-1992 period, and zero otherwise), and  $x_{it}$  is the normalized distance in months of the child’s birth from the relevant reform’s eligibility cutoff. The parameter  $\theta_0$  is the diff-in-disc estimate, which identifies the effect of the maternity leave extension between 1987 and 1992 and corresponds to the cumulative effect of the 1992 reform over its 1987 counterpart.<sup>30</sup> Since we do not consider the 2005 reform, we perform the main exercise using data only up to 2004. Extending the analysis beyond 2004, while excluding women who had a child around the 2005 reform or any time after that, does not change our findings.

The baseline diff-in-disc result in column (a) of Table 4 is unambiguous. The impact of extending maternity leave duration from 18 to 35 weeks with full income replacement on the probability of being in the within-firm top earnings decile is economically negligible

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<sup>28</sup>The inclusion of the 1993 reform in the diff-in-disc framework is straightforward and does not change the results discussed below.

<sup>29</sup>Since we do not have information on board and top executive positions at the time of the introduction of these reforms, the diff-in-disc design cannot be applied to measure cumulative effects on C-suite outcomes.

<sup>30</sup>Although demand-side time-varying factors spurred by the policy introductions, such as statistical discrimination, could affect the difference feature of this estimator, its discontinuity dimension is likely to minimize this potential bias. A more comprehensive analysis of the firms’ response goes beyond the scope of this paper and is left for future research.

(with an increase of less than 1.5% over the outcome mean at the end of the period) and statistically insignificant. The same emerges in column (b), where we exclude women who were exposed to both the 1987 and 1992 reforms and find an insignificant size effect of less than 1% over the outcome mean. The small expansions induced by each reform, therefore, do not seem to pose a problem of statistical power. Differently from Ruhm (1998), Ejrnæs and Kunze (2013), and Bailey et al. (2019), this finding does not lend support to the view that longer maternity leaves induce women to accumulate less work experience and, as a result, face lower labor market earnings and a lower likelihood of being in the upper pay echelon of their firms. Likewise, the same estimates do not lend support to the idea that mothers would benefit from prolonged leaves in terms of deferred wage progression.

A different statistical power concern in RD analysis might arise from limited sample size. This is unlikely in our case, because we use the universe of births around each reform which leads us to an average sample size of about 30,000 mothers for each reform in isolation. Nonetheless, we perform an additional exercise which increases our sample size considerably. That is, we first stack the data for the 1987–1992 reforms into one sample of more than 140,000 births, and then estimate (1) on this pooled sample.

Table 5 shows the estimates for the probability of being in the top decile and the probability of being in the C-suite by year since reform. Referring to 1, 5, 10, 15, 20, and 25 years since reform (or, equivalently, child’s age), the results offer a complete picture of short- and long-run effects. Notice that the estimates at 25 years can rely only on two reforms (the first two ones), which explains the slightly larger confidence intervals, whereas up to 20 years all policy interventions contribute to the estimation, except the 2005 expansion. Both sets of estimates confirm our previous results that the maternal leave extensions had no impact on either outcome. Moreover, for both outcomes, the average RD coefficients are generally small and statistically insignificant. We do observe a positive impact of 0.7 percentage points on mothers’ chances to be in the top earnings decile 20 years after the reform enactment, but this effect is isolated, modest in size (representing an increase of about 3% over the sample mean), and statistically significant only at the 10% level. Also, it is not mirrored by success in the boardroom. Similar results emerge if in the estimation sample we also include the births around the 1993 reform, or if we use a shorter bandwidth of three months around each reform, or if we consider the sample of first-time mothers.

Reiterating the previous benchmark results, we find that cumulating the effect of the first six reforms, which jointly prolonged leave from 18 to 35 weeks, neither improved the relative pay of mothers within their organizations, nor promoted them to positions of leadership in the corporate world both in the short and the long run. The same null result emerges when we combine each reform-specific sample into one larger sample. We interpret these results as evidence of no impact.

## 5.2 Heterogeneity

Although we find no evidence of an impact on mothers' top pay and leadership positions within firms, parental leave expansions might have affected women differently depending on their level of education, the number of children they had at the policy onset, or the type of firm or industry in which they worked. In what follows, we explore these possibilities.

*Education* — It is possible that better educated women are in high-pay jobs even before childbirth. If employers value the stability of job-to-worker matches, better educated mothers may use longer maternity leaves to climb to, or remain at, the top of their organizations' pay scale even after birth. The cross-country evidence presented by Olivetti and Petrongolo (2017) shows instead that longer parental leave is associated with wider earnings gaps among college-educated mothers. We thus repeat the previous analysis focusing only on women with university or higher qualifications for the probability of being in the top earnings decile intra-firm. The results in Appendix Figure A.11 confirm that even for college-educated women the extensions in maternity leave did not contribute, either positively or negatively, to the growth in female representation at the top of the intra-firm pay distribution.

*Parity* — In line with Dahl et al. (2016), we find that the leave expansions have no impact on birth timing. We also find no evidence of any effect on mother's age at first birth and on spacing between first and second births and completed fertility. Nonetheless, as shown by Lalive and Zweimüller (2009), there might be effect heterogeneity of the reforms by birth order. Larger families may impose higher career costs on the main carer, usually the mother, even when women have high earnings potential or are highly educated (Francesconi, 2002; Guryan, Hurst, and Kearney, 2008; Adda, Dustmann, and Stevens, 2017). Some studies emphasize the role of scale effects, and argue that the marginal cost of second (and subsequent) children is usually less than that of the first child (e.g., Browning, 1992).

We therefore re-estimate equation (1) for mothers who are exposed to the reforms when giving birth to their first child separately from those who have their second child. The results do not change when we consider higher parity households (third or fourth child). The estimates on the probability of being in the top within-firm earnings decile among first-time mothers are shown in Appendix Figure A.12.

We find that the 1989 reform pushed this probability up by approximately 2 percentage points from 2006 to 2011, when first-born children were between 17 and 22 years old. The average impact over the post-reform period however is smaller, around 1 percentage point, and statistically insignificant. The responses to all the other reforms are similar to those found for all mothers, indicating no systematic effect of paid leave on mothers' chances of being in top pay jobs in their companies.<sup>31</sup>

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<sup>31</sup>We also find no impact of any of the extensions on first-time mothers' chances of being in the middle

The same null results across all outcomes emerge also for second-time mothers and for mothers of only-children, which are not reported for brevity. The findings for mothers of only children as well as first-time mothers are also confirmed by the diff-in-disc estimates shown in columns (c) and (d) of Table 4, respectively. Taking stock of all these results, we conclude that the extensions to paid leave did little to affect either first- or second-time mothers' chances of entering, or staying in, the top pay decile of their organizations.

*Firm Size* — Career opportunities may vary substantially with firm size, with wage spreads rising with the number of workers in order to compensate for the increased competition for higher-ranked jobs and with larger firms having better defined hierarchies and internal labor market structures (Gabaix and Landier, 2008; Gayle et al., 2015; Huitfeldt et al., 2022). We distinguish three sets of firms based on the number of employees: small (4–9), medium (10–99), and large (100 or more).<sup>32</sup>

The results in Appendix Figures A.16–A.19 show that the 1992 reform raised the probability to be in the top earnings decile for mothers in large firms, from 2008 to 2013. The 1990 reform instead reduced this probability for women employed in medium-size firms between 2003 and 2005. We cannot detect any other significant effect. The aggregate effects are never statistically significant, irrespective of firm size and of the reform we focus on, and so is the cumulative effect for large firms shown in column (e) of Table 4. We also find no significant heterogeneous impact by firm size on leadership positions. Regardless of the complexity of the internal labor market which may be associated with firm size, there is therefore no evidence that the expansions to parental leave influenced mothers' chances to reach their companies' upper earnings decile or to be represented in their executive boards.

*Industry* — Another dimension of heterogeneity through which there might be strong gender wage differentials is the type of industry in which men and women work. Parental leave expansions could magnify or attenuate such differentials.

Goldin (2014) and Goldin and Katz (2016) argue that the extent of temporal job flexibility, without substantial wage penalties, largely depends on industry- or occupation-specific technological features, including characteristics which determine the need for workers to be available at particular (nonstandard) times, the degree of close substitutability among workers, the flexibility of the job with regard to scheduling, and the need for an employee to keep in touch with other workers (above and/or below their position in their own organization) or specific groups, such as clients and stakeholders. Goldin

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or bottom of the intra-firm earnings distribution. Nor can we detect any significant effect on their chances of filling positions of leadership in their organizations. See Appendix Figures A.13–A.15.

<sup>32</sup>Using a sample of approximately 2000 Norwegian firms, Huitfeldt et al. (2022) focus their analysis on “large” firms, which are defined as those with at least 30 employees. Our results remain qualitatively unaltered if we redefine large firms using this alternative definition. Notice also that, because of sample size issues, the analysis cannot be performed on small firm workers. Aggregating them to workers in medium size firms does not change our estimates.

(2014) also provides evidence that there is a strong negative relationship between earnings gender gaps and the elasticity of earnings with respect to hours worked. Larger elasticities correlate positively with the above mentioned technological characteristics when these imply more time pressure. Larger elasticities are also typically observed among workers in business and financial services, while lower elasticities emerge in technology, science and health occupations.

To match those industrial sectors in our data, we use detailed information from the 5-digit 2007 Standard Industrial Classification, and define three separate groups of workers, one in finance (including banking and insurance), another in health (including physicians and dentists) and the last one in business and technology (including R&D, market research, advertisement, and business consultancy). These can be reliably constructed from 1998 onwards. We repeated our benchmark analysis for each industrial group separately. The estimates reported in Appendix Figures A.20–A.22 show that none of the leave reforms had an impact on mothers’ chances to be in the top earnings decile of their companies, irrespective of the grouping of industries.

### 5.3 Channels

Prolonged maternity leaves might have influenced mothers’ labor market performance in ways other than through pay or executive leadership, e.g., through hours of work, promotion opportunities, or job mobility. In what follows, we explore such mechanisms.

*Hours of Work* — As emphasized by Goldin (2014) and Goldin and Katz (2016), some positions have a highly convex pay structure with regard to hours worked, requiring a high degree of workplace commitment and little flexibility to combine work and family life. Such positions are generally held by highly skilled workers at the upper end of the earnings distribution. We ask if the leave reforms had an impact on the hours worked by mothers whose labor income was in the top intra-firm earnings decile.

The results in Appendix Figure A.23 show that women’s hours are unaffected by the parental leave expansion policies, soon after birth as well as in the longer run. Repeating the exercise for all mothers, and not just for those at the top tier of pay within their companies, leads to the same null result (see Appendix Figure A.24). The reforms, therefore, did not alter the strong time bind that locks mothers to jobs, regardless of the pay rank within their companies.

*Promotions* — Male-female wage gaps, especially at the top of an organization, may be driven by differential employer promotion standards due to gender differences in the probability of leaving — or taking career interruptions from — the firm (Lazear and Rosen, 1990). As the cost to employers of job interruptions is greater for workers in high-level jobs than in the low-level jobs, given ability, males are promoted to high-level jobs over females who are equally productive in low-level jobs. Longer breaks arising from the



leave extension reforms will then imply even lower promotion probabilities for mothers at the upper end of the earnings distribution.

Most of the empirical literature has focused on gender differences in promotions.<sup>33</sup> Antecol et al. (2018) is the only study to date that explores the impact of a family policy on promotions by gender. They find that gender-neutral tenure clock stopping policies introduced in the top-50 US departments of economics (in which untenured assistant professors are allowed to stop their tenure clock for one year after childbirth) decrease the probability that a female assistant professor gets tenure where she was initially hired while male tenure chances rise. There is, however, no direct evidence of the impact of parental leave policies on promotions.

To analyze this relationship, we use the unique data described in Section 3 (for more details, see Kunze and Miller [2017]). The results reported in Appendix Figure A.25 document that mothers experienced neither greater nor lower chances of promotion in response to the reforms. This null result is also confirmed in Appendix Table A.3 by the cumulative effect obtained from the diff-in-disc estimator. The same findings also emerge when we consider promotions associated with a change of employers (see Appendix Figure A.26). We also examined the possibility that the reforms affected mothers' chances to be in the top earnings decile within their firms if they were internally promoted at least once after childbirth. The estimates (not reported for brevity) show no evidence of an impact.

*Firm Mobility* — In addition to within-firm mobility, mobility across firms is considered to be a major contributor to wage growth over workers' careers (e.g., Postel-Vinay and Robin, 2002; Del Bono and Vuri, 2011; Bagger et al., 2014; Adda and Dustmann, 2022). Card et al. (2016) explore this channel in combination with the possibility that women are offered systematically lower wages by their employers to study gender disparities in the Portuguese labor market. They find that both channels explain approximately one-fifth of the cross-sectional gender wage gap.

We stratify the sample into two groups of mothers, those who stayed with the same employer and those who moved at least once from one employer to another after childbirth over the sample period. The estimates in Appendix Figures A.27 and A.28 refer to movers. Overall, we find no evidence that the leave extensions affected mothers' probability of reaching the upper pay decile of their company if they moved across firms. The 1992 reform did raise this probability by about 2 percentage points from 2007 to 2011, and so did the 1989 reform between 2006 and 2008, while the 1990 and 1991 extensions led to a reduction between 2003 and 2005 and between 2001 and 2003, respectively.

The general reading of this evidence is that each of the eight reforms did not contribute much to the increased representation of mothers in the upper echelons of their firms'

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<sup>33</sup>The results from this literature are mixed. Some find lower rates of promotion for women than for men with similar observed characteristics (e.g., McCue, 1996; Cassidy et al., 2016), others find the opposite (e.g., Booth et al., 2003; Gayle et al., 2012), and still others find no gender differences (e.g., Giuliano et al., 2011).

earnings, even for women who moved across firms. The same can be said for mothers' chances to be in their firms' board or top executive positions. A null average impact result is confirmed for both outcomes across all reforms. There is also no evidence of a cumulative impact, as shown in column (f) of Table 4. The same set of null results emerges among mothers who stayed with the same employer over the entire period (not reported for the sake of brevity).

*Conditional Transitions Across the Income Distribution* — Mobility to top earnings may be hard, especially for workers who start low in the pecking order (Bagger et al., 2014). To account for this possibility, we examine transitions to the top earnings decile from specific parts of the earnings distribution. In particular, we analyze the impact on the likelihood that mothers have to reach the top pay decile in their own firms in a given year if they were in the second highest decile in the year before each reform. The estimates shown in Appendix Figure A.29 reveal no effect of the reforms on this likelihood. Repeating the analysis when we condition on other starting positions in the intra-firm earnings distribution leads to the same conclusion (not reported for brevity). Thus, the reforms did not affect mothers' probability of reaching the top echelons of their companies' pay, irrespective of their initial position in the firm's earnings distribution.

## 5.4 Fathers

Norway offers an interesting case because it introduced paternal leave, reserving a quota of the total leave for fathers. Two of the eight reforms under analysis provided paternal leave. The 1993 reform gave fathers a four-week quota for the first time. This was extended to five weeks with the 2005 reform.

As evidenced in Figure 7, none of the pre-1993 reforms had an impact on the probability that fathers be in the top earnings decile within their firms. Although this may be unsurprising (because such reforms affected only mothers), it clearly suggests that those reforms generated no wage spillover for fathers. But even the two reforms that provided paid leave for fathers did not affect intra-firm male earnings success.<sup>34</sup> The lack of an impact emerges also in the middle and at the bottom end of the fathers' earnings distribution (see Appendix Figures A.30 and A.31).

Similarly, none of the reforms had an impact (either positive or negative) on fathers' chances to be a member of company boards or in top executive posts, as presented in Appendix Figure A.32. This is true also for the likelihood of being in a managerial occupation, except for the 1991 and 2005 reforms, which respectively reduced and increased that probability by 2–4 percentage points, albeit only temporarily (see Appendix Figure A.33). Taken all together, therefore, there is little evidence of a systematic impact that

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<sup>34</sup>This result does not change even if the ITT estimates are adjusted by the change in paternal leave take-up around the two reforms.

an increased generosity in parental leave had on fathers' intra-firm top pay or leadership jobs.<sup>35</sup>

## 6 Gender Pay Inequality Among Coworkers and Among Partners at the Top

None of the eight reforms influenced the relative pay position of mothers (or fathers) within their organizations. Extended parental leave nonetheless could have affected women's economic power in other ways. For instance, it could have changed the relative earnings power of mothers with respect to their male coworkers, including fathers who are not their partners as well as non-fathers. In addition, the expectation of many proponents of government-mandated paid parental leave extensions was that they would have promoted more intra-household gender equality (Vollset, 2011; Dahl et al., 2016). For both dimensions of inequality — within firms and within households — we present results using full-time equivalent monthly wages, which adjust for differences in hours worked.

*Gender Pay Gaps Within Firms* — Figure 8 shows the estimates of the impact of the leave extensions on the intra-firm gender wage gap,  $\Delta$ , for women and men who are in the top decile of earnings within their firms. A positive estimate captures a reform-led wage improvement in favor of mothers, while a negative estimate reflects a worsening with respect to their male colleagues' wages. There is clear evidence of a zero effect. Not only are the estimates quantitatively small, they are also never statistically significant at conventional levels. This is confirmed by the average effects for each reform, over the whole post-reform period or 10 years after every intervention, reported at the bottom of each panel in the figure. Considering the pay gap among all women and men, and not just those in the top decile, leads to the same null result with even tighter standard errors (not reported for brevity).

Figure 3 documents that  $\Delta$  has declined sharply in the case of annual earnings. We, thus, repeated the previous exercise on the total earnings measure, abstracting from hours worked. The estimates unmistakably confirm that the leave reforms played no significant role in the reduction in the annual earnings gap between mothers and their male coworkers, whether we look at top earners (Appendix Figure A.34) or across the whole earnings distribution (Appendix Figure A.35). The similarity of the impacts found with the two pay measures is likely to reflect the null effect on hours worked, which we have illustrated in subsection 5.3.

*Gender Pay Gaps Within Households* — The results for the effect of the parental leave

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<sup>35</sup>Despite these null results, we cannot exclude that the daddy's quotas might have had an impact on beliefs, yielding counter-stereotypical behavior, as postulated by Bertrand (2020). This is left for exploration in future research.

policies on intra-household wage inequality,  $G$ , are reported in Figure 9. This refers to women who are in the top decile of the full-time equivalent monthly wages distribution within their companies. The interpretation of reform-led pay improvements/worsenings is the same as the one we used before for  $\Delta$ . Overall, we find no impact of the leave extensions. The estimates are typically small and statistically indistinguishable from zero, and this is clearly documented by the aggregate impacts reported at the bottom of each panel. As shown in Appendix Figure A.36, the same null results emerge if we consider all women, and not just those in the top decile of their firms' earnings distribution.

We also detect no changes in the intra-household gap when we look at annual earnings for both mothers in the top decile and all mothers (see Appendix Figures A.37 and A.38). The null result on top earners is quite revealing about the neutrality of the leave extensions to change the intra-household pay gap, despite the considerable progress observed in Figure 4 among top female earners since the late 1990s.

## 7 Conclusion

This paper provides what we believe is the first comprehensive effort to assess whether paid parental leave extensions harm or sustain mothers' economic progress to the top of the career ladder over time and whether they magnify or weaken gender pay inequality within firms and within households up to a quarter of a century after childbirth. We pay special attention to the experience of top earners, using a new measure based on mothers in the upper decile of their firms' earnings distribution, and top executives. The Norwegian case is particularly important. Norway is presently considered to be a beacon of women's rights and gender equality (but, as we have documented, it was not in the 1980s and 1990s). It has passed a series of major parental leave reforms which extended the leave duration for mothers and introduced a daddy's quota and for which there is essentially universal take-up. Norway also allows access to unique high quality data covering the entire population of workers and firms over a long time period with no differential attrition due to nonresponse or income topcoding.

