

Is regulatory compliance by employers possible without enforcement?

Evidence from the Swedish labor market

Axel Cronert

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Is regulatory compliance by employers possible without enforcement?

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by

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Abstract

This study shines new light on an ongoing debate about the extent to which discouraging enforcement activities are necessary to make regulated actors comply with government regulations. Specifically, it evaluates a long-standing but essentially unenforced regulation that mandated employers in Sweden to post their vacancies at the Public Employment Service (PES) to improve matching and the labor market prospects of disadvantaged workers. Using comprehensive vacancy data from the PES, it tests whether the regulation—despite not being enforced—influenced employers’ vacancy posting behavior in the period prior to its partial repeal in 2007. Exploiting the fact that the repeal did not apply to employers in the central government sector, the difference-in-differences analyses conducted in this study identify a substantial and significant negative effect of repealing the unenforced law on employers’ vacancy posting behavior, under reasonable assumptions. This finding is at odds with standard deterrence models of regulatory compliance and hints at an important role for organizational factors related to cultures and norms. A supplementary analysis of heterogeneous effects among local government employers investigates to what extent some organizational factors are correlated to compliance with the unenforced regulation.

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Table of contents

1	Introduction	3
2	The Law on Universal Posting of Vacancies (LUPV)	5
3	Evaluating the repeal of the LUPV to learn about compliance	7
4	Data and classifications	10
4.1	The PES vacancy order dataset	10
4.2	Delimitations of the dataset	11
4.3	Outcome variable	12
4.4	Sector classification	12
5	Empirical strategies and results	13
5.1	Approach I: Regional-occupational labor markets	13
5.2	Approach II: Central and local government entities	17
5.3	What factors may drive compliance with unenforced regulation?	21
6	Concluding discussion	27
	References	30
	Appendix	37

1 Introduction

A growing number of pressing societal challenges—ranging from the prevention of global warming to the promotion of inclusive labor markets—require public policy for which a successful outcome hinges on regulatory compliance by private actors; not least by firms. Accordingly, efforts to monitor and enforce regulations make up a substantial and growing share of contemporary governments’ activities (Parker and Nielsen 2009). For instance, in Sweden, monitoring and enforcement expenditure has been estimated to nearly 1 percent of the general government’s final consumption expenditure and has been increasing over the past decades (Statskontoret 2012).

Hence, it may come as no surprise that, in recent decades, plenty of scholarly effort has been devoted to understanding when and for what reasons regulatory compliance by corporate actors is most likely to come about, and which regulatory strategies are the most effective to that end (for two useful reviews, see Parker and Nielsen 2009; Schell-Busey et al. 2016). A common account of this literature holds that although there is a general agreement that regulatory compliance is a complex process, the field is divided with respect to which type of input factors is more important: external *deterrence* factors or internal *organizational* factors (Coglianese and Kagan 2007; Galle 2017). Work in the former strand stresses the importance of monitoring and enforcement on the part of the regulator, arguing that the existence of a regulatory system that provides sufficiently certain and/or severe formal sanctions against violations is crucial to deter utility-maximizing corporate actors from shirking (e.g., Block et al. 1981; Potoski and Prakash 2011; Markell and Glicksman 2014).

Studies that rather emphasize the role of organizational factors tend to observe that corporate compliance is often higher than a standard deterrence model would predict, and suggest that this may be explained by reference to the intrinsic motivations, such as morals, norms, and duty, among stakeholders and employees (Vandenbergh 2003; Feldman 2011; Kagan et al. 2011; Galle 2017; Parker and Nielsen 2017). In this framework, compliance is motivated not only, or primarily, by fear of formal sanctions but rather by fear of disgrace in the eyes of social peers or by a desire to conform with

internalized beliefs about the appropriate way to act. These factors, in turn, may be affected by the design of regulations, even in cases where these regulations do not entail monitoring and enforcement (Tyler 2006; Kagan et al. 2011).

A recent meta-analysis of studies on regulatory compliance of corporations suggests that the jury is still out with respect to the effectiveness of various regulatory strategies (Schell-Busey et al. 2016). The limitations of existing scholarship highlighted by the authors include the lack of systematic data on corporate violations, the inaccessibility of firms to researchers, and the shortage of methodologically rigorous studies. Specifically, a common identification problem in this literature is that regulations are mostly not exogenous to the outcome of interest, as governments tend to be more likely to select a particular regulatory strategy where they expect it to have an effect (Galle 2017).

Overcoming some of these limitations, this study seeks to fill a gap in the literature by evaluating a particularly informative case of regulatory strategy, namely one for which the deterrence mechanism of corporate compliance is ruled out because the regulation is essentially unenforced (and, arguably, unenforceable). The regulation in question mandated employers in Sweden to post a vacancy at the Public Employment Service (PES) whenever they were looking to hire an additional employee, while the PES was both unwilling and unable to enforce the regulation.

My empirical strategy for evaluating compliance in this context exploits a partial repeal of said regulation enacted in 2007. Because it did not affect central government employers, and not all jobs, the repeal can be evaluated using difference-in-differences (DID) analyses under reasonable assumptions. My analyses identify a substantial and significant negative effect of repealing the regulation on the propensity of employers to post vacancy orders at the PES. Because there is no plausible threat of deterrence in play, I interpret this as an effect of organizational factors. In an attempt to explore the possible drivers of this effect, I assess the impact of a number of previously theorized factors related to organizational norms and duty, by exploiting the heterogeneous effects across local government employers. Tentative results from these analyses indicate that local governments with a more law-abiding organizational culture and a stronger commitment to social responsibility were more prone to comply with the unenforced regulation.

In the next section, I present a background and description of the regulation. The subsequent section describes the data used in the analyses. I then describe the two empirical strategies that are used to identify the effect of repealing the regulation, and report the results. In the following heterogeneity analysis, I assess a set of potential drivers of compliance at the organizational level among local government employers. A concluding section discusses the findings and their implications.

2 The Law on Universal Posting of Vacancies (LUPV)

Since the early 1940s, Swedish central government agencies seeking to recruit civil personnel have (with certain exemptions) been instructed by the government to post their vacancies at the Public Employment Service to give the PES a chance to refer job seekers to these positions¹ (Regeringen 1975, p. 21). Today, this requirement is found in the 1984 Instruction on Posting of State Vacancies (henceforth, the IPSV) (Regeringen 1984).

Beginning in 1976, a similar obligation (again with certain exemptions) was step-wise imposed on all employers, including private firms and local government entities, through an act commonly referred to as the Law on Universal Posting of Vacancies (the LUPV, for short) (Regeringen 1976). The motivation was to improve the PES's information about the available jobs, which in turn was expected to lower search costs for both workers and employers and to improve match quality (SOU 1978, p. 226). The law was also expected to reduce the gaps in information about job opportunities among workers, to the benefit of groups with more limited networks in the labor market, such as youth and non-natives (Regeringen 1975; SOU 2006).

Failure to comply with the law could result in a fine of up to 500 SEK (\$60) (Regeringen 1976; 1990, p. 83). For most of the law's existence, however, this rule was virtually unenforced. Indeed, over the 30 years during which the law was in force, a fine was imposed at no more than two occasions, both of which were in the early 1980s (SOU 2006, p. 315). In an illuminating statement from 1998, the PES, which was responsible

¹ In this paper, the term PES is used to refer both to the Public Employment Service (*Arbetsförmedlingen*), which was established as an independent government agency on January 1, 2008, and to the agency in charge of the public employment services before that date, the National Labor Market Board (*Arbetsmarknadsstyrelsen, AMS*).

Table 1. Overview of two regulations on public posting of vacancies in Sweden

Regulation	Law on Universal Posting of Vacancies	Instruction on Posting of State Vacancies
Title in Swedish	Lag (1976:157) om skyldighet för arbetsgivare att anmäla ledig plats till den offentliga arbetsförmedlingen	Förordning (1984:819) om statliga platsanmälningar
Start date	1976–1979 (step-wise introduction)	1984 (with precursors from the 1940s)
End date	July 2, 2007	Ongoing
Targeted employers	Private sector and local government sector	Central government sector (agencies and quasi-corporations)
Place for posting	The Public Employment Service	The Public Employment Service
Exempted positions	Positions with a duration of up to 10 days Positions intended for: – A current employee – A family member of the employer – A person who according to law or other regulation takes precedence – A person entitled by law to employment promoting measures (granted tripartite approval) Positions that involve work in the employer's household Teaching positions for which benefits are regulated by the state Management positions and equivalent positions Positions that presume a certain ideological or religious affiliation Positions that are appointed through an electoral procedure	Teaching positions in certain state-run schools Positions intended for: – A current employee – A person who has been dismissed from a government position – A disabled person
Sanctioning rules	Non-compliance may result in a fine of 500 SEK (changed in 1991 to a fine of an unspecified amount)	None. The Parliamentary Ombudsmen (JO) and the Chancellor of Justice (JK) may criticize non-compliance
De facto sanctioning	Two instances in the 1980s	One instance in 2017 (by the JO)

for notifying the judicial system of violations, reasoned that the best strategy to promote compliance with the LUPV is by maintaining a good service to recruiting employers (Justitieombudsmannen 1997, p. 539). Moreover, the internal instruction that guided PES caseworkers' handling of vacancy orders at the time did not even mention the possibility of monitoring and sanctioning (AMS 2003).

