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Women's labor market opportunities and equality in the household*

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Abstract

We study how changes in couples' relative wages affect the division of childcare. Using a nationwide wage reform that raised pay in the female-dominated teaching profession, we find that closing 25% of the earnings gap between female teachers and their male spouses led to a 12% reduction in the childcare time gap. This result holds when we extend the analysis to major pay raises for women at the population level. Data support the mechanism that women reduce their childcare time when the spouse can step in by working more from home. Policies that address female pay can foster household equality if men have access to flexible work arrangements.

Keywords: Household behavior; Childcare responsibility; Gender gaps; Working from home

JEL Codes: D13; J16; J22

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1 Introduction

Women still shoulder more unpaid housework and childcare in all OECD countries (OECD 2016). On an average day, American women spend 0.77 hours more on household activities compared to men and 0.62 fewer hours on paid work.[1] Even in Sweden, with almost equal labor force participation of men and women and with extensive childcare policies for mothers and fathers, women spend about an hour more on household chores, and work one hour less, than men each day.[2] Swedish mothers also take 70 percent of the paid parental leave to care for children when they are small (Försäkringskassan 2020).

The time allocation decisions within the family are crucial in understanding the remaining gender pay gap (Goldin 2014, Cortés and Pan 2023).[3] However, what can *change* the couple's time allocation is still debated. A growing literature highlights the role of gender norms as barriers towards a more equal division of household and family responsibilities, and thus calls for norm shifts (Akerlof and Kranton 2000; Bertrand et al. 2015; Kleven et al. 2020a; Siminski and Yetsenga 2022; Cavapozzi et al. 2024; Ichino et al. 2024). Meanwhile, more conventional economic explanations suggest that it starts from the labor market. That is, the gender pay gap may be the cause, not just a consequence, of the entrenched gendered time allocation, where the gender pay gap due to comparative advantages determine time allocation (Becker 1965; Becker 1991) and limit women's bargaining power (Lundberg and Pollak 1993) within a household. Some lingering questions thus follow: If the gender pay gap closes, does it feed back on the division of family responsibilities? And if so, through which mechanisms? Does it interact with gender norms?

To answer these questions, we examine how a sudden and substantial reduction in the gender wage gap within couples affects the division of childcare responsibilities. Figure 1 motivates our inquiry by documenting a strong association between a couple's wage gap (x-axis) and the allocation of childcare duties (y-axis), measured by the proportion of days off taken by mothers and fathers to care for sick children. The data come from cross-sectional, population-wide registry records for heterosexual couples in Sweden. The figure suggests a close link between narrowing the wage gap and achieving a more equal division of childcare. This association raises the question of whether closing the wage gap within the same couple has a causal effect on the reallocation of childcare duties.

[1] Based on 2019 American Time Use Survey data.

[2] Based on the 2010/11 Swedish Time Use Survey, which is the latest survey conducted.

[3] For example, Duchini and Van Effenterre 2022 find that reduced time commitments to child care raised mothers average wages in France, and Cortés and Pan 2019 show that low-skilled immigration inflows to the U.S have decreased the gender pay gap in occupations that disproportionately reward long work hours. Azmat et al. 2022 show that work absence to care for sick children is negatively related to wages in jobs with few employee substitutes suggesting that unpredicted absence is problematic for firms, and Denning et al. 2022 more generally show that differences in hours worked have slowed women's labor market progress.

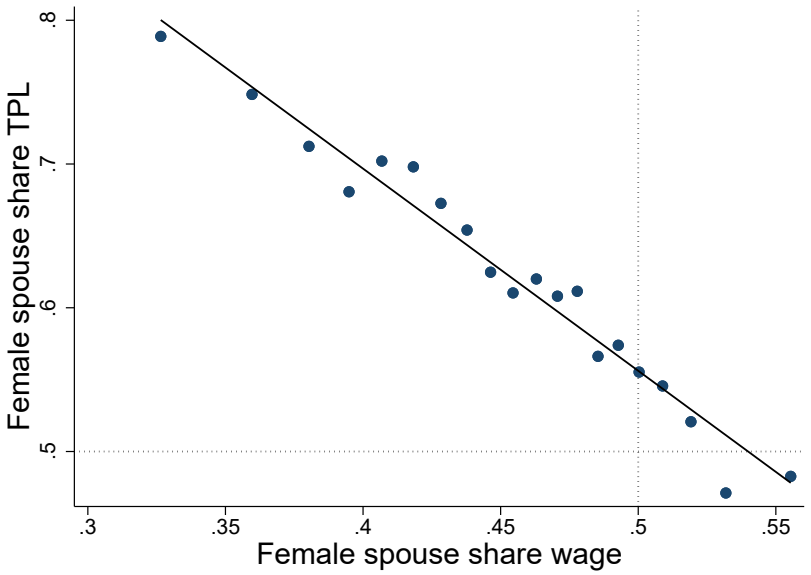


Figure 1: Binscatter plot of the couple gap in childcare responsibility (y-axis) against the couple wage gap (x-axis)

Note: The childcare responsibility gap is measured as the female spouse's share of days per year that the mother and father have taken off from work in order to take care of their sick children. The couple wage gap is measured as the female spouse's share of the mother's and father's before-tax, full-time equivalent monthly wages. The sample is restricted to dual-earner couples with at least one child aged under 10 in the cohort's year of promotion, who had their first child more than 6 years ago, described in Section 5. It uses data only from the year before promotion, and drops outliars with a gender wage gap (in SEK) below the 5th percentile or above the 95th percentile. The slope coefficient is -1.406 (0.048).

Specifically, we ask whether an unexpected reduction in the wage gap triggers a shift in how childcare responsibilities are divided, and if so, what enables such a shift to occur.

We answer these questions using a quasi-natural experiment that shocks the couple pay gap. Specifically, we leverage a national wage reform in Sweden's teaching profession. In 2013, Sweden introduced a new career step in teaching to enhance the occupation's appeal. The policy was gradually implemented across schools from 2013 onward, in line with funding provided by the state. The reform boosted average pre-reform wages by around 15 percent. Although the reform did not explicitly aim to address gender inequality, its impact on the female-dominated teaching profession makes it highly relevant for gender analysis. As a result of the reform, the earnings gap between promoted female teachers and their male spouses narrowed by about 25 percent. Additionally, the share of female teachers earning more than their male partners increased by 12 percentage points, nearly doubling the pre-reform level. This shift provides a unique opportunity to explore how a shock to the couple's wage gap influences the division of caregiving duties between partners.

Measuring the division of care responsibilities is also challenging. Time use surveys

often have limited sample sizes and can be prone to biased reporting. We address these issues by leveraging a unique Swedish social security system and register data. Specifically, we use information on how female and male parents use Temporary Parental Leave (TPL), a benefit available to either parent for caring for a sick child during working hours. Women typically take around 60 percent of these days. While TPL usage is interesting in its own right, our supplementary analysis suggests that it positively correlates with the the division of household duties in general. Thus, while the division of TPL is the primary outcome variable in this study, it is likely that the total impact on gender household equality goes beyond the impact on the childcare time allocation captured in our data.[4,5]

To identify the impact of the reform shock on the division of TPL days, we employ a stacked event study design. The design is a difference-in-differences approach, applied when policy rollout is staggered as is the case with the teacher promotion reform. We restrict our sample to dual-earner heterosexual couples with young children where the female spouse is a teacher. We compare changes in couples' relative TPL uptake in couples affected by female major pay increases in a certain year to couples where the female spouse was eligible for promotion but did not experience the major pay increase. We follow couples over time, even if the couples split up or the promoted teacher leaves the profession, and show that outcomes evolve very similarly prior to promotion in couples with and without a promoted female spouse, supporting the validity of our identification strategy.

We find that the within-couple gap in the number of TPL days decreases by an average of 12 percent per year over the three years following the reform, relative to the control group mean. This narrowing is driven primarily by a reduction in women's TPL days. Interestingly, men also reduce their TPL days, although to a much smaller extent. As a result, the total number of TPL days taken by the household declines. Women's responses are consistent with standard economic reasoning: as their wages increase, so does the relative opportunity cost of their time, making it more costly for them to take leave. However, men's responses warrant closer attention. Since household demand for TPL is largely driven by children's illness, it is not immediately clear why a husband would reduce his TPL days following a pay increase for his wife. If the mother steps back from caregiving responsibilities, one might expect the father to step in, rather than reduce his own involvement.

[4]In 2011, fathers' fraction of the weekly total amount of non-paid work in couples living together with small children was 44 percent (39.55 hours for women and 32.20 hours for men), according to the Swedish Time Use Survey (see Stanfors 2018 for a detailed description of how non-paid work differs between men and women depending on couple characteristics).

[5]We provide evidence on the association between parents' relative TPL uptake and their (self-reported) share of time spent on other domestic chores in Section 2.

It is important to note that these TPL responses may reflect a combination of a pay raise, new tasks and career orientation for promoted teachers. While it is challenging to separate these empirically, we believe that this combined effect is of high relevance in itself, by speaking to the impact of enhanced female career opportunities for household gender equality.

To assess whether the findings from the teacher pay rises generalize to the broader Swedish population, we expand the analysis to include all heterosexual couples with children in Sweden, irrespective of occupation. Using longitudinal register data, we identify instances of large wage increases for female partners as events that shift the within-couple wage gap. Specifically, we define such events as years in which a woman's wage deviates from her predicted wage trajectory by more than 10 log points from one year to the next. We then apply a stacked event study design to estimate the effect of narrowing the wage gap on the division of childcare. The results from the full population mirror those found in the teacher sample, both in terms of the relative division of TPL days and the individual responses of mothers and fathers. This suggests that the dynamics found in the natural experimental setting with teachers are relevant for a much broader segment of Swedish couples.

We then leverage the larger sample size of the broader Swedish population to examine how the impact varies across different couple characteristics. This allows us to explore the mechanisms through which couples respond to a substantial increase in the woman's wage and to shed light on why total household TPL use declines. First, we test the hypothesis that grandparents step in to provide childcare (a mechanism suggested by e.g., Gørtz et al. 2025 in the Danish context) when women reduce their TPL take-up. However, we find no evidence supporting this mechanism in our context: the causal effect of women's pay increases does not differ between couples with and without grandmothers living nearby.

We then turn to the role of gender norms, particularly the hypothesis that men may reduce their TPL days to preserve traditional gender roles in response to a shift in household economic power, which is a form of compensatory behavior discussed in Bertrand et al. (2015). Our findings do not fully support it. While we observe that more gender-egalitarian couples become more equal in their division of TPL (consistent with Ichino et al. 2024), it is, in fact, the men in these progressive couples who reduce their TPL days. This pattern runs counter to the logic of gender role assertion.

Finally, we find evidence consistent with the hypothesis that fathers do step in, though not by taking more TPL days. Instead, they increase their involvement by working from home, an alternative way to care for sick children. A key insight is that a substantial pay increase for the wife not only narrows the within-couple wage gap but also raises the household's implicit price of taking TPL. In response, couples appear to seek a more

cost-effective way to reallocate caregiving duties. For many, this can involve the father working from home, which is fully paid and therefore economically preferable to taking TPL. Indeed, in households where the husband has the option to work from home, both spouses significantly reduce their TPL use following a positive wage shock to the wife. In contrast, in households where the husband lacks work-from-home flexibility, neither spouse significantly adjusts their TPL usage in response to the shock. These findings suggest that, when the cost of formal leave rises, fathers substitute toward working from home as a more efficient way to provide care.

Taken together, our findings suggest that improving women's wages can advance gender equality not only in the labor market but also within the household. Policies aimed at raising pay in female-dominated occupations have the potential to narrow the gender wage gap and reshape the division of domestic responsibilities. A key channel through which higher wages for women translate into greater equality at home is men's ability to work from home. This highlights the importance of expanding flexible work arrangements for men, not only to support their own involvement in caregiving but also to enable their partners' career progression. These findings point to the broader, and often overlooked, spillover effects of family-friendly workplace practices for men on women's outcomes. Exploring these dynamics further offers a promising direction for future research.

Our paper contributes to three lines of literature. First and foremost, our paper contributes to the literature and discussion on the efficacy of policy in addressing the unequal division of household and family responsibilities. Sweden and other countries have implemented a series of family policy reforms to facilitate the combination of parenthood and careers. Contrary to the popular belief, however, such family policies appear to have little impact on fathers' involvement in the care of children (Ekberg et al. 2013, Lindahl et al. 2023) or on the overall child penalty (Kleven et al. 2020b).[6] Our results however suggest that policies that strengthen women's position in the labor market can be an equalizing force for the division of household and family responsibilities. As such, change can be achieved from the market side.

Second, our paper contributes to the literature on family-friendly workplace practices, particularly remote work options, and their impact on women's career advancement and intra-household equality. Existing research has documented that women place a higher

[6]Ekberg et al. 2013 study how the introduction of mandated paternity leave in Sweden affects fathers' share of TPL. While the so-called "daddy-month reform" was supposed to promote gender equality within the household, there is no strong evidence that the reform had an impact on fathers' involvement in household work beyond a positive effect on their paternity leave. In fact, Avdic and Karimi 2018 show that marriage stability was negatively affected by the reform. However, studies from Germany, Canada and Spain do provide more encouraging results regarding the relationship between paternity leave policies and fathers' involvement in childcare (see Schober 2014; Patnaik 2019 and Farré and González 2019). Lindahl et al. 2023 study a change in the child allowance benefit scheme in Sweden finding no impact of the default transfer recipient on parents division of temporary or regular parental leave.

value on flexible work arrangements and that such practices facilitate women's labor market attachment by helping them balance paid and unpaid responsibilities while incurring some cost (e.g., Goldin and Katz 2011; Goldin 2014; Bloom et al. 2015). These studies typically focus on women as the primary users and direct beneficiaries of these practices. In contrast, our findings highlight a complementary and less-studied mechanism: when men have access to family-friendly practices, it enables a reallocation of unpaid household labor that supports their female partners' career advancement. This indirect channel underscores the broader social spillover effects of firm policies that promote work-life balance and points to a possible underprovision of such policies if firms consider only the direct effects on their own employees. As many firms are currently scaling back remote work options, our findings suggest that doing so may have broader implications through intra-household dynamics. This opens up new avenues for research on how workplace policies shape gender inequality not only through direct usage but also through household interactions.

