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The effect of employer incentives in social insurance on individual wages

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WORKING PAPER 2009:13

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ISSN 1651-1166

The effect of employer incentives in social insurance on individual wages[a]

by

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4th June, 2009

Abstract

Several studies have documented that employer incentives, in form of experience rating, co-insurance or deductibles, could decrease the social insurance usage. Such employer incentives may though have unintended side effects, as it gives employers incentives to transfer the costs to their workers, affecting individual wages and inducing cream skimming. Side effects which have been given limited attention. This paper aims to fill one part of this gap in the literature. The effect off employer incentives on individual wages is estimated using a reform in January 1992, which introduced an employer co-insurance system into the Swedish sickness absence insurance. The analysis based on a long population panel database, including survey information on hourly wages, gives no support of any important individual wage effects from the co-insurance reform. This is not a result of lack of variation in individual wage increases, nor is it a result of large standard errors.

Keywords: Wage; employer incentives; co-insurance; sickness absence; work absence; social insurance.
JEL-codes: J39; C23; H55; I18.

[a]This paper have benefited from useful comments from Patrik Hesselius, Per Johansson, Erik Grönqvist and Michael Svarer, as well as seminar participants at University of California at Los Angeles, IFAU and Tinbergen Institute. This paper was written in part when I visited University of Southern California supported by the Tom Hedelius Foundation and in part when employed by VU-Amsterdam. The financial support of the Swedish Council of Working Life and Social Research FAS (dnr 2004-2005) is acknowledged.
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1 Introduction

Experience rating, co-insurance and other types of employer incentives are key components of many social insurance systems. Employer incentives is, for instance, present in workers compensation/disability insurance in USA, Canada, Australia, Netherlands, and New Zeeland, in unemployment insurance in USA, and in sickness insurance in Germany and Sweden. In these schemes the insurance tax rate each firm pay is adjusted upwards or downwards to reflect the costs of the insurance claims made by their workers, and/or the firm is responsible to pay parts of the benefits directly to their workers. The main idea behind these policy instruments are to correct the incentives faced by employers in order to avoid inefficiently high social insurance take up rates. For instance, if workers are covered by social insurance employers have less incentive to reduce temporary lay offs, and less incentive to improve the work environment, as the government is responsible for paying social insurance benefits. The literature mainly confirms that employers incentives indeed can decrease the social insurance usage.[1] These employer incentives may though have unintended side-effects. In this paper I investigate whether employer incentives in social insurance affect individual wages.

Employer incentives in form of experience rating and co-insurance introduce a direct cost for employers when the insurance is used by their workers. Besides taking actions to decrease the take up rates, employers have other ways to avoid this direct cost. Specifically they may shift over the costs to the workers by adjusting individual wages, giving insurance prone workers lower wage increases. If such wage effects are present in health related insurances like disability insurance and sickness insurance, workers with worse health will pay the employers direct costs through lower wages. It will not only have large distributional effects, it will also transform the employer incentives into worker incentives. The direct costs also provide employers with incentives to engage more in

[1] For studies on experience rating in unemployment insurance in USA, see Topel1983, 1985, Deere (1991), Card and Levine (1994), Anderson and Meyer (1994), Anderson and Meyer (1994) and Jurajda (2004). For studies on disability insurance and sickness insurance in Canada see Bruce and Atkins (1993), Hyatt and Thomason (1998), and from Netherlands in de Jong and Lindeboom (2004) and Koning (2004), and finally from USA disability insurance in Ruser (1985), Moore and Viscusi (1989), Ruser (1991), Thomason (1993) and Ruser (1993).

cream skimming, avoiding to hire workers with worse health and fire workers with declining health status. A final side-effect is that employers may try to decrease workers access to the insurances, by contesting individual insurance claims.[2]

These side-effects have largely been ignored in the empirical literature. For social insurance, four exceptions are Anderson and Meyer (2000), Hyatt and Kralj (1995), Thomason and Pozzebon (2002), and Harcourt et al. (2007). Anderson and Meyer (2000) find wage effects and that employer's claim-contesting rate increases as a result of experience rating in unemployment insurance. The results in Hyatt and Kralj (1995) and Thomason and Pozzebon (2002) suggests that employers claim-contesting rate increases as a results of experience rating in Canada disability insurance. Finally, Harcourt et al. (2007) find that experience rating induces firms to more often discriminate against insurance prone workers in their hiring procedure. Related studies are also Gruber (1994) and Baicker and Chandra (2005) who study the individual wage effects from introducing mandated maternity benefits and from growth in health insurance premiums, respectively. This quite limited evidence is unfortunate, since all effects of different incentives have to be taken into account in order to design an optimal insurance.

This paper aims to fill one gap in this literature. I estimate the individual wage effects from an employer co-insurance reform in the Swedish sickness insurance in January 1992. The sickness insurance replaces forgone income due to temporary health problems. Prior to 1992 the benefits were financed by uniform pay-roll taxes and all benefits were paid directly from the government. The reform in January 1992 gave employers the responsibility to pay the full cost for all absence during the first fourteen days of each absence period among their workers. As the incidence of short-term absence varies substantially, the reform increased employers cost of some workers, and for others the costs were reduced.

The individual wage effects are estimated using a long population panel database. The data set have several features which makes it especially suitable for investigating individual wage effects. It includes a large set of individual variables. The individuals can

[2]The employers may also try to discourage workers from submitting claims, or delaying submitting information to the insurance authority.

be followed over a long time period. Each worker can be matched to its current and past employers. The data set is also beneficial since it includes survey information on actual wages, and not wage created from annual earnings and some measure of hours worked. There are also very detailed information on the absence of each individual, including the start and end date of every single absence spell. Since the employer co-insurance cost depends on the number of absence days, I can infer the co-insurance cost the employers have for each worker. In this way we can follow the absence decisions, employment status and the nominal wages for each individual several years before as well as after the reform. All these features of the data allow us to deliver more credible evidence of the individual wage effects.

Besides offering new evidence to the previous limited evidence this study contributes in other ways. First of all, we provide evidence for Swedish sickness insurance, which resemblance many workers compensation and disability insurances around the world. Previous evidence on the other hand is for other types of insurances, and as there is no reason to expect that the effect is the same across insurances this study contributes with valuable insights. Second, previous evidence is concentrated to USA, where wages are more often bargained on individual level compared with many European countries. Our study therefore contribute with estimation results that are very relevant for labor markets with somewhat less degree of individual wage bargaining, such as in many European countries. As comparison, Nilsson (1993) estimate that locally bargained wage increases accounted for 45 percent of the total wage increases in Sweden, in other words individual wage bargaining is an important feature also on the Swedish labor market.

The paper is outlined as follows. Section 2 outlines a simple bargaining model, which can be used to analyze the expected effects of employer incentives in social insurance. The model is set up in two stages with exogenous respectively endogenous sickness absence. It gives several important insights that guide the empirical model. Section 3 describes Swedish sickness insurance and the employer co-insurance reform in 1992. Section 4 presents the empirical strategy, and the main results as well as extensive robustness analysis are presented in section 5. Finally section 6 concludes.

2 Theoretical model

The purpose of the theoretical model is to analyze how employer incentives in form of a direct tax costs for all absence within the firm is expected to affect individual wages and individual sickness absence. The focus is on a mandatory public insurance system where all workers are entitled to benefits, regardless of the size of the tax cost. First a model with exogenous sickness absence considered and then is the model extended to allow for endogenous sickness absence. The simple bargaining model gives a couple of important insights, which is used to guide the empirical model and to interpret our results.

2.1 Model with exogenous sickness absence

The basic set up is as follows. Each firm employs one worker which is permanently attached to the firm. The firms produce one good using labor as the only input, and for simplicity it is assumed that they operate using a constant return to scale technology. The price of the good is further normalized to one. The permanently attached worker has a pre-specified contract of normal working time, h. The contracted working time is set by the labor market institutions, and is therefore taken as exogenous by both the firms and the workers. The productive working time is then the contracted time minus the time the worker is absent from work, s.