The IPSV resembles the LUPV not only in terms of the content of its rules, but also in the sense that it is a virtually unenforced piece of regulation. The instruction contains no rules on sanctions against non-compliant government agencies and it was not until 2017 that an agency was first formally criticized in a judicial review for not having posted a

number of vacancies at the PES (Justitieombudsmannen 2017). *Table 1* compares the two pieces of regulation.

3 Evaluating the repeal of the LUPV to learn about compliance

Soon after the change of government following the general election in 2006 the LUPV was repealed, taking effect on July 2, 2007. The repeal was in line with a pre-electoral declaration by the new center-right governing coalition, which considered the option to also repeal the IPSV but decided to leave it in force. The instruction remains virtually unchanged to date.² In effect, employers in the private sector and local government sector were freed from their vacancy posting obligation as of the second quarter of 2007 while employers in the central government sector remained under uninterrupted regulation.

These circumstances are fortunate from an analytic perspective, because they make it possible to identify, under certain assumptions, the ‘treatment’ effect of repealing the unenforced LUPV by comparing the vacancy posting behavior of treated and non-treated employers before and after July 2, 2007. An obvious threat to such a research design is that there may be factors unrelated to the repeal that caused vacancy posting behavior of central government employers to diverge from that of other employers in the post-repeal period. Failure to account for such factors would result in a biased estimate of the treatment effect. Three such factors should be addressed up-front.

First, there is the possibility that the government introduced changes that increased or decreased the pressure on central government employers to post vacancies at the PES in the post-repeal period. However, there is nothing to suggest that this would be the case. To begin with, it should be noted that the central government agencies enjoy a high degree of autonomy vis-à-vis the government, including, with a few exemptions, with respect to their hiring and firing decisions (Ahlström 2017). A membership organization gathering around 200 of these agencies, the Swedish Agency for Government Employers (*Arbetsgivarverket*), coordinates its members on a range of employment matters. In 2006, the agency issued a strategy for central government employment policy, which

² The only change worth mentioning is the addition of a possibility for the PES to grant central government agencies exemptions from the instruction in special circumstances; a change that took effect on January 1, 2008. I am aware of no such exemptions.

was in force between 2007 and 2010, and which includes no mention of the PES nor of recruitment procedures in general (Arbetsgivarverket, 2006a). Also, the agency's guidelines for central government employment, in which the IPSV is summarized, was not altered between 2006 and 2012 (Arbetsgivarverket, 2006b).

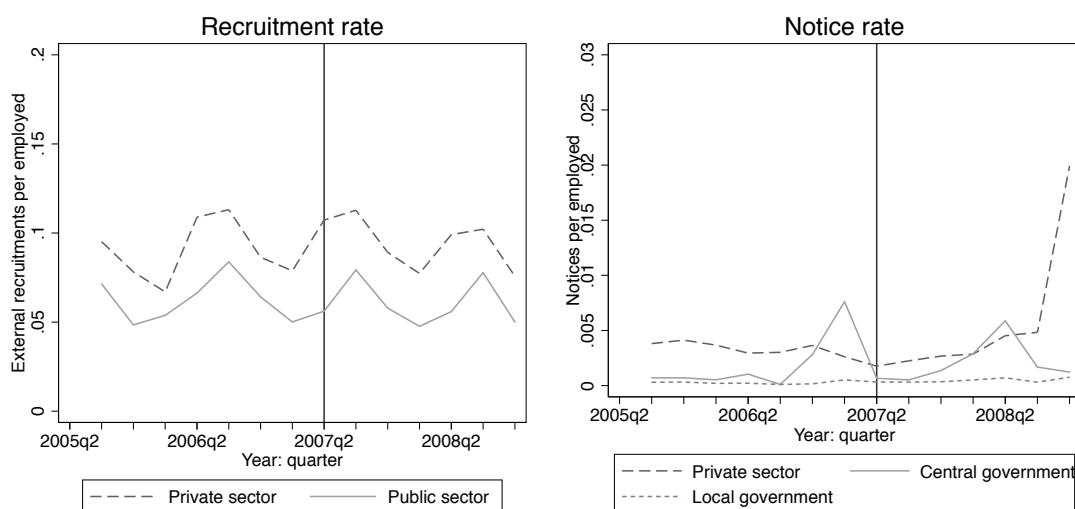
Second, there is the possibility that, following the repeal, the PES's reach-out activities to acquire vacancies from employers was intensified particularly vis-à-vis state employers, perhaps in an effort to compensate for a loss of vacancies from other employers. However, there is little evidence on this front. The instruction that governs the PES does mention the acquisition of vacancies as one of the agency's tasks, but it puts no priority on any particular group of employers. And although the PES did intensify its employer reach-out activities during the years after the repeal, there is nothing to suggest that any particular group of employers was given priority. Indeed, there is little to suggest that the PES considered the repeal of the LUPV to be of much importance to begin with.³

Third, there is the risk that changes in the labor market in the post-repeal period may have affected the recruitment behavior of employers in different sectors differently. An obvious concern in the present case is the outbreak of the Great Recession in 2008, which we would expect to disproportionately affect private sector employers. This issue is explored in *Figure 1*. The panel on the left plots the recruitment rate, measured as the number of externally recruited persons per employee in the private and public sectors, quarterly between 2005q3 and 2008q4. The right-hand panel plots the notice rate, measured as the share of employees that received a notice of dismissal in the private, central government, and local government sectors, per quarter over the same period. Both panels show that the sectors followed largely similar trends until 2008q2, after which, for the private sector, the recruitment rate saw a less marked increase and the notice rate began to deviate upwards.⁴ These patterns should be reason enough to delimit effect evaluations

³ The PES declared no objections to the government's repeal act (Regeringen 2007, p. 70), and in one of the agency's first reports that mentioned the repeal ex-post, it was noted without any elaboration that the repeal "has in no way decreased the inflow of vacancies to the PES" (Arbetsförmedlingen 2008, p. 2, author's translation).

⁴ The spike in the central government sector notice rate in 2007q1 represents that notice was given to approximately 1,600 individuals due to the closing down of the Swedish Integration Board and to cutbacks in the Swedish Social Insurance Agency and the Swedish Forest Agency.

Figure 1. External recruitments and notices of dismissal, 2005q3–2008q4. Horizontal lines indicate the repeal of the LUPV. Sources: Statistiska centralbyrån (2018e) and Arbetsförmedlingen (2018b).



to the first four quarters following the reform, that is, to 2007q3–2008q2.

Against this background, there are favorable conditions for exploiting the partial repeal of the LUPV to analyze the effects of unenforced regulation on employer behavior. However, it should be noted that such analysis relies on the assumption that employers at the time were aware of the law’s existence as well as the lack of enforcement. While it has not been possible to systematically assess this assumption, some indications suggest that there should have been at least a certain level of awareness. For instance, the law was repeatedly brought up in political debates in 2005 and 2006, and the lack of enforcement had at times been highlighted in national media as well as in government reports (e.g., Behtoui et al. 2004; SOU 2006).

As mentioned in the introduction, there are diverging positions in the existing literature as to whether we should expect that employers complied with the unenforced LUPV, and, accordingly, whether we should expect that the repeal of the law affected employers’ vacancy posting behavior. A basic deterrence model of regulatory compliance would predict that due to the overall lack of monitoring and enforcement on behalf of the PES, the LUPV would be ineffectual and we would expect to see little difference in employer behavior before and after repeal (Block et al. 1981; Markell and Glicksman

2014). According to other models, fear of sanctions is not among primary drivers of corporate compliance; instead it is likely that due to some social or normative motives in place within the organization, employers may have chosen to comply with the regulation despite the lack of enforcement (Kagan et al. 2011; Galle 2017).

Before turning to the analysis, a note is warranted about what it means to comply in the case at hand. I apply a narrow definition of compliance as behavior that is obedient to a regulatory obligation, *conditional on the existence of that obligation* (cf. Parker and Nielsen 2017). As for the LUPV, this means that an employer is compliant to the extent that they post vacancies at the PES *that would not have been posted in the absence of the law*. This point is important to keep in mind because it distinguishes cases of compliance from cases—of which there are of course many—where an employer would have posted a vacancy at the PES regardless of the law’s existence, since doing so is in line with their underlying needs or preferences (e.g., a need to extend their search for new recruits outside their own network). Because nothing prevents such cases from taking place on either side of the repeal, what I do here is to test whether the repeal altered the behavior of employers whose underlying preference was to withhold their vacancies from the PES (for a similar approach, see Galle 2017).

