Moral Hazard and Sickness Insurance*

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Abstract

We study if the replacement level in the Swedish national sickness insurance, which replaces foregone earnings due to temporary illnesses, affects work absence behavior. We use micro data and estimate the effects of a major reform, whereby the replacement level during the first 90 days in each absence spell was reduced, on work attendance. To separate out the effect of the reform from any trend in work absence, we distinguish between the implications on the incidence of work absence (i.e., the frequency of spells) and the duration of the spells. We also use a regression-discontinuity approach to estimate the effects on the prevalence of work absence. Finally, we estimate elasticities with respect to the replacement level in the sickness insurance.

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1 Introduction

"Regarding the high absence rate at the Department: Acquiring minor diseases, such as colds or flu, is an act of choice."

Ragnar Frisch, Nobel Laureate and founder of the Econometric Society, in an announcement in 1962 on the notice board at the Department of Economics, University of Oslo, Norway.

The sickness insurance system (SI), which provides compensation for lost earnings due to work absence on the grounds of temporary illness, is one of the largest social welfare programs in Sweden. In 2002, the expenditure amounted to SEK 48.3 billion, or about 2 percent of GDP. Between 1998 and 2002, the work-absence, and hence the costs of the insurance system, increased by almost 75 percent (see e.g. The Economist 26 October 2002). This rapid cost increase and subsequent changes in the labor force have dominated the welfare state debate in Sweden for some time. Among the main themes in this debate has been the issue of to what extent insured workers adapt their work-absence behavior, and their work effort, to the generosity of the sickness-insurance scheme, i.e., the classical welfare state dilemma of moral hazard.

There are two problems in empirically studying moral hazard in sickness insurance.[1,2] The first is unobserved heterogeneity. It is well known that workers with preferences for not being absent from work, or with good health and who therefore have no need for frequent absences, tend to be more productive and earn higher wages. Since the sickness insurance system, like most income security programs, replaces a fraction of forgone labor earnings, it is more expensive

[1] For a general overview of methodological problems in empirical studies on behavior in insurance markets, see Chiappori and Salanié (2000).

[2] Previous empirical studies of absenteeism in economics have used conventional labor supply models (see e.g. Allen, 1981 or Johansson and Palme, 1996 and 2002) or been oriented towards empirical personnel economics (see e.g. Barmby, Orme and Treble, 1991 and 1995). Gruber (2000) analyzes the labor supply effects of the benefit levels in the disability insurance, which replaces foregone earnings due to permanent rather than temporary health problems.

for high-income workers to be absent from work. In measuring the relationship between the cost of being absent and work-absence, unobserved heterogeneity will, therefore, lead to a downward bias of the estimated effect of the program generosity. Second, adverse selection may result in workers with preferences favoring absences choosing more generous insurance programs. This may, on the other hand, lead to an upward bias of the estimated moral hazard effect.

In this study, we use a reform of Sweden's sickness insurance system which came into force on 1 March, 1991 as a source of exogenous variation in the cost of being absent from work. In this reform, the compensation level was reduced from 90 percent of forgone earnings below the social security ceiling to 65 percent during the first three days in a spell of absence and 80 percent between day 4 and day 90. To avoid disadvantageous effects of the reform on the income distribution, the compensation level remained at 90 percent after day 90. Thus, we are able to study how the insured workers change their behavior when a new, less generous policy is introduced. In other words, we can directly study the moral hazard problem.

The design of the reform has at least two testable implications with respect to moral hazard. First, the cost of beginning a work absence period unambiguously increased for all workers due to the reform. To the extent that workers change their work absence behavior as a result of the change in the insurance contract, i.e., moral hazard is present, this effect implies that the *incidence* of work absence (i.e., the frequency of spells) will decrease. Second, there is an unambiguous increase in the cost of *returning* to work after day 90 as a result of the possibility of commencing a new absence spell with an initially lower compensation level after returning to work. If moral hazard is present, workers on long work absence spells will prolong the duration of these spells. Thus, the two testable implications work in different directions with respect to the overall *prevalence* of work absence.

We use individual data containing information on work absence for each of the 730 days in the years 1990 and 1991 for the workers included in the sample. Since the behavior of the same individuals is observed under two different policy regimes, thereby implying a homogenous proportional cost change of being absent from work, our estimates will not be affected by unobserved heterogeneity. An additional advantage with our data is that we do not need to consider potential compositional effects, for example due to the increase in the unemployment rate. Finally, the fact that all workers are covered by the same compulsory insurance program means that we are able to isolate the effect of moral hazard from adverse selection.

