INSTITUTE OF RETAIL ECONOMICS

COMPULSORY STAFF REGISTERS AS A WAY OF INCREASING FIRMS' WAGE REPORTING: A REVENUE-COST ANALYSIS

SVEN-OLOV DAUNFELDT, ANTON GIDEHAG &
NIKLAS RUDHOLM

HFI WORKING PAPER No6

COMPULSORY STAFF REGISTERS AS A WAY OF INCREASING FIRMS' WAGE REPORTING: A REVENUE-COST ANALYSIS

Sven-Olov Daunfeldt[*], Anton Gidehag[†] and Niklas Rudholm[‡]

Abstract: In 2007, the Swedish government tried to prevent firms from underreporting their wage payments by implementing a reform that required restaurants and hairdressers to have staff registers. Employers were required to provide detailed information on when their employees were working, and the Swedish Tax Authority was also given a mandate to carry out unannounced control visits and to impose fines on firms that had not properly filled out their staff registers. We estimate the effect of this reform on firms' wage reporting using propensity score matching combined with a difference-in-differences analysis. Then, we compare the increase in tax revenues with the costs that the staff register system generated for the firms and the Swedish Tax Authority. Our results show that the total costs of the system exceeded the increase in tax revenues by approximately 355 million SEK ($36.6 million) over a four-year period, even when utilizing point estimates that are likely to overstate the effect on wage reporting. We thus conclude that considering the costs associated with the reform, the staff register reform is not economically justified.

Keywords: tax evasion, firm regulation, quasi-experimental method, unreported wages, propensity score matching

JEL classifications: H26, H32, K34, L51

[*] Corresponding author. E-mail: sven-olov.daunfeldt@huiresearch.se. Telephone: +46702957284. Institute of Retail Economics, Stockholm, Sweden, and Dalarna University, Falun, Sweden.
[†] Institute of Retail Economics, Stockholm, Sweden, and Örebro University, Örebro, Sweden.
[‡] Institute of Retail Economics, Stockholm, Sweden.

1. INTRODUCTION

Tax evasion is a major problem in many countries, with some evidence suggesting that noncompliance among firms might be as problematic as noncompliance among individuals (Fisher and Goddeeris, 1988). However, the literature on tax evasion has mainly been concerned with individuals' decision to evade taxes, and it offers little guidance on appropriate policy actions for reducing tax evasion among firms (Joulfaian, 2000; Crocker and Slemrod, 2005).[1]

If wages are taxed at a higher rate than profits, firms can evade taxes by deliberately underreporting wage payments (Yaniv, 1988). Policymakers can then respond by enhancing the ability of tax authorities to detect and punish such behavior. This happened in Sweden on January 1, 2007, when policymakers implemented a law that required restaurants and hairdressers to have a staff register. Employers were required to provide detailed information on when their employees came and left the workplace, and the law also allowed the Swedish Tax Authority to make unannounced control visits to ensure that the staff registers were filled out correctly. If they were not, firms were required to pay fines, and the Swedish Tax Authority could decide on a formal tax audit of the firms if it suspected deliberate tax evasion.

Our aim is to study the impact of the introduction of staff registers in Sweden in 2007 on firms' reported wages and to investigate whether the tax revenues from increased wage reporting were higher than the costs of the reform. Such an investigation is difficult for several reasons. First, the introduction of the staff register reform coincided with a reform that lowered payroll taxes for all employees aged 19-25 by 11 percentage points. In both targeted industries, a large share of young employees is common, and the payroll tax reform increased employment and resulted in wage increases for incumbent workers (Daunfeldt et al., 2019; Saez et al., 2019). Furthermore, the industries targeted by the staff register reform were not randomly chosen; rather, they were chosen because policymakers believed that firms in these industries were especially prone to tax evasion by underreporting wages. Therefore, we must find other industries that can account for the

[1] Notably, there are some theoretical studies on business tax evasion (e.g., Marrelli, 1984; Wang and Conant, 1988), while empirical studies have largely ignored tax evasion among firms.

counterfactual outcome of how wage reporting in these industries would have developed if staff registers had never been implemented.

To deal with the first challenge, we choose to investigate the effects of the introduction of staff registers on wages per employee instead of total wage sums because the former measure is less likely to be influenced by recruitments due to the 2007 youth payroll tax reform. To deal with the second challenge, we first use propensity score matching (Rosenbaum and Rubin, 1983) to identify control industries with similar industry-level pretreatment characteristics regarding trends in wage reporting, among other matters. In the next step, we confirm that firms in the industries affected by the reform and firms in the selected control industries have parallel trends in reported wages per employee in the years leading up to the reform. Finally, we use a difference-in-differences model (Card and Krueger, 1994) to estimate how the introduction of the staff register system affected reported wages per employee and, thus, tax revenues in Sweden.

We estimate two different models, one that is likely to understate the effect of staff registers on wage reporting and another that is likely to produce estimates that are upward biased. Our results show that compulsory staff registers had no effects on reported wages per employee in the hairdresser industry; however, in most instances, they caused an increase in reported wages within the restaurant industry. Our upper bound estimates suggest that the reform resulted in an increase in tax revenues of approximately 120 million SEK ($12.4 million) in 2007 and 562 million SEK ($57.9 million) over the 2006-2010 period.[2]

The staff register system also introduced costs for both the targeted firms and the Swedish Tax Authority. NUTEK (2008) has estimated that the total annual administrative costs for restaurants and hairdressers amounted to approximately 11,400 SEK per firm in 2007. More recent evaluations covering other staff register industries have estimated that the annual administrative cost per firm ranges between 11,000 and 13,900 SEK (Swedish Parliament, 2019; p. 171). Using the

[2] As of November 11, 2019, 1 SEK = 0.103 USD. All conversions made throughout the paper utilize this exchange rate.

NUTEK figure, we estimate that the total administrative costs for the affected firms amounted to 190 million SEK ($19.6 million) in 2007.

The Swedish Tax Authority also required additional resources to carry out the control visits and to administer the staff register system. The Swedish government set aside 225 million SEK for control visits during 2007 and 2008 (Swedish Tax Authority, 2009; p. 97), while the Swedish Tax Authority calculated the costs per visit to be as low as 1,402 SEK (excluding overhead costs) in 2007. The latter figure implies that the total cost for the 31,000 control visits in 2007 was 43.6 million SEK (Swedish Tax Authority, 2009; p. 97). We utilize this figure and estimate that the total annual labor cost, including overhead, for the Swedish Tax Authority was 64 million SEK ($6.6 million) in 2007.

Our revenue-cost analysis implies that using estimates that likely overstate the impact on tax revenues, the staff register reform led to a gain in tax revenues of approximately 120 million SEK over the 2006-2007 period, while the total costs increased by 254 million SEK (190 + 64 million SEK), resulting in a deficit of 134 million SEK ($13.8 million). If instead we perform the calculation over the 2006-2010 period, we find that using estimates that are likely to overstate the effect of staff registers on wage reporting, the deficit amounts to 355 million SEK ($36.6 million). Therefore, our conclusion is that it is hard to justify compulsory staff registers as an efficient instrument for increasing wage reporting among firms.

This article is organized as follows. Section 2 describes the Swedish staff register system as it was when introduced in 2007 and during the years under study in this paper. The empirical methodology used to estimate the effect of staff registers on wage reporting is described in section 3, while the results are presented in section 4. The cost estimates for affected firms and the tax authority are presented in section 5, and a revenue-cost analysis is conducted in section 6. Finally, our results are summarized and discussed in section 7.

2. THE SWEDISH STAFF REGISTER SYSTEM

On January 1, 2007, Swedish policymakers implemented a law that required restaurants and hairdressers to have a staff register. Since then, the law has been amended to include laundry services (2013), construction (2016), vehicle service and maintenance (2019) and grocery and tobacco wholesaling (2019). In 2019, the law was also changed, and it now includes all beauty care industries and not just the hairdresser industry. We focus on the effects of the introduction of the staff register requirement on restaurants and hairdressers because the extension of the reform either concerns very few firms (e.g., laundry services include approximately 300 firms) or has been implemented so recently that sufficient data are not yet available. Additionally, the rules and regulations regarding staff registers in the construction industry are somewhat different from those in the other industries.

The introduction of staff registers among restaurants and hairdressers in 2007 was motivated by the fact that they were mainly cash industries and that in such industries, firms could deliberately understate wage payments to evade taxes (Swedish Parliament, 2019; p. 34). Therefore, controls needed to be put in place at the workplace level, without prior notification that the workplace would be visited by the tax authorities and with some written record to evaluate. However, Swedish law at the time stated that both tax assessment visits and formal tax audits had to be announced to firms by the tax authority, greatly reducing the likelihood of finding any wrong-doing at the time of actual visits (Swedish Parliament, 2019; p 34).

Therefore, the government delivered a proposition to parliament (Prop 2005/06: 169), suggesting a law requiring employers in the restaurant and hairdresser industries to provide detailed daily updated information on who was working at a certain workplace and when their employees came to and left the workplace. In addition to the above requirements, the proposition suggested that staff registers must always be available for the tax authority to review at the workplace and that records must be kept for a minimum period of two years. The proposition also suggested that the Swedish Tax Authority should be allowed to make unannounced control visits to ensure that the staff registers were filled out correctly. The

proposition was passed by parliament on June 2, 2006, and became law on January 1, 2007 (SFS 2006:575).

Regarding the control visits, the law states that the tax authority can make unannounced control visits at any time to ensure that a firm has a staff register and that it contains all of the information required by law; additionally, the tax authority can then also audit any documents necessary to verify the information in the staff registers (Swedish Parliament, 2019; p. 41). Therefore, the control visits are mainly intended to ensure that firms have correctly filled out staff registers, while suspicions of more severe infringements are investigated through formal tax audits. If the tax authority suspects tax evasion, accounting violations or other forms of economic crime, it is obliged to file a report with the Swedish Economic Crime Authority, where a prosecutor will then decide whether the suspicions warrant further action.

In 2007, more than 31,000 control visits were carried out by the Swedish Tax Authority, and 3,515 firms had to pay fines because they had not filled out their staff registers properly (Swedish Tax Authority, 2015; p.54). These firms were required to pay a fixed amount of 10,000 SEK and an additional 2,000 SEK for each individual for whom there was inadequate information.[3] If there was a second infringement, the fixed amount and the fine were doubled to 20,000 SEK and 4,000 SEK, respectively. Since 2007, the number of control visits has been reduced, and in 2010, the total number of control visits amounted to 11,156, with 1,271 of these visits resulting in a control fine (Swedish Tax Authority, 2015; p.54).[4] From 2007 to 2010, the total number of audits made by the tax authority was also reduced from 6,577 to 4,639 (Swedish Tax Authority, 2011; p. 242), while the number of cases investigated by the Tax Fraud Unit of the Swedish Tax Authority remained basically unchanged, with 2,215 cases in 2007 and 2,165 in 2010.

[3] The fixed amount and the fine have since been increased, and currently, they are 12,500 SEK and 2,500 SEK, respectively.

[4] In 2017, the total number of control visits amounted to 16,595, with 2,869 of these visits resulting in a control fine. (Swedish Parliament, 2019; p. 54 and p. 88). By 2017, however, the reform had been extended to two additional industries, and only 5,076 of these visits involved the restaurant and hairdresser industries (Swedish Parliament, 2019; p. 54).

In 2014, the first year for which we have complete data on the types of visits and the number of audits, the tax authority made 43,174 firm workplace visits. Of these, 5,792 were staff register control visits, 13,799 were cash register[5] control visits, and 23,583 were other types of workplace visits (Swedish Tax Authority, 2015; p. 54). In 2014, 19,591 staff and cash register visits resulted in 257 audits (1.31 %), of which 82 % detected faults in the audited documents that needed revisions, while 2,466 audits were initiated based on other grounds (such as other types of company visits), of which 80 % led to audited documents needing revisions (Swedish Parliament, 2019, p. 134). Over the 2014-2017 period, the share of audits leading to revisions in the audited documents ranged from 82 % to 91 % for audits initiated by staff or cash register control visits and from 80 % to 88 % for audits initiated for other reasons. These differences are not statistically significant at conventional levels based on a t-test.