4 Data and classifications

4.1 The PES vacancy order dataset

This study makes use of a dataset generously provided to the author by the PES headquarters, which contains the universe of vacancy orders submitted to the PES between 1992 and 2017 (Arbetsförmedlingen 2018a). The dataset includes several variables at the level of the vacancy order, including the date of submission, the type of order (e.g., for a regular position, a summer internship, a position outside of Sweden, etc.), the occupation and the required level of qualification, the expected duration of the job, and the number of available positions. It also includes a few variables at the level of the posting legal entity—that is, the employer—such as the industry and the location of operation. An anonymized version of each entity’s registration number makes it possible to track individual entities’ posting behavior over time, yet prevents any systematic linking to other data sources.

Usefully, an open-ended variable that contains the name of the recruiting entity in practice makes it possible to identify some entities of particular interest, such as central government agencies and local governments.

4.2 Delimitations of the dataset

From the outset, I delimit the baseline dataset to orders posted between 2004q3 and 2008q2. The latter limit is drawn due to the reasons stated above. The former limit is drawn so as to enable inspections of trends over a sufficient period prior to the repeal.⁵

For analytic reasons, three more delimitations are motivated. First, due to a well-known problem of duplicates in the vacancy order data in the years 2006–2008, I wholly exclude three minor occupations that then made up 2.4 percent of the total employment in Sweden yet represented 13.3 percent of the posted orders.⁶ Inspections performed by the PES in the years 2006–2008 revealed that the rate of duplicates in these particular occupations varied from 5 up to 44 percent over the period. For the remaining occupations, the PES estimated the rate of duplicates to be in the range of 4–6 percent, with no discernible trend (Liss 2008).

Second, I exclude orders posted by employers in the employment and recruitment service industry, which at the time represented around 1.8 percent of total employment.⁷ The motivation is that said industry is largely comprised by the PES itself, which reasonably must not be included in the present study, and staffing and recruitment agencies, which are known to post vacancies at the PES partly to attract staff for potential future assignments rather than to fill existing vacancies⁸ (Cronert 2015). This step excludes an additional 12.3 percent of the orders.

Third, I exclude categories of vacancy orders that, theoretically, should not be affected by the law in the first place. These include positions with a duration of no more than

⁵ In addition, including earlier data comes at the cost of a more uncertain sector classification, due to more observations with lacking registration numbers (see Footnote 9).

⁶ The excluded occupations are technical and commercial sales representatives (ISCO-88: 3415), demonstrators and telephone salespersons (ISCO-88: 5227) and street vendors and related workers (ISCO-88: 9110).

⁷ The SNI-2002 classification for this industry is 74.50.

⁸ This is a good example of behavior that would fall outside the scope of my definition of compliance, because there is no reason why it would not appear just as much also in the absence of a vacancy posting obligation.

10 days, orders that were not posted by the employers themselves but acquired from them by either a PES caseworker or a job-seeker, orders for positions outside of Sweden, orders reserved for subsidized employment, and orders linked to a specific sabbatical year program (*Friår*) in operation in 2005–2006. This operation excludes another 9.2 percent of the orders and leaves us with a baseline sample of approximately 893,000 orders, corresponding to 65.2 percent of the initial observations.

4.3 Outcome variable

The outcome variable used to capture compliant behavior of employers is the *vacancy order rate*, defined as the number of vacancy orders posted at the PES by an employer or a group of employers in a specific period, divided by the average number of employees represented by the employer(s) in question in that period, and then multiplied by 100. A perhaps more accurate measure of compliance would be the number of vacancies posted *per newly recruited employee*. Unfortunately, data on new hires are not readily available at the levels of analysis applied in this study. Hence, I resort to the second-best option, assuming that the trends in the recruitment rate do not vary systematically between employers that were affected by the repeal of the LUPV and employers that were not. Some support for this assumption is provided in the left-hand plot of *Figure 1* and from an auxiliary analysis of data on 462 central and local government entities retrieved from Statistiska centralbyrån (2018d), which finds no notable difference in the average changes in recruitment rates between 2007 and 2008 across the two sectors.

4.4 Sector classification

As described above, whether an employer is affected by the repeal of the LUPV is determined by its legal entity, and more specifically whether or not it is a central government entity. Unfortunately, the vacancy order database contains no such variable. However, for any registered legal entity, the entity's sector identity can be inferred from the initial digits of its registration number, which is available in the source database. Therefore, the PES was asked to create a new variable indicating the sector identity of the employer, before anonymizing the registration numbers and disseminate the data to the author. Based predominantly on this variable, the employers are then categorized

into three major sectors: the central government (3.9 percent of the orders), the local government (34.4 percent), and the private sector (61.7 percent).⁹

5 Empirical strategies and results

The great level of detail in the dataset allows for multiple levels of analysis, and hence makes it possible to apply a number of complementary approaches for estimating the effect of repealing the LUPV on employers' vacancy posting behavior. Key to each approach is to compare the vacancy posting behavior of employers in the private and/or local government sectors, who were 'treated' by the repeal, with employers in the central government sector, who were not, before and after the repeal. Specifically, I apply two approaches: First, I conduct a set of difference-in-differences (DID) analyses where data are organized by sector, region, and occupation. Second, I run another set of DID analyses in which instead the units of analysis are a set of individual central and local government entities. Descriptive statistics for both datasets are provided in *Table A2* in the appendix.

5.1 Approach I: Regional-occupational labor markets

In the first application, the unit of analysis is referred to as the 'regional-occupational labor market' and is defined as a unique combination of the aforementioned three sectors, 113 occupations,¹⁰ and 78 labor market areas,¹¹ that jointly cover the entire Swedish labor market. To give a few examples, private sector architects, engineers, etc. in the

⁹ The central government sector is defined as entities of central government—either central (initial digits 2021; entity type 81) or regional (2022; 89) entities—and social security offices (2420; 85). The local government sector is defined as municipalities (2120; 82), county councils (2321; 84), and federations of local government authorities (2220; 83). Entities with any other registration number are classified as private sector entities. Approximately 5 percent of the observations in the sample lack a registration number. For these observations, sector identity is assigned in two steps: First, I manually inspect the entity names for each one of these entities that has posted four or more orders (57 percent) and assign any identified central or local government entity its correct category. Second, I search among the names of the remaining 2 percent of the entities for key words that help to identify central and local government entities. I make sure that each of the approximately 100 transformation commands used in this step does not erroneously classify any private entity as public. Entities that are not identified in this procedure are classified as private sector entities.

¹⁰ The occupations are defined based on the occupational classification SSK-96, which adheres closely to the international ISCO-88 classification.

¹¹ Constructed based on commuting patterns, the labor market areas divide the country into regional units that are more or less independent with regard to labor supply and demand. As such, they are an adequate unit of analysis in regional analysis of the Swedish labor market (Statistiska centralbyrån 2010).

Malmö-Lund region is a sector-area-occupation observation close to the mean size with approximately 260 employed, while local government care workers in the Stockholm-Solna region is the largest one with approximately 69,000 employed. Each unit is measured in four periods: the post-repeal period 2007/08, and the three pre-repeal periods 2006/07, 2005/06, and 2004/05.¹² Out of the 26,442 possible combinations of sector, area and occupation, there are 14,531 (54.9 percent) that have at least one employee in each of the four periods. Those that do not are considered non-existing labor markets and are excluded from the sample.

The average treatment effect of repealing the LUPV is estimated by means of a two-period DID estimator (Bertrand et al. 2004), specified as:

