THE INCENTIVE EFFECTS OF SICKNESS ABSENCE COMPENSATION
- ANALYSIS OF A NATURAL EXPERIMENT IN EASTERN EUROPE*

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Abstract: This paper examines the evolution of the number of days spent on sick leave following the 2011 reform which halved the maximum sick benefit provided by statutory health insurance in Hungary. This policy change sharply decreased benefits for a large group of high earners, while leaving the incentive to claim sickness benefits unchanged for lower earners, providing us with a "quasi-experimental" setup to identify the incentives effect of sickness benefits. We use a difference-in-differences type methodology to evaluate the short-term effect of the reform. We rely on high-quality administrative data and analyze a sample comprised of prime-age male employees with high earnings and stable employment. Our results show that the number of days spent on sick leave fell substantially for those experiencing the full halving of benefits. Estimating the response of the number of sick days with respect to the fall in potential sickness benefits, we find a significant elasticity of - 0.45.

Keywords: sickness absence, statutory long-term sick pay, difference-in-difference methods

JEL classification: I18, J22, J32

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1   INTRODUCTION

In a European context, where the workforce is aging and the participation rate of older persons - who are particularly prone to suffering from long-term illnesses - is increasing, understanding the economic incentives of sickness benefit systems is enormously relevant. The rate of sickness absence varies considerably across Western European countries, and there is some evidence that these differences are not simply related to the composition of the workforce, but also to the generosity of sickness benefits, in other words, to the incentives provided by countries' sickness insurance systems (Frick – Malo [2008]). This can be considered an important issue from the point of view of public finances, since more generous countries spend 1.5-2% of their GDP on sickness compensation (OECD [2009]), which is often higher than spending on unemployment benefits. In Eastern Europe, sickness absence rates have soared in the 1990s, which was partly due to a wider eligibility rules (mothers caring for sick children, recently unemployed persons), and it is suspected that long-term sickness absence was used as a first step towards disability retirement, as an escape route from unemployment during economic transition. More recently, the average number of compensated sick absence days have declined, but it is still comparable to the figures reported by some of the more generous Western European countries.[1]

In this paper, we examine the role of incentives provided by the sickness compensation system in shaping long-term sickness absence in Hungary.[2] In spite of the fact that the number of sickness absence days has been gradually decreasing since 2006, concern has been voiced over the unwarranted usage of sickness absence compensation. As a response to this issue, as well as due to the budgetary pressure in the wake of the recent recession, a curbing of the generosity of the compensation system has been enacted in several steps since 2009. These legislative changes provide an opportunity to evaluate the influence of financial incentives on the claiming of sickness absence benefit. In particular, the changes introduced in May 2011 cut the maximum of long-term social security financed sick benefit to half its previous value. Since this legislative change affected a well-defined group, those of high earners, while leaving the incentives to take sick benefit for those below the income threshold unchanged, it is possible to study the behavioral response to a cut in sick benefits. Relying on this 'natural experiment', we use difference-in-differences methods to identify the effect of the sickness benefits cut on sickness absence behavior. We do this using a large longitudinal administrative database that allows us to precisely reconstruct eligibility for sick leave, as well as potential sickness benefit. Furthermore, we are able to take into account not only a host of background characteristics but can also proxy health status by having access to medical spending data.

The interest in looking at this reform is twofold. First, most studies rely on relatively small changes in benefit replacement rates to identify the causal effect of sick pay on sick leave behavior, while here we study a large cut in benefits. Thus, we evaluate the effect of a policy change which can be expected to have a real bite, as the effective replacement rate was 42 to 60 percent prior to the policy change and fell to 21 to 45 percent. Second, we are unaware of any papers looking at sick leave behavior in Eastern Europe, where unemployment and welfare benefits are substantially less generous than those in the EU15. The limitation of our paper is that we are only able to identify the immediate, short term effect of a sickness benefit cut on sickness

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absence behavior of relatively high-wage men with a stable employment history.

Our paper is structured as follows. After providing a brief literature overview in Section 2, we describe the sickness benefit system in Hungary, as well as the policy change analyzed in Section 3. We detail our empirical strategy in Section 4, followed by an exposition of the dataset and an explanation of the construction of our variables of interest in Section5. Section 6 presents our main results, as well as a series of robustness and heterogeneity test. Section 6 concludes with a brief discussion.

2   EXISTING EVIDENCE AND LITERATURE

In most countries, the amount of the sickness benefit depends on previous earnings: the level of compensation paid in case of temporary incapacity is defined as a fixed percentage of the foregone earnings. This poses a fundamental identification challenge for estimating the effect of sickness benefits on sickness absence behavior: the economic benefits of returning to work and the economic gains from being on sickness benefit is largely influenced by the same variable. In some countries (for example in Austria, Belgium, the Netherlands, France, Denmark or Hungary; European Commission [2013]), there is also a cap on sickness benefits, which reduces the replacement rate for higher-earner employees. Much of the literature aiming to disentangle the incentive effects of sickness benefits have taken advantage of changes in the regulations affecting replacement rates and benefit maxima to estimate difference-in-differences type models. The "natural experiment" approach was pioneered in this context by Krueger [1990a, 1990b], who focused on the changes in the maximum level of benefits in the worker's compensation (WC) system in the United States, which provides paid leave in case of temporary incapacity caused by a workplace injury.

As sickness absences rates are relatively high in Scandinavian countries and – as this is an important policy issue - a number of papers studying how the generosity of sickness insurance systems affect sick leave behavior come from these countries.[3] Most of the early research about the effect of sickness compensation on sickness absence duration in European countries have examined the effect of changes in the overall replacement rate. Henrekson – Person [2005] relied on aggregate and regional time series variation from Sweden to show that reforms that made sick leave more generous were associated with an increase in the time spent on sickness absence. Johansson – Palme [2002, 2005] use micro data to examine the effect of a 10-percentage point cut in the replacement rate of sickness leave in Sweden and found that the incidence of sickness absence decreased substantially. The latest strand of studies builds on the fact that sickness benefits schedules have 'kinks', and these can be exploited to identify the effect of replacement rates on sickness absence behavior. Böckerman et al. [2018] use a regression kink design and the feature of the Finnish system whereby replacement rate is a piecewise linear function of the level of earnings, and find a high responsiveness of the length of sickness absence spells. Bryson and Dale-Olsen [2018] use a kink due to a cap in sickness benefits in Norway, and estimate a relatively low elasticity for men, and find that (high earning) women's sickness absence behavior is not influenced by benefits.

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[3] It is important to note that papers reviewed estimate the effect of social security financed (mandatory) sick leave. However, a number of papers have shown that employer-provided (voluntary) sick pay also influences sick leave behavior, and can partially offset incentives inherent in public sick pay (see Ben-Halima et al. [2018]; Dale-Olsen [2014]).

A number of recent important studies (Ziebarth and Karlsson [2010], Puhani and Sonderhof [2010] and Ziebarth and Karlsson [2013]) have analyzed the effects of the 1996 cut on the German short-term sick pay (lasting for a maximum of 6 weeks) from 100 to 80% and the consequent re-raise to 100% in 1999. These authors also use a "natural experiment" approach, since these reforms applied only to employees in the private sector, but not to those working in the public sector or self-employed. All these studies find a significant effect of the reforms on the days absent, and find a relatively high elasticity (around 0.9). Another paper by Ziebarth [2013] examines a cut in the long-term sickness benefit in Germany and he finds that it did not affect significantly the whole population but only some subsamples (the poorer quantiles and middle-aged employees working full-time), as those receiving long-term sickness benefit are usually coping with serious health problems.

Statutory health insurance reforms can impact not only absenteeism but also health-related outcomes. Studying these effects are crucial for assessing the welfare impact of these reforms, but this issue has rarely been studied due to lack of data. Exceptions are Puhani and Sonderhof [2010] and Ziebarth and Karlsson [2013]: using subjective health measures as outcomes, neither of them found any effect on health, which leads them to conclude that reactions to the generosity of sickness absence compensation come from shirking behavior, at least when measured at high replacement rates. More recently, Halla et. al. [2015] find - using a series of sickness benefit reforms - that in Austria, not only are workers' sickness absences highly responsive to changes in replacement rates, but workers' health improves subsequent to an increase in sick leave replacement rates, leading them to conclude that the marginal worker in their sample is in the domain of presenteeism.

