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Essays in Labor and Public Economics

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Essays in Labor and Public Economics

by

Attila Sándor Lindner

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Abstract

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Growing inequality and stagnating wages at the bottom of the earning distribution are the most striking social phenomena of the last 30 years. Moreover, the 2009 Great Recession surged unemployment and created unprecedented tension between rich and poor in most developed countries. These circumstances renewed the interest of politicians, policy makers, and economists toward public policies aimed at alleviating inequality. In this thesis, I empirically assess the effectiveness of two prominent public policies in helping the poor: the minimum wage and unemployment insurance.

Minimum wage is the most radical policy tool for elevating the wages of the bottom economic bracket. However, despite several decades of microeconometric evidence for increases, the minimum wage remains a highly controversial policy. The first two chapters of this dissertation are devoted to assessing the economic effects of an unusually large and persistent increase in the minimum wage instituted in Hungary in 2001. The minimum wage to the median wage increased from the current U.S. level (35%) to the level of 55%, which is equivalent with an (~60%) increase in the minimum wage in real terms.

In the first chapter, my co-author Péter Harasztosi and I study the employment effects of this unique minimum wage reform. We propose a new approach to estimating the employment effects of a minimum wage increase that exploits information on the distribution of wages before and after the policy change. We infer the number of jobs destroyed by comparing the number of pre-reform jobs below the new minimum wage to the excess number of jobs paying at (and above) the new minimum wage. The evolution of the earning distribution in Hungary shows that this ratio is close to one, suggesting that most firms responded to the reform by raising wages instead of destroying jobs. We confirm this conclusion using comparisons across subgroups of workers with larger and smaller fractions of worker affected by the minimum wage change. Our group-level estimates, again, imply that the higher minimum wage had, at most, a small negative effect on employment, and with the standard errors we can rule out larger than -0.3 employment elasticities with respect to wages.

In the second chapter, my co-author Péter Harasztosi and I study the economic incidence of the minimum wage polices. If minimum wage increase has a small negative effect on employment and a large effect on wages, the total remunerations allocated to low-wage workers

must increase. Using a large panel of firms and the Hungarian minimum wage increase, we show that this is indeed the case: firms highly exposed to the minimum wage experienced a large increase in their total labor cost. However, this raises a question: who pays for this cost increase? We show that firms' profits are not affected in response to the minimum wage, suggesting that firm-owners do not bear the incidence of the minimum wage increase. Instead, we document that total revenue of low-paying employers increased considerably, indicating that firms passed the effect of the minimum wage to consumers. Consistent with that explanation, we show that firms facing more elastic output demand, and so less ability to pass-through the effect of the minimum wage, experienced larger employment losses and lower increase in their total labor cost.

In the third chapter, Stefanno DellaVigna, Balázs Reizer, Johannes Schmieder and I scrutinize the job search behavior of the unemployed. We propose a model of job search with reference-dependent preferences, where the reference point is given by recent income. Newly unemployed individuals are faced with a loss because their recent past income is higher than the unemployment benefit they receive, and so they search hard. However, over time they get used to lower income, and thus search less. They search harder, again, in anticipation of a benefit cut, only to ultimately get used to the change. The model fits the typical shape of the exit from unemployment, including the spike at the UI exhaustion point. The model also makes unique predictions for the response of benefit changes. Second, we provide evidence using a reform in the unemployment system in Hungary. Most unemployment insurance programs have constant replacement rate for a fixed period, typically followed by lower benefits under unemployment assistance. In November 2005, Hungary switched from this standard single-step UI system to a two-step system, with unchanged overall generosity. We show that the system generated increased hazard rates in anticipation of, and especially following, benefit cuts in ways the standard model has a hard time fitting, even when allowing for unobserved heterogeneity. We structurally estimate the model and estimate a weight on gain-loss utility comparable to the weight of the standard utility term, and a speed of adjustment of the reference point of eight months. The results suggest that a revenue-neutral shift to multiple-step UI systems can speed exit from unemployment.

To my parents, Erzsébet and Sándor.

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Chapter 1

Does a Large Increase in the Minimum Wage Reduce Employment? Evidence from Hungary

with Péter Harasztosi

1.1 Introduction

Despite several decades of microeconometric evidence, the minimum wage remains a highly controversial policy. Opponents argue that an increase in the minimum wage leaves low-skilled workers worse off (e.g., Stigler, 1946; Neumark and Wascher, 2010). Faced with increased costs for low-skilled workers, employers will shift to using more capital and higher-skilled labor. Firms that cannot easily substitute other inputs will be forced to raise prices, leading to a decline in demand that reinforces the substitution effect and ultimately leads to a labor demand elasticity bigger than -1 in magnitude. Proponents, on the other hand, argue that an increase in the minimum wage has at most small employment effects, so incomes of low-skilled workers will rise (e.g., Card and Krueger, 1995; Dube et al., 2010). In addition to claiming that the substitution and scale effects of a minimum wage hike are relatively modest, some recent studies suggest that the costs of the minimum wage are partially offset by reductions in firm profitability – an effect that can emerge in a non-competitive wage setting model (e.g., the search and matching model of Flinn 2010), but is ruled out in the standard neoclassical approach.

In this chapter we present new evidence on the distributional impacts of a higher minimum wage based on worker- and firm-level evidence from Hungary. Figure 1.1 shows the remarkable recent history of the minimum wage in Hungary. Prior to 2000, the ratio of the minimum wage to the median wage in the country was about 35%, comparable to the current ratio in the U.S. Between 2000 and 2002, the minimum jumped to a level of about 55% of the median wage in the country — a level only slightly below the current minimum wage in France. This large step-like increase in the minimum wage makes it possible to implement and test a variety of relatively credible difference-in-difference style estimators. Moreover, the apparent permanence of the new higher level allows us to address the concern that many of the minimum wage increases analyzed in the recent labor economics literature are only temporary (Sorkin, 2013).[1]

[1] Reynolds and Gregory (1965) and Castillo and Freeman (1990) study the impacts of imposing the US federal minimum wage on Puerto Rico which was relatively large but occurred over several years, making it harder to precisely estimate the impact of the law. Kertesi and Köllő (2004) also studied the employment effects of the 2001 raise in the minimum wage in Hungary. Although they use different methods and datasets,

In the first part of the paper we use a new approach to estimate the medium-term distributional impacts on workers of the rise in the Hungarian minimum wage. Figure 1.2 summarizes the key idea of the underlying approach. Following an increase in the minimum wage, workers who were previously earning less than the new minimum will either be laid off or will receive a raise, generating an excess mass (i.e., "bunching") at or just above the minimum.[2] We calculate the size of the excess mass (bunching) in the new distribution (shown in red) relative to the mass who were earning below the new minimum in the old distribution (shown in blue) using a large employer-employee database that contains relatively accurate information on earnings and hourly wages. We show that the number of workers in the excess mass just above the minimum wage in the new distribution is about as big or even bigger than the number who were previously earning less than the new minimum, suggesting that most workers affected by the minimum wage experienced a pay increase, rather than a loss of employment.

