Can Firm-Level Wage Surveys Close the Gender Pay Gap? Evidence from 20 Years of Swedish Policy

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Abstract

This paper studies the effect of mandating firms to produce a Gender Wage Survey on the gender wage gap. We use yearly employer-employee data for Swedish firms for the years 1996—2015. For each firm and year we compute two gender wage gaps that are targeted by the policy: i) the average unwarranted gap for men and women with the same job and ii) the average unwarranted gap for men and women who perform work of "equal value". Using these gaps as the outcome variable, we fail to find any policy effect around implementation thresholds for firm size. We also use a "scope analysis" to show that the maximum effect of the policy on the gender pay-gap would be modest, even under perfect compliance by firms. We end by drawing policy lessons from our results and from a wider discussion of compliance, policy design, and enforcement strategies.

Keywords: Gender wage gap, Wage discrimination, Firm size.

JEL-Classification: J16, J21, J31, J71

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1. Introduction

Women across the globe are still paid less than men for jobs that require the same skills and responsibility, which translates into lower well-being, status and influence. A growing body of economic research focuses on how firm-level factors impact the gender wage gap. Recent studies have considered the role of time-space flexibility afforded to workers (Goldin 2014) and gender differences in bargaining power (Card et al. 2015). It has also discussed how firm-level use of statistics on wages and promotions can reveal implicit gender biases (Bohnet 2016). On the policy side, countries like Iceland, Switzerland, the UK, and Denmark have recently enacted policies that require private businesses to report their gender wage gaps or take actions to reduce them.

Already in the early 1990s, the Swedish government introduced the world's most progressive mandate for company-level action against gender differences in pay. The policy was prompted by a heightened concern about the persistence of gender wage differences despite progressive policies on child care benefits and parental leave (Prop 1991/91:113, Fransson and Thörnqvist 2006). Businesses became required to close unwarranted gender pay gaps in a process of mapping, analysing, and adjusting wages.

This paper evaluates the policy of mandatory gender wage surveys. We use administrative data and variation in implementation thresholds across firm sizes to analyze the causal impacts of this policy on firm-level gender wage gaps. The policy explicitly took aim at two kinds of gender wage gaps. Employers are legally obliged to adjust discriminatory wage differentials between equally qualified women and men with the same occupation. In addition, wage-differentials must be adjusted between men and women who are equally qualified and work in different occupations, but where the occupations are equally demanding. We simulate an implementation of the wage survey on administrative data by estimating gender the two gender wage gaps for each firm and year. We then use firm size thresholds for a firm-level causal analysis.

The mandate to produce gender wage surveys was introduced in 1994 for firms with ten or more employees. But it was not efficient. It was not until 2001 that the regulation took the shape that was "initially intended" by the lawmakers (JämO 2003, 2005). The 2001 reform strengthened the mandate in several important ways. It provided a definition for the concept of work of equal value and also introduced a requirement that employers must adjust unwarranted gender wage gaps as soon as possible, but at the latest within three years. We utilize the difference in the (meaningful) wage survey mandate created by the 2001 reform for firms around the 10-employee threshold. As our second threshold we use a second reform that, in 2009, removed the mandate from firms with 10—25 employees. For this threshold we can estimate the impact of a continued mandate in firms with 25—50 employees compared to smaller firms.

The empirical approach relies on both time and cross-sectional variation. Because the data is a random sample of private sector firms rather than a balanced panel, we cannot use a standard differences-in-differences analysis that follows companies over time. Instead, we first examine the variation across firm size categories over time by estimating an interaction model on the repeated cross-sections. We then use a Regression Discontinuity Design to compare firms across the two firm size thresholds in pooled cross-sectional data for the pre- and post-reform periods delineated by each of the two reforms.

Our estimations provide no evidence that the mandate to produce wage surveys led to a reduction in gender wage discrimination. We fail to find reduced gender wage gaps for women and men doing equal work, and also for men and women doing work of equal value. Over the 25 years of data, gender wage gaps did not shrink more in treated firms than in untreated firms. These null results are robust to various specifications of firm-level wage gaps, specifications using an alternative measure of wage, as well as to specifications removing the observations closest to the threshold.

As a way to understand the null-results of the main estimations we explore the policy scope of the wage survey mandate. More specifically we provide approximations of the mandate's potential to close the raw gender wage gap in the private sector and at the aggregate level. This amounts to a calculation of the proportion of the total gender wage gap that can be derived from firm-level wage-gaps between equally qualified men and women on the same jobs (and, separately, for equally demanding jobs).

The calculations of the policy scope suggest that the policy, even if perfectly implemented, could close only a modest fraction of the pay gap. Perfect implementation would reduce the difference in average wage between women and men in the private sector by at most 19 percent. Critically – wage gaps between men and women with similar qualifications and doing the same or same-value work within a business do account for a large proportion of the overall wage gap. An important reason for this is the task-division within firms. Unless the firm is large, few people have the same job. Among these people, jobs are often single-gender, and if they are not, the men and women often differ in their education, work experience, or job tenure.

Our study contributes to the intensive policy debate on firm-level policies to close the gender pay gap. We use comprehensive employer-employee data to estimate the impacts of a policy type – firm-level mandated actions to create transparency and/or to close pay-gaps – which is currently gaining attention among policy makers around the world. Equal pay for equal work is also part of the European Union's founding principles, and the European Commission encourages pay transparency policies, such as producing wage statistics, in the Strategy for Equal Pay.[1] Sweden's long experience with this policy lets us draw lessons for other countries. We do this not only via our quantitative analysis, but also by giving a broad and thorough account of insights gained by the Swedish government over years of policy evaluation and development. This account is of independent interest, but also lets us relate our findings to the broader insights about compliance, enforcement strategies, and policy formulation.

We argue that policies that require firms to produce gender-based wage statistics, such as the regulations in Denmark and Norway, are unlikely to give much impact on the gender wage gap. Policies including more detailed methods for computing wage gaps have the advantage of defining illegal wage gaps, such as the government-provided Logib tools in Switzerland, Germany, and Luxembourg. These policies might be more efficient, but only if implemented in ways that incentivize meaningful rather than shallow implementation, and with a sanctioning mechanism that does not mainly rely employee or unions.

This paper is structured as follows. We begin by discussing theoretical points of departure. Thereafter we describe the policy and our sources of variation for causal identification. In section 4 we describe our data and in section 5 we describe how we use it to estimate gender wage gaps in a way that comes as close as possible to the gaps targeted by the policy. Section 6 describes our empirical approach. Results are then presented in Section 7, followed by a number of sensitivity checks in Section 8. Section 9 addresses the question of the policy's *potential* impact on the aggregate gender wage gap. The last section contains our conclusions and a broader discussion of the findings based on insights from previous evaluations by Swedish government agencies.

[1] For example, in 2007 the Danish government mandated firms with 35 employees or more to provide gender-divided workplace statistics. In 2017, Iceland introduced a policy similar to the Swedish one, with firm-level wage surveys actively monitored by the government in the first implementation phase, followed by the possibility of sanctions for non-complying firms if reported by employees or unions.

2. Theoretical points of departure

The purpose of the wage survey policy is to close two discriminatory gender wage gaps at the firm-level. One gap arises when an employer pays women (or men) less for the same work despite having the same qualifications, so-called within-occupation-establishment wage discrimination. The other gap arises when women with equal qualifications are paid less because they do work that is more commonly done by women, so-called valuative discrimination (Petersen and Morgan 1995).

A wage survey can be perfectly legal and not adjust any discriminatory pay gaps. A main reason for this is the flexibility in determining the list of jobs within the firm, and which employee should be assigned to each category. Another is the flexibility in accounting for wage gaps with "justified" criteria. The law is not specific on these criteria and no penalties or sanctions can be imposed for an analysis that dismisses actual discrimination as justified. For these reasons, the mechanisms that link the wage survey to reduced discrimination are more complex than simple compliance with the letter of the law.

There are two main reasons that the wage survey could be expected to reduce pay gaps. First, the employer may be unaware of any discriminatory gender wage gaps and achieve more awareness of these gaps from the wage documentation and analysis. Unconscious discrimination could arise from implicit bias (Bohnet 2016) or from taste preferences of which the employer unaware (Becker 1957 (1971)). This implicit bias may be based on gender stereotypes about competence, leadership abilities, motherhood, or preferences for work hours.[2] For valuative discrimination, implicit bias may operate at the level of the job, where work done primarily by women is perceived to be less challenging or difficult (see also Goldin 2016 for an economic theory about the sources of valuative discrimination).

A second mechanism is the wage transparency created by the wage survey document.[3] Done correctly, it informs employees, trade unions or other parties involved in the wage setting process about both wage levels and wage-setting procedures. Such information can be turned into a bargaining tool by these actors (Kim 2015). Notably, recent work has tied gender wage gaps at the firm level to differences in bargaining

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[2] Stereotyping happens as people make the "cognitive shortcut" of recalling the most distinctive types from a group rather than the average type (Bordado et al. 2016).

[3] the argument that transparency increases female bargaining power is articulated by the European Commission, that encourages EU member states to implement policies strengthening wage transparency on the company level (European Commission 2014).

power (Card et al. 2015) and to employer responses to women's and men's bargaining attempts (Säve-Söderberg 2015). Cross-regional analysis has also linked the introduction of pay secrecy bans to smaller gender pay gaps (Kim 2015).[4]

Apart from its' impact on bargaining, transparency can create accountability. Bohnet (2016) asserts that documentation of workplace inequality will be more efficient if the employers can be held accountable for misconduct, tying together transparency and accountability (Hood 2010). In a principal-agent framework, the principal need to have enough information about the action of the agents, to be able to hold the agents accountable. Applied to the wage surveys, a written document may serve as this focal point for accountability.

The surrounding society can also function as an enforcing agent of the wage survey mandate. Pay transparency creates pressure on firms to avoid the label of discriminating women (Kim 2015), corresponding to reputational social motives for compliance (Ayers and Braithwaite 1992). There are, however, several reasons to believe that this mechanism has limited importance in the case of the wage survey policy. Even if ignoring the pay survey might have a reputation cost for the firm, the reputational gain from compliance might be low since the mandate covers most employers (e.g. Benabou and Tirole 2006). More importantly, producing a survey that (correctly) identifies discriminatory pay gaps exposes if not only immoral, but also illegal behavior. Within-job discrimination as well as valuative discrimination is illegal by the Swedish Equality Act (2001, §2).

The efficiency of a regulation can also be judged by theory on regulatory design and enforcement methods. The literature on policy implementation normally ranks the enforcement strategy of a policy along a scale ranging from a hierarchical/legalistic to a flexible/conciliatory style (Ayres and Braitwaite 1993, McAllister 2010, Hood 1998). The first extreme is associated with a "command and control" structure, where the regulators closely monitor the compliance of the regulates and where penalties are imposed on non-compliers. The second is a strategy of persuasion and education.

At the most basic level, a business will comply with a regulation if the probability of compliance costs plus the probability of detection and sanction, interacted with the size of the sanction, is greater than the benefits of noncompliance (Becker 1968; Stigler 1970). But sanctions can also have costs for the enforcer. It is costlier than to induce

[4] In most US firms, employees are not allowed to discuss their wages as employer fear workplace conflicts. Kim (2015) claims that pay secrecy is such a strong norm that it could be classified as a law.

voluntary compliance, in particular if sanctions are needed over a long period of time, if there is back lash against regulations, or if the sanctions deteriorate the relationship between the principal and agent (Ayres and Braitwaite 1993).

Sanctions could also have more wide-ranging impact on the agent, outside of the monetary and reputation costs. While these rules create an incentive to comply, they could crowd out intrinsic incentives. In the case of the wage survey, this could happen both in the mind of the employer or in the practical organization of work with gender equality issues. Employees or other actors engaged in gender equality work could be told to do different things than their original, and perhaps better firm-suited, efforts. To comply perfectly, a firm could hire a consultant and further crowd out employee initiatives.

Since the gender wage survey is meant to make the firm contribute toward a public good – gender equality – the trade-off could be expected to be even starker (Gneezy et al. 2011). Monetary incentives, positive or negative, could change the way that the task is perceived. Small incentives could make the employer perceive the task (to create gender equality) as less important, perhaps even less important than the initial belief. Large penalties could have a positive impact in the short run and may also be more efficient in changing social norms about the importance of the task. But it could also increase the indirect motivational effect. Large penalties (or rewards) might signal to the agent that the task is difficult, unattractive, or that the agent is not well-suited to perform it. They could also signal that the principal does not trust the agent's intrinsic motivation.

The more flexible way of implementation focuses on trust and cooperation between the regulators and regulatees (McAllister 2010). A flexible enforcement regime is a smaller threat to intrinsic motivations. The task may also be carried out more efficiently when it can be closely tailored to, in this case, the agent's organizational structure and internal wage-setting procedure. But flexible enforcement also risks at least two pitfalls. Flexible policies may be vague and thereby raise the risk of "window-dressing"; where a firm will set up symbolic strategies to gain credibility as an employer, but without actual implementing the intention of the lawmaker (Edelman et al. 1991, Fredman 2012). Vague policies are also difficult for the principal to make concrete enough, with low information reducing the chance that agent – there the business – has enough knowledge about the law or technical know-how to behave in the desired way (e.g. Winter and May 2001, Benabou and Tirole 2006).

While flexible enforcement may make the firm's implementation more tailored to the organization, it could also become inefficient – partly for the reason of poor information. The agent is unable to produce the desired outcome due to lack of understanding of key concepts or procedural strategies. This can also produce the sensation that the task has been accomplished when in fact, it has not. A sensation of mission accomplished on weak grounds can create the problem of "moral licensing", a reduction in actions that could be important for gender-equal wages based on the perception that the relevant actions have already been taken through the production of the wage survey.

3. The regulation and identifying variation

The mandate for organizations to produce written wage surveys was introduced by a 1994 amendment to the Equal Opportunities Act. This section describes the development of the mandate over time. While the required content of the wage survey has remained largely constant, there has been some crucial clarifications of definitions, stronger requirements for actively adjusting wage gaps, and stronger wordings on transparency. These changes underpin our identification strategy and are outlined below, after a summary of the mandated analytical process and content of the survey document. We also summarize key details on enforcement, information, and sanctions.

Content of the wage survey The Swedish law regulates both the content of the wage survey and the work process used to obtain that content. As a complement to the description of these regulations we provide an example of a survey document in Appendix Section A1.

A wage survey must start with a description of the wage setting practices in the firms. This includes the criteria for judging employees' merits, the use of bonus systems, wage additions, or criteria for categorizing employees in hierarchical levels of wage-related groups. The core of the survey consists of a three-step work process for analysing the gender wage gaps for "equal" work and, separately, for "work of equal value". The three steps are to (1) map wages, (2) analyse the sources of any gender wage gap, and (3) adjust any unwarranted gap.

First, the surveyor studies equal work, departing from a list of jobs in the firm. For each job, the surveyor produces summary statistics for the wages of men and women, usually means, medians, and standard deviations (Appendix Table A1). Naturally, a gender-wage gap can only be calculated for jobs with both men and women employees. Having computed the wage gaps, the surveyor must scrutinize if the entirety of each gender wage gap is warranted or not (Appendix Table A2). This should be done regardless of whether it is women or men who earn less. A non-warranted gap stems from reasons that are directly or indirectly linked to gender (Kumlin 2016). A warranted – or "factual" – gap stems from formally agreed criteria that have been applied in a consistent and systematic way. The law does not list any specific criteria that make for permissible gaps, but enforcement agencies tend to consider tenure, work experience, education, task composition, or productivity as justified explanations (Kumlin 2016).

If some proportion of a gender wage gap is unwarranted, it must be adjusted. The size and time line for the wage adjustments must be outlined in a section called the "Action Plan for Equal Wages" (Appendix Table A4). It is more common that firms adjust the wages of the disadvantaged group upward rather than lowering the wage or slowing the wage growth for the advantaged group (JämO 2005, Kumlin 2016).

After the analysis of equal work, the survey carries over to work of equal value. Departing again from the list of jobs, these are grouped together in categories. The law requires, in particular, that the average wage in female-dominated jobs is compared to the average wage in male dominated jobs of equal value.[5] Since 2001, the law defines work of equal value as work that requires the same knowledge and skills, responsibility and effort, and has the same working conditions. Within the groups of jobs with work of equal value, women's and men's average wages are compared to see if there are unwarranted differences (Appendix Table A3), and any adjustments are added to the Action Plan.

Treatment intensity by firm size and time The 1994 wage survey mandate applied to firms with 10 or more employees. But it was not efficient. It was not until 2001 that the regulation took the shape that was "initially intended" by the lawmakers (JämO 2003, 2005). The 2001 reform strengthened the mandate in four important ways, motivated by the finding that the 1994 mandate had been largely ignored by firms, trade unions, and employer organizations (JämO 2003).

[5] To help firms make this judgment, the government produces a computer program *Analys Lönelots*, that could be used by the firms. The program helps the surveyor to judge the demands of an occupation according to four parameters: knowledge, skills, responsibility and effort, parameters that are accepted by the parties in the labor market

First and foremost, the reform provided much-needed clarification of the term "work of equal value" (§2 JL).6 Firms struggled with understanding how to practically implement this key part of the mandate (Edelman et al. 1991, JämO 2003). Even with ample time to provide a legally correct documentation to evaluators, and despite the threat of fines, only 20 percent of firms were able to provide any documentation relating to this legal provision to the enforcing agency (JämO 2003).

A second change introduced the rule that unwarranted pay gaps had to be adjusted within three years (§11 JL). A third (set of) changes were aimed at strengthening labor unions' participation in regulatory enforcement. Businesses became required to cooperate with the relevant union in designing active measures to combat gender inequality (in the Action Plan for Equal Wages), and to share "whatever information necessary" with union representatives to facilitate their participation. A fourth change afforded the enforcement agency with the right to make workplace visits to check compliance (§33 JL). Despite this change, the law still suggests the agency to encourage voluntary enforcement before imposing any punishment upon the employer (§31 JL).

The 2001 regulatory changes were followed by a massive facilitation effort (described in JämO 2005, 2007, 2008). In 2002, 10 000 firms were warned about a randomized evaluation, and 500 firms were given ample time to submit their wage survey. These documents were checked by bureaucrats in repeated "revise-and-re-submit" style rounds, until all were in full compliance with the law. In 2004, another 40 firms were evaluated in this way, and 539 firms followed in 2007—2008.

The facilitation effort included several tools to help businesses to voluntarily comply with the law. A motivational leaflet was also distributed. A telephone hotline and face-to-face counselling service was set up. Thousands of CEOs and union representatives attended courses. An online tool, Analys Lönelots (Analysis Wage Pilot) was developed to, in particular, help businesses determine categories of jobs with equal value. The tool also included a sample framework for a wage survey.[7] To encourage

[6] The definition reads as "*A job is to be regarded as equivalent to another job if it based on an overall assessment of work-specific requirements and nature can be considered as equivalent to the other job. The assessment of the requirements the job shall be made with consideration to criteria such as knowledge and skills, responsibility and effort. In assessing the nature of the work, working conditions shall particularly be taken into account.*" (§2, the Equality Act)

[7] Analys Lönelots was downloaded from the agency web site 900-1400 times per month during these years (JämO 2007). In a 2008 survey, 32 percent firms reported using the program (JämO 2008)

cooperation with union representatives, requests for documentation were consistently sent to both the CEO and the union representatives of the firm's largest union.

In 2009, the regulations were again re-written when the new Discrimination Act (DA 2008: 567) replaced the previous law. To reduce the regulatory burden for small firms (Prop. 2007/08:95, p. 399), firms with between 10 and 25 employees became exempt from the mandate of written wage surveys. For firms with more than 24 employees, the periodicity of the survey was dropped from annual to triennial. The triennial planning was expected to be equally efficient in reducing wage gaps since it would facilitate more long-term planning (Prop. 2007/08:95, p. 338). Interviews had also shown that many businesses were already doing major revisions to their wage survey on a three-year interval rather than every year (SOU 2010:7).

The legal changes in 2001 and 2009 creates two thresholds in firm size with varying applications of the wage survey mandate. The 2001 threshold introduced (meaningful) and annual wage survey mandate in firms with more than 9 employees. The 2009 threshold dropped this mandate for firms with 10-24 employees, while keeping it for firms with 25 employees or more. These changes in thresholds and content are summarized in Table 1 below.

Gender wage plans and the raw wage gap The gender wage survey is couched in a broader set of requirements for organizations to actively work to improve gender equality. A gender wage plan mandate was introduced in 2001, on the same firm size threshold as the gender wage survey. According to these regulations, businesses must list actions to be taken to improve gender equality in the following five areas: 1. create employment conditions that are suitable for both women and men; 2. facilitate the combination of gainful employment and parenthood; 3. prevent sexual harassment; 4. promote an equal distribution of men and women across types of work and work categories; 5. hire women for jobs where they are underrepresented in the firm. The plan should also contain a summary of the action plan for equal wages from the gender wage survey.

