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Essays on Employment Protection, Private Equity and Spousal Behavior

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To Lorena, Greta and Sonia

Acknowledgments

This thesis concludes my PhD studies. I know that I would never have come this far without having so many brilliant people supporting me. The two most important, are of course my advisors: Peter Skogman Thoursie and Lars Persson. I always felt that they believed in me, which has comforted and helped me to continue in those moments when the goal felt too far away. As economists they complement each other well. Peter being the empirical economist and Lars the theoretical economist. Peter has always been a great source of inspiration. He was the one that taught me that economics is so much more than I had envisioned during my undergraduate studies; and, he showed me that as an economists, one can provide interesting results with the help of seemingly easy analytical tools, a feature I have tried to adapt. I have also benefited from our unique discussions, sometimes on the run and sometimes on the train to Uppsala. Whether small or large, the topics have generated from his brilliance in economics and about life in general.

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Istanbul, April 2012
Martin Olsson

Contents

Introduction

In this dissertation, I present four self-contained essays. All essays have the common property of considering how institutions affect workers and their behavior. In Essays I and II, I analyze whether the employment protection legislation has an impact on the absence behavior of workers. Employment protection is well understood to influence the economy by changing job and worker flows and thereby potentially the employment level. It is less understood how employment protection affects the behavior of workers. I study if sickness absence is influenced by a softer employment protection in Essay I and if the use of paid childcare is affected in Essay II. In Essay III, I consider how workers are affected by changes in ownership and organizational form in the firm where they work. In particular, I study in what way private equity ownership affects workers in terms of unemployment and labor income. Earlier studies have shown that private equity ownership is associated with a reduction in the employment growth at firms and establishments but no study has yet been able to analyze individual data to see what happens to workers. In Essay IV, I examine if spousal labor supply depends on the partner's temporary disability insurance. By studying the interdependence between spousal labor supply and the partner's insurance, I examine an unnoticed side of the social insurance system that is potentially important for how to think about an optimal benefit level.

Essay I Employment Protection and Sickness Absence

Employment protection insures employees against unemployment but implies an adjustment cost for the firm. Employment protection can have an impact on the macro level of an economy by affecting job and worker flows as well as the employment level (see, for instance, Mortensen and Pissarides (1999)). On a more disaggregated level, employment protection can affect the behavior of workers by altering the link between workers' productivity and continued employment. Ichino and Riphahn (2005) show that sickness absence increased when employees in an Italian bank went from probation time, a period with no employment protection, to a permanent contract, a period with strong employment protection. An alternative way of studying employment protection and worker effort is to analyze the use of unpaid overtime. Engellandt and Riphahn (2005) estimate that workers on temporary contracts are 60 percent more likely to provide unpaid overtime compared to workers on permanent contracts. These studies show that employment protection can shape the behavior of workers and, as such, affect productivity.

In this essay, I estimate that workers report less sickness absence if their employment protection is relaxed. It is often difficult to estimate the causal effect of employment protection on a worker's behavior since the level of employment protection is usually endogenous to worker behavior in itself. However, here the result is identified with the help of a reform that increased the dismissal risk for workers in small firms in Sweden. The reform allows a firm with two to ten employees to exempt two workers from a seniority rule that stipulates that the first hired worker should be the last worker to leave at times of shortage of work. The reform provides a good opportunity to examine the way in which softer employment protection shapes the behavior of workers as it created within country variation in employment protection between small and large firms. I compare sickness absence in firms where the reform relaxed employment protection with sickness absence in firms where the protection was constant with a difference-in-difference estimator that controls for all time invariant group differences as well as all common group effects. Besides changing the behavior of workers, a softer employment protection can change the composition of the workforce via sorting of workers. That would be the case if softer employment protection leads to more dismissals of workers with a high sickness propensity, if employers become less rigorous in the hiring process and hire workers with a high tendency to be sick, or if workers who often are sick choose to seek a new employment.

I use establishment level data to take both short and long sickness spells into account. My results indicate that sickness absence decreases by around 13 percent at establishments where the employment protection was relaxed. While sorting of workers explains part of the result, the decline is also driven by a behavioral response among workers.

My finding that employment protection affects sickness absence indicates that further research is of importance. Does lower sickness absence induced by softer employment protection mainly come from increasing presenteeism (attending work sick) or less cheating of the sickness insurance system? In the light of labor productivity, the two scenarios have opposite effects: (i) by attending work sick, the worker can aggravate and prolong the sickness and thereby reduce 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 increases labor productivity. In this essay, I conclude that employment protection affects sickness absence, but the relationship between employment protection and labor productivity remains ambiguous.

Essay II Employment Protection and Parental Childcare

In Essay II, I further deepen the understanding of how employment protection shapes the behavior of workers. This time I focus on working parents and whether their provision of paid childcare is influenced by a softer employment protection. I also examine if the division of childcare in families depends on parents' relative employment protection.

In OECD, the dual earner family has become the most common family form, stressing the importance of being able to combine work and family. Although it is well understood that family policies can influence parents' use and division of paid childcare, it is less clear how labor market institutions affect working parents.

Parenthood implies absence when caring for the child, suggesting that working parents, as a group, may be sensitive to a relaxation of the employment protection legislation. In this essay, I once again use the reform of the Swedish seniority rule in 2001. But this time, I examine whether a higher dismissal risk affects the use of paid parental leave and temporary parental leave (care of sick a child). I use individual level data with the upside that I can carefully disentangle a compositional effect from a behavioral effect. A compositional effect occurs if the reform changes the composition of the workers in such a way that the average use of childcare is altered and a behavior effect occurs if workers change how they use paid childcare. I use treatment status in the spring of 2000 as an instrument for future treatment status to identify the behavioral response to the softer employment protection. The reform was decided on in October 2000, so the treatment status in the spring of 2000 is arguably unrelated to later outcomes caused by the reform but related to future treatment status.

I show that the share of workers with young children decreases by four percent in firms where the employment protection was relaxed. That indicates that parents might be disproportionally affected by softer employment protection. Then, I show that a softer employment protection makes parents less willing to provide childcare. I estimate that a greater average dismissal risk caused by the reform in 2001 reduces the total days of paid parental leave by on average 7.7 percent among employees in firms affected by the reform. For the total days of temporary parental leave, the decline is estimated to 5.6 percent. Two mechanisms are consistent with the observed result of the reduction in childcare: i) a sorting effect changing the composition of workers in firms affected by the reform, and ii) a behavioral effect affecting working parents' willingness to provide childcare.

I also find that parents' relative dismissal risks are important for the within family distribution of childcare: in families where only one spouse got a higher dismissal risk, the use of parental childcare increased in general for the unaffected spouse in reaction to the higher dismissal risk for the partner.

The contribution of Essay II is three-fold. First, it extends the literature on how employment protection shapes the behavior of workers by analyzing working parents' use of paid childcare, an outcome that has previously been unexplored. Second, the result confirms that employment protection can affect different groups of workers differently and thereby change the composition of the workforce. Third, the finding that the intra-household distribution of childcare is partly based on parents' relative degree of employment protection relates to a literature on how distributional factors can change the relative bargaining power within families (see, for instance, Browning and Chiappori (1998)).

Essay III Private Equity and Employees (joint work with Joacim Tåg)

A private equity buyout is a transaction where a private equity firm buys, restructures, and resells a mature firm with the help of capital invested in a private equity fund. The firm that is being bought is typically restructured for a period of 5 to 7 years, taken out of the stock market and highly leveraged. The number of buyouts has increased during the last few decades. When the industry started in the early 1970's, one or two deals were undertaken worldwide per year. In 2007, the number of deals per year had risen to 4 440 worldwide. Davies et al (2011) report that 2 percent of all workers in the US non-agricultural sector are today employed in a private equity-owned firm.

The spread of the buyout industry has not escaped criticism. Labor unions have claimed that buyouts, through layoffs and wage cuts, generate returns to investors at the expense of employees. Evidence from the US suggests that the employment growth declines in firms and establishments after a buyout (see, for instance, Davies et al 2011) while evidence from France suggests the opposite (Boucly et al 2011). But it is unclear how employees are affected by a buyout. In the public debate, a decline in employment growth is typically interpreted as evidence that buyouts affect employees negatively, but establishment or firm level data does not allow such an analysis. An observed decline in employment growth could, for instance, be due to layoffs (hurting employees) or to natural attrition and reductions in hiring (not affecting employees but unemployed).

In Essay III, we go beyond previous studies by evaluating how a buyout affects employees' unemployment risk and labor income. We analyze 201 buyouts undertaken between 1998 and 2004 in Sweden. Studying the Swedish private equity market has the upside of its being active and that rich linked employer-employee data is available. Additionally, the access to population data allows the construction of a control group of employees not affected by a buyout using detailed demographic data. Thereby, we can identify the effect of buyouts on employees by using a difference-in-difference estimator. We also perform several robustness checks, all in line with the main results, to ensure that our results are not driven by the fact that whether a firm is part of a buyout transaction or not is not random.

Our results show that employees benefit from buyouts by experiencing a reduction in unemployment risk and an increase in labor income. The result is consistent with the theory of private equity proposed by Norbäck, Persson and Tåg (2010) who emphasize that private equity ownership is temporary, which implies aggressive restructuring of the targeted firm to generate a high exit price. We go further and show that the reduction in unemployment risk is mainly concentrated to industries dependent on external capital to grow, non-divisional buyouts in which the target firm is more likely to be financially constrained, and buyouts undertaken just prior to the economic slowdown following the IT stock market crash in 2001. These findings are consistent with the hypothesis that buyouts relax financial constraints.

Essay IV Temporary Disability Insurance and Spousal Labor Supply (joint work with Peter Skogman Thoursie)

In Essay IV, we investigate if spousal labor supply is influenced by the partner's temporary disability insurance (sickness insurance). Social insurances are important for people's well being but they can, as pointed out by Krueger and Meyer (2002), have significant disincentive effects on labor supply. Although most empirical research has considered labor supply responses by the directly insured person, other individuals, for example their spouses, may also be affected by it. Spousal labor supply responses from social insurances are not well documented but important when designing the optimal benefit level in the insurance.

When a couple shares an economic budget, spousal labor supply can be affected by the partner's insurance via three types of income effects: 1) a *direct effect* if the partner's disposable income is affected by a change in insurance benefits. The sign of the direct effect is a priori unknown since a higher benefit level might reduce the partner's labor supply making the effect on the partner's disposable income ambiguous, 2) a pure *insurance effect* as the partner's benefit level alters the cost of future illness by the partner. A greater compensation leads to a decrease in spousal labor supply if leisure is a normal good, and 3) a *joint leisure effect* if higher compensation increases a couple's demand for spending leisure time together.

In an attempt to estimate a causal relationship between the partner's temporary disability insurance and spousal labor supply, we analyze spouses in couples for which a reform in Sweden in 1987 increased the benefit level when reporting sick for one of the partners. The reform affected workers when temporary ill in the private and the local governmental sector but left the benefit level unchanged for central government workers. Labor supply is measured as sickness absence using the complete personal sickness history for individuals from 1986 to 1990. The labor supply for central government workers married to a partner in the private sector or local government sector is compared to the labor supply for central government workers married to another central government worker. This means that all workers are employed in the same sector and the variation in insurance coverage stems from the partner's temporary disability insurance, making the treatment group and the control group similar to each other and the reform arguably independent to labor supply in the treatment group; two important features when trying to estimate a causal relation. Labor supply is measured as sickness absence.

Our results show that the partner's temporary disability insurance has a negative impact on spousal labor supply. We find that the incidence of sickness absence is unaffected but ongoing spells are prolonged by 0.4 days on average. The spousal elasticity of sick days with respect to the partner's sickness benefit is estimated to 0.74, corresponding to three-quarters of the direct elasticity of labor supply in the unemployment and temporary disability insurances.

Essay IV deals with an unnoticed side of the social insurance system. It complements earlier studies by showing that not only the labor supply of directly insured individuals is affected by the design of the social insurance. Our result is potentially important when thinking about the optimal benefit level in a social insurance.

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Employment protection and sickness absence ☆

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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 of the exemption is found to decrease sickness absence by more than 13% 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.

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 of employment as well as on the total employment level.[1] At the micro level, the behavior and performance of the firm may be affected.[2] Moreover, at the micro level it can induce 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% higher probability of working unpaid hours and Ichino and Riphahn (2005) show that absence due to sickness increased on average 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). Therefore, by 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 causal 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 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 of the exemption.

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

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[1] See Acemoglu and Angrist (2001), Boeri and Jimeno (2005) and Kugler and Pica (2008) 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.

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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.

The contribution of this paper is that the results take both long and short sickness spells into account, a crucial feature if the effect of the exemption varies with the duration of the spell. This is in contrast to register based data that during the period from 1998 to mid 2003 only contains sickness spells longer than 14 days and therefore do not catch short sickness spells.[4] The Swedish Social Insurance Agency reports that in 2001, short sickness spells, between 1 and 3 days, accounted for almost 50% of the total sickness spells at firms with less than 50 employees in the private sector while only about 9% of the spells had a longer duration than 14 days. This means that using register based data only allows analysis of around one-tenth of the total number of sickness spells which makes the external validity of the results limited since workers on short spells are likely to differ systematically from those on long spells. It is also likely that the source of the sickness absence might differ since short spells are in some sense easier for the worker to affect with presenteeism or cheating. This suggests that short sickness spells are from a labor productivity view the ones that best correspond to an effort measure. The nature of the data set used also allows an analysis over a long period since no consideration has to be taken of changes in the sick pay periods. To have consistent data for many periods before the exemption is crucial to identifying a proper control group when applying a DiD.

The results show that the exemption in LAS decreased the average sickness absence rate by about 13% in establishments with a maximum of 10 employees relative to establishments with 12 to 50 employees. This is due to 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 2. Data and results are described in section 3. Finally, conclusions are presented in section 4.

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 studying Italian firms in the private sector. Since the introduction of the Chart of Workers' Rights (*Statuto dei Lavarotori*) in 1970, private firms with more than 15 employees face larger dismissal costs as compared to

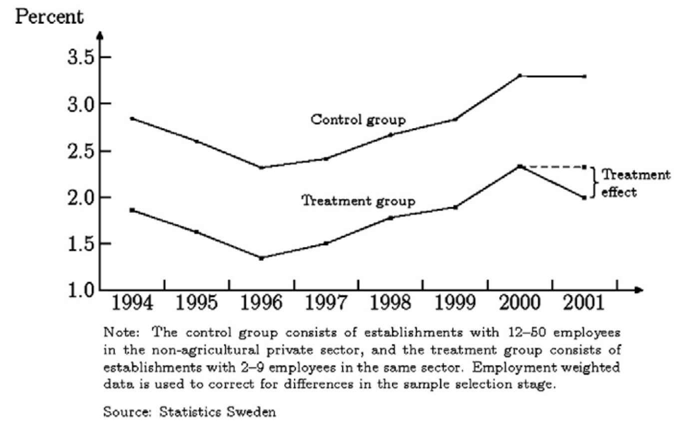

Fig. 1. Sickness rates in treatment and control group, annual averages 1994–2001.

firms with less than 16 employees in Italy. Their results show that employees who 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.[5]

Lindbeck et al. (2006) exploit the same natural experiment as used in this paper, i.e. the change in LAS in 2001.[6] Their conclusion is that the exemption decreased absence due to sickness by an average of around 0.25 days per year and treated employee which corresponds to a 3.3% 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 relatively high sickness rates left their jobs or were dismissed to a larger extent; and (iii) sickness decreased among employees who 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 National Insurance Board. 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 Agency, only 9% of the registered sickness spells at private firms with less than 50 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 relies only 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.[7] Further, Lindbeck et al. (2006) have only three pre-periods which is not enough to convincingly determine if the compared groups had parallel trends before the reform. This is a common, informal test of the assumption of parallel trends in the absence of treatment; if it does not hold, then estimated effects of the reform are biased.

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 pooled cross section data set consisting of 175 261 establishments, divided into two groups

[4] This is due to the fact that sick pay from the National Insurance Board starts after 14 days of sickness, while the employer pays sickness benefits for the first 14 days.

[5] 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.

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

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

(establishments with 2–9 employees and establishments with 12–50), is used.[8] Sickness in this data set is defined as absenteeism due to sickness for a given Wednesday. A consequence of this definition is that both short- and long-run sickness spells are included in the data even though they are not separable. Another advantage is that no consideration has to be taken about different lengths of sick pay periods from the employer making it possible to compare if parallel trends were present before the reform, a scenario that strengthens the identifying assumption of parallel trends in the absence of treatment.[9] Also, the data do not suffer from attrition and the accompanying selection problem since it is a pooled cross section collected through randomized samples from the target population. A drawback with the data set is that it covers establishments, 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.[10]

When trying to identify an effect of 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 the 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 Fig. 1, yearly average sickness rates between 1994 and 2001 are plotted for establishments with 2–9 employees and establishments with 12–50 employees.[11] The two series are almost perfectly parallel from 1994 to 2000. In 2001, the same year as the exemption in the employment protection legislation is implemented, 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 rates 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 Fig. 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 the absence of treatment is fulfilled, it is not necessary that the outcome levels are the same for the two groups. Fig. 1 strongly suggests that this assumption is fulfilled.

Table 1
Mean values before and after the exemption in LAS

		Treatment			Control			DiD
		2000	2001	Diff	2000	2001	Diff	
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	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	0.105	0.100	−0.005	0.134	0.123	−0.011	0.006

Note: Employment weighted data.
Source: Statistics Sweden.

A DiD between the first quarter of 2000 and the first quarter of 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%. 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 the two groups have not followed the same trend.[12] The different trends in the group composition suggest that it cannot be ruled out that the change in the sickness rate is due to a compositional change of the groups that is not caused by the exemption in LAS, i.e. an omitted interaction. 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 causing a bias in the estimated treatment effect, we use regression analysis (OLS) when computing the DiD. The model has the following form

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

where Y_{it}^{j} is the sickness rate at establishment i in group j 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 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[13]. 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 unemployment 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 employment contracts.[14] The unemployment rate controls for the possibility that the behavioral effect, which runs through unemployment, differs between the two groups.[15] The third model also includes 21 county-

[8] 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.

[9] The period for which the employer paid sickness benefit was 28 days between January 1997 and 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.

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

[11] Sickness rate is defined as the number of employees on sickness absence leave on a given Wednesday as a proportion of the total number of employees at an establishment.

[12] The variable Female measures the share of the employees that are females while the variable Temporary measures the share of the employees that are on a temporary contract.

[13] The estimates from the extra explanatory variables should not be seen as causal but instead seen as controls for omitted interactions.

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

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

Table 2
Estimated average treatment effect of exemption in LAS 2001

Model	1	2	3
DiD	−0.0034[b]	−0.0034[b]	−0.0034[b]
	(0.0017)	(0.0017)	(0.0017)
d01	0.0071[a]	0.0082[a]	0.0037[c]
	(0.0013)	(0.0015)	(0.0021)
Treatment	−0.0095[a]	−0.0096[a]	−0.0095[a]
	(0.0005)	(0.0005)	(0.0005)
Female		0.0108[a]	0.0134[a]
		(0.0011)	(0.0013)
Temporary contracts		0.0037[a]	0.0036[b]
		(0.0011)	(0.0016)
Unemployment		−0.0220	−0.1007[a]
		(0.0219)	(0.0444)
Constant	0.0267[a]	0.0205[a]	0.0228[a]
	(0.0009)	(0.0017)	(0.0032)
County effect			Yes[a]
Industry effect			Yes[a]
R^2	0.006	0.009	0.011
N	175 261	175 261	175 261

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

and 14 industry-specific effects.[16] In all models the estimated treatment effect is 0.34 percentage points, representing an average effect of more than 13%. Even though the share of females and temporary contracts did not show the same trend in Table 1 the regression results for the DiD are unaffected. The other co-variates do not affect the estimate of the reform either. This indicates that the identifying assumption of parallel trends in the absence of treatment is fulfilled and the estimate for the average treatment effect 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 is no common time effect that has an equivalent impact on both groups' average sickness rates when county and industry differences are taken into consideration.

3.1. Further analysis

Since the data are collected through randomized samples from the target population on a quarterly basis we can create a panel data set in order to take establishment fixed effects into account. There is a trade-off between the number of time periods and the number of establishments when creating the panel data set, as the number of establishments that are present for all time periods decreases when the number of time periods increases. In the end we estimate three models with different time periods. The procedure may also cause a selection problem as only establishments that stay in business are present. All establishments that change from the untreated group to the treated or vice versa are dropped. In total, 414 observations are dropped in model 1 in Table 3, 432 in model 2, and 448 in model 3. A few establishments change industry or county and those are also dropped. The weights that are used in Table 2 are not constant over time and can therefore not be used in the fixed effect models. This suggests that the results from the fixed effects models should not be directly compared to the earlier results, but should instead be seen as a complement.

In Table 3 the results when using establishment fixed effects are shown. The estimate for the treatment effect is negative and significant, although not comparable with the earlier estimates generated using pooled cross section data since here unweighted data are used. Once again the result indicates that the exemption in LAS decreased the average sickness rate on those establishments that were treated relative to the untreated establishments when controlling for establishment fixed effects. The negative treatment effect is also insensitive to different time specification of the pretreatment period. Using different pre-periods will result in different averages for both groups before the reform and thereby, as in Table 3, yield different estimates.[17] The results suggest, as in Fig. 1, that the identifying assumption of parallel trends in the absence of treatment between the two groups is fulfilled, since they hardly differ from the results in Table 9 in the Appendix, i.e. using establishment fixed effects does not change the result.

3.2. 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 4 shows that all the estimated treatment effects for the placebo regression models are insignificant and that the only DiD estimator that is significant is that for 2001, the actual year of treatment.[18] This indicates that the effect that occurred in 2001 is not random. The significance level for the treatment effect for 2001 (DiD01) decreases as an increasing number of placebo effects are added, but it is the only DiD estimator that is near a satisfactory level (p-value — 0.131) in the full DiD-specification.

By changing the size of the control group we can further test the robustness of the result. In Table 5 three models are estimated with upper limits on the control group of 20, 30 and 40. The reform effects are roughly the same in magnitude as in Table 2 for all these models, but now with slightly larger standard errors. No time effect is present and the time-invariant group effect is positive and highly significant as before.

A potential problem with DiD is that the standard errors may be underestimated when common group errors are present. This may cause the researcher to draw too strong an inference about the treatment effect, the so called Moulton-problem (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.[19]

Since the results utilize between-group variation, and not within-group variation, this should quantitatively give the same average 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.

A common objection against the use of a reform is that if individuals anticipate the reform and begin to behave in a certain way before the reform is implemented it will bias the treatment

[16] Messina and Vallanti (2007) find evidence that strong employment protection legislation influences job creation in contracting industries to a larger extent relative to non-contracting industries. Further, MacLeod and Nakavachara (2007) show that the good faith rule, an exemption from the Employment At-Will rule in the US, has different effects on employment in highly and less populated areas.

[17] For comparison, see Table 9 in Appendix for results using unweighted data as in Table 3 but without establishment fixed effects.
[18] A placebo model estimates DiD for periods where no treatment should be found.
[19] The number of observations is further reduced when applying a first-difference and fixed-effect estimation procedure.

Table 3
Estimated average treatment effect of exemption in LAS 2001 using establishment fixed effects

Model	1	2	3
DiD	−0.0078[b]	−0.0060[b]	−0.0056[b]
	(0.0032)	(0.0027)	(0.0023)
d01	0.0096[a]	0.0057	0.0027[c]
	(0.0036)	(0.0029)	(0.0015)
Female	0.0184	0.0253[c]	0.0133
	(0.0129)	(0.0133)	(0.0129)
Temporary contracts	−0.0081	0.0059	0.0025
	(0.0072)	(0.0077)	(0.0083)
Unemployment	−0.1785	−0.2472[b]	−0.1183
	(0.1220)	(0.1129)	(0.1135)
Constant	0.0283	0.0312[a]	0.0321[a]
	(0.0283)	(0.0073)	(0.0066)
R^2	0.007	0.006	0.007
Number of establishments	393	669	1083
Periods	1998:1–2001:4	1999:1–2001:4	2000:1–2001:4

Note: Huber–Whites standard errors in parentheses. [a] significant at 1%, [b] significant at 5% and [c] significant at 10%. All models include fixed effects for establishment, quarter and year.
Source: Statistics Sweden.

estimate. Here potential threats are that some workers may have anticipated the reform and actively tried to change their job in order to be in the treatment group or to be in the control group, that the employers may have changed their screening process or that workers may have changed their sickness behavior. The compositional effects are not likely since, as discussed in Lindbeck et al. (2006), the reform was due to an unexpected collaboration between the Green Party and the non-socialist parties against the will of the Social Democratic government and was discussed openly as late as 2000, i.e. less than a year before the implementation. Actually, it was as late as October 2000 that the Parliament decided the final version of the reform and the exact design was set. This narrow time window makes it unlikely that an endogeneity problem is present since the matching process on the labor market usually takes time.[20] Also, if a compositional and/or a behavioral effect was already present during 2000 that would have been seen in the DiD for 2000 in Table 4 and in Fig. 1, where no evidence can be found of an anticipation effect during 2000.[21]

Table 4
Robustness test with placebo regressions

Model	1	2	3
DiD01	−0.0035[b]	−0.0032[c]	−0.0030
	(0.0017)	(0.0018)	(0.0020)
DiD00	−0.0002	0.0000	0.0002
	(0.0017)	(0.0017)	(0.0020)
DiD99	−0.0000	0.0003	0.0005
	(0.0014)	(0.0014)	(0.0014)
DiD98		0.0009	0.0010
		(0.0017)	(0.0020)
DiD97		0.0006	0.0008
		(0.0012)	(0.0015)
DiD96			0.0003
			(0.0014)
DiD95			0.0001
			(0.0015)
Treatment	−0.0095[a]	−0.0098[a]	−0.0099[a]
	(0.0005)	(0.0006)	(0.0011)
Constant	0.0265[a]	0.0267[a]	0.0293[a]
	(0.0006)	(0.0006)	(0.0008)
Falsification test	0.992	0.976	0.998
R^2	0.006	0.006	0.006
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%, [b] significant at 5% and [c] significant at 10%. All models include controls for year, quarter. Employment weighted data. The Falsification test refers to an *F*-test of jointly significance for the estimated placebo effects.
Source: Statistics Sweden.