A limitation of this simple analysis is that other factors may have affected aggregate labor demand in Hungary in 2001. While our reading of the evidence is that this is unlikely to be the case, we go on to implement a grouped version of the bunching estimator which exploits differences in the fraction of workers who were earning less than the new minimum wage across demographic groups and regions, and allows us to control for aggregate trends.[3] Again, we estimate that the group-level excess mass (bunching) generated by the minimum wage is very close to the mass of workers earning below the new minimum wage. The implied estimates of the employment elasticity facing the group who were previously earning less than the new minimum wage is -.13 (s.e. 0.14) in 2001 and -.18 (s.e. 0.17) in 2002.

This chapter relates to several branches of the minimum wage literature. First, we contribute to the extensive literature on the employment effects of the minimum wage (e.g., see surveys by Neumark and Wascher, 2010, and Card and Krueger, 1995 Neumark and Wascher (2010); Card and Krueger (1995)). Number of papers in this literature find close to zero effect of the minimum wage, sometimes positive sometimes negative (Doucouliagos and Stanley, 2009). However, these papers are often criticized on the basis that only small and temporary shocks to the minimum wage are used for identification (Sorkin, 2013). In the presence of adjustment costs firms might not respond to the minimum wage, which leads to bias in the estimated effects toward zero (Chetty et al., 2011). In this chapter, we show that the effect of the minimum wage is small even for an unusually large and persistent increase in the minimum wage.

This chapter also relates to the literature on wage inequality and minimum wage. Many papers demonstrated that the minimum wage has a substantial effect at the bottom of the wage distribution (DiNardo et al., 1996; Lee, 1999; Autor et al., 2014). We go further here, and use the change in the distribution to identify the employment effects of the minimum wage. Meyer and Wise (1983) were the first to propose this idea. However, their implementation was criticized by Card and Krueger (1995) and Dickens et al. (1998), since their results strongly relied on the functional form assumptions they made. We extend Meyer and Wise (1983) in two important ways that address these criticisms. First, we use wage distributions from

many of their estimates are close to ours.

[2]As noted by Ashenfelter and Smith (1979), employers may also choose not to comply with the law. This appears to be a relatively infrequent occurrence in Hungary, though in our empirical approach we allow for non-compliance.

[3]This idea is used in Blundell et al. (1998).

before and after the minimum wage increase to provide a more credible counter-factual for the shape of the wage distribution. Second, we use the actual *number* of workers to calculate the excess mass (bunching) rather than the fraction of workers, so we explicitly account for lost employment arising from the imposition of the minimum wage.

Our chapter also builds on the previous research analyzing the minimum wage reform in Hungary. Kertesi and Köllő (2004) looked at the employment effects of the 2001 raise in the minimum wage. Although they use different methods and datasets, many of their estimates are close to ours. Tonin (2011) documented that low-wage workers whose wage increased as a result of the 2001 minimum wage hike responded by decreasing their consumption. He explains this counter-intuitive result by tax evasion: the minimum wage hike turns some informal side-payments into reported income, which increases the tax burden on low-wage workers. Moreover, Elek et al. (2011) provide evidence for wage under-reporting at the minimum wage in Hungary. Throughout the chapter we do many robustness checks to show that our results are not contaminated by tax evasion. In particular, our results hold for large firms that are less likely to be engaged in tax evasion and for excluding occupations with considerable tax evasion practices.

1.2 Institutional Context and Data

The minimum wage in Hungary is negotiated annually by a national-level tripartite council — a consultative body that consists of unions, employers' associations and the government.[4] Upon failing to reach conclusions, the government is authorized to decide unilaterally, a right which they invoked every year between 1998 and 2002.

The right-wing government announced on April 6th 2000 that they would raise the minimum wage from 25,500 HUF to 40,000 HUF on 1st January 2001. The government (including the prime minister) also pledged to increase further the minimum wage in 2002. Keeping their promise, a year later, the minimum wage was raised to 50,000 HUF. Government officials claimed that the primary goal of the minimum wage change was to alleviate income differences, to raise government revenue and to diminish tax evasion (Cserpes and Papp, 2008). However, political commentators also mentioned political reasons: the governing party in preparation for the 2002 election aimed to implement a salient and radical policy tool that would raise their popularity among blue-collar workers. In any case, in 2002 the right-wing party lost the election and the new socialist government decided to keep the minimum wage at 50,000 HUF.[5] However, they exempted the minimum wage from personal income taxes to appeal to their working class voters. Figure 1.1 summarizes the evolution of the minimum wage in relation to the median wage in the private sector between 1996 and 2008.[6]

[4]The council set the the minimum monthly base earnings (total earnings net of overtime pay, shift pay and bonuses) for a full-time worker. For part-timers, accounting for only 5% of all employees, the minimum is proportionally lower.

[5]To alleviate the employer's burden due to the minimum wage increase, the government set up a fund in 2001. Firms with high labor share were eligible for a one-time non-repayable grant. In 2001 the available fund was approximately 2 billion HUF and around 5000 firms received the grant. In 2002 the government raised the fund to 15 billion HUF. Around 30,000 had applied for and received the one-time non-repayable grant (GVI 2008). The grant amount was very small in both years. The wage compensation alleviates only 2-3% of the increase in total labor cost.

[6]Public sector wages were raised by 50% between 2001 and 2003. This increased the median earnings in

The economy was stable at the time of the introduction of the minimum wage. Figure 1.3 Panel (a) depicts the evolution of real GDP growth, which was 4-5% around 2000. The employment-to-population ratio and the unemployment rate over time is shown in Panel (b). The growth in the employment-to-population ratio slowed down after 2001 and the fall in unemployment rate stopped, albeit at very low level, around 2001. The presence of pre-trends in the key labor market variables makes it difficult to draw any inference from the aggregate data about the effects of the minimum wage increase. For our analysis, we rely on disaggregated data sources to cleanly identify the effects of the minimum wage increase, net of any aggregate trends.[7]

Tax evasion is not uncommon in Hungary. There are two basic forms of evading taxes: unreported employment and under-reporting of earnings (grey employment). Unreported employment is estimated to be 16-17% of the total employment between 2001 and 2005 (Elek et al., 2011). The minimum wage increase might push some formal workers to the informal sector. In datasets where we cannot observe informal employment (e.g. in the corporate income tax data), we will over-estimate the true effect on (total) employment. Given that we find very small effects on employment this channel cannot be large. The second channel for avoiding taxes is under-reporting earnings. Tonin (2006) and Elek et al. (2011) show that some workers reporting earnings at the minimum receive some of their earnings under the table.

