ESSAYS IN LABOR ECONOMICS

by

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Detecting Wage Under-reporting using a Double Hurdle Model

With Péter Elek, János Köllő and Péter A. Szabó

The nature of cooperation and roles of the individual co-authors and approximate share of each co-author in the joint work are the following: The econometric model was developed by Péter Elek, János Köllő and Szabó A. Péter and I participated mainly in the empirical application of the model. I chose the final specification of the Double-Hurdle model and I made the individual level analysis. The final version was developed in continuous cooperation with my co-authors.

Detecting Wage Under-reporting using a Double Hurdle Model

With Attila Lindner

The nature of cooperation and roles of the individual co-authors and approximate share of each co-author in the joint work are the following: I started the project alone with examining the Hungarian reform that changed the UI benefit path. I managed to get access to administrative data set and provided the preliminary results. Attila Lindner was responsible for the theoretical model. The final version of the paper was developed in continuous cooperation with Attila Lindner

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Abstract

This thesis consists of one single-authored and two co-authored chapters, which investigate how changes in wages and unemployment benefits affect the transition between employment and unemployment.

The first chapter examines the effect of bonus payments on labor market fluctuations. A large share of workers receives bonus payments besides their base wage. The benefits of flexible wage components in remuneration are twofold: they can incentivize workers and make it easier to adjust wages downward in response to negative shocks. Using data on bonus payments of Hungarian workers from linked employer-employee data, I disentangle the importance of these two factors to assess their respective importance. First, I show that bonus payments flexibly adjust to the revenue shocks of firms. At the same time, the separation rate of workers without bonuses do not react more to revenue changes than the separation rate of workers with bonuses. Bonus paying firms are shown to be financially more stable, larger and more productive, and they have less volatile revenue than firms not paying bonuses. These facts are consistent with a wage posting model with incentive contracting, but they are hard to reconcile with models emphasizing the role of bonus payments in alleviating wage rigidity. These results indicate that wage flexibility regulations may not affect the employment responses of firms to negative shocks

The second chapter is co-authored with Péter Elek, János Köllő and Péter András Szabó. In this chapter, we estimate a double-hurdle (DH) model of the Hungarian wage distribution assuming censoring at the minimum wage and wage under-reporting (i.e. compensation consisting of the minimum wage, subject to taxation and an unreported cash supplement). We estimate the probability of under-reporting for minimum wage earners, simulate their genuine earnings and classify them and their employers as 'cheaters' and 'non-cheaters'. In the possession of the classification, we check how cheaters and non-cheaters reacted to the introduction of a minimum social security contribution base, equal to 200 per cent of the minimum wage, in 2007. The findings suggest that cheaters were more likely to raise the wages of their minimum wage earners to 200 per cent of the minimum wage, thereby reducing the risk of tax audit. Cheating firms also experienced faster average wage growth and slower output growth. The results suggest that the DH model is able to identify the loci of wage under-reporting with some precision.

The third chapter is co-authored with Attila Lindner and it estimates welfare consequences of frontloading the unemployment benefit. In November 2005, the Hungarian government frontloaded the UI benefit path, while keeping constant the total benefit amount that could be collected over the UI spell. We estimate the effect of this reform on non-employment duration using an interrupted time series design. We find that non-employment duration falls by 1.5 weeks after November 2005. We show that this response is large enough to make the policy revenue neutral. Our evaluation for this reform is positive: frontloading increased job finding, did not make any unemployed worse off, and did not cost anything to the government.

Chapter 1

“Do Firms Pay Bonuses to Protect Jobs?”

The use of flexible wage elements over the base wage is widespread: approximately 50% of employees in developed countries receive flexible wage elements such as bonuses, allowances and overtime payments, and these components have fed into higher wage inequality over the last decades.

Flexible wages elements may be dictated by different rationales. For example, they may be paid to incentivize workers’ effort by linking total wage compensation to output. Or they may help cushion the effects of negative revenue shocks on employment, in the face of downward-rigid base wages. This channel is particularly important whenever job loss is a major source of inequality.

In this paper, I provide evidence that the bonuses are paid to incentivize workers. I develop a tractable wage posting model where firms can share their revenue with the workers in form of bonus payments. The model formally distinguishes formally between the consequences of wage flexibility and the incentive contract explanation for bonus payments. I test the implications of the model with a unique linked employer-employee database that contains detailed worker-level information on the structure of earnings (and bonus payments) and also firm-level income statement information. These data allow me to estimate employment and wage responses to idiosyncratic revenue shocks, and to test whether these responses are different for workers with and without bonuses.

According to my main results bonus payments are flexibly adjusted to firm-level revenue shocks, while base wages are more rigid. I show that workers with bonuses are not more likely to keep their job in response to negative revenue shocks compared to fixed-wage workers. This reduced-form evidence indicates that bonuses affect the adjustment of wages more than the adjustment of employment.

Bonus paying firms are also more productive, and they have more employees and less volatile growth rates than firms without bonuses. The relationship between the prevalence of bonus payments and revenue volatility is strictly decreasing in contrast to the non-monotonic relationship implied by the endogenous separation model.

Chapter 2

”Detecting Wage Under-reporting using a Double Hurdle Model”

with Péter Elek, János Köllő and Péter A. Szabó

The evasion of payroll taxes has two main forms. One is unreported (black) employment, when the employee is not registered and neither she nor her employer pays any taxes. The other main form is the under-reporting of wages, or grey employment, when the compensation consists of an officially paid amount, subject to taxation, and an unreported supplement also known as an “envelope wage” or “under the counter payment”. In order to maximize the total evaded tax, the officially paid wage is often (but not always) chosen as the minimum wage (MW).

In this paper we estimate the prevalence of disguised MW earners with the double hurdle (DH) model, first proposed by Cragg (1971), using linked employer-employee data. The DH is a potentially suitable method for disentangling genuine from 'fake' MW earners, relying on the assumption that MW payment is governed by two different processes: market imperfections implying censoring at the MW, on the one hand, and non-random selection to wage under-reporting, on the other. Our application of the DH for Hungary assumes that a spike at the MW was observed for two reasons (i) because of constraints and costs preventing firms from firing all low-productivity workers after a wave of exceptionally large hikes in the MW and (ii) because of tax fraud. That said, a worker's genuine wage is observed only if her productivity exceeds the MW and her wage is fully reported. The DH model simultaneously deals with the censoring problem and selection to tax fraud, and estimates the probability of cheating for each MW earner. In the possession of the parameters one can also simulate the 'genuine' wages of MW earners.

The DH model's reliance on distributional properties (as well as the difficulty in finding exclusion restrictions for the selection equation) warns us not to take the estimates at face value. Therefore, we test the validity of the DH results by exploiting a unique episode of Hungary's unconventional MW policies. The test examines the introduction of a minimum contribution base amounting to 200 per cent of the minimum wage (2MW), in 2007. After the introduction of the reform, firms paying wages lower than 2MW faced an increased probability of tax authority audit and a higher risk of being detected as cheaters. Firms were required to report that they paid wages below 2MW and provide evidence, upon request, that their low-wage workers were paid at the going market rate. The reform created incentives for cheating firms to raise the reported wages of MW earners to 2MW while non-cheaters (those paying genuine minimum wages) had no interest to do so. We distinguish cheaters from non-cheaters on the basis of DH estimates for 2006 and check how the cheating proxies affected the probability that a worker earning the MW in 2006 earned 2MW in 2007. We also study how the wages of MW earners changed in 2006-2007. We find that suspected cheaters were more likely to shift their workers from MW to 2MW compared to non-cheating firms. Furthermore, we find that the sales revenues of cheating firms were adversely affected by the reform.

Chapter 3

Frontloading the Unemployment Benefit: An Empirical Assessment

With Attila Lindner

Unemployment insurance programs aim to protect against financial distress at job loss and to maintain incentives to search for jobs. Unfortunately, these two goals are often in conflict: an insurance that provides better protection often leads to moral hazard and, as a result, to longer unemployment duration. This classic trade-off between insurance value and moral hazard determines the optimal level of the unemployment benefit.

However, the classic analysis of optimal unemployment insurance (UI) assumes that the benefit is constant throughout the unemployment spells. Changing the benefit path, in principle, can maintain the insurance aspects of UI and can provide more incentives to search for a job at the same time. For instance, consider a change that frontloads the benefit profile by raising the unemployment benefit with $1 in the first period and by cutting it with $1 in the second period. Under this benefit change, the short-term unemployed can collect more benefits, while the long-term unemployed collect the same amount of benefit throughout their unemployment spell. Therefore, benefit frontloading makes none of the unemployed worse off and makes some of them better off.

The benefit frontloading described here can lead to a win-win situation where some of the unemployed are made better off without making any other actors worse off. However, it remains an empirical question whether the cost savings caused by the behavioral responses is large enough to offset the mechanical cost increase induced by the reform. This paper provides the first empirical assessment to answer this question. We exploit a unique Hungarian reform that changed radically the time profile of UI payments. The unemployed who claimed benefit before 1st of November 2005 could rely on a constant benefit for 270 days. However, those who claimed benefit after November 1st were eligible to the same benefit amount, but in a different structure: they had higher benefit in the first 90 days and then lower in the next 180 days. Putting it simply, the Hungarian UI reform frontloaded the benefit profile while the total benefit that could be collected remained the same.

We assess the effect of this unique policy change on non-employment duration using administrative data on UI claimants and social security contributions. Our main empirical strategy compares non-employment durations for those who claimed benefit before the UI change, and were, therefore, left with the old benefit schedule, to those who claimed afterwards. We implement an interrupted time series analysis and show that the average non-employment duration was stable preceding the reform, while there was a sharp drop in non-employment duration that coincides with the timing of the reform. We estimate that non-employment duration decreased by 10 days, or 1.5 weeks after the reform.

We also examine the effect of the benefit change on the quality of jobs found. We do not find any evidence for a change in reemployment wages or in the duration of new jobs. Therefore, our estimates suggest that the shortened unemployment duration did not lead workers to accept worse (or better) jobs.

We then we translate the estimated effects into changes in the UI budget. The new benefit mechanically increased governmental spending, because short-term unemployed collected more benefits. However, it also fastened up job finding, which decreased spending on unemployment benefits. These effects offset around 50% of the mechanical cost increase. Another offsetting channel is the increase in personal income tax and social security contributions. This latter offset another 70% of the mechanical cost increases, and so the behavioral responses were large enough to counterbalance the mechanical cost increase caused by the reform.

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Chapter 1

Do Firms Pay Bonuses to Protect Jobs?

Abstract[1]

A large share of workers receives bonus payments besides their base wage. The benefits of flexible wage components in remuneration are twofold: they can incentivize workers and make it easier to adjust wages downward in response to negative shocks. Using data on bonus payments of Hungarian workers from linked employer-employee data, I disentangle the importance of these two factors to assess their respective importance. First, I show that bonus payments flexibly adjust to the revenue shocks of firms. At the same time, the separation rate of workers without bonuses do not react more to revenue changes than the separation rate of workers with bonuses. Bonus paying firms are shown to be financially more stable, larger and more productive, and they have less volatile revenue than firms not paying bonuses. These facts are consistent with a wage posting model with incentive contracting, but they are hard to reconcile with models emphasizing the role of bonus payments in alleviating wage rigidity.

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1.1 Introduction

Bonus compensations are widespread at workplaces. Recent evidence shows that half of the workers receive bonus payments in addition to their base wage in the United States (Bloom et. al. 2011). The share of workers with bonuses has increased over time both in the United States and in Western European countries (Lawler and Mohrman, 2003; Lazear and Shaw, 2008).

The causes and consequences of bonus payments are not well understood. One strand of the literature argues that bonuses are paid to incentivize workers (Holmström 1979; 1982; Card and Hyslop, 1997; Grossman and D, 1981; Levin, 2003)[2]. By linking wage compensation to output, firm owners reduce the moral hazard in their workers' effort. As a result, the total compensation of bonus receiving workers co-moves with the changes in revenues of firms. These models also imply that firms with less volatile revenue shocks are more likely to pay bonuses.

In other papers, bonuses are perceived as a way to cushion the effects of negative revenue shocks on employment (Weitzman, 1983; 1985;). In these models, flexible wages allow firms to react at the level of the wage margin rather than the employment margin in response to negative revenue shocks. When adjusting employment is costly, these models predict that firms with more volatile revenues are more likely to have flexible wage components.

While both of these explanations might play a role in paying bonuses, estimating their relative importance has major policy implications. If the flexibility of bonuses leads to lower separation rates in case of negative revenue shocks then public policies subsidizing bonus payments can "grease the wheels" and decrease frictional unemployment when inflation is low (Tobin, 1972; Weitzman, 1987). By contrast, if bonus payments do not protect jobs, such policies are unlikely to impact the level of employment.

In this paper, I provide evidence that the bonuses are paid to incentivize workers. I develop

[2]Field experiments showed also that the productivity of workers significantly increases after the introduction of output-based compensation (Lazear, 000a; Shearer, 2004a; Bandiera et al., 2005).

a tractable wage posting model where firms can share their revenue with the workers in form of bonus payments. The model formally distinguishes formally between the consequences of wage flexibility and the incentive contract explanation for bonus payments. I test the implications of the model with a unique linked employer-employee database that contains detailed worker-level information on the structure of earnings (and bonus payments) and also firm-level income statement information. These data allow me to estimate employment and wage responses to idiosyncratic revenue shocks, and to test whether these responses are different for workers with and without bonuses.

According to my main results bonus payments are flexibly adjusted to firm-level revenue shocks, while base wages are more rigid. I show that workers with bonuses are not more likely to keep their job in response to negative revenue shocks compared to fixed-wage workers. This reduced-form evidence indicates that bonuses affect the adjustment of wages more than the adjustment of employment. .

In the theoretical part of my paper I derive additional testable implications to distinguish the incentive contract and wage flexibility explanations of bonus payments. I build on the standard wage posting model of Manning (2003; 2004) that examines optimal wage setting in an equilibrium framework. In this model, firms offering a higher wage are able to fill their jobs more quickly, but they earn less profit per worker. In equilibrium, wages are determined by the level of unemployment, the (exogenous) job separation rate and the productivity of firms.

In the standard wage posting model, firms are restricted to offer fixed-wage contracts. To analyze bonus payments, I extend the model in two directions. First, I capture the incentivizing effect of bonuses by assuming that the effort of workers is unobserved. Accordingly, as in the hidden action model of Hölmstrom (1979), firms make inferences about the effort of workers by observing the actual output (total revenue). However, the more volatile the revenue shocks are, the harder it is to draw such an inference, and if the revenue is too noisy, firms simply opt for a fixed-wage contract. In the model, firms (exogenously) differ in the volatility of revenue shocks which also explains why some firms choose to pay bonuses, while others do not.

The second extension to the model introduces endogenous job separation by allowing firms to fire workers. A temporary negative shock in revenue pushes firms to reduce employment at least temporarily. However, laying off employees is costly, because finding a worker later takes time. Therefore, firms will keep their workers even if their marginal product is somewhat lower than their actual wage. While flexible wages allow firms to adjust wages to the marginal product of labor, and so reduce employment fluctuations, they also create fluctuations in wages that workers dislike. Again, the volatility of revenue plays a crucial role in determining whether bonus payments are optimal. When volatility is low, fixed wages are offered and firms do not react to temporary revenue shocks. For medium-sized shocks, bonus payments are provided, and as a result, employment fluctuations are attenuated relative to the fixed contract arrangement. Finally, for very high volatility in revenue, a fixed-wage contract is chosen and firms respond to negative shocks at the level of the employment margin.

While both hidden action and endogenous job separation can explain why some firms pay bonuses while others do not, they have radically different predictions for the type of firms paying bonuses. The incentive contract model predicts that firms with bonuses have less volatility in revenue, they are more productive and are larger in general. By contrast, endogenous job separation anticipates that firms with bonuses will be smaller and predicts an inverted U-shape relationship between bonus payments and revenue volatility.

I compare these theoretical predictions with the pattern of bonus payments in Hungary. My empirical results are in line with the incentive contract explanation. Bonus paying firms are more productive, and they have more employees and less volatile growth rates than firms without bonuses. The relationship between the prevalence of bonus payments and revenue volatility is strictly decreasing in contrast to the non-monotonic relationship implied by the endogenous separation model. The the employment and wage reactions of firms are also in line with the incentive contract explanaition: bonus paying firms adjust wages more but they do not smooth employment more in the event of negative revenue shocks.

I also carry out several robustness checks of the empirical findings. Using a broad set of control variables and alternative sample selections barely affects the point estimates. The results are also robust to changing the definition of bonus payments. Bonuses have similar effects across the various subsamples.

At the end of the paper, I briefly discuss alternative explanations for bonus payments. First, firms may pay bonuses to screen the best workers. In this case, the optimal strategy for firms is to offer a menu of wages and let workers choose between a fixed wage and revenue sharing. However, I find that a high share of firms pay bonuses to all of their workers. Second, firms may pay bonuses mainly to cope with outside wage offers. However, in this case, it is hard to understand why bonus paying firms are more productive than firms without bonuses. Third, firms may be larger and more productive, and decide to pay bonuses because they have a more able management. I used firm-fixed effects to control for the differences in time-invariant managerial skills and the results remained the same.

My results relate to the empirical investigation of incetive wage schemes. Previous literature in this area concentrated on plant level experiments and argued that piece rate contracts have incentive effects(Lazear, 000a; Shearer, 2004b; Bandiera et al., 2005). I show evidence that bonuses have incentive effect regardless the industry and the occupation of the workers.

This paper also draws on the extensive literature on downward wage rigidity. Recent research (Card and Hyslop, 1997; Altonji and Devereux, 2000; Dickens et al., 2006; Kátay, 2011; Daly et al., 2012) provides ample evidence of downward wage rigidity in many countries and industries. Bonuses, however, have been found to respond more to aggregate shocks (Oyer 2005; Messina et al. 2010; Anger 2011; Lemieux et al. 2012). My results confirm these previous findings, but also extend them by connecting the flexibility of bonus payments to firm-level revenue shocks.

In spite of its policy relevance, there is little direct evidence on the negative effect of wage rigidity on the level of employment. The only exceptions are Fehr and Goette (2005);

Stokes et al. (2014) and Schoefer (2015). On the contrary, Card and Hyslop (1997); Altonji et al. (1999); Elsby (2009) do not find significant employment cost of wage rigidity. My results support the latter findings as the effect of revenue changes does not affect more the separatation rate of workers with rigid wages. These results indicate the wage flexibility, or at least the flexibility of bonus payments, does not protect jobs in case of netagive revenue shocks.

The paper is organized as follows: Section 2 sets forth a simple wage posting model with incentive contracts and endogenous separations. Section 3 describes the Hungarian institutional context. Section 4 introduces the database. Section 5 shows the wage adjustment and separation rates of workes with and without bonuses. Section 6 tests the implications of the model for the volatility of firm revenue. Section 7 assesses alternative explanations for bonus payment, and finally Section 8 presents the conclusions of the paper.

1.2 Model

In this section, I provide a theoretical framework for analyzing why firms pay bonuses and what empirically testable consequences the underlying reasons have. In Section 2.1, I introduce the baseline wage posting model of Manning (2003; 2004) with worker-level productivity shocks. The idea of bonus payment is incorporated using linear contracts. I assume that firms can offer a fixed base wage and share part of the revenue with the worker. Firms maximize profit and choose a wage base wage and a revenue sharing parameter based on the variance of their revenue shocks. The worker receives a wage offer with a probability less than one and they can decide whether to accept or reject the wage offer. I also show that in my setup every wage offer is accepted which provides higher utility than the current utility of workers. I follow the strategy of Manning (2003; 2004) and I only describe the steady-state characteristics of the economy without evaluating model dynamics, so time indeces are suppressed in the derivations.

My contribution to the literature is that I derive the optimal strategy for bonus payments if bonuses have incentive effects and firms can lay off workers upon case of negative revenue shocks. My ultimate goal is to distinguish the two explanations that is why I discuss the two models separately and I derive empirically testable predictions.

First I incorporate the incentive effects of bonus payment to the baseline model in Section 2.2. I assume that workers have two discrete effort levels which are not observed by the employer. In this setup the revenue sharing is an instrument to motivate workers to exert higher effort.

Second, I allow firms to lay off workers if a negative shock hits the firm and the value of the worker-firm match turns negative (Section 2.3)[3]. This kind of endogenous separation catches the idea that firms may fire workers if they cannot cut wages. Here the firms use revenue sharing to increase the profit of the match in recession by allocating part of the negative revenue shocks to the worker.

1.2.1 Setup of the baseline model

This section introduces the baseline wage posting model with worker level revenue shocks. The extensions and testable predictions can be found in Section 2.2. and 2.3.

Workers

There are M mass of workers with identical productivity. The workers seek for the job with the highest expected utility. The outside option of workers ensures U_0 utility which can be conceived of as the utility value of the unemployment benefit or the value of leisure time. The workers are risk averse and maximize the expected utility of their income without caring about temporary revenue shocks. The utility of worker i employed by firm j over her income has mean variance form:

$$U(W_{ij}) = E(W_{ij}) - r * Var(W_{ij}) \quad (1)$$

[3]For case of simplicity I assume in Section 2.3. that revenue sharing has no incentive effect.

Firms

There is a unit mass of of firms and every firm is infinitesimally small compared to the labor market. Firms observe only the total revenue produced by the workers. The total revenue can be decomposed to into two parts:

$$\pi_{ij} = p + \varepsilon_{ij}$$

where p denotes the expected value of the revenue and ε_{ij} is a random revenue shocks. For analytical convenience, I assume that the ε_{ij} has normal distribution with zero mean and $Var(\varepsilon)$ variance.[4]. The shocks are independent across workers but they have the same variance within firms. $H(var(\varepsilon_j))$ stands for the distribution of the variance of revenue shocks across firms. The only cost of production is the wage paid to employees. As workers are identically risk averse, firms offer the same linear contract to every worker:

$$W_{ij} = w_j + b_j * \pi_{ij}$$

where $w_j \geqslant 0$ is the fixed wage and firms share $b_j \in [0, 1]$ part of the total revenue with the workers. $b_j * \pi_{ij}$ can be interpreted as the bonus part of worker compensation. $Var(\varepsilon_{ij})$ is common knowledge, so workers know the expected utility of wage offers before they accept or reject them. I follow Manning (2003) and I assume that the output of the firms is linear in the number of employees. Besides firms are risk-neutral and aim at maximizing expected profit:

$$\underset{w_j,b_j}{max} E((1 - b_j) * \pi_{ij} - w_j) * N_j(w_j, b_j) \tag{2}$$

where N_j is the number of workers at the firm. N_j depends on the wage, as firms engaging in oligopsonistic competition have more workers if they pay higher wages.

[4]The predictions of the results are robust against changing the distribution of shocks and the utility function of the workers as long as the workers are risk averse.

U_j is used to denote the expected utility of workers at firm j.

$$U_{ij} = w_j + b_j * E(\pi_{ij}) - r * b_j^2 var(\varepsilon_{ij}) \tag{3}$$

Substituting Equation 3 into 2 we get the following profit maximization problem:

$$\max_{U_j, b_j} E((\pi_{ij} - r * b_j^2 var(\varepsilon_j) - U_j) * N_j(U_j, b_j) \tag{4}$$

This form of the profit maximization problem is more convenient as I will show below that the size of the firm depends only on the utility offered by firm j.

Matching

Individuals receive a wage offer described by $\{w_j, b_j\}$ in every period with probability λ from a random firm[5] and workers lose their job and become unemployed with a probability of δ. The probability of getting an offer is independent from the labor market status of individuals and the separation rate is independent from the characteristics of firms. These assumption ensures that accepting a wage offer has no negative effects on the future income[6]. Individuals maximize only the certainty equivalent value of their income, so conditionally on U_j they do not care about the value of b_j and individuals accept every wage offer which provides a higher expected utility than their current utility. Subsequently this extended model inherits the equilibrium characteristics of the original Manning model as in equilibrium: (i) the expected size of the firms are constant over time, (ii) the distribution of firm sizes is a deterministic function of a non-degenerate wage offer distribution $F(U_j)$.

Lemma 1: The cumulative distribution function of U_j is strictly increasing and continuous

[5]Although the firms are infinitesimally small compared to the labor market, they have some monopsony power over workers as the probability of receiving a better wage offer than the current wage is less than 1.

[6]If a firm offers a lower expected utility to the individuals than her outside option, no worker would accept that offer. That is why any wage offer should provide at least U_0 utility to the worker and the unemployed always accept the wage offers.

between the minimum and the maximum of U_j.

Proof: Assume that the distribution of U_j is not strictly increasing, then there is a $(\underline{U}, \bar{U})$ interval without a corresponding wage offer. Firms initially offering $\bar{U}$ utility could raise profit by decreasing wages as the wage cut would raise the profit per worker without affecting firm size. Similarly, if the distribution of U_j is non-continuous, it means that a non-negligible share of firms would offer the same utility to their workers (U_j^*). However, in this case, it is profitable for any firm offering U_j^* utility to increase the offered utility with an infinitesimally small amount and attract some part of the employees from the firms that still offer U_j^* utility. That is why, in equilibrium, the wage offer distribution is dispersed even if $var(\varepsilon_j)$ is the same for every firm. It can be also showen that there is an equilibrium even if firms are heterogeneous with respect to productivity and the firms which have higher revenue per worker also offer higher wages.

Up until now I assumed that the workers dislike revenue sharing and it is not beneficial for the firms either. That is why in the following sections I made further assumptions. In Section 2.2 I assume that the revenue sharing can be an incentive for workers, while in Section 2.3 I assume that firms can lay off workers in case of negative revenue shocks. I also demonstrate how the revenue sharing parameter depends on the variance of the revenue of firms under these assumptions and derive empirically testable predictions.

1.2.2 Bonus payment as a tool of incentive contracts

In this section, I assume that workers can make either a high or either a low effort level. The effort of workers is denoted by e. Low effort level is normalized to 0 while high effort makes $\bar{e}$ profit to the firm and costs $c\bar{e}$ to the worker. Under these assumptions, the utility of the worker has the following form:

$$U(W(e_{ij}), e_{ij}) = E(W_{ij}) - r * var(W_{ij}) - ce_{ij} \quad (5)$$

and the revenue produced by worker i at firm j is:

$$\pi_{ij} = \begin{cases} p + \bar{e} + \varepsilon_{ij} & if\, the\, worker's\, effort\, is\, high \\ p + \varepsilon_{ij} & if\, the\, worker's\, effort\, is\, low \end{cases} \tag{6}$$

.

Similarly to the previous section, workers are identical so firms offer the same U_j and b_j to all of their employees and workers make the same effort within firm. In equilibrium, the wage offer distribution of firms has to meet the condition under Proposition 1 regardless of the distribution of U_j.

Proposition 1. *In equilibrium, there are two possible values of the profit sharing parameter* b_j.

$$b_j = \begin{cases} c & if\ \frac{\bar{e}*(1-c)}{c^2*r} \geq var(\varepsilon_j) \\ 0 & otherwise \end{cases} \tag{7}$$

Proof: see Appendix

According to Proposition 1, firms with low enough variance in their sales can make their workers to exert high effort. . However, if workers are more risk-averse (r is larger) or the cost of making higher effort (c) is larger, fewer firms will want to choose incentive contracts. The second implication of Proposition 1 is that firms that use incentive contracts share the same proportion of their gross profit with their workers independently from $var(\varepsilon_j)$. The lower bound of the profit sharing parameter is pinned down by the incentive compatibility constraint of workers. If b_j is too low, workers will shirk. As workers are risk averse firms want to use the lowest possible profit sharing which ensures high effort so b_j is the same at every revenue sharing firm. Therefore, in equilibrium, workers should be indifferent to shirking and making a high effort even if they are offered a positive b_j. By contrast, the firms which cannot observe the effort of workers precisely enough are better off by providing fixed wage contracts and allowing low effort. Since I interpret revenue sharing as bonus payment, Proposition 1 suggests that the volatility of sales revenue at bonus paying firms is lower than in the case of firms not paying bonuses.

Using the results of Proposition 1, the following notation can be applied:

$$P_j = \begin{cases} p + \bar{e} - c^2 * r * var(\varepsilon_j) & if \ \frac{\bar{e}*(1-c)}{c^2*r} \geq var(\varepsilon_j) \\ p & otherwise \end{cases} \tag{8}$$

P_j only depends on exogenously given parameters and it can be interpreted as a measure of productivity as this is the output per worker remaining after compensating workers for income uncertainty. Equation 8 suggests that firms characterized by a lower uncertainty in their output can achieve higher profit per worker. The strength of this approach is that the distribution of P_j is a deterministic function of $H(var(\varepsilon_j))$. Using P_j we can also write up the firms' problem only as the function of the utility provided and the distribution of utilities[7] offered by other firms (F). As mentioned before, in the equilibrium of the economy, the size of firms is constant. Using the notation P_j the profit maximization problem in Equation 4 can be rewritten in the following way:

$$\max_{U_j}(P_j - U_j) * N(U_j, F(U_j)) \tag{9}$$

Equation 9 suggests that the profit depends only on the exogenously given productivity measure and the utility provided by the firm. After this restructuring of the profit equation, the equilibrium properties of the model become identical with the original Manning (2003) model with heterogeneity in firms' productivity. Burdett and Mortensen (1998) also showed that there is no general formula for F but derived the sufficient conditions for equilibrium.

The empirically testable characteristics of the equilibrium in my extended model are as follows:

[7]Note: At firms offering fixed wage contracts $b_j = 0$ and $U_j = w_j$ while at firms offering incentive contracts $b_j = c$ and $U_j = w_j + c(p+e) - c * r * var(\varepsilon_j)$.

Proposition 2. *Firms using incentive contracts offer a higher utility to the workers and have larger size than firms offering fixed wage contracts.*

Proof: see Appendix

As Equation 6 illustrates, firms offering incentive contracts can achieve higher profit per worker even after compensating the workers for the uncertainty in their wage. In an oligopsonistic environment, more profitable firms offer higher wages to attract the workers of less productive firms. Although it is possible that these firms will have an even lower profit per worker, as they will have more workers, their total profit will be higher. As an another consequence of Proposition 2, if a worker having an incentive contract got a fixed wage offer she would not accept it as the fixed wage contract would provide her lower utility. On the contrary, workers who have a fixed wage contract always accept wage offers which come with an incentive contract.

1.2.3 Bonus payment as a tool of wage flexibility

In this section, I derive the optimal strategy for bonus payments if firms can fire workers in case of negative revenue shocks. As I want to separate the incentive contract and wage flexibility explanation of bonus payments, I assume that revenue sharing does not have incentive effects and the interest rate is 0. Now, suppose that worker-level revenue shocks have binary outcomes, and they take the value of $-\varepsilon_{ijt}$ or ε_{ijt} randomly with equal probability. This setup is equivalent with a simple Markov-chain process where there is a "recession" state and a "boom" state and the probability of regime change is 50 percent. I also assume that first firms observe the actual state of ε_{ijt} and they can decide whether they want to separate the workers before the payoffs are realized. So firms can separate workers if the expected value of the match turns negative:

$$P_j - U_j + (1-b_j)\varepsilon_{ijt} + \sum_{s=1}^{\infty}(\lambda(1-F(U_j)) + \delta_j)^s E(P_j - U_j + (1-b_j)\varepsilon_{ij,t+s}) < 0 \tag{10}$$

As the expected profit of firms is always positive, Equation 10 formalizes the intuition that firms want to separate workers only in a "recession" period when ε_{ijt} is negative. Separation is also more likely if the variance of revenue shocks is larger. On the contrary, firms can protect jobs and increase profit during recession by raising the revenue sharing parameter b_j. Since the expected value of revenue shocks in the next period is zero, the revenue sharing parameter decreases the chance of layoffs. On the other hand, larger revenue sharing decreases the utility of the worker who will therefore want to leave voluntarily with a higher probability. Similarly, firms will be more likely to fire workers if the exogenous separation rate is larger because in this case the discounted value of profit decreases. If the profitability measure P_j is larger than a more extreme negative shock is needed to change the sign of the present value of the job. At last, it is not obvious how the utility provided by the firm affects the likelihood of separations. On the one hand, it decreases the per period profit of the firm so even smaller negative shocks can turn the value of the match negative and induce layeoffs. On the other hand, a highherU_j also decreases the probability of voluntary exits.