Table 5
Robustness test with different control groups

Model	1	2	3
DiD	−0.0037[c]	−0.0032[c]	−0.0035[b]
	(0.0021)	(0.0019)	(0.0018)
d01	0.0035	0.0033	0.0038[c]
	(0.0025)	(0.0022)	(0.0021)
Treatment	−0.0068[a]	−0.0084[a]	−0.0090[a]
	(0.0006)	(0.0005)	(0.0005)
Constant	0.0220[a]	0.0223[a]	0.0225[a]
	(0.0035)	(0.0033)	(0.0032)
R^2	0.009	0.010	0.010
Upper limit of control group	20	30	40
N	125 044	148 855	163 994

Note: Huber–Whites standard errors in parentheses. [a] significant at 1%, [b] significant at 5% and [c] significant at 10%. All models include controls for the share of females, the share of temporary contracts, the unemployment rate at the county level, and fixed effects for quarter, year, industry and county. Employment weighted data.
Source: Statistics Sweden.

3.3. 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 establishments in the treatment group that had any in- or outflows of workers (irrespective of contract form) after the change in LAS, i.e. for the period 2001:1 to 2001:4, a compositional effect can be ruled out. As can be seen in Table 6, the average treatment effect increases to about 0.44 percentage points when the behavioral effect is isolated. This is an increase of 0.1 percentage points compared to the results that allowed for a compositional effect. The estimated coefficients for most of 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 causing a bias in the estimate of the treatment effect.[22] 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 increased 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).

A remarkable result is that the compositional effect is still positive when only enterprises with outflows are included.[23] This suggests

[20] This would especially be the case if workers with high propensity to report sick tried to avoid the treatment group since sickness history of a worker is an important indicator in the screening process.

[21] When using an instrumental variable model with a DiD for 1999 as the instrument, the negative treatment effect is maintained but with a larger standard error, a feature of using this approach.

[22] As pointed out by an anonymous referee, one problem with this procedure is that if the reform caused the treated establishments to hold on to their employees to a greater extent than before this would not be captured with the above procedure. In Von Below and Thoursie (2008) the 2001 reform is used to analyse if the relaxation had any effect on firms' employment behavior. Different measures of separations are compared for the periods 1999–2000 against 2001–2002 and no significant difference can be found using population data except for young workers aged 18–25 where an increase is found. These empirical results suggest that the hypothesis of lock-in effects is not supported by evidence.

[23] This result is not shown but can be provided upon request.

that the compositional 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 who were not fired.

Regardless of the reason for the compositional effect, it was dominated by the negative behavioral 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.

3.4. Is the effect homogeneous among the 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 at the median value of 35%. The threshold for the share of temporary contracts is set at 17%. The reason is that more than 50% of the establishments have no employees at all on a temporary contract. Among those establishments that have at least one employee on a temporary contract, the median proportion of employees on temporary contracts is 17%. It is clear from Table 7 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 could be several reasons for this. One is that, as mentioned above, employees who 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 indicate that women do not cheat on the sickness insurance system and that they do not attend work while sick — they stay at home when sick and work when healthy. Another, more controversial, explanation would be that when sick, women use temporary parental benefits instead of reporting sick when sick to a greater extent than men.[24] 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% of the females supports this idea.

Table 7
Heterogeneous treatment effect of exemption in LAS 2001

Model	1	2	3	4	5	6
DiD	−0.0020	−0.0045[b]	0.0022	−0.0049[a]	−0.0050[b]	−0.0055[a]
	(0.0027)	(0.0022)	(0.0042)	(0.0019)	(0.0023)	(0.0020)
d01	−0.0003	0.0026	−0.0010	0.0025	0.0024	0.0025
	(0.0031)	(0.0028)	(0.0048)	(0.0024)	(0.0029)	(0.0024)
Treatment	−0.0101[a]	−0.0098[a]	−0.0053[a]	−0.0086[a]	−0.0098[a]	−0.0087[a]
	(0.0007)	(0.0006)	(0.0010)	(0.0005)	(0.0006)	(0.0005)
Constant	0.0205[a]	0.0302[a]	0.0213[a]	0.0259[a]	0.0304[a]	0.0257[a]
	(0.0046)	(0.0058)	(0.0067)	(0.0027)	(0.0065)	(0.0037)
R^2	0.012	0.014	0.021	0.012	0.014	0.012
N	87 491	87 770	37 355	137 906	87 039	136 987
Threshold	Fem	Fem	Temp	Temp	Fem	Temp
	>0.35	<=0.35	>0.17	<=0.17	<=0.35	<=0.17

Note: Hubert–Whites standard errors in parentheses. [a] significant at 1%, [b] significant at 5% and [c] significant at 10%. All models include controls for the share of females, the share of temporary contracts, the unemployment rate at the county level, fixed effects for quarter, year, industry and county. Employment weighted data. Column 5 and 6 controls, as opposed to the other columns, for a compositional effect.
Source: Statistics Sweden.

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 3.3. 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 mitigating positive compositional effect.

Lastly, the reform will change the employment protection differently according to the number of employees at an establishment. The largest change in protection happened among establishments with 3 employees; before the reform two workers were protected according to the seniority rule, but with the exemption none of the workers is safe. This is a reduction in protected workers with 67%. At establishments with two or four employees the reduction in protected workers is 50% and with five the decrease is 40%, which reduces gradually to a 22% decrease for establishments with nine workers. In Table 8 a DiD is estimated for three categories of establishments with different numbers of employees. The categories are based on the reduction of employment protection due to the reform. The effect is clearly different between the categories and is only significant in the category where the change of protected workers was largest.

Table 6
Estimated average treatment effect when excluding a compositional effect

Model	1	2	3
DiD	−0.0045[b]	−0.0044[b]	−0.0043[b]
	(0.0018)	(0.0018)	(0.0018)
d01	0.0048[a]	0.0060[a]	0.0012
	(0.0013)	(0.0016)	(0.0021)
Treatment	−0.0095[a]	−0.0096[a]	−0.0095[a]
	(0.0005)	(0.0005)	(0.0005)
Female		0.0107[a]	0.0134[a]
		(0.0011)	(0.0013)
Temporary contracts		0.0036[b]	0.0034[b]
		(0.0016)	(0.0017)
Unemployment		0.0246[a]	−0.0984[b]
		(0.0220)	(0.0445)
Constant	0.0291[a]	0.0227[a]	0.0251[a]
	(0.0010)	(0.0018)	(0.0032)
County effect			Yes[a]
Industry effect			Yes[a]
R^2	0.006	0.009	0.011
N	173768	173768	173768

Note: Huber–Whites standard errors in parentheses. [a] significant at 1%, [b] significant at 5% and [c] significant at 10%. All models include fixed effect for quarter and year. Employment weighted data.
Source: Statistics Sweden.

4. Conclusions

In this paper, the direct relationship between employment protection and sickness absence is empirically investigated. Because sickness absence is in some sense a measure of effort from the employee, the result will also reflect the indirect relationship between employment protection and labor productivity. To empirically

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

Table 8
Heterogeneous treatment effect regarding size of establishment of exemption in LAS 2001

Number of workers	2–4	5–7	8–9
DiD	−0.0043[b]	−0.0015	−0.0005
	(0.0021)	(0.0028)	(0.0030)

Note: Huber–Whites standard errors in parentheses. [a] significant at 1%, [b] significant at 5% and [c] significant at 10%. The model controls, as model 3 in Table 3, for the share of females, the share of temporary contracts, the unemployment rate at the county level, fixed effects for quarter, year, industry and county. Employment weighted data. (N=175 261).
Source: Statistics Sweden.

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 that 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%. This effect can be compared with the one found by Lindbeck et al. (2006) of 3.3%, a study that, as opposed to this one, 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 of the exemption is also found to vary within the treatment group and 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 how 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 could aggravate and prolong the sickness and thereby reduce 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.

Appendix A

Table 9
OLS regressions using an unweighted balanced panel data set

Model	1	2	3
DiD	−0.0076[b]	−0.0058[c]	−0.0056[b]
	(0.0038)	(0.0031)	(0.0028)
d01	0.0137[a]	0.0101[a]	0.0033[b]
	(0.0027)	(0.0021)	(0.0016)
Treatment	−0.0101[a]	−0.0093[a]	−0.0087[a]
	(0.0019)	(0.0018)	(0.0020)
Female	0.0052[c]	0.0100[a]	0.0106[a]
	(0.0029)	(0.0029)	(0.0027)
Temporary contracts	−0.0023	0.0114[b]	0.0081
	(0.0051)	(0.0053)	(0.0051)
Unemployment	−0.0239	0.0180	−0.0329
	(0.0562)	(0.0520)	(0.0569)
Constant	0.0288[a]	0.0280[a]	0.0340
	(0.0039)	(0.0035)	(0.0031)
R^2	0.017	0.014	0.012
Number of establishments	393	669	1083
Periods	1998:1–2001:4	1999:1–2001:4	2000:1–2001:4

Note: Huber–Whites standard errors in parentheses. [a] significant at 1%, [b] significant at 5% and [c] significant at 10%. All models include controls for quarter and year.
Source: Statistics Sweden.

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Employment Protection and Parental Childcare

Martin Olsson

Abstract

I examine if employment protection affects parental childcare. I find that a greater risk of being dismissed has a substantial effect on how parents use and divide paid childcare between them. The identification relies on a reform that made it easier for employers in Sweden to dismiss workers in small firms. I estimate that an increased dismissal risk reduces the total days of parental childcare by around six percent in targeted firms, measured as total days of parental leave or temporary parental leave. Both a sorting effect and a behavioral effect can explain the reduced childcare. I also find evidence of a redistribution effect of temporary parental leave within households if only one partner was affected by the reform. I interpret the redistribution effect as a way of evading an external cost on the child.

Keywords: employment protection; parental childcare; within family distribution

JEL: J13, K31

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1 Introduction

The dual earner family has become the most common family form in developed countries today.[1] Consequently, the importance of being able to combine work and family has increased.[2] In the European Union, reconciliation of work and family has been on the political agenda since the mid 1980´s and is today stated in the Lisbon Treaty.[3] More explicitly, family policies such as parental leave and subsidized childcare have been implemented to help parents juggle work and family. Economic research has shown that fertility as well as the use of paid parental childcare can be affected by family policies.[4] But how non-family friendly labor market institutions affect working parents is less understood, even though a good understanding of their effects ought to be fundamental for creating a successful reconciliation of work and family.

In this study, I examine the impact of employment protection on working parents' willingness to provide childcare. The level of employment protection is directly linked to the risk of being dismissed if an employee is absent and has been shown to affect workers' absence behavior.[5] Besides the insight that the use of sickness absence is affected, little is known of how employment protection affects other types of behavior.

To understand if employment protection influences parental childcare, I analyze an exemption in a seniority rule that increased the average dismissal risk in firms with two to ten

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[1] The dual earner family is the most common family form in a majority of the OECD countries (OECD (2010)). In 2007, the median employment rate for partnered mothers aged 15-64 was 66.5 percent in OECD countries. For the U.S., the Current Population Study for 2008 shows that 57.3 percent of all married-couple families were dual earner families.

[2] *The Economist*, Dec 30th 2009, "Female Power" writes: "Many women – and indeed many men – feel they are caught in an ever-tightening tangle of commitments. If the empowerment of women was one of the great changes of the past 50 years, dealing with its social consequences will be one of the great challenges of the next 50."

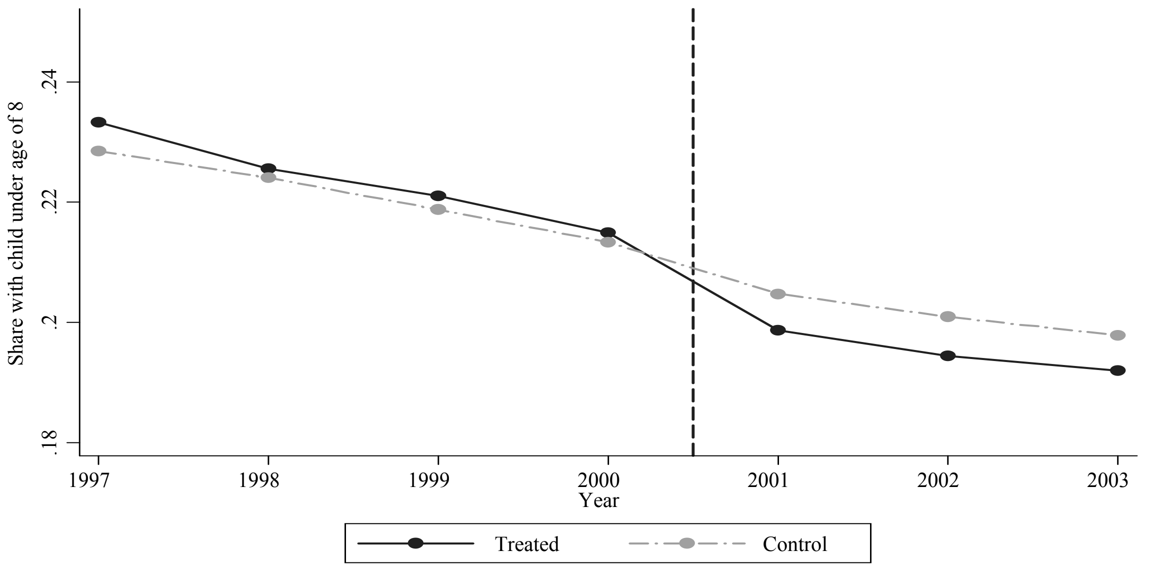
[3] The legislation is stated in article 33 of the Charter of Fundamental Rights in the Lisbon treaty. Since 2002, reconciliation of work and family is also one of three main objectives in the EU gender policy.

[4] For instance, Lalive and Zweimüller (2009) provide evidence that job protected parental leave has a positive effect on fertility and mothers' time between birth and return to work. Skyt-Nielsen (2009) and Ekberg et al (2005) find that family policies can affect the within household distribution of parental leave by introducing economic incentives or by earmarking certain days to the father A reason for parents to be unwilling to provide childcare is that a career interruption can have a negative effect on subsequent earnings as discussed by Albrecht et al (1999).

[5] See Riphahn and Thalmaier (2001), Ichino and Riphahn (2005), Lindbeck et al (2006), Olsson (2009) and Jacob (2010). See also Engellandt and Riphahn (2005) for how worker effort in terms of unpaid overtime may vary with employment protection.

employees in Sweden. The seniority rule is applied if a firm has shortage of work and specifies the dismissal order in accordance with workers' firm-specific tenure. Starting in 2001, small firms can exempt two workers from the tenured-based dismissal order. The exemption increases the number of workers at risk for being dismissed in small firms and creates within country variation in employment protection and thereby an opportunity to overcome the fact that a worker's level of employment protection is not random. The reform of the seniority rule has been shown to decrease the sickness absence in small firms (Lindbeck et al (2006) and Olsson (2009)) and increase hires and separations in small firms (von Below and Skogman Thoursie (2010)).

Figure 1 Share of employees having a child under the age of eight in treated and control firms from 1997 to 2003. The figure uses a repeated cross-sectional sample where firm size is measured as the average yearly number of employees. "Treated" refers to firms with two to ten employees and "control" refers to firms with 11 to 50 employees



This paper focuses on working parents and their use of paid childcare. In general, parenthood implies absence from work when caring for the child. To the extent that the absence period implies a greater dismissal risk, one can expect working parents, as a group, to be extra sensitive to softer employment protection. For instance, a softer protection can lead to dismissals of working parents if employers favor workers that are less absent, or working parents may seek jobs with a higher level of protection. In Figure 1, it is seen that the share of employees with young children drops by almost four percent (0.8 percentage points) in firms with two to ten employees in 2001, the year of the reform.[6] Whether the sudden drop reflects that employers made use of the exemption to dismiss parents with young children or that parents with young children decided to leave small firms in fear of being dismissed is unclear. But Figure 1 indicates that working parents may be extra sensitive to softer employment protection.

To measure parental childcare, I use comprehensive data on paid parental leave and temporary parental leave (care of a sick child) combined with detailed individual information for the whole Swedish working population. The connection between employment protection and parental childcare is investigated with a difference-in-difference strategy that compares childcare for workers in firms with two to ten employees with childcare for workers in firms with eleven to 50 employees, before and after the reform in 2001. I conduct a graphical analysis that shows that the groups have similar trends in key outcomes prior to the reform, which suggests that my results can be interpreted as causal.

I find that a softer employment protection reduces parental childcare. More specifically, I estimate that a greater average dismissal risk reduces the total days of paid parental leave by on average 7.7 percent among workers in treated firms. The decline in total days of temporary parental leave is 5.6 percent. Two mechanisms can explain the decline: a sorting effect that changes the worker composition in targeted firms and a behavioral effect that changes working parents' willingness to provide childcare. Parents' relative dismissal risks seem important for the within family distribution of childcare. I estimate that in families where one partner got a higher dismissal risk, the use of parental childcare increased in general for the unaffected partner in reaction to the higher dismissal risk for the other partner.

This study provides evidence that labor market institutions unrelated to family policies can be important for parents' willingness to provide paid childcare. While this seems obvious, it has attracted little attention so far. I present three novel findings. First, I estimate that a higher dismissal risk makes working parents less willing to provide paid childcare. This extends the literature on how employment protection affects workers' behavior where sickness absence

[6] The decline of 4 percent refers to the drop relative to the pre-reform average for the treatment group.

has been assessed earlier (Riphahn and Thalmaier (2001), Ichino and Riphahn (2004, 2005), Engellandt and Riphahn (2005), Lindbeck et al (2006), Olsson (2009) and Jacob (2010)). Second, I show that a softer employment protection in certain firms can offset a sorting effect that alters the composition of the workforce in targeted firms in such a way that average parental childcare among employees is decreased. This confirms that employment protection can affect groups of workers differently (Delacroix (2003) and Kugler and Saint-Paul (2004)). Third, I find empirical support for the fact that the intra-household distribution of childcare is partly based on parents' relative degree of employment protection which relates to the literature on how distributional factors can change the relative bargaining power within families (see Browning and Chiappori (1998), Chiappori, Fortin, and Lacroix (2002), and Chiappori and Ekeland (2006) for how the distributional factor affects spouses' relative bargaining strength, and in particular Parys and Schwerhoff (2010) for how distributional factors can affect the within family distribution of parental leave).

2 Institutional Setting

In this section, I briefly describe the Swedish employment protection legislation with a focus on the seniority rule and how it was reformed in 2001. I also describe the general outline of the Swedish parental insurance system.

2.1 The Employment Protection Legislation and the Reform

The Swedish employment protection legislation act, *Lagen om anställningsskydd*, was introduced in 1974 and regulates three areas: regular employment, temporary employment, and collective dismissals. A debated part of the legislation is the seniority rule that stipulates the tenured-based order of dismissal at times of redundancies. The rule is often referred to as the "last-in-first-out"-rule as it states, somewhat simplified, that the last worker that was hired should be the first worker to be dismissed.[7] The rule includes all workers apart from family members of the firm owner and those in managerial positions and is applied within job task-

[7] If two workers have the same tenure, the oldest worker is prioritized.

specific groups at a firm.[8] The seniority based dismissal order was introduced to reduce the risk for older workers to end up in permanent unemployment.[9] A critique against the seniority rule is that employers can become reluctant to hire "wild cards", such as young individuals and immigrants, since bad matches are hard to resolve. Proponents of the rule often emphasize that the rule is dispositive since collective-agreements allow departures from it. Whether the rule is strictly followed or not appears to be correlated with the number of employees in a firm: Calleman (2000) studies 30 Swedish firms in the years 1994 and 1995 to see how the seniority rule is applied and finds that it is only strictly followed in small firms (in terms of the number of employees).

The aim of the reform was to alleviate the cost of the seniority rule for smaller firms: a key worker was considered to be relatively more important for the operation of smaller firms. An amendment was thus added to the Swedish employment protection legislation in January 2001 that allowed firms with at most ten employees to exempt two workers of significant importance from the stipulated order of dismissal. The amendment came about through an unexpected collaboration between the Green Party and the non-socialist parties and was decided in October 2000. Lindbeck et al (2006) present the chronological order of the decision process. During the early spring of 2000, the discussion concerned whether to allow exemptions in all firms or only in firms with at most ten employees. In May the same year, it became clear that the first alternative could never be implemented, and it was not until October 2000 that it was finally decided that only small firms were to exempt two workers from the seniority rule.

Already in 1994, firms of any size were allowed to exempt two workers from the stipulated order of dismissal, but the exemption was abolished one year later when the Social Democratic Party came into power with the argument that the exemption had given employers too much power in their relationship to employees as employers were already able to decide when to dismiss workers due to shortage of work.

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[8] In Swedish, this is referred to as *"turordningskretsar"*.

[9] A tenured-based dismissal order implies that employers cannot sort employees according to their productivity, as pointed out by Edin and Holmlund (1993). By having employees of different productivities share the unemployment risk, the signaling cost of unemployment becomes less costly since the signal contains less information about an employee's productivity.

Exempting two workers from the seniority rule increases the average dismissal risk. However, in what way the individual worker was affected is a function of the worker's relative tenure and size of the firm in terms of the number of employees. Table 1 displays how the reform affected workers in firms with two to ten employees in the case of one dismissal. All workers in firms with two and three employees were affected by the reform: the reform decreased the dismissal risk for the worker with the shortest tenure and increased the dismissal risk for those with the longest tenure. In firms with more than three employees, the dismissal risk for workers with the longest tenure was unaffected. The share of unaffected workers increases with firm size while the share of workers who faced a higher or lower dismissal risk decreases with firm size. A priori, it is unclear how short-tenured workers react to the reform. The lower dismissal risk can induce more absence as its cost decreases. But the reform implied that low tenured workers can continue their employment at times of shortage of work, which can decrease the absence by making productivity and continued employment more closely connected.

Table 1 How the reform changed the dismissal risk The share of workers in firms with two to ten employees that got a higher dismissal risk, a lower dismissal risk or were unaffected by the reform at times of one dismissal

Size	Higher dismissal risk %	Unaffected %	Lower dismissal risk %	Size	Higher dismissal risk %	Unaffected %	Lower dismissal risk %
2	50	0	50	**7**	29	57	14
3	66	0	33	**8**	25	63	12
4	50	25	25	**9**	22	67	11
5	40	40	20	**10**	20	70	10
6	33	50	17				

Notes: Size refers to number of employees at a firm. All figures are based on the assumption of one job task-specific group per firm.

Earlier studies show that the reform of the seniority rule had an impact on both workers and employers: Lindbeck et al (2006) and Olsson (2009) report that workers in eligible firms decreased their sickness absence once the reform was implemented and von Below and Skogman Thoursie (2010) discover that the reform increased hires and separations in small firms by five percent each. Moreover, when the Confederation of Swedish Enterprise in 2009 made a survey of 600 firms with five to ten employees, 32 percent of the 174 firms that reported that they had had displacements during the previous year claimed that they had made use of the exemption.