3   SICKNESS INSURANCE IN HUNGARY

All employees in Hungary are covered by the Statutory Health Insurance, which covers absences due to both work-related and work-unrelated illnesses. Sick leave is comprised of two components: short-term and long-term sick leave. The first component (short-term sick leave) covers up to 15 working days in a calendar year, and it is mandatory for the employer to pay the sick pay. Short-term sick leave is paid only if the person's own health condition does not allow one to work, which needs to be certified by a GP. There is no waiting period, and the employee receives 70 percent of her earnings as sick pay, which is fully paid for by the employer. All employees are entitled to employer-financed short-term sick leave, however self-employed, owners of companies, and those working under a civil law contract are not. It is important to emphasize that this paper is only about the second, long-term sick leave (as we do not have data on short-term sick leave).

3.1   Long-term sickness benefit rules in Hungary

Upon having exhausted her short-term sick leave, a person can enter long-term sick leave, under the condition that the she has contributed to (mandatory) health insurance (to the Hungarian National Health Insurance Fund). The person applying for sickness benefits needs to be working in an employment relationship entailing sickness insurance, which includes a much wider array of employment relationships (for example: self-employed) than for the short-term sick leave. Long-term sick leave is co-financed by the employer (1/3 part) and social security (2/3

part). Similarly to the short-term sick pay, a GP or a specialist needs to certify the health condition and there is no 'waiting period' for the sickness benefit. It is worth noting that the person can take sick leave not only on account of her own health condition, but if they have a child who is sick and is less than 12 years of age.

This sickness benefit depends on insurance history and its replacement rate is lower than that of the short-term sickness benefit.[4] A health-impaired worker is entitled to long-term sickness pay for a maximum of one year, unless she was (continuously) insured for less than a year, in which case the length of the entitlement is equivalent to the duration of the insurance relationship. This means that the number of sick leave days used by the worker during the 365 days prior to applying for a (new) long-term sickness leave is subtracted from the length of maximum entitlement period. It is worth noting that due to the particular structure of short-term sick leave (that entitlement is a fixed number of days within a calendar year), long-term sick leave has a strong seasonal pattern. The number of long-term sick leave days grows at an increasing pace throughout the calendar year (as employees exhaust their short-term sick leave). The average number of long-term sick days in the fourth quarter is close to double the number in the first quarter. Finally, we need to note that private health sickness insurance exists in Hungary[5], and – while there are no reliable statistics – in 2012 less than 7 percent of employees benefited from private health insurance.[6]

The sickness benefit received during a long-term sickness spell depends on the employee's work (insurance) history and her previous earnings. It is worth pointing out that there is no distinction between full- and part-time jobs in terms of health insurance: every day a person is insured counts, regardless of the hours of work. Likewise, there is no possibility to take up part-time sickness leave. The starting point of calculating sick pay is finding the 'reference period' for previous earnings, which in essence, is a 180-day paid employment spell that can be anywhere between the starting day of the long-term sick leave and January 1st of the *previous* calendar year. As a rule, previous earnings are calculated based on work income during the *past calendar year*. More precisely, if the employee had at least 180 paid working days (for which she received earnings) in the previous calendar year, then the sick pay is based on the daily average earnings during this period. Otherwise, the 'reference period' for calculating previous earnings is the last employment spell where the employee was paid for 180 continuous days. For those without such an employment spell, sick pay is based on statutory minimum wages.

The second building block for calculating sick pay is the replacement rate, which is higher for those with longer contribution histories. The general rule is that those with at least two years of continuous work histories face higher replacement rates. Work (insurance) histories that had breaks of no more than 30 days count as being 'continuous', where breaks are those periods when the individual's health care insurance is 'suspended' or the person is not insured (i.e. unpaid leave, periods of employer initiated or unlicensed absences for work, incarceration, non-employment).[7]

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[4] In this respect, the sickness insurance system is very similar to that of a number of European countries, such as Austria or Germany.

[5] Private health insurance generally covers deductibles for medical drugs, medical devices and hospital stays, and provides fully covered access to physicians that have no contract with the public health insurer.

[6] Note that this proportion started to climb rapidly in 2012, since employers did not have to pay taxes and contributions after health (and sickness) insurance they financed for their workers.

[7] Note that periods of licensed sickness leave, and parental leave and periods on UI benefits do not count as a 'break'.

3.1.1 Changes in long-term sickness benefit rules: 2009-2011

During the period under study two large policy changes affected the replacement rate and the maximum sickness benefit. Specifically, since 1st of August 2009, - when an across-the-board 10 percentage point cut in replacement rates was legislated - those with at least two years of continuous work histories had a replacement rate of 60 percent, while those with shorter work histories faced a replacement rate of 50 percent.[8] It is worth noting that the replacement rate of (employer-financed) short-term sick pay was also cut by 10 percentage points. At the same time a *cap* on sick pay was introduced, which could not exceed 400 percent of minimum wages. The policy change *we study* came into effect on May 1st 2011 (it was legislated on March 25th, 2011), which essentially entailed a drastic reduction in the maximum amount of sick pay: the new cap on benefits was 200 percent of minimum wages. Thus, after the legislative changes, for those (with longer work histories) with earnings above 333,3 percent of minimum wages the sick pay replacement rate was substantially below 60 percent; while before the change the cap affected those earning above 666,6 percent of minimum wages. This meant that the new sickness benefit cap affected those with (gross monthly) earnings above 260 thousand HUF (roughly 960 EUR/month at the time).

To get a better understanding of the structure of the long-term illness compensation, in *Figure 1* (left panel), we display a stylized version of the benefit schedule relating the benefit amount to previous earnings. Since the 2009 policy change, the benefit schedule contains a kink – above 520 thousand HUF in our figure -, the sick pay of those above the benefit cap was constant. Those affected were individuals who earned 1.67 times the quadruple of the minimum wage during the 'reference period' (under the assumption that they faced a 60 percent nominal replacement rate). The 2011 policy change affected a wide group of high earners: all those above 260 thousand HUF (equal to 1.67 times the double of the minimum wage) experienced a reduction in their long-term illness benefits. In the right panel of Figure 1, we present some results about the effective replacement rate of male employees from the top three deciles of the earnings distribution. In this graph, we relate effective replacement rate (on the vertical axis) to the percentile of earnings (horizontal axis). From this figure, one can see that the introduction of an upper limit on benefits in 2009 affected only the top 5% of male employees, and that their effective replacement rate was much lower than 60 percent. The halving of maximum benefits in 2011 reduced the effective replacement rate for a much larger group, roughly the top 23% earners, and we can see that due to the flat-rate for benefits, the effective replacement rate is a decreasing function of earnings above the upper threshold.

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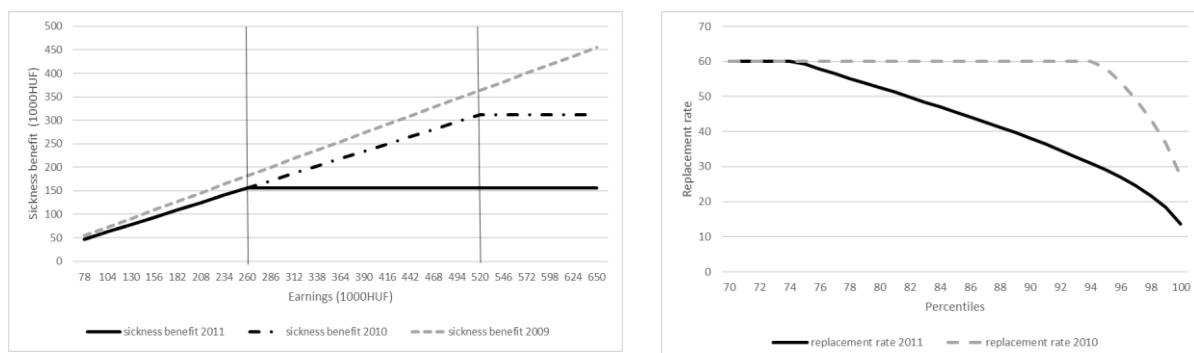
[8] The lower replacement rate also applied to those who were hospitalized during their long-term sickness leave.

(a) The benefit schedule between 2009-2011   (b) Effective replacement rates, 2010-2011

Figure 1: The effect of the policy changes on sickness benefits and replacement rates

The empirical approach of the paper is to study the sick leave behavior of three groups. The 'high earners', those earning above the 2009 threshold – who are the ones earning above 520 thousand HUF in our figure. These individuals experienced the full effect of the 2011 benefit cut, and who saw their (potential) sick pay cut in half. The second, 'medium earner' group, comprised of those earning below the 2009 threshold but above the 2011 one (those earning between 260 and 520 thousand HUF in Figure 1) were also negatively affected by the cut in the sick pay cap. Finally, the 'low earner' group are those with labor income below the 2011 threshold (below the 260 thousand HUF earnings) and were unaffected by the policy change. From the left-hand panel, one can see that the policy change decreased the sick pay in a piecewise linear fashion (with larger reductions for those with higher earnings between 260 and 520 thousand HUF and a flat reduction for those earning above 520 thousand HUF). Effective replacement rates – displayed in the right-hand panel – fell substantially due to the policy change. In our sample of prime-age men, the average rate dropped from 42 to 21 percent for the 'high earner' group, while it decreased from 60 to 46 percent for the 'middle earner' group.