Third, our findings contribute to the emerging literature on the impact of a female promotion on intrahousehold outcomes. For example, Folke and Rickne (2020) show that female promotions to top political and corporate positions increase the likelihood of divorce in Sweden. Interestingly, they show that divorces are more prevalent among couples who adhere to traditional gender roles.[7] Our heterogeneity results complement their empirical evidence by providing further insights into why traditional couples are less resilient: couples who adhere to traditional norms have limited ability to reallocate domestic responsibilities, narrowing the scope for renegotiating the marital term. Consequently, divorce becomes more likely.

The paper is organized as follows. Section 2 describes the institutional context and the teacher promotion reform. In Section 3, we outline our data, empirical strategy, and summary statistics. Section 4 shows the results from the teacher sample. Section 5 presents the results from the broader Swedish population. Finally, Section 6 concludes.

2 Institutional Background

We study childcare time responses to substantial changes in relative wages within couples. To address the endogeneity concern, we make use of a unique policy-induced change in couples' relative wages. Specifically, we examine wage increases resulting from a national wage reform in Sweden's teaching profession. To make the teaching profession more attractive by aligning teacher pay more closely to teacher skills, the Swedish government

[7]More broadly, evidence shows that traditional couples are more likely to resort to marital dissolution when facing unexpected changes in men's and women's relative economic positions. For example, Avdic and Karimi (2018) demonstrate that the increase in fathers' earmarked parental leave increases the likelihood of divorce, and the impact is larger for couples who adhere to traditional gender roles.

introduced a new career step for teachers in 2013 called 'career teachers'. This promotion program was launched as a response to deteriorating student achievement and aimed to improve student outcomes by keeping and encouraging high quality teachers (Regeringen 2013b). The main component of the reform was a significant monthly wage increase of 5,000 SEK (520 USD), which was fully funded by the state and corresponds to around 15 percent of mean pre-reform wages. Career teachers primarily continue to teach but also engage in tasks like coaching their colleagues (see Grönqvist et al. 2022 for details). Total working time did not increase in general due to a promotion, and it was delegated to the school districts (and principals) to free up time for the new responsibilities while maintaining at least a 50 percent teaching load (Statskontoret 2017).[8] [9]

According to the reform's regulation (Regeringen 2013a), the teacher needed to fulfill certain criteria to qualify for the new title. They needed to be certified; have at least four years of teaching experience; demonstrate an ability to improve student outcomes and a keen interest to develop teaching; and be deemed particularly qualified as a teacher. The reform was rolled out over time starting in 2013, and the number of positions increased successively, year-on-year, in line with the funding provided by the state. Career teacher positions were allocated to school districts in proportion to their total student population. School districts, in turn, allocated career teacher positions to individual schools at their discretion. In practice, school principals at individual schools often recruited teachers to become career teachers.

Although the career teacher reform did not specifically target gender inequality, the female-dominated teaching profession makes it relevant for gendered outcomes. As of 2022, over 80 percent of primary school teachers and 65 percent of lower secondary school teachers in Sweden are women. Additionally, teaching is one of the most common occupations for mothers. The Swedish register data show that 47 percent of all working mothers with children under the age of 11 work in the public sector compared to 17 percent of fathers. Furthermore, 14 percent are found within the three detailed 4-digit occupations nursing, lower secondary teaching and childcare. This is by no means unique to Sweden. According to the American Community Survey, for example, 40 percent of women with children work within the industry code "Educational services, health care and social assistance" compared with 23 percent overall. Prior literature suggests that women's preferences for these public sector jobs are highly linked to motherhood (Adda et al. 2017, Hotz et al. 2017, Pertold-Gebicka et al. 2016). All of these facts make the case that the wage reform in the teaching profession implies a wage increase for women,

[8] In Section 4.1, we confirm that the relative working time between the female partner and male partner is largely unaffected by the reform.

[9] This section draws on Grönqvist et al. (2022). There, we show that the reform had full pass-through onto wages, and that schools that implement career teacher promotions have lower teacher turnover and a more qualified teaching pool.

and therefore, we expect broader gendered consequences.

3 Empirical Design

This study uses administrative data from Sweden. In this section, we first explain the main data sources and how we measure two key variables: teacher promotions and child-care duties. Next, we outline our analytical design. Finally, we present summary statistics for the sample under study.

3.1 Data Sources and Key Variables

Data Sources. We construct our main dataset using annual Swedish register data for the full working-age population aged 16-74. Ultimately, we end up with a balanced panel of couples that we follow for seven years around the time of the pay increase. The panel includes detailed information about the couple such as their wages and earnings, place of work, use of social insurance benefits, number of children and demographic variables.

Data are drawn from the following sources and calendar years: a longitudinal individual-level database that covers all individuals in Sweden aged 16 to 74, available to us from 2007 to 2017 (*LOUISE*; Statistics Sweden 2022b), matched employee-employer data, 2007–2018 (*RAMS*, Statistics Sweden 2022c); structural wage statistics, 2007–2018 (Statistics Sweden 2022a)[10]; days of temporary (2007–2016) and regular (1993–2013) parental leave from the Social Insurance Agency; and information on children from the multi-generational register.

We identify each person's main place of work as the workplace where they have the highest labor earnings each year. The wage measure shows monthly full-time equivalent wages at the main place of work in Swedish crowns (SEK) and is measured between September and November. Annual earnings, on the other hand, sum labor earnings across all workplaces for the individual. Finally, if the person has no recorded earnings or days of temporary leave in the registers, we assume that these are zero.

Teacher pay raise. Our data does not include information on actual career teacher promotions. To identify the policy-driven pay increases we therefore exploit the institutional features of the reform: a teacher is classified as treated from year s until the last period of observation if the monthly full-time equivalent wage rises by 5,000 SEK or

[10]The structural wage statistics include information on wages, occupations and hours worked. They are available for everyone working in the public sector and an approximately 50 percent stratified sample of workers in the private sector.

more between $s-1$ and s, where s starts in 2013.[11,12] The year of treatment is the year when the wage increase is observed. In the main analysis we restrict attention to teachers promoted in 2013 and 2014 to keep the panel balanced in event time, since we only have access to TPL data until 2016 and follow the couples three years following promotion.

Due to the unprecedented size of the stipulated wage increase in relation to the normal variation in wages, we are confident that our strategy allows us to isolate the reform-driven variation in wages. To corroborate this, Figure 2 shows the evolution of mean wages of non-promoted teachers and promoted career teachers (CT), by year of promotion (2013 to 2016) using supplementary data on individual wages linked to information about actual promotions.[13] Prior to the pay increase, mean wages are very similar for promoted and non-promoted teachers while they increase sharply upon becoming career teacher. Thus, it is clear that the wage policy had a meaningful effect on the level of wages: mean full-time wages were around SEK 27 000 (approx. USD 2 800) between 2010 and 2012, and nearly SEK 31 000 (approx. USD 3 200) in 2015, representing a 15 percent increase in mean wages after introducing the career teacher wage reform. Moreover, the number of pay increases we identify is slightly lower, but closely follows, official statistics.[14]

[11] We identify teachers as individuals who have an occupational code as a teacher at their main place of work. Precisely, we use occupation ($SSYK$) codes 2321, 2322, 2323, 2330, 2340, and 2351 until 2014, and codes 2320, 2330, 2341, 2351 from 2014 onward.

[12] With our data, we can in principle identify teacher promotions between 2013 and 2017; however we stop in 2015. Our method is not appropriate to define promotions in 2016 or 2017 because another teacher salary reform, the Teachers' Salary Boost, was implemented in 2016. This entailed a smaller unconditional wage increase to about half of the teaching pool.

[13] It is not possible to link the promotion data to the social security records, thus these data are nor appropriate for our main analysis.

[14] Statskontoret (2017) report 3,076 promoted teachers in autumn 2013, 12,114 in autumn 2014 and 14,340 in autumn 2015, compared to 3,062, 10,627 and 13,500 total promotions in our data. That we have slightly fewer promotions could be because we cannot capture all teachers in private (voucher) schools.

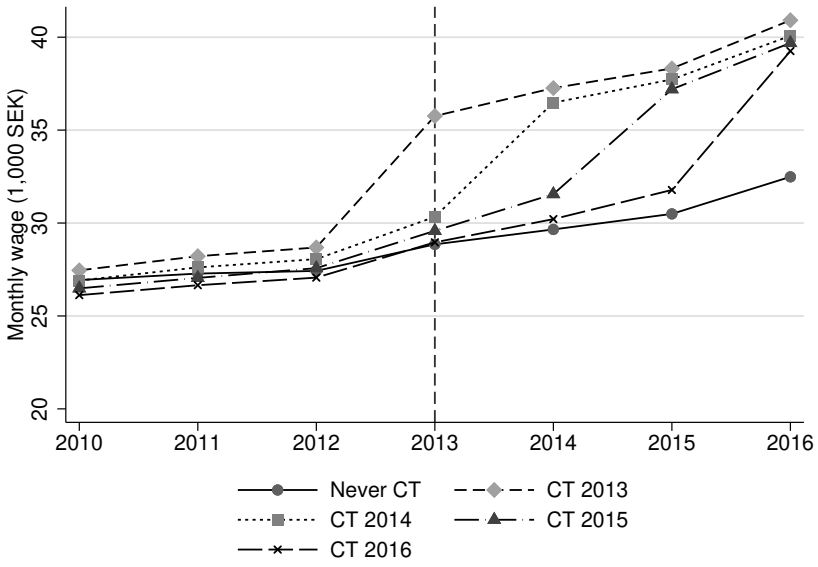


Figure 2: Reform impact on teacher wages

Note: The figure, taken from Grönqvist et al. 2022, shows mean teacher wages separately by year of promotion. The solid line shows the wage growth of teachers that never become career teacher (CT). Wages are expressed in nominal terms since this is a near zero inflation period. From 2010 to 2016 CPI increased by 4.3 percent with an average inflation rate of 0.07 percent.

Childcare time. Measuring the division of care responsibilities is challenging. Time use surveys often have limited sample sizes and can be prone to biased reporting. We address these issues by leveraging the Swedish social security system. We measure childcare duties by the gross number of days per year that each parent took off from work to care for their sick children (the *temporary parental leave* or *TPL*). The Swedish temporary parental leave system is very flexible, allowing parents up to 120 days of paid leave annually to care for sick children during work hours.[15] Parents with children aged 8 months up to 12 years are eligible for paid leave which is reimbursed at 80 per cent of current earnings (up to a cap).[16] This generous system, which facilitates parental reconciliation of work and family duties, is not utilized to the same extent by mothers and fathers. On average, about 60 percent of TPL is taken by mothers (Försäkringskassan 2020).

While interesting to study in its own right, TPL has also been found to serve as a good indicator of the gender division of household chores. Unlike other time-use data, TPL is available in registry data (Eriksson and Nermo 2010). In Figure 3 we use supplementary information from a Swedish survey linked to administrative data on TPL uptake

[15]In 2019, 877,000 parents received TPL benefits to care for 801,000 children (Försäkringskassan 2020). The leave can be taken out as full days, three quarters of a day, half days, one quarter of a day or one eight of a day.

[16]If earnings are above a cap, TPL is instead reimbursed at 80% of the cap. The cap is set at 7.5 so-called price basis amounts. In 2020, the cap was 29,563 SEK (3,400 USD) per month. A price basis amount is used in Sweden for the calculation of various benefits and fees, and is adjusted annually for inflation.

to examine the relationship between wages, TPL use and other household responsibilities of mothers and fathers.[17] While the underlying sample is much smaller it confirms the strong negative correlation between the female contribution to household wages and her relative TPL uptake, suggested by Figure 1. Figure 3 shows that relative wages are not only linked to the use of TPL but also to other household responsibilities such as picking up children from school and daycare on weekdays (Panel b), and hours spent on cooking, cleaning and laundry (Panel c). Consequently, couples with larger gender wage differentials are also found to have a more unequal division of domestic chores more generally in the cross-section (Panel d). Women's temporary parental leave and other domestic time inputs are thus complements in our data.[18]

[17]The survey is the Swedish Level of Living Survey, a survey to a 0.0001 random sample of the Swedish population aged 15 to 75 years of age. We use respondents surveyed in 2000 and 2010 and link responses to administrative data on TPL uptake during the same year using a person identifier. We restrict the analysis to couples with children where both partners are employed (to calculate relative wages) and who took at least one day of TPL.

[18]The slope coefficient in Figure 1d is 0.3 and is unaffected if we also control for the households' total number of hours spent on cleaning, cooking and laundry (see Appendix C, Table B.1). This evidence is also consistent with Ichino et al. (2024) who show that there is a positive correlation between fathers' own TPL use and their total home production within dual earner couples with children aged 3 and above (conditioning on total spouse home production time and their human capital levels).

