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

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December 31, 2017

Abstract: This paper examines the incidence and the number of days spent on sick leave following the halving of the maximum sick benefit provided by statutory health insurance in Hungary. This policy change in 2011 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 used a difference-in-difference type methodology to evaluate the short-term effect of the reform. We relied on high-quality administrative data and analysed a sample comprised of prime-age male employees with high earnings and stable employment. Our results show that while the incidence of sick leaves decreased only moderately, 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 sickness benefits, we find a significant elasticity of - 0.8. We found considerable heterogeneity in the behavioural response, with older and those in poor health showing a large reduction in length of sick leave.

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

JEL classification: I18, J22, J32

* I would like to thank Katalin Bördős for expert research assistance, János Köllő for advice, Zuzsanna Bogdán for clarifications on the rules of sick pay, the Databank of the Institute of Economics, Hungarian Academy of Sciences for giving me access to the data used. Financial support from the Hungarian Scientific Research Fund is gratefully acknowledged (Grant OTKA PD 113253). Comments from the participants of the Joint Annual Meeting of the Slovak Economic Association and the Austrian Economic Association (2016); the Annual Meeting of the Hungarian Society of Economics (2016); the Annual Meeting of Hungarian Labour Economist (2016); as well as the IZA Workshop: Health and Labor Markets (2017) are gratefully acknowledged. All errors and omissions are my responsibility. Email: marton.csillag@budapestinstitute.eu

1 INTRODUCTION

The analysis of (dis)incentive effects of welfare programs has been a central research area in labour economics, and while these aspects of unemployment benefit regimes are fairly well understood, the design of programs compensating workers for long-term illness has only recently come under scrutiny. The elasticity of worker absence to the sickness leave benefit is the key parameter when thinking about the design of an “optimal” sickness benefit system, and our paper is the first to estimate this in an Eastern European context.

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. 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 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.

In this paper, we examine the role of incentives provided by the sickness compensation system in shaping the sickness absence rates in Hungary. In spite of the fact that the number of sickness absence days has been gradually decreasing in the last ten years, 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 benefit absence. In particular, the changes introduced in May 2011 cut the maximum of sick pay 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 pay for those below the income threshold unchanged, it is possible to study the behavioural response to a cut in sick benefits. Relying on this ‘natural experiment’, we use difference-in-differences methods to identify the effect of sickness benefits on the incidence and the number of sick days. We do this using a large longitudinal administrative database that allows us to precisely reconstruct eligibility for sick leave, as well as potential sick pay. 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 particular reform is threefold. First, most studies rely on relatively small changes in benefit replacement rates to identify the causal effect of sick pay on sick leave behaviour, while here we study a large cut in benefits. Second, most prior papers rely on variation in replacement rates in the region of 60 to 80 percent, here we study a case where the replacement rate was 42 to 60 percent prior to the policy change and fell to 21 to 45 percent. Third, we are unaware of any papers looking at sick leave behaviour in Eastern Europe, where unemployment and welfare benefits are substantially less generous than those in the EU15 and are closer to those in the United States.

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 analysed 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. 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. Most 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-difference type models.

The “natural experiment” approach was pioneered in this context by Krueger [1990a], who focussed on the incentives inherent 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. He studied a policy change when the minimum and maximum level of the WC benefit was raised by 5 percent, affecting the lowest and highest percentiles of workers, but leaving the incentives for the middle-level earners unchanged and allowing them to become the control group for those who were affected by the policy change. Using data from administrative records on WC claims and applying a difference-in-differences estimator, he found a significant 8 percent increase the length of absence spells after the change in legislation for the treated groups. Similarly to Krueger, Curington [1994] and Meyer, Viscusi and Durbin [1995] studied policy changes that affected the benefit maximums in different states of the US and applied difference-in-differences estimators. Both Curington and Meyer et al. found evidence of a positive elasticity regarding the duration of sickness absence spells, although their results are somewhat smaller than Krueger’s (0.27 to 0.62 and 0.129 to 0.238, respectively). In a similar vein, Krueger [1990b] uses variations in benefit rules to estimate the effect of benefits on the take-up of the worker’s compensation and found that a 10% increase in benefits lead to an about 7% rise in benefit receipt.

Most of the 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. A number of recent important studies (Ziebarth and Karlsson [2010], Puhani and Sonderhof [2010] and Ziebarth and Karlsson [2013]) have analysed 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 estimate relatively high elasticity (around 0.9). Another paper by Ziebarth [2013] examines a cut on the long-term sickness benefit 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. A substantially higher elasticity of sickness leave duration to sick pay is found in Böckerman et. al. [2014], who study the behaviour of Finnish workers using a regression kink design.