Exempting two workers from the seniority rule can, in particular, increase the dismissal risk for working parents. Besides the decline in the share of working parents in eligible firms, see Figure 1, the Swedish Trade Union Organization (LO) claims that around half of all exemptions involving their members up until 2003 were unacceptable and involved in particular pregnant women, parents on parental leave and elderly workers (Arbetaren (2003)). Earlier experience of reforming the seniority rule also suggests that working parents can be negatively affected. In 1999, the Swedish ombudsman of equality (JämO) assessed the firm size neutral exemption of two workers introduced for one year in 1994. One of the main conclusions is that the exemption in 1999 had had a disproportionally negative effect on workers on parental leave and older women. There have also been three parliamentary bills with the objective to abandon the 2001-reform, all of them arguing that this does, in particular, hit pregnant women and employees on parental leave.[10]

2.2 The Parental Leave Insurance

The Swedish parental insurance system dates back to 1974 and provides economic compensation for foregone labor income when taking care of a child during regular working hours.[11] The insurance covers parental leave and temporary parental leave (leave for care of a sick child) and it is generous in an international comparison. Job protected parental leave is provided during the first 18 months of a child's life. After that, parents are entitled to work part time (minimum one fourth of regular working hours) until the child turns eight. The parental leave insurance provides economic compensation for a total of 480 days per child and can be drawn until the child turns eight years old. For the first 390 days, the benefit amounts to around 80 percent of the yearly income (up to a cap of almost 432 000 SEK per year). Those with no or very low income are guaranteed 180 SEK per day called the basic level. For the last 90 days of the 480 days, a fixed amount of 180 SEK per day is paid.[12] To encourage dual earner families, 60 days are earmarked for each parent. The earmarked days were introduced in 1995 with a so-called daddy month and were extended by an additional

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[10] Two bills came from members of the Social Democratic Party in 2001 and 2009, and one from members of the Left Party of Sweden in 2002.
[11] Before 1974, only mothers were covered by the insurance.
[12] Before 2003, the replacement for the last 90 days was 60 SEK.

month in 2002.[13] The use of parental leave among fathers has become more common over time: in 2007 around 21 percent of all paid parental leave days were taken by fathers, compared to around one percent in 1974.[14]

Parents with children aged less than 12 are eligible for temporary parental leave benefits. The insurance compensates parents for economic losses when staying home from work to take care of a sick child. The compensation rate is 80 percent and is paid for at most 120 days per year and child without any waiting period. To allow part-time work, benefits can be drawn for a full day, 3/4, 1/2, 1/4, or 1/8 of a day. The use of temporary parental benefits is widespread: in 2009, 367 543 mothers took a total of 2 898 916 days of temporary parental leave corresponding to an average of 7.7 days.[15] In the same year, 271 661 fathers took 1 590 462 days of temporary parental leave corresponding to 5.8 days. The total number of used temporary parental leave days was thereby 4 489 378, corresponding to around one day of leave per person in the Swedish workforce per year.

The Swedish parental leave law states that employers cannot disfavor employees for reasons connected to parental leave. But employers with small firms need not to motivate why they exempt a worker from the seniority rule. Employers can thereby partly circumvent the parental leave law by systematically exempting workers in such a way that they are able to dismiss pregnant women or workers on parental leave, as argued by the Swedish ombudsman of equality (1999) and the Swedish Trade Union Organization (Arbetaren (2003)).

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[13] In 2002, an extra 30 days of leave were also added.

[14] These figures do not consider unpaid leave. In a survey made by the Swedish National Insurance Board, (2004) the correlation between the number of days on parental leave and days with paid parental benefits was 0.34 for mothers and 0.65 for fathers. So a third of the days a mother spends on leave are compensated compared to around two-thirds for fathers. Comparing used days of paid leave between parents is likely to not reflect the actual division of parental leave.

[15] The number of days is measured as net days so spells lasting 3/4, 1/2, 1/4, or 1/8 of a day are recalculated into whole day equivalents.

3 Conceptual Framework

3.1 The Direct Effect

Employment protection is often justified on the basis that it protects employees from income fluctuations. But employment protection comes with side effects. Theoretically, employment protection increases a firm's adjustment cost and thereby the matching of employers and employees. With rigid wages, the adjustment cost leaves some unproductive employer-employee matches unresolved and hinders some productive employer-employee matches to occur. Hence, employment protection dampens job flows and worker flows on the labor market and can thereby affect the employment level (see, for example, Hopenhayn and Rogerson (1993), Mortensen and Pissarides (1999), Pries and Rogerson (2005)). Besides these outcomes, employment protection can affect productivity by distorting firms' choice of capital and labor in their production process (Wasmer (2006) and Autor et al (2007)), by providing workers with incentives to accumulate firm-specific human capital (Mortensen and Pissarides (1999)), and by influencing workers' absence behavior (Ichino and Riphahn (2005), Lindbeck et al (2006), Olsson (2009) and Jacob (2010)).[16]

The reform of the seniority rule in 2001 reduced the adjustment cost for small firms. I expect the lower adjustment cost to affect the use of childcare among workers in targeted firms in two ways: i) by sorting of workers that changes the composition of the workforce in small firms, and ii) by changing the behavior of existing workers. A sorting effect arises if workers in small firms who usually use paid childcare or intend to use paid childcare move to employers not entitled to exempt workers from the seniority rule. If workers who move from a firm use more leave than the average worker at that firm, the average use of paid childcare decreases at the firm. A decrease of the average use of childcare occurs also if employers make use of the exemption to dismiss working parents that use more leave than the average worker.[17] It could also be the case that workers who usually do not use paid childcare move to small firms with the result that the average use of paid childcare drops. Moreover, when

[16] Lazear (1990) shows theoretically that inefficiency can be offset by specifying employment contracts to have an ex ante payment from the worker to the employer.

[17] But going from a small firm to a large firm means that a worker will be the last worker at the new workplace. Switching employers may thereby increase the dismissal risk in the short run.

dismissals become easier, workers previously rejected on the basis of their current or expected future parental status can be employed (see Kugler and Saint-Paul (2004) for a general discussion on employers' screening behavior and employment protection). This may raise the average use of paid childcare among employees. The overall effect from sorting of workers is ambiguous: the sorting effect increases the average use of paid childcare if the effect from less rigorous screening dominates, and negative otherwise.

A softer employment protection makes a worker's productivity and continued employment more closely linked to each other and can thereby affect the worker's absence behavior. A lower protection means that an absence period is more likely to terminate an employment – the cost of an absence period rises. Costs associated with absence can vary with the job characteristics: if the job involves complementarities between co-workers, the absence of one worker affects other workers' productivity; unexpected absence can be extra costly when a substitute is hard to find at short notice. The type of absence sends different signals to employers, paid childcare can reveal how a worker values an employment relation relative to the family while sickness absence reveals information on an individual's health status. The signaling cost of absence may vary with social norms. Albrecht et al (1999) present empirical results that can be interpreted as if parental leave by fathers signals a lack of career commitment while parental leave by mothers contains no such information. Bygren and Duvander (2006) find that workplace norms affect parents' use of parental leave, especially among fathers, and a key finding is that men at workplaces where men tend not to use parental leave are less likely to use parental leave themselves.

3.2 The Indirect Effect

In a collective family model, the intra-household distribution of parental childcare depends on parents' relative bargaining power (see, for instance, Blundell, Chiappori, and Meghir (2005)). Distributional factors can change parents' relative bargaining power and are defined as factors that influence the decision process within a household without having an effect on parents' preferences or their joint consumption set (Browning and Chiappori (1998)). Examples of distribution factors are legislations and relative income changes. Parys and Schwerhoff (2010) use a collective family model to analyze how parental leave is divided within a household and show that a distribution factor that increases one of the partner's bargaining power leads to shorter parental leave by that partner and longer leave by the other partner if the level of parental leave is kept constant.[18]

The cost, in terms of dismissal risk, for being absent depends on the level of employment protection. A worker with strong employment protection is less likely to be dismissed and miss out on future income if absent in comparison to a worker with weak employment protection. Hence, parents' relative cost of providing paid childcare depends on their relative employment protection. The reform of the seniority rule can thereby be viewed as a distribution factor potentially altering the intra-household distribution of parental childcare in families with treatment variation.[19] In these families, the reform increased the cost for providing childcare for the treated partner, but had no direct impact on the cost for the untreated partner. A reallocation of care from the treated partner to the untreated partner allows the treated partner to react to the softer employment protection without causing an external effect on the child. A redistribution of care is particularly motivated in economic terms if the treated partner is the main income provider, since then, the expected cost for being absent is relatively higher for the family compared to when the treated partner is the secondary income provider.

4 Data Description and Empirical Strategy

4.1 Data Description

I base the analysis on linked employer-employee data from Statistics Sweden that holds information on all registered employees in Sweden. I analyze a yearly balanced panel containing 302 766 individuals employed in firms with two to 50 employees between the years 1997 and 2003. The data contain information on an individual's sex, age, childbearing, whether being parent of a child under eight, industry code of the firm, and county. Monthly information on firm size, in terms of the number of employees, is collected from the Swedish employment register (*Anställningsregistret*). To provide a measure that reflects the whole year, I define the size of a firm as its average monthly size during the year. A person who is

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[18] Another approach is found in Amilon (2007) where a non-cooperative Stackelberg model is used to explain spouses' use of temporary parental leave.

[19] Considering the reform as a distribution factor presupposes that the reform did not affect wages in small firms.

employed by more than one firm during a year is matched to the firm that the highest annual wage earning came from.

I then add individual information on parental leave and temporary parental leave from the Swedish National Insurance Board. These data include start and end dates for all spells in Sweden during the given period. I measure parental leave and temporary parental leave in two ways: i) the yearly incidence defined as the probability of having at least one paid spell during a given year, and ii) the total days of paid leave for spells started in a given year.

The use of paid parental leave may underestimate the reform effect for total days of parental leave. Uncompensated parental leave can be used to prolong the leave beyond the given days of benefits. In such cases, paid parental leave underreports the actual number of days on parental leave. If the reform implied a reduction of uncompensated parental leave rather than compensated parental leave, the use of paid parental leave underestimates the total effect.[20] The reform effect for parental leave can be overestimated if the earmarked daddy days that were introduced in 2002 affected fathers in the treatment group and the control group differently. But, it is common for fathers to take parental leave during regular vacation – a pattern that should not be altered by a higher dismissal risk. If anything, I expect the bias from the earmarked daddy days to occur in 2003 at the earliest since fathers do, on average, start their parental leave when the child is older than one year (Swedish National Insurance Board (2004)).[21]

4.2 Empirical Strategy

In my empirical strategy, I utilize the exemption in the seniority rule introduced in 2001 to examine if employment protection influences working parents' provision of childcare. The reform created within country variation in the level of employment protection as it was targeted towards firms with two to ten employees. Thus, I can use a difference-in-difference

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[20] The correlation between paid days of parental leave and the actual number of days on parental leave is stronger for fathers. The estimate of the reform effect on total days of parental leave might therefore be more biased for mothers (Swedish National Insurance Board (2004)). For temporary parental leave, there are no reasons for not drawing full benefits while caring for the sick child.

[21] I have tested for differential effects from the daddy month reform in small and large firms. The effect does not seem to vary with the level of employment protection. A potential explanation is that leave can be taken during vacation in order to not displease an employer.

estimator to examine if a softer employment protection affects parents' willingness to provide childcare. The regression version of the difference-in-difference estimator has the following form:

$$Y_{igt} = \alpha + \lambda_t + \beta X_{it} + \pi D_{gt} + \delta\left(D_{gt} \times Post_t\right) + \varepsilon_{igt} \qquad (1)$$

where Y_{igt} is the average outcome for individual i in group g at time t, λ_t represents time effects, X_{it} includes individual covariates, D_{gt} indicates whether a person belongs to the treatment group at time t, $Post_t$ takes the value of one for periods from 2001 and onwards, and zero otherwise. The interaction term $D_{gt} \times Post_t$ represents the difference-in-difference estimator so δ captures the average treatment effect under the assumption that the trends in average childcare would have been the same in treated and control firms if the reform had not happened.

The treatment group consists of individuals employed in firms with two to ten employees and the control group consists of individuals employed in firms with eleven to 50 employees.[22] The upper limit of the control group is based on two arguments: i) the pre-reform trend for temporary parental leave and parental leave is similar in the control group and the treatment group, and (ii) when I use a balanced panel I implicitly assume that the reform had the same effect on the probability of staying in the panel for employees in the treatment and the control group.[23] A high upper limit of the control group allows individuals to switch employers or be employed in firms that grow over time without being excluded from the sample – an important feature for the behavioral analysis.

Treatment is a function of firm size and it is consequently endogenous. Workers can seek employment in treated or untreated firms and firms can adjust the number of employees by hiring or separating workers. The composition of the workforce in small firms may therefore have changed via a sorting effect when the employment protection was relaxed.[24] The

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[22] The treatment group is also referred to as small firms and the control group as large firms.

[23] For the repeated cross-sectional sample, the difference-in-difference estimate for the probability of being in the sample the following year is insignificant (the estimate is –0.0009 with a standard error of 0.0007) when comparing the period before and after the reform.

[24] The probability for a small firm to enter or exit from the market was not changed by the reform in 2001 (von Below and Skogman Thoursie (2010)).

difference-in-difference estimator in equation (1) captures both a behavioral effect and a sorting effect. A sorting effect is interesting on its own but will bias the estimate of the behavioral effect. Separating the two effects is difficult, but under the assumption that the effects are additive separable and that the treatment effect is constant, the behavioral effect is isolated by an instrumental variable approach, as done by Lindbeck et al (2006). The sorting effect is then the difference between the total effect and the behavioral effect. Treatment status in the spring of 2000, a period when the reform had not yet become public information, is arguably a valid instrument for treatment status in later years. The instrument is correlated with later treatment status but uncorrelated with later outcomes caused by the reform since an individual in the spring of 2000 did not know if the reform was to be implemented or not.[25] The instrumental version of the difference-in-difference estimator identifies the behavioral effect for workers employed in small firms in the spring of 2000.[26] The behavioral effect can be expressed as a Wald estimator of the following form:

$$\frac{\left(E\left[\overline{Y}_T^A \middle| Z=1\right]-E\left[\overline{Y}_T^B \middle| Z=1\right]\right)-\left(E\left[\overline{Y}_C^A \middle| Z=0\right]-E\left[\overline{Y}_C^B \middle| Z=0\right]\right)}{\left(E\left[D^A \middle| Z=1\right]-E\left[D^A \middle| Z=0\right]\right)} \tag{2}$$

where Z is the treatment status in the spring of 2000 and $\overline{Y}_g^t$ the average outcome for group g={Treated (T), Control (C)} at time t ={After reform (A), Before reform (B)}. D^g represents the share of treated at time t. The numerator identifies the intention-to-treat effect, which keeps the composition of the control group and the treatment group as of the spring of 2000 constant over time. The intention-to-treat effect attenuates the behavioral effect if people switch treatment status over time. Dividing the intention-to-treat effect with the difference in average post reform compliance rates for individuals who in the spring of 2000 belonged to the treatment group and the control group identifies the behavioral effect. In total, 82 percent of those that belonged to the treatment group in the spring of 2000 were treated at

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[25] Recall that the reform was an outcome of an unexpected collaboration between the Green Party and the non-socialist parties and was publicly discussed during the spring of 2000. The instrumental variable model will estimate a local average treatment effect. Treatment status in the spring of 2000 is arguably a valid instrument as the reform became public information in mid 2000 and that matching on the labor market is not instantaneous.

[26] The sorting effect can be identified under the assumptions that the sorting and behavioral effects are additive separable and that the treatment effect is constant. In that case, the sorting effect is the difference between the overall effect and the behavioral effect.

least one year after the reform while 18 percent of those who belonged to the treatment group in the spring of 2000 were treated at least one year after the reform.

The standard error of the difference-in-difference estimate can be too small if intra-class correlation is not taken into account (Moulton (1986)). Donald and Lang (2007) show that collapsing data on a group-year level can be an efficient way of accounting for intra-class correlation. The error term in equation (1) is assumed to have an individual random error component (υ_{igt}) and a group-specific error component (η_{gt}):

$$\varepsilon_{igt} = \upsilon_{igt} + \eta_{gt} \tag{3}$$

A group-time aggregated model is efficient if the underlying common group errors are normally distributed, an unproblematic assumption if the number of individuals in each group is large. To control for individual variation and still use group-time variation, I apply the so-called two-step approach described by Angrist and Pischke (2009). In a first step, the group-year effects are purged on individual variation by estimating:

$$Y_{igt} = \mu_{gt} + \beta X_{it} + \upsilon_{igt} \tag{4}$$

where $\mu_{gt} = \lambda_t + \alpha D_{gt} + \delta(D_{gt} \times Post_t) + \eta_{gt}$. This adjusts the group-year effects, $\hat{\mu}_{gt}$, for individual variation included in X_{it}. In a second step, I difference the adjusted group-year effects between small and large firms and estimate the following model:

$$\Delta\hat{\mu}_t = \kappa + \delta Post_t + \Delta u_t \tag{5}$$

where $\Delta\, \mu_{gt} = \hat{\mu}_{Tt} - \hat{\mu}_{Ct}$ and $\Delta u_t = (\eta_{Tt} + (\hat{\mu}_{Tt} - \mu_{Tt}))$- $(\eta_{Ct} + (\hat{\mu}_{Ct} - \mu_{Ct}))$. The model deals with all types of arbitrary correlation within group and year and the estimation is based on seven time observations and is equivalent to a group fixed-effect model. A somewhat modified version of equation (5) is used for the Wald-estimator:

$$\Delta\frac{\hat{\mu}_t}{W_j} = \kappa + \delta Post_t + \Delta u_t \tag{6}$$

where $\Delta\frac{\hat{\mu}_t}{W} = \frac{\hat{\mu}_{Tt} - \hat{\mu}_{Ct}}{W}$, treatment (T) is defined as being employed in a small firm in the spring of 2000, control (C) is defined as being employed in a large firm in the spring of 2000, and W represents the difference on the post average compliance rate for workers that belonged to the treatment or the control group in the spring of 2000.

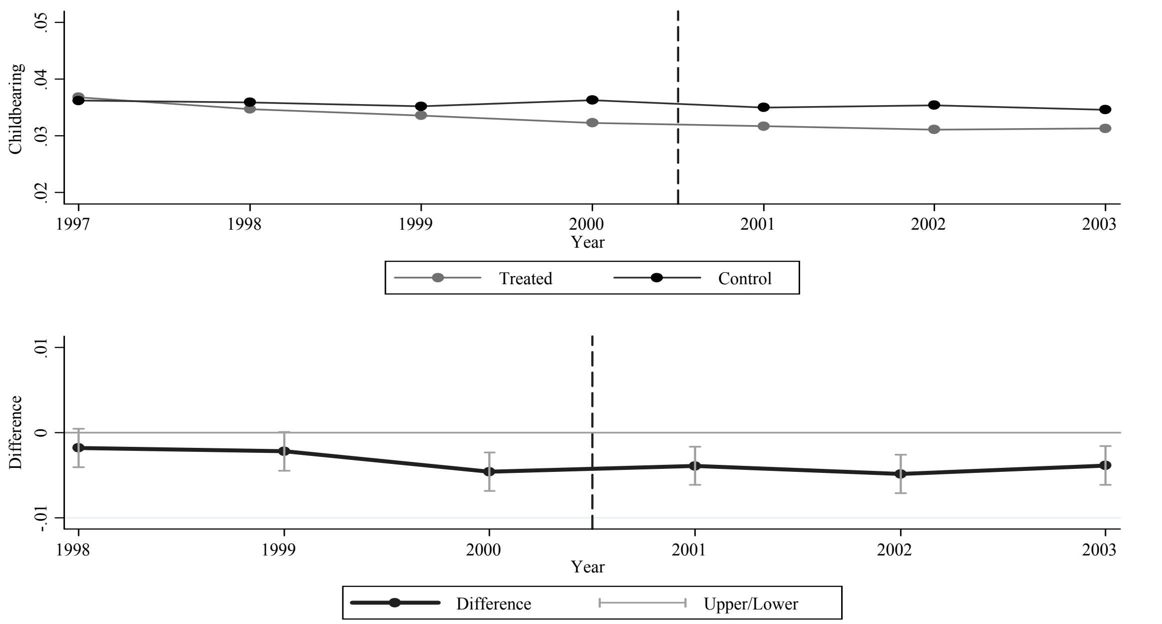
Table 2 Summary statistics. Average values for employees in treated and control firms before and after the reform.

	Treated firms		Control firms		
	After	Before	After	Before	DiD
Age	45.378	41.576	44.317	40.997	0.482***
	(11.220)	(11.227)	(11.209)	(11.266)	(0.032)
Female	0.303	0.302	0.303	0.304	0.002
	(0.460)	(0.459)	(0.460)	(0.460)	(0.001)
Parents with child aged<8	0.202	0.245	0.217	0.246	-0.013***
	(0.401)	(0.430)	(0.412)	(0.430)	(0.001)
Parents	0.777	0.726	0.754	0.701	-0.001
	(0.416)	(0.446)	(0.431)	(0.458)	(0.001)
Parental leave: incidence	0.080	0.063	0.098	0.075	-0.006***
	(0.271)	(0.243)	(0.297)	(0.263)	(0.001)
Parental leave: total days	7.455	6.719	9.062	7.583	-0.372***
	(48.899)	(48.438)	(53.818)	(50.999)	(0.087)
Temporary Parental leave: incidence	0.133	0.129	0.167	0.158	-0.005***
	(0.340)	(0.335)	(0.373)	(0.365)	(0.001)
Temporary Parental leave: total days	0.864	0.810	1.056	0.955	-0.047***
	(3.574)	(3.377)	(3.918)	(3.591)	(0.010)
Childbearing	0.031	0.034	0.035	0.035	-0.002***
	(0.174)	(0.182)	(0.184)	(0.186)	(0.001)
Observations	370 985	529 531	537 313	681 533	

Notes: Standard deviations in parentheses except for difference-in-difference (DiD) estimates where robust standard errors are displayed. ***, **, * denote statistical significance at the 1, 5 and 10 percent levels using critical values for 7 degrees of freedom to mitigate any intra-class problem. Treated firms refer to firms with two to ten employees and control firms refer to firms with 11 to 50 employees. The pre-period covers the years 1997 to 2000 and the post-period the years 2001 to 2003.

Table 2 presents summary statistics for the treatment group and the control group for the pre-period of 1997 to 2000 and for the post-period of 2001 to 2003. The balanced panel implies that I examine well-established workers mainly in the private sector since the focus is on small firms. This is reflected in the low share of females. In the pre-period, the average age, the share of females and average childbearing are similar in the two groups, while the use of parental leave and temporary parental leave is higher in the control group. Comparing the pre-period and the post-period, the share of employees with small children and employees who are childbearing becomes relative smaller in the treatment group while the average age of an employee in the treatment group increases.

Figure 2 Average childbearing for employees in treated and control firms from 1997 to 2003 The figure in the upper panel displays average yearly childbearing in absolute levels. The figure in the bottom panel displays the yearly difference in childbearing between treated and control relative to the group difference in 1997. For each difference, a 95 percent confidence interval has been calculated using a critical value of seven degrees of freedom. Treated refers to firms with two to ten employees and control refers to firms with eleven to 50 employees.



Compositional changes can bias the estimate of a behavioral effect. The regression version of the difference-in-difference estimator represented by equation (5) allows me to control for observed compositional changes as long as I do not control for an outcome itself. The identification of a behavioral effect requires the trends in childbearing to be constant between the treatment group and the control group. But childbearing might, in itself, change in reaction to less employment protection. The upper panel of Figure 2 plots the share of workers in the treatment group and the control group that got at least one child during a given year from 1997 to 2003. No sign of a reform effect on childbearing is seen, a conclusion that becomes more evident if I analyze yearly childbearing effects; see the bottom panel of Figure 2. No change in childbearing is seen at the reform year or later, but a relative drop occurs between 1999 and 2000 in small firms. The drop is not an outcome of the 2001-reform, as it became public information in the mid 2000. But the drop can bias the estimate of the reform effect since the impact from a change in childbearing can influence parental leave and temporary parental leave for several years. Given that childbearing is clearly not affected by the reform, see Figure 2, I control for childbearing in the upcoming analysis to guarantee that the results for parental leave and temporary parental leave are not driven by the drop in childbearing between 1999 and 2000.

5 Results

5.1 The Direct Effect

A crude means comparison of parental leave and temporary parental leave in Table 2 provides a first indication that employment protection influences parental childcare. In the years following the softening of the employment protection, the average used days of paid parental leave decreases by 0.37 days per year in small firms. In relative terms, the decline of 0.37 days corresponds to a reduction of 5.5 percent compared to the pretreatment average. Temporary parental leave drops by 0.05 days on average, a relative decline of 5.8 percent. The results from Table 2 also suggest that the incidence of parental leave and temporary parental leave declines in reaction to a softer employment protection.