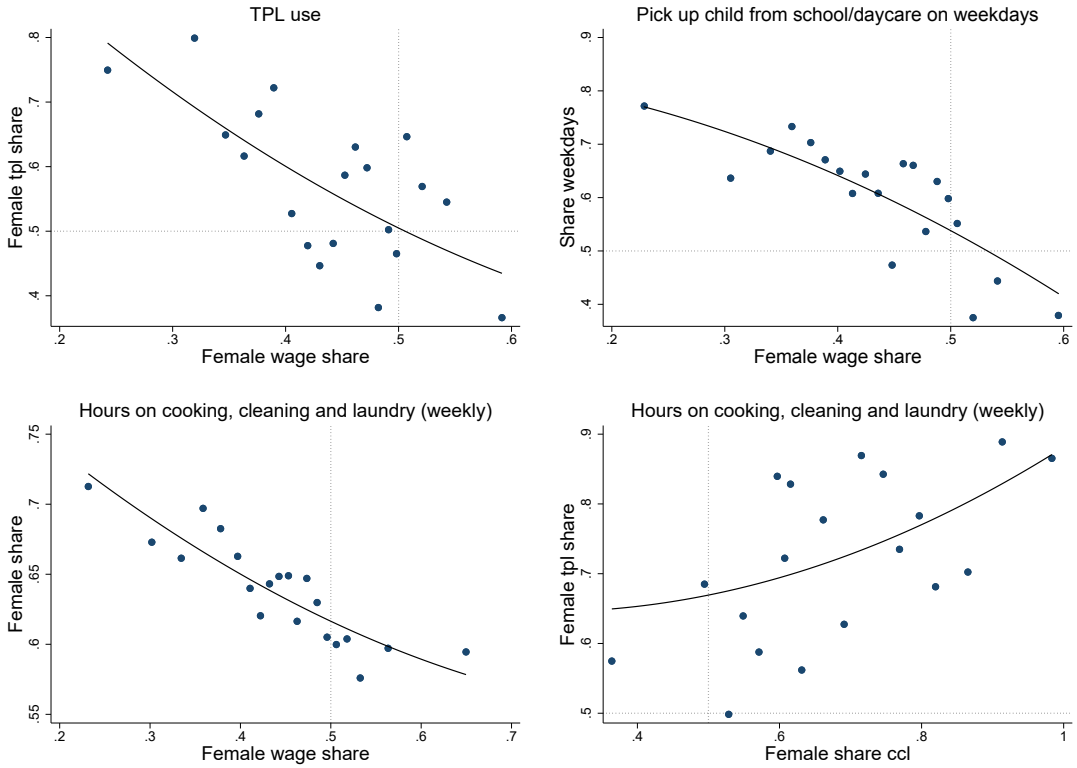


Figure 3: Relative wage, TPL and other domestic chores

Note: In Panel (a), we revisit the relationship between the female wage share and share of TPL shown in Figure 1. Panels (b) and (c) show the associations between the female wage and her self-reported time spent on other childcare and domestic activities relative to partner. Finally, in Panel (d) we show the association between self-reported fraction of time spent on household chores and relative TPL uptake.

3.2 Empirical Design

We use a "stacked" event study design to identify the impact of a shock to the spousal pay gap on the division of childcare duties. This design is a variation of the difference-in-differences approach, applied when policy rollout is staggered. Specifically, we treat each year of policy implementation ($c = [2013, 2014]$) as a separate "sub-experiment." Within each sub-experiment, we create an analytical sample for a difference-in-differences estimate. Finally, we combine all analytical samples into a "stacked" dataset.

The sample includes treated units and clean controls. The treated units are female teachers (and their partners) who were eligible for promotion and actually promoted in that event year. The clean controls are female teachers (and their partners) who were also eligible for promotion in the same year but were not promoted.[19] Both the promoted teachers, and their controls, have occupation codes as teachers in the year of promotion,

[19]Precisely, we can observe the teachers' level of education and years of experience. We restrict to teachers who at least had post-secondary education and four years of potential experience. You cannot be a control if you are defined as promoted in another year, i.e. the control teachers are never treated.

but we do not require them to be teachers in other years.

Each observation refers to a couple h in cohort (sub-experiment) c and event year e. The study window spans from four years before the event to three years after ($e = [-4, \ldots, 2]$). Notice that we have a balanced sample in event time, and that we define the event time for both treated and control couples. We restrict the sample to couples with children under the age of 10 in the pay rise year, and whose firstborn child was at least two years old four years prior to the pay rise. This restriction ensures that these couples are eligible for temporary parental leave throughout the study window. The couples themselves are defined four years prior to the event using family identifiers, combined with the restriction that the age difference between the male and female partner is less than 18 years. We track couples throughout the study period, even if the partnership dissolves.

With the stacked dataset, we run the *weighted stacked event study regression* proposed by Wing et al. 2024. For a couple h of experiment cohort c in event year e, we run

$$y_{hec} = \alpha + \beta D_{hc} + \sum_{k=-4,\ldots 2, k\neq -1} \Big[\lambda_e 1[e=k] + \theta_e D_{hc} \times 1[e=k]\Big] + \varepsilon_{hec} \tag{1}$$

where y_{hec} is the outcome of interest, such as the division of childcare duties. D_{hc} is a dummy variable equal to one if the household belongs to the treatment group (i.e. where the female spouse has received a major pay raise) in cohort c, and zero if the household belongs to the control group in cohort c. e denotes event time relative to the year of the major pay increase, which occurs when e equals 0, and $e = -1$ is omitted from the regression. Standard errors are clustered at household level.

When running the regression (1), we weight the sample with the following weights:

$$Q_c = \begin{cases} 1 & if D_{hc} = 1 \\ \frac{N_c^D / N^D}{N_c^O / N^O} & if D_{hc} = 0 \end{cases} \tag{2}$$

where N_c^D are the number of treated in cohort c and N^D are the total number of treated across cohorts, while N_c^O are the number of control in cohort c and N^O are the total number of control observations across cohorts.

The key parameter of interest is θ_e, which identifies the trimmed average on the treated (ibid.). Furthermore, we compute a linear combination of the estimated θ_e for the three post-treatment years to summarise the overall effect of the major pay raise.

In our main analysis, we follow the procedure proposed by ibid. We perform a number of robustness checks to assess the stability of the estimates to alterations of model (1). These are included in Appendix D. We replace the simple treatment indicator D_{hc} with household fixed effects to more fully control for couple type, and also include controls

for the number of children in different age intervals. Our results are robust to these alterations.

3.3 Summary Statistics

In our stacked data, we observe about 2,000 female teachers who received the pay increase. In Table 1 we describe treated and control couples in the year prior to the pay increase, focusing on key variables. For an extended set of observable characteristics for both individuals and couples, see Appendix Table E.1.

Table 1: Summary statistics in year before teacher major pay rise

	Treated Mean	Control Mean
Youngest child 0–3	0.19	0.31
Youngest child 4–7	0.66	0.57
Youngest child 8–11	0.15	0.12
Annual earnings gap (SEK)	-129,529	-152,006
Female spouse has higher earnings	0.26	0.21
Female spouse claims TPL	0.80	0.76
Female spouse annual TPL days	4.64	5.24
TPL gap (days)	1.35	2.06
Observations	2042	11644

Note: The table shows summary statistics for treated and control couples in the year prior to the female teacher major pay rise. If control couples appear in several cohorts ("experiments"), only one observation is included in the table.

Compared to control couples, treated couples have slightly older children while their total household use of TPL is relatively similar, corresponding to around 8 days per year on average, of which the female spouse takes around 5. The full distribution of household TPL in the year prior to promotion is included in Figure E.1. Women tend to be more educated than their male partners but have lower earnings. In line with the eligibility requirements for career teacher promotion, all promoted teachers have post-secondary education. On average, they are secondary earners in both treated and control couples and the wage difference is substantial: Women contribute to less than 45 percent of the household earnings and is only primary earner in around a quarter of the couples. Mothers are also the primary childcare takers: 80% of treated female teachers claim benefits the year prior to the wage increase, compared to 56% of their partners, and the female spouse claims around 60% of the couple's total TPL. While the general patterns hold, control couples appear slightly more unequal than the treated couples before the wage shock: they have a larger within-household earnings and TPL gap, which in turn could relate to the age of their children. Appendix D shows that our results are robust to controlling for child age.

4 Results from the Teacher Wage Reform

4.1 The Couple Wage Gap

First, we investigate whether the teacher promotion reform worked as we expected. Specifically, we test if the reform increased promoted female teachers' earnings and wages relative to non-promoted female teachers, and therefore whether the spousal wage gap closed post reform. To do so, we run the weighted stacked event study regression (1) with four different measurements of the couples' relative earning power.

Table 2 shows the results. Column 1 indicates that the control couple earnings gap pre-reform, defined as the female teacher's annual labor income minus the male partner's, is -145,908 SEK (14,600 USD). This gap narrows by 36,829 SEK due to the reform, or 25 percent. Column 2 shows that the fraction of female primary earners increases by 11.9 percentage points, which accounts for 54 percent of the control mean.

Column 3 shows the impact on the within-couple full-time equivalent monthly pay difference. These results are interesting as they hold hours worked fixed; however, it is important to note that wage information is available for a sub-sample due to sampling in the structural wage statistics.[20] There is a stark pre-reform couple wage gap of -9,576 SEK. This gap narrows by 5,427 SEK due to the reform shock. As expected, the effect size is very close to the stipulated wage increase from the teacher promotion reform. Column 4 finally shows the couple wage gap in log terms, where the policy shock narrows the gap by 15.1 log points (or 16.3 percentage points). This change accounts for about 60 percent of the pay gap at the control mean. Overall, the teacher promotion reform caused a statistically and economically significant reduction in the couples' pay gap post-reform.

Figure 4 shows event study graphs corresponding to the regression in Table 2 Columns 1 (earnings gap) and column 3 (wage gap). There is no statistically significant difference in the couple earnings/wage gap between the treatment and control groups before the reform shock. We also see no anticipation effects. This result supports the identifying assumption that outcome trends should evolve similarly between the treatment and control groups.

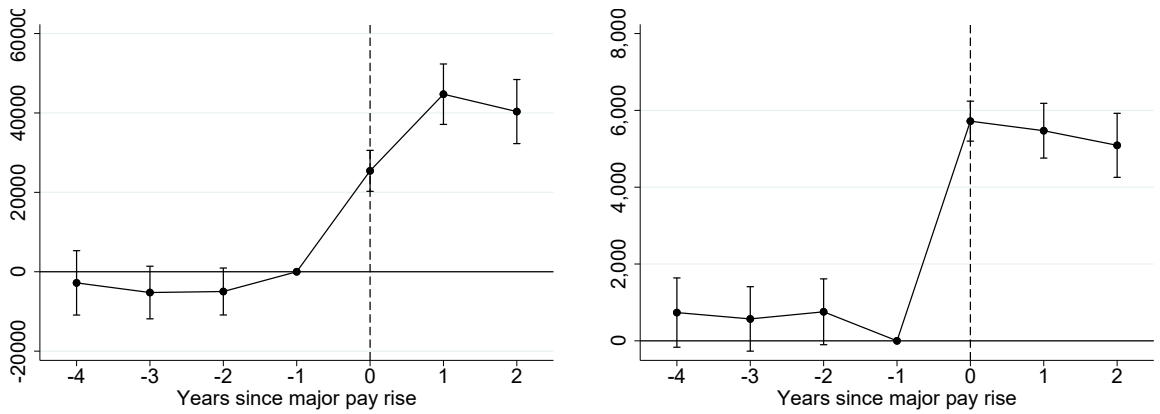
We also examine female teachers' and their partners' earnings and wages separately. Table 3 Column 1 confirm that the reform shock increased female teachers' annual earnings by 44,077 SEK, or 16 percent. Meanwhile, Column 2 shows a small positive impact on the earnings of the male partner, corresponding to 7,248 SEK, or 1.7 percent. This male response may be driven by labor supply responses; we see no impact on male partners' full-time wages (column 4). Overall, these findings confirm that the change in the couple wage gap is due to the increase in female teachers' wages from the promotion reform.

[20]The TPL responses on this unbalanced sample are similar to the overall TPL response, albeit at times somewhat smaller in magnitude and statistical significance (see Table D.5)

Table 2: Effect of teacher pay increase on household relative wage

	(1)	(2)	(3)	(4)
	Earnings diff. (SEK)	Woman out-earns partner	Monthly wage diff. (SEK)	Difference log wages
Average effect	36829.387***	0.119***	5426.529***	0.151***
	(2991.761)	(0.009)	(297.241)	(0.006)
N	140,392	140,392	61,956	61,956
Control mean	-145907.79	.22	-9575.82	-.25

Note: The estimates are based on equation 1 for the outcomes listed in the column headings. The average effect is found by a linear combination of the three post-treatment years. The within-couple difference is defined as female minus male spouse. Standard errors are clustered by couple. Control mean shows the unconditional mean of the outcome for the control group in the year prior to the pay increase.



(a) Annual earnings (SEK) (b) Monthly wages (SEK)

Figure 4: Within-couple difference in earnings & wages before and after teacher pay rise

Note: The figure plots estimated effects for teachers with 95% confidence intervals based on equation (1). The within-couple difference is the female spouse minus the male partner.

Finally, we examine whether the reform's impact on female teachers' wages was accompanied by a change in hours worked. Table 3 Column 5 shows the effect on female teachers' average monthly hours, and Column 6 shows the effect on male partners'. Hours worked are measured in total hours worked in the same month as wages are measured.[21] We find no economically meaningful impact on hours worked in either case, although the point estimates suggest that female teachers slightly increased their hours while male partners slightly decreased theirs.

In sum, we find that the individual wage shocks had a sudden and clear impact on

[21]Hours are not available for state employees. Hours are missing for 13 percent of the individuals for whom we observe wages, and are measured between September and November.