Using Equation 10, Proposition 3 follows:

Proposition 3. *Firms with medium-size variance in their sales pay bonuses and never fire their workers. Firms with the lowest variance do not share their sales and do not fire workers either. If $var(\varepsilon_j)$ is above a certain threshold level, firms offer fixed-wage contracts and fire their workers in case of negative revenue shocks.*

Proof: see Appendix

The first-order conditions of Equation 4 show that total profit of the firm is deceasing in b_j. So firms smoothing employment choose the smallest b_j which ensures that the expected value of the match is not negative in recession. If $var(\varepsilon_j)$ is small enough, the expected value of the match is positive during recession even without any profit sharing, but if $var(\varepsilon_j)$ exceeds a certain threshold then firms need to share their sales with the worker to increase the expected value of the match during recession. Revenue sharing decreases the utility of workers and firms have to pay more to compensate workers for income uncertainty. That is why firms with larger $var(\varepsilon_j)$ have lower profit per worker. As in the original Manning (2003) model these firms will offer lower utility to the worker which implies smaller employment and larger turnover. Finally, if the variance of the sales revenue is very large, it is not profitable to share sales because the utility cost of uncertainty is too large. In this case, firms offer a fixed wage but fire workers if the match is hit by a negative revenue shock.

The testable implications of this extension to the model are as follows:

Proposition 4. *If profit sharing does not affect the effort of workers, firms without bonuses have (a) a larger variance in their sales revenue and a pro-cyclical separation rate or (b) lower variance in their sales revenue and an acyclical separation rate.*

Proposition 4 reveals that there are two types of firms that do not pay bonuses to their workers. Firms of the first type have so large variance in their sales that is is more costly to counterbalance the effects of negative shocks that they are better off by providing fixed wages. These firms fire their workers in the case of negative shocks. By contrast, firms with the lowest variance in their sales can smooth employment without profit sharing even in case of negative revenue shocks. As these firms do not need to compensate their workers for uncertainty, they can offer the highest utility and will be the largest as well. The net effect of these two channels can be estimated empirically. On the one hand, if there are firms which cannot smooth employment then the separation rate of firms without bonuses will have to be more negatively correlated with sales than the separation rate of firms paying bonuses.

On the other hand, if every firm can smooth employment, firms without bonuses will have the lowest variance in their sales revenue. These firms will offer the highest utility to their workers and will have the largest firm size.

Based on these results, we can compare the "wage flexibility" explanation and the "incentive contract" explanation for bonus payments. If firms pay bonuses mainly to enhance worker effort, we may expect that firms paying bonuses are larger, more productive and have lower variance in their sales revenue subject to their size of employment[8]. If the most important motivation for paying bonuses is to smooth revenue shocks then the largest firms do not pay bonuses. On the contrary, bonus paying firms have a larger variance in their sales revenue but they are smaller on the average and adjust their employment less due to sales revenue shocks. After introducing the data, I outline the empirical tests of these predictions.

1.3 Institutional background

Employment contracts in Hungary have to specify the amount of the monthly base wage which can be decreased only with the consent of workers. However, if worker compensation is based on piece rate or is paid on an hourly basis, the minimum amount of monthly payment has to exceed only half of the base wage [9]. According to the Wage Dynamics Network Survey, Hungarian firms adjust base wage every 13.8 months and 80 percent of firms adjust wages once a year. The frequency of wage changes is slightly lower in other European countries, for example, firms in the eurozone change wages every 15 month on average (Druant et al., 2012). Firms can modify other elements in the compensation package of workers without any legal constraints. Additional monetary elements over the base wage account for approximately 10 percent of total worker compensation. This share is close to the Western European average

[8]If sales revenue shocks are not perfectly correlated across workers, the relative volatility in sales revenue is decreasing with the size of employment. For this reason, I also control for the number of workers in the regressions.

[9]According to the Wage Survey, 15 percent of the workers are paid on an hourly basis or based on a piece rate.

(Kézdi and Kónya, 2011).

Employment protection institutions in general are more similar to the Anglo-Saxon regimes than to those found in Continental countries. It is relatively simple to dismiss workers (Riboud et al., 2002; Tonin, 2009) and collective wage bargaining is also based on the firm-level agreements of the unions (Rigó, 2012). The share of union members is approximately 20 percent, which is relatively low compared to other OECD countries (OECD, 2004). Apart from firm-level bargaining, industry-level agreements are rare and set only very week requirements (Neumann, 2006). The unions participate also in the country-level bargaining forum called National Interest Reconciliation Council. The Council is a tripartite forum of union federations, employer associations and the government, and it makes recommendations for wage increases and sets an obligatory minimum wage for the next year [10]. The recommendations for wage increases are not legally enforced and the share of firms using automatic wage indexation policies is also low (Druant et al., 2012).

The macroeconomic environment can be divided into two different periods. As Panel (a) of Figure 1 in the Appendix demonstrates, the inflation rate was relatively high before 2001 and moderately low afterwards. As inflation greatly affects wage adjustment, I repeat my estimations on these two subsamples separately. My results are robust to changes in inflation. Panel (b) shows real GDP growth and the unemployment rate. This figure reveals that the economy was relatively stable and there was no recession before 2008. While the unemployment rate was somewhat higher in the early transition years, settled down in the early 2000s and started to rise again at the crisis years

[10]While the government can set the minimum wage unilaterally, the parties managed to agree on the minimum wage in every year except for 2001Rigó (2012).

1.4 Data

I use the Hungarian linked employer-employee survey for estimation. The wage information comes from the Hungarian Structure of Earnings Survey. The survey is repeated every year and involves a quasi-random 6 percent sample of Hungarian employees and their income in May. The workers can be followed between years if they do not leave the firm. The appendix of Chapter 1 discusses the construction of panel on the worker level The database contains a wide range of personal information (age, gender, education, occupation). The database is unique as it contains information not only about total compensation but also about the different wage parts. In addition to the base wage, the Wage Survey records extra payments for overtime, night and weekend shifts, allowances for special working conditions, knowledge of foreign languages, premia as well as regular and irregular bonuses[11]. Moreover, wage information is reported by the firms and not by the individuals, so measurement error is less of an issue. I define workers as receiving bonus if they got at least one type of extra payment in addition to their base wage in any year during the periods observed Lemieux et al. (2009).

Firm-level data come from the corporate income tax returns collected by the National Tax and Customs Administration. The database contains the balance sheet and income statement of every double entry book-keeping firm. The firms also have a unique identifier so they can be followed over time and firm-level revenue shocks can also be measured.

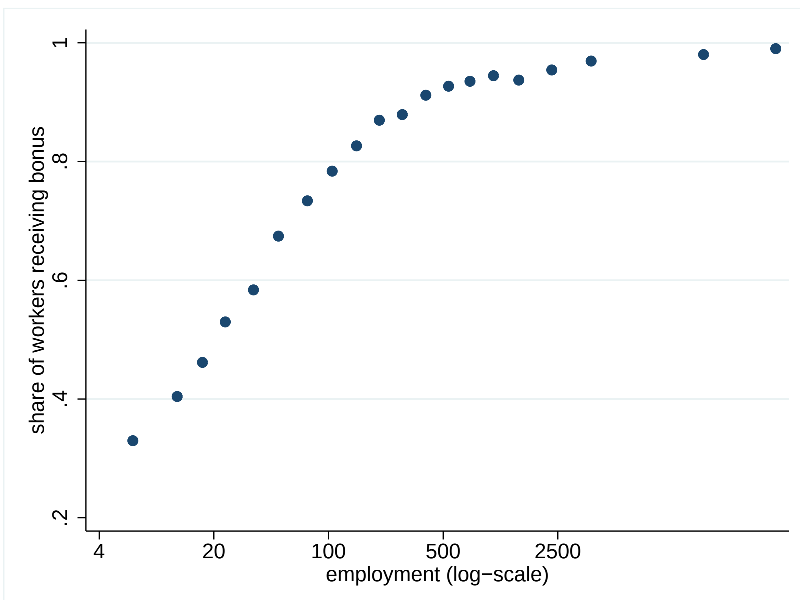
1.4.1 Descriptive statistics

Figure 1.1 outlines the relationship between the size of the firm and bonus payments. I grouped the worker-year observations into 20 bins by firm size and plotted the average share of workers receiving a bonus in every bin. This non-parametric estimate shows that the larger the firms are the more likely it is that their workers receive a bonus. This result is in line with

[11] The sum of the base wage and other wage parts do not need to be equal to the total compensation in the database. Such difference is defined by paid and unpaid leaves.

the wage flexibility explanation for bonus payments. To ensure common support for workers receiving a bonus, I confine my attention to firms having less than 2500 workers. For the purpose of robustness checks, I repeat every estimation also on the sub-sample of firms with less than 500 employees. I also drop observations where the firm has less than 20 workers so it cannot be followed automatically over time. The vertical lines show sample restrictions. Due to data availability issues, I use the waves of wage surveys conducted between 1995 and 2010 for the present analysis. The analysis is restricted to private sector firms since the wage and employment decisions of public sector firms are substantially affected by politics in Hungary (Telegdy 2013a, 2013b).

Figure 1.1: The share of workers receiving a bonus by the size of the firm



Note: In this figure, worker-year observations are grouped into 20 equally-sized categories by the size of the firm. The figure plots the share of workers receiving a bonus in every bin.

Table 1.1 summarizes the descriptive statistics of the different wage elements. The first column shows that approximately 78 percent of workers receive at least one type of additional wage element and workers earn usually more than one type of additional wage elements. The most widespread type of additional elements are occasional bonuses while monthly bonuses have the largest share in the compensation package of workers, provided that they receive such a wage element

Table 1.1: The share of different wage components in total worker compensation

	prob. of receiving	share of wage parts conditional on receiving			
	the wage element	mean	sd	p25	p75
overtime payments	0.202	0.105	0.081	0.047	0.141
monthly bonuses and premia	0.210	0.216	0.189	0.078	0.300
occasional bonuses	0.440	0.085	0.078	0.033	0.112
allowances for special work conditions	0.387	0.124	0.094	0.054	0.175
reimbursements	0.368	0.054	0.075	0.020	0.061
total	0.778	0.221	0.182	0.082	0.312

Note: This table shows the probability of receiving additional wage elements over the base wage and the share of these in total worker compensation.

.

Table 1.2 shows the means and standard deviations for the final sample. As the change of wages can be computed only for workers remaining at the same firm over the years, I show the means for this group as well. The summary statistics are also in line with the incentive contract explanation for bonus payments. Bonus-receiving workers have a higher wage and work at larger, more productive and more profitable firms. Workers receiving bonuses work at firms where the share of new entrants is lower. This is not surprising as in equilibrium firm size is constant so the separation rate and the share of new entrants are equal in every firm. As firms offering fixed wage contracts are less attractive to workers of bonus paying firms, the separation rate for bonus paying firms will be lower. We cannot see considerable differences in the case of other characteristics. Workers receiving a bonus have a similar age, years of education and there is no great difference in the sex ratio either. The main conclusion to be drawn from the right panel is that workers remaining at the firm are similar to the total sample. The only difference is that workers in this sub-sample work at slightly larger firms.

Table 1.2: Descriptive statistics: comparing the main characteristics of workers receiving and not receiving a bonus

	Total sample				Conditional on remaining at the firm until next May			
	no bonus	bonus	diff	t-stat	no bonus	bonus	diff	t-stat
Average wage (log)	11.25	11.64	0.4	39.22	11.21	11.64	0.4	35.30
	(0.0)	(0.00)			(0.0)	(0.00)		
Share of males	0.61	0.60	-0.01	-1.27	0.63	0.61	-0.02	-1.54
	(0.00)	(0.00)			(0.00)	(0.00)		
Years of education	10.8	10.8	-0.02	-1.04	10.8	10.8	0.03	0.98
	(0.02)	(0.01)			(0.03)	(0.01)		
Average age	38.77	39.83	1.054	9.06	39.86	40.47	0.609	3.79
	(0.10)	(0.08)			(0.15)	(0.07)		
Number of employees	216.8	550.6	333.8	17.76	198.8	562.9	364.13	15.40
	(12.7)	(17.8)			(15.83)	(19.91)		
Value added per worker (log)	7.494	7.870	0.38	15.49	7.309	7.786	0.48	15.34
	(0.022)	(0.019)			(0.027)	(0.021)		
Earnings Before Interest & Tax (Million HUF)	22511	67741	4523	4.41	12574	63638	5106	5.20
	(6851)	(1011)			(3976)	(1063)		
Share of exporting firms	0.371	0.528	0.16	15.94	0.374	0.573	0.20	14.32
	(0.007)	(0.008)			(0.011)	(0.010)		
Proportion of new entrants last year	0.194	0.124	-0.07	-24.59	0.148	0.097	-0.05	-13.75
	(0.002)	(0.001)			(0.003)	(0.001)		
Age of firms	10.11	11.17	1.05	3.92	10.33	10.97	0.64	2.18
	(0.18)	(0.25)			(0.22)	(0.25)		
Number of observations	221,881	903,411			49,528	393,957		

Note: This table shows the weighted means and standard deviations for the worker-level data in the Wage Survey. Firm-level variables show the characteristics of the employing firms.

Using the individual-level panel, I construct the distribution of wage changes for workers with and without a bonus. These distributions are able to reflect the downward nominal rigidity of the different wage elements. If wages are downward rigid, firms can only decrease average labor compensation by firing their workers and hiring new ones for a lower wage. If replacing workers is costly, wage rigidity results in upward pressure on wages and positive excess mass or "bunching" may be expected at small increases and a spike at 0 in the distribution of wage changes. By contrast, if wages are flexible, it is expected that the distribution

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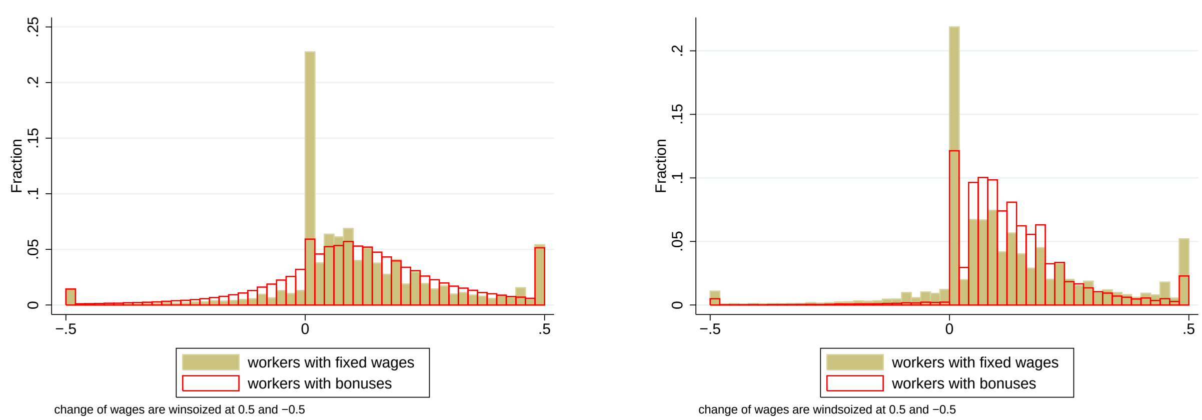
of wage changes is continuous around 0. This means that the probability of an infinitesimally small wage decrease should be roughly the same as the probability of an infinitesimally small wage increase. Figure 1.2 presents the log-changes of wages. The distributions are winsorized at a 50 percent change. The filled bars show the changes of wages for employees who do not get a bonus while the empty bars indicate the distribution for workers receiving a bonus. Panel A shows that the nominal wage of workers without a bonus is completely rigid downward while the wage of workers receiving a bonus is flexible. Panel B shows that the base wage is downward rigid for workers with and without a bonus alike. Consequently, we can conclude that bonus payments are the reason for wage flexibility.

Figure 1.2: The distribution of changes in worker compensation

(a) Total worker compensation

(b) Base wage



Note: Panel (a) shows the distribution of wage changes for workers who do and do not receive bonuses. Panel (b) shows the distribution of changes in base wage for both types of workers. The figures show that workers with a fixed wage (filled bars) only occasionally experience a nominal wage decline. Moreover, the large spike at zero suggests that many firms prefer to keep wages intact to decreasing them. In contrast to this, workers with bonuses (empty bars) often experience a negative decline in their wages.

Inflation can ease the effects of wage rigidity as firms can decrease real wages without cutting the nominal value of the compensation of workers if the inflation rate is higher.

Therefore, I compare the wage change distribution of workers in a low and high-inflation environment. As inflation was much higher in Hungary before 2001, Panel (a) and (b) of Figure A.3 in the Appendix plots the distribution of wage changes by decade. Panel (a) shows the distribution of wage changes for workers without a bonus. In the high-inflation period before 2001, the median of the wage changes was larger and the spike at 0 was smaller than in the low-inflation period. In addition, nominal wage drops were scarce irrespective of the inflation rate. We can conclude that higher inflation eases but does not eliminate downward nominal wage rigidity in the case of workers without a bonus. On the other hand, Panel (b) shows that the wages of bonus receiving workers are flexible regardless of the inflation rate. If the inflation rate is higher, average wage growth is also higher and nominal wage drops are less frequent. At the same time, there is no large spike at 0 and the probability of small wage decreases is approximately the same as the probability of small wage increases. Last but not least, Panel (c) of Figure A.3 in the Appendix shows the distribution of real wage changes for workers with and without a bonus. It is clearly observable that wage change distribution is continuous around 0, and we cannot find either a spike or bunching around 0. This figure suggests that wages in Hungary are only nominally rigid but not in real terms[12]. The employment and wage response of firms

1.5 Employment and wage reaction of the firms

1.5.1 Estimation strategy

To determine the effect of bonus payments on separation rates and wage adjustment I estimate the following equation:

[12] This result is in line with the estimates of Kátay (2011) who also found a low downward real wage rigidity in Hungary.

$$\Delta log(wage_{jit}) = \alpha_1 \Delta log(sales_{j(it)}) + \alpha_2 bonus_{ji} + \alpha_3 bonus_{ji} * \Delta log(sales_{j(it)}) + \alpha X_{jit-1} + \mu_t + \varepsilon_{it} \tag{11}$$

where the dependent variable is the change in the wage of worker i at firm j between year $t-1$ and t. $\Delta log(sales_{j(i,t)})$ stands for the change of the nominal sales revenue of firm j between year $t-1$ and t. This variable is the same for every worker of the firm. $Bonus_{ij}$ indicates whether worker i at firm j received extra compensation elements in addition to the base wage at least once during the observed periods. denotes the control variables while μ_t stand for year dummies to get rid of the effect of inflation. The main variable of interest is the interaction between bonuses and changes in sales revenue. If α_3 is positive, firms can adjust the wages of incumbents more by paying bonuses.

To compute the employment response of firms with and without bonuses, I estimate Equation 12 with a dummy variable on the left hand side denoting whether the worker of the firm is separated between year $t-1$ and t.

$$I(sep_{jit} = 1) = \beta_1 \Delta log(sales_{j(it)}) + \beta_2 bonus_{ji} + \beta_3 bonus_{ji} * \Delta log(sales_{j(it)}) + \beta X_{jit-1} + \mu_t + \varepsilon_{it} \tag{12}$$

If firms pay bonuses to decrease wage rigidity then we expect that the probability of separations at the firm co-moves with sales revenue more tightly in the case of workers without bonuses. This implies thatβ_1 is negative while β_3 is positive. In contrast, the incentive contract explanation for bonus payments suggests that the probability of separation is independent from firm-level revenue shocks which implies that β_1 and β_3 are both zero in this case. Finally, the sign of β_2 can be used to distinguish between the two explanations of bonus payments. The incentive contract explanation for bonus payments suggests that the expected utility of workers with bonuses is higher, so they are less likely to leave the firm, which implies that β_2 is negative. By contrast, the wage flexibility explanation suggests that bonus receiving workers have lower utility than workers with fixed wages which implies that β_2 is positive.

Individual-level estimations have two important weaknesses. First, they implicitly assume that workers are independent within firms in the sense that the wage rigidity of one worker does not affect the separation rate of other workers. In addition, firms may be able to decrease average wages without adjusting the number of employees if they fire workers and hire new ones at lower wages. This mechanism provides wage flexibility at firm-level even if individual wages are downward rigid and the separation rate is independent from sales revenue shocks[13]. To control for this mechanism, I aggregate Equations 11 and 12 at firm level and estimate the following equations:

$$\Delta log(wage_{jt}) = \gamma_1 \Delta log(sales_{jt}) + \gamma_2 bonus_{jt-1} + \gamma_3 bonus_{jt-1} * \Delta log(sales_{jt}) + \gamma X_{jt-1} + \mu_t + \varepsilon_{it} \tag{13}$$

$$\Delta log(emp_{jt}) = \delta_1 \Delta log(sales_{jt}) + \delta_2 bonus_{jt-1} + \delta_3 bonus_{jt-1} * \Delta log(sales_{jt}) + \delta X_{jt-1} + \mu_t + \varepsilon_{it} \tag{14}$$

where the dependent variable is either the change of average wages or the change of employment at firm j between year $t-1$ and t. $\Delta log(sales_{jt})$ denotes the change of sales revenue between years $t-1$ and t while $bonus_{jt-1}$ denotes the share of workers receiving a bonus at year $t-1$. If bonus payments provide the firms additional flexibility then we expect that γ_3 is positive in the wage equation. In the employment equation, we expect that β_1 is positive due to reverse causality. If the number of workers changes due to exogenous reasons, the output of the firms will change as well because workers are one of the production factors of firms. Still, if firms pay bonuses to smooth employment, we expect that δ_3 is negative,

[13] A large body of literature shows that the wages of newly hired workers are more pro-cyclical than the wages of incumbents (Pissarides, 2009; Carneiro et al., 2012; Haefke et al., 2013; Kudlyak, 2014).

but if firms pay bonuses to incentivize workers, we expect that δ_3 is not negative[14].

1.5.2 Results

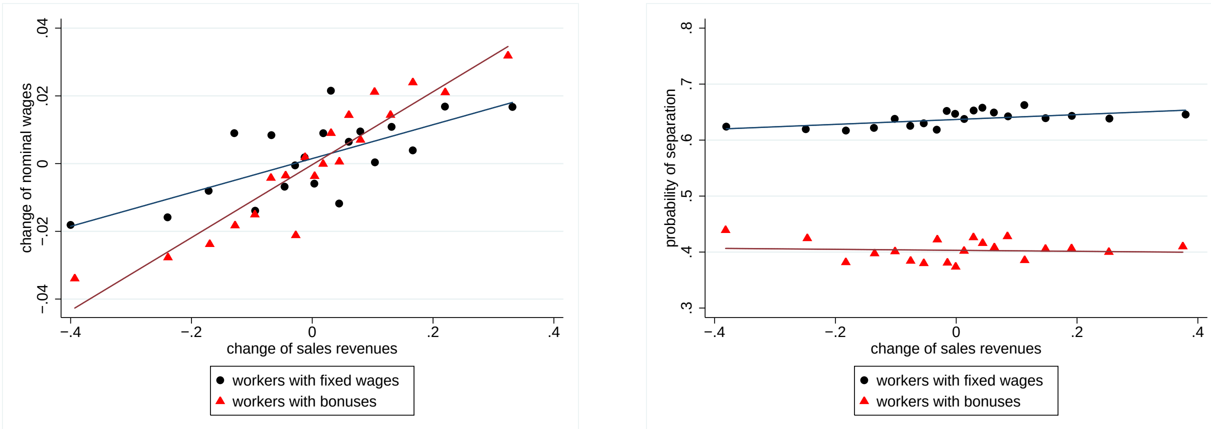
Panel A in Figure 1.3 shows a non-parametric estimate for Equation 11 . I grouped worker-year observations in twenty equally sized bins by the change of the sales revenue of the employers and plotted the average change of wages for workers with and without a bonus. It is clear that the wages of workers receiving a bonus change more due to revenue shocks than the wages of workers without a bonus. The only difference between the theoretical and empirical investigation is that the wages of workers without a bonus also co-moves with the revenue of the firms to some extent. Contrary to the model, the sales of firms are not stationary over time. If the productivity of firms shows a positive trend, their sales revenue and wages increase over time as well. If there are differences in firm-level growth rates, the time dummies cannot control for the positive correlation between the growth rate of sales revenue and wages. This phenomenon is true independent from the structure of wages[15].

[14]Note: Firm-level estimations are not sufficient either as a tool to compare the different explanations for bonus payments as only individual level regressions can show the wage adjustment of incumbents and the lower separation rate of bonus receiving workers.

[15]Note: I also estimate equations 11 and 12 with firm fixed effects to control for differences in the growth rates of the firms. The results are virtually the same. Besides Section 6.1 directly adresses this issue.

Figure 1.3: The effect of a change in sales revenue on wage and employment

(a) Wage change (b) Probability of job separation



Note: In these figures, workers are grouped into equally-sized bins based on the change in the sales revenue of their firms. Panel (a) shows the average change of wages for workers with and without bonuses. Panel B shows the conditional probability of remaining at the firm if the firm remained in the sample the next year. Both panels control for sex, experience, square of experience, years of education, capital and sales revenue per worker in the base year, 2-digit occupation codes (ISCO 88), 2-digit industry codes (NACE) and year dummies. The wage of workers receiving a bonus co-moves with the sales revenue of firms more tightly than the wage of workers without a bonus, but there is no such difference in the probability of separations.

In contrast to wages, the probability of separation does not co-move with the change of the sales revenue of the firm if the size of the shock is not very large. As panel B in Figure 1.3 illustrates, the probability of remaining at the firm is approximately constant for workers receiving and not receiving a bonus alike. Moreover, the probability of separations is lower if the worker receives a bonus in a given year. This contradicts the wage flexibility explanation for bonus payments but is in line with the incentive contract explanation as the latter model suggests that bonus paying firms offer a higher utility to their workers so they can attract the workers of firms not paying bonuses.

Panel B in Figure 1.3 shows the survival rate of jobs, which is conditional on the employing firm remaining in the Wage Survey the next year. As a firm can only participate in the Wage Survey if it had not gone bankrupt earlier, estimates for job survival rates are biased if the probability of bankruptcy is correlated with the decision to pay bonuses. To control for this possibility, Figure A.4 shows the survival rates of jobs regardless of the participation of the firms in the Wage Survey. In this figure, I consider a job as separated if the firm is not observed in the Wage Survey the next year. As firms do not necessary go bankrupt if they do not participate in the Wage Survey, this method underestimates the survival rate of jobs. In line with the expectations, the estimated probability of job survival dropped but the results are qualitatively similar. Survival rates are almost uncorrelated with the changes in revenue and workers without bonuses are more likely to be separated.

Table 1.3: Main results

VARIABLES	(1)	(2)	(3)	(4)
Panel A: change in wages				
worker got bonus	0.000456	-0.000575	0.00222	0.000499
	(0.00204)	(0.00210)	(0.00213)	(0.00224)
change in sales revenue	0.0393***	0.0365***	0.0315***	0.0310***
	(0.0106)	(0.0104)	(0.0106)	(0.0111)
interaction	0.0766***	0.0752***	0.0763***	0.0796***
	(0.0115)	(0.0115)	(0.0116)	(0.0120)
Observations	379,998	379,998	374,488	254,680
R-squared	0.049	0.051	0.057	0.049
Panel B: probability of job separation				
worker got bonus	-0.244***	-0.247***	-0.255***	-0.240***
	(0.00507)	(0.00484)	(0.00461)	(0.00472)
change in sales revenue	0.0478***	0.0365**	0.0146	0.00551
	(0.0157)	(0.0152)	(0.0148)	(0.0146)
interaction	-0.0714***	-0.0638***	-0.0693***	-0.0501***
	(0.0187)	(0.0180)	(0.0173)	(0.0167)
year fe.	x	x	x	x
firm-level controls		x	x	x
individual-level controls			x	x
without large firms*				x
Observations	711,945	711,945	697,676	480,763
R-squared	0.033	0.043	0.062	0.066

Note: The table shows the effect of bonus payment and sales revenue changes on different outcomes. Panel A shows the effect of bonus payment and sales revenue changes on the wages of workers. Panel B shows the effect of these variables on the probability of job separation. Columns (1) to (3) differ in the control variables. Every column includes year dummies to get rid of the effect of inflation. Column (2) controls for log-capital per worker and log-sales per worker, the age of the firm and 2-digit industry codes (NACE) while Column (3) also controls for sex, years of education, experience, square of experience, a dummy indicator for being a new entrant and 2-digit occupation codes (ISCO 88). In Column (4), I restrict the sample to the firms having less than 500 employees. Standard errors are clustered at firm level.

The point estimates for Equation 11 are shown in Panel (a) of Table 1.3 and the first column corresponds to Figure 1.3(a). The sales revenue of the firm increases by 10 percent while the wages of workers without a bonus increase by approximately 0.3-0.4 percent. Conditional and unconditional wage adjustment are approximately the same but wage adjustment is slightly lower depending on the observables. More importantly, wage adjustment in the case of workers receiving a bonus is almost three times as large as wage adjustment in the case of workers without a bonus. If the sales revenue of firms changes by 10 percent, the wages of workers receiving a bonus changes by 0.7-0.8 percent more than the wages of workers without bonuses.[16] In addition, this result is highly significant and robust to the inclusion of control variables and sample restrictions.

Panel B in Table 1.3 summarizes the point estimates for the employment equation. Similarly, the first column shows the slope parameters of the lines in Figure 1.3(b). It is observable that the probability of separation is approximately 25 percent lower if the worker received a bonus in a given year. This difference is robust to including control variables and to omitting firms with more than 500 employees. These point estimates are in line with the predictions of the incentive contract explanation for bonus payments, as bonus payments are connected with a higher utility and lower separation rate of workers. By contrast, the connection between the separation rate and changes in sales revenue is very weak in the case of moderate revenue shocks. Furthermore, the separation rate of workers receiving a bonus is negatively correlated with the revenue shocks hitting firms. The estimated coefficient for the interaction term suggests that if the revenue of firms increases by 10 percent, the separation rate of workers receiving a bonus decreases by 0.6 percent more than the separation rate of workers without a bonus. Thus, the empirical findings definitely contradict the wage flexibility explanation for bonus payments as bonus payments do not help firms to smooth employment[17].

[16]These results are similar to the estimates of Kátay (2008). He found that wage elasticity to productivity shocks is between 0.05 and 0.1.

[17]Theoretically, it is possible that one type of the firms can smooth employment without smoothing wages while another type of the firms cannot smooth employment even by paying bonuses and having downward flexible wages. However, in this case, we would expect that bonus paying firms have a larger separation rate as well.

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It may be possible that workers with different characteristics cannot be incentivized with the same wage structure. Therefore, I re-estimate Equation 11 by different worker groups separately. The result are shown in Table A.1. First, I do not find any difference in the effect of bonuses in the case of males and females. Second, I estimate the parameters of interest differently for blue and white collar workers because the effort of blue collar workers may be observed more easily and their employment dropped more during the Great Recession (Köllő, 2011). Finally, I estimate the model separately for tradeable and non-tradeable sectors. As Hungary is a small open economy this separation is motivated by the assumption that the firms in tradeable sectors face more fierce competition which may affect the wage and employment adjustment of firms[18]. The point estimates are qualitatively the same in all of the subgroups.

How do macroeconomic variables confound to wage and employment adjustments? Firms can decrease real wages when inflation is high so nominal wage rigidity is an important issue only if the inflation rate is low. Therefore, Table A.4 analyze the effect of inflation. Fist, I divide the sample into a time period before and after 2001. With an average rate of 13.9 percent, inflation before 2001 was high in Hungary , followed by a moderately low 4.8 percent afterwards. The results are shown in Columns (1) and (2) and are very similar in both cases. The only difference between the two subsamples is that the wages of workers without bonuses co-move with sales revenue in the high-inflation sample only. This result is in line with Elsby (2009) as in a high-inflation environment downward nominal wage rigidity is less binding so firms are more willing to raise wages even for workers with rigid wages. Column (3) shows the effect of real sales changes on wages and separations. In this column the nominal sales changes are deflated with two-digit industry level deflators. Here again, the wages of workers with bonuses are adjusted 2.6 percentage more to real sales changes than the wages of workers without bonuses. Still, the workers with rigid wages cannot more likely retain their jobs if the sales of the firm decreases.