[5] On January 1, 2010, a new law came into effect requiring all firms that sold goods for cash to have a detailed cash register. The new law also stipulated that the Swedish Tax Authority could carry out unannounced control visits to verify that firms were using the cash register.

3. EMPIRICAL METHOD

3.1 Identification strategy

When estimating the effect of staff registers on wage reporting, the fundamental identification problem that we need to address is that we cannot observe firms that were required to introduce staff registers in the counterfactual state of not being subject to the reform. The targeted industries were not randomly chosen; rather, they were targeted because policymakers believed that firms in these industries were especially prone to tax evasion by underreporting wages. Therefore, we must account for the counterfactual outcome of how wage reporting in these industries would have developed if compulsory staff registers had never been implemented.

A true counterfactual outcome is impossible to observe for obvious reasons, but different statistical methods have been developed to find measures of counterfactual outcomes. We use a two-step method to estimate the effect of staff registers on reported wages. First, since the staff register requirement was implemented at the industry level, there were industry-level factors that explained the implementation in certain industries. Therefore, we use propensity score matching (Rosenbaum and Rubin, 1983) to identify control industries that had trends in wages per employee, the number of employees and average firm size similar to those of the treated industries in the years leading up to the reform. Having similar trends in wage reporting across treated and control industries in the years leading up to the reform makes it probable that firms in the treated industries and control industries would have had similar trends in wage reporting in the postreform years if staff registers had not been implemented. Identifying control industries that are characterized by having a size (number of employees) and average firm size similar to those of the treated industries is important, as these factors are positively correlated with the size of tax evasion that is possible.

In the next step, we first verify that the trends in the outcome variable, i.e., reported wages per employee, are parallel for firms in the treated and control industries. We then estimate the treatment effect by comparing the development of wages per employee among firms in the treated industries (i.e., restaurants and hairdressers)

and firms in the matched control industries. Specifically, we estimate a difference-in-differences model to compare the pre- and posttreatment changes between treated and control group firms (Card and Krueger, 1994; Abadie 2005; Angrist and Pischke, 2008).

3.2 Data

Our empirical analysis is based on the individual-level LISA (Longitudinal Integration Database for Health Insurance and Labour Market Studies) database, which is provided by Statistics Sweden (2016). LISA is a database that consists of numerous registers and that includes information on all Swedish residents who are at least 16 years old.

The RAMS (Labour Statistics Based on Administrative Sources) register in LISA provides information on individuals' employment status and potential employers in November each year (Statistics Sweden, 2017). The dataset also includes identification numbers for employers, which means that we can match all employees with their employer. Thus, we can construct a panel of Swedish firms and their employees from 2003 to 2010 that includes information on the number of employees and total gross wages, allowing us to calculate wages per employee at firm i in year t.

All firms in LISA are assigned industry codes that reveal their industrial affiliation. The industry codes are derived from the SNI2002 (Swedish Standard Industrial Classification) system. SNI2002 consists of 17 industry groups at the most aggregated level and 776 industry groups at the most detailed five-digit level. To be able to identify representative control industries using propensity score matching, we construct a panel at the five-digit industry level containing measures of gross wages, for example.

The firm-level variables are measured in the month of November each year, while information on individuals' wages is linked to their primary employer throughout the year. To obtain the total gross wages of each firm (and industry) observed in November, we aggregate the gross wages of workers who have that firm as their primary income source (their primary workplace). We define the number of

employees as the number of individuals who are employed according to RAMS and who also have the November firm as his/her primary workplace throughout the year. Consequently, our outcome variable (wages per employee) is based solely on employees who work at their primary workplace in November.

As our outcome variable, we choose to utilize wages per employee rather than firms' total wages because the former variable is less likely than the latter to be influenced by the payroll tax cut for young employees that was implemented in 2007. This reform had a significant positive effect on the number of employees of firms (Egebark and Kaunitz, 2013; Daunfeldt et al., 2019; Saez et al., 2019) and, therefore, on the total wages paid by firms. However, wages per employee can also be affected and cause biased estimates of the treatment effect (i) if the reduced payroll tax causes more wage spillovers among incumbent workers in the treated industries than in the control industries or (ii) if the reform leads to an increased employment of young individuals with below average wages in the treated industries. All else equal, the first mechanism causes wages per employee to become upward biased; in contrast, the second mechanism leads to a downward bias.

We begin our empirical analysis by investigating how the staff register system affected the average wages per employee among all employees: both incumbents and new employees. However, it is likely that these estimates provide a lower bound of the effect of staff registers since previous research (Daunfeldt et al., 2019; Saez et al., 2019) suggests that the second mechanism described above is likely to dominate the first mechanism. Therefore, in the next step, we study wage reporting among incumbent workers, i.e., those who were working for the same firm throughout the time period studied. By limiting our analysis to incumbent workers, we determine that the payroll tax reform can only cause upward bias through mechanism (i), which means that these estimates will provide an upper bound of the true reform effect.

In addition, our sample is restricted to firms that have at least two employees. The reason is that most one-employee firms in the targeted industries were exempted

from the staff register requirement.[6] Throughout the paper, we also exclude firms with extreme values in the outcome variable. A firm is defined as an outlier if the annual growth in our outcome variable (wages per employee) deviates by more than three standard deviations from the average annual growth. For our lower bound estimates, this definition means that we exclude 4,610 out of 629,662 firms (or 16,748 out of 2,022,590 observations) over the 2006-2010 period. Furthermore, for the upper bound estimates, we exclude 4,044 out of 310,927 firms to 6,938 out of 210,989 firms, respectively, depending on the period under study.

3.3 Identification of control industries

The staff register reform targeted specific industries because policymakers believed that firms in these industries were especially prone to underreporting wages. Therefore, we use data at the industry level to identify control industries that had characteristics similar to those of the reform industries based on a number of observables in the years leading up to the reform. To obtain propensity scores for the five-digit industries under study, i.e., their estimated probability of receiving treatment, we estimate the following probit model:

$$\begin{aligned} Treated_{j,t=07} = {} & \alpha + \beta_1 Wage/emp_{j,t=06} + \beta_2 \Delta Wage/emp_{j,t=05} \\ & + \beta_3 \Delta Wage/emp_{j,t=04} + \beta_4 Size_{j,t=06} + \beta_5 Firm\ size_{j,t=06} \\ & + \beta_6 Wage/emp^2_{j,t=06} + \beta_7 \Delta Wage/emp^2_{j,t=05} + \beta_8 \Delta Wage/emp^2_{j,t=04} \\ & + \beta_9 Size^2_{j,t=06} + \beta_{10} Firm\ size^2_{j,t=06} + \varepsilon_{jt} \end{aligned} \quad (1)$$

where the dependent variable $Treated_{j,t=07}$ is a variable that is equal to one for all SNI2002 five-digit restaurant and hairdresser industries and zero for all other industries during the treatment year.[7] We control for past wage development by including wages per employee in five-digit industry j in 2006, $Wage/emp_{j,t=06}$; its annual growth over the 2003-2005 period, represented by $\Delta Wage/emp_{j,t=05}$ and

[6] Specifically, independent contractors, closely held companies and closely held partnerships in which only the chief executive or his/her family were active were exempted. Moreover, firms whose main operation was within a nontargeted industry were exempted. The rule for being exempted was that at least 75 % of the firm's turnover was associated with a nontargeted industry.
[7] The lower bound regression results are presented in the Appendix (Table A1).

$\Delta Wage/emp_{j,t=04}$; the total number of employees in each industry in 2006, $Size_{j,t=06}$; and the average firm size in each industry in 2006, $Firm\ size_{j,t=06}$. Following the suggestion by Angrist and Pischke (2008), we also include all variables in their squared forms to control for nonlinear relationships. We thus seek to identify control industries that have a wage development and size similar to those of the restaurant and hairdresser industries in the years before the staff register requirement was implemented.

Estimating equation (1), we obtain propensity scores for all five-digit industries. Using nearest neighbor matching, we then assign the five industries with the most similar propensity scores to each five-digit restaurant and hairdresser industry. For the lower bound estimation, the treated restaurant and hairdresser industries and their corresponding control industries are presented in Table A2 in the Appendix. In total, we identify 20 unique control industries at the five-digit SNI2002 level that have propensity scores similar to those of the restaurant and hairdresser industries. In the sample of firms having at least two employees in 2007, our identified control industries for the restaurant sector include 4,365 firms that are matched to 9,083 restaurant firms. For the hairdresser industry, we identify 277 firms in the control industries that are matched to 1,781 hairdresser firms.

For our upper bound estimates, which are limited to the wage reporting among incumbent employees, we perform the statistical matching procedure again using the exact same matching variables but limited to incumbent employees only.[8] Specifically, we identify control industries with similar wage developments for individuals staying at the same workplace for at least two consecutive years in the prereform period. In total, this procedure yields 10,523 and 1,902 control firms having at least two employees in 2007 that are matched to 9,083 restaurant firms and 1,781 hairdresser firms, respectively. The probit regression results and the control industries can be found in the Appendix (Tables A3-A4).

[8] We consider reperforming the matching to be important since the wage reporting among incumbent employees may be different from the overall wage reporting, i.e., among all employees.

To ensure that our matching variables have similar characteristics in the treated and control industries, we also perform balancing tests. We identify industries that constitute a valid control group if the underlying variables that affect treatment assignment have similar characteristics, implying that treatment should be *as if* randomly assigned between the treated and matched control industries. The results of our balancing test for the lower bound estimates are presented in Table 1, which shows that the means of the matching variables are not significantly different between the treated and control industries after matching. For example, the average one-year lagged wages per employee and the average wage development over 2004-2005 are very similar. These findings indicate that we have found valid control groups for the treated industries. Similarly, the balancing test for our upper bound estimates (Table A5) also shows that none of the means of the matching variables are significantly different between the industry groups postmatching.

Table 1. Balancing test for treated and control industries. Lower bound – all employees.

