Reducing the gender pay gap: can we let firms take action?*

Caroline COLY †

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Abstract

State interventions to decrease the gender wage gap are often criticized for creating one-approach-for-all which may be inappropriate for the specific difficulties faced by each sector and firm. In this paper, I study a unique policy where French firms were mandated by law to negotiate agreements on gender equality with union representatives. I estimate the causal effect of the signature of such agreements on the wage gap and other measures of gender inequalities. Using a unique combination of administrative datasets, I exploit the staggered signature of agreements over the 2010-2013 period and find that the law had an effect on the signature of those agreements but did not alter the gender wage gap or many other outcomes reflecting gender inequalities. The absence of gender-related changes can plausibly be explained by the lack of obligation of result in the law and by the weak oversight of agreements' content.

JEL codes: J16, J31, J71, K38

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†Department of Economics and Institut d'Economia de Barcelona (IEB), University of Barcelona. Contact: caroline.coly@ub.edu. Address: Carrer de John Maynard Keynes, 1, 11, 08034 Barcelona.

1 Introduction

All around the world, the fact that women's earnings are lower than those of men remains the most common and persistent characteristic of labor markets (Goldin, 2014).

To fight this so-called gender pay gap, many countries have passed laws to forbid discrimination and decrease gender inequalities. In particular, initiatives that promote pay transparency have been increasingly adopted in many countries. Those initiatives have been well studied (see Frey (2021) for an overview) but led to conflicting results.

Some studies also highlight the drawbacks of pay transparency. It can lead workers to experience feelings of injustice and envy, reduce job satisfaction, well-being and happiness, and increase absenteeism (Luttmer, 2005; Card et al., 2012; Godechot and Senik, 2015; Breza et al., 2018; Perez-Truglia, 2020); which can then decrease their productivity (Obloj and Zenger, 2017). Several studies also find that pay transparency leads to a compression in salaries (Mas, 2017; Baker et al., 2023). By making more visible wage inequalities, pay transparency, although often decreasing the gender wage gap, can thus have negative effects on average salaries and other outcomes. This raises the question of whether any alternative exists to those pay transparency measures that could also reduce the gender pay gap while avoiding its negative effects.

In this paper, I look at the effect of a French law passed in November 2010 that mandated firms to negotiate agreements on gender equality. As part of the process, employers needed to first establish a diagnosis of gender inequalities - including the gender wage gap - within their firms and then negotiate with union delegates on measures to reduce them. The key question I address in this paper is whether mandating firms to negotiate gender equality agreements impacts firms' gender wage gap.

To identify the effect of negotiating gender quality agreements, I exploit the fact that the signature of agreements takes place on different dates for firms between 2010 and 2013. I show that those dates of signature are quasi-random. Using a difference-in-differences strategy in a staggered adoption design, I compare the evolution of the gender wage gap among early (*treated*) and late (*control*) signatories firms before and after the signature of an agreement. Under the identifying assumption that the treatment timing is uncorrelated with the evolution of outcomes over time, this design allows me to causally identify the effect of negotiating a gender equality agreement on the gender wage gap. I also look at the effects on a wide range of outcomes reflecting other gender inequalities such as gender differences in promotions, women's access to top earnings positions, or the likelihood of moving from a fixed-term to an open-ended contract.

To address issues raised by the latest developments in the econometric literature on staggered adoption difference-in-difference designs (Borusyak et al., 2021; Goodman-Bacon, 2021; De Chaisemartin and d'Haultfoeuille, 2020), I also implement the De Chaisemartin and d'Haultfoeuille (2020) estimator, using as control group firms that signed at a later date. In addition, I also implement a stacked regression estimator (Cengiz et al., 2019).

For this analysis, I build on a unique administrative dataset created by combining two databases. The first one, the D@ccord database, registers information on firms that signed agreements on professional equality between women and men over the 2010-2013 period and the year in which they were signed. From 2008 to 2013, I also use the DADS database, a comprehensive administrative dataset that contains detailed information on workers, providing both job and demographic variables, and firms characteristics. These two databases can be matched using a unique firm identifier.

The French setting differs from previous policies studied by the literature. Although pay transparency often refers to public disclosure of wages or to an employee's right to request information on pay levels, it also includes an employer's duty to conduct audits on the gender wage gap. In my setting, firms must first conduct internal audits on gender inequalities, including the gender wage gap, to diagnose potential inequalities before negotiating an agreement. This alternative to more classic pay transparency measures could hence potentially avoid some of the negative effects they are associated with. This public policy also has the advantage of responding to the criticism that state intervention is often creating a one-approach-for-all, which is inappropriate for the specific difficulties faced by each sector and firm. Hence, one could think that each firm is in the best position to identify what the issues are, how it can solve them, and act accordingly. This approach also has the benefits of allowing firms to address both the unexplained and explained parts of the gender pay gap, for example by making sure there is no discrimination at hiring and by pushing women to apply to higher positions within the firm. The French policy might therefore provide a powerful alternative to classic pay transparency legislation.

However, there could be several drawbacks to letting firms handle gender inequalities. First, there could be some conflict of interest and they could forego tackling issues they potentially benefit from. Second, given the various channels through which gender wage gaps can arise, there is a risk that firms might not have the knowledge and resources to properly identify and tackle them.

I find that the law had an impact on the signature of gender equality agreements as there is a large increase in the share of firms above fifty employees signatories of such agreements. Yet, my results show that signing an agreement on gender equality has no effect on the average wage gap, even adjusted by socioeconomic status. Effects on other measures of inequalities between men and women such as wage promotion or the likelihood of moving from a fixed-term contract to a permanent contract are also null. Those results are confirmed when using the De Chaisemartin and d'Haultfoeuille (2020) estimator and the stacked regression approach. The lack of effect of those agreements can be explained by the setting. The law made the *signature* of an agreement on gender equality mandatory but did not mandate any obligation of deliverable *results*. In addition, the policy enforcement was rather superficial: when controlling a firm, labor inspectors must verify that an agreement has been signed. However, they do not assess the *content* of the agreement. Firms could hence negotiate agreements void of any binding actions for them and not face any consequences. I hence show that decentralizing the level of action at the firm level without proper monitoring does not lead to a decrease in gender inequalities within firms.

This paper contributes to a vast literature on the effects of policies aiming at reducing the gender wage gap. First, this paper adds to the growing literature on the effects of pay transparency laws on the gender wage gap. Pay transparency can refer to a wide range of different measures. In Canada, Baker et al. (2023) examine the impact of laws that enabled public access to salaries of public sector employees above a certain threshold. Focusing on university faculty salaries, they find that those laws significantly reduced the gender wage gap by 30%. Similarly, exploiting staggered shocks to public access to wage information on public universities faculty in the United States, Obloj and Zenger (2022) find evidence that pay transparency is associated with significant increases in the equity and equality of pay. Denmark introduced a different measure for pay transparency: firms with more than 35 employees had to report salary data broken down by gender for employee groups large enough for individuals' anonymity to be respected. In that case, the data was not available publicly but accessible only by employees. This law led to a 13% decrease in the gender wage gap relative to the pre-legislation mean (Bennedsen et al., 2022). The UK also mandated firms to report gender pay gap statistics but the law differed in two important ways. First, it focused on large firms (250 employees or more), which left half of all UK employees uncovered. Second, those gender statistics had to be made publicly available. This obligation also led to a significant reduction in the gender wage gap (Blundell, 2021; Duchini et al., 2020; Gamage et al., 2020). Austria implemented another pay transparency reform that required firms above certain thresholds to report annual gross incomes by gender and occupation. Similarly to Denmark, those reports were made available only to employees. However, unlike the other studies, Gulyas et al. (2023) find no effect on the gender wage gap. Results by Böheim and Gust (2021) confirm this limited impact of pay transparency in Austria and show that the reform only had an impact on the wage gap of newly hired. The context I study is closer to the ones of Gulyas et al. (2023) and Bennedsen et al. (2022) as employers had to conduct internal audits and measure the gender wage gap. The French setting goes further as employers are responsible for identifying gender inequalities that are not limited to the pay gap but also in other areas. Another important difference is that the French one does not just state the obligation of providing those gender statistics but obliges employers to negotiate precise measures with union delegates and lay them in a signed agreement.

Second, the paper builds upon studies analyzing policies that target employers to alter gender inequalities without the use of pay transparency. In the United States, Kurtulus (2012) finds that the share of women in high-paying skilled occupations particularly grew at firms holding federal contracts, which were subject to affirmative action obligations. Baker and Fortin (2004) analyze the effect of a pay equity act in Ontario, which legislated a proactive application of comparable worth to all public and private employers of 10 or more employees. Unlike Kurtulus (2012), they find no effect on the aggregate wages in female jobs nor on the gender wage gap. One advantage of my approach is that I can identify exactly which firms signed an agreement and in which year, and hence measure precisely the causal effect on the gender wage gap of negotiating a gender equality agreement. In addition, these policies differ as they do not include a negotiation process with unions.

Lastly, this paper contributes to a vast literature looking at the role of unions in decreasing inequalities (Freeman, 1980, 1982; Card, 1992, 1996; DiNardo and Lemieux, 1997; Card, 2001; Callaway and Collins, 2018; Collins and Niemesh, 2019; Farber et al., 2021). Using establishment-level data in the US, Freeman (1982) finds that dispersion of within-establishment wages is significantly smaller in unionized than in non-unionized establishments and attributes it to union wage policies. Other studies study the link between union density and income inequality. DiNardo and Lemieux (1997) show that de-unionization can explain a sizeable share of the rise in wage inequality in the US from 1979 to 1988. Card (2001) estimates that the decreasing unionization rate of men in the US can explain about 15 to 20% of the rise of male wage inequality between the 70s and the 90s. He also shows that unions significantly contributed to slowing the growth in wage inequality in the public sector. More recently, Farber et al. (2021) find that unions reduce inequality and can explain a significant share of the huge fall in inequality between the mid-1930s and late 1940s. Consequently, they wonder whether unions could be an important part of a feasible policy package to lower inequality. In this paper, I show that even if unions are involved in the negotiation process, signing agreements do not lead to a decrease in gender inequalities within establishments.

The remainder of the paper is organized as follows. Section 2 explains the context and setting of the law of November 2010. In section 3, I describe my data and some indicators of gender inequalities in France. Sections 4 and 5 present the empirical strategy and results. I verify the robustness of my results in section 6 and section 7 discusses my results and concludes.

2 Context

In 2006, the European Union directive (2006/54/EC) on equal opportunities and equal treatment of women and men in employment and occupation marked a new step towards the fight against gender discrimination by requiring, among others, the implementation of the prohibition of direct and indirect sex discrimination.

Following that, the November 09th, 2010 bill was passed in France. This law implemented financial sanctions for companies with 50 or more employees who would have not signed an agreement or a unilateral decision in favor of equality between men and women by January 1st, 2012. The law hence covered all private-sector firms with 50 employees or more and the financial penalty could be equal to up to 1% of the net wage bill.

The process leading to the signature of an agreement was the following. First, employers had to establish a diagnosis of gender inequalities in nine action areas: (1) hiring, (2) training, (3) promotion, (4) vocational qualification, (5) occupational classification, (6) working conditions, (7) health and safety at work, (8) wages, and (9) balance between work and family life. For each of these action areas, firms had to develop their own indicators, broken down by gender and professional category.

Second, depending on the size of the firm, employees had to develop an action strategy in several action areas in order to reduce the inequalities that had been identified. For firms between 50 to 299 employees, they had to select at least two action areas. For firms with more than 300 employees, at least three action areas had to be selected. Employers were free to choose any of these action areas to focus on except for one which was mandatory to act on: the wages. In 2012, a new decree increased the number of mandatory action areas from two to three for firms of 50 to 299 employees and from three to four for firms above 300 employees. The action area on wages is also imposed as a mandatory action area among the three or four that have to be chosen.