1.2.1 Data

We use two main data sources, namely the Wage Survey (WS) of the National Employment Service and the Labor Force Survey (LFS) conducted by the Hungarian Statistical Office. The Wage Survey (WS) is a linked employer-employee data set comprising observations on over 100,000 individuals in about 8,000 private businesses employing at least 5 workers.[8] Firms between 5-20 workers are randomly selected from the census of enterprises. Individual data on each employee working at the firm as of May 31st are reported for these firms. Larger firms employing more than 20 workers are supposed to report data for the Wage Survey. The response rate is very high for firms with more than 300 employees (≈90%), while it is lower (60%) for firms with 20-300 employees.[9] Firms responding to the survey report information on a roughly 10% random sample of their workers as of May 31st based on the workers' date of birth. The sampling is designed to over-sample white-collar workers.[10] The survey contains

the public sector, while it only had a small spillover effect on the private sector Telegdy (2014). Looking at the minimum wage to median wage in the private sector captures this spillover effect, but helps us abstracting away from the salary increase in the government sector.

[7]Hungary is a small open economy, so the exchange rate dynamics can also influence the economy. Kovács (2000) argued that the exchange rate appreciation at the beginning of 2000 put exporting firms under competitive pressure. However, the results presented here are robust to flexibly control for pre-2000 export dynamics.

[8]The WS include firms with 5-10 employees only from 2000. In the worker-level analysis, we calculated the earning distribution using firms with more than 10 employees. This allows us to use a consistent sample of firms between 1997 to 2004.

[9]These non-response rates are very similar to the non-response rate for the establishment surveys conducted by the BLS in the U.S (CPAF, 1998). Moreover, we also found that the firm's non-response rate in the Wage Survey is not related to employment changes in the Corporate Income Tax data.

[10]Every blue-collar worker born on 5th or 15th day of any month are selected into the sample. For white-collar workers, the 5th, the 15th and 25th day of any month is used for selecting.

detailed information on wages, job characteristics, and the workers' demographic and human capital variables. Due to the complex sampling design for the Wage Survey, observations are weighted.[11] While we can link firms in the Wage Survey over time, there is no individual ID for linking workers over time.

In Table 1 we report the summary statistics for the sample that we use in this section. We restrict the sample to workers between the ages of 23 and 60 to mitigate concerns about expansions in higher education over this period that affected those 22 and under, and a 1999 pension reform that affected the over-60 population. The weighted and unweighted means are very close to each other except for education, which is not surprising given that white-collar workers are over-sampled in our dataset. In Panel B we reported workers for whom the minimum wage binds. These workers are younger, lower educated and more likely to be female.

The second important data source is the EU harmonized Labor Force Survey. The survey is a household-based survey and contains information on labor force status, occupation, and some employer characteristics, if it applicable. Unfortunately, in this survey we do not have wage information. We use the weights that were created by the Hungarian Statistical Office to make the sample representative o the whole country.

1.3 Empirical Strategy

1.3.1 Bunching Estimator

We propose a novel approach to estimate the employment effects of the minimum wage that relies on the earning distribution. Our approach is based on the assumption that the minimum wage has negligible effect on employment above a threshold $\bar{W}$. We infer the number of jobs destroyed by comparing the number of pre-reform jobs below the new minimum wage to the excess number of jobs between the minimum wage and $\bar{W}$. Therefore, our estimation infers the employment loss from the bunching of workers (or the lack of it) at and above the minimum wage.

We assess this new method by comparing it to the common estimation procedure that examines the changes in total employment. We show that our bunching method generalizes this classic procedure, which infers the employment changes from aggregate movement in employment. We also discuss that the classic procedure can be seriously biased in the presence of aggregate employment shocks, while our estimation method substantially attenuates this bias.

[11] Weights are calculated by the following procedure. For large firms, where not all individuals were observed, within-firm weights were calculated based on a blue-collar indicator and a full-time worker indicator. Between-firm weights were calculated based on 1-digit NACE industry codes and 4 firm size categories (11-20, 21-50, 51-300, more than 300) using all double-book keeping firms. To get the individual weights, within- and between-firms weights have been multiplied together. Finally, we adjusted the weights to follow the aggregate employment trends of firms with more than 20 employees reported by the Hungarian Statistical Office. We decided to use that time series, because this is what the Hungarian Statistical Office has been consistently reporting since 1998.

1.4 The Bunching Estimator

Let us suppose that a new minimum wage introduced at the level of MW. Now for any $\bar{W}$ and for MW the bunching estimator, $B(\bar{W}, MW)$, introduced the following:

$$B(\bar{W}, MW) \equiv 1 - \frac{Emp^1\left[MW \leq w < \bar{W}\right] - Emp^0\left[MW \leq w < \bar{W}\right]}{Emp^0\left[w < MW\right]}.$$

The nominator of this estimator ($Emp^1\left[MW \leq w < \bar{W}\right] - Emp^0\left[MW \leq w < \bar{W}\right]$) calculates the employment changes between the new minimum wage, MW, and threshold $\bar{W}$ as a result of the reform, while the denominator ($Emp^0\left[w < MW\right]$) calculates the number of sub-minimum wage jobs in the pre-reform distribution. The intuition behind this estimator is highlighted on Figure 1.2, where we show the effect of the minimum wage on the (frequency) distribution of hourly earnings. The blue solid line shows a hypothetical earnings distribution before the introduction of the minimum wage. The blue solid bar at zero represents the workers not having jobs. The destroyed jobs disappears from the earnings distribution and adds to the number of workers in non-employment. On the other hand, the jobs that are retained generate an excess mass at and above the minimum wage in the new earning distribution as highlighted by the dashed red line on Figure 1.2. Note that in Figure **??** the frequency distribution above $\bar{W}$ is the same before and after. This is assured by the assumption that the employment is not affected above $\bar{W}$.

It is easy to derive that the bunching estimator proposed here is equivalent to the employment change below $\bar{W}$:

$$B(\bar{W}, MW) = \frac{Emp^1\left[w < \bar{W}\right] - Emp^0\left[w < \bar{W}\right]}{Emp^0\left[w < MW\right]} \tag{1.1}$$

This formula highlights the logic of the bunching estimator: once the minimum wage does not affect employment above $\bar{W}$, one can use the employment changes below $\bar{W}$ to identify the employment effects.

It is also worth considering a special case of this estimator, when $\bar{W}$ is set to infinity:

$$B(\infty, MW) = \frac{Emp^1 - Emp^0}{Emp^0\left[w < MW\right]}$$

In that case the bunching estimator just calculates the total employment change before and after. This is a common approach used in many studies to evaluate the employment effect of the minimum wage (Card and Krueger, 1995; Neumark and Wascher, 2005).[12] Therefore, the bunching estimator presented here is, in fact, a generalization of the estimation methods used in most of the papers in the literature.