[18] I estimated the model separately for exporters and non-exporters but the results were similar, so I do not present them.

Columns (4) and (5) consider the effect of local unemployment rate. If the local unemployment level is higher than firm have larger bargaining power against workers so they may cut the wages of workers easier. Therefore the wage rigidity may be a less important issue if the local unemployment rate is higher. Similarly firms may fire workers in case of negative revenue changes if the unemployment is higher because they can find new workers easier. To test this hypothesis, I divided the sample to a below and to a above median unemployment rate samples based on the yearly average unemployment rate at the location of the firm[19]. According to the results the wage adjustment of workers without bonus are somewhat larger in the high unemployment sample (0.052) than in the low unemployment sample (0.041) but the difference is not statistically significant. The wages of workers with bonuses are adjusted with 4.1 percentage point more than the wages of if the unemployment rate is low. This difference is only 2.9 percentage point in the large unemployment sample but we cannot reject the hypothesis that the two point estimates are the same at any conventional significance level. Panel (b) of Table A.4 reveals that the separation rates of workers without bonuses are uncorrelated with the revenue changes both in the low- and in the high-unemployment samples. These suggest that the firms do not more likely fire workers if the unemployment rate is higher.

Robustness The bonus definition I use in the main analysis is arbitrary, so Table A.2 shows the robustness of my results to different bonus definitions. In Column (1), a worker is defined as receiving a bonus if she got a bonus in the previous year. Although the point estimates changed, the results qualitatively remained the same since the wage response of workers receiving a bonus is larger if the revenue of the firm changes. In comparison, the average wage growth of workers without a bonus is 5 percent lower than the wages of workers receiving a bonus. The reason for this is that although some workers do not receive a bonus because of temporary weak performance they expect to get a bonus in the next year. This effect increases the average wage growth of workers who are categorized in this specification

[19]The average unemployment rate in below the median was 3.6 percent and 11.1 percent in above the median.

as not receiving a bonus. Similarly, the conditional separation rate of workers with a bonus increased compared to workers without a bonus. The result suggests that this definition of bonus payment mistakenly categorizes some workers as not receiving a bonus. Still, in the case of this definition, the partial effect of sales revenue changes on the probability of the separation of workers receiving a bonus is not lower either. The results are qualitatively the same if I define workers as receiving a bonus if the additional compensation elements over their base wage comprised at least 10 percent of their total wages (Column 2) or if their base wage is lower than their total compensation even if they did not receive any additional elements over the base wage (Column 3)[20].

Column (4) of Table A.2 regards workers as receiving extra elements over their base wage if they got monthly or occasional bonuses or premia. Under this specification, I do not consider overtime payment, reimbursements and allowances for special working conditions as extra elements over the base wage. One could argue that overtime can be directly controlled by the firms and firms only pay them because of legal obligations. The requirements for allowances and reimbursements can also be independent of the unobserved effort of individuals. Accordingly, these wage elements may similarly have only weak incentive effects. The point estimates are very close to the main results and they are in line with the incentive contract explanation for bonus payments.

Finally, Column 5 shows that non-financial remuneration does not co-move with sales revenue so firms without bonuses do not smooth employment costs by adjusting non-financial remuneration.

Table A.3 concerns robustness to changing the estimation sample. In the first column, I include firms with less than 20 or more than 2500 workers in the sample and in Column

[20] If the worker is partly or completely paid on an hourly basis or based on a piece rate, the Wage Survey reports a base wage lower than the total compensation, even without any additional elements over the base wage indicated.

(2) I re-estimate the model without weighting. The point estimates are basically unchanged. Another concern about the results may be that I arbitrarily trimmed the distribution of sales revenue shocks at 50 percent. For this reason, Column (3) and Column (4) take into account revenue changes which are lower than 30 and 20 percent, respectively, while Column (5) winsorizes the wage distribution instead of trimming. The results remained the same.

In the last three columns of Table A.2, I deal with the issue of wage under-reporting in Hungary. Previous research in Hungary highlighted that some employers under-report wages to decrease tax liability. In Column (6), I re-estimate Equation 11, using firm-fixed effects. The implicit assumption here is that there is no heterogeneity in wage under-reporting within firms. In Column (7), I omit workers receiving a minimum wage. The assumption here is that if the wage of a worker is under-reported, the reported wage is the lowest possible, i.e. the minimum wage. These specifications are in line with the previous results. The wages of workers receiving a bonus co-move more tightly with the sales revenue of firms and the flexibility of wages does not help firms in smoothing employment. Interestingly, under this specification, the wages of workers without a bonus are conditionally uncorrelated with the sales revenue of the firm. I re-estimated the model also by omitting firms with less than 100 employees because it is more like that smaller firms try to evade taxes (Kleven et al., 2011). As each of these specifications produce results similar to the main specifications, I conclude that my results are not driven by wage under-reporting.

Firm-level evidence Table 1.4 shows firm-level estimations. Similarly to the individual-level analysis, the average wages received at firms not paying a bonus increase by 0.3 percent in the aftermath of a 10 percent revenue shock and wages at bonus paying firms are adjusted by 0.3-0.7 percent more. This results is robust to introducing control variables (Columns (3) and (4)) and to weighting with employment. On the other hand, average nominal wage growth is sightly lower at bonus paying firms. To sum up, we can reject the hypothesis that firms not paying bonuses adjust wages as much as bonus paying firms by firing workers and hiring new ones for a lower wage. The most important difference between the firm-level and the individual-level analysis is in the employment equation. I find that a one percent change in sales revenue corresponds to a 0.3 percent change in employment level although the separation rate is nearly uncorrelated with sales revenue shocks. The difference between the two results is caused by reserve causality. For example, if the employment level changes accidentally for an exogenous reason, firm output will also change as labor is one of the inputs of production[21]. On the other hand, the interaction between bonus payments and sales revenue is very close to zero and has small standard error, indicating that firms paying a bonus do not smooth employment more[22]. In Columns (5) and (6), I omit firms with more than 500 workers and in the last two columns of Table 1.4 I define a worker as receiving a bonus if she got additional elements besides the wage base in the previous year. The results remained the same. Therefore, we can conclude that the firm-level analysis is in line with individual-level results and supports the incentive contract explanation for bonuses.

[21] If we assume that the production function of the firms is Cobb-Douglas then these estimates are consistent with a labor share of 1/3.

[22] Note: It may be possible that the labor share is larger in the production function of bonus paying firms. That is why the interaction term may be upward biased. To rule out this possibility, I control for the share of labor with the ratio of the total wage bill and the sales revenue of the firm and interact it with changes in sales revenue. The results remained the same.

Table 1.4: main results - firm-level evidence

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	raw effects		conditional effects		less than 500 workers		received a bonus last year	
Panel A: percentage change in wages								
Share of workers with bonus	-0.0303***	-0.0308***	-0.0386***	-0.0350***	-0.0366***	-0.0347***	-0.0464***	-0.0466***
	(0.00345)	(0.00279)	(0.00388)	(0.00305)	(0.00352)	(0.00309)	(0.00421)	(0.00278)
change in sales revenue	-0.000436	0.0260*	-0.00262	0.0256*	0.0112	0.0280**	0.0111	0.0361***
	(0.0168)	(0.0136)	(0.0169)	(0.0137)	(0.0139)	(0.0138)	(0.0187)	(0.0114)
interaction	0.0721***	0.0437***	0.0674***	0.0394**	0.0609***	0.0377**	0.0553***	0.0272*
	(0.0177)	(0.0159)	(0.0178)	(0.0160)	(0.0162)	(0.0163)	(0.0207)	(0.0143)
Observations	59,872	59,872	58,809	58,809	54,711	54,711	55,616	55,616
R-squared	0.076	0.046	0.080	0.052	0.065	0.050	0.080	0.052
Panel B: percentage change in employment								
Share of workers with bonus	0.00320	0.00599**	0.00788*	0.00431	0.00422	0.00357	0.00333	-0.000277
	(0.00395)	(0.00274)	(0.00407)	(0.00282)	(0.00368)	(0.00284)	(0.00349)	(0.00243)
change in sales revenue	0.372***	0.350***	0.360***	0.342***	0.356***	0.340***	0.345***	0.323***
	(0.0177)	(0.0123)	(0.0167)	(0.0121)	(0.0158)	(0.0122)	(0.0140)	(0.00977)
interaction	0.00649	-0.00403	0.0112	-0.00579	0.00276	-0.00883	0.0364**	0.0255**
	(0.0208)	(0.0146)	(0.0197)	(0.0144)	(0.0184)	(0.0146)	(0.0178)	(0.0126)
Controls	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Weights	Yes	No	Yes	No	Yes	No	Yes	No
Observations	59,826	59,826	58,764	58,764	54,668	54,668	55,580	55,580
R-squared	0.146	0.122	0.176	0.149	0.163	0.148	0.180	0.152

Note: The table shows the firm level estimates for Equation 11. Panel A shows the effect of bonus payment and sales revenue changes on the average wages of workers. Panel B shows the effect of these variables on the average change in employment. In Columns (1) to (6), I define a worker as receiving a bonus if she received a bonus at least once during the observed periods. In Columns (5) and (6), I omit firms with less then 500 workers. In Columns (7) and (8), I define a worker receiving bonus if she received a bonus last year. The first two columns include no controls. In Columns (3) to (8), I control for log-capital per worker and log-sales per worker, the age of the firm, 2-digit industry categories, the share of females and new entrants, average years of education, experience and year dummies to get rid of the effect of inflation. Columns (2), (4), (6) and (8) are weighted with the number of workers. Standard errors are clustered at firm level.

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1.6 The expected value and volatility of growth rates

1.6.1 Estimation strategy

One possible threat of my estimation strategy is that the growth rate of firms and bonus payment strategy are correlated. For example firm with rigid wages may not fire workers even in case of negative revenue shocks because they have larger and less volatile growth rates. In this case, firms not paying bonuses smooth employment because their prospects are better than those of firms not paying any bonus. To test this hypothesis, I run the following regressions:

$$\Delta log(sales_{j(it)}) = \lambda_0 + \lambda_1 bonus_{ji} + \lambda X_{jit} + \varepsilon_{it} \tag{15}$$

where the dependent variable is the growth rate of sales revenue and $bonus_{ij}$ indicates whether the worker received a bonus. X_{it} refers to the control variables, including year dummies . For a better understanding, I demean the control variables so λ_0 shows the conditional growth rate of firms employing workers without paying a bonus[23]. The main coefficient of interest is λ_1, showing whether workers receiving a bonus work at firms with a lower growth rate.

I also estimate the conditional variance of growth rates using a method similar to White (1980). First, I predict the residuals $\hat{\varepsilon^2_{it}}$ from Equation 15 and estimate the following equation:

$$\hat{\varepsilon^2_{it}} = \kappa_0 + \kappa_1 bonus_{it} + \lambda X_{it} + \nu_{it} \tag{16}$$

where the control variables are exactly the same as in Equation 15. κ_0 shows the conditional variance of the growth rate of firms employing workers without bonus payment. The most important parameter is again the coefficient of the bonus indicator. If firms pay a bonus to motivate high effort with profit sharing, we may expect that workers receiving a bonus

[23]Note: I demean the control variables in Equations 15 and 16.

work at firms where the conditional volatility of the growth rate is lower. As opposed to this, if firms pay a bonus to smooth their profit, it is expected that bonus receiving employees work at firms with a more volatile growth rate.

Table 1.5: Growth rate of firms

VARIABLES	(1)	(2)	(3)	(4)
Panel A: change in sales revenue				
constant	0.0454***	0.0564***	0.0556***	0.0474***
	(0.00199	(0.00222)	(0.0022)	(0.00179)
worker got bonus	0.0124***	-0.00138	-0.000363	-0.00159
	(0.00214	(0.00204)	(0.00202)	(0.00185)
Observations	1,075,581	1,049,736	1,049,586	774,539
R-squared	0.072	0.094	0.095	0.072
Panel B: conditional variance of sales revenue				
constant	0.0394***	0.0331***	0.0330***	0.0363***
	(0.000565)	(0.000564)	(0.000558)	(0.000489)
worker got bonus	-0.0101***	-0.00367***	-0.00359***	-0.00298***
	(0.000633)	(0.000542)	(0.000535)	(0.000508)
year fe.	x	x	x	x
firm-level controls		x	x	x
individual-level controls			x	x
without large firms*				x
Observations	1,075,581	1,049,736	1,049,586	774,539
R-squared	0.008	0.063	0.064	0.047

Note: The table shows the estimated coefficients of Equation 15 and 16. Panel A shows the difference in the growth rate of firms employing workers with and without bonuses. In Panel B, the dependent variable is the square of the predicted residual of Panel A. The coefficients in panel B show the conditional variance of the growth rate of firms employing workers with and without bonuses. Columns (1) to (3) differ in the control variables. Every column includes year dummies to get rid of the effect of inflation. Column (2) controls for log-capital per worker and log-sales per worker, the age of the firm and 2-digit industry categories while Column (3) also controls for sex, years of education, experience, square of experience, a dummy indicator for being a new entrant and 2-digit occupation categories. In Column (4), I restrict the sample to firms having less than 500 employees. Standard errors are clustered at firm level.

1.6.2 Results

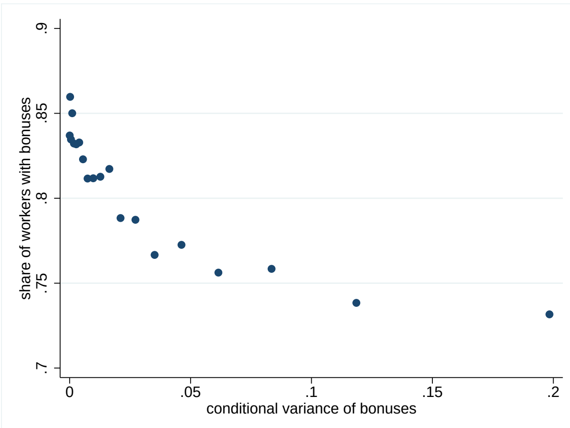
The parameter estimates for Equation 15 are shown in the upper panel of Table 1.5. The most important finding is that workers receiving a bonus do not work at companies with a lower growth rate. Based on the raw difference, workers receiving a bonus work at firms which have a 1 percent larger growth rate than the firms of workers without a bonus. The difference disappears if we take into account firm-level control variables; the estimated coefficient is very close to zero and not significant. Based on these results, we cannot conclude that firms pay a bonus to smooth the effect of lower growth rates.

The lower panel of Table 1.5 shows the conditional volatility of growth rates. The dependent variable is the square-residual of equations from the upper panel. The upper and lower panel feature the same control variables in their columns. According to the first column, workers not receiving a bonus work at firms where the unconditional variance of growth is approximately 4 percentage point. In contrast, in the case of workers receiving a bonus, the unconditional variance is 1 percentage point lower. The point estimates do not change significantly if we take into account the differences in firm-level characteristics. However, the difference in variance more than halves if we include every control variable. By contrast, the conditional variance of the growth rate is approximately the same in the case of both smaller and larger firms. Although the point estimates are small, they are significant in economic terms. The -0.0035 coefficient for the bonus payment dummy means that the variance of the growth rate is more than 10 percentage points lower in the case of firms employing workers with bonus payment. Based on the results, we can reject the hypothesis that firms pay a bonus to counterbalance the larger uncertainty in sales revenue.

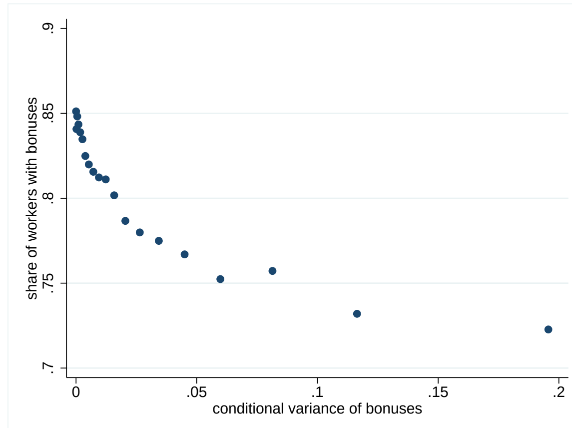
The model with endogenous separations suggests that the relationship between the volatility of growth rates and the prevalence of bonuses is not linear. Therefore, Figure 1.4(a) shows the probability of receiving bonuses as a function of the volatility of growth rates. I grouped the worker-year observations into twenty bins by unconditional variance in the growth rates of the employer and plotted the share of workers receiving a bonus in every bin. In line with the incentive contract explanation of bonus payments, the probability of bonus payments is strictly decreasing with the volatility of growth rates. It is unlikely that the model with endogenous separations can explain this relationship as the model predicts that firms with very low volatility in growth rates do not pay bonuses. Figure 1.4(b) controls for confounding factors but the result is qualitatively unchanged.

Figure 1.4: The relationship between bonus payments and the volatility of growth rates

(a) Unconditional variance



(b) Conditional variance



Note: In these figures, workers are grouped into equally-sized bins based on the volatility of the growth rates of their firms. The vertical axis shows the share of workers with bonuses. Panel (a) has no controls while Panel (b) controls for sex, experience, square of experience, years of education, capital and sales revenue per worker in the base year, 2-digit occupation codes (ISCO 88), 2-digit industry codes (NACE) and year dummies. The wage of workers receiving a bonus co-moves with the sales revenue of firms more tightly than the wage of workers without a bonus, but there is no such difference in the probability of separations. The figures show that workers are less likely to get bonuses if the growth rate of the firm is more volatile. See Section 6.1 for the estimation procedure.

1.7 Assessing alternative explanations for bonus payments

Screening of workers: Some theoretical models (Lazear 1986; 000b Park and Sturman, 2015) show that firms may use state-dependent contracts to screen workers but empirical results are not conclusive as to whether this type of contract attracts the most productive (Bandiera et al., ming) or the least risk-averse workers (Kandilov and Vukina, 2015). In my setup, it is possible that firms share the revenue with the workers to select the best of them but if the volatility of sales is too large, sales are not informative enough to differentiate between employees. However, in this case, every firm should offer a menu of wages and let the worker choose between the fixed-wage and the output-dependent wage structure. On the contrary, Figure 1.1shows that almost every worker of the largest firms receives bonuses. This suggests that the largest firms do not maximize profit by only offering wages with bonus payments or the main motivation of paying bonuses is not to screen workers.

Retention effect: Oyer (2004); Oyer and Schaefer (2005) show that stock options decrease turnover if the value of stock options are correlated with labor market conditions and with the outside options of workers. It is possible that firms with the lowest variance try to cope with outside wage offers by paying state-dependent wages. This theory can explain the lower separation rates of bonus paying firms but cannot explain why the bonus receiving workers are more productive.

Managerial practices: Differences in the skills of the management can be one important factor in the decision about bonus payment. It is possible that high-ability managers can monitor workers' effort more precisely or they can more efficiently anticipate and avoid sales revenue shocks, and that is why firms with a better management use incentive contracts. These kinds of differences in managerial practices do not contradicts the incentive contract explanation for bonus payments. On the other hand, managerial practices can affect the firm-level outcome through other channels as well. Therefore, Table A.3, Column 5 includes firm-fixed effects to control for managerial differences which are constant over time. In addition, Bloom and Van Reenen (2007); Bloom et al. (2013) showed that better management practices lead to a higher growth rate. As Table1.5 shows that average sales growth is not larger at bonus paying firms, I conclude that differences in managerial practices which are conditional on contract types cannot drive the results.

Tax optimization: Oyer and Schaefer (2005) suggests that stock options may be paid partly because they are taxed at lower average rates. However, the base wage and bonuses are taxed exactly the same way, so tax optimization cannot explain bonus payments. Also, this is why personal income tax rates cannot account for the cross-sectional differences in bonus payments either.

Wage under-reporting: Some firms under-report wages to evade taxes in Hungary (Elek et al., 2009, 2012; Tonin, 2011). It may be possible that firms without bonuses adjust unreported wages in case of negative revenue shocks. I address this concern first by re-estimating the main results without minimum wage earners (Table A.3, Column 6). This controls for wage under-reporting, since if a worker gets unreported wage, her wage is the lowest possible, i.e. the minimum wage. In Column 7, I re-estimate the model after omitting firms having less then 100 workers because the smallest firms are the most likely to engage in tax evasion activities (Kleven et al., 2011)[24]. Finally, firm-fixed effects also control for wage under-reporting if the wages of all workers within firms are under-reported to the same extent. As my results are robust against these changes, I conclude that it is not wage under-reporting that helps firms to smooth employment in case of negative revenue shocks.

1.8 Conclusion

I propose an equilibrium search model augmented by idiosyncratic productivity shocks and linear contracts, where firms can share one part of the revenue with the workers. I use the model to compare the incentive contract and wage flexibility explanations for bonus payments. If the main motivation for bonus payments is to smooth the wage bill without firing workers, the model predicts that bonus paying firms will be smaller, with a larger variance in their sales revenue. By contrast, if firms pay bonuses to provide an incentive for high worker effort, the model predicts that bonus paying firms will be larger and more productive but they will also have a lower variance in their sales revenue and lower separation rates. In the second case, the downward wage flexibility of bonus payment is only the side effect of incentive contracts. I also tested the predictions of my model using the Hungarian linked employer-employee database and found that the data support the incentive contract

[24] I cannot omit medium-size firms because in this case I would also omit almost every workers without a bonus.

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explanation for bonus payments.

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Chapter 2

Detecting Wage Under-reporting using a Double Hurdle Model

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2.1 Introduction

The evasion of payroll taxes has two main forms. One is unreported (black) employment, when the employee is not registered and neither she nor her employer pays any taxes. The other main form is the under-reporting of wages, or grey employment, when the compensation consists of an officially paid amount, subject to taxation, and an unreported supplement also known as an "envelope wage" or "under the counter payment". In order to maximize the total evaded tax, the officially paid wage is often (but not always) chosen as the minimum wage (MW).

In this paper we estimate the prevalence of disguised MW earners with the double hurdle (DH) model, first proposed by Cragg (1971), using linked employer-employee data. The DH is a potentially suitable method for disentangling genuine from 'fake' MW earners, relying on the assumption that MW payment is governed by two different processes: market imperfections implying censoring at the MW, on the one hand, and non-random selection to wage under-reporting, on the other. Our application of the DH for Hungary assumes that a spike at the MW was observed for two reasons (i) because of constraints and costs preventing firms

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from firing all low-productivity workers after a wave of exceptionally large hikes in the MW and (ii) because of tax fraud. That said, a worker's genuine wage is observed only if her productivity exceeds the MW and her wage is fully reported. The DH model simultaneously deals with the censoring problem and selection to tax fraud, and estimates the probability of cheating for each MW earner. In the possession of the parameters one can also simulate the 'genuine' wages of MW earners.

The DH model's reliance on distributional properties (as well as the difficulty in finding exclusion restrictions for the selection equation) warns us not to take the estimates at face value. Therefore, we test the validity of the DH results by exploiting a unique episode of Hungary's unconventional MW policies. The test examines the introduction of a minimum contribution base amounting to 200 per cent of the minimum wage (2MW), in 2007. After the introduction of the reform, firms paying wages lower than 2MW faced an increased probability of tax authority audit and a higher risk of being detected as cheaters. Firms were required to report that they paid wages below 2MW and provide evidence, upon request, that their low-wage workers were paid at the going market rate. The reform created incentives for cheating firms to raise the reported wages of MW earners to 2MW while non-cheaters (those paying genuine minimum wages) had no interest to do so. We distinguish cheaters from non-cheaters on the basis of DH estimates for 2006 and check how the cheating proxies affected the probability that a worker earning the MW in 2006 earned 2MW in 2007. We also study how the wages of MW earners changed in 2006-2007. We find that suspected cheaters were more likely to shift their workers from MW to 2MW compared to non-cheating firms. Furthermore, we find that the sales revenues of cheating firms were adversely affected by the reform.

An alternative approach to test the assumptions of our model would be to use a fractional detection model (Feinstein, 1991). These models assume that wages may be also underreported if the reported wage is above the MW. According to anecdotal evidence, the wage under-reporting is much harder to detect In Hungary than other forms of tax fraud (e.g. cost over-reporting). So firms have no incentives to pay wages above the MW if they want to under-report wages.

Our research has important policy relevance as well. At least in the East and South-East of Europe, MW policies are strongly influenced by the conviction that nearly all MW workers earn untaxed side payments. While the suspicions are not groundless they are overstated: we estimate the share of ‘disguised’ MW earners to be around 50 per cent and the share of cheating enterprises to fall short of 40 per cent. Based on our results, the high share of non-cheating firms and genuine MW earners are high so radical, fiscally motivated experiments with the MW may put unskilled jobs at risk. Still our results confirm that the DH model gives reliable information on the probability of tax evasion on the individual level. Using the statistical profiles derived from the DH model may help the better targeting of tax authority inspection in countries with high tax evasion and help to facilitate more circumspect MW policies.

The paper is organized as follows. Section 2 gives a brief overview of the literature while Section 3 the MW regulations and the wage distribution in Hungary. Section 4 introduces the DH model, explains the estimation of its parameters, shows how the probability of cheating and ‘genuine’ wages are simulated and how we classify workers and firms on the basis of the DH estimates. Section 5 introduces the data. Section 6 presents the estimates of the DH model. Section 7 presents the methods, data and results of the test and Section 8 concludes.

2.2 Wage under-reporting and the minimum wage – An under-researched area

Compared to the vast literature on income under-reporting and MW regulations, respectively, the body of research on how these two areas relate to each other seems rather thin. Most of what we know empirically about this relationship comes from anecdotal evidence, inspection of aggregate data, scarce survey results and a few attempts to identify the incidence of envelope wages indirectly. Theoretical work is largely missing.

Although several mechanisms may cause a spike of the wage distribution at the MW, including the tacit collusion of employers (Shelkova, 2015) or the extrusion of wages due to the effective MW (DiNardo et al., 1996), grey employment is certainly among the suspects. Cross-country data suggest a positive correlation between the size of the spike and estimated size of the informal economy (Tonin, 2007). Several accession countries including Hungary, Latvia, Lithuania and Romania have (or had) high shares of MW earners, while their Kaitz-indices are (were) in the middle range, suggesting that disguised MWs may be particularly widespread in these countries. Similar observations are interpreted in a similar way in World Bank (2005).

Erdogdu (2009) reports on the basis of several surveys that under-the-counter payments are prevalent in the wage policy of Turkish firms. There is a relatively extensive literature focusing on grey employment in the Baltic states. Relying on survey results, Masso and Krillo (2009) point out that 16-23 percent of the MW earners received envelope wages in Estonia and Latvia but only 8 percent in Lithuania in 1998.Meriküll and Staehr (2010) show that young employees and people working in construction and trade are most likely to get unreported cash supplement on top of their official salary in the three Baltic countries. Kriz et al. (2007) present similar results on the distribution of envelope wages using three different Estonian data sets. According to the Eurobarometer survey conducted by the European Commission in 2007 (European Commission 2007), 5 per cent of employees in the EU receive part or all of their regular income untaxed and this ratio is over 10 per cent in some central and eastern European countries (8 per cent in Hungary) but there is no information on how many of them are officially paid the MW.

Some studies obtain evidence on disguised MWs indirectly, by comparing the reported consumption-income profiles of households. Using household budget survey data from Hungary, Benedek et al. (2006) looked at the winners and losers from the 2001-2002 MW hikes. They observed income loss without the loss of a wage earner in the high-income brackets where substantial under-reporting is most likely to occur. For these households the increasing MW may have implied higher taxes and lower net income. Based on the same data set, Tonin (2011) analyzed changes in the food consumption of households affected by the minimum wage hike compared to unaffected households of similar income. He found that food consumption fell in the treatment group relative to the controls – a fact potentially explained by a fall in their unreported income in response to the MW hike and growth of the associated tax burden.

The theories of wage under-reporting (Allingham and Sandmo, 1972; Yaniv, 1988) shed light on the incentives to engage in tax fraud under alternative penalty and withdrawal schemes but they do not explicitly discuss the case of reporting the MW to tax authorities. This is the cost-minimizing choice for the firm (unless MW payment provokes audits thereby decreasing the expected gain from cheating) but it also requires the cooperation of workers. As Madzharova (2010) notes: if the actual or perceived linkages between contribution payments and pensions or access to health services are weak and/or workers see that their payments feed corruption rather than are used to finance public services, they will be willing to accept the lowest possible reported wage. Theoretical models explicitly addressing the issue of wage under-reporting cum MW regulations include Tonin (2011) and Shelkova (2015). Tonin argues that the MW induces some workers whose productivity is above the MW, but who would have declared less if there was no MW, to increase their declared earnings to the MW level. Workers with productivity below the MW either work in the black market or withdraw from the labor force while high-productivity workers are unaffected. This is a possible explanation of why a spike at the MW appears in the distribution of declared earnings. Shelkova assumes that low productivity labor is homogenous and easy to replace thanks to the low fixed costs of hiring. If a non-binding MW exists and employers act symmetrically then tacit collusion and offering the MW to low productivity workers is profit maximizing and dominant strategy for the companies. An increase in the minimum wage increases the probability of collusion since the incentive for deviation is weaker. This implies that a higher

MW increases the spike there.

Our empirical work attributes the sudden nascence and decease of a huge spike at the MW to state intervention, on the one hand, and tax evasion, on the other. We look at a unique period in Hungary's MW history, which quadrupled the spike at the MW in only two years (when the MW was nearly doubled in 2001-2002) and decreased it by a factor of 2.5 in only one year (when a double contribution base was introduced in 2007). We do not believe that these sudden and enormous changes could be explained by the established strategic behavior of enterprises underlying Shelkova's model. It is also hard to trust that Tonin's assumption, stating that the marginal products of those at the spike exceed the MW, was valid in the period we are looking at. When the plan of increasing the MW from Ft 25,500 to Ft 50,000 was announced, 32.7 per cent of the private sector employees earned less than that. When the idea of the minimum contribution base came up, 58 per cent had wages below 2MW. It is quite obvious that the vast majority of the affected workers remained in employment for a protracted period (or until recently) and many of them had productivity below the aforementioned thresholds after the hikes. It took time until mobility between jobs, changes of the product mix and technology, adult training and other forms of adjustment could restore (if at all) the optimum condition for mutually gainful employment without causing massive unemployment in between[26]. Therefore, we stick to the assumption that in the period under examination the spike at the MW was explained by under-reporting and the continuing employment of many low-productivity workers – two different processes that we try to model following the DH approach.

[26]Independent studies by Halpern et al. (2004) and Kertesi and Köllő (2003) estimated the short-run aggregate disemployment effect of the first MW hike to fall to the range of 1-1.5 per cent in 2001.