Together with the gender wage survey, the gender equality plan could be expected to affect the raw wage gap. This could stem from any adjustments to parental leave policies, working conditions or promotion patterns (following e.g. Goldin 2014, Angelov et al. 2016, Kleven et al. 2018). Notably, however, the mandate for the Gender Equality Plan has only been the subject of very minor facilitation efforts by authorities and will not be further discussed from that perspective. No penalty or legal case has been brought against any organization based on this mandate.

Table 1: Mandated wage survey content and documentation requirements.

	A. Mandated content, regardless of firm size		
	1994-2000	2001-2008	2009-2015
	Review wage setting criteria. Map, analyze, and adjust gender wage gaps within jobs. Map, analyze, and adjust gender wage gaps in groups of jobs of equal value.	+ Definition and procedure for analyzing jobs of equal value. + Unjustified gender wage gaps must be corrected within three years. +Cooperation/information sharing with the unions	No changes
Size	B. Documentation requirements		
5-9	None	None	None
10-24	Annual	Annual	None
25+	Annual	Annual	Trennial

Enforcement and implementation From 1994 and to present day, the Swedish government's enforcement strategy for the wage survey mandate has been to encourage voluntarily participation (SFS 1991:1438, Prop. 2015/16:135). A specific goal has been to create gender equal wages without court conflicts. Soft persuasion, facilitation and education have been the key methods applied by the regulating agency. First and foremost, organizations have been given tools, information and hands-on instruction to facilitate their full implementation of the law. Arguably, this strategy rhymes with the content of the regulations, which do not prescribe particular wage adjustments, analysis methods for judging the gender wage gap, or what adjustments to wages must be made. Unless businesses have an intent to correct gaps, the regulation clearly would not cause them to do so.

Punishment methods have essentially not been used. No system for public shaming has been developed, as businesses (at most) need to share the wage survey document with employees and trade union representatives. Workplace visits have also not been used. Sanctions and criminal persecution have been used very sparsely, although it is theoretically possible for a firm to be fined (§35 JL, §5 4 kap DL). Even after the strengthened rules in 2001, only 13 applications were submitted by the regulatory agencies or by trade unions in the first five years, where of two by unions (JämO 2005, Fransson and Andersson 2006).

Government evaluations have suggested that compliance has developed gradually over time and reached a high level in more recent years. In a survey of 6,000 firms with a 50 percent response rate in 2005, 30 percent reported that they had a wage survey (JämO and SCB 2005). In 2008—2010, all firms among 240 responding firms (33 percent response rate) reported doing regular wage surveys, and 87 percent reported to have an Action Plan for Equal wages (Stadskontoret 2011). By the mid-2010s, all firms in a mid-size sample of 100 firms, and 62 percent of a sample of small firms (25—50 employees) reported to have a survey (DO 2016).

The regulatory agency has repeatedly argued that the survey mandate is not more burdensome or complicated for a small than a large business (e.g. JämO 2005). Some firm-level surveys of compliance have found lower compliance rates in small firms (Unionen 2015), but in combination with larger average wage adjustments and a higher probability of having informed employees about the survey document (JämO and SCB 2005, but no correlation between compliance and firm size in JämO 2008).

4. Data

Our analysis is based on several datasets from Sweden's administrative records. The main data source is the annual survey on the Swedish salary structure (Lönestrukturstatistiken). This survey collects information on monthly wages in a random sample of the private sector. Firms must report the wage for every employee with at least one work hour in the sampling week in September. No reporting is required for people outside the 18—66 age interval, interns and trainees, people on probationary work, disability training programs, or volunteers. The coverage of the survey is a close approximation of the employees that should be covered by the wage survey, namely all employees regardless of contract type and working hours (JämO 2003). The wage variable used is capturing the monthly wage in full-time equivalents, including contracted bonuses and piece rate payments[8]. The law does not explicitly state which measure of wage to be used in the wage survey.[9] Analysis of how firms choose to

[8] Extra (not contracted) payments from for example working shifts and inconvenient hours are not included. Using an alternative measure including such additional payments, did not qualitatively change our main results, but generated slightly higher wage gaps. The trend in the wage gap development over time did not change using this alternative wage measure.

[9] If the firm only analyzes basic pay in the wage comparison, a correct application of the law requires separate and additional analysis of other wage components (JämO 2005, p. 47).

measure wage show that there is large variation across firms in this regard (Kumlin 2016).

The wage structure statistics is stratified by firm size, giving us an unbalanced panel of firms for each year in 1996—2015. In each year, coverage is about 2 percent of all private sector firms and 50 percent of all private sector employees. Further discussion of the stratification and its' role for our analysis is provided in Section 9.

Socio-demographic variables are obtained from the Longitudinal Integration Database for Health Insurance and Labour Market Studies (LISA by Swedish acronym, for a detailed description see SCB (2016)). These variables can be linked to the wage data via the (encrypted) personal ID code, which is mandatory to all permanent residents in Sweden. LISA includes annual observations for the full population of adults (18+) and is not limited to the stratified firm-sample. Variables include education, industry, occupation, sex, and age (but not the monthly wage). Because LISA only includes the three largest sources of labor income, we add data for all labor incomes above 99 SEK (10 dollars) from the job-registry (Jobbregistret), which is available since year 1990.

The dataset spans the period of 1996—2015 with a total of 22 306 769 individual-year observations of private sector employees; whereof 1 252 717 individual-year observations of employees in firms with 5—50 employees. Out of these employees, 0.8 percent cannot be matched to information about their labor market experience.

5. Simulating the gender wage survey in register data

To evaluate the efficiency of the wage survey mandate we want to create outcome variables that come as close as possible to approximating the policy's intended targets. We want to extract the wage-variation between equally qualified men and women doing equal work and – separately – work of equal value. This section explains how we compute these two gaps for each firm in each year. A third outcome variable is the raw wage gap, calculated simply as the absolute value of the difference between the average wage of men and women in the firm.

Gender wage gaps for i) equal work and ii) work of equal value are calculated in a wage regression with a female dummy as well as a fixed effect structure to account for "warranted" wage differences. The regression is not used to estimate the wage gap from a standpoint of statistical significance. What we are interested in is simply the quantitative size of the wage gap after adjusting for warranted wage difference. Using the log of the monthly wage in full-time equivalents as the outcome variable, we use OLS to estimate:

$$\log(wage)_{it} = \beta_f F_i * \alpha_j * \alpha_t + (\alpha_{job} * \alpha_{educ} * \alpha_{experience} * \alpha_{tenure} * \alpha_j * \alpha_t) + \varepsilon_{it} \quad (1)$$

where the coefficient β_f on the female dummy F_i is the gender wage gap – our quantity of interest. The interacted fixed effects ($\alpha_{job} * \alpha_{educ} * \alpha_{experience} * \alpha_{tenure}$) restrict the wage differences to men and women in the same job and with the same categorical value of each of the characteristics (further discussed below). The fixed effects for firm and year ($\alpha_j * \alpha_t$) means that the gender wage gap β_f is calculated as the average of these wage difference in each firm and each year.

We use two different categorizations of jobs to match the concepts of "equal work" and "work of equal value", denoted α_{job} in Equation (1). Starting with equal work, the law does not prescribe a list of jobs to form the basis for this comparison but leaves the construction of that list up to each firm in order to make the analysis flexible to its' particular organization. We approximate the list of jobs with the Swedish occupation code (SSYK). This code is highly similar to the ISCO-88 code and captures both horizontal and vertical occupation categories.[10] During the years 2002-2005[11], the Swedish occupation code became more detailed, going from three to four digits. In the main analysis we use the three-digit level (355 occupations), but also report results for the four-digit level (429 occupations) regarding the 2009 policy threshold as a sensitivity analysis (see Appendix Section A8, table A41). Concerning the computation of the policy scope (see Section 9), both the three and four-digit levels are applied.

For work of equal value, the 2001 reform clarified the definition as jobs that are equally demanding in terms of five factors: i) educational requirements, ii) problem

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[10] The SSYK codes are constructed based on four levels. The first digit corresponds to occupational group, for example "manager" or "service occupations". The rest of the digits provide more and more fine-tuned categorizations of an individual's occupation. The classifications are done based on the concepts "work" and "skills". "Work" is referring to which work-tasks an employee is expected to do. An "occupation" refers to which *type* of work that is being done. "Skills" refers to the knowledge needed to perform an occupation. Skills are measured by comparing work tasks, average length of education and how much informal training and work experience that is needed. Since the updated system SSYK 2012 was not implemented until 2014, only two years of our 1996-2015 dataset includes the updated categorization.

[11] The implementation of the four-digit SSYK codes was done at a differing pace across sectors, meaning that some occupations will have four-digit codes the years preceding 2005 (2002-2004). In 2005, the four-digit codes were fully implemented.

solving capacity, iii) social skills, iv) responsibility and v) working conditions and environment (§2JL; Lönelotsarna 2017). To produce a variable for the value of jobs we contracted with the experts who developed the government's tool for work valuation (*Lönelots*). These experts evaluated 369 four-digit jobs in the *Lönelots* program, covering all non-managerial jobs in the Swedish occupation code.[12] Managerial jobs were excluded due to the difficulty of reliably judging job content. Excluding these jobs from the wage survey is wholly in accordance with the practical implementation of the policy, where managerial jobs usually lack other jobs of "equal value" within the firm (see e.g. JämO 2005).[13] In doing the valuations, the experts relied on vast expertise from decades of theoretical and empirical work on work valuation, including intimate knowledge of the Swedish wage survey policy. The precise methodology for these valuations is further described in Section A2 of the Appendix.

The continuous variable for job valuations is cut into 11 categories of jobs with equal value and following a methodology previously applied by the Swedish government (SOU 2015:50). We then use the values for the 4-digit occupation codes to calculate the mode category in the 3-digit occupations. The analysis of equal valued jobs relies on the 4-digit values when available, otherwise the mode of the job valuation categories is used. To assess the magnitude of the measurement error arising due to the approximation using the 3-digit values, we compute the share of individuals "changing" occupational category when moving from a 3-digit to a 4-digit definition. Here we use the year 2005, which is the first year for which we have 4-digit occupation codes for the full sample. The calculations show that 75.2 percent of the individuals did not change occupational category, and 87.2 percent did not change or changed category to the nearest category (e.g. changing from 10 to 11 or 10 to 9).

Within job categories, the wage survey analyses if any gender wage gap can be justified with objective criteria. Can the entire gap be justified or not? As previously discussed, Swedish law does not provide a definition of permissible justifications. We follow the praxis discussed in Section 3 to include education, labour market experience,

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[12] In the software, the surveyor judges each job along the dimensions ascribed in the law. The software splits these up in five areas, whereof education requirements are given by the occupation code, and other values are ascribed by the surveyor in the categories of i) responsibilities for material and human resources; ii) education, iii) demands on creativity, independence and development at work; iv) social skills (of intercultural understanding, communication, cooperation and service-mindedness), and v) working conditions and working environment. Scores on these five dimensions are weighted together to a single index to reflect what is valued in wage setting processes on the Swedish labour market (Lönelotsarna 2017).

[13] Using the fixed effect structure in Equation (1), we can compute how many managers that are alone in their occupation-qualification cell, which turns out to be 81.4 percent if the managers in our sample.

and job tenure. Unfortunately, we cannot include any measure of job performance, an omission that is likely to bias our analysis toward finding larger unexplained wage gaps and a larger scope of the policy, since productivity is often used by firms to motivate men's higher wages in a job or job category (JämO 2005, Kumlin 2016).

The three variables education, labor market experience and job tenure are included in Equation (1) as dummy variables for categories within each variable.[14] These dummies are interacted with each other, denoted in Equation (1) as $\alpha_{\text{educ}} * \alpha_{\text{experience}} * \alpha_{\text{tenure}}$. The structure of the estimation equation hence follows the work process of the gender wage survey by investigating the sources of wage gaps separately within each category of equal or equal-value work. In other words, education, experience or tenure are used to remove warranted wage differences between men and women in each category of α_{job}. The fraction of a gender wage gap that should be "left" after the comparison is that between people in the same job and with the same qualifications.

Note that the gender wage gap calculations ignore wage differences across jobs, for example if a woman with higher education in one job is paid less than a man with lower education in another job. This is entirely in accordance with the law up to 2017, when an amendment added an obligation to also consider such cross-job differences in the wage survey (Prop 2015/16:135). In the sensitivity analysis (Section 8) we relax the calculation of the gender wage gaps to also include wider comparisons of qualifications between jobs in the firm, and also between firms and jobs in 2-digit industrial sectors.

Job and labor market tenure are measured with some error. In both cases we make use of labor income levels to classify a person as employed or not. This income data is available from 1990 and onward. A person with an annual labor income exceeding one Price Base Amount is counted as being employed in that year. A person is counted as employed in the same workplace if he or she received labor income in excess of one Price Base Amount from that same firm. This method truncates both experience measures in 1990. For labor market experience, we add the number of years between 1990 and the person's graduation year from their current education, hence assuming that the person worked in each year between graduation and 1990. For people without

[14] Education is measured as three categories: less than primary education (5.73 percent); primary or secondary education (64.31), or at least some college education (29.96 percent). Labor market experience is classified in five-year intervals of 0—5, 6—10, 11—15, 16—20, and 21 or more. Job tenure is classified as 0—2 years, 3—5 years and 6 years or more.

a recorded graduation year (40 percent of the total sample), we instead add the number of years since the person's 20th birthday – as a proxy for graduation year.[15] This method could give some measurement error from overestimating women's labor market participation. But given Sweden's high level of labor force participation among women, exceeding 70 percent in the late 1970s (SCB, 2011), the lack of labor income data prior to 1990 is less of a problem than in a country with lower participation rate (see e.g. the discussion on measurement error in Kuntze (2017)).

After estimating Equation (1) with OLS for each firm and each year, we save the coefficients of β_f and collapse the data to the firm-year level. Because the gender pay survey is meant to adjust discriminatory wage gaps for either men or women, we use the absolute value of this variable (we later adjust the calculation of the wage gap to only include situations where women are underpaid, see Section 8). We clean the gender wage gap variables by dropping outliers according to the "extreme outlier rule", which defines outliers as observations that lie more than 3 inter-quartile ranges above or below the top and bottom quartile.[16] In the cleaned variable, about 29 percent of the firm-year observations have equal jobs and 32 percent for equal value work, have gaps where men are underpaid rather than the women.

Table 2 shows descriptive statistics on number of firm-year observations for the three periods studied (columns 1-3). We also report what proportion these sub-samples represent relative to the full sample of firms in each particular size category in the salary structure database (column 4). Last, we report the means and standard deviations for the three gender wage gaps (columns 5 and 6). The Appendix Section A3 provides a complementary description of the distribution of the control variables. Table 2 illustrates how the share of the firms in the sample for which we can estimate a wage gap, in pooled data across all three time periods, declines dramatically when we move from estimating the raw wage gap to the wage gaps within equal or equal value work. This drop in N is particularly large for the smallest firms (5-9 employees). While we can calculate a raw wage gap for 75 percent of the small firms in the full sample (1996-2015), we can only estimate an equal work wage gap for 15 percent. Raw wage gaps cannot be calculated for single-sex workplaces. The wage gaps for equal or equal

[15] For example, if the income measure gives a labour market experience of 25 years from 1990, and the individual graduated in 1985, his or her total labour market experience will equal 30 years.

[16] For the three time periods studies, the cut-off for the raw wage gap equaled 0.73, 0.66 and 0.64 and 99, 323 and 284 observations were dropped. For the equal jobs wage gap, the cut-off lands at 0.49, 0.48 and 0.48 and 91, 227 and 228 observations are dropped. Corresponding numbers for the equal value wage gap is 0.51, 0.48 and 0.47 and 106, 261 and 262.

valued work relies on the existence of at least one job with a male and a female employee with the same qualification-category combination. This practical restriction across and within firms of the employees who can become subject of wage comparisons have been duly noted by Swedish enforcement agencies and evaluators and is further discussed in Section 8.

Table 2**:** Sample size and descriptive statistics for gender wage gap variables.

	Time period (# obs)			Prop. of full firm sample	Gender wage gap	
	1996 — 2000	2001 — 2008	2009 — 2015		Mean	Std. dev.
	(1)	(2)	(3)	(4)	(5)	(6)
5-9 Employees						
Raw wage gap	3,120	7,010	4,247	0.75	16.5	13.6
Equal work wage	574	1,344	997	0.15	9.6	10.3
Equal value wage	667	1,524	1,128	0.17	9.8	10.1
10-24 Employees						
Raw wage gap	5,899	14,461	9,130	0.90	14.3	12.4
Equal work wage	2,325	5,767	4,495	0.38	9.6	9.9
Equal value wage	2,747	6,780	5,048	0.74	9.8	9.8
25-50 Employees						
Raw wage gap	3,165	7,319	6,128	0.97	13.0	11.3
Equal work wage	2,272	5,495	5,072	0.75	8.8	8.6
Equal value wage	2,489	5,965	5,280	0.80	8.8	8.6

The raw gender wage gap is the largest in all size categories, and slightly larger in the smallest firms. The wage gap within equal work is smaller, below 10 percent, while the wage gap for equal value work is only slightly larger and has the exact same value in the 25—50 employee category.

6. Methodology and identification

We apply two methods to test the impact(s) of the wage survey mandate. First, we examine the variation across firm size categories over time, both graphically and in an interaction model on the repeated cross-sectional data. Second, we use a Regression Discontinuity approach to compare firms across the firm size threshold and in pooled cross-sectional data for the pre- and post-reform periods. This section explains each of these two approaches regarding the identifying assumptions and possible threats to identification.

Following the policy variation described in Table 1, we use two policy thresholds for identification. The first reform is the changes made to the wage survey mandate in 2001, where the treated group are firms with more than 9 employees, and where the smallest firms (5—9 employees) form the control group. The second reform is the changes made in 2009, which exempted firms with 10—24 employees from the wage survey mandate. Here, we use the firms with 25-50 employees as our treatment group, since they are the ones that are remained "treated" in the sense of being required to have a wage survey. Firms with less than 24 employees make up the control group.

Time variation We examine the impact of the wage survey mandate by comparing the pre-and post-reform trends in the gender wage gap variables in the treated and control group(s) for each policy threshold. This is similar to a difference-in-difference approach, although with variation across a random sample of firms over time, rather than within firms.

As a descriptive analysis, we plot the development of the gender wage gaps. Then, we estimate the following interaction model at the firm-level, *j*

$$\mathrm{wagegap}_{jt} = \gamma D_{jt} + \beta_t (D_{jt} * T_t) + T_t + \varepsilon_{jt} \tag{2}$$

where D_{jt} is a dummy variable for the treatment status of firm *j* in time *t*. The vector of coefficients β_t captures the treatment-control difference in the average firm-level wage gap for each year compared to a chosen benchmark year. The year-dummy for the year immediately before the threshold is dropped to set this year as the benchmark year (2000 for the 2001 threshold, and 2008 for the 2009 threshold). The results are presented by plotting the interaction coefficients β_t. As a sensitivity analysis we show estimated coefficients with and without control variables for region and 2-digit industry codes (see Appendix Section A4).

The plots of the time-trends and interaction estimates both let us assess whether treated the outcome variable developed in similar ways in the treatment and control group in the pre-treatment period. To some extent, this lets us assess if the reform was pre-dated by time trends in the treatment-control difference that could confound the treatment effect.

Cross-sectional RD analysis We apply a Regression Discontinuity Design to estimate the difference in firm-level gender wage gap(s) between treated and non-treated firms. This design compares firms that are have similar values on the variable that assigns treatment, namely firm size. In our case, the mandate to produce the gender wage survey is a deterministic function of firm size, where the survey becomes mandatory exactly at the cut-off. For the 2001 reform, the cut-off value for treatment is 10 employees, and for the 2009 reform the cut-off is 25 employees. Treatment is a discontinuous function of firm size, so that the treatment status will not change until we reach the cut-off, independent of the distance to the cut-off.

The idea behind the RD identification strategy is that firms close to these thresholds are similar in size and hence similar to each other in all respects but their treatment status. Differences in the gender wage gaps between treated and un-treated firms close to the thresholds should hence be a consequence of the policy rather than other firm-level characteristics. While the forcing variable can be associated with the outcome variable, this correlation is assumed to be smooth, implying that any discontinuity of the conditional distribution of the outcome as a function of the forcing variable can be interpreted as a treatment effect.