While it is clear that statutory health insurance reforms can have other impacts besides absenteeism, mostly on health-related outcomes, which is crucially important in assessing the welfare impact of these reforms, 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 behaviour, at least when measured at high replacement rates. More recently, Halla et. al. [2015] find that in Austria, workers' health subsequent to an increase in sick leave replacement rates improves, 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. Sick leave is comprised of two components: short-term and long-term sick leave. The first component covers up to 15 working days in a calendar year, during this period, the worker receives 70 percent of her earnings as sick pay, which is fully paid for by the employer.[1] Upon having exhausted her short-term sick leave[2], the person can enter long-term sick leave, which is co-financed by the employer (1/3 part) and social security (2/3 part). A health-impaired worker[3] 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.

The sick pay received during a long-term sickness spell depends on the employee's work (insurance) history and her previous earnings. 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 general 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

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[1] Note that contrary to many countries, there is no 'waiting period' neither for short-term, nor long-term sick leave in Hungary.
[2] In case of a work-related injury or sickness, as well as pregnancy requiring bed-rest, the employee enters long-term sick leave from the first day of the absence (without having to exhaust her short-term sick leave entitlement).
[3] 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 of less than 12 years of age.

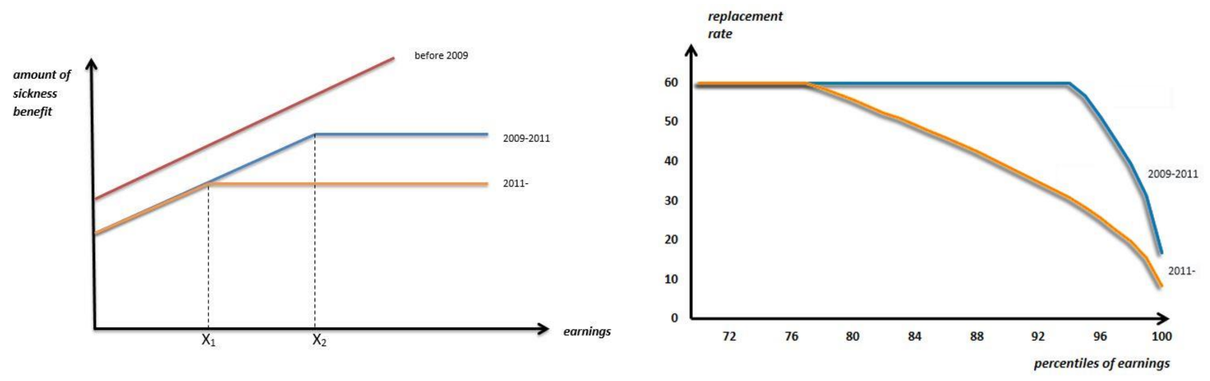
days. For those without such an employment spell, sick pay is based on statutory minimum wages.

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 does is not insured (ie. unpaid leave, periods of employer initiated or unlicensed absences for work, incarceration, unemployment).[4]

During the period under study the main policy changes affected the replacement rate. 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.[5] At the same time a *cap* on sick pay was introduced, it 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.

To get a better understanding of the structure of the long-term illness compensation, in *Figure 1* (left panel), we display the benefit schedule relating the benefit amount to previous earnings. Since the 2009 policy change, the benefit schedule contains a kink – denote *X2* 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 point *X1* (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 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.

[4] Note that periods of licensed sickness leave, and parental leave do not count as a 'break'.
[5] The lower replacement rate also applied to those who were hospitalised during their long-term sickness leave.



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

Figure 1: The effect of the policy change on sick pay and replacement rates

The empirical approach of the paper is to study the sick leave behaviour of three groups. The 'high earners', those earning above the 2009 threshold – who are the ones earning above point X2 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 X1 and X2 in our figure) were also negatively affected by the cut in the sick pay cap. Finally, the 'low earner' group are those with labour income below the 2011 threshold (below the point X1) were unaffected by the policy change. From the left hand panel, one can be see that the policy change decreased the sick pay in a piecewise linear fashion (with larger reductions for those with higher earnings between X1 and X2 and a flat reduction for those earning above X2). 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.

4 EMPIRICAL STRATEGY

To evaluate the effect of the policy change our first approach is to estimate difference-in-difference type models while controlling for workers' background characteristics. Thus, we will compare the change in incidence and 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 already 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:

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

where *Y* represents the outcome variable: the incidence of sick leave or the number of days spent on sick leave in a given month; *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) labour income[6], the vector *X* represents the individual's observable characteristics, finally *Month* are month fixed effects. The coefficients of interest are β_1 and β_2, as the estimates represent the differential change in sickness absence behaviour 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-difference 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 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.