We emphasize six main results. First, both the expansion of paid maternity leave from 18 to 38 weeks and the introduction of a quota of leave reserved to fathers had no effect on mothers' pay, whether we consider all mothers or those who at the time of birth were in the top decile of the salary distribution for their age, leaving unchanged their chances to reach the top of their companies' pay ranking in the short, medium or long run. Second, the reforms neither lifted nor compromised mothers' economic empowerment, as we find no evidence of a significant change in the likelihood of entering the C-suite of their organization. Third, the expansions had no heterogeneous effect on intra-firm pay success across a number of characteristics, such as maternal education, number of children (or child parity), firm size, and industry.

Fourth, the extensions also left unaffected other outcomes which could have triggered a change in within-firm pay ranking, including hours worked, internal promotions, and firm mobility. Fifth, none of the eight policy interventions had any impact of fathers' pay, and the presence of daddy's quotas did not affect mother's economic position, whether negatively or positively. Finally, the leave reforms had no effect on the gender pay gaps between mothers and their male colleagues, whether at the top or across the whole pay distribution within their companies. We find the same null result in the case of the pay differentials between mothers in top positions and their partners.

Taken together, our results suggest that either short or long paid parental leave does not help mothers to break the glass ceiling; but it also does not hinder their chances of success. The case for long paid maternity leave provisions, therefore, cannot be made on the basis of pay advancement for mothers or redressing gender pay imbalances, especially at the top of the earnings distribution. Even if, as in the case of Norway, the leave legislation guarantees a generous income replacement and strong job continuity, these features do not seem to be enough to power economic progress for mothers both in the short and the long run. At the same time, however, long leave enables mothers to retain a strong labor market attachment after childbirth and this, in turn, means no wage loss associated with longer job interruptions.

In times of hard budget constraints for governments worldwide, our findings are relevant to countries that are considering reforms in paid parental leave or are planning to align with the UN policy agenda which has listed gender equality among its key sustainable development goals for 2030. It may be important to revisit our evaluation in the post-Covid-19 context, which seems to have deepened old divides in employment and child care between mothers and other members of society. We view our current contribution and its findings as a significant input into this future policy discussion.

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Table 1: Parental Leave Reforms in Norway, 1978–2006

| Reform Date | Total Weeks of Leave | Income Replacement | Maternal Quota | Paternal Quota |
|-------------|----------------------|--------------------|----------------|----------------|
| 01.07.1978  | 18                   | 100%               | 6 weeks        |                |
| 01.05.1987  | 20                   | 100%               | 6 weeks        |                |
| 01.07.1988  | 22                   | 100%               | 6 weeks        |                |
| 01.04.1989  | 24 (30)              | 100 (80)%          | 6 weeks        |                |
| 01.05.1990  | 28 (35)              | 100 (80)%          | 6 weeks        |                |
| 01.07.1991  | 32 (40)              | 100 (80)%          | 2+6 weeks      |                |
| 01.04.1992  | 35 (44.4)            | 100 (80)%          | 2+6 weeks      |                |
| 01.04.1993  | 42 (52)              | 100 (80)%          | 3+6 weeks      | 4 weeks        |
| 01.07.2005  | 43 (53)              | 100 (80)%          | 3+6 weeks      | 5 weeks        |

*Note:* ‘Maternal Quota’ refers to the leave reserved to mothers, which consists of 6 weeks after childbirth. Since 1991, additional 2 or 3 weeks must be taken before childbirth. ‘Paternal Quota’ refers to the leave reserved to fathers after childbirth, which consists of 4 weeks after the 1993 reform and 5 weeks after the 2005 reform.

Table 2: Mean Characteristics of Women and Men in the RD Sample Around the 1993 Reform

| Years since reform                       | 0       | 0       | 10      | 10      | 20      | 20      |
|--|---------|---------|---------|---------|---------|---------|
|  | Women   | Men     | Women   | Men     | Women   | Men     |
| Age (years)                              | 29.97   | 32.35   | 40.03   | 42.21   | 49.73   | 51.35   |
| Education (years)                        | 13.52   | 13.47   | 13.59   | 13.51   | 13.60   | 13.50   |
| Married or cohabiting (=1 if yes)        | 0.60    | 0.60    | 0.69    | 0.71    | 0.64    | 0.68    |
| Total number of children                 | 1.82    | 1.94    | 2.49    | 2.60    | 2.55    | 2.66    |
| Age at first birth                       | 26.41   | 28.24   | 26.38   | 28.20   | 26.33   | 27.90   |
| Weekly hours worked                      |         |         | 32.55   | 36.70   | 33.08   | 36.31   |
| Years of work experience                 | 9.98    | 12.50   | 20.05   | 22.42   | 29.78   | 31.71   |
| Years of firm tenure                     | 3.75    | 4.08    | 5.51    | 6.35    | 8.90    | 9.69    |
| Number of jobs held                      | 2.90    | 2.73    | 3.05    | 2.87    | 3.09    | 3.00    |
| Annual earnings (in NOK)                 | 176,678 | 289,568 | 252,036 | 416,042 | 376,022 | 566,332 |
| Proportion in top earnings decile        | 0.16    | 0.35    | 0.21    | 0.48    | 0.24    | 0.57    |
| Proportion in top earnings-by-age decile | 0.14    | 0.46    | 0.18    | 0.55    | 0.21    | 0.71    |
| Proportion in executive positions        |         |         | 0.19    | 0.51    | 0.30    | 0.65    |
| Proportion in managerial occupations     |         |         |         |         | 0.31    | 0.55    |
| Annual rate of internal promotions       | 0.04    | 0.08    |         |         |         |         |
| Number of individuals                    | 32,177  | 36,853  | 28,585  | 33,273  | 28,890  | 29,883  |

*Note:* Figures refer to all mothers and fathers who had a child in the six months around the April 1993 reform. All parents are followed over time. Annual earnings are real and deflated with CPI (1998=100). The reported outcome means indicate the mean proportions of mothers and fathers among all employees within the top decile of their firm's earnings distribution or in executive and managerial positions in a given year. The outcome means are computed for all mothers and mothers for the indicated years, rather than just for the RD sample.

Table 3: Balance Tests

| Sample<br>Outcome                 | <i>All Women</i> |                  | <i>First-birth</i> |                  | <i>Large firms</i> |                  | <i>Movers</i> |                  |
|-----------------------------------|------------------|------------------|--------------------|------------------|--------------------|------------------|---------------|------------------|
|                                   | <i>Age</i>       | <i>Education</i> | <i>Age</i>         | <i>Education</i> | <i>Age</i>         | <i>Education</i> | <i>Age</i>    | <i>Education</i> |
| Reform 1987                       | -0.059           | -0.198***        | 0.048              | -0.142           | -0.282             | -0.217           | -0.047        | -0.188***        |
| <i>F</i> -test ( <i>p</i> -value) | 0.00252          |                  | 0.125              |                  | 0.0536             |                  | 0.0065        |                  |
| Observations                      | 24528            |                  | 12481              |                  | 9100               |                  | 22311         |                  |
| Reform 1988                       | -0.050           | -0.104           | -0.034             | -0.069           | -0.015             | -0.148           | -0.000        | -0.099           |
| <i>F</i> -test ( <i>p</i> -value) | 0.0911           |                  | 0.419              |                  | 0.159              |                  | 0.125         |                  |
| Observations                      | 28209            |                  | 14098              |                  | 10377              |                  | 25620         |                  |
| Reform 1989                       | -0.088           | 0.012            | 0.012              | 0.030            | -0.126             | -0.060           | -0.079        | 0.009            |
| <i>F</i> -test ( <i>p</i> -value) | 0.834            |                  | 0.711              |                  | 0.561              |                  | 0.884         |                  |
| Observations                      | 30217            |                  | 14866              |                  | 10622              |                  | 27290         |                  |
| Reform 1990                       | -0.103           | -0.079           | 0.025              | -0.039           | -0.260             | -0.235**         | -0.013        | -0.064           |
| <i>F</i> -test ( <i>p</i> -value) | 0.157            |                  | 0.628              |                  | 0.0169             |                  | 0.273         |                  |
| Observations                      | 32366            |                  | 15274              |                  | 10948              |                  | 29260         |                  |
| Reform 1991                       | -0.092           | -0.104           | -0.054             | -0.163**         | -0.134             | -0.133           | -0.059        | -0.111           |
| <i>F</i> -test ( <i>p</i> -value) | 0.0592           |                  | 0.0443             |                  | 0.187              |                  | 0.0544        |                  |
| Observations                      | 33124            |                  | 14856              |                  | 10786              |                  | 29913         |                  |
| Reform 1992                       | 0.042            | 0.041            | -0.014             | 0.055            | -0.018             | -0.023           | 0.005         | 0.010            |
| <i>F</i> -test ( <i>p</i> -value) | 0.452            |                  | 0.499              |                  | 0.822              |                  | 0.861         |                  |
| Observations                      | 33711            |                  | 14682              |                  | 10666              |                  | 30303         |                  |
| Reform 1993                       | -0.170           | 0.012            | -0.166             | -0.086           | 0.208              | -0.048           | -0.178        | -0.003           |
| <i>F</i> -test ( <i>p</i> -value) | 0.83             |                  | 0.301              |                  | 0.626              |                  | 0.952         |                  |
| Observations                      | 33912            |                  | 14155              |                  | 10521              |                  | 30541         |                  |
| Reform 2005                       | 0.027            | -0.046           | 0.113              | -0.069           | 0.022              | -0.120           | -0.133        | -0.068           |
| <i>F</i> -test ( <i>p</i> -value) | 0.344            |                  | 0.373              |                  | 0.171              |                  | 0.232         |                  |
| Observations                      | 39645            |                  | 16209              |                  | 12711              |                  | 29270         |                  |
| Joint <i>p</i> -value             | 0.3594           |                  | 0.3935             |                  | 0.3246             |                  | 0.5073        |                  |

*Note:* All estimates are obtained from a linear RD model with triangular weights using a bandwidth of 6 months before and after each reform. In each panel and for all samples and subsamples, ‘*F*-test (*p*-value)’ refers to the *p*-value of the *F*-test that all coefficients are jointly statistically significant. ‘Joint *p*-value’ reported at the bottom of the table refers to the *p*-value of the test that all 16 coefficients across reforms are statistically significant. \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

Table 4: Difference-in-Discontinuities Estimates, 1987–2004

|                    | Probability of Being in Top Within-Firm Earnings Decile |                                   |                              |                         |                    |                  |
|--------------------|---|-----------------------------------|------------------------------|-------------------------|--------------------|------------------|
|                    | All mothers<br>(a)                                      | Excluding<br>1987 and 1992<br>(b) | First-time<br>Mothers<br>(c) | Only<br>children<br>(d) | Large firms<br>(e) | Movers<br>(f)    |
| $\theta_0$         | 0.003<br>(0.004)  | 0.002<br>(0.004)                  | 0.004<br>(0.005)             | -0.006<br>(0.007)       | 0.006<br>(0.007)   | 0.002<br>(0.004) |
| Observations       | 744,721   | 683,133                           | 328,657                      | 118,379                 | 241,957            | 676,473          |
| Outcome mean, 2004 | 0.224   | 0.223                             | 0.224                        | 0.303                   | 0.186              | 0.234            |
| Outcome mean, 2013 | 0.277   | 0.276                             | 0.277                        | 0.338                   | 0.215              | 0.288            |

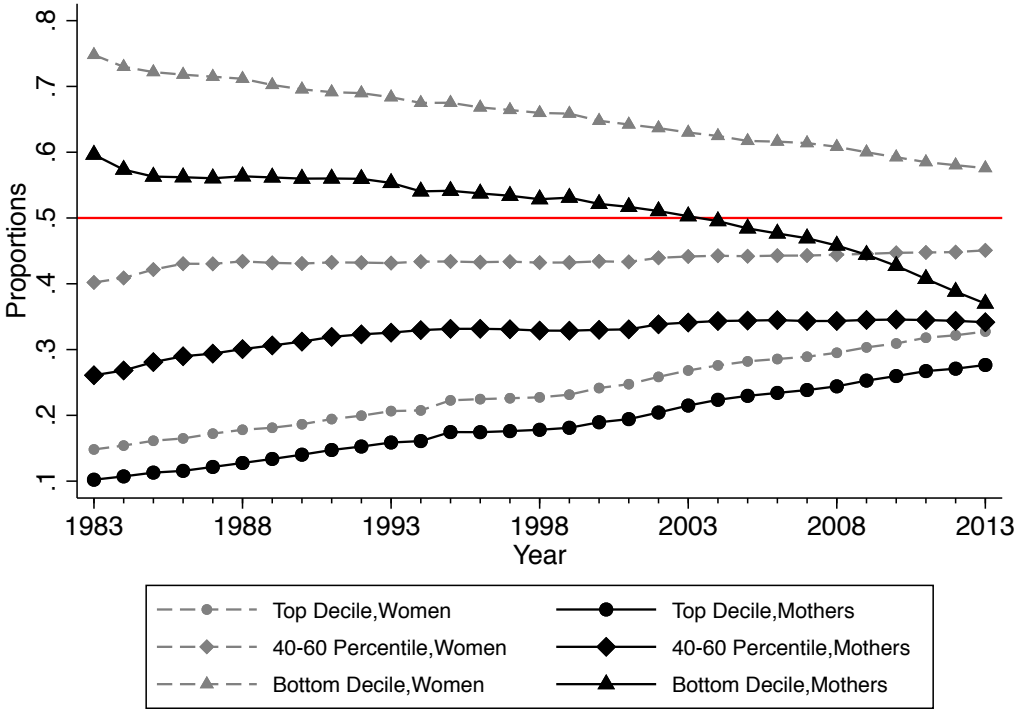
*Note:* The time period under analysis is restricted to 1987–2004, in order to eliminate confounding by the extension of paternity leave in 2005. In the estimation of (2), we use a linear control function of the running variable on each side of the cutoff with a bandwidth of 6 months. Controls include a cubic function of age and years of education measured in the pre-reform year, as well as municipality fixed effects. Robust standard errors are clustered at the firm level. Outcome means in the last two rows report the proportion of mothers among all employees within the top decile of their firms' earnings distribution in 2004 and 2013.

Table 5: Mothers' Probability of Being in the Within-Firm Top Earnings Decile and in the C-Suite, Pooled Sample

| Years since reform  | 1       | 5       | 10      | 15      | 20      | 25      |
|---|---------|---------|---------|---------|---------|---------|
| <i>Outcome: Probability of Being in the Top Earnings Decile</i> |         |         |         |         |         |         |
| RD Coefficient  | -0.0001 | 0.0009  | 0.003   | 0.003   | 0.007*  | -0.003  |
| SE  | (0.001) | (0.002) | (0.002) | (0.002) | (0.004) | (0.005) |
| <i>Outcome Mean</i>   | 0.088   | 0.134   | 0.169   | 0.237   | 0.286   | 0.306   |
| <i>Outcome: Probability of Being in the C-suite</i>             |         |         |         |         |         |         |
| RD Coefficient  |         |         |         | 0.005*  | 0.0006  | 0.007   |
| SE  |         |         |         | (0.003) | (0.004) | (0.008) |
| <i>Outcome Mean</i>   |         |         |         | 0.265   | 0.320   | 0.341   |
| <i>Observations</i>   | 149,808 | 140,836 | 145,858 | 155,121 | 154,991 | 40,816  |

*Note:* Estimates are obtained on the pooled sample of the reform-specific samples of the first six reforms, 1987–1992.

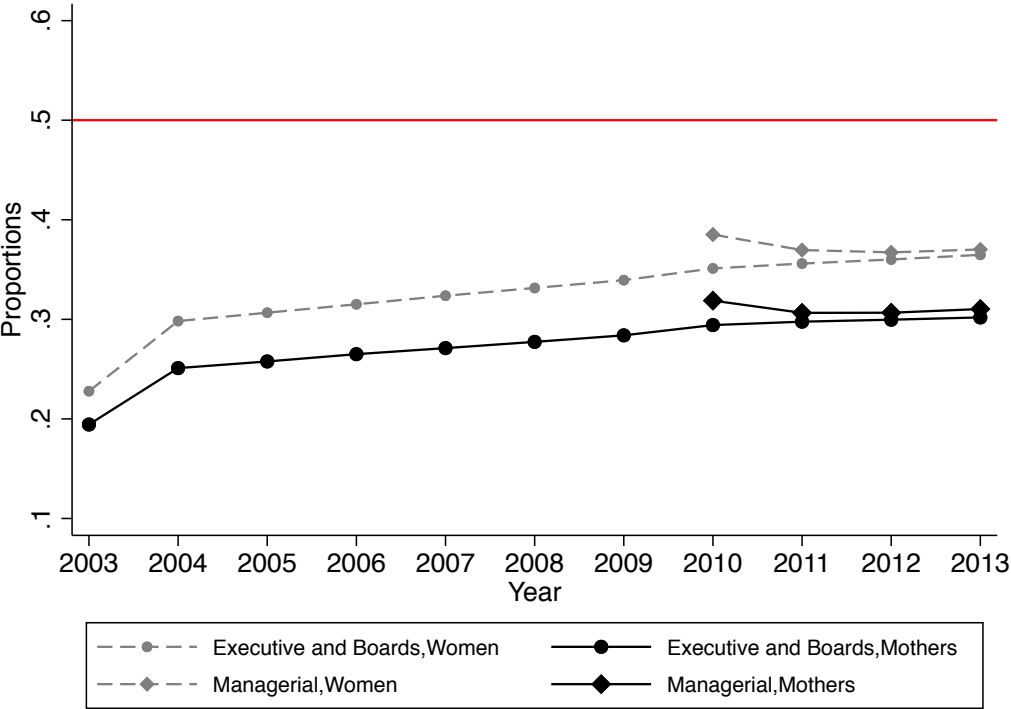
Figure 1: Proportions of Women and Mothers in the Top, Middle and Bottom of the Within-Firm Earnings Distribution, 1983–2013



*Note:* The sample includes the population of women aged 18–60 with completed education cycles. The horizontal line at 0.5 indicates equality between women (or mothers) and the rest of the population in the same age brackets.