3.2 The use of long-term sickness benefit in Hungary

The prime reason used by governments proposing cuts in the long-term sick pay was that it would provide incentives for insured employees to return to work, and would curb moral hazard, with the government explicitly mentioning free-riders who took up sick leave without a well-founded reason.[9] This was a move that was – unsurprisingly, since employers footed one-third of the bill - welcomed by employers' organizations. However, unions were of the view that these cuts were primarily motivated by budgetary reasons and called for a more adequate control of GPs certification behavior.

The debate surrounding sick leave notwithstanding, the use of sick leave in Hungary was not particularly pronounced prior to the changes in 2009-2011 in sick pay. In 2005-2006 the number of total sick days per (insured) person was around 13 days per year.[10] To put this figure in perspective, this was slightly higher than in Austria, and slightly lower than in Germany, but was substantially below sick leave behavior reported in Norway (with around 11, 14 and 17 days per year, respectively).[11] With the onset of the recession in 2007, the use of sick leave (similarly to other countries) started to decline – in 2009, number of compensated sick days were around 11.9, while the reforms of the sick pay were associated with a more pronounced decrease with he number of sick days falling to 6,9.[12] The statistics on long-term sickness leave show similar

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[9] It is worth mentioning that in 2012, sick pay fraud became a criminal act, and could be penalized with up to two years' imprisonment.

[10] See OECD.Stat, see https://stats.oecd.org/index.aspx?queryid=30123

[11] Note that in Austria and Germany, the replacement rate for long-term sickness was 70 percent (equivalent to the one prevalent in Hungary at the time), while in Norway workers were fully compensated (up to a cap).

[12] We also show the evolution of unemployment rates of men in Hungary (and in the EU28) for the relevant period in Appendix Figure 2, as it has been shown that the threat of unemployment act as a disciplining device for absenteeism. Since the policy change affected high earnings individuals, we display the unemployment rates of high and medium schooling groups. From this figure, it is clear that unemployment peaked in 2010 for both education groups. While unemployment rates started slowly decreasing in 2011 for the high education group, it remained

patterns: in 2005-2006 roughly 3,5% of insured persons were on long-term sick leave on any given day on average (HCSO [2014]) , and this fell to 2,6% by 2009; while following the reforms in 2009 and 2011 , only 1,5% of those eligible for long-term sickness benefits took it up.

4   EMPIRICAL STRATEGY

To evaluate the effect of the policy change our first approach is to estimate difference-in-differences type models of long-term sickness absence behavior, while controlling for workers' background characteristics. Thus, we will compare the change in the number of days spent on sick leave between 2010 and 2011 across the high, medium and low earnings groups. The high earnings group are those who were above the benefit cap which was in place in 2010, and for whom the benefit cut resulted in a halving of sickness compensation. The medium earnings group are those below the 2010 earnings threshold, but above the one in 2011. The low earnings group are those who were below the new earnings threshold, and who were thus unaffected by the reform.

Thus, in a regression-type analysis, we estimate equations of the form:

$$Y_{it} = \beta_1 HE_i * After_t + \beta_2 ME_i * After_t + \gamma_1 HE_i + \gamma_2 ME_i + \pi After_t \qquad (1)$$
$$+\theta \ln(earnings)_{it} + X_{it}\delta + \varepsilon_{it}$$

where *Y* represents the outcome variable: the number of days spent on sick leave in a given period; *HE* and *ME* stand for the high and the medium earnings groups, respectively, the variable '*After'* is a dummy for the year 2011, *earnings* represents current (daily) labor income, which is included to represent the incentives to remain in work as opposed to being on sick leave. The vector *X* represents the individual's observable characteristics: it is worth noting that we have access to a host of control variables, including historical data on a person's health care spending and sickness leave behavior. The coefficients of interest are β_1 and β_2, as the estimates represent the differential change in sickness absence behavior of the two groups affected by the policy change relative to the control group. Note that this an 'intention-to-treat' parameter, since we use a sample composed of all individuals who were eligible for long-term sickness benefit.

The idea then is that – conditional on a set of observable characteristics – the low earnings group represents the counterfactual, i.e. what would have happened to the medium and high earning group in absence of the policy change. Similarly to other studies using a difference-in-differences type methodology we rely on some crucial identifying assumptions which cannot be directly tested. First, the allocation to the different groups is likely to be exogenous since long-term sickness compensation is based on *previous* calendar year's earnings it is very unlikely that individuals could have manipulated this 'assignment variable'. Second, having access to longitudinal data, we are able to keep the composition of the different earnings groups fixed, hence selection into and out of the employment, as well as 'switching' across treatment and control groups based on unobservables can be ruled out. Third, we will show circumstantial

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roughly constant for the medium education group. If anything, this small difference (and the slight improvement in the labor market prospects of highly educated males) should be working against finding significant effects of the sickness benefit cut, since in the highest earning group 90% of the sample had tertiary education, while only 50% of the in the control group were comprised of college educated males.

evidence on the validity of the parallel trends assumption, as we are able to test whether sickness absence evolved differently for the alternative groups in periods when no policy change happened.[13]

An additional concern might be that workers adjust their behavior in anticipation of the planned legislative changes. More precisely, once the planned sick benefit cap decrease is announced affected workers might re-schedule some absences (such as related to medical interventions where the patient has some leeway over the exact timing) to occur before the cut is enacted. This would also invalidate the parallel trends assumption. To rule out such contamination of our estimates, we will only use the July-December months of both 2010 and 2011, as the change in rules was announced on March 25th of 2011 and took effect on May 1st 2011. We expect that those who could manipulate the timing of absences would start their sickness absence spell in April 2011, and that this spell would likely finish before July 2011.

Next, in order to directly estimate the effect of the change in the sick pay, we use models of the form:

$$Y_{it} = \beta_1[\ln(sick\ pay^{Before})_{it} - \ln(sick\ pay^{After})_{it}] * After \qquad (3)$$

$$+\beta_2\ln(sick\ pay^{Before})_{it} + \pi After_t + \theta \ln(earnings)_{it} + X_{it}\delta + \varepsilon_{it}$$

In this specification, the first variable represents the difference between the potential benefits that an individual would have received in 2011 under the 2010 benefit schedule and the potential benefits under the rules in place in 2011. The coefficient associated with this variable is our primary interest, and it is identified from the changes in benefits. This model can also be viewed as estimating the effect of the *'intensity' of treatment*, since the difference between the sick pay under the old and the new benefit schedules varies between 0 for the low earnings group to 0,693 log points for the high earnings group. This variable represents the (log of) the proportional reduction in sick pay due to the policy change, and it can be seen as an exogenous source of variation. In essence, the intensity of treatment in 2011 was related to the earnings of individuals in 2010, which is arguably exogenous to current health outcomes (conditional on a set of observable characteristics, most notably indicators of long-term health).

The identification of the effect of the sick pay under the 2010 rules (and disentangling it from the effect of earnings) is more tenuous. First, since sick pay is based on previous year's earnings, it might not be perfectly correlated with current earnings. Second, even if current earnings are highly correlated with past earnings, identification can come from the 'bend' in the benefit schedule. Thus, two individuals with the same sick pay might have different earnings due to the benefit cap.

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[13] In the Appendix Figure 1, we display the evolution of sickness absence in the three income groups. The sickness absence follows a particular, seasonal pattern: they slowly start increasing throughout the year, and they jump in the last quarter of the calendar year (as this is when workers have exhausted the short-term sickness absence days). Despite the large differences in the average number of sickness leave days, the patterns are strikingly similar. In all groups, sickness absence started decreasing in 2009, even before the first cut in sick benefits, and it fell substantially in 2010. However, as opposed to previous years, in 2011, the high income group's sickness absence hardly increases in the second half of the year.

5 DATA AND SAMPLE SELECTION

Our analysis is based on a large linked longitudinal administrative dataset that were compiled from several sources for research purposes for the Centre for Economic and Regional Studies of the Hungarian Academy of Sciences. The complete dataset contains a 50 percent sample of the adult population (coming from a simple random sampling procedure) of Hungary for the years 2003-2011. We use data from 2007-2011, since earlier data from the National Health Insurance Fund is partially missing; the information we use comes from three sources.