An additional concern might be workers that might adjust their behaviour in anticipation of the planned legislative changes. More precisely, that affected workers, once the planned sick pay cap decrease is announced 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 also estimate models where the key explanatory variable is the (potential) sick pay:

$$Y_{it} = \beta \ln(sick\ pay)_{it} + \pi After_t + \theta \ln(earnings)_{it} + X_{it}\delta + \pi Month_t + \varepsilon_{it} \qquad (2)$$

In relation to this regression model, it is worth discussing the issue of disentangling the effect of sick pay (representing the economic benefits of staying on benefits) and the effect of current earnings (the economic benefits of returning to work) on workers' time spent on sick leave. In a cross-section, there can be two sources of identification, though both are 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. However this latter identification strategy relies on functional form assumptions. By contrast, having access to panel data and the time-series variation in the benefit schedule due to the policy change creates an additional exogenous source of identification.

Finally, to directly the effect of the change in the sick pay, we estimate models of the

[6] This variable is included to represent the incentives to remain in work as opposed to being on sick leave.

form:

$$Y_{it} = \beta_1[\ln(sick\ pay^{Before})_{it}{}^{\cdot} - \ln(sick\ pay^{After})_{it}] * After$$
$$+\beta_2 \ln(sick\ pay^{Before})_{it} + \pi After_t + \theta \ln(earnings)_{it} + X_{it}\delta + \pi Month_t + \varepsilon_{it} \quad (3)$$

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. The second variable, the sick pay under the 2010 rules is identified based on the bend in the benefit schedule. 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.

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.[7] The information we used comes from two primary sources.

The National Pension Insurance data contains detailed insurance (employment) histories. All periods when the individual was insured - eg. 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[8]. Intermittent 'breaks' in insurance spells are also contained in the dataset, along with the reason for this non-insured period.[9] This dataset thus allowed 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. The data also contains (labour) income data aggregated to monthly spells, which enabled us to reconstruct both the earnings that serve as 'reference income' for sick pay, 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.[10]

The National Health Insurance Fund data provide important information on two aspects. First, long-term sickness absence spells are recoded – but unfortunately sickness pay is not contained in the dataset. We used 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,

[7] The complete dataset contains a 25 percent simple random sample of the adult population of Hungary for the years 2004-2011.
[8] More precisely: it is a period that contributes towards 'number of insured days', but no contributions (neither health nor pension) are deducted.
[9] There are negligible differences in what count as contributory days towards pensions and sickness insurance.
[10] Note that level of education is not contained in the dataset.

medications)[11], as well as the number of visits to the individual's general practitioner.

5.1 Sample construction

We used a simple random sample of the complete linked administrative data, such that we had access to a 2,5 percent sample of the Hungarian adult population. Furthermore, we only relied on data from between 2007 and 2011, since earlier data from the National Health Insurance Fund is partially missing.

We only used prime age males in this study. We did 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 limited 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 were concerned that persons in ill health (or low tastes for work) would selectively withdraw from the labour 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 applied when selecting the sample to be analysed is related to employment (insurance) histories. In essence, we selected 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 pay 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 pay rise. If the same person was eligible for the (nominal) 60 percent replacement rate in both years, he experienced a sick pay cut due in 2011 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. As a consequence, 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 included employees, discarding self-employed and owners of corporations. Furthermore, we only included months when the person was fully insured, and excluded those individuals who did not have at least one fully insured month in the relevant period (the second half of the calendar year) both in 2011 and 2010. This was done to ensure that the sample analysed had stable composition across the pre- and post-treatment periods, such that selection out of employment (that could be related to health condition)

[11] Unfortunately, besides the sum spent on the health care of the individual, we no nothing about the person's illnesses or health status.

does not contaminate our results.

The final issue in constructing our sample relates to the definition of the treatment and control groups. We selected individuals based on their earnings in the ‘reference period’ for 2011. First, since the sick pay cap affected all those above the 77th percentile of earnings (among men with stable insurance histories) in 2011, to form a control group, we needed to have individuals with (slightly) lower earnings, but who were not completely dissimilar in terms of observable characteristics. Hence, we opted for including all those in the control group who had earnings above the 60th 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 pay levels – comes primarily from changes in sick pay due to legislative changes, we restricted the sample based on the relative value of reference period earnings in 2010 and 2011. More specifically, we only used those individuals whose reference earnings did not differ across the two periods by more than 20 percent. In practice, this meant that we discarded about 10 percent of observations. Thus, in essence, we did not allow for ‘moving’ between treatment and control groups, we will later test the sensitivity of our results to this assumption.

