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Employment Protection and Sickness Absence

Martin Olsson

Research Institute of Industrial Economics
P.O. Box 55665
SE-102 15 Stockholm, Sweden
info@ifn.se
www.ifn.se

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Martin Olsson[*†]

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Abstract

An exemption in the Swedish Employment Security Act (LAS) in 2001 made it possible for employers with a maximum of ten employees to exempt two workers from the seniority rule at times of redundancies. Using this within-country enforcement variation, the relationship between employment protection and sickness absence among employees is examined. The average treatment effect from the exemption is found to decrease sickness absence by more than 13 percent at those establishments that were treated relative to those that were not and this was due to a behavioral, rather than a compositional, effect. The results suggest that the exemption had the largest impact on shorter spells and among establishments with a relatively low share of females or temporary contracts.

Keywords: Employment Protection; Sickness Absence; Economic Incentives
JEL Classification: J88; J63: I19

1 Introduction

Employment protection may affect the economy at the macro and the micro level. At the macro level, there may be an impact on the flow into and out

[*]Research Institute of Industrial Economics, Box 55665, SE-102 15 Stockholm, Sweden. E-mail: martin.olsson@ifn.se

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of employment as well as on the total employment level.[1] At the micro level the behavoir and performance of the firm may be affected.[2] Another potential effect of employment protection at the micro level is that it can entail behavioral effort-responses from employees and thereby affect labor productivity. Some studies show that effort seems to vary with the strictness of the employment protection; Engellandt and Riphahn (2005) find that, in Switzerland, workers on temporary contracts had a 60 percent higher probability of working unpaid hours and Ichino and Riphahn (2005) show that absence due to sickness on average increased among employees at an Italian bank once the probation time had ended.

Absence due to sickness can be seen as a measure of effort (labor productivity) and examining the impact of lower employment protection on sickness absence, we can empirically investigate the indirect relationship between employment protection and labor productivity. The nature of the linkage between sickness absence and labor productivity is somewhat ambiguous – a reduction in the sickness absence rate does not, by necessity, correspond to higher labor productivity. Attending work sick (presenteeism) should, in the short run, increase labor productivity, but could in the long run have a negative effect due to contagion of co-workers and aggravated and prolonged sickness status for the individual worker.

A natural experiment is used to identify the casual relationship between employment protection and sickness absence. The natural experiment occurred in January 2001, when an exemption in the seniority rule in the Swedish Employment Security Act (LAS) was implemented.[3] The exemption made it possible for employers with a maximum of ten employees to exempt two workers from

[1] See Acemoglu and Angrist (2001), Boeri and Jimeno (2005) and Kugler and Pica (2007) for some good examples.

[2] Autor et al. (2007) find that higher dismissal costs offset a deepening of capital and skill in firms which has a negative impact on total factor productivity but a positive impact on labor productivity.

[3] The seniority rule can be described as "first-in-last-out", i.e. employment protection varies with seniority.

the seniority rule at times of redundancies. Using establishments with 12 to 50 employees (a fraction of the population that was not exposed to treatment) as a control group, a difference-in-differences estimator (DiD) is applied to quantify the effect from the exemption. We find that the exemption in LAS decreased the average sickness absence rate by about 13 percent in establishments with a maximum of 10 employees relative to establishments with 12 to 50 employees. An alteration of the employment protection can, as pointed out by Lindbeck et al. (2006), affect the sickness rate at an establishment in several different ways. First, it may have an impact through a behavioral effect, because weaker employment protection results in an increasing risk of redundancy, especially for workers with high sickness absence. A possible scenario would be that due to higher costs of absence, the employed worker may not report sick in fear of being laid off. Second, the sickness rate can be affected through a compositional effect. This would be the case if less rigorous employment protection leads to more redundancies of workers with high sickness propensity, thereby decreasing average sickness at the establishment level. Less employment protection also makes the matching process on the labor market easier and thereby creates a compositional effect. This will happen if the employer becomes less rigorous in the hiring decision and in doing so hires workers with a high tendency towards sickness, thereby increasing the average sickness rate at the establishment. With less employment protection it also becomes easier to switch jobs for the worker. If workers with high sickness rates choose to switch jobs because of a bad match on the labor market to a greater extent after the softening of the employment legalization, the average sickness rate will fall. By excluding all enterprises with inflows or outflows of workers after the implementation of the exemption in LAS, it is shown that a negative behavioral effect dominated a positive compositional effect, suggesting that higher sickness absence costs on average made the workers change their sickness behavior. Furthermore, the effect is found to be largest among establishments with a relatively low share of females or temporary contracts.