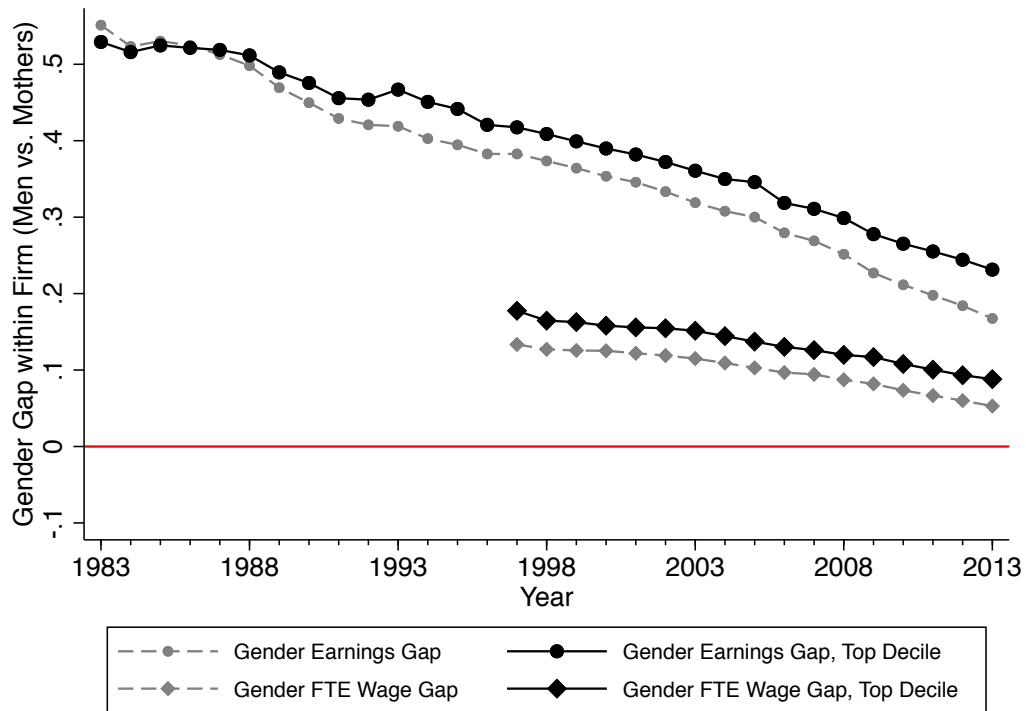


Figure 2: Proportions of Women and Mothers in Executive (2003–2013) and Managerial Positions (2010–2013)



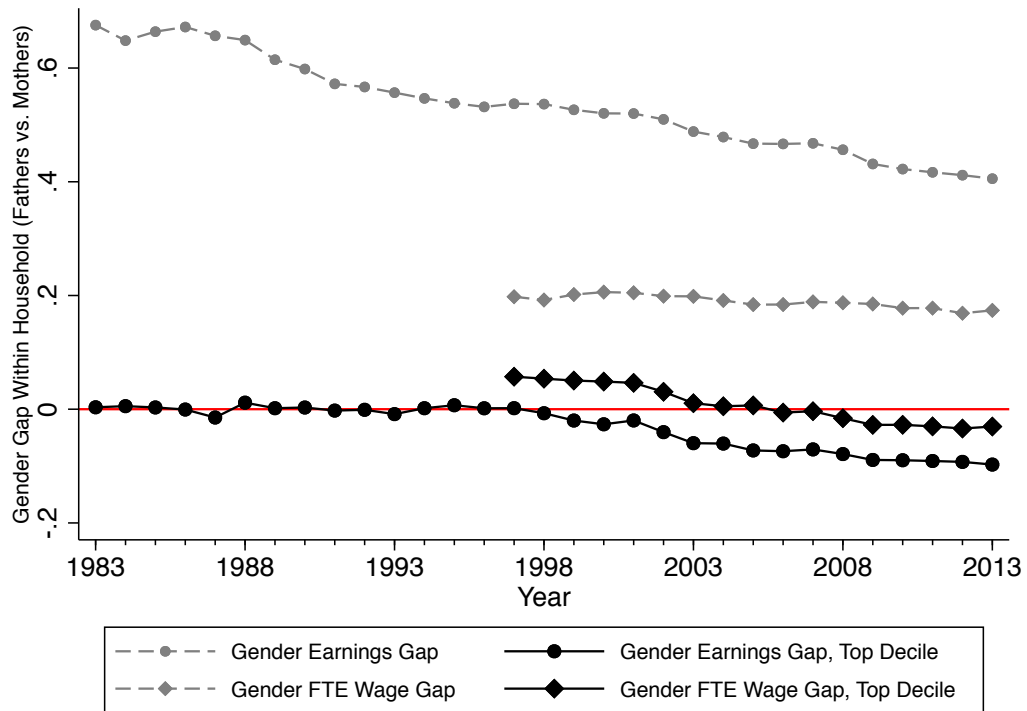
Note: See the note to Figure 1 and the text for more details.

Figure 3: Gender Earnings Gaps for Mothers Within the Firm



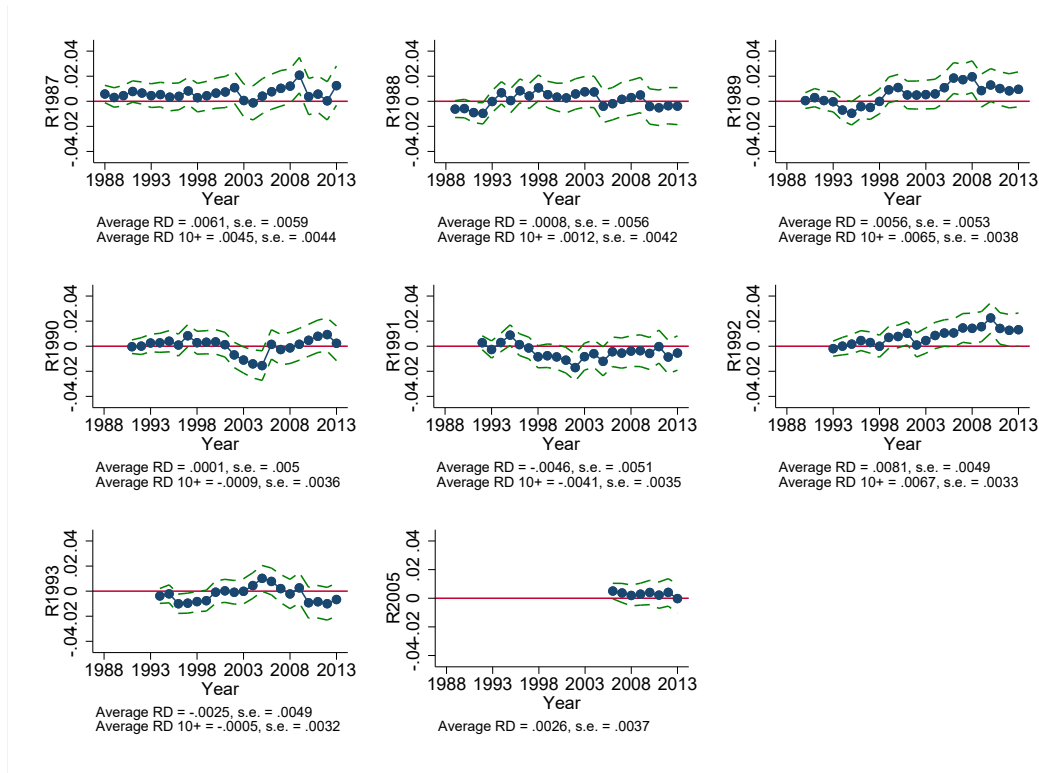
*Note:* ‘Gender Earnings Gap’ is calculated using annual earnings for all mothers and all men in a given year. ‘Gender FTE Wage Gap’ is computed using full-time equivalent wages (available from 1997 onwards). The figures with ‘Top Decile’ report the corresponding gaps for mothers and men in the top decile of the firm’s earnings distribution. All figures are averages across all firms in the sample. The horizontal line at 0 indicates where gender equality is achieved.

Figure 4: Gender Gaps Within the Household



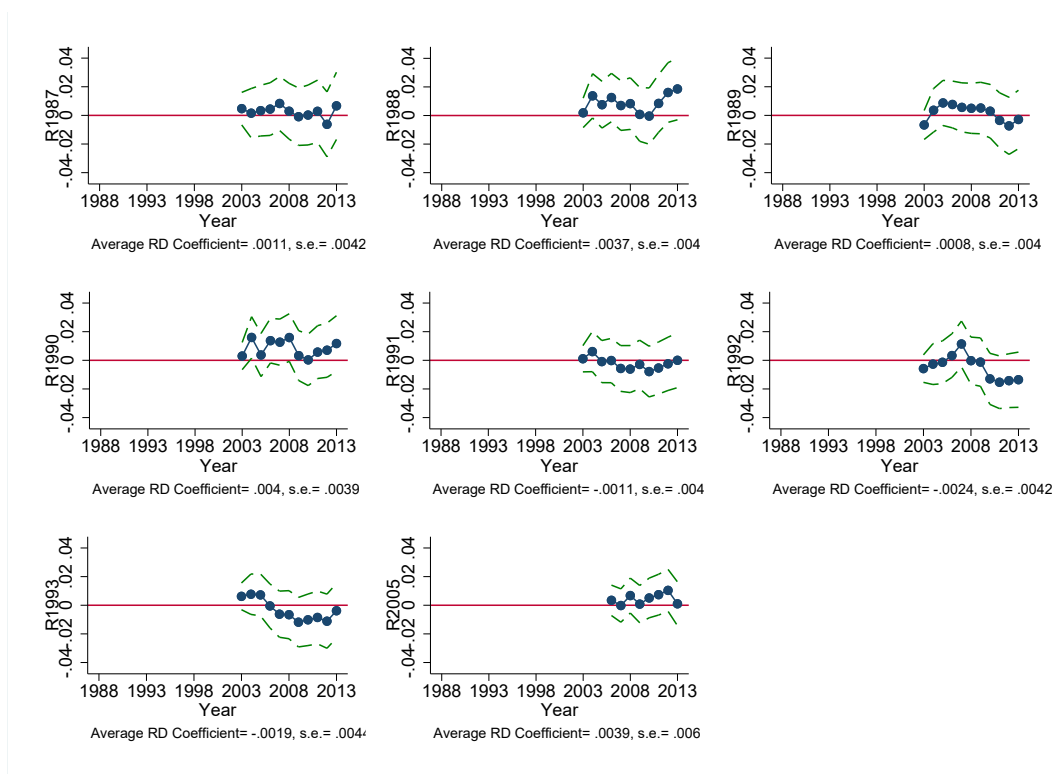
*Note:* The figure shows intra-household gaps separately for all mothers and for mothers in the top earnings decile using annual earnings and full-time equivalent wages. For other details, see the note to Figure 3.

Figure 5: Mothers' Probability of Being in the Top Earnings Decile of their Firms



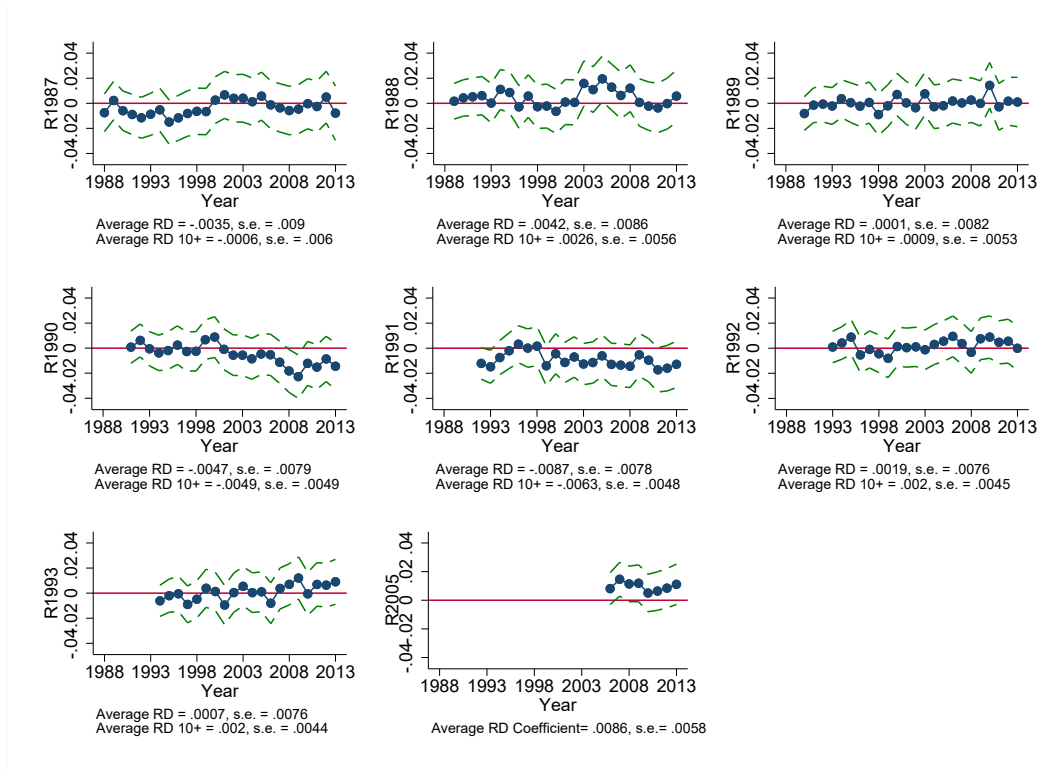
*Note:* Each panel shows the estimated RD coefficients (as dots) for each reform by post-reform calendar year (or, equivalently, by child age). The dashed line around the coefficients are the 95% confidence intervals. Estimates are obtained from (1), using a linear RD model with triangular weights and selecting the sample of mothers whose children were born 6 months before and 6 months after each reform. The sample of analysis is restricted to mothers. At the bottom of each panel, we report the average RD coefficient and its standard error, which are obtained as the weighted sum of the yearly RD coefficients and standard errors, weighted by the number of observations in a given year (or, equivalently, at a given child age) divided by the total number of observations in the entire post-reform period for that specific reform. The outcome variable is a binary indicator that a mother is in the top earnings decile within firm.

Figure 6: Mothers' Probability of Being in Top Executive or Board Director Posts



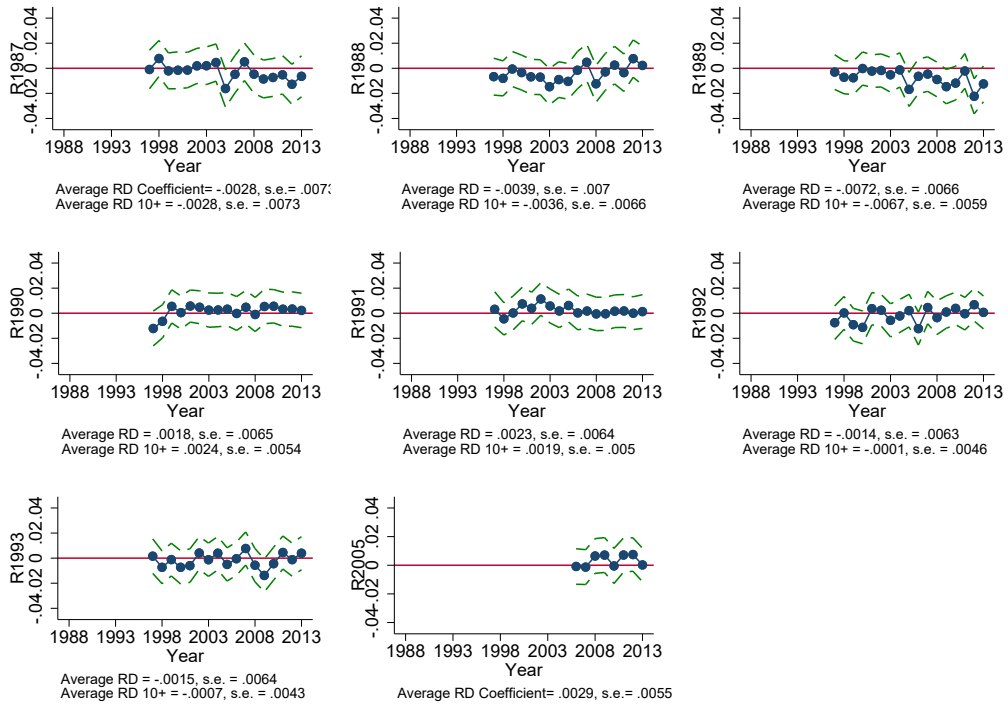
*Note:* The outcome variable is a binary indicator that a mother holds a CEO, CFO, or board director post. For other details, see the note to Figure 5.

Figure 7: Fathers' Probability of Being in the Top Earnings Decile of their Firms



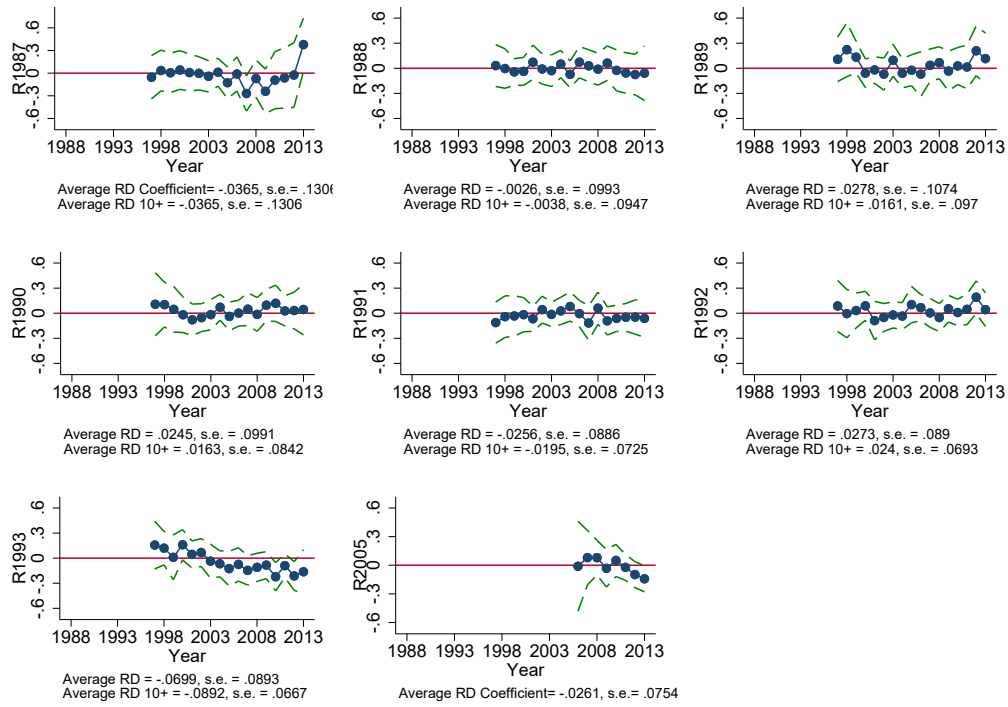
*Note:* The outcome variable is a binary indicator that a father is in the top earnings decile within firm. The sample of analysis is restricted to fathers. For other details, see the note to Figure 5.

Figure 8: Within-Firm Gender Wage Gap at the Top Decile



*Note:* The outcome variable is intra-firm gender pay gap computed using full-time equivalent monthly wages in top earnings decile within each firm in the sample. For other details, see the note to Figure 5.

Figure 9: Within-Household Gender Wage Gap at the Top Decile



*Note:* The outcome variable is the gender wage gap between women at the top earnings decile of their firms and their partners, computed using full-time equivalent monthly wages. For other details, see the note to Figure 5.



# Expansions in Paid Parental Leave and Mothers' Economic Progress

## Appendix For Online Publication Only

August 31, 2022

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Table A.1: Mean Characteristics of Top Earning Women and Men in the RD Sample Around the 1993 Reform

| <i>Child's age</i>                 | 0       | 0       | 10      | 10      | 20      | 20      |
|------------------------------------|---------|---------|---------|---------|---------|---------|
|                                    | Women   | Men     | Women   | Men     | Women   | Men     |
| A. Top 10% Earners                 |         |         |         |         |         |         |
| Age (years)                        | 32.09   | 34.94   | 41.69   | 43.09   | 50.55   | 51.71   |
| Education (years)                  | 15.43   | 15.02   | 15.35   | 14.85   | 15.16   | 14.73   |
| Married or cohabiting (=1 if yes)  | 0.64    | 0.72    | 0.72    | 0.80    | 0.67    | 0.75    |
| Total number of children           | 1.80    | 2.24    | 2.52    | 2.69    | 2.61    | 2.72    |
| Age at first birth                 | 28.58   | 29.33   | 27.50   | 28.71   | 26.92   | 28.25   |
| Weekly hours worked                |         |         | 36.15   | 37.05   | 36.12   | 36.81   |
| Years of work experience           | 11.48   | 14.41   | 20.99   | 22.87   | 30.12   | 31.74   |
| Years of firm tenure               | 3.57    | 4.41    | 5.29    | 6.39    | 9.03    | 10.32   |
| Number of jobs held                | 3.20    | 2.93    | 3.53    | 3.10    | 3.41    | 3.12    |
| Annual earnings (in NOK)           | 333,947 | 471,019 | 479,255 | 679,408 | 641,456 | 910,686 |
| Annual rate of internal promotions | 0.18    | 0.08    |         |         |         |         |
| Number of individuals              | 442     | 3,310   | 1,345   | 5,135   | 2,522   | 5,697   |
| B. Executives and Board Members    |         |         |         |         |         |         |
| Age (years)                        |         |         | 40.39   | 42.66   | 49.77   | 51.38   |
| Education (years)                  |         |         | 13.89   | 14.03   | 14.04   | 13.91   |
| Married or cohabiting (=1 if yes)  |         |         | 0.69    | 0.75    | 0.66    | 0.71    |
| Total number of children           |         |         | 2.48    | 2.69    | 2.57    | 2.70    |
| Age at first birth                 |         |         | 26.60   | 28.32   | 26.50   | 27.95   |
| Weekly hours worked                |         |         | 33.25   | 36.71   | 33.67   | 36.41   |
| Years of work experience           |         |         | 20.54   | 22.79   | 29.78   | 31.67   |
| Years of firm tenure               |         |         | 5.00    | 5.68    | 8.52    | 9.22    |
| Number of jobs held                |         |         | 3.02    | 2.83    | 3.10    | 3.01    |
| Annual earnings (in NOK)           |         |         | 328,677 | 536,611 | 440,632 | 650,590 |
| Number of individuals              |         |         | 1,128   | 4,667   | 5,855   | 10,792  |

*Note:* Statistics for men and women in the top decile of their firm's earnings distribution are in Panel A, and for men and women with executive and board roles are in Panel B. The RD sample refers to individuals having a child within 6 months of April 1993. All parents are followed over time.