5.2 Variable definition

We use two key dependent variables in our analysis: the incidence of having a long-term sickness absence spell, and the number of days spent on long-term sickness absence, both defined over a one-month interval. 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 was 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 earned above the cap already 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 60th percentile of earnings in 2011). As described above, these groups were formed relative to ‘reference earnings’ in 2011.

We used a host of control variables. The most important of these is ‘current earnings’, which is equivalent to the mean daily earnings in the previous three calendar months. The explanatory variables related to personal background characteristics were five-year birth cohorts, region of residence, while work-related variables were two-digit occupation and public servant status. Finally, we controlled for the maximum number of long term absence days the person was eligible for, as well as the number of visits to the GP in the previous year and the amount of health care spending (the total spent on medications, in-patient and out-patient care).

5.3 Descriptive evidence

In *Figure 2* below, we show the number of sick leave days (per month) 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 (denoted by X1 for 2010 and X2 for 2011). It is clear that the ‘slope’ of the relationship between reference earnings became 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 much lower level in 2011 than in 2010, which is consistent with the halving of the effective replacement rate for the very high earning group.

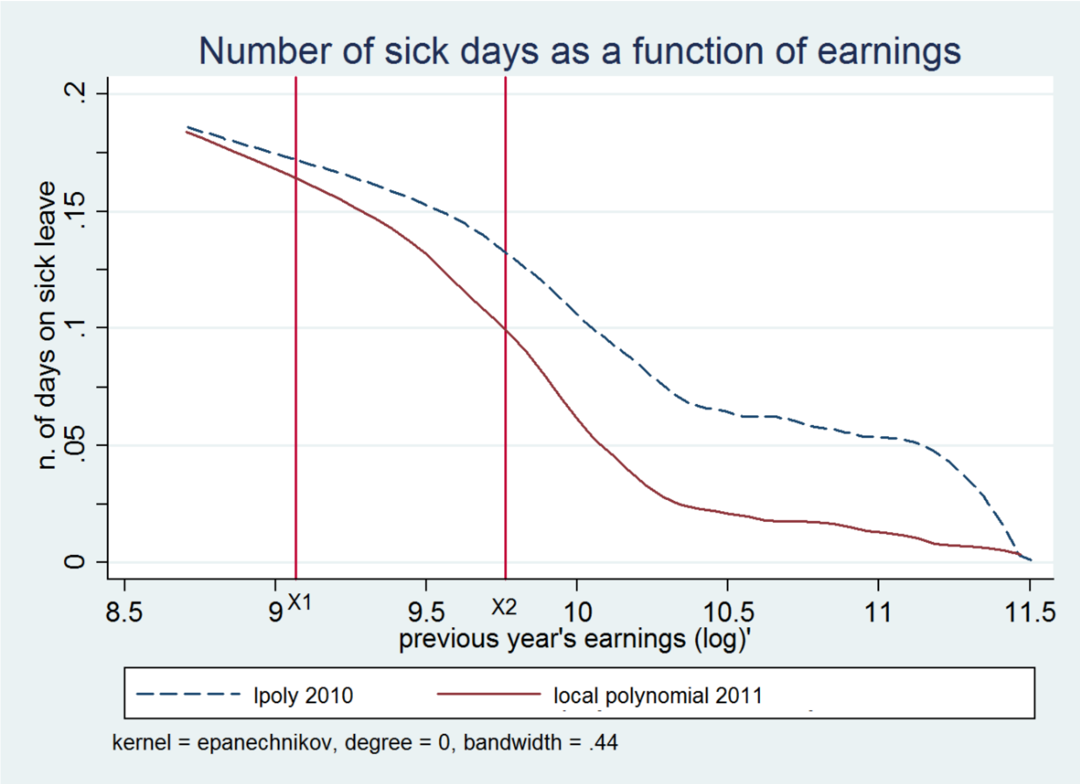


Figure 2: The number of sick leave days as a function of reference earnings, 2010 and 2011 (local polynomial smooth)

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 19 percent lower than that of the two higher earnings groups, while – for example – the current (daily) earnings of the medium earnings group were 59 percent higher than that of the low earnings group. The data on the mean monthly incidence and 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 incidence rates and sick leave days, with the low earnings group having around three times higher means in both respects compared to the high earnings groups. We can also see that while there was no change in the incidence and 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 – both the incidence and the number of sick days fell to less than half of that in 2010. These data also provide a first estimate of the effect of the policy change: a simple difference-in-difference estimate reveals there was a moderate reduction in the number of sick days for the medium earnings group and a pronounced fall for the high earnings group relative to the low earnings group, these estimates are not significantly different from zero at conventional levels.