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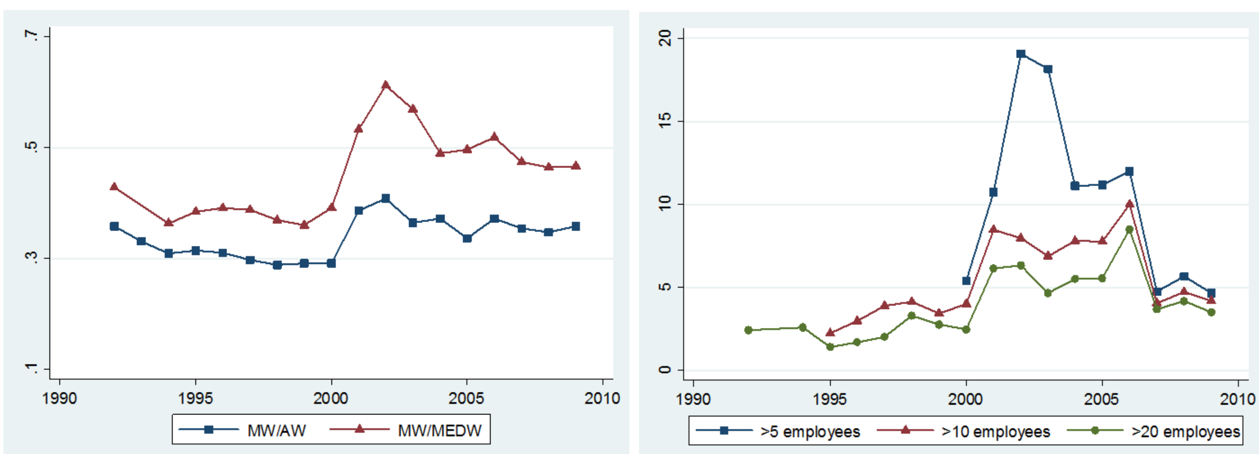
2.3 The minimum wage and the wage distribution in Hungary

MW regulations had minor impact on the Hungarian wage distribution until the millennium[27] As shown in Figure 2.1, the MW-average wage ratio slightly decreased in 1992-2000 and fell short of Spain's, the laggard within the EU in that period. The fraction of workers paid 95-105 per cent of the minimum amounted to 5 per cent, a ratio similar to those reported by Dolado et al. (1996) for Austria, Belgium, the Netherlands, Denmark, and the US.

In 2001–2002 the MW was nearly doubled in nominal terms, resulting in a 14 percentage point rise in the Kaitz-index[28]. The fraction of private sector employees earning near the MW jumped to 11 per cent in 2001 and 18 per cent in 2002.

Figure 2.1: The minimum wage and minimum wage earners in Hungary 1992-2009



(a) The MW compared to the average wage and the median wage 1992-2009

(b) Fraction paid 95-105 per cent of the MW, 1992-2009

The data relate to gross monthly earnings in the private sector. Data source: Wage Surveys

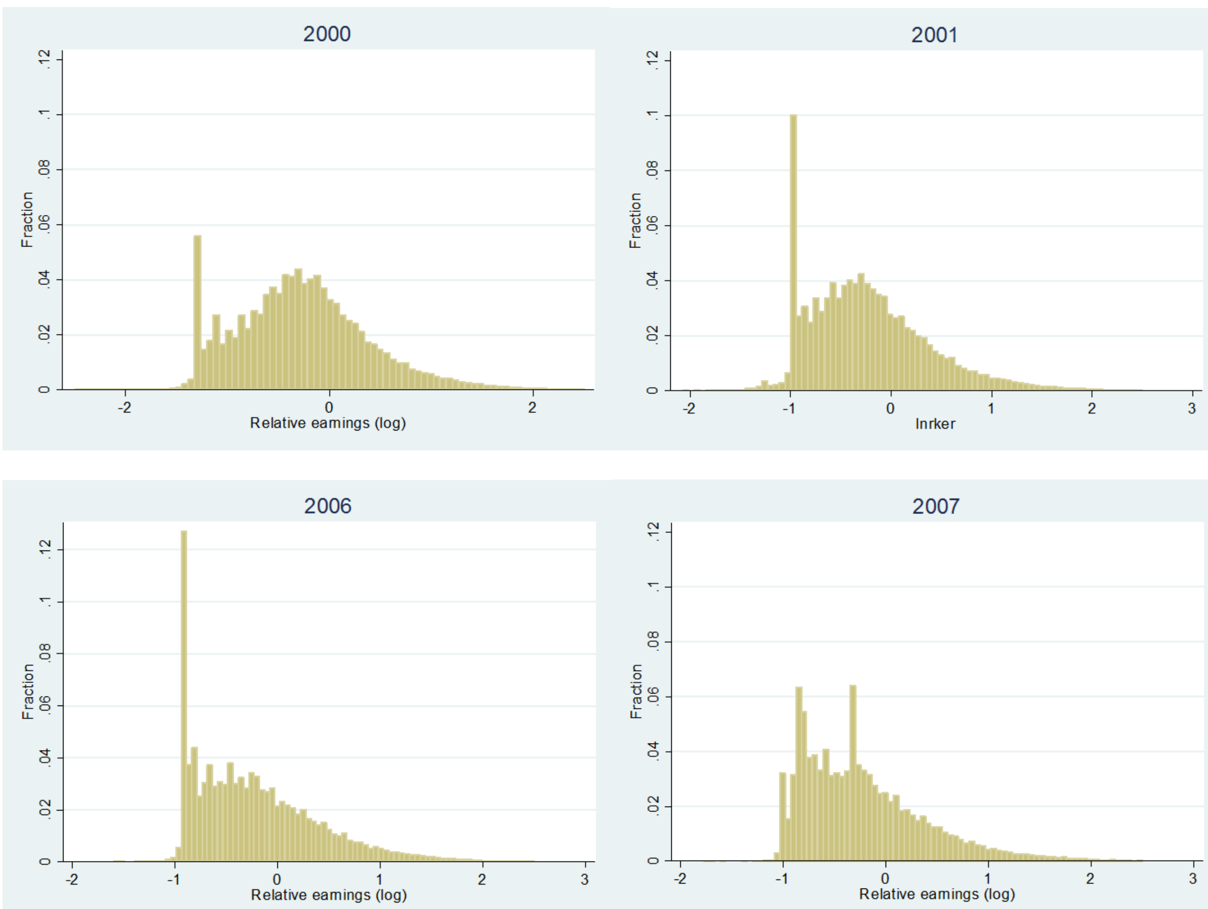
The wage distribution preserved its distorted shape until 2007, when a second spike

[27] See Appendix 2 for further details of Hungary's MW regulations.

[28] The MW increased from Ft 25,500 in 2000 to Ft 40,000 on January 1, 2001 and Ft 50,000 on January 1, 2002. See Kertesi and Köllő (2003) on the motives and aftermaths of the large hikes.

appeared at 200 per cent of the MW, as shown in Figure 2.2[29]. That year, the Hungarian government introduced a minimum social security contribution base amounting to 2MW. Firms were allowed to pay wages lower than 2MW but in case they did so they faced an increased probability of tax authority audit and a higher risk of being detected as cheaters (for paying disguised MW or for other reasons)[30].

Figure 2.2: The wage distribution in selected years



Data: Wage Surveys. Samples: full-timers in the private sector

The suspicion that the crowding of workers at the MW in 2001-2006 was partly explained

[29]At the same time further minima were introduced for young and older skilled workers (1.2MW, 1.25MW) that flattened the spike near the MW.

[30]Similar minimum contribution levels were introduced in Bulgaria and Croatia in 2003. The Hungarian regulations remained in effect until January 2010

by wage under-reporting is difficult to avert. In 2006, the fraction of MW earners amounted to 18 per cent among small firm managers, and close to 10 per cent among top managers in larger firms also earned the MW. High shares could be observed in a number of freelance occupations such as architects, lawyers, accountants, business and tax advisors, agents, brokers, artists, writers, film-makers, actors and musicians (13-17 per cent). The fraction was particularly high in those sectors, where cash transactions with customers frequently occur such as shops, hotels and restaurants (23 per cent), house building (21 per cent), personal services (18 per cent) and farming (21 per cent). In some low-wage occupations such as cleaners, porters and guards the fraction earning the MW fell short of the above-mentioned levels (Table 2.1).

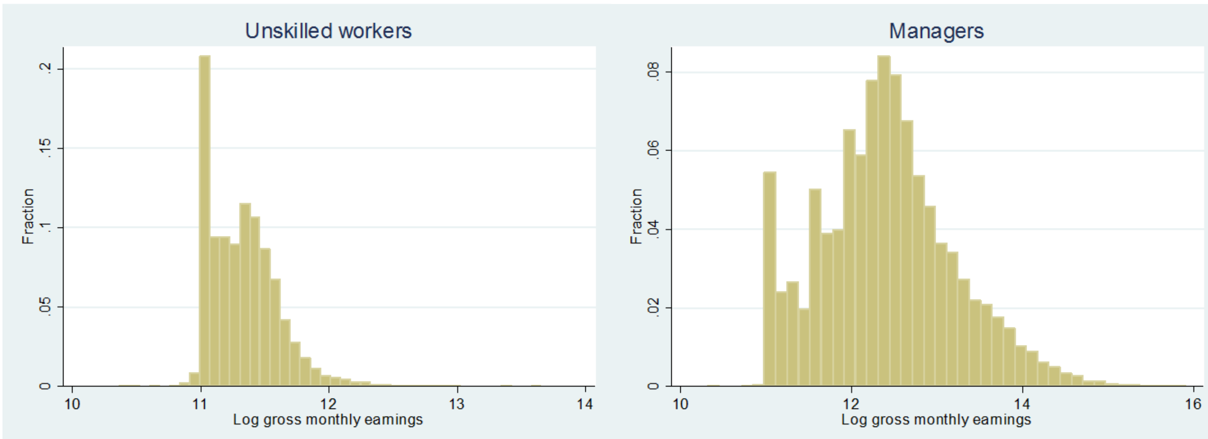
Table 2.1: Fraction paid the exact amount of the minimum wage in 2006

	Per cent paid the MW	Composition All MW earners=100
Top managers	9.7	1.6
Managers (heads of department, foremen, etc.)	3.6	2.2
Managers of small firms (5-20 employees)	18.0	1.6
Engineers	2.4	0.6
Architects and construction technicians	9.5	0.3
Professionals in health, education and social services (private)	3.5	0.0
Other professionals	3.0	0.5
Lawyers, business and tax advisors, accountants	8.8	0.7
Freelance cultural occupations (musicians, actors, writers etc.)	16.5	0.5
Technicians	7.3	2.7
Administrators	8.3	6.8
Agents, brokers	12.6	0.8
Office workers	11.5	5.6
Blue collars in retail trade and catering	22.5	15.3
Blue collars in transport	7.7	0.1
Services A (other than B and C)	12.7	1.5
Services B (health and social services, private)	0.0	0.0
Services C (personal services)	17.7	0.7
Farmers and farm workers	20.9	5.1
Blue collars in heavy industry and engineering	8.9	6.6
Blue collars in light industry	14.6	9.2
Blue collars in construction (house building)	21.0	10.0
Blue collars in civil engineering (roads, railways, bridges)	20.0	0.6
Assemblers and machine operators	4.7	4.3
Truck drivers	20.5	3.8
Porters, guards, cleaners	18.2	7.6
Unskilled laborers, casual workers	37.6	11.2
Total	10.8	100.0

Source: Wage Survey, 2006, estimation sample of the DH model. Number of observations = 91,240
Note: For this table some occupations were divided into parts on the basis of industrial affiliation and firm size in order to capture differences in the scope for cash transactions with customers (personal versus other types of services, small firm versus large firm managers).

Further doubts arise if we look at the wage distribution within occupations (Figure II.3). In 2006, the distribution for unskilled workers was strongly skewed at the MW with a small number of workers earning substantially more than that. By contrast, the wage distribution of managers, for instance, had a spike at the MW and another at 440 per cent of the MW, clearly pointing to a minority of managers under-reporting their earnings.

Figure 2.3: The wage distribution in two occupations, 2006



Data: Wage Survey 2006, private sector. Occupational codes: managers 1311-1429, unskilled workers 9190

With the help of the double hurdle model we can utilize the information content of the different shapes of the wage distributions. In the next section we summarize how the estimation proceeds, how the probability of under-reporting and the MW earners' 'genuine' wages are derived, and how we classify workers and firms as cheaters or non-cheaters.

2.4 The double hurdle model

2.4.1 The set-up of the model

Let us use the notation y for the (normalized) logarithm of the "true" wage, i.e. of the wage which would prevail in the absence of MW and under-reporting. (We normalize y to be zero at the true MW.) The value of y is determined by some characteristics X of the employee and the firm, and we assume that its distribution is conditionally normal with expectation $X\beta$ and variance σ^2. (This is a standard assumption in the literature; see e.g. Meyer and Wise 1983a and 1983b.) In the presence of MW and under-reporting, a spike appears at the MW in the wage distribution. The observed wage (the logarithm of which – normalized again to be zero at the MW – will be denoted by $y*$) *) may be equal to the MW for two reasons: because of constraints and costs preventing firms from firing low-productivity workers (in the simplest case those whose genuine wage would fall below the MW), or because of tax fraud (when the MW is reported to the authorities but an unobserved cash supplement is also given). The probability of cheating is determined by some characteristics Z of the employee and the firm, and X may be different from Z. Formally, omitting subscript i for the individual, the following model governs y and $y*$

$$y = X\beta + u \tag{17}$$

and we observe the reported log-wage $y*$ according to the rule:

$$y* = \begin{cases} y & if\; X\beta + u \; and \; \gamma Z + v > 0 \\ 0 & otherwise \end{cases} \tag{18}$$

Under-reporting occurs when both $X\beta + u > 0$ and $\gamma Z + v \leq 0$hold, and in this case the observed wage is equal to the MW. The residuals u and v are zero-mean normally distributed, possibly correlated (ρ) random variables. σ^2 stands for the variance of u while the variance of v is set equal to unity without loss of generality, hence the covariance matrix of (u, v) is given by:

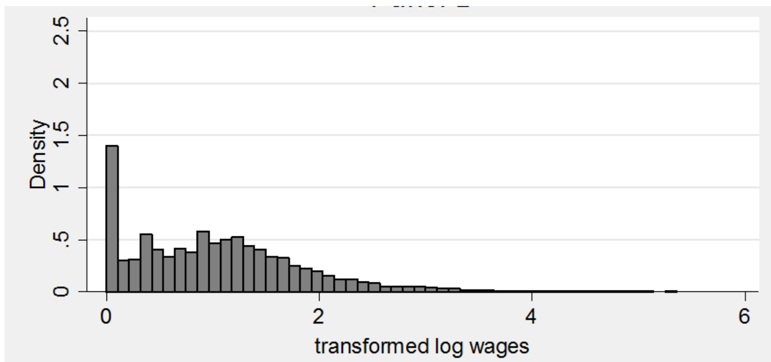
$$S = \begin{Bmatrix} \sigma^2 & \rho\sigma^2 \\ \rho\sigma^2 & 1 \end{Bmatrix} \tag{19}$$

This is the double hurdle model first proposed by **Cragg (1971)**, with the restriction $\rho = 0$, to model the purchase of consumer goods in a setting where the decision to buy and the decision of how much to buy are governed by different processes. The name of the model comes from the fact that the spike of the distribution (in our case at the MW) is determined by two "hurdles": a standard tobit-type constraint (in our case following from the wage equation: $X + u \leq 0$) and a different second hurdle (following from the selection equation: $Z\gamma + v \leq 0$). Note that the standard tobit model is obtained as a special case when the second hurdle is not effective, e.g. when Z contains a sufficiently large constant and all other terms in γ are zero, or when $X = Z$, $\beta = \gamma$ (apart from a constant), $\rho = 1$ and $\sigma = 1$. In our case, a second hurdle is needed because under-reporting and wage determination are governed by partly different processes.

Since the paper of Cragg the model and its extensions have been widely used to analyze con-sumer and producer behavior as well as problems in environmental and agricultural economics and banking (e.g. Labeaga 1999; Martínez-Espiñeira 2006; Moffatt 2005; del Saz-Salazar and Rausell-Köster 2008; Teklewold et al. 2006Labeaga 1999, Martinez-Espineira 2006, Moffatt 2005, Saz-Salazar and Rausell-Köster 2006, Teklewold et al. 2006). However, to our knowledge, only Shelkova (2015) used the model to analyze wage distributions, in a setting discussed earlier.

Figure 2.4: Wage distribution before and after the transformation

(a)



(b)

Data: Wage Survey 2006, private sector, full-timers

In our application, the baseline DH model (1)-(3) has to be slightly modified in order to better capture the features of the wage formation process. The first problem to be addressed is that the log wage distribution is not censored normal because of the crowding of wage earners just above the MW[31] (see Panel A in Figure 2.44). While at and above the median the distribution is close to the normal we have more workers on the left tail than expected under normality. This poses a problem because – as usual for nonlinear models – maximum likelihood estimation of the DH model yields consistent results only if the underlying distributions are well-specified. Therefore we apply a preliminary transformation that is roughly linear at higher wages and accounts for 'crowding' at lower wages. We assume that instead of $y*$ we observe $g(y*)$, where r is a coefficient to be determined:

[31] This is explained by spillover effect as argued in Dickens et al. (1994) and elsewhere.

$$g(x) = x + r * exp((-x/r)\, if\ x \geq 0 \quad (20)$$

By the preliminary transformation g^{-1} we can ensure that $y*$ is close to a (censored) normal distribution and hence the DH model can be applied. Our approach is in line with the double hurdle literature, where a preliminary transformation is often needed to achieve normality: Martínez-Espiñeira (2006); Moffatt (2005) use the Box-Cox, while Yen and Jones (1997) apply the inverse hyperbolic sine transformation.

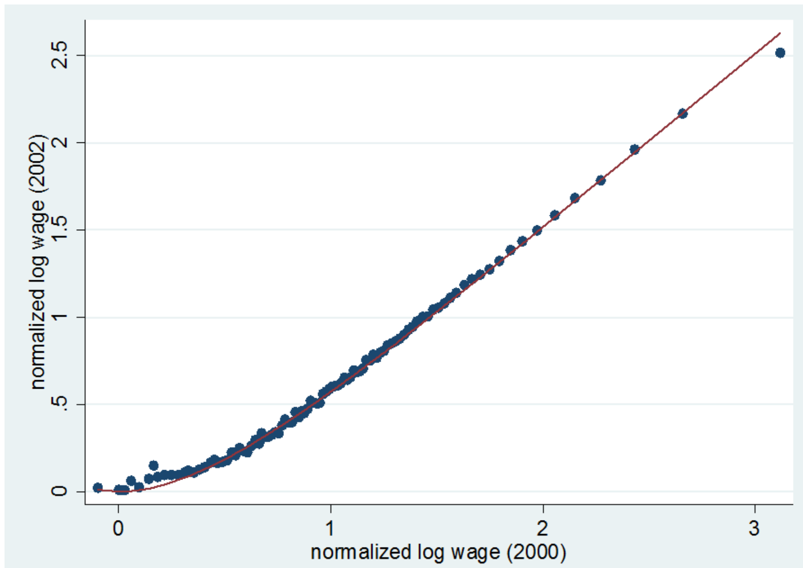
The second possible problem concerns our assumption that cheating employers report the MW (and not a larger wage) to the authorities. This is a reasonable assumption for 2001-2006 because firms could maximize the evaded tax this way and the chance of tax audit was not increased for MW-reporting firms before 2007. The model can be extended to allow for cheating above the MW (see Elek et. al. (2009)) but external (e.g. survey-based) information is need-ed to identify its parameters. In this paper, we use the simpler formulation.

2.4.2 Parameter Estimation

First, the parameter r of the preliminary transformation (4) should be determined. Instead of a likelihood-based statistical procedure, we make use of the fact that the wage distribution was close to lognormal in 2000 (see Figure II.3), changed substantially because of the MW increase and spillover effects in 2001-2002, and – in the absence of further drastic MW hikes – was practically unaltered in 2003-2006. Thus we create a quasi panel subsample of the LEED data for 2000-2002, and assign the median of the 2002 logarithmic wages to the median of the 2000 logarithmic wages for each percentile of the wage distribution in 2000. (See section 5 for details of the LEED data set.) Then the wage-wage percentile graph obtained this way is normalized to be zero at the MW each year and the function g (with unknown parameter r) is fitted to it with nonlinear least squares. This method gives a transformation for the normalized wages in 2002 and – for the reasons mentioned above – for 2006 as well.

Our method yields $r = 0.49$. Figure 2.5 displays the function and its appropriate fit to the 2000-2002 wage percentiles, while Panel B in Figure 2.4 shows that the transformed log wages ($g^{-1}(y*)$) are approximately censored normal. For ease of notation, in what follows, we refer to $g^{-1}(y*)$ as $y*$.

Figure 2.5: The function g(x) for r = 0.49 and its fit to the percentile graph 2000-2002



Using the properties of the conditional distributions of the bivariate normal distribution, the likelihood function of the DH model (1)-(3) can be shown to have the following form (for the sake of clarity, here we use subscripts i for the individuals):

$$L = \prod_{y_i*=0} [1 - \Phi_{\rho,\sigma,1}(x_i\beta, z_i\gamma)] \prod_{y_i*>0} \left[\Phi\left(\frac{z_i\gamma + \frac{\rho}{\sigma}(y_i^* - x_i\beta)}{\sqrt{1-\rho^2}} \right) \frac{1}{\sigma}\phi\left(\frac{y_i^* - x_i\beta}{\sigma}\right) \right] \qquad (21)$$

where $\Phi_{\rho,\sigma,1}$ denotes the bivariate normal distribution with covariance matrix given in (3), while $\Phi$and ϕ stand for the univariate standard normal distribution and density, respectively. Parameter estimation can be carried out with maximum likelihood, where we use cluster-robust standard errors to tackle the potential within-firm correlation in the error terms.

If the DH model is correctly specified (including the distributional assumptions), then identi-fication can be carried out even if $X = Z$, i.e. based merely on nonlinearities. However, to make the results more robust to deviations from the distributional assumptions, it is worth including variables that only influence the selection equation but not the wage equation (i.e. making valid exclusion restrictions). Therefore, in the wage equation we include the usual variables thought of as influencing the productivity of a worker such as her individual characteristics (experience, education, sex) and the characteristics of her firm (industry, productivity, fixed assets, location, size and ownership)[32]Since the majority of these variables affect cheating behavior as well, they are also present in the selection equation. (e.g. for larger firms it is more difficult to hide envelope wages from the tax authority thus they tend to be less involved in grey employment.)

We also include individual and firm-level proxies directly affecting the decision to evade taxes. In particular, we distinguish some occupational categories that are more prone to cheating than others, mainly due to the lower risk of being caught such as managerial and freelance occupations, occupations with frequent cash transactions or jobs in trade, hotels and restaurants (see Table 2.2 and Appendix A.5 for definitions). We also choose proxies for tax evasion from the corporate tax returns. It is expected that wage-underreporting firms tend to evade corporate taxation, thus tax liability correlates negatively with cheating. Another proxy is "other personnel related expenses" which contain fringe benefits: these are rather complementary to wage payments hence a high share of personnel related costs indicate compliance to the tax rules. The chosen indicators are indicative of compliance with the tax rules in fields other than wage payment. It is reasonable to assume that, after controlling for the usual factors in the wage equation, the firm-level instruments only influence the probability of cheating but not the genuine wages thus we have valid exclusion restrictions in the model.

[32]For robustness check, in an alternative specification we use occupation dummies instead of industry dummies in the wage equation

2.4.3 Under-reporting probabilities, 'genuine' wages and classification of workers and firms

In the possession of the DH parameters the probability of cheating for each MW earner can be estimated as:

$$
\begin{aligned}
P(underreporting) &= P(X\beta + u > 0, Z\gamma + v \leq 0 | y^* = 0 = \\
&= \frac{P(u > -X\beta) - P(u > -X\beta, v > -Z\gamma)}{1 - \Phi_{\rho,\sigma,1}(X\beta, Z\gamma)} \\
&= \frac{\Phi(X\beta/\sigma) - \Phi_{\rho,\sigma,1}(X\beta, Z\gamma)}{1 - \Phi_{\rho,\sigma,1}(X\beta, Z\gamma)}
\end{aligned}
\tag{22}
$$

Also, we can simulate the genuine wage of each MW earner as follows. We generate independent cop-ies of bivariate normal random variables (u, v) with covariance matrix given in equation (3), and accept max $(X + u, 0)$as the normalized genuine log-wage of an MW earner if $X\beta + u \leq 0$ or $Z\gamma + v \leq 0$. If none of these conditions hold, the person cannot earn MW according to the model. Technically, for each MW-earner, the (u, v) variables are simulated until at least one condition holds.

Let us denote the estimated probability of under-reporting by a MW earner with P and the simulated wage with w (i.e. $w = MW * exp(g(y)\ )$. As a benchmark definition cheating behavior is assumed in case of $P > 0.5$, but $w > MW$ and $w > 1.5MW$ will also be used for robustness checks.[33]. If we find at least one MW earner classified as "cheater" in a firm we treat the firm as a cheater. Since the majority of cheating firms are small, the use of other, more advanced criteria such as a certain threshold for the ratio of cheaters would be of limited practical importance.

[33]The definition P>0.5 is preferable to e.g. w>MW because the latter includes some extra simulation uncertainties.

2.5 Data

Throughout the paper we rely on the Wage Survey (WS) of the National Employment Service. The WS is a linked employer-employee data set recently comprising observations on over 150,000 individuals in about 20,000 firms and budget institutions. The survey was carried out triannually until 1992 and annually since then. In the enterprise sector the WS covers businesses employing at least 5 workers. All Hungarian firms employing more than 20 workers are obliged to report data for the WS while smaller firms are randomly selected from the census of enterprises. In the years considered in our paper, firms employing 5-20 workers had to report individual data on each employee while larger ones reported data on a (roughly 10 per cent) random sample of their workers, selected on the basis of their day of birth. The observations are weighted by the Employment Service to correct for the selection of firms and individuals. The survey contains information on the wages and demographic and human capital variables of the workers and their job characteristics. The firm-level variables comprise industry, region, firm size, location, ownership, union coverage and financial variables including sales revenues, the net value of fixed assets, average wages, profits and several cost items. Our estimation sample covers the private sector and comprises 92,140 observations.

Table 2.2: DH estimates of wages for 2006

	Coefficient	St. error[a]
Wage equation for normalized log wages (also includes industry controls)		
Experience / 10	0.327***	0.013
Exp squared / 100	-0.049***	0.002
Male	0.205***	0.011
Vocational edu.	0.183***	0.013
Secondary edu.	0.485***	0.015
Higher edu.	1.191***	0.019
Budapest	0.135***	0.023
Value added per worker log	0.147***	0.010
Fixed assets per worker log	0.007	0.005
Firm of foreign ownership	0.255***	0.020
Firm with 5-10 employees	-0.404***	0.037
Firm with 11-20 employees	-0.371***	0.026
Firm with 21-50 employees	-0.233***	0.024
Firm with 51-300 employees	-0.112***	0.020
Constant	0.255***	0.020
Selection equation		
Experience / 10	-0.408***	0.070
Exp squared / 100	0.108***	0.015
Male	-0.214***	0.050
Vocational edu.	0.050	0.138
Secondary edu.	-0.128	0.127
Higher edu.	-0.012	0.136
Managerial and freelance[b]	-0.392**	0.161
Cash transactionsc[c]	-0.226**	0.113
Retail traded[d]	-0.333***	0.100
Budapest	-0.244**	0.123
Works in a city	0.112	0.091
Works in a village	-0.133	0.105
Corporate tax payment / sales revenues	10.03**	4.00
Other personnel related expenses /payroll	2.261***	0.844
Firm of foreign ownership	0.778***	0.187
Firm with 5-10 employees	-2.007***	0.272
Firm with 11-20 employees	-1.709***	0.264
Firm with 21-50 employees	-1.404***	0.270
Firm with 51-300 employees	-0.930***	0.270
Constant	3.074***	0.315
Rho	-0.302***	0.047
Sigma	0.547***	0.008
N of observations	91,240	

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

a) Cluster robust standard errors, adjusted for firm-level clustering b) Managerial and freelance occupations (the latter includes professionals in culture and arts, agents and brokers) c) Occupations where cash transactions occur frequently. Includes car mechanics, electricians, plumbers, household employees, couriers, truck drivers and workers in personal services and house building (see Table A.6). d) Occupations in retail trade (see Table A.6) Data source: Wage Surveys 2006, private sector

In section 7, we use panels of individual and firm-level observations. Firms in the WS can be directly linked and followed over time. Individuals cannot be linked directly but they can be identified across waves with acceptable precision using data on their firm identifier, location of their workplace, year of birth, gender, education and four-digit occupational code. The worker and firm panels are non-randomly selected from the base-period (2006) populations because of the survey design, on the one hand, and group-specific differences in firm survival, job destruction and quits, on the other. We control for selection on observables by estimating probit equations and using the inverse of the predicted probabilities of being in the panel as weights in those models, where weighting is allowed (for the method used see e.g. Moffit et al. 1999). The probits are presented in Tables A.6 and A.7 of the Appendix. While the probability of making it to the panel was clearly non-random, weighting still had negligible effect on the estimated parameters.

2.6 Results of the double hurdle model

Table 2.2 presents the parameter estimates of the DH model. The parameters of the selection equation largely conform to intuition. 'Grey' occupations, male workers and employees in Budapest tend to under-report wages significantly, while foreign ownership, firm size, higher corporate tax liability and larger 'other personnel related expenses' of the firm are positively correlated with labor tax compliance. After controlling for other factors, education does not seem to have a direct effect on cheating. The correlation between the error terms (ρ) is significantly negative, implying that unobserved factors leading to higher genuine wages tend to increase the probability of cheating. Similar results are obtained in the alternative specification, when occupation dummies (defined in Table A.5) are used instead of industry dummies in the wage equation.

Using the estimated parameters, the probability of under-reporting among MW earners and their genuine wages was calculated. The results suggest that around half of all workers paid the MW hid part of their earnings from the tax authority. We estimate that the average „genuine" wage of the MW earners amounted to approximately 170 per cent of the MW and the average wage of cheating MW employees (using $w > MW$ as the criterion for cheating) was around 250 per cent of MW. We should note that the exact share of cheaters and their simulated genuine wages are quite sensitive to the parameter r of the preliminary transformation but – more importantly from a modeling point of view – the partial effects of the different factors (occupations etc.) are robust across different specifications.

Table 2.3 displays the estimated probability of under-reporting among MW earners, their average genuine wage and a "cheating indicator" by occupation, industry and firm size for the two different specifications.[34] (The cheating indicator is defined as the share of cheating MW earners among all employees.) Looking at occupations, the estimated fraction of cheaters among MW earners is small for cleaners (10-20 per cent), unskilled laborers and agricultural workers (20-30 per cent), while it is much larger than average e.g. for drivers. We find the managers and professionals have the largest predicted cheating probability conditional on earning MW. It is also clear that the share of MW earners is not a good indicator of under-reporting because fraud is relatively frequent for some occupations with a high share of MW earners (e.g. in construction), while infrequent for others (e.g. among cleaners, unskilled laborers). The cheating indicator, which is the product of these two terms, is substantially higher than average in construction and trade professions and among drivers.

[34]One is the baseline specification containing industry dummies in the wage equation, while the other contains occupation dummies instead.