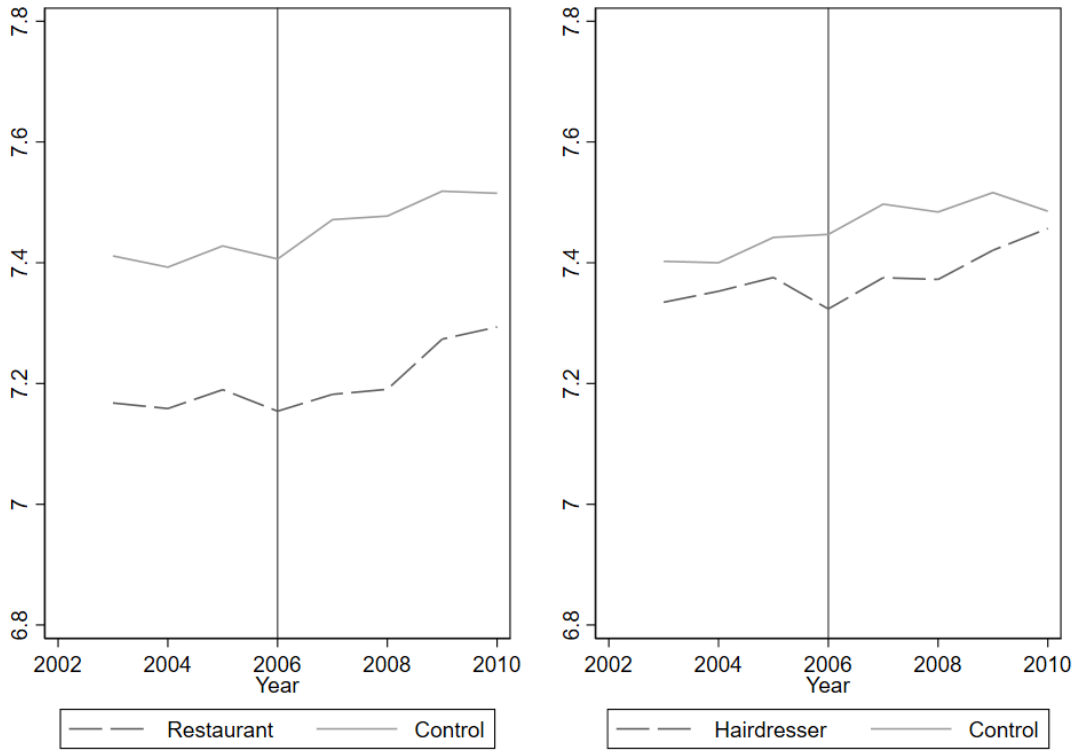
Variables	B/A = Before/ After matching	Mean			t-test	
		Treated	Control	Bias (%)	t	p-value
Wage/emp., t=2006	B	1847.9	2713.5	-144	-2.89	0.004
	A	1847.9	1759.8	14.7	0.56	0.585
(Wage/emp.)^2, t=2006	B	3.5e+06	8.0e+06	-130.4	-2.51	0.012
	A	3.5e+06	3.2e+06	9.8	0.62	0.546
ΔWage/emp., t=2005	B	52.796	86.476	-16.9	-0.32	0.752
	A	52.796	50.813	1.0	0.19	0.850
(ΔWage/emp.)^2, t=2005	B	3104.3	86873	-13.4	-0.25	0.802
	A	3104.3	2898.6	0.0	0.18	0.858
ΔWage/emp., t=2004	B	-17.497	37.56	-29.6	-0.68	0.498
	A	-17.497	22.12	-21.3	-0.59	0.565
(ΔWage/emp.)^2, t=2004	B	20200	47247	-9.5	-0.18	0.858
	A	20200	7549.8	4.4	0.67	0.518
Size, t=2006	B	11141	4884.5	28.5	0.71	0.481
	A	11141	8429.4	12.4	0.31	0.764
(Size)^2, t=2006	B	4.8e+08	5.7e+08	-1.2	-0.02	0.983
	A	4.8e+08	1.8e+08	3.9	0.66	0.524
Firm size, t=2006	B	40.349	132.77	-11.0	-0.21	0.836
	A	40.349	26.536	1.6	0.52	0.611
(Firm size)^2, t=2006	B	4565.3	1.4e+06	-8.4	-0.16	0.876
	A	4565.3	1953.1	0.0	0.70	0.498

3.4 Regression model

Difference-in-differences analysis rests on the assumption of parallel trends in the outcome variable among treated and control units during the posttreatment period in the absence of treatment. Since the control industries are identified using industry-level data, we must verify the veracity of this assumption based on the firm-level data used in the regression analysis. This verification is commonly performed

by examining whether the pretreatment trends for the treated and control group firms are parallel (Angrist and Pischke, 2008; Ryan et al., 2015). In this regard, the argument is that if the trends are parallel for the treated and control group firms in the years leading up to the reform, then this would likely also have been the case in the years following the reform if it had not been implemented. The average log wages per employee among the restaurant and hairdresser firms and their respective control firms in the pretreatment years 2003-2006 and the posttreatment years 2007-2010 are presented in Figure 1.

Figure 1. Wage development for all employees in restaurant and hairdresser firms and the corresponding control firms in the 2003-2006 pretreatment period and the 2007-2010 posttreatment period. Minimum of two employees. Lower bound – all employees.



Note: Average ln(wage/employee). All firms in the treated and control industries are included. Outliers (defined as annual changes in the outcome variable by more than +/- three standard deviations from the average change) are excluded.

Figure 1 shows that both pretreatment and posttreatment average wages are lower for firms in the staff register industries compared to firms in their corresponding

control industries. This result is especially prominent for restaurant firms. However, in difference-in-differences estimations, it is important not that the pretreatment levels are similar but that the pretreatment trends are parallel. We observe very similar trends in the pretreatment years 2003-2006 for restaurant firms and their controls. In the posttreatment period, there is an upward sloping trend not only for restaurants but also for their control firms up until 2009, indicating that it is hard to visually observe any strong reform effect. The pretreatment trends are also relatively similar for hairdressers and their control firms in 2003-2005, but they differ in 2005-2006, with a downward trend for hairdressers and a slight upward trend for the control firms. Finally, we note a convergence in average wages in the posttreatment period, as shown by a steady increase among hairdressers.

Notably, the differences in trends that we observe are small in practice since wages are expressed in log values and are presented at a fine-grained scale, and considering our upper bound estimates, we also find similar pretreatment trends (Figure A1 in the Appendix).

To investigate how the introduction of staff registers affected wage reporting in the treated firms, we estimate the following firm-level difference-in-differences model:

$$\ln Y_{it} = \alpha + \gamma TI_i + \lambda TP_t + \sigma(TI_i * TP_t) + \eta_i + \varepsilon_{it} \qquad (2)$$

where the dependent variable $\ln Y_{it}$ is the natural logarithm of wages per employee at firm i in year t. There are two reasons for expressing the outcome variable in log form. First, the variable becomes approximately normally distributed, which benefits statistical inference. Second, it yields a semi-elastic model in which the estimated treatment effects can be interpreted as percentage changes.[9]

TI_i is a treatment indicator that is equal to one for firms in the treated industries and equal to zero for firms in the matched control industries. The treatment indicator accounts for potential level differences in wages per employee between firms in the

[9] The exact effect in percentage terms of a parameter estimate σ can be calculated using the formula $100 \times [\exp(\sigma) - 1]$. However, since the parameter estimates in our setting are small, the differences are negligible.

treated and control groups. TP_t is an indicator that takes the value of one during the posttreatment years (2007-2010) and zero in the pretreatment year 2006. We decide to use only one pretreatment year in our baseline estimations for three reasons. First, the estimated reform effect is the most accurate closest to the year of the introduction (Mian and Sufi, 2012). Second, using earlier pretreatment years implies a loss in the number of observations. Third, the pretreatment trends in the outcome variable are the most similar for the last year before the intervention. As a robustness check, we also utilize 2005 as the pretreatment year.[10]

By setting restrictions on the years included, we estimate the effects over gradually longer time periods, meaning that the posttreatment period ranges from 2007 to 2010. Note that each estimation includes only one posttreatment year at a time, meaning that the estimations are built on yearly – rather than joint – conditional means in the posttreatment period. This aspect is important when calculating the corresponding gain in tax revenues, which is further described in section 6. By including TP_t, we control for time-variant effects that are common for both the treated and control firms. For instance, it captures the general economic factors affecting the development of wages per employee in all industries.

Our main variable of interest is the interaction term between TI_i and TP_t, which is equal to 1 for the treated firms in the posttreatment period. Its parameter σ can be expressed as follows:

$$\sigma = E[ln\, Y_{it}|Tr_i = 1, t = Post] - E[ln\, Y_{it}|Tr_i = 1, t = Pre] - (E[ln\, Y_{it}|Tr_i = 0, t = Post] - E[ln\, Y_{it}|Tr_i = 0, t = Pre])$$

Thus, parameter σ represents the differences in the conditional means in the treatment and control groups before and after treatment. Consequently, our estimated parameter $\hat{\sigma}$ compares how the average log wages per employee changed in the treatment and control group firms at the time of the staff register reform.

[10] In general, the statistical significance (or lack thereof) of our findings in Figures 2-5 is unaltered. Two exceptions are the 2007 estimate in Figure 2 (which becomes nonsignificant) and the 2008 and 2009 estimates in Figure 5 (which become negative and significant). These robustness checks are available upon request.

To ensure that our results are not driven by any remaining heterogeneity among firms in the intervention and control industries, we also include firm-specific random effects η_i.[11] Finally, ε_{it} is an idiosyncratic error term.

[11] Due to higher efficiency, we use random effects rather than fixed effects in the estimations used to create Figures 2-5 to account for firm-specific time-invariant heterogeneity. However, using fixed effects mostly provides similar point estimates. The one-year estimate of Figure 2 no longer has a p-value<0.05. The estimates of Figure 4 become slightly larger in magnitude. The one-year estimate of Figure 5 becomes slightly larger, with a p-value<0.05. Compare the third and fourth columns of Tables A6-A9.

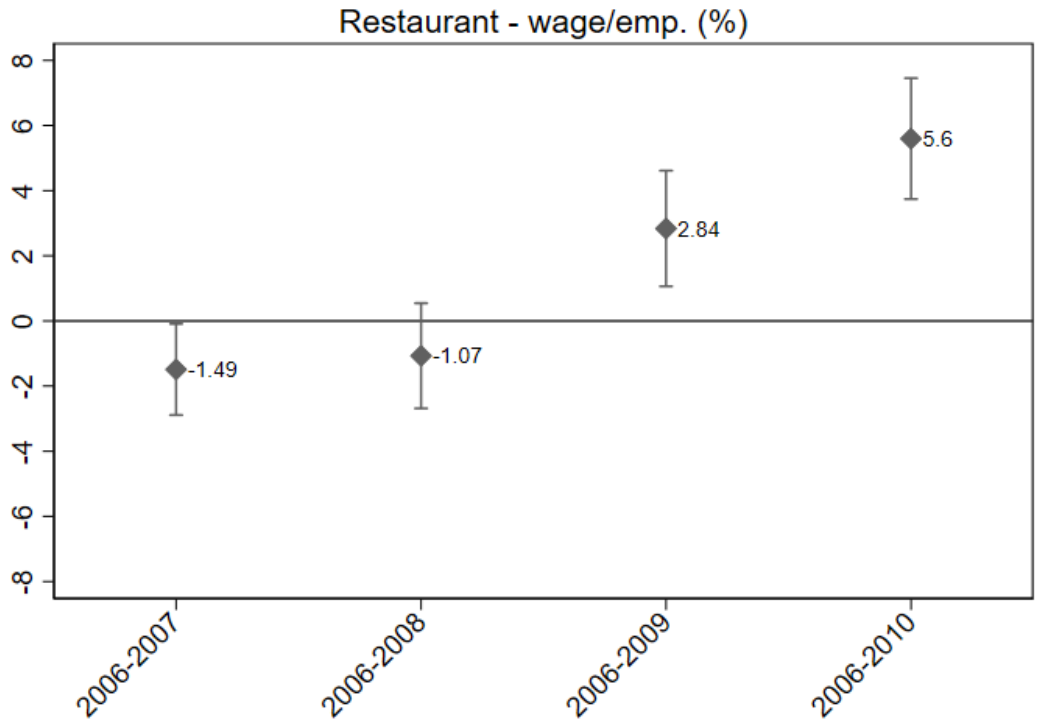
4. EFFECTS OF STAFF REGISTERS ON FIRMS' WAGE REPORTING

4.1 Lower bound effects

We begin by presenting our estimates for wage reporting among all employees, i.e., both new employees and incumbents. Note, however, that if the youth payroll tax reform led to an increased employment of young individuals with below average wages, then the estimated effect of the staff register requirement on the average wages per employee in this case will be downward biased. Therefore, the estimates presented in this section are likely to be lower bounds of the true reform effect.

The estimation results are presented in Figures 2 and 3. The estimates of the treatment effect are significantly different from zero with 95 % certainty if the confidence intervals do not cross the x-axis at zero. The full regression results are presented in Tables A6-A7 in the Appendix; the estimates in the figures correspond to the third column of each table.

Figure 2. Effects of staff registers on wages per employee. Including both new employees and incumbents. Restaurant firms with at least two employees.

