



When the paper tiger bites: Evidence of compliance with unenforced regulation among employers in Sweden

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Abstract

Little evidence exists on whether and why organizations comply with regulations that are not monitored or enforced. To address that shortage, this study evaluates the 2007 repeal of an essentially unenforced regulation mandating private and local government employers in Sweden to post their vacancies publicly at the Public Employment Service. Exploiting the fact that central government employers were not affected, difference-in-differences analyses identify a substantial negative effect of the repeal on local government employers' vacancy posting propensity – with similar results for the private sector. At odds with deterrence models of regulatory compliance, these findings hint at an important role for organizational factors related to cultures and norms. Heterogeneity analyses indicate that local governments with more law-abiding organizational culture and stronger social responsibility commitment were more prone to comply with the unenforced regulation. The results thus simultaneously point to an untapped potential and to some possible preconditions for unenforced regulatory strategies.

Keywords: enforcement, expressive legal theory, labor market regulation, organizational culture, regulatory compliance.

1. Introduction

A growing number of pressing societal challenges – ranging from the prevention of global warming and pandemics 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 & Nielsen 2009). For instance, even in Sweden – a country known for its comparatively extensive reliance on soft governing tools (Blomqvist 2016) – monitoring and enforcement expenditure is estimated to nearly 1 percent of the general government consumption expenditure and has been rising over the past decades (Statskontoret 2012).

Hence, it may come as no surprise that 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 & Nielsen 2009; Schell-Busey *et al.* 2016). Although there is a general agreement in this literature 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 & 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 & Prakash 2011; Markell & 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 & Nielsen 2017). In this framework –

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associated with expressive legal theory (e.g., McAdams 2015) – 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. Such considerations, in turn, may be stimulated 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 an important gap in the literature by being one of the first to rigorously evaluate a particularly informative case of regulatory strategy, namely one for which the deterrence mechanism of organizations' compliance is ruled out because the regulation is essentially unenforced (and, arguably, unenforceable). Such regulation is fairly common not least in the labor market realm (see Harel *et al.* 2017), and gained topicality in 2020 as governments worldwide began imposing physical distancing requirements on individuals and organizations to curb the spread of COVID-19 – in many cases with no credible threat of enforcement (e.g., Witte 2020).

The regulation under investigation here 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 affected private and local government employers, but not central government employers, the repeal can be evaluated using difference-in-differences (DD) analyses under reasonable assumptions.

I first evaluate its effect on the local government sector, which – it is worth emphasizing – in Sweden enjoys high functional and political autonomy in international comparison (Kuhlmann & Wollmann 2014). The analyses identify a substantial and significant negative effect of repealing the regulation on local government employers' propensity 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 governments. The 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.

Second, by resorting to a slightly less favorable research design I can also evaluate the effect of the repeal on private sector employers. The results indicate that the regulation had a behavioral impact here too, but the effect size depends on what assumptions are made about the counter-factual trends in vacancy posting. Lastly, supplementary analyses of the Swedish labor market as a whole identify a substantial and significant effect regardless of specification.

Taken together, these findings hint at an important role of internal organizational factors in achieving regulatory compliance. Furthermore, as elaborated in the concluding section, they point to an untapped potential for governments to impose legal obligations on organizations whose behavior they seek to affect, even if they lack the capacity to enforce these obligations. This is at least so in contexts where the cost of compliance is not too high and where the organizations in question generally can be expected to be responsive to soft governing tools (cf., Ayres & Braithwaite 1992).

2. The Law on Universal Posting of Vacancies

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 PES to give the PES a chance to

refer job seekers to these positions (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. 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 nonnatives (Regeringen 1975; SOU 2006).

Failure to comply with the law could result in a fine of up to 500 SEK (60 USD) (Regeringen 1976). 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 in the early 1980s (SOU 2006, p. 315). In an illuminating statement from 1998, the PES, which was responsible 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 its contents, but also in the sense that it is a virtually unenforced piece of regulation. The instruction contains no rules on sanctions against noncompliant 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 to reduce businesses' administrative obligations. The government considered the option to also repeal the IPSV but decided to leave it in force (it remains virtually unchanged to date). In effect, employers in the private and local government sectors were freed from their vacancy posting obligation as of 2007q3, 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 nontreated employers before and after July 2, 2007. Such a research design is not threatened by time-invariant differences between "treated" and "nontreated" employers, nor by changes in contextual factors between the pre- and post-repeal periods as long as those changes affected both employer groups similarly. However, an obvious threat 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 effect estimate. Four 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, inspections of relevant documents do not suggest that this is the case. First, 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 was in force between 2007 and 2010 and 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).