This paper is organized as follows: Earlier research is introduced in section two. Data and results are described in section three. Finally, conclusions are presented in section four.

2 Earlier research on employment protection and absenteeism

In Ichino and Riphahn (2005), sickness behavior before and after probation among employees in an Italian bank is examined. By examining the same worker during and after probation, i.e. with different levels of employment protection, Ichino and Riphahn show that the days of absence due to sickness increased on average, especially for men, once stronger employment protection was granted after probation. This indicates that employment protection can affect the effort from the individual worker. Ichino and Riphahn (2004) investigate the relationship between employment protection and absenteeism by looking at Italian firms in the private sector. According to the Chart of Workers Right *(Statuto dei Lavarotori)* from 1970, private firms with more than 15 employees face larger dismissal costs as compared to firms with less than 16 employees in Italy. Their results show that employees that hold stronger employment protection on average reported more sickness absence than employees with weaker protection, i.e. those at firms with a maximum of 15 employees.[4]

Lindbeck et al. (2006) exploit the same natural experiment used in this paper, i.e. the change in LAS in 2001.[5] Their conclusion is that the exemption decreased absence due to sickness by on average around 0.25 days per year and treated employee which corresponds to a 3.3 percent decrease. Lindbeck et al. (2006) claim that this stems from three different sources: (i) Firms in the treatment group became less reluctant to hire individuals that were likely to have health problems; (ii) employees with a relatively high sickness rate left

[4]A drawback with the study is that the data used only admit a comparison between firms with 20 employees or less and firms with more than 20 employees.

[5]The study uses establishments with 25 employees at most.

the job or were dismissed to a larger extent; and (iii) sickness decreased among employees that were in the treatment group. The first two sources affected sickness due to changes in the composition of the labor force, while the third source was due to differences in the sickness absence behavior among employees.

A disadvantage of Lindbeck et al. (2006) is that their data only cover sickness spells registered by the Swedish Social Insurance Administration. As a consequence, sickness spells shorter than 15 days are not taken into account, and the external validity of the results can thereby be called into question.

According to the Swedish Social Insurance Administration, only 9 percent of the registered sickness spells at private firms with a maximum of 49 employees were longer than 14 days in Sweden 2001. Sickness spells between 1 and 3 days accounted for almost half of the total registered cases of sickness absence for the same year. A result that only relies on data for long sickness spells is therefore likely to underestimate the total effect that the change in LAS might have had on sickness absence.[6]

3 Data and empirical results

In order to take both short and long sickness spells into account, we use data from Short Term Employment Statistics collected by Statistics Sweden. It contains detailed quarterly information for a selection of Swedish establishments in the non-agricultural private sector for the period 1994:1 to 2001:4. In total, a data set consisting of 175 261 establishments, divided into two groups (establishments with 2-9 employees and establishments with 11 - 49), is used.[7] Sickness

[6]Lindbeck et al. (2006) point out that their results are likely to be underestimated due to this limitation in their data set.

[7]Establishments with 10 and 11 employees are excluded. The reason is that the data are collected by Statistics Sweden through questionnaires, which makes it likely that the respondent forgets to include himself as an employee with the consequence that an establishment would falsely be included in the control group. It is also likely that some respondents have falsely included themselves in the questionnaires as an employee, with the outcome that some establishments are falsely included in the control group.

in Short Term Employment Statistics is defined as absenteeism due to sickness for a given Wednesday. A consequence of the definition is that both short- and long-run sickness spells are included in the data set. Another advantage is that no consideration has to be taken about different lengths of sick pay periods from the employer.[8] A drawback in using these data is that they cover enterprises (as compared to Lindbeck et al. (2006) that use data on an individual level), and the aggregation level thus makes it difficult to find out if the effect differs among gender, age groups, employment contracts or between workers that stay or leave the enterprise after the implementation of the exemption.[9]