Table A.2: RD Effects of the Leave Extensions on Additional Fertility Outcomes

| <i>Reform Year</i>             | 1987              | 1988              | 1989              | 1990              | 1991             | 1992              | 1993              | 2005              |
|--------------------------------|-------------------|-------------------|-------------------|-------------------|------------------|-------------------|-------------------|-------------------|
| Completed fertility            | -0.043<br>(0.026) | 0.003<br>(0.025)  | -0.020<br>(0.022) | -0.015<br>(0.021) | 0.018<br>(0.020) | 0.014<br>(0.020)  | -0.008<br>(0.020) | -0.008<br>(0.017) |
| Observations                   | 17915             | 22018             | 24551             | 27324             | 28741            | 29259             | 29838             | 36460             |
| Spacing first and second birth | 0.074<br>(0.078)  | -0.009<br>(0.075) | 0.052<br>(0.068)  | -0.055<br>(0.065) | 0.031<br>(0.065) | -0.050<br>(0.061) | -0.065<br>(0.061) | 0.048<br>(0.052)  |
| Observations                   | 16592             | 20338             | 22746             | 25299             | 26619            | 27177             | 27878             | 33969             |

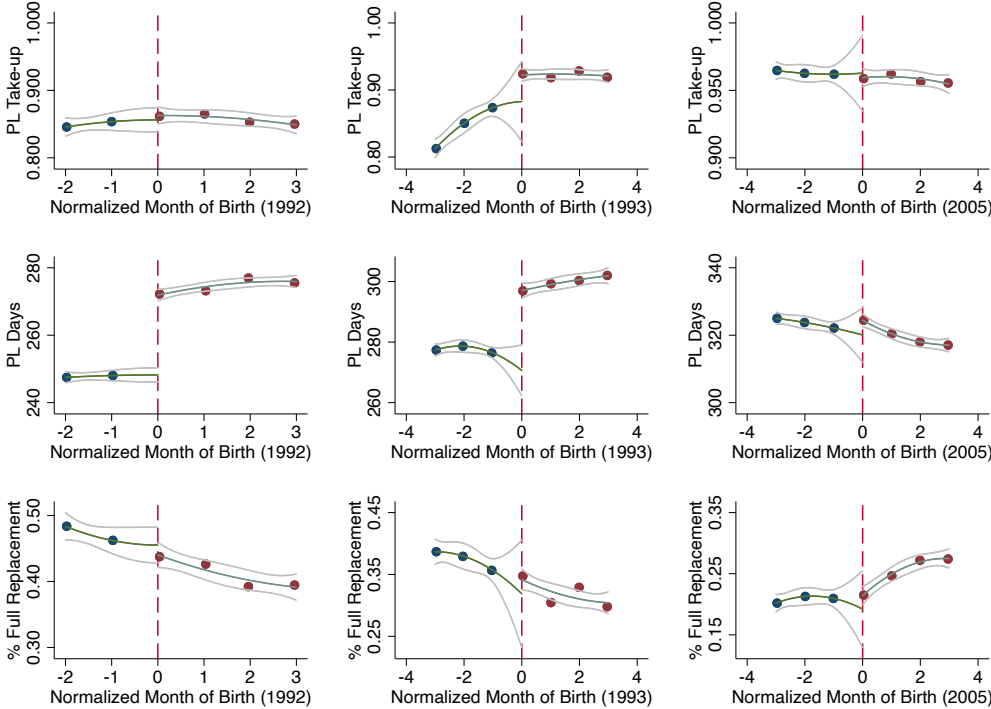
*Note:* All estimates are obtained from a linear RD model with triangular weights using a bandwidth of 6 months before and after each reform. Standard errors are reported in parentheses.

Table A.3: Difference-in-Discontinuities Estimates (1987-2004)

| <i>Sample</i>               | <i>Outcome: Internal Promotion</i> |                                 |                           |                      |                         |                           |
|-----------------------------|------------------------------------|---------------------------------|---------------------------|----------------------|-------------------------|---------------------------|
|                             | <i>All Women</i><br>(1)            | <i>Not 1987 and 1992</i><br>(2) | <i>Large firms</i><br>(3) | <i>Movers</i><br>(4) | <i>One-birth</i><br>(5) | <i>First-birth</i><br>(6) |
| Reform <sub>1992-1987</sub> | -0.030<br>(0.019)                  | -0.033*<br>(0.019)              | -0.032<br>(0.025)         | -0.024<br>(0.020)    | -0.025<br>(0.029)       | -0.017<br>(0.023)         |
| <i>Bandwidth</i>            | 6                                  | 6                               | 6                         | 6                    | 6                       | 6                         |
| <i>Observations</i>         | 10656                              | 9864                            | 6301                      | 9316                 | 3822                    | 5655                      |
| <i>Outcome Mean 2004</i>    | 0.224                              | 0.223                           | 0.186                     | 0.234                | 0.303                   | 0.224                     |
| <i>Outcome Mean 2013</i>    | 0.277                              | 0.276                           | 0.215                     | 0.288                | 0.338                   | 0.277                     |

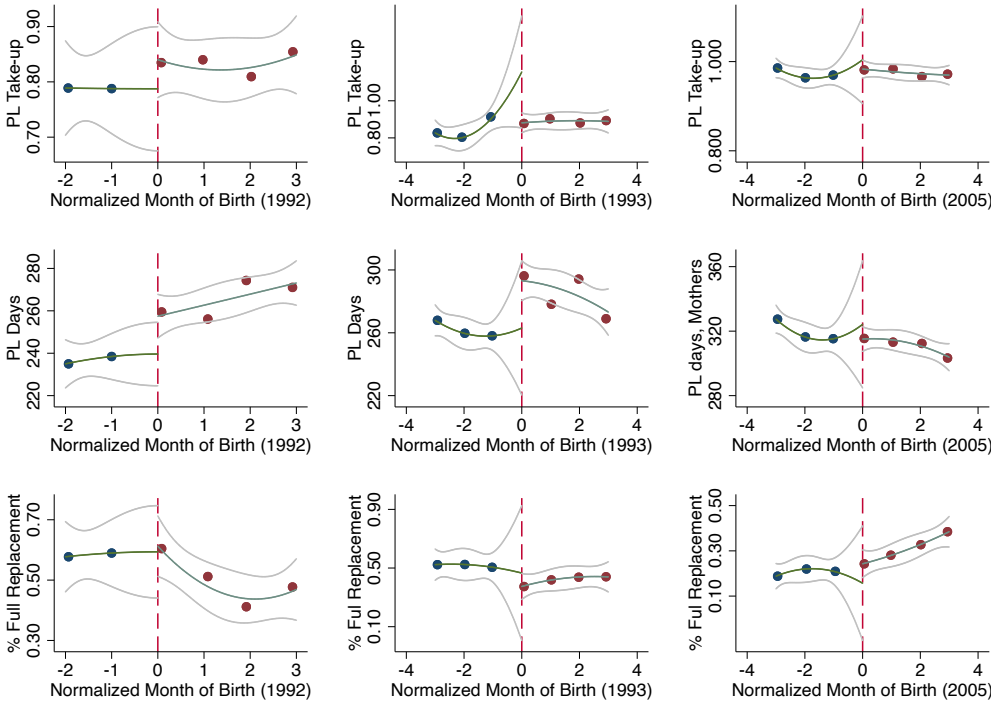
*Note:* The time period is restricted to 1987-2004, in order to eliminate confounding by the extension of paternity leave in 2005. We include a linear control function of the running variable on each side of the cutoff. Controls include a cubic function of age and years of education measured in the pre-reform year, as well as municipality fixed effects. Robust standard errors are clustered at the firm level. \*\*p<0.05,\*\*\*p<0.01.

Figure A.1: Parental Leave take-up and duration (in days) of mothers giving birth 3 months around each reform date



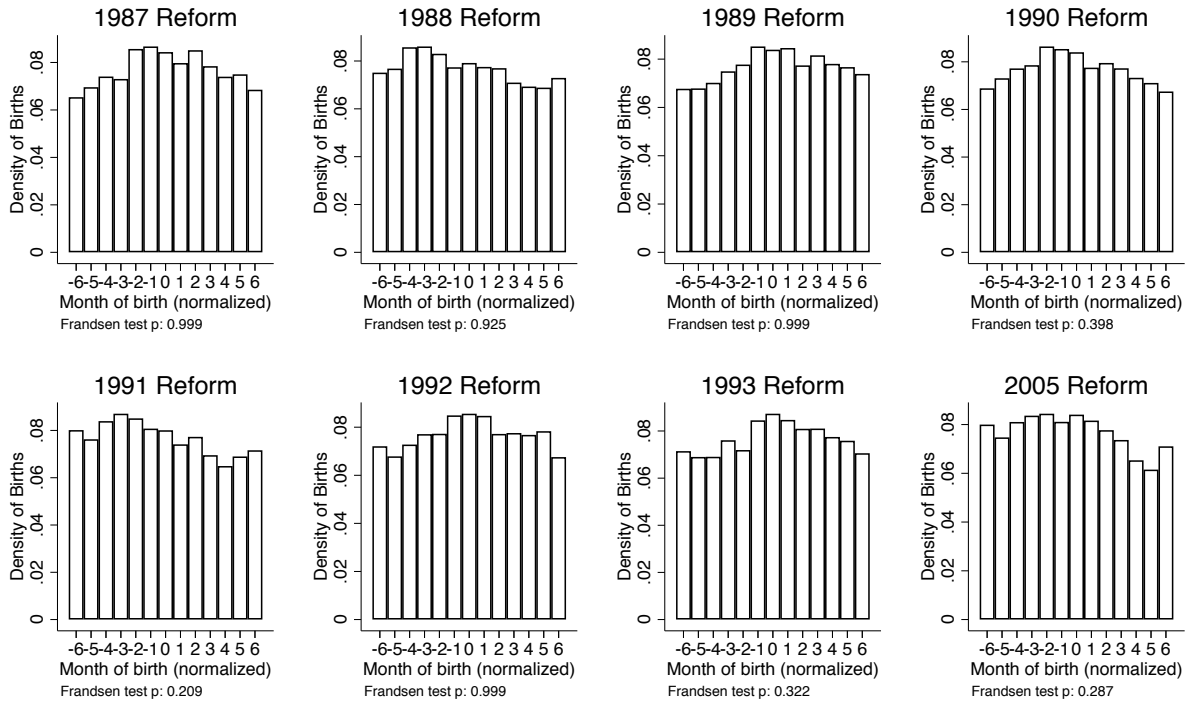
*Note:* Duration refers to average leave duration of mothers who take maternity leave. Conditional means in bins of birth month are displayed. Reforms: 1992, 1993, 2005.

Figure A.2: Parental Leave take-up and duration (in days) of mothers in the top earnings decile within their firms 10 years after giving birth 3 months around each reform date



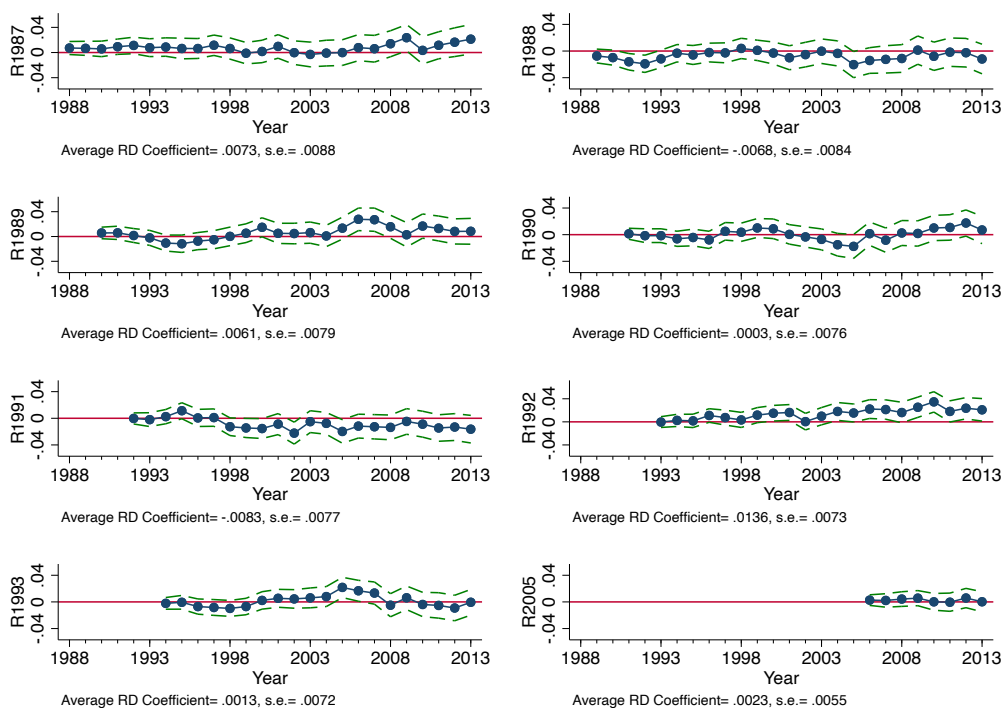
*Note:* Duration refers to average leave duration of mothers who take maternity leave. Conditional means in bins of birth month are displayed. Reforms: 1992, 1993, 2005.

Figure A.3: Density of births by distance in months from each reform date



*Note:* A reform window of 6 months is considered. The p-values from Frandsen Test are reported below each figure.

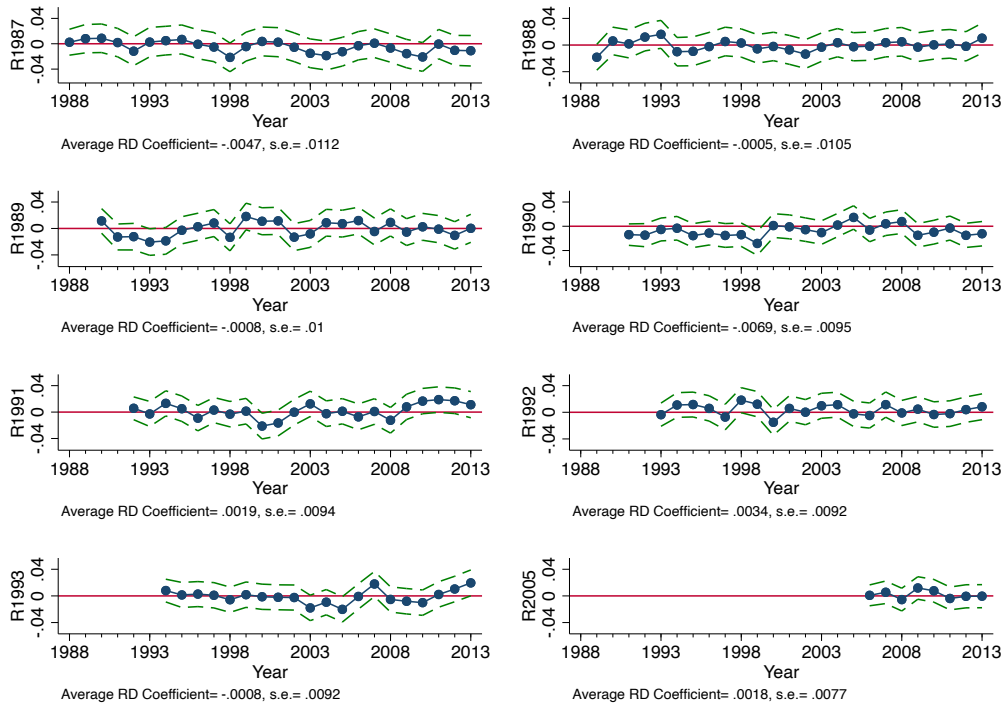
Figure A.4: Outcome: Top Earners, Women, 3-month window



*Note:* See the note to Figure 5. The outcome variable is an indicator of being in the top decile of the earnings distribution within firm for women giving birth within a 3-month window of each reform.

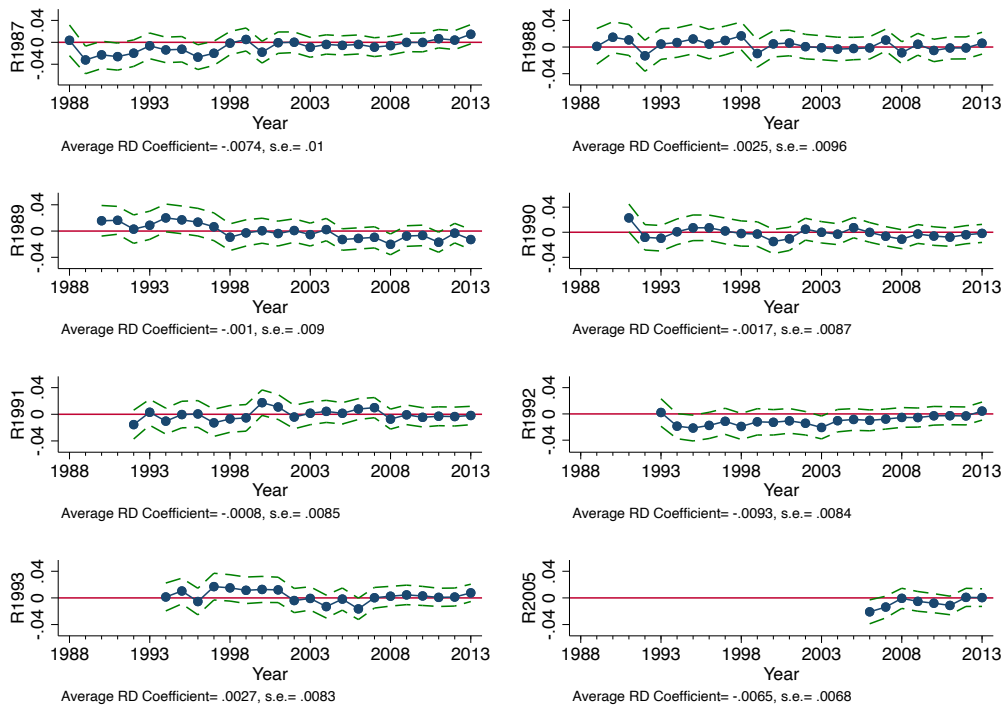


Figure A.5: Outcome: Middle Earners, Women



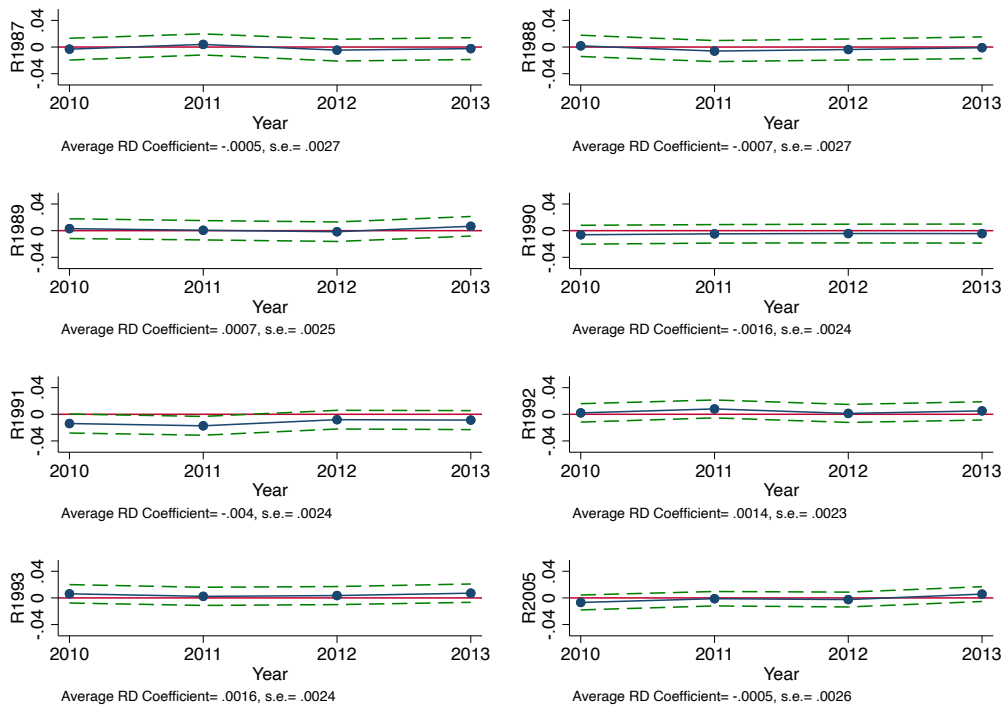
*Note:* See the note to Figure 5. The outcome variable is an indicator of being in the middle of the earnings distribution (40-60th percentiles) within firm.