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

	Incidence		N. of sick days		Sick pay		Current earnings	
	Mean	St. error	Mean	St. error	Mean	St. error	Mean	St. error
2010								
Low earnings	0,0175	0,0015	0,1795	0,0211	4,402	0,011	7,672	0,015
Medium Earnings	0,0127	0,0012	0,1406	0,0191	6,928	0,026	12,159	0,042
High Earnings	0,0057	0,0014	0,0595	0,0172	10,129	0,007	29,846	0,506
2011								
Low earnings	0,0153	0,0015	0,1834	0,0236	4,444	0,008	7,873	0,031
Medium Earnings	0,0121	0,0011	0,1270	0,0165	5,200	-	12,427	0,059
High Earnings	0,0031	0,0008	0,0236	0,0083	5,200	-	31,040	0,639
DiD estimates								
Medium Earnings	0,0016	0,0023	-0,0176	0,0382				
High Earnings	-0,0003	0,0024	-0,0397	0,0354				

Note: Standard errors are robust to clustering at individual level. The sample includes only July-December months. The number of observations is 2304 individuals in the low earnings group, 2911 individuals in the medium earnings grop, and 912 individuals in the high earnings group. Sick pay and current earnings are given in thousand Hungarian Forints (2011). Diff-in-diff estimates are calculated by OLS only controlling for month fixed effects.

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, and region of residence.[12] Thus, it is important to control for these observable determinants of sickness absence behaviour in econometric models.

[12] For example, while more than 42 percent of individuals in the high earnings group work in managerial occupations, only 14,5 percent and 6,5 of the medium and low earnings groups work in these occupations. Similarly, close to 60 percent of persons in the high earnings group live in the Central region of Hungary (containing the capital, Budapest), while these figures are 38 and 27 percent for the medium and low earnings groups respectively.

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. For incidence rates – where the dependent variable is equal to one if the individual had a long-term absence spell in the given month - , we used a Logit model, while for the number of sick days – where the outcome variable is the number of long-term sickness absence days in a given month - , we used a Zero-inflated Negative Binomial model, since these are better suited for modelling binary response and count data than OLS.[13]

Our main results for the difference-in-difference type models are displayed in Table 2, where we show models without control variables (only month fixed effects) and with control variables discussed in Section 5.2. In every table, 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).[14] As shown in the top panel of Table 2, the estimates for the medium earnings groups are positive and very close to zero. The effect of the policy change for the high earnings group is more pronounced, showing a decrease of about 0,48 percentage points, it however is not statistically significant at conventional levels. Related to the mean incidence rate, this translates into a 37,5% decrease. Turning to the number of sick days in the bottom panel, we can again conclude that the policy change had no effect of the behaviour of individuals in the medium income group. On the other hand, we see a large negative and statistically significant effect for the high earnings group. The reduction of about 0,08 sick days per month due to the policy change represents a more than 55% fall in the number of sick days.

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

.	*Incidence of sick leave*				*Number of sick leave days*			
	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	0,0012	0,0022	0,0016	0,0022	-0,0176	0,0315	-0,0038	0,0283
High Earnings	-0,0048	0,0030	-0,0048	0,0031	-0,0850	0,0289	-0,0796	0,0295
N. of obs.	79931 (6809 ind.)				79931 (6809 ind.)			

Note: standard errors are robust to clustering at individual level. Control variables are: (log) of current earnings, five-year birth cohorts, region of residence, occupation, public servant status, maximum number of sick leave days, lagged number of visits to GP, lagged health care spending, month fixed effects..

The specifications using the (potential) sick pay as main independent variable show large and statistically significant results. A one-percent increase in (daily) sick pay increases the (monthly) incidence of long-term sickness absence by 0,9 percentage points

[13] Note that we also experimented with Poisson and Negaitve Binomial, as well as Zero-Inflated Poisson regression models, however, based on goodness-of-fit diagnostics, the Zero-Inflated Negative Binomial model was retained.

[14] Note that we display robust standard errors that have been corrected for clustering at the individual level.

and the (monthly) number of sick days by 0,175 days. These translate to elasticities of 0,77 for the incidence rate and 1,33 for number of sick days. It is important to note also the large negative effect of current earnings on both the incidence and the number of sick days.

Table 3: The effect of sick pay and current earnings on incidence and number of sick days.

.	*Incidence of sick leave*				*Number of sick leave days*			
	Marginal effect	Standard error	Elasticity	Standard error	Marginal effect	Standard error	Elasticity	Standard error
Sick pay	0,0092	0,0029	0,776	0,239	0,1749	0,0385	1,335	0,282
Earnings	-0,0141	0,0019	-1,190	0,150	-0,1945	0,0251	-1,516	0,174
N. of obs.	79931 (6809 ind.)				79931 (6809 ind.)			