Table 3: Individual wage and hours responses for teachers and their partners

	(1)	(2)	(3)	(4)	(5)	(6)
	Annual earnings (SEK)		Monthly wages (SEK)		Hours worked	
	Promoted female	Male partner	Promoted female	Male partner	Promoted female	Male partner
Average effect	44077***	7248***	5223***	-204	1.375*	-1.287
	(1231)	(2807)	(62)	(297)	(0.833)	(0.972)
N	140,392	140,392	61,956	61,956	54,180	54,180
Control mean	269960	415867	27371	36947	140.97	147.75

Note: This table includes results on the effect of the reform-induced female pay increase on annual earnings (columns 1–2), monthly wages (columns 3–4) and hours worked (columns 5–6). Estimations are done separately for women and their male partners. The estimates are based on equation 1, and the sample is restricted to only include observations where the within-couple difference in the outcome is not missing. The average effect is found by a linear combination of the three post-treatment years. Standard errors are clustered by couple. Control mean shows the unconditional mean of the outcome for the control group in the year prior to the pay increase.

the couples' wage gap. This change is driven by the wage increases of female teachers, not by their male partners. In the next section, we present our main results: the impact of wage shocks on the division of days caring for sick children.

4.2 The Division of Childcare Duties

We now turn to the main outcome: the division of childcare duties between female teachers and their male partners. We measure childcare duties using TPL, the days parents can take off to care for sick children. The division is calculated as the within-couple difference in TPL days between the promoted female and her male partner.

Table 4 shows the estimates from the weighted stacked event study regression (1) with the division of TPL as the outcome. Column 1 shows that the treatment effect, averaged over the three years post-reform, implies a statistically significant narrowing of the childcare division. To put these estimates into context, female teachers generally take five days of TPL per year before the wage increase, while their partners take three days. As a result of the pay increase, 12 percent of the gender gap in temporary parental leave closes, relative to the control group mean in the year prior to the pay increase.

In Columns 2-7 we further investigate how female teachers and their male partners adjusted their TPL days separately by estimating the regression (1) with individual- and household level outcomes. Columns 2-3 consider TPL days taken by the female teacher (Column 2), male partner (Column 3), and the total days taken by both (Column 4), capturing responses to the reform shock at the intensive margin. The last three columns focus on the extensive margin: Column 5 considers whether a female teacher takes any

TPL days, Column 6 considers the male partner, and Column 7 considers the couple together. Table 4 Column 2 shows that female teachers take 5.24 TPL days per year on average. The reform shock reduced this by 0.440 days, about eight percent of the control mean. Interestingly, Column 3 shows that male partners also reduced their TPL days by 0.187 days. As a result, the total TPL days taken by both partners declined by 0.626 days, about seven percent of the control mean (Column 3). The effects at the extensive margin show a similar pattern (Columns 5-7).

Figure 5 shows event study graphs corresponding to the regression in Table 4 columns 1, 2 and 5. The graphs show no statistically significant pre-trend, supporting the validity of our identification strategy. It also shows a significant and immediate response in the division of TPL days at the time of the treatment, with the impact persisting until the end of the study window.

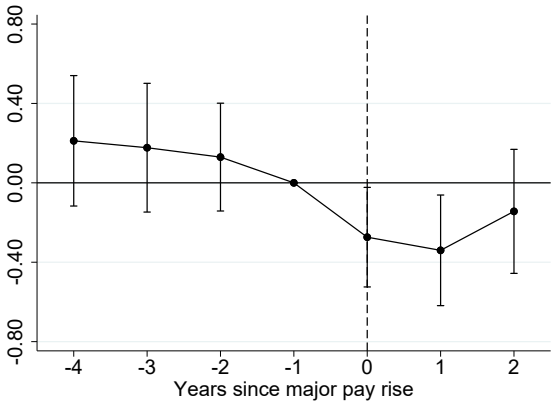
Our results are robust to various alterations of our main model. Tables D.1 to D.3 in the appendix include results separate by event period and cohort, as well as when we include couple fixed effects instead of treatment dummies and add controls for the number of children. The majority of couples take relatively few days of TPL but some couples take much more (see Figure E.1), in line with that TPL can be used to care for seriously ill children. Our main results are robust to excluding couples where annual TPL is ever in the 99th percentile or above (see appendix Table D.6).

In summary, a substantial wage increase for women shifts the responsibility of caring for sick children away from the woman, reducing her TPL share. This suggests that a positive shock to women's wages can effectively promote equality in childcare duties between partners. However, the adjustment is primarily made by the woman, with the male partner also reducing his TPL days, leading to a decrease in total household TPL usage. Would these findings apply to the broader Swedish population beyond teachers and their partners? We explore this question further in the next section.

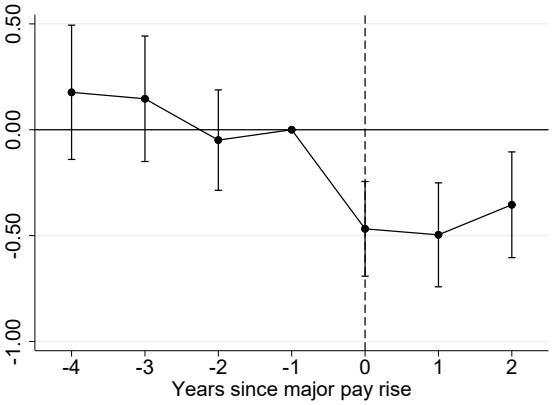
Table 4: Effect of teacher pay increase on individual and household TPL responses

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Difference	TPL days			Any TPL		
	TPL days	Promoted female	Male partner	Household	Promoted female	Male partner	Household
Average effect	-0.253**	-0.440***	-0.187*	-0.626***	-0.059***	-0.012	-0.046***
	(0.124)	(0.106)	(0.096)	(0.160)	(0.010)	(0.010)	(0.008)
N	140,392	140,392	140,392	140,392	140,392	140,392	140,392
Control mean	2.03	5.24	3.21	8.45	.78	.55	.85

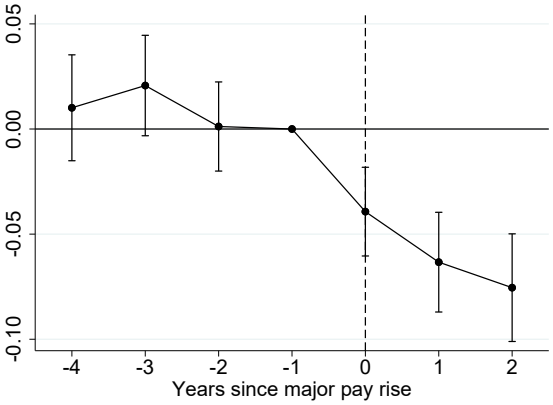
Note: This table includes results on the effect of the reform-induced female pay increase on the within-couple difference in TPL days (column 1), the number of leave days (columns 2–4) and whether the person takes any leave (columns 5–7). Estimations are done separately for women, their male partners, and at the household level. The estimates are based on equation 1. Standard errors are clustered by couple. The average effect is found by a linear combination of the three post-treatment years. Control mean shows the unconditional mean of the outcome for the control group in the year prior to the pay increase.



(a) Difference in TPL



(b) TPL days by female spouse



(c) Any TPL by female spouse

Figure 5: TPL uptake before and after teacher pay rise

Note: The figure plots estimated effects for teachers with 95% confidence intervals for the within-couple difference in TPL (panel a), the female TPL days (panel b) and if the female takes any TPL (panel c). The estimates are based on equation (1).

5 More Results from the Expanded Sample

To examine if the findings from the teacher sample apply to the broader population, we expand the sample to include all couples with children in Sweden. Using this expanded sample, we again employ a stacked event study design to identify the impact of a shock to the couple wage gap on the division of childcare duties. The expanded sample further allows us to examine the heterogeneous impact of a shock to the wage gap by couple types, particularly regarding adherence to gender norms and spousal workplace flexibility.

5.1 Expanded Sample and Empirical Specification

As with the teacher sample, we build the expanded sample using Swedish register data. However, the expanded sample includes all working women and their male spouses, regardless of occupation. Unless noted otherwise, all other variables are defined the same

as in the teacher sample. We measure couples' childcare duties by their TPL days. Data sources and details on TPL days are provided in Section 3.1.

A major challenge in expanding the sample is finding an event similar to the teacher promotion reform that shocks the couple pay gap. Following previous studies (Folke and Rickne 2020, Bronson and Thoursie 2021), we define the event as a woman's major pay increase from one year to the next. Specifically, we first estimate a wage equation for individuals based on their observable characteristics and wage history, then generate wage predictions. For an individual i in calendar year t, we estimate the following:

$$log(wage)_{it} = \alpha + \beta_1 log(wage)_{it-1} + \beta_2 female_i + \beta_3 X_{it} + \lambda_t + \lambda_m + \varepsilon_{it} \tag{3}$$

where $log(wage)_{it-1}$ are full-time equivalent wages lagged one year, $female_i$ is a female dummy, X_{it} are controls for age (in five categories) and level of education (in six categories), λ_t are year fixed effects and λ_m are municipality fixed effects. We define a major pay increase as a case where a woman's actual log monthly full-time wage in year t exceeds her predicted wage $log(\hat{wage})_{it}$ from the above regression by at least 10 log points. In other words, it is an event where a woman experiences a wage increase that is statistically unpredictable.

To implement the stacked event study design, we construct a stacked dataset similar to the teacher sample described in Section 3. Specifically, the expanded stacked dataset contains data for all women experiencing a major pay raise in four event cohorts ("sub-experiments") $c = [2011, 2012, 2013, 2014]$ and their male partners. Like for the teachers, the study window spans from four years before the event to three years after, and each observation refers to a couple h in cohort (experiment) c and event year e. As eventually promoted women may differ from women who are not promoted, in each sub-experiment, the control group consists of women who later experience a major pay increase outside the event window.[22] As with the teacher sample, couples are defined four years before the pay increase in each cohort. We also apply the same sample restrictions with regards to the age of the children. The resulting stacked sample contain about 22,000 women who experience a major pay raise. Table E.2 presents summary statistics of observable characteristics for the stacked treated couples and the stacked control couples.

Using the expanded stacked dataset, we run the weighted stacked event study regression (1), applying the weights defined in equation (2). Standard errors are clustered at the household level. The key parameters of interest are the trimmed average treatment effects, θ_c, in each post-event period. We also summarize the treatment effect by averaging the effects over the three post-treatment years.

[22]For the 2011 cohort, the controls receive pay increases between 2014 and 2017; for the 2012 cohort between 2015 and 2017; for the 2013 cohort between 2016 and 2017; and for the 2014 cohort in 2017.

5.2 Findings

Overall, the results from the expanded sample align with those from the teacher sample. Table 5, Panel A, shows the impact of women's major pay increases on the couple earnings gap (Column 1), the percentage of women who out-earn their male partners (Column 2) and the within couple gender wage gap (Columns 3 and 4). Following women's major pay increases, the earnings gap narrows substantially (by 28 percent), and the percentage of women who out-earn their husbands increases by 7 percentage points. There is also a stark narrowing of the gender wage gap within couples, which is slightly larger- although of similar magnitude- as in our main analysis. In Appendix Table E.3 we report the mediating impacts of women's major pay increase on their own log earnings (Column 1), and their male partners' earnings (Column 2). As in the teachers sample we conclude that the pay increase has a large impact on the earnings and wages of the female spouse, with a small positive impact on the earnings of the male partners. Women's major pay increase is associated with increases in working hours by about 5 hours per month, or four percent of the control mean. Their partners, however, do not change their hours worked (Columns 5 and 6).

Turning to the main outcome, Table 5, Panel B, shows the treatment effects of the women's major pay increase on the division of childcare duties. As Column 1 indicates, the gender gap in TPL uptake decreased significantly by 0.376 days. This impact size is slightly larger than the 0.253 days decrease observed in the teacher sample.

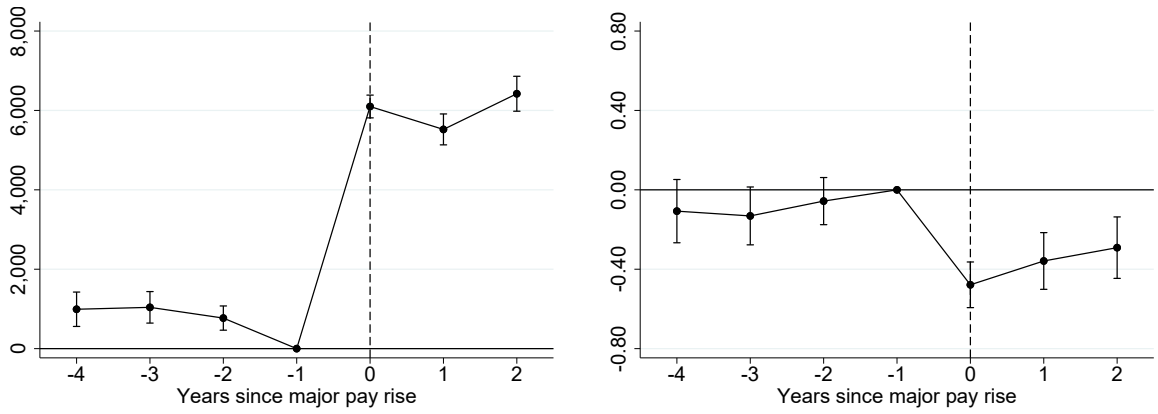
The pattern of individual responses, shown in Columns 2 and 3 also aligns with the teacher sample results. As expected, female partners decrease their TPL days by 0.546 days, or 11 percent of the control mean. This size is comparable to the teacher result of 0.440 days (8.3 percent of the control mean). Although smaller, male partners also reduce their TPL days by 0.17 days. Consequently, the total TPL days taken by both partners decrease by 0.716 days, similar to the teacher result of 0.626 days. Similarly, we find negative and significant impacts on the spousal difference in TPL days on the extensive margin that are very similar to those of teachers. Our findings suggest that the narrowing childcare gap, accompanied by declining total TPL days, is not unique to the teacher sample. We dig deeper into the mechanisms behind these findings in Section 5.3.