Thirdly, the employer representatives will use this action strategy to negotiate an agreement on professional equality with union delegates. This agreement should include actions to put in place to decrease the identified inequalities between men and women as well as an estimation of the costs of these actions. If no agreement can be found with the union delegates, it is the responsibility of the employer to set up a unilateral action plan. A synthesis of the measures that have been decided then must be brought to the attention of the employees.

Importantly, those agreements must be negotiated by two sides: the employer's side and the employees' side. In a company, the employer's side will be usually represented by authorized persons who have been given the power to negotiate in the company. This can for example be the Director of Human Resources (HR) or the Director of Social Relations. On the employees' side though, the French law is very strict and only union delegates are authorized to negotiate collective agreements with the employer[1]. If the employer oversteps this union monopoly, this may be considered an obstruction offense. Those union delegates are chosen by trade unions that are representative at the firm level[2]. Those agreements are hence negotiated

[1] There are only a few exceptions to this, especially if no union is representative in the company

[2] A trade union is considered as representative if it has obtained at least 10% of the votes in the first round of the last elections of the members of the works council or of the single staff delegation or, failing that, of the staff representatives.

by workers experienced in negotiating with the employer and that benefit from some protection against the employer [3].

3 Data and descriptive statistics

3.1 Data sources and sample

My analysis relies on two main data sources. First, I use the Adep (*Accords et Décision Unilatérale sur l'Egalité Professionnelle*) database[4]. Constructed by the French Ministry of Labor through a meticulous cleaning process of the D@ccord database, this dataset identifies firms that signed gender equality agreements during the 2010-2013 period. A firm is considered covered by an agreement on professional equality in a given year if it was subject to an agreement or a unilateral action plan for at least one trimester of that year. Throughout this paper, unless explicitly stated otherwise, the term "agreement" encompasses both negotiated agreements and unilateral action plans. For simplicity, I refer to the Adep dataset as D@ccord 2010-2013 in the subsequent sections of this paper.

Second, I use a linked employee-employer administrative dataset, the **Déclaration annuelle des données sociales** (DADS), spanning the years 2008 to 2013. Based on an annual form comparable to the W-2 form in the United States, the DADS is an obligatory annual submission for French firms. By law, firms are mandated comprehensive reporting for all employees subject to payroll taxes. Failing to fill it out or providing incorrect or missing answers are punished with fines. As a result, the data is comprehensive and of exceptional quality with low measurement error compared to survey data. This database provides detailed information about employees, including their gross and net wages, working hours, age, gender, and socio-professional category. It also includes firm-specific information such as sector and the unique firm identifier, facilitating the linkage of all employees affiliated with the same firm.

By summing the full-time equivalent of all employees in a given firm, I can identify firms above the fifty employees threshold [5], as it is the one defined by the law. However, that measure of the threshold is a bit fuzzy for two reasons. First, apart from a few exceptions, temporary workers are supposed to be taken into account when calculating the number of employees. Nevertheless, they are registered in the DADS under the siren of their temporary employment agency and not under the one of the firm in which they were affected. Hence, I am underestimating the number of employees for firms who use temporary workers. Second, the fifty employees threshold implies other additional obligations that might incite firms

[3] At the end of their mandate, union delegates are protected for one year against dismissal if they have held the position of union delegate for at least one year.

[4] This dataset can be accessed by demanding access to the DARES, the research and statistics department of the French Ministry of Labor.

[5] In France, the 50 employees threshold is based on the number of employees in full-time equivalence.

with slightly more than fifty employees to declare themselves below the threshold. Empirical studies tend to support that hypothesis: looking at the distribution of firms around several thresholds, including the fifty employees one, Ceci-Renaud and Chevalier (2010) found that there was indeed a discontinuity at those points in fiscal data but small to none in the DADS. As the fiscal data is the legal frame of reference, it is likely that some of the firms I observe at or just above the threshold are declared as under the threshold in fiscal data. Using information on wages, hours worked and the socio-professional category, I construct several measures of gender inequalities [6]: the gender wage gap within a firm, the wage promotion gap, the gap in promotion from fixed-term to permanent contract, etc. I also look at the share of women among the new hires in permanent contracts. Several studies have indeed proved the existence of discrimination against women during the hiring process (Kübler et al., 2018; Neumark et al., 1996; Goldin and Rouse, 2000; Petit, 2007). For example, Goldin and Rouse (2000) studied the impact of the adoption of blind auditions by orchestras where a screen hid the identity of the candidate. They found that the presence of a screen dramatically increased the probability that a woman would pass the preliminary rounds and be hired at the final round, explaining 25% of the increase in the percentage of women in the top 5 symphony orchestras in the United States. Without having to install a screen, firms in their agreements could take measures to promote women's hiring or at least ensure that women are less discriminated against in the hiring process. For example, a firm could decide that when having to choose between two candidates of equal competence, the person from the least represented gender is hired. As I only observe the newly hired workers but do not have data on candidates for a position, I use as a proxy the percentage of women among the new hires in permanent contracts. I focus on permanent contracts since fixed-term contracts are more precarious and can be used to fill the temporary absence of a worker.

For my analysis, I include only the non-annexed posts of workers in the DADS[7]. I also drop all employees who work as apprentices and workers having a subsidized job (*emploi aidé*). Indeed, since the wage paid by their employer is much lower thanks to the subsidy, they lower the mean wage for their gender, which might bias the mean pay gap computed for their firm. Additionally, firms with no employees are excluded. Lastly, I eliminate all observations related to workers in the agricultural sector from the DADS, as the D@ccord database does not encompass firms in this sector.

I am then able to match information from the DADS and the D@ccord databases thanks to the SIREN, a unique firm identifier. The information on the date of treatment makes it possible to exploit the gradual implementation of agreements on professional equality instead of relying only on the 2012 deadline.

[6] The construction of these variables is described in Appendix A

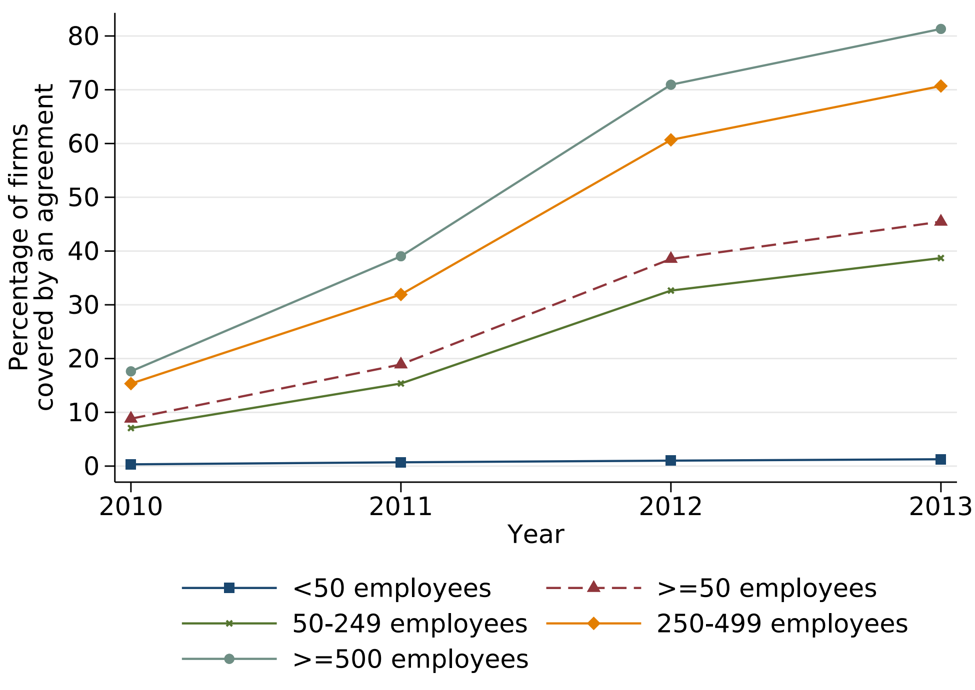
[7] A post is considered as non-annexed if the volume of work and the corresponding level of pay are "sufficient". I hence drop all employees' secondary jobs

3.2 Description of the sample

My sample contains 905,625 firms, out of which 25,193 signed an agreement over the 2010-2013 period, which represents about 3% of the firms in the sample.

One potential concern could be the lack of impact of the law on the signature of agreements by firms with more than fifty employees. To address this issue, figure 1 presents the evolution of the percentage of firms covered by an agreement over the 2010-2013 period. There is a spike in the percentage of firms of fifty employees or more covered by an agreement in 2012 going from less than 10% in 2010 to about 40% in 2012 and reaching close to 50% in 2013. This is consistent with the fact that firms would have to pay a fine if an agreement or a unilateral plan was not put in place by January 1st, 2012. The coverage of firms with strictly less than fifty employees is much lower and there is no such spike in 2012 as the one observed for firms of fifty employees or more. This suggests that the rapid growth of firms of fifty or more employees covered by an agreement on professional equality does not merely reflect a trend in favour of professional equality but is rather a reaction to the requirements of the law.

Figure 1: Evolution of the percentage of firms covered by agreements



Source: DADS 2008-2013 and D@ccord 2010-2013.

Note: The graph represents the cumulative percentage of firms covered by an agreement on gender equality depending on the size of the firm computed in full-time equivalence.

Table B.1 looks in more detail at the proportion of signatories depending on the size of the firm. As anticipated, larger firms exhibit a higher proportion of signatories. Among firms with 500 employees or more in 2010, the take-up was close to 80% in 2013. In contrast, less than half of firms with 50 to 249 employees have signed agreements by 2013. Two factors can explain this. First, largest firms are more likely to be controlled by labor inspectors, providing stronger incentives for timely agreement signing. The perceived threat of substantial fines likely spurred firms to engage in gender equality negotiations, consistent with Milner et al. (2019), who observed that a significant number of the 186 equality agreements they studied explicitly referenced the fines introduced by the 2010 law. Second, large firms tend to have larger human resources departments, hence more means to know about the obligations of the law and to implement them (Giordano and Santoro, 2019). On the contrary, smaller firms around the 50 employees threshold might not have even been aware of this obligation or might have lacked the resources to implement this negotiation (Pochic et al., 2019).

A potential concern is whether firms above the fifty employees threshold might manipulate their employee count to evade the obligation. However, this manipulation is improbable, as many other bargaining obligations in France commence for firms at the 50-employee threshold. Negotiating gender equality is just one among several negotiation topics for firms around this threshold. To verify this, figure B.1 plots the distribution of firms between 40 and 60 employees in 2010 and 2012, revealing no discernible discontinuity at the 50-employee threshold. This suggests that no manipulation of the number of employees occurred among companies near that threshold.

Table 1: Mean wage gap by size of firm

	(1) Never signatories	(2) Signatories	(1)-(2) Difference
01-09 employees	5.5 %	15.2%	-9.7***
10-49 employees	7.4%	14.6%	-7.2***
50-249 employees	11.8%	12.6%	-0.8***
250-499 employees	12.4%	12.4%	0.0
500+ employees	12.6%	13.4%	-0.8

Source: DADS 2008-2013 and D@ccord 2010-2013.
Note: $* \, p < 0.05$, $** \, p < 0.01$, $*** \, p < 0.001$. This table presents the mean wage gap by signatory status and reports the difference between the mean of each group. It also reports whether the difference is significant with a two-sample t-test. The gender wage gap is measured in 2009. "Never signatories" refers to firms that never signed an agreement between 2010 and 2013 while "signatories" refers to firms that signed an agreement between 2010 and 2013.

When examining gender pay gaps, a notable pattern emerges in my sample. Small firms that have signed gender equality agreements exhibit significantly larger gender pay gaps compared to their non-signatory counterparts. In particular, for firms with less than 10 employees, the average wage gap is 10 percentage points higher among signatories, as illustrated in the third column of table 1. Similarly, for firms with 10 to 49 employees, the difference amounts to 7 percentage points. This suggests a self-selection process among small firms, wherein those deciding to implement gender equality agreements tend to have larger initial pay gaps. The trend persists for firms with 50 to 249 employees, albeit with a smaller but still significant 1 percentage point difference. In contrast, for firms surpassing the 250 employees threshold, the disparity in mean wage gaps between signatory and non-signatory firms is minimal and statistically insignificant.