Our primary source was the National Pension Insurance data, which contains detailed insurance (employment) histories.[14] All periods when the individual was insured – e.g. accumulated days that contribute towards pensions - were recorded (including the exact dates of the beginning and the end of a spell), as well as the 'title' of the contribution spell. It is important to note that long-term sickness absence spells are also indicated as an insured period.[15] Intermittent 'breaks' in insurance spells are also contained in the dataset, along with the reason for this non-insured period. This dataset thus allows us to calculate the number of continuously insured days for each individual (the determinant of the replacement rate), as well as defining the 'reference period' for calculating sickness benefits.[16] The data also contains (labor) income data aggregated to monthly spells, which enables us to reconstruct both the earnings that serve as 'reference income' for sick benefits, and 'current' earnings. Finally, the person's gender, day of birth, detailed occupation codes (for employment spells) and the employer's identification number is recorded.

The National Health Insurance Fund data provide important information on two aspects. First, long-term sickness absence spells are recorded – but unfortunately sickness benefit is not contained in the dataset. We use this information to cross-validate spells found in the National Pension Insurance dataset. Second, we have information on yearly health-care spending on the individual (by categories: in-patient, out-patient, medications)[17], as well as the number of visits to the individual's general practitioner.

Finally, data from the National Tax Authority on firms essentially contains the most important figures from firms' balance sheets including industry, ownership structure, number of employees, total revenues, export revenues, profits and the total wage bill. Given that employees of the public sector were included in the sample, we only extracted a basic set of variables which were relevant for these firms as well.

We need to note that the data we use does not contain information about short-term sickness absence spells. It is important to underline that the policy change we study did not influence short-term sick pay. The dynamic model in Ziebarth [2013] shows that short term

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[14] There are negligible differences in what count as contributory days towards pensions and sickness insurance.

[15] More precisely: it is a period that contributes towards 'number of insured days', but no contributions (neither health nor pension) are deducted.

[16] Note that in principle, the fact that we had to estimate the (potential) sickness benefit could lead to a measurement error in our key explanatory variables. However, the data reported to National Pension Insurance Fund and the National Health Insurance Fund is exactly the same for employees (for other entitlement to insurance, there are small differences). Thus, we have all access to the same information as the National Health Insurance Fund, when calculating (long-term) sickness benefits; and we consulted with a specialist, in order to ensure that our interpretation of benefit rules was correct.

[17] Unfortunately, besides the sum spent on the health care of the individual, we know nothing about the person's illnesses or health status.

sickness absence is directly affected by (short-term) sick pay; and is also indirectly affected by long-term sickness benefit. When a (forward-looking) worker is considering to continue being on (short-term) sick pay, is considering also the decrease in the discounted value of being on (long-term) sickness benefit. Arguably, it might be interesting to study all sickness absence, as high earning persons might have refrained from taking (short-term) sickness absence with milder illnesses, but this is clearly beyond the scope of this paper. Using the results from Ziebarth [2013] and the fact that the incentives for low earning persons (the control group) did not change, we can argue that we might be underestimating the 'true (total) effect' of the cut in (long-term) sickness benefits on sickness absence behavior, due to the lack of data on short-term sickness absence.

5.1 Sample construction

We only use prime age males in this study. We do not consider females, since they can have access to long-term sickness leave in order to take care of ill children, for bedrest around child birth, as well as having more intermittent work histories. We limit the sample to those born between 1955 and 1984, as the issue of sample selection is more important among older and younger individuals. Since at the time (in 2010), individuals could retire at 57 years of age using an early retirement scheme, we are concerned that persons in ill health (or low tastes for work) would selectively withdraw from the labor market (and not be in our 'risk group'). Similarly, among younger men, the issue is that only those who finished their education relatively early could accumulate sufficient insurance history.

The second type of criterion we apply when selecting the sample to be analyzed is related to employment (insurance) histories. In essence, we select those individuals with continuous insurance histories of at least two years both in both 2010 and 2011, and who worked for pay at least 180 days of the previous calendar year. This rules out the possibility that a person's replacement rate changed due not to legislative changes, but rather because of an increase (or a loss of) in insured days. A simple example elucidates this point: consider a person with reference earnings above the cutoff point for the sick benefit cap in 2011 (but below the cutoff of 2010) if he was eligible for a 60 percent replacement rate. If this person in 2010 did not accumulate sufficient insurance days to be eligible for the 60 percent replacement rate, but by 2011 he did, he saw a sick benefit rise. If the same person was eligible for the (nominal) 60 percent replacement rate in both years, he experienced a sick pay cut in 2011 due to the halving of the sick pay cap. Thus, we want to rule out having to simultaneously control for (or estimate a model of) employment histories and long-term sickness absence. Therefore, because we only include persons with stable, long-term employment, we implicitly select individuals with high tastes for work (or high unobserved productivity). The second restriction – having at least 180 days of working days with earnings in the previous calendar year – which rules out individuals with presumably the worst health condition, is largely innocuous, since it affects less than 0,7 percent individuals (from among those with stable work histories).

The third type of selection criterion is related to the type and stability of employment. We only include employees, discarding self-employed and owners of corporations. Furthermore, we only include months when the person was fully insured and exclude those individuals who were not fully insured in the relevant period (the second half of the calendar year) both in 2011 and 2010. This is done to ensure that the sample analyzed had stable composition across the pre- and

post-treatment periods, such that selection out of employment (that could be related to health condition) does not contaminate our results.

The fourth issue in constructing our sample relates to the definition of the treatment and control groups. We select individuals based on their earnings in the 'reference period' for 2011. First, since the sick benefit cap affected all those above the 77th percentile of earnings (among men with stable insurance histories) in 2011, to form a control group, we need to have individuals with (slightly) lower earnings, but who are not completely dissimilar in terms of observable characteristics. Hence, we opt for including all those in the control group who had earnings above the 65th percentile of 'reference earnings' in 2011. Second, to ensure that the identification of our models – in particular those used for estimating the responsiveness of sick leaves to sick benefit – comes primarily from changes in sick benefit due to legislative changes, we restrict the sample based on the relative value of reference period earnings in 2010 and 2011. More specifically, we only use those individuals whose reference earnings did not differ across the two periods by more than 20 percent. In practice, this meant that we discard about 10 percent of observations.

Finally, in order to exclude individuals who might have strategically taken a long-term sick leave before the legislation took effect, we exclude all those who were already on long-term sick leave on the 30th of April 2011. Please see Appendix Table 1 on how the different steps of the sample selection procedure affected the number of observations retained.

5.2 Variable definition

Our key dependent variable is the number of days spent on long-term sickness absence, both defined over a *six-month* interval (July-December). We also have two key independent variables: treatment group and the (potential) daily sick compensation. Both of these variables are based on daily earnings in the 'reference period' (i.e. in the previous calendar year), which is calculated using National Pension Insurance data. Daily sick compensation was then computed based on the rules in place in the given year. The three treatment groups are the following: high – those who would have earned above the cap in place in 2010 and hence saw their sick compensation cut in half; medium – those whose earnings were below the earlier cap, but above the lower cap in 2011; and low – those who were unaffected by the fall in the benefit cap (and who had earnings above the 65th percentile of earnings in 2011). As described above, these groups are formed relative to 'reference earnings' in 2011.

We use a host of control variables. The most important of these is 'current earnings', which is equivalent to the mean daily earnings in the previous six calendar months. The explanatory variables related to personal background characteristics are five-year birth cohorts, region of residence, education, while work-related variable is one-digit occupation. We also control for indicators of (long-term) health: the amount of health care spending (the total spent on medications, in-patient and out-patient care) in the previous two years; as well as a dummy variable for those individuals who have spent time on sick leave in the previous two years. Finally, we use firm-level data: the (logarithm of) number of employees and average earnings; as well as the industrial affiliation of the employer.