We run a standard set of RD specifications using each of or three firm-level wage gaps (equal jobs, jobs of equal value and raw) as outcome variables. The simplest specification compares the mean of the outcome variable for the observations close to the treatment threshold (local randomization). The regression equation is simply

$$\mathrm{wagegap}_{\mathrm{jt}} = \delta \mathrm{D}_{\mathrm{jt}} + \varepsilon_{\mathrm{it}} \tag{3}$$

where D_{jt} is the dummy variable for treatment with the wage survey mandate. Next, we add linear control functions in firm size to regression (2). By centering these control functions at the threshold (i.e. setting the cut-off value to 0) we can directly interpret the coefficient on the dummy variable δ as the discontinuity of the two estimated intercepts precisely at the reform threshold. Equation (3) is specified as

$$\mathrm{wagegap}_{\mathrm{jt}} = \delta \mathrm{D}_{\mathrm{jt}} + \beta_1 \mathrm{firmsize}_{\mathrm{jt}} + \beta_2 (\mathrm{firmsize}_{\mathrm{jt}} * \mathrm{D}_{\mathrm{jt}}) + \varepsilon_{\mathrm{it}} \tag{4}$$

where $\mathrm{firmsize}_j$ is the linear control for the relationship of firm size and the firm-level gender wage gap. Our forcing variable (firm size) is discrete and has few mass point, that is, it has few values to each side of the threshold. As explained by Cattaneo et al. (2017), local randomization specifications are the most reliable in this case, while local polynomial specifications are not applicable. The small number of values also makes it redundant to apply standard methods for selecting the estimation window around the threshold. We use the windows 1, 2 and 4 employees for the local randomization estimations, and 3, 5 and 8 employees for the specifications with local linear control functions. Additional "doughnut" specifications are estimated as part of the sensitivity analysis (see Appendix Section A8 Table A38).

Manipulation of the forcing variable Manipulation or confounding selection of the forcing variable can violate the assumption of local randomization around the RD threshold (McCrary 2008). This would happen, for example, if firms manipulate the number of employees to avoid the mandate. After manipulation, firm can no longer be assumed to be "as good as randomly" distributed around the RD threshold(s). Depending on how firms sort, this could result in either upward or downward bias of the results.

To examine sorting around the threshold we plot the distribution of firms and inspect the distributions for each of our time periods for any "jumps" (Figure 1). Because the sampling of the salary structure statistics uses the same cut-off values as our treatments, there are automatic discontinuities in that dataset.[17] We instead use the LISA dataset, which covers all firms and thereby averts discontinuities caused by sampling methods. An employee is included in LISA if he or she worked more than four hours at the firm in the month of November.[18]

The distributions in Figure 1 shows no jumps in the number of firms around the thresholds. Running the McCrary test for discontinuities in the number of firms at the thresholds show no significant bunching in three of the four pre-post samples (see Appendix Section 5 Figure A1). For the 2001 threshold in the 2001—2008 sample, the coefficient is significant, which most likely stems from the sharp decline in the distribution of firms for the smaller firm sizes. Running the McCrary test for each firm size from 6 to 14 employees shows that we can reject balance for every "placebo" threshold from 6 up to 10 on the 5 percent level, and up to 12 employees. Results from the supplemental test for discrete forcing variables (Frandsen, forthcoming) are shown in Appendix Section 5 Table A11. The test cannot reject the null of no bunching for any time period at "rule-of-thumb" test specifications.[19]

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[17] The wage structure survey samples a substantially larger number of firms with 4—9 employees than firms with 10-50 employees. This is not a problem for our identification strategy but does not show us the true distribution of firms around the 10-employee threshold.

[18] If we would use the LISA definition of firm size rather than that from the salary structure statistics, 81 percent of our observations appear in the same firm size group.

[19] Using the *rddisttestk.ado* package in Stata, the strictness of the test is determined by the parameter k, the maximal degree of non-linearity in the probability mass function that can be accepted as compatible

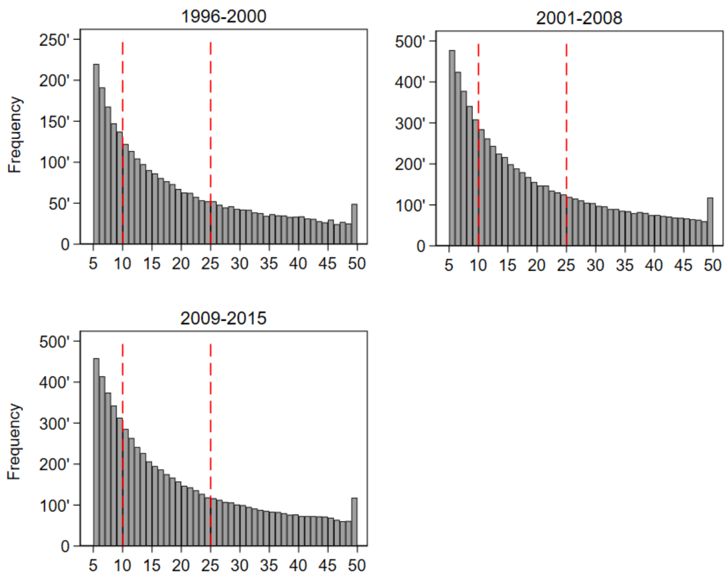

Figure 1: Distribution of firm size.

If different firms sort in different directions across the threshold, analysing the distribution of firms may not reveal that sorting. We therefore conduct the standard tests of running our RD-estimations for observable characteristics that are unlikely to be affected by the treatment. These balance tests are shown in the Appendix Section A6, using various outcome variables based on individual level of education, working experience, tenure and occupation. The estimates do not reveal any imbalance that could indicate systematic differences between the treated and control firms. Industry-level norms could develop based on business regulation and be an institution that shapes compliance (March and Olsen 1989). To check if industry-level factors creates imbalances in our results, we present balance of traits test for a set of industry dummies.

Bundling of treatments Multiple government policies may affect firms at the firm size thresholds. Multiple behavioural changes or sorting incentives could hence exist at once and complicate interpretations of estimates. Swedish tax policy does not change on our thresholds, but there are two other policies that do.

with no manipulation. Frandsen (forthcoming) suggests using a k in a range between 0.025-0.04 as a rule of thumb. We implement the bunching test with k ranging from 0 to 0.04, 0 being the strictest implementation possible. Only for k=0 cannot reject the null of no manipulation for the post 2001 reform period. Notably, however, this test specification is not appropriate due to the quite strong non-linearity in the mass distribution in our data, as observed in Figure 1.

In 2001, the Swedish Employment Protection Act (SFS 1982:80) was amended to create more slack in the Last-In-First-Out (LIFO) rule. Firms with less than 10 permanent employees became allowed to exempt two employees from the LIFO rule. Previous evaluations of this reform have shown that it increased turnover (von Below and Skogman Thoursie 2010) and productivity (Bjuggren 2017), and perhaps also employment (Bornhäll et al., 2015, but c.f. von Below and Skogman Thoursie 2010). The threshold of this reform differs somewhat from ours, mainly by counting only permanent employees rather than all employees. It is, however, conceivable that a facilitation of turnover in small firms could shrink the gender wage gap by either replacing overpaid men or by obtaining a larger proportion of new hires where the gender wage gap might be smaller. Based on our tests for balance of traits it would seem that both these impacts are unlikely. If firms in our control group hired more women to replace overpaid men, the share of women should be larger in small firms, which is it not (see Appendix Section 6 Table A30 and Table A31). We also do not find that firms in the control group have a larger share of employees with short tenures (see Appendix Section 6 Table A18 and Table A19).

A second confounding reform is the fact that the gender wage survey is couched in a broader mandate for firms to work on various issues of gender equality (as discussed in Section 3). This should, if anything, bias our results toward finding an effect. Actions taken to improve women's work conditions should facilitate wage convergence. Actions that impact on the proportion of women are more uncertain, since an effort to hire women could potentially give a downward push on wages. An analysis of the proportion of women in the firm does not, however, give any indication of a change at the policy thresholds (see Appendix Section 6 Table Table A30 and Table A31).

7. Results

Time variation We start by comparing the development of the three gender wage gaps over time in treated and control firms. Figure 2 shows the raw wage gap, the wage gap for equal work, and the wage gap for equal value work over the 1996-2015 time period. The red vertical lines mark the reform years of 2001 and 2009. We are particularly interested in the behaviour of the time trend around the two reforms and differences in that trend between the treated and non-treated firms post-reform. To facilitate interpretation, a line is marked in bold for the time-period when a given size-group is subject to the wage survey mandate.

If the wage survey mandate contributes to closing gender gaps, time trends should turn downward for the lines marked in bold. Looking at the time trends around 2001, we see no sign of such a deviation in the trend for the treated firms (>9 employees). Similarly, the trends for the different firm size groups are parallel also the years around the second reform in 2009. Notably, the raw gender wage gap is the only gap that is declining over time, and perhaps a bit more so for the treated firms.

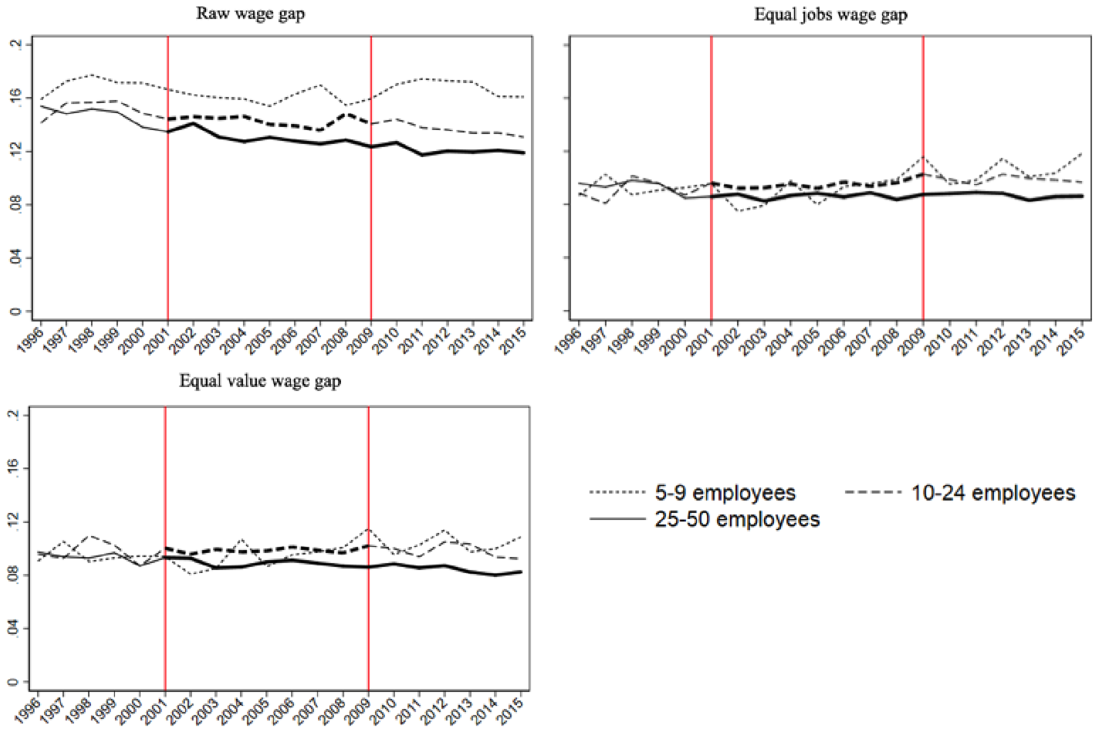

Figure 2: Time trends in gender wage gaps by firm size.

Notes: The figure shows the development of the three types of gender wage gaps calculated in Section (5). Pooled cross-sections of firm-level data are grouped in each year by firm size. A bold trend line marks the years that a size group is subject to the wage survey mandate. Red vertical lines mark the reform years.

Our difference-in-difference estimations estimates the year-by-year difference between the treated and non-treated firms. In each year, treated firms are grouped together, and non-treated firms as well, within the 5-50 size sample. We estimate Equation (2) and plot the coefficient vector of (β_t) in Figure 3. The vector captures the treatment-control differences in the average firm-level wage gap compared to a benchmark year (2000 and 2008). In other words, the difference-in-difference estimations compare the between-group difference in the trend lines plotted in Figure 2 above, compared to the benchmark year.

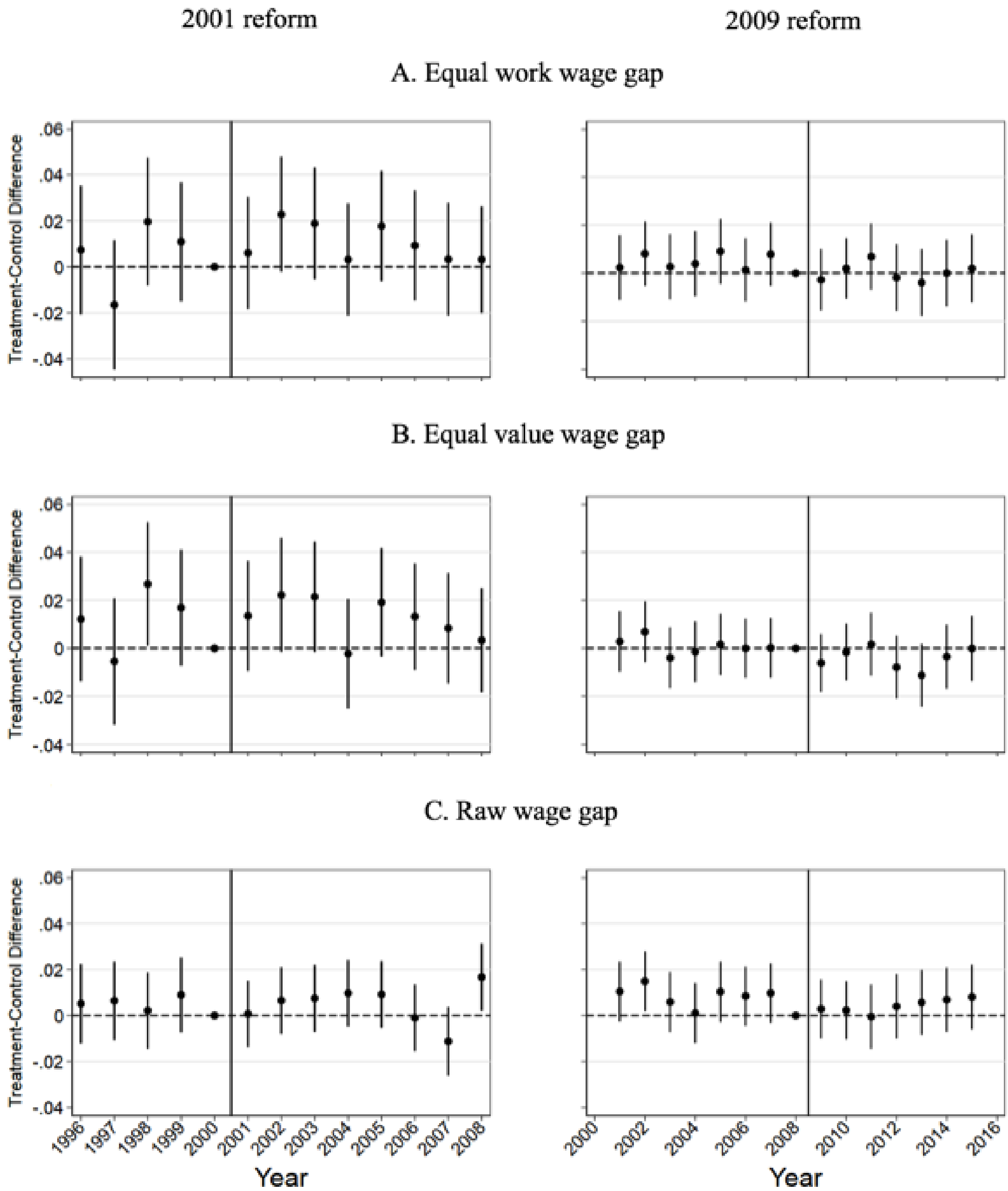

Figure 3: Difference-in-difference estimates.

Notes: The Figure shows estimates from Equation (2) on pooled cross-sections of firm-level data. Vertical lines mark 95% confidence intervals. The left-hand side of plots show the analysis of the 2001 reform that assigned the gender wage survey to firms with 10-50 employees. The right-hand side plots show the results for the 2009 reform that kept the mandate from firms with 25-50 employees. The top row contains the results for the gender wage gap for equal work; the middle row for the wage gap for work of equal value; and the bottom row the raw wage gap. The estimation details for these gaps can be found in Section (5).

If the reform had any effect on the treated firms, we would expect negative estimates in the post-reform time periods. We find no indication of a reform effect; the post-reform difference in wage gaps between the treatment and control groups is not significantly different from the benchmark year for any of the two reforms.

Cross-sectional RD analysis The results from our RD estimations are presented below. We plot the average gender wage gap – of all three kinds – for each discrete value of our running variable firm size before and after treatment. A treatment effect would be indicated by a discontinuous jump downwards in the gender wage gap at the size threshold in the post-reform period, and a lack of such a discontinuity in the pre-reform period. Figure 4 shows no such discontinuous jumps in the distribution of gender wage gaps around the treatment threshold 2001 for any of the three gender gap variables.

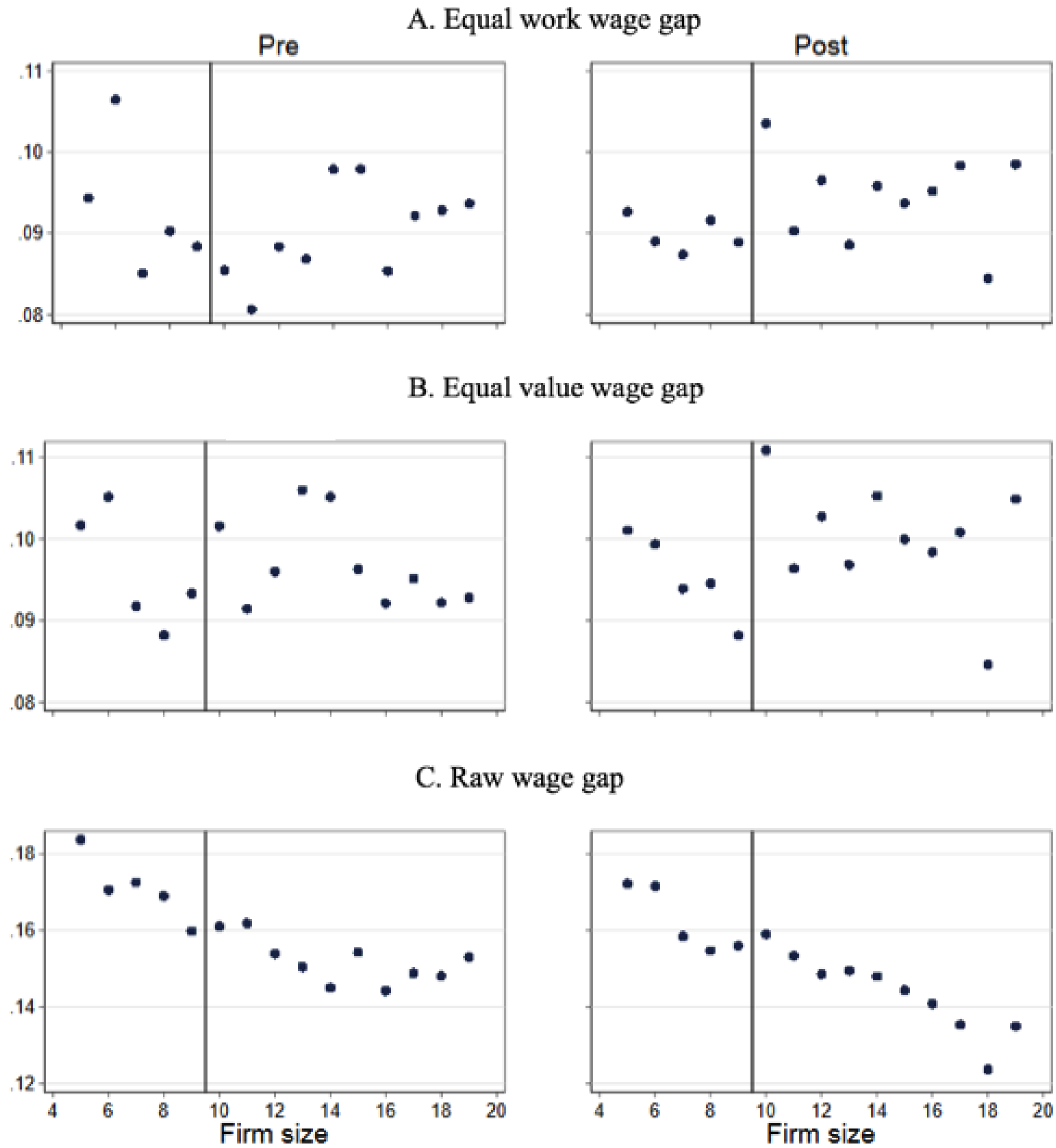

Figure 4: Regression discontinuity plots for the 2001 reform.

Figure 5 reproduces the analysis for the 2009 reform, where the cut-off value for treatment is 25 employees. Again, we see no discontinuous jumps downward at the treatment cut-off.