Figure 6 shows the event study graphs for wages and TPL use. These figures are comparable to those seen in the teacher sample. As with the teacher promotion reform, women's major pay increases also serve as a shock to the couple wage gap which coincides with a sharp reduction in the couple's TPL gender gap. Results presented in Appendix Table D.4 show that the main results are robust to variations in the main model and also hold separately by pay increase cohort.

Table 5: Effect of major pay increase in expanded sample

	(1)	(2)	(3)	(4)
Panel A: Pay effects				
	Earnings diff. (SEK)	Woman out-earns partner	Monthly wage diff. (SEK)	Difference log wages
Average effect	37756.795***	0.069***	6013.901***	0.171***
	(1640.855)	(0.003)	(160.304)	(0.003)
N	520,583	520,583	180,096	180,096
Control mean	-132780.32	.24	-7302.77	-.19
Panel B: TPL effects				
	Difference TPL days		TPL days	
		Promoted female	Male partner	Household
Average effect	-0.376***	-0.546***	-0.170***	-0.716***
	(0.061)	(0.056)	(0.039)	(0.075)
N	520,583	520,583	520,583	520,583
Control mean	1.98	4.77	2.79	7.57
			Any TPL	
		Promoted female	Male partner	Household
Average effect		-0.058***	-0.019***	-0.041***
		(0.004)	(0.004)	(0.003)
N		520,583	520,583	520,583
Control mean		.63	.48	.74

Note: This table replicates the analysis in Table 2 and Table 4 in the expanded sample described in Section 5.1.



(a) Monthly wage difference (SEK) (b) TPL difference

Figure 6: Effect of a female major pay increase on the within-couple difference in wages and TPL uptake

Note: The figure plots estimated effects with 95% confidence intervals based on equation 1.

5.3 Heterogeneous Effects

Our findings show that, on average, a woman's pay increase reduces the gap in temporary parental leave (TPL) days taken by couples. This narrowing occurs primarily because women reduce their own TPL days. Interestingly, men also reduce their TPL days, although to a much smaller extent. As a result, the total number of TPL days taken by the household declines. The first two observations align with economic reasoning: a higher wage increases the opportunity cost of a woman's time, making it more costly for her to take leave. However, the third finding, that men also reduce their TPL days, contributing to an overall decline in household TPL, is less intuitive. Given that the household demand for TPL is largely driven by children's sickness, it is not obvious why the husband would take fewer TPL days following his wife's pay increase. If the mother steps back from caregiving responsibilities for sick children, we would expect the father to step in, not reduce his own involvement.

To make sense of this pattern, we explore three possible explanations. One possible explanation is that, rather than fathers stepping in, parents turn to other sources to take over child care responsibilities amid women's pay increase. In the Swedish context, where the use of market-based childcare services is relatively uncommon, grandparents are a likely source of informal support. To explore this possibility, we first examine whether the treatment effects vary depending on the proximity of grandparents. Specifically, we test whether couples with grandmothers living nearby respond differently by estimating the following regression specification.

$$y_{hec} = \alpha + \beta D_{hc} + \Omega H_h + \sum_{k=-4,\ldots 2, k\neq -1} \Big[\lambda_e 1[e=k] + \theta_e D_{hc} \times 1[e=k] + \omega_e H_h \times D_{hc} \times 1[e=k]\Big] + \varepsilon_{hec} \quad (4)$$

where H_h takes one if grandmothers are nearby, and zero otherwise. Other variables represent the same as in the main regression equation (1). Compared to the main specification (see equation (1)), this interacts $D_{hc} \times 1[e=k]$ (the indicator variable equal to 1 in event year e for treated couples) with the couple type H_h, and also allows for a baseline difference in the outcome variable by couple type.[23] Note that in these analyses we only make use of the expanded sample, as we lack precision in the heterogeneity analysis for teachers.

Table 6, Panel A, presents the results. The availability of nearby grandmothers does not appear to moderate the treatment effect of women's pay increases on the division of TPL days. If anything, mothers with no nearby grandmothers appear to reduce their TPL uptake slightly more. Moreover, fathers reduce their TPL days regardless of whether grandmothers are present, leaving our initial puzzle unresolved. These findings suggest that informal support outside the household is not the primary margin of adjustment when women experience an unexpected increase in pay.

Another possibility is that responses differ across households depending on their gender norms. In more progressive households, where traditional gender roles are less binding, couples may be more responsive to economic incentives. In these households, women reduce their TPL days in response to the pay increase, while men increase theirs to compensate, resulting in a stronger overall adjustment. In contrast, in households with more traditional gender norms, the couple may resist reallocating childcare in response to economic changes. Instead of adapting to the wife's rising economic position, such households may preserve traditional roles by allowing the man to reduce his TPL contribution, reinforcing established gender dynamics. This kind of pattern has been observed in prior research, such as Bertrand et al. (2015), where men reduce their contribution to domestic duties and women increase when their partners earn more, as a form of role reassertion.

To test this hypothesis, we construct a measure of household gender progressivity based on couples' division of standard parental leave. Specifically, we define a couple as progressive if the father took more parental leave within the first two years following the birth of their first child than required by the "daddy months" policy.[24] According to this

[23] Compared to a full triple difference specification, this does not allow different counterfactual time trends by couple type nor different level differences in y_{hec} between treated and control couples.

[24] Between 2002 and 2015, two of the 16 months of parental leave were reserved for each parent.

definition, 31 percent of couples in our sample are classified as progressive. In Appendix C, we present correlational evidence from a nationally representative longitudinal survey showing that individuals' stated beliefs about gender equality within couples are associated with the actual division of standard parental leave. Taken together, this supports the notion that patterns of parental leave uptake reflect underlying couple equality norms.

Using the gender norm as the basis for heterogeneity H_h, we run the regression (4). Table 6 presents the results. As Column 3 indicates, we do find a slightly larger treatment effect among progressive couples (the first row of Panel B) than the traditional couples (the second row of Panel B), consistent with the view that gender norms may matter for how households allocate childcare duties. However, contrary to our initial expectation, it is the progressive fathers, but not the traditional ones, who reduce their TPL days in response to the wife's wage increase. This unexpected pattern deepens the puzzle, as we anticipate that progressive fathers would step in to offset the wife's reduced TPL take-up, not withdraw further themselves.

Finally, we explore the possibility that fathers do step in, but not by taking up more TPL days but rather working more at home, an alternative way to be with sick children at home. As the wife's opportunity cost of time rises, the couple seeks a cheaper option to take over the child care responsibilities, which is fathers' working from home that is fully paid. If this mechanism is the major margin to adjust in response to women's pay increase, then we should see the treatment effect only for couples where fathers' jobs actually allow them to work from home (WFH). For those with fathers without WFH possibilities, we should not see the treatment effects. To test this hypothesis, we construct a WFH index (whether or not WFH is possible for the job) for fathers' occupations based on a representative firm survey in Sweden, and run the regression equation (4). We are able to define WFH status for around 30 percent of the couples, and out of these, around two thirds of fathers can work from home (see Appendix A for further details). Given our earlier result showing heterogeneity based on gender norms, we estimate the regression with and without a control for the couple's progressiveness.

The regression results, presented in Appendix E Table E.4, support the work-from-home (WFH) mechanism. Figure 7 summarizes the findings by showing the treatment effects separately for households in which the husband has the option to work from home and those in which he does not. In households where the husband can work from home, the wife significantly reduces her childcare time following a positive wage shock, and so does the husband. In contrast, when the husband lacks WFH flexibility, neither spouse significantly adjusts their TPL days in response to the shock. These results support the interpretation that, when faced with a higher implicit cost of TPL, husbands substitute away from formal leave and toward working from home as a more cost-effective form of caregiving. Crucially, these findings hold even after controlling for couples' gender norms

(Table E.4, Panel B). This suggests that occupational constraints, rather than attitudes alone, play a central role in shaping household responses, underscoring the importance of job characteristics in mediating behavioral adjustments to economic shocks.

The WFH mechanism also explains the seemingly puzzling result of the heterogenous responses based on couples' gender norms presented above. Couples progressive gender norm attitudes and the fact that fathers have jobs with WFH possibilities are positively correlated (correlation coefficient of 0.126). This could be because, for example, one's gendered attitude and occupational choice are related. Hence, the heterogeneity results based on gender norms are partly driven by the heterogeneous treatment effects based on WFH: on average, progressive men, who are more likely to have WFH options, significantly reduce their TPL days while traditional men, who are less likely to have the option, do not do so significantly. At the same time, progressive women, who are more likely to have WFH-possible partners, reduce greatly their TPL take-up, yielding a greater adjustment in terms of the division of TPL days among the progressive couples than the traditional couples. Thus, our findings provide a nuanced view on the role of gender norms for the household to readjust the intrahousehold decision in response to a positive economic shock to women.

Taken together, our findings indicate that the primary mechanism supported by the data is that women who experience an unexpected and significant pay increase substantially reduce their TPL take-up, but only if their partners have the option to provide childcare by working from home. In cases where fathers lack this flexibility, neither partner makes a significant adjustment to their TPL usage, leaving the division of childcare responsibilities unchanged. Our results thus underscore the critical role of men's workplace flexibility in translating women's economic advancements into meaningful changes in household caregiving arrangements. While prior literature, such as Goldin (2014), emphasizes the importance of job flexibility for mothers' career progression, our findings highlight that workplace flexibility for fathers is also crucial for promoting gender equality within the household.[25]

[25]Our takeaway on the importance of fathers' access to remote work broadly aligns with Sundberg and Scott (2025), who show that not only mothers' access to remote work, but also their male partners' access moderates the size of the child penalty on maternal employment and wage income following the first childbirth in Sweden.

Table 6: Heterogeneity in TPL responses

Outcome:	(1) Female TPL	(2) Male TPL	(3) Difference TPL
Panel A: Informal care			
Nearby grandmother	-0.472***	-0.148***	-0.324***
	(0.076)	(0.054)	(0.081)
No nearby grandmother	-0.677***	-0.153**	-0.524***
	(0.087)	(0.062)	(0.093)
N	454,251	454,251	454,251
P-value equal coef.	.0586	.9491	.0806
Control mean nearby grandmother	5.10	2.98	2.12
Control mean no nearby grandmother	4.86	2.85	2.02
Panel B: Progressive/traditional norms			
Progressive	-0.829***	-0.427***	-0.402***
	(0.090)	(0.069)	(0.097)
Traditional	-0.401***	-0.039	-0.362***
	(0.070)	(0.048)	(0.075)
N	496,874	496,874	496,874
P-value equal coef.	.0001	0	.7303
Control mean progressive	4.81	3.52	1.28
Control mean traditional	4.83	2.57	2.27

Note: This table includes results on the heterogeneity in couples' TPL responses based on informal care (Panel A) and gender norms (Panel B) for all female pay increases in the expanded sample. The estimates are based on equation (4), where H_h is a dummy for the couple being defined as having a nearby grandmother (Panel A) or being progressive (Panel B). Nearby grandmother (No nearby grandmother), or alternatively Progressive (Traditional), shows the linear combination of $\theta_e + \omega_e$ (θ_e) in the post-period. Standard errors are clustered by couple. P-value equal coef. shows the p-value of an F-test on whether the average (post-pay increase) effects are equal across couple types.

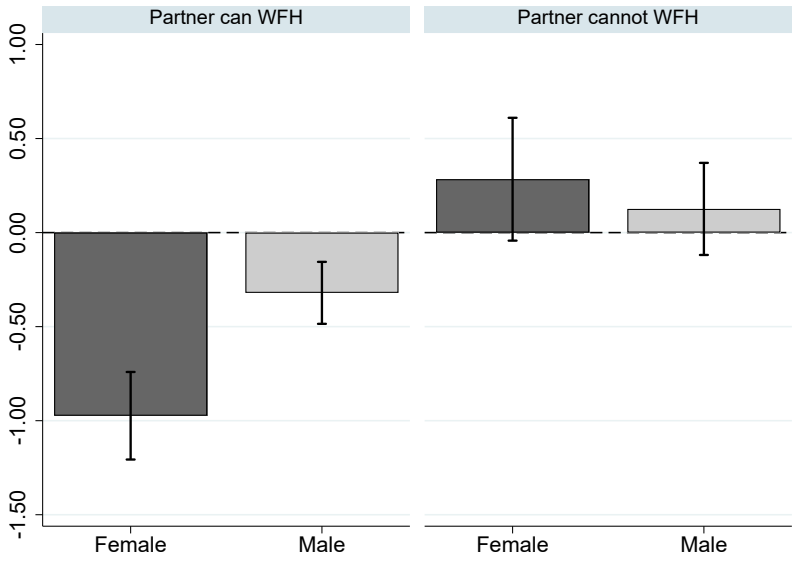


Figure 7: Heterogenity in TPL responses by partner WFH ability

Note: This shows heterogeneity in couples' TPL responses based on the partner's work-form-home (WFH) ability for all female pay increases in the expanded sample. The estimates are based on equation (4), where H_h is a dummy for WFH. In the regressions, WFH is defined based on the sector (blue collar or white collar) and industry that the spouse works in the year prior to the pay increase, and equals one if the spouse has the ability to WFH (a mean score of 2 or above). The regressions also include controls for being a progressive couple. Standard errors are clustered by couple. Full results are available in Appendix Table E.4.