To quantify the reform effect, I use a difference-in-difference estimator given by equation (5) for the total effect and by equation (6) for the behavioral effect. A comparison of pre-reform outcomes for the treatment group and the control group is the only way of evaluating the crucial assumption of a parallel trend in the absence of treatment for the difference-in-difference estimator (Card et al (2011)). Similar trends in years prior to the reform tell that economic shocks have affected the groups similarly in the past, indicating that the control group can serve as the counterfactual trend for the treatment group also after the reform. Figure 3 plots the average use of paid parental leave in the treatment group and the control group for the period 1997 to 2003. The yearly incidence is displayed in the top panel and total days of compensated parental leave in the bottom panel. All figures use normalized data purged on individual variation in childbearing and refer to total changes (compositional changes plus behavioral changes). Sub-figures in the right-hand column display yearly effects with a 95-percent confidence interval where the group difference in 1997 is used as the base year. Looking at the trends, a subtle statistically significant relative drop in the incidence and the total days of leave is seen in small firms in 2001. The new lower level is sustained for the whole post period. What is more, the pre-trends for the treatment and the control group are similar – indicating that the decline in childcare stems from a higher dismissal risk induced by the reform in 2001.

Figure 3 Average usage of parental leave in treated and control firms from 1997 to 2003
"Incidence" refers to the probability of having at least one spell in a year. "Year Effects" refers to the yearly group difference relative to the group difference in 1997 and are displayed with a 95 percent confidence interval using seven degrees of freedom. "Treated" refers to firms with two to ten employees and "control" refers to firms with eleven to 50 employees.

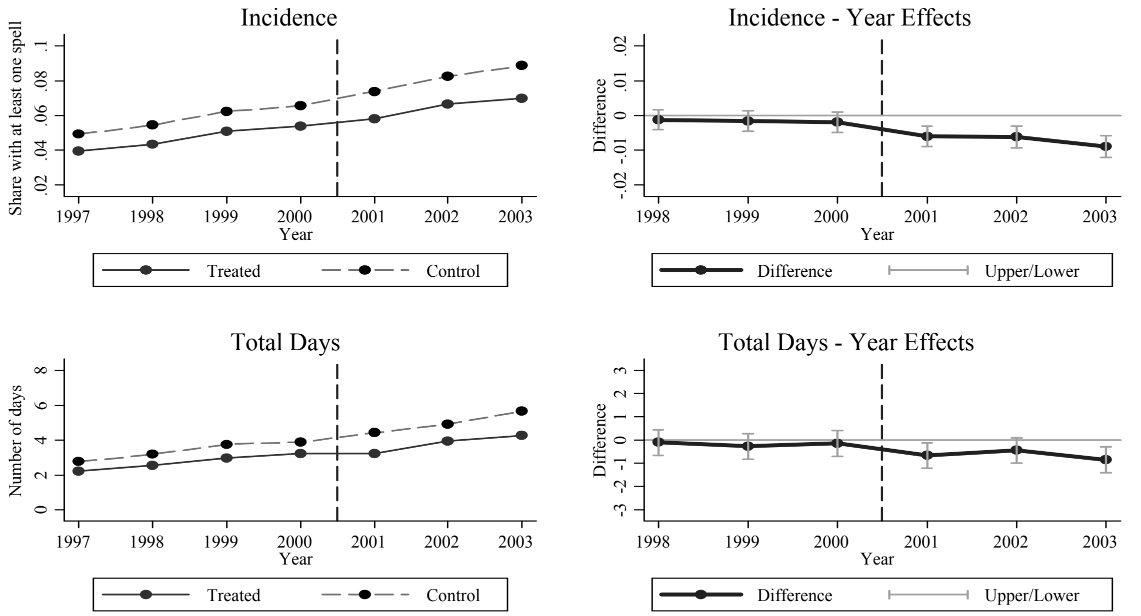
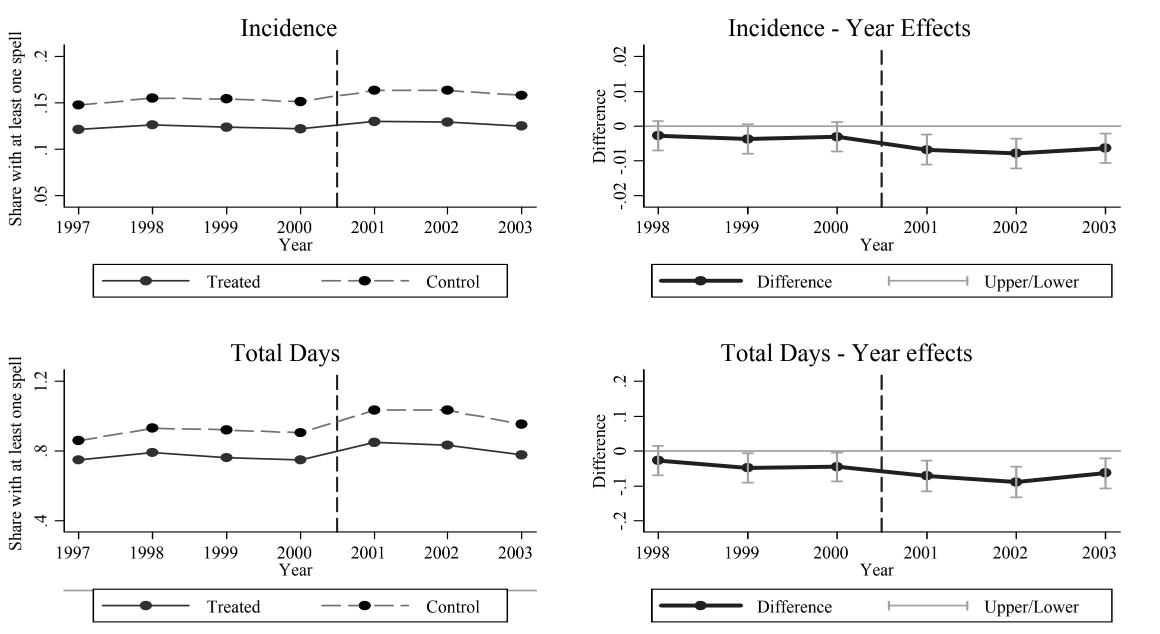


Figure 4 displays the use of temporary parental leave for employees in small and large firms between 1997 and 2003. Once again, the groups show parallel trends before the reform, which suggests that the subsequent drop of childcare in small firms at the year of the reform stems from softer employment protection.

Figure 4 Average usage of temporary parental leave in treated and control firms from 1997 to 2003 "Incidence" refers to the probability of having at least one spell in a year. "Year Effects" refers to the yearly group difference relative the group difference in 1997. All differences are displayed with a 95 percent confidence interval using seven degrees of freedom. "Treated" refers to firms with two to ten employees and "control" refers to firms with eleven to 50 employees.



Difference-in-difference estimates for the reform effect on parental leave are displayed in Table 3. The probability of having at least one spell of parental leave in a year is estimated to decrease by 0.6 percentage points in small firms in reaction to the reform.[27] The estimate of −0.6 percentage points refers to a combined sorting effect and behavioral effect and is found among both men and women. If the effect is compared to the pretreatment average, the effect corresponds to a decrease of 9.5 percent. When I control for childbearing, age of workers and the share of workers with small children, the effect diminishes to −0.2 percentage points but is still significant. For the intention-to-treat effect, the composition in the treatment and the control group is kept constant as of the spring of 2000. The intention-to-treat effect on the incidence is −0.3 percentage points and in line with the estimate for the behavioral effect of −0.4 percentage points. The estimate for total days of parental leave indicates a decline of

[27] The total effect refers to a combined sorting and behavioral effect. The model controls for childbearing to account for the relative drop in childbearing in small firms in 2000.

0.515 days in reaction to the softer employment protection (the decline is 0.552 days for women and 0.404 days for men). The estimate for the behavioral effect is similar. A reduction of 0.515 days is equivalent to a reduction of 7.7 percent as compared to the pretreatment average.

Table 3 Parental leave. Average reform effects.

	Incidence			Total days		
	All	Women	Men	All	Women	Men
	(1)	(2)	(3)	(4)	(5)	(6)
Total effect controlling for childbearing	-0.006***	-0.006***	-0.006***	-0.515***	-0.552**	-0.404***
	(0.001)	(0.001)	(0.001)	(0.121)	(0.173)	(0.070)
Total effect controlling for age, under8, childbearing	-0.002**	-0.003***	-0.002**	-0.316**	-0.197	0.241**
	(0.001)	(0.001)	(0.001)	(0.111)	(0.194)	(0.073)
ITT controlling for age, under8, childbearing	-0.003**	-0.004***	-0.002**	-0.360*	-0.424**	-0.179*
	(0.001)	(0.001)	(0.001)	(0.166)	(0.156)	(0.077)
IV controlling for age, under8, childbearing	-0.004***	-0.005***	-0.003**	-0.487*	-0.571**	-0.242*
	(0.001)	(0.001)	(0.001)	(0.229)	(0.210)	(0.105)
Pretreatment average	0.063	0.068	0.061	6.719	15.461	2.942
Observations	7	7	7	7	7	7

Notes: Each estimate represents a separate model. ***,**,* denote statistical significance at the 1, 5 and 10 percent levels, respectively. All models use time-group aggregated data and the two-step approach is used to control for individual varying covariates. The underlying sample contains 2 119 362. All models control for at least childbearing. Under8 indicates whether an employee has a child under the age of eight. ITT refers to the intention-to-treat estimate where treatment status in the spring of 2000 is used. The difference in the average post compliance rate in the IV model is 64 percent (82 percent of those belonging to the treatment group in the spring of 2000 were treated for at least one post year while 18 percent of those in the control group in the spring of 2000 were treated for at least one post year).

Table 4 presents reform effects for temporary parental leave. Once more, I find that parental childcare decreases in small firms after the relaxation of the employment protection: an average worker in a small firm is estimated to be 0.5 percentage points less likely to have a case of temporary parental leave during a given year after the reform and the total days of temporary parental leave are estimated to decrease by 0.045 days (a decline of 5.6 percent compared to the pretreatment average). While the incidence drops among men and women, only women are decreasing their use of total days of paid temporary parental leave. That only women's total days of temporary parental leave are reduced by the reform is further confirmed by the estimate for the behavioral effect. In sum, the above results support that both a sorting effect and a behavioral effect were offset by the softer employment protection.

Table 4 Temporary parental leave. Average reform effects.

	Incidence			Total days		
	All	Women	Men	All	Women	Men
	(1)	(2)	(3)	(4)	(5)	(6)
Total effect controlling for childbearing	-0.005***	-0.009***	-0.003***	-0.045***	-0.108***	-0.020
	(0.001)	(0.001)	(0.001)	(0.015)	(0.028)	(0.070)
Total effect controlling for age, under8, childbearing	0.001	-0.001	0.002*	-0.005	-0.041**	0.010
	(0.001)	(0.001)	(0.001)	(0.010)	(0.015)	(0.014)
ITT controlling for age, under8, childbearing	0.000	-0.002*	0.000	-0.029	-0.059**	-0.014
	(0.001)	(0.001)	(0.001)	(0.018)	(0.020)	(0.018)
IV controlling for age, under8, childbearing	-0.001	-0.002*	-0.001	-0.040*	-0.080**	-0.019*
	(0.002)	(0.001)	(0.002)	(0.024)	(0.027)	(0.025)
Pretreatment average	0.129	0.164	0.144	0.810	1.266	0.613
Observations	7	7	7	7	7	7

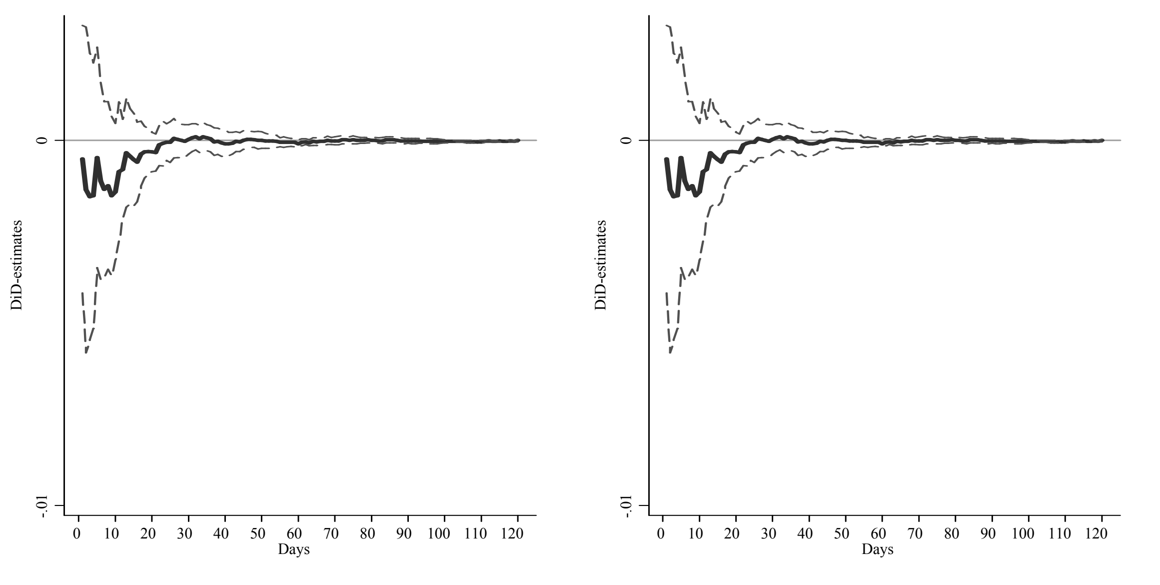
Notes: Each estimate represents a separate model. ***, **, * denote statistical significance at the 1, 5 and 10 percent levels, respectively. All models use time-group aggregated data and the two-step approach is used to control for individual varying covariates. The underlying sample contains 2 119 362. All models control for at least childbearing. Under8 indicates whether an employee has a child under the age of eight, ITT refers to the intention-to-treat estimate where the treatment status in the spring of 2000 is used. The difference in the average post compliance rate in the IV model is 64 percent (82 percent of those belonging to the treatment group in the spring of 2000 were treated for at least one post year while 18 percent of those in the control group in the spring of 2000 were treated for at least one post year)..

A higher dismissal risk can affect short and long durational spells differently. For temporary parental leave, the severity of a child's illness is to some extent reflected in the length of the spell. The duration of a temporary parental leave spell and the severity of the child's illness are likely to be positively correlated. Thus, if each time the child is sick, a parent weighs the cost for the child to not be taken care of against the cost for the parent of missing out on work, I expect the behavioral reform effect to diminish with the length of sick spells. In other words, a cold is easier to ignore for the parent compared to the measles. Durational effects are analyzed with the following linear probability difference-in-difference model:

$$P(Y_{igt} \geq s) = \alpha + \lambda_t + \pi D_{gt} + \delta_s(D_{gt} \times Post_t) + u_{igt}, \qquad (7)$$

where Y_{igt} takes the value of 1 if individual i is employed in group g at time t and has a spell of at least s days ($s=1, 2, \ldots, 120$). Year effects are represented by λ_t, the variable D_{gt} indicates treatment status in the spring of 2000 and $Post_t$ takes the value of one from 2001 and onwards (zero otherwise). δ_s represents the average reform effect on the probability that a spell lasts at least s days. The reform effect on the incidence is seen at threshold 1 (δ_1) and durational effects are seen at thresholds of higher order. I use a time-group aggregated version of equation (7) where the behavioral effect is estimated as by equation (6). The results for temporary parental leave are displayed in Figure 5. As seen for the incidence in Table 4, a behavioral effect is only estimated for women. More interestingly, the behavioral reform effect for women diminishes in general with the duration of the spell. The diminishing effect is consistent with women weighing the cost of an absence period from work relative to the cost for the child not being taken care of under the assumption that the severity of the child's illness is reflected by the length of a spell.

Figure 5 Difference-in-difference estimates for the probability that a temporary parental leave spell exceeds 1 to 120 days A 95-percent confidence interval is displayed in grey. Estimates in both figures refer to behavioral effects for employees in firms with two to ten employees. Employees in firms with 11 to 50 employees are used as the control group.



The average dismissal risk increased in firms with two to ten employees in 2001. However, in what way the individual worker's dismissal risk was affected depends on the worker's relative tenure. In a scenario with one dismissal per firm, a worker's dismissal risk was increased, decreased or remained unchanged by the reform, see Table 1. To examine differential effects, I estimate separate difference-in-difference models for groups of workers who faced an increased, decreased and unchanged dismissal risk. I calculate a worker's relative tenure in a firm in the spring of 2000 using employment information from 1995. Then, I analyze the years 2000 and 2001 with disaggregated individual data where treatment status in the spring of 2000 instruments treatment status in 2001. Analyzing just one post year has the advantage that a worker's relative tenure is more stable. Table 5 presents the results. Besides total days of parental leave, all estimates are highly significant. The largest response, both in absolute and relative terms, is seen for the group of workers who got a decreased dismissal risk; these workers were hired last, so the reform implied a greater chance to remain employed as they could be exempt from the seniority rule. In this group, the incidence of temporary parental leave drops by 30.8 percent and the total days of temporary parental leave drops by 34.8 percent in the year of the reform. A decline in temporary parental leave is also found for the group of workers where the dismissal risk increased: in this group, the incidence declines by 11.8 percent and total days declines by 15.8 percent. In the group where the dismissal risk was unchanged, the incidence decreases by 4.2 percent and total days decreases by 4.6 percent. That paid childcare drops both among those who got a decreased and increased dismissal risk can be explained by the fact that the reform strengthened the link between an employee's productivity and continuation of the employment.

Table 5 Behavioral reform effects for groups of workers for which the dismissal risk increased, decreased or was unchanged

	Parental Leave		Temporary Parental Leave	
	Incidence	Total Days	Incidence	Total Days
	(1)	(2)	(3)	(4)
Decreased dismissal risk	-0.012***	-1.422*	-0.037***	-0.279***
	(0.004)	(0.848)	(0.005)	(0.057)
Relative change (%)	-21.8	-30.9	-34.3	-41.4
Pretreatment average	0.055	4.593	0.108	0.674
Increased dismissal risk	-0.007***	-0.752*	-0.015***	-0.140***
	(0.002)	(0.454)	(0.002)	(0.028)
Relative change (%)	-11.3	-11.8	-13.0	-18.4
Pretreatment average	0.062	6.343	0.115	0.762
Unchanged dismissal risk	-0.003***	-0.641**	-0.006***	-0.042***
	(0.001)	(0.282)	(0.001)	(0.016)
Relative change (%)	-4.3	-9.2	-4,6	-5.3
Pretreatment average	0.070	6.981	0.130	0.792

Notes: Each estimate represents a separate model. ***, **, * denote statistical significance at the 1, 5, and 10 percent levels, respectively. The changes in dismissal risk are based on a scenario where a firm with two to ten employees is to dismiss one employee and each firm represents an job-task specific dismissal group. The estimates for "Decreased dismissal risk" use 376 573 observations, the estimates for "Increased dismissal risk" use 413 516 observations and the estimates for "Unaffected dismissal risk" use 520 974 observations. The relative change relates the effect to the pretreatment average.

5.2 The Indirect Effect

Do parents redistribute leave within the family if one parent gets a higher dismissal risk? I try to answer this question by examining if childcare by spouses who themselves were unaffected by the reform in 2001 changes if their partner was affected by the reform. If spousal childcare increases in reaction to the partner's higher risk of being dismissed, it indicates a redistribution of leave since the spouse was only affected by the reform via the directly affected partner. Partners are not linked in the data so I define a couple as two individuals that live in the same household, are registered as cohabiting or married and with an age difference of at most 15 years. Conditioning on within-couple variation in treatment reduces the sample size substantially. I therefore create an unbalanced panel with individuals that were at least employed in a firm with 11 to 50 employees in the spring of 2000 to estimate the behavioral effect. I then define the treatment group as spouses who were directly unaffected by the reform but who lived with a partner in 2001 who was affected by the reform. I label individuals in the treatment group as indirectly treated spouses. The control group contains partners in couples where both were unaffected by the reform in 2001. The sample contains 265 318 observations.

Table 6 presents behavioral reform effects for indirectly treated spouses. I interpret a positive effect for an indirectly treated spouse as redistribution of leave. For indirectly treated women, the incidence of parental leave increases with their partner's dismissal risk. The estimates for total days of parental leave are less precise, but indicate an indirect reform effect for both men and women. For temporary parental leave, only indirectly treated men are estimated to increase their use.

Spouses' relative bargaining power within the family can be reflected by their relative income. In that case, the preferences of the main income provider should be relatively more important for the decision to redistribute leave or not. I examine if the redistribution effect differs if the indirectly affected partner is the main or the secondary income provider. The results are displayed in Table 6.[28] A redistribution of temporary parental leave occurs only

[28] The average values for parental childcare in the post period show that men that are main income providers use more leave compared to men that are secondary income providers and is consistent with Duvander and

when the indirectly affected man is the main income provider: the total days of temporary parental leave are estimated to increase by more than 11.3 percent (0.166 days) and the probability of having at least one spell of temporary parental leave per year increases by 9.6 percent (2.8 percentage points). The opposite is found for parental leave where only a redistribution effect is found for the indirectly treated. A possible explanation is that a flexible job makes it easier to work at home while being on temporary parental leave and having a high income is associated with a more flexible job.

In couples where both partners are treated, reallocation of leave to an unaffected spouse is not possible. Still, a reallocation can be economically motivated if the expected cost from a higher unemployment risk differs between partners. Table 7 displays how individuals in couples where both partners got a weaker employment protection reacted, separately. The control group consists of partners in couples where neither got treated. Once again, I use an unbalanced sample where all individuals are at least present in the spring of 2000. The sample is divided into main and secondary income providers, so the cross diagonals for each outcome reflect the average total reaction for couples. For parental leave, a decline is found for men with a relatively high income, but no clear effect is estimated for their low-income partners, which gives no support for redistribution of leave. For temporary parental leave, low-income earners are estimated to reduce their leave. This holds for both men and women. I find no sign of reallocation of leave suggesting that the couple's total use of paid parental childcare was reduced if both partners faced a higher risk of being dismissed.

All in all, I find some evidence that the distribution of parental childcare within the family depends on parents' relative employment protection. An interpretation is that parental childcare decisions are, at least to some extent, taken in collaboration within the family and are partially based on indirectly expected economic costs (as the reform only altered indirect expected costs from absence). The results also highlight that the dual earner family can function as a social safety net by reallocating parental child care: a negative reform effect on the child was partially absorbed by reallocating childcare from an affected parent to an unaffected parent.

Sundström (2002) who also show that fathers' educational level increases the use of parental leave when earnings are held constant.

Table 6 Redistribution effects Reform effects for indirectly treated partners, overall and by relative income status**.**

	Parental leave				Temporary parental leave			
	Incidence		Total days		Incidence		Total days	
	Women	Men	Women	Men	Women	Men	Women	Men
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Indirect treated	0.011***	0.007	2.095*	0.884*	0.005	0.024***	0.123	0.125**
	(0.002)	(0.005)	(0.853)	(0.411)	(0.009)	(0.005)	(0.073)	(0.048)
Pretreatment average	0.168	0.128	39.677	6.188	0.309	0.224	2.384	1.228
Indirectly treated with high income	0.012	0.015	0.880	1.095	0.011	0.028***	0.191	0.166***
	(0.012)	(0.007)	(0.558)	(0.529)	(0.013)	(0.007)	(0.147)	(0.034)
Pretreatment average	0.099	0.142	17.545	6.762	0.270	0.239	1.966	1.275
Indirectly treated with low income	0.012***	-0.002	2.331*	-0.030	0.004	0.010	0.107	-0.062
	(0.002)	(0.009)	(1.007)	(0.874)	(0.011)	(0.007)	(0.106)	(0.217)
Pretreatment average	0.195	0.070	48.465	3.772	0.325	0.158	2.550	1.030
Observations	7	7	7	7	7	7	7	7

Notes: Each estimate represents a separate IV-model. ***, **, * denote statistical significance at the 1, 5 and 10 percent levels, respectively. All models use time-group aggregated data and the two-step approach is used to control for individual varying covariates. The underlying sample contains 265 318 observations of which 139 190 are defined as high-income observations. A person is defined as the high-income earner if the yearly wage earnings are higher than the partner's yearly wage earnings. All models control for yearly childbearing, age and whether an employee has a child not older than eight.

Table 7 Reform effects for partners in couples where both partners are treated Overall and by relative income status.