5.3 Descriptive evidence

In *Figure 2* below, we show the number of sick leave days (over a 6-month period) as a function of the natural logarithm of reference earnings (the average earnings in the previous calendar

year). We display a kernel-weighted local polynomial smoothing of the number of days, separately for 2010 and 2011, as well as the threshold values for the benefit cap for the two years. It seems that the 'slope' of the relationship between reference earnings was slightly steeper in 2011 in line with the fact that the effective replacement rate fell (as a result of the halving of the benefit cap) at an increasing pace with higher reference earnings. The second phenomenon which can be seen is that above a certain level of earnings, there is a certain 'levelling out' of the number of sick days, this however happens at a lower level in 2011 than in 2010, which is consistent with the halving of the effective replacement rate for the very high earning group.
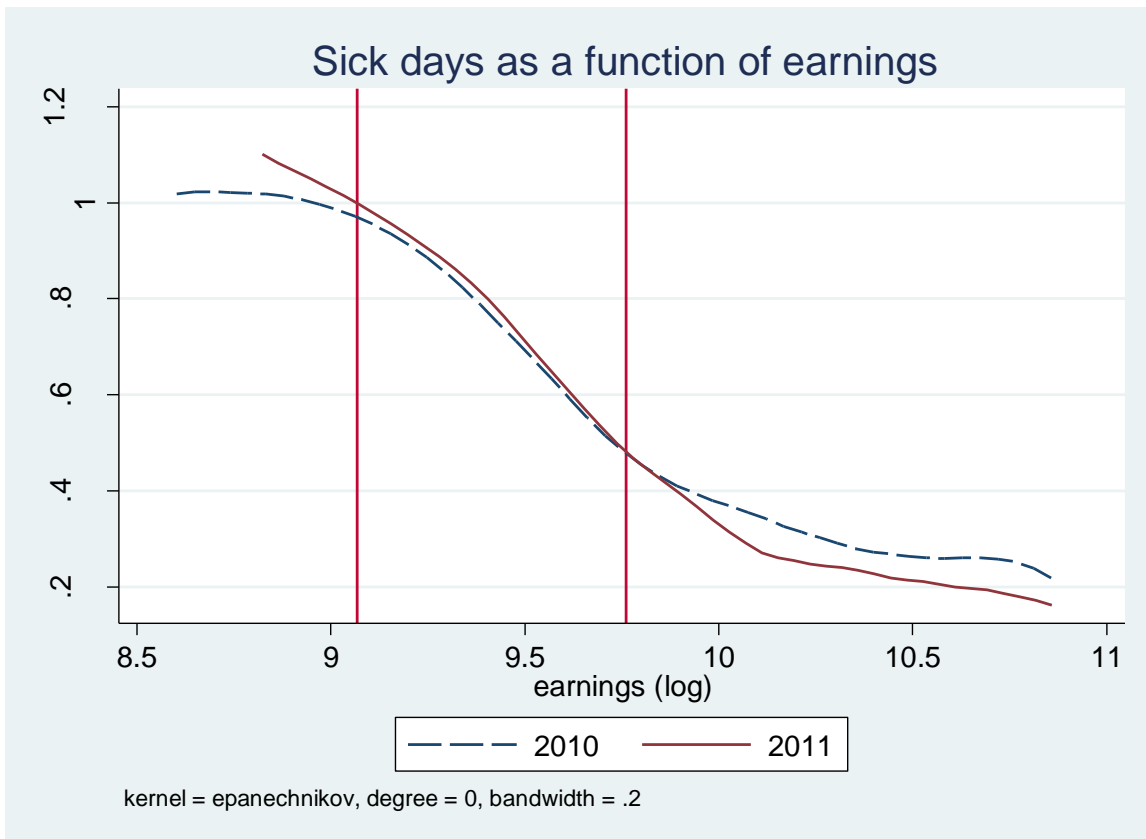
Figure 2: The number of sick leave days as a function of reference earnings, 2010 and 2011 (local polynomial smooth)

In Table 1, we provide some descriptive statistics about our key variables of interest, by earnings group and year. We can see that in the initial year, there were large differences across earnings groups both in terms of (potential) sick pay and current earnings. However, after the policy change in 2011, the sickness benefits of the low earnings group were only 14 percent lower than that of the two higher earnings groups, while – for example – the current (daily) earnings of the medium earnings group were 49 percent higher than that of the low earnings group. The data on the mean 6-month number of days spent on long-term sick leave show that there were large differences across the earnings groups. Higher earning persons had substantially lower sick leave days, with the low earnings group having around three times higher means of sick leave days compared to the high earnings groups. We can also see that while there was a small increase in the number of sick days for the low earnings group in 2011, these figures did not change in medium earnings group. However, in the high earnings group – who saw their sickness

compensation cut in half –the number of sick days fell by 20 percent in 2011. [18]

Table 1: The number of sick days; sick pay and current earnings before and after the policy change

	N. of sick days		Sick pay		Current earnings	
	Mean	St. error	Mean	St. error	Mean	St. error
2010						
Low earnings	1,089	0,0362	8,441	0,001	12,401	0,004
Medium Earnings	0,726	0,022	8,8458	0,001	12,813	0,008
High Earnings	0,309	0,025	9,244	-	13,547	0,002
2011						
Low earnings	1,192	0,046	8,430	0,001	12,394	0,001
Medium Earnings	0,741	0,026	8,556	-	12,802	0,001
High Earnings	0,251	0,022	8,556	-	13,529	0,003

Note: The sample includes only July-December months. The number of observations is 34205 individuals in the low earnings group, 58490 individuals in the medium earnings group, and 16390 individuals in the high earnings group. Sick pay and current earnings are given in natural logarithm of thousand Hungarian Forints (2011).

However, it must be emphasized that – as in most studies building on natural experiments – there are large differences between the treatment groups in terms of observable characteristics. Some of the most notable differences existing in terms of occupational distribution (please see Appendix Table 2 for background characteristics in the three earnings groups). Thus, it is important to control for these observable determinants of sickness absence behavior in econometric models.

6 REGRESSION RESULTS

In this section we first present our main estimation results based on the regression models discussed in *Section 4*, then we turn to robustness checks and heterogeneous effects. We present both results based on OLS and Zero-inflated Negative Binomial models and show models which do not include any controls, as well as with a full set of control variables.[19] Our preferred

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[18] In the Appendix Figure 1, we display the evolution of sickness absence in the three income groups. Sickness absence follows a particular, seasonal pattern: they slowly start increasing throughout the year, and they jump in the last quarter of the calendar year (as this is when workers have exhausted the short-term sickness absence days). Despite the large differences in the average number of sickness leave days, the patterns are strikingly similar. In all groups, sickness absence started decreasing in 2009, even before the first cut in sickness benefits, and it fell substantially in 2010. However, as opposed to previous years, in 2011, the high-income group's sickness absence hardly increases in the second half of the year.

[19] Note that we also experimented with Poisson and Negative Binomial, as well as Zero-Inflated Poisson regression

estimates are the count data models with full set of controls, which are better suited for modelling cases with a large number of observations with zero long term absence days.

Our main results for the difference-in-differences type models are displayed in Table 2, where average marginal effects are calculated and presented for the variables of interest – the post-reform difference between medium and high earnings groups (the treatment) and the low earnings group (the control).[20] As shown in the top panel of Table 2, the estimates for the medium earnings groups are small, negative, but statistically insignificant. The effect of the policy change for the high earnings group is more pronounced, showing a decrease of about 0,16 days (for the OLS models) and around 0,20 days (for the count data models). This fall in long-term sickness absence is large, as it represents a more than 55% fall in the number of sick days.

Table 2: Diff-in-diff type models of the number of sick days.

.	*OLS*				*Zero-Inflated Negative Binomial*			
	No controls		With controls		No controls		With controls	
	Marginal effect	Standard error	Marginal effect	Standard error	Marginal effect	Standard error	Marginal effect	Standard error
Medium Earnings	-0,089	0,064	-0,083	0,064	-0,052	0,050	-0,048	0,052
High Earnings	-0,162***	0,064	-0,164***	0,064	-0,203***	0,077	-0,233***	0,081
N. of obs.	218122 (109086 ind.)				218122 (109086 ind.)			

Note: standard errors are robust to clustering at individual level. Control variables are: (log) of current earnings, schooling, five-year birth cohorts, region of residence, occupation, lagged sick leave (dummy), lagged health care spending, (log) firm employment, (log) average firm wages, sector of activity.

Next, we estimate the responsiveness to the intensity of treatment. In other words, we disentangle the effect of the reduction in (potential) sick benefit by separately including the sick benefit under the 2010 benefit schedule and the difference between the sick benefit under the old and the new benefit schedules. The results (the marginal effects and the estimated elasticities for the models where we control for the full set of control variables) are shown in Table 3.[21] As expected, we estimate a negative effect of the fall in sick benefit, and our results are largely in line with the findings of the difference-in-differences models. These imply that a one-percent reduction in (potential) sickness benefit leads to a 0,45 percent reduction in the number of days spent on sick leave. We can note that the effect identified by the reduction in sick benefit is substantially (and statistically) different from the effect identified by the nonlinearity of the benefit schedule, with the former being roughly one-third size of the latter.

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models, however, based on goodness-of-fit diagnostics, the Zero-Inflated Negative Binomial model was retained.

[20] Please see Appendix Table 3 for the full set of parameter estimates.

[21] Please see Appendix Table 4 for the full set of coefficient estimates.

Table 4: The effect of the sick pay reduction on the number of sick days.