The firm has several costs, in addition to the labor cost which is simply the number of hours worked times the hourly wage, w. They have a fixed cost, c. Further a direct cost, τ, for each hour their single worker is absent from work. If $\tau = 0$ it corresponds to a social insurance system with no direct employer incentives. The firms profit function is then

$$\pi = A(h-s) - w(h-s) - \tau s - c, \tag{1}$$

where A represents the productivity of the firm.

The worker receives utility from consumption and leisure. Consumption equals the sum of income from work, and the income in form of social insurance benefits collected while being absent from work. Leisure equals the number of absence hours. I assume the

following utility function

$$u = (h-s)w + bs + \delta \ln(s), \tag{2}$$

where b is the hourly social insurance benefit level, and δ is the value the worker places on leisure. Assuming $\delta > 0$, we have that the worker values leisure but at a declining rate.

The worker and the employer bargains over the wage, and I assume that the outcome of the bargaining game is given by the Nash bargaining solution. The firms agreement point π_0 is assumed to be zero, and the workers agreement point u_0 is assumed to be

$$u_0 = bh + \delta \ln(h). \tag{3}$$

The utility the worker gets if the work is terminated, under the assumptions that the worker then can collect social insurance benefits corresponding to full working time.

Introducing β as the bargaining power of the worker. The solution to the Nash problem is then given by

$$w = \arg\max[\pi]^{1-\beta}[u-u_0]^{\beta}. \tag{4}$$

Solving for the first order condition for the maximum gives a closed form solution for the hourly wage, w, as

$$w = A\beta + b(1-\beta) + \frac{\delta(1-\beta)(\ln h - \ln s)) - c\beta - s\tau\beta}{h-s}. \tag{5}$$

Note that the wage is increasing in the workers bargaining power, the productivity of the firm, the social insurance benefit level, the workers value of leisure, and decreasing in the costs associated with sickness absence. As individual wage effects is the topic of this paper, we more closely investigate how the wage depends on the sickness absence rate, s. We have

$$\frac{dw}{ds} = -\frac{cs\beta + hs\tau\beta + \delta(1-\beta)(h-s-s\ln h + s\ln s)}{s(h-s)^2}. \tag{6}$$

This expression provides several interesting insights. The worker suffers a ”wage

penalty" for each day of absence.[3] The effect goes through two channels. It is increasing in the fixed cost for the firm, c, and in the direct tax cost, τ, associated with sickness absence. Naturally if the tax costs the employers have to pay is larger we expect a larger individual wage effect. The wage penalty effect which goes through c is less expected and something that might have been missed without a formal model. The intuition is however straightforward. If the worker is often absent the fixed cost per actual hour worked is larger, making the firm less profitable which in turn affect wages. It means that even if the tax cost τ is zero we expect to find a negative relation between the wage and the sickness absence. Note that this conclusion is made even under assumption that there are no additional costs associated with sickness absence. In a real world economy one could think about costs associated with, for instance, finding replacement workers. If such costs are present it would be another reason to find a negative relationship between wages and absence. It is also clear that the "wage penalty" depend on the bargaining power, β. The wage penalty is low for individuals with low bargaining power.

2.2 Model with endogenous sickness absence

In the above model the individual sickness absence was assumed to be exogenous. However, when the worker decides to go to work or not, for example when having a cold, it is reasonable to believe that they take any wage effect from being absent into consideration. The model is therefore extended into to a simple game allowing for endogenous sickness absence. The set up of the game is as follows; in the first step the worker decides their sickness absence, and in the second step of the game the worker and the employer bargains over the wage.[4] We further assume that the worker have full information of the outcome of the wage bargaining. The solution to the second step is thus the same as for the model with exogenous sickness absence.

Now consider the first step of the game. In the absence decision the worker faces a trade-off, higher sickness absence means increased utility from leisure, but also decreased

[3] Note that the second part of the expression $\delta(1-\beta)(h-s-s\ln h+s\ln s)$ always is positive, as δ, the value of leisure, is assumed to be positive, β, the individual bargaining power, is between zero and one, and because h, the contracted number of hours, is larger or equal to s, the number of hours of sickness absence.

[4] One could also consider a repeated game. The solution to our simple game would then be the equilibrium solution to the repeated game.

consumption. The solution to this optimization problem is found, by substituting the wage as a function of sickness absence in equation (5) into the worker utility function in equation (2), the utility is then only a function of sickness absence and for the worker exogenous variables

$$u = (h-s)(A\beta + b(1-\beta)) + \delta(1-\beta)(\ln h - \ln s)) - c\beta - s\tau\beta + bs + \delta(\ln(s) \qquad (7)$$

Solving the first order condition gives us a closed expression for the sickness absence rate

$$s = \frac{\delta}{A - b + \tau}. \qquad (8)$$

This expression shows the expected relations. The absence rate is decreasing in the productivity of the firm A, since higher productivity implies a larger production loss if absent from work and thereby a higher wage. The absence rate is further increasing in the workers value of leisure, δ, and the social insurance benefit level, b. This is natural since they both increase the value of being absent from work. The absence rate is decreasing in the size of the employer incentives tax, τ. If the tax is high it implies a larger wage penalty and thereby increases the cost of being absent from work. It is also clear that we predict that individuals who value leisure to a high degree, will be the once who relatively more often is absent from work both in a world with small respectively large employer incentives. The endogeneity of the absence level of course also have important implications for the specification of the empirical model.

To summarize, the models predict that employer incentives in form of a direct tax costs for all absence within the firm affect individual wages, and that the effect is relative to the absence level of each individual. The model also shows that due to fixed costs we expect a negative relationship between wages and absence even without such direct costs. Furthermore, it shows that the absence level should be treated as endogenous. These two last points have important implications for the empirical model.

3 Institutional background and the reform in 1992

3.1 Wage bargaining in Sweden

Any paper investigating individual wage effects in Sweden have to discuss the so called 'Swedish model'. The Swedish model is an often used term for describing the institutions in the Swedish labor market. Some key features are/were centralized collective wage bargaining and extensive use of active labor market policy. Collective bargaining aimed at promoting wage equality. But this stylized description is however not fully accurate. Historically there have always been wage bargaining at different levels, including local and individual wage bargaining. For example Nilsson (1993) estimate that locally bargained wage increases accounted for 45 percent of the total wage increases. Wage-setting institutions have also changed during the last three decades. The degree of centralized bargaining started to decrease in the beginning of the 1980's, see e.g. Edin and Holmlund (1995). The wage data used in this paper further demonstrate that individually bargained wage changes are important.

There are also large differences between sectors in the bargaining power as well as in the degree of individual wage bargaining. In general, individual wage bargaining is more important for highly educated workers and workers employed in the private sector. The wages for public servants is more often dictated by collective agreements. It suggests that any individual wage effects should be more prominent for highly educated in the private sector. As we theoretically expect larger wage effects for individuals with high bargaining power, this also suggests larger individual wage effects for highly educated workers. Detailed analysis of heterogeneous effects is therefore performed.

3.2 Swedish sickness insurance

This section presents the main feature of the Swedish sickness insurance during the research period (1989-1994). Sweden has compulsory national sickness insurance. It is mainly financed by a proportional payroll tax and replaces earnings forgone due to (temporary) health problems that prevent the insured worker from doing his regular work tasks. The benefits could be collected for any health problem, ranging from a cold to a serious

work related injury. Sickness benefits from the public insurance are and have been generous in an international comparison. This can for instance be shown by the fact that most workers received 90 percent of their lost income from the first day in the late 1980's. A benefit cap excluded workers at the very top of the income distribution from receiving the full 90 percent. Most Swedish workers were, however, also covered by negotiated sickness insurance programmes regulated in agreements between the labor unions and the employer confederations.