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	Noncompliance may result in a fine of 500 SEK (changed in 1991 to an unspecified amount)	None. The Parliamentary Ombudsmen (JO) and the Chancellor of Justice (JK) may criticize noncompliance
De facto sanctioning	Two instances in the 1980s	One instance in 2017 (by the JO)

Second, it is possible 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, no such reaction seems to have occurred. First, the PES does not appear to have considered the LUPV to be of much importance to begin with.¹ Second, the instruction that governs the PES does mention vacancy acquisition 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 are no indications that employers in any particular sector were given priority. A 2010 report by the Swedish National Audit Office (*Riksrevisionen*) investigating the PES's reach-out activities made no mention of such prioritizations, and data from a representative employer survey carried out as part of that investigation – re-analyzed here by the author – revealed no significant differences across sectors in the likelihood of having been in contact with the PES during the past 18 months (*Riksrevisionen* 2010).

A third possible risk would be that more of those jobseekers looking specifically for non-central-government jobs stopped checking the PES's database after the repeal, which could disincentive non-central-government employers more from posting. Whereas no data exist to evaluate this claim, it is at least clear that PES remained a significant labor market intermediary. Data from 2013 show that the PES's online job board continued to be by far the most visited one, and a similar share of unemployed found their new jobs through that channel then as before the repeal – around 10 percent (Cronert 2015).

Fourth, there is the risk that labor market changes 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 left-hand panel 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.² These patterns should be reason enough to delimit all effect evaluations 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 is impossible 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 above, there are diverging positions in 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 lack of monitoring and enforcement, the LUPV would be ineffectual and we should expect to see little difference in employer behavior before and after the repeal (Block *et al.* 1981; Markell & Glicksman 2014). According to other models, fear of sanctions is not among the primary drivers of corporate compliance; instead it is likely that due to some social or normative motives within the organization, employers may have chosen to comply with the regulation despite the lack of enforcement (Kagan *et al.* 2011; Galle 2017).

To be clear, in many cases posting vacancies at the PES is fully in line with the employers’ underlying preferences – for instance a desire to extend their search for new recruits outside their own network. In other cases, however, it is not. Posting involves an administrative cost – albeit fairly small³ – which was the government’s key motivation for the repeal and also why it was welcomed by employers’ associations (Regeringen 2007). Furthermore, according to surveys, employers oftentimes prefer to not post their vacancies at the PES because they do

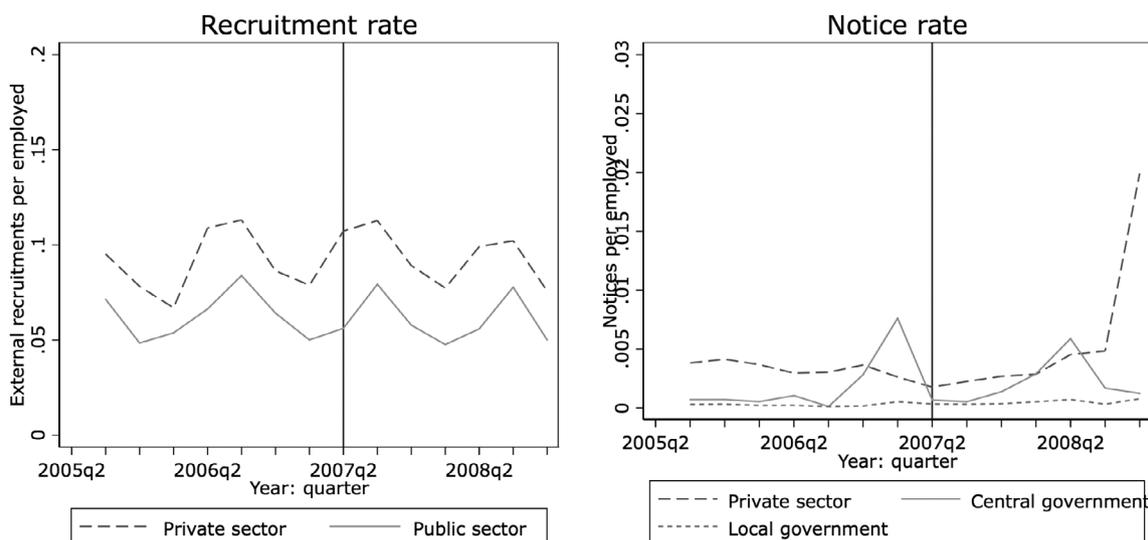


Figure 1 External recruitments and notices of dismissal, 2005q3–2008q4. *Note:* Horizontal lines indicate the repeal of the LUPV. *Sources:* Statistiska centralbyrån (2018d) and Arbetsförmedlingen (2018b).

not expect it to yield suitable applicants and because they want to avoid the hassle of too many applications (Riksrevisionen 2010).