.	OLS				Zero-Inflated Negative Binomial			
	Marginal effect	Standard error	Elasticity	Standard error	Marginal effect	Standard error	Elasticity	Standard error
Sick pay 2010-2011	-0,274***	0,082	-0,474***	0,137	-0,320***	0,099	-0,449***	0,117
Sick pay 2010	1,015***	0,138	1,655***	0,508	0,956***	0,135	1,285***	0,150
N. of obs.	218122 (109086 ind.)				218122 (109086 ind.)			

Note: standard errors are robust to clustering at individual level. Control variables are: (log) of current earnings, schooling, five-year birth cohorts, region of residence, occupation, lagged sick leave (dummy), , lagged health care spending, (log) firm employment, log average firm wages, sector of activity.

6.1 Robustness checks

The first issue we address is the sensitivity of our results to allowing for individual fixed effects. While the sickness benefit (and hence the policy) is related to earnings in the previous year, and is thus a pre-determined variable, one might still be concerned about identification. More specifically, if long-term (unobserved) determinants of health might influence both past earnings through work capacity and current health, then this can potentially mean that the intensity of the sick benefit cut is endogenous. While we did control for indicators of health in the main specifications, we also estimated fixed-effects linear regressions. The results, shown in the first column of *Table 5* below are consistent with the OLS estimates: the cut in the maximum sick pay leads to a halving of the use of sick benefit among those most affected, while the response of the 'middle earnings' group to the sick benefit cut is not statistically significant. Controlling for time-invariant unobservables slightly reduces our estimates of the elasticity of sick days to benefit cuts.[22]

In the estimations presented above, we do not account for the fact that individuals could 'switch' between treatment and control groups across the two periods (we assigned everybody to the group based on their 'reference earnings' for 2011). On the one hand, we can rule out that individuals strategically adjusted their 'reference earnings' due to the policy change. On the other hand, an individual's earnings growth across two years might be correlated with their health status. We approach this issue by excluding all individuals who switched 'earnings group' across the two years; and we limiting the sample to those who experienced no more than a 10% change in their reference earnings between 2010 and 2011.[23] The results of this exercise – shown for the Zero-Inflated Negative Binomial model in Column 2 of Table 5 – are very similar to those obtained for the larger sample.

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[22] We also experiment with functional form assumptions: we allow for current earnings to have a non-linear effect on our outcomes (while we keep the linearity assumption for the effect of sick pay). We add a four-piece earnings spline by quartile of 'reference earnings', but having piecewise linear model in earnings hardly affects results.
[23] Note that we lose about 30 percent of our original sample due to these restrictions.

Table 5: Robustness checks: fixed effect estimation, and alternative sample definitions

	Fixed effects		*No switchers*		*Leave P75-80 out*	
	Marginal effect	Standard error	Marginal effect	Standard error	Marginal effect	Standard error
Medium Earnings	-0,096	0,064	-0,010	0,062	-0,117**	0,051
High Earnings	-0,166***	0,065	-0,229**	0,091	-0,234***	0,071
	Elasticity	Standard error	Elasticity	Standard error	Elasticity	Standard error
Sick pay 2010-2011	-0,293***	0,085	-0,412***	0,145	-0,450***	0,117
Sick pay 2010	0,598**	0,318	1,328***	0,195	1,065***	0,160
N. of obs.	218122 (109086 ind.)		147618 (73809 ind.)		186280 (101640 ind.)	

Note: standard errors are robust to clustering at individual level. Control variables are: (log) of current earnings, schooling, five-year birth cohorts, region of residence, occupation, lagged sick leave (dummy), lagged health care spending, (log) firm employment, (log) average firm wages, sector of activity. Statistical Models used: OLS FE for 'fixed effects' specification; Zero-Inflated Negative Binomial for the 'No switchers' and the 'Leave P75-80 out'.

Next, we also allow for potential mismeasurement of reference earnings and thus misclassification of some individuals. This problem is most relevant in the definition of the control group. As a straightforward alternative, we exclude some individuals who were near the cut-off of the 'low earnings' group. This means that control group will be those between the 65th and 75th percentile of earnings, and that the medium earnings group will start at the 80th percentile of earnings. We find that the estimates of the response of the middle earning group increases due to this sample restriction. This likely primarily comes from the fact that we are using individuals from the treatment group who had a larger cut in sick pay. This is supported by the result that the elasticity of the response to the sick pay cut is essentially the same as on the full sample.

Table 6: 'Placebo' tests, models based on the first three months of 2010 and 2011

.	*Quarter 1*		*Different treatment groups*	
	Marginal effect	Standard error	Marginal effect	Standard error
Medium Earnings	-0,050	0,034	0,856	1,347
High Earnings	-0,034	0,036	0,212	1,030
N. of obs.	209708 (104884 ind.)		155378 (77706 ind.)	

Note: standard errors are robust to clustering at individual level. Control variables are: (log) of current earnings, schooling, five-year birth cohorts, region of residence, occupation, lagged sick leave (dummy), lagged health care spending, (log) firm employment, (log) average firm wages, sector of activity. Statistical Models used: Zero-Inflated Negative Binomial.

Finally, we provide some circumstantial evidence on the plausibility of the assumptions underlying difference-in-differences methods by performing two 'placebo tests'. We do this by contrasting the first three months of 2011, when the change in benefit rules was not announced

yet with the same period in 2010, using the same sample as in our baseline specification. We present results for the regression version of the diff-in-diff model in the first column of *Table 6*: reassuringly, none of the estimates for all 'treatment effects' are significant.

Our second placebo exercise entails running the difference-in-differences models on groups where no change in sick pay happened in 2011. Specifically, we will now assume that the 'low income' group is comprised of men whose earnings were between the 50th and the 59th percentile in the wage distribution; persons between the 60th and the 69th percentile are designated to be in the 'medium income' group; final the 'high income' group is formed by those between the 70th and the 74th percentile of earnings. Similarly to the exercise above, we find that there was no 'effect' of the policy change on a group which was not targeted by the sick pay cut.

6.2 Heterogeneous effects

Finally, we look at whether the cut in the long-term sickness benefits affected separate groups differently. We estimate our models for subsamples defined first by birth cohorts, and second by our proxy for long-term health condition.

We first present the results where we separated older (those born between 1955-69) and younger (born between 1970-84) individuals.[24] The diff-in-diff models (displayed in the top panel) demonstrate that the sick benefit cut led to a pronounced decrease in the number of sick days for older men, in the highest earner group. By contrast, the sick benefit cut had a negligible (statistically insignificant) effect for younger men. In line with this finding, the 'intensity of treatment' models suggest that older males responded more sensitively to the cut in sick benefit than the younger cohort.

Table 8: Diff-in-diff and 'treatment intensity' models, by age group

	Older cohort		Younger cohort		High healthcare spending		Low healthcare spending	
	Marginal effect	Standard error	Marginal effect	Standard error	Marginal effect	Standard error	Marginal effect	Standard error
Medium Earnings	-0,124	0,081	-0,035	0,142	-0,120	0,080	0,045	0,067
High Earnings	-0,429***	0,096	-0,165	0,264	-0,212	0,139	-0,247***	0,070
	Elasticity	Standard error	Elasticity	Standard error	Elasticity	Standard error	Elasticity	Standard error
Sick pay 2010-2011	-0,631***	0,151	-0,324**	0,165	-0,386**	0,167	-0,603***	0,205
Sick pay 2010	1,224***	0,203	1,294***	0,205	1,388***	0,248	1,639***	0,269
N. of obs.	106060 (53030 ind.)		112664 (56056 ind.)		109504 (54543 ind.)		109504 (54543 ind.)	

Note: standard errors are robust to clustering at individual level. Control variables are: (log) of current earnings, schooling, five-year birth cohorts, region of residence, occupation, lagged sick leave (dummy), lagged health care spending, (log) firm employment, (log) average firm wages, sector of activity. Statistical Models used: Zero-Inflated Negative Binomial.

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[24] It is worth noting that the number of days spent on sick leave was substantially higher in the older cohorts (0,92days) than in the younger cohort (0,64 days).

We also find some differences across men who are to likely be in poor health and those who are likely in good health, which is proxied by lagged health care spending.[25] We can indeed see that the length of time spent on long-term absence is more than twice as high among those with high health care spending than among those with low spending (1,05 days vs 0,51 days). Our results show qualitatively similar patterns of adjustment to the sick pay cut in the two groups, however, the decrease in sickness absence behavior for those with high health care spending is not statistically significant. Indeed, the sick leave behavior of those in low spending group seems more affected by payoff to staying out of work, while those with high spending are less strongly influenced by economic incentives: the elasticity of sick days to the cut in sick benefit is substantially higher for the first group (albeit the difference is not statistically significant). This is consistent with the idea that those likely in bad long-term health are seriously sick and are less responsive to economic incentives.