First, we analyze the overall change in the prevalence of work absence as a result of the reform. To distinguish the effect of the reform from any trend, or seasonal variation in the work absence rate, we use a differences-in-differences estimator. We compare the change between January/February and March/April 1991 with the corresponding change in 1990. The second difference removes the effect of any seasonal pattern, while the first difference removes any pre-existing trend in work absence, but not the effect of the reform. To investigate the first testable implications of the reform described above, we estimate Cox proportional hazard regression models for the work spells between the work absence periods, i.e., we model the incidence of work absence spells. To test the second implication we, once again, estimate Cox proportional hazard regression models, but now for the *duration* of work absence. To measure the magnitude of the estimated effects, a policy measure, we obtain elasticity estimates for both the incidence and duration of work absence, with respect to the replacement level in the sickness insurance.

2 Sickness Insurance in Sweden

Sweden has a compulsory national sickness insurance replacing forgone earnings due to temporary health problems that prevent the insured worker from doing his regular job. Since it is very hard to judge whether a worker is able to perform his or her regular job, abuse is very lightly monitored during the first seven days of a sickness period. However, a certificate from a physician is required for entitlement to sickness insurance payments from the eighth day in a sickness period. The sickness insurance is financed by a proportional payroll tax.

The compensation level - the proportion of earnings paid to the worker by the insurance system - has been modified on several occasions in recent years. In the major reform covered by our longitudinal data - implemented on 1 March 1991 - the compensation level was reduced from 90 percent of labor earnings below the social security ceiling[3] from the first day in a sickness spell, to 65 percent in the first three days in a spell and 80 percent from day four to day 90. The reform applied to new spells only, i.e., not retroactively to ongoing spells. The 1991 reform of the sickness insurance system was the most important of several budget cuts proposed in the early spring of 1991. The reason for these cuts was an increasing budget deficit and excess aggregate demand.

In addition to the compulsory national sickness insurance, most Swedish workers are covered by negotiated sickness insurance programs regulated in agreements between labor unions and employers' confederations. In general, these insurances replaced about 10 percent of the forgone earnings. We have, however, chosen not to consider these programs in our analysis. The reason for excluding them is that they differ somewhat between different groups of workers and assigning the right insurance to each worker in the sample is complicated.

[3] In 1995, about 6.7 percent of all insured workers had labor earnings above the social security ceiling. For a description of the construction and indexation of the social insurance ceiling, see e.g. Palme and Svensson (1999).

In addition, our policy variable is the compensation level in the national sickness insurance and since the negotiated insurances were not affected by the reform, their exclusion should not affect our results.

3 Testable Implications and Empirical Modelling

As noted in the introduction, the design of the reform of the sickness insurance implies that the change in the cost of absence from work depends on the state of the insured worker, i.e., whether he is absent and, if absent, for how long the work absence period has lasted. We define the *direct* cost of being absent from work (i.e. entering into absence or remaining in absence) as the percentage share of earnings not replaced by the sickness insurance. This direct cost changed from 10 to 35 percent during the first three days in a spell, but remained at 10 percent from day 91 in an absence spell.

We define the cost of *returning* to work as the difference in the percentage share of earnings not replaced by the sickness insurance between remaining in an ongoing absence period and the corresponding share in a new absence spell after returning to work. Since the replacement level is 90 percent irrespective of the spell length before the reform, this cost is zero in all states. After the reform, it is zero only if the duration of the absence spell is less than 3 days; if the duration is between 3 days and 90 days, the cost of returning is 15 percent; and in spells longer than 90 days, it is 25 percent.

An attractive feature of the reform for our purposes is that these cost changes are independent of the income tax rate, which means that the costs defined above are not affected by the 1991 income tax reform. The reform has the following implications for the direct cost and the cost of returning to work:

i. There is an unambiguous *direct* cost increase in beginning a work absence period, due to the reduction in the compensation level for the first 90 days of the period.[4]

ii. For work absence spells of less than 91 days, there is an ambiguous effect of the reform. First, there is the increased *direct* cost of continuing a spell. Second, there is an increased cost of *returning* to work.

iii. For absence periods longer than 90 days, there is no change in the direct cost of absence. There is, however, a 25 percentage point increase in the cost of returning to work. Thus, the reform implied an unambiguous *decrease* in the relative cost of remaining absent in such spells.