6 Conclusions

Despite significant progress in education and the labor market, women continue to bear the primary responsibility for child-rearing at home. This pattern persists even in countries like Sweden, which has long implemented universal and gender-neutral family policies. In this paper, we examine the impact of a sudden positive shock to the couples' wage gap on the division of childcare duties at home. Our analysis is motivated by a strong cross-sectional correlation between the couples' wage gap and their childcare gap, measured by the relative uptake of temporary parental leave (TPL).

In our main empirical analysis, we utilize a reform in the Swedish labor market for teachers that began in 2013. The reform targeted skilled teachers, providing those promoted with a pay increase of about 15 percent of the pre-reform average wages. Since the teaching profession is female-dominated, this reform serves as a natural experiment that unexpectedly reduced the wage gap between female teachers and their male partners. Additionally, we extend our analysis to include all couples in Sweden, regardless of occupation, where the female spouse has received a large pay rise. Using both the teacher sample and the extended sample, we run a stacked difference-in-differences regression to identify the impact of a shock to the couples' wage gap on the division of childcare duties. We demonstrate that parallel pre-trends are satisfied in both samples.

We find that a substantial increase in women's wages, induced by a nationwide reform, leads to a narrowing of the within-couple gap in temporary parental leave (TPL), primarily because women reduce their leave days. Interestingly, men also reduce their TPL usage slightly, resulting in an overall decline in household leave days. While women's responses align with standard economic reasoning, men's reactions are less intuitive, prompting further exploration. Using a broader sample of Swedish couples, we confirm that the pattern extends beyond the teaching profession. We rule out explanations involving support from grandparents or compensatory behavior driven by traditional gender norms. Instead, the evidence points to a substitution mechanism: fathers increasingly work from home, a fully paid and flexible alternative, rather than taking formal leave. This shift is observed only where work-from-home arrangements are available, suggesting that flexible job structures play a central role in how couples adjust caregiving in response to wage shocks.

Taken together, our findings indicate that closing the gender wage gap has the potential to foster greater equality not only in the labor market but also at home. However, this effect is contingent on the institutional context, specifically, men's access to job flexibility. By enabling fathers to take on more caregiving responsibilities through alternative means like working from home, family-friendly workplace practices for men can have important spillover effects on women's careers. These results underscore the value of integrating

labor market and family policy perspectives in the pursuit of gender equality and open a promising avenue for future research.

Lastly, the welfare consequences for children are beyond the scope of this study. While we find that couples reduce their total TPL days as the wage gap narrows, the impact on children's welfare depends on the relative productivity of home care versus alternatives such as daycare or formal schooling. Additionally, we provide suggestive evidence that male partners' reduction in TPL uptake may reflect a shift to more remote work, as those without remote work options do not reduce their TPL usage. This finding adds nuance to the interpretation of fathers' reduced TPL uptake. Overall, the effect of reduced household TPL on children's well-being remains unclear, highlighting an area for future research.

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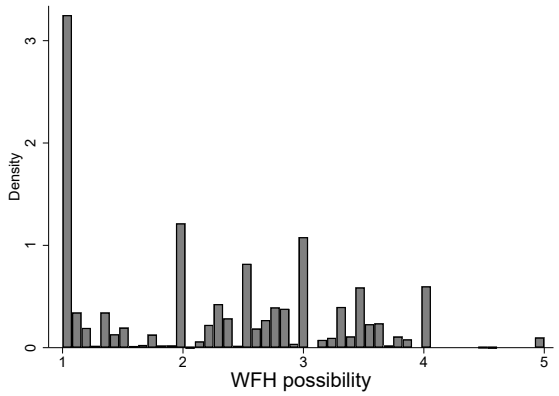
A Measure the possibility of working-from-home (WFH)

Our measure of partner's WFH possibility is based on information from the UCLS Survey of Collective Agreements and Flexible Work Conditions. The survey was conducted by Statistics Sweden to a representative sample of the Swedish private sector in 2013 with stratification done on industry and size (427 strata). A total of 8 352 firms were included in the sampling frame with a response rate of 39 %, corresponding to 3 259 firms. Firms were asked various questions about collective agreements, wage policies, time-space flexibility and work-life balance conditions for different types of workers in the firm (white collar workers, blue collar workers and executives). For our purpose we focus on the question: *Which opportunities do workers have to work from home?* with answers ranging from 1-5 with 1 corresponding to no possibilities and 5 corresponding to great possibilities.

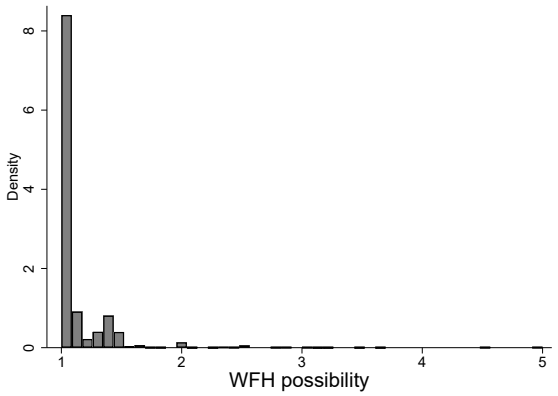
We can match the survey responses to our data at the industry×worker type (white or blue collar) level. For each partner-year, we therefore first identify his type and industry from the Structural Wage Statistics. In the next step, we add the mean WFH score from the survey. Due to sampling in the Structural Wage Statistics and the design of the survey targeting the private sector, the sample for this analysis is restricted to women who have partners working in private sector firms included in the Structural Wage Statistics.

Figure A.1 shows the distribution of male partner's WFH possibility. Around 35 percent of partners in this subsample have no possibilities to work from home. Not surprisingly, this inflexibility is primarily driven by blue-collar workers whereas white collar workers display more variation in the possibility of WFH.[26] In Section 5.3 we explore heterogeneous TPL responses by the male partner to wage shocks depending on partner WFH possibility. In the regressions, we divide couples by WFH ability depending on whether the spouse has any possibility to work from home in the year prior to the pay increase (a mean score of 2 or above).

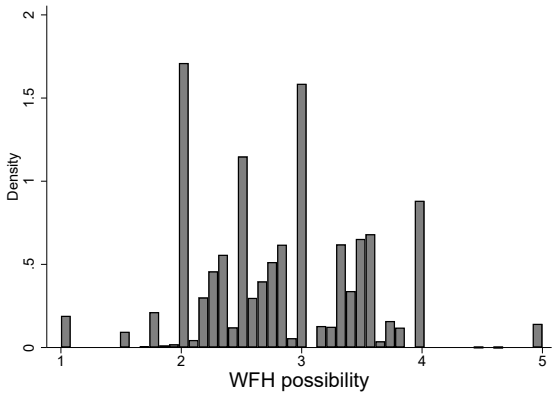
[26]The following five 2-digit sectors have the lowest WFH possibility for white-collar workers: Veterinary activities, Healthcare, Personal service activities, Animal production and Wholesale and retail trade and repair of motor vehicles and motorcycles. The following five sectors have the highest WFH possibility for white-collar workers: Legal and accounting activities, Telecommunications, Remediation activities and other waste management services, Mining support service activities and Water transport.



(a) WFH possibility



(b) WFH - blue collar workers



(c) WFH - white collar workers

Figure A.1: Partner's WFH possibility

Note: The figure shows the distribution of mean WFH possibilities based on 5-digit industry and worker type according to the firm-level survey. 1 implies no possibilities and 5 implies great possibilities

B Relationship between division of TPL and other domestic chores

In Section 3.1, we examine the correlations between couples' division of temporary parental leave (TPL) captured in administrative social security records and their self-reported division of other types of household chores measured by the Swedish Level of Living Survey (LNU). LNU is a nationally representative, longitudinal survey that collects information on individual living conditions, including family life, education, working life, economic circumstances, and health. For our purposes, the survey includes questions about time spent on various household chores. We use the two most recent waves of the survey, LNU2000 and LNU2010 including 4,400 and 5,000 respondents respectively. We restrict the sample to couples with children below age 10. Furthermore, we restrict to couples where both partners are employed and who took at least one day of TPL. For each couple, we calculate the female share of TPL as well as her share of total time spent on cooking, cleaning and laundry (CCL).

Column (1) in Table shows the bivariate relationship between female share of CCL and her share of total TPL takeup (also displayed in Figure 1 in the main paper. The correlation is 0.3 suggesting that couples with a larger gender gap in TPL uptake also have a larger gap in household chores more generally. In Column (2), we control for the sum of hours spent on CCL within the couple which does not affect the estimates. While based on a small sample, we conclude from this exercise that couples' TPL division and their division of other household chores appear to be complements.

Table B.1: Correlation TPL share and share of time spent on cleaning, cooking and laundry (CCL)

Dep. var: Female share- TPL		
Female share-CCL	0.330**	0.333**
	(0.127)	(0.126)
Household total hours- CCL		-0.004**
		(0.002)
Num.Obs.	167	167
R2	0.070	0.070
R2 Adj.	0.059	0.071

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

C Validate parental leave takeup as a proxy for gender norms

In Section 5.3, we proxy couples' gender norm by using the amount of paternity leave. In this section, we provide supportive evidence that the amount of paternity leave do correlate with one's expressed gender norm using survey data.

We draw data from the same survey data as in Appendix B (i.e. the Swedish Level of Living Survey (LNU)). Here, we primarily utilize data from the 2010 survey and supplement it with data from the 2000 wave, both of which includes the amount of parental leave each parent has taken as well as the question on views regarding couple equality on child care responsibilities.

Specifically, the 2010 survey asked respondents, "What do you think of the idea of going for a society where men take as much responsibility for children and the household as women do?" Participants answered on a 1-5 scale, with options such as "Very good idea," "Rather good idea," "Neither good nor bad," "Rather bad idea," and "Very bad idea." We aggregate the answers into three groups: "Very good or rather good idea," "Neither good nor bad," and "Very bad or rather bad idea." The same question was also asked in the previous 2000 wave. If a respondent did not answer this question in 2010 but did answer in 2000, we use their response from the 2000 wave instead.

In the following analysis, we will focus on the sample that consists of female respondents who meet the following criteria: they have at least one child born between 1992 (18 years old at the time of the 2010 survey) and 2008 (2 years old at the time of the 2010 survey)[27], their parental leave takeup and gender norm variables are available, and they have a heterosexual partner.[28]

Figure C.1 shows the respondents' (women's) share of PL takeup with the first child by one's view on the couple equality. Women shoulder a larger share of PL when they strongly disagree with the idea that men take as much responsibility for children and the household as women do: Their unconditional average of the respondents' PL takeup is 57.6 percent (shown by the top bar). The share, however, nears equal when they strongly agree with the couple equality: their PL share goes down to 53.0 percent (shown by the bottom bar).

Furthermore, Table C.1 shows the regression result by regressing the paternity leave

[27]The LNU survey collects information on parental leave for parents who have children under the age of 18 at the time of the survey. We also exclude respondents who have their first child under the age of two, as their parental leave is still ongoing and their parental leave variable may not accurately represent their final leave duration. In Sweden, parents are entitled to take parental leave until the child turns eight. However, in practice, the majority of leave is taken by the time the child is two years old. Therefore, the parental leave taken when the child is two years old closely reflects the final leave duration.

[28]Due to the small sample size, we did not include respondents with same-sex partners in our final sample.

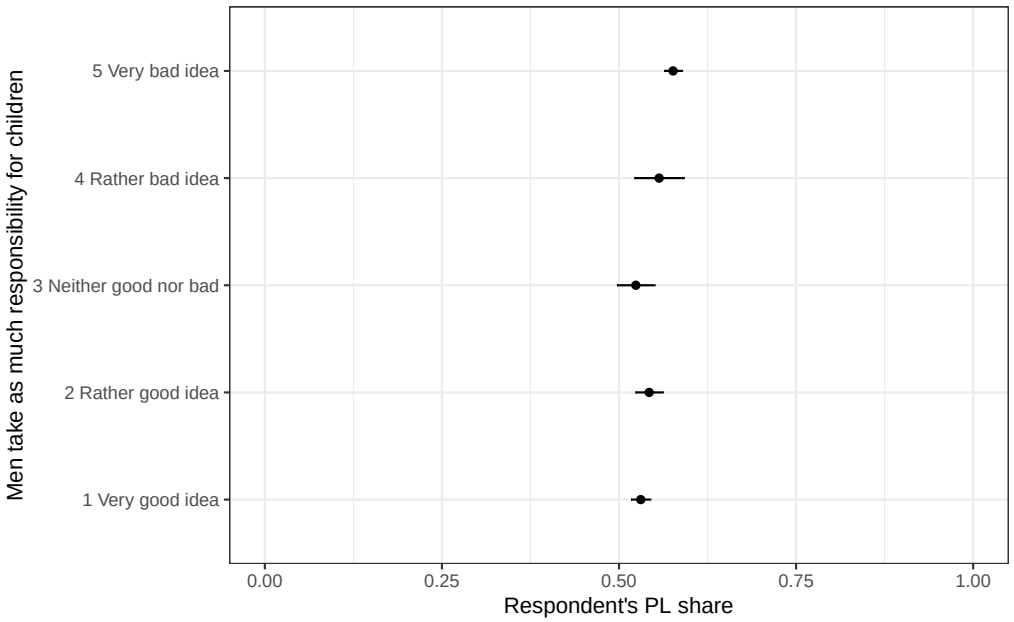


Figure C.1: Respondents (women's) PL share by their expressed couple-equality norm. Data on the parental leave uptake are drawn from the 2010 wave. Data on the couple equality norm are primarily drawn from the 2010 wave. If missing, then we use the data from the 2000 wave instead.
Source: The Swedish Level of Living Survey (LNU) 2000 and 2010.

length on gender norm, respondent's sex and age, the couple's relative wage rate, and the birth cohort of the child. In the regression, we group the answers "Very bad idea" and "Rather bad idea" into one category and the answers "Very good idea" and "Rather good idea" in to another category, resulting in having three norm groups ("bad," "neither," or "good"). The first column shows that, without controlling for any other respondents' characteristics, the "bad idea" group's PL share is higher by 3.6 percentage points compared to the "good idea" group. In the second column, we control for the year of the child's birth, aiming to control for the different parental leave policies that parents were exposed to. Finally, the third column additionally controls for respondents' and households' characteristics including the respondent's educational achievement (college or not), the couple education gap, the couple age gap, and the respondent's age at which the firth child was born. In either case, the correlation between the couple equality norm and one's PL share survives: with all the control variables, the "bad idea" group's PL share is higher by 4.8 percentage points compared to the "good idea" group.