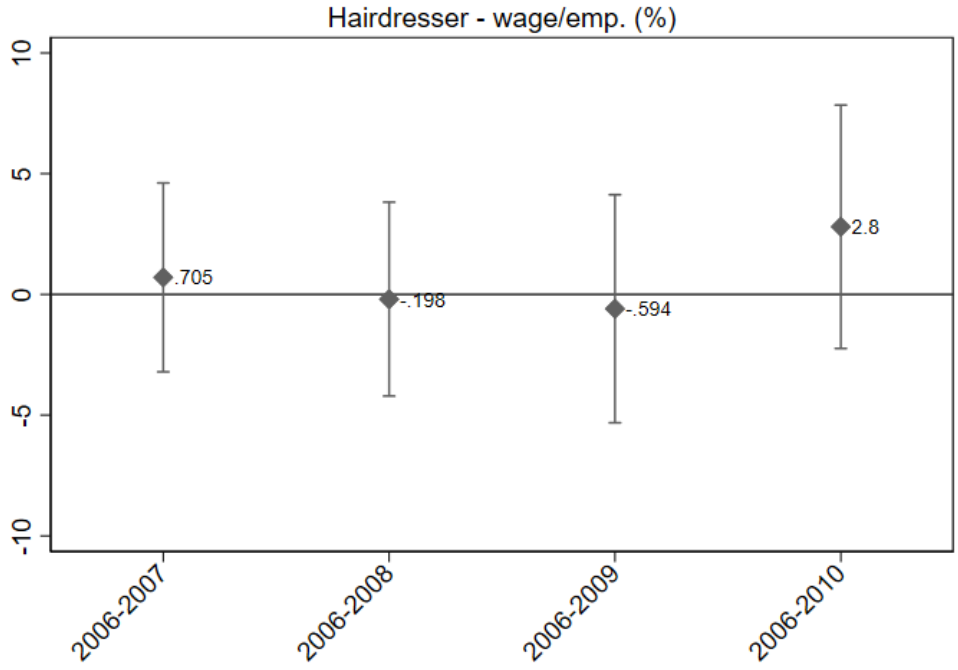


Note: Point estimates and their 95 % confidence intervals from the estimation of equation (2). Firm clustered standard errors. Only surviving firms remaining in the same industry are included. Outliers (defined as annual changes in the outcome variable by more than +/- three standard deviations from the average change) are excluded.

Figure 2 shows an upward trend in the average wage reporting among restaurant firms. The point estimate for the 2006-2007 period is negative and suggests that the average wages per employee decreased by 1.49 % in relation to the control firms. In contrast, the long-run estimates suggest increases in wage reporting of 2.84 % and 5.6 % over the 2006-2009 and 2006-2010 periods, respectively.

Figure 3 depicts the estimated effect of the staff register requirement on wage reporting among hairdresser firms. The point estimates for all time periods are statistically nonsignificant, indicating that staff registers on average had no effect on wage reporting in the hairdresser industry.

Figure 3. Effects of staff registers on wages per employee. Including both new employees and incumbents. Hairdresser firms with at least two employees.



Note: Point estimates and their 95 % confidence intervals from the estimation of equation (2). Firm clustered standard errors. Only surviving firms remaining in the same industry are included. Outliers (defined as annual changes in the outcome variable by more than +/- three standard deviations from the average change) are excluded.

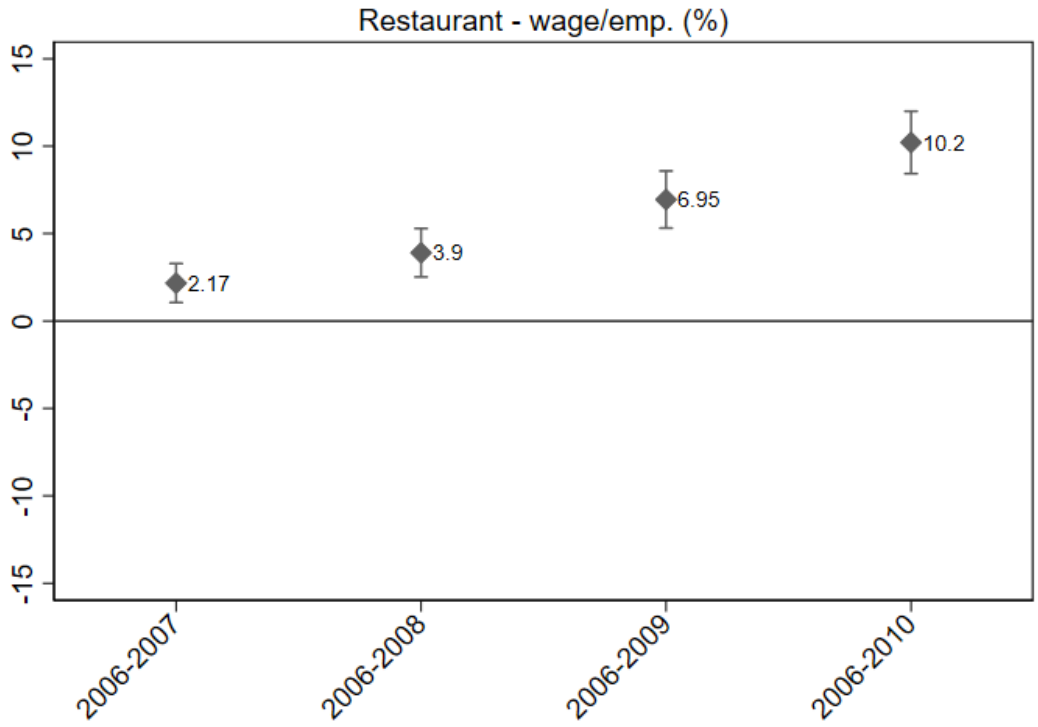
Studying the wages per employee among all employees, we conclude that the staff register reform appears to have had no positive effect on restaurants' wage reporting during the first two posttreatment years but that the average wages increased during the third and fourth years following the introduction of the reform. Hairdresser firms do not appear to have altered their average wage reporting in response to the staff register requirement.

4.2 Upper bound effects

We now turn to our findings for incumbent employees at restaurant and hairdresser firms. Different from the previous section, we focus solely on the wage reporting among officially employed individuals who were staying at the same firm during the period of study. Therefore, we ensure that the estimated staff register effects are unaffected by any employment changes along the extensive margin.

Our results for incumbent employees are presented in Figure 4 (restaurants) and Figure 5 (hairdressers), while the complete regression results are included in the Appendix (Tables A8-A9).

Figure 4. Effects of staff registers on wages per employee among incumbent workers (staying at the same firm) in restaurants with at least two employees**.**



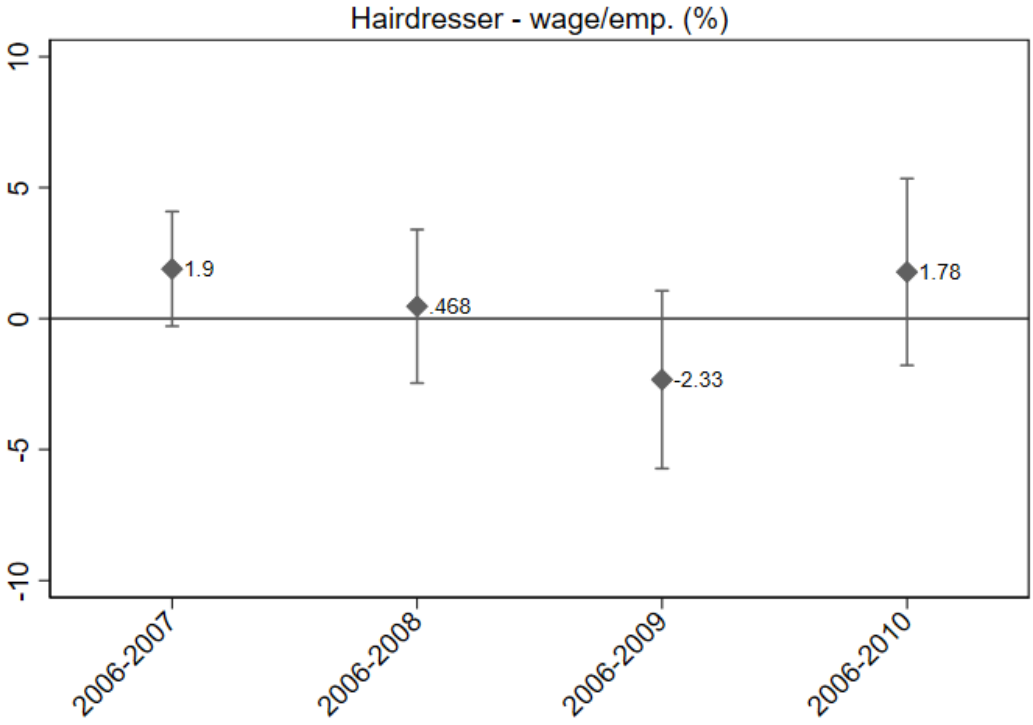
Note: Point estimates and their 95 % confidence intervals from the estimation of equation (2). Firm clustered standard errors. Only surviving firms remaining in the same industry are included. Outliers (defined as annual changes in the outcome variable by more than +/- three standard deviations from the average change) are excluded.

The results in Figure 4 indicate that the staff register requirement increased wages per incumbent employee by an average of 2.17 % and 3.9 % during the first and second postreform years, respectively. The estimates remain statistically significant in the long run, suggesting wage increases of 6.95 % over the 2006-2009 period and 10.2 % over the 2006-2010 period.

However, the results for hairdresser firms in Figure 5 show that neither the short-run nor the long-run estimates are significantly different from zero. This finding

implies that staff registers did not have any impact on the reported wages of incumbents in the hairdresser industry.

Figure 5. Effects of staff registers on wages per employee among incumbent workers (staying at the same firm). Hairdresser firms with at least two employees.



Note: Point estimates and their 95 % confidence intervals from the estimation of equation (2). Firm clustered standard errors. Only surviving firms remaining in the same industry are included. Outliers (defined as annual changes in the outcome variable by more than +/- three standard deviations from the average change) are excluded.

Recall, however, that the findings in Figures 4 and 5 are upper bound estimates, meaning that they are likely to overstate the effect of staff registers. The reason is that the payroll tax reform might have caused wage spillovers for incumbent workers, particularly in industries with a high share of young employees. For example, Saez et al. (2019) found that the payroll tax cut resulted in average wage increases for noneligible workers of 1.5-3.7 %, depending on firms' share of young employees. They also found that this wage effect was driven by incumbent workers, suggesting that our estimated effects of staff registers on the wage reporting of incumbents are likely to be overestimated because they are a combined effect of the staff registers and wage spillovers from the payroll tax reform.

5. COSTS OF THE SWEDISH STAFF REGISTER SYSTEM

5.1 Administrative costs for the affected firms

Several attempts have been made to estimate the administrative costs for firms generated by the staff register requirement, reporting annual costs in the range of 11,000 to 13,900 SEK per firm (Swedish Parliament, 2019; p. 171). We utilize the evaluation by NUTEK (2008), which estimated the annual administrative cost in the restaurant and hairdresser industries as 11,424 SEK per firm. We decide to use the figure from this evaluation because it was calculated specifically for restaurants and hairdressers and because it is at the lower end of the range of administrative costs suggested by other evaluations, making our cost estimate conservative.

The estimate of the administrative cost in the NUTEK (2008) study was obtained by considering the average time spent filling out staff registers and the average hourly wages of employees. It was assumed that it took an average of 1-2 minutes for an employee, having an average hourly wage of 185 SEK, to fill out the staff register each day. Assuming that the average firm had 15 employees and a total of 247 working days in 2007, NUTEK found that the annual administrative cost was (185/60)*1*247*15=11,424 SEK per firm.[12] First, we convert 11,424 SEK in 2007 to 11,176 SEK measured at the 2006 price level (using the CPI). Throughout these calculations, we express all revenues and costs below in 2006 prices. To obtain the total administrative costs, we multiply this number by the number of restaurant and hairdresser firms having nonzero wage reporting in 2007 according to the Swedish Tax Authority (2009; p. 248-255). In total, 14,958 restaurant firms and 2,040 hairdresser firms had nonzero wage reporting in 2007, which results in a total annual administrative cost of approximately 167 million SEK ($17.2 million) for restaurants and 23 million SEK ($2.4 million) for hairdressers.