The public insurance does not verify claimants' eligibility during the first benefit week. At the start of a spell, the worker has to call the public social insurance office (and the employer) to report sickness. The individuals are then entitled to collect benefits from the first day of their absence spell.[5] Within a week, at the latest on the eighth day of sickness, the claimant should verify eligibility by showing a doctor's certificate that proves reduced work capacity due to sickness. The public insurance office judges the certificate and decides about further sick leave. The public insurance had until recently no limit to how often or how long benefits would be paid. Many sickness absence spells continue for more than a year. These spells end mostly in disability insurance, early retirement or in old age retirement.

3.3 The 1992 employer co-insurance reform

Before 1992 the government was responsible for paying all sickness absence benefits. Every employee could file a claim and receive benefits directly from the government if they had temporary health problems. These benefits were financed by a uniform proportional pay-roll tax. In January 1992 the system was changed, and employers were obliged to pay sickness benefits for their own workers during the first two weeks of every sickness absence period. Hence, these benefits are denoted sickness pay, and we will refer to absence during the first two weeks of every absence period as short-term absence, and all other absence as long-term absence. Since there were no qualifying day in 1992, this meant that employers were given the responsibility to pay sickness pay even if the worker was absent from work for a single day. For absence spells longer than two weeks the

[5] In 1993 was a qualifying day introduced.

government continued to pay the benefits in the same way as before the reform. In return the social insurance part of the pay-roll taxes was reduced from 10.1% to 8.2%. The new system can therefore most accurately described as an employer co-insurance system, where the financial costs for a single worker claim is divided between the government and the employer.[6]

The government declared several reasons for changing the system. First, there were budget reasons. Second, administrating every short-term sick absence were an administrative burden for the insurance system. Third, it was believed that the reform would induce the employers to improve the work environment and increase the firms monitoring efforts. Fourth, it was intended to make the insurance fairer. Employers in general have more information about their workers compared to the government. It was therefore believed that employers would be able to make more accurate benefit payments, which would make the insurance more fair.

The reform has several features, which makes it suitable for investigating individual wage effects. The reform was rapidly implemented, and thus individuals had small possibilities to change their behavior before the reform was implemented. It is also reasonable to expect costs associated with short-term absence to be important for the firms. The relative individual short-term absence is quite stabile over time, and in contrast with long-term absence, workers with regular spells of short-term absence usually stay in the workplace. Combined with the fact that short-term absence varies a lot between individuals, it means that the employer co-insurance reform introduced large stable insurance cost for some workers and small stable insurance costs for other workers. The employers therefore have large incentives to shift over the cost, introduced by the reform, to their workers

4 Data

The data set used in the analysis comes from several different databases. From Statistics Sweden we have a set of socio-economic variables (e.g. age, sex, income, immigration status and employment status), and also information that allows us to match each worker

[6]The system was extended in 1997, requiring that employers pay sickness pay during the first four, instead of two, weeks of absence.

to it current as well as past employers. I have sickness absence data from the Swedish Social Insurance Agency (SSIA). The work absence database covers all absence periods for which sickness benefits are paid from the government. Before the reform in 1992 forgone earnings due to work absence were replaced from day one of each spell, and thus include the register information on all absence due to sickness before the reform. Unfortunately didn't the government collect information on the sickness pay paid by the employers after the reform. This means that we have no information on short-term absence, i.e. absence up until day 14 of every absence spell, after the reform. Long-term absence data is on the other hand available both before and after the reform.

Our data also includes survey data on wages from Statistics Sweden's wage statistics, consisting of high-quality information on actual wages, and not wages created from annual earnings and some measurement of hours worked. These wage data are collected by Statistics Sweden in cooperation with employer organizations, and includes the whole public sector, all large private firms and a random sample of small firms (firms with less than 200 workers). In total it cover about 50 percent of all private sector workers.[7]

In the analysis wage data for 1989-1994 and sickness absence data from 1986 and onwards is used. The sample consists of all workers in working age (25-55), who worked in the same firm during two consecutive years. Working is defined as having income above one base price amount, and collecting no unemployment insurance benefits. The reason for this is that we want to focus our analysis on wage effects, and rule out any variation in wages due to individuals changing firm. The analysis is restricted to individuals working at least three consecutive years and for which we have wage data.[8] I also exclude some extreme observations, those with 20 percent wage increase/decrease and/or more than 100 days of short-term absence on average. Extensive robustness analysis shows that our results are quite insensitive to these restrictions.

[7] As with all data sets there are some problems with the wage data-set; the measurement period differs between sectors, the type of payments included in the hourly wage differs between the sectors, and the sampling scheme for the private sector have changed over time. However these problems are all mitigated since we only compare the wage increase between two years for individuals who stay at the same firm.

[8] We impose this condition since we study the wage increase using sickness absence lagged two period as instrument for current absence, and the difference in absence lagged two period as an instrument for the difference in absence. In order to take the wage difference and observe lagged sickness absence we need that the individual worked four consecutive years.

Table 1: Summary statistics for the main sample used in our analysis.

		Wage		Short-term absence	
	Nr. Obs.	Mean	Std.	Mean	Std.
1990	640,577	13,960	4220	9.0	11.1
1991	760,507	13,880	4140	8.5	10.8
1992	777,297	14,550	4140		
1993	847,711	14,630	4200		
1994	878,611	15,580	4840		
For 1991					
Central government	184,392	14,960	3870	8.3	10.8
Regional municipality	195,145	13,350	4380	8.6	10.7
Local municipality	266,631	12,470	2670	9.6	11.4
Private Blue-Collar	19,603	11,630	1530	10.6	12.4
Private White-Collar	94,736	17,320	5180	5.4	7.9
Female	485,702	12,580	2570	9.7	11.4
Male	274,805	16,180	5240	6.4	9.3
Non-immigrant	704,809	13,910	4130	8.4	10.6
Immigrant	55,700	13,460	4250	10.5	12.5
Age -30	71,462	13,330	3660	9.0	11.1
Age 30-45	382,036	13,560	3590	8.7	10.8
Age 45-	307,009	14,410	4780	8.2	10.8
Absence 0-10 days	541,136	14,330	4500		
Absence 10-20 days	126,569	13,060	3060		
Absence 20 days	92,802	12,390	2240		

Notes: Wages is monthly full-time wages in SEK (not deflated). Absence is yearly absence in days. Short-term all days from day 1-14 of every spell. Sector of employment is defined using Statistic Sweden's wage statistics.

4.1 Descriptive statistics

Table 1 presents some descriptive statistics over wages and absence for the individuals in our sample. The fact that a population database is used is reflected in the large number of observations. Also note that there are more females than males in our main sample. This is because females more often work in public sector, and we observe wages for everyone working in the public sector, but only for sub-set of everyone working in the private sector. The summary statistics show the expected patterns. Males, non-immigrants, and more experienced workers have higher wages. There is an gradual increase in the mean wage during the period. Private white-collar workers have the highest wages. There is a clear correlation between wages and sickness absence. Those who are more often absent from work earn substantially less than those who never is absent from work. The descriptive statistics for the absence data also show the expected patterns. Females, immigrants and older workers are more often absent from work. Finally, is the absence much higher among blue-collar workers compared to white-collar workers.

4.2 Is there any wage and absence differences?

The focus in this paper is to investigate whether the employer incentives introduced by the co-finance reform affected individual wages. Two important questions is therefore how large the variation in absence and wage increases are within firms: that is if everyone in the same workplace receives the same wage increase? In order to answer these questions I have produced three figures. *Figure 1* displays the histogram for short-term absence in 1991, and *Figure 2* and *Figure 3* presents the residuals from regressions for short-term absence in 1991 respectively for the wage change between 1992 and 1991. In these regressions include controls for gender, immigrant status, number of children, education level, type of education, sector of employment and workplace fixed effects. So that all focus is on the size of the within firm variation.