Table 2: Descriptive statistics in 2009 for signatories firms by year of signature

	2010 signatories	2011 signatories	2012 signatories	2013 signatories
Number of firms	4,913	5,496	7,474	4,480
Mean age of workers	39.3	39.3	39.2	38.8
% of women	38.6	42.0	42.5	45.0
% of executives	16.6	18.9	18.0	17.0
% of female executives	28.2	30.5	33.4	34.0
% of full-time workers	85.4	85.4	83.1	82.3
% of women among part-time workers	67.4	72.1	68.7	71.9
% of fixed-term contracts	10.2	11.4	12.2	12.1
% of women among fixed-term contracts	59.8	61.9	59.8	62.7
Mean hourly wage	19.6	20.5	19.9	19.4
Industry	25.9%	18.7%	22.3%	17.6 %
Construction	7.65 %	9.6 %	6.1%	5.4%
Tertiary excl. OQ	62.8%	64.7%	59.2%	63.2 %
Tertiary OQ	3.8%	7.0 %	12.4%	13.9 %
01-09 employees	13.1%	16.1 %	6.9 %	11.9%
10-49 employees	33.6%	33.3%	22.1%	26.8%
50-249 employees	35.0%	34.0%	51.6%	44.8%
250-499 employees	9.69%	8.53%	10.84%	7.19%
500+ employees	9.6%	8.3%	10.3%	8.6%

Source: DADS 2008-2013 and D@ccord 2010-2013.
Note: This table presents descriptive statistics for the sample of firms that signed an agreement between 2010 and 2013 by year of signature. Descriptive statistics are measured in 2009. Tertiary OQ refers to public administration, education, human health and social work activities.

Table 2 presents descriptive statistics for all signatories firms broken down by their year of first signature. The mean hourly wage is similar across those firms. However, signatories of the 2010 cohort tend to have a slightly lower percentage of women and female executives, respectively about 39% and 28% whereas for later cohorts, those proportions are around 43% and 33%.

The column on the 2012 cohort also shows a sizeable jump in the proportion of firms of 50 to 249 employees compared to the other size categories, which is consistent with the law taking effect that year. Some differences emerges by sector as well. Firms in the tertiary OQ sector are in majority late adopters, that sector representing only 3.8% of signatories in 2010 versus close to 14% in 2013.

For the analysis as described in the next section, I will restrict my sample to firms above 50 employees that ever signed an agreement. Table B.2 presents the same descriptive statistics as table 2 but for this sub-sample of firms. On average, firms are quite similar across waves in terms of mean age of workers, mean hourly wage, share of executives in the workforce. 2010 signatories have a slightly lower share of females compared to later signatories (36% versus 40-43%). Interestingly, firms in the industry sector are over-represented among signatories across all waves of signature. Whereas the industry represents 10% of firms in 2009[8], industry firms represent between 24 and 35% of signatories depending on the waves. On the opposite, the tertiary sector is particularly under-represented. It represented 78% of firms in 2009[9] but represent only 6 to 15% of signatories between 2010 and 2013. This can be explained by size of firms in these industries no?

4 Empirical strategy

4.1 Main specifications

To quantify the effects of an agreement on professional equality [10] on different measures of gender inequalities, I exploit their staggered date of signature and estimate the following static specification:

$$Y_{it} = \alpha + \beta Agreement_{it} + \mu_i + \gamma_t + \epsilon_{it} \tag{1}$$

where Y_{it} is one of the outcomes of interest, μ_i are firms fixed-effects, γ_t are year fixed effects and $Agreement_{it}$ is a dummy taking value one for all years after the year the firm signed an agreement.

Our key identifying assumption here is that, conditional on firm and year fixed effects, the year of signature is orthogonal to the error term. The coefficient of interest here is thus β.

Then, to verify the pre-trends and look at the dynamic effects, I estimate the following dynamic specifi-

[8] Author's calculations based on the DADS.

[9] Author's calculations based on the DADS

[10] A regression discontinuity design using the fifty employees threshold could have been an alternative strategy but there is no discontinuity in the probability of assignment at this threshold (see figure B.2 in appendix).

cation:

$$Y_{it} = \alpha + \beta_{-4}D_{-4} + ... + \beta_0 D_0 + ... + \beta_6 D_{+2} + \mu_i + \gamma_t + \epsilon_{it}, \tag{2}$$

where Y_{it} is the outcome of interest, μ_i are firms fixed-effects, γ_t are year fixed effects, D_0 is a dummy for the year of signature, D_{-s} is a dummy for s years before the signature and D_{+s} is a dummy for s years after the signature. The coefficients of interest are the β_k where k>0. As there are no never-treated firms in the sample, two relative time indicators need to be omitted to avoid multicollinearity (Borusyak et al., 2021). The reference categories are hence here the lags -1 and -4. The standard errors are clustered at the firm level and the model includes firm and year fixed effects. Adding firm fixed effects addresses the concern that a firm's unobserved characteristics may be correlated to the timing of adoption of an agreement: early-adopting firms might systematically differ from late-adopting firms.
I will then look at the effect of signing an agreement on the wage gap, on the wage gap among top earners, and on other indicators of gender inequalities.

However, the literature has recently highlighted several issues of two-way fixed effects estimators with staggered adoption (Sun and Abraham, 2020; Borusyak et al., 2021; Goodman-Bacon, 2021; De Chaisemartin and d'Haultfoeuille, 2020). Staggered difference-in-difference designs may not provide valid estimates of the average treatment effect (ATE) as the β from equation 1 is a weighted sum of all the possible 2x2 comparisons in my sample, with weights that may be negative. In a two-way fixed effects regression, units whose treatment status doesn't change over time serve as the comparison group for units whose treatment status does change over time. In a staggered design with multiple time periods and variation in treatment timing, already-treated units serve also as a comparison group for newly treated units. This can lead to a substantial bias in the presence of heterogeneous treatment effects across groups.

I hence also apply the De Chaisemartin and d'Haultfoeuille (2020) estimation procedure to estimate the causal effect of signing an agreement. The key advantage of their method is that their estimator is robust to treatment effect heterogeneity across groups and time periods:

$$\beta^S = E\left[\frac{1}{N_S} \sum_{(it)t\geq 2, D_i \neq D_{t-1}} [Y_{i,t}(1) - Y_{i,t}(0)]\right] \tag{3}$$

with firm i and calendar year t. $N_S = \sum_{t\geq 2, D_t \neq D_{t-1}} N_t$ with N_t the number of firms at period t. D_t denotes the average treatment at period t, while $Y_{i,t}(0)$ and $Y_{i,t}(1)$ respectively denote the average potential outcomes without and with treatment. β^S is the average treatment effect at the time when the treatment is received across all treated firms.

4.2 Identifying assumptions

One main concern regarding my estimation strategy is the selection into treatment. For instance, it is likely that firms above the fifty employees threshold that signed an agreement have different unobservable characteristics than firms that did not sign an agreement. To address this concern, I will estimate equations (1) to (3) on the sample of firms of fifty employees or more that ever signed an agreement.

The key identification assumption underlying my identification strategy is that the date of signature of an agreement, although not random, was uncorrelated with pre-existing differences in wage gap trends once fixed effects are controlled for. To verify that, I test for the presence of pre-trends by plotting the $\hat{\beta}_k$ of equation (2) where k<0 and examine whether they are equal to zero. I also verify whether any observable characteristics of firms consistently predict the date of signature of agreements. To do so, I regress indicators for the four cohorts of signature (2010, 2011, 2012, 2013) on a set of characteristics measured in 2009. Results presented in table 3 show that all characteristics do not significantly predict the date of signature across the different columns.

Table 3: Firm characteristics predicting treatment timing

	(1) Signing in 2010	(2) Signing in 2011	(3) Signing in 2012	(4) Signing in 2013
Female CEO	0.0326*	0.000211	-0.0395*	0.00667
	(2.57)	(0.02)	(-2.47)	(0.53)
25-50% women	-0.0222	0.00855	0.0401**	-0.0265*
	(-1.84)	(0.72)	(2.83)	(-2.43)
50-75% women	-0.0604***	0.0140	0.0417**	0.00471
	(-4.94)	(1.11)	(2.74)	(0.38)
75-100% women	-0.114***	-0.00251	0.0747**	0.0416*
	(-7.47)	(-0.14)	(3.28)	(2.24)
Industry	-0.0456*	0.0706**	-0.0299	0.00491
	(-2.21)	(3.17)	(-1.22)	(0.26)
Construction	-0.0304**	0.00508	-0.00203	0.0274**
	(-2.87)	(0.48)	(-0.16)	(2.78)
Tertiary OQ	-0.0607***	-0.0147	0.112***	-0.0365*
	(-3.83)	(-0.79)	(4.74)	(-2.00)
250-499 employees	0.0645***	0.0401**	-0.0286	-0.0760***
	(5.01)	(3.07)	(-1.90)	(-7.23)
500+ employees	0.0579***	0.0524***	-0.0318	-0.0785***
	(4.08)	(3.56)	(-1.91)	(-6.79)
N	8,406	8,406	8,406	8,406
R^2	0.020	0.004	0.013	0.012

Source: DADS 2008-2013 and D@ccord 2010-2013. Note: $* \, p < 0.05$, $** \, p < 0.01$, $*** \, p < 0.001$. t statistics in parentheses. This table presents results from 4 separate OLS regressions where the dependant variable are indicators for signing an agreement in 2010, 2011, 2012 or 2013. The explanatory variables are measured in 2009. The sample is restricted to firms above 50 employees between 2009 and 2013. The reference categories for gender of CEO, share of women, sector and size of the firm are respectively: having only male CEOs, having a share of women below 25%, the tertiary sector excluding OQ, and being a firm with 50 to 249 employees.

5 Results

5.1 Main results

The results are presented for a balanced sample of firms, present from 2008 until 2013, that had at least 50 employees over the whole period. Additionally, I verify the robustness of my results by examining whether the observed effects persist when considering firms with more than 50 employees in 2009, one year before the law was passed. Table 5 presents the results of the estimation of equation (1) on the effect of the signature of an agreement on the raw wage gap and the raw wage gap by socioeconomic category. The first column shows that the signature of agreements does not lead to any change in the gender wage gap. The coefficient on the *Agreement* dummy is a precisely estimated zero and not significant. Similarly, the agreements do not seem to lead to any change in the gender wage gaps by socioeconomic categories. All the estimates are precise and very close to zero, bringing further support for the lack of effects of the law and proving the null effects are not driven by a lack of power. My setting is however not immune to the negative weighting issue mentioned previously in section 4: depending on the outcomes, between 15 and 20% of the average treatment effects have negative weights. I hence turn to the De Chaisemartin and d'Haultfoeuille (2020) specification. As can be seen in the second part of table 5, the results remain virtually the same. There is no clear impact of the signature of an agreement on the gender wage gap or on any of the wage gaps by socioeconomic category. The results estimated are also insignificant and precise zeros, suggesting that a lack of power is not driving the null effects.

Figure 2 presents the estimated dynamic effects on the same outcomes using both the staggered difference-in-differences specification and the De Chaisemartin and d'Haultfoeuille (2020) estimation procedure. These figures plot the coefficients of equation 2 and 3 along with 95% confidence intervals for the raw wage gap and the gender wage gap by socio-economic status. The figures show there is no effect of signing an agreement on any of these outcomes. The coefficients β_k for k<0 are equal to zero, indicating an absence of pre-trends, which supports the identifying assumption that the date of signature of an agreement, although not random, was un-correlated with pre-existing differences in wage gap trends once fixed effects and controls are controlled for. Both estimations lead to similar results. There is no clear impact of the signature of an agreement on the wage gap for the wage gaps of executives, intermediate occupation workers, or blue-collar workers. For employees, there is a small decrease in the gender wage gap for years one and two after the signature of an agreement in the staggered difference-in-differences specification but this result is insignificant when turning to the De Chaisemartin and D'Haultfoeuille estimation.