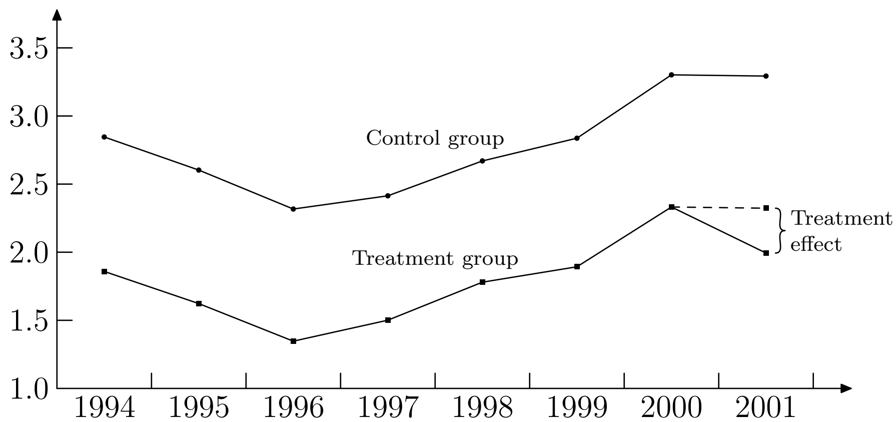
When trying to identify an effect from a reform, it is important to distinguish the effect of interest from other contemporary effects. In an ideal world, one would like to estimate the outcome for an individual that is both treated and untreated at the same point in time. Naturally, this is impossible, but if it is feasible to find a control group that, in the absence of treatment, is on average the same as the treatment group, the average treatment effect can be correctly estimated. All time effects should thereby be common across the two groups, that is, the average outcome for the groups should be parallel over time in absence of treatment. There is no formal test of this crucial assumption, but if parallel trends are present before treatment, this strongly suggests that the assumption is fulfilled (see Moffitt (1991) for an extensive discussion).

In Figure 1, yearly average sickness rates between 1994 and 2001 are plotted for establishments with 2-9 employees and establishments with 11–50 employees. The two series are almost perfectly parallel from 1994 to 2000. In 2001, the same year as the exemption in the employment protection legislation was im-

[8]The period for which the employer paid sickness benefit was 28 days between January 1997 to March 1998. During this period, absence due to sickness did not become registered until after 28 days if the data were based on payments of sick pay. From April 1998 to June 2003, the period was 14 days, from July 2003 to December 2004 21 days and from January 2005 and onwards the period is 14 days.

[9]Lindbeck et al. (2006) find that the effect on sickness absence was greatest for those workers in the treatment group that stayed at the same firm after the implementation of the change.

Figure 1: Sickness rates in treatment and control group, annual averages 1994-2001. Percent



Note: The control group consists of establishments with 12–50 employees in the non-agricultural sector, and the treatment group consists of establishments with 2–9 employees in the same sector. Employment weighted data is used to correct for differences in the sample selection stage.

Source: Short Term Employment Statistics

plemented, the average sickness rate for the treatment group falls sharply, while the average sickness rate for the control group levels out. After being parallel for 6 years, the parallelism between the groups suddenly disappears. If no other contemporary interaction has affected one of the groups, i.e. the assumption of parallel trends in the absence of treatment is fulfilled, the average treatment effect can be estimated as the difference between the groups' average sickness rate before and after the implementation of the exemption. In other words, a difference-in-differences (DiD) estimator can be applied to quantify the effect. The effect is illustrated in Figure 1 as the outcome difference between what the treatment group would have had if the exemption had not been implemented (dotted line) and the actual outcome.

DiD controls for all differences that are fixed between the groups and all symmetric time effects that affect the groups. As long as the assumption of parallel trends in absence of treatment is fulfilled, it is not necessary that the

Table 1: Mean values before and after the exemption in LAS

		Treatment			Control			
		2000	2001	Diff	2000	2001	Diff	*DiD*
Quarterly data	Sick rate	0.027	0.023	−0.004	0.037	0.039	0.002	−0.006
	Female	0.405	0.381	−0.024	0.375	0.368	−0.007	−0.017
	Temporary contracts	0.095	0.081	−0.014	0.112	0.110	−0.002	−0.012
Yearly data	Sick rate	0.028	0.024	−0.004	0.036	0.036	0.000	−0.004
	Female	0.410	0.374	−0.036	0.409	0.388	−0.021	−0.015
	Temporary contracts	0.105	0.100	−0.005	0.134	0.123	−0.011	0.006

Note: Employment weighted data.
Source: Statistics Sweden

outcome levels are the same for the two groups. Figure 1 strongly suggests this assumption to be fulfilled.