Against this background, a note is warranted about what it means to comply in the case at hand. I define compliance as behavior that is obedient to a regulatory obligation, *conditional on the existence of that obligation*. This, in my view, is a more useful definition for effect evaluation purposes than a more general definition of compliance as (all) behavior that is obedient to a regulatory obligation (cf. Parker & Nielsen 2017, p. 218). As for the LUPV, this means that an employer is compliant to the extent that they post vacancies at the PES *that they would not have posted in the absence of the law*. This point is important to keep in mind, as it distinguishes cases of compliance from cases where an employer would have posted a vacancy at the PES regardless of the law's existence because doing so would be in line with their underlying needs or preferences. Because nothing prevents such cases from taking place on either side of the repeal, what I do here is to evaluate 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 uses a registry 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, the occupation and the required level of qualification, the expected job duration, and the number of available positions. It also includes some 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. However, a variable containing the name of the recruiting entity makes it possible to reliably 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 2004q1 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. In addition, including earlier data comes at the cost of a more uncertain sector classification, due to more observations with lacking registration numbers (see below).

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.⁴ 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 represented around 1.8 percent of total employment. The motivation is that said industry is largely comprised by the PES itself, and by staffing and recruitment agencies that are known to post vacancies at the PES partly to attract staff for potential future assignments rather than to fill existing vacancies (Cronert 2015). This step excludes an additional 12.3 percent of the orders. Third, I exclude categories of vacancy orders that, in principle, should not be affected by the law in the first place, such as orders for positions with a duration of no more than 10 days and orders for positions outside of Sweden. This operation excludes another 10.2 percent of the orders and leaves us with a baseline sample of approximately 894,000 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. Dividing by employment is meant to make the units of analysis more comparable and to account for variations in posting needs due to variations in workforce size.

A perhaps more accurate measure for our purposes 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 (2018c), which finds no notable difference in the average changes in recruitment rates between 2007 and 2008 across the two sectors. Another, albeit imperfect way to assess the importance of this factor is to check whether the results change when controlling for year-to-year change in net employment (i.e., combining hires and separations). I report such robustness checks below.

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. For any registered legal entity, the entity's sector identity can be inferred from the initial digits of its registration number, which is mostly available in the PES's vacancy database. Therefore, the PES was asked to create a new variable indicating the sector identity of the employer, before anonymizing the data. 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).⁵

5. Empirical strategies and results

The great level of detail in the dataset allows for estimating the effect of repealing the LUPV on employers' vacancy posting behavior in multiple ways. For reasons detailed below, I estimate the effects on local government employers and private sectors employers separately, using different approaches. Key to both approaches, though, is to compare the vacancy posting behavior of employers in the private or local government sector, who were "treated" by the repeal, with employers in the central government sector, who were not, before and after the repeal. Specifically, for the local government sector I conduct a set of DD analyses where the units of analysis are a set of individual central and local government entities. For the private sector, I run another set of DD analyses in which data are instead organized by sector, region, and occupation. Descriptive statistics for the two datasets are provided in Table 2 and additional details are reported in Appendix S1.

5.1. Local government employers

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

For a couple of reasons, this approach only includes 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 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 observing entity-level employment and 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 create a balanced panel that includes only entities that were in operation over the full period. I furthermore exclude three small entities which due to no more

Table 2 Descriptive statistics

Variable	Mean	Standard deviation	Min.	Max.	<i>N</i>
The main legal entity dataset (2005q1-2008q2) [†]					
Vacancy order rate (quarterly)	1.80	1.79	0	28.57	6,874
Employment (quarterly)	2,071	3,894	3	51,089	6,874
Caseworker intensity	0.04	0.03	0	0.37	4,060
Employee unionization	72.73	11.3	25	100	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
The main sector-area-occupation dataset (2006/2007–2007/2008)					
Vacancy order rate (annually)	8.33	22.04	0	800	19,706
Employment (annually)	286	1,592	1	67,403	19,706

[†]Central government entities lack data on the five last 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.

than one or a few posted vacancies exhibit a vacancy order rate of more than 33 percent in occasional quarters.⁶ Having done so, I arrive at a main sample of 201 central government entities and (all of the) 290 municipalities.

The average treatment effect of repealing the LUPV is estimated by means of a DD-estimator with the following specification:

$$V_{et} = \gamma_e + \lambda_t + \beta R_{st} + \varepsilon_{et} \quad (1)$$

Here, V_{et} is the vacancy order rate⁷ for entity e in quarter t , where $t \in \{2005q1 \dots 2008q2\}$, γ_e is an entity-specific effect, λ_t 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. β represents the estimated average treatment effect on the treated of repealing the LUPV; the ATT, for short. Standard errors are clustered by entity.

The identifying assumptions in this model include the assumption that, in the absence of the repeal, the average vacancy order rate among central government employers and local government employers would have followed parallel trends – that is, they would have the same time dynamics. 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 and local government employers 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 assessment may be done by means of visual inspection as well as a statistical placebo test.

Beginning with a visual inspection, Figure 2 reports, for the central and local government sector respectively, the average quarterly vacancy order rate among all observed entities between 2005q1 and 2008q2. 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. The plots indicate that the two groups of employers exhibit fairly similar trends in the pre-repeal periods, whether weighted or not.