Since the unambiguous implications (i) and (iii) work in opposite directions, the a priori effect of the reform on the prevalence of work absence is ambiguous.

We start the empirical analysis by investigating the net effect of the reform on the prevalence of work absence. To be able to distinguish the effect of the reform from any trend in the work absence rate, we exploit the discontinuous nature of the reform. The idea behind this strategy is that behavioral changes that can be referred to trends, or gradual changes in society, cannot be recorded as "jumps" or discontinuous changes in the data,[5] as opposed to the effect of policy interventions implemented at a particular date. To measure this effect, it would be possible to take the difference between the prevalence immediately before and after the reform, e.g. during January and February 1991 and March and April the same year. However, since there is an apparent seasonal pattern in the work absence rate (see Figure 1), such an estimate would be distorted. To deal with this problem, we use a differences-in-differences estimator, where the difference between January/February and March/April 1990 is used as a

[4] It can be noted that the reform did not only give an unambiguous relative cost increase, but also an unambiguous absolute cost increase.

[5] Angrist and Krueger (1999) use the famous quote "Natura non facit saltum" or "Nature does not make jumps" from Marshall's *Principles of Economics*, as a motivation for this observation.

"control". Formally, the estimator is defined as

$$DD = (\overline{m}_{jf}^{91} - \overline{m}_{ma}^{91}) - (\overline{m}_{jf}^{90} - \overline{m}_{ma}^{90}), \tag{1}$$

where $\overline{m}_{ma}^{91}$ and $\overline{m}_{ma}^{90}$ constitute the mean prevalence in March and April 1991 and 1990, respectively, and $\overline{m}_{jf}^{91}$ and $\overline{m}_{jf}^{90}$ the mean prevalence in January and February 1991 and 1990, respectively.

To estimate the effect of the testable implication under (i) and (iii) above, we use (discrete time) Cox regression models in the empirical analysis. For the incidence, we use the following specification:

$$\lambda_1(t) = \lambda_0(t)e^{\delta I^R}, \tag{2}$$

where $\lambda_0(t)$ is the baseline hazard (i.e. the hazard before the reform) and I^R is a step function, taking the value of one after the reform and zero before.

For the absence spells, we use the following specification:

$$\lambda_1(t) = \lambda_0(t)e^{I^R(1-3)\beta_1 + I^R(4-7)\beta_2 + I^R(8-90)\beta_3 + I^R(91-)\beta_4}, \tag{3}$$

where $I^R(j-k)$ are impulse functions, such that $I^R(j-k) = I^R\mathbf{I}(j \leq t \leq k)$ where $\mathbf{I}(.)$ takes the value of one if the argument within the parenthesis is true, and $I^R(91-) = I^R\mathbf{I}(91 \leq t)$ is a step function. We also include $\mathbf{I}(1-3)$, $\mathbf{I}(4-7)$ and $\mathbf{I}(8-90)$ in the specification. The interpretation of β_1, β_2, β_3 and β_4 is thus the change in hazard rate caused by the reform. If moral hazard is present, then these coefficients can be expected to differ from zero. Because of the control of absence from work, we expect that $\beta_3 < \beta_2$, even though the cost change due to the reform is the same. Furthermore, (iii) is reflected in that β_4 is expected to be less than zero.

As pointed out above, an important empirical issue is to distinguish the effect of the reform from any macro economic trend in the work absence rate. The unemployment rate increased substantially in the fall of 1991. Figure 1 shows

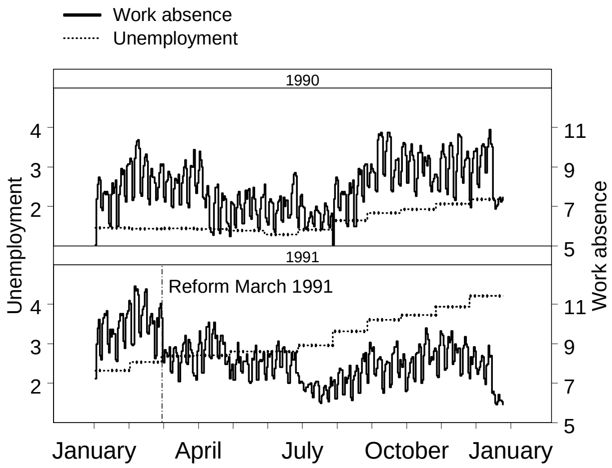

Figure 1: Prevalence of work absence in the sample and the monthly average unemployment rate in 1990 and 1991.