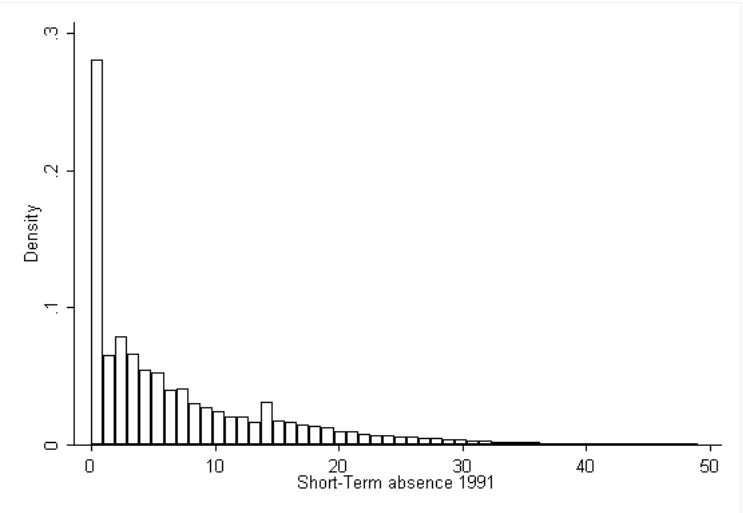

Figure 1: Histogram for short-term absence in 1991.

These figures clearly show that there is large variation in both the sickness absence and in the wage changes. *Figure 1* shows that there is a considerable amount of workers who is never absent from work a given year, whereas there are some workers who are absent more than 20 days a year. The large variations imply substantial differences in employer costs. Consider a worker who works about 220 days a year and is absent 20 days. Take a mean replacement rate of 80 percent. The employer provided sickness pay then amounts to about percent 7 percent of the wage cost. One could also note the spike for 14 days of short-term absence. The reason for this is that short-term absence is defined as the total

number of absence days up until day 14 of every absence period (the only absence that is covered by the employer co-insurance). It means that an individual that have one single absence spell of 14 days or longer will have 14 days of short-term absence. As apparent from *Figure 2* these large absence differences also persists after controlling for a large set of control variables, including workplace fixed effects.

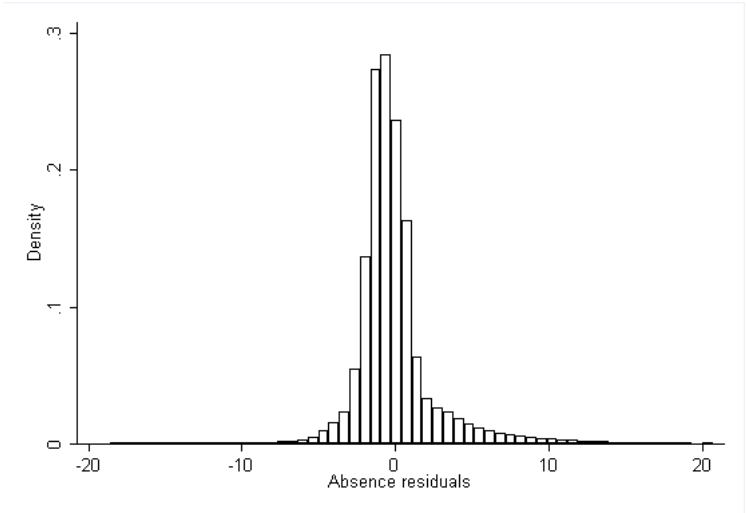

Figure 2: Histogram for short-term absence residuals (1991).

Similarly Figure *Figure 3* displays large variation in the wage increases, even after controlling for a large set of variables, including workplace fixed effects and education level. This figure together with the institutional details in Section 3 give a clear indication that there is room for individual wage bargaining on the Swedish labor market.

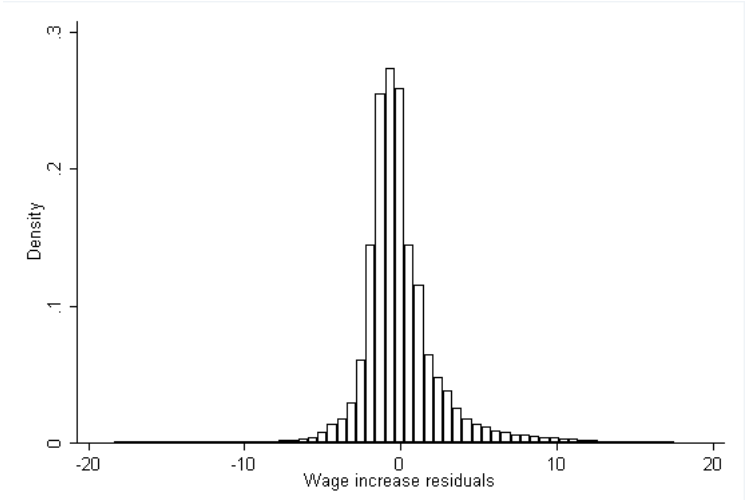

Figure 3: Histogram for wage increase residuals (1992-1991).

5 Empirical strategy

From the theory it follows that the individual wage, w, is likely to depend on productivity A, the individual social insurance benefit level b, the individual value of leisure δ, the individual bargaining power β, the fixed cost c and the tax cost associated with sickness absence τ. Furthermore, the wage effect steaming from the fixed costs c and the tax costs τ is directly related to the sickness absence, S, for individual i. Without loss of generality the wage effect steaming from individual bargaining power, the benefit level and the leisure value can be separated into a fixed individual part α_i and a time changing individual part v_{it}. The wage for individual i in time period t is then

$$\ln w_{it} = \alpha_t + \gamma_c S_{it} + \gamma_\tau \tau_t S_{it} + \alpha_i + v_{it}. \tag{9}$$

Here γ_c and γ_τ measures the impact of sickness absence on the individual wage going through c and τ, respectively. My main interest is to estimate γ_τ, which measures the causal effect of an additional day of employer paid absence on individual wages. If $\gamma_\tau < 0$, it means that after the co-insurance have been implemented those often absent suffer an wage penalty for each day they are absent from work.

In order to consistently estimate γ_τ some identification problems have to addressed; i) how to separate γ_τ from γ_c, ii) the selection problem that α_i most likely is correlated with S_i, iii) the endogeneity problem that w_{it} also affects S_i, and iv) that there may be trends in v_{it} correlated with S_i. In the following I give intuition behind and explain in detail how these four identification problems are addressed.

The first problem arises since the individual absence level is expected to have an causal effect on the individual wage even without employer co-insurance. In order to solve this problem I exploit the exogenous variation in τ the size of employers tax cost associated with each additional day of worker absence, offered by the co-insurance reform in January 1992. In equation (9) it can be expressed as $\tau = 0$ before the reform, and if one normalize τ according to the size of the Swedish co-insurance employers incentives as $\tau = 1$ after the reform. Utilizing the panel structure of our data it is then possible to separate the

general individual wage effects from being absent from any additional wage penalty as a result of the co-insurance reform in 1992.

Second, α_i is most likely correlated with S_i. For instance, it is reasonable to believe that individuals with high productivity and high ambitions, both have a high wage as well as low sickness absence. In addition individuals with high bargaining power likely have both a high wage as well as an employment with good work environment, implying low sickness absence. Controlling for individual heterogeneity is therefore central. I control for all fixed individual heterogeneity by focusing on wage increases. Taking the first difference of equation (9) it follows

$$\Delta \ln w_{it} = \alpha_t - \alpha_{t-1} + \gamma_c(S_{it} - S_{it-1}) + \gamma_\tau(\tau_t S_{it} - \tau_{t-1} S_{it-1}) + v_{it} - v_{it-1}.$$

Consider the wage evaluation for different individuals after the reform. As $\tau_t = 0$ before the reform in 1992 and $\tau_t = 1$ after the reform, we have for 1992

$$\gamma_\tau(\tau_{92} S_{i92} - \tau_{91} S_{i91}) = \gamma_\tau S_{i92}.$$

However, note that this only holds if the employers are able to immediately transfer the full cost to their workers. This is not likely, instead the wage increases are most likely negatively affected for often absent individuals also in 1993 and 1994. It can be taken into account by adding a time subscript on γ_τ, so that $\gamma_{\tau,92}$ measures the wage effect in 1992 and so on. Making the approximation that the absence level is constant, and noting that $\tau_{93} = \tau_{92} = 1$ we have for 1993

$$\gamma_{\tau,93}\tau_{93} S_{i93} - \gamma_{\tau,92}\tau_{92} S_{i92} \approx (\gamma_{\tau,93} - \gamma_{\tau,92}) S_{i93}.$$