Table 3 reports the main results of the analyses. Beginning with Model 3:1 (DD), the repeal of the LUPV is estimated to have caused, on average, a 0.21 percentage points reduction in the quarterly vacancy order rate of municipality employers, corresponding to 11.7 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 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 reassuring, as the effect estimate is slightly larger than in the baseline model.

In Model 3:3, entities are instead weighted according to their average employment, in which case the estimated effects can be interpreted as the population average partial effects for the local government sector as a

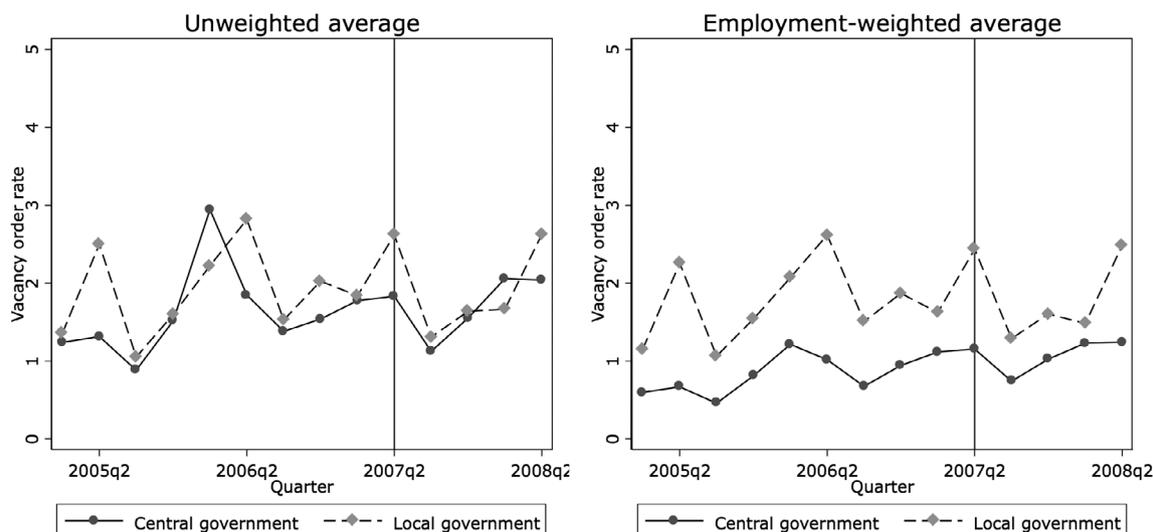


Figure 2 Average vacancy order rate across 201 central government agencies and 290 local governments, by sector, 2004q3–2008q2. Sources: Arbetsförmedlingen (2018a) and Statistiska centralbyrån (2018a).

Table 3 Results for the local government sector[†]

	DD: Main	DD: Robust	DD: Weighted	DD: Time	Placebo repeal (DD _{t-1})	
	model (3:1)	regression (3:2)	model (3:3)	trends (3:4)	Unweighted (3:5)	Weighted (3:6)
Repeal of the LUPV	-0.211** (0.090)	-0.217*** (0.038)	-0.294*** (0.075)	-0.240* (0.133)		
Placebo repeal (at t - 1)					-0.011 (0.090)	-0.047 (0.047)
Constant	1.842*** (0.016)	-0.433** (0.187)	1.670*** (0.017)	19.367*** (0.856)	1.710*** (0.015)	1.521*** (0.011)
Unit & period effects	Yes	Yes	Yes	Yes	Yes	Yes
Linear time trends	No	No	No	Yes	No	No
Observations	6,874	6,874	6,874	6,874	7,098	7,098
Units	491	491	491	491	507	507
Sample average (\bar{V})	1.80	1.49	1.60	1.80	1.76	1.51
β/\bar{V}	-11.7%	-14.5%	-18.3%	-13.3%	-0.1%	-3.1%
Overall Adj. R ²	0.302	0.699	0.633	0.297	0.327	0.639

* $P < 0.10$; ** $P < 0.05$; *** $P < 0.01$. [†]Dependent variable: Vacancy order rate V . Robust standard errors in parentheses, clustered at the entity level (except for Model 3:2, for which standard errors are calculated using the pseudo values approach).

whole (Solon *et al.* 2015, p. 312). This model – which from a policy perspective is the perhaps most informative – suggests that the population average partial effect for the sector as a whole is higher, at around 18 percent of the average vacancy order rate. Model 3:4, next, checks whether the results from the main model are robust to the inclusion of entity-specific linear time trends. The result is encouraging; although the large number of additional parameters inflates the standard errors slightly, the effect estimate is larger than the baseline. Models 3:5 and 3:6 lastly, report a placebo test (DD_{t-1}) performed by re-estimating Equation 1 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.⁸ Neither the unweighted or the weighted model show any sign of pre-reform trends.

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 of the central and local government sectors – which were less affected by the economic downturn – extending the period of evaluation into, and beyond, the years of recession. I retain the set of entities analyzed in Table 3, except those central government agencies that were closed down during the period of investigation.