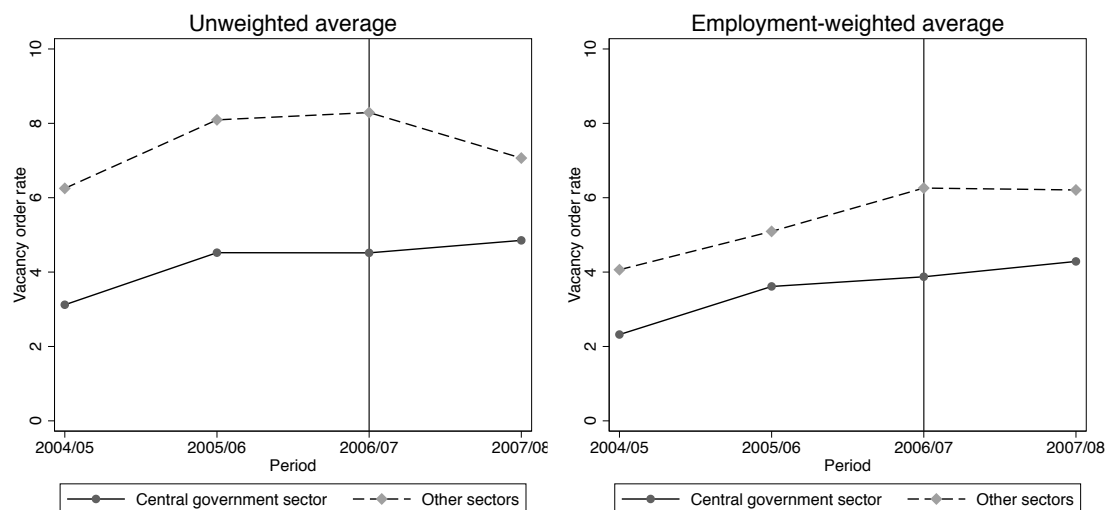
$$V_{saot} = \gamma_{sao} + \lambda_t + \beta R_{st} + \varepsilon_{saot} \quad (1)$$

where V_{saot} represents the vacancy order rate in the sector-area-occupation sao in period t where $t \in \{2006/07, 2007/08\}$, γ is a sao -specific effect, λ is a period-specific effect and ε_{saot} is an error term. R_{st} is a dummy variable that scores 1 for observations that belong to the private or local government sectors in the post-repeal period. β represents the estimated average treatment effect on the treated of repealing the LUPV; the ATT, for short. A two-way robust variance estimator is used to compute the standard errors to control for the possibility that error terms are correlated both within labor market areas and within occupations (Cameron et al. 2011).

The identifying assumptions in this model include the assumption that, in the absence of treatment, the average vacancy order rate among central government employers and other employers operating in the same regional-occupational labor market would have followed parallel trends. Whereas this assumption is not directly testable, a common diagnostic is to assess whether their trends are parallel in the pre-treatment period. If central government employers and other employers in the same regional-occupational labor market would show diverging trends already in the pre-repeal period, we would have stronger reasons to question the validity of the parallel trends assumption. This

¹² Each period consists of four quarters: q3 and q4 of the first year and q1 and q2 of the second year.

Figure 2. Average vacancy order rate across 14,531 sector-area-occupations, by sector, 2004/2005–2007/2008. Sources: Arbetsförmedlingen (2018a) and Statistiska centralbyrån (2018a).



assessment may be done by means of visual inspection as well as a placebo test.

Figure 2 explores these trends visually. For the time being, consider the panel on the left, which plots, for employers in the central government sector and other employers, respectively, the unweighted average of the vacancy order rate among all observed sector-area-occupations during the four quarters immediately following the repeal, as well as three equally long periods prior to the reform. The plot indicates that the two groups of employers exhibit fairly similar trends in the pre-repeal periods.

A statistical placebo test corresponding to this visual inspection may be performed by dropping the 2007/08 period, assigning the 2006/07 period as the placebo treatment period, and then re-estimating the model in Equation 1 on the two pre-repeal periods, $t \in \{2005/06, 2006/07\}$.¹³

As a second placebo test, the model in Equation 1 can be re-estimated on the original two periods using an alternative outcome variable that, theoretically, should be unaffected by the treatment. This time, I use the vacancy order rate, V_{saot}^P , calculated like above, but based only on two categories of orders which were in principle not covered by the LUPV:

¹³ An equivalent test has been run on the two periods $t \in \{2004/05, 2005/06\}$, but because the results of this analysis are similar to those of the first placebo test, this test is left out here to conserve space.

Table 2. Results from DID on the sector-area-occupation level

	Unweighted regression			Employment-weighted regression		
	Main model (2:1)	Placebo model A (2:2)	Placebo model B (2:3)	Main model (2:4)	Placebo model A (2:5)	Placebo model B (2:6)
Repeal of the LUPV	-2.507*** (0.791)			-0.465** (0.179)		
Placebo: Repeal at t-1		0.069 (0.562)			0.906*** (0.182)	
Placebo: Unaffected Orders			-0.019 (0.043)			-0.005 (0.004)
Constant	10.192*** (0.320)	9.190*** (0.227)	0.110*** (0.017)	6.326*** (0.084)	5.137*** (0.086)	0.049*** (0.002)
Unit & period effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	29,062	29,062	29,062	29,062	29,062	29,062
Units	14,531	14,531	14,531	14,531	14,531	14,919
Sample average (\bar{V})	9.18	9.22	0.10	6.11	5.56	0.05
β/\bar{V}	-27.3%	0.7%	-18.1%	-7.6%	16.3%	-10.0%
Adjusted R^2	0.461	0.492	0.202	0.820	0.786	0.390

Two-way clustered standard errors in parentheses. Non-nested clustering on labor market area and occupation, computed using the `-reghdfe-` package for Stata (Correia 2017). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

orders posted for positions with a duration of no more than 10 days, and orders posted for positions outside of Sweden.¹⁴

Table 2 reports the main results. The three first models report the results from specifications where each sector-area-occupation observation is given equal weight regardless of size. In Model 2:1, the average effect of repealing the LUPV on the vacancy order rate in a treated regional-occupational labor market is estimated at -2.5 percentage points, a reduction corresponding to a substantial 27 percent of the mean vacancy order rate in the sample (\bar{V}). The placebo analysis reported in Model 2:2 shows no sign of diverging trends in the pre-treatment period, thus posing no challenge to the parallel trends assumption. In addition, Model 2:3 reports that the effect of the repeal on the placebo outcome, while considerable in size, is far from statistically significant. In sum, and at odds with H_1 , the results of the first round of analyses lend support to the notion that the LUPV did affect employer behavior despite being unenforced.

This conclusion is largely corroborated by the three latter models in *Table 2*, which correspond to the three first models but weigh each sector-area-occupation by its average number of employed. Given the large variation in size among sector-area-occupations, the employment-weighted models are particularly useful, because with these weights applied

¹⁴ These orders made up approximately 4.5 percent of all orders posted during the period of investigation.

the estimated effects can be interpreted as the population average partial effects for the Swedish labor market as a whole (Solon et al. 2015, p. 312).

From a policy perspective, then, the perhaps most informative estimate is that from the weighted Model 2:4, which suggests a 7.6 percent reduction in the vacancy order rate. However, as indicated by the substantial *positive* placebo effect identified in Model 2:5, the vacancy order rate among non-central government observations increased considerably more than the rate among central government observations in the pre-treatment period when these weights are applied (the employment-weighted trend plot on the right in *Figure 2* also illustrates this). This suggests that the 7.6 percent estimate is likely underestimated, to the extent that one is willing to assume that the deviant pre-treatment trends would have continued had the LUPV not been repealed. It may also be noted that the placebo outcome effect in the weighted Model 2:6 is again substantial in size but not statistically significant. The two placebo outcome coefficients in *Table 2* suggest, against expectations, that affected and unaffected orders followed similar trends after the repeal of the law, which would be a cause of concern. On the other hand, likely due to the small number of orders of this kind, the precision of these coefficients is consistently low and closer examination shows that these two models are particularly sensitive to outliers. Hence, their results should be interpreted with extra caution.

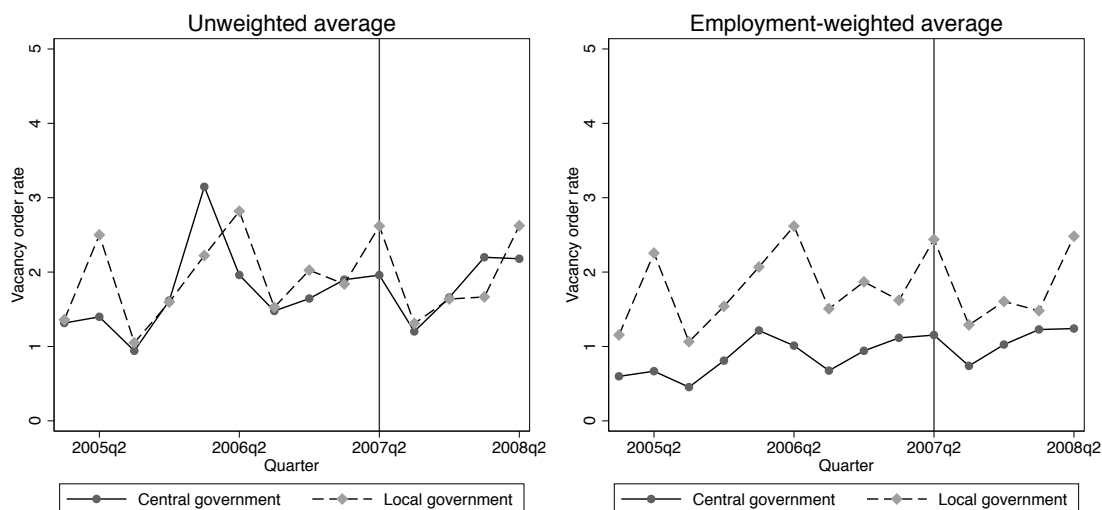
Lastly, it can be mentioned that auxiliary analyses reported in the appendix (*Table A1*), which interact the repeal indicator from Model 2:1 with a set of occupation-level characteristics, find no significant variation in the effect across occupations.