Making the same approximation for 1994 gives

$$\Delta \ln w_{it} = \lambda_t + \gamma_c(S_{it} - S_{it-1}) + \gamma_{\tau,92} D_{92} S_{it} + (\gamma_{\tau,93} - \gamma_{\tau,92}) D_{93} S_{it} \tag{10}$$

$$+(\gamma_{\tau,94} - \gamma_{\tau,93}) D_{94} S_{it} + \varepsilon_{it}$$

where D_t is a indicator function taking the value one in year t, and zero otherwise. The hypothesis to test are then that $\gamma_{\tau,92} < 0$, $\gamma_{\tau,93} - \gamma_{\tau,92} < 0$, and $\gamma_{\tau,94} - \gamma_{\tau,93} < 0$, i.e. an initial wage increase effect in 1992, and additional wage increase effects under 1993 and 1994. Note that the equation have been simplified by defining $\lambda_t \equiv \alpha_t - \alpha_{t-1}$ and $\varepsilon_{it} \equiv v_{it} - v_{it-1}$.

The third problem, the endogeneity of S_t, follows directly from the theory, which show that the wage is an important determinant of individual absence. The outcome of interest is the wage increase between time period t and $t-1$. One way to address the endogeneity problem is to instrument current absence level, S_t, using absence lagged two period, S_{t-2}. Unless individuals are extremely forward locking the absence level today should by quite unaffected by future wage increase. If such forward looking behavior is present it is likely of second order. The exclusion restriction is thus likely to be fulfilled. Later it is also shown that lagged absence is highly correlated with present absence, yielding a strong instrument. In a similar way is the difference in absence instrumented using the absence difference lagged two periods.

The final problem arise since we may suspect trends in v_{it} to be correlated with S_i. For instance, the wage increases may vary across sectors and/or across individual characteristics in a way that are correlated with individual sickness absence. If such trends are not taken into account the estimates will be biased. I control for this problem in two ways. First of all, I stepwise introduce different observed variables, like gender, immigrant status, sector of employment, firm controls and residence area into the wage difference equation. We also flexibly interact these variables with calendar time. This will control for all trends in by us observed variables. Second, we include S_t into the model also before the reform, and measure the effect of the reform by S_t time interactions. It will control for trends in by us unobserved variables, as it controls for trends that are correlated with S_t. Our final model to estimate using IV is then

$$\Delta \ln w_{it} = \lambda_t + \beta X_{it} + \gamma_c (S_{it} - S_{it-1}) + \gamma_s S_{it} + \gamma_{\tau,92} D_{92} S_{it} + (\gamma_{\tau,93} - \gamma_{\tau,92}) D_{93} S_{it} \quad (11)$$

$$+ (\gamma_{\tau,94} - \gamma_{\tau,93}) D_{94} S_{it} + \varepsilon_{it}$$

The final model could be interpreted as a Difference-in-Differences (DID) model, as it contrasts the change between before and after the reform in the wage increases for those with high amount of short-term absence compared with those with low amount of short-term absence. If the wage increases jumps downward for those often absent from work at the same time as the reform, this is evidence of an effect of employer co-finance on individual wages.

To summarize, our estimation strategy have several advantages, I have exogenous variation in the absence tax cost, I can control for unobserved heterogeneity in a flexible way, and I handle the endogenous relation between wages and sickness absence. The detailed information on every single absence spell enables a detailed estimate of the co-insurance cost that the employers have for each worker, as the number of short-term absence days. However, the reform also introduces a data problem. As described in Section 4 section we only have data on short-term absence before the reform, i.e. there is no information on short-term absence in 1992,1993 and 1994. Obviously as we don't have information on our endogenous explanatory variable after the reform we cannot obtain proper IV estimates. Instead I run informative reduced form regressions using sickness absence lagged two periods.[9] In addition I estimate informative first regressions, for the years before the reform. In that we way we can, given that the first stage relationship stays the same before and after the reform, reconstruct an IV estimate.

6 Results

6.1 First step estimates

This section presents the first step estimates. Remember that the analysis data set doesn't include information on short-term absence after the reform, and thus no IV estimates could be obtained. As mentioned, instead I run informative placebo first-step regressions for the pre-reform period, and reduced form regressions of the individual wage effect for the full period. Let's start with the first-step regressions for the pre-reform period. For the first step estimates absence data for 1991 is used. The results from different first step re-

[9]For 1994 we have to use sickness absence lagged three periods as short-term absence is only available up until 1991.

gressions are displayed in *Table 2*. Column 1 and 2 report first-step estimates for the level of sickness absence, with and without the control variables. The control variables include individual variables, and controls for municipality and firm fixed effects, the same variables as in the most extended specification of equation (11), the final model. The results show a very strong positive correlation between sickness absence and lagged sickness absence, and the relation is basically the same with or without control variables. Column 3 presents the results when we use absence lagged three periods instead of absence lagged two periods. This result is presented since for 1994 absence lagged three periods is used as instrument, as information on short-term absence is only available up until 1991. Note that the coefficient is almost identical as for absence lagged two periods.

Table 2: First step estimates.

Outcome	(1) S_t	(2) S_t	(3) S_t	(4) $S_t - S_{t-1}$	(5) $S_t - S_{t-1}$
S_{t-2}	0.463**	0.438**			
	(0.00431)	(0.00427)			
S_{t-3}			0.405**		
			(0.00434)		
$S_{t-2} - S_{t-3}$				-0.00977**	-0.0101**
				(0.00145)	(0.00146)
Controls	No	Yes	No	No	Yes
Observations	760,507	760,507	760,507	760,507	760,507
R^2	0.248	0.262	0.212	0.000	0.002
F	11553.6	10501.0	8745.3	45.40	47.42

Notes: The table reports first stage estimates for number of short-term absence days in 1991 and change in absence between 1991 and 1990. Controls include a set of individual variables and section of occupation (2 digits). Standard errors robust to heteroscedasticity and within firm correlation in parentheses. *(**) indicates significance at 5(1) percent level.

The last two Columns of *Table 2* present the first step estimates for the one period difference in absence, with and without controls. There is a strong negative correlation, which means that those who previously increased their absence two years later in general experience a decrease in their absence. This is likely an effect of mean reversion, where the past increase (decrease) reflects a negative (positive) health shock and the later decrease (increase) reflects the temporary nature of the previous shock.

The presented first step estimates clearly show that lagged absence is highly correlated with present absence during the pre-reform period. But the theoretical prediction is that individuals change their absence as an response to the reform. If all individuals is less absent after the reform we still have a valid instrument, but with a lower coefficient for the

first stage relationship between absence and lagged absence. Using the above first stage relationship to reconstruct an IV estimate would then underestimate the true effect. More importantly, in a worst case scenario, those often absent from work before the reform is not so often absent after the reform, and those not absent before the reform starts to be absent after the reform. If such flipping behavior is present, it means that we only have a valid instrument before the reform. From the perspective of my theoretical model this is an unlikely outcome of the reform. It is also possible to perform a informal test of the flipping hypothesis using long-term absence. If the first stage relationship for long-term absence stays the same before and after the reform, it strengthens the argument against flipping behavior.

Table 3: Placebo first step estimates. Long-term absence explained by lagged long-term sickness absence.

	Pre-reform 1991		Post-reform 1992	
	Day 14-	Day 14-56	Day 14-	Day 14-56
S_{t-2}	0.114**	0.149 **	0.083 **	0.097 **
	(0.0033)	(0.0024)	(0.0032)	(0.0025)
Controls	Yes	Yes	Yes	Yes
Observations	735,803	735,803	750,618	750,618
R^2	0.021	0.035	0.016	0.025
F	1235.7	3740.0	668.1	1521.4

Notes: The table reports first stage estimates for number of long-term absence days in 1991 and 1992. Day 14- refers to total number of absence days from day 14 and onwards of each spell, and 14-56 all such days between day 14 to day 56. Controls include a set of individual variables and section of occupation (2 digits). Standard errors robust to heteroscedasticity and within firm correlation in parentheses. *(**) indicates significance at 5(1) percent level.