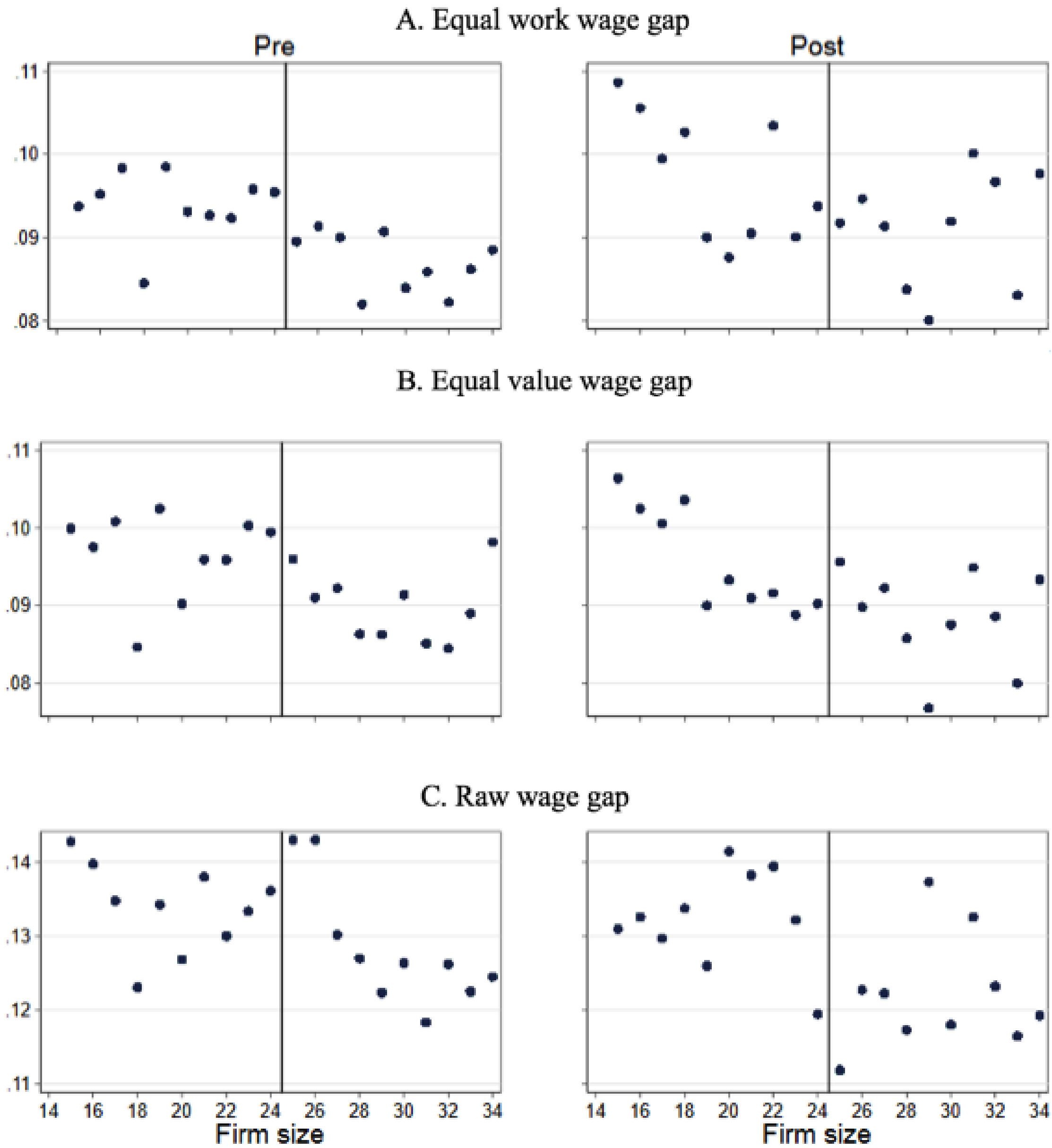

Figure 5: Regression discontinuity plots for the 2009 reform.

The results from our battery of RDD estimations for the 2001 reform is presented in Table 3, and the ones for the 2009 threshold in Table 4. Panels A, B and C shows the estimates for the gender wage gaps for equal work, work of equal value, and the raw wage gap, respectively. In each of these panel we estimate the jump at the threshold in the pre-period (expecting zeroes) and in the post period (where negative estimates would indicate an effect of the wage survey mandate in closing gender gaps).

Table 3: Regression Discontinuity estimates for 2001 gender wage survey reform.

		Local randomization			Local linear control functions		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		*1*	*2*	*4*	*2*	*5*	*8*
A. Equal work							
PRE, 1996-2000	Mandate	-0.003	-0.006	-0.006	-0.007	-0.008	-0.001
		(0.011)	(0.007)	(0.006)	(0.017)	(0.012)	(0.010)
	N	305	634	1,184	918	1,466	1,949
POST, 2001-2008	Mandate	0.015**	0.007	0.005	0.013	0.012	0.007
		(0.007)	(0.005)	(0.004)	(0.011)	(0.008)	(0.007)
	N	807	1,584	2,929	2,306	3,546	4,737
B. Equal value work							
PRE, 1996—2000	Mandate	0.012	0.007	0.007	0.012	0.010	0.016
		(0.011)	(0.007)	(0.005)	(0.016)	(0.011)	(0.010)
	N	382	765	1,412	1,087	1,751	2,323
POST, 2001-2008	Mandate	0.021***	0.011**	0.008**	0.023**	0.018**	0.017***
		(0.007)	(0.005)	(0.003)	(0.010)	(0.007)	(0.006)
	N	943	1,854	3,409	2,692	4,133	5,550
C. Raw wage gap							
PRE, 1996—2000	Mandate	-0.001	-0.003	-0.012***	0.010	0.010	0.005
		(0.008)	(0.006)	(0.004)	(0.012)	(0.008)	(0.008)
	N	1,274	2,489	4,830	3,675	6,073	7,254
POST, 2001-2008	Mandate	0.002	0.001	-0.007***	0.009	0.012**	0.012***
		(0.005)	(0.003)	(0.002)	(0.007)	(0.005)	(0.004)
	N	3,156	6,199	11,653	8,979	14,173	17,036

Notes: The table shows estimates from Equation (3) and (4) and for the wage survey reform that introduced the mandate for firms with 10 or more employees (mandate=1), but not for firms with <10 employees (mandate=0). Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

The results in Tables 3 and 4 confirm the graphical analysis in pointing to an absence of impacts of the wage survey mandate on the three gender wage gaps. In all of the pre-reform periods, the estimates are close to zero and lack statistical significance at conventional levels. But the same is true for the post periods, where negative estimates would indicate an effect in the expected direction. The lack of negative jumps in the gender wage gap around the size threshold is true both for the 2001 reform, which assigned the mandate to firms with 10-50 employees, and the 2009 reform which kept the mandate for firms with 25-50 employees.

Some of the post-treatment estimates are positive, indicating – if anything – larger wage gaps in treated firms compared to control firms. In the case of the post-period estimates for equal value work, this result is clearly driven by outliers close to the threshold, as can be seen from panel B in Figure 5. Indeed, these positive results cannot be detected in the “doughnut” specification that removes the observations closest to the threshold (see Appendix Section 8 Table A38).

Table 4: Regression Discontinuity estimates for 2009 gender wage survey reform.

		Local randomization			Local linear control functions		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		*1*	*2*	*4*	*2*	*5*	*8*
A. Equal work							
	Mandate	-0.006	-0.005	-0.006	-0.008	-0.005	-0.003
PRE, 2001-2008		(0.008)	(0.006)	(0.004)	(0.012)	(0.008)	(0.006)
	N	572	1,146	2,333	1,724	2,927	4,771
	Mandate	-0.002	0.001	-0.004	0.007	0.002	-0.003
POST, 2009-2015		(0.008)	(0.006)	(0.004)	(0.012)	(0.008)	(0.006)
	N	487	1,019	2,097	1,567	2,620	4,141
B. Equal value work							
	Mandate	-0.003	-0.007	-0.006*	-0.006	-0.006	-0.004
PRE, 2001-2008		(0.008)	(0.005)	(0.004)	(0.012)	(0.008)	(0.006)
	N	656	1,297	2,628	1,943	3,285	5,435
	Mandate	0.004	0.002	-0.000	0.005	0.010	0.004
POST, 2009-2015		(0.007)	(0.005)	(0.004)	(0.011)	(0.008)	(0.006)
	N	538	1,100	2,220	1,665	2,779	4,419
C. Raw wage gap							
	Mandate	0.007	0.008	0.001	0.013	0.012	0.009
PRE, 2001-2008		(0.008)	(0.005)	(0.004)	(0.012)	(0.008)	(0.006)
	N	937	1,885	3,823	2,818	4,821	8,236
	Mandate	-0.008	-0.009	-0.014***	-0.002	-0.011	-0.016**
POST, 2009-2015		(0.008)	(0.005)	(0.004)	(0.012)	(0.008)	(0.006)
	N	715	1,494	3,019	2,272	3,754	6,109

Notes: The table shows estimates from Equation (3) and (4) and for the wage survey reform that kept the mandate for firms with 25-50 employees (mandate=1) while removing it from firms with 10-24 employees (mandate=0). Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Consistently negative and, in some cases, statistically significant estimates is only detected for the raw wage gap and the 2009 reform (Panel C, Table 4). The estimates in the corresponding pre-reform are also positive or close to zero. This could indicate that the larger firms, where the mandate was kept in place, saw a reduction in the gender wage gap compared to the smaller firms that did not have the mandate. Comparing the size of the coefficients in the pre- and post-reform periods, the policy may have reduced the gender wage gap in the treated firms with up to 1.5 percentage points over 2009-2015 period compared to the 2001-2008 period. Comparing the results across different sizes for the estimation window, relatively large and significant estimates are only detected in the largest windows (columns 3 and 6). Web appendix

Table A42 shows that the difference between the pre and post periods is statistically significant in a Difference-in-Discontinuity regression.

One possible explanation for the results for the raw wage gap is that the mandate of gender equality plans, which were removed at the same time as the mandate for the wage survey, was making a difference in firms that kept this mandate in 2009-2015. It is also possible that our regression specification of the wage gaps for equal work and equal value is too restrictive, missing the actual margin of impact on men's and women's wages. In the sensitivity analysis below, we estimate wage gaps that depart somewhat from the policy content by "looser" specifications in terms of wage comparisons that are included in the calculation.

8. Sensitivity analysis

Alternative methods of simulating the gender wage survey The largest source of error in our analysis is undoubtedly the choice of method to simulate the gender pay survey. We vary this method in three important ways to show the insensitivity of the results.

First, we test for impacts of the mandate on women's wage-disadvantages, dropping men's disadvantages from the analysis. Although in theory, firms are mandated to close gaps in both directions, practice is to pay more attention to underpaid women than to underpaid men (JämO 2005). Kumlin (2016) cites wage surveys where employees restrict the wage analysis "occupations with more than five employees and where women are paid lower than men" or dismiss male disadvantages entirely by concluding "no wage disadvantage for women". This behavior might stem from a broader understanding of the purpose of the law, described in the pre-amble as improving the labor market position of women.

We re-calculate the gender wage gap to drop any job – within a firm – where men have a wage disadvantage for given observable traits. We then summarize the gender wage gap at the level of the firm after removing these jobs. This is done for both wage gaps (equal work and work of equal value). The results for replicating Equation (3) and (4) with these alternative specifications of the wage gap are placed in Appendix Section 7 Figure A2, Table A32 and Table A33. These results are not qualitatively different from our main results.

Our second alternative simulation method loosens the restriction on how firms compare justified wage gaps. The main analysis assumed comparisons of education, work experience, and tenure between men and women in the same work-category (as motivated in Section 5). We now re-specify the estimation Equation (1) to compare qualifications across work-categories. The wage of an employee in a work-category now will be compared to the linear relationship between a qualification variable and the average salary within the firm. This opens up for gender wage gaps driven by underpaid women in one job category compared to men with, for example, longer education. As such, the specification adjusts a key concern about the main analysis, which only allowed unjustified gaps to exist between employees with the exact same education-work experience-tenure combinations. We estimate Equation (5)

$$\log(wage)_{it} = \beta_f F_i * \alpha_j * \alpha_t + (\alpha_j * \alpha_t) \sum_{q=1}^{Q} \mathrm{Q}_{it} * + \alpha_{job} * \alpha_j * \alpha_t * \varepsilon_{it} \tag{5}$$

where the variable vector Q represents our three qualification variables, education, work experience, and tenure. These are included in the regression as ordinal variables and interacted with the firm and year dummies. The gender wage gap coefficient at the level of the firm and year (β_f) captures the average difference between men and women in the same job (keeping the interacted fixed effects between job, firm, and year), but relating the returns to education, work experience and tenure levels to the firm-level average salary.

A third specification loosens the comparison of justified wage gaps even further, from the firm level to the industry level. We return to the flexible, fixed effects specification of the qualification variables. But instead of estimating wage gaps between men and women with the same qualifications in a specific work category in a specific firm, we benchmark the gender wage gap in the work category against the average pay-off to that qualification level in the industry as a whole. Notably, this specification does not drop people for whom there is no variation in a qualification variable within a firm. We specify the regression as

$$\log\ wage\ _{it} = \beta_f F_i * \alpha_j * \alpha_t + (\alpha_{ind} * \alpha_t) * (\alpha_{educ} + \alpha_{exp} + \alpha_{ten}) + \\ (\alpha_{job} * \alpha_j * \alpha_t) + \varepsilon_{it} \tag{6}$$

where *ind* is a new sub index for the 2-digit industry code of the firm. We re-estimate the main analysis with these alternative specifications and place the results in Table A34-Table A37 in the Appendix Section 7.

The results from both these alternative specifications indicate a positive (albeit small) and significant discontinuity at the 2001 threshold, and for both equal work and equal value work. We find no evidence that the reform reduced these gaps, further corroborating the robustness of the main findings.

Measurement error in firm size Swedish law lacks some clarity on the exact definition of which employees should count toward the employee-thresholds in the wage survey mandate (as described in Section 3). This could create measurement error in the RD analysis in particular, since it relies on estimating a treatment effect right at the employee-cut off. For this reason, we repeat our RD analysis with a doughnut design, removing the firms closest to the threshold. For the estimations with local randomization (Equation (2)) and local linear control functions (Equation (3)) we remove firms that are 2 employees to each side of the threshold. The results are presented in Appendix Section 8 Table A38, and do not differ from our baseline results.

Alternative wage variable Our wage variable in the main analysis includes the fixed salary, benefits, and any piece rate payments. Our dataset also includes an alternative variable which further adds payments for uncomfortable working hours (OB) and other forms of additional payments, for example monetary compensation for being on duty[20]. The Equality Act is not specific on exactly which wage measure to use in the wage survey. We choose to use the first measure of the wage variable since it captures the contracted salary and will not be as sensitive to additional payments specific to the measurement month. Reassuringly, and perhaps unsurprisingly given the small difference between the two wage variables, the main results are unaffected by the switching to the wider compensation measure (see Appendix Section 8 Table A39 and Table A40).

9. Policy scope

If every firm implemented the wage survey policy to the letter of the law, how would this affect the aggregate gender wage gap? Answering this question can help us interpret our main findings. A null effect of the wage survey is less contradictory to a high level of compliance if the policy, even if perfectly implemented, is not an efficient tool to close the gender pay gap.

[20] For 63 percent of the total sample (1996-2015) the two variables are equal, and for the remaining 47 percent, the second measure is larger. The gender wage gap is also slightly larger for the second variable. In pooled data for the entire period, the average difference in pay is 3142.24 SEK using our main variable, and 3368.76 SEK for the alternative (37 percent of the men and 36 percent of the women receive nonzero additional benefits).

We calculate the proportion of the aggregate gender wage gap that can be derived from unwarranted firm-level gender gaps that can be affected by the policy. This is done both for the subsample of firms with 10-50 employees and for all firms with more than 10 employees. We look at three years of data: 1996, the first year of data and as close as we can get to 1994 when the wage survey policy was first conceived; 2000, the year before the reform that shaped the policy into its' (supposed) efficient format; and 2005, the first year of full availability of four-digit occupation codes.

The scope of the policy is derived by, first, computing the mean-difference between women and men's wages in the pooled individual-level data for firm sample and year. These numbers are reported in the first row of Table 5. They constitute the raw wage gap that the policy is intended to affect. Next, we create an artificial data set where we have adjusted unwarranted gender wage gaps. Departing from Equation (1), which extracts unwarranted gender wage gaps, we replace the salary of each person in the data with the average salary in each category of work and qualifications (delineated by our fixed-effects structure). In work-qualification categories with either a single man or woman, the average equals their own salary and no change is made. For others, wage adjustments to the average corresponds to closing the unwarranted wage gaps by an even mix of upward and downward adjustments (see JämO 2005).

Having obtained the artificial data with adjusted wages, we again compute the mean-difference in wages and compare it to gap on the unadjusted data. In Table 5 we report the size of the adjusted gap in proportion to the raw gap, and for different margins of wage adjustments. Adjusting for both equal work and equal value work is done by first adjusting equal work gaps, and then – on the adjusted data – additionally adjusting for equal-value gaps. All calculations apply the official sample weights from Statistics Sweden to account for the size-industry stratification of the wage structure statistics.

The main take-away from the calculations is that the wage survey mandate has a limited policy scope. In our estimation sample, the policy had the potential of closing 6—7 percent of the aggregate gender wage gap in 1996, and 4—5 percent in year 2000. When larger firms are included, these numbers grow to 13—19 percent. It is, however, doubtful that the large firms see more wage adjustments in real life. Facilitation efforts by the Swedish government has shown that large firms are only marginally more likely to make any wage adjustment in their wage survey compared to smaller firms (Stadskontoret, 2011).

Another important perspective on the numbers is that we most likely capture an upper bound of the true policy scope. This is we lack data on some of the relevant variables that are commonly used to explain women's wage disadvantages, most importantly individual job performance. We also know that other, non-relevant, variables are commonly used by firms to explain gaps, most importantly market demand and assessments of the employee's future potential.

Returning to the calculations in Table 5, they also inform us that the policy scope of adjusting wage gaps for equal work is about the same size as the policy scope of adjusting wage gaps for work of equal value. Moreover, these two policy scopes largely overlap, so that adjusting both gaps only result in a minor improvement in the policy effect compared to adjusting only one of them. From the calculation of gaps on the 4-digit occupation level rather than the 3-digit level we can also see that the results are similar. This means that our main analysis on the 3-digit level is less likely to give misleading results if firms are in reality using lists of jobs that are more detailed.

Table 5: Policy scope of the wage survey mandate

	10—50 employees			10+ employees		
	1996	2000	2005	1996	2000	2005
Raw, aggregate gender wage gap	0.135	0.171	0.146	0.147	0.163	0.163
% of gap remaining after adjustment for:						
equal work	94	96	94	83	87	84
work of equal value	93	95	94	81	85	83
equal work + equal-value work	93	95	95	82	86	85
equal work (4 digit)			95			87
equal work (4-digit) + equal-value			95			85

10. Discussion and conclusions

Across the world, governments are targeting business organizations with regulations to promote social and economic goods. Gender equality is one such case. Governments are pondering how to make firms take action to close the gender pay gap, and Swedish policy makers have for more than 20 years actively been trying to induce firm-level action. In this paper we investigated the impacts of the Swedish gender wage survey mandate. In the paper, we have simulated the wage survey in firms using register data and used implementation thresholds across firm size to analyze causal impacts on the gender wage gap.

For those expecting policy impacts, the results are disappointing. We do not find evidence that the regulation led to smaller wage gaps between men and women performing i) equal work or ii) work of equal value. Even over the 25 years of data, we do not find that any of our three estimated wages gap shrunk more in larger (treated) firms than in smaller (untreated) firms.

A key finding that can explain the lack of impacts is the fact that even if the policy had been perfectly implemented, results would have been modest. We compute the impact on the raw, aggregate gender wage gap for the private sector from closing the gaps that are targeted by the policy. These are gaps between women and men in the same firm, performing either the same or same-value work, and having similar qualifications. Closing these gaps would account for 7 percent of the raw gender wage gap in firms with 10—50 employees, and 18 percent in firms with 10 or more employees. The take-away from this analysis is, hence, similar to previous work showing that only a small fraction of the total gender wage gap comes from within-job discrimination (e.g. following Peterson and Morgan 1995). We add to this that the contribution of differences between same-value work, within a firm, is equally modest. Moreover, the wage gaps for equal work and equal-value work overlap to a very large degree, so that requiring firms to adjust both does not imply a greater impact than only adjusting one.