5.2 Approach II: Central and local government entities

For the second approach, vacancy orders are instead collapsed by the legal entity by which they were posted and the quarter in which they were submitted. This allows us to track and compare treated and non-treated entities directly and to explore factors that may drive compliance at the organizational level.

For a couple of reasons, this part of the analysis is delimited to central government entities and local government entities (that is, municipalities). First, the vacancy dataset largely lacks data on entity-level characteristics and it is practically impossible to

Figure 3. Average vacancy order rate across 188 central government agencies and 290 local governments, by sector, 2004q3–2008q2. Sources: Arbetsförmedlingen (2018a) and Statistiska centralbyrån (2018a).



systematically link all private entities in the dataset to other data sources. The comparably few central and local government entities, in contrast, are possible to identify and link to various other sources of data that can be used for exploring heterogeneous effects among entities. Second, because we know in which years each central government entity has been in operation (and because all municipalities have existed throughout the studied period), I can make sure to create a balanced panel that includes only entities that were in operation over the full period.¹⁵ I exclude entities that did not post a single vacancy order during the period (approximately 9 percent of central government entities) as well as three small entities which due to no more than one or a few posted vacancies exhibit a vacancy order rate of more than 33 percent in occasional quarters.¹⁶ Having done so, I arrive at a main sample of 188 central government entities and (all of the) 290 municipalities.

The average quarterly vacancy order rates for the entities in the two sectors are shown in *Figure 3*. The left-hand plot shows the unweighted average while the right-hand plot shows the weighted average where entities are weighted by their average employment.

¹⁵ This means that I exclude from the main sample the 7 agencies that were started later than January 1, 2005, and the 22 agencies that were closed down between 2005 and 2008.

¹⁶ Including these entities does not change the main result but it makes the unweighted analyses as well as the supplementary generalized synthetic control analyses generate misleadingly large effects.

Table 3. Results from DID on the legal entity level

	Main model (3:1)	Robust regression (3:2)	Weighted model (3:3)	Time trends (3:4)	Placebo A (Timing)	
					Unweighted (3:5)	Weighted (3:6)
Repeal of the LUPV	-0.221** (0.090)	-0.223*** (0.039)	-0.294*** (0.075)	-0.271** (0.133)		
Placebo: Repeal at t-1					-0.003 (0.090)	-0.047 (0.047)
Constant	1.890*** (0.016)	-0.450** (0.191)	1.669*** (0.017)	19.479*** (0.880)	1.762*** (0.015)	1.520*** (0.011)
Unit & period effects	Yes	Yes	Yes	Yes	Yes	Yes
Linear time trends	No	No	No	Yes	No	No
Observations	6,692	6,692	6,692	6,692	6,874	6,874
Units	478	478	478	478	491	491
Sample average (\bar{V})	1.85	1.54	1.60	1.85	1.76	1.51
β/\bar{V}	-11.9%	-14.5%	-18.4%	-14.4%	-0.2%	-3.1%
Adjusted R^2	0.285	0.685	0.638	0.279	0.312	0.639

Robust standard errors in parentheses, clustered at the entity level (except for the robust model 3:2, for which standard errors are calculated using the pseudovalues approach). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In this application, the DID-estimator has the following specification:

$$V_{et} = \gamma_e + \lambda_t + \beta R_{st} + \varepsilon_{et} \quad (2)$$

where V_{et} is the vacancy order rate¹⁷ for entity e in quarter t , where $t \in \{2005q1 \dots 2008q2\}$, γ is an entity-specific effect, λ is a quarter-specific effect and ε_{et} is an error term. R_{st} is a dummy variable that scores 1 for local government entities in the post-repeal quarters 2007q3–2008q2, and β again is the estimated average treatment effect of repealing the LUPV. In this analysis, standard errors are clustered by entity.

Table 3 reports the main results. The findings corroborate the results from the previous section: In Model 3:1, the repeal of the LUPV is estimated to have caused, on average, a 0.22 percentage points reduction in the quarterly vacancy order rate of municipality employers, corresponding to 11.9 percent of the sample average (\bar{V}). Because the vacancy order rate is expressed as a fraction of the entity's employment, we may be worried that small central government agencies in particular may occasionally display very high values that unduly affect the regression results. Therefore, as a robustness check, Model 3:2

¹⁷ Similar to above, V_{et} is computed as the number of vacancy orders posted in a quarter, divided by the number of employees in that quarter, and then multiplied by 100. Employment data for the municipalities were retrieved from Statistiska centralbyrån (2018b), while data for the central government agencies were provided to the author by the Swedish Agency for Government Employers. These data are only available on an annual basis and refer to q4. A quarter specific employment indicator is calculated as a moving average of the yearly figure for the current quarter plus the yearly figure for the three preceding quarters.

Table 4. Results from DID on the legal entity level: Long-term effects

	Placebo B (Orders)		Post-repeal = 2007q3–2012q2		Post-repeal = 2011q3–2012q2	
	Unweighted (4:1)	Weighted (4:2)	Unweighted (4:3)	Weighted (4:4)	Unweighted (4:5)	Weighted (4:6)
Repeal of the LUPV			-0.616*** (0.082)	-0.461*** (0.077)	-0.472*** (0.128)	-0.341*** (0.104)
Placebo: Orders	0.003 (0.006)	0.000 (0.001)				
Constant	0.010*** (0.001)	0.008*** (0.000)	2.164*** (0.035)	1.836*** (0.041)	2.049*** (0.023)	1.770*** (0.024)
Unit & period effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,692	6,692	13,680	13,680	6,384	6,384
Units	478	478	456	456	456	456
Sample average (\bar{V})	0.01	0.00	1.90	1.59	1.96	1.69
β/\bar{V}	28.6%	0.4%	-32.4%	-29.0%	-24.1%	-20.2%
Adjusted R^2	0.190	0.267	0.308	0.616	0.327	0.658

Robust standard errors in parentheses, clustered at the entity level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

reports a robust regression, which handles such concerns by first excluding gross outliers and then down-weighting observations with large absolute residuals. The results are encouraging, as the effect estimate is slightly larger than in the baseline model. Model 3:3, in which entities are instead weighted according to their average employment, suggests that the population average partial effect for the municipal sector as a whole is even higher, at around 18 percent of the average vacancy order rate. Model 3:4, next, confirms that the results from the main model are robust to the inclusion of entity-specific linear time trends. Models 3:5 and 3:6 lastly, report a placebo test performed by re-estimating Equation 2 on a sample from which the four post-repeal quarters are dropped and the four pre-repeal quarters 2006q3–2007q2 are assigned as the post-repeal quarters.¹⁸ Neither the unweighted Model 3:5 or the weighted Model 3:6 show any sign of pre-reform trends.

Turning next to *Table 4*, Models 4:1 and 4:2 are run on the placebo outcome V_{saot}^P , like Models 2:3 and 2:6 above. They report one positive and one negligible coefficient that are both far from statistically significant and thus do not pose a threat to the results.

Although, for the aforementioned reasons, the main evaluation is limited to the period before the outbreak of the Great Recession, it might be of some interest to explore the long-term effects of the reform. To this end, I conduct additional analyses in which I extend the period of evaluation into, and beyond, the years of recession. Restricting these analyses to the public sector, which was less affected by the economic downturn, I retain

¹⁸ For the sake of symmetry, the sample used here is extended back to 2004q1.

the set of central and local government entities analyzed in *Table 3*, except those that were closed down during the period of investigation. Also reported in *Table 4*, Models 4:3 and 4:4 report the unweighted and employment-weighted results for the five-year period between the immediate post-repeal quarter 2007q3 and 2012q2. The results indicate that the effect increased over time; seen over the full period it is estimated at -32.4 and -29.0 percent, respectively. However, it is still possible that the economic downturn affected the central and local government sectors differently. To reduce the risk of such bias, the two last columns in *Table 4* report the results from a model in which the post-repeal period is limited to the four quarters between 2011q3 and 2012q2, that is, a year and a half after the end of the recession. As expected, the effects are reduced, to 24.1 and 20.2 percent, respectively, but are still larger than those in the short-term evaluation reported in *Table 3*.

As a further robustness check, I have re-estimated a number of the unweighted DID models reported above using the more advanced generalized synthetic control (GSC) approach developed by Xu (2017), which has the key advantage of relaxing the parallel trends assumption, allowing the treatment to be correlated with unobserved factors that may vary across units and time. The results from these models are presented in the appendix (*Figure A1*). In short, it turns out that in none of these models the GSC algorithm finds any unobserved factors to add to the specification. Consequently, the effect estimates from the GSC analyses are identical to those obtained in the corresponding DID analyses, albeit with slightly larger standard errors. This finding is worth highlighting as it buttresses the parallel trends assumption that underpins the DID analyses.