7   DISCUSSION AND CONCLUSIONS

In this paper, we estimate the effect of halving the maximum sickness benefits on sick leave behavior of prime-age males in Hungary. This policy change led to a halving of the effective replacement rate for the top 5 percent of employees, and to a sharp decrease (a 25 percent reduction) in the replacement rate for a further 17 percent of workers, while leaving the incentives for the workers with lower earnings unchanged. Using a difference-in-differences type methodology and relying on a large administrative dataset, we show that the short-term effect of the policy change was pronounced among high earners, with a large drop in the number of sick days. We however find no effect on those with lower earnings. Based on our models, we can predict the reduction in the number of sick days due to the policy change.: among those affected, the number of sick days over a 6-month period decreased from 0,64 to 0,55 days. The savings due to the benefit cut was substantial; we predict that the per person total outlay over a 6-month period decreased by 1840 HUF among those (potentially) affected by the policy change, which is equal to a 40 percent reduction in costs. Only a smaller part of this reduction came from behavioral responses, as 60 percent of the total fall in costs was due to the cut in the value of sick benefit.

Our estimates imply an elasticity to the reduction in sickness benefits of 0,45 which is higher than Ziebarth [2013]. There might be several reasons why we find a moderate significant elasticity. First, since the reform led to a substantial decrease in effective replacement rates it might have induced even relatively seriously sick persons to change their sickness absence behavior. Second, the fact that persons whom we expect to be in relatively good health are more responsive to incentives might be an indication that the stringency of sickness leave monitoring was not wholly adequate.

There are a few caveats to our study. First, we are unable to assess the effect of the reduction in sick pay on workers' subsequent health outcomes due to data limitations, thus we

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[25] We proxy health status in the following way: we take the (logarithm of) total care health spending in the years 2007-2008 and estimate regression models, controlling for basic background characteristics (birth cohort, schooling and region of residence). We use the residuals from this model to proxy health status, with those above the median being in 'bad health'.

are unable to assess whether the sick pay cut led to a reduction in shirking behavior or rather and increase in 'presenteeism'. Second, we are only able to estimate the short-term adjustment to the sick pay cut, as the data are only available for up to eight months after the policy change. Third, the effects we estimate are for a specific group of workers – high-earning, prime-age males with stable employment – so it is an open question whether these are generalizable. However, according to general results in labor economics, it is precisely the group that we study that are the least responsive in economic incentives in terms of their labor supply. If this holds true for sickness absence, then we can hypothesize that our elasticities are the lower bound for the general population.

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APPENDIX

Appendix Table 1: Selection of sample

	Number of observations
All men age 25-54	871 371
Employees	796 147
Insured for the whole month, had 180 days' insurance in previous year	503 975
Earnings not outlier	501 142
Was insured in July-December 2010 and 2011	333 125
Earnings did not change by more than 20% across 2010-2011	294850
Earnings in the relevant range	109 273
Not on sick leave 1st of May; is eligible for 182 days sick leave	109 086

Appendix Table 2: Selected characteristics, by treatment group, 2011

	Low earnings		Medium earnings		High earnings	
	Mean	St. dev.	Mean	St. dev.	Mean	St. dev.
Current Earnings (log)	12,394	0,193	12,803	0,272	13,530	0,398
Top manager	0,012	0,107	0,034	0,181	0,069	0,254
Lower management	0,059	0,235	0,103	0,304	0,282	0,450
Clerical	0,182	0,386	0,217	0,412	0,145	0,353
Skilled manual	0,283	0,450	0,162	0,369	0,026	0,159
Machine operator	0,236	0,425	0,175	0,380	0,020	0,141
Basic manual	0,040	0,196	0,013	0,114	0,003	0,058
Age 25-29	0,112	0,316	0,110	0,312	0,060	0,237
Age 35-39	0,189	0,391	0,194	0,396	0,245	0,430
Age 40-44	0,177	0,382	0,180	0,384	0,193	0,395
Age 45-49	0,163	0,369	0,156	0,363	0,136	0,343
Age 50-54	0,163	0,369	0,150	0,357	0,123	0,328
Inpatient spending	0,579	1,592	0,575	1,584	0,589	1,584
Outpatient spending	5,681	3,032	5,759	3,038	5,760	3,037
Medicine spending	6,025	3,643	6,154	3,635	6,292	3,546
Lagged sickness absence	0,096	0,294	0,072	0,258	0,033	0,180
Number of observations	34 205		58 491		16 390	

Appendix Table 2: Estimates of the Difference-in-Difference model

|	OLS		Zero Inflated Negative Binomial			
			Number of sick days		Inflate	
	Coefficient	St. error	Coefficient	St. error	Coefficient	St. error
Medium Earnings After	-0,083	0,064	-0,058	0,059	-0,001	0,042
High Earnings After	-0,164	0,064	-0,268	0,118	0,058	0,081
Medium Earnings	0,234	0,058	0,097	0,054	-0,178	0,039
High Earnings	0,893	0,115	0,191	0,135	-0,793	0,089
Year=2011	-0,083	0,055	-0,194	0,042	-0,200	0,031
Current Earnings (log)	-1,203	0,100	-0,360	0,105	1,340	0,066
Schooling: medium	-0,142	0,096	0,037	0,055	0,069	0,046
Schooling: high	-0,212	0,103	0,075	0,080	0,215	0,057
Top manager	0,113	0,055	0,250	0,120	0,272	0,109
Lower management	0,073	0,038	0,366	0,076	0,220	0,057
Clerical	-0,086	0,042	0,037	0,069	0,008	0,051
Skilled manual	0,379	0,062	0,244	0,082	-0,349	0,056
Machine operator	0,473	0,085	0,328	0,089	-0,393	0,061
Basic manual	0,420	0,165	0,303	0,118	-0,296	0,093
Age 25-29	-0,051	0,040	-0,029	0,060	0,062	0,046
Age 35-39	0,001	0,037	0,002	0,048	-0,001	0,037
Age 40-44	0,002	0,040	0,192	0,048	0,218	0,039
Age 45-49	0,092	0,049	0,400	0,050	0,340	0,041
Age 50-54	0,203	0,055	0,495	0,052	0,367	0,041
Northern Hungary	-0,107	0,047	0,077	0,048	0,185	0,037
Northern Great Plain	-0,159	0,045	0,052	0,051	0,232	0,040
Southern Great Plain	-0,204	0,058	0,087	0,064	0,323	0,051
Central Transdanubia	0,029	0,058	0,167	0,049	0,192	0,040
Inpatient spending	-0,230	0,051	0,140	0,057	0,453	0,046
Outpatient spending	-0,216	0,052	0,022	0,060	0,352	0,047
Medicine spending	0,027	0,014	0,014	0,007	-0,004	0,007
Lagged sickness absence	0,022	0,005	0,010	0,006	-0,032	0,005
Firm employment (log)	0,051	0,004	0,000	0,006	-0,083	0,004
Agriculture	1,992	0,096	-0,002	0,033	-1,207	0,030
Mining & quarrying	0,031	0,007	-0,048	0,009	-0,099	0,007
Electricity & Gas	-0,292	0,055	0,090	0,089	0,492	0,060
Water & sewage	-0,200	0,109	-0,035	0,143	0,309	0,109
Construction	-0,089	0,215	-0,222	0,165	-0,161	0,167
Wholesale & retail trade	0,562	0,098	0,031	0,060	-0,569	0,049
Transport & storage	0,058	0,127	0,037	0,092	0,028	0,071
Hotels & restaurants	0,406	0,101	0,191	0,063	-0,202	0,055
Information & communication	-0,037	0,050	0,144	0,074	0,275	0,054
Financial intermediation	0,110	0,061	0,205	0,048	0,176	0,039
Real estate	-0,423	0,127	-0,456	0,226	0,547	0,203
Science; R&D	-0,062	0,045	-0,138	0,086	0,002	0,061
Business Services	0,110	0,064	0,154	0,120	0,027	0,079
Public Administration	-0,251	0,093	-0,357	0,174	0,021	0,147
Education	-0,047	0,057	0,351	0,138	0,462	0,093
Health Care	-0,148	0,096	-0,037	0,122	0,090	0,094
Arts & Culture	-0,399	0,043	-0,067	0,081	0,594	0,060
Other Services	-0,211	0,174	0,074	0,493	0,580	0,288
Sector NEC	-0,152	0,166	0,346	0,222	0,575	0,219
Constant	15,304	1,272	6,964	1,305	-12,733	0,829

| N. obs. | 218122 (109086 ind.) | R-squared | 0,02 | Log-likelihood | -73283 | Alpha | 1,581 |