The division of parental leave upon the birth of the first child appears to reflect gender norms. Our findings provide supportive evidence that the pattern of parental leave uptake can be used as a measure of the gender norm level to which a household conforms. This measurement is useful for assessing the level of norm at a population-wide scale without relying solely on survey data.

Table C.1: Couple equity norm and PL division

	(1)	(2)	(3)
Couple equality (ref. Good idea)			
Neither good or bad idea	-0.010	-0.012	-0.011
	(0.015)	(0.015)	(0.016)
Bad idea	0.036+	0.040*	0.048*
	(0.020)	(0.020)	(0.023)
Earmarked PL (ref. zero month)			
One daddy month		0.011	0.009
		(0.016)	(0.018)
Two daddy months		-0.031*	-0.025
		(0.014)	(0.016)
Couple relative education (ref. Hypergamy)			
Hypogamy			0.024
			(0.027)
Same			0.000
			(0.015)
University			-0.052**
			(0.016)
Couple age gap			-0.001
			(0.001)
Wife's age at the first childbirth			-0.001
			(0.001)
Constant	0.535***	0.550***	3.265
	(0.006)	(0.012)	(2.135)
Num.Obs.	733	733	607
R2	0.005	0.025	0.043
R2 Adj.	0.003	0.019	0.028

Note: This table reports the results from regressing the couple equality norm and other control variables on the division of parental leave. The couple equality norm is categorized as either "Good idea," "Neither good nor bad idea," or "Bad idea." During the studied period, the duration of earmarked parental leave varied between zero months (reference group), one month, and two months. Couples' relative education is classified as either "hypergamy" (a female partner's education is strictly higher than her male partner), "hypogamy" (a female partner's education is strictly lower), or "same." Additionally, the binary variable "University" equals one if the female partner has completed some college, and zero otherwise. The variable "Couple age gap" represents the age difference between the female and male partners. Lastly, we include the female partner's age at the time of her first child's birth.

Data: The Swedish Level of Living Survey (LNU) 2000 and 2010.

D Robustness checks

Table D.1: Robustness: Effect of major pay increase on difference in TPL days – teachers

	(1)	(2)	(3)	(4)	(5)
Panel A: Average effect					
Average effect	-0.253**	-0.449**	-0.173	-0.241*	-0.239*
	(0.124)	(0.222)	(0.150)	(0.124)	(0.124)
Panel B: By event time					
Event time, -4 × D	0.212	0.349	0.156	0.188	0.211
	(0.168)	(0.289)	(0.201)	(0.167)	(0.168)
-3 × D	0.177	0.141	0.191	0.164	0.178
	(0.165)	(0.289)	(0.199)	(0.165)	(0.166)
-2 × D	0.130	0.133	0.128	0.122	0.129
	(0.139)	(0.246)	(0.170)	(0.138)	(0.138)
0 × D	-0.274**	-0.320	-0.255	-0.267**	-0.267**
	(0.128)	(0.238)	(0.158)	(0.128)	(0.128)
1 × D	-0.340**	-0.663**	-0.210	-0.330**	-0.327**
	(0.142)	(0.258)	(0.168)	(0.142)	(0.142)
2 × D	-0.144	-0.363	-0.055	-0.127	-0.123
	(0.159)	(0.283)	(0.190)	(0.160)	(0.160)
Cohort	All	2013	2014	All	All
N	140,392	65,527	74,865	140,392	140,392
Event year FE	Yes	Yes	Yes	Yes	Yes
Treatment dummy	Yes	Yes	Yes	Yes	No
Couple FE	No	No	No	No	Yes
Child controls	No	No	No	Yes	Yes

Note: This includes results of the effect of the teacher major pay increase on the difference in TPL days (female - male) to alterations of equation (1). Panel A shows the average affect while Panel B shows the estimated coefficients by event time. In column (1) we include the original estimation. Columns (2) and (3) perform the same estimations separately by promotion cohort. Column (4) adds child controls to the main specification (number of children aged 0–3, 4–7 and 8–11) and column (5) additionally replaces the treatment dummy with a couple fixed effect. Standard errors are clustered by couple.

Table D.2: Robustness: Effect of major pay increase on female TPL days – teachers

	(1)	(2)	(3)	(4)	(5)
Panel A: Average effect					
Average effect	-0.440***	-0.427**	-0.445***	-0.428***	-0.412***
	(0.106)	(0.201)	(0.126)	(0.107)	(0.107)
Panel B: By event time					
Event time, -4 × D	0.177	0.334	0.113	0.042	0.109
	(0.162)	(0.300)	(0.189)	(0.161)	(0.161)
-3 × D	0.146	0.097	0.166	0.063	0.104
	(0.151)	(0.256)	(0.184)	(0.152)	(0.152)
-2 × D	-0.049	-0.082	-0.036	-0.089	-0.071
	(0.121)	(0.215)	(0.149)	(0.122)	(0.121)
0 × D	-0.468***	-0.297	-0.537***	-0.458***	-0.455***
	(0.114)	(0.220)	(0.138)	(0.115)	(0.114)
1 × D	-0.496***	-0.626***	-0.444***	-0.485***	-0.469***
	(0.125)	(0.232)	(0.147)	(0.126)	(0.126)
2 × D	-0.354***	-0.359	-0.352**	-0.340***	-0.313**
	(0.127)	(0.244)	(0.147)	(0.128)	(0.128)
Cohort	All	2013	2014	All	All
N	140,392	65,527	74,865	140,392	140,392
Event year FE	Yes	Yes	Yes	Yes	Yes
Treatment dummy	Yes	Yes	Yes	Yes	No
Couple FE	No	No	No	No	Yes
Child controls	No	No	No	Yes	Yes

Note: This includes results of the effect of the teacher major pay increase on the female spouse TPL days to alterations of equation (1). Panel A shows the average affect while Panel B shows the estimated coefficients by event time. In column (1) we include the original estimation. Columns (2) and (3) perform the same estimations separately by promotion cohort. Column (4) adds child controls to the main specification (number of children aged 0–3, 4–7 and 8–11) and column (5) additionally replaces the treatment dummy with a couple fixed effect. Standard errors are clustered by couple.

Table D.3: Robustness: Effect of major pay increase on female taking any TPL days – teachers

	(1)	(2)	(3)	(4)	(5)
Panel A: Average effect					
Average effect	-0.059***	-0.055***	-0.061***	-0.055***	-0.054***
	(0.010)	(0.018)	(0.011)	(0.010)	(0.010)
Panel B: By event time					
Event time, -4 × D	0.010	0.045*	-0.004	0.004	0.007
	(0.013)	(0.024)	(0.015)	(0.013)	(0.013)
-3 × D	0.021*	0.038*	0.014	0.017	0.019
	(0.012)	(0.023)	(0.014)	(0.012)	(0.012)
-2 × D	0.001	0.002	0.001	-0.001	-0.000
	(0.011)	(0.021)	(0.013)	(0.011)	(0.011)
0 × D	-0.039***	-0.026	-0.045***	-0.037***	-0.037***
	(0.011)	(0.021)	(0.013)	(0.011)	(0.011)
1 × D	-0.063***	-0.074***	-0.059***	-0.059***	-0.058***
	(0.012)	(0.023)	(0.014)	(0.012)	(0.012)
2 × D	-0.075***	-0.066***	-0.079***	-0.069***	-0.066***
	(0.013)	(0.023)	(0.016)	(0.013)	(0.013)
Cohort	All	2013	2014	All	All
N	140,392	65,527	74,865	140,392	140,392
Event year FE	Yes	Yes	Yes	Yes	Yes
Treatment dummy	Yes	Yes	Yes	Yes	No
Couple FE	No	No	No	No	Yes
Child controls	No	No	No	Yes	Yes

Note: This includes results of the effect of the teacher major pay increase on the female spouse extensive margin response (taking any TPL days) to alterations of equation (1). Panel A shows the average affect while Panel B shows the estimated coefficients by event time. In column (1) we include the original estimation. Columns (2) and (3) perform the same estimations separately by promotion cohort. Column (4) adds child controls to the main specification (number of children aged 0–3, 4–7 and 8–11) and column (5) additionally replaces the treatment dummy with a couple fixed effect. Standard errors are clustered by couple.

Table D.4: Robustness: Effect of major pay increase on difference in TPL days – expanded sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Average effect							
Average effect	-0.376***	-0.272***	-0.465***	-0.350***	-0.424***	-0.379***	-0.408***
	(0.061)	(0.101)	(0.115)	(0.117)	(0.141)	(0.061)	(0.056)
Panel B: By event time							
Event time, -4 × D	-0.107	-0.185	-0.240	0.046	-0.052	-0.099	-0.133*
	(0.081)	(0.140)	(0.158)	(0.145)	(0.175)	(0.081)	(0.071)
-3 × D	-0.131*	-0.064	-0.211	-0.024	-0.220	-0.123*	-0.157**
	(0.074)	(0.136)	(0.142)	(0.140)	(0.169)	(0.074)	(0.066)
-2 × D	-0.057	-0.067	-0.132	-0.090	0.044	-0.050	-0.084
	(0.061)	(0.113)	(0.124)	(0.121)	(0.145)	(0.061)	(0.055)
0 × D	-0.478***	-0.356***	-0.547***	-0.510***	-0.511***	-0.483***	-0.512***
	(0.059)	(0.106)	(0.119)	(0.119)	(0.145)	(0.059)	(0.055)
1 × D	-0.358***	-0.312***	-0.409***	-0.371***	-0.350**	-0.363***	-0.392***
	(0.073)	(0.120)	(0.138)	(0.142)	(0.170)	(0.073)	(0.068)
2 × D	-0.291***	-0.147	-0.438***	-0.168	-0.410**	-0.292***	-0.320***
	(0.079)	(0.128)	(0.139)	(0.150)	(0.175)	(0.079)	(0.073)
Cohort	All	2011	2012	2013	2014	All	All
N	520,583	183,785	144,158	113,715	78,925	520,583	520,583

Note: This includes results of the effect of the female major pay increase on the difference in TPL days (female - male) to alterations of equation (1). Panel A shows the average affect while Panel B shows the estimated coefficients by event time. In column (1) we include the original estimation. Columns (2) to (5) perform the same estimations separately by promotion cohort. Column (6) adds child controls to the main specification (number of children aged 0–3, 4–7 and 8–11) and column (7) additionally replaces the treatment dummy with a couple fixed effect. Standard errors are clustered by couple.

Table D.5: Individual and household TPL responses (wage sample)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Difference	TPL days			Any TPL		
	TPL days	Promoted female	Male partner	Household	Promoted female	Male partner	Household
Panel A: Teacher pay increase							
Average effect	-0.278	-0.357**	-0.079	-0.437*	-0.031**	0.007	-0.020*
	(0.177)	(0.155)	(0.139)	(0.234)	(0.014)	(0.014)	(0.012)
N	61,956	61,956	61,956	61,956	61,956	61,956	61,956
Control mean	2.01	5.45	3.43	8.88	.82	.62	.89
Panel B: Expanded sample							
Average effect	-0.193**	-0.285***	-0.092	-0.377***	-0.028***	-0.011*	-0.020***
	(0.097)	(0.091)	(0.063)	(0.123)	(0.006)	(0.006)	(0.005)
N	180,096	180,096	180,096	180,096	180,096	180,096	180,096
Control mean	2.28	5.48	3.2	8.68	.74	.56	.84

Note: This table replicates the analysis in Table 4 and Panel B of Table 5, only including observations if we also observe the couple wage gap.

Table D.6: Individual and household TPL responses (excluding large TPL households)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Difference		TPL days			Any TPL	
	TPL days	Promoted female	Male partner	Household	Promoted female	Male partner	Household
Panel A: Teacher pay increase							
Average effect	-0.248**	-0.452***	-0.205**	-0.657***	-0.059***	-0.013	-0.046***
	(0.109)	(0.094)	(0.081)	(0.137)	(0.010)	(0.010)	(0.009)
N	133,238	133,238	133,238	133,238	133,238	133,238	133,238
Control mean	1.91	4.75	2.84	7.59	.77	.54	.84
Panel B: Expanded sample							
Average effect	-0.395***	-0.531***	-0.136***	-0.667***	-0.059***	-0.018***	-0.042***
	(0.049)	(0.044)	(0.032)	(0.058)	(0.004)	(0.004)	(0.004)
N	493,794	493,794	493,794	493,794	493,794	493,794	493,794
Control mean	1.78	4.22	2.45	6.67	.62	.46	.73

Note: This table replicates the analysis in Table 4 and Panel B of Table 5, omitting couples who ever have TPL at or above the 99th percentile of annual household TPL usage in the respective samples, which corresponds to 39 days in the teacher sample and 42 days in the expanded sample.