5.3 What factors may drive compliance with unenforced regulation?

So, it appears that despite the lack of enforcement, many employers did comply with the LUPV. But what factors may have driven their compliance? In this section, I report a set of additional analyses run on the main sample of central government agencies and local government entities analyzed in *Table 3* to explore whether the effects of repealing the LUPV varied across local governments in some systematic and informative manner. The purpose of this exercise is twofold. First, it serves to investigate whether some other type of external enforcement-like activity was the factor that prompted compliance

among local government employers. Second, it will be used to assess some existing theories about what kind of social and normative motives within the local government organizations that might have promoted their compliance in the case at hand.

Such analysis is performed by means of interacting the repeal variable R_{st} in Equation 2 with some theoretically relevant variable that varied across local government entities around the time of the repeal. Thus, for each such local government variable X_e , the terms

$$+ \beta_{x_e}(R_{st} \times X_e) \quad (3)$$

is appended to the right-hand side of the equation.¹⁹ In this setup, a negative coefficient for the interaction term for a variable in question indicates that following the repeal of the LUPV, local governments with higher scores on that variable saw a larger reduction in the vacancy order rate than others. That is to say, in the pre-repeal period, these local governments were more prone than others to comply with the regulation—that is, to post vacancies against their underlying preferences—despite the lack of enforcement.

Six variables are included in this manner, the first three of which concern external factors. First, as mentioned above, the PES had the responsibility for monitoring compliance and notifying the judicial system of violations. Although it appears that the PES centrally lacked the resources and motivation to do so, outreach activities targeted at employers to acquire vacancies is part of the job description of many PES caseworkers at the street-level. Possibly, the existence of a formal obligation, to which caseworkers could refer employers to deter them from shirking, made these outreach activities more effective; in effect turning these caseworkers into enforcement agents. If that were the case, the negative impact of the repeal on the inflow of vacancies would likely be higher in areas where PES outreach activities before the repeal were more intense.

To capture this factor at the municipality level, I create a measure of the PES

¹⁹ As reported in the appendix (*Table A2*), the variables used here are typically measured only for the local government entities and not for the central government agencies. However, such missingness does not cause any problem in the current application. Since for all entities in the central government sector, the repeal variable R_{st} is always 0, this is also true for the multiplicative interaction term $R_{st} \times X_e$, regardless of what value X_e would have had if it were measured for the entities in this sector.

caseworker intensity, calculated as the number of caseworkers employed by the PES in the municipality divided by total employment in the municipality (Statistiska centralbyrån 2018b). To account for the fact that the number of caseworkers is driven partly by the number of unemployed, the measure is divided by the municipality unemployment rate.²⁰

Second, considering the salience of this regulation to the Swedish unions—both the blue-collar federation LO and the white-collar federation Saco raised some objections against the repeal (Regeringen 2007)—it is possible that local union representatives put pressure on employers to post their vacancies at the PES and could do so more effectively while the LUPV was in place. If unions served this enforcing role, we would expect that workplaces where they had a larger say would see a more reduced vacancy order rate after the repeal of the LUPV. Lacking data on actual union influence within the local government, I use a proxy on *employee unionization*, which measures the union membership rate of local-level (and county-level) employees, constructed based on 13,600 responses to representative surveys from across Sweden in the years 2000–2012 (Weibull et al. 2014).

A third deterrence-related factor is scrutiny by local media. Previous studies have shown that the monitoring of independent mass media may prompt corporations to engage in potentially costly socially responsible behavior—including in the realm of employment practices—to strengthen their reputation among stakeholders (e.g., Kanagaretnam et al. 2018; El Ghouli et al. 2019). Similarly, with respect to Swedish local governments, Svaleryd and Vlachos (2009) have found that high local media coverage tend to discourage high-level local politicians from engaging in reputationally costly, albeit legal, rent extraction. According to this logic, it is possible that local government employers were more prone to comply with the LUPV where they were more closely monitored by local media. To assess this possibility, I use a measure of the *media influence* on local politics, as gauged by local politicians in a large survey carried out in 2008 (Gilljam et

²⁰ The results pertaining to this variable do not change even if the unemployment rate adjustment is omitted when constructing the variable.

al. 2010).²¹

Leaving external deterrence factors aside, existing research offers a number of theories about what kind of social and normative motives that might promote compliance within an organization, and thus increase the effect of repealing the LUPV.

One relates to organizational culture, by which I refer to a set of values or expectations that are shared within the organization. As argued by Galle (2017), a compliant organization culture could emerge, for instance, from public expectations or from examples set by top management, and could influence behavior by motivating employees to adopt practices that are rewarded within the organization. If cultural factors are in play here, we would expect that employers' compliance with the LUPV is positively correlated with general measures of a law-abiding organizational culture. In an attempt to capture this elusive construct, I make use of a corruption index developed by Erlingsson et al. (2008) based on six questions about corruption perceptions posed to top politicians and high-ranking civil servants in the local governments in an anonymous survey carried out in 2008. Higher scores indicate less reported corruption and hence, in my interpretation, a more law-abiding culture in the local government organization.²²

A related factor discussed by Parker and Nielsen (2017) concerns the degree of agreement within the organization with the policy objectives and principles underpinning a particular regulation. Considering the objectives that motivated the law, we might thus expect to see that organizations which exhibit a stronger commitment to social responsibility in general, and to improving the labor market situation for disadvantaged workers in particular, were also more compliant with the LUPV.

As a proxy for the local government's commitment to alleviating labor market problems, I create an indicator on the local policy effort in the realm of active labor market policy (ALMP) in the year 2007. This *ALMP effort* indicator represents the local government's total net expenditure on policy measures to promote employment

²¹ The wording of the item translates as follows: "In your opinion, how much influence does each of the following actors exert on the municipality's political activities? – Journalists that monitor municipality politics". The item ranges from 0 to 10. For each municipality, I compute the mean score for all responding politicians (between 9 and 64 respondents). An alternative indicator used by Svaleryd and Vlachos (2009), based on the coverage of local newspapers, do not change the results for this factor.

²² Data is lacking for 11 out of 290 municipalities.

Table 5. Results from heterogeneous effects analyses with interaction effects

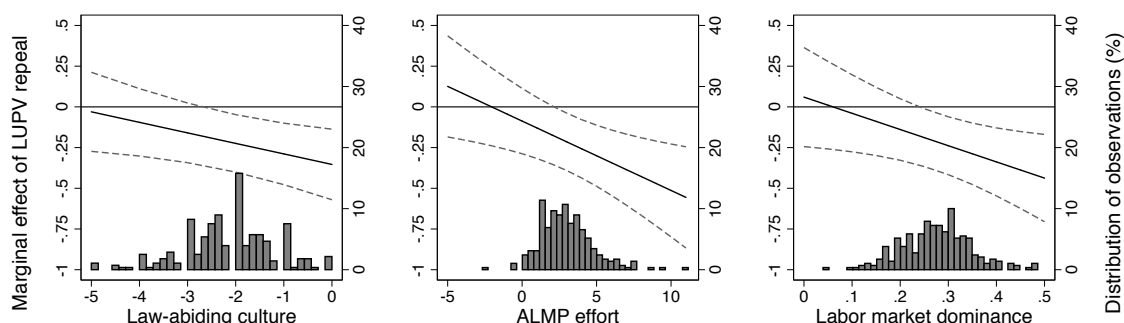
	(5:1)	(5:2)	(5:3)	(5:4)	(5:5)	(5:6)	(5:7)
Repeal of the LUPV	-0.26*** (0.10)	0.09 (0.23)	0.10 (0.21)	-0.33*** (0.11)	-0.05 (0.10)	0.09 (0.13)	0.43 (0.32)
Repeal of the LUPV × ...							
... Caseworker intensity	0.97 (0.79)						1.14 (0.79)
... Employee unionization		-0.42 (0.29)					-0.27 (0.28)
... Media influence			-0.07* (0.04)				-0.06 (0.04)
... Law-abiding culture				-0.05* (0.03)			-0.06** (0.03)
... ALMP effort					-0.06*** (0.01)		-0.04*** (0.02)
... Labor market dominance						-1.12*** (0.35)	-0.99** (0.46)
Constant	1.89*** (0.02)	1.89*** (0.02)	1.89*** (0.02)	1.88*** (0.02)	1.89*** (0.02)	1.89*** (0.02)	1.88*** (0.02)
Unit & period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality type interactions	No	No	No	No	No	No	Yes
Observations	6,692	6,692	6,692	6,538	6,692	6,692	6,538
Units	478	478	478	467	478	478	467
Adjusted R ²	0.257	0.257	0.257	0.255	0.257	0.257	0.254

Robust standard errors in parentheses, clustered at the entity level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

(Kolada 2018) divided by the number of working-age persons (aged 20–64) who are not working (Statistiska centralbyrån 2018c). An alternative way to assess this factor would be to assume that in regions where the local government is a more dominant employer, the sense of social responsibility among local government recruiters tends to be higher, which would lead to a higher degree of compliance with labor market regulation. To assess this mechanism, I construct an indicator on the local government's *labor market dominance*, which measures local government employment divided by total employment in the municipality in 2007. (Statistiska centralbyrån 2018b).