Figure A.6: Outcome: Bottom Earners, Women



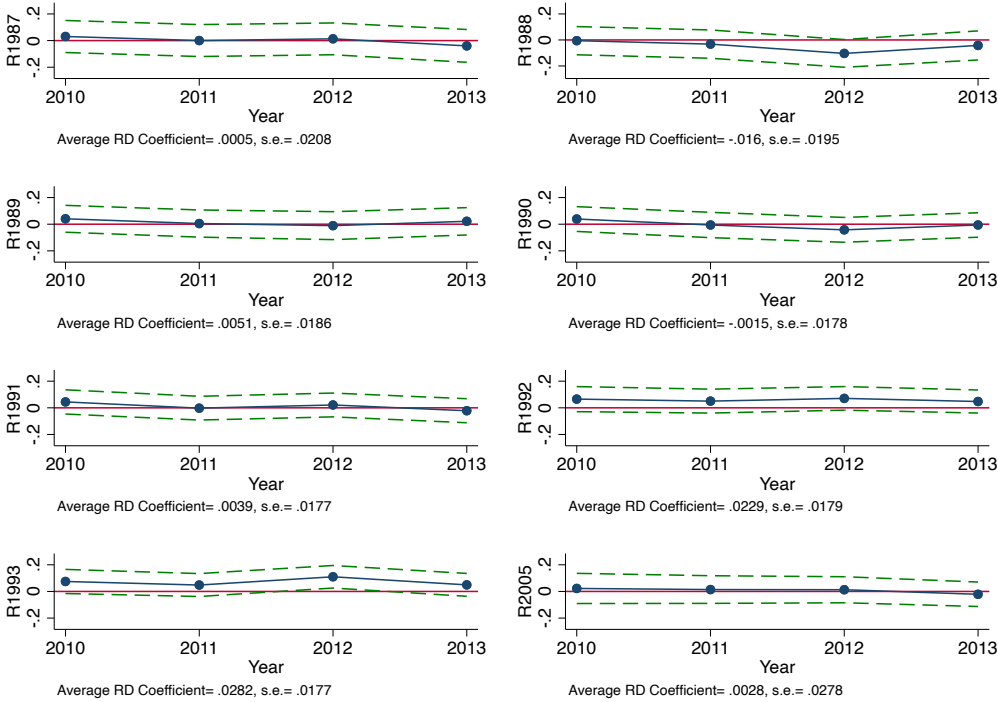
*Note:* See the note to Figure 5. The outcome variable is an indicator of being in the bottom decile of the earnings distribution within firm.

Figure A.7: Outcome: Managers (Occupational definition), Women



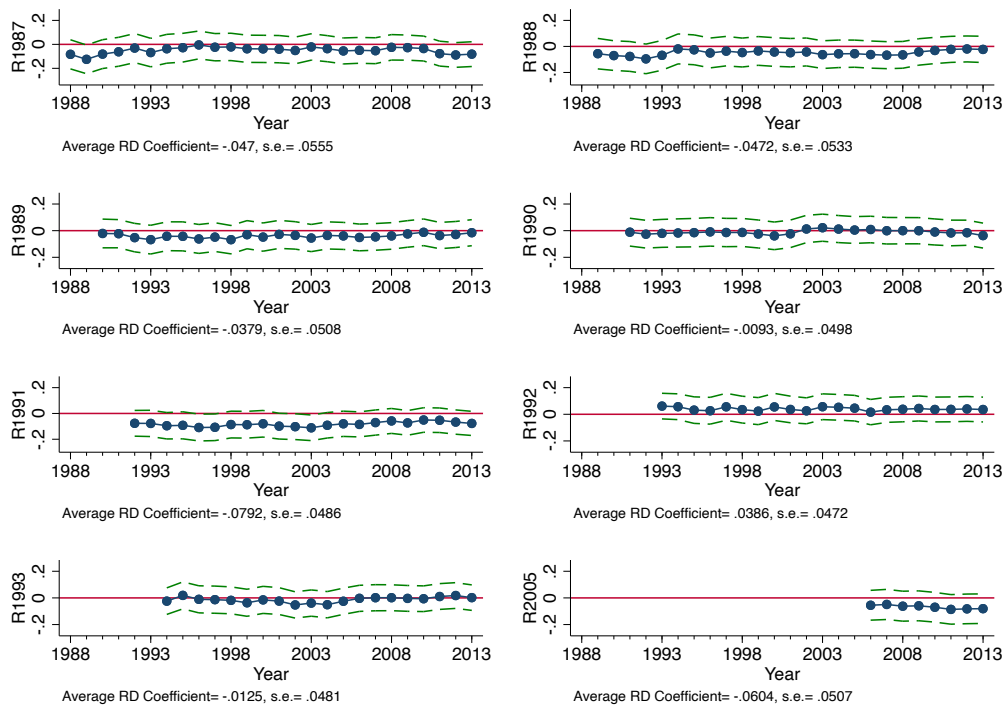
*Note:* See the note to Figure 5. The outcome variable is an indicator of having a managerial position, based on the occupational definition.

Figure A.8: Outcome: Managers (Occupational definition), Women in Top Earnings Decile



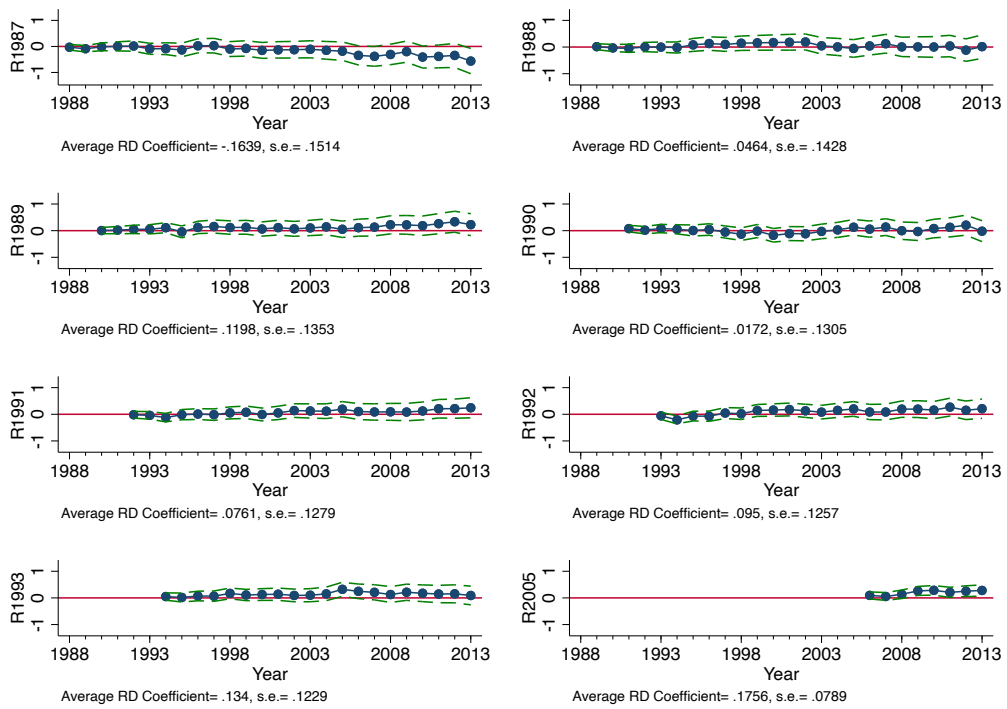
*Note:* See the note to Figure 5. The outcome variable is an indicator of having a managerial position, based on the occupational definition.

Figure A.9: Outcome: Work Experience (years)



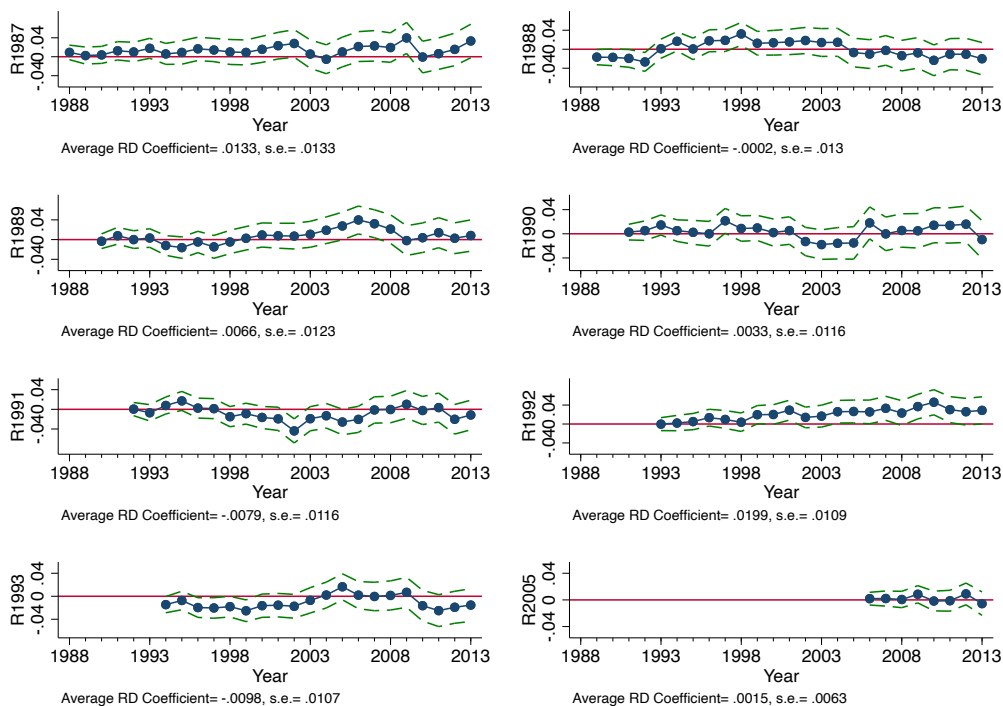
*Note:* See the note to Figure 5. The outcome variable is work experience in years, which is left-censored in the year 1983.

Figure A.10: Outcome: Firm Tenure (years)



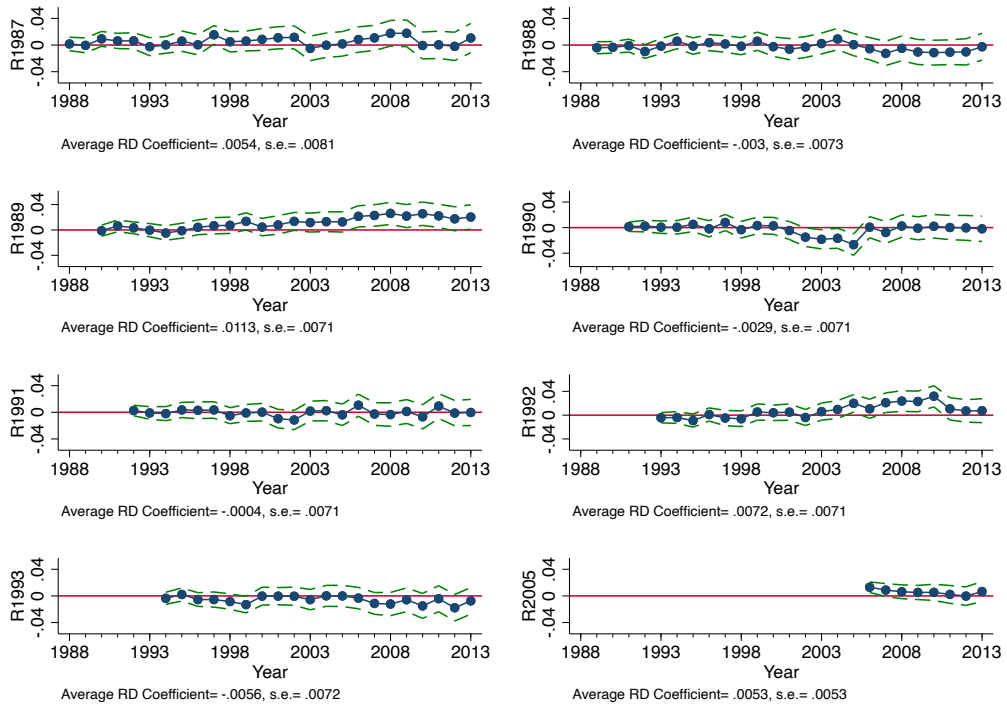
*Note:* See the note to Figure 5. The outcome variable is firm tenure in years, which is left-censored in the year 1983.

Figure A.11: Outcome: Top Earners, Women with a University Degree



*Note:* See the note to Figure 5. The outcome variable is an indicator of being in the top earnings decile within firm.

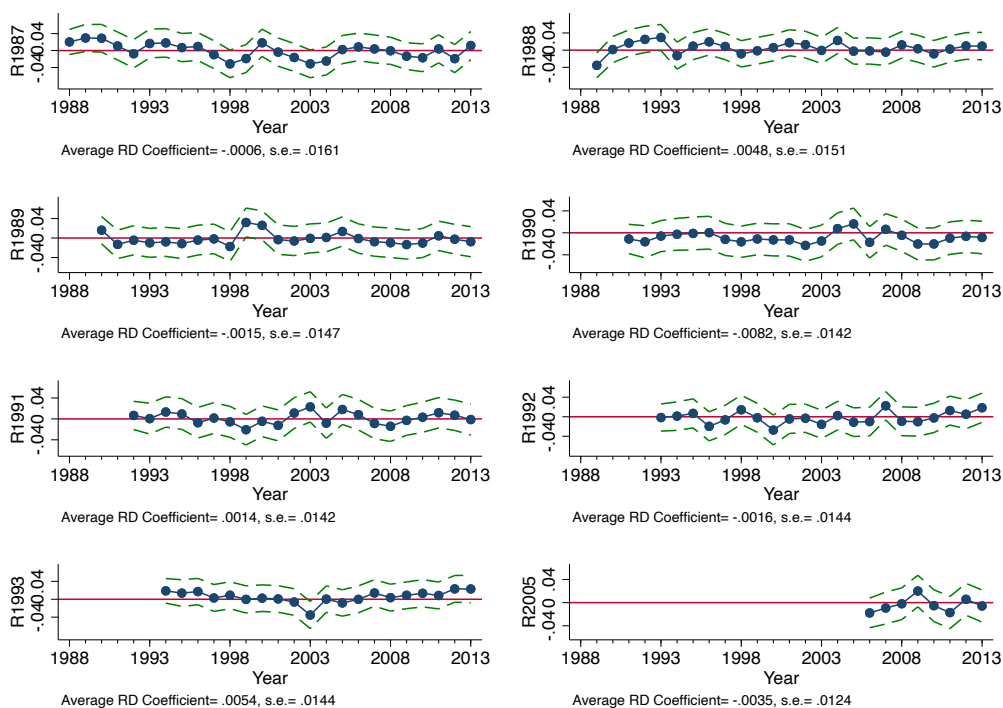
Figure A.12: Outcome: Top Earners, First-time Mothers



*Note:* See the note to Figure 5. The outcome variable is an indicator of being in the top earnings decile within firm.

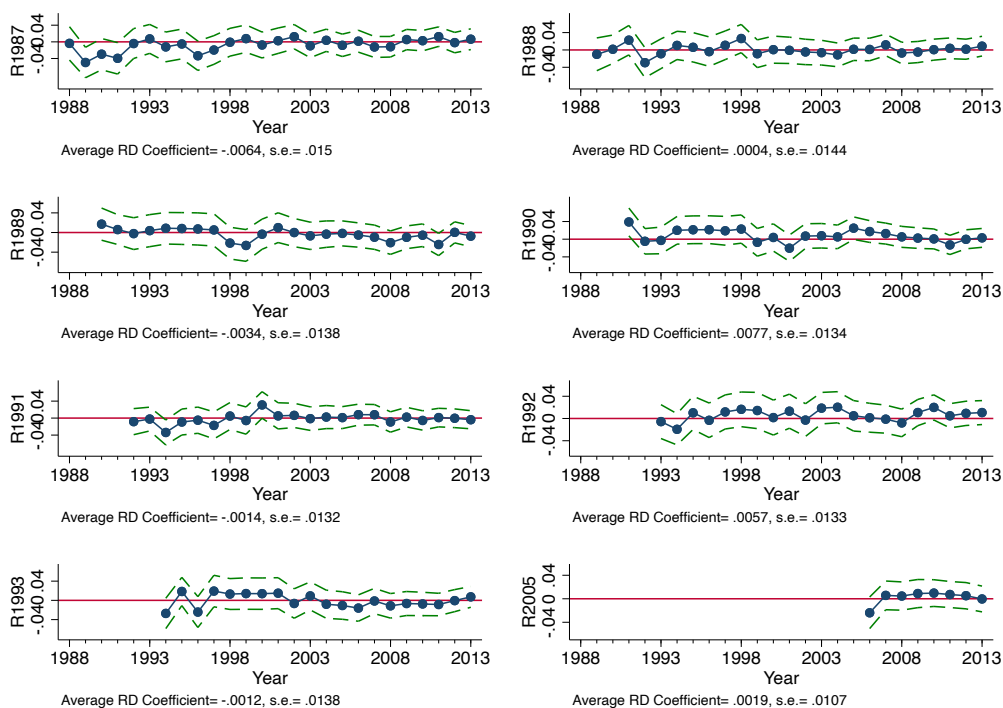


Figure A.13: Outcome: Middle Earners, First-time Mothers



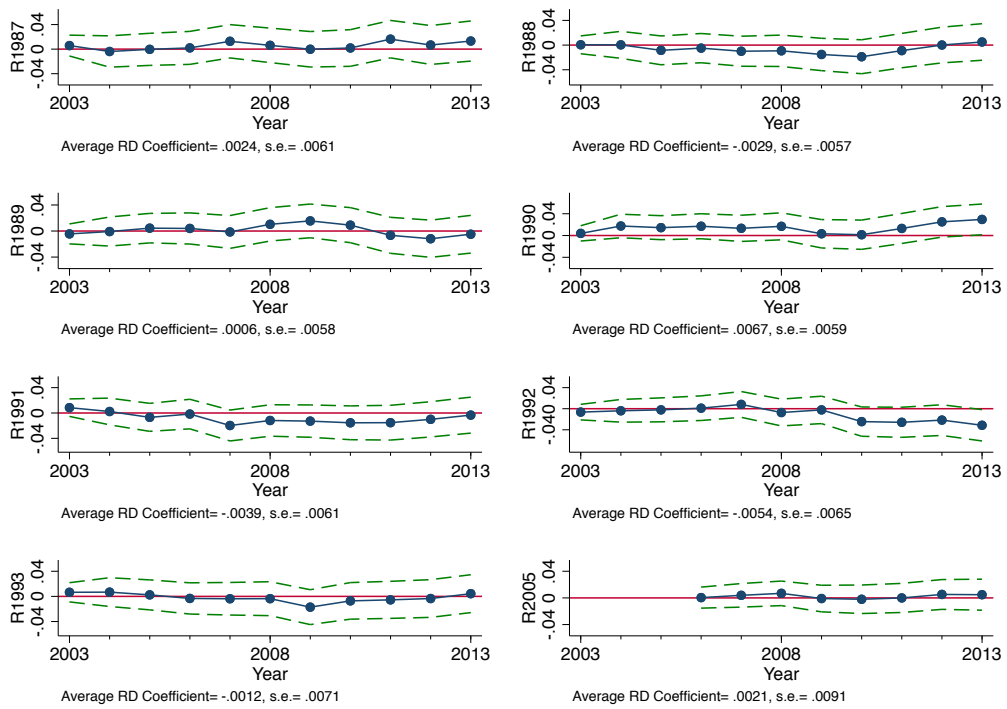
*Note:* See the note to Figure 5. The outcome variable is an indicator of being in the middle of the earnings distribution (40-60th percentiles) within firm.