Table 4: Effects on the wage gap and the wage gap by SES

	(1) Wage gap	(2) Executive	(3) Inter. Prof	(4) Employees	(5) Blue collars
Method: Staggered difference-in-differences					
Agreement	0.00006	-0.00059	-0.00057	-0.00094	0.001946
	(0.00089)	(0.00219)	(0.00172)	(0.00231)	(0.00195)
$Mean$	0.12951	0.16113	0.07597	0.04420	0.08402
N	55,545	42,450	46,030	40,045	28,415
R^2	0.90	0.67	0.63	0.53	0.55
Method: De Chaisemartin & D'Haultfoeuille					
Agreement	0.00021	-0.00162	-0.00145	-0.00326	0.00151
	(0.00140)	(0.00282)	(0.00245)	(0.00278)	(0.00282)
$Mean$	0.12928	0.15984	0.07519	0.05044	0.08426
N	43,935	33,123	36,042	31,594	22,120
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes

Source: DADS 2008-2013 and D@ccord 2010-2013. Note: $* \ p < 0.05$, $** \ p < 0.01$, $*** \ p < 0.001$. The dependent variable is the mean wage gap in (1) and the mean wage gap by SES in (2) to (5). Standard errors in parentheses are clustered at the firm level.

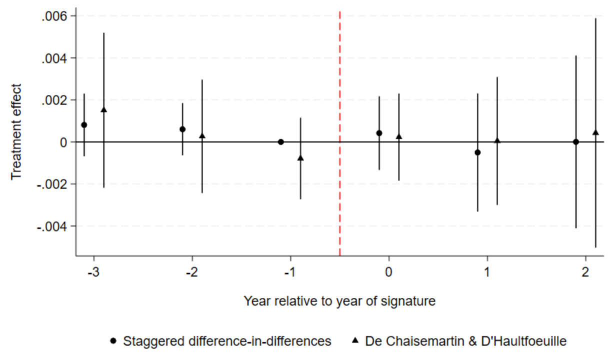
Previous studies on wage transparency measures have found that their effects on the wage gap were stemming from a slower salary growth of men rather than women (Baker et al., 2023; Bennedsen et al., 2022; Duchini et al., 2020). Table C.1 in the appendix reports the static estimates when looking at the effect of agreements on the wages of women and men separately. The coefficients are also very close to zero and lack statistical significance, suggesting that the implementation agreements did not yield any discernible increase in average wages for women, nor did it notably influence the earnings of men.

To discern potential impacts concentrated solely on newly hired personnel, I extend the analysis to investigate the influence of agreement signings on the wage gap among new employees, as presented in appendix table C.2. I also find no significant effect on their wage gap, underscoring the limited substantive impact of agreement implementations on this particular subset of employees.

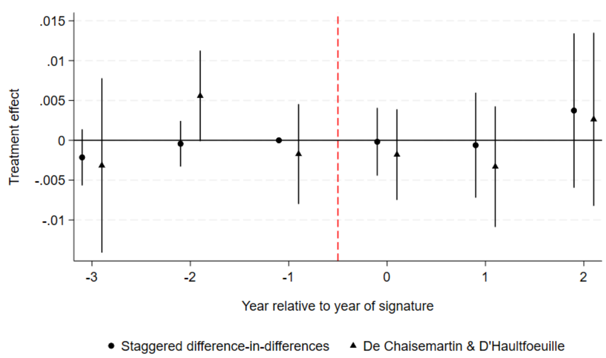
Overall, my findings align with observations from two sociological studies in France (Giordano and Santoro, 2019; Milner et al., 2019). These studies, delving into the content of agreements, revealed a prevalent trend wherein firms simply recalled the existing law or formalized established workplace practices instead of putting in place new measures. The absence of notable effects on the different wage gap measures resonates with these documented outcomes.

Figure 2: Effect of signing an agreement on the wage gap by socioeconomic status

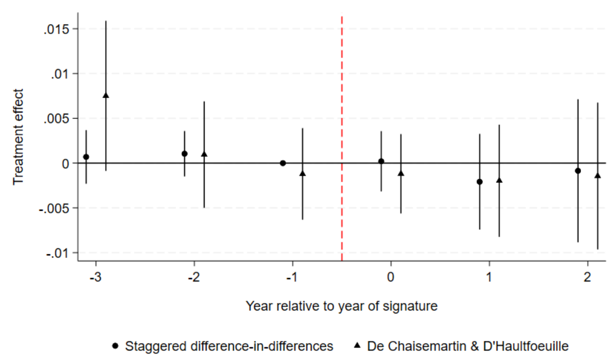
(a) All workers



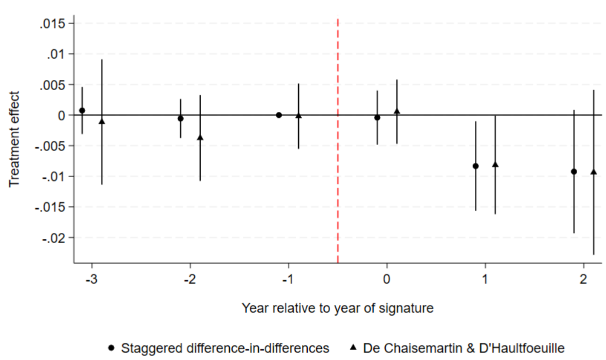
(b) Executive



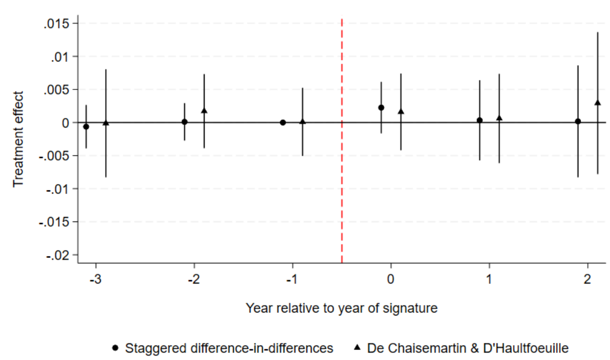
(c) Intermediate profession



(d) Employees



(e) Blue collar workers



Source: DADS 2008-2013 and D@ccord 2010-2013.
Note: The dependent variables are the wage gap for all workers in (a), for executives in (b), for intermediate professions in (c), for employees in (d), and for blue-collar workers in (e).

Table **??** presents the effects of agreements on the wage promotion gap across various socioeconomic statuses. Examining the mean of the wage promotion gap by category reveals an anticipated trend where women on average receive smaller wage promotions than men. For example, executive-level women tend to experience wage promotions approximately 0.4 percentage points lower than their male counterparts. One notable exception is for the employees category, in which women tend to be over-represented, where women earn wage promotions higher by 0.4 percentage points than men. Despite these disparities, for all four socioeconomic groups, the results on the wage promotion gaps are null and lack statistical significance. The effect of agreements is positive, meaning a decrease in the wage promotion gap, although not significant, for executives, intermediate professions and blue-collar workers where women received on average lower wage promotions than men.

I analyze the effects on outcomes related to the glass ceiling. In their study looking at a selected subsample of firms agreements, Pochic et al. (2019) find that firms tend to associate wage gap with glass ceiling problems. I hence first verify whether more women are accessing the very top positions in their firms as proxied by the share of women among top earners. Figure 4 shows no improvement regarding this outcome, with no significant change in the share of women among the top 10, 5, and 1% earners. If anything, there seems to be a slight decrease in the share of women among the top earners, although not significant. As women could obtain wage increases but still not reaching the very top positions, I extend my analysis by looking at the wage gap between the top female earners and the top male earners at different points of the wage distribution. Figure 6 shows that there is no visible change either in the wage gap between the top 25%, top 10%, and top 5% earners.

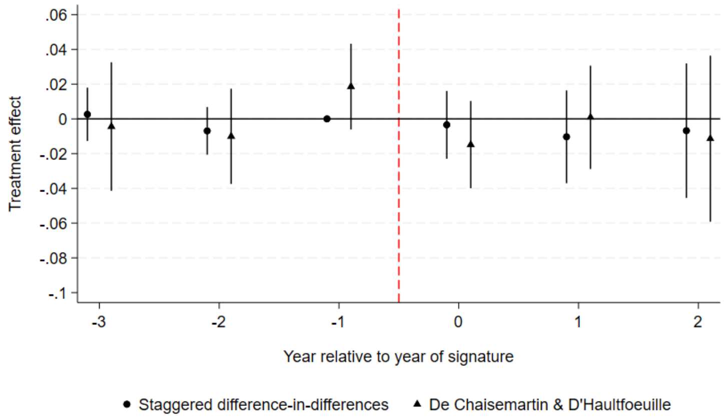
I then turn to explore other outcomes unrelated to wages. Given that firms could decide to address other gender inequalities, I test whether women in fixed-term contracts get more access to permanent contracts compared to men. Figure C.1 plots the coefficients of interest of equation 2 and shows no impact on that outcome too. I also look at whether there are any changes in the percentage of women among the newly hired. This could change if firms decide that, at a similar level of skills, they will hire a woman. In their study on the agreements signed in the Aquitaine region, Giordano and Santoro (2019) found that one common action area chosen was the hiring one. For this analysis, I focus only on permanent contracts as fixed-term contracts can be used to replace temporarily unavailable workers. The results presented in table **??** indicate no significant change in that outcome either. This is consistent with Giordano and Santoro (2019)'s study whose results suggest that firms merely chose to raise their partners' (universities or employment agencies) awareness on gender diversity issues so that they could be offered more diverse candidates, or simply changed the wording of job offers to include both genders ("Looking for woman/man").

Table 5: Effects on the wage promotion gap by socioeconomic status

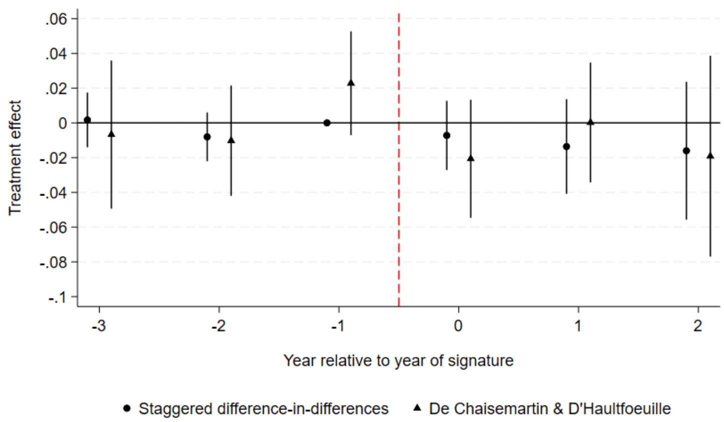
	(2) Executive	(3) Inter. Prof	(4) Employees	(5) Blue collars
Method: Staggered difference-in-differences				
Agreement	-0.00055	0.00088	0.00006	-0.00008
	(0.00114)	(0.00091)	(0.00092)	(0.00100)
$Mean$	0.12951	0.16113	0.07597	0.04420
N	21,655	29,485	25,280	19,970
R^2	0.25	0.26	0.24	0.25
Method: De Chaisemartin & D'Haultfoeuille				
Agreement	-0.00178	0.00121	-0.0008781	0.00015
	(0.00147)	(0.00127)	(0.00113)	(0.00128)
$Mean$	0.12928	0.15984	0.07519	0.05044
N	16,729	22,867	19,963	15,463
Year fixed effects	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes

Source: DADS 2008-2013 and D@ccord 2010-2013. Note: $* \; p < 0.05$, $** \; p < 0.01$, $*** \; p < 0.001$. The dependent variable is the wage promotion gap by socioeconomic category in (1) to (4). Standard errors in parentheses are clustered at the firm level.