	Parental leave				Temporary parental leave			
	Incidence		Total days		Incidence		Total days	
	Women	Men	Women	Men	Women	Men	Women	Men
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Both treated	-0.005**	-0.020***	-0.336	-1.295***	-0.009***	-0.010	-0.112**	-0.109*
	(0.002)	(0.005)	(0.614)	(0.295)	(0.002)	(0.005)	(0.031)	(0.047)
Pretreatment average	0.087	0.058	21.996	3.959	0.169	0.086	1.572	0.537
High income partner	-0.004	-0.023***	-1.008	-1.304**	-0.005	-0.010	-0.090	-0.107*
	(0.004)	(0.006)	(0.935)	(0.387)	(0.007)	(0.006)	(0.069)	(0.045)
Pretreatment average	0.045	0.064	9.355	4.247	0.129	0.091	1.236	0.572
Low income partner	-0.005*	-0.007	-0.156	-1.167	-0.009**	-0.010**	-0.112**	-0.117*
	(0.002)	(0.004)	(0.696)	(0.660)	(0.003)	(0.004)	(0.037)	(0.053)
Pretreatment average	0.103	0.034	26.791	2.835	0.184	0.067	1.699	0.402
Observations	7	7	7	7	7	7	7	7

Notes: Each estimate represents a separate IV-model. ***,**,* denote statistical significance at the 1, 5 and 10 percent levels, respectively. The underlying sample contains 396 122 observations. All models control for yearly childbearing, age and whether an employee has a child not older than eight. A person is defined as the high-income earner if the yearly wage earnings are higher than the partner's yearly wage earnings.

5.3 Robustness and Further Analysis

The average effect on the dismissal risk varies with the number of employees in a firm − see Table 1. To test for firm size specific reform effects, I estimate a group time aggregated version of the following model:

$$Y_{ift} = \alpha + \lambda_t + \sum_{f=2}^{20} \pi_f D_{ift} + \sum_{f=2}^{20} \delta_f (D_{ft} \times Post_{ift}) + u_{ift} \qquad (8)$$

where Y_{ift} is the average outcome for individual i in firm f at time t, λ_t contains time effects, the vector X_{it} includes individual covariates and D_{ft} indicates whether a person is employed in a firm of size f at time t or not. $Post_t$ takes the value of one for periods from 2001 and onwards, and zero otherwise. Note that firms with 21 to 50 employees serve as the benchmark in the model. Figure 6 displays difference-in-difference estimates for the incidence and total days of parental leave and temporary leave for employees in firms with two to 20 employees. All estimates refer to the overall effect. A decline in parental childcare is estimated in smaller firms within the treatment group. A significant reduction is estimated in firms with two to six employees. In these firms, the average dismissal risk became at least 33 percent higher when one employee is to be dismissed.[29] The magnitude of the change in dismissal risk thereby seems to be important for whether the reform had an effect or not.

The construction of the control group partly relies on a high firm size threshold (50 employees) to allow individuals to flow within the balanced sample, an important feature since the reform affected hires and separations. But how important is the choice of the upper limit of the control group for the validity of my results? Figure 7 presents separate reform effects for models where the upper firm size limit of the control group increases gradually in steps of five, from 15 to 50 employees.[30] None of the previously estimated reform effects are sensitive to the selection of the upper limit of the control group.

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[29] von Below and Skogman Thoursie (2010) estimate that only firms with at most five employees reduce their hiring while the effect on separations is independent of firm size.

[30] Individual data from a balanced panel data set are used for each estimate.

Figure 6 Firm size specific difference-in-difference estimates for firms with two to 20 employees "Size" is to the number of employees in a firm. All models use individual data and standard errors are clustered at the firm level. Firms with 21 to 50 employees serve as the benchmark. "Effect" refers to firm-specific effects and "Upper/Lower" refers to a 95 percent confidence interval.

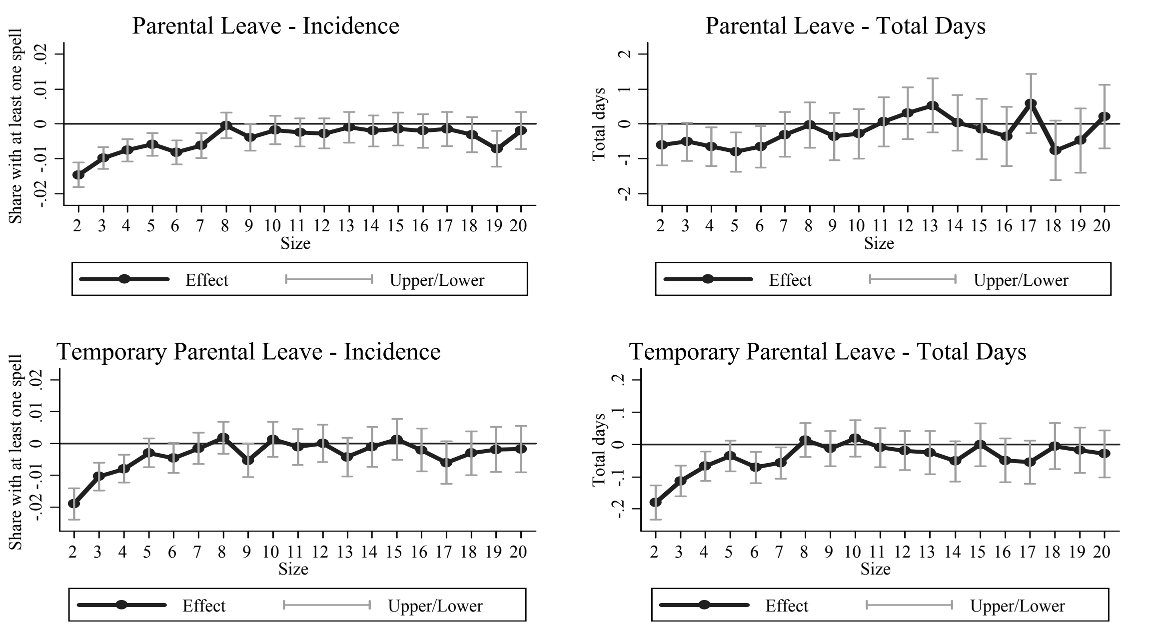
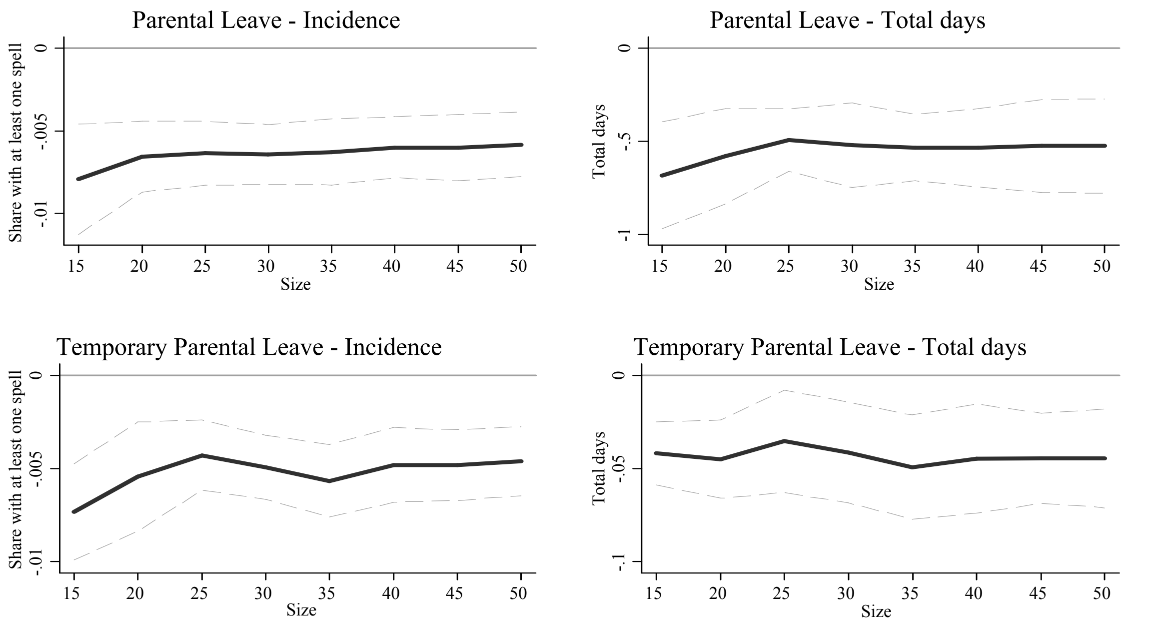


Figure 7 Difference-in-difference estimates using different control groups. "Size" refers to the upper limit of the control group in terms of the number of employees in a firm. All models use aggregated group-year data from a balanced panel data set. The dark line refers to reform effects and grey dashed lines to a 95 percent confidence interval.



The identification of the behavioral effect relies on the assumption that the treatment status as of the spring of 2000 was unaffected by the reform in 2001. The reform was discussed publicly as late as in the first half of 2000 and it was not until October in the same year that it was decided on. Treatment status in the spring of 2000 thereby seems to be a valid instrument, especially considering that matching on the labor market takes time. Nevertheless, I re-estimate the behavioral effect for parental leave and temporary parental leave using treatment status in 1997, 1998 and 1999, respectively, as instruments for later treatment status. I also restrict the sample to cover the period from the instrumental year to 2001 to minimize the sorting of workers. As I only cover one post reform year, individual variation is used to estimate different versions of equation (1) and the results are presented in Table 8. The results are in line with previous findings and I find no support that the estimate of the behavioral effect is driven by the choice of instrument.

Table 8 Behavioral reform effects using different instruments

	1999 (1)	1998 (2)	1997 (3)
Parental Leave – Incidence	-0.005*** (0.001)	-0.006*** (0.001)	-0.005*** (0.001)
Parental Leave – Total Days	-0.496* (0.257)	-0.541* (0.271)	-0.237 (0.291)
Temporary Parental Leave – Incidence	-0.006*** (0.001)	-0.006*** 0.002)	-0.006*** (0.002)
Temporary Parental Leave – Total Days	-0.048*** (0.016)	-0.040** (0.018)	-0.063*** (0.020)
Compliance Rate for Treated	0.81	0.76	0.72
Compliance Rate for Control	0.89	0.88	0.76
Observations	908 298	1 211 064	1 513 830

Notes: Each estimate represents a separate model. ***, **, * denote statistical significance at the 1, 5 and 10 percent levels, respectively. Standard errors clustered at an individual level. All models control for childbearing, whether a person has a child under the age of eight, age and sex. The model in column (1) uses treatment status in 1999 for future treatment status and analyzes the period 1999 to 2001. The model in column (2) uses treatment status in 1998 for future treatment status and analyzes the period 1998 to 2001. The model in column (3) uses treatment status in 1997 for future treatment status and analyzes the period 1997 to 2001. The compliance rate for treated refers to the share of employees that were defined as treated in the instrumental year and in 2001. The behavioral effect weights the intention-to-treat effect with (compliance rate for treated)-(1-compliance rate for control).

6 Conclusions

In this paper, I provide new evidence on how employment protection affects workers' absence behavior revealing that indirect economic incentives can have a substantial effect on how parents use and divide paid childcare between them. In Sweden, firms with at most ten employees were allowed to exempt two workers from the tenure based dismissal order in 2001. I compare paid parental childcare for workers in firms around the size threshold over time with the help of a difference-in-difference estimator. The results can be summarized as follows. The higher dismissal risk reduces the total days of paid parental leave among workers in small firms by 7.7 percent and the total days of temporary parental leave by 5.6 percent. A sorting effect and a behavioral effect can explain the reduction in childcare. The higher dismissal risk is also estimated to influence the distribution of paid childcare within some families: in families with one treated and one untreated parent, I estimate a general increase in childcare for the unaffected partner. I interpret the reallocation of leave as if parents try to avoid an external effect on the child.

I conclude that the supply of childcare by working parents is influenced by the general employment protection legislation and that the division of parental childcare within families is partly based on indirect economic incentives.

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Private Equity and Employees

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Abstract

Using linked employer-employee data from Sweden, a difference-in-difference approach, and 201 private equity buyouts undertaken between 1998 and 2004, we show that unemployment risk declines and labor income increases for employees in the wake of a private equity buyout. Unemployment risk declines despite lower employment growth for continuing establishments—attributable to hiring freezes rather than to layoffs—and a lack of change in firm level employment growth. A plausible explanation is relaxed financial constraints: the effects are strongest in industries dependent on external finance for growth, for non-divisional buyouts, and for buyouts just prior to 2001.

Keywords: Buyouts, Employment, Financial Constraints, LBO, Private Equity, Restructuring.
JEL Codes: G24, G32, G34, J20, L25

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"The question is whether or not these companies were being manipulated by the guys who invest to drain them of their money, leaving behind people who were unemployed," Gingrich said. "Show me somebody who has consistently made money while losing money for workers and I'll show you someone who has undermined capitalism. That's an indefensible model."

- *Guardian (2012) quoting Newt Gingrich.*

1 Introduction

Every morning an increasing number of employees across the world find themselves working in a firm targeted for a buyout. Buyouts are undertaken by private equity firms who buy, improve, and resell mature firms using capital invested in private equity funds. During the period 1985 to 2006, private equity firms bought corporate assets in the US at an average yearly value of approximately 1% of the total US stock market value, with a top value of 3% in 2006 (Kaplan and Strömberg, 2009).

But the spread of the buyout business model has not escaped criticism. In the wake of the financial crisis in Europe, labor unions have claimed that buyouts, through layoffs and wage cuts, generate returns to investors at the expense of employees.[1] The question whether private equity firms are job creators or job destroyers has also stirred an intense media debate in the US, as one of the candidates for the 2012 Republican Party presidential nomination, Mitt Romney, is a former private equity executive. Our opening quote from Newt Gingrich, another candidate commenting on deals undertaken by Mr Romney's team, gives a good example of commonly heard views on buyouts.[2]

In light of the costs of unemployment, to employees and society, claims of systematic layoffs should be taken seriously.[3] Recent academic evidence on the employment effects of buyouts in the US suggest modest declines in firms employment growth, but indicate internal reorganization with employment growth declines in old establishments offset by the creation of new establishments (Davis, Haltiwanger, Jarmin, Lerner, and Miranda, 2011).[4] Evidence from the UK also suggest declines in firm level employment growth, but the effect appears to be weaker for more recent buyouts than for buyouts in the 80s (Wright, Thompson, and Robbie, 1992; Amess and Wright, 2007; Amess, Girma, and Wright, 2008). Things look different in France, where buyouts provide capital to credit constrained firms and thereby spur firm level employment growth (Boucly, Sraer, and Thesmar, 2011). These studies are important for enhancing our understanding on how buyouts affect employment. Yet data limitations have prevented

[1] See, for example, FSA (2008); ITUC (2007); PSE (2007).

[2] For more on the debate, see for example The Economist, Jan 28th 2012, "Monsters, Inc?"; The Economist, Jan 28th 2012, "Bain or blessing?"; Financial Times, Jan 10th 2012,"Video attacks Romney's record"; Financial Times, Jan 13th 2012, "The bane of Bain"; or The Wall Street Journal, Jan 9th 2012, "Romney at Bain: Big Gains, Some Busts".

[3] Evidence suggests that unemployment can lead to, among other things, wage cuts after accepting a new job offer, a consumption reduction and loss of income as well as a general decline in happiness. See, e.g.,Farber (2005), Katz and Mayer (1990), Jacobson, LaLonde, and Sullivan (1993), Gruber (1997) and Di Tella, MacCullock, and Oswald (2001).

[4] Declines in firm level employment growth rates are consistent with other papers utilizing data for shorter time spans (Kaplan, 1989; Muscarella and Vetsuypens, 1990; Lichtenberg and Siegel, 1990).

them from pinning down a central question in the heated debate: what happens to individual employees? The question cannot be fully answered using data at the establishment or firm level. Take the observed declines in employment growth as an example. These could be due to layoffs (hurting employees) or to natural attrition and reductions in hirings (not affecting employees). Given the growth of the buyout industry, the extensive media coverage, and the global political consequences of the debate, evidence on how individual employees are affected is of general interest.

In this paper, besides performing analyzes of employment growth at the firm and establishment level, we go beyond previous studies by evaluating the effects on individual employees' unemployment risk and labor income. We base our analysis on 201 buyouts undertaken between 1998 and 2004 in Sweden. Sweden has an active private equity market (with over a third of all buyouts undertaken by foreign private equity firms), and rich linked employee-employee data is available. Additionally, access to population data allows the construction of a control group of employees not affected by a buyout using detailed demographic data.

We present three novel findings.

1. The yearly unemployment risk is reduced by 1.1 percentage points or 12.7% on average for four years after the buyout. Yearly labor income increases by 3734 SEK or 1.4% on average over the same period. The labor income increase is not driven entirely by the lower unemployment risk in itself and occurs across most quartiles of the labor income distribution.

2. The effect on firm and establishment level employment growth are similar to effects observed in the US and the UK, but not those observed in France. The cumulative four year difference in employment growth rate between treated and control establishments is -6.0 percentage points in Sweden compared to -5.4 percentage points reported for the US in Davis et al. (2011). The point estimate for the average establishment employment growth rate in the four years following a buyout is -1.2 percentage points. The decline in establishment employment growth rate is driven by reduced hirings rather than by increased layoffs. In line with previous evidence from Sweden in Bergström, Grubb, and Jonsson (2007), we find no statistically significant effects on firm level employment growth.

3. A plausible explanation for the reduction in unemployment risk is that the buyout improves access to capital for financially constrained firms. The reduction in unemployment risk is mostly concentrated to industries dependent on external capital to grow, non-divisional buyouts in which the target firm is more likely to be financially constrained, and buyouts undertaken just prior to the economic slowdown following the IT stock market crash in 2001. Labor market regulations do not seem to restrict private equity firms from firing employees. On the contrary, employees with softer protection are more likely to benefit from a buyout.

These findings are based on a difference-in-difference estimator identifying the effects from variation between a treated group and a control group of employees over time. The central assumption behind the difference-in-difference estimator is parallel trending in the absence of treatment. Then, the trend in the outcome of the control group serves as the counterfactual outcome and the estimated effect can be interpreted as causal. To ensure parallel trends, a good control group is essential. We match treated employees with similar employees not affected by a buyout using detailed information on current and historic observable characteristics such as sex, age, skill level, income, unemployment incidence, geographical location and firm/establishment characteristics. To address any remaining concerns of potential selection on unobservables, we perform two robustness checks. We estimate a staggered treatment model using employees in firms who have been or will be affected by a buyout as controls and, construct an alternative control group of employees who are affected by strategic acquisitions. Overall, the sign and the statistical significance of the average estimated effect remains constant indicating that the estimates in our main specification can be interpreted as causal.

Our paper contributes to the literature on the employment effects of buyouts by going beyond existing firm and establishment level studies to focus on the effects on individual employees. Moreover since most benefits to employees occur when financial constraints are important, our paper also offers a contribution to the more general literature on finance and labor which has not yet studied how financial constraints affect individual employees' unemployment risk.[5]

The next section presents a theoretical background and Section 3 discusses our data and identification strategy. Our main results are presented in Section 4 and we discuss interpretations of our results in Section 5. Additional analysis and robustness checks are available in an online appendix.[6]

2 Theoretical Background

In a foundational paper on the role of buyouts, Jensen (1989) argues that a private equity firm – or leveraged buyout association – is an organizational form superior to the public corporation because it is designed to reduce agency problems between dispersed owners and the manager of the firm. Dispersed ownership allows managers to avoid hard and unpopular tasks such as firing employees and reducing wages. Without careful monitoring and the right incentives, managers can engage in empire building by hiring too many employees, acquiring too many companies, or diversifying activities too much. A buyout could reduce these problems since private equity firms concentrate ownership, implement pay-for-performance schemes, and increase leverage (Leslie and Oyer, 2009). But an increase in leverage is also accompanied with an increase in bankruptcy risk (Andrade and Kaplan, 1998; Strömberg, Hotchkiss, and Smith, 2011), and slimming the organization to get rid of slack could involve layoffs and a reduction in wages. Indeed, a motivation for hostile takeovers could be to capture value from employees through breach of implicit contracts between managers and employees (Shleifer and Summers, 1988). As Lazear (1979) points out, moral hazard can make it optimal to pay employees a lower wage than the value of their marginal productivity early in their careers and a wage higher than the value of their marginal productivity later in their careers. If writing an explicit contract is not possible, employees and managers can implicitly agree on wages increasing with tenure. Such

[5] For more aggregated empirical studies on the connection between financial constraints and labor markets see, for example, Caggese and Cuñat (2008), Matsa (2010), Benmelech, Bergman, and Seru (2011) or Pagano and Pica (2012) and the references therein.

[6] See http://www.ifn.se/joacimt

agreements can be broken after an ownership change. If wages are hard to reduce, due to labor market regulations or collective agreements, dismissing employees with longer tenure being paid a wage higher than the value of their marginal productivity is optimal. Thus, cost cutting, a high debt load, and breach of implicit contracts can lead to *increased unemployment risk*.

But there are also reasons to why buyouts can benefit employees. As several authors have pointed out, private equity firms are, today, more oriented towards operational improvements and helping firms grow than cost cutting.[7] Boucly et al. (2011) argue that buyouts can be a substitute for other sources of capital and thereby accelerate firm level employment growth if firms are financially constrained prior to the buyout. There are several reasons to expect a buyout to relax financial constraints at the firm level. For example, private equity firms have connections and experience in dealing with banks; they are good at monitoring the firms so banks are more willing to lend; and they are more likely to reinvest earnings rather than pay out dividends because of tax reasons and a focus on the exit. If better access to capital makes the firm more resilient to negative profitability chocks and allows new investments to be undertaken, buyouts can lead to *decreased unemployment risk*.

It remains an empirical question whether the negative effects on employees from cost cutting and a higher debt load outweighs the positive effects of relaxed financial constraints.

3 Data and Identification

3.1 Sample construction

To study if the negative effects on employees from cost cutting and a higher debt load outweighs the positive effects of relaxed financial constraints we create a comprehensive data set on buyouts and employees. We use two sources of information on buyouts: the Capital IQ database and buyouts identified in Bergström et al. (2007). Our starting point is transactions in the Capital IQ database with the target's geographic location being Sweden and the announcement date being between 1998 and 2004 (10 397 transactions). From there on, we use similar selection criteria as Strömberg (2008). We select all transactions having secondary transaction features tagged as "Leveraged Buy Out" or "Management Buyout" and those having buyers/investor stage of interest tagged as "Buyout".[8] We then keep transactions marked as "Closed" or "Effective" and remove transactions involving minority stakes or which are private investments in public equity.[9] To this list we add buyouts identified in Bergström et al. (2007) that Capital IQ did not record (39 transactions) providing us with a sample of 322 buyouts.[10]

[7] See, for example, Boucly et al. (2011) or Kaplan and Strömberg (2009).

[8] Capital IQ defines a leveraged buyout as follows: "This feature is assigned when a financial sponsor acquires a mature business by combining equity with debt, raised by leveraging the business. This is only applicable: [i)]To strategic buyer transactions when it is explicitly mentioned in the press release. [ii)]To transactions where a majority stake is being acquired (i.e. 50% or more). "

[9] Capital IQ defines a transaction as closed when the transaction has been closed, but no hard information is available on whether it is effective. An effective transaction is a transaction that has been closed and where Capital IQ has found information that it is also effective. In practice, all closed transactions should be effective unless the transaction is recent.

[10] Bergström et al. (2007) describe their sample of buyouts as follows: "Our sample contains all private equity sponsored exits with a deal value of over $5 million exited in the period 1998 to the first half of 2006. The sample is further limited to deals where at least one of the private equity sponsors in the investor syndicate

We use the IFN Corporate Database containing information on names and the registration number for all firms in Sweden to add the firm registration number to each buyout.[11] Since Capital IQ only gives us the name of the target firm, we manually match names from Capital IQ to names and firm registration numbers in the IFN Corporate Database. After this procedure, we are left with 255 buyouts with firm registration numbers. The most likely reason for why we fail to find registration numbers is that the firm has changed its name or that it is not registered as a Swedish limited liability corporation.

Correcting for the group structure of limited liability corporations in Sweden allows us to keep track of majority ownership of firms by other firms. For example, a buyout can take place in a firm that is a holding company with majority ownership in several other firms (who in turn can own other firms). If we do not correct for the group structure, the buyout would show up as affecting zero employees since all employees are registered as working in firms owned by the holding company.

We use the IFN Corporate Database to obtain information on the ownership structure of firms in Sweden (information is available for 1997-2007). We take the date the buyout was announced and apply last year's ownership structure to the buyout if the ownership structure was reported before the first of November and this year's ownership structure if the buyout was reported after the first of November. We use the first of November as the basis for our merge because the employer-employee link is made for the first of November each year and because we want to ensure that employees are not treated before the buyout is announced. We then mark firms as being part of a buyout if they were directly or indirectly majority owned by the targeted firm. If there are two buyouts in the same firm registration number in a given year, we drop the second buyout making our sample unique on firm registration number and year.

Using the firm registration numbers we can identify employees affected by buyouts in the LISA database, available from Statistics Sweden.[12] The LISA database covers the population over 16 years of age in Sweden from 1990 to 2008 and links employees to employers. The yearly variables we gather from this database are age, sex, highest attained education level, the firm registration number for the individual's main source of income, establishment identifier for the individual's main source of income, labor income, registered number of days in unemployment, and the establishment's industry code and municipality. For each buyout, if it was announced before the first of November, we match that buyout with last year's employee information. If it was announced after the first of November, we use this year's employee data to ensure tagging employees as treated before the buyout was announced.