The Swedish Tax Authority criticized the NUTEK evaluation for overstating the administrative costs of the system (Swedish Parliament, 2019; p. 174), and before expanding the system to the laundry and construction industries, another evaluation was performed. The increase in administrative costs for the 300 affected

[12] For further information, see NUTEK (2008) and Swedish Parliament (2019; p. 173-175).

laundry firms was then estimated to be 3.4 million SEK, resulting in an average increase in administrative costs per firm in the laundry industry of approximately 11,000 SEK (Proposition to the Swedish Parliament 2012/13:34; p. 31). The proposition to parliament regarding the expansion of the staff register system to the construction industry included two different estimates of the increase in administrative costs. The first was based on the administrative costs related to the voluntary staff register system ID06, which was already used by some firms in the construction industry; the average annual administrative cost related to that system was reported to be 12,900 SEK (Proposition to the Swedish Parliament 2014/15:6; p. 88). The other calculation was based on estimates of the time spent filling out the staff registers in other industries; it found that the annual administrative costs would increase by approximately 11,000 SEK if staff registers were introduced. These numbers have since been criticized by the Swedish Construction Federation, which estimated the costs to be approximately twice as high (Swedish Parliament, 2019; p. 182).

5.2 Costs for the Swedish Tax Authority

The reform also led to increased costs for the tax authority because additional resources were required to carry out the control visits and to administer the staff register system. The Swedish government set aside 225 million SEK ($23.2 million) for control visits during 2007 and 2008 (Swedish Tax Authority, 2009; p. 97), suggesting a yearly total cost of 112.5 million SEK ($11.6 million). However, in 2007, the Swedish Tax Authority made 31,108 control visits, with an average wage cost per visit of 1,402 SEK (excluding overhead costs), and each employee made on average 1.4 control visits per day (Swedish Tax Authority, 2009; p. 97).

We utilize these numbers to construct a measure of the labor cost for the tax authority. First, we measure the average cost per visit at the 2006 price level, obtaining an amount of 1,372 SEK per visit. A total of 31,108 control visits at an average cost per visit of 1,372 SEK implies that the total labor cost for the 2007 control visits amounted to 42,680,176 SEK, excluding overhead costs. Next, based on a calculation by the Swedish National Financial Management Authority regarding overhead costs in Swedish government agencies and authorities (ESV,

2005:3; p 10), we add overhead costs corresponding to 49 % and thus obtain a total cost for the tax authority of 63,593,462 SEK.

According to a survey of the tax authority personnel making the control visits, a considerable amount of time is spent preparing and administrating the visits. This survey suggests that a control visit can take from five minutes to one hour, while preparations and administration each take between 30 minutes and one hour (Swedish Parliament, 2019; p. 64). However, since these numbers are only approximations and we want to be conservative in our cost estimates, we decide to not add any additional costs related to preparing for and administrating the visits.

Finally, since we perform all calculations separately for the restaurant and hairdresser industries, we need to calculate the shares of the total labor costs for the tax authority that are related to restaurants and hairdressers. Unfortunately, we lack information on the number of control visits made in the two industries in 2007. Therefore, we utilize that 88 % of the firms with nonzero wage reporting in 2007 were restaurants and that the remaining 12 % were hairdressers.[13] Using these percentages generates a total labor cost in 2007 of approximately 56 million SEK ($5.8 million) for restaurants and 7.6 million SEK ($0.8 million) for hairdressers. Given that we know the total number of control visits made during 2008-2010 and otherwise using the same assumptions as above, we calculate the corresponding annual labor costs over these years.[14]

[13] 14,958/(14,958+2,040)=0.88 and 2,040/(14,958+2,040)=0.12. We find the shares of restaurant and hairdresser firms in 2007 to be similar to the corresponding shares of control visits in later years. Over the 2015-2017 period, 13,389 and 926 control visits were made in the restaurant and hairdresser industries, respectively, yielding a restaurant share of 94 % rather than 88 %. See Swedish Parliament (2019; p. 54).

[14] The number of control visits in 2008, 2009 and 2010 amounts to 17,754, 17,288 and 11,156, respectively.

6. REVENUE-COST ANALYSIS

In this section, we evaluate whether the staff register reform can be justified on economic grounds by first calculating the increase in tax revenues and then subtracting the administrative costs for firms and the labor costs for the tax authority.

In 2007, there were 14,958 restaurants and 2,040 hairdressers with nonzero wage reporting, i.e., that had employees with income statements (Swedish Tax Authority, 2009; p. 248-255). The 2006 total wage sum for restaurants amounted to 11.416 billion SEK ($1.76 billion), while the corresponding number for hairdressers was 978 million SEK ($101 million). Next, we relate these industry-level wage sums to our point estimates from sections 4.1 and 4.2. Recall that if the number of officially employed individuals with below average wages increased as a result of the payroll tax reform, the effect of staff registers on wage reporting among all employees (section 4.1) will be underestimated, meaning that these estimates likely provide a lower bound of the true reform effect. In contrast, the wage reporting among incumbent employees (section 4.2) is likely to be overestimated due to wage spillover effects that were generated by the payroll tax reform (Saez et al., 2019), which means that these estimates are considered an upper bound of the effect of compulsory staff registers on wage reporting.

We multiply our lower and upper bound wage effect estimates by the total wage sums, and assuming an average wage tax rate of 48.3 %,[15] we obtain estimates of the increased tax revenues across the 2006-2007 to 2006-2010 periods. For instance, based on the 2006-2007 wage estimate for restaurants in Figure 4, the gain in tax revenues is given by 0.0217*11,416,000,000*0.483=119,652,238 SEK. Finally, we consider the one-year estimate in Figure 2, which is negative and statistically significant, to be associated with no – rather than a negative – effect on tax revenues since it is unlikely that compulsory staff registers led to decreased tax revenues. Additionally, since we do not obtain any statistically significant estimates for

[15] The Swedish Government (2017) estimates the average tax rate on labor income (including social security fees) to be 48.3 %.

hairdressers, we consider the gain in tax revenues in the hairdresser industry to be equal to zero.

Table 2 includes the derived tax revenues and costs associated with the staff register reform. The tax revenues are calculated based on the wage reporting among all employees, and thus, they are considered to be lower bounds of the actual increase in tax revenues. During the first two postreform years, the gain in tax revenues is zero in the restaurant industry. Over the 2006-2009 and 2006-2010 periods, the collected tax revenues amount to approximately 157 million SEK ($16.2 million) and 309 million SEK ($31.8 million), respectively. Since we do not obtain any statistically significant point estimates for hairdressers, the tax revenues are considered zero throughout. The administrative costs faced by firms are the same each year, and thus, we observe a linear increase in these costs over time, with the two-year costs being twice those of the one-year costs, etc. Moreover, the labor costs for the tax authority grow over time but at a decreasing pace due to the decreased number of control visits over the years after 2007. The fifth column subtracts the costs from the increased tax revenues and thus indicates whether the staff register reform is economically justified. The gain in tax revenues falls short of the costs in every instance, and the gap gradually becomes larger. Jointly considering the two industries, over the 2006-2010 period, we find that total costs exceed tax revenues by almost 610 million SEK ($62.8 million).

Table 2. Revenues versus costs. Lower bound estimates.

Restaurants	Tax revenues	Administrative costs	Labor costs for tax authority	Tax revenues - costs
2006-2007	0	167,170,608	55,961,349	-223,131,957
2006-2008	0	334,341,216	87,899,686	-422,240,902
2006-2009	156,595,555	501,511,824	118,999,718	-463,915,987
2006-2010	308,779,968	668,682,432	139,068,665	-498,971,129
Hairdressers	**Tax revenues**	**Administrative costs**	**Labor costs for tax authority**	**Tax revenues - costs**
2006-2007	0	22,799,040	7,632,113	-30,431,153
2006-2008	0	45,598,080	11,987,923	-57,586,003
2006-2009	0	68,397,120	16,229,404	-84,626,524
2006-2010	0	91,196,160	18,966,445	-110,162,605
Total	**Tax revenues**	**Administrative costs**	**Labor costs for tax authority**	**Tax revenues - costs**
2006-2007	0	189,969,648	63,593,462	-253,563,110
2006-2008	0	379,939,296	99,887,609	-479,826,905
2006-2009	156,595,555	569,908,944	135,229,122	-548,542,511
2006-2010	308,779,968	759,878,592	158,035,110	-609,133,734

Note: Measured at the 2006 price level. As of November 2019, 1 SEK=0.103 USD.

In Table 3, the tax revenues are derived from the average wage reporting among incumbent employees, which, as we have argued, gives an upper bound of the magnitude of the tax revenues. We now observe an instant and steady increase in collected tax revenues in the restaurant industry, ranging from approximately 120 million SEK ($12.4 million) over 2006-2007 to 562 million SEK ($57.9 million) over the whole 2006-2010 period. Since the upper bound point estimates for hairdressers are also statistically nonsignificant (Figure 5), there is no gain in tax revenues. Although the upper bound estimates are associated with a larger increase in tax revenues, the costs still exceed the tax revenues in every period, and over the 2006-2010 period, the total deficit amounts to approximately 355 million SEK ($36.6 million).

Table 3. Revenues versus costs. Upper bound estimates.

Restaurants	Tax revenues	Administrative costs	Labor costs for tax authority	Tax revenues - costs
2006-2007	119,652,238	167,170,608	55,961,349	-103,479,719
2006-2008	215,043,192	334,341,216	87,899,686	-207,197,710
2006-2009	383,217,996	501,511,824	118,999,718	-237,293,546
2006-2010	562,420,656	668,682,432	139,068,665	-245,330,441
Hairdressers	**Tax revenues**	**Administrative costs**	**Labor costs for tax authority**	**Tax revenues - costs**
2006-2007	0	22,799,040	7,632,113	-30,431,153
2006-2008	0	45,598,080	11,987,923	-57,586,003
2006-2009	0	68,397,120	16,229,404	-84,626,524
2006-2010	0	91,196,160	18,966,445	-110,162,605
Total	**Tax revenues**	**Administrative costs**	**Labor costs for tax authority**	**Tax revenues - costs**
2006-2007	119,652,238	189,969,648	63,593,462	-133,910,873
2006-2008	215,043,192	379,939,296	99,887,609	-264,783,713
2006-2009	383,217,996	569,908,944	135,229,122	-321,920,070
2006-2010	562,420,656	759,878,592	158,035,110	-355,493,046

Note: Measured at the 2006 price level. As of November, 2019, 1 SEK=0.103 USD.

7. SUMMARY AND DISCUSSION

Wage underreporting among firms can have significant negative impacts on tax revenues. One possibility for preventing such behavior is to implement reforms that make it easier for tax authorities to detect and punish firms that underreport their wage payments. In 2007, Sweden implemented such a reform, requiring most restaurants and hairdressers with at least two employees to introduce staff registers. The reform also allowed the Swedish Tax Authority to make unannounced control visits, and firms were required to pay substantial fines if they were found to have misreported.

We investigated the effects of this reform on firms' wage reporting by first creating a control group of firms that were active in industries that had a wage development and number of employees that were similar to those of firms in the treated industries during the pretreatment period. We then compared how reported wages per employee evolved pre- and postreform for firms in the treatment and control groups by estimating a firm-level difference-in-differences regression model.