Table 3 presents the first step estimates for the pre-reform period in 1991 and the post-reform period in 1992 for long-term absence. Long-term absence is defined in two ways; as total number of days from day 14 and onwards of each spell, and also as all absence between day 14 to 56 of each absence spell. The estimates show that in general are lagged long-term absence a less strong predictor of future long-term absence compared with short-term absence and lagged short-term absence. But most importantly are lagged long-term a strong predictor of present absence both before and after the reform. The relationship is somewhat weaker after the reform, but the two estimates are only significantly different from each other when using full long-term absence (day 14-). I am therefore confident in the validity of the instrument both before and after the reform.

6.2 Main results

I now turn to the main reduced form estimates. Before presenting the estimates of equation (11), consider the results from a simple cross-sectional model as presented in Column 1 of *Table 4*. The outcome is the wage in 1992 (the year of the reform) and as explanatory variable we have the absence level lagged two periods. The estimates show that there is a strong significant cross-sectional relation between the wage and lagged absence. We have multiplied the wage with 100 and the coefficient should therefore be interpreted as a 0.5 percent wage decrease for each additional day of short-term absence. For an individual who is often absent this implies a considerable wage effect. However, as previously argued this estimate can reflect both selection as well as a general wage effect from absence, and is not necessarily an effect of the co-insurance reform.

Table 4: Reduced form estimates of the relationship between short-term absence and wages.

	(1)	(2)	(3)	(4)	(5)
Outcome	$\ln w_{i92}$	$\Delta \ln w_{it}$	$\Delta \ln w_{it}$	$\Delta \ln w_{it}$	$\Delta \ln w_{it}$
$D_{92}S_{t-2}$	-0.461**	-0.0094	-0.00091	-0.0013	-0.0014
	(0.0205)	(0.0054)	(0.0047)	(0.0018)	(0.0018)
$D_{93}S_{t-2}$		-0.0212**	-0.0102*	0.00070	0.00061
		(0.0063)	(0.0052)	(0.0024)	(0.0024)
$D_{94}S_{t-2}$		-0.0208**	-0.0064	-0.0073**	-0.0074**
		(0.0052)	(0.0038)	(0.0019)	(0.0019)
S_{t-2}		0.0140*	-0.00079	-0.0026	-0.0017
		(0.0056)	(0.0029)	(0.0019)	(0.0018)
$S_{t-2} - S_{t-3}$		-0.0010	0.0024**	0.0019**	
		(0.0011)	(0.00040)	(0.00040)	
Observations	777,297	3,903,359	3,903,359	3,903,359	3,903,359
R^2	0.052	0.480	0.542	0.605	0.605
Time	Yes	Yes	Yes	Yes	Yes
Individual	No	No	Yes	Yes	Yes
Sector(2 dig)	No	No	Yes	Yes	Yes
Municipality	No	No	Yes	Yes	Yes
Firm	No	No	Yes	Yes	Yes
Time X Ind.	No	No	No	Yes	Yes
Time X Sector	No	No	No	Yes	Yes

Notes: The outcome variable is the wage in 1992 (column1) and the difference between time period t and time period t-1 in the logarithm of the wage times 100. Individual variables include sex, immigrant, age, age squared, type of education and level of education. Standard errors robust for within firm correlation and reported in parenthesis. *(**) indicates significance at 5(1) percent level.

Next, consider our causal estimates of equation (11) presented in column 2-5 of *Table 4*. The coefficients of interest are the three interaction variables, $D_{92}S_{t-2}$, $D_{93}S_{t-2}$, and $D_{94}S_{t-2}$, which measure the reduced form estimate of the additional wage increase

penalty from an additional day of absence introduced by the employer co-insurance reform. Remember that the outcome is the difference in the logarithm of the wage times 100, so that for instance the estimate for $D_{92}S_{t-2}$ in Column 2 of -0.0094 means that one additional day of absence decreases the wage change with about 0.01 percent. It also means that the coefficients for 1993 and 1994 measures the additional wage penalty in 1993 and 1994, respectively. The model in Column 2 includes only our five main variables and a set of time controls. The coefficient for 1992 the year of the reform is negative but insignificant, and the coefficients for 1993 and 1994 are both negative and significant at one percent confidence level. This suggests that the effect of the reform is delayed one year. Column 3-4 present results from models when additional control variables are added stepwise into the model. Adding more and more control variables changes the initial conclusion. The size of the 1993 and 1994 coefficients gradually decreases as more controls are included into the model. The full model, presented in Column 4, includes a full set of individual variables and sector dummies, as well as these interacted with calendar time. In this model only the 1994 coefficient is significant. The estimate for 1993 even has incorrect sign. We obtain the same result from a simpler model excluding the lagged difference in absence, presented in Column 5.

The results for 1992 and 1993 suggest that there is no individual wage effect. The question then becomes if the significant result for 1994 means that there are important wage effect that is delayed two years. If we take the pre-reform period first step estimate and reconstruct an IV. It suggests that one day of additional sickness absence decreases the wage increase with about 0.018 percent.[10] The difference between P75 and P25 of short-term absence is 13 days, which implies that an individual at P75 can expect about 0.24 percent (0.018*13) lower wage change compared to an individual at P25. This can be compared with the mean wage increase in 1992 of 3.4 percent, in other words a quite small effect. Moreover, the average worker in Sweden works about 220 day years, it means that

[10]The reduced form estimate is -0.0073 and the first step estimate is 0.40, which gives an IV estimate of $-0.0073/0.40 \approx -0.018$. Also note that the outcome is measured as the wage increase in percent (the logarithm times 100).

13 days of absence amount to about 4.7 percent of the labor costs.[11]. Compared to this the the wage change effect of 0.24 is very small. All coefficients are also very precisely estimated. Based on these main results, with small and precisely estimated coefficients, we can rule out any sizeable individual wage effects from the co-insurance reform.

Before proceeding to a more detailed robustness analysis I will explore a potential threat with using the co-insurance reform as a quasi-experiment. The beginning of the 1990's was a turbulent period for the Swedish economy. In the late 1980's the unemployment rate in Sweden was extremely low (about 2% in 1988), and by 1994 it had increased to about 8%. It is natural to expect that this affect wages. The worsened economic conditions will decrease workers bargaining power. Crucially, it may affect the bargaining power asymmetrically across workers with different absence levels. As discussed above those often absent include workers with bad health. In a recession it is reasonable to expect that these workers face a higher risk of being fired, and their bargaining power is most likely more negatively affected compared with other workers. This creates two potential problems.

First, the composition of employed workers in the late 1980's will be different compared with the composition in the beginning of the 1990's. It is taken into account by re-estimating the final model for a smaller sample of individuals including only those who are employed during the whole research period (1989-1994). The results from this exercise is presented in *Table 5*. Column 1 restates the main results (full sample), and Column 2 presents the results from our smaller sample of individuals employed during the whole period. The results from the smaller sample differ somewhat from the results from the main analysis. The estimate of the wage effect in 1992 is negative and significant, the estimate for 1993 is positive and insignificant, and the estimate for 1994 is insignificant. The size of all the estimates is very small. These estimates thus give us no reason to alter our conclusion about no sizeable wage effects.

Second, if the bargaining power of insurance prone workers decreases more as a result of the recession it will bias the results towards showing stronger wage effects. Workers

[11] Take a replacement rate of 80 percent. The wage and the benefit are both taxable. We then have labor cost percentage as $13 * 0.8/220 \approx 0.047$

Table 5: Sample selection. Reduced form estimates of the relationship between absence and wages.