Our analysis will be based on data for six years prior to the buyout to four years after the buyout. Since the LISA database contains information on individuals above the age of 16, we

belongs to the 300 largest sponsors in the world by capital under management and the buyout firm is Swedish. This gives a total of 73 unique exits. [...] Private equity sponsored exits were identified through the mergers and acquisition database Mergermarket." We do think there is a slight cause for concern about coverage in the Capital IQ database since it only picked up 46% of the transactions analyzed by Bergström et al. (2007).

[11]The original source of the data is Swedish Companies Registration Office ("Bolagsverket"), the government agency that keeps track of official names, firm registration numbers, and accounting information of all limited liability corporations in Sweden. It does not cover one person firms with unlimited liability ("enskild firma") or partnerships. The information was gathered and validated by the consulting firm PAR.

[12]For more on the Longitudinal Integration Database for Health Insurance and Labour Market Studies (LISA) database, see the description at http://www.scb.se/Pages/List____257743.aspx

Table I: The Sample

Year	Buyouts	Firms	Establishments	Employees
1998	19	71	179	6 733
1999	18	52	131	5 031
2000	34	131	318	12 357
2001	23	69	185	6 720
2002	36	60	181	6 980
2003	26	49	344	8 076
2004	45	164	566	20 161
# Observations	201	596	1 904	66 058

Notes. This table displays how the sample of treated employees, establishments and buyouts are spread out over time. "Firms" refer to firm registration numbers. Correcting for the group structure, on average a buyout affects around three firm registration numbers.

drop all employees younger than 22 years to ensure that each individual has at least six years of data before being affected by a buyout. In the merge we lose 54 buyouts (we go from 255 to 201). These are firm registration numbers which have no employees reporting that firm registration number as their main source of income. Out of our 201 buyouts, 25 are "divestitures" in the sense that we located the firm registration number not in the year the buyout is announced but one year later.[13]

The final sample is summarized in Table I. We end up with 201 buyouts affecting 596 firm registration numbers, 1 904 establishments and 66 058 employees between 1998 and 2004.[14] The average buyout affects 329 employees in 2.97 firm registration numbers. The number of buyouts per year increases in over time. The sample of 201 buyouts for which we can identify employees corresponds well to the total sample of 255 buyouts in Sweden registered in Capital IQ database.

Table II shows that average transaction values and the distribution of transaction types in our sample are similar to all transactions registered in Sweden. In both samples, corporate divestitures, cross border buyouts and management buyouts account for the bulk of all buyouts. During 1998 to 2004, in terms of the number of buyout in the Capital IQ database, Sweden ranked ninth in the world, with 1.7% of all buyouts worldwide being undertaken in Sweden. The final columns in Table II illustrate that buyouts in our sample do have smaller mean transaction value than in the U.S, but larger than those in the U.K and France. Sweden also has a lot of cross-border buyouts and corporate divestitures, but fewer management buyouts.

Table III presents average values for our sample and a 10% random sample of all employees in Sweden.[15] Employees in buyout targeted firms have on average higher yearly income and are more likely to be men; otherwise the samples correspond well to each other in terms of average age, average highest attained educational level (a seven degree scale from no education to having a PhD) and the geographical location of the establishment the employee works at.

[13] These are likely newly formed firms as a result of a divestiture in connection with the buyout.

[14] An establishment is defined as a geographical place of work. For example, a company with two stores at different locations has two establishments (one for each store).

[15] We weight the random sample such that its yearly size corresponds to the yearly distribution of employees in targeted firms.

Table II: Comparison to Capital IQ Sample

	Our Sample	Sweden	US	UK	France
Transaction Types					
Going Private	2.5%	2.7%	5.7%	6.6%	2.5%
Corporate Divestiture	31.8%	33.6%	28.1%	31.8%	16.4%
Secondary Buyout	4.9%	5.5%	4.0%	5.2%	6.3%
Bankruptcy Sale	0%	0.4%	2.8%	1.8%	0.8%
Management Buyout	24.8%	25.3%	30.6%	71.8%	45.6%
Family Succession	2.5%	2.3%	1.7%	1.8%	3.0%
Cross Border	33.8%	32.4%	4.8%	10.3%	31.1%
Platform	3.0%	4.7%	9.1%	3.1%	3.9%
Transaction value					
Mean ($mm 31.12.08)	212.29	206.13	267.50	106.17	206.84
Standard deviation	(400.54)	(379.13)	(3123.74)	(479.75)	(535.18)
# Observations	201	255	4958	2210	881

Notes. This table displays how our sample compares to the full Capital IQ sample for Sweden and to other countries. Data on transaction values are missing for around 70% of all observations. Transaction types are not mutually exclusive.

Table III: Characteristics of Treated employees

	Our sample	Random sample (10%)
Age	40.67	41.94
	(11.41)	(12.46)
Share female	0.42	0.48
	(0.49)	(0.50)
Education Level	3.25	3.36
	(1.12)	(1.87)
Labor Income	260 151 SEK	218 030 SEK
	(169 553 SEK)	(161 759 SEK)
Geographical Location	1.83	1.90
	(0.79)	(0.95)
# Observations	66 058	4 079 996

Notes. This table displays a comparison of our treated employees to a random sample of all those employed in Sweden during 1998 and 2004. We have 21 189 missing values for education in the random sample of all those employed and 215 missing values in our sample. We weight the random sample such that its yearly size corresponds to the yearly distribution of employees in targeted firms. Standard deviations are given in parenthesis.

3.2 Identification

To identify the effect of buyouts on employees, we use a difference-in-difference estimator coupled with population data on employees in Sweden. The difference-in-difference estimator compares the relative unemployment risk between employees in a treatment group and a control group over time, and allows us to control for unobserved time invariant group effects as well as common time effects. The model we base our estimates on is specified as

$$Y_{igt} = \alpha + \lambda_t + \gamma BO_g + \beta(Post_t \times BO_g) + \epsilon_{igt}, \tag{1}$$

where Y_{igt} is a dummy measuring whether employee i in group g was officially registered as unemployed for at least one day at time t.[16] Time effects are represented by λ_t and BO_g is the treatment indicator taking on the value one if an employee is treated and zero otherwise. The interaction term, $Post_t \times BO_g$, takes the value one at the buyout year and all years after for the treated group and zero otherwise. Consequently, β captures the causal effect of the buyout on those employed at the targeted firm when the buyout was announced and represents a local average treatment effect.

To estimate Equation 1 we normalize time such that year zero is the year the buyout was announced. There is no consensus on how to compute the standard error for the difference-in-difference estimator but a conservative approach is suggested by Donald and Lang (2007). They point out that the relevant variation for a difference-in-difference estimator is at the group level and not at the individual level. Estimation with group-time aggregated data is efficient and inference can be made under the assumption that the underlying common group errors are normally distributed. The assumption likely holds in our individual level analysis because our groups have both the same number of observations and a large and constant number of observations. Therefore, to account for possible intra-class correlation within time periods we aggregate our data to a group-time level and estimate the following model:

$$\triangle Y_t = \rho + \beta(Post_t) + \triangle\varepsilon_t, \tag{2}$$

where

$$\triangle Y_t = Y_{Tt} - Y_{Ct}, \triangle\varepsilon_t = \varepsilon_{Tt} - \varepsilon_{Ct}. \tag{3}$$

The variable $Post_t$ indicates the year of the buyout and all years after, so β is the difference-in-difference estimator. In the establishment and firm level analysis we fall back to reporting estimates using the disaggregated data because the number of observations in each group is smaller.

The construction of the control group is crucial for identification. For β to have a causal interpretation the treatment and the control group must have parallel trends in the absence of treatment. Then the trend of the control group serves as the counterfactual trend in unemploy-

[16] Our formal definition of unemployment risk is thus "the share of employees in a group that has registered for receiving at least one day of unemployment benefits this year". Robustness checks indicate that our results remain all the way up to defining the dummy as the employee at time t being registered as unemployed for at least 100 days.

ment risk for the treatment group. Parallel trends in the absence of treatment is by definition unknown but is more likely to be fulfilled if the composition of the control group is similar to the composition of the treatment group. As pointed out by Card, Ibarraran, and Villa (2011), analyzing trends before treatment is the only way to examine the validity of the control group. A similar pattern for the treatment and control group in the period before the treatment makes a common pattern after treatment more likely, because common shocks have previously affected the groups in similar ways. We are well positioned to examine trends before the buyout because we have six years of data before each buyout.

We use the universe of registered employees in Sweden each year to construct the control group. We create yearly cells based on individual sex; age in ten year intervals; skill level measured by a dummy taking the value one if the employee has at least two years of undergraduate studies; four regional locations of the establishment; 17 categories of industry classification of the establishment; a dummy taking the value one if the employee has been registered as unemployed at least one time during the last three years; firm size quartiles one year prior to the buyout; a growth dummy taking the value one if the firm has positive employment growth during the last two years prior to the buyout; and five quantiles of average labor income over three years prior to the buyout.[17] If the number of control observations within a cell exceeds the number of treated observations, we randomly drop controls to have a perfectly balanced sample within each cell. If an employee is treated multiple times, we only include the employee as treated the first time he or she is treated.

The matching process leaves us with 65 395 treated employees paired with 65 395 controls. We lose 664 treated employees for which we fail to find a match. Table IV presents summary statistics for the quality of the match. The groups balance well: we have performed a normalized t-test for difference in means finding no statistically significantly difference.[18] Employees in our sample are on average 40 years of age at the time of the buyout, 21% have at least two years of undergraduate studies and thereby classified as high skilled, 42% are women, 44% were employed in the same firm three years prior to the buyout and 67% are employed in a firm that has had positive employment growth during the last two years. The employees were employed in firms with on average 116 employees.[19] The average yearly labor income is 260 423 SEK in the treatment group and 255 725 SEK in the control group. We fail to match employees with a history of unemployment and with more days in unemployment. The unmatched sample consist of on average older, more skilled employees with lower tenure and higher share with at least one day of registered unemployment during the last three years and total days in unemployment. A difference between the matched and unmatched sample is not a problem because the unmatched sample consists of less than 1% of all treated employees.

[17] An alternative matching procedure would have been to use propensity score matching. We chose not to go this route since we have such a large pool of employees to select controls from that we have no problems finding cell matches even when we use all the demographic information. A drawback of using propensity score matching is that the procedure relies on a correctly specified model for estimating the propensity score and a careful choice of matching algorithm. Using matching is simpler and more transparent.

[18] Imbens and Wooldridge (2009) suggests that if the normalized difference is less than a quarter, one need not worry about the model specification when using linear methods.

[19] Measured per firm registration number. The average buyout still affects 329 employees, because a buyout affects on average 2.97 firm registration numbers.

Table IV: Characteristics of Treated and Control Employees

	Treated	Control	Unmatched
Age	40.62	40.62	46.31
	(11.36)	(11.47)	(14.76)
Female	0.42	0.42	0.49
	(0.49)	(0.49)	(0.50)
High Skill	0.21	0.21	0.52
	(0.40)	(0.40)	(0.50)
Tenure	0.44	0.44	0.29
	(0.50)	(0.50)	(0.45)
Unemployment Incidence	0.06	0.07	0.20
	(0.24)	(0.25)	(0.40)
Unemployment Days	5.13	5.72	19.28
	(26.06)	(27.73)	(54.89)
Labor Income	260 424 SEK	255 725 SEK	233 351 SEK
	(169 962 SEK)	(177 056 SEK)	(119 835 SEK)
# of Employees in Firm at $t-1$	115.71	120.00	291.49
	(265.12)	(296.78)	(452.55)
Share in Growing Firms	0.67	0.67	0.57
	(0.47)	(0.47)	(0.50)
Geographical Location	1.83	1.83	2.08
	(0.79)	(0.79)	(1.23)
Mean Buyout Year	2001.62	2001.62	2002.05
	(2.09)	(2.09)	(2.18)
Most Common Industry	Manufacturing	Manufacturing	Health and Social Work
# Observations	65 394	65 394	664

Notes. This table displays summary statistics on how well our control group matches up with the treated employees at the matched year. The right most column present summary statistics for unmatched treated employees. Employees in the treatment and control group are on average equal on observable characteristics (we have performed a normalized t-test of difference in means between the treated and control group and all differences are statistically insignificant). "High Skill" refers to whether an employee has at least two years of undergraduate studies. "Tenure" displays the share of employees that had been employed for at least three years when the buyout occurred. "Share in Growing Firms" measures the share of firms that had a positive employment growth during the last two years prior to the buyout. Standard deviation in parenthesis.

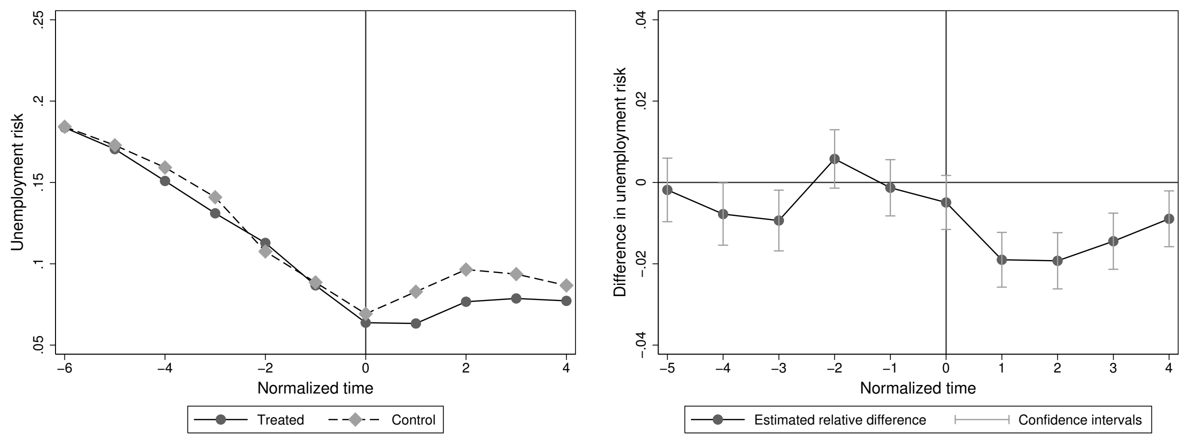

Figure I: Average Effect on Unemployment Risk

Notes. The left panel displays average unemployment risk for normalized time around the buyout for treated and control employees. The right panel presents the difference in unemployment risk for each year in normalized time around the buyout with the vertical lines representing 95% confidence intervals for tests of differences in mean between the normalized time and our base year $t-6$. We observe a declining unemployment risk before treatment and an increasing risk after treatment because all employees are employed at the buyout year. At $t=0$, the unemployment risk is non-zero as some employees are employed at the first of November but have been unemployed prior or after that.

4 Analysis

Armed with data and an estimation technique, we start with estimating the effects on employees' unemployment risk. For comparison with studies on the effects of buyouts on employment in other countries, we then turn to estimating the effects on establishment and firm level employment growth.

4.1 Effects on Employees

Unemployment risk. We start out by graphically analyzing unemployment risk around the time of the buyout. The left part of Figure I plots unemployment risk in a given year in the treatment and control group from six years prior to treatment to four years after. The right part of Figure I displays yearly differences between the treated and control group with confidence intervals showing whether the difference is statistically different at the 5% level in comparison with the difference at $t-6$. We use a critical value for the confidence intervals based on 11 degrees of freedom since we have 11 years of variation.

Because all employees are employed at the buyout year, a mean reversion pattern clearly shows up in the left figure. Unemployment risk decreases up until $t=0$ and then starts to increase.[20] Trends in unemployment risk before treatment are similar for treated and control

[20] At $t=0$, the unemployment risk is non-zero as some employees are employed in at the first of November but have been unemployed prior or after that. The mean reversion pattern can be compared to the "Ashenfelter dip" in the unemployment literature. A potential problem with a mean reverting pattern is that it occurs even if a buyout has no effect on employees. Only by careful matching to find a control group experiencing a similar dip can the effect of the "dip" be minimized.

Table V: Difference-in-Difference Estimates.

	DiD estimate	% change.
Baseline Sample	-0.011*	-12.7%
	(0.004)	
Labor Income	3733.60*	1.4%
	(1522.10)	
Strategic as Controls	-0.006**	-7.9%
	(0.001)	
# Observations	11	11

Notes. This table displays difference-in-difference estimates at the individual level. We estimate the effect using group aggregated averages implying that we use 11 observations in our estimations to ensure that our standard errors are robust to all arbitrary correlations within groups at each point in time. The difference-in-difference estimate can be interpreted as percentage point effect. The percentage change is given with reference to the average value for the four year period after the buyout for the control group. Statistical significance at the 5% and 1% level is denoted by * and **. Standard deviations are given in parenthesis. Labor income is measured in SEK.

employees indicating that our estimates can be interpreted as causal. The right figure shows that the effect is strongest for the first two years and weakens slightly thereafter. Since we follow employees independently of whether they stay with the firm or not, a weakening of the treatment effect over time is not surprising as fewer employees remain with the treated firm.

The first row in Table V displays the estimate of the effect of the buyout using the aggregated difference-in-difference model outlined in Equation 2. The point estimate for unemployment risk is -1.1 percentage points on average for the four year post period and it is statistically significant at the 5% level. Using the post period average for controls as comparison, the effect of -1.1 percentage points constitutes a 12.7% decline in unemployment risk for treated employees. The effect is not concentrated to specific firm size quartiles or quartiles of the labor income distribution.[21]

Labor income. An alternative way to examine how buyouts affect employees is to study labor income. Because labor income is the result of a combination of wage and labor supply, a lower unemployment risk should translate into higher income in comparison to the control group. Figure II is equivalent to Figure I but plots labor income instead of unemployment risk. Again, the treatment and the control group have similar trends in average labor income before treatment. The increase in labor income before treatment reflects the requirement that both treated and control individuals must be employed at time zero. The difference-in-difference estimate reported in the second row of Table V reveals that labor income increases by 3734 SEK which converts to a 1.4% increase, on average, for four years after the buyout. The labor income increase is not driven by a specific quartile in the labor income distribution.[22]

The higher labor income is it not entirely driven by shifts to unemployment. We know that the probability of becoming unemployed after the buyout is reduced, so we expect that a larger share of individuals in the treatment group remain with their firm, relative individuals in the

[21] See the online appendix for details.

[22] See the online appendix for details.

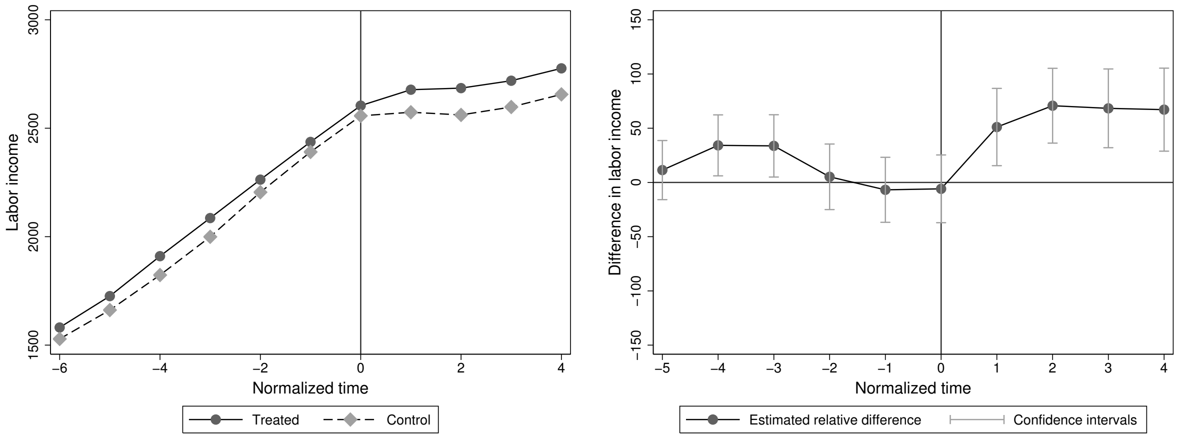

Figure II: Average Effect on Labor Income

Notes. The left panel displays average labor income for normalized time around the buyout for treated and control employees. The right panel presents the difference in labor income for each year in normalized time around the buyout with the vertical lines representing 95% confidence intervals for tests of differences in mean between the normalized time and our base year $t-6$.

control group. Because the control group will have a higher share of unemployed individuals, the difference-in-difference estimate conditional on the employee remain with the treated firm can be seen as a lower bound on the effect of the buyout on labor income alone. Conditioning on being employed for the four years following a buyout, the difference-in-difference estimate is 2 991 SEK (or 1.1%) and it is statistically significant at the 1% level. Because we estimate a positive effect, the labor income effect in our main specification is not likely to be driven entirely by shifts to unemployment.

Strategic acquisitions. Even though we use rich demographic information in constructing the control group, there could still be unobservable time invariant characteristics making a firm a likely buyout target. If these unobserved firm characteristics correlate with idiosyncratic unemployment risk, we could falsely attribute the estimated effect on unemployment risk to buyouts rather than to selection. Given that we estimate a reduction in unemployment risk, a particular concern is that our matching procedure fails to capture that buyouts take place in firms that are positioned well to grow and thereby layoffs will be less likely.

The best way for us to deal with potential selection on unobservables is to use a different control group. If there are unobserved firm level characteristics that make a firm a particularly attractive target, our control group should be based on employees from potential or realized acquisition targets. We thus perform a robustness check by restricting the sample of possible control employees to employees in firms that were acquired by another firm in the same year as the buyout took place. We refer to these acquisitions as strategic acquisitions (as opposed to the financially motivated acquisitions undertaken by private equity firms).

To create the pool of potential control employees, we use the IFN Corporate Database to identify all firms that were acquired between 1998 and 2004 in Sweden.[23] We correct for the

[23]All mergers of firm registration numbers have to be reported to the Swedish Companies Registration Office,

Table VI: Estimates for Staggered Treatment

	(1)	(2)	(3)	(4)
Unemployment Risk	-0.025**	-0.019**	-0.020**	-0.024**
	(0.005)	(0.005)	(0.005)	(0.005)
Year Dummies	yes	yes	yes	yes
Industry*Year Dummies	no	yes	yes	yes
Year*Skill Dummies	no	no	yes	yes
Year*Tenure Dummies	no	no	no	yes
# Observations	641 454	641 454	641 454	641 454

Notes. This table displays difference-in-difference estimates for our staggered treatment approach using treated employees in other buyout years as controls for treated employees in a given buyout year. Columns (2), (3) and (4) present estimates accounting for industry-year specific trends, year-skill specific trends and tenure year specific trends. Standard errors clustered on buyout firm. Statistical significance at the 5% and 1% level is denoted by * and **. Standard deviations are given in parenthesis.

ownership structure by tagging firms as being a part of a merger or acquisition if, for a given year, they are directly or indirectly majority owned by a firm acquired in that year. We select controls from employees in these firms by applying the same matching procedure as for our baseline sample.

The second row of Table V displays the results from estimating Equation 2 on the aggregated data using the newly created treated and control group. The point estimate is a statistically significant and reveals a 0.6 percentage point decline in unemployment risk corresponding to a 7.9% reduction in unemployment risk. Though the result is slightly weaker, it gives us confidence that unobservable characteristics making a firm a particularly good acquisition target play a minor role. The result also suggests that strategic acquisitions do not affect employees in the same way as buyouts do: buyouts reduce unemployment risk to a greater degree.

Staggered treatment. Another way of dealing with potential selection on unobservables is to make use of the staggered treatment dates in our sample. The different treatment dates allow us to construct a control group within the group of affected employees along the lines of Bertrand and Mullainathan (1999) and Arai and Skogman-Thoursie (2009). That is, for a given year t, the control group consists of employees who are treated in earlier or later years. We estimate the following model using information for six years prior to each buyout and four years after each buyout:

$$Y_{it} = \alpha_i + \theta_t + \beta Deal_{it} + \varepsilon_{it}, \tag{4}$$

where Y_{it} measures whether employee i was registered for unemployment for at least one day during year t. Time constant employee heterogeneity is controlled for through employee fixed effects α_i, while θ_t captures all common time effects that influence unemployment risk. The variable $Deal_{it}$ is a dummy taking the value one if an employee is treated at time t or later. The effect on unemployment risk or labor income is identified by β. The identifying assumption is, as before, that the groups have parallel trends in absence of a buyout. Since the staggered

which is the basis for the merger information in the IFN Corporate Database.

treatment dates do not allow us to use time-aggregated data to take group specific shocks into account, the standard errors are clustered at the firm registration number level for the buyout year. Unobservable characteristics within the group of treated employees are more likely to be similar, which mitigates any potential unsolved selection problems.[24]

Results are presented in Table VI. With our staggered treatment approach, the unemployment risk after a buyout decreases. The basic model (column one in Table VI), estimates the effect on unemployment risk to be -2.5 percentage points. Accounting for industry-year specific trends, year-skill specific trends and tenure year specific trends (columns two to four), the effect varies from -1.9 to -2.4 percentage points.[25] Because we still estimate statistically significant reductions in unemployment risk, it is not likely that our initial approach suffered from a severe selection problem.