AA *DiD* between the first quarter 2000 and the first quarter 2001 reveals that the average direct effect of the change in LAS decreased sickness absence in the treatment group by roughly 0.6 percentage points (see Table 1), which corresponds to an effect of more than 22 percent. A problem might appear because of the large time window. The group composition, as can be seen in Table 1, has changed over time and it cannot be ruled out that the change in the sick rate is due to a compositional change of the groups that is not caused by the exemption in LAS. When using yearly data, the *DiD* decreases to 0.4 percentage points, but still it cannot be ruled out that the effect stems from changes in the composition not caused by LAS. To control for potential omitted interactions, i.e. events or changes that affect the outcome variable for the treatment or the control group occurring at the same time as the event of interest and thereby biasing the estimated treatment effect, we use regression analysis (OLS) when computing the *DiD*. The model gets the following form

$$Y_{it}^{j} = \alpha + \lambda d_t + \delta d^j + \beta dd_t^j + z_{it}^j \tau + \epsilon_{it}^j \tag{1}$$

where Y_{it} is the sickness rate at establishment i at time t, d_t is a time dummy that is equal to one in the treatment period, d^j is a dummy for being in the treatment group, and dd_t^j is a dummy for being in the treatment group in the

Table 2: Estimated average treatment effect from exemption in LAS 2001

Model	1	2	3
DiD	-0.0034^b (0.0017)	-0.0033^b (0.0017)	-0.0034^b (0.0017)
d01	0.0057^a (0.0011)	0.0039^a (0.0012)	0.0008 (0.0013)
Treatment	-0.0096^a (0.0005)	-0.0095^a (0.0005)	-0.0095^a (0.0005)
Female		0.0107^a (0.0011)	0.0135^a (0.0013)
Temporary contracts		0.0042^a (0.0016)	0.0036^b (0.0016)
Unemployment		-0.0698^a (0.0179)	-0.1784^a (0.0247)
Constant	0.0280^a (0.0006)	0.0279^a (0.0013)	0.0270^a (0.0023)
County effect			Yes^a
Industry effect			Yes^a
R^2	0.004	0.007	0.011
N	175 261	175 261	175 261

Note: Huber-Whites standard errors in parentheses. [a] significant at 1 percent, [b] significant at 5 percent and [c] significant at 10 percent. All models include controls for quarter and use employment weighted data.
Source: Statistics Sweden

treatment period. Z_{it}^j is a vector of explanatory variables controlling for omitted interactions. The coefficient of interest, β, estimates the treatment effect of the change in LAS.

The results are presented in Table 2. Three models are estimated where more explanatory variables are gradually included. The first model is the basic DiD-model that does not control for any omitted interactions. The second model includes the share of females, the share of temporary contracts and the employment rate at the county level. The share of females controls for differences in sickness rates between genders and the share of temporary contracts controls for differences in sickness rates between different types of employmentcontracts.[10] The unemployment rate controls for the possibility that the behavioral effect,

[10] As discussed earlier, Ichino and Riphahn (2005) show that sickness varies with the type of contract the employee holds.

which runs through unemployment, differs between the two groups.[11] The third model also includes county- and industry-specific effects. All models estimate a significant treatment effect of around 0.34 percentage points, representing an average effect of more than 13 percent. The fact that the treatment effect does not change when explanatory variables are added indicates that the assumption of parallel trends in the absence of treatment holds and that the DiD is unbiased.

The group variable, d^j, remains unchanged when adding explanatory variables to the model. In other words, the mean time-invariant difference between the two groups is not affected by the added explanatory variables. The time variable d_t, on the other hand, turns insignificant when controlling for county- and industry-specific effects, implying there to be no common time effect that has an equivalent impact on both groups' average sickness reportingwhen county and industry differences are taken into consideration.