the weighted average of the county-level unemployment rate, together with the aggregate work-absence rate in the sample. There is a literature in the field which suggests a correlation between general conditions in the labor market and absence rate as a result of the disciplining effect of high unemployment (see e.g. Arai and Skogman Thoursie, 2001, Henreksson and Persson, 2003, or Lantto, 1991). To control for this potential effect, we use monthly data on unemployment levels for each of Sweden's 24 counties (local labor markets) matched with the data.

Figure 1 also reveals a seasonal pattern in work absence. Since the 1991 reform of the sickness insurance system did not occur until March 1, this is a potential problem. To balance the pre-reform and post-reform samples, we exclude spells beginning in January or February in both 1990 and 1991 in the Cox regressions.

Exact maximum likelihood estimators in discrete time are used to estimate the Cox regression models (see Kalbfleich and Prentice, 1980, Chapter 4). The baseline hazard for the incidence is specified using a dummy variable for each day in a work spell. In the case of duration in work absence, the baseline hazard is piecewise constant.[6]

4 Data

We use the 1991 Swedish Level of Living Survey (SLLS). The SLLS is a micro data set that contains information compiled from interviews and official public registers for a random sample of about 6,000 individuals. This survey is described in detail in Fritzell and Lundberg (1994). Data on the dependent variable - absence from work compensated by the sickness insurance - was obtained from the National Social Insurance Board by matching with the SLLS sample. As the data was collected from registers for actual transactions to insured individuals, there are likely to be much fewer measurement errors as compared with survey data based on interviews.[7]

We restricted the sample to blue-collar workers aged between 20 and 64, who were employed during 1990 and 1991.[8] The final sample consisted of 1,396 individuals (738 males and 658 females).

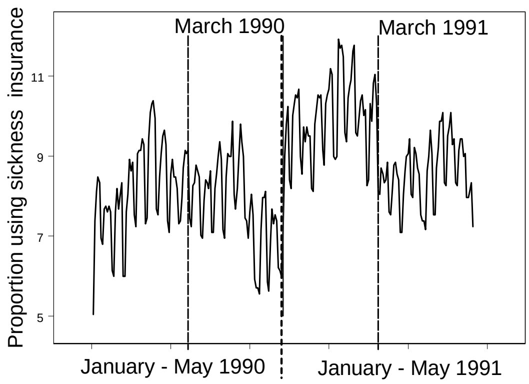

Figure 2: Daily work absence rate January 1 to April 30, 1990 and January 1 to April 30, 1991. Reform date in March 1, 1991 is marked. Males and Females.

5 Results[9]

5.1 The Effect of the Reform on the Prevalence of Work Absence

Figure 2 shows the samples day-by-day work absence prevalence for the period January to May 1990 (left panel) and the corresponding period in 1991 (right panel), including the date of the reform, March 1. It is apparent that there is a discontinuous shift in the work absence prevalence around the date of the reform, which did not take place around that date in the previous year. The overall level of work absence is, however, somewhat higher in 1991.

Table 1 shows the estimated components and the overall result from the

[6]The following specification is used: a dummy variable for each day for days 1 to 3, and dummy variables for days 4 -7, 8-14, 14-21, 22-28, 29-42, 43-56, 57-70 and 71-84.

[7]The working paper version (Johansson and Palme, 2004) of this paper contains descriptive statistics and a more detailed description of how the sample is selected.

[8]For practical reasons, we have restricted the sample to workers employed through the entire two-year period. It can be argued that workers with a weaker attachment to the labor market are more sensitive to economic incentives. If this is the case, our estimates should be seen as a "lower bound" of the moral hazard effect, although it is very unlikely to have a strong effect on the results.

[9]In the working paper version (Johansson and Palme, 2004), sensitivity tests of the Cox regression models are performed. For instance, unobserved heterogeneity is controlled for by using a stratified proportion hazard estimator (a fixed-effect estimator). The results presented here are robust to the different specifications.