Table 2.3: Predictions of the DH model for 2006

	Prob. of under-rep. among MW earners (%)		Share of MW earners (%)	Cheating indicator (per cent)[b]		Simulated wage of cheaters (MW = 1.0)	
	(1)[a]	(2)[a]		(1)[a]	(2)[a]	(1)[a]	(2)[a]
Total	46	48	11.9	5.5	5.7	2.6	2.4
Occupationsc							
Agriculture	31	29	27.5	8.6	7.9	2.3	1.8
Construction	45	56	23.4	10.6	13.0	1.9	1.8
Services	40	43	6.7	2.7	2.9	2.4	2.2
Trade	52	39	20.5	10.6	7.9	2.2	1.8
Industry	41	49	12.8	5.3	6.3	2.1	2.0
Other blue collar							
Cleaners	18	13	23.8	4.2	3.2	2.5	1.6
Unskilled laborers	30	22	33.3	10.1	7.5	2.0	1.6
Machine operators	35	45	5.7	2.0	2.5	2.1	2.1
Porters and guards	38	24	15.6	5.9	3.7	2.5	1.6
Drivers	59	72	15.8	9.3	11.4	2.2	2.2
White collar							
Office clerks	52	59	11.0	5.7	6.5	2.7	2.4
Technicians. assistants	72	84	5.3	3.8	4.4	3.2	2.8
Administrators	64	78	6.4	4.1	5.0	3.1	2.9
Managers	74	96	5.2	3.8	5.0	3.7	4.4
Professionals	94	97	2.5	2.4	2.4	5.1	5.1
Industries							
Agricult., fishing	34	37	15.9	5.4	5.9	2.4	2.5
Manufacturing	40	43	7.5	3.0	3.2	2.6	2.4
Construction	42	49	27.9	11.6	13.8	2.1	2.0
Trade	52	57	17.4	9.1	9.9	2.4	2.4
Hotels. restaurants	40	36	22.2	8.8	7.9	2.2	2.3
Transport	68	61	6.3	4.3	3.8	4.0	2.5
Financial services	72	35	2.4	1.7	0.8	3.6	3.6
Real est.. business activ.	51	47	12.0	6.2	5.6	3.1	2.8
Other	49	42	8.1	4.0	3.4	2.8	2.3
Firm size							
5-10 employees	58	60	32.3	18.9	19.5	2.2	2.1
11-20 employees	50	52	23.3	11.6	12.2	2.3	2.2
21-50 employees	44	46	14.1	6.2	6.6	2.7	2.5
51-300 employees	30	36	6.9	2.1	2.5	3.1	3.1
300 + employees	7	7	1.0	0.1	0.1	5.8	7.7

a) Models: wage equation with (1) industry dummies; (2) with occupation dummies . b) Cheating indicator: share of cheating MW earners among all employees c) Occupations: see Table A.5 in Appendix. Data source: Wage Survey 2006. Number of observations: 91,240

As far as *firm characteristics* are concerned, Table 2.3 also displays the relation of economic branch and firm size to under-reporting. The cheating indicator is higher than average in construction, trade and hotels and restaurants, while it is the lowest in financial services (where the share of MW earners is the smallest as well). Both the ratio of MW earners and cheating behavior are strongly negatively correlated with firm size: the cheating indicator is ten times higher for firms with 5-10 employees than for larger firms with more than 50 employees. Foreign-owned enterprises tend to employ much less workers at the MW than domestic and mixed ones but the ratio of under-reporting among them does not differ substantially.

2.7 Testing the predictions of the DH: responses to the introduction of a minimum contribution base

As was briefly discussed earlier, the 2007 reform created incentives to raise the reported wages of disguised MW earners. Cheating firms could fully avert the risk of audit by officially paying 2MW or more to their grey employees instead of MW. Furthermore, the public debate preceding the reform gave a clear warning that the tax authority would treat MW payment as a signal of tax evasion. Therefore, cheating firms had stronger motivation to shift their grey employees away from the MW while non-cheating enterprises, in the position to demonstrate that they pay 'genuine' MWs, had less incentive to raise the wages of their MW earners.

The sudden shift of the spike of the wage distribution from MW to 2MW in 2007 (shown earlier by Figure 2.2) clearly indicated that firms – especially smaller ones – considered tax audit a credible threat. Before 2007, tax inspections were rather lax in Hungary. While firms employing more than 50 workers were checked by independent auditors and/or the tax authority annually and the monitoring activities of the tax authority concentrated on "accentuated tax payers" (companies having the largest tax liabilities), entities without legal personality were monitored only in every 7th year and individual entrepreneurs only in every

23rd year on average. Penalties were insignificant. Consistent with the reform's intentions, the new regulation changed the wage distribution of small firms dramatically while larger firms were weakly affected. [35]

The proportion of cheating *firms* (i.e. firms with at least one cheating employee) amounted to 17.3 per cent of all firms and 37.0 per cent among enterprises having at least one MW earner. While the estimates confirm that, in 2006, envelope wages existed at a large scale, they sug-gest that more than half of the MW earners did not receive cash supplement and the majority of firms paying MW did not cheat on taxes. However, for reasons discussed earlier the esti-mates should be treated with caution and the model's predictive power needs to be checked.

We check how the wages of grey employees changed in response to the reform by estimating a probit equation (7) for a quasi-panel of individuals earning the MW in May 2006 and also observed in May 2007:

$$P\left(w^* = 2MW_1 | w_0^* = MW_0\right) = \Phi\left(C\beta + Z\gamma\right) \tag{23}$$

In the equation, C denotes the dummy for cheating, Z comprises worker and firm characteristics, and MW0 and MW1 stand for the minimum wage in 2006 and 2007. Base period MW earners are defined as those earning the exact amount of the minimum and those earning 95-105 per cent of the minimum, alternatively. The expectation is that $\beta > 0$.

We use fraud indicators defined on the *individual* level since the reform affected only the fake minimum wage earners within firms: by shifting these particular employees away from the MW the enterprise could reduce the risk of audit.

The cheating proxies in equation (7) come from the DH model hence they are predicted regressors and the estimation of their effect by simple maximum likelihood would not yield

[35] The reform was initiated by a high (close to 10 per cent) budget deficit in 2006, and might be regarded as a simple form of presumptive taxation. For a discussion of the idea of presumptive taxation, practices in Italy, and an application to Bulgaria see Jantscher and Casanegra (1987); Arachi and Santoro (2007); Pashev (2006), respectively.

valid results. Therefore, in calculating the standard errors in the equation we follow a two-step procedure. First, we simulate the parameter vector of the DH model from its asymptotic nor-mal distribution with its variance matrix, and create 100 simulated draws of firm-level cheat-ing variables from the models. Second, using the different cheater classifications, we estimate equation (7) by bootstrap and finally take the sample mean and standard deviation of all simu-lated parameters. This way, the cumulated parameter uncertainty of the two stages is quanti-fied – by using the asymptotic variance matrix in the first stage and direct bootstrap in the second. For simplicity, in the following text and tables we refer to this procedure as "two-step bootstrap". Note also that the resulting standard errors are only about 5 per cent larger than the ML standard errors of equation (7) because the error of the DH model, based on nearly 100 thousand observations, is negligible compared to the error of the test equation.

Table 2.4: The wages of year 2006 MW earners in 2007

	Earned the MW in 2006 and estimated to be	
Wage in 2007	non-cheater (per cent)	cheater (per cent)
MW	23.2	14.3
Between MW and 2MW	70.0	71.0
2MW	5.9	13.4
Above 2MW	0.9	1.3
Total	100.0	100.0

Source: Wage Survey, MW earners in the worker panel of 2006-2007, Number of observations 3,940

The descriptive statistics in Table II.4 yield preliminary support to our hypothesis: cheat-ing en-terprises were more likely to move their (apparent) low-wage workers away from the MW and shift them to 2MW than non-cheating firms. The MW earners (as of 2006) em-ployed by fraudulent firms were 40 per cent less likely to earn the MW in 2007, 2.3 times more likely to earn 2MW, and 2.2 times more likely to earn 2MW or more.

The results from equation (7) are presented in Table 2.5. As shown in the first row, base-period MW earners classified as cheaters (victims of cheating) were significantly more likely to earn 2MW in 2007 than non-cheating MW earners. The estimated marginal effect of being a cheat-er amounts to 2.4 per cent when all controls are included – a remarkable impact if we take into account that the probability of earning 2MW1 in 2007 conditional on earning MW0 in 2006 amounted to approximately 13 percent. In the second row of the table an alternative to equation (7) estimates the probability that a MW0 earner was shifted to or beyond 2MW1 i.e. the worker was moved out of the 'danger zone'. The partial effects are positive and significant but lower.

Table 2.5: The effect of estimated cheating behavior[a] on wage adjustment between May 2006 and May 2007

	Controls[b]						
	No		Education		All		# of obs
Model[c]	Partial eff.	Z-value[d]	Partial eff.	Z-value[d]	Partial eff.	Z-value[d]	
probit1	0.072	9.511***	0.049	6.263***	0.024	3.580***	3940
probit2	0.049	8.219***	0.026	4.262***	0.009	2.124**	22996

*** p<0.01, ** p<0.05, * p<0.1.
a) Individuals suspected of cheating in 2006 on the basis of the DH model
b) Controls (all variables relate to 2006): dummies for education (college graduate, secondary school and vocational school), work experience in years, dummies for gender, municipality, and the logarithm of firm size.
c) At probit1, the dependent variable is , at probit2 .
d) Based on two-step bootstrap standard errors, adjusted for clustering by firms
Data source: Wage survey, MW earners in the worker panel of 2006-2007

We may try to assess the magnitude of change induced by the 2007 reform and evaluate its economic significance in two ways. First, one can make back-on-the-envelope calculations relying on the results in Table II.4, and taking into consideration that the share of MW earners amounted to 47.9 per cent in cheating firms and 5.7 per cent in non-cheating ones. This im-plies that the reported wages of 6.4 per cent and 0.3 per cent of the employees were doubled in the two groups of firms, respectively.[36]. Holding other wages constant these pay rises implied 6.3 and 0.3 per cent increase in the average reported wages, respectively. Second, one may try to estimate the effect of cheating behavior on firm-level outcomes by

[36]Recall that cheaters shifted 13.4 per cent of their MW earners to 2MW while the respective share was only 5.9 per cent with non-cheaters.

estimating regressions of the form:

$$\Delta lnx = \beta C + \boldsymbol{Z\gamma} + \varepsilon \tag{24}$$

where lnx stands for the log changes in average wages, sales revenues and employment, alternatively, while C and Z denote the firm-level cheater dummy and the controls, respectively. The equations are estimated for 5230 firms observed in 2006 and 2007, and the standard errors are estimated with the two-step bootstrap procedure described earlier. In the wage equa-tion we expect $\beta > 0$ since raising the reported wages of grey employees must have increased the average reported wages of the cheating firms to some extent. The question of how actual costs and, therefore, output and employment were affected is more difficult to answer *a priori*. First, firms may have cut the cash payments of the affected workers, offsetting the impact of increased payroll taxes. Second, some of them may have increased the share of cash trans-actions in order to economize on VAT instead of payroll taxes.

Table 2.6: The effects of estimated cheating behavior[a] on the changes of selected firm-level indicators in 2006-2007

	Controls[b]	Partial effect	Z- value[c]	Number of obs.
Change of average wage (log)	No	0.1294	14.37***	5230
	Yes	0.1146	11.41***	
Change of employment (log)	No	0.0073	1.02	5230
	Yes	0.0048	0.79	
Change of sales revenues (log)	No	-0.0454	-3.21**	4824
	Yes	-0.0352	-2.33**	

*** p<0.01, ** p<0.05, * p<0.1.
a) Firms suspected of cheating in 2006 on the basis of the DH model
b) Controls include skill shares, average wage, average age and dummies for sectors, regions, type of municipality and state ownership.
c) Based on two-step bootstrap standard errors, adjusted for clustering by firms
Data source: panel of firms observed in the Wage Survey in 2006 and 2007.

The results presented in Table 2.6 suggest that the firm-level cheating proxy had positive effect on the change of observed average wages. Reported wages grew faster by 12 percentage points after controlling for industry, region, firm size, ownership and skill composition. The estimated gap between honest and dishonest firms is larger than the 6 percentage points dif-ference calculated beforehand. This may result from the effect of the reform on other reported wages, or from unobserved shocks, for which we can not effectively control with the firm-level variables at hand.

In either case, the budgetary effect of the reform seems modest. According to the DH esti-mates, approximately 170,000 workers, or 11 percent of the labor force represented by the Wage Survey, were employed by cheating firms. The average wages of these firms equaled 1.3 times the MW. Starting from these data and considering that the combined (em-ployer and employee) social security contribution rate was 49 per cent and the lowest personal income tax rate was 18 per cent, we can estimate that the excess increase of reported wages in fraudulent firms resulted in an extra revenue of 12 billion Ft, or about 0.05 per cent of GDP. If we accept the back-on-the-envelope calculations, the budgetary effect is proportionally lower (about 6 billion Ft).

The results indicate a significant negative effect on sales revenues and no effect on employ-ment. A possible interpretation of this result is that the 2007 reform directed cheating enter-prises to alternative forms of tax evasion and/or urged them to cut envelop wages.

The results presented in this section proved robust to changes in the definition of cheating and specification of the individual and firm level regressions. Weighting had practically no impact on the parameters. Using the exact amounts of the MWs rather than brackets around them left the qualitative results unchanged in the individual regressions. We also examined the sensitiv-ity of results to alternative cheating indicators based on the simulated wage ($w > MW$, $w > 1.1\,MW$, $w > 1.5MW\, and\, w > 2MW$). Since there was no significant deviation from the pre-sented results, the regressions using alternative indicators are not presented.

2.8 Conclusions

While grey employment and disguised MWs are widely debated issues in many emerging market economies, few attempts have been made to measure their magnitude and distribution. We applied a double hurdle model to this issue for Hungary in a period in which the presump-tions of the model seemed to fit i.e. censoring at the MW and wage under-reporting (at the MW) occurred simultaneously. If these preconditions are met, a properly specified DH model can estimate the 'genuine' wage distribution, permits the calculation of cheating probabilities and allows the simulation of 'true' earnings.

The DH results for 2006 suggest that employers paid cash supplement to around half of all minimum wage employees, and hinted at a wide (150 per cent) gap between reported and actual wages in these cases. The estimated distribution of under-reporting across occupations, industries and firm size seem to be consistent with the anecdotal evidence and survey-based results. The DH model makes strong assumptions about the wage distribution, and finding variables, which affect selection to cheating without affecting wages, is also rather difficult.

Driven by the resulting uncertainty of the estimates, we conducted an experiment aimed at testing if the DH estimates have predictive power. It seems that the estimates worked well in the quasi-experimental setting analyzed in the paper: firms and workers suspected of tax eva-sion responded differently to the strong shock under investigation.

We obviously make both type 1 and type 2 errors in disentangling cheaters from non-cheaters but the results are encouraging for the analysis of 'grey employment' and, we believe, they also have practical importance. On the one hand, audits may be targeted by statistical profiles derived from the DH model, thereby improving compliance. However, by showing the loci of under-reporting the DH estimates also draw attention to the limits of tax enforcement. Dis-guised minimum wages have high shares in services provided to households and small busi-nesses, freelance occupations, and small firm management – an attribute that limits the poten-tial budgetary intakes from more stringent inspection. Cash transactions between households and the providers of personal services are difficult, if not impossible, to detect.

Grey transac-tions of this kind can rather be whitened indirectly, by creating incentives to require receipts and making clear the link between reported income and access to publicly financed services and transfers such as pensions.

On the other hand, the DH results call for more cautious MW policies. The micro-data do not support the popular belief that in Hungary 'millions' are fraudulently paid the minimum wage – an assumption that served as a justification for regulations like the minimum contribution to be paid after 2MW. Reducing the under-reporting of wages by means of substantially increas-ing the MW and/or the tax burden on it is an undoubtedly cheap alternative to independent checks and carefully designed presumptive taxation. However, raising the costs of low-wage employment across the board is a poorly targeted policy, which can further reduce unskilled job opportunities: an undesirable outcome in a country, where six out of ten low-educated prime-age adults are out of work.

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Chapter 3

Frontloading the Unemployment Benefit: An Empirical Assessment

With Attila Lindner

Abstract[37]

In November 2005, the Hungarian government frontloaded the unemployment benefit path, while kept constant the total benefit amount that could be collected over the unemployment spell. We estimate the effect of this reform on non-employment duration using an interrupted time series design. We find that non-employment duration fell by 1.5 weeks after November 2005, while reemployment wages and the duration of new jobs remained the same. We show that the decrease in non-employment duration was large enough to make the benefit reform revenue neutral. Our welfare evaluation for this reform is positive: frontloading increased job finding, it made some of the unemployed better off, and did not cost anything to the taxpayers.

3.1 Introduction

Unemployment insurance programs aim to protect against financial distress at job loss and to maintain incentives to search for jobs. Unfortunately, these two goals are often in conflict: an insurance that provides better protection often leads to moral hazard and , as a result,

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to longer unemployment duration. This classic trade-off between insurance value and moral hazard determines the optimal level of the unemployment benefit (Baily, 1978; Chetty, 2008).

However, the classic analysis of optimal unemployment insurance (UI) assumes that the benefit is constant throughout the unemployment spell. Changing the benefit path, in principe, can maintain the insurance aspects of UI and can provide more incentives to search for a job at the same time. For instance, consider a change that frontloads the benefit profile by raising the unemployment benefit with $1 in the first period and by cutting it with $1 in the second period. Under this benefit change, the short-term unemployed can collect more benefits, while the long-term unemployed collect the same amount of benefit throughout their unemployment spell. Therefore, benefit frontloading makes none of the unemployed worse off and makes some off them better off.

The potential downside effect of such a policy change is that the total revenue of the UI system might increase. Such an increase in costs would eventually increase taxes and make taxpayers worse off. However, the effect of frontloading on government spending is ambiguous. On the one hand, the cost of UI increases mechanically as some of the unemployed collect more benefits. On the other hand, frontloading might speed up the transition to employment which leads to less benefit pay-outs and more tax revenues. In fact, this behavioral response can be large enough to fully offset the mechanical cost increase caused by benefit frontloading.

Therefore, the benefit frontloading described here can lead to a win-win situation where some of the unemployed are made better off without making any other actors worse off. However, it remains an empirical question whether the cost savings caused by the behavioral responses is large enough to offset the mechanical cost increase induced by the reform. This paper provides the first empirical assessment to answer this question. We exploit a unique Hungarian reform that changed radically the time profile of UI payments. The unemployed who claimed benefit before 1st of November 2005 could rely on a constant benefit for 270 days. However, those who claimed benefit after November 1st were eligible to the same benefit amount, but in a different structure: they had higher benefit in the first 90 days and then lower in the next 180 days. Putting it simply, the Hungarian UI reform frontloaded the benefit profile while the total benefit that could be collected remained the same.

We assess the effect of this unique policy change on non-employment duration using administrative data on UI claimants and social security contributions. Our main empirical strategy compares non-employment durations for those who claimed benefit before the UI change,and were, therefore, left with the old benefit schedule, to those who claimed afterwards. We implement an interrupted time series analysis and show that the average non-employment duration was stable preceding the reform, while there was a sharp drop in non-employment duration that coincides with the timing of the reform. We estimate that non-employment duration decreased by 10 days, or 1.5 weeks after the reform.

We also examine the effect of the benefit change on the quality of jobs found. We do not find any evidence for a change in reemployment wages or in the duration of new jobs. Therefore, our estimates suggest that the shortened unemployment duration did not lead workers to accept worse (or better) jobs.

We then we translate the estimated effects into changes in the UI budget (Table 3.6). The new benefit mechanically increased governmental spending, because short-term unemployed collected more benefits. However, it also fastened up job finding, which decreased spending on unemployment benefits. These effects offset around 50% of the mechanical cost increase. Another offsetting channel is the increase in personal income tax and social security contributions. This latter offset another 70% of the mechanical cost increases, and so the behavioral responses were large enough to counterbalance the mechanical cost increase caused by the reform.

Our estimates allow us to examine the welfare implications of the reform. The benefit frontloading made the short-term unemployed better off as they were able to collect more benefits after the reform. Moreover, long-term unemployed have collected the same amount benefit throughout the unemployment spelland, as a result, they were able to consume the same as before.[38] Therefore, no unemployed was made worse off by this reform, and many of them was made better off.

Our estimates also imply that the burden on taxpayers did not increase after the reform. This is because the extra benefit collected by the unemployed was offset by the benefit savings and extra taxes paid as a result of the shorter unemployment spells. Moreover, the unemployed did not accept lower paying or less stable jobs. Therefore, the evidence presented here shows that benefit frontloading was a win-win policy: both the unemployed and the employed were made better off byreceiving more generous unemployment benefit schedule but in a structure that reduced moral hazard. Therefore, the Hungarian UI reform was a Pareto improving policy change.

The key assumption behind our empirical strategy is that there were no other policy or economic changes that could explain the sharp drop in non-employment duration after the reform The aggregate unemployment rate was stable in this period and the composition of the unemployed who claimed benefit was similar before and after the reform suggesting that economic changes cannot explain the change in non-employment duration. The only important policy change that could affect our results is a voulantary reemployment bonus scheme (RB), which was introduced parallel with the benefit reform.

To separate the effect of the reemployment bonus scheme from benefit frontloading, we exploit the local variation in knowledge about the availability of the new bonus scheme similarly to Chetty et al. (2013). While UI offices provided clear and straightforward information to all newly unemployed about the level and the timing of their benefit, the availability ofthe reemployment bonus scheme was less salient. Moreover, the reemployment bonus scheme was quite complicated and it was also associated with substantial hassle costs. Therefore, the role of local UI offices was crucial to advocate the scheme.

We infer the unemployed access to information from the average bonus take-up rate at

[38] Unemployed in the new system can replicate the old consumption profile by saving some of the extra dollars they got at the beginning of their unemployment spell. However, even if the unemployed can not save, they are better off as long as the pre-reform benefit was constant throughout the unemployment spell. Moreover, it is easy to show that hand-to-mouth unemployed are also better off in that case.

the local UI office where the benefit was claimed. There are a large and persistent differences in take-up rates across UI offices that are not related to observable characteristics of the unemployed. In some locations the take-up rate was close to zero, while in others it went above 10%. We show that the size of the drop in non-employment duration after the reform was very similar in zero or very low take-up and high take-up locations. This suggest that access to information on the voluntary RB scheme is unlikely to have had any significant effect on non-employment duration.

This paper is related to the literature on estimating moral hazard implications of unemployment insurance. Numerous studies scrutinized the effect of changing the benefit level (e.g. Meyer 1990; Lalive et al. 2006; Landais 2015; Card et al. 007a) and most papers found that there is a considerable effect of unemployment benefits on job search behavior (see a survey of this literature by Krueger and Meyer 2002; Chetty and Finkelstein 2013). Other aspects of unemployment insurance systems have been examined, such as reemployment bonuses (Van der Klaauw and Van Ours, 2013) and enforcement (Van den Berg and Van der Klaauw, 2006; Cockx and Picchio, 2013). However, the empirical evidence on the effect of changing the benefit path is surprisingly limited. A notable exception is Kolsrud et al. (2015), who empirically estimate the moral hazard costs of unemployment benefits paid at different times during the unemployment spell. They find that the unemployed respond more to benefit changes at the beginning of the UI spell than towards the end. Our results imply the opposite: the effect of increasing the benefit at the beginning has a smaller effect than the decrease later on. One possible explanation for this discrepancy is that the reform in Hungary is more radical and more salient than the one analyzed in Kolsrud et al. (2015) In our setup, therefore, the unemployed are more likely to be aware of future drops in their benefits and so they will respond more to them.

Our results also contribute to the extensive theoretical literature on the optimal time profile of unemployment insurance (e.g. Shavell and Weiss 1979; Hopenhayn and Nicolini 1997; Cahuc and Lehmann 2000; Werning 2002; Shimer and Werning 2008). These papers derive the fully optimal UI profile but they need to make strong assumptions about the environment in which the unemployed make their decisions (e.g. borrowing constraints). It turns out that the optimal UI profile is very sensitive to these assumptions (Hopenhayn and Nicolini, 1997; Werning, 2002). Moreover, the fully optimal benefit schedule is often quite complicated and hard to implement. Therefore, instead of searching for the fully optimal UI benefit schedule, we look at the welfare implication of an easily implementable reform that moves away from the standard constant benefit schedule to a frontloaded one. Our approach will not come up with the first-best benefit profile, but may help to inform policy makers as to which direction they should deviate in order to find it.

We also contribute to the effect of unemployment insurance on job quality. Recent research finds mixed results on the UI wage effect (Schmieder et al., 2013; Nekoei and Weber, 2015). Similarly to Lalive (2007) and Van Ours and Vodopivec (2008) we do not find a significant relationship between the length of unemployment and reemployment wages.

The paper is set out as follows. Section 2 describes the data and institutional details of our unemployment insurance reform. Section 3 presents the empirical results. In Section 4 we use our empirical estimates to assess the welfare implications of reform. Section 5 concludes.

3.2 Institutional Background and Data

3.2.1 The Benefit Reform in Hungary

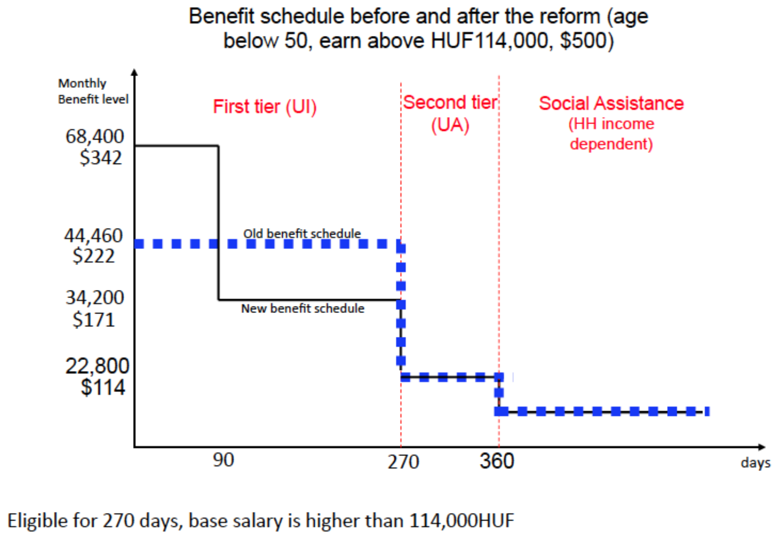
Hungary had a two-tier unemployment insurance system around 2005. In the first tier the unemployment benefit depended on the length and amount of contributions[39]. After exhausting the first tier, the unemployed were eligible for unemployment assistance. The amount of the benefit in the second tier was the same for all unemployed and the length of it depended on the age of the UI claimants. After both tiers were exhausted, the unemployed were eligible

[39] The length of eligibility was the number of working days in the last four years divided by 5 and it was capped at 270 days. The amount of the benefit was based on the average monthly taxable income in the last year before unemployment and it was also capped.

for welfare. However, welfare payments, unlike the UI benefit, depended on family income and it were lower than the unemployment benefit.

The UI reform in 2005 changed the benefit schedule dramatically in the first tier for those who claimed benefit after November 1st, 2005, while it kept unaffected the length of unemployment benefit. In our analysis we concentrate on the unemployed who experienced a frontloaded benefit as a result of the reform. These unemployed are individuals who worked more or less uninterrupted in the preceding four years of their job loss and whose earnings base was above HUF108,000 ($504) in 2005 (around the 70th percentile of UI claimants). Figure 3.1 summarizes the benefit path for this group before and after the reform. Unemployed individuals who claimed benefit before November 1st were eligible for HUF44,460 ($222) for the first 270 days. As opposed to this, those who claimed benefit after November 1st got HUF68,400 ($342) in the first 90 days and HUF34,200 ($171) in the next 180 days. An important feature of the reform was that the total benefit that could be received throughout the unemployment spell remained approximately the same and only the timing of the benefit payouts changed.

Figure 3.1: Benefit Schedule Before and After the Reform



The figure shows the benefit schedule if UI is claimed on October 31, 2005 (old benefit schedule, dashed blue line) and the benefit schedule if UI is claimed on November 1st, 2005 (new benefit schedule, solid red line) for individuals who had 270 potential durations in the first-tier, were less than 50 years old and earned more than 114,000 HUF ($570) prior to entering the UI scheme. The hypothetical benefit level is shown under social assistance. Benefit levels of social assistance depended on family income, household size and wealth and we do not observe these variables in our data.

Newly unemployed individuals who wished to collect unemployment benefits had to go the local UI office and attend a 30-minute session which explained their rights and obligations as a claimant. Then each individual received a personalized letter which characterized their benefit schedule in the first tier. Figure A.5 shows an example of the first page of such a letter for an unemployed individual who claimed benefit under the new rules. The benefits are highlighted in the table in the middle of the page, wherethe length of the disbursement period in days and the daily amount are shown. It is obvious that the benefit schedule was salient from the beginning of the unemployment spell.

There were two other changes that were implemented in 2005. First, unemployment assistance (UA- the second tier) was shortened from 180 days to 90 for those who claimed benefit after February 5th, 2005. Second, the government introduced a voluntary reemployment bonus (RB) scheme in parallel with the benefit reform. Under this new scheme, the unemployed who claimed benefit after November 1st, 2005 and found a job in the first 270 days could claim 50 percent of the remaining unemployment benefit as a lump sum. The take-up rate of the RB scheme was very low as only 6 percent of the unemployed took advantage of this new scheme. Claiming UI benefit had two important drawbacks. First, the default option was not to take up RB and if someone decided to make use of it, she had to go through a complicated administrative process[40]. Second, claiming RB also meant that the remaining benefit eligibility was lost. Therefore, RB claimants had to start to collect benefit eligibility from zero again, and this may have seemed a risky step to take for many newly employed worker on probation. In Section 3 we do a couple of robustness checks to show that the changes in non-employment durations were unlikely to be driven by the shorter UA benefits in the second tier or by the voluntary RB scheme.

Finally, it is worth highlighting that the economy was growing at around 3-4% before the reform and a somewhat lower level afterwards (see Figure A.6 Panel a). Nevertheless, aggregate labor market conditions were not affected by the lower performance of the economy and aggregate unemployment was stable around the period of our analysis (see Figure A.6 Panel b).[41]

3.2.2 Database and sample definition

We observe a 50 percent random sample of the unemployed registered at the Hungarian National Employment Service between January 2004 and 2008[42]. During this time period we have information on the amount to which one is eligible and the starting and ending date of unemployment benefit spells. We also observe employment history and the earnings from social security contributions between 2002 and 2008.

We restrict attention to prime age workers (25-49 years) who had 270 days benefit eligi-

[40]RB could only be claimed in person at the local unemployment office when 270 days elapsed after the benefit claim. Moreover, the employment status had to be continuous between the reemployment and the RB claim.

[41]The lower GDP growth rate would predict that non-employment duration is higher after the reform. However, in Section 3 we show that the average length of employment was in fact lower after the reform. Therefore, if the change in GDP had some effect on our results, then we are likely to underestimate the "true" effect of the reform.

[42]The sample includes individuals who were born every second day after January 1st, 1927.

bility. To analyze the effect of the reform, we compare the average length of benefit duration before and after the reform. As figure Figure 3.1 shows, the before group consists of the unemployed who claimed benefit between 15th November 2004 and 15th October 2005. We leave out workers who claimed benefit around 1st of November to make sure we do not include in the analysis workers who postponed their benefit claims in order to get into the new system. In any case, the number of claimants around November 1st is not unusual relative to previous years, which indicates that most of the unemployed did not manipulate their claiming date because of the reform.

The after group is made up by the unemployed who claimed benefit between November 15th, 2005 and October 15th, 2006. By using this sample definition, the before and the after group consists ofthe same months of the year , so seasonality does not confound our results.

The basic descriptive statistics are shown in Table 3.1. We observe approximately 7500 unemployed both before and after the reform.The observable characteristics of the two groups are very similar. The share of women and the average year of education is slightly larger in the after sample but the average income before unemployment was the same in both groups. The average time spent between job loss and benefit claim was 31 days both before and after the reform, which indicates that people who lost their jobs before the reform did not postpone their benefit claim to become eligible for the new benefit schedule. Finally, less than 6 percent of the unemployed claimed reemployment bonus.