As equation 1.1 highlights, the bunching estimator only estimates the total employment effects of the minimum wage if employment above $\bar{W}$ is not affected. This assumption trivially holds for $\bar{W} = \infty$, but not necessarily if $\bar{W} \ll \infty$. To understand the benefits of lowering $\bar{W}$, consider a case where the economy is not just affected by a minimum wage shock but also

[12] In fact, many of these studies go beyond looking at the simple before and after differences in employment and take a difference-in-difference (DID) type of estimation by using an unaffected control group to take out shocks not related to the minimum wage change. We will also implement later the group-level version of our bunching estimate.

by some random aggregate shock, θ, and so the total employment would be $Emp^0(1+\theta)$ in the absence of the minimum wage shock. Moreover, from now on we also assume that the employment loss of the minimum wage is a linear function of the number of people directly exposed to the minimum wage $pEmp^0[w < MW](1+\theta)$[13]. Under these assumption the new employment level is going to be the following:

$$Emp^1 = Emp^0(\theta+1) - p(\theta+1)Emp^0[w < MW]. \tag{1.2}$$

The next Lemma derives what the bunching estimator will look like under this assumption:

Lemma 1. *Suppose that the employment process follows 1.2 and that the minimum wage does not affect employment above $\bar{W}$. Then the Bunching Estimator leads to the following estimates:*

$$B(\bar{W}, MW) = \theta\frac{F^0(\bar{W})}{F^0(w < MW)} - \theta p - p$$

where $F^0()$ is the commutative distribution function before the minimum wage hike.

Lemma 1 highlights that the bunching estimator gives consistent estimates only if $\theta = 0$ and so there is no aggregate shock. In the presence of aggregate shocks the estimator is biased by two reasons. The first part of the bias, $\theta\frac{F^0(\bar{W})}{F^0(w<MW)}$, is due to the fact that part of the changes in employment are attributed to employment changes. Note that this bias is increasing function of $\bar{W}$ and can be quite substantial for reasonable parameter values. For instance, if aggregate employment grows by 2% ($\theta = 2\%$); 10% of the workers are affected by the minimum wage ($F^0(w < MW)$), and the threshold $\bar{W}$ is set to infinity, then the size of the bias is going to be 20%. Therefore, lowering that part of the bias by setting low values of $\bar{W}$ is very important in the presence of aggregate shocks. However, setting a too low level of $\bar{W}$ is also problematic, since the assumption that employment is only affected below $\bar{W}$ is more likely to be violated.

The second part of the bias, $-\theta p$, emerges because aggregate shocks affect the estimates on the number of workers affected by the minimum wage. The Bunching Estimator uses $Emp^0[w < MW]$ instead of $Emp^0[w < MW](1+\theta)$. Note that this part of the bias plays only a minor role for reasonable size aggregate employment shocks.

1.4.1 Empirical Implementation

First we compare the empirical frequency distribution of monthly earnings four years before and four years after the MW hike. To make the wage distributions comparable over time we adjust them by the nominal GDP growth. The second important issue is to set $\bar{W}$, the earnings level that depends on how large the spillover effect of the minimum wage is. We choose $\bar{W}$ empirically by finding the wage at which the post-reform frequency distribution converges to the pre-reform frequency distribution. We also show the main results with alternative thresholds, including $\bar{W} = \infty$.

Aggregate earnings distribution might be contaminated by changes in the sample composition or because of aggregate shocks. Therefore, we estimate the relationship between the

[13]Note that the employment loss depends on the number of people who would earn sub-minimum wage at time 1, if it were not introduced. Therefore, the employment loss will depend on $Emp^0[w < MW](1+\theta)$ and not simply on $Emp^0[w < MW]$.

excess number of jobs and the number of jobs in the below mass using a grouping estimator, à la Blundel et al (1998). We assign workers to mutually exclusive groups formed out of combinations of the 7 NUTS2 regions, workers' age in four categories (22-30, 30-40, 40-50, 50-55), workers' gender, and workers' education (less than high school, high school or above).[14] We run the following group-level regression:

$$\frac{Emp_g^t\left[MW \leq w < \bar{W}\right] - Emp_g^{2000}\left[MW \leq w < \bar{W}\right]}{Emp_{2000,g}} = \alpha + \beta^B \frac{Emp_g^{2000}\left[MW^t < \bar{W}\right]}{Emp_{2000,g}} + \varepsilon_g \tag{1.3}$$

We divide our key variables with $Emp_{2000,g}$ to adjust for heteroskedasticity and we also weight the regressions by $Emp_{2000,g}$.[15] Throughout the text we will refer to the left hand side, the excess number of jobs at year t divided by the employment in 2000, as an Excess Mass at year t. The right hand side of this regression equation represents the fraction of workers affected by the minimum wage, which is the number of jobs below the minimum wage divided by employment in 2000.

The parameter β^B in equation (1.3) estimates one minus the fraction of workers laid off because of the minimum wage. The key identification assumption here is that the group-level excess mass would be uncorrelated with the excess mass in the absence of the minimum wage hike. We test this assumption by looking at the relationship between Excess Mass in the pre-minimum wage hike years and the fraction of workers affected by the 2002 minimum wage.

Most studies in the minimum wage literature focus on the relationship between the percent change in employment ($\triangle logEmp$) and in the minimum wage ($\triangle logMW$). However, this definition does not take into consideration that the fraction of jobs for which the minimum wage binds might differ across years, research designs and level of aggregation.[16] In this paper, following a handful of recent papers (e.g., Dube et al., 2010), we focus on the relationship between the percent change in employment ($\triangle logEmp$) and in wages induced by the minimum wage increase ($\triangle logW$). In the standard competitive model with binding minimum wage this would measure the labor demand elasticity. Focusing on that elasticity is also more relevant from the welfare point of view as it has been recently highlighted by Lee and Saez (2002). In Figure 1.11 we summarizes some of the estimates in the literature including our estimates from this part and the part using firm-level evidence.

We calculate the relationship between $\triangle logW$ and $\triangle logEmp$ in the following way. First we estimate the employment effects of the minimum wage using $\beta^B - 1$ from equation (1.3). Then we estimate, β^{AW}, the group level relationship between the excess mass ratio, $\frac{BM_g\left(MW^t\right)}{Emp_{2000,g}}$, and the change in group level average wage. The ratio of $\beta^B - 1$ and β^{AW} will give us the

[14] We do not use workers between age 16 and 22, because their employment rate declined by 30% between 1997 and 2000 and this decline continued at the same rate after 2000. This large shift in teenage employment related to the rapid expansion of higher education around that time.

[15] Suppose there is no employment effect of the minimum wage, and so $\beta^B = 1$. Then $VAR\left[EM_g\left(MW^t\right)\right] = VAR\left[BM_g\left(MW^t\right)\right]$. Since $BM_g\left(MW^t\right) = P(w < \bar{W})Emp_{2000,g}$ this variance will be $P(w < MW^t)(1 - P(w < MW^t)Emp_{2000,g}$ an increasing function of the group size. Therefore, running simply $EM_g\left(MW^t\right)$ on $BM_g\left(MW^t\right)$ would cause heteroskedasticity. Normalizing by $Emp_{2000,g}$ would make the variance $\frac{P(w<MW^t)(1-P(w<MW^t)}{Emp_{2000,g}}$. This also highlights that we should weight this regression by employment in 2000.