Table 1: Estimated means and standard errors (s.e.) for prevalence of work absence for different sub-periods.

Jan/Feb 1990	Mar/Apr 1990	Jan/Feb 1991	Mar/Apr 1991	DD
		Males and Females		
8.13	7.75	9.94	8.62	0.93
(0.15)	(0.14)	(0.14)	(0.11)	(0.27)
		Males		
7.31	7.16	8.40	6.69	1.56
(0.16)	(0.13)	(0.14)	(0.11)	(0.27)
		Females		
9.07	8.41	11.68	10.81	0.21
(0.16)	(0.18)	(0.18)	(0.15)	(0.33)

Table 2: Discrete-time Cox proportional hazard model estimates (*Est.*) and standard errors (*s.e.*) of the effect of the sickness insurance reform on the incidence of work absence.

	Males				Females			
	Est.	*s.e.*	*Est.*	*s.e.*	*Est.*	*s.e.*	*Est.*	*s.e.*
I^R	-0.316	0.005	-0.310	0.007	-0.211	0.005	-0.240	0.007
Unemployment	-		-0.114	0.010	-		-0.077	0.003
Unemployment^2	-		0.022	0.002	-		0.018	0.001
County factor	No		Yes		No		Yes	
Log likelihood	-9668.6		-9646.6		-9929.2		-9892.9	
$\chi^2(25)$; $p-value$	44.0; 0.01				72.6; <0.001			

Note: χ^2 statistics and $p-value$ for likelihood ratio test of joint significance of local labor market unemployment rate and county factor.

differences-in-differences estimator specified in equation (1) both for males and females, and separately for the respective gender. The results show a significant decrease in the prevalence in the sample as a result of the reform. The effect is much stronger for males than for females.

5.2 Incidence of Work-absence

Table 2 shows the results from the Cox regressions on the incidence of work absence. We use two different specifications - one which only includes an indicator for the reform of the sickness insurance and one which also includes a quadratic specification for the unemployment rate in the county (local labor market), as well as indicators for each county (i.e. a county factor). The χ^2 statistics and the p-value provided in the table show that the local labor market unemployment rate together with the county factors are jointly significant.

Table 3: Discrete-time Cox proportional hazard regression estimates (*Est.*) and standard errors (*s.e.*) of the effect of the reform on the duration in work hazard (hazard of ending a work absence spell).

	Males				Females			
	Est.	*s.e.*	*Est.*	*s.e.*	*Est.*	*s.e.*	*Est.*	*s.e.*
$I^R(1-3)$	0.062	0.029	0.099	0.033	0.068	0.028	0.077	0.032
$I^R(4-7)$	0.015	0.300	0.040	0.045	0.050	0.031	0.074	0.046
$I^R(8-90)$	-0.022	0.021	0.006	0.029	-0.101	0.020	-0.095	0.027
$I^R(91-)$	-0.127	0.030	-0.100	0.036	-0.639	0.027	-0.591	0.035
Unemployment			0.150	0.028			-0.013	0.027
Unemployment^2			-0.032	0.004			0.017	0.042
County factor	No		Yes		No		Yes	
Log likelihood	-4362.5		-4350.8		-4256.7		-4241.9	
$\chi^2(25)$; $p-value$	23.32; 0.57				29.6 ;0.27			

Note: The baseline hazard is specified as piecewise constant. Indicators for 1-3 days, 4-7 days and 8-90 days in a spell are also included in the specification. χ^2 statistics and $p-value$ for the likelihood ratio test for the joint significance of the local labor market unemployment rate and county factors.

These estimates show that there is a significant moral hazard effect on incidence. This effect is robust, although the magnitude changes somewhat when we include controls for counties and the unemployment rate in the local labor market. Males react significantly more strongly to the reform than females.[10]

5.3 Duration in Work-absence

Table 3 shows the results of the Cox regressions on the duration of work absence spells. Once more, we use two different specifications - with and without controls for the county and county-level unemployment rate. However, unlike the model for incidence of work absence, the χ^2 statistics and the p-values show that we cannot reject the exclusion of the unemployment rate and county effects from the specification.