	(1) Main sample	(2) Only if working all years
$D_{92}S_{t-2}$	-0.00133	-0.00603**
	(0.00179)	(0.00228)
$D_{93}S_{t-2}$	0.000698	0.00387
	(0.00238)	(0.00341)
$D_{94}S_{t-2}$	-0.00729**	-0.00515
	(0.00193)	(0.00279)
S_{t-2}	-0.00260	-0.00378
	(0.00191)	(0.00263)
$S_{t-2} - S_{t-3}$	0.00187**	0.00236**
	(0.000400)	(0.000579)
Observations	3,903,359	1,142,229
R^2	0.605	0.740

Notes: The main sample is the sample presented in the data section. The smaller sample imposes the additional restriction that the individual should be employed all years between 1989 and 1994. The outcome variable is the difference between time period t and time period t-1 in the logarithm of the wage times 100. Individual variables include sex, immigrant, age, age squared, type of education and level of education. Standard errors robust for within firm correlation and reported in parenthesis. *(**) indicates significance at 5(1) percent level.

that are often absent may simply experience smaller wage increases in the beginning of the 1990's as a result of decreasing bargaining power. This means that our small and precisely estimated insignificant wage effects could be considered as an upper bound on the wage effects. To investigate this conclusion more carefully I now turn to different kinds of robustness analysis.

6.3 Robustness analysis

6.3.1 Placebo regressions

To further analyze the conclusion of no sizeable wage effects from the co-insurance reform I run placebo regressions, which test for any pre-existent trends by interacting S_{t-2} with a dummy for each of the five years used in our analysis. The coefficients for 1991 and 1990 then represent "treatment effects" for non-existent reforms. Any significant estimates for these two years before the reform, indicates a problem with pre-existent trends unaccounted for by our large set of control variables. Column 1 of *Table A-1* presents results for the full sample and Column 2 for the smaller sample of individuals employed during the whole research period. In both models we find insignificant coefficients for 1990 and 1991 the two years before the reform. It seems that the full model is able to account for all pre-reform trends, thereby strengthening our main conclusion.

6.3.2 Effect on firm level?

In the baseline specification it was assumed that the employers could shift their insurance cost over on individual wages. Even if there are individual wage differences, it may be the case that instead of individual wage effects, all workers in high absence firm receives lower wage increases as an result of the co-insurance reform. To test this hypothesis I estimate the same models again, but replace individual absence with firm absence. The results from first step estimates as well as different reduced form estimates is displayed in *Table A-2*. The first step estimates, reported in Column 1, shows a very high correlation between present firm absence and firm absence lagged two periods. The correlation is even stronger than for individual absence.

Next consider the results from the reduced form estimates. Here I simplify the exposition by assuming that the wage effect is the same in 1992, 1993 and 1994. The same simplification is used throughout the remaining robustness analysis. Column 2 reports results from our main model with a full set of control variables. Column 3 presents a nested model including both firm absence and individual worker absence into the same model. The results from these two specifications suggest significant firm level wage effects from the co-insurance reform. However, the results in Column 4 reverse this conclusion. Column 4 reports the results from a placebo regression, where we have interacted firm level absence with a dummy for each year. These results reveal strong pre-existent trends in 1991 and 1990. In addition the sign of the effect for 1992 is now reversed, indicating a positive wage effect of the co-insurance reform. Based on these results I conclude that there is no robust evidence of any important wage effects at the firm level.

6.3.3 Heterogeneous treatment effects

One key assumption for the analysis presented so far is that wages are set at least partly individually. The degree of individualized wages differs a lot between different types of workers. Workers with high education and workers employed in the private sector face more individualized wages, which theoretically suggests larger individual wage effects. In principle it could the case that any important individual wage effects in some sectors or in some education levels are hidden in the insignificant estimates above. I test for this

by re-estimating the model allowing for heterogeneous γ_τ by sector respectively by level of education.

The results from this analysis is presented in *Table A-3* and *Table A-4*. If the hypothesis about only wage effects in sectors and education groups with high degree of individualized wages is true, there should be negative and significant signs for white-collar workers, central government workers, and for highly educated workers. The results for different sectors are inconsistent with this pattern. I find negative significant coefficients for central government, regional government workers and blue-collar workers and significant positive coefficients for white-collar workers. The pattern for different education groups is more inline with the individual wage hypothesis, as expected the coefficients for for the most highly educated groups are significant and negative coefficients, but again the size of estimates are very small. Taken together there is no reason to alter the conclusion that there are no important individual wage effects from the co-insurance reform.

6.3.4 Functional form

The basic model estimated above specifies a linear effect, which may be too restrictive. As an additional robustness analysis I therefore present results from two additional more flexible specifications, including a model with four polynomials of the lagged mean absence, and a second model were the individuals have been grouped into six groups accordingly to their lagged sickness absence. *Table A-5* displays these results. The results from the polynomial model give very similar results as the baseline specification. The linear effect is similar to above and counteracted by a positive and significant second order polynomial coefficient. In addition the second specification with individuals divided into groups accordingly to their lagged absence produces no significant estimates.

6.3.5 Sample selection

The final robustness analysis regards the sample selection. As discussed in Section 4, several criterions have to be fulfilled in order to include the individual in the analysis sample. The main restrictions are that only working individuals who stay at the same firm during at least two consecutive years are used in the analysis. In addition some individuals with extreme wage increases respectively some individuals with extreme sickness absence are

excluded from the sample. In this section I investigate if these restrictions influence our estimates. *Table A-6* presents this robustness analysis. Column 1 restates our main results in order to simplify the comparison. The sample used in the second model excludes all individuals with exactly zero wage increase, since this is a indication of misreporting in the wage survey data. The sample used in model 3 excludes additional individuals with extreme sickness absence, and model 4-5 exclude individuals with additional extreme wage increase. The results from these specifications show that the results are quite insensitive to these different sample restrictions.

7 Conclusions

This paper has investigated whether introducing direct employer incentives in form of employer co-insurance into the Swedish sickness insurance affect individual wages. The reform introduced a direct cost for employers for each day of short-term absence among their workers. Since sickness absence varies substantially between individuals, the reform meant that employer's costs increased sharply for some workers and decreased the costs for other workers. Using detailed information on the absence of each individual, past and current employment, and survey information on wages, we provide a direct test of a wage effect from increased labor costs in form of co-insurance.

The result interestingly shows small and insignificant individual wage effects from the co-insurance reform. Since we are using a population database the estimates are also very precisely estimated. Extensive robustness analyses have also been performed, with respect to placebo regressions, functional form, sample selection and we have checked for wage effects in certain sectors and for certain education level groups. They all support the main conclusion of no sizable wage effects. In addition any bias due to business cycle effects would have been towards showing wage effects. I can therefore rule out any sizeable wage effect from the Swedish co-insurance reform.

These results could be related to the previous scarce evidence on individual wage effects and employer incentives. Anderson and Meyer (2000) find wage effects from experience rating in unemployment insurance, Gruber (1994) find wage effects from mandated

maternity benefits, and Goldman et al. (2005) find wage effects as an result of increased health insurance premiums. Interestingly Baicker and Chandra (2005) find no significant wage effects from a growth in health insurance premiums; instead they find significant effects on hours worked and on individual employment. This study contributes to this literature for at least two reasons. First of all, I provide evidence for Swedish sickness insurance, which resemblance many workers compensation and disability insurances around the world. Previous evidence on the other hand is for other types of insurances, and as there is no reason to expect that the effect is the same across insurances this study contributes with valuable insights. Second, previous evidence is concentrated to USA, where wages is more often bargained on individual level compared with many European countries. This study gives results that are very relevant for labor markets with somewhat more centralized wages, such as many European countries. It is however important to note that our results is not entirely driven by lower level of individual wage bargaining, as our data reveal sizeable wage increase difference among workers within the same workplace, even after controlling for a rich set of control variables.