Note: standard errors are robust to clustering at individual level. Control variables are: five-year birth cohorts, region of residence, occupation, public servant status, maximum number of sick leave days, lagged number of visits to GP, lagged health care spending, month fixed effects..

Next, we attempt to directly disentangle the effect of the reduction in (potential) sick pay by separately including the sick pay under the 2010 benefit schedule and the difference between the sick pay under the old and the new benefit schedules. The results are shown in Table 4. As expected, we estimate a negative effect of the fall in sick pay. Our results are largely in line with the findings of the difference-in-difference models. The effect the reduction in sick pay due to the policy change is moderate and insignificant for the incidence of sick leave, it is large and marginally statistically significant for the number of sick days. We can note that the effect identified by the reduction is sick pay is substantially (and statistically) different from the effect identified by the nonlinearity of the benefit schedule (and the changes due to the evolution of earnings), with the former being roughly half size of the latter.

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

.	*Incidence of sick leave*				*Number of sick leave days*			
	Marginal effect	Standard error	Elasticity	Standard error	Marginal effect	Standard error	Elasticity	Standard error
Sick pay 2010	0,0112	0,0029	0,946	0,238	0,1903	0,0390	1,460	0,286
Sick pay 2010-2011	-0,0043	0,0043	-0,361	0,365	-0,1058	0,0605	-0,795	0,456
Earnings	-0,0152	0,0019	-1,277	0,157	-0,2078	0,0259	-1,619	0,182
N. of obs.	79931 (6809 ind.)				79931 (6809 ind.)			

Note: standard errors are robust to clustering at individual level. Control variables are: five-year birth cohorts, region of residence, occupation, public servant status, maximum number of sick leave days, lagged number of visits to GP, lagged health care spending, month fixed effects..

6.1 Robustness checks

The first issue we address is the sensitivity of our results to functional form assumptions. More precisely, 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 experimented with two models: first, having a quadratic term in (log) current earnings; second, adding a four-piece earnings spline by quartile of 'reference earnings', thus having piecewise linear model. Since the functional form assumptions are primarily important in models where we use sick pay as the variable of interest, we only include those results in Table 5. It is clear that controlling for current earnings in a more flexible way hardly changes our conclusions.[15] Including a quadratic term in earnings produces almost the same results as our baseline model. The piecewise linear model leads to slightly higher effects of the sick pay under the 2010 benefit rules and attenuates the effect of the sick pay cut.[16]

Table 5: The effect of the sick pay reduction on incidence and number of sick days, alternative functional forms.

.	*Quadratic in earnings*				*Spline in earnings*			
	Incidence		N. of sick days		Incidence		N. of sick days	
	Marginal effect	Standard error	Marginal effect	Standard error	Marginal effect	Standard error	Marginal effect	Standard error
Sick pay 2010	0,0094	0,0032	0,1707	0,0429	0,0149	0,0055	0,2067	0,0732
Sick pay 2010-2011	-0,0041	0,0043	-0,1137	0,0599	-0,0020	0,0049	-0,0918	0,0668
Earnings	-0,0147	0,0021	-0,1987	0,0287	-0,0147	0,0020	-0,2045	0,0274
N. of obs.	79931 (6809 ind.)				79931 (6809 ind.)			

Note: standard errors are robust to clustering at individual level. Control variables are: five-year birth cohorts, region of residence, occupation, public servant status, maximum number of sick leave days, lagged number of visits to GP, lagged health care spending, month fixed effects..

In the estimations presented above, we did 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 earning growth across two years is likely correlated with their health status. We approach this issue in two ways: first, we exclude all individuals who switched 'earnings group' across the two years; second, we limit the sample to those who experienced no more than a 5% change in their reference earnings between 2010 and 2011.[17] The results of both exercises are displayed in Table 6.

[15] In fact, the Akaike and the Bayesian information criteria do not show any improvement in the fit of the models relative to the baseline model.
[16] The fact that standard errors on sick pay under the 2010 law are considerably higher in the latter model also shows that it is more difficult to identify the effect of sick pay based on the non-linearity of benefit schedules once one allows for more flexible specifications.
[17] Note that the sample mean of neither the incidence nor the number of sick days differs when we exclude 'switchers', they are 1,25% and 0,134 days. However, these figures are slightly higher when we only keep those that had very limited change in their reference period earnings across the two years, they are 1,31% incidence rate and 0,148 sick leave days.