[16] For instance, this definition of employment effect makes more comparable the worker level and firm level results.

labor demand elasticity. We calculate the standard errors by bootstrapping.

Finally, to get a better understanding of the effect of the minimum wage, we compare our results to the "classic approach", where $\bar{W}$ is infinity. Moreover, we also report results on the group-level relationship between the fraction of affected workers and the employment to population ratio, unemployment rate, and participation rate.

1.5 Results

1.5.1 Aggregate Earnings Distribution

Figure 1.4 shows the effect of the minimum wage on the (frequency) distribution of monthly earnings. It compares the pre-reform wage distribution (brown solid bar), year 2000 earnings distribution.[17] The minimum wage is raised from the level represented by the brown dashed line (10.1) to the red long-dashed line (10.55), which is a .45 log point increase in the minimum wage on the top of nominal GDP growth. This gigantic increase in the minimum wage clearly altered the earnings distribution. First, in 2000 only a small spike was present at the minimum wage. On the contrary, a much larger spike appears in the 2002 distribution indicating that many workers who earned below the 2002 minimum wage were swept up to the new minimum wage level. Second, an excess mass is present above the new minimum wage too. This could happen if the minimum wage pushes up earnings even for those who are not directly affected. This spillover effect on the wage distribution is quite large and fades out slowly.[18] Finally, the fraction of bunchers (reported at the top right corner) is greater than unity suggesting that no employment loss happened as result of the minimum wage hike.

In Figure 1.5 we show the evolution of the earnings distribution from 1998 to 2004. The timing of the reform is visible on the histograms. Panel (a) and Panel (b) show that the pre-reform distributions lied on top of each other indicating that the earning distribution is quite stable in the pre-reform years. The first hike in the minimum wage generated a large excess mass (bunching) in the 2001 earnings distribution. The size of this excess mass (bunching) is slightly larger than the below the 2001 minimum wage mass indicating no loss of jobs. Then in 2002, when minimum wage was raised by .1 log point above the 2001 minimum wage, the excess number of workers (bunching) increases. However, the fraction of bunchers is about the same, since the higher minimum wage mechanically creates a larger below mass. In 2003 the minimum wage is slightly lower in real terms than the 2002 minimum wage. In line with the predictions of Figure 1.2, we see that both the excess number of workers (bunching) and the number of sub minimum-wage workers decreased relative to its 2002 level. Again the fraction of bunchers stayed very similar. Finally, in 2004 the minimum wage declined close to its 2001 level, but an unrealistically high level of excess number of jobs showed up in the new earnings distribution. This highlights a limitation of our analysis. Our underlying assumption is that the earnings distribution would be stable without the effect of the minimum wage. As we go further in time from 2000 this assumption is less likely to hold. This can be seen more directly

[17] We report results on monthly (and not daily or hourly) earnings, because we do not observe hours worked before 1999. However, in Hungary 90% of the workers work full-time (CSO, 2000) so this is not a real restriction. Moreover, later we show on the post 1999 sample, that the results are very similar if hourly wage were used.

[18] The large ripple effect also suggest that the results are driven by real economic responses and not by wage underreporting.

in Figure 1.6 where we report the kernel densities.[19] As in the histograms, the timing of the minimum wage hike is clearly visible. Moreover, the density function above $\bar{W}$ (dotted dash black line) are very stable until 2004 (Panel (f)). Therefore, the results presented for 2004 should be treated cautiously.

So far we have compared the excess number of jobs in the post-reform distribution to the number of jobs earning sub-minimum wage in the pre-reform distribution. However, the jobs showing up in the new earning distribution might not employ the same "type" of workers as the jobs affected by the minimum wage. Since in our data we cannot connect workers over time, we cannot directly test whether sub-minimum wage workers were able to keep their jobs or they were substituted with more productive ones. However, we can test whether workers employed at and above the new minimum wage substantially differ in terms of observable characteristics before and after the minimum wage hike. In Appendix Figure 1.7 we show predicted earnings distribution for the jobs that earned less than $\bar{W}$=11. The prediction is based on year 2002 observable characteristics (age, age square, education, region, sex) and on the year 2000 estimated relationship between earnings and observables. We contrast this prediction to the predicted earning distribution based on year 2000 observables and the same estimated relationship. The basic idea is that in the presence of substitution between low skilled and high skilled there would be substantial changes in observables that would shift the earning distribution. However, the predicted earnings lie on the top of each other indicating the lack of substitution between workers based on observable characteristics.

1.5.2 Group-level Analysis.

Since the aggregate earnings distribution might be contaminated by aggregate shocks we estimate group-level regressions proposed in equation (1.3). In Table 1.2 we report the main results. In Column (1) we examine the relationship between exposure to the minimum wage and the change in the average wage. Panel A and Panel B show the changes from 2000 to 2001 and 2002, respectively. The point estimate tells us that a (hypothetical) group with 100% of workers affected by the minimum wage experiences a 42% (39%) increase in average wage between 2000 and 2001 (2002). These effects are slightly lower than the percent increase in the minimum wage, because some affected workers earned a bit more than the old minimum wage and for them the minimum wage is likely to have had a lower effect. In Panel C we also report the relationship between changes in the average wage and exposure to the minimum wage. There is only a small statistically insignificant difference between highly and not-highly exposed groups in the pre-reform years.

In Column (2) to (3) in Table 1.2 we estimate the employment effects of the minimum wage. In Column (2) we set $\bar{W}$ to be infinity, which is equivalent to estimating the group-level relationship between exposure and the changes in employment. The point estimate in Column (3) tells us that 94% (85%) of the workers directly affected by the minimum wage were employed in 2001 (2002). We calculate the implied employment changes, which is 6% in 2001 and 15% in 2002, indicating that some jobs were destroyed as a result of the minimum wage. However, the estimated employment effects are very noisily estimated (e.g. the confidence interval indicates that [-32%,+20%] of the workers affected by the minimum wage lost their

[19] Note that our main analysis relies on using frequency distributions and not densities, because the density function forced to be one would complicates the whole analysis.

job) and, therefore, we cannot reject neither of a considerable negative effect or no effect. Column (2) Panel C shows the relationship between pre-reform employment growth and the fraction of workers affected by the minimum wage. The point estimate shows a strong negative, albeit insignificant, relationship between exposure to the minimum wage and the pre-reform employment changes. In fact, the size of the pre-reform employment changes is very similar to the post-reform one which questions the interpretation that the post-reform effects were caused by the minimum wage.