The estimations were performed in two different ways: one that likely underestimated the effect of staff registers on reported wages and another that overestimated the effect. The results from the lower bound estimates for restaurant firms showed that the staff register requirement was associated with average wage increases of 2.8 % and 5.6 % over the 2006-2009 and 2006-2010 periods, respectively. For hairdresser firms, the point estimates for all periods were statistically nonsignificant. Our upper bound estimates showed average wage increases among restaurants ranging from 2.17 % over 2006-2007 to 10.2 % over the 2006-2010 period, while no statistically significant effects for hairdressers were found. Overall, statistically significant and positive effects on wage reporting were found in only six of the 16 models that we estimated.

We also calculated the total costs of the staff register reform and compared them to the estimated gains in tax revenues. Our revenue-cost analysis showed that the total costs of the reform exceeded the tax revenues in the two industries and in every year, even when considering estimations that were likely to overstate the effect of the staff

registers on wage reporting. Over the 2006-2010 period, we found that when jointly considering the two industries and using the upper bound estimates of the increase in tax revenues, the total costs exceeded the tax revenues by approximately 355 million SEK ($36.6 million). Thus, we conclude that it is highly unlikely that the staff register reform is economically justified.

One might perhaps argue that these costs are acceptable because the reform was intended to reduce tax evasion and economic crime. However, the control visits for staff registers resulted in fewer audits than other types of visits made by the Swedish Tax Authority. Of the 19,591 staff and cash register visits in 2014, 257 resulted in audits, while the 23,583 other types of workplace visits resulted in 2,466 audits (Swedish Tax Authority, 2015; p. 54; Swedish Parliament, 2019; p. 134). The considerably lower share of staff register visits leading to audits is not particularly surprising given that such visits are generally supposed to focus not only on firms with a high probability of having incorrect staff registers or other infringements but on firms in the targeted industries, with the purpose of making the system highly visible in these industries (Swedish Parliament, 2019; p. 56). Finally, the system could perhaps be justified if the share of audits leading to revisions of the audited documents or other measures from the authorities were much higher for the 257 staff register-induced audits than for the other 2,466 audits. However, in both groups, the share of audits leading to revisions is between 80 % and 90 %, and the differences between the groups are not statistically significant.

What, then, can explain the limited success of the reform? One possible explanation is that only a small number of the targeted firms evaded taxes from the beginning. Another explanation could be that firms found ways to circumvent the staff register requirement. For example, they could have ensured that the official taxable wage matched the reported number of work hours, but they could have continued to pay part of the total wage unofficially.

Following the initial introduction of the reform in 2007, compulsory staff registers were introduced in the laundry service and construction industries in 2013 and 2016, respectively. On January 1, 2019, the requirement was also implemented in industries such as vehicle repair and maintenance, beauty care, and food and

tobacco wholesaling. Our findings indicate that it is doubtful whether the extensions of the reform can be justified on economic grounds. The reform has also increased the regulatory burden on firms and is therefore likely to have induced indirect costs that are difficult to measure and that are not included in the analysis above.

This aspect leads to some suggestions for future research. Taking a broader perspective, we believe that it is necessary to conduct more research on tax evasion among firms, particularly on how firms respond to different institutional reforms that are supposed to reduce tax evasion. We also believe that it is necessary to conduct more research on how the increased regulatory burden affects firms and whether the staff register system has become a growth barrier. Another interesting question is whether the introduction of the staff register system affected employees and jobseekers trying to land their first job. For example, did the introduction of the staff register system make it more difficult for immigrants and people with low education to get hired? Recall that employees themselves make the entries into the staff registers. If immigrants or employees with less education are more likely to make mistakes, then employers have a clear economic incentive to avoid these groups when recruiting. All these questions fall beyond the scope of the present paper, but we consider them to be interesting avenues for future research.

REFERENCES

Abadie, A. (2005). Semiparametric difference-in-differences estimators. *The Review of Economic Studies*, 72(1), 1-19.

Angrist J.D., & Pischke, J.S. (2008). *Mostly harmless econometrics: an empiricist's companion*. Princeton, NJ: Princeton University Press

Card, D., & Krueger, A. (1994). Minimum Wages and Employment: A Case Study of the Fast- Food Industry in New Jersey and Pennsylvania. *American Economic Review*, 84(4), 772-793.

Crocker, K.J., & Slemrod, J. (2005). Corporate tax evasion with agency costs. *Journal of Public Economics*, 89(9), 1593-1610.

Daunfeldt, S-O., Gidehag, A. & Rudholm, N. (2019). How do firms respond to reduced labor costs? Evidence from the 2007 Swedish payroll tax reform.. *Institute of Retail Economics Working Paper, no. 3.* .

Egebark, J., & Kaunitz, N. (2013). Do payroll tax cuts raise youth employment?. *IFAU Working Paper 2013:27*.

ESV, 2005:3 Nyckeltal för OH-kostnader.

Fisher, R.C., & Goddeeris, J.H. (1988). Participation in state tax amnesties: the case of business taxes. In Proceedings of the 81st annual conference on taxation held under the auspices of the National Tax Association-Tax Institute of America (pp. 139-145). National Tax Association-Tax Institute of America, Columbus, OH.

Joulfaian, D. (2000). Corporate income tax evasion and managerial preferences. *The Review of Economics and Statistics*, 82(4), 698-701.

Marrelli, M. (1984). On indirect tax evasion. *Journal of Public Economics*, 25(1-2), 181-196.

Mian, A., & Sufi, A. (2012). The effects of fiscal stimulus: Evidence from the 2009 cash for clunkers program. *The Quarterly Journal of Economics*, 127(3), 1107-142.

NUTEK. (2008). Näringslivets administrativa kostnader för skatteområdet – uppdatering 2007 R 2008:42. Stockholm.

Proposition to the Swedish Parliament. (2006). Effektivare skattekontroll m.m. Prop. 2005/06:169.

Proposition to the Swedish Parliament. (2012). Personalliggare för tvätteribranschen, Prop. 2012/13:34.

Proposition to the Swedish Parliament. (2014). Minskat svartarbete i byggbranschen. Prop. 2014/15:6.

Rosenbaum, P.R., & Rubin, D.B. (1983). The central role of the propensity score in observational studies for causal effects. *Biometrika*, 70(1), 41-55.

Ryan, A.M., Burgess, J.F., & Dimick, J.B. (2015). Why we should not be indifferent to specification choices for difference-in-differences. *Health Services Research*, 50(4), 1211-1235.

Saez, E., Schoefer, B. & Seim, D. (2019). Payroll Taxes, Firm Behavior, and Rent Sharing: Evidence from a Young Workers' Tax Cut in Sweden. *American Economic Review,* 109(5), 1717-1763.

SFS (2006:575). Lag om särskild skattekontroll i vissa branscher.

Statistics Sweden. (2016). Longitudinell integrationsdatabas för Sjukförsäkrings- och Arbetsmarknadsstudier (LISA) 1990–2013. Documentation. http://www.scb.se/Statistik/AM/AM9901/_dokument/AM9901_1990I13_BR_AM76BR1601.pdf. Accessed January 29, 2019

Statistics Sweden. (2017). Registerbaserad arbetsmarkadsstatistik/Labour statistics based on administrative sources register. Overview.

https://www.scb.se/hitta-statistik/statistik-efter-amne/arbetsmarknad/sysselsattning-forvarvsarbete-och-arbetstider/registerbaserad-arbetsmarknadsstatistik-rams/produktrelaterat/Fordjupad-information/beskrivning-av-registerbaserad-arbetsmarknadsstatistik-rams/. Accessed January 29, 2019

Swedish Government. (2017). Lagrådsremiss – Personalliggare i fler verksamheter, Stockholm.

Swedish Parliament. (2019). En utvärdering av personalliggarsystemet. Rapporter från riksdagen. 2018/19:RFR4.

Swedish Tax Authority. (2009). Skapar personalliggare fler vita jobb? En utvärdering av personalliggarnas effekter på skatteundandragande i restaurang- och frisörbranscherna. Bilaga 4. In: Närvaroliggare och kontrollbesök – en utvärdering och förslag till utvidgning, Ds 2009:43, Ministry of Finance, 239–348.

Swedish Tax Authority. (2011). Skatter i Sverige – Skattestatistisk årsbok 2011 (Tax Statistical Yearbook of Sweden 2011).

Swedish Tax Authority. (2015). Skatter i Sverige – Skattestatistisk Årsbok 2015 (Tax Statistical Yearbook of Sweden 2015). Solna, Stockholm.

Wang, L.F., & Conant, J.L. (1988). Corporate tax evasion and output decisions of the uncertain monopolist. *National Tax Journal*, 41(4), 579-581.

Yaniv, G. (1988). Withholding and non-withheld tax evasion. *Journal of Public Economics*, 35(2), 183-204.

APPENDIX

Table A1. Probit model estimation. Propensity score matching.

Probit model estimation	
Wage/emp., t=2006	0.0074142*
	(0.0037933)
(Wage/emp.)^2, t=2006	-2.39e-06**
	(1.00e-06)
ΔWage/emp., t=2005	0.0708595**
	(0.0335647)
(ΔWage/emp.)^2, t=2005	-0.0006509**
	(0.0002437)
ΔWage/emp., t=2004	-0.0005572
	(0.0013744)
(ΔWage/emp.)^2, t=2004	1.20e-06
	(1.94e-06)
Size, t=2006	0.0000149
	(0.0000168)
(Size)^2, t=2006	1.36e-10*
	(7.02e-11)
Firm size, t=2006	0.0304675**
	(0.0091692)
(Firm size)^2, t=2006	-0.0000868**
	(0.0000283)
Constant	-9.137582**
	(4.257959)
Obs.	754
Pseudo R^2	0.4814

Note: The dependent variable is equal to one for all five-digit restaurant and hairdresser industries in 2007 and equal to zero for all other five-digit industries in 2007. Standard errors clustered at the five-digit industry level.
***p<0.01, **p<0.05, *p<0.1.

Table A2. Treated and control industries. Lower bound – all employees.

Treated industries		Control industries	
SNI2002	**Description**	**SNI2002**	**Description**
55300	Restaurants	52121	Other retail sales in department stores and the like
		85328	Day care activities for disabled persons
		74701	Cleaning of premises
		36630	Other manufacturing NEC
		64120	Courier activities other than national post activities
55510	Canteens	52121	Other retail sales in department stores and the like
		85328	Day care activities for disabled persons
		36630	Other manufacturing NEC
		74701	Cleaning of premises
		64120	Courier activities other than national post activities
55522	Catering for hospitals	52501	Retail sales of antiques and second-hand books
		93022	Beauty treatment
		52710	Repair of boots, shoes, and other leather articles
		52633	Ambulatory and occasional retail sales of other goods
		01217	Farmers of animals, mixed, mainly cattle
55529	Other catering	52632	Ambulatory and occasional retail sales of food
		01122	Growing of nursery products etc. in the open
		52442	Retail sales of home furnishing textiles
		18300	Dressing and dyeing of fur; manufacture of fur articles
		92611	Operation of ski facilities
93021	Hairdressing	52632	Ambulatory and occasional retail sales of food
		52442	Retail sales of home furnishing textiles
		18300	Dressing and dyeing of fur; manufacture of fur articles
		01122	Growing of nursery products etc. in the open
		92611	Operation of ski facilities
55523	Catering for schools, welfare and other institutions	92310	Artistic and literary creation and interpretation
		92729	Various other recreational activities
		71401	Video and DVD film renting
		74860	Call center activities
		52509	Retail sales of other second-hand goods in stores
55521	Catering for the transport sector	36630	Other manufacturing NEC
		52121	Other retail sales in department stores and the like
		85328	Day care activities for disabled persons
		74701	Cleaning of premises
		64120	Courier activities other than national post activities

Note: Industry code 93021 constitutes the hairdresser sector. Other industry codes jointly constitute the restaurant sector.

Table A3. Probit model estimation. Propensity score matching. Upper bound – incumbent employees.