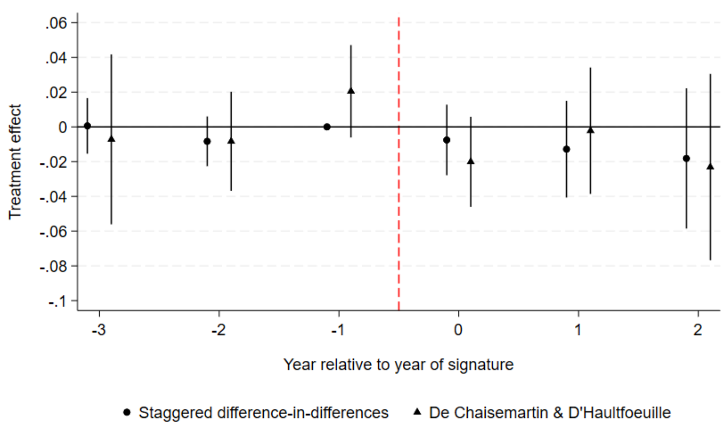
Figure 4: Effects on the proportion of women among top earners



(a) Proportion of women among top 10% earners



(b) Proportion of women among top 5% earners

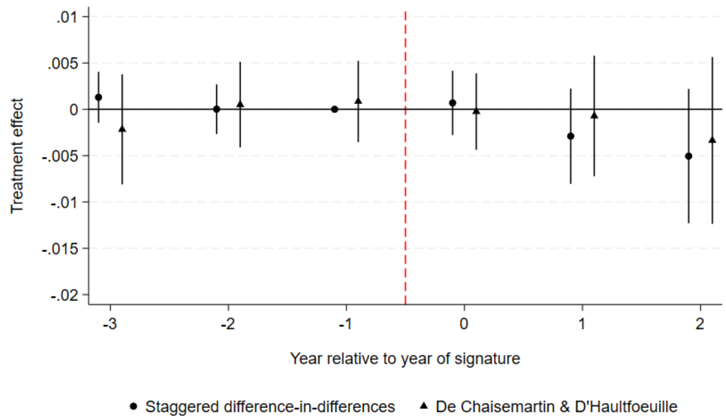


(c) Proportion of women among top 1% earners

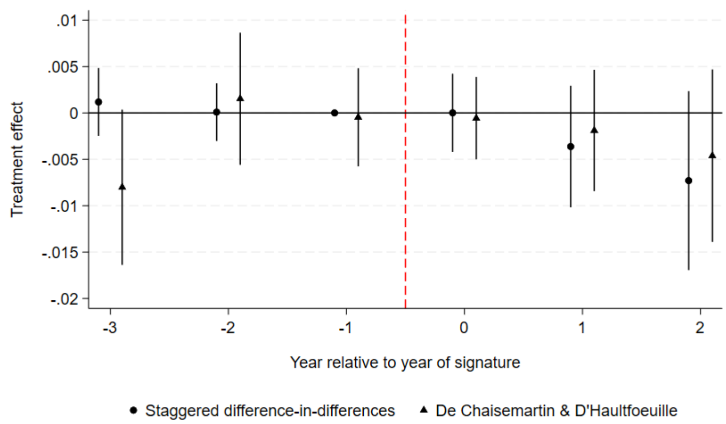
Source: DADS 2008-2013 and D@ccord 2010-2013.

Note: Estimated using equation 2 and De Chaisemartin and d'Haultfoeuille (2020). Standard errors in parentheses are clustered at the firm level. The dependent variable is the proportion of women among the top 10%, 5% and 1% earners.

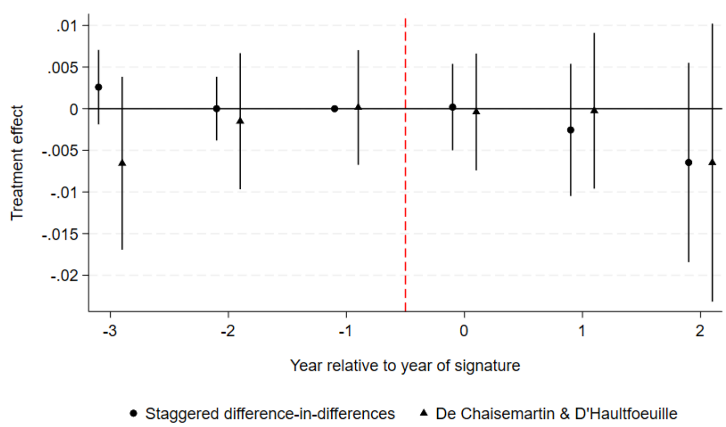
Figure 6: Effects on the gender wage gap between top earners



(a) Gender wage gap between top 25% earners



(b) Gender wage gap between top 10% earners

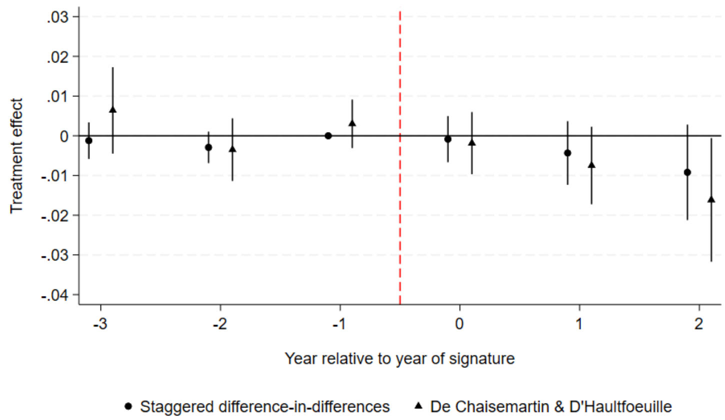


(c) Gender wage gap between top 5% earners

Source: DADS 2008-2013 and D@ccord 2010-2013.

Note: Estimated using equation 2 and De Chaisemartin and d'Haultfoeuille (2020). Standard errors in parentheses are clustered at the firm level. The dependent variable is the wage gap between the female top earners and the male top earners.

Figure 8: Effect on the percentage of women among the newly hired



Source: DADS 2008-2013 and D@ccord 2010-2013.
Note: Estimated using equation 2 and De Chaisemartin and d'Haultfoeuille (2020). Standard errors in parentheses are clustered at the firm level. The dependent variable is the wage gap between the female top earners and the male top earners.

5.2 Heterogeneity analysis

Table 6 presents the heteregeneous effects for the wage gap along several dimensions: depending on the firm's sector, the gender of the CEO, the share of women in the workforce.THe first column of table 6 shows that the interaction term between the *Agreement* variable and the "female CEO" dummy is positive and significant. The signature of an agreement when the CEO is a woman has resulted in an increase of the gender wage gap of 1.8 percentage points. This might seem a bit surprising as other results in the literature suggest that the wage gap tends to decrease when females lead (Cardoso and Winter-Ebmer, 2010; Hirsch, 2013; Tate and Yang, 2015). These results might be partly explained when looking at column (2) of table 6, which presents the heterogeneous effects depending on the firm's sector. Here, the construction sector is used as the reference category. The results show that compared to the construction sector, the gender wage gap has increased for the sector of public administration, education, health, and social work activities (OQ). This might explain the increase in the gender wage gap when women are CEOs. There hence seems to be some unintended negative effects for women working in very female-dominated sectors. This is consistent with the analysis carried on by Pochic et al. (2019) where they found that in female-dominated sectors, some agreements tended to implement measures favouring men.

Table 6: Heterogeneous effects on the wage gap

	(1)	(2)	(3)	(4)	(5)
	Wage gap	Wage gap	Wage gap	Wage gap	Wage gap
					Agreements only
Agreement	0.0076	-0.0129	-0.0005	0.0068	0.00045
	(0.0022)	(0.0040)	(0.0012)	(0.0014)	(0.00123)
Agreement*Female CEO	0.0181*				
	(0.0054)				
Agreement*Industry		0.0116			
		(0.0041)			
Agreement*Tertiary		0.0061			
		(0.0041)			
Agreement*OQ		0.0125*			
		(0.0046)			
Agreement*250-499			0.0013		
			(0.0013)		
Agreement*500+			-0.0010		
			(0.0015)		
Agreement*25-49%				-0.0083**	
				(0.0016)	
Agreement*50-74%				-0.0078**	
				(0.0016)	
Agreement*75-100%				0.0015	
				(0.0026)	
Mean	0.13	0.13	0.13	0.13	0.13
Observations	21,905	55,545	55,545	55,545	30,675
R^2	0.55	0.90	0.90	0.90	0.90
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes

Source: DADS 2008-2013 and D@ccord 2010-2013. Note: $* \, p < 0.05$, $** \, p < 0.01$, $* * * \, p < 0.001$. Standard errors in parentheses are clustered at the firm level. This table presents estimation results from equation 1 when interacting the Agreement variable with dummies for sector and for size groups. The construction sector is used here as the reference category for sectors and being a firm with 50 to 249 employees is the reference category for the size.

I then turn to looking at the effects depending on share of women in the firm and construct dummies *25-49%*, *50-74%*, *75-100%* equal to one when women respectively represent between 25-49%, 50-74%, and 75-100% of a firm's workforce. Column (4) of table 6 indicates that there is no significant effect on the gender wage gap of having signed an agreement when women represent the vast majority of workers. This is consistent with results from Pochic et al. (2019), which showed that in female-dominated firms, there tends to be a bias in which employers think that there are no gender inequalities because the workforce is mostly female. On the contrary, when women represent between 25 and 75% of the workforce, signing an agreement on gender equality results in a decrease in the gender wage gap. The magnitude of the effect is very small however, the gender wage gap decreasing by about 0.8 percentage points.

Table **??** also presents the heterogeneity of results depending on the size of the firm. Even if larger firms were more likely to sign agreements in the first place, signing an agreement and being a large firm is not associated with a decrease in the gender wage gap. This shows that even large firms, with organised HR departments and more means, were not able to reduce gender inequalities through negotiating on gender equality. This is supported by results from Brochard and Letablier (2017) who found that large firms prefer to avoid setting quantified targets that could then be used in litigation.

Finally, I analyze the effects on the wage gap restricting the sample to firms that signed a negotiated agreement only, excluding thus the unilateral action plans. It is likely that in firms where negotiations failed, the action plan put in place would be less ambitious than a negotiated agreement. Hence, mixing the two in our analyses could bias our results downwards. The static results are presented in the last column of table 6 and the dynamic ones in figures F.1 to F.2 and show again no effects on the wage gap nor on the wage gap by socio-economic status.

5.3 Long-term effects

One potential concern with this analysis is that it looks only at short-term effects, only up to two years after the signature of agreements, due to data limitations. To go around this, I use an extension of D@ccord to later years that registers agreements signed by firms between 2014 and 2017. A dummy variable indicates if those agreements contain professional equality measures. Agreements dealing with a theme (in this case, professional equality) may deal with it in a specific way (specific agreement on professional equality) or in a secondary way (professional equality dealt with in the framework of mandatory annual negotiations). The level of analysis of the agreements, which varies from one department to another[11], is reflected in a more or less precise identification of the different themes. As a consequence, I look at the long-term effects of signing an agreement that contains *some* gender equality measures but that does not necessarily focus only on that. A second issue is that in 2014, a new law was passed that mandated firms above 50 employees to introduce equal value evaluations for their workers. Our long-term results are hence potentially biased by firms who could have decided to implement it.

Using the same empirical strategies as above, the results presented in table **??** suggest mostly no effects

[11] Each department has its own reporting classification, that is not harmonized at the national level.

on the gender wage gap across the different specifications. There seems to be a very small decrease in the gender wage gap for executive workers using the De Chaisemartin and d'Haultfoeuille (2020) specification. This is consistent with the evidence that those were the workers for which agreements tended to focus on (Pochic et al., 2019). The executives' gender wage gap decreases by 0.8 percentage points, which represents a 0.06% decrease of the mean gender wage gap of 15.4%. The magnitude of the effect is hence very small. Furthermore, when looking at dynamic effects in the online appendix figure **??**, the results seem to indicate a decrease in the wage gap only for officers 3 years after the signature of agreements but that effect is not significant. It hence seems that those effects are very small and not very robust, compared to some of the effects found in the pay transparency literature.