In Table 4, Models 4:1 and 4:2 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 -32.7 and -29.1 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 -24.8 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 DD 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 Appendix S1 (Fig. S4). 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 DD analyses, albeit with slightly larger standard errors. This finding is worth highlighting as it buttresses the parallel trends assumption that underpins the DD analyses. Another robustness check reported in Appendix S1 shows that the results are strengthened when changes in net employment are accounted for in the model (Table S1).

5.2. What factors drive compliance with unenforced regulation?

So, it appears that despite the lack of enforcement, many local government 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 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.

Table 4 Results for the local government sector: long-term effects[†]

	Post-repeal = 2007q3–2012q2		Post-repeal = 2011q3–2012q2	
	DD: Unweighted (4:1)	DD: Weighted (4:2)	DD: Unweighted (4:3)	DD: Weighted (4:4)
Repeal of the LUPV	-0.610^{***} (0.082)	-0.462^{***} (0.076)	-0.476^{***} (0.128)	-0.342^{***} (0.104)
Constant	2.125^{***} (0.035)	1.838^{***} (0.041)	2.009^{***} (0.024)	1.771^{***} (0.024)
Unit and period effects	Yes	Yes	Yes	Yes
Observations	13,980	13,980	6,524	6,524
Units	466	466	466	466
Sample average (\bar{V})	1.86	1.59	1.92	1.69
β/\bar{V}	-32.7%	-29.1%	-24.8%	-20.2%
Overall Adj. R^2	0.322	0.616	0.346	0.658

* $P < 0.10$; ** $P < 0.05$; *** $P < 0.01$. [†]Dependent variable: Vacancy order rate V . Robust standard errors in parentheses, clustered at the entity level.

Such analysis is performed by means of interacting the repeal variable R_{st} in Equation (1) 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

$$+ \beta_{X_e}(R_{st} \times X_e) \quad (2)$$

are appended to the right-hand side of the equation. 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.

Five variables are included in this manner, the first two of which concern external factors. First, as mentioned above, the PES – operated by the central government – 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 *caseworker intensity*, based on the number of caseworkers employed by the local branches of the PES in the municipality. Resource allocation priorities are set by the PES centrally to meet its overall objectives, which includes providing placement services to jobseekers and recruitment services to employers. To account for the fact that the number of caseworkers in a given area is thus driven partly by recruitment needs (proxied by employment) and partly by jobseekers' needs (Riksrevisionen 2010), I divide the number of caseworkers by the number of employed in the municipality and then by the local unemployment rate.⁹

A second 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., Campa 2018; El Ghouli *et al.* 2019). Similarly, with respect to Swedish local governments, Svaleryd and Vlachos (2009) have found that high local media coverage tends 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 government affairs, as gauged by local politicians in a large survey carried out in 2008 (Gilljam *et al.* 2010).¹⁰

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.¹¹

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 (Kolada 2018) divided by the number of working-age persons (aged 20–64) who are not working (Statistiska centralbyrån 2018b).

Third, 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 were able to influence employers so as to post their vacancies at the PES and could do so more effectively while the LUPV was in place. If unions served this motivating function, 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 (0–100 percent) 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).

Table 5 reports the results of the heterogeneity analyses. Models 5:1–5:5 include one each of the six interaction terms discussed earlier, whereas the preferred model 5:6 adds all of them together. To further reduce the risk of omitted variable bias, each model furthermore adds a set of interactions between the repeal indicator and the dummy variables for nine different structural municipality categories. These interaction terms are meant to control for the possibility that variations in local government compliance are confounded by underlying structural factors. The categorization used here is developed by the Swedish Association of Local Authorities and Regions (SKR) based primarily on population size and density, industry structure, and commuting patterns (see Appendix S1 for details).

Although we cannot fully 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 either of the two

Table 5 Results for the local government sector: heterogeneous effects analyses[†]

	DD (5:1)	DD (5:2)	DD (5:3)	DD (5:4)	DD (5:5)	DD (5:6)
Repeal of the LUPV	−0.071 (0.086)	−0.071 (0.086)	−0.071 (0.086)	−0.071 (0.086)	−0.071 (0.086)	−0.071 (0.086)
Repeal of the LUPV × ...						
Caseworker intensity	1.052 (0.766)					1.011 (0.770)
Media influence		−0.042 (0.044)				−0.051 (0.043)
Law-abiding culture			−0.047 (0.029)			−0.060** (0.030)
ALMP effort				−0.042*** (0.016)		−0.046*** (0.016)
Employee unionization					−0.004 (0.003)	−0.003 (0.003)
Constant	1.842*** (0.016)	1.842*** (0.016)	1.834*** (0.016)	1.842*** (0.016)	1.842*** (0.016)	1.834*** (0.016)
Unit and period effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality category interactions	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,874	6,874	6,720	6,874	6,874	6,720
Units	491	491	480	491	491	480
Overall Adj. R^2	0.302	0.302	0.300	0.302	0.302	0.300

* $P < 0.10$; ** $P < 0.05$; *** $P < 0.01$. [†]Dependent variable: Vacancy order rate V . Robust standard errors in parentheses, clustered at the entity level.