Probit model estimation	
Wage/emp., t=2006 (stay 05-06)	0.0113506
	(0.0074781)
(Wage/emp.)^2, t=2006 (stay 05-06)	-3.13E-06*
	(1.80E-06)
ΔWage/emp., t=2005 (stay 04-05)	0.0004709
	(0.0020603)
(ΔWage/emp.)^2, t=2005 (stay 04-05)	2.61E-06
	(2.35E-06)
ΔWage/emp., t=2004 (stay 03-04)	-0.0000369
	(0.0015818)
(ΔWage/emp.)^2, t=2004 (stay 03-04)	-1.23E-08
	(1.06E-07)
Size, t=2006 (stay 05-06)	0.0000463
	(0.0000763)
(Size)^2, t=2006 (stay 05-06)	-4.26E-10
	2.20E-09
Firm size, t=2006 (stay 05-06)	0.0296093***
	(0.0095331)
(Firm size)^2, t=2006 (stay 05-06)	-0.000074***
	(0.0000304)
Constant	-12.32164
	(7.669879)
Obs.	753
Pseudo R^2	0.4032

Note: The dependent variable is equal to one for all five-digit restaurant and hairdresser industries in 2007 and equal to zero for all other five-digit industries in 2007. Standard errors clustered at the five-digit industry level.
***p<0.01, **p<0.05, *p<0.1.

Table A4. Matched control industries. Upper bound – incumbent employees.

Treated industries		Control industries	
SNI2002	**Description**	**SNI2002**	**Description**
55300	Restaurants	52112	Retail sales in other nonspecialized stores with food, beverages and tobacco predominating
		64120	Courier activities other than national post activities
		74701	Cleaning of premises
		60211	Urban and suburban scheduled passenger transport
		85328	Day care activities for disabled persons
55510	Canteens	52121	Other retail sales in department stores and the like
		52112	Retail sales in other nonspecialized stores with food, beverages and tobacco predominating
		64120	Courier activities other than national post activities
		74701	Cleaning of premises
		60211	Urban and suburban scheduled passenger transport
55522	Catering for hospitals	01228	Breeding of horses etc.
		01124	Growing of flowers and ornamental plants under glass
		60220	Taxi operation
		93012	Washing and dry cleaning for households
		01302	Mixed farming, mainly animals
55529	Other catering	52431	Retail sales of footwear
		52443	Retail sales of glassware, china and kitchenware
		01129	Growing of mushrooms etc.
		52260	Retail sales of tobacco products
		52486	Retail sales of games and toys
93021	Hairdressing	05025	Growing of aquatic plants
		52423	Retail sales of women's clothing
		17120	Preparation and spinning of woolen-type fibers
		52487	Retail sales of flowers and other plants
		01300	Growing of crops combined with farming of animals (mixed farming)
55523	Catering for schools, welfare and other institutions	52410	Retail sales of textiles
		01500	Hunting, trapping and game propagation including related service abilities
		01259	Raising and breeding of other animals
		01232	Raising of swine for slaughter
		01121	Growing of vegetables in the open
55521	Catering for the transport sector	52121	Other retail sales in department stores and the like
		52112	Retail sales in other nonspecialized stores with food, beverages and tobacco predominating
		64120	Courier activities other than national post activities
		74701	Cleaning of premises
		36630	Other manufacturing NEC

Note: Industry code 93021 constitutes the hairdresser sector. Other industry codes jointly constitute the restaurant sector.

Table A5. Balancing test for matched treated and control industries. Upper bound – incumbent employees.

Variables	B/A = Before/After matching	Mean			t-test	
		Treated	Control	Bias (%)	t	p-value
Wage/emp., t=2006 (stay 05-06)	B	2058.7	2944	-145.7	-2.91	0.004
	A	2058.7	2009.4	8.1	0.34	0.743
(Wage/emp.)^2, t=2006 (stay 05-06)	B	4.30E+06	9.30E+06	-127.8	-2.45	0.015
	A	4.30E+06	4.10E+06	6	0.39	0.702
ΔWage/emp., t=2005 (stay 04-05)	B	105.92	145.79	-30	-0.66	0.511
	A	105.92	116.95	-8.3	-0.24	0.815
(ΔWage/emp.)^2, t=2005 (stay 04-05)	B	19473	46684	-21.3	-0.4	0.687
	A	19473	18141	1	0.11	0.913
ΔWage/emp., t=2004 (stay 03-04)	B	49	145.82	-21.5	-0.43	0.669
	A	49	88.243	-8.7	-0.47	0.649
(ΔWage/emp.)^2, t=2004 (stay 03-04)	B	41234	3.80E+05	-5.2	-0.1	0.922
	A	41234	11446	0.5	1.23	0.241
Size, t=2006 (stay 05-06)	B	6939.1	3895.4	18.9	0.41	0.682
	A	6939.1	8322.7	-8.6	-0.22	0.832
(Size)^2, t=2006 (stay 05-06)	B	1.60E+08	4.00E+08	-4.4	-0.08	0.935
	A	1.60E+08	2.00E+08	-0.6	-0.15	0.884
Firm size, t=2006 (stay 05-06)	B	53.44	112.72	-8.4	-0.16	0.874
	A	53.44	21.717	4.5	0.85	0.411
(Firm size)^2, t=2006 (stay 05-06)	B	9907.7	9.90E+05	-7.4	-0.14	0.89
	A	9907.7	1730.1	0.1	0.92	0.375

Note: The variables are built based on individuals staying for at least two consecutive years at the same firm. For instance, 'stay 05-06' refers to individuals remaining at the same firm in the years 2005-2006.

Table A6. Regression results. Restaurant firms with at least two employees. Lower bound estimates. The estimates in Figure 2 correspond to the ATE estimate in the third column.

2006-2007	1	2	3	4	2006-2008	1	2	3	4
TI	-0.247***	-0.124***	-0.247***	.	TI	-0.248***	-0.0740	-0.248***	.
	(0.00912)	(0.0480)	(0.00919)	.		(0.00936)	(0.0472)	(0.00936)	.
TP	0.0769***	0.0774***	0.0825***	0.0849***	TP	0.0896***	0.0906***	0.0910***	0.0917***
	(0.0105)	(0.0104)	(0.00549)	(0.00543)		(0.0107)	(0.0106)	(0.00638)	(0.00639)
ATE	-0.0191	-0.0196	-0.0149**	-0.0128*	ATE	-0.0132	-0.0142	-0.0107	-0.00896
	(0.0128)	(0.0127)	(0.00715)	(0.00715)		(0.0132)	(0.0130)	(0.00824)	(0.00831)
Constant	7.429***	7.449***	7.409***	7.257***	Constant	7.447***	7.409***	7.433***	7.280***
	(0.00748)	(0.0421)	(0.00757)	(0.00182)		(0.00762)	(0.0407)	(0.00758)	(0.00208)
Observations	22,161	22,161	22,161	22,161	Observations	19,136	19,136	19,136	19,136
R-squared	0.072	0.089	.	0.044	R-squared	0.080	0.100	.	0.049
Industry FE	No	Yes	No	No	Industry FE	No	Yes	No	No
Firm RE	No	No	Yes	No	Firm RE	No	No	Yes	No
Firm FE	No	No	No	Yes	Firm FE	No	No	No	Yes
Firm clustered s.e's	No	No	Yes	Yes	Firm clustered s.e's	No	No	Yes	Yes
Number of firms	.	.	12,336	12,336	Number of firms	.	.	10,720	10,720
2006-2009	**1**	**2**	**3**	**4**	**2006-2010**	**1**	**2**	**3**	**4**
TI	-0.251***	-0.106**	-0.250***	.	TI	-0.245***	-0.122**	-0.242***	.
	(0.00967)	(0.0492)	(0.00954)	.		(0.00981)	(0.0485)	(0.00980)	.
TP	0.120***	0.120***	0.124***	0.127***	TP	0.121***	0.121***	0.127***	0.133***
	(0.0110)	(0.0109)	(0.00706)	(0.00716)		(0.0111)	(0.0110)	(0.00728)	(0.00732)
ATE	0.0286**	0.0283**	0.0284***	0.0283***	ATE	0.0604***	0.0602***	0.0560***	0.0520***
	(0.0136)	(0.0135)	(0.00906)	(0.00921)		(0.0138)	(0.0137)	(0.00946)	(0.00959)
Constant	7.466***	7.441***	7.455***	7.299***	Constant	7.472***	7.441***	7.461***	7.311***
	(0.00783)	(0.0421)	(0.00760)	(0.00227)		(0.00789)	(0.0407)	(0.00772)	(0.00240)
Observations	16,713	16,713	16,713	16,713	Observations	14,905	14,905	14,905	14,905
R-squared	0.091	0.108	.	0.124	R-squared	0.095	0.112	.	0.161
Industry FE	No	Yes	No	No	Industry FE	No	Yes	No	No
Firm RE	No	No	Yes	No	Firm RE	No	No	Yes	No
Firm FE	No	No	No	Yes	Firm FE	No	No	No	Yes
Firm clustered s.e's	No	No	Yes	Yes	Firm clustered s.e's	No	No	Yes	Yes
Number of firms	.	.	9,377	9,377	Number of firms	.	.	8,377	8,377

Dependent variable: ln(wage sum/employee). Both new employees and incumbents are included. ATE*100 = point estimates in the paper. Standard errors within parentheses.
*p<0.10, **p<0.05, ***p<0.01.

Table A7. Regression results. Hairdresser firms with at least two employees. Lower bound estimates. The estimates in Figure 3 correspond to the ATE estimate in the third column.

2006-2007	1	2	3	4	2006-2008	1	2	3	4
TI	-0.104***	-0.115**	-0.111***	.	TI	-0.0994***	-0.0620	-0.107***	.
	(0.0299)	(0.0469)	(0.0280)	.		(0.0305)	(0.0463)	(0.0275)	.
TP	0.0654	0.0658*	0.0616***	0.0607***	TP	0.0729*	0.0735*	0.0626***	0.0591***
	(0.0398)	(0.0398)	(0.0185)	(0.0185)		(0.0403)	(0.0404)	(0.0187)	(0.0187)
ATE	0.000643	0.000202	0.00705	0.00881	ATE	-0.0102	-0.0108	-0.00198	0.000945
	(0.0426)	(0.0427)	(0.0200)	(0.0200)		(0.0432)	(0.0432)	(0.0205)	(0.0206)
Constant	7.444***	7.455***	7.425***	7.352***	Constant	7.455***	7.418***	7.445***	7.371***
	(0.0279)	(0.0457)	(0.0259)	(0.00348)		(0.0285)	(0.0450)	(0.0252)	(0.00385)
Observations	3,770	3,770	3,770	3,770	Observations	3,460	3,460	3,460	3,460
R-squared	0.012	0.012	.	0.054	R-squared	0.012	0.013	.	0.038
Industry FE	No	Yes	No	No	Industry FE	No	Yes	No	No
Firm RE	No	No	Yes	No	Firm RE	No	No	Yes	No
Firm FE	No	No	No	Yes	Firm FE	No	No	No	Yes
Firm clustered s.e's	No	No	Yes	Yes	Firm clustered s.e's	No	No	Yes	Yes
Number of firms	.	.	2,114	2,114	Number of firms	.	.	1,971	1,971
2006-2009	**1**	**2**	**3**	**4**	**2006-2010**	**1**	**2**	**3**	**4**
TI	-0.108***	-0.0825*	-0.108***	.	TI	-0.0962***	-0.0659	-0.0960***	.
	(0.0317)	(0.0483)	(0.0285)	.		(0.0304)	(0.0455)	(0.0276)	.
TP	0.103**	0.102**	0.103***	0.103***	TP	0.0970**	0.0986**	0.107***	0.113***
	(0.0425)	(0.0425)	(0.0222)	(0.0224)		(0.0408)	(0.0408)	(0.0239)	(0.0244)
ATE	-0.00176	0.000167	-0.00594	-0.00782	ATE	0.0451	0.0436	0.0280	0.0189
	(0.0454)	(0.0454)	(0.0241)	(0.0243)		(0.0436)	(0.0436)	(0.0257)	(0.0263)
Constant	7.475***	7.450***	7.460***	7.384***	Constant	7.482***	7.452***	7.470***	7.402***
	(0.0297)	(0.0470)	(0.0262)	(0.00428)		(0.0284)	(0.0442)	(0.0253)	(0.00445)
Observations	3,197	3,197	3,197	3,197	Observations	2,950	2,950	2,950	2,950
R-squared	0.021	0.023	.	0.083	R-squared	0.034	0.036	.	0.145
Industry FE	No	Yes	No	No	Industry FE	No	Yes	No	No
Firm RE	No	No	Yes	No	Firm RE	No	No	Yes	No
Firm FE	No	No	No	Yes	Firm FE	No	No	No	Yes
Firm clustered s.e's	No	No	Yes	Yes	Firm clustered s.e's	No	No	Yes	Yes
Number of firms	.	.	1,860	1,860	Number of firms	.	.	1,760	1,760

Dependent variable: ln(wage sum/employee). Both new employees and incumbents are included. ATE*100 = point estimates in the paper. Standard errors within parentheses.
*p<0.10, **p<0.05, ***p<0.01.