Table 3.1: Descriptive Statistics: Comparing Means of Main Variables Pre- and Post UI Reform

	before	after	diff	t-stat
Percent Women	40.37%	44.70%	4.33%	5.43
	(0.55%)	(0.57%)		
Age in Years	36.82	36.89	0.07	0.60
	(0.08)	(0.08)		
Imputed Education (years)	11.87	12.04	0.17	4.70
(based on occupation in the last job)	(0.02)	(0.02)		
Log earnings in 2002	11.08	11.12	0.04	1.04
	(0.02)	(0.02)		
Log earnings in 2003	11.29	11.31	0.02	0.48
	(0.02)	(0.02)		
Waiting period*	31.24	31.56	0.32	0.52
	(0.42)	(0.44)		
Reemployment bonus claimed	0.00%	5.91%	0.06	21.67
	(0%)	(0.27%)		
Number of observations**	7,879	7,476		

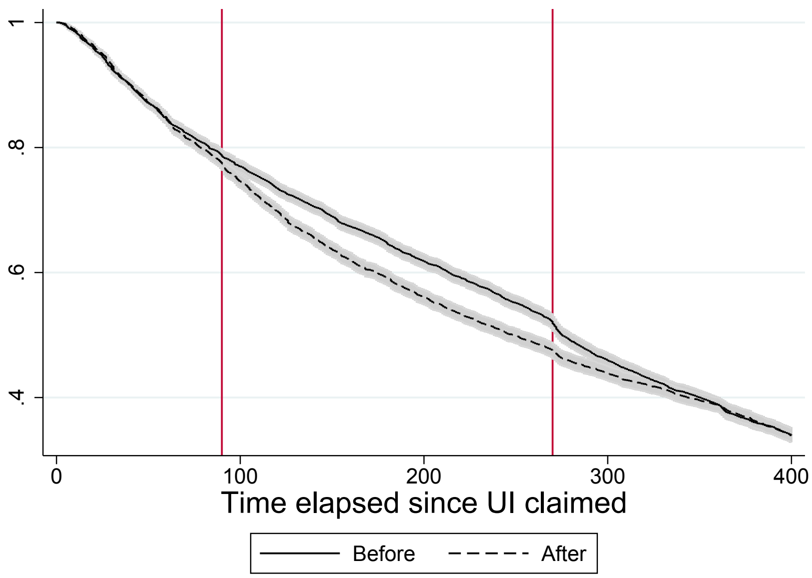
* number of days between job loss and UI claim

**there are some missing values for log earnings in 2002, 2003, 2004.

3.3 Results

In this section we evaluate the impact of the reform on non-employment duration and on the quality of jobs found. Figure 3.2 shows the Kaplan-Meier survivor rate for those who claimed benefit before (between November 15th, 2004 and October 15th, 2005) and after the reform (between November 15th, 2005 and October 15th, 2006). In the first 90 days, the two job survivor functions are very similar. After 3 months the job finding rate of the after group rises compared to the before group. As a result, a significantly higher share of workers finds a job during the first 270 days after the reform than before the reform.

Figure 3.2: Kaplan-Meier Survival Rates Before and After the Reform



The figure shows the Kaplan-Meier survivor rates of the unemployed before and after the reform. The vertical red line shows the drop in the benefits after the reform at 90 and 270 days. The shaded area shows the confidence intervals of the survivor estimates.

To estimate the effect of the reform on the length of unemployment, we estimate the following regression:

$$NonEmpDur_i = \alpha + \beta after_i + \gamma X_i + \varepsilon_i \tag{25}$$

where the dependent variable shows the time elapsed between benefit claim and re-employment. We cap the length of unemployment at 270 days because the reform affected the benefit eligibility only in the first 270 days. However, capping at a higher level does not substantially change the results. The main variable of interest is the $after_i$ dummy which indicates whether the unemployed individual claimed benefit after the reform. X_i denotes the control variables that include age, age square, years of education and its square, log income in 2002, log income in 2003, sex, dummies that control for the day of the month the benefit was claimed, one digit occupation and location dummies.

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Table 3.2 summarizes the main findings of the paper. According to Column 1, the length of non-employment decreased by 10.46 (s.e. 2.11) days after the reform. In Column 2 we take into account the fact that the characteristics of UI claimants differ slightly before and after the reform. The results show that the decline in duration is even bigger now: 11.28 (s.e. 2.10) days. In Column 3 we also control for the location where the benefit was claimed. The estimated effect of the reform is 12.18 (s.e. 2.90) in that case.

As robustness check for the functional form we also estimate the effect of the reform using the Cox proportional hazard model:

$$h_d = \delta_d exp(\lambda after + \kappa X) \tag{26}$$

where h_d denotes the re-employment hazard d days after the benefit has been claimed, δ_d is an unrestricted day effect (baseline hazard), and the control variables, X, are the same as in equation 25. The Cox hazard model shows similar effects. According to the right panel of Table 3.2, the reemployment hazard increased with 4-6 percent after the reform and the inclusion of control variables do not significantly alter the point estimates.

Table 3.2: Baseline results: Effect of the Reform on Non-Employment Duration

	Non-employment duration (OLS)			Reemployment hazards (Cox-estimation)		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
After	-10.46***	-11.28***	-12.18***	0.043**	0.057***	0.064***
	(2.11)	(2.09)	(2.29)	(0.020)	(0.021)	(0.025)
Controls	no	yes	yes	no	yes	yes
Location FE	no	no	yes	no	no	yes
Observations	15,009	15,009	15,009	15,009	15,009	15,009
R-squared	0.002	0.042	0.063			

*** p<0.01, ** p<0.05, * p<0.1

Note: This table shows the effect of the reform on non-employment duration. Column 1-3 estimate regression in equation 25. Column 4-6 estimate the Cox proportional hazard in equation 26. The non-employment duration is capped at 270 days in all columns. After is a dummy, which is 1 if the unemployed individual claimed benefit after the benefit reform (between November 15th, 2005 and October 15th, 2006). The control variables are sex, age, age square, waiting period (the number of days between job lost and UI claimed), the county of residence, day of the month UI claimed , education, occupation (1 digit) in the last job, log earnings in 2002 and 2003. The location fixed effects control for the local UI office where the unemployed individual claimed benefit. Standard errors in parentheses are clustered at the local UI office level.

Our estimates indicate that after the reform, non-employment duration was lower by 10-12 days. Figure 3.3 panel (a) plots the average length of non-employment by six month periods relative to the benefit change. The gap shows that non-employment duration was around 197 days in the preceding 6 month periods and that has been dropped to 187 days immediately after the reform. In Figure 3.3 panel (b) we show the average non-employment duration after controlling for observables and location fixed effects. Again the the change in non-employment duration is very much coincided with the implementation of the new benefit schedule. The figures also highlight that the average length of non-employment was very similar in the last 18 month before the reform. Therefore, the change in the second tier after February 5th, 2005 had at most a small effect on non-employment duration. Given that the benefit level in the second tier is quite low (HUF22, 800 or $114 per month) this is not surprising.

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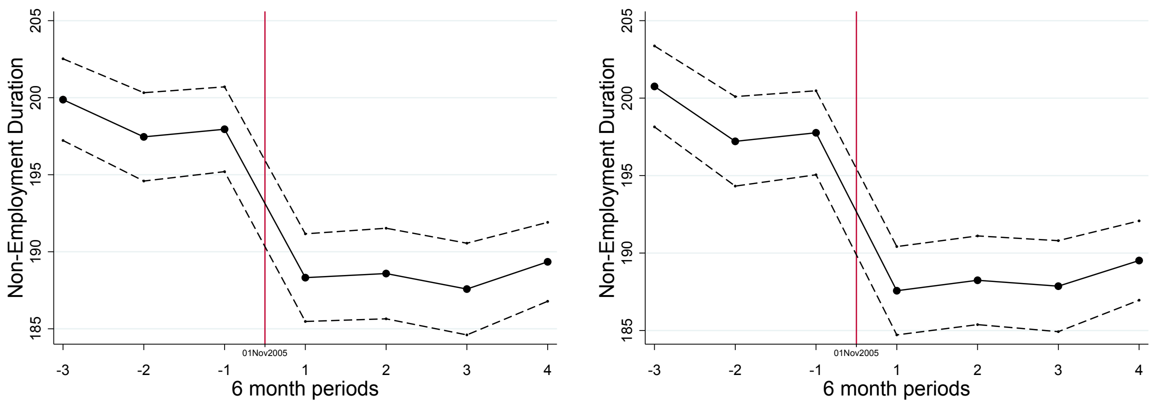
Figure 3.3: Baseline Results: Non-Employment Duration by 6-month Periods Relative to the Reform

(a) No controls (b) With controls



The figure shows the seasonally adjusted average length of unemployment spells by 6-month periods. Panel (a) shows the unconditional averages while Panel (b) controls for sex, age, age square, waiting period (the number of days between job lost and UI claimed), the county of residence, day of the month UI claimed, education, occupation (1 digit) of the last job, and log earnings in 2002 and 2003.The figure highlights that the average length of non-employment duration dropped immediately after the reform. The vertical red line show the timing of the benefit frontloading. .

Did the faster reemployment hurt job quality? To answer this question we analyze other outcomes besides the non-employment. duration. For example worker may accept a less stable job after the reform to exit unemployment earlier *(Jarosch, 2014)*. In Table 3.3 Column (1) to (3) we estimate equation 25, where the outcome variable is the tenure at the new job. All columns show a negative effect on job tenure, but the estimated effects (e.g. less than 1,5 days in Column 3) are negligible in statistical and economic sense. The lack of effect on job tenure at the new job has been also confirmed in Figure 3.4 where we plot the average tenure by six month periods relative to the benefit change (in Panel a without controls in Panel b with controlling for observables and UI location fixed effects).

Table 3.3: Job Quality: Effect of the Reform on Job Tenure in the New Job

	Average tenure in days (OLS)			Separation hazards (Cox-estimation)		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
After	-0.58	-1.03	-1.03	0.003	0.039	0.037
	(1.78)	(1.79)	(2.00)	(0.036)	(0.037)	(0.042)
Controls	no	yes	yes	no	yes	yes
Location FE	no	no	yes	no	no	yes
Observations	9,181	9,181	9,181	9,181	9,181	9,181
R-squared	0.000	0.017	0.045			

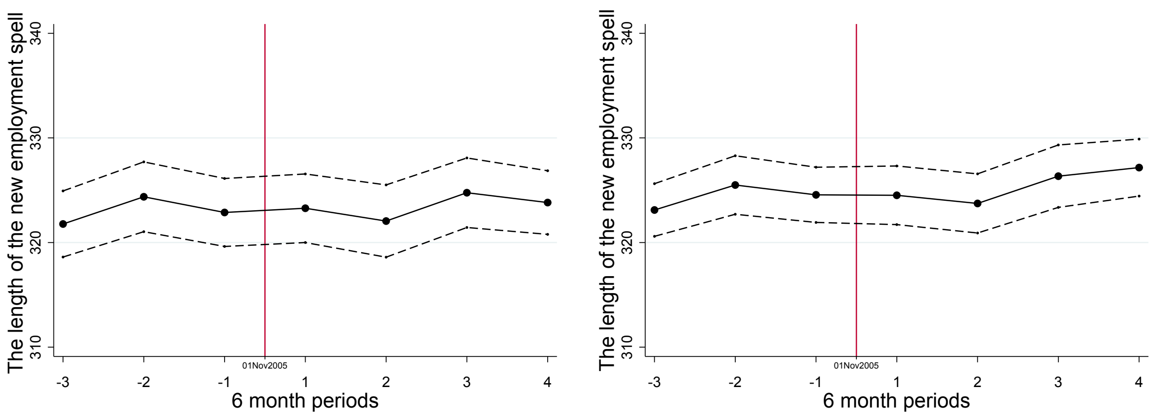
*** p<0.01, ** p<0.05, * p<0.1

Note: This table shows the effect of the reform on the duration of the new job (measured in days). Column 1-3 estimate regression in equation 25 and Column 4-6 estimate the regression in 26 using the job tenure upon reemployment. Only workers who found a job within 360 days are included in the sample. The tenure is capped at 360 days in all columns. After is a dummy, which is 1 if the unemployed individual claimed benefit after the benefit reform (between November 15th, 2005 and October 15th, 2006). The control variables are sex, age, age square, waiting period (the number of days between job lost and UI claimed), the county of residence, day of the month UI claimed , education, occupation (1 digit) in the last job, log earnings in 2002 and 2003. The location fixed effects control for the local UI office where the unemployed individual claimed benefit. Standard errors in parentheses are clustered at the local UI office level..

Figure 3.4: Job Quality: Job Tenure at the New Job Before and After the Reform

(a) No controls

(b) With controls



The figure shows the average length of the new employment spells by 6-month periods. The length of employment is capped at 360 days and only workers who found a job within 360 days are included in the sample. Panel (a) shows the unconditional averages while Panel (b) controls for sex, age, age square, waiting period (the number of days between job lost and UI claimed), the county of residence, day of the month UI claimed, education, occupation (1 digit) in the last job, and log earnings in 2002 and 2003. The vertical red line shows the timing of the benefit frontloading. The figure highlights that thelength of the new employment spells did not change after the reform.

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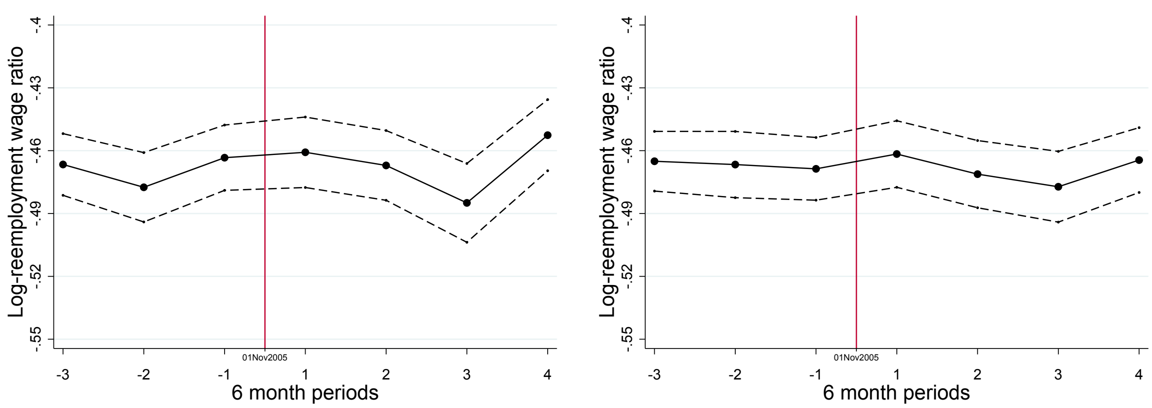
Figure 3.5 shows that frontloading did not affect the reemployment wages either[43]. We plot the log-ratio of the reemployment wage and the unemployment benefit base wage by six month periods.[44] We control for a linear time trend to rule out the effect of the inflation and economic growth. The figure shows that the average reemployment monthly wage is 46-48 log-point lower[45] As the unemployment benefit base wage calculated based on the average earnings in the last four years, this measure overestimates the income loss after unemployment (Card et al., 007a; Schmieder et al., 2013). In any case, Figure 3.5 highlights that reemployment wages are not affected around the time of the unemployment benefit reform.

Figure 3.5: Job Quality: Reemployment Wages Before and After the Reform

(a) No controls (b) With controls



The figure shows the log ratio of reemployment wage and the benefit base by 6-month periods. Panel (a) shows the unconditional averages while Panel (b) controls for sex, age, age square, waiting period (the number of days between job lost and UI claimed), the county of residence, day of the month UI claimed, education, occupation (1 digit) in the last job, and log earnings in 2002 and 2003. Both regressions include linear time trends and only workers who found a job within 360 days are included in the sample. The vertical red line show the timing of the benefit frontloading. The figure highlights that reemployment wages did not change after the reform.

[43]We calculate the daily reemployment wage from the social security data by dividing the monthly earnings by the number of days worked in that month.

[44]The unemployment benefit base wage was calculated by the unemployment insurance office based on the average (daily) wage in the last four years. The unemployment benefit base wage was not affected by severance payment, which was 1 to 6 months' salary depending on the tenure. The average daily wage calculated from the social security data also include severance payments. This means that the log-ratio of the reemployment wage and the wage in the last job overestimates the true wage loss for those who received severance payments.

[45]This difference is equivalent to a 37 percent decrease.

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Table 3.4: Job Quality: Effect of the Reform on Reemployment Wages

	Log - reemployment wage			Log(reemployment wage/last wage)			Log(reemployment wage/wage in 2002)		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
After	0.016	0.027	0.027	-0.0003	0.0027	0.002	0.023	0.022	0.024
	(0.021)	(0.019)	(0.017)	(0.021)	(0.020)	(0.019)	(0.024)	(0.020)	(0.017)
Controls	no	yes	yes	no	yes	yes	no	yes	yes
Location FE	no	no	yes	no	no	yes	no	no	yes
Observations	9,118	9,118	9,118	9,118	9,118	9,118	8,644	8,644	8,644
R-squared	0.002	0.198	0.234	0.001	0.063	0.086	0.003	0.311	0.343

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Note: This table shows the effect of the reform on reemployment wages. All Columns estimate equation 25 using various version of reemployment wages as an outcome variables (see text for the details):Column 1 to3 use the the log-reemployment wage, Column 4 to6 use the log-ratio of reemployment wage and the unemployment benefit base and Column 7 to9 use the ratio of reemployment wage and average wage in 2002. We deflate wages with the nominal GDP growth and we control for a linear time trend in all regressions. Only workers who found a job in 360 days included in the sample. In Column 7 to 9 we only use workers with non-missing information on the average wage in 2002. The after dummy is 1 if the unemployed individual claimed benefit after the benefit reform. The control variables are sex, age, age square, waiting period (the number of days between job lost and UI claimed), the county of residence, day of the month UI claimed, education, occupation (1 digit) in the last job, log earnings in 2002 and 2003. The location fixed effects control for the local UI office where the unemployed claimed benefit.Standard errors in parentheses are clustered at the local UI office level.

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Table 3.4 reports the point estimates for the log-ratio of the reemployment wage and the base wage. According to Column 1, the reemployment wage ratio was 1.6 (s.e. 2.1) percentage point larger after the reform, but the raise was not significant. The effect on reemployment wage is slightly higher, 2.7 (s.e. 1.7) once we control for observable characteristics of the unemployed and the location fixed effects. While these point estimates are significant in economic terms, we should be cautious in drawing strong conclusions. First, none of these estimates are statistically significant at the conventional levels. Moreover, as it has been shown in Figure 3.5, the timing of the increase in reemployment wages does not perfectly align with the implementation of the reform.

In Table 3.4 Column 4-9 we also explore alternative definitions of reemployment wages. Results with log-ratio of the reemployment wage and the wage in the last job are shown in Column 4-6. The results are slightly different relative to the results in Column 1-3 as the point estimates are near zero here. However, the wage in the last job is also is affected by severance payments, and so these estimate might be biased. Therefore, in Columns (7) to (9) we show the results for log-ratio of reemployment wage and the average wage in 2002. The point estimate is again around 2 percentage points and statistically insignificant.

Overall, these results suggest that the effect on reemployment wages might be positive or zero, but it is unlikely to be negative. Therefore, we find no evidence that the reform hurt job quality.

Can reemployment bonus explain the decrease in non-employment duration? As we discussed in the previous section, those who claimed benefit after November 1st, 2005 was not only faced with the frontloaded benefit schedule but were also eligible to claim voluntary reemployment bonus if they found a job within 270 days. The reemployment bonus was associated with substantial hassle costs and it was a less salient policy than the benefit frontloading. Still, it is possible that the parallel introduction of the reemployment bonus explains part of the decline in non-employment duration. To separate the effect of benefit frontloading from the reemployment bonus, we exploit the anecdotal evidence that at some local UI offices the reemployment bonus was advertised more by UI officials than at other ones. While we do not observe directly which UI offices have been more keen on advocating the reemployment bonus scheme, we use the local level take-up rate of the reemployment bonus as a proxy for information provided to the unemployed.

Two empirical observations motivate that the take-up rate is related to access to information and not to other factors. First, Figure 3.6 panel (b) shows a scatter plot between the take-up rate one year after the reform and the take-up rate 2 years after. The figure uncovers a strong correlation (0.64) between take-up rate in the two periods. Therefore, the take-up rate differences across locations are persistent. Second, and more importantly, Figure 3.7 shows scatter plots between different measures of the composition of the unemployed and the take-up rate by UI locations. Panel (a) measures the composition of the unemployed by the average *pre-reform* non-employment duration. We use the *pre-reform* non-employment duration and not the *post-reform* one, because the post-reform does not just measure the composition of the unemployed but the effect of the reemployment bonus as well.[46] Figure 3.7 Panel (b) measures the composition of unemployed by the predicted non-employment duration for those who claim benefit after the reform. To get the predicted values we run a regression of non-employment duration on observable characteristics (age, age square, years of education and its square, log income in 2002, log income in 2003, sex, dummies that control for the day UI claimed, one digit occupation) in the pre-reform sample and predict the average non-employment duration for the post-reform.

[46] The measure of pre-reform non-employment duration is a good proxy of the post-reform composition of the unemployed if the composition is stable over time. The correlation in non-employment duration between 1 year before and 2 years before the reform is 0.31. Moreover, with all the caveats of using *post-reform* non-employment duration to measure the composition of the unemployed, it is worth highlighting that there is no relationship between non-employment duration and reemployment bonus take-up rate in the post-reform sample (results available on request).

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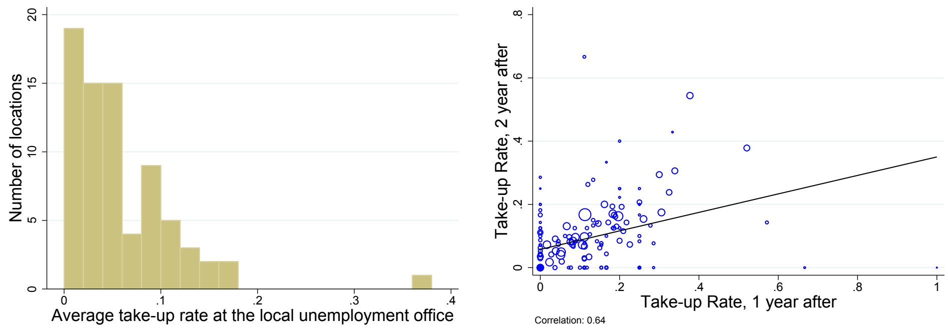
Figure 3.6: Take-up Rate of Reemployment Bonus

(a) Frequency distribution of the take-up rate across locations

(b) Relationship between take-up rate 1 year and 2 years after the reform

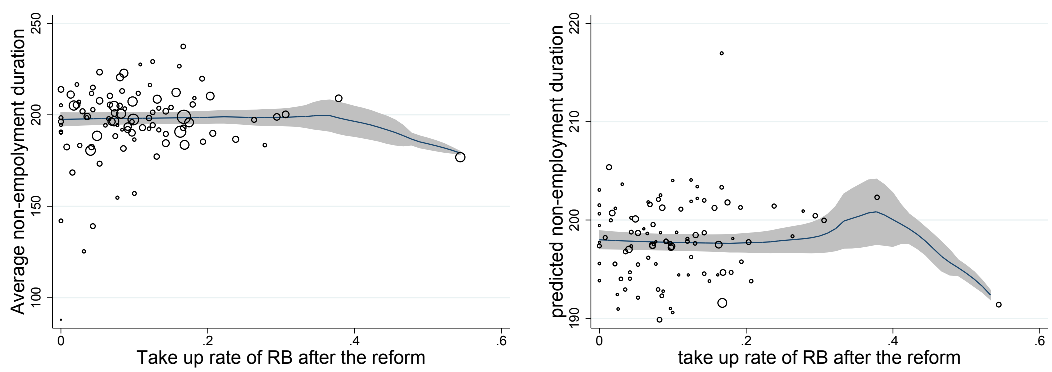


Panel (a) shows the frequency distribution of local UI take-up rates . Panel (b) shows the take-up rate of reemployment bonus at local unemployment offices one year and two year after the reform. The graph highlights that the local take-up rate is persistent over time. In both panels only UI offices with at least 30 UI claimants were used.

Figure 3.7: Relationship between the Composition of UI Claimants and the Take-up Rate accross Locations

(a) Non-employment duration before the reform

(b) Predicted non-employment duration after the reform



The figure plots the relationship between the composition of UI claimnts and the take-up rate of reemployment bonus after the reform at all UI locations. Panel (a) measures the composition of UI claimants with the average non-employment duration before the reform while panel (b) measures the composition of the unemployed by the predicted non-employment duration for those who claimed benefit after the reform. To get these predicted values we run a regression of non-employment duration on observable characteristics in the pre-reform sample and predict the average non-employment duration for the post-reform. The blue line shows the local polynomial fit weighted by the number of benefit claims before the reform. In both panels only UI offices with at least 30 UI claimants were used. The figure shows that the reemployment bonus take-up is uncorrelated with the length of non-employment before the reform.

Both Panel (a) and Panel (b) in Figure 3.7 depicts the Kernel-weighted local polynomial smoothing to show the non-parametric relationship between composition and take-up rate. In both panels we see no relationship between these two variables if we abstract away from the few outliers with very high take-up rates. This indicates that the reemployment bonus take-up rate is persistently higher at some locations and the differences are not related to the composition of the unemployed. This empirical pattern across UI locations is what we would expect to emerge if the take-up rate was determined by the behavior of local UI officers and not some underlying economic factors.

The effect of reemployment bonus on non-employment duration is likely to vary by the access to information on the scheme. Similarly to Chetty et al. (2013), the variation in access to information across locations can be used to better understand how reemployment bonus affects our baseline results. To do that, we compare low take-up rate (limited information) and high take-up rate (more information) locations that experienced differences in non-employment duration. In particular, we estimate the following regression:

$$unemployment_i = \beta_1 + \beta_2 after_i + \beta_3 high_i + \beta_4 high_i * after_i + \gamma X_i + \varepsilon_i \qquad (27)$$

where the dummy variable $high_i$ takes the value of 1 if the location is in the top quarter (take-up rate is higher than 16.2%) and 0 if the location is in the lowest quartile (take-up rate is lower than 4.9%) with respect to the reemployment bonus take-up rate. While this is a common difference-in-difference type regression, our main parameter of interest is not β_4, namely the effect of reemployment bonus on non-employment duration, but β_1, the effect of the reform on non-employment duration at locations with close to zero take-up rate and limited information access.

Table 3.5 Column (1) to (4) summarizes the estimation results. In Column (1) and (3) we saw the baseline results for the sample that includes the lowest and highest quartile locations with low reemployment bonus take-up rate. The point estimates are slightly lower here than in the baseline Table 3.2 (-8.65 vs. -10.46 in the specification with no control and -12.18 vs. -10.70 in the specification with control and location FEs) and the differences are not statistically significant. In Column (2) and (4) we show the results on the same sample but estimating equation 27. The results show that the effect of the after dummy is virtually unaffected by controlling for high take-up and its interaction with the after dummy. Moreover, the effect of the interaction term is very small and always insignificant. This indicates that the effect of the reform does not vary by the reemployment bonus take-up rate.

Table 3.5: The effect of Frontloading by the Reemployment Bonus Take-up Rate

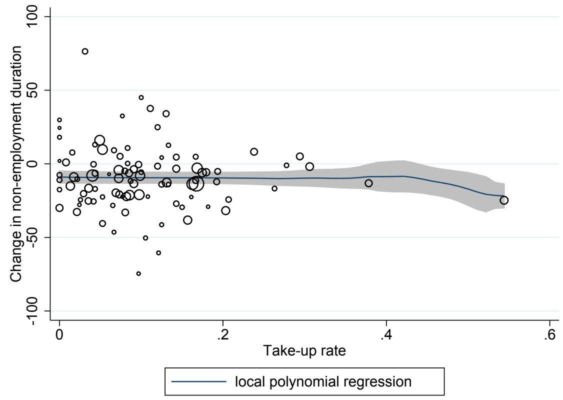
	Non-employment duration (OLS)				Reemployment hazards (Cox-estimation)			
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
after	-8.65***	-8.20***	-10.70***	-10.01***	0.119***	0.107**	0.151***	0.129***
	(1.85)	(2.74)	(1.88)	(2.77)	(0.033)	(0.047)	(0.030)	(0.042)
high take-up		2.30				-0.032		
		(4.30)				(0.047)		
high take-up*after		-0.85		-1.36		0.023		0.044
		(3.70)		(3.70)		(0.066)		(0.058)
controls	no	no	yes	yes	no	no	yes	yes
location FE	no	no	yes	yes	no	no	yes	yes
Observations	7,217	7,217	7,217	7,217	7,217	7,217	7,217	7,217
R-squared	0.002	0.002	0.064	0.064				

*** p<0.01, ** p<0.05, * p<0.1

Note: This table shows the effect of the reform on non-employment duration by the local reemployment bonus take-up rate. The sample in all columns includes unemployed who claim benefit in the UI locations with the lowest quartile take-up rate and in the UI locations with the highest quartile take-up rate. Column 1, 3, 5 and 7 show the baseline results for this partiuclar sample. Column 2 and 4 estimate equation 27 and Column 6 and 8 the estimate a Cox proportinal hazard model. The length of non-employment is capped at 270 days in all Columns. The after dummy is 1 if the unemployed claimed benefit after the benefit reform. The high take-up is a dummy denoting that the unemployed claimed benefit at a location withhighest quartile reemployment bonus take up The control variables are sex, age, age square, waiting period (the number of days between job lost and UI claimed), the county of residence, day of the month UI claimed, education, occupation (1 digit) in the last job, log earnings in 2002 and 2003. The location fixed effects control for the local UI office where the unemployed claimed benefit. Standard errors in parentheses are clustered at the local UI office level.

In Figure 3.8 we plot the relationship between the before-after change in non-employment duration and take-up rate across locations. We also plot the Kernel-weighted local polynomial smoothing to show the non-parametric relationship between these two variables. The figure supports our regression results in Table 3.5: there is no relationship between the effect of the reform on non-employment duration and the take-up rate.

Figure 3.8: The Effect of the Reform by the Take-up Rate of the Reemployment Bonus



The figure plots the relationship between the before-after change in the average non-employment duration at the local UI office and the reemployment bonus take-up rate. The blue line shows the local polynomial fit weighted by the number of benefit claims before the reform. The figure shows no relationhsip between the change in non-employment duration and the reemployment bonus take-up rate.

As a robustness check, we report the estimates using Cox proportional hazard models. The results are presented in Table 3.5 Column (1) to (4). The point estimates in Column (1) and (3) are considerably higher in this sample. However, Column (2) and (4) highlights that these higher effects are virtually unaffected by whether the high take-up rate and its interaction with the after dummy are included. Therefore, these results confirm again that the effect of the reform does not depend on the take-up rate.[47]

The results presented here underline that access to information (measured by variation in take-up rate) on the reemployment bonus does not affect the estimates in non-employment duration. This is not surprising given that the reemployment bonus scheme was a very complicated, non-salient policy with some substantial drawbacks, such as losing the remaining benefit eligibility if claimed. Therefore, our estimates indicate the the effect of the reemployment bonus was negligible, and the approximtaley 10 days decrease in non-employment duration can be attributed to frontloading the benefit schedule.

[47]As a further robustness check, in Appendix Figure A.8 we show that the results are robust to controlling directly for the share of workers who claimed reemployment bonus (and its interaction with the after dummy).

3.3.1 Effect of the Reform on the Budget

Our results presented in the previous section indicate that non-employment duration decreased considerably as a results of the benefit frontloading. We use our estimates to understand the budget consequences of this reform. The total budget needed to finance the first 360 days of the unemployed can be summarized by the following equation:

$$G = \sum_{t=1}^{360} b_t S_t + \sum_{t=1}^{360} \tau w(1 - S_t)$$

where τ is the tax rate, w is the reemployment wage, and b_t and S_t is the benefit schedule and the survival rate t days after unemployment benefit was claimed, respectively. We decompose the change in total budget into two components:

$$\begin{aligned}
\Delta G &= \left(\textstyle\sum_{t=1}^{360} b_t^{post} S_t^{post} + \sum_{t=1}^{360} \tau w(1 - S_t^{post})\right) - \left(\textstyle\sum_{t=1}^{360} b_t^{pre} S_t^{pre} + \sum_{t=1}^{360} \tau w(1 - S_t^{pre})\right) \\
&= \underbrace{\sum_{t=1}^{360} S_t^{pre}\left(b_t^{post} - b_t^{pre}\right)}_{\substack{\text{mechanical UI spending increase} \\ \text{caused by the reform}}} + \underbrace{\sum_{t=1}^{T} \left(S_t^{post} - S_t^{pre}\right)\left(b_t^{post} + \tau w\right)}_{\substack{\text{UI spending decrease} \\ \text{caused by behavioral responses}}}
\end{aligned} \tag{28}$$

where b_t^{post} and b_t^{pre} are the daily pre- and post benefit shown on Figure 3.1, while S_t^{post} and S_t^{pre} is the daily pre and post survival rate shown in Figure 3.2. The first term in the decomposition shows that an unemployed individual who finds a job quickly collects more benefit under the new system and this mechanically increases the government spending on UI. The second term captures the budget consequences of the behavioral responses to the reform: due to faster reemployment, spending on UI decreases and tax revenues increase. It remains an empirical question whether the mechanical or the behavioral effect has a larger influence on the budget.