3.1 Can we trust the estimated treatment effect?

Is the treatment effect estimated in Table 2 unique and attributable to the exemption in LAS? An easy and straightforward way of testing the robustness of an estimated treatment effect is to estimate placebo effects at different points in time. If any of these placebo effects turns out to be significant, it casts serious doubts on the estimated treatment effect.

Table 3 shows that all the estimated treatment effects for the placebo regression models are insignificant and that the only DiD estimator that is significant is the one for 2001, the actual year of treatment.[12] This indicates that the effect that occurred in 2001 is not random. The significance level for the treatment effect of 2001 ($DiD01$) decreases as an increasing number of placebo effects is added, but it is the only DiD estimator that is near a satisfactory level (p-value

[11]Results in Arai and Skogman Thoursie (2004) suggest that the business cycle has an impact on the sickness behavior among employees through the employment rate.

[12]A placebo model estimates DiD for periods where no treatment should be found.

Table 3: Robustness check with placebo regressions

Model	1	2	3
*DiD*01	-0.0035^{b} (0.0017)	-0.0032^{c} (0.0018)	-0.0030 (0.0020)
*DiD*00	-0.0002 (0.0017)	-0.0000 (0.0017)	-0.0002 (0.0020)
*DiD*99	-0.0000 (0.0014)	0.0003 (0.0014)	0.0005 (0.0014)
*DiD*98		0.0009 (0.0017)	0.0010 (0.0020)
*DiD*97		0.0006 (0.0012)	0.0008 (0.0015)
*DiD*96			0.0003 (0.0014)
*DiD*95			0.0001 (0.0015)
Treatment	-0.0095^{a} (0.0005)	-0.0098^{a} (0.0006)	-0.0099^{a} (0.0011)
Constant	0.0265^{a} (0.0006)	0.0267^{a} (0.0006)	0.0293^{a} (0.0008)
Falsification test	0.992	0.976	0.998
R^2	0.006	0.006	0.011
N	175 261	175 261	175 261

Note: All models are estimated with time dummy variables. Huber-Whites standard errors in parentheses. [a] significant at 1 percent, [b] significant at 5 percent and [c] significant at 10 percent. All models include controls for year, quarters and use employment weighted data. The Falsification test refers to a F-test of jointly significance for the estimated placebo effects.
Source: Statistics Sweden

–0.131) in the full *DiD*-specification.

A potential problem with *DiD* is that the standard errors may be underestimated when common group errors are present. This may make the researcher draw too strong an inference about the treatment effect (Moulton, 1990). According to Donald and Lang (2007), this is particularly problematic if the number of groups is small. To control for common group errors, we estimate models with aggregated data on group and year and the number of observations is reduced from 175 261 to 16.[13] Since the results utilize between-group variation, and not within-group variation, this should quantitatively give the same average

[13]The number of observations is further reduced when applying a first-difference and fixed-effect estimation procedure.

treatment effect, but now with standard errors taking common group errors into account. The treatment effect remains significant at a conventional level when underestimated standard errors are controlled for.

3.2 What caused the effect?

The exemption in LAS affected the average sickness absence in the treatment group, but the question of why the treatment effect occurred still remains unanswered. As discussed earlier, one would like to know how much of the treatment effect came from changes in the labor composition and how much originated in altered sickness behavior among employees. So far, we have not distinguished between these two effects. By excluding all enterprises that had any in – or outflows of workers (irrespective of contract form) after the change in LAS, a compositional effect can be ruled out. As can be seen in Table 4, the average treatment effect increases to about 0.4 percentage points when the behavioral effect is isolated. This is an increase by 0.1 percentage points as compared to the results that allowed for a compositional effect. The fact that the treatment effect increases when a compositional effect is excluded indicates that the softening of the employment protection legislation created both a behavioral and a compositional effect.

The behavioral effect decreased absence due to sickness while it was, all in all, decreased by the compositional effect. The increasing compositional effect stems from lower hiring costs, which result in more workers with higher tendencies toward sickness being hired than before (i.e. a less rigorous hiring process). Note that the estimated coefficients for the other variables remain nearly unchanged, thereby suggesting that the procedure to exclude establishments with flows of workers did not result in any selection problem biasing the estimate of the treatment effect.