Figure A.14: Outcome: Bottom Earners, First-time Mothers



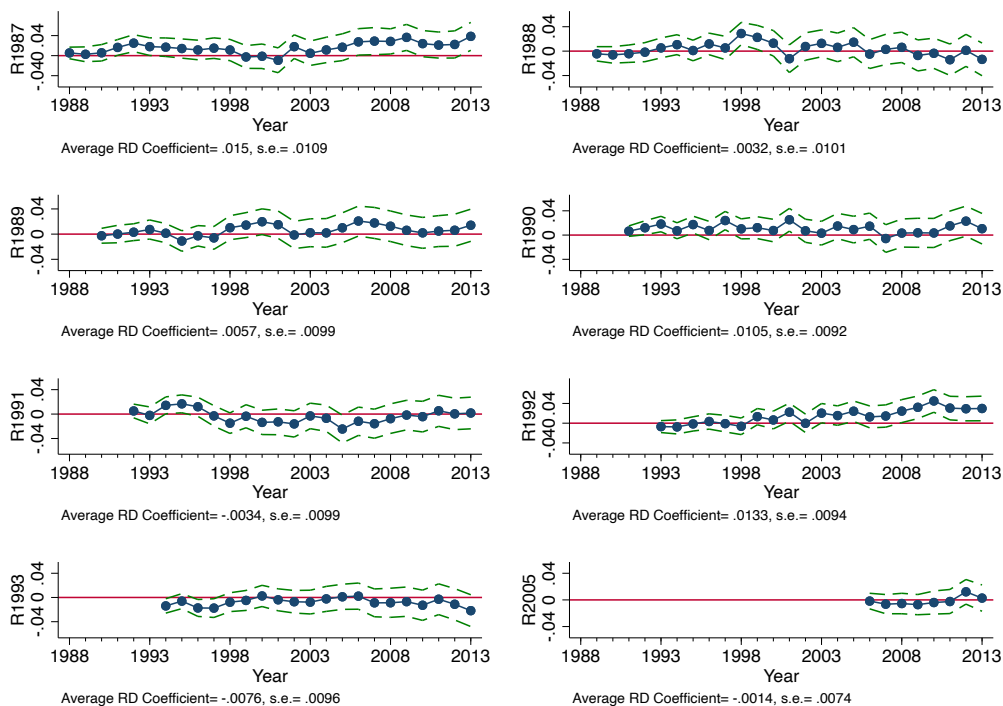
*Note:* See the note to Figure 5. The outcome variable is an indicator of being in the bottom decile of the earnings distribution within firm.

Figure A.15: Outcome: Executives and Board Directors, First-time Mothers



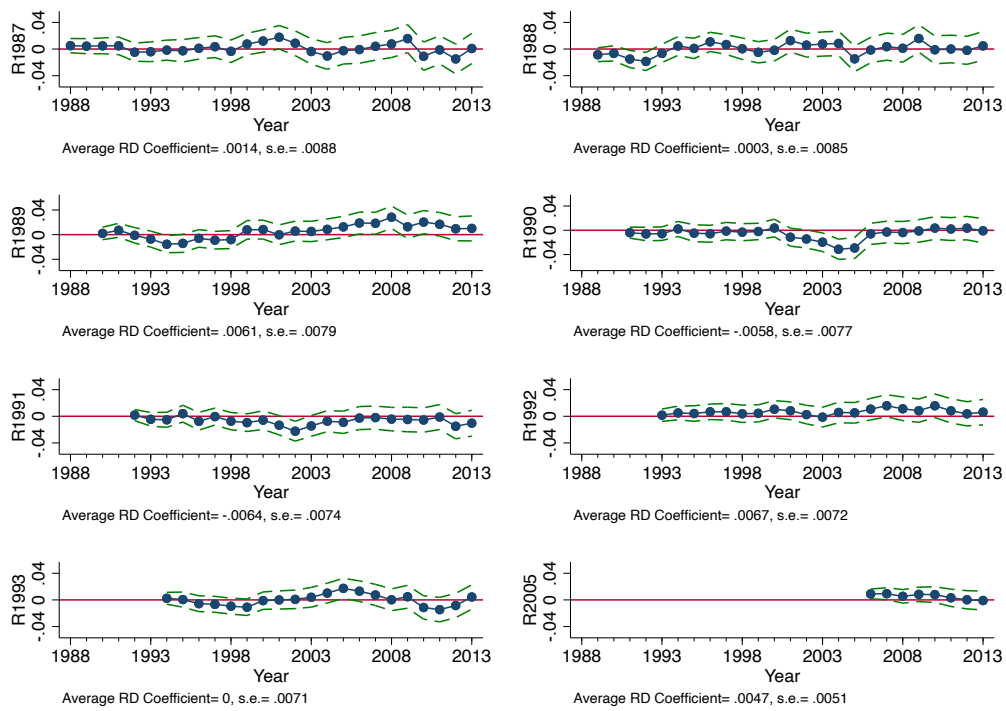
*Note:* See the note to Figure 5. The outcome variable is an indicator of having an executive role (CEO or CFO) or being a board director.

Figure A.16: Outcome: Top Earners in Large Firms



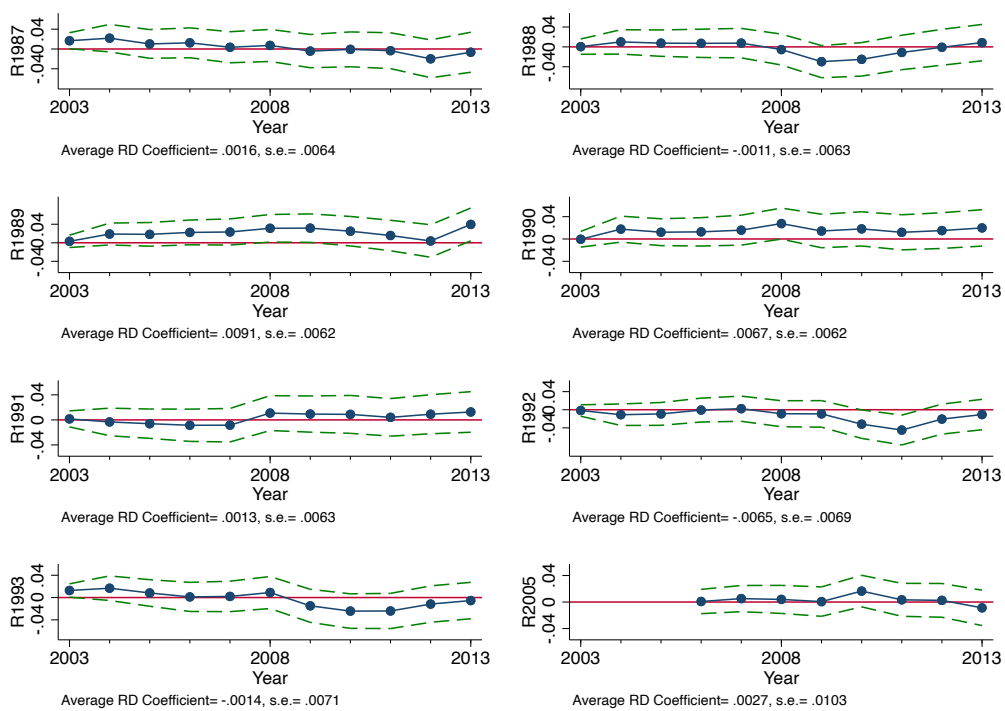
*Note:* See the note to Figure 5. The outcome variable is an indicator of being in the top earnings decile within firm.

Figure A.17: Outcome: Top Earners in Medium Firms



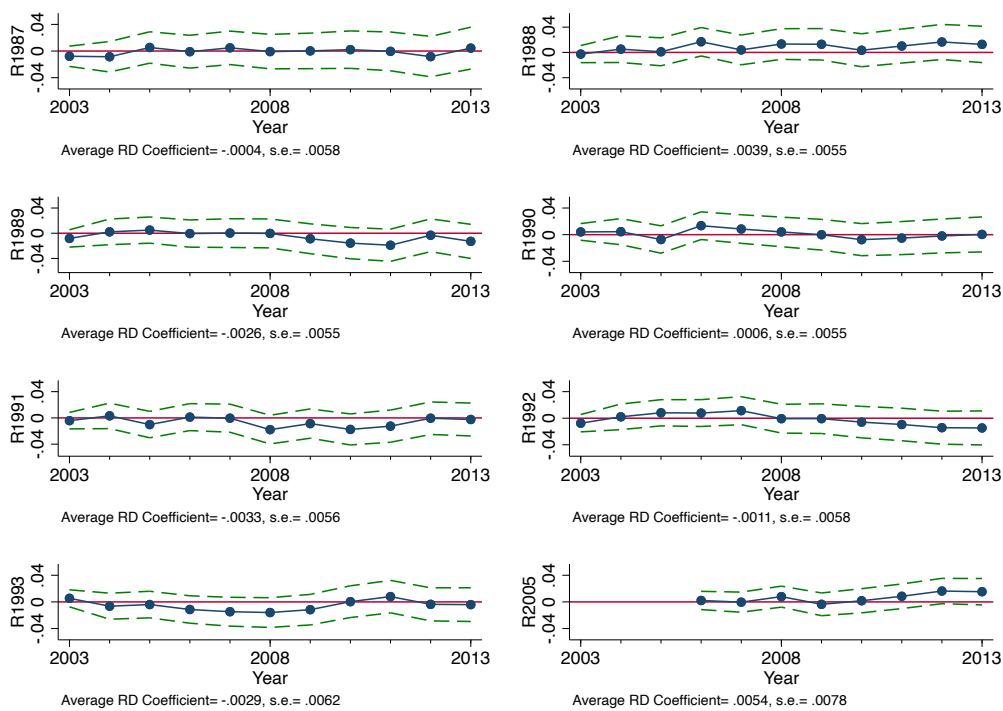
*Note:* See the note to Figure 5. The outcome variable is an indicator of being in the top earnings decile within firm.

Figure A.18: Outcome: Executives and Board Directors in Large Firms



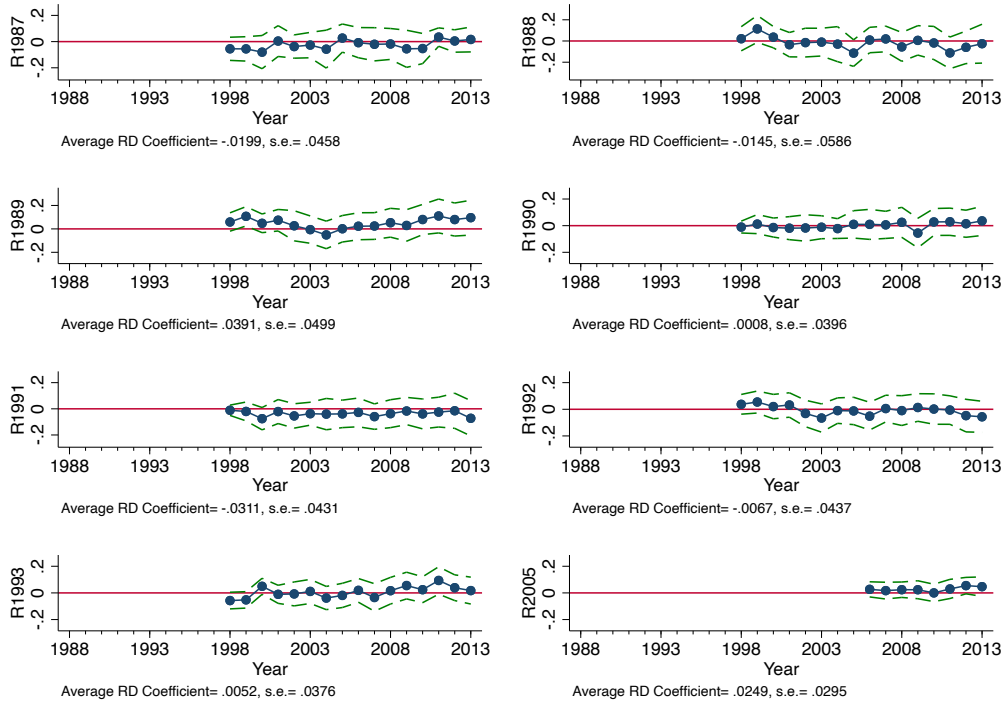
*Note:* See the note to Figure 5. The outcome variable is an indicator of having an executive role (CEO or CFO) or being a board director.

Figure A.19: Outcome: Executives and Board Directors in Medium Firms



*Note:* See the note to Figure 5. The outcome variable is an indicator of having an executive role (CEO or CFO) or being a board director.

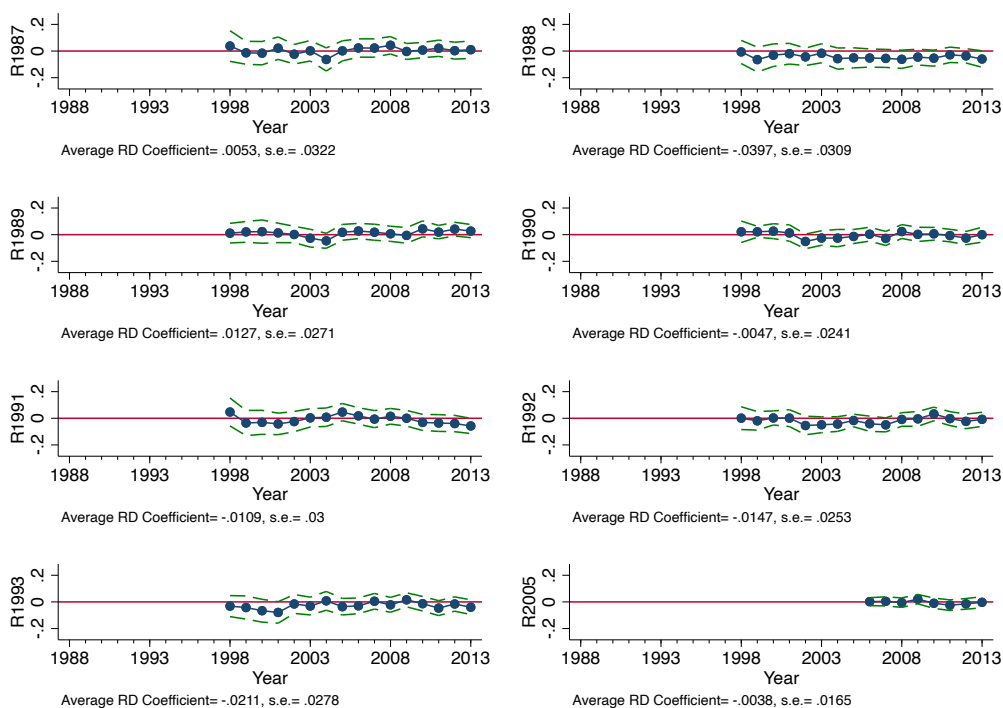
Figure A.20: Outcome: Top Earners in the Finance Sector



*Note:* See the note to Figure 5. The outcome variable is an indicator of being in the top earnings decile within firm. The sample of analysis is restricted to women working in the finance sector, which includes financial service activities, insurance and pension funding (except compulsory social security), and activities auxiliary to financial services and insurance activities.

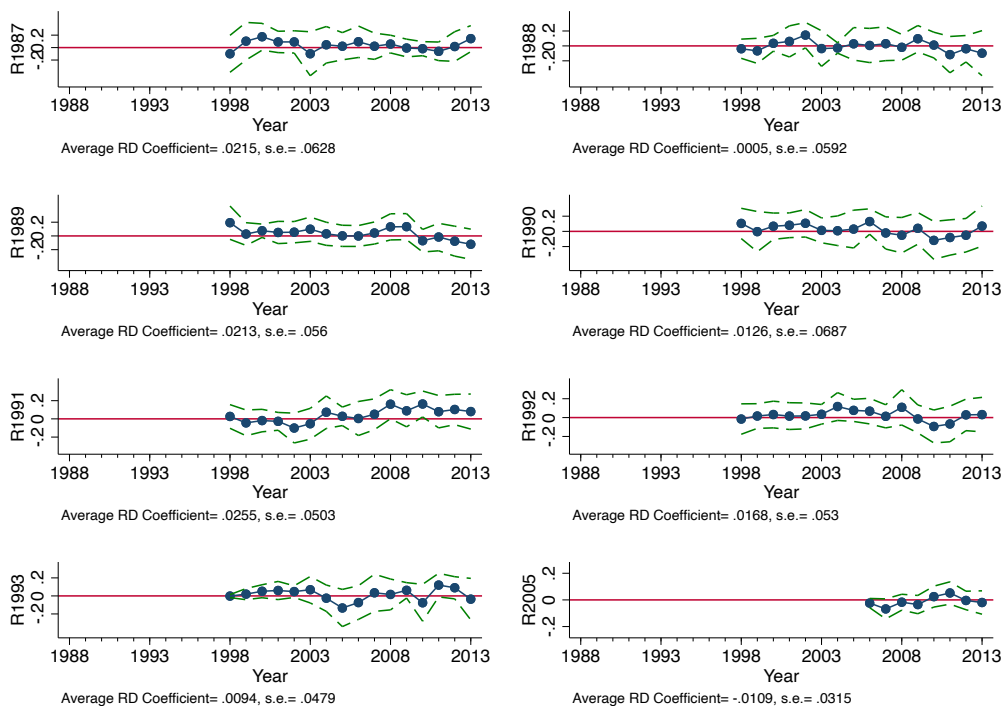


Figure A.21: Outcome: Top Earners in the Health Sector



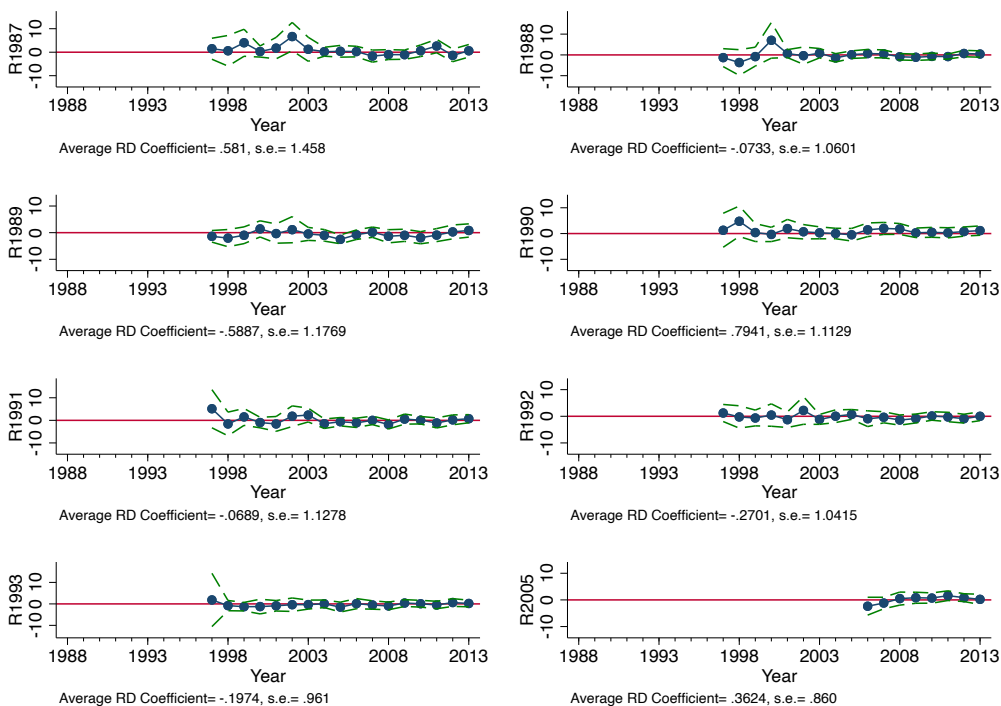
*Note:* See the note to Figure 5. The outcome variable is an indicator of being in the top earnings decile within firm. The sample of analysis is restricted to women working in the health sector, which includes medics in hospitals, medical and dental practices, and other human health activities.