Regardless of which route we take rule out the effect of the 'contamination' of the treatment groups from switchers, are results are similar, and largely confirm our previous conclusions. We in fact find stronger negative effects for both the incidence and the number of long-term sick leave using the difference-in-difference specification which are statistically significant.

Table 6: Diff-in-diff type models of the incidence and number of sick days, controlling for switching between treatment and control groups

.	*No 'switchers'*				*Change in ref. earnings +/-5%*			
	Incidence		N. of sick days		Incidence		N. of sick days	
	Marginal effect	Standard error	Marginal effect	Standard error	Marginal effect	Standard error	Marginal effect	Standard error
Medium Earnings	0,0011	0,0023	-0,0038	0,0283	0,0044	0,0040	-0,0153	0,0448
High Earnings	-0,0069	0,0026	-0,0796	0,0295	-0,0075	0,0033	-0,1004	0,0339
N. of obs.	71280 (6073 ind.)				31801 (2700 ind.)			

Note: standard errors are robust to clustering at individual level. Control variables are: (log) of current earnings, five-year birth cohorts, region of residence, occupation, public servant status, maximum number of sick leave days, lagged number of visits to GP, lagged health care spending, month fixed effects..

Finally, we provide some circumstantial evidence on the plausibility of the assumptions underlying difference-in-difference methods by performing a 'placebo test'. 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.[18] We present results for the regression version of the diff-in-diff model; as well as for the models that rely on the 'intensity of treatment', the models with the difference in sick pay under the old and the new rules, that is. Reassuringly, the estimates for all 'treatment effects' are insignificant. The coefficients on the 'high earnings' group are negative in the diff-in-diff model, but they are only roughly two-thirds in size relative to the baseline results. By contrast, the coefficient on the (non-existent) decrease in sick pay is not only insignificant, but has the wrong sign.

[18] Note that both the incidence rate and the number of sick days is lower in the first quarter, they are 1% and 0,115 days.

Table 7: 'Placebo' tests, models based on the first three months of 2010 and 2011

.	*Diff-in-diff model*				*'Intensity of treatment' model*			
	Incidence		N. of sick days		Incidence		N. of sick days	
	Marginal effect	Standard error	Marginal effect	Standard error	Marginal effect	Standard error	Marginal effect	Standard error
Medium Earnings	-0,0002	0,0026	0,0168	0,0435				
High Earnings	-0,0030	0,0029	-0,0063	0,0570				
Sick pay 2010					0,0089	0,0041	0,0671	0,0603
Sick pay 2010-2011					0,0001	0,0051	0,0189	0,0768
N. of obs.	39563 (6743 ind.)				39563 (6743 ind.)			

Note: standard errors are robust to clustering at individual level. Control variables are: (log) of current earnings, five-year birth cohorts, region of residence, occupation, public servant status, maximum number of sick leave days, lagged number of visits to GP, lagged health care spending, month fixed effects.

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.[19] The diff-in-diff models (displayed in the top panel) demonstrate that the sick pay cut led to a pronounced decrease both in the incidence and the number of sick days for older men, but had negligible (statistically insignificant) effect for younger men. When looking at the models using the 'intensity of treatment', we find analogous results: the two estimates differ sharply across the age groups - younger men seem not to have responded to the benefit cut, while older men adjusted their behaviour. We find similar patterns for the effect of sick pay under the old benefit schedule, hence it is likely that older men are more responsive to financial incentives.[20]

[19] It is worth noting that while the incidence rate is only slightly higher for the older group than for the younger group (1,39% vs 1,18%), the number of days spent on sick leave is markedly higher (0,179 vs 0,103 days).
[20] Note that one cannot reject the hypothesis that the two coefficients are equal for the 'old benefit schedule'. It is also interesting to note that the elasticity of sick days (and incidence) to current earnings is very similar for the two groups, around – 1,6.

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

.	*Older cohort*				*Younger cohort*			
	Incidence		N. of sick days		Incidence		N. of sick days	
	Marginal effect	Standard error	Marginal effect	Standard error	Marginal effect	Standard error	Marginal effect	Standard error
Medium Earnings	0,0007	0,0035	0,0122	0,0529	0,0029	0,0029	0,0088	0,0316
High Earnings	-0,0097	0,0026	-0,1225	0,0402	-0,0040	0,0079	-0,0419	0,0424
Sick pay 2010	0,0156	0,0046	0,2995	0,0683	0,0082	0,0035	0,1312	0,0405
Sick pay 2010-2011	-0,0116	0,0073	-0,2306	0,1115	-0,0024	0,0051	-0,0273	0,0608
N. of obs.	37656 (3255 ind.)				42225 (3645 ind.)			