In Table 1.2 Column (3) we present results with $\bar{W} = 11$. As we explained earlier, this estimation method only takes into consideration the employment changes that occur below $\bar{W}$. The point estimates are very similar to Column (1) in 2001 and they are somewhat closer to one in 2002. The estimated coefficient reveals that 94% (92%) of the jobs affected by the minimum wage showed up in the excess mass in 2001 (2002). We calculate the implied employment changes which are 6% and 8% in 2001 and 2002, respectively. However, the confidence intervals include zero employment effects and so we cannot rule out that the minimum wage has no effect on employment. Panel C also highlights that, contrary to the estimation with $\bar{W} = \infty$, there are no pre-reform changes in employment once we cut off workers above $\bar{W}$. To deepen our understanding of the benefits of lowering $\bar{W}$ in Figure 1.10 we compare the implied effect on employment for $\bar{W} = \infty$ (Panel (a)) and for our main specification with $\bar{W} = 11$ (Panel (b)) over time. There are a two important things worth noting here. First, the standard errors are much larger for $\bar{W} = \infty$. This might be because employment changes above $\bar{W}$ are not relevant for estimating the effect of the minimum wage, but they add some noise into the estimation. Second, the placebo estimates for $\bar{W} = \infty$ show that highly exposed groups have different employment trends before 2000. This is a clear violation of the parallel trend assumption that we need for identifying the true effect of the minimum wage. In contrast, once we trim out workers (log) earning above 11, the parallel trend assumption is satisfied before the minimum wage hike.

In Figure 1.8 we presents graphically the results shown in Table 1.2 Column (3). In Panel (a) we show the scatter plot between demographic-region group-level Excess Mass in 1998 (excess number of jobs in 1998 divided by employment in 2000) and the fraction of affected workers by the minimum wage in 2002. The relationship between exposure to the minimum wage and Excess Mass in 1998 is zero supporting the assumption that the earnings distribution was stable before the minimum wage hike. Panel (b) shows that after the minimum wage hike there is a strong positive relationship between Excess Mass and the fraction of affected of workers. Panel (c) on Figure 1.8 shows the relationship between Excess Mass and the Below Mass over time for the whole period. As in the histograms, the timing of the reform is strongly visible in the evolution of excess mass.

Figure 2.6 summarizes the key results of this section. Panel (a) transforms the results shown in Figure 1.8 Panel (c) (and also in Table 1.2 Column (3)) into a percentage change in jobs affected by the minimum wage increase. For the pre-2000 years we report the relationship between the Excess Mass and the fraction affected by the minimum wage in 2002. We use this to demonstrate that the employment changes in the relevant earnings range are not related to the exposure to the minimum wage. For the post minimum wage years (after 2000) we show the estimated effect on Excess Mass minus one. The employment effects of the minimum wage are always negative but very close to zero.

In Panel (b) we show the effect of the minimum wage increase on wages. For the pre-2000 years the relationship between the average wage change and the fraction of workers affected by the 2002 minimum wage are shown. In the post minimum wage years the relationship between average wage change at year t and the fraction of workers affected by the year t minimum wage are reported. The graph shows that the minimum wage raised wages substantially after 2000. The ratio of Panel (a) and Panel (b) gives us the employment elasticity with respect to wages. We report these elasticities in Table 1.2. The implied employment elasticity is -.13 (s.e. 0.14) in 2001 and -.18 (s.e. 0.17) in 2002.

In Figure 1.11 we compare our estimates for the employment elasticity to the one estimated in the previous literature. We report elasticities estimated in this Chapter and Chapter 2. Even though our empirical strategy differs considerably in the two chapters, the employment elasticities are surprisingly close to each other. We denoted our preferred estimate for the elasticity with a grey dashed line -.2. Note that this number is in fact included in the confidence intervals of the most the existing estimates.

Robustness checks. Now we turn to check the robustness of our results. Table 1.3 explores the alternative specifications of Equation 1.3. In columns (2) and (3) we report estimation results for large firms (more than 50 employee) and small firms (less than 50 employees) separately. The employment loss at large firms is very close to our benchmark specifications. Since large firm are less likely to engage in tax evasion activities (see e.g. Kleven et al., 2011), this suggest that our results are not driven by wage under-reporting. The estimated coefficient for small firms is slightly higher than for the large firms, but the difference is not statistically significant. Moreover, in Column (4) we show that our results are robust to including firms with 5-10 employees.

Results using hourly earnings are reported in Column (5). The estimated coefficient on Excess Mass is very similar to our main specification. This is not surprising given that 95% of the employees work full-time in Hungary. In Column (6) and (7) we look at how changing $\bar{W}$ affects our results. Column (6) with a lower threshold ($\bar{W} = 10.8$) is almost the same as our benchmark specification ($\bar{W} = 11$). When we set $\bar{W}$ to a higher level ($\bar{W} = 11.15$) the relationship between Excess Mass and the fraction of workers affected by the minimum wage gets slightly weaker and less precise. In column (8) and (9) of Table 1.3 we explore alternative ways for locating the wage distribution over time. In Column (8) we use nominal GDP growth in the private sector, but the estimated coefficient stays the same. In Column (9) we adjust the wage distribution by the 75th percentile wage growth and the coefficient is slightly lower, but not statistically different from our main specification.[20]

Interpreting the estimated effects in the previous literature. So far we have focused on estimating the relationship between employment change ($\triangle logEmp$) and the wage

[20]Other possible way to adjust earnings are using consumer price increase (CPI) or median earnings growth. Both of these adjustments are problematic though. When we use CPI for making the distributions comparable over time, CPI adjusted earnings still grow by 4-5% yearly rate (approximately by the real GDP growth). When earnings are adjusted by the median earnings we face a different problem. In our main specification $\bar{W}$ is very close to the median wage and so if we adjust the earning distribution by median growth, we force the number of people below the median earnings and so $\bar{W}$ and above it to be the same in each year. This makes the employment change below the median wage (and so $\bar{W}$) by construction the same as the employment change in the whole economy. As we show in equation 1.3, our estimate is basically using the employment change below $\bar{W}$. Therefore, median adjustment, by construction, will give us a similar estimate to Table 2 column (3), where we use the employment change in the whole earnings distribution. It is worth noting that this problem also affects the adjustment by 75th percentile wage growth, though less severely.

change induced by the minimum wage ($\triangle logW$). This differs from the measure that is often reported in the literature: the relationship between a 10% increase in minimum wage ($\triangle logMW$) on employment ($\triangle logEmp$). Our estimate can be transformed to that latter measure if the fraction of people for whom the minimum wage binds is known. For instance, in the U.S. 25% of the teenage population is working at the minimum wage BLS (2013). Suppose that all the 25% of the teenage workers at the old minimum wage gets a 10% increase in their wages, but the wages of the remaining 75% is not affected. Then our estimates indicate that the 10% increase in the minimum wage induce a 2.5% increase in wages, which is translated into $2.5\% * -.18 = -.48\%$ (s.e. 0.48%) employment change. Neumark and Wascher (2010) and Brown (1999) concluded that a 10% increase in the minimum wage decreases the employment of teenagers by 1-3%. Our estimate, therefore, is considerably lower than their preferred estimate, however it should be noted that our sample population is the working-age population rather than teenagers.