Table A8. Regression results. Restaurant firms with at least two employees. Upper bound estimates. The estimates in Figure 4 correspond to the ATE in the third column.

2006-2007	**1**	**2**	**3**	**4**	**2006-2008**	**1**	**2**	**3**	**4**
TI	-0.243***	-0.120*	-0.257***	.	TI	-0.229***	-0.0532	-0.236***	.
	(0.00729)	(0.0693)	(0.00751)	.		(0.00801)	(0.0737)	(0.00829)	.
TP	0.105***	0.103***	0.105***	0.105***	TP	0.0901***	0.0887***	0.0928***	0.0941***
	(0.00681)	(0.00676)	(0.00311)	(0.00309)		(0.00734)	(0.00728)	(0.00382)	(0.00379)
ATE	0.00602	0.00736	0.0217***	0.0268***	ATE	0.0300***	0.0314***	0.0390***	0.0431***
	(0.0103)	(0.0102)	(0.00569)	(0.00569)		(0.0113)	(0.0112)	(0.00708)	(0.00709)
Constant	7.536***	7.525***	7.530***	7.425***	Constant	7.597***	7.536***	7.590***	7.496***
	(0.00483)	(0.0651)	(0.00442)	(0.00137)		(0.00519)	(0.0688)	(0.00473)	(0.00168)
Observations	32,368	32,368	32,368	32,368	Observations	27,089	27,089	27,089	27,089
R-squared	0.075	0.090	.	0.113	R-squared	0.062	0.077	.	0.087
Industry FE	No	Yes	No	No	Industry FE	No	Yes	No	No
Firm RE	No	No	Yes	No	Firm RE	No	No	Yes	No
Firm FE	No	No	No	Yes	Firm FE	No	No	No	Yes
Firm clustered s.e's	No	No	Yes	Yes	Firm clustered s.e's	No	No	Yes	Yes
Number of firms	.	.	17,775	17,775	Number of firms	.	.	14,920	14,920
2006-2009	**1**	**2**	**3**	**4**	**2006-2010**	**1**	**2**	**3**	**4**
TI	-0.222***	-0.134*	-0.224***	.	TI	-0.226***	-0.0518	-0.233***	.
	(0.00864)	(0.0764)	(0.00879)	.		(0.00898)	(0.0790)	(0.00940)	.
TP	0.107***	0.106***	0.112***	0.115***	TP	0.132***	0.131***	0.134***	0.135***
	(0.00782)	(0.00777)	(0.00458)	(0.00456)		(0.00798)	(0.00792)	(0.00488)	(0.00490)
ATE	0.0671***	0.0684***	0.0695***	0.0708***	ATE	0.0961***	0.0971***	0.102***	0.107***
	(0.0122)	(0.0122)	(0.00833)	(0.00838)		(0.0127)	(0.0126)	(0.00911)	(0.00919)
Constant	7.637***	7.652***	7.630***	7.541***	Constant	7.661***	7.590***	7.657***	7.568***
	(0.00552)	(0.0705)	(0.00489)	(0.00197)		(0.00565)	(0.0725)	(0.00489)	(0.00214)
Observations	23,317	23,317	23,317	23,317	Observations	20,436	20,436	20,436	20,436
R-squared	0.060	0.072	.	0.119	R-squared	0.072	0.086	.	0.169
Industry FE	No	Yes	No	No	Industry FE	No	Yes	No	No
Firm RE	No	No	Yes	No	Firm RE	No	No	Yes	No
Firm FE	No	No	No	Yes	Firm FE	No	No	No	Yes
Firm clustered s.e's	No	No	Yes	Yes	Firm clustered s.e's	No	No	Yes	Yes
Number of firms	.	.	12,875	12,875	Number of firms	.	.	11,275	11,275

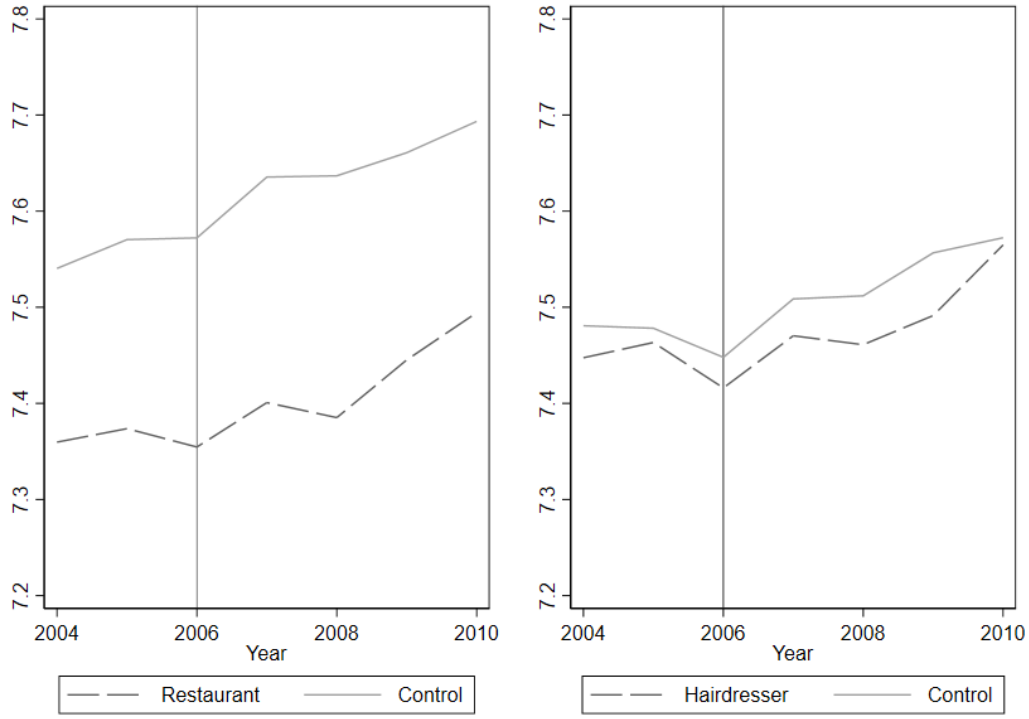
Dependent variable: ln(wage sum/employee). Incumbent employees working at the same firm during the period of study. ATE*100 = point estimates in the paper. Standard errors within parentheses.
*p<0.10, **p<0.05, ***p<0.01.

Table A9. Regression results. Hairdresser firms with at least two employees. Upper bound estimates. The estimates in Figure 5 correspond to the ATE in the third column.

2006-2007	1	2	3	4	2006-2008	1	2	3	4
TI	-0.0357**	-0.0917***	-0.0505***	.	TI	-0.0340*	-0.0962***	-0.0420**	.
	(0.0165)	(0.0238)	(0.0166)	.		(0.0183)	(0.0271)	(0.0184)	.
TP	0.0910***	0.0907***	0.0790***	0.0762***	TP	0.0858***	0.0845***	0.0723***	0.0672***
	(0.0165)	(0.0165)	(0.00750)	(0.00749)		(0.0186)	(0.0186)	(0.0102)	(0.0101)
ATE	0.00438	0.00462	0.0190*	0.0225**	ATE	-0.0132	-0.0120	0.00468	0.0117
	(0.0235)	(0.0234)	(0.0112)	(0.0112)		(0.0261)	(0.0260)	(0.0150)	(0.0150)
Constant	7.412***	7.468***	7.406***	7.397***	Constant	7.445***	7.507***	7.438***	7.431***
	(0.0116)	(0.0207)	(0.0111)	(0.00276)		(0.0131)	(0.0239)	(0.0127)	(0.00371)
Observations	6,583	6,583	6,583	6,583	Observations	5,791	5,791	5,791	5,791
R-squared	0.011	0.015	.	0.078	R-squared	0.008	0.013	.	0.036
Industry FE	No	Yes	No	No	Industry FE	No	Yes	No	No
Firm RE	No	No	Yes	No	Firm RE	No	No	Yes	No
Firm FE	No	No	No	Yes	Firm FE	No	No	No	Yes
Firm clustered s.e's	No	No	Yes	Yes	Firm clustered s.e's	No	No	Yes	Yes
Number of firms	.	.	3,660	3,660	Number of firms	.	.	3,243	3,243
2006-2009	**1**	**2**	**3**	**4**	**2006-2010**	**1**	**2**	**3**	**4**
TI	-0.0321	-0.0844***	-0.0406**	.	TI	-0.00800	-0.0714**	-0.00368	.
	(0.0197)	(0.0289)	(0.0196)	.		(0.0197)	(0.0287)	(0.0197)	.
TP	0.132***	0.131***	0.120***	0.114***	TP	0.136***	0.134***	0.133***	0.131***
	(0.0201)	(0.0201)	(0.0120)	(0.0120)		(0.0202)	(0.0201)	(0.0132)	(0.0133)
ATE	-0.0373	-0.0363	-0.0233	-0.0166	ATE	0.0224	0.0246	0.0178	0.0149
	(0.0280)	(0.0280)	(0.0173)	(0.0174)		(0.0280)	(0.0279)	(0.0182)	(0.0185)
Constant	7.478***	7.530***	7.473***	7.465***	Constant	7.493***	7.557***	7.486***	7.493***
	(0.0142)	(0.0255)	(0.0135)	(0.00429)		(0.0142)	(0.0252)	(0.0144)	(0.00456)
Observations	5,222	5,222	5,222	5,222	Observations	4,715	4,715	4,715	4,715
R-squared	0.015	0.019	.	0.062	R-squared	0.023	0.030	.	0.102
Industry FE	No	Yes	No	No	Industry FE	No	Yes	No	No
Firm RE	No	No	Yes	No	Firm RE	No	No	Yes	No
Firm FE	No	No	No	Yes	Firm FE	No	No	No	Yes
Firm clustered s.e's	No	No	Yes	Yes	Firm clustered s.e's	No	No	Yes	Yes
Number of firms	.	.	2,974	2,974	Number of firms	.	.	2,724	2,724

Dependent variable: ln(wage sum/employee). Incumbent employees working at the same firm during the period of study. ATE*100 = point estimates in the paper. Standard errors within parentheses.
*p<0.10, **p<0.05, ***p<0.01.

Figure A1. Wage development in restaurant and hairdresser firms and control firms in the 2004-2006 pretreatment period and the 2007-2010 posttreatment period. Minimum of two employees. Based on individuals staying at the same firm for at least two consecutive years.



Note: Average ln(wage/employee). Incumbent workers for at least two consecutive years. All firms in the treatment and control industries are included. Outliers (defined as annual changes in the outcome variable by more than +/- three standard deviations from the average change) are excluded.