Figure A.22: Outcome: Top Earners in the Business and Technology Sector



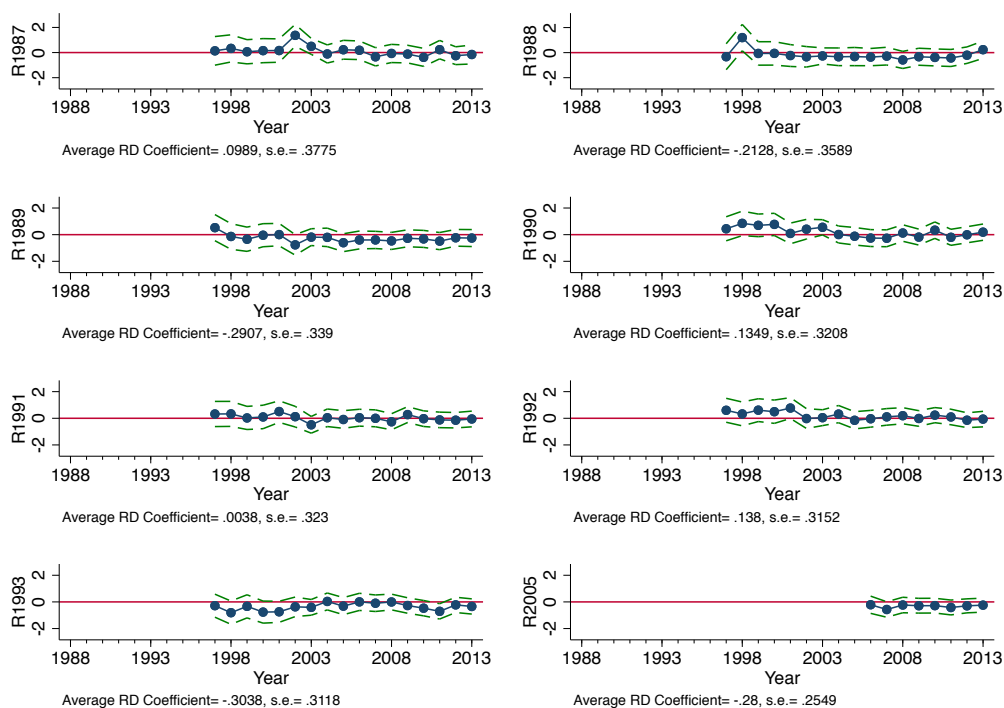
*Note:* See the note to Figure 5. The outcome variable is an indicator of being in the top earnings decile within firm. The sample of analysis is restricted to women working in the business and technology sector, which includes scientific research and development, market research, and advertising.

Figure A.23: Outcome: Hours, Women in the Top Earnings Decile



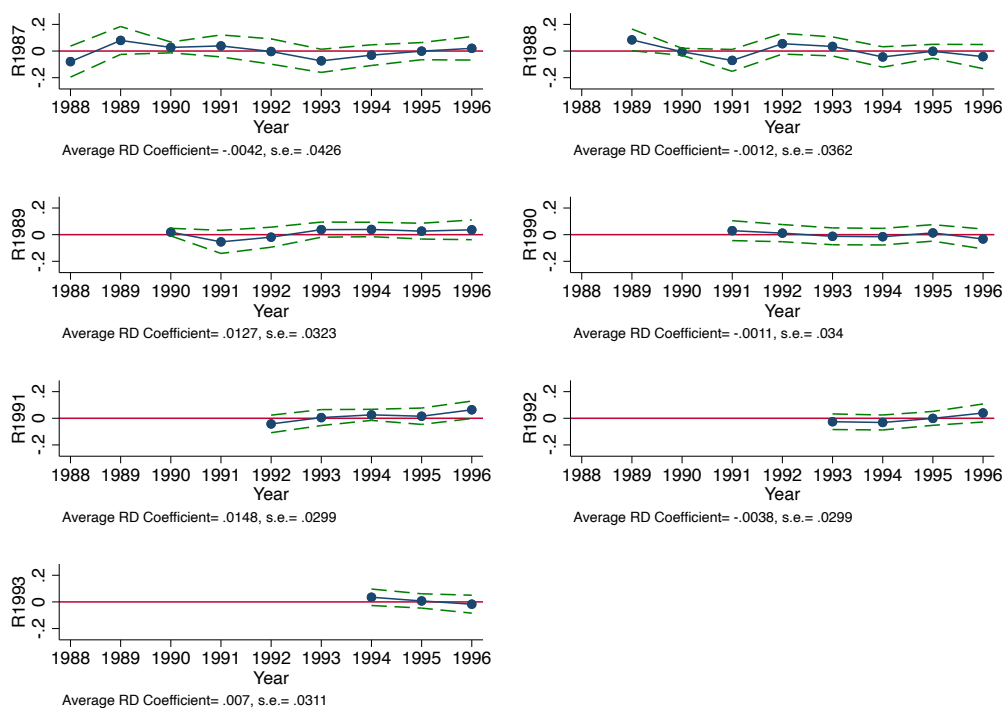
*Note:* See the note to Figure 5. The outcome variable is a continuous measure of working hours.

Figure A.24: Outcome: Hours, Women



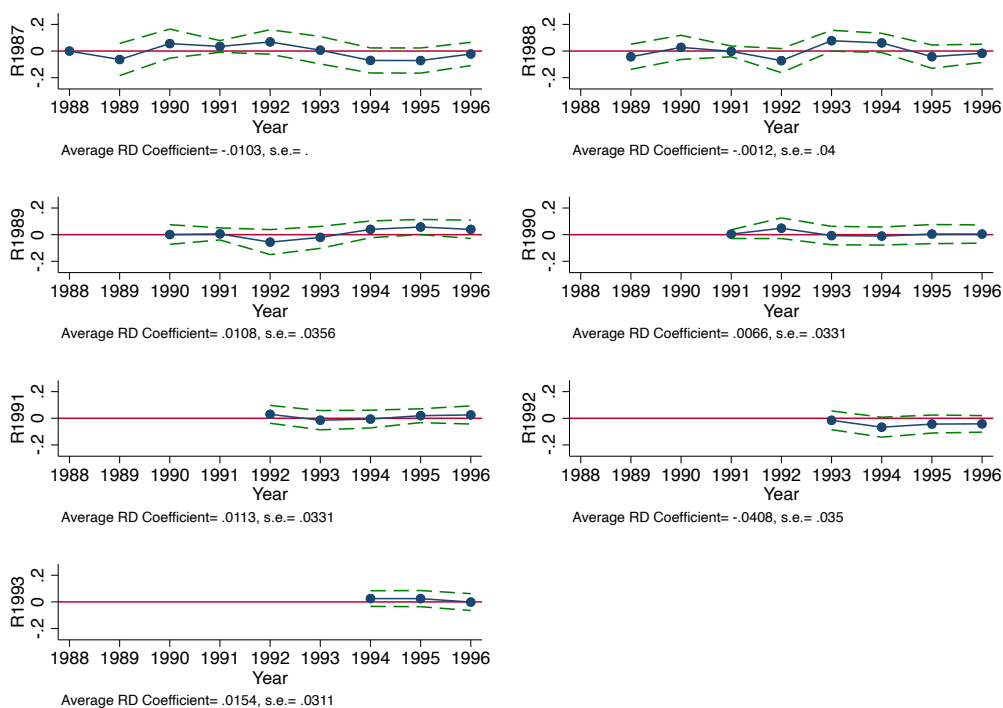
*Note:* See the note to Figure 5. The outcome variable is a continuous measure of working hours.

Figure A.25: Outcome: Internal Promotions, Women



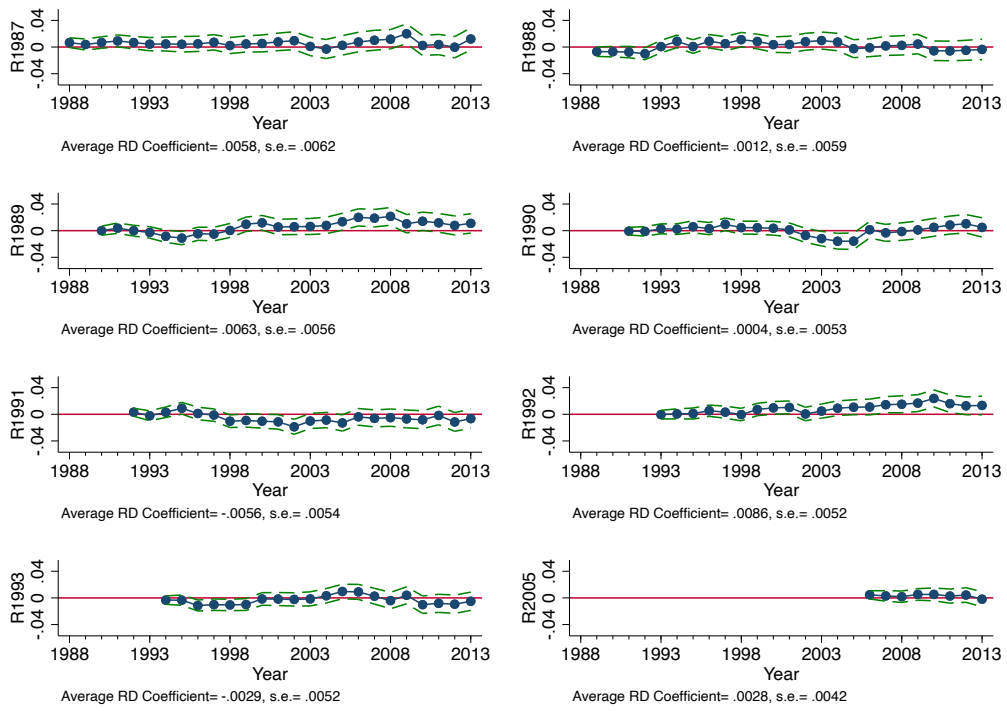
*Note:* See the note to Figure 5. The outcome variable is an indicator of an increase in rank (hierarchies are measured in 7 ranks) within firm.

Figure A.26: Outcome: Promotions, Women



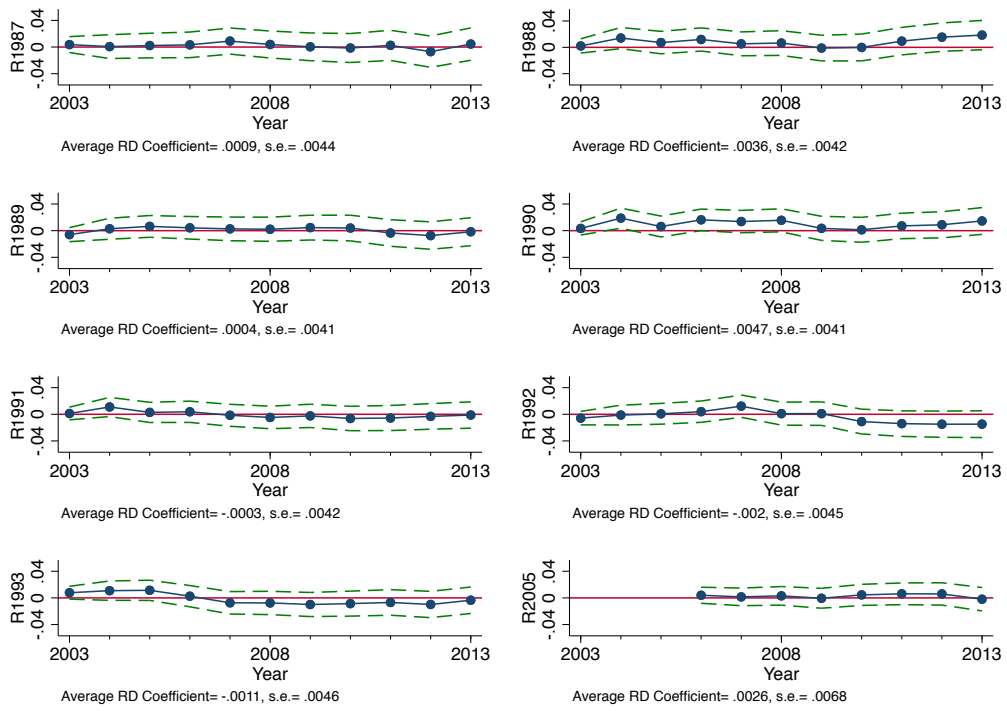
*Note:* See the note to Figure 5. The outcome variable is an indicator of an increase in rank (hierarchies are measured in 7 ranks) within firm or in relation to firm change.

Figure A.27: Outcome: Top-Pay Movers



*Note:* See the note to Figure 5. The outcome variable is an indicator of being in the top earnings decile within firm.

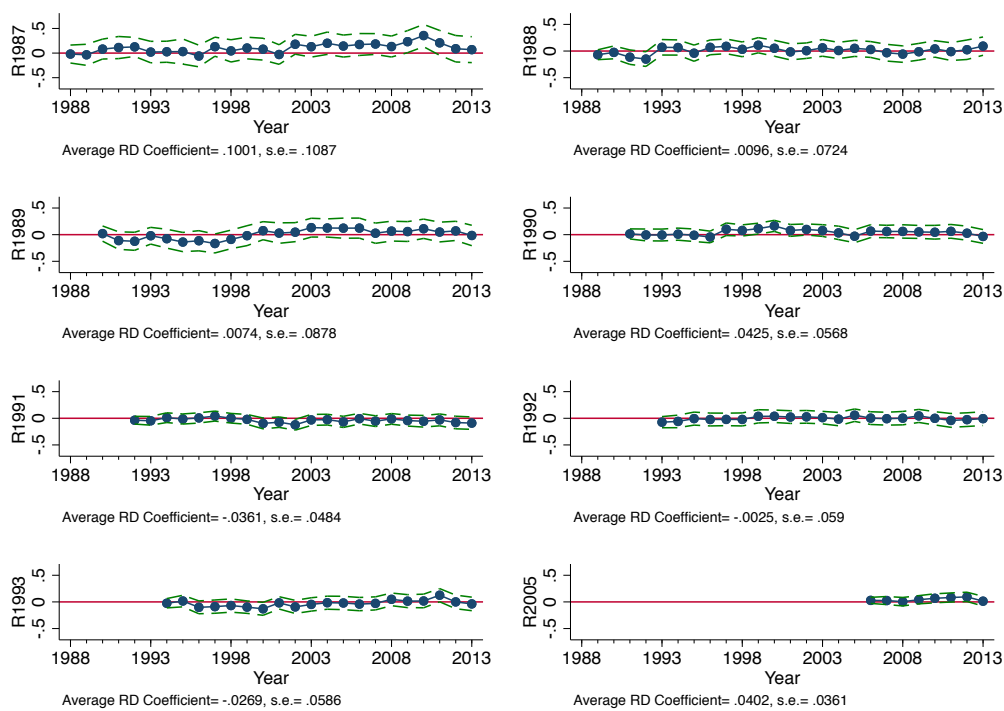
Figure A.28: Outcome: Executives and Board Directors who Change Firms



*Note:* See the note to Figure 5. The outcome variable is an indicator of being in the top earnings decile within firm.

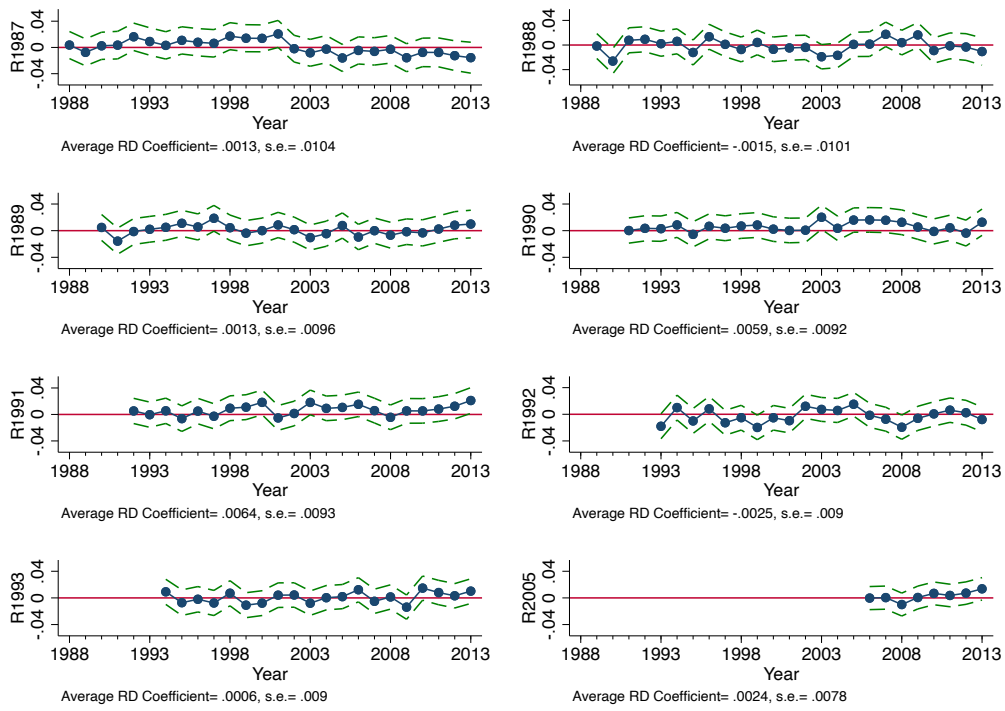


Figure A.29: Outcome: Top Earners, Women in the Second Top Decile



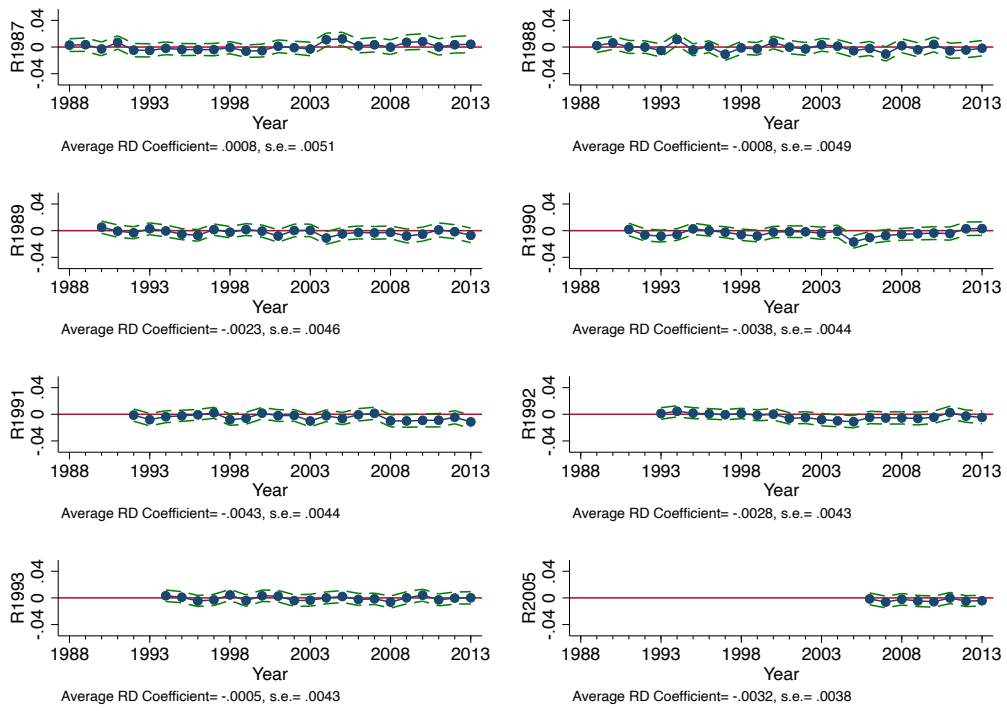
*Note:* See the note to Figure 5. The outcome variable is an indicator of being in the top earnings decile within firm.