4.2 Effects on Establishments and Firms

Continuing establishments. Having established statistically significant declines in unemployment risk for employees, we now turn our attention to what goes on at the establishment and firm level to compare to studies from other countries. We start by examining employment growth in continuing establishments, that is, we look at establishments existing at the time the buyout was announced. As with the individual level analysis, we use a difference-in-difference methodology with cell matching.

Following Davis et al. (2011), we measure employment growth at time t for a group as the net change in employment at the group level defined as

$$g_t = \sum_i \left(\frac{0.5(E_{it} + E_{it-1})}{\sum_i [0.5(E_{it} + E_{it-1})]} \right) g_{it} \tag{5}$$

where E_{it} is number of employees at establishment i at time t and the growth rate at establishment i at time t is

$$g_{it} = \frac{E_{it} - E_{it-1}}{0.5(E_{it} + E_{it-1})}. \tag{6}$$

Weighting with the sum of the average firm sizes ensures that a group's employment growth rate is not driven by higher variance in employment growth in small firms.

Going from individual data to establishment data is trivial for the treatment group as we can simply use the establishments the treated employees were employed in. To create the control group, we match each treated establishment with an establishment that has not undergone a buyout using matching on a 17 category industry code; geographical location (four regions); deciles of firm size of the firm the establishment belongs to; deciles of the three year average size of the establishment; whether the average educational level at the establishment is at least two years of undergraduate studies; whether the firm that the establishment belongs to has had a positive employment growth over the last two years; and whether the firm is a multi-establishment firm or not.

[24] An alternative way of constructing a control group within the group of affected employees is to only use employees who are affected later as controls for employees subject to a buyout (thus omitting already treated employees from the control group). We do not use this approach because it reduces the usable sample size.

[25] The results are unchanged using firm fixed effects.

Table VII: Characteristics of Treated and Control Establishments

	Treated	Control	Unmatched
# of Employees in Establishment	43.67	43.48	39.46
	(86.89)	(88.37)	(145.48)
# of Employees in Firm	143.38	324.35	429.70
	(325.56)	(612.41)	(543.04)
Multi-establishment Firm	0.77	0.77	0.97
	(0.42)	(0.42)	(0.18)
Share in Growing Firms	0.70	0.70	0.88
	(0.46)	(0.46)	(0.33)
High Skill Establishment	0.02	0.02	0.09
	(0.13)	(0.13)	(0.28)
Geographical Location	1.83	1.83	2.07
	(0.87)	(0.87)	(1.14)
Most Common Industry	Wholesale/Retail	Wholesale/Retail	Transp./Storage/Commun.
# Observations	1376	1376	92

Notes. The first two columns display summary statistics for the treated and control establishments at the matched year. The right most column present summary statistics for unmatched treated establishments. We have performed a normalized t-test of difference in means between the treated and control group and all differences are statistically insignificant. Standard deviations are given in parenthesis. "High Skill" refers to whether the average employee at the establishment has at least two years of undergraduate studies. "Share in Growing Firms" measures the share of establishment that had a positive employment growth during the last two years prior to the buyout.

To improve the accuracy of the match, we initially restrict the pool of potential control establishments to those employing fewer employees than 1.25 times the largest establishment in the treated group. We start out with a sample of 1 904 treated establishments that are directly or indirectly majority owned by a firm targeted for a buyout. We focus on establishments that are stable in the sense that they have been active for at least two years before the buyout was announced. After the match, we are left with 1 376 treated establishment and 1 376 controls. We are unable to find a match for 92 establishments and we lose 436 establishments because they are not present for two periods before the buyout was announced.

Table VII displays summary statistics for the matched data at the time the buyout was announced (at $t = 0$). The control group is on average identical or similar to the treatment group in terms of average education level, share of establishments in a multi-establishment firm and the average size of the establishment. We fail to create balance in firm size: the average firm size is 143 employees in the treatment group and 324 employees in the control group. We have performed a normalized t-test of difference in means between the treated and control group and all differences are statistically insignificant from each other except for firm size. If we compare the matched sample of establishments with the sample of treated establishment of which we fail to match, we see that the unmatched sample almost exclusively consists of establishments belonging to firms with more than one establishment (a multi-establishment firm) and that failed matches tend to employ high skill employees to a greater extent.

We start by examining how the sample of treated and control establishments evolve over

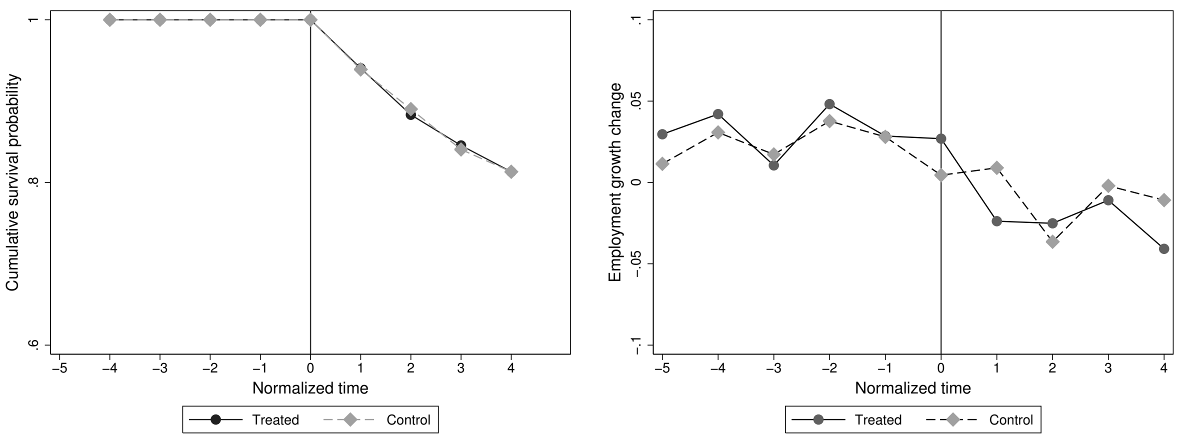

Figure III: Establishment Growth Change and Survival Probability

Notes. This figure displays the survival probability for an establishment (left) and the net employment growth change for the treated and control groups over time (right).

time. The left panel of Figure III displays the cumulative share of establishments in each group that, for a given year, are present in the next year. The survival rates for treated and control establishments are stable and around 80% of all establishments that were present at time zero are still active four years later. The similar survival rates suggests that treated establishments are no more likely to be shutdown than control establishments. The similar survival rates also make our results more robust to possible compositional changes in the treatment and the control group.

The right part of Figure III plots the average employment growth rates for the treatment and the control group. A similar pattern in employment growth prior to treatment reassures us of the quality of the matched control group. A slight relative decline in treated establishments occurs in the years following a buyout. Estimating Equation 1 using disaggregated data reveals that the effect corresponds to a statistically significant decline of 1.2 percentage points (with a standard error of 0.0004) for the post period when clustering the standard errors at the firm level.

Table VIII displays how our establishment level results compare to the establishment level results in Davis et al. (2011). Both studies apply the same measure for employment growth but Davis et al. (2011) analyzes buyouts during a period from 1980 to 2000 and has a data set covering more than 150 000 establishments in the US. Even though the US sample contains more buyouts over a longer period, the average change in employment growth is similar: the effect on the cumulative employment growth rate four years after the buyout is -6.0 percentage points in Sweden and -5.4 percentage points in the US. However, Davis et al. (2011) report that targeted establishments in the US are more likely to be shut down than control establishments, which we do not observe in Sweden.

Our individual level data allows us to go further than Davis et al. (2011) by decomposing changes in employment growth at the establishment level into hirings and separations. Our estimation is based on the following model:

Table VIII: Comparison Between Sweden and US

	Sweden	US
Time Period	1998-2004	1980-2000
# Obs Establishment (Treated)	1376	151 529
Establishment Match Variables	- Industry	- Industry
	- Size	- Size of Parent Firm
	- Size of Parent	- Age of Parent Firm
	- Location	- Multi-unit Status
	- Multi-unit Status	- Transaction Year
	- Average Skill Level	
	- Transaction Year	
Establishment Growth Difference		
$t+1$	-3.28%	-0.93%
$t+2$	1.13%	-2.23%
$t+3$	-0.88%	-0.55%
$t+4$	-3.00%	-1.64%
Cumulative $t+1$ to $t+2$	-2.15%	-3.16%
Cumulative $t+1$ to $t+4$	-6.03%	-5.35%

Notes. This table displays a comparison between establishment level employment growth in Sweden to that of the US (Table 3 in Davis et al. (2011)).

$$Y_{igt} = \alpha + \lambda_t + \gamma BO_{gt} + \beta(Post_t * BO_{gt}) + X_{et}\eta + \epsilon_{egt} \tag{7}$$

where Y_{igt} takes on different forms. For each year we track all employees at an establishment one year back and one year forward. To estimate the overall effect on hirings, Y_{igt} is a dummy taking on the value one if an employee is hired during a given year. To measure the effect on separations, Y_{igt} is defined as a dummy taking on the value one if an employee is doing his or her last year at an establishment given that the establishment is observed in data in the following year.

When we use aggregated data and the model represented by Equation 2, we find that the overall drop in hiring following a buyout is 4.1 percentage points and statistically significant (with a standard error of 0.009) while the estimate for separations is not significant (point estimate of -1.3 percentage points with a standard error of 0.009). Hence, reduced employment growth at the establishment level is due to hiring freezes rather than to increased separations.

Firm level employment growth. Moving up to the firm level, we start by examining net employment growth for group-adjusted firms consisting of the firm within an ownership group that was directly targeted by a private equity firm and its subsidiaries. Tracking firms over time is substantially more difficult than tracking individuals or establishments as firm registration numbers and group structures change frequently. A benefit of our data is that we can adjust for the group structure year by year and thereby can track acquisitions, divestitures, shutdowns and the creation of new divisions more accurately. However, we are still unable to correct for ownership structures that puts the targeted firm itself as a subsidiary; adjusting "upwards" risks that we aggregate up to the private equity fund level and thus include other portfolio firms as

Table IX: Characteristics of Treated and Control Firms

	Treated	Control	Unmatched
Year	2001.63	2001.63	2001.24
	(1.98)	(1.98)	(2.06)
# of Employees in Firm	184.68	119.13	106.52
	(404.60)	(221.43)	(105.99)
# Establishments	4.95	4.36	3.45
	(14.40)	(11.31)	(4.26)
Geographical Location	1.97	1.97	2.19
	(0.94)	(0.94)	(1.30)
Share of Growing Firms	0.58	0.43	0.72
	(0.50)	(0.43)	(0.46)
# Est. in Group	46.09	46.26	80.31
	(92.72)	(99.08)	(161.11)
# Group Size	2407.19	2081.14	2596.76
	(7107.04)	(5903.54)	(4382.46)
Most Common Industry	Manufacturing	Manufacturing	Real estate, renting and business act.
# Observations	139	139	29

Notes. The first two columns display summary statistics for the treated and control firms at the matched year. The right most column presents summary statistics for unmatched treated firms. "Share of Growing Firms" measures the share of firms that had a positive employment growth during the last two years prior to the buyout. "Group" refers to corporate group. Standard deviations are given in parenthesis.

part of the treated firm. Because of the difficulty of accurately tracking firms over time, we restrict our analysis to two years before and after the buyout.

We create a control group of firms matching on firm size divided into five quantiles and corporate size in five quantiles in terms of number of employees, number of establishments within the corporate group, geographical location (four districts), industry classification (17 categories), levels within a corporate group (five levels) and restrict the sample of potential control firms to those employing less than 1.25 times what the largest treated firm employs. The matching is done for each separate year between 1998 and 2004. Table IX shows summary statistics for treated and control firms at the year the buyout was announced. While the groups are similar, there is a clear difference in the number of establishments within the group and group size in terms of employment: treated firm registration numbers are more likely to come from larger corporate groups. Many buyouts we study are divisional buyouts from larger corporate groups. Matching these buyouts well is hard because of considerable variation in how corporate groups are structured and at what levels within the group subsidiaries are placed.

We follow the same procedure as in the establishment level analysis for calculating employment growth for the different groups over time.[26] Figure IV reveals that both groups show

[26]Because we adjust for the group structure we end up with a few cases in which our growth measure at the firm level is close to 2 (the maximum value) as a result of the size of the firm growing by over 1000%. The high growth likely captures a misallocation of a subsidiary within a group (as a result of, for example, changing the firm registration number for administrative reasons). Since we weight each observation by the number of employees, our group aggregated measure gives a large weight to these incorrect observations. To deal with this, we drop 44 observations for which the growth from one year to the next is above 1.9 and in which the resulting

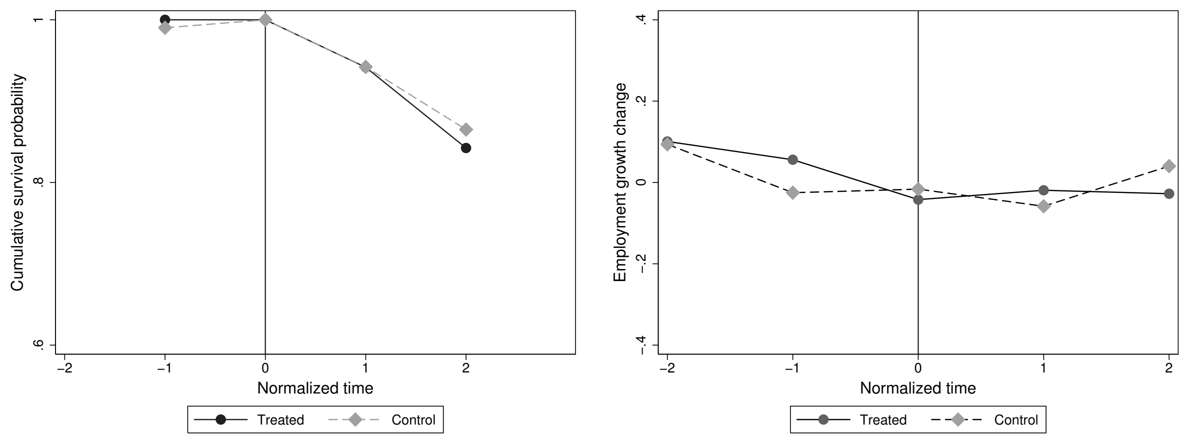

Figure IV: Firm Employment Growth and Survival Probability

Notes. This figure displays the survival probability for legal firm entities (left) and the net employment growth change for the treated and control group of firms (right).

fairly similar trends in employment growth change before the buyout was announced. Figure IV also reveals no difference in employment growth change after the buyout. As shown by the right graph, there are no large differences between the treated and control group of firms in terms of survival rate after the buyout. Estimating Equation 1 using disaggregated data, the difference-in-difference estimate for employment growth is not statistically different from zero.

A central part of Davis et al. (2011) is showing that private equity firms seem to speed up the creative destruction process by closing down more establishments and opening up more new ones compared to their control group. As the establishment level analysis showed, an important difference between the US and Sweden is that we do not observe a greater degree of establishment shutdowns after the buyout. That there is no change in shutdowns is also evident from Table X, which presents descriptive statistics on the share of establishments shutdown, acquired created or divested between the buyout year and two years after. The only type of reorganization that seems to occur to a greater degree for treated firms is divestments of establishments. Neither greenfield creation nor new acquisitions of establishments are more common in treated firms relative control firms.[27] In sum, evidence of private equity firms speeding up the creative destruction process in Sweden is weaker than evidence from the US.

5 Interpreting the Results

We now turn to possible mechanisms that could explain a reduced unemployment risk for employees. Two interpretations strike us as probable. The first one draws on the theoretical background provided in Section 2: employees benefit because of relaxed financial constraints at the firm level. The second interpretation draws on the the argument that labor market regulations in Sweden restrict private equity firms from firing employees.

firm size size is over 1000 employees.

[27]A greenfield is identified in the data as a new establishment not present earlier. An acquisition is defined as an establishment existing before joining a firm.

Table X: Descriptives on Reorganization

		Treated	Control	Difference
		Panel A: Establishments		
At $t+2$	Remaining	87.2%	86.4%	0.8%
	Greenfield	7.4%	7.8%	-0.4%
	Acquired	5.4%	5.9%	-0.5%
From t to $t+2$	Remaining	72.5%	75.8%	-3.3%
	Divested	12.2%	8.7%	3.5%
	Shut down	15.3%	15.4%	0.1%

Notes. "At $t+2$" refers to a sample of establishments that were all present at $t+2$ where "Remaining" is the share of establishments that also were present at t, "Greenfield" is the share of newly started establishments and "Acquired" is the share of establishments that were not connected to a target firm at time t but were acquired later. "From t to $t+2$" refers to what had happened in $t+2$ to the sample of establishments that were all present in t, here "Remaining" refers to the share of establishments that were also present at t, "Divested" refers to the share of establishments that belonged to a target firm in t but not in $t+2$, and "Shut down" is the share of establishment that belonged to a targeted firm in t but did not exist at $t+2$.

5.1 Alleviating Financial Constraints

The financial constraints story receives support in the data. We perform three tests. The first relies on variation across industries in dependence of external capital needed for new investments. To measure industry dependence of external capital we follow Rajan and Zingales (1998) and Boucly et al. (2011) and calculate the difference between capital expenditure and gross cash flow divided by capital expenditures for all firms in a given industry and year. A high dependence of external capital to finance new investments is associated with a high ratio. We then take the average ratio for each industry over the whole period and define all industries with a ratio above the median as dependent on external capital.[28] We estimate separate difference-in-difference estimators for employees in industries with high dependence and for employees in industries with low dependence. The first two rows of Table XI display the result using the aggregated data. Consistent with the idea that a buyout relaxes financial constraints, employees in financially dependent industries are estimated to have a reduced unemployment risk of, on average, 1.6 percentage points in the post four-year period in contrast to employees in non-financially dependent industries for which we find no effect of the buyout on unemployment risk.

The second test is based on comparing divisional buyouts to other types of buyouts. Corporate divestitures involve taking parts of a larger firm out as a separate entity, so the parts bought out are less likely to be financially constrained prior to the buyout, due to internal capital markets. To investigate, we estimate a difference-in-difference model on aggregated data in accordance with Equation 2 where treatment is defined as being part of a corporate divestiture and control defined as being part of a non-corporate divestiture. The difference-in-difference estimate, row three in Table XI, shows a 1.8 percentage points or a 24% higher unemployment

[28] The main two industries classified as dependent on external capital are the manufacturing industry and the transport, storage and communication industry. These account for over 95% of the treated employees in all industries with a financial dependence ratio above the median.

Table XI: Heterogeneity in Financial Constraints

	Unemployment risk estimate	% change.
Financially dependent	-0.016**	-17.9%
	(0.004)	
Non-financially dependent	-0.006	-7.3%
	(0.003)	
Divestitures vs Non-Divestitures	0.018**	24.0%
	(0.004)	
Buyouts 1999-2000	-0.026**	-27.6%
	(0.006)	
Buyouts 2003-2004	-0.002	-2.5%
	(0.004)	
# Observations	11	

Notes. Statistical significance at the 5% and 1% level is denoted by * and **. The estimates for business cycle effects measures the average two years post effect based on 9 observations.

risk for employees in corporate divestitures relative to employees in non-corporate divestitures. A relative decrease in unemployment risk for non-corporate divestitures is consistent with unemployment risk decreasing because of relaxed financial constraints.

The third test relies on studying how the effects on employees vary across the business cycle. If a buyout relaxes financial constraints, it should make firms more likely to be able to withstand negative economic shocks because of easier access to capital injections from the private equity fund and the financial engineering expertize private equity firms possess. Our sample covers the IT stock market crash in 2001 that hit the Swedish economy hard. We divided our sample of buyouts into buyouts taking place before and after 2001. The final two rows of table XI display difference-in-difference estimates for unemployment risk for buyouts undertaken between 1999-2000 and 2003-2004 (for two periods after the buyout to ensure the first sample does not overlap the second) using aggregated data. We find no statistically significant effects for buyouts undertaken between 2003 and 2004, but statistically significant reductions in unemployment risk buyout between 1999 and 2000. These results are consistent with unemployment risk decreasing because of relaxed financial constraints.

5.2 Labor Market Regulations

While the financial constraints story is consistent with the data, we fail to find evidence that labor market regulations restrict private equity firms from laying off employees. We undertake three tests based on variation in employment protection across employees. The idea is that if labor market regulations are indeed an obstacle for private equity firms, they would seek to avoid them by concentrating layoffs to employees with weaker protections.

The first test is based on that employment protections in Sweden are increasing with tenure due to a "first-in-last-out" seniority rule. We can thus compare the effect of the buyout on employees with shorter tenure to those with longer tenure in the firm. We estimate Equation 2 using aggregated data separately for employees with three or more years of tenure at their place of work and for employees with less than two years of tenure. The first two rows of

Table XII: Heterogeneity in Labor Market Protections

	Unemployment risk estimate	% change.
Tenure > 2 years	-0.008*	-17.0%
	(0.003)	
Tenure < 3 years	-0.013**	-11.1%
	(0.004)	
Employed for three years	-0.005	
	(0.003)	
Not employed for three years	-0.041**	-14.8%
	(0.007)	
Age<30	-0.031**	-22.8%
	(0.006)	
Age>29	-0.005	
	(0.004)	
# Observations	11	11

Notes. Statistical significance at the 5% and 1% level is denoted by * and **.

Table XII present our estimates. For both samples we find a statistically significant decrease in unemployment risk, and the effect is slightly stronger for employees with shorter tenure.

The second test is based on variation in employment protection in Sweden that stems from the fact that employees on temporary contracts are protected less by laws than those on permanent contracts. Though we do not observe the types of employment contracts employees have, employees on temporary contracts are more likely to jump in and out of unemployment. Employees with unemployment days in the years prior to the buyout are then more likely to be on a temporary contract than employees that have not been unemployed prior to the buyout. The middle two rows of Table XII display the results of estimating Equation 2 using aggregated data for employees with at least one day of unemployment during the period $t-2$ to t and those that had no unemployment during the same period. We only estimate statistically significant declines in unemployment risk for employees with an unemployment history, suggesting that the buyout benefits employees with weaker employment protections to a greater extent.

As a third test for whether employment protection is important for how buyouts affect employees, we estimate separate effects for young and old employees. OECD reports that 41.3% of employees between 15 and 24 years old had temporary contracts in 2000 (OECD, 2002). The same figure for the group of employees aged 25 to 54 was 10.5%. The relative softer protection for younger individuals should make them more sensitive to cost cutting through downsizing of the workforce. We divide our sample into employees older and younger than 30 years when the buyout was announced. The last two rows of Table XII show estimates from using aggregated data. Most of the reduction in unemployment risk can be attributed to younger employees.

In sum, these tests suggest that labor market regulations do not restrict private equity firms from laying off employees. The minor role played by labor market regulations is in line with Boucly et al. (2011), who compares industries with different labor law rigidities and find no support that strong French labor market regulations affect their results of greater firm level employment growth after the buyout. But as they underscore, labor market regulations are likely to restrict the types of buyouts that can be undertaken. In our setting, that would entail biasing buyouts towards only those types of buyouts that do not involve layoffs leading to unemployment. But even if labor market regulations are important for the effects on employees, our results say something about the effects on employees in countries with similar levels of employment protections as in Sweden. When OECD ranked the overall employment protections in member countries and other selected non-OECD countries in 2004, Sweden was ranked as having the seventh strongest protection among a total of 30 countries (OECD, 2004). Countries such as France, Germany, Netherlands, Italy, Spain and Finland are all indexed as having an overall employment protection in parity with Sweden's. In total, countries with similar strengths in employment protections as Sweden account for around 20% of all buyouts undertaken worldwide between 1998 and 2004 according to the Capital IQ database.

6 Conclusion

Using data from Sweden, we show that employees benefit from buyouts by experiencing a reduction in unemployment risk and an increase in labor income. These benefits pertain to employees across most quartiles of the labor income distribution. Moving up from the individual level to the establishment and the firm level, we show that the effect on firm and establishment level employment growth is surprisingly similar to those observed in the US and the UK. The reduction in unemployment risk is mostly concentrated to industries dependent on external capital to grow, non-divisional buyouts in which the target firm is more likely to be financially constrained, and buyouts undertaken just prior to the economic slowdown following the IT stock market crash in 2001. We find no evidence that labor market regulations restrict private equity firms from laying off employees. These findings are consistent with the hypothesis that the potenital negative effects of buyouts on employees, from cost cutting and higher debt load, are outweighed by the positive effects of relaxing financial constraints at the firm level.