1.6 Concluding Remarks

In this Chapter we estimated the employment effects of the 2001 Hungarian minimum wage. We found that the minimum has at most a small negative effect on employment, and the main effect was the pushing up of wages. Our preferred estimate indicates that a 10% increase of the low-wage workers' wage induces a -0.2% decrease in employment. This finding is in harmony with the extensive literature on the minimum wage that finds close to zero effect on employment (Doucouliagos and Stanley, 2009). However, our result demonstrate that this close to zero effect of the minimum wage holds for large and permanent changes in the minimum wage as well.

Our estimates imply that the government can push up wages at the bottom of the wage distribution without considerable employment loss. The point estimate suggests that far more people get the benefit of the wage increase than suffer from the negative consequences of the employment loss. In that context, as it has been recently pointed out by Lee and Saez (2012), the government might want to introduce minimum wage to redistribute income to the low-wage workers even in the presence of optimal taxes and tranfers. However, to assess the welfare implication of minimum wage policies one need to understand where the extra money paid to the workers come from. Who pays for the minimum wage? In the following Chapter we turn to answer that question.

Tables

Table 1.1: Descriptive Statistics - Wage Survey

	(1) unweighted mean	(2) weighted mean	(3) weighted sd	(4) min	(5) max
Panel A - Whole sample					
Male	0.59	0.60	0.49	0	1
Education: high school or more	0.49	0.43	0.49	0	1
Age	40.01	39.97	10.00	23	60
Log earnings*	11.22	11.22	0.66	10.12	13.13
Number of obsevation	110,274	110,274	110,274	110,274	110,274
Panel B - Earn below the 2002 minimum wage					
Male	0.57	0.53	0.50	0	1
Education: high school or more	0.29	0.24	0.42	0	1
Age	38.46	38.48	9.99	23	60
Log earnings*	10.29	10.30	0.18	10.12	10.58
Number of obsevation	20,069	20,069	20,069	20,069	20,069
Panel C -Earn between the 2002 minimum wage and 1.5 times of that					
Male	0.55	0.55	0.50	0	1
Education: high school or more	0.31	0.25	0.43	0	1
Age	39.62	39.50	9.98	23	60
Log earnings*	10.80	10.81	0.12	10.58	11
Number of obsevation	22,116	22,116	22,116	22,116	22,116

* Log earnings are winsorized at the bottom 1% and at the top 99%

Note: This table presents descriptive statistics of worker-level Wage Survey data, 2000 wave. Column (1) presents unweighted means of the listed variables. Columns (2) and (3) present weighted means and standard deviations, using weights reflecting the sampling design of the Wage Survey (see the text for the details). Panel A shows the demographics for the whole sample, Panel B the workers for whom the 2002 minimum wage binds, while Panel C the workers who earn between the 2002 minimum wage and 1.5 times of that. The 1.5 times of the 2002 minimum wage is very close to the $\bar{W}$ that we use in our benchmark specification. Workers with binding minimum wage are more likely to be female, are younger and lower educated. We restrict the sample to workers between the ages of 23 and 60 to mitigate concerns about expansions in higher education over this period that affected those 22 and under, and a 1999 pension reform that affected the over-60 population.

Table 1.2: Group-Level Relationship between Excess Mass and fraction affected

	(1) Average Wage	(2) Excess Mass Classic Approach	(3) Excess Mass Bunching Approach
Panel A: Changes between 2000 and 2001			
Fraction Affected (2001 MW)	0.422	0.941	0.944
	[0.064]	[0.127]	[0.059]
Constant	-0.027	0.005	0.021
	[0.006]	[0.02]	[0.01]
R-squared	0.41	0.00	0.59
% Change in employment		-0.059	-0.056
		[0.127]	[0.059]
Implied Elasticity		-0.16	-0.13
		[0.34]	[0.14]
Panel B: Changes between 2000 and 2002			
Fraction Affected (2002 MW)	0.393	0.854	0.924
	[0.035]	[0.132]	[0.065]
Constant	-0.027	0.012	0.032
	[0.006]	[0.023]	[0.009]
R-squared	0.45	0.01	0.68
% Change in employment		-0.146	-0.076
		[0.132]	[0.065]
Implied Elasticity		-0.36	-0.18
		[0.37]	[0.17]
Panel C: Placebo Test: Changes between 1998 and 2000			
Fraction Affected (2002 MW)	-0.051	-0.100	0.010
	[0.040]	[0.135]	[0.079]
Constant	0.010	0.038	-0.005
	[0.007]	[0.026]	[0.011]
R-Squared	0.01	0.003	0.0001
Thershold, W		Inf	11
Firm-Size	>10	>10	>10
Wage Adjust	nominal GDP	nominal GDP	nominal GDP
Earnings	monthly	monthly	monthly

Note: This table shows the group-level relationship between average wage change (Column 1) and the fraction of workers affected by the minimum wage change. Columns 2 shows the same for the excess mass when $\bar{W}$ is set to infinity and Column when it is set to $\bar{W} = 11$. Panel A shows the changes between 2000 and 2001, Panel B between 2000 and 2002, while Panel C looks at pre-reform changes between 1998 and 2000. (see equation 1.3 in the main text). For the post-reform results we also report the percentage change in employment in response to the minimum wage increase. A positive number means jobs were created, while a negative number means jobs were destroyed. We also calculate the employment elasticity.