Even our calculated effect on the aggregate wage gap from perfect implementation should be considered an upper bound. The fact that we measure equal work with 3-digit occupation codes means that we get larger gender wage gaps than firms, which often use jobs at the 4-digit level, and government agencies' recommendations, which is to use even narrower categorizations (JämO 2003, 2007). We also lack variables that can control for employee productivity, and hence cannot capture warranted wage-differences due to this factor, which is commonly used by businesses to explain why men earn more than women (Kumlin 2016). An additional source of upward bias is the fact that businesses often explain gender wage gaps with characteristics that are, in fact, not warranted, but yet fully legal for the firm to refer to. Most important among these are "market demand", a person's "future earnings potential", or age (Fransson and Andersson 2006, Kumlin 2016). All these factors imply that the potential of the policy to reduce the aggregate gender wage gap is most likely even smaller than our calculation would suggest.

The lack of sizeable effects corroborates previous findings from Swedish government agencies. An important insight from surveying firms' implementation over numerous rounds of revisions is that perfect implementation is associated with a small number of wage adjustments. Among 569 firms with a total of 750 000 employees were surveyed in 2007—2008, only 5 246 employees (0.7 percent) had their wages adjusted, and with an average of 1 120 SEK (100 USD) per month. The adjustments amounted to 70.3 million SEK in total (JämO 2007, 2008). Later studies confirm this: few firms make adjustments, and if so for a small number of employees and at small amounts (SOU 2010:7, Kumlin 2016).

There are several reasons to believe that most firms have remained far from full compliance with the regulation. These reasons, brought forth in reports by Swedish government agencies over time, also give important insights about the null-findings of our analysis. First, businesses are struggling with the vagueness of the regulation. Survey and interview data reveal that firms view the wage survey mandate as abstract, complex, unnecessary and inefficient (JämO 2003, SOU 2010:7, Stadskontoret 2011, Lehto and Lindholm 2014, DO 2016). Organizations have pointed out the ease with which the format of the wage survey lets the surveyor shuffle employees between job-categories to eliminate wage gaps.

The policy has also failed to find support among the vast majority of employer organizations (JämO 2007, 2008, Confederation of Swedish Enterprise 2015). Resistance from employers is also a likely reason for that the wage survey mandate has not gained momentum by becoming an integral part of the Swedish wage bargaining process.

A second reason for weak compliance lies in the formulation of the mandate. The law has repeatedly been shown to be hard to implement, in particular the definition and analysis of "equal-value work" and the content of the action plan for achieving equal wages (JämO and SCB 2005, JämO 2007, DO 2016). Notably, firms lack strong incentives to invest time in making meaningful changes to wages or work policies. The law requires firms to analyze wages and establish a written action plan but says nothing about which wage gaps that are unwarranted or which type of action to take. This differs from the voluntary wage-gap analysis tools provided by the Swiss Government (Logib), with later spin-off versions for Germany and Luxemburg. These tools give a list of jobs, control variables and a size and significance of the wage gap coefficient that is not allowed for firms taking part in public procurement.

Vague regulation in the area of workplace equality has previously been shown to incentivize shallow and symbolic rather than deep and meaningful compliance structures (Ahmed 2007, Fredman 2012). It has also been linked to de-coupling of the compliance products (plans, training etc.) from the day-to-day operations of the business (Sutton and Dobbin 1996). In the Swedish case, surveys and interviews have highlighted that gender wage surveys often turn into pure paper products (e.g. Lehto and Lindholm 2014). Consultants are sometimes hired to ensure legal compliance. This removes responsibility from the manager and makes it harder for employee organizations to cooperate with the employer (Fransson and Andersson 2006) and obstructs real changes in wages or workplace policies (DO 2016).

The enforcement strategy chosen by the government might also be at some fault. The facilitation effort of training and helping firms to comply may not have accomplished to make the wage surveys a more integrated part of firms' business. A couple of years after the intense scrutiny of nearly 600 firms in 2007—2008, a re-survey showed that these firms were no more likely to recollect this experience than a control sample of firms which had not been scrutinized (Stadskontoret 2011).

The Swedish government has argued that the wage survey policy is an important tool for gender equality, despite affecting few employees (JämO 2005, JämO 2007, Prop 2015/16:135). A survey among union representatives revealed that the wage survey is seen as providing important input in the wage bargaining process (Unionen 2015). These gains should, however, be judged against alternative use of resources by politicians, regulatory agencies, and businesses. Enforcing the wage survey mandate is not free, neither for the government nor for the firms. With a survey process external to the firms' regular operations, costs also rise since a consultant will "start from scratch" each time (SOU 2010:7). A single survey is approximated to cost about 200 USD per firm in 2015 (Prop. 2015/135).

Arguably, our results suggest that the energy that policy makers and firms have for closing gender wage gaps should be directed toward margin(s) where gaps are larger and/or where policy has greater potential of closing them. This could include policies to shift men and women between the private and public sectors, between firms, and up to management positions within firms. It could also include efforts to shift men's time use toward working part-time or taking a larger share of parental leave.

The (disappointing) results in this paper come from the private sector. It is, however, possible that the wage survey policy has been more efficient in the public sector where the government has more opportunities for direct (and indirect) enforcement. Regulatory agencies have indeed concluded that the policy may have had larger quantitative impacts in local or national public organizations.

Enforcement agencies have also argued that the law's small effects on wage setting in organizations is couched in a wider, aggregate-level impact of the law on wage setting and hiring at large. The law has signalled gender equality to be a prioritized area and could not be removed without signalling the opposite. Contradicting this argument, the private sector wage gap has not moved much on the aggregate level, and the same intention to fight gender inequality could be signalled via some other, and more efficient, policy.

For the 2009 reform, we find some small but significant negative effect on the raw gender wage gap in the post-reform period. Even if the wage survey mandate is targeting wage gaps between men and women with equal or equal valued work, we might have a treatment effect on the raw wage gap if employers discover and adjust more general discriminatory patterns while working on the wage survey. As discussed in Section 3, the effects on the raw wage gap might reflect that the wage surveys are embedded in a broader mandate to improve gender equality. If for example the share of women in higher-paying positions increase due to a renewed focus on gender equality, this might decrease the raw pay gap.

Finally, our results can provide input on policies being developed in Europe and elsewhere. Asking firms to produce gender-based wage statistics, such as the regulations in Denmark and Norway, are unlikely to give much impact on the gender wage gap. Policies that include audits, such as the one discussed at the EU level, and/or national-level transparency via a depository and media, such as in the UK, might be more efficient. But the Swedish experience speaks loudly against policies like the Icelandic one where an initial, mandatory, audit is followed by a system where audits only follow if the firm is reported by unions or workers. Our results also indicate some advantage with policies that have clearer methods of how to compute wage gaps have the advantage of defining illegal wage gaps, such as the government-provided Logib tools in Switzerland, Germany, and Luxembourg. But these policies are voluntary or has minimal risks for sanctions in public procurement firms, something that our findings point to, are likely to result in very small wage changes.

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Appendix

Section A1: Example of a gender wage survey

This section replicates an instruction for how to produce a gender wage survey (JämO 2005). The original instruction has been slightly redacted to save space. The description has been somewhat abbreviated from the original version provided by the regulatory agency.

1. Survey the company's provisions and practice for pay, such as pay provisions, fringe benefits, and other employment terms. Analyze how they affect women and men.

2. Divide the jobs in the company into categories with equal or nearly equal duties. Summarize the number of men and women in each job. Then report the average monthly wage, measured in full-time equivalents, for men and women, as well as the gender wage gap.

Table A1: List of jobs and summary statistics.

Fictive Firm Ltd.							
	Gender distribution				**Average wage**		
	Number		**Percent**				**Women's wage in % of men's**
Occupation	**Women**	**Men**	**Women**	**Men**	**Women**	**Men**	
Manager	0	1		100		45 000	
Department head	1	2	33	67	31 000	35 000	89
Communication officer	1		100		19 000		
Engineer		2		100		22 000	
Sales officer	1	1	50	50	20 500	19 500	105
Accountant	1	1	50	50	21 000	22 000	95
Fitter	2	8	20	80	17 500	19 000	92
Laboratory staff	4		100		17 000		
Repair technician		2		100		18 500	
Warehouse worker		1		100	15 500		
Receptionist	1		100		16 000		
Janitor	1	1	50	50	13 500	13 500	100
Total	13	18					

3. Departing from the list of gender wage gaps, analyze if each gap can be explained with factual reasons. The whole gap needs to be explained.

Table A2: Analysis of gender wage gaps in equal jobs.

Fictive Firm Ltd.	
Gender-mixed job with wage gap	Analysis of the wage gap between men and women
Department head	The woman head of department is newly hired, as is one of the two men. The newly hired have shorter job tenure, so the wage difference is justified.
Sales officer	The woman sales officer's higher wage is motivated by her better sales results.
Accountant	The male accountant's higher wage is motivated by his longer work experience in the occupation.
Fitter	The women fitters' lower average wage is because they are not yet able to perform all the tasks of the job.

4. From the complete list of jobs, mark which ones are female-dominated. Review the remaining jobs to find ones that are of equal value to each female-dominated job. Equal value shall be judged by four criteria: i) knowledge and skills, ii) responsibility, iii) mental or physical effort, and iv) working conditions. Within each group of jobs, report the average wage across men and women. Analyze if factual reasons can explain the entire wage difference.

Table A3: Analysis of wage gaps between jobs of equal value.

Fictive Firm Ltd.			
Group	Job	Average wage for men + women	Analysis of the wage gap between jobs within groups of equally demanding jobs
1	Manager		Not comparable to other jobs
2	Department head		Not comparable to other jobs
3	Com. officer	19 000	The communications officers have the lowest salary in the group. The sales officers' salaries can be motivated based on sales results. The analysis has shown that objective criteria cannot explain the wage difference between the communications officers and the engineers and accountants.
3	Engineer	22 000	
3	Sales officer	20 000	
3	Accountant	21 500	
4	Fitter	18 900	The lower average wage among the laboratory staff is related to less wage dispersion in this job. The productivity of the laboratory staff workers is not reflected in the wage trajectory in the same way as for the other jobs.
4	Laboratory staff	17 000	
4	Repair technician	18 500	
5	Warehouse worker	15 000	The job as janitor is equally demanding as that of the warehouse worker. No objective criteria for the wage difference has been found. The receptionist's wage is in line with the warehouse worker.
5	Receptionist	16 000	
5	Janitor	13 00	

5. Produce an action plan for equal wages. Any wage gap which cannot be explained by factual reasons shall be corrected as soon as possible, and at the latest within three years. Also investigate if other measures are needed for equal wages, for example training.

Table A4: Action plan for gender equal wages.

Fictive Firm Ltd.	Action	Cost/month			Finish by?	In charge?
		Year 1	Year 2	Year 3		
Wage setting practices and criteria						
The criteria used for individual wage setting are not entirely clear, or known among the employees	Document the criteria used for individual wage setting and inform about these.	10 000				Board
Wages for equal jobs						
The woman department head's lower earnings could not be motivated by objective criteria	Wage adjustment for one department head	1 000	1 000		Year 2	Closest superior
Women fitters have lower average wages because they cannot perform all tasks in the job.	Training	No additional funding needed			Year 2	Closest superior
Wages for jobs of equal value						
Group 3. The wage level of the communication officer relative to the accountants and engineers could not be explained by objective criteria	Wage adjustments for one communication officer	1 000	1 000	1 000	Year 3	Closest superior
Group 4. The laboratory staff's lower wages are related to a smaller wage dispersion, indicating an inequality in how performance is reflected in wages.	Faster wage-growth for 4 laboratory staff and reduced wage growth for 10 fitters.	2 000	2 000	2 000		Closest superior
Group 5. Objective criteria cannot explain the wage difference between janitors and warehouse workers.	Wage adjustment for one janitor	500	500	500	Year 3	Closest superior
The action plan is valid from January year 1 to December 31^{st} in the same year.						

Reference:

JämO. 2005. Survey, analysis and action plan for equal pay: An in-depth analysis on effects of the regulations of 2001. Public report from the Swedish Ombudsman for Gender Equality.

Section A2: Valuation methodology for work of equal value

The classification of work of equal value is based on the Swedish occupation code (SSYK). It is based on the ISCO code, the International Standard Classification of Occupations 2008) developed by the International Labor Organization (ILO). Each four-digit code has a brief description of the content of the job.[21] For example, the job a high school teacher is described as "teaching classes at the high school level in theoretical or practical fields. Plans, implements, and evaluates the teaching in accordance with general and field-specific guidelines. Grades and evaluates students."

The first Swedish SSYK code was developed after ISCO-88, the International Standard Classification of Occupation 1988, develop by the ILO. In 2012, the code was updated in accordance with the ILO update in ISCO-08. The first digit of the code indicates the qualification level. Managerial professions start with the digit 1 and are excluded from our categorization. These professions are judged to be too variable at the organization level to be classified according to a single number. Military professions (first digit 0) are also excluded. This leaves 324 occupations in SSYK96 (91 percent) and 369 in SSYK12 (86 percent).

We recruited experts to evaluate the four-digit occupation codes using the online tool *Analys Lönelots* and the texts that describe each occupation.[22] Analys Lönelots was developed in the late 1990s at the research division of the National Institute for Working Life and later revised in the early 2000s. The model is based on three dimensions of job demands – knowledge and skills, responsibility, and working conditions and environment. Knowledge and skills is subdivided into three sub-categories, resulting in five parameters that are assessed for each job: *i)* educational requirements, *ii)* problem solving capacity, *iii)* social skills, *iv)* responsibility and *v)* working conditions and environment. Our experts are the same people who developed this tool, and who have been actively applying it and fine tuning it since them.

A job is evaluated by scoring it on each of the five parameters. These scoring steps are explained in Table A5. When a business uses the tool it can itself adjust the number of scoring steps used for different jobs. For the scoring in this paper, educational requirements given by the first digit of the occupation code were used to score each job on the

[21] For examples in Swedish, see https://www.h5.scb.se/yreg/ssyk2012.asp.
[22] https://www.lonelotsarna.se/analyslonelots/

education parameter. For the remaining four, the experts used the scoring scheme in Table A5.

Having obtained five parameter scores for each occupation, these are weighed together to a single index. For the score in this paper, responsibility is given the single highest weight of 30 percent. Knowledge and skills – made up of education, problem solving capacity and social skills – is given a weight of 55 percent. Strain and working conditions account for the final 15 percent. Jobs with similar index numbers are considered to be of equal value. For each of the SSYK96 index and the SSYK2012 index, values for the list of jobs are split into 11 categories with roughly 30 jobs in each one. Jobs that are in the same category are considered to be of equal value.

Table A5: Summary of scoring steps for work evaluations.

Knowledge and skills			**Responsibility**	**Strain, working conditions**
1. Education	**2. Problem solving**	**3. Social skills**	**4. Responsibilities for people and other values**	**5. Work life conditions**
Measured by	Measured by	Measured by	Measured by	Measured by
Education duration	Analytical tasks, creativity, independence, development	Communication, cooperation, cultural understanding, service	Responsibilities for human values, planning, development, and output	Physical effort, psychological effort, work conditions
Levels	Levels	Levels	Levels	Levels
1. Basic education, no requirements for formal education.	1. No special demands for problem solving. The problems that generally arise on the job has simple, single-dimensional solutions. The information needed is easy to access. Problems can be solved by selecting from a menu of common approaches.	1. Normal demands on cooperation and service, either to/with individuals or groups.	1. Little responsibility for people, planning, development or output. The employee's own work is to some extent planned by others. The employee can contribute ideas and proposals within his or her own area of responsibility.	1. No specific requirements on physical efforts. The physical environment is satisfying. Little psychological effort. Mental focus is sometimes needed for shorter periods. High-strain situations can occur on occasion.

2. High school or college/university programs of < 2 years	2. Mid-level	2. Demands for communication and/or service in situations when there can exist conflicts of interest or cultural differences.	2. Some responsibility for people, values, planning or output. The job can be carried out by following rules, instructions and/or plans, but neglecting one's responsibilities can have consequences for the organization or for people.	2. Mid-levels of physical or psychological strain. The physical environment may sometimes be straining and/or in combination with: Mental focus is sometimes required for long periods of time and for complicated tasks. Straining situations and communication difficulties exist.
3. Practical or job-specific post-secondary educations of 2—3 years.	3. Mid-level demands for problem solving. The work requires information processing from different sources to solve problems and to decide on solutions. There are several potential solutions to choose between, but they follow well-known methods and routines. When unusual problems arise, solutions and decisions must be discussed and approved by a superior.	3. Mid-level demands for social skills. The work can entail demanding situations with communication difficulties.	3. Mid-level.	3. Mid-level.
4. Academic basic education, normally for three years (Bachelor's)	4. Mid-level.	4. Large demands for social skills. The job can imply difficult and complicated contacts with persons/groups, within as well as outside the workplace. Demanding contacts can imply severe conflicts of interest or difficult and controversial situations.	4. Mid-level responsibility for people or values and/or planning, development or output. The job can be carried out by following rules, instructions or programs, but the responsibility is individual and concerns work methods and time management. Failures in responsibilities can have large consequences	4. Large demands for one or several types of physically straining tasks. The physical environment is often straining and/or in combination with: Large psychological strains. Mental focus is often demanded and for complicated tasks. The job implies demanding situations with strong emotional adaptations.

			for the organization and/or people.	
5. Graduate studies at the Master's or PhD level.	5. Very large demands for problem solving. The job involves analysis of theories and/or methods. Problems are varied, complicated and demand creative solutions. The job requires complex information gathering. The employee mush aggregate complicated data to judge possible consequences.		5. Mid-level.	
			6. Large responsibilities for people or values, as well as planning, development or output. The employee has a significant responsibility for people or for the design of products or services that are produced, and/or norms, routines and instructions that manage the organizations. Situations that can arise are difficult and/or demand fast and encompassing. A mistake or neglect of responsibilities can have very severe economic and/or human consequences.	

Further readings

Andersson, Eva. R. and Anita Harriman. 1999. Rätt lön på rätt sätt. Metod för bedömning av kvalifikationer vid individuell lönesättning *(The right wage in the right way. A method for evaluating qualifications for individual wage setting)* National Institute for Working Life.

Lönelotsarna 2017a. 76 miljarder – priset för den strukturella löneskillnaden (*76 billion – the price for different wages for equal value work*). Lönelotsarna Report.

Section A3: Description of observables

Table A6: Descriptive statistics for individual characteristics.

	1996—2000		2001—2008		2009—2015	
Education	N	%	N	%	N	%
1	25,459	10.15	36,240	6.25	9,465	2.24
2	169,516	67.58	379,682	65.44	251,756	59.70
3	54,749	21.83	162,210	27.96	158,276	37.54
.	1,118	0.45	2,074	0.36	2,172	0.52
Work experience						
1	34,386	13.71	73,603	12.69	51,161	12.13
2	38,755	15.45	72,243	12.45	53,343	12.65
3	42,714	17.03	78,709	13.57	54,101	12.83
4	31,163	12.42	89,099	15.36	52,573	12.47
5	101,983	40.66	263,055	45.34	208,051	49.34
.	1,841	0.73	3,497	0.60	2,440	0.58
Tenure						
1	88,658	35.34	193,534	33.36	123,596	29.31
2	63,270	25.22	147,641	25.45	109,332	25.93
3	97,073	38.70	235,534	40.59	186,301	44.18
.	1,841	0.73	3,497	0.60	2,440	0.58
Female						
1	92 834	37.01	226,673	39.07	179,966	42.68
0	158,008	62.99	353,533	60.93	241,703	57.32

Table A7: Comparison of descriptive statistics across wage variables.

	Mean	Std. dev	Min	Max
Log(fixed monthly wage)	9.75	0.31	8.62	12.96
Log(fixed monthly wage + benefits)	9.78	0.30	8.62	12.98

Table A8: Descriptive statistics for occupational valuation categories

	1996-2000		2001-2008		2009-2015	
Value category	N	%	N	%	N	%
1	15,957	6	36,913	6	25,812	6
2	35,549	14	85,305	15	41,290	10
3	9,597	4	21,030	4	23,052	5
4	48,132	19	102,471	18	68,261	16
5	49,355	20	106,480	18	70,571	17
6	27,222	11	56,453	10	43,753	10
7	7,893	3	23,273	4	20,966	5
8	5,393	2	24,572	4	21,148	5
9	21,770	9	55,764	10	47,191	11
10	5,167	2	10,371	2	12,728	3
11	4,946	2	14,240	2	13,338	3
.	19,861	8	43,334	7	33,559	8

Section A4: Difference-in-Difference models with controls

Table A9: Interaction model with control variables for the 2001 threshold.