6 Robustness

To verify the robustness of my results, I also implement a stacked regression as in Cengiz et al. (2019). To do this, I create a separate dataset for each of the 3 treatment waves before the last one (2010, 2011, 2012). In each of these datasets, firms that sign an agreement in that year are considered treated while firms that signed an agreement at a later date serve as control. Since I cannot observe the signature of agreements after 2013, firms that sign an agreement in 2013 only serve as controls as there would not be a control group for them in my sample. For that reason, all observations from calendar year 2013 are excluded from the estimation. Then, for every dataset, I create event-time dummies relative to the year of signature of the agreement. My estimating equation then becomes:

$$Y_{it} = \alpha_i + \delta_t + \beta_0 Treated_{ic} + \beta_{DD} Treated_{ic} \times Post_{it} + \sum_{k=-4}^{k=2} \beta_k * D^k + \epsilon_{it} \tag{4}$$

where $Treated_{ic}$ is a dummy taking value 1 if the firm i is a treated firm in cohort c. As the datasets are stacked, the same firm can appear multiple times both as treated and control: $Treated_{ic}$ is hence not collinear with the firm fixed effect. $Post_{ic}$ is equal to 1 for all the years after which an agreement was signed. The D^k are a set of relative event-time dummies that take the value 1 if year t is k periods after (or before) the treatment. The difference between this estimation and the standard event-study DiD is that one need to saturate the unit and time fixed effects with indicators for the specific stacked dataset. Those indicatord allow me to control for event-time trends that are not captured by the calendar year fixed effects. Standard errors are clustered at the firm level.

To look at dynamic effects and to verify there are no pre-trends, I also estimate the following specification:

$$Y_{it} = \alpha_i + \delta_t + \beta_0 Treated_{ic} + \sum_{k=-4}^{k=2} \gamma_k * D^k \times Treated_{ic} + \sum_{k=-4}^{k=2} \beta_k * D^k + \epsilon_{it} \tag{5}$$

The γ_k's measure the change in outcomes of treated firms (early signatories) k years after treatment, relative to pre-treatment year, compared to the change in outcomes of control firms (late signatories).

Similarly to the results found previously, I found no significant effects on the gender wage gap or any of the other outcomes (figures E.1 to **??**).

To check the validity of my results, I also implement a simple difference-in-differences strategy where I compare in 2009 and 2013, firms of 35-45 employees who never signed an agreement over the 2010-2013 period to firms of 55-65 employees who signed an agreement:

$$Y_{it} = \alpha + \beta_0 Post_t + \beta_1 Treated_i + \gamma Post_t \times Treated_i + \epsilon_{it} \tag{6}$$

where $Post_t$ equals 1 for the year 2013 and 0 for the year 2009 and $Treated_i$ is a dummy equal to 1 for firms of 55-65 employees and 0 for firms of 35-45 employees. Standard errors are clustered at the firm level. Firms between 45 and 55 employees are dropped due to the fuzziness of the size measurement: temporary workers, who count for the number of employees, are linked in the DADS to their temporary employment agency and not to the firm they are actually working in. As a result, if a firm with 47 employees in the DADS is employing 3 or more temporary workers, she will actually be declared as a firm of 50 employees or more in fiscal data and hence will be under the obligation of the law. Results from equation 6 presented in table 7 show no significant effect of signing an agreement on the overall wage gap nor on any gender wage gap by socio-economic profession.

Table 7: Effects on the wage gap and the wage gap by SES

	(1)	(2)	(3)	(4)	(5)
	Wage gap	Executive	Inter. Prof	Employees	Blue collars
Post	-0.03106***	-0.00805	-0.02681**	-0.02906***	-0.04296***
	(0.00177)	(0.00522)	(0.00419)	(0.00317)	(0.00377)
Treated	0.04253***	0.02700*	0.01382	0.02055*	0.01093
	(0.00567)	(0.01032)	(0.00736)	(0.00729)	(0.00680)
Treated*Post	0.00094	-0.01254	0.00739	-0.00856	-0.00283
	(0.00404)	(0.01158)	(0.00880)	(0.00945)	(0.00919)
$Mean$	0.0937	0.138	0.0692	0.0462	0.0915
N	25,198	11,756	14,383	16,106	10,264
R^2	0.003	0.001	0.001	0.001	0.002

Source: DADS 2008-2013 and D@ccord 2010-2013. Note: $* \ p < 0.05$, $** \ p < 0.01$, $*** \ p < 0.001$. The dependent variable is the mean wage gap in (1) and the mean wage gap by SES in (2) to (5). Standard errors in parentheses are clustered at the firm level.

In figures D.1 to **??**, I plot the evolution of exogenous characteristics around the time of signature in order to verify that there is no change in the composition of firms around the date of treatment that could explain those null effects. No significant change can be seen in the number of employees, share of young workers, share of women nor the proportion of full-time workers around the date of signature.

The results are also robust when I restrict the sample to firms above the 50 employees threshold in 2009, instead of firms above 50 employees for the whole period, as can be seen in table **??** and figures **??** to **??**.

Finally, I verify using the D@ccord database from 2005 to 2009 whether firms that signed agreements on gender equality between 2010 and 2013 are firms that had never negotiated on the topic before. It could be that it is not negotiating on gender inequalities has no effects but simply that forcing reluctant firms to negotiate might not work. Table **??** shows that if anything, firms above 50 employees that signed agreements between 2010 and 2013 were *more likely* to have already negotiated on the topic. Table **??** looks at the heterogeneous effects of signing an agreement on the gender wage gap depending on whether firms had negotiated on gender equality before. The coefficient on the interaction between the dummies for agreement and having negotiated before is close to zero and not significant, showing that firms who had negotiated on this topic before did not perform better in reducing gender inequalities.

It is also possible that some firms were covered by sectoral agreements which contained gender equality measures and that those firms performed better as they might have had an example to build on to negotiate on gender equality. As D@ccord does not include sector-level agreements, this cannot be verified. However, this issue is likely to be minor as in their analysis of the agreements' content, Milner et al. (2019) found that only a very small minority of texts referred to sectoral agreements.

7 Discussion

This paper explored the effects of signing agreements on professional equality between men and women on the gender wage gap using the staggered year of signature. I find no significant results on the mean wage gap, on the wage gap of different socio-professional categories, and on other measures of gender inequalities. The heterogeneity of results suggest that when women represent between 25 and 75% of the workforce, there is a small decrease of the gender wage gap but the magnitude of the effect is very small compared to the pay transparency literature. The results also suggest that in the female-dominated sector of health and public administration, the law had unintended negative effects as the wage gap actually increased after the signature of agreements. Overall, those results suggest that the law of November 2010 was not effective in reducing the pay gap between men and women. Those results are consistent with the findings of different studies in sociology (Milner et al., 2019; Pochic et al., 2019; Giordano and Santoro, 2019) that found that firms did sign agreements but those often lacked proper indicators of gender inequalities in the firm and avoided taking any constraining measures.

These results can be explained by several factors. First, by requiring only to sign an agreement without giving a mandatory target of eliminating the wage gap (as the 2006 law required), the law was not very binding and gave firms a lot of room for maneuver. This was reinforced by the fact that labour inspectors were required to verify only the signature of an agreement but their content was not subjected to checks. Second, the threat of the fine might not have been strong enough to push firms to sign agreements and implement them correctly. Indeed, given the problem of understaffing of the Labour Inspectorate (Giordano and Santoro, 2019), firms, especially the medium-sized ones, might decide to not sign an agreement as they might not be inspected. Besides, the risk is not really important as, if they are caught not having signed one, they receive a formal notice giving them six months to negotiate an agreement so that they can still avoid the fine.

Finally, those results also point to the limits of negotiation through unions to reduce gender inequalities. Despite the negotiation having to take place with delegates mandated by trade unions, the agreements do not seem to have any effect on many different outcomes on gender inequalities. This can be explained by several factors. First, in France, unions are often strong at the sectoral level but weak at the company level. Second, gender equality is not always a priority for union as shown by a study by Cristofalo (2014) on the process of negotiation on gender equality by unions. She finds that unions do not see gender equality as a priority but rather as a second-rank issue, less urgent than wages and job retention. Unions' workers also consider that this issue requires a great deal of investment to achieve few results in the end. Making detailed diagnoses, developing and following up on the agreements signed requires a great deal of effort on the part of both the referents and the teams. Under these conditions, pragmatic calculations take precedence and lead to this issue being left aside. Lastly, Milner et al. (2019) highlights also that employers were often providing scattered statistics, not systematically gendered and not always verifiable. In their sample, about 40% of agreements had not any data broken down by gender, complicating the negotiation process for unions.

This could explain why I do not find effects whereas Kurtulus (2012) was able to. In his context, a Commission was created to actually verify that the actions in favor of affirmative action were *actually* put in place by firms and the threat of loosing a federal contract was quite strong for firms. This suggests that letting firms negotiate without strong binding constraints is not enough to reduce the gender wage gap and there should be a supervisory body monitoring the content of those agreements for it to work. Otherwise, transparency on wages, although it has some negative effects, might be the best alternative to reduce the gender wage gap.

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Appendices

A Construction of the outcome variables

Then, as no indicator is a perfect measure of gender inequalities within a firm, I construct several outcomes of interest and present them below.

My first measure of gender inequalities is the **mean raw gender wage gap**, $GWG_{jt} = \frac{W_{F,jt}-W_{M,jt}}{W_{M,jt}}$, where GWG_{jt} is the gender wage gap in firm j at year t, $W_{F,jt}$[12] is the mean gross hourly wage of women in firm j at year t and $W_{M,jt}$ is the mean gross hourly wage of men in firm j at year t.

However, that measure compares the wages of men and women in very different positions within the firm. To partly address this concern, I also compute the **gender wage gap by socio-professional category**: $GWG_{cjt} = \frac{W_{F,cjt}-W_{M,cjt}}{W_{M,cjt}}$, where GWG_{cjt} is the gender pay gap in firm j at year t for socio-professional category c, $W_{F,jt}$ is the mean gross hourly wage of women in socio-professional category c in firm j at year t and $W_{M,jt}$ is the same but for men.

To see any potential effects on the glass ceiling, I compute the percentage of women in the top 10, 5 and 1% earners in a firm:

$\%ofWomen_{topX,jt} = \frac{\#ofWomenintopX\%earners_{jt}}{\#ofTopX\%earners_{jt}}$.

As I am also interested on effects along the wage distribution, I also look at the wage gap along the wage distribution. For example, I compute the **top earners wage gap**, which corresponds to the wage gap between the top α% female earners and the top α% male earners within a firm, where α=5, 10 or 25%, in this way : $GWG^{\alpha}_{jt} = \frac{W^{\alpha}_{F,jt}-W^{\alpha}_{H,jt}}{W^{\alpha}_{H,jt}}$.

Another measure of gender inequalities in the workplace I build is the wage promotion gap. For each employee, I compute the average wage promotion received between year N-1 and year N and average it at the firm level for each gender and socio-professional category. Then, I define the wage promotion gap as: WPG_{cjt}=$WP_{M,cjt}$, - $WP_{F,cjt}$, where $WP_{F,cjt}$ is the wage promotion received by women of SES c in firm j in year t and $WP_{M,cjt}$ the same for men.

I also compute the percentage of women promoted from a fixed-term contract (CDD) to a permanent contract (CDI) between year N-1 and year N, and the same for men. This allows us to build an indicator of the difference in promotion to a permanent contract between women and men, *which the literature identifies as a mechanism for the wage gap*: $Permanent_{jt} = \frac{Permanent_{F,jt}-Permanent_{H,jt}}{Permanent_{H,jt}}$.