E Additional Tables and Figures

Table E.1: Summary statistics in year before teacher major pay rise

	Treated		Partner		Control		Partner	
	Mean	sd	Mean	sd	Mean	sd	Mean	sd
Female	1.00	(0.00)	0.00	(0.00)	1.00	(0.00)	0.00	(0.00)
Married	0.78	(0.41)	0.78	(0.41)	0.74	(0.44)	0.73	(0.44)
Age	38.99	(3.37)	40.98	(4.28)	38.70	(3.90)	40.67	(4.79)
Compulsory	0.00	(0.02)	0.03	(0.16)	0.00	(0.00)	0.03	(0.17)
Upper secondary	0.00	(0.04)	0.31	(0.46)	0.00	(0.02)	0.37	(0.48)
Post-secondary or higher	1.00	(0.04)	0.66	(0.47)	1.00	(0.04)	0.60	(0.49)
Education missing	0.00	(0.00)	0.00	(0.04)	0.00	(0.00)	0.00	(0.03)
Youngest child 0–3	0.19	(0.39)	0.19	(0.39)	0.31	(0.46)	0.31	(0.46)
Youngest child 4–7	0.66	(0.47)	0.66	(0.47)	0.57	(0.50)	0.57	(0.50)
Youngest child 8–11	0.15	(0.36)	0.15	(0.36)	0.12	(0.32)	0.12	(0.32)
Claim TPL	0.80	(0.40)	0.56	(0.50)	0.76	(0.43)	0.55	(0.50)
Annual TPL days	4.64	(4.86)	3.29	(4.98)	5.24	(6.62)	3.18	(5.01)
Non-missing wage	1.00	(0.00)	0.55	(0.50)	0.81	(0.39)	0.49	(0.50)
Monthly wage at main workplace (SEK)	28767	(2580)	38800	(17473)	26660	(2483)	36757	(14591)
Annual earnings (SEK)	318828	(61224)	448358	(250038)	258458	(87196)	410464	(231236)
Hours worked per month at main workplace	147.02	(27.34)	150.74	(30.22)	139.76	(31.51)	146.70	(31.83)
Household claims TPL	0.87	(0.33)			0.83	(0.37)		
Difference wages main workplace	-9910	(17278)			-9678	(14092)		
Annual earnings gap (SEK)	-129529	(258372)			-152006	(246095)		
TPL gap (days)	1.35	(6.04)			2.06	(7.24)		
Female spouse has higher earnings	0.26	(0.44)			0.21	(0.41)		
Observations	2042		2042		11644		11644	

Table E.2: Summary statistics in year before female major pay rise

	Treated		Partner		Control		Partner	
	Mean	sd	Mean	sd	Mean	sd	Mean	sd
Female	1.00	(0.00)	0.00	(0.00)	1.00	(0.00)	0.00	(0.00)
Married	0.70	(0.46)	0.70	(0.46)	0.68	(0.47)	0.68	(0.47)
Age	37.99	(4.55)	40.48	(5.25)	37.06	(4.66)	39.64	(5.40)
Compulsory	0.04	(0.18)	0.07	(0.26)	0.04	(0.21)	0.08	(0.27)
Upper secondary	0.33	(0.47)	0.44	(0.50)	0.32	(0.47)	0.46	(0.50)
Post-secondary or higher	0.64	(0.48)	0.48	(0.50)	0.63	(0.48)	0.45	(0.50)
Education missing	0.00	(0.03)	0.00	(0.05)	0.00	(0.03)	0.00	(0.05)
Youngest child 0–3	0.27	(0.44)	0.27	(0.44)	0.35	(0.48)	0.35	(0.48)
Youngest child 4–7	0.60	(0.49)	0.60	(0.49)	0.54	(0.50)	0.54	(0.50)
Youngest child 8–11	0.13	(0.34)	0.13	(0.34)	0.10	(0.30)	0.10	(0.30)
Claim TPL	0.66	(0.47)	0.50	(0.50)	0.63	(0.48)	0.48	(0.50)
Annual TPL days	4.92	(7.08)	2.99	(5.13)	4.78	(6.87)	2.83	(4.97)
Non-missing wage	1.00	(0.00)	0.47	(0.50)	0.66	(0.47)	0.45	(0.50)
Monthly wage at main workplace (SEK)	29018	(10787)	38131	(17841)	27756	(8077)	35332	(16503)
Annual earnings (SEK)	305083	(171820)	409017	(282809)	240937	(160325)	370948	(288370)
Hours worked per month at main workplace	126.44	(44.54)	147.15	(35.86)	131.93	(38.35)	148.44	(36.72)
Household claims TPL	0.77	(0.42)			0.74	(0.44)		
Difference wages main workplace	-8167	(16664)			-7196	(15111)		
Annual earnings gap (SEK)	-103934	(286847)			-130011	(294770)		
TPL gap (days)	1.93	(7.65)			1.95	(7.34)		
Female spouse has higher earnings	0.30	(0.46)			0.24	(0.43)		
Observations	22451		22451		26309		26309	

Table E.3: Individual wage and hours responses in expanded sample

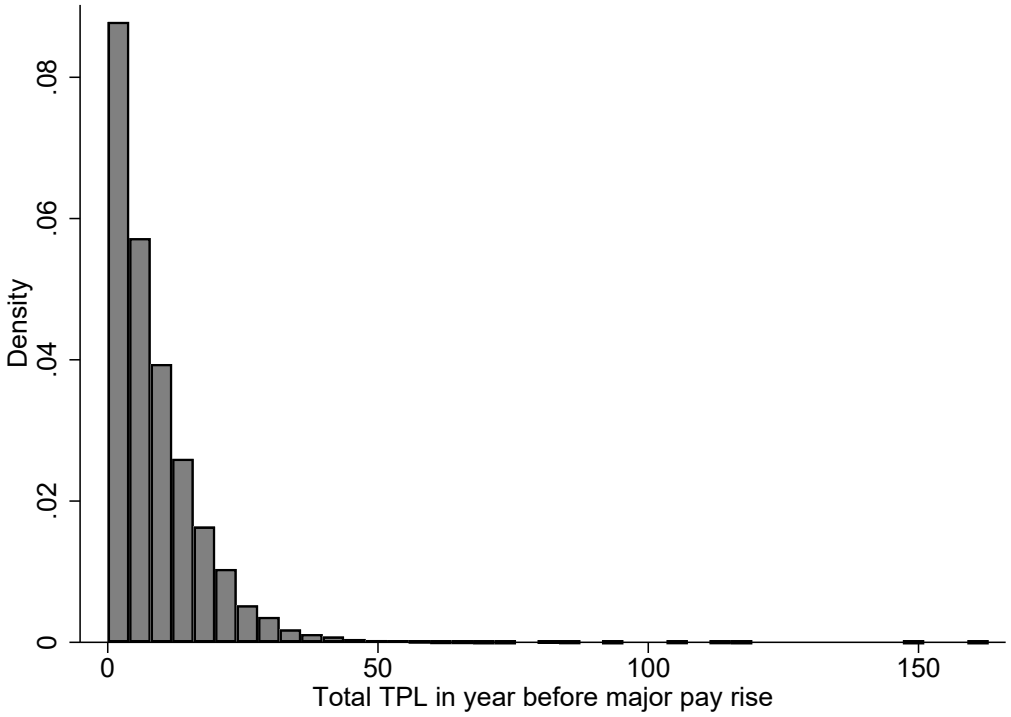
	(1)	(2)	(3)	(4)	(5)	(6)
	Annual earnings (SEK)		Monthly wages (SEK)		Hours worked	
	Promoted female	Male partner	Promoted female	Male partner	Promoted female	Male partner
Average effect	39520***	1763	6322***	308*	5.337***	-0.066
	(871)	(1437)	(102)	(161)	(0.592)	(0.510)
N	520,583	520,583	180,096	180,096	148,180	148,180
Control mean	250983	383764	28797	36099	134.05	148.14

Note: This table replicates the analysis in Table 3 for the expanded sample.

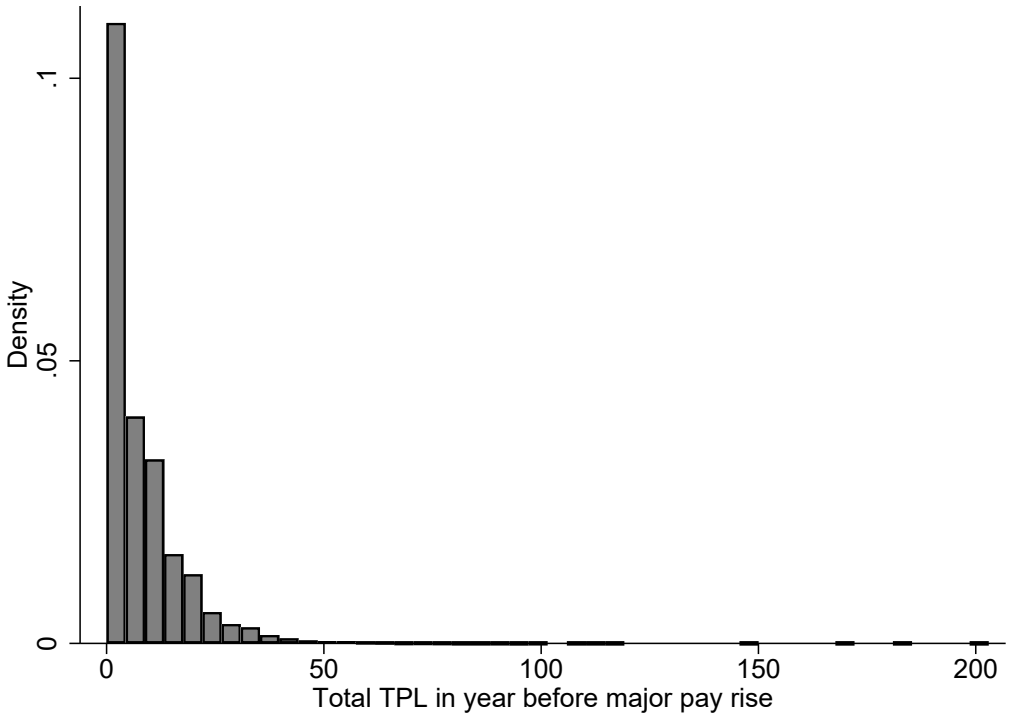
Table E.4: Effect of major pay increase on TPL days in expanded sample - by spousal WFH capacity

	(1)	(2)
	Female TPL	Male TPL
Panel A: No control		
WFH	-0.978***	-0.320***
	(0.117)	(0.082)
No WFH	0.305*	0.092
	(0.163)	(0.121)
N	147,875	147,875
P-value equal coef.	0	.0047
Control mean PossWFH=1	5.19	2.62
Control mean PossWFH=0	4.71	3.81
Panel B: Progressive control		
WFH	-0.974***	-0.320***
	(0.119)	(0.084)
No WFH	0.284*	0.126
	(0.167)	(0.125)
N	142,072	142,072
P-value equal coef.	0	.0029
Control mean PossWFH=1	5.19	2.62
Control mean PossWFH=0	4.71	3.81

Note: This table includes results on the heterogeneity in couples' TPL responses based on the partner's WFH capabilities for all female pay increases in the expanded sample. The estimates are based on equation (4), where H_h is a dummy if the spouse can work from home. In panel B, we additionally control for the couple being progressive. WFH (No WFH) shows the linear combination of $\theta_e + \omega_e$ (θ_e) in the post-period. Standard errors are clustered by couple. P-value equal coef. shows the p-value of an F-test on whether the average (post-pay increase) effects are equal across couple types.



(a) Teacher sample



(b) Expanded sample

Figure E.1: Distribution of household TPL days

Note: The figure plots the distribution of household TPL days in the year before the major pay rise in the teacher sample (panel a) and the expanded sample (panel b).

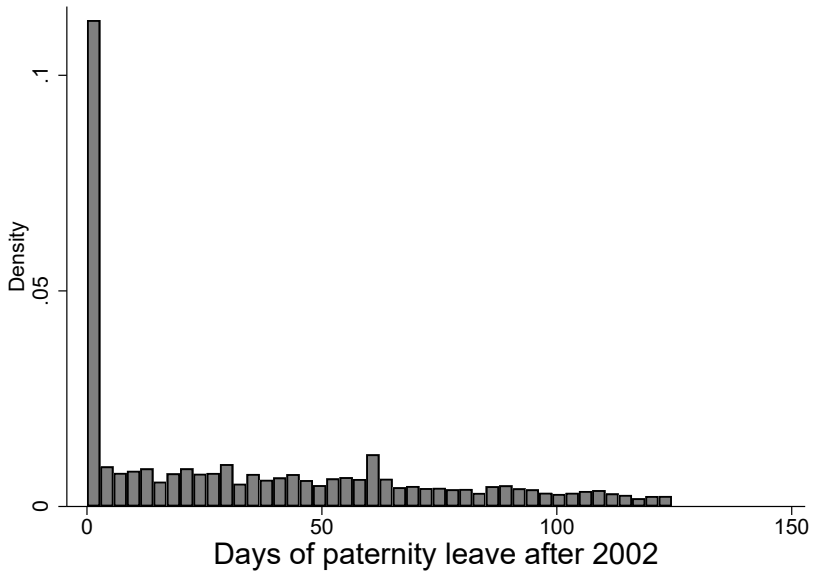


Figure E.2: Distribution of fathers' number of regular parental leave days within two years of birth