	Wage gap for equal work	Wage gap for equal work	Wage gap for work of equal value	Wage gap for work of equal value	Raw wage gap	Raw wage gap
	(1)	(2)	(3)	(4)	(5)	(6)
Mandate*1996	0.010	0.013	0.011	0.013	0.004	0.004
	(0.014)	(0.014)	(0.013)	(0.013)	(0.009)	(0.008)
Mandate*1997	-0.015	-0.011	-0.006	-0.005	0.006	0.005
	(0.014)	(0.014)	(0.013)	(0.013)	(0.009)	(0.008)
Mandate*1998	0.023	0.027*	0.027**	0.029**	0.002	0.001
	(0.014)	(0.014)	(0.013)	(0.013)	(0.008)	(0.008)
Mandate*1999	0.017	0.018	0.018	0.018	0.009	0.007
	(0.013)	(0.013)	(0.012)	(0.012)	(0.008)	(0.008)
			2000 = Reference Year			
Mandate*2001	0.010	0.009	0.014	0.013	0.001	-0.001
	(0.012)	(0.012)	(0.012)	(0.011)	(0.007)	(0.007)
Mandate*2002	0.027**	0.028**	0.024**	0.026**	0.007	0.008
	(0.013)	(0.013)	(0.012)	(0.012)	(0.007)	(0.007)
Mandate*2003	0.021*	0.024**	0.022*	0.024**	0.008	0.007
	(0.012)	(0.012)	(0.012)	(0.011)	(0.007)	(0.007)
Mandate*2004	0.006	0.008	-0.003	-0.001	0.009	0.009
	(0.013)	(0.012)	(0.012)	(0.011)	(0.007)	(0.007)
Mandate*2005	0.019	0.024**	0.018	0.021*	0.009	0.008
	(0.012)	(0.012)	(0.012)	(0.011)	(0.007)	(0.007)
Mandate*2006	0.012	0.015	0.012	0.015	-0.002	-0.002
	(0.012)	(0.012)	(0.011)	(0.011)	(0.007)	(0.007)
Mandate*2007	0.006	0.009	0.008	0.009	-0.011	-0.012*
	(0.013)	(0.012)	(0.012)	(0.011)	(0.008)	(0.007)
Mandate*2008	0.006	0.009	0.003	0.005	0.016**	0.017**
	(0.012)	(0.012)	(0.011)	(0.011)	(0.007)	(0.007)
Region FE		x		x		x
2-Digit Industry FE		x		x		x
Obs.	9,944	9,944	11,642	11,642	30,287	30,287
R-squared	0.003	0.066	0.003	0.059	0.007	0.090

Notes: The table contains OLS estimates of Equation (2), with and without fixed effects for region and 2-digit industrial sector. Only interaction terms are reported. The outcome variables are three versions of the gender wage gap, described in Section 5. Standard errors are in parentheses and *** p<0.01, ** p<0.05, * p<0.1.

Table A10: Interaction model with control variables for the 2009 threshold.

	Wage gap for equal work	Wage gap for equal work	Wage gap for work of equal value	Wage gap for work of equal value	Raw wage gap	Raw wage gap
	(1)	(2)	(3)	(4)	(5)	(6)
Mandate*2001	0.002	-0.000	0.002	-0.000	0.010	0.008
	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)	(0.006)
Mandate*2002	0.008	0.003	0.007	0.002	0.015**	0.010
	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)	(0.006)
Mandate*2003	0.003	-0.001	-0.004	-0.007	0.006	0.005
	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)	(0.006)
Mandate*2004	0.003	-0.000	-0.002	-0.005	0.001	-0.001
	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)	(0.006)
Mandate*2005	0.009	0.004	0.002	-0.002	0.011	0.008
	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)	(0.006)
Mandate*2006	0.001	-0.003	-0.000	-0.002	0.009	0.007
	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)	(0.006)
Mandate*2007	0.008	0.006	0.000	-0.001	0.010	0.008
	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)	(0.006)
			2008 = Reference year			
Mandate*2009	-0.003	-0.004	-0.006	-0.007	0.004	0.002
	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.006)
Mandate*2010	0.002	0.003	-0.001	-0.001	0.003	0.003
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Mandate*2011	0.007	0.007	0.002	0.002	-0.000	-0.001
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Mandate*2012	-0.002	-0.003	-0.008	-0.010	0.004	-0.001
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Mandate*2013	-0.004	-0.004	-0.011*	-0.012*	0.006	0.002
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Mandate*2014	-0.000	-0.003	-0.004	-0.006	0.008	0.002
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Mandate*2015	0.002	0.000	0.000	-0.001	0.009	0.004
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Region FE		x		x		x
2-Digit Industry FE		x		x		x
Obs.	20,733	20,733	22,970	22,970	36,882	36,882
R-squared	0.004	0.059	0.005	0.056	0.006	0.105

Notes: The table contains OLS estimates of Equation (2), with and without fixed effects for region and 2-digit industrial sector. Only interaction terms are reported. The outcome variables are three versions of the gender wage gap, described in Section 5. Standard errors are in parentheses and *** p<0.01, ** p<0.05, * p<0.1.

Section A5: Checks for manipulation of the running variable

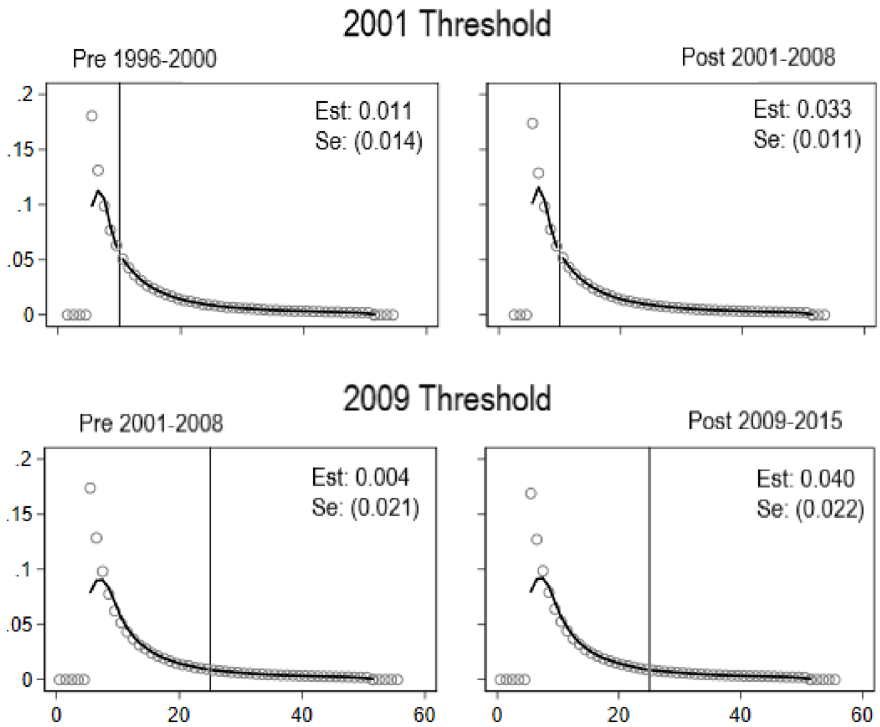

Figure A1: McCrary (2008) test of manipulation of the running variable.

Table A11: P-values from Frandsen (2017) test for manipulation of running variable.

	k=0	*k=0.024*	*k=0.03*	*k=0.04*
2001 reform				
PRE	0.000	0.960	1.000	1.000
POST	0.344	1.000	1.000	1.000
2009 reform				
PRE	0.007	1.000	1.000	1.000
POST	0.026	1.000	1.000	1.000

Notes: the parameter k is the maximum degree of non-linearity in the probability mass function that can be acceptable as compatible with no manipulation.

Section A6: Testing for balance of traits

Table A12: RDD estimations, share of employees in higher education, 2001 reform.

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
PRE, 2001-2008	Treated	0.030	-0.019	0.018	-0.021	-0.026	-0.034
		(0.038)	(0.028)	(0.020)	(0.059)	(0.041)	(0.037)
	N	320	652	1,219	947	1,506	2,000
POST, 2009-2015	Treated	0.055*	0.022	0.031**	0.034	0.004	-0.006
		(0.029)	(0.021)	(0.015)	(0.045)	(0.031)	(0.028)
	N	828	1,625	2,996	2,361	3,634	4,858
B. Equal value work							
PRE, 2001-2008	Treated	0.034	-0.014	0.018	-0.017	-0.019	-0.025
		(0.034)	(0.024)	(0.017)	(0.051)	(0.036)	(0.032)
	N	397	784	1,450	1,118	1,800	2,383
POST, 2009-2015	Treated	0.044*	0.011	0.023*	0.016	-0.006	0.001
		(0.025)	(0.018)	(0.013)	(0.039)	(0.027)	(0.024)
	N	970	1,904	3,504	2,764	4,251	5,708
C. Raw wage gap							
PRE, 2001-2008	Treated	0.003	0.003	0.017**	-0.020	-0.002	0.013
		(0.016)	(0.011)	(0.008)	(0.025)	(0.017)	(0.012)
	N	943	1,896	3,851	2,840	4,856	8,293
POST, 2009-2015	Treated	-0.012	-0.006	0.003	-0.025	-0.016	-0.008
		(0.020)	(0.013)	(0.009)	(0.030)	(0.020)	(0.015)
	N	720	1,506	3,046	2,291	3,786	6,171

Notes: Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table A13: RDD estimations, share of employees in higher education, 2009 reform

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
PRE, 2001-2008	Treated	0.015	0.008	0.021	-0.004	0.018	0.001
		(0.037)	(0.026)	(0.018)	(0.056)	(0.038)	(0.028)
	N	586	1,168	2,377	1,759	2,979	4,854
POST, 2009-2015	Treated	0.018	0.026	0.009	0.046	0.015	0.011
		(0.040)	(0.027)	(0.019)	(0.060)	(0.041)	(0.030)
	N	494	1,040	2,139	1,598	2,670	4,221
B. Equal value work							
PRE, 2001-2008	Treated	0.017	0.010	0.020	-0.004	0.027	0.007
		(0.033)	(0.023)	(0.016)	(0.050)	(0.034)	(0.025)
	N	666	1,315	2,671	1,971	3,336	5,514
POST, 2009-2015	Treated	-0.011	0.000	0.007	-0.025	-0.007	-0.009
		(0.036)	(0.025)	(0.018)	(0.055)	(0.037)	(0.027)
	N	546	1,121	2,259	1,693	2,829	4,514
C. Raw wage gap							
PRE, 2001-2008	Treated	0.003	0.003	0.017**	-0.020	-0.002	0.013
		(0.016)	(0.011)	(0.008)	(0.025)	(0.017)	(0.012)
	N	943	1,896	3,851	2,840	4,856	8,293
POST, 2009-2015	Treated	-0.012	-0.006	0.003	-0.025	-0.016	-0.008
		(0.020)	(0.013)	(0.009)	(0.030)	(0.020)	(0.015)
	N	720	1,506	3,046	2,291	3,786	6,171

Notes: Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.

Table A14: RDD estimations, share of employees $<$ 6 years of work exp, 2001 reform.

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
PRE, 2001-2008	Treated	-0.010	-0.044	-0.055**	0.006	-0.006	-0.045
		(0.045)	(0.033)	(0.024)	(0.070)	(0.049)	(0.044)
	N	320	652	1,219	947	1,506	2,000
POST, 2009-2015	Treated	0.038	0.001	-0.023	0.092**	0.033	0.004
		(0.027)	(0.020)	(0.015)	(0.042)	(0.030)	(0.027)
	N	828	1,625	2,996	2,361	3,634	4,858
B. Equal value work							
PRE, 2001-2008	Treated	-0.020	-0.046	-0.062***	-0.002	-0.024	-0.051
		(0.038)	(0.029)	(0.022)	(0.060)	(0.042)	(0.039)
	N	397	784	1,450	1,118	1,800	2,383
POST, 2009-2015	Treated	0.031	-0.003	-0.027**	0.082**	0.036	0.016
		(0.024)	(0.018)	(0.013)	(0.037)	(0.027)	(0.024)
	N	970	1,904	3,504	2,764	4,251	5,708
C. Raw wage gap							
PRE, 2001-2008	Treated	0.007	0.008	0.006	0.016	0.009	0.004
		(0.010)	(0.007)	(0.005)	(0.015)	(0.010)	(0.008)
	N	943	1,896	3,851	2,840	4,856	8,293
POST, 2009-2015	Treated	0.011	0.004	0.006	0.026*	-0.005	0.006
		(0.010)	(0.007)	(0.005)	(0.016)	(0.011)	(0.008)
	N	720	1,506	3,046	2,291	3,786	6,171

Notes: Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table A15: RDD estimations, share of employees < 6 years of work exp., 2009 reform.

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
	Treated	0.035	0.004	-0.001	0.047	0.016	0.000
PRE, 2001-2008		(0.029)	(0.021)	(0.014)	(0.045)	(0.031)	(0.023)
	N	586	1,168	2,377	1,759	2,979	4,854
	Treated	-0.003	0.000	0.012	0.023	-0.041	0.020
POST, 2009-2015		(0.027)	(0.019)	(0.014)	(0.041)	(0.029)	(0.021)
	N	494	1,040	2,139	1,598	2,670	4,221
B. Equal value work							
	Treated	0.023	0.005	0.001	0.040	0.015	-0.004
PRE, 2001-2008		(0.026)	(0.019)	(0.013)	(0.040)	(0.028)	(0.020)
	N	666	1,315	2,671	1,971	3,336	5,514
	Treated	-0.011	-0.000	0.007	0.027	-0.045*	0.004
POST, 2009-2015		(0.025)	(0.018)	(0.013)	(0.038)	(0.027)	(0.020)
	N	546	1,121	2,259	1,693	2,829	4,514
C. Raw wage gap							
	Treated	0.007	0.008	0.006	0.016	0.009	0.004
PRE, 2001-2008		(0.010)	(0.007)	(0.005)	(0.015)	(0.010)	(0.008)
	N	943	1,896	3,851	2,840	4,856	8,293
	Treated	0.011	0.004	0.006	0.026*	-0.005	0.006
POST, 2009-2015		(0.010)	(0.007)	(0.005)	(0.016)	(0.011)	(0.008)
	N	720	1,506	3,046	2,291	3,786	6,171

Notes: Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table A16: RDD estimations, share of employees < 11 yr of working exp., 2001 reform.

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
PRE, 2001-2008	Treated	0.044	-0.030	-0.067**	0.120	0.003	-0.050
		(0.053)	(0.038)	(0.028)	(0.082)	(0.057)	(0.051)
	N	320	652	1,219	947	1,506	2,000
POST, 2009-2015	Treated	-0.011	-0.012	-0.022	0.016	0.002	-0.025
		(0.032)	(0.023)	(0.017)	(0.049)	(0.035)	(0.032)
	N	828	1,625	2,996	2,361	3,634	4,858
B. Equal value work							
PRE, 2001-2008	Treated	0.024	-0.029	-0.079***	0.103	-0.018	-0.057
		(0.047)	(0.034)	(0.025)	(0.073)	(0.051)	(0.045)
	N	397	784	1,450	1,118	1,800	2,383
POST, 2009-2015	Treated	-0.004	-0.015	-0.028*	0.027	0.018	0.005
		(0.028)	(0.020)	(0.015)	(0.044)	(0.031)	(0.028)
	N	970	1,904	3,504	2,764	4,251	5,708
C. Raw wage gap							
PRE, 2001-2008	Treated	0.010	0.008	0.008	0.017	0.008	0.008
		(0.013)	(0.009)	(0.006)	(0.020)	(0.013)	(0.010)
	N	943	1,896	3,851	2,840	4,856	8,293
POST, 2009-2015	Treated	0.015	0.016	0.011	0.047**	0.001	0.008
		(0.014)	(0.010)	(0.007)	(0.022)	(0.015)	(0.011)
	N	720	1,506	3,046	2,291	3,786	6,171

Notes: Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table A17: RDD estimations, share of employees < 11 yr of working exp., 2009 reform.

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
PRE, 2001-2008	Treated	0.032	-0.020	-0.001	0.015	-0.002	-0.000
		(0.035)	(0.024)	(0.017)	(0.053)	(0.036)	(0.027)
	N	586	1,168	2,377	1,759	2,979	4,854
POST, 2009-2015	Treated	0.053	0.041*	0.035**	0.106**	0.012	0.055**
		(0.034)	(0.024)	(0.017)	(0.052)	(0.036)	(0.026)
	N	494	1,040	2,139	1,598	2,670	4,221
B. Equal value work							
PRE, 2001-2008	Treated	0.012	-0.015	0.002	0.003	-0.004	-0.009
		(0.032)	(0.022)	(0.016)	(0.049)	(0.033)	(0.024)
	N	666	1,315	2,671	1,971	3,336	5,514
POST, 2009-2015	Treated	0.037	0.035	0.031*	0.091*	0.004	0.033
		(0.033)	(0.023)	(0.016)	(0.050)	(0.034)	(0.025)
	N	546	1,121	2,259	1,693	2,829	4,514
C. Raw wage gap							
PRE, 2001-2008	Treated	0.010	0.008	0.008	0.017	0.008	0.008
		(0.013)	(0.009)	(0.006)	(0.020)	(0.013)	(0.010)
	N	943	1,896	3,851	2,840	4,856	8,293
POST, 2009-2015	Treated	0.015	0.016	0.011	0.047**	0.001	0.008
		(0.014)	(0.010)	(0.007)	(0.022)	(0.015)	(0.011)
	N	720	1,506	3,046	2,291	3,786	6,171

Notes: Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table A18: RDD estimations, share of employees with <3 yrs of tenure, 2001 reform

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
PRE, 2001-2008	Treated	0.005	-0.002	-0.024	0.034	0.009	0.014
		(0.053)	(0.038)	(0.028)	(0.082)	(0.057)	(0.051)
	N	320	652	1,219	947	1,506	2,000
POST, 2009-2015	Treated	-0.016	-0.017	-0.018	-0.039	0.012	-0.021
		(0.034)	(0.024)	(0.018)	(0.052)	(0.037)	(0.033)
	N	828	1,625	2,996	2,361	3,634	4,858
B. Equal value work							
PRE, 2001-2008	Treated	0.001	-0.010	-0.042*	0.043	-0.014	-0.006
		(0.047)	(0.034)	(0.026)	(0.073)	(0.051)	(0.046)
	N	397	784	1,450	1,118	1,800	2,383
POST, 2009-2015	Treated	0.008	-0.008	-0.025	0.020	0.036	0.012
		(0.031)	(0.022)	(0.016)	(0.048)	(0.034)	(0.031)
	N	970	1,904	3,504	2,764	4,251	5,708
C. Raw wage gap							
PRE, 2001-2008	Treated	0.032**	0.015	0.017**	0.029	0.028*	0.023*
		(0.016)	(0.011)	(0.008)	(0.025)	(0.017)	(0.013)
	N	943	1,896	3,851	2,840	4,856	8,293
POST, 2009-2015	Treated	0.002	0.002	0.006	0.015	-0.013	0.008
		(0.018)	(0.012)	(0.009)	(0.027)	(0.019)	(0.014)
	N	720	1,506	3,046	2,291	3,786	6,171

Notes: Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table A19: RDD estimations, share of employees with <3 yrs of tenure, 2009 reform

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
PRE, 2001-2008	Treated	0.024	-0.019	-0.000	-0.019	0.013	0.008
		(0.036)	(0.026)	(0.018)	(0.055)	(0.038)	(0.028)
	N	586	1,168	2,377	1,759	2,979	4,854
POST, 2009-2015	Treated	0.043	0.037	0.012	0.112**	0.023	0.049*
		(0.036)	(0.025)	(0.017)	(0.054)	(0.037)	(0.027)
	N	494	1,040	2,139	1,598	2,670	4,221
B. Equal value work							
PRE, 2001-2008	Treated	0.034	0.001	-0.001	0.037	0.021	0.004
		(0.034)	(0.024)	(0.017)	(0.051)	(0.035)	(0.026)
	N	666	1,315	2,671	1,971	3,336	5,514
POST, 2009-2015	Treated	0.029	0.034	0.007	0.108**	0.003	0.029
		(0.034)	(0.024)	(0.017)	(0.051)	(0.035)	(0.026)
	N	546	1,121	2,259	1,693	2,829	4,514
C. Raw wage gap							
PRE, 2001-2008	Treated	0.032**	0.015	0.017**	0.029	0.028*	0.023*
		(0.016)	(0.011)	(0.008)	(0.025)	(0.017)	(0.013)
	N	943	1,896	3,851	2,840	4,856	8,293
POST, 2009-2015	Treated	0.002	0.002	0.006	0.015	-0.013	0.008
		(0.018)	(0.012)	(0.009)	(0.027)	(0.019)	(0.014)
	N	720	1,506	3,046	2,291	3,786	6,171

Notes: Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table A20: RDD estimations, share of employees in managerial occ., 2001 reform.