Table 5 reports the results of the heterogeneity analyses. Models 5:1–5:6 include one each of the six interaction terms discussed above, whereas the preferred model 5:7 adds all of them together. To further reduce the risk of omitted variable bias, the latter model furthermore adds a set of interactions between the repeal indicator and the dummy variables for nine different municipality *types*, using a classification developed by the Swedish Association of Local Authorities and Regions (SKL) based on a number of structural variables. These interaction terms are meant to control for the possibility that differences in local government compliance are caused by differences in the population

Figure 4. Marginal effects of repealing the LUPV on vacancy order rate conditional on a local government characteristic, following model 5:7, with all other variables held at their means among local governments (left axis). Dashed lines denote a 95% confidence interval. Bars display the observed distribution of the characteristics across local governments (right axis).



size, industry structure, or other structural characteristics across municipalities.²³

Although we cannot rule out the risk of omitted variable bias in these analyses, a number of findings are worth highlighting. First, the analyses show no evidence that compliance was driven by any of the enforcement-related external factors. None of the models reports a statistically significant negative coefficient for *caseworker intensity*, *employee unionization*, or *media influence*; for the former, coefficients even point in the opposite direction.²⁴ In contrast, there is some evidence to suggest that the factors related to social and normative motives within the local government organization mattered for compliance. Indeed, the three significant interaction effects in the preferred model 5:7 suggests that local governments with a more law-abiding culture and a stronger social commitment in the labor market realm were more compliant with the unenforced LUPV, when municipality type and the other discussed factors are controlled for.

The three plots in *Figure 4* present the result from specification 5:7 visually. Each of the plots report, for one of the three local government characteristics of interest, the average change in the vacancy order rate following the repeal of the LUPV, conditional

²³ In the applied classification from 2005 the nine types are large cities, medium-sized municipalities, suburban municipalities, commuter municipalities, manufacturing municipalities, rural municipalities, other municipalities > 25,000 inhabitants, other municipalities 12,500–25,000 inhabitants, and other municipalities < 12,500 inhabitants.

²⁴ With regards to *employee unionization*, also see the analyses in *Table A1*, which find no evidence that compliance is higher with respect to vacancy orders for *occupations* with higher degrees of unionization.

on that characteristic and with all other variables set at their local government means. These results indicate that it is only at a sufficiently high level of law-abiding culture, ALMP effort, and labor market dominance that the repeal of the LUPV had a statistically significant negative effect on the local government's vacancy order rate. What this suggests is that a certain presence of these three characteristics appears to have been necessary for a local government to be compliant with the unenforced regulation while it was in force.

6 Concluding discussion

The analyses in this paper provide largely consistent evidence that the repeal of the LUPV, taking effect in 2007q3, led to a considerable reduction in the propensity of employers to post vacancy orders at the PES. What this implies is that while the regulation was in force, it had an effect on employers' vacancy posting behavior despite not being actively enforced. The different relevant estimates suggest that between 8 and 18 percent of all vacancy orders came about because employers' chose to comply with the LUPV, in the sense that these orders would not have been posted in the absence of the law.

The applied research design helps to preclude the possibility that compliance was a result of alignment between employer preferences and regulation, because there was nothing after the repeal which prevented employers who desired to recruit from the PES from continuing to do so at the same rate (see Galle 2017). Considering that the LUPV was essentially unenforced, and assuming that this was known among employers, we may also rule out the possibility that compliance was caused by fear of formal sanctions. Instead, the more plausible interpretation is that organizational factors prompted some employers to comply with the law.

Although the study provides no definitive answer to the question of which such factors may have mattered, the heterogeneity analyses point to two mechanisms: First, in municipalities where top politicians and high-ranking civil servants reported less scope for corruption, local government employers were more prone to comply with the LUPV, presumably reflecting a more law-abiding organizational culture. Second, in municipalities that exert a stronger effort in the field of active labor market policy and

which have a stronger labor market dominance, compliance with the LUPV among local government employers was higher, possibly due to a stronger sense of commitment and responsibility in the labor market realm at large. However, as these results are based on rather crude measurements and cannot be given a causal interpretation, future research may do well to explore other possible drivers of compliance at the organizational level.

My findings also have implications for policy-making. Specifically, they indicate that governments who desire to change the behavior of legal entities may find it worthwhile to impose formal obligations on these actors, even if it lacks the capacity to back up these obligations with a credible threat of sanctions against violations. In this respect, my results are in line with those of Galle's (2017) analysis of tax compliance among non-profit organizations in the USA—to my knowledge the only study to date that rigorously evaluates an unenforced piece of regulation targeting organizations.²⁵ However, my results show that this is not only the case for the non-profit sector, but that the private sector more broadly, as well as the local government sector, might be susceptible to unenforced or unenforceable regulation.

However, a few remarks are in order about the extent to which these results may be generalized across policy fields and institutional context. First, in the case at hand the cost of compliance was fairly low, at least compared to much of the regulation in, for instance, the fields of environmental policy or labor rights.²⁶ For behavior that demands more time or money from the target group, soft governing tools such as the LUPV may not be as effective (Parker and Nielsen 2017). Second, comparative research has pointed to the existence of national *administrative styles*, whereby countries vary in, among other things, the degree of reliance on soft governing tools (e.g., Howlett 2003; Jordan et al. 2003). In this literature, the institutional features that characterize Sweden—such as a far-reaching delegation of power to administrative agencies, high levels of consensus and social trust, and weak legal traditions—are typically seen as creating more favorable conditions for

²⁵ Another study that identifies a positive compliance effect in a zero deterrence environment, focusing on individuals rather than legal entities, is Dwenger and colleagues' (2016) analysis of an unenforced local church tax in Germany.

²⁶ In 2006, a government agency estimated that posting a vacancy order costs an employer the equivalent of 10 minutes working time (Nutek 2006).

the use of soft governing tools (Blomqvist 2016). This implies that there are likely certain scope conditions with respect to the institutional contexts in which unenforced or unenforceable regulation is a viable option.

Nevertheless, within these scope conditions, my results suggest that it may be possible to concentrate monitoring and enforcement activities to the actors that are deemed less prone to comply (cf. the ‘enforcement pyramid’ outlined by Ayres and Braithwaite 1992). At a time when the interest in how to optimize regulatory strategies is growing across the OECD (OECD 2011), these results should come as good news to many regulators.

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Appendix

Table A1. Versions of Model 2:1, interacting the repeal of the LUPV with occupation-level characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Repeal of the LUPV	-3.870*** (1.078)	-3.348*** (1.217)	-3.317*** (1.210)	-2.914** (1.400)	-2.840** (1.207)	-2.418 (1.886)	-3.989 (3.428)
Repeal of the LUPV × ...							
... Highly qualified	0.367 (0.725)						1.727 (1.574)
... Routine task intensity		0.147 (0.311)					0.665 (0.468)
... Offshorability			-0.345 (0.324)				-0.451 (0.390)
... Automatability				-0.022 (1.097)			0.951 (2.056)
... Skill specificity					-0.550 (0.416)		-0.275 (0.666)
... Unionization rate						-1.765 (2.386)	-2.765 (3.441)
Constant	10.263*** (0.325)	9.639*** (0.324)	9.639*** (0.323)	10.181*** (0.338)	10.263*** (0.325)	10.263*** (0.323)	9.500*** (0.337)
Unit & period effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28,874	25,318	25,318	28,000	28,874	28,874	24,634
Adjusted R^2	0.460	0.435	0.435	0.466	0.460	0.460	0.442

Two-way clustered standard errors in parentheses. Non-nested clustering on labor market area and occupation, computed using the `-reghdfe` package for Stata (Correia 2017). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Sources for occupation-level characteristics:

- Highly qualified: ISCO-88, occupations that require post-secondary qualifications.
- Routine task intensity and Offshorability: Goos et al. (2014).
- Automatability: Frey and Osborne (2017), applied to Sweden by Fölster (2014).
- Skill specificity: Iversen and Soskice (2001).
- Unionization rate: Membership among SOM survey respondents in years 2006–2011 (Weibull et al. 2014).