Given these findings, concerns that private equity firms operating in Sweden have generated returns for investors at the expense of employees are unwarranted. Our analysis also suggests that caution should be exercised in interpreting declines in firm and establishment level employment growth observed in the UK and the US as detrimental to individual employees. Moreover, the connection between unemployment risk and access to capital we establish suggests more generally that policies designed to relax firms' financial constraints could trickle down to benefit individual employees by reducing transitions to unemployment.

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Temporary Disability Insurance and Spousal Labor Supply°

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Abstract

We use a reform in the Swedish temporary disability insurance to show that the partner's benefit level affects spousal labour supply: an eleven percent increase of the partner's benefit level is estimated to prolong spousal sick spells with around eight percent, which corresponds to an elasticity of sick days with respect to the partner's benefit of three-quarters of the own labor supply elasticity of unity.

Keywords: spousal labor supply, income effect, social insurance programs

JEL codes: D10; J12; J22

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Introduction

By providing economic security at times of unemployment and disability, the social insurance system is perhaps the most important policy program in OECD countries. Most OECD countries also have a temporary disability insurance to compensate foregone earnings in case of short-term illness. While social insurances are important for people's well being, they can, as pointed out by Krueger and Meyer (2002), have significant disincentive effects on labor supply. Although most empirical research has considered labor supply responses by the directly insured person, others persons, for example their spouses, may also be affected by it.[1] When couples have a common household budget the partner's insurance can crowd out spousal labor supply. Both from a theoretical as well as an empirical perspective, total disincentive effects can be underestimated by ignoring such indirect effects. Understanding how labor supply response to social insurances is important when designing the optimal benefit level but empirical evidence of spousal effects is not well documented.[2]

In this paper we examine if spousal labor supply depends on the partner's temporary disability insurance. Traditionally, the family has been modelled as if all earnings are pooled in a common budget. The controversy has been on how to think about family members' utility. The earliest attempts applied a common utility function for all members, but later models have taken a more individualistic approach by letting each family member have its own utility.[3] With personal utilities, we should expect that the partner's income influences spousal labor supply via an income effect. Consequently, the negative income effect when the partner is sick is mitigated by the partner's temporary disability insurance benefits, which makes spousal labor less important as a guarantee for the household income.[4]

Spousal labour supply can be affected by the partner's insurance via three types of income effects:[5] 1) a *direct effect* if the partner's disposable income is affected by a change in the insurance benefits. The sign of the direct effect is a priori unknown since a higher benefit

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[1] For recent studies on disincentive effects in the temporary disability insurance, see e.g., Johansson and Palme (2002, 2005), Henrekson and Persson (2004), Ziebarth and Karlsson (2010) and Pettersson-Lidbom and Skogman Thoursie (2011). For recent studies on the disability insurance see for example Gruber (2000), Autor and Duggan (2006), Chen and van der Klaauw (2008) while Lalive (2008) represents a recent study on the unemployment insurance.

[2] Krueger and Meyer (2002) discuss that a balance between intended and unintended consequences of social insurance is needed when setting the optimal benefit level.

[3] See for example Killingsworth (1983) for a more detailed discussion how to treat the family in labor supply models.

[4] This is related to the added worker effect discussed in the unemployment insurance literature where spousal labor supply increases in order to compensate for the earnings loss when the partner becomes unemployed (Ashenfelter (1980), Heckman and MaCurdy (1980), and Lundberg (1985)).

[5] This presupposes a common family budget.

level might reduce the partner's labor supply making the effect on the partner's disposable income ambiguous, 2) a pure *insurance effect* as the partner's benefit level alters the cost of future illness by the partner. A greater compensation leads to a decrease in spousal labor supply if leisure is a normal good, and, 3) a *joint leisure effect* if higher compensation increases a couple's demand for spending leisure time together.[6]

Studying indirect effects from the temporary disability insurance offers an opportunity to estimate the impact from income changes on family members' supply of labor. Cullen and Gruber (2000) show that spousal labor supply can be crowded out by the partner's unemployment insurance and Baker (2002) finds that spousal labor supply is influenced by the partner's pension payments. Other studies examine spousal responses to health shocks hitting the partner.[7] But no study has investigated the connection between the partner's temporary disability insurance and spousal labour supply.

We examine the relationship between spousal labor supply and the partner's temporary disability insurance by analysing a reform in Sweden in December 1987. The reform increased the benefit level for workers when temporary ill in the private and the local governmental sector but left the replacement rate unchanged for central government workers. We can thus analyse spouses for which the reform increased the replacement rate for the partners without altering the replacement rate for the spouses themselves. Spousal responses are identified with a difference-in-difference estimator and labor supply is measured as sickness absence using complete personal sickness history covering the years from 1986 to 1990 linked with individual register data.

Our results indicate that the partner's temporary disability insurance has a negative impact on spousal labor supply: we estimate that the average duration of sick spells increase by 0.4 days while the incidence is unaffected. The spousal elasticity of sick days with respect to the partner's sickness benefit is estimated to 0.74, corresponding to three-quarters of the direct elasticity of labour supply elasticity in the unemployment and temporary disability insurances.[8]

To study how spousal labor supply is affected by the partner's temporary disability insurance is important as it reveals an unintended side of the social insurance system. By

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[6] Domestic work may be performed during sickness absence. If so, an increased sickness absence by the partner can decrease spousal sickness absence if the spouse's domestic responsibility is lowered.

[7] See for instance Berger and Fleisher (1984) and Colie (2004).

[8] Krueger and Meyer (2002) report that the direct labor supply elasticity in the unemployment insurance is close to one. The few studies in the temporary disability insurance literature that have estimated direct labor supply elasticity also suggest that the elasticity is close to one (see the references of studies in footnote1).

using a reform targeted towards the partner, the variation is exogenous to spousal labor supply that makes us believe that our estimates represent causal responses. The paper relates to a literature focusing on unemployment insurance and spousal labor supply as well as to a more general literature trying to understand the importance of income effects for labor supply, where isolation of income effects have been proven to be difficult as they almost always are accompanied with a substitution effect.[9]

The reform

Sweden has a compulsory publicly administered temporary disability insurance funded primarily through a payroll tax levied on employers. Income compensation constitutes the major part of the program but the replacement rate has varied over time. As of today, no economic compensation is given for the first sick day and a physician's certificate is required from the eight day. For a majority of workers, collective agreements top-up the compensation from the public system.

In December 1987, the one-day-waiting period was abolished and the replacement rate increased to 90 percent for spells lasting less than a week in the public temporary disability insurance (see, e.g., Proposition 1986/87:69 and Ds S 1986:8).[10][11] The government decision was taken on December 18, 1986 as some workers received a relatively small fraction of their income if they were sick for a short period - a feature that was considered as unfair by many policy makers (SOU 1981:22 and SOU 1983:48).

In 1987, Swedish workers were employed in three sectors: the central government (16 percent), local governments (39 percent), and the private sector (45 percent). The reform had differential effects on workers depending on their sector of employment. The replacement rate for central government workers was unaffected by the reform. The central government had namely made use of the Social Security Act (1962:381) that allowed employers to administer their employees' sick leave payments while collecting the workers' sickness benefits (i.e., arbetsgivarinträde). The replacement rate was therefore 92 per cent of current earnings both before and after the reform and cash benefits were paid from the first day of temporary

[9] Imbens et al. (1999) look at lottery winners and estimate that unearned income reduces labor earnings and savings rates. Autor and Duggan (2010) find that the income effect from a non-work contingent veterans' disability insurance decreased labor-force participation among Vietnam veterans.

[10] The changes occurred during the first week of a sick case so the difference in total compensation before and after the reform is decreasing for each day that a spell exceeds the 7th day. To phrase it more "economically"; asymptotically the reform did not change the compensation for a sickness spell (but perhaps luckily, as Angrist and Pischke (2009) points out, "real life does not play out in asymptotia", p.209).

[11] Incomes over the cap of 7.5 times the yearly price base were not compensated.

sickness. In the private sector, the replacement rate was 90 percent after the reform. The exact pre-replacement rate is unknown as it was based on job characteristics and collective agreements.[12] Local government workers received sickness benefits from the public system and in addition they got paid sick leave from their employers as a result of collective agreements between the unions and the employers' federations. Before the reform, their effective wage replacement rate was 90 percent from the first day of temporary disability. After the reform, local government workers received full wage replacement (100 percent) from the first day because the collective agreements were renegotiated at the same time. Thus, the reform induced an eleven-percentage increase in the replacement rate for local government workers.[13]

Pettersson-Lidbom and Skogman Thoursie (2011) estimate that the reform increased the sickness incidence among directly affected persons but decreased the average duration. In sum, the average net effect implied a reduction of the total number of reported days of sickness absence by 3 per cent. The dissimilar effects on the incidence and the duration reflect that the reform made it less costly for a worker to be absent for short periods once the waiting day was abolished. These types of economic incentives are not present for the population of indirectly affected spouses studied in this paper.

Data

Spousal labour supply is measured by sickness absence in this paper. We match information on start- and end dates for all sick spells in Sweden from the Swedish National Insurance Board with yearly individual information from the data register LINDA that covers 3.3 percent of the Swedish population.[14] We analyse married workers, aged 20–64 with a yearly income above 6 000 SEK, and who are employed in the central governmental sector in 1986

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[12] The replacement rate for a blue-collar worker in the private sector could before December 1987 was dependent on a number of factors such as whether she worked part time or full time, had irregular working hours, was a shift worker or not etc. As a consequence the replacement rate could vary from zero to one (e.g., see the government report Ds S 1986:8). In other words, some blue-collar workers experienced an increase in benefits after the reform and others experienced a decline depending on their job characteristics. White-collar workers in the private sector received benefits from the public system but they were also entitled to paid-sick leave from their employers from collective agreements between the unions and the employers.

[13] All workers in Sweden received a letter with detailed information about the reform from the Swedish Social Insurance Agency (previously known as the National Insurance Board) a couple of months before December 1, 1987. The letter stated that all workers were required to provide information about their number of working days per year in order for them to get the benefits. The reform was also extensively covered in the media: both by the public television and by all big newspapers.

[14] See Edin and Fredriksson (2000) for a general description of LINDA.

to 1990.[15] Two persons are defined as a married couple if both are reported as married, registered in the same household, of different sexes and with an age difference of at most fifteen years. The reform was implemented in December 1987. We therefore exclude all spells that start in one year and end in another year in order to keep treatment constant within all spells.[16] Since only central government workers are included in our sample, their replacement rates were unaltered by the reform. Instead, we exploit variation in the partner's replacement rate, which was affected by the reform in 1987. We compare the outcome for spouses with partners employed in the local or private sector (the treatment group) with the outcome for spouses with partners employed in the central governmental sector (the control group). Relying on variation in the partner's replacement rate allows us to use a difference-in-differences strategy to answer the question whether spousal labor supply is affected by the partner's benefit level. Our final sample is a repeated cross section consisting of 88 929 individuals.

Table 1 displays average values for the individual sample and the corresponding spell data, for the periods before and after the reform. Spousal sickness incidence seems to be unaffected by the reform but spousal duration of ongoing spells is prolonged by 0.4 days relative the control group. The composition of the sample with positive spells in the treatment group is stable over time.[17]

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[15] To be eligible for sickness insurance, a person's earnings have to be at least 6 000 SEK per year. The period we examine is based on that information on sickness absence starts in 1986 and a new comprehensive reform of the sickness insurance was implemented in 1991.

[16] To exclude yearly overlapping cases has a minor consequence for our result: the estimate for the overall effect on spousal sick duration including all spells is 0.353 days compared to 0.384 days when we exclude spells overlapping two years.

[17] We have examined average compositional values for the individual sample. No statistically significant differences prevail between the treatment and the control group between the pre- and post-period.

Table 1 Summary statistics. Average values for the treatment and the control group before and after the reform.

	Treated		Control		
	After (1)	Before (2)	After (3)	Before (4)	Difference-in-difference [(1)-(2)]-[(3)-(4)]
Individual sample					
Yearly incidence of sickness absence	0.65	0.63	0.68	0.67	0.00
	(0.48)	(0.48)	(0.47)	(0.47)	(0.01)
# Observations	40 166	28 347	11 859	8 557	
Positive spells					
Duration of sickness absence spells	5.54	5.35	5.92	5.71	0.40***
	(8.82)	(8.53)	(9.27)	(9.13)	(0.16)
Family size	3.44	3.45	3.36	3.36	0.00
	(1.11)	(1.06)	(1.09)	(1.07)	(0.02)
Wives	0.43	0.42	0.59	0.59	0.01
	(0.49)	(0.49)	(0.49)	(0.49)	(0.01)
Urban	0.39	0.41	0.40	0.42	0.01
	(0.49)	(0.49)	(0.49)	(0.49)	(0.01)
Age	42.46	41.94	42.71	42.40	0.22
	(8.63)	(8.73)	(8.56)	(9.12)	(0.19)
# Spells	74 504	49 661	23 951	16 416	

Notes: Columns (1)–(4) report standard deviations in parentheses. For the difference-in-difference estimates, standard errors clustered on individuals are displayed in parentheses. All sick spells are right censored at 50 days to reduce the effect from outliers. “Before” refers to the period 1986 to 1987 and “After” refers to the period 1988 to 1990. Treated refers to spouses employed in the central government and who are married to a partner affected by the reform, and Control refers to spouses employed in the central government and who are married to a partner unaffected by the reform.

To further motivate our differences-in-difference strategy, Figure 1 displays the development of the average sickness absence duration for spouses in the treatment and the control group, respectively, from 1986 to 1990. Until the reform in late 1987, ongoing spells are shorter on average in the treatment group. After the reform, the duration of spells in the treatment group and in the control group converge to about the same level and the duration even becomes lower for the treated group in the second quarter of 1989. That the duration in the two groups converge to the same level after the reform is important and shows that we do not need to worry about the functional form for the differences-in-difference estimator, an issue raised by Athey and Imbens (2006). Taken together, preliminary results suggest that average sickness duration increases as a reaction to their partners’ higher replacement rate.

Figure 1 Average duration of sick spells Comparing the treatment and the control group for the period 1986-1990, half-year data.

Empirical strategy and results

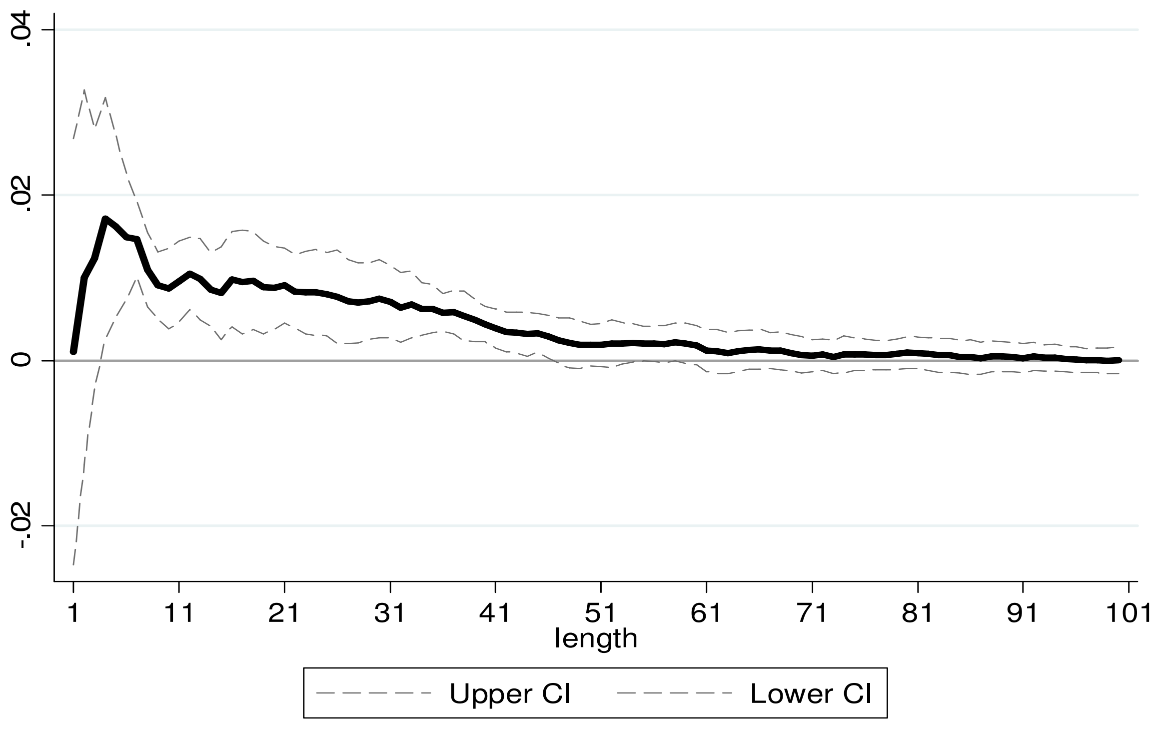
As a starting point we begin the analysis by estimating reform effects on the occurrence of each spell length. By doing so, we investigate whether the reform had any effect on the incidence of sick spells and at what durational levels. We estimate distributional effects using the following linear probability difference-in-difference model:

$$P(Y_{igt} \geq s) = \alpha + \lambda_t + \pi D_{gt} + \delta_s(D_{gt} \times Post_t) + u_{igt}, \qquad (1)$$

where Y_{igt} takes the value one if spousal i has a sick spell of at least s days ($s=1, 2, \ldots, 100$) started in time period t. Time effects are represented by λ_t where t is defined as half-years, D_{gt} indicates whether the spouse is married to a treated partner or not and $Post_t$ takes the value one from December 1987 (*zero* otherwise). δ_s measures the average reform effect on the probability that sick spells lasted at least s days. If the reform had an effect on spouses' probability to start a new spell, we expect a positive difference-in-difference estimate at the first threshold, i.e., δ_1>0. If ongoing spells were affected, thresholds of higher orders will turn out to be statistically significant.

Figure 2 displays one hundred difference-in-difference estimates based on the model in equation (1). At threshold one, it is seen that the reform had no effect on the probability to start a new sick spell. But spells lasting at least 4 to 45 days become more common after the reform, where the largest effect is estimated for the shorter spells. That the incidence is unaffected might reflect that starting a sick absence period is a worse career signal than merely continuing an ongoing spell with an additional day. A scenario consistent with the idea that it is more costly for an employer to adjust the work plan and find a substitute for a new sick case than for a person who has already been on sick leave for a while.

Figure 2 Estimated reform effects on the probability that a sick spell is at or exceeds a given number of days. Upper/Lower CI refers to the upper and lower bound of a 95-percent confidence interval. Length refers to number of days for a sick spell.



Given the result that the reform had no effect on the incidence, we can be confident to continue analysing positive sick spells without worrying about compositional changes. Sick spells are right-censored at 50 days to avoid extreme outliers. We estimate the following difference-in-difference regression model:

$$Y_{igt} = \alpha + \lambda_t + \beta X_{it} + \pi D_{gt} + \delta(D_{gt} \times Post_t) + \varepsilon_{igt}, \qquad (2)$$

where Y_{igt} is the duration of a sick spell for individual i in group g at time t, and X_{it} contains individual covariates included to improve precision and control for potential compositional changes.[18] The total effect on spousal duration, δ, is identified by contrasting the outcome for spouses married to affected partners with the outcome for spouses married to unaffected partners. The total effect can be decomposed into three types of income effects and even though we can't separate between them, we still try to perform various exercises to understand if each of them are important. First, we ignore spells for couples that overlap in time (we drop 8.2 percent of the total sample) to investigate the joint leisure effect. Conditioning on overlapping spells is potentially endogenous but can still help us understand if the effect is of any relevance. We also estimate a probability difference-in-difference model of whether overlapping sick cases become more common after the reform. Second, we test if the effect from the partner's insurance depends on the couple's wealth. We estimate spousal effects for a sample with a common wealth above the median and for a sample with a wealth below the median. The idea is that the higher the wealth is, the less important is the partner's social insurance as couples can self-insure to a great extent.

Our findings are reported in Table 2. For the full sample we estimate that the duration of an ongoing spousal sickness spell increases by on average 0.38 days as a reaction to the partner's increased replacement rate. The effect corresponds to an increase of 7.2 percent compared to the pre-treatment average. We estimate that overlapping sick spells becomes 1.4 percentage points more likely to occur.[19] But the estimate for the total reform effect is only slightly reduced when we ignore overlapping spells. A joint leisure effect seems thereby to be of second order for the overall effect. We find that only spouses in relatively unwealthy families react to the change in the partner's insurance, a result in line with the idea that the partner's social insurance are more important if the family can't self-insure, and that men and women react in a similar way.

To quantity the effect we re-estimate the model for treated spouses married to local governmental workers, as we know that the replacement rate was increased by eleven percent for these partners. The results are reported in the second column of Table 2. The total reform effect is estimated to 0.42 days, which implies an elasticity of spousal sick reporting with respect to the partner's benefit of 0.74, corresponding to three-quarter of previously estimates

[18] The covariates are age, whether the spouse is a wife, lives in an urban area and the number of household members. Including these covariates hardly change the estimate of the reform effect.
[19] With longer durations, one should expect a natural increase in overlapping sick cases that is unrelated to a joint leisure effect.

of the own labor supply elasticity.[20] Overlapping cases are once again found to be less important for the overall estimate while family wealth is important for whether the spouse reacts to the partner's benefits or not.

Table 2 Estimates of the reform effect on the duration of sick spells.

	Full sample	Local government workers
Total Income Effect (IE)	0.384***	0.423***
	(0.126)	(0.141)
	[5.354]	[5.228]
Ignoring cases with overlap	0.336***	0.360***
	(0.004)	(0.127)
	[4.861]	[4.701]
Probability of overlap	0.014**	0.013***
	(0.004)	(0.005)
	[0.072]	[0.078]
Family Wealth Above Median	0.233	0.356
	(0.199)	(0.225)
	[5.334]	[5.336]
Family Wealth Below Median	0.483***	0.448**
	(0.165)	(0.181)
	[5.370]	[4.832]
Men	0.372**	0.446**
	(0.179)	(0.186)
	[5.268]	[5.137]
Women	0.449***	0.565*
	(0.181)	(0.303)
	[5.472]	[5.584]
Spells	164 532	99 094

Notes: All estimates represent a difference-in-difference estimate from a separate model. Standard errors clustered on a firm level and displayed within ( ). ***,**,*, denote statistically significance at the 1, 5 and 10 percent levels, respectively. The pre-treatment average is displayed within [ ]. All models control for county, age, sex and the number of household members. When ignoring overlapping sick cases the sample size is 151 024 for the full sample and 90 392 for the sample of local government workers.

The final question we ask us is whether the positive durational response is an outcome of systematic cheating of the temporary disability insurance. For instance, the insurance can be used to prolong the weekend by taking sick absence on a Friday or a Monday. While no direct

[20] The lack of effect on spousal incidence implies that the elasticity measure the elasticity of spousal sick report with respect to the partner's benefit and not just the elasticity of spousal sick duration with respect to the partner's benefit. The relative effect for workers in the local governmental sector is 0.423 days. Comparing to the pre-treatment average of 5.228 days, the increase is 8.1 percent so the elasticity equals 8.1 divided by 11.

test is applicable for the question, we examine if the distribution of start- and end dates of spousal sick cases are altered by the reform. If longer spells stems from cheating we expect to find different effects on the probability to start or end a spell at different days of the week. Table 3 displays weekday specific reform estimates for the probability that a sick spell starts or ends on a given weekday. Results provide no evidence that the weekly distribution of start and end days was affected. We take this as tentative evidence that the previously estimated spousal reaction is not associated with a cheating behavior.

Table 3 Weekday specific estimates for the probability that a spell starts or ends on a given day.

	Start	End
Monday	0.002 (0.006)	-0.006 (0.004)
Tuesday	-0.002 (0.005)	-0.001 (0.004)
Wednesday	0.000 (0.005)	0.008* (0.005)
Thursday	-0.004 (0.005)	-0.002 (0.004)
Friday	-0.001 (0.004)	-0.002 (0.006)
Saturday	-0.002 (0.004)	-0.002 (0.002)
Sunday	-0.001 (0.001)	0.004* (0.003)
Observations	164 532	164 532

Notes: Each estimate represents a difference-in-difference estimate for separate models. Standard errors are clustered on a firm level. ***,**,*, denote statistically significance at the 1, 5 and 10 percent levels, respectively. All models control for county, age, sex and the number of household members. All estimations are based of the full sample. Standard errors clustered on a firm level and displayed within ( ).

Conclusion

We conclude that spousal labor supply is influenced by the partner's temporary disability insurance where an increased replacement rate for the partner leads to longer spousal sick spell durations. While we find that a joint leisure effect has a minor impact we are not able to pin down the relative importance of a direct income effect and an insurance effect – a question for future research to answer.

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