Table 3.6 summarizes the key effects of the reform on the budget. It shows that in the absence of behavioral responses, benefit frontloading would have increased mechanically benefit payments by $119. However, benefit frontloading sped up reemployment, which decreased spending on UI benefits by $57. Moreover, finding jobs earlier also lead to higher UI contributions, which is equivalent to an additional $8. From the government point of view, revenues outside the UI budget should also be taken into account. The wage related taxes and contributions paid because the unemployed find jobs quicker increased the revenue of the budget with an additional $90.

Table 3.6: The Effect of the Reform on the Budget

Balance of the unemployment benefit system		s. e.**
before*	$1605	(9.51)
Mechanical cost change	$119	(2.21)
Change in benefit spending because faster reemployment	-$57	(12.86)
Change in UI contribution because more time in work	-$8	(1.66)
after*	$1662	(9.91)
Net increase in UI cost	$54	(15.03)
Net gain in tax revenue		
Taxes and contributions paid by the worker because more time in work	$38	(8.14)
Contributions paid by the firm because more time in work	$52	(11.12)
Change in government revenue	$90	(19.07)
(Net gain in tax revenue - Net increase in UI cost)	$36	(33.75)

*in the 1st year after UI claimed **bootstrapped standard errors in parenthesis

Note: This table shows the effect of the reform on the government budget. We decompose the effect of the reform into different components based on equation 28 (see the text for details). Bootstrapped standard errors in parentheses are reported in the right column.

To sum up, the mechanical increase of UI expenditures were $119 while the behavioral response of the unemployed improved the balance of the budget by $156, which suggests that frontloading improved the budget by $36 per unemployed. We also calculated the standard errors around these estimates by bootstrapping.[48] While at the conventional confidence levels we cannot rule out that the effect of the reform on the UI budget is negative, our estimates indicate that it is unlikely that the reform had a negative effect on it.[49]

[48]We take 1000 random sample with replacement, then calculate the Kaplan-Meire survival rates and the implied UI budget.

[49]The p-value of a one-sided hypothesis test on whether the budget effect is negative is .14

3.4 Welfare Assessment

Our estimates in the previous section can be used to assess the welfare implications of front-loading. We use the stylized job search model of Chetty (2008) and Kolsrud et al. (2015) to highlight the key channels through with benefit frontloading affect welfare.

3.4.1 Set-up

We consider a discrete time model of job search in which agents live for T periods. The representative agent starts as unemployed and searches for jobs in each period. Employment is an absorbing state[50], and so once a job is found, the unemployed will be employed at wage w for the rest of her life.[51]

In each periods agents make two decisions: they choose search intensity s_t and consumption level c_t. Search intensity is costly and these costs are represented by $c(s_t)$. We assume that the cost function is convex, strictly increasing and twice differentiable. The value function of the employed if $t < T$ is

$$V_t^E(A_t) = max_{A_{t+1}} u(c_t^e) + v(G) + \delta V_{t+1}^E(A_{t+1}),$$

where δ is the discount factor, and $V_T^E(A_t) = max_{A_{t+1}} u(w + A_T) + v(G)$. The value of employment depends on private consumption, $u(c_t^e)$, and on the consumption of public goods $v(G)$. Both $u()$ and $v()$ are strictly increasing, concave, twice differentiable functions. Assets earn a return r per period so that consumers face a per-period budget constraint $c_t^e = w + A_t - \frac{A_{t+1}}{1+r}$ and a borrowing constraint $A_t \geq L$.[52]

[50]Relaxing this assumption complicates the calculation of the value of employment, but the main conclusions of this section are not affected.

[51]We assume that the change in benefit profile does not affect reemployment wages, which is confirmed by our empirical analysis in Section 3.

[52]The presence of borrowing constraints does not affect our results.

The value function of the unemployed if $t < T$ is

$$V_t^U(A_t) = max_{A_{t+1},s_t} u(c_t^u) - c(s_t) + v(G) + \delta \left[s_t V_{t+1}^E(A_{t+1}) + (1-s_t) V_{t+1}^U(A_{t+1}) \right],$$

where $c_t^u = b_t + A_t - \frac{A_{t+1}}{1+r}$ and $V_T^U(A_t) = u(b_t + A_T) + +v(G)$. Again the value of employment depends on public and private consumption.

Spending on the unemployment insurance system depends on the fraction of agents that stay unemployed at period, S_t, and the benefit paid out to these workers, b_t. The total unemployment benefit payout equals $\sum_{t=1}^{T} \left(\frac{1}{1+r}\right)^t S_t b_t$. The tax that can be collected depends on the fraction of workers who are employed, $1 - S_t$, and on the tax rate, τ[53]. Finally, the government spends G on public goods and so the government deficit, D, is defined by the following formula:

$$D = \sum_{t=1}^{T} \left(\frac{1}{1+r}\right)^t G + \sum_{t=1}^{T} \left(\frac{1}{1+r}\right)^t S_t b_t - \sum_{t=1}^{T} \left(\frac{1}{1+r}\right)^t (1 - S_t)\, \tau w.$$

We assume that the deficit must be kept constant, and so more spending on unemployment insurance (while keeping constant the tax revenue), will decrease the amount of public goods provided in the economy.

The UI benefit was constant before November 1st, 2015 and so $b_t = b$.[54] The Hungarian reform increased the benefit by $\widetilde{\triangle b}$ in the first N periods and decreased by $\utilde{\Delta b}$ afterwards, while the total benefit that can be collected throughout the unemployment spell remained constant, formally,

$$\sum_{k=0}^{N} \Delta b_1 + \sum_{k=N+1}^{T} \Delta b_2 = \sum_{k=1}^{T} \Delta b_k = 0. \tag{29}$$

Notice that we require here that the total benefit is kept constant in nominal terms and not in present value terms. These two differ if the interest rate, r, is positive. We make this assumption to stick to the exact reform that occurred, however, the results are unaffected if

[53]We also include also social security contributions in taxes, because the link between contributions and future benefits is very weak for most workers (Summers, 1998).

[54]If the interest rate, r, is positive, then this benefit path is slightly declining in present value terms.

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the present value of the total benefit is kept constant instead.

3.4.2 Welfare implications

The value of unemployment at period 0 captures the expected utility of a newly unemployed agent. We examine the effect of benefit change on this measure to understand the welfare implications of frontloading.

Proposition 1. Suppose that the unemployment benefit is increased by $\widetilde{\triangle b}$ in the first N periods and decreased by $\utilde{\Delta b}$ afterwards, while the total benefit that can be collected throughout the unemployment spell remained constant and so equation 29 applies.

Then the effect of benefit change on the value of unemployment at the beginning of the UI spell is determined by the following formula:

$$\triangle V_0^U(A_0) = \underbrace{u'\left(c_0^{u*}\right)\Delta b_0 + \sum_{k=1}^{N}\delta^k\prod_{i=1}^{k}\left(1-s_i^*\right)u'\left(c_k^{u*}\right)\Delta b_k}_{\substack{\text{welfare effect caused by}\\ \text{change in the benefit}\\ \geq 0}} \qquad \underbrace{-\sum\delta^k v'(G)\triangle G}_{\substack{\text{welfare effect caused by}\\ \text{change in public spending}\\ \lesseqgtr 0}}$$

The first part of this expression, welfare effect caused by change in the benefit, is always non-zero and it becomes positive if optimal search s_t^* is positive for at least one period throughout the unemployment spell or if the interest rate, r, is positive. Moreover, the second part of this expression, the welfare effect caused by the change in public spending, can be positive, negative or zero depending on the sign of $\triangle G$. This $\triangle G$ is the following:

$$\Delta G = \underbrace{\frac{\sum_{t=1}^{T}\left(\frac{1}{1+r}\right)^t S_t\triangle b_t}{\sum_{t=1}^{T}\left(\frac{1}{1+r}\right)^t}}_{\substack{\text{mechanical UI spending increase}\\ \text{caused by the reform}}} + \underbrace{\frac{\sum_{t=1}^{T}\triangle S_t b_t}{\sum_{t=1}^{T}\left(\frac{1}{1+r}\right)^t}}_{\substack{\text{UI spending decrease}\\ \text{caused by shorter UI spell}}} + \underbrace{\frac{\sum_{t=1}^{T}\triangle S_t\tau}{\sum_{t=1}^{T}\left(\frac{1}{1+r}\right)^t}}_{\substack{\text{increase in tax reven}\\ \text{caused by finding job s}}}$$

Proposition 1 highlights that the benefit change induced by frontloading increases the welfare of the unemployed by increasing private consumption. This is because under the new UI benefit schedule the consumption profile under the old rules can be replicated by saving the benefit increase in the first N periods and consuming them later. The new benefit schedule, therefore, must provide at least as high consumption utility as the old one, and as the proposition highlights, under some week conditions it will be strictly higher.

However, the new benefit schedule can increase the funding need of the UI system, which can lead to cutting back spending on public goods, $\triangle G$. In principe, lowering public goods can offset the welfare gain caused by the consumption increase of the unemployed, but this is not necessarily the case. Proposition 1 shows that the effect on public spending is ambiguous and determined by three different factors. First, benefit frontloading mechanically increases the spending on UI, because the unemployed individuals who find jobs relatively quickly collect more benefits under the new rule. Second, a sizable decline in non-employment duration decreases spending on UI benefits. Third, unemployed individuals who find jobs quicker pay more taxes and increase government revenue. While the first effect increases the cost of the unemployment insurance system, the latter two effects decrease it. It remains an empirical questions, therefore, which of these effects dominates.

The results in Section 3.1 calculate the change in $\triangle G$ and show that in the Hungarian case the behavioral responses were large enough to offset the mechanical cost increase in the UI. This implies that, in fact, $\triangle G$ in fact increased and not decreased after the reform. Therefore, the Hungarian benefit change was clearly welfare improving, because not only did it increase private consumption consumption of the unemployed, but it also saved some money for the government.

It is worth highlighting that the result presented in Proposition 1 is very robust to alternative modeling assumptions. The presence of borrowing limits, unobserved heterogeneity among the unemployed, or hand-to-mouth consumers do not influence the welfare implications presented here.

3.5 Conclusion

This paperpresented the Hungarian unemployment benefit reform where a new frontloaded benefit path replaced the flat benefit system. The virtue of the reform was that the timing of the benefit was changed while the total amount of the benefit that could have been collected stayed constant. We provided evidence that benefit frontloading speeded up reemployment and did not increase the cost of the unemployment insurance system. This implies that the new benefit schedule made some unemployed definitely better off and none of them worse off. Moreover, given that the reform increased government revenue , we conclude here that the Hungarian reform was welfare increasing.

Our results are in stark contrast with Kolsrud et al. (2015), who conclude that increasing the benefit profile is likely to be welfare improving. The key difference between their findings and ours is that they find that the behavior response to a benefit change at the beginning of the UI spell does not differ substantially from benefit changes happening latter on. If this were true, we should have found that the benefit increase at the beginning of the UI offsets the effect of the benefit decrease that happened towards the end of the UI, and so the behavioral responses to frontloading should be limited. As we showed above, our results does not support this prediction. While more studies are needed to understand better the behavioral responses to a benefit change, the key advantage of our setup relative to Kolsrud et al. (2015) is that we analyze here a very transparent and radical change in the UI benefit that is likely to induce responses in job search even in the presence of some adjustment costs (Chetty et al., 2013).

Finally, while this paper aims to evaluate the welfare implication of this reform, , in a related paper DellaVigna et al. (2016) we exploit the same reform to evaluate competing job search models. In that paper we show that a behavioral search model does a better job explaining the hazard rate to employment than the standard search models in the literature. Both papershighlight the importance of the benefit path, and suggest that redesigning the UI systems can sometimes break the classic trade-off between moral hazard and insurance.

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Chapter A

Appendix

Appendix of Chapter 1

Appendix A

Proof of Proposition 1 It is assumed that the expected utility of workers at firm j is U_j. It is obvious that firms opt for $b_j = 0$ and $w_j = U_j$ if they do not want to incentivize workers. If they intend to incentivize workers, they have to solve the following profit maximization problem:

$max \prod(b_j, w_j) = (1 - b_j)(p + \bar{e}) - w_j$

such that: $(1 - b_j)(p + \bar{e}) - w_j \geq p - U_j$

$w_j + b_j(p + \bar{e}) - b_j^2 * r * var(\varepsilon_j) - c\bar{e} \geq U_j$

The two constraints are the incentive compatibility constraints which have to be met at optimum. The first condition states that the profit per worker of firms should be at least as large in the case of incentive contracts as in the case of fixed wage contracts. The second constraint ensures that workers exerting high effort cannot have a lower utility than shirking workers.

As firms want to maximize profit, they should decrease the expected value of wages until the incentive compatibility condition of the worker allows. In this case, $b_j = c$ and $c^2 * r * var(\varepsilon_j) + c\bar{e} + U_j = w_j^e$. If this is combined with the incentive compatibility constraint of the firm, it is optimal to use incentive contracts, if and only if $\frac{\bar{e}*(1-c)}{c^2*r} \geq var(\varepsilon_j)$.

Proof of Proposition 2 b is used to denote a firm offering an incentive contract and f for one that offers a fixed wage contract. In this case, the following inequalities apply:

$(P_b - U_b) * N(U_b, F) \geq (P_b - U_f) * N(U_f, F) \geq (P_f - U_f) * N(U_f, F) \geq (P_f - U_b) * N(U_b, F)$

The first and the third inequalities are implied by the equilibrium condition of Equation 5. The second inequality applies as $P_b \geq P_f$[55]. These inequalities imply that

$(P_b - P_f) * N(U_b, F) \geq (P_b - P_f) * N(U_f, F) \Rightarrow N(U_b, F) \geq N(U_f, F)$

As firm size is a strictly monotonous function of wages, the last inequality implies that $U_b \geq U_f$.

Proof of Proposition 3

The first order condition of profit maximization is the following:

$$\frac{dProfit_j}{dU_j} = 0 \Rightarrow (P_j - U_j) * \frac{\partial N((F(U_j), b_j, var(\varepsilon_j))/\partial w_j}{N((F(U_j), b_j, var(\varepsilon_j))} = 1 \tag{30}$$

Using Equation 30 and the fact that $\frac{\partial F(U_J)}{\partial b_j} = \frac{\partial F(U_J)}{\partial U_j} * (-2b_j r var(\varepsilon))$ we arrive at the following equation:

$$\frac{dProfit_j}{db_j} = -4rbvar(\varepsilon_j) * N((F(U_j), b_j, var(\varepsilon_j)) \tag{31}$$

Equation 31 shows that the profit of the firm is decreasing in the profit sharing parameter. So the firms which smooth employment choose the lowest b_j which satisfies Equation 10. If the $var(\varepsilon_j)$ is small enough then Equation 10 holds even if $b_j = 0$. That is why firms with less volatile revenue can offer fixed wages but do not fire workers during recession.

Firms do not fire workers if the expected profit of revenue sharing is also larger than the expected profit of offering a fixed wage and firing workers during recessions. To compute this incentive compatibility constraint, I derive the expected profit of firms if they offer a fixed wage and do not smooth employment. After hiring a worker, the firm has $p - U_j + \varepsilon_j$ profit with 50 percent probability and 0 otherwise. The probability that the worker gets a better wage offer is $\lambda(1 - F(U_j))$ so the worker wants to stay at the firm in the next period with a probability of $(1 - \lambda(1 - F(U_j)) - \delta)$. The probability of a negative shock is 50 percent so

[55] The equality holds if and only if $\frac{e*(1-c)}{c^2 * r} = var(\varepsilon_j)$.

the worker remains at the firm with $0.5*(1-\lambda(1-F(U_j))-\delta)$ probability. To sum up, the expected present value of a worker is

$$E(prof.|not\,smooth)=\sum_{t=0}^{\infty}(0.5*(1-\lambda(1-F(U_j))-\delta))^t*(\frac{p-U_j+\varepsilon_j}{2})=\frac{p-U_j+\varepsilon_j}{1+\lambda(1-F(U_j))+\delta} \tag{32}$$

If the firm smooths employment by revenue sharing then the expected per period profit is P_j-U_j. Now the firms do not want to fire workers so the probability of remaining at the firm is $1-\lambda(1-F(U_j))-\delta)$ which implies that the expected profit is

$$E(prof.|smooth)=\sum_{t=0}^{\infty}(1-\lambda(1-F(U_j))-\delta)^t*(P_j-U_j)=\frac{P_j-U_j}{\lambda(1-F(U_j))+\delta} \tag{33}$$

To sum up, the firm does not fire workers if and only if

$$\frac{p-U_j+\varepsilon_j}{1+\lambda(1-F(U_j))+\delta}\leq\frac{P_j-U_j}{\lambda(1-F(U_j))+\delta} \tag{34}$$

After plugging in Equation 10, we get the following expression:

$$rvar(\varepsilon_j)\left[b(1-b)(1+\lambda(1-F(U_j))+\delta)-b\right]\leq P_j-U_j \tag{35}$$

It is easy to see that the left hand side is increasing and the right hand side is linearly decreasing in $var(\varepsilon_j)$ so if the variance of the individual level shocks are large enough then firms do not pay bonuses but fire workers in case of negative sales revenue shocks.

Data Construction

The Structure of Earnings Survey are made by the National Employment Service. A random sample of firms having at least 5 workers but less than 20 workers and all firms having at least 20 workers have to report detailed information about their employees.

Companies having less than 20 workers have to report information about each employee and firms having more than 20 workers have to report about 10 percent of their employees. The Survey is repeated cross-section on the individual level. Firms with less than 20 employees have to report on all of their workers. The sample selection at larger firms is based on date of birth, as employers have to report on blue collar workers born on the 15^{th} or 25^{th} day and white collar workers born on the 5^{th}, 15^{th} or 25^{th} day of the month. I use the method of Csillag and Koren (2011) to construct individual level panel using the Survey data. First, I construct cells within firms using variables which unlikely change between during the employment contract. These variables are the year and month of birth, gender, the highest level of education completed and the 4-digit occupational code. Using this method, 97 percent of the workers are uniquely identified as they are alone in their cells which. It is improbable that firms fire somebody and hire a new worker with exactly the same characteristics. Therefore, the cells allow me with high certainty to link workers between the years if workers do not change employer or occupation between the years[56].

Firm-level data come from the corporate income tax returns sheets collected by the National Tax and Customs Administration. The database contains the balance sheet and income statement of every double entry book-keeping firm. The firms also have a unique identifier so they can be followed over time and firm-level revenue changes can be linked to the wage information of the Structure of Earning Survey. Besides the revenue changes I also use the tax return sheets of the firms to compute the value-added and fixed-affects per worker. To rule out extreme shocks, I drop individuals who work at firms with very large changes in sales revenue. More precisely, I use only observations where sales revenue of the firm changes by less than 50 percent from one year to the next. This affects approximately the largest and smallest 5 percentile of sales growth distribution.

[56]Between 2002 and 2008, the tenure of workers is also observable. When I used tenure instead of occupation code for matching workers I found that less then one percent of workers changes occupation without leaving the firm. The probability of changing occupation is uncorrelated with bonus payments.

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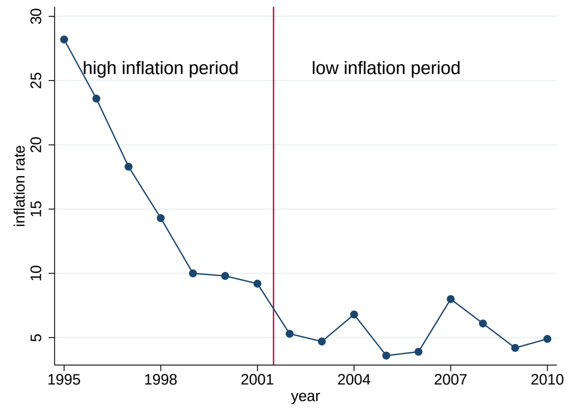
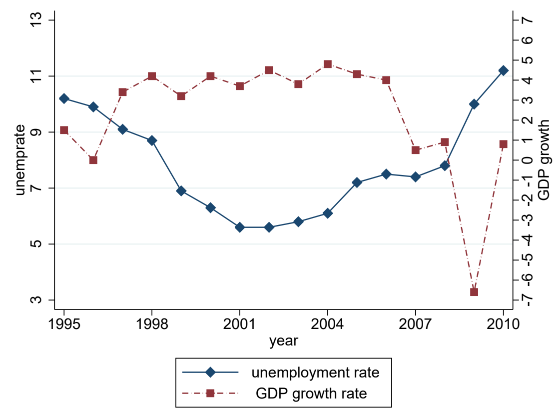
Figure A.1: Macroeconomic environment

(a) Inflation





(b) GDP growth and unemployment rate

Note: Panel (a) shows the annual inflation rate. I refer to the years before 2001 as the high-inflation period and the years after 2001 as the low-inflation period in the robustness checks. Panel (b) shows that the economy was relatively stable and there was no recession during the period under scrutiny. The source of the data are the Central Bank of Hungary and the Hungarian Labor Force Survey.

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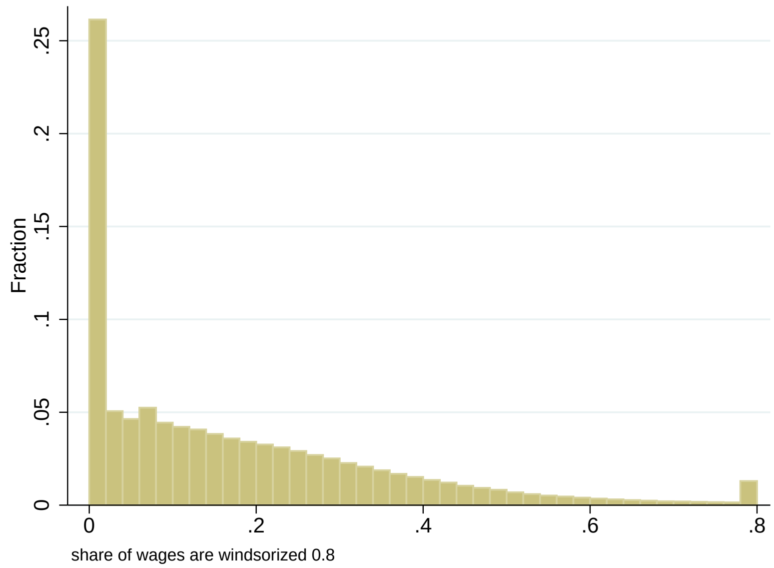
Figure A.2: The share of bonuses over the base wage

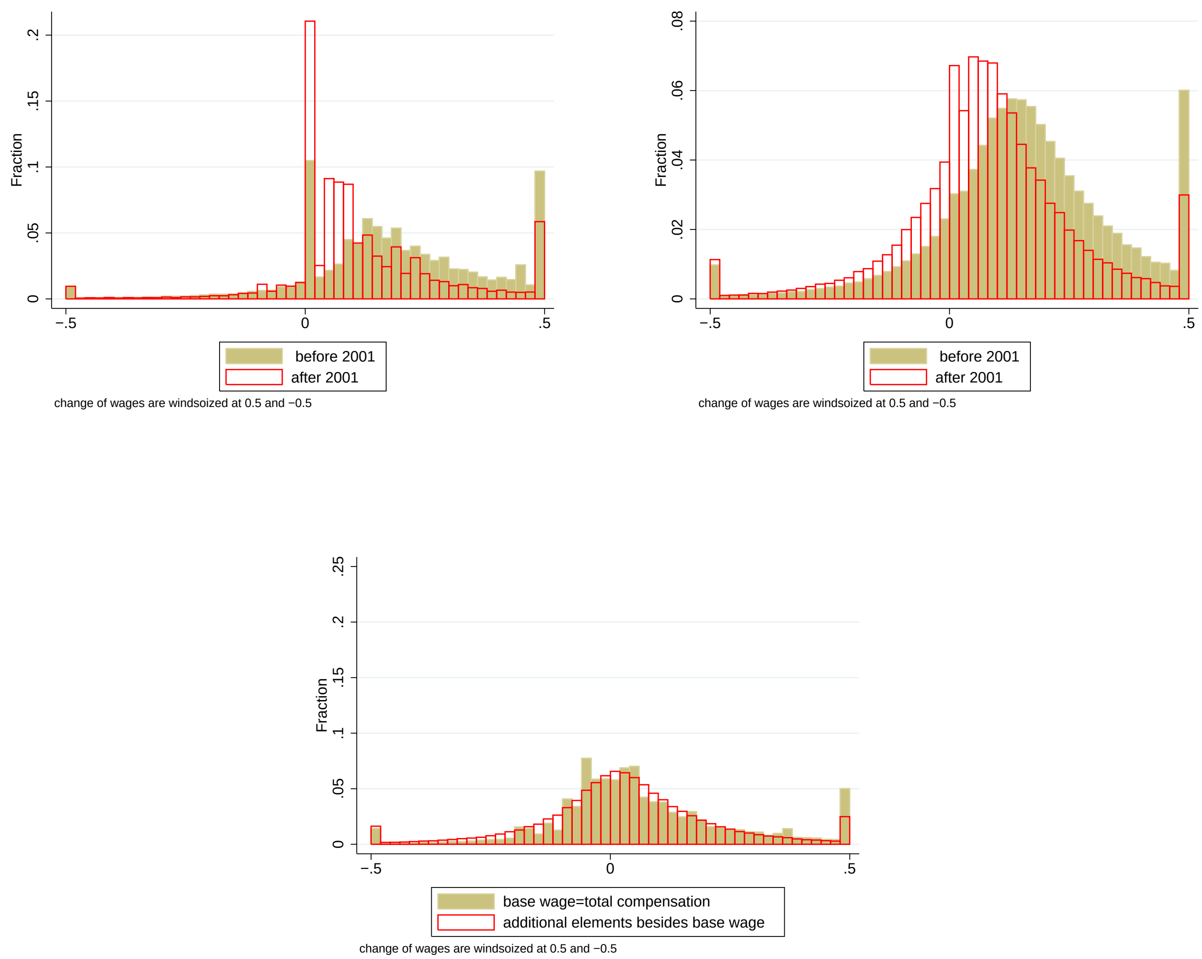


Note: This figure presents the distribution of workers by the share of bonuses over the base wage.

Figure A.3: The change of worker compensation and inflation

(a) workers without bonus

(b) Workers receiving bonus



(c) The change of real wages

Note: Figure (a) show the distribution of wage changes by decade for workers who do not receive a bonus. Panel (b) shows the same for workers receiving a bonus. Changes of wages before 2001 when the inflation was higher than 10 percent are included and Panel (b) shows the changes of wages after 2001 when the inflation was below 8 percent. The third panel shows the distribution of changes in real wages for the two worker groups. The figures demonstrate that only nominal wages are downward rigid.

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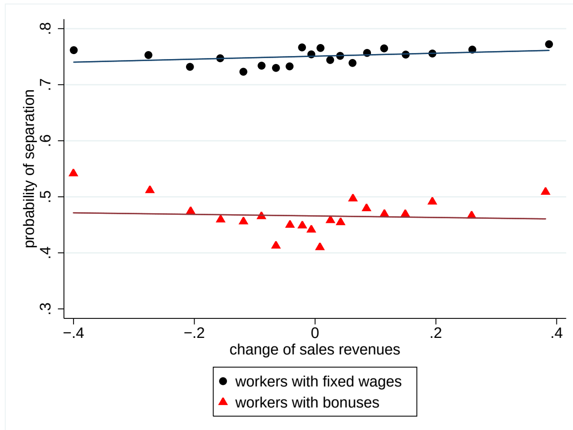
Figure A.4: Probability of job separation



Note: Workers are grouped into equally-sized bins based on the change of the sales revenue of the firm employing them. The figure shows the conditional probability of remaining at the firm. Contrary to Figure 2, I consider a job to be separated if the firm does not participate in the Structure of Earnings Survey in the next year. The control variables are sex, experience, square of experience, years of education, capital and sales revenue per worker, 2-digit occupation codes (ISCO 98), 2-digit industry codes (NACE) and year dummies. The figure shows that the probability of job survival is not correlated with the change in sales revenueand the probability of job survival is larger if the worker received a bonus. .

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Table A.1: Heterogeneity in the wage and employment responses of the firm

	females	males	tradeable industries	non tradable industries	white collar	blue collar
Panel A: percentage change in wages						
Share of workers with bonus	0.00991***	-0.00234	0.00115	0.00514	0.0169***	-0.00397
	(0.00299)	(0.00267)	(0.00286)	(0.00332)	(0.00330)	(0.00252)
change in sales revenue	0.0495***	0.0226*	0.0230	0.0402***	0.0494***	0.0252**
	(0.0151)	(0.0130)	(0.0148)	(0.0155)	(0.0183)	(0.0123)
interaction	0.0515***	0.0893***	0.0919***	0.0491***	0.0380**	0.0913***
	(0.0165)	(0.0142)	(0.0159)	(0.0172)	(0.0192)	(0.0136)
Observations	148,384	226,104	226,479	135,457	148,296	226,192
R-squared	0.066	0.053	0.064	0.046	0.068	0.053
Panel B: probability of job separation						
Share of workers with bonus	-0.258***	-0.252***	-0.271***	-0.234***	-0.262***	-0.252***
	(0.00668)	(0.00514)	(0.00642)	(0.00687)	(0.00554)	(0.00551)
change in sales revenue	0.0221	0.00908	-0.0126	0.0489**	0.0272	0.00845
	(0.0212)	(0.0170)	(0.0203)	(0.0222)	(0.0201)	(0.0174)
interaction	-0.0906***	-0.0552***	-0.0435*	-0.0972***	-0.0803***	-0.0648***
	(0.0245)	(0.0196)	(0.0229)	(0.0276)	(0.0233)	(0.0201)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	281,707	415,969	403,970	269,123	269,348	428,328
R-squared	0.065	0.061	0.056	0.070	0.067	0.062

Note: The table shows the heterogeneous effects of bonus payments. Panel A shows the effect of bonus payment and sales revenue changes on the average wages of workers. Panel B shows the effect of these variables on the probability of remaining at the firm. Every column shows the effects of bonus payments on a different subsample. Column (1) shows the effect of bonuses on females and Column (2) on males. Column (3) restricts attention on on workers in tradeable industries and Column (4) on worker in non tradeable industies. Finally, Column (5) shows white collar workers and Column (6) blue collar workers. Every column includes the the full set of control variables: log-capital per worker and log-sales per worker, the age of the firm, 2-digit industry codes (NACE), sex, years of education, experience, square of experience, a dummy indicator for being a new entrant and 2-digit occupation codes (ISCO 88) and year dummies to get rid of the effect of inflation. Standard errors are clustered on the firm level.

Table A.2: Robustness to different bonus definitions

VARIABLES	(1) got bonus last year	(2) bonus>0.1 wage	(3) wage>base wage	(4) only perform. pay.	(5) non-financial remuneration
Panel A: percentage change in wages					
worker got bonus	-0.0467***	-0.0586***	-0.0478***	0.00487**	0.00338
	(0.00207)	(0.00163)	(0.00229)	(0.00199)	(0.00286)
change in sales revenue	0.0656***	0.0876***	0.0650***	0.0493***	0.00610
	(0.00935)	(0.00641)	(0.0103)	(0.00972)	(0.0163)
interaction	0.0433***	0.0225**	0.0420***	0.0623***	0.00687
	(0.0106)	(0.00882)	(0.0114)	(0.0109)	(0.0167)
Observations	361,936	361,936	361,936	361,936	365,616
R-squared	0.061	0.069	0.061	0.056	0.302
Panel B: probability of job separation					
worker got bonus	-0.0827***	-0.0545***	-0.0812***	-0.269***	
	(0.00431)	(0.00350)	(0.00421)	(0.00481)	
change in sales revenue	0.0574***	0.0532***	0.0582***	-0.0206	
	(0.0146)	(0.0109)	(0.0151)	(0.0142)	
interaction	0.0215	0.0246	0.0212	-0.0884***	
	(0.0177)	(0.0154)	(0.0178)	(0.0178)	
controls	yes	yes	yes	yes	
Observations	673,093	673,093	673,093	673,093	
R-squared	0.037	0.035	0.036	0.074	

Note: The table shows the estimated coefficients of Equation 11. Panel A shows the effect of bonus payment and sales revenue changes on the wages of workers. Panel B shows the effect of these variables on the probability of separation. Columns (1) to (4) show different bonus definitions. In Column (1), I define a worker as receiving a bonus if she received a bonus last year, in Column (2) if the bonus part was more than 10 percent of base wage, in Column (3) if the base wage was less than the total wage and in Column (5) if the worker received any performance payment except overtime payments. The dependent variable in the last column is the amount of non financial renumeration at the firm. Every column includes the the full set of control variables: log-capital per worker and log-sales per worker, the age of the firm, 2-digit industry categories, sex, years of education, experience, experience^2, a dummy indicator for being a new entrant and 2-digit occupation categories as well as year dummies to get rid of the effect of inflation. Standard errors are clustered at firm level.