Table A2. Descriptive statistics for the main datasets

Variable	Mean	Std. Dev.	Min.	Max.	N
<i>The main sector-area-occupation dataset (2006/07–2007/08)</i>					
Vacancy order rate (annually)	9.18	23.04	0	800	29,062
Placebo order rate (annually)	0.1	2.34	0	200	29,062
Average employment	263	1552	1	69,099	29,062
<i>The main legal entity dataset (2005q1–2008q2)^a</i>					
Vacancy order rate (quarterly)	1.85	1.79	0	28.57	6,692
Placebo order rate (quarterly)	0.01	0.12	0	6.9	6,692
Average employment	2,086	3,823	5.6	46,225	6,692
Caseworker intensity	0.04	0.03	0	0.37	4,060
Employee unionization	0.73	0.11	0.25	1	4,060
Media influence	4.71	0.70	3.11	6.60	4,060
Law-abiding culture	-2.16	0.93	-5	0	3,906
ALMP effort	2.98	1.74	-2.67	11.15	4,060
Labor market dominance	0.28	0.07	0.04	0.49	4,060

^a Central government entities lack data on the six bottom variables, denoted X_e . However, this is not a problem here, because whatever value these observations would have had, the interaction term $R_{st} \times X_e$ will be set to 0 for these entities, since for them the repeal indicator R_{st} is always 0 (see Fn. 19).

Generalized synthetic control analysis

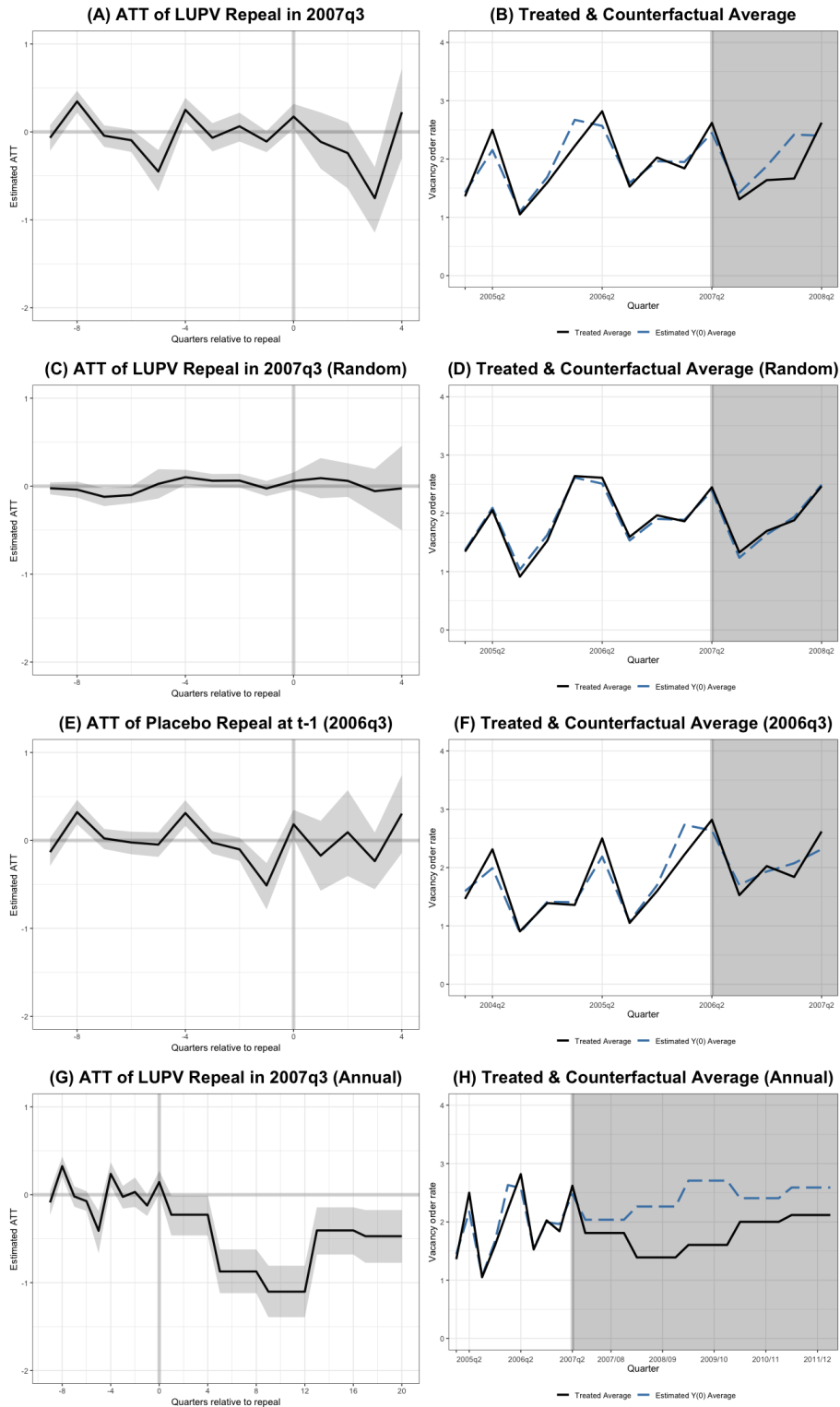
As an additional robustness check, I supplement the entity level DID analyses with a set of generalized synthetic control (GSC) analyses conducted on the same samples. The GSC method developed by Xu (2017) is in the spirit of the original synthetic control approach introduced by Abadie et al. (2010) in the sense that it predicts counter-factual outcomes for the treated observations in the post-treatment period, using a procedure that involves re-weighting of the control units based on the cross-sectional correlations between treated and control units in the pre-treatment period (Xu 2017, p. 58). More specifically, the procedure involves three steps.

In the first step, data from only the control units are used to estimate a linear interactive fixed effects (IFE) model of the outcome, that incorporates unit-specific intercepts (factor loadings) interacted with a number of unobserved time-varying coefficients (latent factors).²⁷ In the second step, factor loadings for each treated unit are estimated by minimizing the mean squared error of the predicted treated outcome in the ten pre-treatment quarters, using the factors and coefficients estimated in the first step. The third step makes out-of-sample predictions of the counter-factual outcomes for the treated observations in the four post-treatment quarters based on the parameters estimated in the previous steps. The individual treatment effect on each treated unit in the post-treatment period is then calculated as the difference between the actual outcome and the predicted counter-factual in each quarter, and the ATT is simply the average of these differences.

The results from four different GSC models are presented in eight panels in *Figure A1*. It turns out that in none of the four models the GSC algorithm finds any latent factors to include, which means that the effect estimates from the GSC analyses are identical to those obtained from the corresponding DID analyses, albeit with slightly larger standard errors. Still, the graphical output of the GSC analyses make them worthwhile examining.

²⁷ These factors are derived from an iterated factor analysis of the residuals from a linear model. The number of factors to include is determined automatically through a cross-validation procedure that runs models with varying number of factors, each of which repeatedly excludes a small portion of the data and then predicts the held-back information through an OLS regression estimated on the remaining data. The model that on average makes the most accurate predictions is then selected by the algorithm, thus avoiding overfitting (Xu 2017). To resemble the DID approach above, all such models also incorporate entity and quarter fixed effects. The algorithm used is an EM algorithm (Gobillon and Magnac 2016) that produces slightly more precise coefficients than the one originally applied by Xu (2017).

Figure A1. Results of generalized synthetic control analyses of central government and local government entities.



Consider first the panels in the top row, which correspond to the main DID analysis reported in Model 3:1 (*Table 3*). The panel on the left plots the estimated ATT of the repeal on the quarterly vacancy order rate of municipality employers, which is again estimated at -0.221 percentage points ($p = 0.034$). The shaded area denotes the 95 percent confidence interval, produced using a parametric bootstrap procedure based on resampling of the residuals (Xu 2017). As mentioned above, the ATT represents the difference between the actual outcome of the treated unit in the post-treatment period, $Y(1)$, and its estimated counter-factual, $Y(0)$. The averages of these two variables are plotted in the panel on the right, revealing a close fit in trends between the two sectors throughout the full pre-repeal period, although the seasonal patterns vary somewhat between sectors. Perhaps most noticeably, the local government sector tends to have a higher vacancy order rate in q2, including in the post-repeal 2008q2.

The panels in the second row report the corresponding results from a placebo GSC model in which treatment is randomly assigned to 290 out of the 478 entities. Reassuringly, the effect estimate from this model is essentially zero ($ATT = 0.017$, $p = 0.838$). The GSC analysis reported in the third row corresponds to the placebo analysis reported in Model 3:5 (*Table 3*), where the repeal is brought forward to 2006q2. Again, there is no sign of a placebo effect ($ATT = -0.003$, $p = 0.984$).

The bottom row, lastly, reports the results of the long-term analysis. For ease of the interpretation, each quarter is assigned the period average vacancy order rate, yet this does not affect the results ($ATT = -0.616$, $p = 0.000$) which are identical to those in Model 4:3 (*Table 4*). However, we learn from these plots that the two sectors exhibited different trajectories during the recession, and then converged again but from then on with a difference in levels that was not observed prior to the repeal of the LUPV.

All in all, the GSC analyses lend additional support to the conclusions drawn from the DID analyses reported in *Table 3* and *Table 4*.