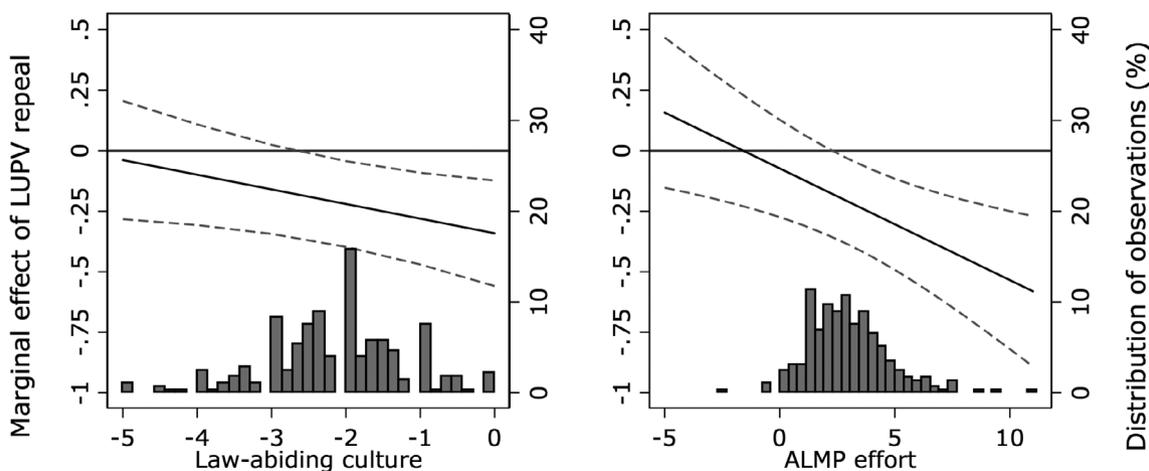


Figure 3 Marginal effects plot. *Note:* Marginal effects of repealing the LUPV on vacancy order rate conditional on law-abiding culture and ALMP effort respectively, following model 5:6, with all other variables held at their means among local governments (left axis). Dashed lines denote 95 percent confidence intervals. Bars display the distribution of the characteristics across local governments (right axis).

enforcement-related external factors. None of the models reports a statistically significant negative coefficient for *caseworker intensity* or *media influence*; for the former, coefficients even point in the opposite direction. 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 results from the full model 5:6 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. For employee unionization, the interaction coefficient is expectedly negative, but insignificant.

The two plots in Figure 3 present the result from specification 5:6 visually. The plots report, for law-abiding culture and ALMP effort respectively, the average change in the vacancy order rate following the repeal of the LUPV, conditional 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 and ALMP effort 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 characteristics appears to have been necessary for a local government to be compliant with the unenforced regulation while it was in force.

5.3. Private sector employers

Let us turn next to analyze the effect of repealing the LUPV on private sector employers, for which entity-level data on employment are unfortunately lacking. Here, I therefore instead apply a unit of analysis which is referred to as the “regional-occupational labor market” and is defined as a unique combination of the two relevant sectors (central government and private sector), 113 occupations, and 78 regional labor market areas, that jointly cover the entire Swedish labor market. To give a few examples, private sector architects and engineers in the Malmö-Lund region is a sector-area-occupation combination close to the mean size with approximately 260 employed, while central government business professionals in the Stockholm-Solna region is close to the 99th percentile with approximately 3,500 employed. Each unit is measured in four periods: the post-repeal period 2007/2008, and the three pre-repeal periods 2006/2007, 2005/2006, and 2004/2005.¹² Out of the 17,628 possible combinations of sector, area, and occupation, 9,853 (55.9 percent) have at least one employee in each of the four periods. Those that do not are considered nonexisting labor markets and are excluded from the sample.

For private and central government employers, respectively, Figure 4 reports 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 the three pre-repeal periods prior. Although both sectors saw upward pre-repeal trends, the plots raise some concerns about the parallel trends assumption – especially for the immediate pre-repeal period