There are several possible explanations to these precisely estimated insignificant wage effects of the co-insurance reform. Even if firms had no direct tax cost each time their workers are absent from, employers have substantial indirect costs for absent workers. For example costs due to production losses and costs associated with finding a replacement worker. If these costs are very large the additional cost in form of the co-insurance tax may be less important. This conclusion is indirectly supported by the results in Andren and Palmer (2001), Hansen (2000) and Hesselius (2004), which indicate that work absence in general have a large impact on individual wage.

Another possible explanation is that employer regulates their costs by firing or avoiding hiring insurance prone workers. If employers cannot shift the co-insurance cost over to individual wages, they can avoid the co-insurance costs by firing and/or avoiding hiring insurance prone workers. In other words the non-existent wage effects indicate that cream-skimming may have intensified as a result of the reform. Cream-skimming have severe negative impacts on the employment possibilities of insurance prone workers, i.e. workers with bad health. A conclusion that is supported by the results in Harcourt et al. (2007) and Baicker and Chandra (2005), who both find employment effects.

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Appendix

Table A-1: Placebo regressions. Reduced form estimates of the relationship between absence and wages.

	(1) Basic Model	(2) Only if working all years
$D_{90}S_{t-2}$	-0.00555	-0.00756
	(0.00344)	(0.00438)
$D_{91}S_{t-2}$	0.000127	0.000781
	(0.00112)	(0.00121)
$D_{92}S_{t-2}$	-0.00382**	-0.00970**
	(0.00103)	(0.00157)
$D_{93}S_{t-2}$	-0.00214	-0.000825
	(0.00115)	(0.00154)
$D_{94}S_{t-2}$	-0.00804**	-0.00800**
	(0.000901)	(0.00116)
Observations	3,904,703	1,142,229
R^2	0.605	0.740

Notes: The main sample is the sample presented in the data section. The smaller sample imposes the additional restriction that the individual should be employed all years between 1989 and 1994. The outcome variable is the difference between time period t and time period t-1 in the logarithm of the wage times 100. Controls include a set of individual variables, section section of occupation (2 digits), firm fixed effects, and interactions between time and individual and time and of occupation. Standard errors robust for within firm correlation and reported in parenthesis. *(**) indicates significance at 5(1) percent level.

Table A-2: Firm absence. First step and reduced form estimates of the relationship between absence and wages.

Outcome	(1) $Sfirm_t$	(2) $\Delta \ln w_{it}$	(3) $\Delta \ln w_{it}$	(4) $\Delta \ln w_{it}$
$DSfirm_{t-2}$	0.798**	-0.195**	-0.198**	
	(0.000323)	(0.0343)	(0.0345)	
$Sfirm_{t-2}$		0.196**	0.204**	
		(0.0281)	(0.0285)	
$Sfirm_{t-2} - Sfirm_{t-3}$		-0.0942**	-0.0969**	-0.160**
		(0.0187)	(0.0188)	(0.0262)
S_{t-2}			-0.00808**	
			(0.00158)	
DS_{t-2}			0.00197	
			(0.00147)	
$S_{t-2} - S_{t-3}$			0.00330**	
			(0.000396)	
$D_{90}Sfirm_{t-2}$				0.406**
				(0.0577)
$D_{91}Sfirm_{t-2}$				-0.103**
				(0.0376)
$D_{92}Sfirm_{t-2}$				0.160**
				(0.0330)
$D_{93}Sfirm_{t-2}$				-0.129**
				(0.0377)
$D_{94}Sfirm_{t-2}$				-0.0342
				(0.0225)
Observations	760,507	3,903,359	3,903,359	3,903,359
R^2	0.889	0.606	0.606	0.609

Notes: The table reports first stage estimates for number of short-term absence days in 1991 and reduced form estimates for the wage increase between time period t and time period $t-1$ in the logarithm of the wage times 100. Controls include a set of individual variables, section section of occupation (2 digits), firm fixed effects, and interactions between time and individual and time and of occupation. Standard errors robust for within firm correlation and reported in parenthesis.*(**) indicates significance at 5(1) percent level.

Table A-3: Reduced form estimates. Heterogeneous effects by sector of employment.

	Estimate	S.e.
Central Gov.	-0.00767*	(0.00335)
Regional Gov.	-0.00748*	(0.00300)
Municipal. Gov.	0.00301	(0.00249)
Blue-Collar	-0.00700*	(0.00310)
White-Collar	0.00243	(0.00302)
Observations	3,904,703	
R^2	0.605	

Notes: The outcome variable is the difference between time period t and time period t-1 in the logarithm of the wage times 100. Controls include a set of individual variables, section section of occupation (2 digits), firm fixed effects, and interactions between time and individual and time and of occupation. Standard errors robust for within firm correlation and reported in parenthesis. *(**) indicates significance at 5(1) percent level.

Table A-4: Reduced form estimates. Heterogeneous effects by education level.

	Estimate	S.e.
Education Level 1	-0.000349	(0.00198)
Education Level 2	-0.00179	(0.00199)
Education Level 3	0.0000685	(0.00187)
Education Level 4	-0.00187	(0.00196)
Education Level 5	-0.00593**	(0.00174)
Education Level 6	-0.00854**	(0.00189)
Observations	3,904,703	
R^2	0.605	

Notes: The outcome variable is the difference between time period t and time period t-1 in the logarithm of the wage times 100. Controls include a set of individual variables, section section of occupation (2 digits), firm fixed effects, and interactions between time and individual and time and of occupation. Standard errors robust for within firm correlation and reported in parenthesis. *(**) indicates significance at 5(1) percent level.

Table A-5: Reduced form estimates. Polynomial models and individuals grouped by lagged absence.

	(1)	(2)
DS_{t-2}	-0.00840**	
	(0.00199)	
DS^2_{t-2}	0.000265**	
	(0.0000684)	
DS^3_{t-2}	-0.00000296*	
	(0.00000132)	
DS^4_{t-2}	9.56e-09	
	(7.80e-09)	
$DS_{group2,t-2}$		-0.0405
		(0.0282)
$DS_{group3,t-2}$		-0.0627
		(0.0430)
$DS_{group4,t-2}$		-0.0625
		(0.0578)
$DS_{group5,t-2}$		-0.0976
		(0.0745)
$DS_{group6,t-2}$		-0.0918
		(0.115)
Observations	3,904,703	3,904,703
R^2	0.605	0.605

Notes: The outcome variable is the difference between time period t and time period t-1 in the logarithm of the wage times 100. Controls include a set of individual variables, section section of occupation (2 digits), firm fixed effects, and interactions between time and individual and time and of occupation. Standard errors robust for within firm correlation and reported in parenthesis. *(**) indicates significance at 5(1) percent level.

Table A-6: Robustness analysis sample selection. Reduced form estimates of the relationship between absence and wages.

	(1) Basic Model	(2) Exclude if $w_t = w_{t-1}$	(3) Exclude if $S_{t-2} > 40$	(4) Exclude if $w_t - w_{t-1} < -(\bar{w}_t - \bar{w}_{t-1})$	(5) Exclude if $w_t - w_{t-1} < 0$
DS_{t-2}	-0.00223	-0.00272	-0.00445*	-0.00323	-0.00213
	(0.00185)	(0.00183)	(0.00188)	(0.00175)	(0.00183)
S_{t-2}	-0.00247	-0.00262	-0.00106	-0.000426	-0.000714
	(0.00193)	(0.00195)	(0.00200)	(0.00189)	(0.00194)
$S_{t-2} - S_{t-3}$	0.00159**	0.00165**	0.00135**	0.00115**	0.000824
	(0.000407)	(0.000486)	(0.000503)	(0.000422)	(0.000446)
Observations	3,904,703	3,426,057	3,183,205	3,017,082	2,910,583
R^2	0.605	0.584	0.579	0.687	0.708

Notes: The outcome variable is the difference between time period t and time period t-1 in the logarithm of the wage times 100. Controls include a set of individual variables, section section of occupation (2 digits), firm fixed effects, and interactions between time and individual and time and of occupation. Standard errors robust for within firm correlation and reported in parenthesis. *(**) indicates significance at 5(1) percent level.

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