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
	Treated	-0.001	0.003	-0.002	0.005	0.002	0.012
PRE, 2001-2008		(0.015)	(0.013)	(0.011)	(0.027)	(0.018)	(0.017)
	N	320	652	1,219	947	1,506	2,000
	Treated	-0.015	-0.001	-0.007	-0.002	0.007	0.016
POST, 2009-2015		(0.013)	(0.009)	(0.007)	(0.021)	(0.015)	(0.014)
	N	828	1,625	2,996	2,361	3,634	4,858
B. Raw wage gap							
	Treated	0.005	0.005	-0.000	0.013	0.004	0.001
PRE, 2001-2008		(0.005)	(0.004)	(0.003)	(0.008)	(0.006)	(0.004)
	N	943	1,896	3,851	2,840	4,856	8,293
	Treated	-0.009	-0.007	-0.005*	-0.007	-0.014**	-0.009*
POST, 2009-2015		(0.006)	(0.004)	(0.003)	(0.010)	(0.007)	(0.005)
	N	720	1,506	3,046	2,291	3,786	6,171

Notes: Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1. No estimates for equal valued work sample, since managers are excluded from the valuation methodology.

Table A21: RDD estimations , share of employees in managerial occ., 2009 reform.

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
PRE, 2001-2008	Treated	0.002	0.007	0.004	0.012	0.010	0.003
		(0.012)	(0.010)	(0.007)	(0.019)	(0.014)	(0.010)
	N	586	1,168	2,377	1,759	2,979	4,854
POST, 2009-2015	Treated	-0.030**	-0.016	-0.019***	-0.016	-0.034**	-0.035***
		(0.014)	(0.010)	(0.007)	(0.022)	(0.014)	(0.011)
	N	494	1,040	2,139	1,598	2,670	4,221
B. Raw wage gap							
PRE, 2001-2008	Treated	0.005	0.005	-0.000	0.013	0.004	0.001
		(0.005)	(0.004)	(0.003)	(0.008)	(0.006)	(0.004)
	N	943	1,896	3,851	2,840	4,856	8,293
POST, 2009-2015	Treated	-0.009	-0.007	-0.005*	-0.007	-0.014**	-0.009*
		(0.006)	(0.004)	(0.003)	(0.010)	(0.007)	(0.005)
	N	720	1,506	3,046	2,291	3,786	6,171

Notes: Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1. No estimates for equal valued work sample, since managers are excluded from the valuation methodology

Table A20: RDD estimations, share of employees in occupations requiring higher education (SSYK=1 or 2), 2001 reform.

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
	Treated	-0.040	-0.026	0.002	-0.097	-0.053	-0.051
PRE, 2001-2008		(0.041)	(0.028)	(0.020)	(0.061)	(0.042)	(0.038)
	N	320	652	1,219	947	1,506	2,000
	Treated	0.052*	0.044**	0.031**	0.092**	0.043	0.034
POST, 2009-2015		(0.028)	(0.020)	(0.015)	(0.043)	(0.030)	(0.027)
	N	828	1,625	2,996	2,361	3,634	4,858
B. Equal value work							
	Treated	-0.022	-0.020	0.006	-0.066	-0.041	-0.050*
PRE, 2001-2008		(0.032)	(0.022)	(0.016)	(0.048)	(0.033)	(0.030)
	N	397	784	1,450	1,118	1,800	2,383
	Treated	0.052**	0.029*	0.031***	0.065*	0.025	0.015
POST, 2009-2015		(0.022)	(0.016)	(0.012)	(0.034)	(0.024)	(0.022)
	N	970	1,904	3,504	2,764	4,251	5,708
C. Raw wage gap							
	Treated	-0.011	-0.005	0.000	-0.027	-0.011	0.005
PRE, 2001-2008		(0.016)	(0.011)	(0.008)	(0.024)	(0.016)	(0.012)
	N	943	1,896	3,851	2,840	4,856	8,293
	Treated	-0.004	0.001	0.005	-0.021	0.006	0.010
POST, 2009-2015		(0.020)	(0.014)	(0.010)	(0.030)	(0.020)	(0.015)
	N	720	1,506	3,046	2,291	3,786	6,171

Notes: Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table A21: RDD estimations, share of employees in occupations requiring higher education (SSYK=1 or 2), 2001 reform.

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
	Treated	0.001	0.010	-0.001	-0.001	0.008	-0.000
PRE, 2001-2008		(0.036)	(0.025)	(0.018)	(0.054)	(0.037)	(0.027)
	N	586	1,168	2,377	1,759	2,979	4,854
	Treated	-0.000	0.012	-0.008	0.021	0.015	-0.004
POST, 2009-2015		(0.039)	(0.026)	(0.019)	(0.059)	(0.039)	(0.029)
	N	494	1,040	2,139	1,598	2,670	4,221
B. Equal value work							
	Treated	0.002	0.012	0.008	-0.010	0.014	0.014
PRE, 2001-2008		(0.031)	(0.021)	(0.015)	(0.047)	(0.032)	(0.023)
	N	666	1,315	2,671	1,971	3,336	5,514
	Treated	0.010	0.007	0.005	-0.004	0.014	0.013
POST, 2009-2015		(0.034)	(0.023)	(0.016)	(0.051)	(0.035)	(0.025)
	N	546	1,121	2,259	1,693	2,829	4,514
C. Raw wage gap							
	Treated	-0.011	-0.005	0.000	-0.027	-0.011	0.005
PRE, 2001-2008		(0.016)	(0.011)	(0.008)	(0.024)	(0.016)	(0.012)
	N	943	1,896	3,851	2,840	4,856	8,293
	Treated	-0.004	0.001	0.005	-0.021	0.006	0.010
POST, 2009-2015		(0.020)	(0.014)	(0.010)	(0.030)	(0.020)	(0.015)
	N	720	1,506	3,046	2,291	3,786	6,171

Notes: Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table A22: RDD estimations, share of manufacturing, retail or services, 2001 reform.

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
PRE, 2001-2008	Treated	-0.046	0.002	0.032	-0.090	-0.041	-0.005
		(0.052)	(0.037)	(0.027)	(0.080)	(0.056)	(0.050)
	N	320	652	1,219	947	1,506	2,000
POST, 2009-2015	Treated	0.016	0.033	0.076***	-0.004	-0.024	0.022
		(0.031)	(0.022)	(0.017)	(0.048)	(0.034)	(0.031)
	N	828	1,625	2,996	2,361	3,634	4,858
B. Equal value work							
PRE, 2001-2008	Treated	-0.022	0.022	0.052**	-0.075	-0.025	0.013
		(0.046)	(0.033)	(0.025)	(0.072)	(0.050)	(0.045)
	N	397	784	1,450	1,118	1,800	2,383
POST, 2009-2015	Treated	-0.016	0.018	0.064***	-0.054	-0.058*	-0.011
		(0.028)	(0.020)	(0.015)	(0.044)	(0.031)	(0.028)
	N	970	1,904	3,504	2,764	4,251	5,708
C. Raw wage gap							
PRE, 2001-2008	Treated	-0.033	-0.006	0.024*	-0.062	-0.033	-0.017
		(0.025)	(0.018)	(0.013)	(0.039)	(0.027)	(0.024)
	N	1,287	2,517	4,891	3,716	6,152	7,340
POST, 2009-2015	Treated	0.007	0.027**	0.035***	0.006	-0.015	0.005
		(0.015)	(0.011)	(0.008)	(0.024)	(0.016)	(0.015)
	N	3,189	6,278	11,822	9,107	14,396	17,294

Notes: Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table A23: RDD estimations, share of manufacturing, retail or services, 2009 reform.

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
PRE, 2001-2008	Treated	-0.053	-0.046*	-0.046**	-0.016	-0.091**	-0.059**
		(0.038)	(0.026)	(0.018)	(0.057)	(0.039)	(0.029)
	N	586	1,168	2,377	1,759	2,979	4,854
POST, 2009-2015	Treated	-0.023	-0.006	0.026	-0.089	0.028	0.008
		(0.040)	(0.028)	(0.019)	(0.061)	(0.042)	(0.031)
	N	494	1,040	2,139	1,598	2,670	4,221
B. Equal value work							
PRE, 2001-2008	Treated	-0.062*	-0.047*	-0.049***	-0.031	-0.102**	-0.053**
		(0.035)	(0.024)	(0.017)	(0.053)	(0.036)	(0.027)
	N	666	1,315	2,671	1,971	3,336	5,514
POST, 2009-2015	Treated	-0.008	-0.001	0.018	-0.047	0.027	0.016
		(0.039)	(0.027)	(0.019)	(0.059)	(0.040)	(0.030)
	N	546	1,121	2,259	1,693	2,829	4,514
C. Raw wage gap							
PRE, 2001-2008	Treated	-0.050*	-0.032	-0.035**	-0.021	-0.071**	-0.031
		(0.029)	(0.021)	(0.014)	(0.044)	(0.030)	(0.023)
	N	943	1,896	3,851	2,840	4,856	8,293
POST, 2009-2015	Treated	-0.033	-0.012	0.003	-0.076	0.009	0.000
		(0.034)	(0.023)	(0.017)	(0.051)	(0.035)	(0.026)
	N	720	1,506	3,046	2,291	3,786	6,171

Notes: Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table A24: RDD estimations, share of manufacturing, 2001 reform.

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
PRE, 2001-2008	Treated	0.004	0.028	0.047**	-0.023	0.001	0.017
		(0.045)	(0.032)	(0.023)	(0.069)	(0.047)	(0.042)
	N	320	652	1,219	947	1,506	2,000
POST, 2009-2015	Treated	0.001	0.003	0.044***	-0.051	-0.020	0.010
		(0.026)	(0.019)	(0.014)	(0.041)	(0.029)	(0.026)
	N	828	1,625	2,996	2,361	3,634	4,858
B. Equal value work							
PRE, 2001-2008	Treated	-0.017	0.004	0.035	-0.046	-0.032	-0.013
		(0.043)	(0.030)	(0.022)	(0.067)	(0.045)	(0.041)
	N	397	784	1,450	1,118	1,800	2,383
POST, 2009-2015	Treated	-0.005	0.013	0.039***	-0.038	-0.016	0.004
		(0.026)	(0.019)	(0.014)	(0.041)	(0.028)	(0.026)
	N	970	1,904	3,504	2,764	4,251	5,708
C. Raw wage gap							
PRE, 2001-2008	Treated	0.005	0.021	0.031***	-0.007	-0.014	-0.006
		(0.022)	(0.015)	(0.011)	(0.034)	(0.023)	(0.020)
	N	1,287	2,517	4,891	3,716	6,152	7,340
POST, 2009-2015	Treated	0.018	0.022**	0.034***	-0.000	0.007	0.008
		(0.014)	(0.010)	(0.007)	(0.021)	(0.015)	(0.013)
	N	3,189	6,278	11,822	9,107	14,396	17,294

Notes: Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table A25: RDD estimations, share of manufacturing, 2009 reform.

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
PRE, 2001-2008	Treated	0.015	-0.006	-0.021	0.053	-0.008	-0.024
		(0.031)	(0.022)	(0.016)	(0.047)	(0.033)	(0.024)
	N	586	1,168	2,377	1,759	2,979	4,854
POST, 2009-2015	Treated	0.014	0.022	0.017	0.005	0.030	0.022
		(0.033)	(0.023)	(0.016)	(0.050)	(0.034)	(0.026)
	N	494	1,040	2,139	1,598	2,670	4,221
B. Equal value work							
PRE, 2001-2008	Treated	-0.005	-0.011	-0.027*	0.029	-0.023	-0.027
		(0.031)	(0.022)	(0.016)	(0.048)	(0.033)	(0.024)
	N	666	1,315	2,671	1,971	3,336	5,514
POST, 2009-2015	Treated	0.037	0.030	0.016	0.029	0.050	0.037
		(0.033)	(0.023)	(0.016)	(0.051)	(0.034)	(0.026)
	N	546	1,121	2,259	1,693	2,829	4,514
C. Raw wage gap							
PRE, 2001-2008	Treated	0.014	-0.015	-0.022	0.034	-0.025	-0.021
		(0.027)	(0.018)	(0.013)	(0.041)	(0.028)	(0.021)
	N	943	1,896	3,851	2,840	4,856	8,293
POST, 2009-2015	Treated	0.038	0.026	0.006	0.033	0.049	0.022
		(0.030)	(0.021)	(0.015)	(0.046)	(0.031)	(0.023)
	N	720	1,506	3,046	2,291	3,786	6,171

Notes: Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table A26: RDD estimations, share of retail, 2001 reform.

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
	Treated	0.041	0.033	-0.004	0.081	0.075	0.023
PRE, 2001-2008		(0.045)	(0.030)	(0.022)	(0.068)	(0.046)	(0.041)
	N	320	652	1,219	947	1,506	2,000
	Treated	-0.039*	0.005	-0.013	0.002	0.003	0.013
POST, 2009-2015		(0.023)	(0.017)	(0.013)	(0.037)	(0.026)	(0.024)
	N	828	1,625	2,996	2,361	3,634	4,858
B. Equal value work							
	Treated	0.034	0.040	0.011	0.052	0.077*	0.039
PRE, 2001-2008		(0.039)	(0.027)	(0.020)	(0.061)	(0.041)	(0.037)
	N	397	784	1,450	1,118	1,800	2,383
	Treated	-0.048**	-0.008	-0.013	-0.027	-0.025	-0.011
POST, 2009-2015		(0.022)	(0.016)	(0.012)	(0.034)	(0.024)	(0.022)
	N	970	1,904	3,504	2,764	4,251	5,708
C. Raw wage gap							
	Treated	0.015	0.004	0.004	0.016	0.032	0.024
PRE, 2001-2008		(0.021)	(0.015)	(0.011)	(0.032)	(0.022)	(0.020)
	N	1,287	2,517	4,891	3,716	6,152	7,340
	Treated	-0.008	0.011	0.005	-0.008	0.022*	0.026**
POST, 2009-2015		(0.012)	(0.008)	(0.006)	(0.018)	(0.013)	(0.011)
	N	3,189	6,278	11,822	9,107	14,396	17,294

Notes: Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table A27: RDD estimations, share of retail, 2009 reform.

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
	Treated	-0.025	-0.016	-0.013	-0.018	-0.028	-0.007
PRE, 2001-2008		(0.025)	(0.019)	(0.013)	(0.038)	(0.027)	(0.020)
	N	-0.025	-0.016	-0.013	-0.018	-0.028	-0.007
	Treated	-0.049**	-0.030*	-0.016*	-0.052*	-0.055**	-0.037**
POST, 2009-2015		(0.019)	(0.013)	(0.009)	(0.030)	(0.020)	(0.015)
	N	494	1,040	2,139	1,598	2,670	4,221
B. Equal value work							
	Treated	-0.025	-0.022	-0.017	-0.018	-0.040	-0.017
PRE, 2001-2008		(0.023)	(0.017)	(0.012)	(0.036)	(0.025)	(0.018)
	N	666	1,315	2,671	1,971	3,336	5,514
	Treated	-0.044**	-0.031*	-0.018**	-0.042	-0.055**	-0.040***
POST, 2009-2015		(0.018)	(0.013)	(0.009)	(0.028)	(0.019)	(0.014)
	N	546	1,121	2,259	1,693	2,829	4,514
C. Raw wage gap							
	Treated	-0.031	-0.014	-0.013	-0.022	-0.025	-0.020
PRE, 2001-2008		(0.019)	(0.014)	(0.010)	(0.029)	(0.020)	(0.015)
	N	943	1,896	3,851	2,840	4,856	8,293
	Treated	-0.047**	-0.022*	-0.013*	-0.042*	-0.043**	-0.032***
POST, 2009-2015		(0.016)	(0.011)	(0.008)	(0.024)	(0.016)	(0.012)
	N	720	1,506	3,046	2,291	3,786	6,171

Notes: Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table A28: RDD estimations, share of service, 2001 reform.

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
PRE, 2001-2008	Treated	-0.040	-0.028	-0.003	-0.053	-0.083*	-0.022
		(0.045)	(0.031)	(0.023)	(0.069)	(0.047)	(0.042)
	N	320	652	1,219	947	1,506	2,000
POST, 2009-2015	Treated	0.045	0.022	0.029*	0.079	-0.030	-0.013
		(0.032)	(0.023)	(0.017)	(0.050)	(0.035)	(0.031)
	N	828	1,625	2,996	2,361	3,634	4,858
B. Equal value work							
PRE, 2001-2008	Treated	-0.008	-0.004	0.005	-0.013	-0.048	0.006
		(0.040)	(0.028)	(0.021)	(0.062)	(0.042)	(0.038)
	N	397	784	1,450	1,118	1,800	2,383
POST, 2009-2015	Treated	0.040	0.015	0.027*	0.056	-0.035	-0.016
		(0.030)	(0.021)	(0.016)	(0.046)	(0.032)	(0.029)
	N	970	1,904	3,504	2,764	4,251	5,708
C. Raw wage gap							
PRE, 2001-2008	Treated	-0.009	-0.005	-0.006	0.004	-0.023	-0.007
		(0.023)	(0.016)	(0.012)	(0.035)	(0.024)	(0.021)
	N	1,287	2,517	4,891	3,716	6,152	7,340
POST, 2009-2015	Treated	-0.007	-0.019	-0.020**	0.021	-0.066**	-0.058***
		(0.017)	(0.012)	(0.009)	(0.026)	(0.018)	(0.016)
	N	3,189	6,278	11,822	9,107	14,396	17,294

Notes: Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table A29: RDD estimations, share of service, 2009 reform.

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
PRE, 2001-2008	Treated	-0.018	-0.005	0.004	-0.030	-0.044	-0.007
		(0.038)	(0.027)	(0.019)	(0.058)	(0.040)	(0.030)
	N	586	1,168	2,377	1,759	2,979	4,854
POST, 2009-2015	Treated	0.005	0.020	0.018	-0.021	0.068	0.020
		(0.043)	(0.029)	(0.020)	(0.064)	(0.043)	(0.032)
	N	494	1,040	2,139	1,598	2,670	4,221
B. Equal value work							
PRE, 2001-2008	Treated	-0.005	-0.002	0.005	-0.023	-0.034	0.009
		(0.035)	(0.025)	(0.018)	(0.054)	(0.038)	(0.028)
	N	666	1,315	2,671	1,971	3,336	5,514
POST, 2009-2015	Treated	-0.011	0.007	0.005	-0.026	0.042	0.010
		(0.040)	(0.028)	(0.020)	(0.061)	(0.042)	(0.031)
	N	546	1,121	2,259	1,693	2,829	4,514
C. Raw wage gap							
PRE, 2001-2008	Treated	-0.012	-0.002	0.005	-0.034	-0.023	0.022
		(0.030)	(0.021)	(0.015)	(0.045)	(0.031)	(0.023)
	N	943	1,896	3,851	2,840	4,856	8,293
POST, 2009-2015	Treated	-0.012	-0.005	0.001	-0.035	0.011	0.010
		(0.034)	(0.023)	(0.016)	(0.051)	(0.035)	(0.026)
	N	720	1,506	3,046	2,291	3,786	6,171

Notes: Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table A30: RDD estimations, share of female employees, 2001 reform.

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
PRE, 2001-2008	Treated	-0.002	-0.004	-0.001	-0.006	0.001	0.001
		(0.013)	(0.009)	(0.007)	(0.020)	(0.014)	(0.012)
	N	320	652	1,219	947	1,506	2,000
POST, 2009-2015	Treated	0.008	0.003	-0.004	0.019	0.009	0.009
		(0.008)	(0.006)	(0.004)	(0.012)	(0.009)	(0.008)
	N	828	1,625	2,996	2,361	3,634	4,858
B. Equal value work							
PRE, 2001-2008	Treated	-0.004	-0.000	-0.001	-0.005	0.006	0.001
		(0.012)	(0.009)	(0.006)	(0.018)	(0.013)	(0.011)
	N	397	784	1,450	1,118	1,800	2,383
POST, 2009-2015	Treated	0.008	0.002	-0.005	0.017	0.012	0.007
		(0.008)	(0.005)	(0.004)	(0.012)	(0.008)	(0.007)
	N	970	1,904	3,504	2,764	4,251	5,708
C. Raw wage gap							
PRE, 2001-2008	Treated	-0.003	-0.005	-0.008	0.002	0.016	0.015
		(0.014)	(0.010)	(0.007)	(0.021)	(0.014)	(0.013)
	N	1,287	2,517	4,891	3,716	6,152	7,340
POST, 2009-2015	Treated	-0.019**	-0.011*	-0.019***	-0.010	-0.010	-0.011
		(0.009)	(0.006)	(0.004)	(0.013)	(0.009)	(0.008)
	N	3,189	6,278	11,822	9,107	14,396	17,294

Notes: Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table A31: RDD estimations, share of female employees, 2009 reform.