Note: standard errors are robust to clustering at individual level. Control variables are: (log) of current earnings, five-year birth cohorts, region of residence, occupation, public servant status, maximum number of sick leave days, lagged number of visits to GP, lagged health care spending, month fixed effects..

We also find very marked differences among men whom we proxy to likely be in poor health and those who are likely in good health.[21] We can indeed see that the incidence of long-term absence is close to four times as high among those predicted to be in poor health (1,96% vs 0,57%) and the number of days spent on sick leave is more than three times as high in this group (0,211 vs 0,065 days).[22] Our results show that for individuals in presumably good health, the cut in sick pay had no effect on sick leave behaviour. By contrast, the effect of the policy change lead to a large fall in the number of sick leave days among those in poor health and who saw their sick pay cut in half (the 'high earnings' group), but only a small, statistically not significant decrease in the incidence of sick leaves. The same result holds when looking at the response to the change in the daily sick pay, the decrease in sick leave days is large and marginally significant for those n poor health. Indeed the sick leave behaviour of those in good health seems unaffected by payoff to staying out of work, while those in poor health are strongly influenced by economic incentives.[23]

[21] We proxy health status in the following way: we take the number of visits to the GP in the years 2007-2008 and estimate count data models (zero-inflated negative binomial), controlling for basic background characteristics (birth cohort and region of residence). We use the 'raw residuals' from this model to proxy health status, with those above the median being in 'bad health'.

[22] Our proxy for poor health could partially pick up 'tastes for work'. However, it is also very strongly correlated with health care spending, it can explain roughly 11 percent of the variation in health care spending.

[23] In fact the estimated elasticity of sick leave days to the cut in sick benefits in 2011 is 0.97 for those in poor health, while the elasticity for incidence is 0,55 and is statistically insignificant.

Table 9: Diff-in-diff and 'treatment intensity' models, by health proxy

.	*Good health*				*Poor health*			
	Incidence		N. of sick days		Incidence		N. of sick days	
	Marginal effect	Standard error	Marginal effect	Standard error	Marginal effect	Standard error	Marginal effect	Standard error
Medium Earnings	0,0019	0,0024	0,0550	0,0449	0,0005	0,0037	-0,0431	0,0461
High Earnings	-0,0025	0,0022	-0,0359	0,0267	-0,0054	0,0062	-0,1165	0,0567
Sick pay 2010	0,0027	0,0024	0,0441	0,0383	0,0243	0,0056	0,4212	0,0702
Sick pay 2010-2011	-0,0003	0,0039	-0,0049	0,0557	-0,0103	0,0081	-0,1979	0,1135
N. of obs.	39492 (3360 ind.)				39063 (3334 ind.)			

7 DISCUSSION AND CONCLUSIONS

In this paper, we estimated the effect of halving the maximum sickness benefits on sick leave behaviour 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-difference type methodology and relying on a large administrative dataset, we show that the effect of the policy change was pronounced among high earners, with a small reduction in the incidence and 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 reduction in the number of sick days due to the policy change. Among those affected, the number of sick days per month decreased from 0,121 to 0,097 days per month, representing a 20 percent fall. Unsurprisingly, the response to halving the maximum sick pay was much more pronounced among the 'high earner' group, they reduced the number of days spent on sickness benefits by 42 percent. The savings due to the benefit cut was substantial, in total, we predict that the per month per person total outlay decreased by 366 HUF among those affected by the policy change, which is equal to a 42 percent reduction in costs. Only a smaller part of this reduction came from behavioural responses, fully 63 percent of the fall in costs was due to the cut in the value of sick pay.

Our estimates imply an elasticity of sick leave days with respect to sick pay of about 1.3. which is on the higher side of previous estimates and an elasticity to the reduction in sick pay of 0.8 which is in line with the results of Ziebarth and Karlsson [2010]. The interest of our results is threefold. First, most studies rely on relatively small increases in benefit replacement rates to identify the causal effect of sick pay on sick leave behaviour, while here we study a large cut in benefits. Second, most prior papers rely on variation in replacement rates in the region of 60 to 80 percent, here we study a case where the replacement rate was 42 to 60 percent prior to the policy change and fell to 21 to 45 percent. Third, we are unaware of any papers looking at sick leave behaviour in Eastern Europe, where unemployment and welfare benefits are substantially less generous than those in the EU15 and are closer to those in the United States.

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 are unable to assess whether the sick pay cut led to a reduction in shirking behaviour 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 labour economics, it is precisely the group that we study that are the least responsive economic incentives in terms of their labour supply. If this hold true for sickness absence, then we can hypothesise that our elasticities are the lower bound for the general population.

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