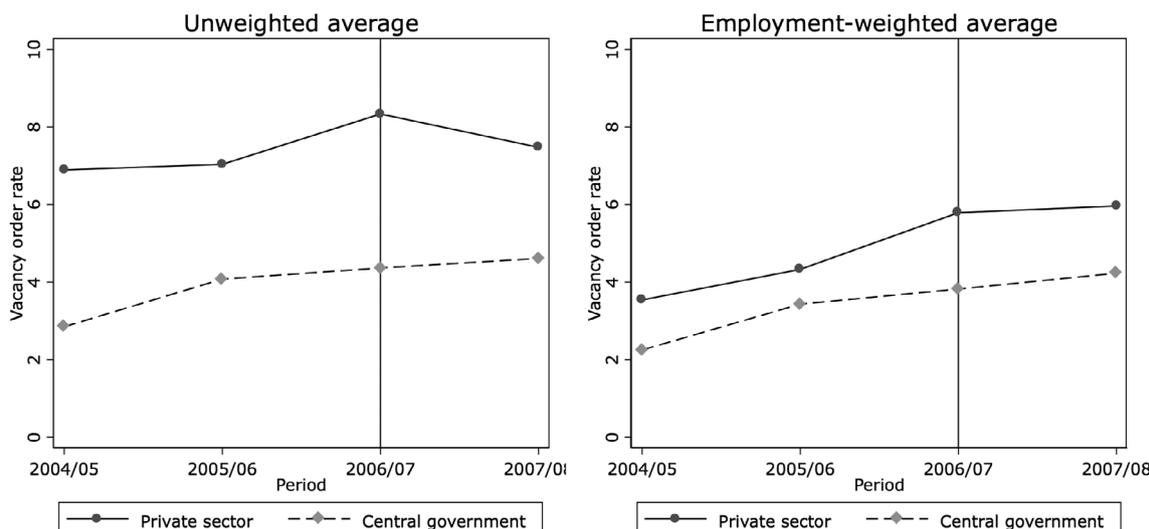


Figure 4 Average vacancy order rate across 9,853 sector-area-occupations, by sector, 2004/2005–2007/2008. Sources: Arbetsförmedlingen (2018a) and Statistiska centralbyrån (2018a).

2006/2007 when employers in the private sector saw a larger increase than central government employers operating in the same regional-occupational labor market. We will return to these concerns shortly.

In this application, the baseline estimator is a two-period DD estimator recommended by Bertrand *et al.* (2004), specified as:

$$V_{saot} = \gamma_{sao} + \lambda_t + \beta R_{st} + \varepsilon_{saot} \tag{3}$$

where V_{saot} represents the vacancy order rate in the sector-area-occupation sao in period t where $t \in \{2006/2007, 2007/2008\}$, γ_{sao} is a sao -specific effect, λ_t is a period-specific effect and ε_{saot} is an error term. R_{st} is a dummy variable that scores 1 for observations in the private sector 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 address the possibility that error terms are correlated within labor market areas and within occupations.

Table 6 presents the main results. The first set of models report the results from specifications where each sector-area-occupation observation is given equal weight regardless of size. In Model 6:1 (DD), the average effect of repealing the LUPV on the vacancy order rate in a treated regional-occupational labor market is estimated at -1.77 percentage points, a reduction corresponding to a substantial 21.3 percent of the mean vacancy order rate in the sample (\bar{V}). Model 6:2 (DD_{t-1}) performs a placebo test by dropping the 2007/2008 period, assigning the 2006/2007 period as the placebo treatment period, and then re-estimating the model in Equation (3) on the two pre-repeal periods, $t \in \{2005/2006, 2006/2007\}$. Reflecting Figure 4, the model reports a moderately sized *positive*, albeit nonsignificant, placebo coefficient at 0.55 percentage points, indicating that in an average regional-occupational labor market, the vacancy order rate increased by 7.2 percent more among private employers than among central government employers in the immediate pre-repeal period.

As the pre-repeal trends are diverging rather than parallel, the effect of the baseline DD model is likely to be biased downward to the extent that this divergence would have continued had the repeal not happened. One way to account for such bias, when using a two-period DD estimator like this one, is to apply a so-called triple-differences (DDD) estimator (e.g., Lindgren & Vernby 2016). This model (Model 6:3) estimates the effect of the repeal on the *rate of divergence* of the vacancy order rates in the two sectors (ΔV_{saot}). In other words, it represents the repeal effect under the assumption that in the absence of the repeal, the private sector would again have seen a 0.55 percentage points' larger increase than the central government sector in the 2007/2008 period. Usefully, for the two-period DD estimator used here, the DDD effect simply equals the difference between the baseline effect (DD) and the placebo effect (DD_{t-1}), and it is thus larger than the baseline (-27.9 percent of \bar{V}).

Table 6 Results for the private sector[†]

	Unweighted regression			Employment-weighted regression		
	DD (6:1)	DD _{t-1} (6:2)	DDD (6:3)	DD (6:4)	DD _{t-1} (6:5)	DDD (6:6)
Repeal of the LUPV	-1.772** (0.786)		-2.326** (1.103)	-0.236 (0.159)		-1.309*** (0.235)
Placebo repeal (at $t - 1$)		0.554 (0.554)			1.073*** (0.195)	
Constant	8.972*** (0.283)	7.540*** (0.200)	1.432*** (0.397)	5.843*** (0.074)	4.455*** (0.090)	1.388*** (0.109)
Unit and period effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	19,706	19,706	19,706	19,706	19,706	19,706
Units	9,853	9,853	9,853	9,853	9,853	9,853
Sample average (\bar{V})	8.33	7.74	8.33	5.73	4.95	5.73
β/\bar{V}	-21.3%	+7.2%	-27.9%	-4.1%	+21.7%	-22.8%
Overall Adj. R^2	0.469	0.513	-0.454	0.846	0.807	-0.188

* $P < 0.10$; ** $P < 0.05$; *** $P < 0.01$. [†]Dependent variables: Models 6:1, 6:2, 6:4, 6:5: Vacancy order rate V . Models 6:3 and 6:6: ΔV . Two-way clustered standard errors in parentheses. Nonnested clustering on labor market area and occupation, computed using the `-reghdfe-` package for Stata (Correia 2017).