The results confirm that the hazard from short absence spells (shorter than 8 days) increased after the reform. Following the anticipated behavioral effects outlined in Section 3, this means that the effect of the increased *direct* cost dominates the effect of the increased cost of *returning* to work and being exposed to the risk of embarking on a new spell of absence. This result applies to both

[10] A 95 percent confidence interval for the difference is $[0.05, 0.09]$.

gender groups and is robust to alternative specifications. The effect is, however, not statistically significant for the 4-7 days for the male sub-sample.

Another potentially interesting result is that, for both genders, $\widehat{\beta}_3 < \widehat{\beta}_2$. The difference is statistically significant for females. One interpretation of this outcome is that control in the form of demanding a certificate from a physician reduces moral hazard, since the effect of the reform is smaller for spells in the interval 8 to 90 days as compared to the shorter spells between 4 and 7 days, which are not subject to this form of control.

Finally, Table 3 shows that the reform had a significantly negative effect on the hazard for spells of more than 90 days. This result, which is robust to the alternative specifications, is statistically significant in both gender groups, although the effect is stronger among females. It supports the hypothesis that the increase in the cost of *returning* to work has an impact on worker behavior.[11]

5.4 Cost Elasticities

To obtain a summary measure of the magnitudes of the behavioral response, we use the estimates presented in the previous sub-section to obtain elasticity estimates with respect to the compensation level (i.e. the *direct* cost) in the sickness insurance system. We obtain separate measures for the incidence and the duration of work absence, respectively.

For the incidence and duration of work absence, the elasticity measures are calculated as $\widehat{\varepsilon} = -(1 - \exp(\widehat{\delta}))/\Delta(1)$ and $\widehat{\varepsilon}_j = -(1 - \exp(\widehat{\beta}_j))/\Delta(j)$, $j = 1, 2$ and 3, where $\Delta(j)$ is the relative change in the direct cost of work absence (i.e. entering or remaining in work absence). For spell lengths shorter than 4 days, the cost change, $\Delta(1)$, is the same as for the work state (28 percent). However,

[11] The identification of an incentive effect on work absence spells longer than 90 days is potentially difficult. The decreased incidence after the reform may have a detrimental effect on the general health status, which, in turn, can result in longer work-absence spells. In the working paper version (Johansson and Palme, 2004) we test for if this health effect may be the cause for the decreasing hazard for spell longer than 90 days. We do not, however, find any evidence for this.

Table 4: Estimates of elasticity with respect to the cost of being absent on the incidence and duration of work absence.

	Males		Females	
	$\widehat{\varepsilon}$	*s.e.*	$\widehat{\varepsilon}$	*s.e.*
Incidence	-0.93	0.02	-0.72	0.03
Duration, 1-3 day spells	0.23	0.11	0.25	0.11
Duration, 4-7 day spells	0.14	2.77	0.47	0.30
Duration, 8-90 day spells	-0.20	0.19	-0.87	0.16

Note: Standard errors (s.e.) are obtained using Gauss approximations.

for spells longer than 4 days and shorter than 91 days, the cost increase is 11 percent.

Table 4 shows the elasticity estimates. Comparing the precision in the estimates of the elasticities for incidence with those for the duration, it can be seen that the precision is superior for incidence elasticities.[12]

6 Conclusions

Three separate results on the effects of the reform obtained in this study suggest that there is a moral hazard problem in the Swedish sickness insurance. (1) The *direct* cost of beginning a work absence spell increased, and we found that the incidence of work absence declined after the reform; (2) the cost of *returning* to work increased and we found that the duration of spells longer than 90 days also increased; (3) the overall prevalence of work absence increased.

A key empirical question is if we can be sure that the estimated effects are really due to the policy change, rather than a trend in the prevalence of work absence. Two features of the results provide support for a causal interpretation. First, the fact that we can distinguish separate effects of the reform on the incidence and duration of work absence, respectively. These effects work in different directions with respect to the prevalence of work absence. Thus, it is very unlikely that two separate trends - working in opposite directions -

[12] One limitation of this manner of calculating elasticities is that, since we only consider the effects of the change in direct cost, we neglect dynamic effects of the reform.

would be present at the same time. Second, the results from the differences-in-differences analysis. Once more, it is unlikely that the discontinuous change, which is apparent in Figure 2, could be attributed to something else than the policy change.

The presence of moral hazard shows that there is a policy trade-off between providing insurance for temporary health deficiencies and disincentive effects on work effort. The results obtained in this study suggest that this applies to both short and long work absence periods, which correspond to small and large income losses, respectively.

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