Appendix Table 3: Estimates of the Intensity of treatment model

	OLS		Zero Inflated Negative Binomial			
			Number of sick days		Inflate	
	Coefficient	St. error	Coefficient	St. error	Coefficient	St. error
Sick pay 2010	1,015	0,138	0,103	0,147	-1,248	0,101
Sick pay (2011-2010)r	-0,274	0,082	0,073	0,108	0,552	0,074
Current Earnings (log)	-1,251	0,112	-0,381	0,121	1,612	0,085
Year=2011	-0,089	0,040	0,056	0,056	0,074	0,046
Schooling: medium	-0,128	0,096	0,076	0,080	0,217	0,057
Schooling: high	-0,204	0,103	0,243	0,121	0,288	0,110
Top manager	0,100	0,055	0,356	0,077	0,194	0,057
Lower management	0,101	0,038	0,037	0,069	0,010	0,051
Clerical	-0,092	0,042	0,229	0,083	-0,402	0,056
Skilled manual	0,424	0,062	0,340	0,089	-0,413	0,061
Machine operator	0,511	0,085	0,324	0,120	-0,317	0,093
Basic manual	0,483	0,165	-0,020	0,060	0,052	0,046
Age 25-29	-0,052	0,040	0,002	0,049	0,004	0,037
Age 35-39	0,005	0,037	0,197	0,049	0,231	0,039
Age 40-44	0,000	0,040	0,406	0,051	0,357	0,041
Age 45-49	0,084	0,049	0,506	0,053	0,386	0,041
Age 50-54	0,195	0,055	0,080	0,049	0,173	0,037
Northern Hungary	-0,107	0,047	0,062	0,052	0,218	0,040
Northern Great Plain	-0,157	0,045	0,098	0,066	0,306	0,051
Southern Great Plain	-0,195	0,058	0,174	0,050	0,183	0,040
Central Transdanubia	0,031	0,058	0,138	0,057	0,436	0,046
Inpatient spending	-0,223	0,051	0,034	0,061	0,339	0,046
Outpatient spending	-0,213	0,052	0,014	0,008	-0,004	0,007
Medicine spending	0,028	0,014	0,011	0,007	-0,031	0,005
Lagged sickness absence	0,021	0,005	-0,001	0,006	-0,083	0,004
Firm employment (log)	0,051	0,004	-0,002	0,034	-1,211	0,030
Agriculture	1,999	0,096	-0,050	0,009	-0,098	0,007
Mining & quarrying	0,029	0,007	0,132	0,091	0,519	0,060
Electricity & Gas	-0,278	0,055	-0,042	0,148	0,291	0,109
Water & sewage	-0,179	0,109	-0,223	0,167	-0,098	0,166
Construction	-0,122	0,214	0,020	0,060	-0,537	0,049
Wholesale & retail trade	0,535	0,099	0,044	0,094	0,006	0,070
Transport & storage	0,082	0,127	0,186	0,064	-0,166	0,055
Hotels & restaurants	0,384	0,101	0,144	0,075	0,283	0,054
Information & communication	-0,030	0,050	0,198	0,049	0,194	0,040
Financial intermediation	0,102	0,061	-0,429	0,240	0,554	0,203
Real estate	-0,421	0,128	-0,138	0,087	0,010	0,061
Science; R&D	-0,052	0,045	0,153	0,123	0,011	0,079
Business Services	0,139	0,065	-0,392	0,170	0,036	0,147
Public Administration	-0,257	0,093	0,331	0,136	0,471	0,093
Education	-0,038	0,057	-0,052	0,125	0,107	0,094
Health Care	-0,147	0,096	-0,085	0,082	0,590	0,060
Arts & Culture	-0,397	0,043	0,018	0,490	0,583	0,299
Other Services	-0,199	0,174	0,376	0,222	0,585	0,220
Sector NEC	-0,146	0,166	0,260	0,243	0,432	0,230
Constant	15,304	1,272	6,250	0,737	-5,637	0,582

N. obs.	218122 (109086 ind.)	R-squared	0,02	Log-likelihood	-73280	Alpha	1,559
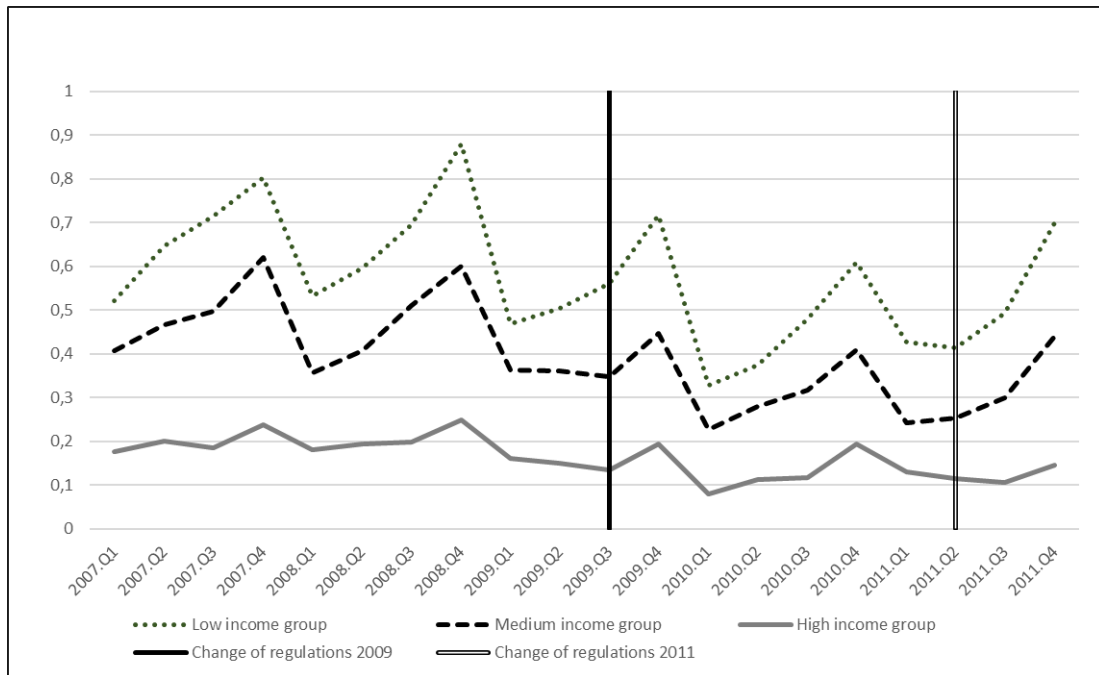
Appendix Figure 1: Evolution of sickness absence in the three income groups: quarterly average number of sickness absence days. Source: own calculation based on National Pension Insurance data.
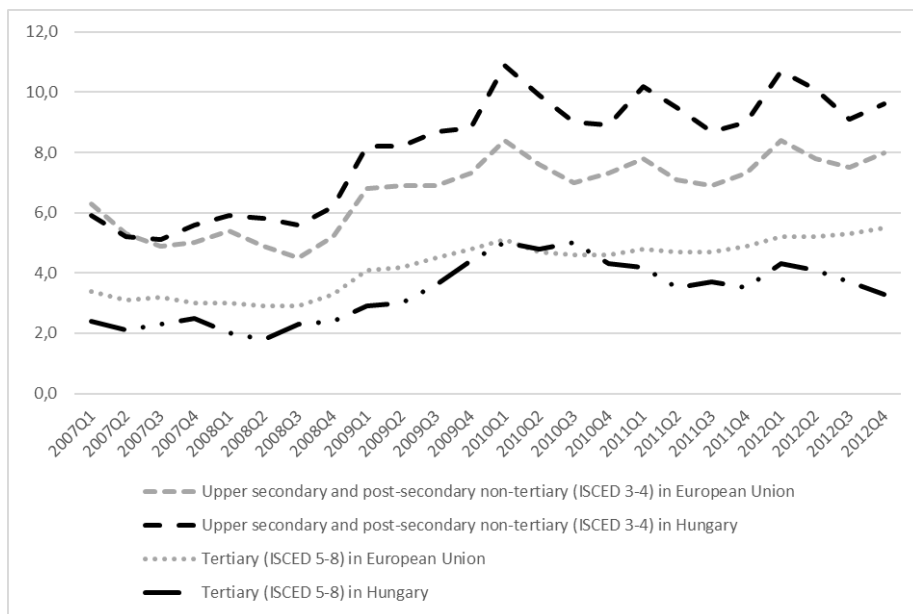
Appendix Figure 2: Evolution of unemployment rates of medium and high educated males, Hungary and EU28. Source: Eurostat.