A remarkable result is that the compositional effect is still positive when only enterprises with outflows are included.[14] This suggests that the compo-

[14]This result is not shown but can be given upon request.

Table 4: Average treatment effect excluding a compositional effect

Model	1	2	3
DiD	-0.0045^b (0.0018)	-0.0043^b (0.0018)	-0.0043^b (0.0018)
d01	0.0057^a (0.0011)	0.0039^a (0.0012)	0.0008 (0.0013)
Treatment	-0.0096^a (0.0005)	-0.0095^a (0.0005)	-0.0095^a (0.0005)
Female		0.0105^a (0.0011)	0.0135^a (0.0013)
Temporary contracts		0.0041^b (0.0016)	0.0034^b (0.0017)
Unemployment		-0.0688^a (0.0180)	-0.1782^a (0.0247)
Constant	0.0281^a (0.0006)	0.0279^a (0.0013)	0.0270^a (0.0023)
County effect			Yes^a
Industry effect			Yes^a
R^2	0.004	0.007	0.011
N	173 768	173 768	173 768

Note: Huber-Whites standard errors in parentheses. [a] significant at 1 percent, [b] significant at 5 percent and [c] significant at 10 percent. All models include controls for quarter and use employment weighted data.
Source: Statistics Sweden

sitional effect is not entirely driven by lower hiring costs for the employer. A conceivable explanation, at least in the short run, could be that workers who remain employed after redundancies, and who thus feel more secure, start to report sick to a greater extent than before the redundancies. If this is true, the weaker employment protection caused a compositional effect which sequentially caused a behavioral effect among those workers that were not fired.

Regardless of why the compositional effect emerged, it was dominated by the behavioral negative effect. The total average effect on sickness from the exemption in the employment protection legislation thus originates from a behavioral effect, since higher costs associated with sickness absence on average made the workers change their sickness pattern.

3.3 Is the effect homogenous among treated?

It is reasonable to suspect that the treatment effect can be heterogeneous among the treated due to specific establishment characteristics. An example would be if employees with temporary contracts, who already have relatively weak employment protection, react differently to the exemption than those with permanent contracts. Another example would be if the effect varies between genders. One way of investigating this with the aggregation level present in the data set is to examine establishments that are above or beneath a given threshold for the share of females or the share of temporary contracts. The threshold for the share of females is set to the median value of 35 percent. The threshold for the share of temporary contracts is set to 17 percent. The reason is that more than 50 percent of the establishments have no employees at all on a temporary contract. Half of those establishments that have at least one employee on a temporary contract have more than 17 percent of their employees on a temporary contract. It is clear from Table 5 that the effect varies with establishment characteristics. A treatment effect is not found among establishments with a relatively high share of females and temporary contracts, while it is found among those with a relatively low share of females and temporary contracts. There might be sev-

Table 5: Heterogenous treatment effect from exemption in LAS 2001

Model	1	2	3	4	5	6
DiD	-0.0021 0.0027	-0.0045^b 0.0022	0.0021 0.0042	-0.0049^a 0.0019	-0.0050^b 0.0023	-0.0055^a 0.0020
d01	0.0003 0.0018	0.0016 0.0019	-0.0008 0.0028	0.0016 0.0015	0.0016 0.0019	0.0016 0.0015
Treatment	-0.0100^a 0.0007	-0.0099^a 0.0006	-0.0053^a 0.0010	-0.0086^a 0.0005	-0.0099^a 0.0006	-0.0086^a 0.0005
Female	0.0228^a 0.0029	-0.0075^b 0.0030	0.0169^a 0.0023	0.0113^a 0.0015	-0.0073^b 0.0030	0.0111^a 0.0015
Temporary contracts	0.0046^c 0.0025	0.0009 0.0022	-0.0225^a 0.0030	0.0577^a 0.0068	0.0004 0.0022	0.0568^a 0.0067
Unemployment	-0.1903^a 0.0349	-0.1745^a 0.0349	-0.1871^a 0.0537	-0.1788^a 0.0277	-0.1763^a 0.0349	-0.1780^a 0.0277
Constant	0.0213^a 0.0031	0.0330^a 0.0058	0.0229^a 0.0043	0.0280^a 0.0026	0.0330^a 0.0058	0.0230^a 0.0026
County effect	Yes	Yes	Yes	Yes	Yes	Yes
Industry effect	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.012	0.012	0.011	0.011	0.013	0.011
N	87 491	87 770	37 355	137 906	87 039	136 987
Treshold	Fem > 0.35	Fem $<= 0.35$	Temp > 0.17	Temp $<= 0.17$	Fem $<= 0.35$	Temp $<= 0.17$