The latter three models in Table 6 replicate the first three but weigh each sector-area-occupation by its average number of employed so that the estimated effects can be interpreted as the population average partial effects for the Swedish private sector as a whole (Solon *et al.* 2015, p. 312). The problem of diverging pre-repeal trends is now substantially larger, as evidenced by the much bigger difference between the DD estimate (-4.1 percent of \bar{V}), which no longer reaches statistical significance, and the DDD estimate (-22.8 percent of \bar{V}).

Four sets of robustness checks reported in Appendix S1 (Tables S2–S5) show that very similar results to those in Table 6 emerge when (i) controlling for changes in net employment, (ii) including a second pre-repeal period and *sao*-specific linear time trends, (iii) omitting vacancies from the last quarter in each period (q2), and (iv) using quarterly observations.

In sum, the results for the private sector point in the same direction as those for the local government sector but the effect size depends on what assumptions are made about the counter-factual post-repeal scenario. Under the questionable parallel trends assumption, the effect of repealing the LUPV on employers' behavior is smaller and only significant in the unweighted model. If assuming instead a sustained rate of divergence, the effect is consistent and considerably larger than for the local government sector. While impossible to say with certainty, the most plausible assumption probably lies somewhere between the two.

Lastly, supplementary analyses (see Table S6) that extend the private sector analyses also to the local government sector, confirm that for the Swedish labor market as a whole, the repeal did have a substantial and significant effect regardless of specification (-7.5 percent and -20.2 percent of \bar{V} in the employment-weighted DD and DDD analyses, respectively).

6. Concluding discussion

The analyses reported here provide largely consistent evidence that the repeal of the LUPV, taking effect in 2007q3, led to a considerable reduction in the propensity of regulated employers to post vacancy orders at the PES. What this implies is that while the regulation was in force, it had an effect on at least some employers' vacancy posting behavior despite not being actively enforced. For the local government sector, the weighted DD estimate suggest that approximately 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. For the private sector, the estimates point in the same direction but are sensitive to the underlying assumptions, while

for the Swedish labor market as a whole a substantial and significant effect is observed regardless of specification (ranging between -7.5 and -20.2 percent of all vacancy orders).

The applied research design precludes 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. Considering that the LUPV was essentially unenforced, and assuming that this was known among employers that knew about the law's existence, 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, 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, including unionization.

My findings also have implications for policy-making. Specifically, they indicate that governments who seek to change the behavior of legal entities may find it worthwhile to impose formal obligations on these actors, even if they lack 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 nonprofit organizations in the USA – to my knowledge the only other study to date that rigorously evaluates an unenforced piece of regulation targeting organizations. However, my results show that this is not only the case for the nonprofit sector, but that a highly autonomous local government sector, and seemingly also the private sector, can be susceptible to unenforced or unenforceable regulation.

Lastly, a few remarks are in order about the extent to which these results may be generalized across policy fields and institutional contexts. 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. For behavior that demands more time or resources from the target group, soft governing tools such as the LUPV may not be as effective (Parker & 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). 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 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 those actors that are deemed less prone to comply (cf. the “enforcement pyramid” outlined by Ayres & Braithwaite 1992; but also see Shimshack & Ward 2018 on the potential drawbacks of that approach). At a time when the interest in how to optimize regulatory strategies is growing across the Organisation for Economic Co-operation and Development countries, these results should come as good news to many regulators.

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Endnotes

- ¹ 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).
- ² The spike in the central government 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.
- ³ In 2006, a government agency estimated that posting a vacancy order costs an employer the equivalent of 10 minutes working time (Nutek 2006).
- ⁴ The excluded occupations are technical and commercial sales representatives, demonstrators and telephone salespersons, and street vendors.
- ⁵ See Appendix S1 for details.
- ⁶ 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.
- ⁷ *Vet* is the number of vacancy orders posted in a quarter, divided by the number of employees in that quarter, multiplied by 100.
- ⁸ For the sake of symmetry, the sample used here is extended back to 2004q1.
- ⁹ The results for this variable do not change even when omitting the unemployment rate adjustment.
- ¹⁰ An alternative indicator based on the coverage of local newspapers (Svaleryd & Vlachos 2009), do not change the results for this factor.
- ¹¹ Data is lacking for 11 municipalities.
- ¹² Each period consists of four quarters: q3 and q4 of the first year and q1 and q2 of the second year.

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Supporting information

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Appendix S1: Supporting Information