Finally, I look at another mechanism between gender differences, discrimination in hiring. Several stud-

[12]The mean gross hourly wage by gender is computed as the average of the gross hourly wages of workers of that gender in a firm weighted by the number of hours worked. The gross wage is used instead of the net wage because during the study period, social security contributions from employees increased, which widened the gap between gross and net wages.

ies have proved the existence of discrimination during the hiring process (Kübler et al., 2018; Neumark et al., 1996; Goldin and Rouse, 2000; Petit, 2007). For example, Goldin and Rouse (2000) studied the impact of the adoption of blind auditions by orchestras where a screen hid the identity of the candidate. They found that the presence of a screen dramatically increased the probability that a woman would pass the preliminary rounds and be hired at the final round, explaining 25% of the increase in the percentage of women in the top 5 symphony orchestras in the United States. Without having to install a screen, firms in their agreements can take measures to promote women's hiring or at least ensure that women are less discriminated against in the hiring process. For example, a firm could decide that when having to choose between two candidates of equal competence, the person from the least represented gender is hired. As I only observe the newly hired workers but do not have data on candidates for a position, I construct an imperfect measure that I use as a proxy for discrimination against women: the percentage of women among the new hires in permanent contracts. I focus on permanent contracts since fixed-term contracts are more precarious and can be used to fill the temporary absence of a worker.

B Additional descriptive statistics

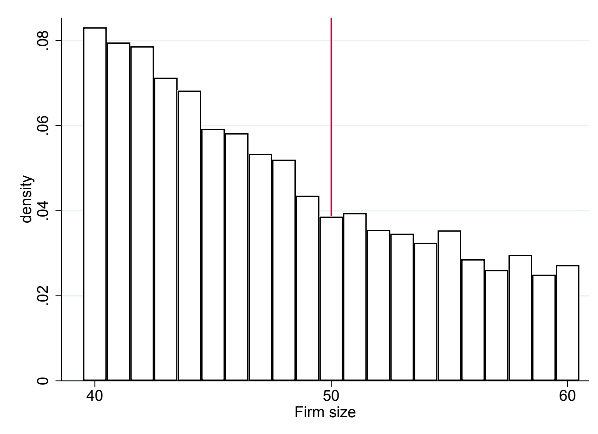
Table B.1: Repartition of signatories vs never signatories by size in 2010

	(1) Never signatories	(2) Signatories
01-09 employees	99.77%	0.23%
10-49 employees	95.26%	4.74%
50-249 employees	59.82%	40.18%
250-499 employees	28.86%	71.14%
500+ employees	22.07%	77.93%

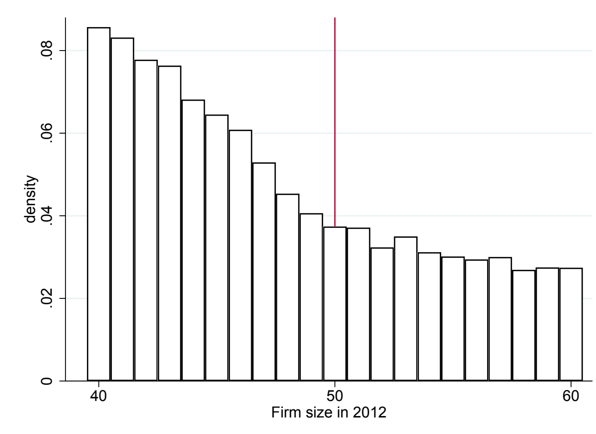
Source: DADS 2008-2013 and D@ccord 2010-2013. Note: "Never signatories" refers to firms that never signed an agreement between 2010 and 2013 wh
firms that signed an agreement between 2010 and 2013

Figure B.1: Distribution of firms around the 50 employees threshold

(a) (a) Size in 2010



(b) (b) Size in 2012



Source: DADS 2010 and 2012.

Note: The size of firms is computed in full-time equivalence.

Figure B.2: Proportion of firms signing an agreement between 2010 and 2012 by size in 2010

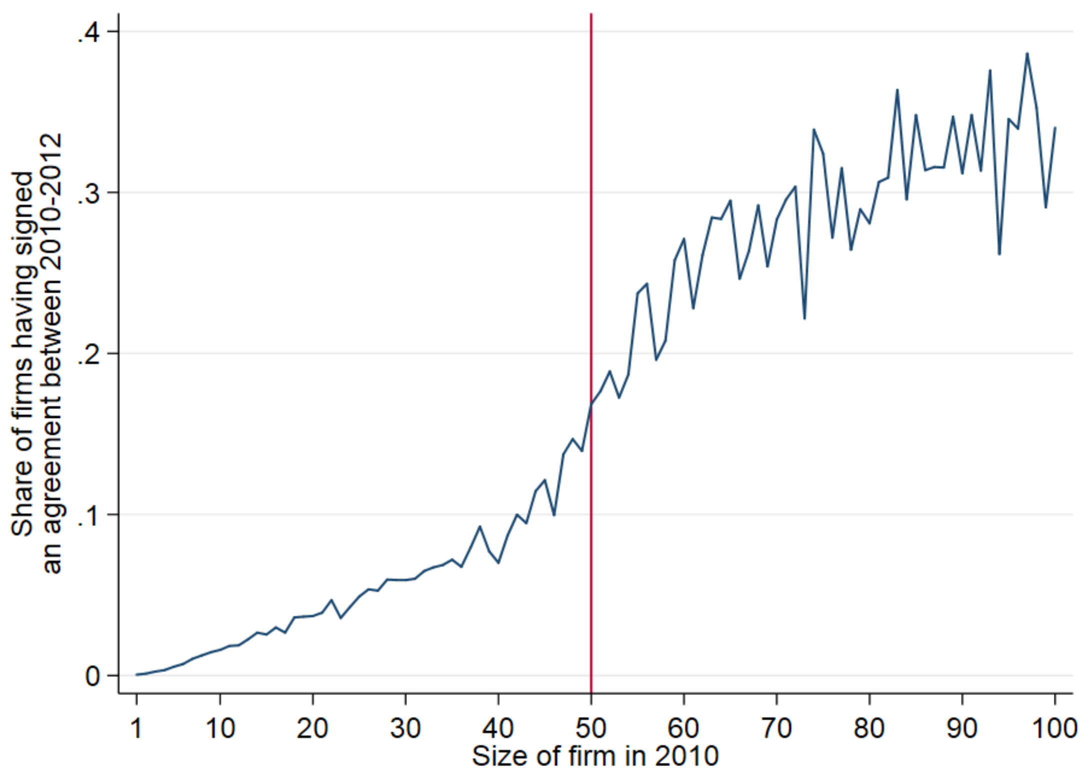


Table B.2: Descriptive statistics in 2009 for signatories firms above 50 employees by year of signature

	2010 signatories	2011 signatories	2012 signatories	2013 signatories
Number of firms	2,305	2,583	5,083	2,241
Mean age of workers	40.2	39.8	39.5	38.9
% of women	35.6	39.7	42.7	41.3
% of executives	16.5	17.7	16.9	17.0
% of female executives	28.9	31.9	33.6	30.9
% of full-time workers	87.3	86.3	84.4	84.1
% of women among part-time workers	62.7	66.2	67.4	68.1
% of short-term contracts	9.4%	11.0%	12.1%	12.1%
% of women among short-term contracts	52.6%	55.3%	57.7%	56.5%
Mean hourly wage	19.4	19.3	18.9	18.9
Industry	34.8%	26.7%	26.6%	24.1%
Construction	7.0%	8.9%	5.5%	6.3%
Tertiary excl. OQ	52.2%	54.9%	53.4%	59.2%
Tertiary OQ	6.1%	9.5%	14.5%	10.4%
50-249 employees	62.7%	64.8%	71.1%	77.9%
250-499 employees	19.8%	17.6%	15.4%	12.5%
500+ employees	17.5%	17.6%	13.5%	9.7%

Source: DADS 2008-2013 and D@ccord 2010-2013.
Note: This table presents descriptive statistics for the sample of firms that signed an agreement between 2010 and 2013 by year of signature and were above 50 employees in between 2010 and 2013. Descriptive statistics are measured in 2009. Tertiary OQ refers to public administration, education, human health and social work activities. In the full sample in 2009, the construction sector represented 12% of firms, industry 10%, OQ 6% and the tertiary sector 78%.

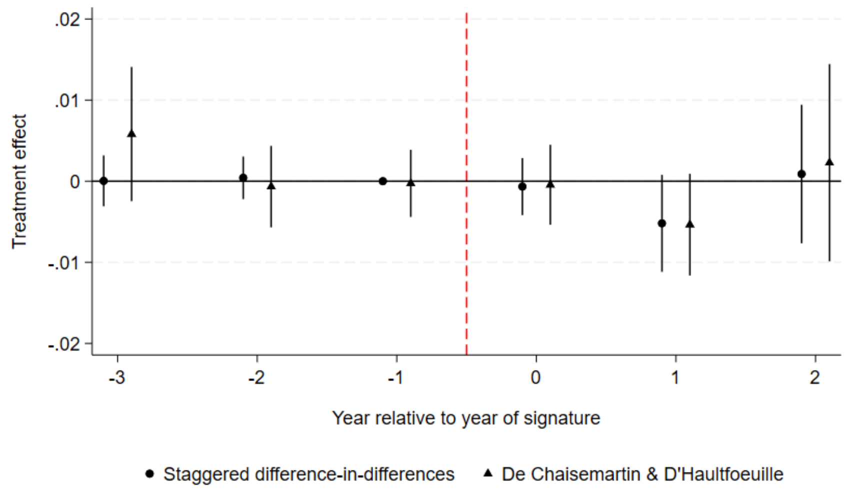
C Additional results

Table C.1: Effects on the wages of women and men

	ln(women's wage)	ln(men's wage)
Agreement	0.00113	0.00158
	(0.00105)	(0.00108)
Mean	2.81636	2.97003
N	55,545	55,545
R^2	0.96	0.97
Firm fixed effects	Yes	Yes
Year fixed effects	Yes	Yes

Source: DADS 2008-2013 and D@ccord 2010-2013. Note: $* \, p < 0.05$, $** \, p < 0.01$, $* * * \, p < 0.001$. Standard errors in parentheses are clustered at the firm level. This table presents estimation results from equation 1 when the outcome variables are the natural logarithm of the wages of men and women separately.

Figure C.1: Effect on the gender gap in moving from fixed-term to permanent contract.



Source: DADS 2008-2013 and D@ccord 2010-2013. Note: The outcome variable is the difference between the share of men that moved from a fixed-term contract to a permanent one in firm i and the share of women that moved from a fixed-term contract to a permanent one in firm i.

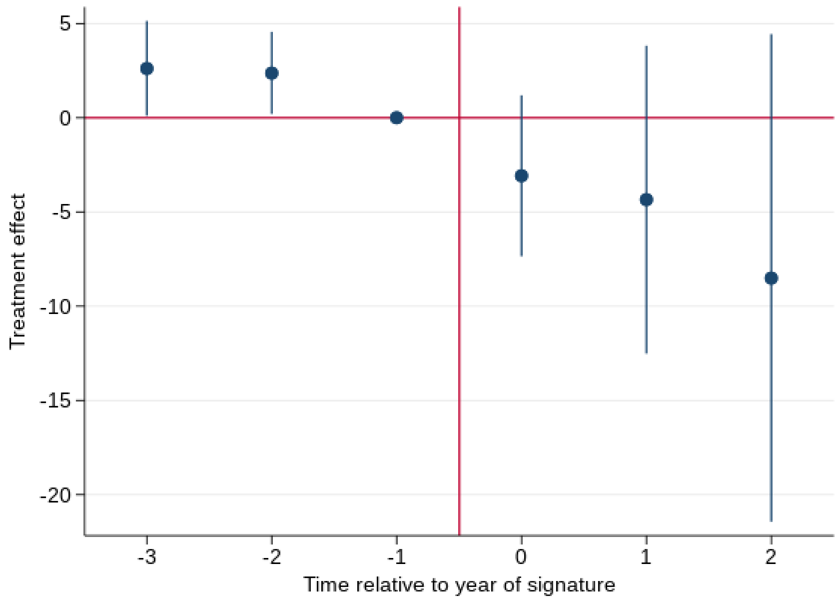
Table C.2: Effects on the wage gap of new employees

	(1)	(2)	(3)	(4)	(5)
	All	Executive	Inter. Prof	Employees	Blue collars
Agreement	-0.00365	0.01294	-0.00325	0.00233	-0.00344
	(0.00403)	(0.00774)	(0.00698)	(0.00528)	(0.00591)
$Mean$	0.108	0.142	0.050	0.011	0.047
N	37,205	11,895	11,070	13,245	6,775
R^2	0.36	0.26	0.30	0.29	0.30
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes

Source: DADS 2008-2013 and D@ccord 2010-2013. Note: $* \; p < 0.05$, $** \; p < 0.01$, $*** \; p < 0.001$. The dependent variable is the wage gap for newly hired employees in (1), and the wage gap for newly hired employees by socioeconomic category in (2) to (5). Standard errors in parentheses are clustered at the firm level.