Note: Hubert-Whites standard errors in parentheses. [a] significant at 1 percent, [b] significant at 5 percent and [c] significant at 10 percent. All models includes controls for quarter and uses employment weighted data
Source: Statistics Sweden

eral reasons for this. One is that, as mentioned above, employees that were on a temporary contract already had relatively weak employment protection and thus, were less affected by the exemption. The fact that there is no effect among establishments with a relatively high share of females is somewhat of a puzzle. This might reflect the fact that women do not cheat on the sickness insurance system and that they do not attend work while sick – when they are sick they stay at home and when they are healthy they work. Another, more controversial, explanation would be that women use temporary parental benefits instead of reporting sick when sick to a greater extent than men.[15] It could also be the case that women were on temporary contracts to a greater extent than men before 2001 and thereby were less affected. The finding that the estimate for temporary contracts turns insignificant for establishments with no more than 35 percent of the females support this idea.

The sickness absence pattern for females seems to differ with the gender composition at the establishments. Women who work at establishments with a relatively high share of men have a lower sickness rate than their male colleagues. But women who work at establishments with a relatively low share of men have a higher sickness absence rate than men. The same pattern seems to hold for those who are on a temporary contract.

To see whether the effect is the outcome of a behavior effect, *the same procedure is applied as in section* 3.2 . As before, the negative impact from the exemption increases (see columns 5 and 6) and the overall effect stems from a negative behavioral effect and a positive compositional effect that mitigates it.

4 Conclusions

In this paper, the direct relationship between employment protection and sickness absence is empirically investigated. Because sickness absence is in some

[15]Engström et al. (2006) estimate that 22.5 percent of all payments for temporary parental benefits during the spring of 2006 were due to excessive use – a way for the parent of evading the day of qualifying period when no reimbursements are paid for his/her own sickness.

sense a measure of effort from the employee, the result will also reflect the indirect relationship between employment protection and labor productivity. To empirically investigate the relationship, within-country enforcement variation in the Swedish Employment Security Act (LAS) is used. The variation arose when an exemption was implemented on January 1, 2001 which made it possible for employers with a maximum of ten employees to exempt two workers from the seniority rule ("first-in-last-out") at times of redundancies. The sickness absence rate on average decreased by approximately 0.3 percentage points at those establishments that were treated relative those that were not, i.e. a decrease of around 13 percent. This effect can be compared with the one found by Lindbeck et al. (2006) of 3.3 percent, a study that, as opposed to this, was not able to pick up spells shorter than 15 days. This suggests that the change in LAS had the largest impact on shorter durations. Furthermore, it is shown that the negative treatment effect came from a behavioral change among employees – employment protection affects the worker's sickness behavior through the accompanying economic incentives that follow from it. The effect from the exemption is also found to vary with establishment characteristics; no effects are found among establishments with a relatively high share of females or temporary contracts, while a large negative effect on the reported sickness absence is found among establishments with a relatively low share of females and temporary contracts. All in all, the results reveal that employment protection is a decisive force for sickness absence behavior, especially for shorter spells among male workers or those that hold permanent contracts.

Even though it is clear that the softer employment protection had an impact on sickness absence, the question of whether labor productivity was affected remains unanswered. The key lies in whether the lower sickness absence among the treated mainly came from increasing presenteeism (attending work sick) or less cheating of the sickness insurance system. In light of labor productivity, the two scenarios might have completely opposite effects: (i) By attending work sick, the worker might aggravate and prolong the sickness status and thereby have reduced labor productivity over a longer period of time or the worker might infect co-workers – both resulting in lower overall labor productivity; (ii) less cheating of the sickness insurance system should increase labor productivity. The conclusion of this article is that employment protection affects sickness absence, but the indirect relationship between employment protection and labor productivity remains ambiguous.

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