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
	Treated	0.010	0.001	0.004	0.011	0.005	0.004
PRE, 2001-2008		(0.012)	(0.008)	(0.006)	(0.018)	(0.012)	(0.009)
	N	586	1,168	2,377	1,759	2,979	4,854
	Treated	-0.001	-0.007	-0.010	0.009	-0.006	-0.017*
POST, 2009-2015		(0.012)	(0.008)	(0.006)	(0.018)	(0.013)	(0.009)
	N	494	1,040	2,139	1,598	2,670	4,221
B. Equal value work							
	Treated	0.020*	0.011	0.011**	0.023	0.018	0.018**
PRE, 2001-2008		(0.011)	(0.008)	(0.006)	(0.017)	(0.012)	(0.009)
	N	666	1,315	2,671	1,971	3,336	5,514
	Treated	-0.001	-0.005	-0.007	0.011	-0.000	-0.014
POST, 2009-2015		(0.012)	(0.008)	(0.006)	(0.018)	(0.013)	(0.010)
	N	546	1,121	2,259	1,693	2,829	4,514
C. Raw wage gap							
	Treated	-0.002	0.016	0.019**	-0.018	0.010	0.030**
PRE, 2001-2008		(0.016)	(0.012)	(0.008)	(0.025)	(0.017)	(0.013)
	N	943	1,896	3,851	2,840	4,856	8,293
	Treated	-0.008	-0.010	-0.013	-0.001	-0.012	-0.018
POST, 2009-2015		(0.019)	(0.013)	(0.009)	(0.028)	(0.019)	(0.014)
	N	720	1,506	3,046	2,291	3,786	6,171

Notes: Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Section A7: Alternative estimations of the gender wage gap

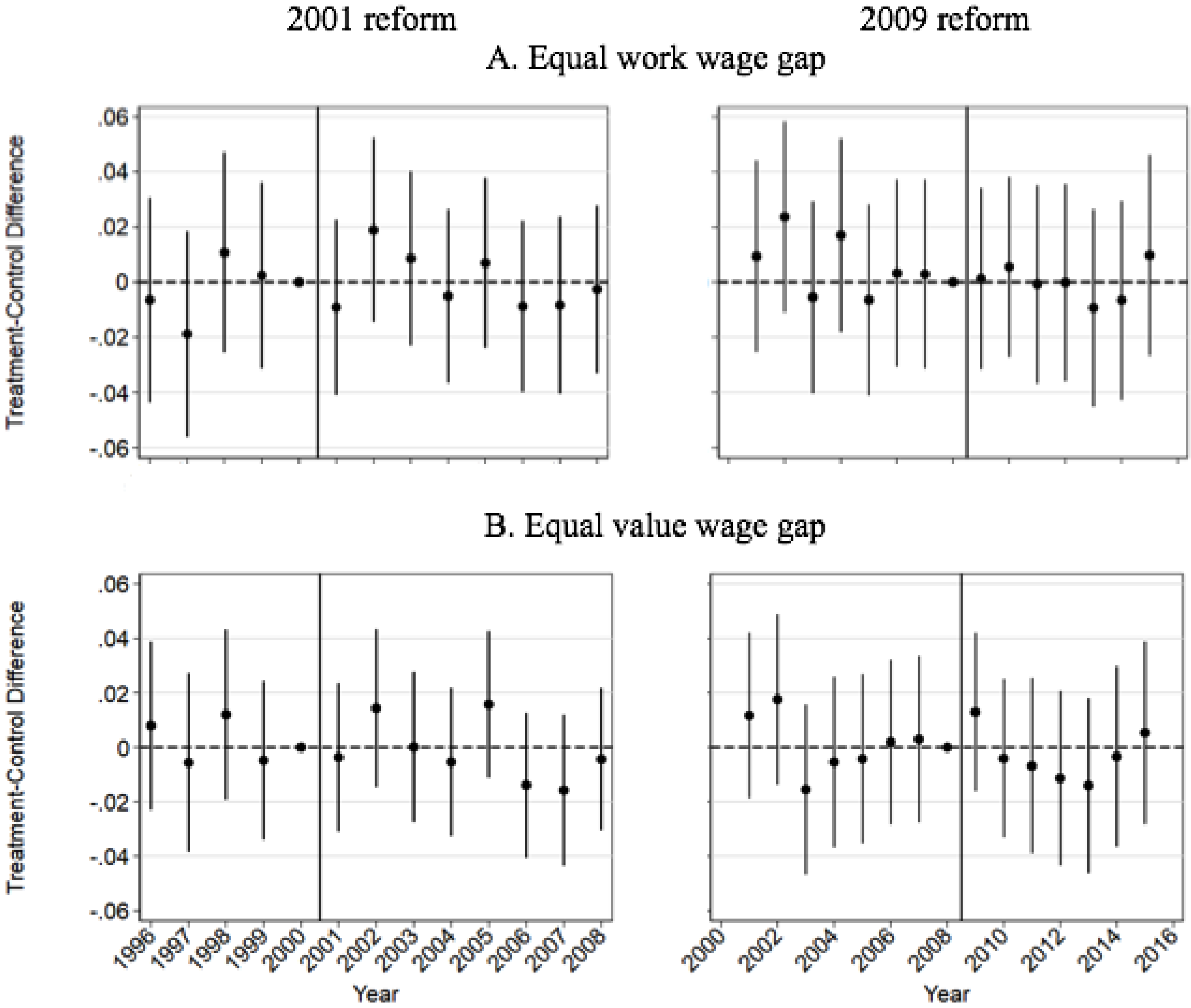

Figure A2: Difference-in-difference estimates for the gender wage gaps estimated for women's wage disadvantages only, 2001 and 2009 reform.

Notes: The Figure shows estimates from Equation (2) on pooled cross-sections of firm-level data. Vertical lines mark 95% confidence intervals. The left-hand side of plots show the analysis of the 2001 reform that assigned the gender wage survey to firms with 10-50 employees. The right-hand side plots show the results for the 2009 reform that kept the mandate from firms with 25-50 employees. The top row contains the results for the gender wage gap for equal work; the middle row for the wage gap for work of equal value; and the bottom row the raw wage gap. The estimation details for these gaps can be found in Section (5).

Table A32: RDD estimations for the gender wage gaps estimated for women's wage disadvantages only, 2001 reform.

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
PRE, 1996—2000	Mandate	-0.007	-0.003	-0.001	-0.025	-0.005	-0.002
		(0.014)	(0.010)	(0.007)	(0.022)	(0.015)	(0.013)
	N	213	427	838	631	1,046	1,397
POST, 2001—2008	Mandate	0.012	0.009	0.010**	0.008	0.009	0.007
		(0.009)	(0.007)	(0.005)	(0.014)	(0.010)	(0.009)
	N	570	1,115	2,087	1,651	2,530	3,390
B. Equal value work							
PRE, 1996—2000	Mandate	-0.002	-0.000	0.000	-0.017	-0.003	0.002
		(0.013)	(0.009)	(0.007)	(0.020)	(0.013)	(0.012)
	N	260	502	977	732	1,234	1,641
POST, 2001—2008	Mandate	0.013	0.010*	0.008*	0.013	0.016*	0.009
		(0.008)	(0.006)	(0.004)	(0.012)	(0.009)	(0.008)
	N	656	1,287	2,398	1,897	2,925	3,928

Notes: The table shows estimates from Equation (3) and (4) and for the wage survey reform that introduced the mandate for firms with 10 or more employees (mandate=1), but not for firms with <10 employees (mandate=0), but on a reduced sample considering only wage gaps where women are underpaid. Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table A33: RDD estimations for the gender wage gaps estimated for women's wage disadvantages only, 2009 reform.

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
PRE, 1996—2000	Mandate	-0.003	-0.001	-0.003	-0.002	-0.003	-0.004
		(0.010)	(0.007)	(0.005)	(0.016)	(0.010)	(0.008)
	N	440	875	1,762	1,311	2,220	3,586
POST, 2001—2008	Mandate	-0.006	0.001	0.004	-0.007	0.001	0.001
		(0.010)	(0.007)	(0.005)	(0.016)	(0.011)	(0.008)
	N	347	749	1,573	1,180	1,947	3,075
B. Equal value work							
PRE, 1996—2000	Mandate	-0.004	-0.003	-0.002	-0.010	-0.002	-0.004
		(0.009)	(0.006)	(0.004)	(0.014)	(0.009)	(0.007)
	N	488	962	1,920	1,417	2,401	3,938
POST, 2001—2008	Mandate	0.004	0.006	0.007	0.004	0.011	0.006
		(0.009)	(0.006)	(0.004)	(0.014)	(0.010)	(0.007)
	N	369	801	1,656	1,233	2,065	3,286

Notes: See notes for Table A33, but for the wage survey reform that kept the mandate for firms with 25-50 employees (mandate=1) while removing it from firms with 10-24 employees (mandate=0).

Table A34: RDD estimations for alternative specification 1, Equation (5), for the gender wage gap, 2001 reform.

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
PRE, 1996—2000	Mandate	0.004	0.006	0.004	0.004	0.005	0.002
		(0.007)	(0.005)	(0.004)	(0.011)	(0.007)	(0.007)
	N	823	1,630	3,193	2,412	3,973	4,896
POST, 2001—2008	Mandate	0.007*	0.010**	0.013***	0.003	0.009**	0.009**
		(0.004)	(0.003)	(0.002)	(0.006)	(0.004)	(0.004)
	N	2,401	4,709	8,857	6,864	10,703	13,155
B. Equal value work							
PRE, 1996—2000	Mandate	0.000	0.002	-0.001	-0.000	0.003	-0.000
		(0.006)	(0.004)	(0.003)	(0.010)	(0.007)	(0.006)
	N	994	1,939	3,728	2,833	4,622	5,657
POST, 2001—2008	Mandate	0.007**	0.009**	0.012***	0.004	0.007*	0.009***
		(0.003)	(0.002)	(0.002)	(0.005)	(0.004)	(0.003)
	N	2,663	5,218	9,776	7,567	11,789	14,412

Notes: The table shows estimates from Equation (3) and (4) and for the wage survey reform that introduced the mandate for firms with 10 or more employees (mandate=1), but not for firms with <10 employees (mandate=0), but with the wage gap estimated according to Equation (5). Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table A35: RDD estimations for alternative specification 1, Equation (5), for the gender wage gap, 2009 reform.

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
PRE, 1996—2000	Mandate	-0.013**	-0.005	-0.007***	-0.011	-0.006	-0.002
		(0.006)	(0.004)	(0.003)	(0.009)	(0.006)	(0.004)
	N	863	1,727	3,500	2,588	4,381	7,453
POST, 2001—2008	Mandate	0.002	-0.001	-0.008***	0.007	0.006	-0.005
		(0.006)	(0.004)	(0.003)	(0.009)	(0.006)	(0.005)
	N	658	1,379	2,806	2,102	3,501	5,662
B. Equal value work							
PRE, 1996—2000	Mandate	0.001	0.004	-0.001	0.009	0.004	0.006
		(0.006)	(0.004)	(0.003)	(0.009)	(0.006)	(0.004)
	N	909	1,820	3,675	2,719	4,600	7,837
POST, 2001—2008	Mandate	-0.004	-0.005	-0.006**	-0.005	0.001	-0.004
		(0.006)	(0.004)	(0.003)	(0.009)	(0.006)	(0.004)
	N	683	1,428	2,894	2,175	3,602	5,850

Notes: See notes for Table A34, but for the wage survey reform that kept the mandate for firms with 25-50 employees (mandate=1) while removing it from firms with 10-24 employees (mandate=0).

Table A36: RDD estimations for alt. wage gap estimation in Eq. (6), 2001 reform.

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
PRE, 1996—2000	Mandate	-0.001	0.002	0.001	-0.003	0.001	-0.002
		(0.007)	(0.005)	(0.003)	(0.010)	(0.007)	(0.006)
	N	821	1,626	3,188	2,405	3,967	4,890
POST, 2001—2008	Mandate	0.006*	0.009**	0.012***	0.001	0.008**	0.008**
		(0.004)	(0.003)	(0.002)	(0.006)	(0.004)	(0.003)
	N	2,395	4,704	8,850	6,856	10,694	13,141
B. Equal value work							
PRE, 1996—2000	Mandate	0.002	0.000	-0.003	0.000	0.004	-0.002
		(0.006)	(0.004)	(0.003)	(0.009)	(0.006)	(0.006)
	N	996	1,938	3,722	2,828	4,615	5,648
POST, 2001—2008	Mandate	0.006*	0.007**	0.011***	0.001	0.007**	0.009***
		(0.003)	(0.002)	(0.002)	(0.005)	(0.004)	(0.003)
	N	2,662	5,218	9,778	7,563	11,793	14,407

Notes: The table shows estimates from Equation (3) and (4) and for the wage survey reform that introduced the mandate for firms with 10 or more employees (mandate=1), but not for firms with <10 employees (mandate=0), but with the wage gap estimated according to Equation (6). Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table A37: RDD estimations for alt. wage gap estimation in Eq. (6), 2009 reform.

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
PRE, 1996—2000	Mandate	-0.010*	-0.006	-0.007**	-0.009	-0.008	-0.002
		(0.006)	(0.004)	(0.003)	(0.008)	(0.006)	(0.004)
	N	862	1,726	3,502	2,586	4,381	7,449
POST, 2001—2008	Mandate	0.003	-0.001	-0.005*	0.007	0.004	-0.002
		(0.006)	(0.004)	(0.003)	(0.009)	(0.006)	(0.005)
	N	656	1,376	2,802	2,099	3,494	5,658
B. Equal value work							
PRE, 1996—2000	Mandate	0.001	0.002	-0.001	0.007	0.001	0.004
		(0.005)	(0.004)	(0.003)	(0.008)	(0.006)	(0.004)
	N	909	1,821	3,676	2,720	4,599	7,828
POST, 2001—2008	Mandate	-0.006	-0.006	-0.006**	-0.007	-0.002	-0.004
		(0.006)	(0.004)	(0.003)	(0.008)	(0.006)	(0.004)
	N	679	1,424	2,887	2,168	3,595	5,846

Notes: See notes for Table A37, but for the wage survey reform that kept the mandate for firms with 25-50 employees (mandate=1) while removing it from firms with 10-24 employees (mandate=0).

Section A8: Additional tables and figures

Table A38: Regression Doughnut Design for the main results.

		2001 Reform		2009 Reform	
		Local randomization	Local linear	Local randomization	Local linear
A. Equal work					
PRE, 2001-2008	Mandate	-0.003	-0.009	-0.005	-0.004
		(0.008)	(0.036)	(0.004)	(0.022)
	N	832	832	1,781	1,781
POST, 2009-2015	Mandate	0.004	0.018	-0.009**	-0.018
		(0.005)	(0.022)	(0.004)	(0.022)
	N	1,962	1,962	1,601	1,601
B. Equal value work					
PRE, 2001-2008	Mandate	0.003	0.016	-0.005	-0.001
		(0.007)	(0.034)	(0.004)	(0.021)
	N	986	986	1,988	1,988
POST, 2009-2015	Mandate	0.004	0.016	-0.007*	0.027
		(0.004)	(0.022)	(0.004)	(0.020)
	N	2,279	2,279	1,679	1,679
C. Raw wage gap					
PRE, 2001-2008	Mandate	-0.027***	0.011	-0.005	-0.000
		(0.005)	(0.024)	(0.004)	(0.021)
	N	3,584	3,584	2,936	2,936
POST, 2009-2015	Mandate	-0.018***	0.011	-0.014***	-0.040
		(0.003)	(0.014)	(0.005)	(0.025)
	N	7,974	7,974	2,260	2,260

Notes: The table shows estimates from Equation (3) and (4) and for the wage survey reform that introduced the mandate for firms with 10 or more employees (mandate=1), but not for firms with <10 employees (mandate=0), but excluding firms that are 2 employees to the side of the reform threshold. Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table A39: RDD estimations for wage measure including payments for uncomfortable working hours (OB) and other forms of additional payments, for example monetary compensation for being on duty, 2001 reform.

		Local randomization			Local linear		
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
	Mandate	0.007	-0.001	-0.007	0.013	-0.002	0.000
PRE, 2001-2008		(0.012)	(0.008)	(0.006)	(0.018)	(0.012)	(0.011)
	N	306	636	1,187	920	1,468	1,954
	Mandate	0.014*	0.009	0.005	0.017	0.013	0.010
POST, 2009-2015		(0.008)	(0.005)	(0.004)	(0.012)	(0.008)	(0.007)
	N	807	1,587	2,932	2,309	3,549	4,739
B. Equal value work							
	Mandate	0.016	0.008	0.001	0.026	0.010	0.014
PRE, 2001-2008		(0.011)	(0.007)	(0.006)	(0.016)	(0.011)	(0.010)
	N	381	765	1,412	1,086	1,751	2,323
	Mandate	0.022**	0.015**	0.009**	0.027**	0.021***	0.021***
POST, 2009-2015		(0.007)	(0.005)	(0.004)	(0.011)	(0.008)	(0.007)
	N	945	1,859	3,417	2,699	4,141	5,557
C. Raw wage gap							
	Mandate	-0.000	-0.003	-0.011**	0.012	0.009	0.004
PRE, 2001-2008		(0.008)	(0.006)	(0.004)	(0.013)	(0.009)	(0.008)
	N	1,275	2,490	4,836	3,678	6,082	7,267
	Mandate	0.006	0.003	-0.005**	0.013*	0.014***	0.014***
POST, 2009-2015		(0.005)	(0.003)	(0.002)	(0.007)	(0.005)	(0.004)
	N	3,156	6,206	11,670	8,992	14,187	17,056

Notes: The table shows estimates from Equation (3) and (4) and for the wage survey reform that introduced the mandate for firms with 10 or more employees (mandate=1), using an alternative wage measure. Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table A40: RDD estimations for wage measure including payments for uncomfortable working hours (OB) and other forms of additional payments, for example monetary compensation for being on duty, 2009 reform.

		Local randomization			Local linear		
Window size around treatment threshold		1	2	4	3	5	8
A. Equal work							
	Mandate	-0.007	-0.004	-0.004	-0.008	-0.002	-0.001
PRE, 2001-2008		(0.008)	(0.006)	(0.004)	(0.013)	(0.009)	(0.006)
	N	574	1,149	2,336	1,727	2,930	4,772
	Mandate	-0.002	-0.001	-0.006	0.008	0.001	-0.009
POST, 2009-2015		(0.009)	(0.006)	(0.004)	(0.013)	(0.009)	(0.007)
	N	489	1,022	2,102	1,570	2,624	4,147
B. Equal value work							
	Mandate	-0.004	-0.004	-0.004	-0.003	-0.004	-0.002
PRE, 2001-2008		(0.008)	(0.005)	(0.004)	(0.012)	(0.008)	(0.006)
	N	656	1,297	2,626	1,941	3,284	5,433
	Mandate	0.002	0.000	-0.002	0.003	0.010	0.001
POST, 2009-2015		(0.008)	(0.005)	(0.004)	(0.011)	(0.008)	(0.006)
	N	539	1,102	2,223	1,667	2,784	4,430
C. Raw wage gap							
	Mandate	0.005	0.007	0.000	0.013	0.013	0.007
PRE, 2001-2008		(0.008)	(0.006)	(0.004)	(0.012)	(0.008)	(0.006)
	N	939	1,890	3,833	2,826	4,832	8,248
	Mandate	-0.007	-0.009	-0.014***	-0.002	-0.011	-0.016**
POST, 2009-2015		(0.008)	(0.006)	(0.004)	(0.012)	(0.008)	(0.006)
	N	714	1,492	3,017	2,270	3,750	6,104

Notes: The table shows estimates from Equation (3) and (4) for the wage survey reform that kept the mandate for firms with 25-50 employees (mandate=1) while removing it from firms with 10-24 employees (mandate=0), using an alternative wage measure. Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table A41: RDD estimations for equal jobs wage gaps, using 4-digit SSYK codes for 2009 reform, years 2005-2015.

		Local randomization			Local linear		
		(1)	(2)	(3)	(4)	(5)	(6)
Window size around treatment threshold		1	2	4	3	5	8
Equal work							
	Mandate	0.003	0.002	-0.000	0.006	0.010	0.005
PRE, 2001-2008		(0.012)	(0.008)	(0.005)	(0.018)	(0.012)	(0.009)
	N	274	562	1,113	831	1,412	2,353
	Mandate	0.006	0.006	-0.001	0.015	0.011	0.003
POST, 2009-2015		(0.008)	(0.006)	(0.004)	(0.012)	(0.008)	(0.006)
	N	465	967	1,985	1,483	2,488	3,935

Notes: The table shows estimates from Equation (3) and (4) for the wage survey reform that kept the mandate for firms with 25-50 employees (mandate=1) while removing it from firms with 10-24 employees (mandate=0), defining occupations using 4-digit SSYK codes. Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

Table A42: Difference-in-discontinuity estimates for the raw wage gap, 2009 reform.

	Local randomization	Local randomization	Local randomization
Window size around treatment threshold	1	2	4
Mandate	0.007	0.008	0.002
	(0.007)	(0.005)	(0.004=
Post	-0.017**	-0.008	-0.002
	(0.007)	(0.005)	(0.004)
Mandate*Post	-0.014	-0.017**	-0.016**
	(0.011)	(0.008)	(0.005
N	1,650	3,376	6,838