D Evolution of exogenous characteristics

Figure D.1: Evolution of the number of employees



Source: DADS 2008-2013 and D@ccord 2010-2013. Note: The outcome variable is the number of employees in full-time equivalence.

Figure D.2: Evolution of the number of full-time workers

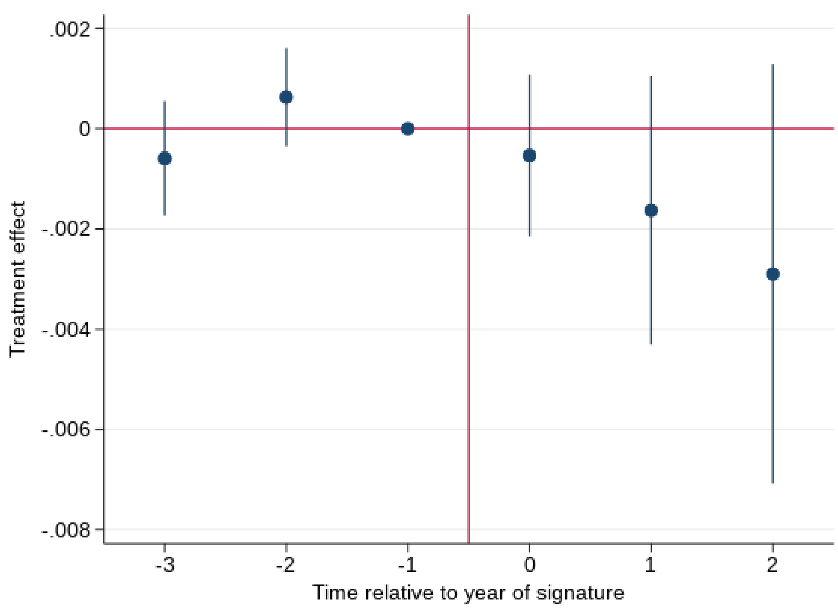
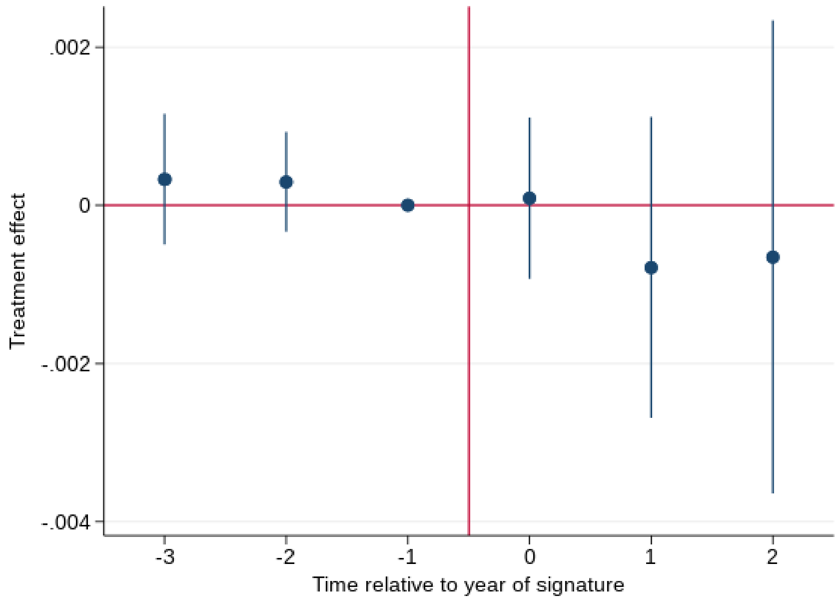


Figure D.3: Evolution of the share of young workers in the workforce



E Dynamic effects using the stacked regression design estimator

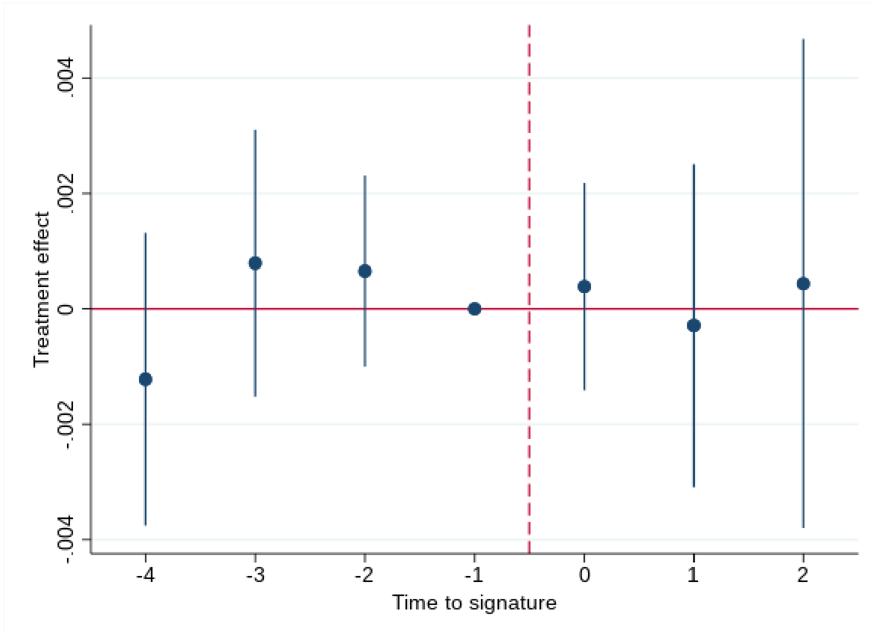
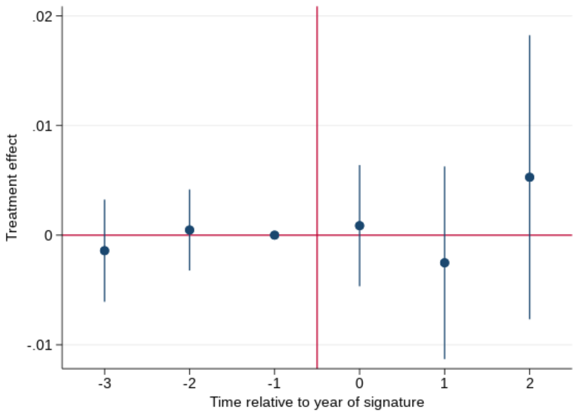


Figure E.1: Effect on the gender wage gap

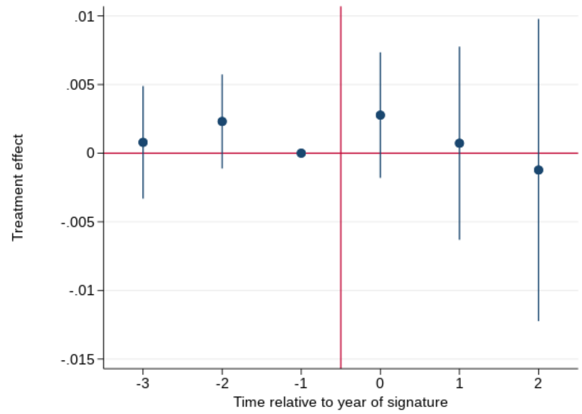
F Effects on agreements only

Figure F.1: Effect of signing an agreement on the wage gap by socio-economic status - agreements only

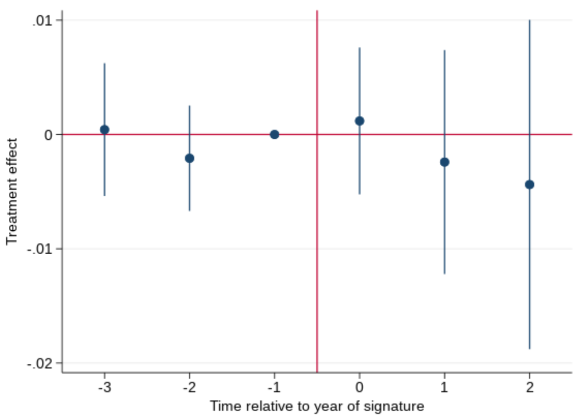
(a) Executive



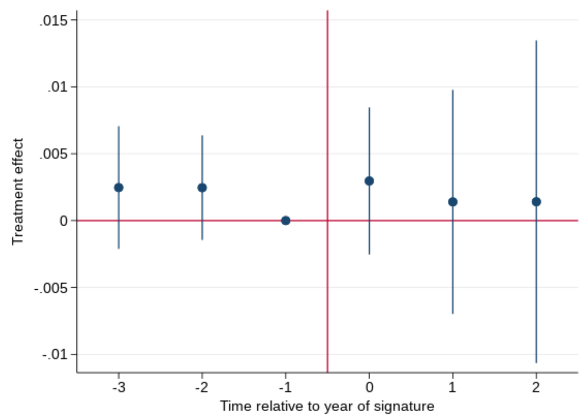
(b) Intermediate profession



(c) Employees

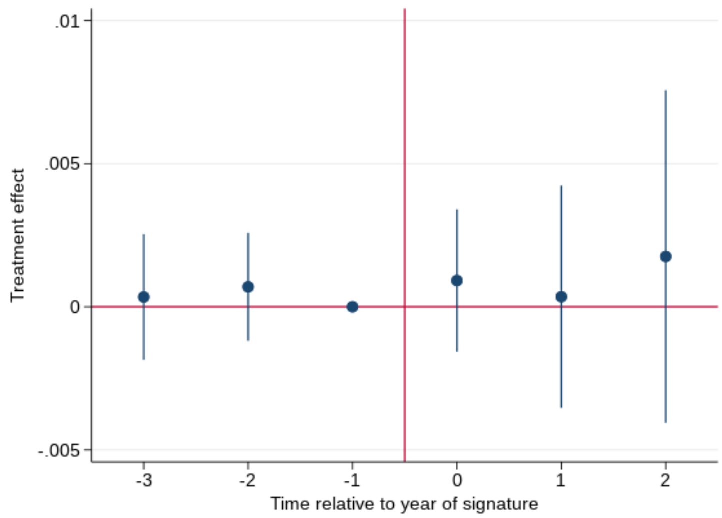


(d) (Blue collar workers



Source: DADS 2008-2013 and D@ccord 2010-2013. Note: The dependant variables are the wage gap for executives in (a), for intermediate professions in (b), for employees in (c) and for blue collar workers in (d). The sample is restricted to negotiated agreements and unilateral action plans are excluded from the analysis.

Figure F.2: Effect of signing an agreement on the gender wage gap - agreements only



Source: DADS 2008-2013 and D@ccord 2010-2013. Note: The dependant variables are the wage gap for executives in (a), for intermediate professions in (b), for employees in (c) and for blue collar workers in (d). The sample is restricted to negotiated agreements and unilateral action plans are excluded from the analysis.