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Table A.3: Robustness to alternative samples

VARIABLES	(1) full sample	(2) change in sales<30%	(3) change in sales<20%	(4) winsorized at 50% dlog(sales)	(5) firm-fixed effects	(6) above MW	(7) # emp >100
Panel A: percentage change in wages							
worker got bonus	-0.00193	-4.99e-05	0.000817	0.000717	0.00137	0.0154***	0.00893***
	(0.00185)	(0.00212)	(0.00228)	(0.00308)	(0.00317)	(0.00255)	(0.00339)
change in sales revenue	0.0265***	0.0286***	0.0590***	0.0608**	0.00196	0.0387***	0.0516***
	(0.00856)	(0.0104)	(0.0138)	(0.0245)	(0.0132)	(0.0119)	(0.0166)
interaction	0.0772***	0.0681***	0.0443***	0.0537**	0.0930***	0.0703***	0.0613***
	(0.0101)	(0.0111)	(0.0152)	(0.0247)	(0.0140)	(0.0128)	(0.0176)
Observations	517,347	364,414	321,603	262,568	363,868	350,953	245,281
R-squared	0.056	0.053	0.050	0.045	0.136	0.062	0.069
Panel B: probability of job separation							
worker got bonus	-0.237***	-0.240***	-0.240***	-0.243***	-0.297***	-0.259***	-0.298***
	(0.00423)	(0.00466)	(0.00502)	(0.00559)	(0.00479)	(0.00486)	(0.00743)
change in sales revenue	0.0186	0.0303*	0.0431*	0.00427	-0.00836	0.0124	-0.00766
	(0.0125)	(0.0164)	(0.0249)	(0.0393)	(0.0159)	(0.0157)	(0.0254)
interaction	-	-	-0.118***	-0.108**	-	-	-0.0495*
	0.0636***	0.0803***			0.0587***	0.0652***	
	(0.0170)	(0.0187)	(0.0284)	(0.0437)	(0.0172)	(0.0183)	(0.0275)
controls	yes	yes	yes	yes	yes	yes	yes
Observations	964,968	677,663	593,146	481,248	676,748	643,865	444,772
R-squared	0.058	0.065	0.064	0.063	0.160	0.059	0.062

Note: The table shows the estimated coefficients of Equation 11. Panel A shows the effect of bonus payment and sales revenue changes on the wages of workers. Panel B shows the effect of these variables on the probability of separation. The first column shows includes the firms having less than 19 or more than 2500 employees. In Columns (2) and (3) I confine my attention to observations where the sales revenue of the firms changed by less than 30 and 20 percent, respectively. Column 4 winsorizes the data at a 50 percent wage change instead of trimming. Column (5) indicates firm-fixed effects. Column (6) omits minimum wage earners and Column (7) focuses on firms having more than 100 workers. Every column includes the the full set of control variables: log-capital per worker and log-sales per worker, the age of the firm, 2-digit industry codes (NACE), sex, years of education, experience, square of experience, a dummy indicator for being a new entrant, 2-digit occupation codes (ISCO 88) and year dummies to get rid of the effect of inflation. Standard errors are clustered at firm level.

Table A.4: Robustness to macroeconomic factors

VARIABLES	(8) before 2001/ high infl.	(9) after 2001/ low infl.	(3) real sales changes	(4) high unemp. rate	(5) low unemp. rate
Panel A: percentage change in wages					
worker got bonus	0.00549	-0.00635**	-0.0035*	-0.0035	-0.0057*
	(0.00427)	(0.0029)	(0.0020)	(0.0022)	(0.0030)
change in sales revenue	0.0281*	0.0361**	0.0402***	0.0521***	0.0419***
	(0.0156)	(0.0161)	(0.0081)	(0.0113)	(0.0129)
interaction	0.0838***	0.0429**	0.0266***	0.0418***	0.0291*
	(0.0159)	(0.0167)	(0.0093)	(0.0137)	(0.0150)
Observations	167,584	196,830	322,885	213,742	185,817
R-squared	0.028	0.020	0.023	0.025	0.023
Panel B: probability of job separation					
worker got bonus	-0.269***	-0.220***	-0.230***	-0.233***	-0.237***
	(0.00547)	(0.00666)	(0.00453)	(0.00560)	(0.00593)
change in sales revenue	0.0146	0.0175	0.0201	0.0156	-0.0140
	(0.0193)	(0.0249)	(0.0131)	(0.0175)	(0.0199)
interaction	-0.0531**	-0.0704**	-0.0138	-0.0555***	-0.0111
	(0.0231)	(0.0282)	(0.0156)	(0.0204)	(0.0242)
controls	yes	yes	yes	yes	yes
Observations	298,006	379,657	608,122	385,928	357,479
R-squared	0.073	0.063	0.059	0.054	0.061

Note: The table shows the estimated coefficients of Equation 11. Panel A shows the effect of bonus payment and sales revenue changes on the wages of workers. Panel B shows the effect of these variables on the probability of separation. Columns (1) and (2) separate the sample by time. Column (3) considers the effect or real sales changes. Column (4) and (5) separate the sample to a low and high unemployment sub-sample. See the text for details. Every column includes the the full set of control variables: log-capital per worker and log-sales per worker, the age of the firm, 2-digit industry codes (NACE), sex, years of education, experience, square of experience, a dummy indicator for being a new entrant, 2-digit occupation codes (ISCO 88) and year dummies to get rid of the effect of inflation. Standard errors are clustered at firm level.

Appendix of Chapter 2

Minimum wage regulations in Hungary

Target and coverage. A single national monthly gross minimum wage was introduced by Hungary's last communist-led government in 1989. The minimum wage relates to monthly pre-tax base wages, that is, total monthly earnings net of overtime pay, shift pay and bonus-es. Starting from 2007 weekly, daily and hourly levels are determined, too. The minimum wage is legally binding and covers all wages, including those paid to the self-employed by their own businesses. For part-timers, who account for about 5 per cent of total employment, the wage floor is proportionately lower. In 2006-2008 further minima applied to skilled workers (1.25MW) and young skilled workers (1.2MW). In 2009 the minimum for young skilled workers was abolished.

MW setting. The minimum wage is negotiated in a consultative body of employers and unions (Council of the Reconciliation of Interests). The government usually steps into the pro-cess at the end, by accepting the recommendations of the Council, but it is authorised to make a unilateral decision in case the negotiations fail, as it happened in 2001.

Level of the MW. At its introduction the MW amounted to 35 per cent of the average wage (AW), while in 2000 it stood at 29 per cent. Viktor Orbán's first government (1998–2002) nearly doubled the MW, by raising it from Ft 25,500 in December 2000 to Ft 40,000 in January 2001 and Ft 50,000 in January 2002. The two hikes raised the minimum wage–average wage ratio to 39 per cent and 43 per cent, respectively. Since 2003, the MW/AW ratio slightly fell but remained above its pre-hike level[57].

Compliance. The Wage Survey's data suggest that sub-minimum wages accounted for less than 1 per cent of all wages in each year since 1989. Estimates based on personal income tax reports and pension contributions hint at higher rates, but these data do not allow proper adjustment for time out of work during the year.

Fraction of employees affected. The fraction of workers paid 95–105 per cent of the MW

[57] All data quoted in the Appendix come from the Wage Survey.

amounted to 5 per cent in 2000. It jumped to 19 per cent in May 2002 in firms employing five or more workers and increased substantially in larger firms, too. The ratio fell to 10–12 per cent by 2004 and fell further substantially after 2006, when the tax authority started to interpret MW payment as a signal of wage under-reporting.

Taxing the MW. In 1989-2001 the MW was subject to linear social security contribution and progressive personal income tax. In 2002 it became free of personal income tax. In 2007, a minimum social security contribution base amounting to 2MW was introduced, as discussed in Section 3 of the text. This measure was abandoned in 2010.

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Table A.5: Occupational classification used in the double-hurdle model

Occupations	Type*	Definition (based on standard classification of occupations)
Agricultural	E	Codes 61-64 and 92 comprising the drivers of agricultural vehicles
Construction	S	Code 76
Service	S	Codes 52-53 except 532, 533 and 536. Includes transport, mail and telecommunication
Trade	S	Codes 51 and 421, 422 and 429 comprising cashiers
Industrial	S	Codes 71-75
Other blue-collar		
Cleaners	E	Code 911
Unskilled laborers	E	Codes 913-919
Machine operators	E	Codes 81-83. Includes the operators of mobile machines such as cranes,
Porters and guards	E	Codes 912 and 536 comprising porters and security guards, respectively
Drivers	S	Code 833, 835, 836 Car, truck and bus. Excludes the drivers of agricultural vehicles
White-collar		
Office clerks	W	Codes 41-42 and 532-533 comprising office based jobs in health and social services
Technicians, assistants	W	Codes 31-34
Administrators	W	Codes 35-39
Managers	W	Codes 11-14
Professionals	W	Codes 21-29

* E: elementary; S: secondary; W: white-collar

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Table A.6: Selection to the worker panel used in the test (probit)

Dependent variable: 1 if made it to the panel, 0 otherwise	Marginal effect	Z-vaule
Male	0.013	6.05***
Years in school	-0.000	-1.27
Experience	0.009	25.68***
Experience squared	-0.000	-19.94***
Earned more than the MW * log wage	0.013	5.52***
Earned the MW	0.099	3.13***
Firm size: 5-20 employees	-0.110	-28.95***
Firm size: 21-50 employees	-0.102	32.89***
Firm size: 51-300 employees	-0.138	-65.07**
Firm size: 301-1000 employees	-0.003	-1.25
Ownership: majority domestic private	-0.048	-19.92***
Ownership: majority foreign	-0.013	-4.54***
Ownership: mixed	0.021	4.21***
Sales revenues per worker (log)	-0.008	8.53***
Negative value added	-0.009	-0.64
Micro-region unemployment rate (log)	-0.414	-5.69***
Western Hungary	0.027	7.23***
Northern Transdanubia	0.064	16.20***
Southern Transdanubia	0.059	12.15***
Southern Plain	-0.024	-5.26
Northern Plain	0.091	19.69***
Northern Hungary	0.114	25.78***
Agriculture, forestry, fishing	0.131	24.92***
Mining	0.154	6.69***
Construction	-0.013	3.41***
Trade, tourism	0.030	11.49***
Transport	-0.051	-8.17***
Financial services	-0.032	-8.50***
Services	-0.164	-4.46***
Education and health (private establishments)	0.035	6.22***
Observations	132115	
LR chi2 (30), significance	8473.98	0.0000

* significant at 10%; ** significant at 5%; *** significant at 1%
Reference categories: female, more than 1000 employees, majority state-owned, Central Hungary, manufacturing Data: Wage Survey 2006, enterprise sector. All variables relate to May 2006.

Table A.7: Selection to the firm panel used in the test (probit)

Dependent variable: 1 if observed in the 2006 WS, 0 otherwise	Marginal effect	Z-value
Share of men	0.000	0.01
Average years in school	0.000	0.01
Average experience	0.000	0.02
Average wage	0.000	0.00
Share of workers affected by the 2001 MW hike	-0.000	-0.000
Firm size: 5-20 employees	-0.368	-12.47***
Firm size: 21-50 employees	-0.355	-12.27***
Firm size: 51-300 employees	-0.375	-13.49***
Firm size: 301-1000 employees	-0.053	-2.17***
Ownership: majority domestic private	-0.036	-2.54**
Ownership: majority foreign	-0.058	-3.08***
Ownership: mixed	0.059	1.58
Sales revenues per worker (log)	0.000	0.00
Negative value added	-0.101	-1.20
Micro-region unemployment rate (log)	0.008	0.02
Western Hungary	-0.022	-0.96
Northern Transdanubia	-0.018	-0.75
Southern Transdanubia	0.031	1.29
Southern Plain	0.019	0.83
Northern Plain	0.011	0.51
Northern Hungary	0.050	2.38
Agriculture, forestry, fishing	0.013	0.61
Mining	0.091	1.05
Construction	-0.037	-1.93*
Trade, tourism	-0.021	-1.53
Transport	-0.053	-1.75*
Financial services	-0.020	-1.11
Services	-0.248	--1.37
Education and health (private establishments)	-0.030	-1.19
Firms in WS 2006	9574	
Firms also observed in WS 2007	6348	

* significant at 10%; ** significant at 5%; *** significant at 1%
Data: Wage Survey 2006

Appendix of Chapter 3

A.1 The Effect of the Reform on the Budget

Table 3.6 summarizes the effect on the budget. We use equation 28. The b_t^{post} and b_t^{pre} are the daily pre- and post benefits shown on Figure 3.1. $S_t^{post}$and S_t^{pre} are the daily pre- and post- survival rates shown in Figure 3.2. The average monthly gross reemployment wage was \$509.

The following items are paid to the government:

1. Unemployment insurance contributions. The UI contribution was 4.5% of the gross wage and paid directly into the budget of the unemployment benefit system. Given that the behavioral effect of the reform was around 10 days, the additional revenue of the benefit budget was around $\$509 * (10/30) * 4.5\%)$.

2. Personal Income Tax. The income taxes were based on monthly earnings. The tax rate below the minimum wage (\$285) was 0, while above the minimum wage it was 18 percent. This means that around $(\$509 - \$285) * (10/30) * 18\% = \$13.4$ was paid in taxes.

3. Health insurance contribution. The health insurance contribution was a fixed \$9.75 per month. The additional revenue effect of that item was around $(10/30) * \$9.75 = 3.25$

4. Social security contribution (employee part). The social security contribution was 12.5 percent of the gross wage, and so the sum of taxes paid by the workers were around $\$509 * (10/30) * 12.5\% = \$21.2$

5. Social security contribution (employer part). Firms also needs to pay social security contributions which is 30% of the gross wage so the contributions paid by the firm were around $\$509 * (10/30) * 30\% = \$50.9$

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A.2 Proof of Proposition 1

Proposition 1. Suppose that the unemployment benefit is increased by $\widetilde{\triangle b}$ in the first N periods and decreased by $\underset{\sim}{\Delta b}$ afterwards, while the total benefit that can be collected throughout the unemployment spell remained constant, formally,

$$\sum_{k=0}^{N} \widetilde{\triangle b} + \sum_{k=N+1}^{T} \underset{\sim}{\Delta b} = \sum_{k=1}^{T} \Delta b_k = 0. \tag{36}$$

Then the effect of benefit change on the value of unemployment at the beginning of the UI spell is determined by the following formula:

$$\triangle V_0^U(A_0) = \underbrace{u'\left(c_0^{u*}\right)\Delta b_0 + \sum_{k=1}^{N} \delta^k \prod_{i=1}^{k} \left(1 - s_i^*\right) u'\left(c_k^{u*}\right)\Delta b_k}_{\substack{\text{welfare effect caused by} \\ \text{change in the benefit} \\ \geq 0}} \quad \underbrace{- \sum \delta^k v'(G)\triangle G}_{\substack{\text{welfare effect caused by} \\ \text{change in public spending} \\ \lesseqgtr 0}}$$

The first part of this formula, welfare effect caused by change in the benefit, is always non-negative, and it only becomes positive if optimal search s_t^* is positive for at least one period throughout the unemployment spell or if the interest rate, r, is positive. The second part of this formula, the welfare effect caused by change in public spending, can be positive, negative or zero depending on the sign of $\triangle G$. Moreover, $\triangle G$ will be determined by the following equation.

$$\Delta G = \underbrace{\frac{\sum_{t=1}^{T} \left(\frac{1}{1+r}\right)^t S_t \triangle b_t}{\sum_{t=1}^{T} \left(\frac{1}{1+r}\right)^t}}_{\substack{\text{mechanical UI spending increase} \\ \text{caused by the reform}}} + \underbrace{\frac{\sum_{t=1}^{T} \triangle S_t b_t}{\sum_{t=1}^{T} \left(\frac{1}{1+r}\right)^t}}_{\substack{\text{UI spending decrease} \\ \text{caused by shorter UI spell}}} + \underbrace{\frac{\sum_{t=1}^{T} \triangle S_t \tau}{\sum_{t=1}^{T} \left(\frac{1}{1+r}\right)^t}}_{\substack{\text{increase in tax reven} \\ \text{caused by finding job s}}}$$

Proof:

The value of unemployment is defined by the following equation:

$$V_t^U(A_t) = u\left(c_i^{u*}\right) - c(s_t^*) + v(G) + \delta\left[s_t V_{t+1}^E(A_{t+1}^*) + (1-s_t)V_{t+1}^U(A_{t+1}^*)\right]$$

Based on this the value of unemployment in period 0 can be rewritten as

$$V_0^U(A_0) = \sum_{k=0}^{T}\delta^k v(G) + u\left(c_0^{u*}\right) - c\left(s_0^*\right) + \sum_{k=1}^{T}\delta^k\prod_{i=1}^{k}\left(1-s_i^*\right)\left[u\left(c_k^{u*}\right) - c\left(s_k^*\right)\right] + \sum_{k=1}^{T}\delta^k\prod_{i=1}^{k-1}(1-s_i^*)s_k^* V_k^E(A_{t+1}^*)$$

Now we look at the change in benefits described by equation 36. By the envelop theorem the indirect effect on the value function will be second order, and so the effect of benefit change on the value function will be the following:

$$\triangle V_0^U(A_0) = \underbrace{u'\left(c_0^{u*}\right)\Delta b_0 + \sum_{k=1}^{N}\delta^k\prod_{i=1}^{k}\left(1-s_i^*\right)u'\left(c_k^{u*}\right)\Delta b_k}_{\substack{\text{welfare effect caused by}\\ \text{change in the benefit}\\ \geq 0}} \qquad \underbrace{-\sum\delta^k v'(G)\triangle G}_{\substack{\text{welfare effect caused by}\\ \text{change in public spending}\\ \lesseqqgtr 0}} \tag{37}$$

As we show next, the first term is always positive, while the second term can be positive or negative depending on the sign of$\triangle G$. We will provide the expression for $\triangle G$ later.

To show that the welfare effect caused by the benefit change is non-negative, we stipulate that the optimal consumption path must satisfy the usual Euler equation:

$$u'\left(c_t^{u*}\right) \geq \delta(1+r)\left[s_t^*\frac{\partial V_{t+1}^E(A_{t+1}^*)}{\partial A_{t+1}} + \left(1-s_t^*\right)u'\left(c_{t+1}^{u*}\right)\right]$$

This equation can be easily derived from the FOC of the value function with respect to A_{t+1} and from the envelop theorem that indicates that $\frac{\partial V_{t+1}^U(A_{t+1}^*)}{\partial A_{t+1}} = u'\left(c_{t+1}^{u*}\right)$. This equation holds for equality in the absence of borrowing constraints while in the presence of binding borrowing constraints the left hand side is strictly greater than the right hand side.

Given that $\frac{\partial V^E_{t+1}(A^*_{t+1})}{\partial A_{t+1}} > 0$, $s^*_t \geq 0$, and $r \geq 0$, the Euler equation implies that $u'\left(c^{u*}_t\right) \geq \delta\left(1 - s^*_t\right) u'\left(c^{u*}_{t+1}\right)$ for all t and this inequality holds strictly if $s^*_t > 0$ or $1+r > 1$. This equation also implies that $\delta^t \prod_{i=1}^{t}\left(1 - s^*_t\right) u'\left(c^{u*}_t\right) \geq \delta^T \prod_{i=1}^{T}\left(1 - s^*_t\right) u'\left(c^{u*}_T\right)$ for all t. Therefore,

$$u'\left(c^{u*}_0\right)\Delta b_0 + \sum_{k=1}^{T}\delta^k \prod_{i=1}^{k}\left(1 - s^*_i\right) u'\left(c^{u*}_k\right)\Delta b_k \geq \prod_{i=1}^{T}\left(1 - s^*_i\right) u'\left(c^{u*}_T\right)\sum_{k=1}^{T}\Delta b_k$$

and whenever $s^*_t > 0$ for at least one period or $r > 0$, this inequality holds strictly. Moreover, given that equation 36 $\sum_{k=1}^{T}\Delta b_k = 0$, this inequality implies that the first part of equation 37 is positive:

$$u'\left(c^{u*}_0\right)\Delta b_0 + \sum_{k=1}^{N}\delta^k \prod_{i=1}^{k}\left(1 - s^*_i\right) u'\left(c^{u*}_k\right)\Delta b_k \geq 0$$

and the inequality holds strictly if $s^*_t > 0$ for at least one period or if $r > 0$.

Now we derive the expression for ΔG. By total differentiating the government budget we get the following expression:

$$0 = \sum_{t=1}^{T}\left(\frac{1}{1+r}\right)^t \Delta G + \sum_{t=1}^{T}\left(\frac{1}{1+r}\right)^t \Delta S_t b_t + \sum_{t=1}^{T}\left(\frac{1}{1+r}\right)^t S_t \Delta b_t + \sum_{t=1}^{T}\left(\frac{1}{1+r}\right)^t \Delta S_t \tau w.$$

where we specify that taxes and deficit are kept constant. This leads to the expression in the proposition:

$$\Delta G = \underbrace{\frac{\sum_{t=1}^{T}\left(\frac{1}{1+r}\right)^t S_t \triangle b_t}{\sum_{t=1}^{T}\left(\frac{1}{1+r}\right)^t}}_{\substack{\text{mechanical UI spending increase} \\ \text{caused by the reform}}} + \underbrace{\frac{\sum_{t=1}^{T}\triangle S_t b_t}{\sum_{t=1}^{T}\left(\frac{1}{1+r}\right)^t}}_{\substack{\text{UI spending decrease} \\ \text{caused by shorter UI spell}}} + \underbrace{\frac{\sum_{t=1}^{T}\triangle S_t \tau}{\sum_{t=1}^{T}\left(\frac{1}{1+r}\right)^t}}_{\substack{\text{increase in tax reven} \\ \text{caused by fining job so}}}$$

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Figure A.5: Information Sheet Received by the Unemployed

Közép-Magyarországi Regionális Munkaügyi Központ
[redacted] Kerületi Kirendeltség

Ügyiratszám: BP-16/06[redacted]2005
Ügyintéző: [redacted]
Telefon: (1) 403-0002

Tárgy: [redacted] álláskeresési járadék megállapító határozat

HATÁROZAT

Ügyfél neve: [redacted]
Születési neve: [redacted]
Anyja neve: [redacted]
Születési helye, ideje: BUDAPEST, [redacted]
Levelezési címe: 1223 Budapest 22.ker[redacted]
TAJ száma: [redacted]

Kérelme alapján az **álláskeresési járadékra való jogosultság** 2005.11.14. napjától

megállapítom,

és 2005.11.14.-től

Időszak hossza	Összeg
90 napig	naptári nap 2280FT
91-180 napig	naptári nap 1140FT

összegben való **folyósítását rendelem el**.

A döntés ellen a kézhezvételt követő naptól számított 15 napon belül a Közép-Magyarországi Regionális Munkaügyi Köszpont címzett (címe: 1082 Budapest 08. Ker. Kisfaludy utca 11), de a XVI. Kerületi Kirendeltséghez (címe [redacted] 2 példányba benyújtott illetékmentes fellebbezéssel lehet élni.
A határozat a fellebbezésre tekintet nélkül végrehajtható.

INDOKLÁS

[redacted] 2005.11.14. napján álláskeresési ellátás iránti kérelmet nyújtott be. Nevezett 2005.11.14. napjától álláskeresőként nyilvántartott.

A rendelkezésre álló iratokból megállapítottam, hogy az álláskeresővé válást megelőző öt éven belül 1728 nap munkaviszonyban töltött idővel rendelkezik.

Az álláskeresési járadék napi összegét 902500 Ft a társadalombiztosítás ellátásaira és a magánnyugdíjra jogosultakról, valamint e szolgáltatások fedezetéről szóló 1997. Évi LXXX. Törvény 19. §-ának (3) bekezdésében meghatározott havi átlagos munkaerőpiaci járulékalap figyelembevételével – a rendelkezésekre álló igazolások alapján – állapítottam meg.

Előzőkre tekintettel megállapítottam, hogy az álláskeresési járadék megállapításának a foglalkoztatás elősegítéséről és a munkanélküliek ellátásáról szóló 1991. évi IV. törvény (Flt.) 25. § (1) bekezdésében foglaltak alapján határoztam meg.

[redacted]

Telefon: 1/403-0002, Fax: 1/403-3433, Holnap: http://fovaros.munka.hu; http://www.afsz.hu
E-mail: KMRVivoutca@lab.hu

The figure shows an example of the the first page of the personalized information sheet received by an unemployed individual when UI was claimed. According to the table in the middle, the receiver of the form was eligible for daily HUF2280 for 90 days and daily HUF1140 for another 180 days.

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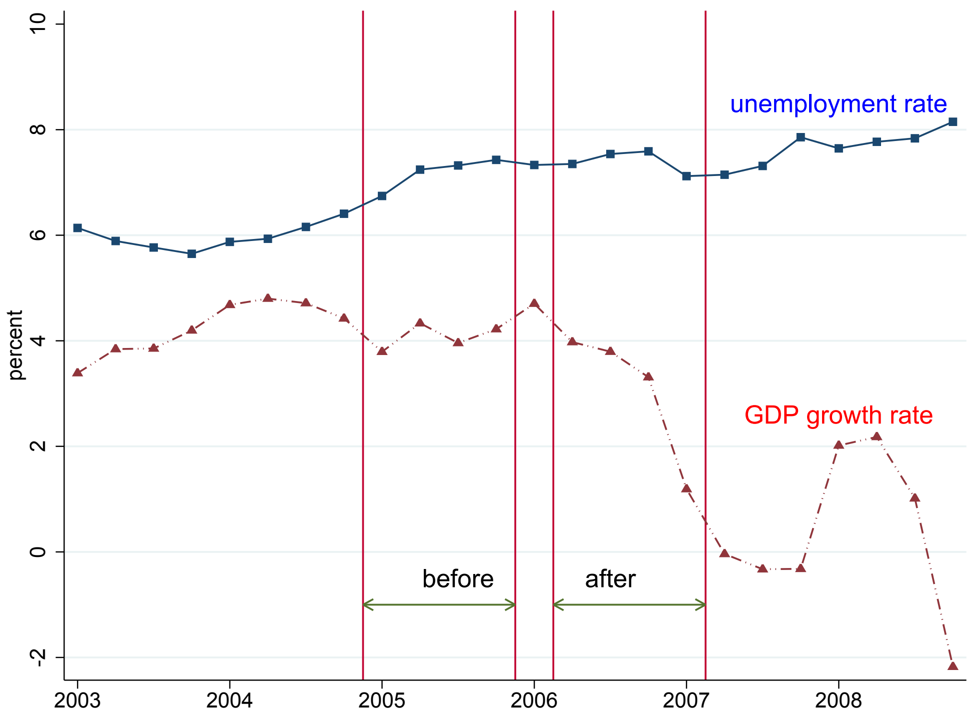
Figure A.6: GDP growth and unemployment rate in Hungary



The figure shows the seasonally adjusted GDP growth rate (dashed red line) and the seasonally adjusted unemployment rate (solid blue) between 2003 and 2008 in Hungary. The major (red) vertical lines indicate the period we use for the before-after comparison. The data was obtained from the Hungarian Central Statistical Office.

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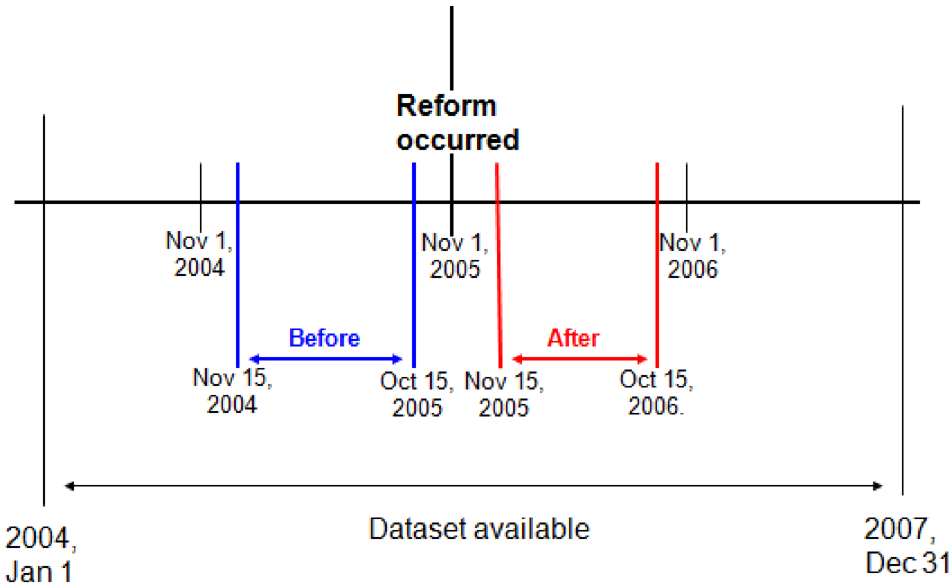
Figure A.7: Before and After Comparison Groups



The figure shows the time frame for which we have access to administrative data on unemployment insurance records, the time of the reform and how we define the before and after periods that we use for our before-after comparison. The before sample consists of those unemployed who claimed UI between November 15th, 2004 and October 15th, 2005, and the after sample consist unemployed who claimed UI between November 15th, 2005 and October 15th, 2006.

Table A.8: The effect of Frontloading by the Reemployment Bonus Take-up Rate

	Non-employment duration (OLS)[1]				Reemployment hazards (Cox-estimation)			
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
after	-9.50***	-8.67***	-10.29***	-8.74***	0.143***	0.132***	0.164***	0.140***
	(1.72)	(2.61)	(1.70)	(2.55)	(0.028)	(0.041)	(0.027)	(0.038)
take-up rate		-0.135				0.002		
		(0.193)				(0.003)		
take-up rate*after		-0.0800		-0.133		0.001		0.002
		(0.136)		(0.134)		(0.002)		(0.002)
controls	no	no	yes	yes	no	no	yes	yes
location FE	no	no	yes	yes	no	no	yes	yes
Observations	13,011	13,011	13,011	13,011	13,011	13,011	13,011	13,011
R-squared	0.002	0.042	0.003	0.043				

Clustered standard errors by UI take-up locations in parentheses

[1]Capped at 270 days.

*** p<0.01, ** p<0.05, * p<0.1

Note: This table shows the effect of the reform on non-employment duration by the local reemployment bonus take-up rate. The sample in all columns includes those unemployed individuals who claimed benefit in a UI office that has had at least 30 RB claimants in our sample. Column 1, 3, 5 and 7 show the baseline results for this particular sample. Column 2 and 4 estimate equation 27 and Column 6 and 8 estimate a Cox proportional hazard model. We use continuous measure of take-up rate instead of using the high take-up rate dummy variable as in Table 3.5. The length of non-employment is capped at 270 days in all columns. The control variables are sex, age, age square, waiting period (the number of days between job lost and UI claimed), the county of residence, day of the month UI claimed, education, occupation (1 digit) in the last job, log earnings in 2002 and 2003. The location fixed effects control for the local UI office where the unemployed claimed the benefit. Standard errors in parentheses are clustered at the local UI office level.