Figure A.30: Outcome: Middle Earners, Men



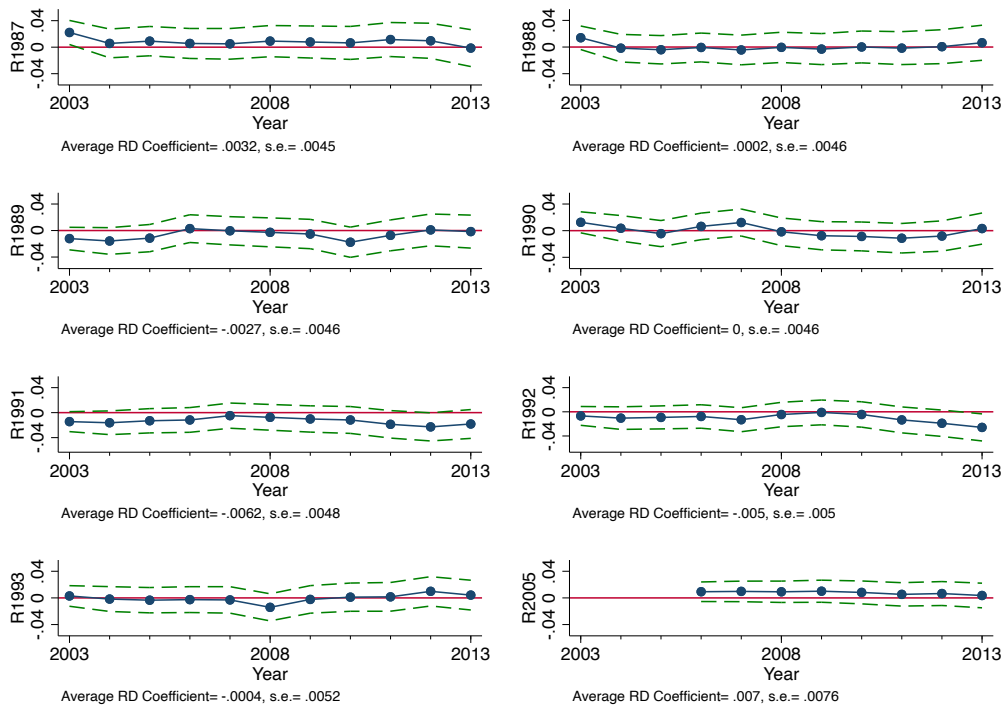
*Note:* See the note to Figure 5. The outcome variable is an indicator of being in the middle of the earnings distribution (40-60th percentiles) within firm.

Figure A.31: Outcome: Bottom Earners, Men



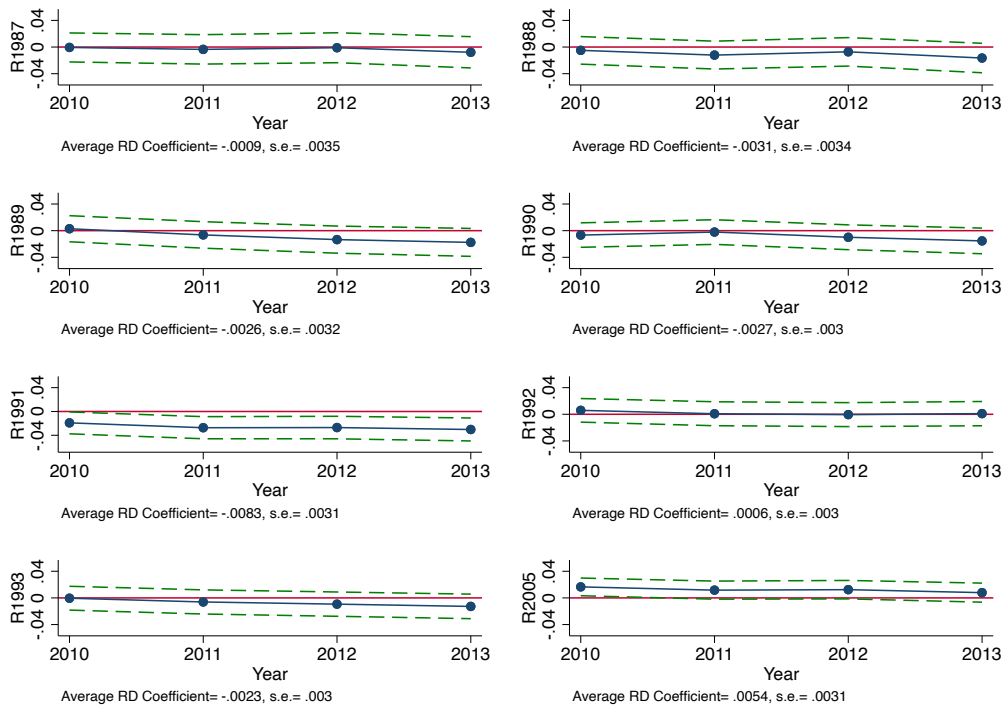
*Note:* See the note to Figure 5. The outcome variable is an indicator of being in the bottom decile of the earnings distribution within firm.

Figure A.32: Outcome: Executives and Board Directors, Men



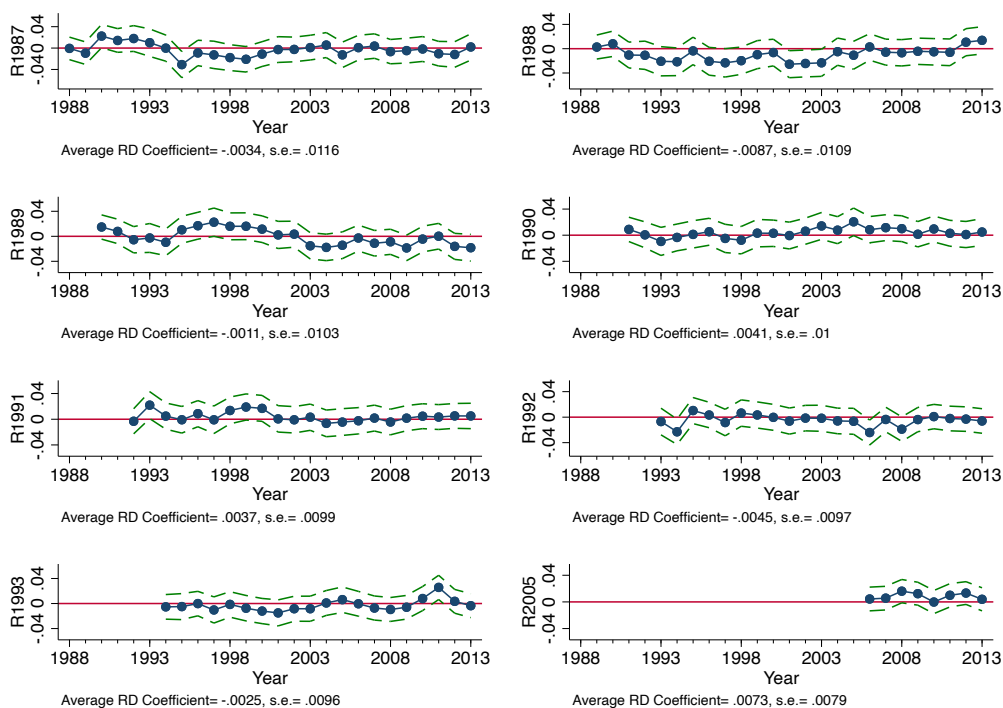
*Note:* See the note to Figure 5. The outcome variable is an indicator of having an executive role (CEO or CFO) or being a board director.

Figure A.33: Outcome: Managers (Occupational definition), Men



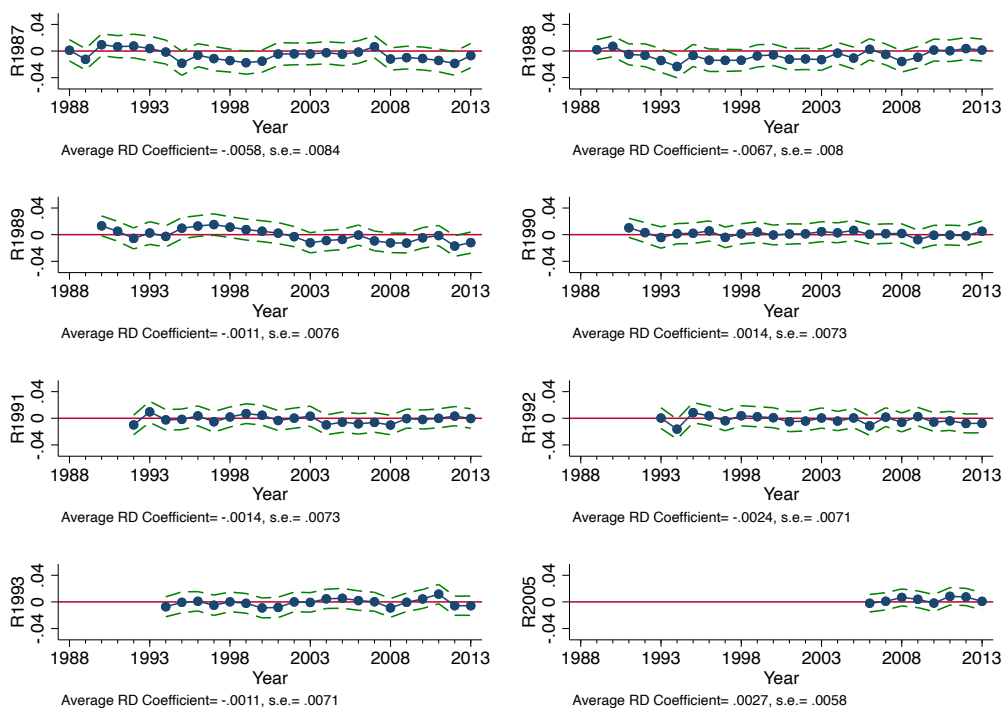
*Note:* See the note to Figure 5. The outcome variable is an indicator of having a managerial position, based on the occupational definition.

Figure A.34: Outcome: Within firm Gender Earnings Gap at the Top Decile



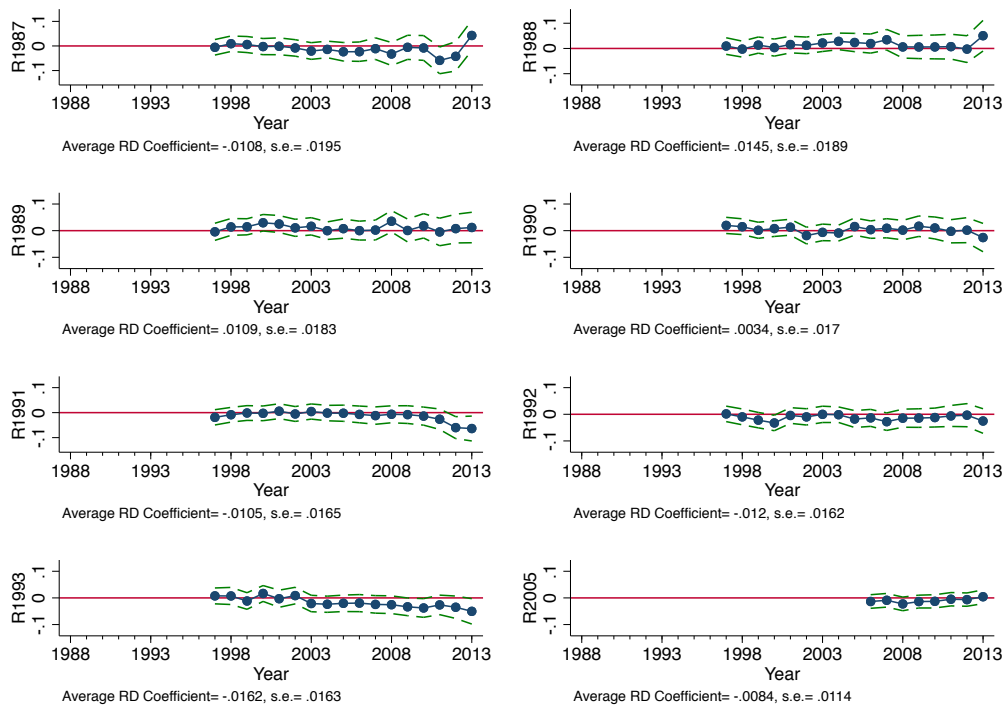
*Note:* See the note to Figure 5. The outcome variable is the within firm gender earnings gap computed using real annual earnings from register data on women and men at the top decile of their firms' earning distributions.

Figure A.35: Outcome: Within firm Gender Earnings Gap



*Note:* See the note to Figure 5. The outcome variable is the within firm gender earnings gap computed using real annual earnings from register data.

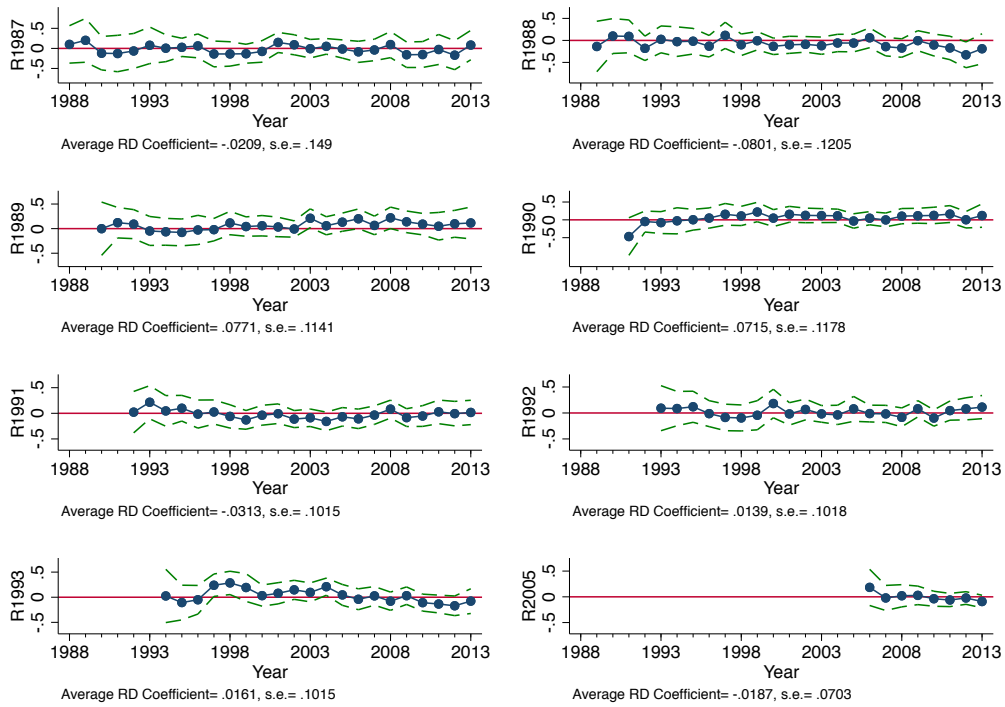
Figure A.36: Outcome: Within Household Gender Wage Gap (FTE)



*Note:* See the note to Figure 5. The outcome variable is the gender wage gap among partners computed using full-time equivalent hourly wages from the wage statistic.

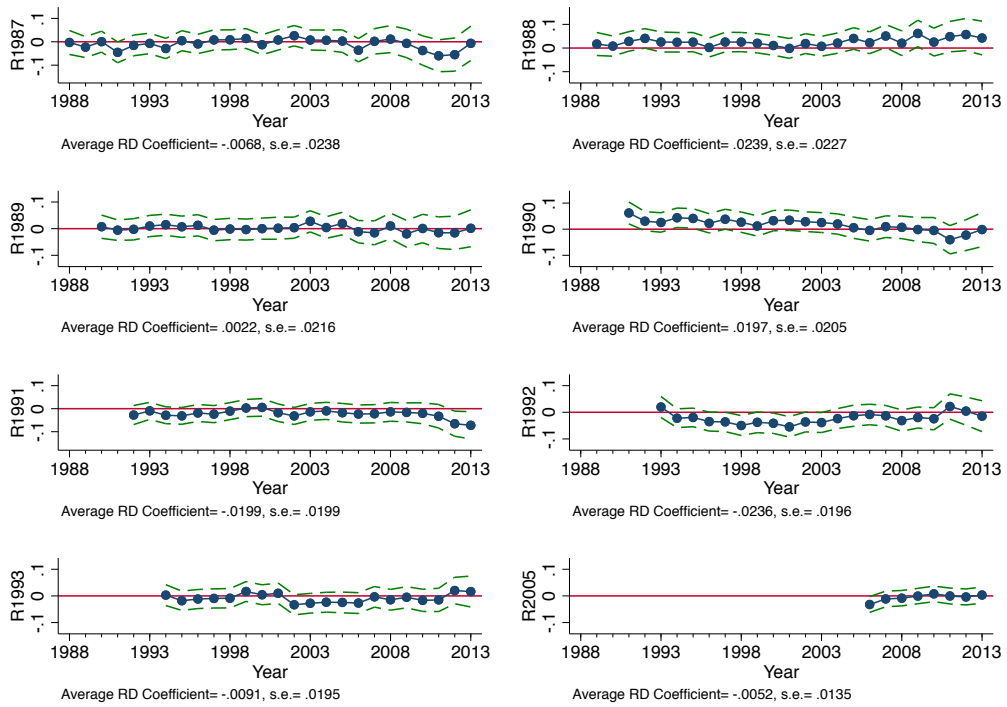


Figure A.37: Outcome: Within household Gender Earnings Gap at the Top Decile



*Note:* See the note to Figure 5. The outcome variable is the within household gender earnings gap computed using real annual earnings from register data on women at the top decile of their firms' earning distributions and their partners.

Figure A.38: Outcome: Within Household Gender Earnings Gap



*Note:* See the note to Figure 5. The outcome variable is the within household gender earnings gap computed using real annual earnings from register data.

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