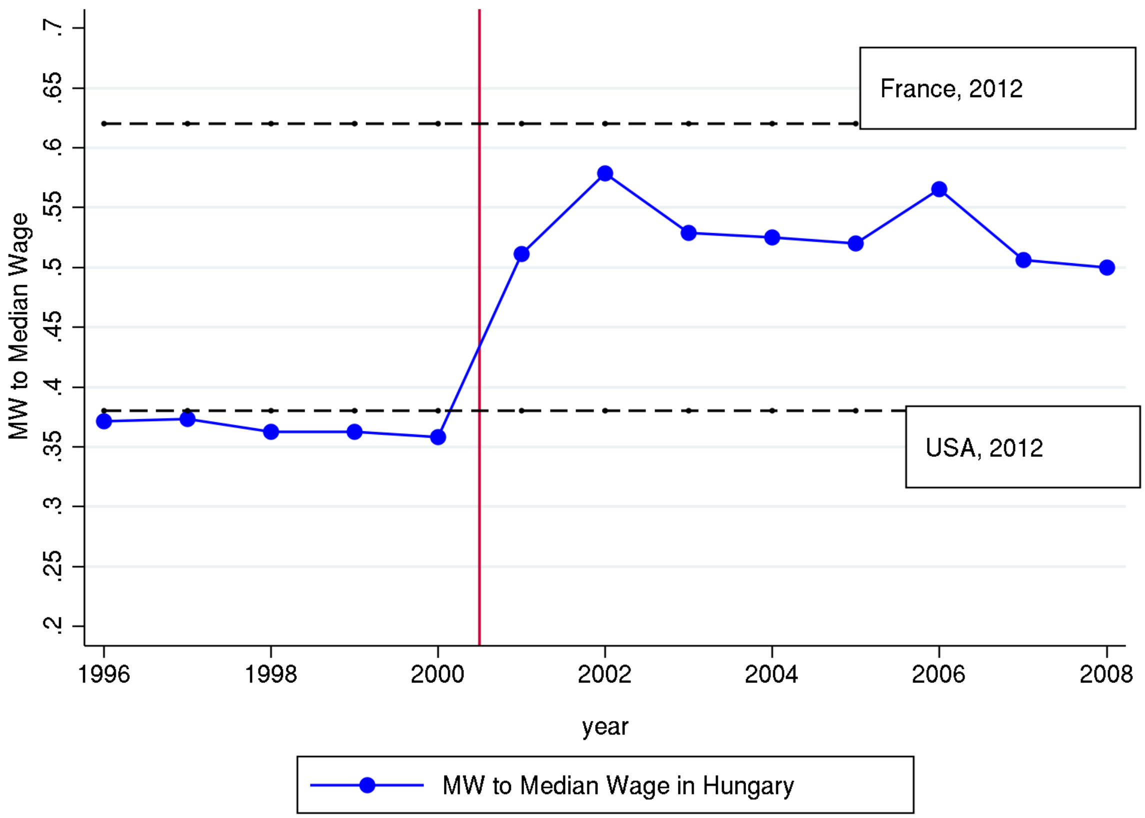
Table 1.3: Group-Level relationship between Excess Mass and fraction affected - Robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Changes between 2000 and 2002									
Fraction Affected	0.944	0.915	1.008	0.924	0.930	0.860	0.856	0.933	0.910
	[0.059]	[0.103]	[0.010]	[0.070]	[0.091]	[0.068]	[0.080]	[0.070]	[0.062]
Constant	0.021	0.008	0.090	0.032	0.010	0.028	0.038	0.035	0.028
	[0.009]	[0.013]	[0.026]	[0.009]	[0.012]	[0.011]	[0.010]	[0.009]	[0.009]
R-squared	0.59	0.55	0.54	0.68	0.52	0.68	0.58	0.67	0.69
Implied Elasticity	-0.13	-0.19	-0.31	-0.12	-0.11	-0.15	-0.41	-0.12	-0.25
	[0.14]	[0.36]	[0.17]	[0.15]	[0.11]	[0.14]	[0.21]	[0.15]	[0.14]
Panel A: Placebo Test: Changes between 1998 and 2000									
Fraction Affected	0.010	-0.15	0.36	0.01		-0.0004	0.06	-0.03	0.05
	[0.079]	[0.010]	[0.080]	[0.080]		[0.070]	[0.090]	[0.080]	[0.071]
Constant	-0.005	-0.019	-0.027	-0.005		0.001	-0.009	-0.015	0.005
	[0.011]	[0.009]	[0.030]	[0.011]		[0.001]	[0.012]	[0.012]	[0.010]
R-Squared	0.0001	0.02	0.14	0.0001		0	0.004	0.001	0.004
Thershold, W	11	11	11	11	11	10.8	11.15	11	11
Firm-Size	Emp>10	Emp>50	50>Emp>10	Emp>5	Emp>10	Emp>10	Emp>10	Emp>10	Emp>10
Wage Adjust	nominal GDP	nominal GDP	nominal GDP	nominal GDP	nominal GDP	nominal GDP	nominal GDP	nominal GDP (private)	75th percent
Earnings	monthly	monthly	monthly	monthly	hourly	hourly	monthly	monthly	monthly

Note: Panel A shows the group-level relationship between excess mass in 2002 (excess mass in 2002 divided by employment in 2000) and the fraction of workers affected by the minimum wage change for different specifications. We report the implied percentage change in employment and the employment elasticity as well. Panel B present results on the pre-reform changes. Column (1) shows our benchmark specification, Columns (2)-(4) explore different firm sizes, Column (5) examines the consequences of using hourly wages, Column (6)-(7) explore different thresholds of $\bar{W}$, while Column (8)-(9) displays the different adjustment of earnings.

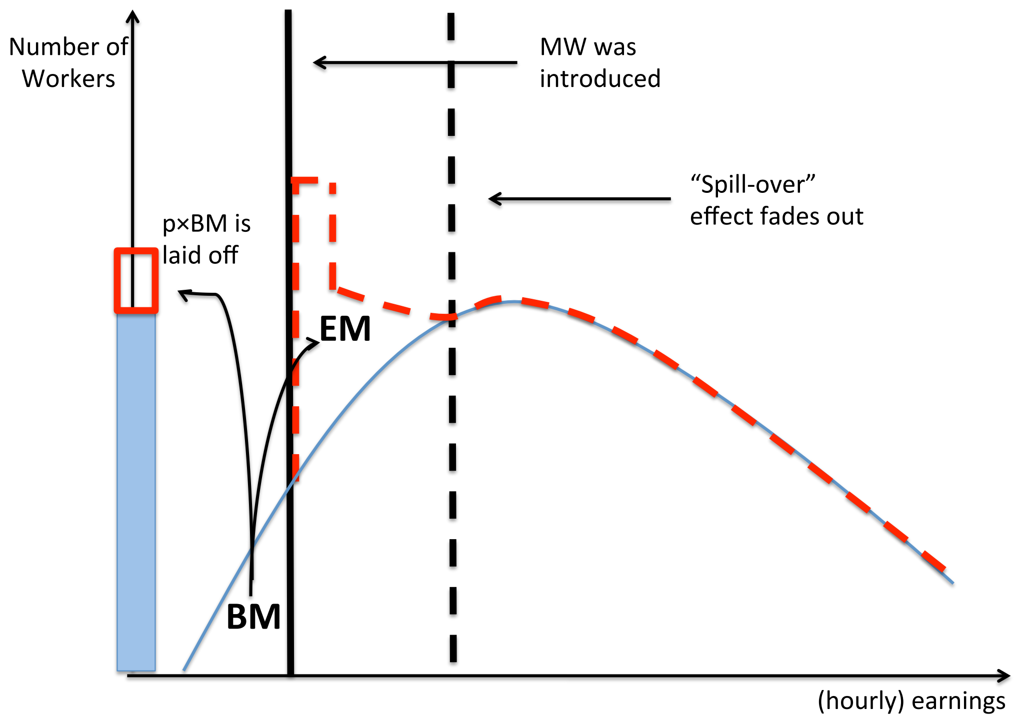
Figures

Figure 1.1: Minimum wage in Hungary



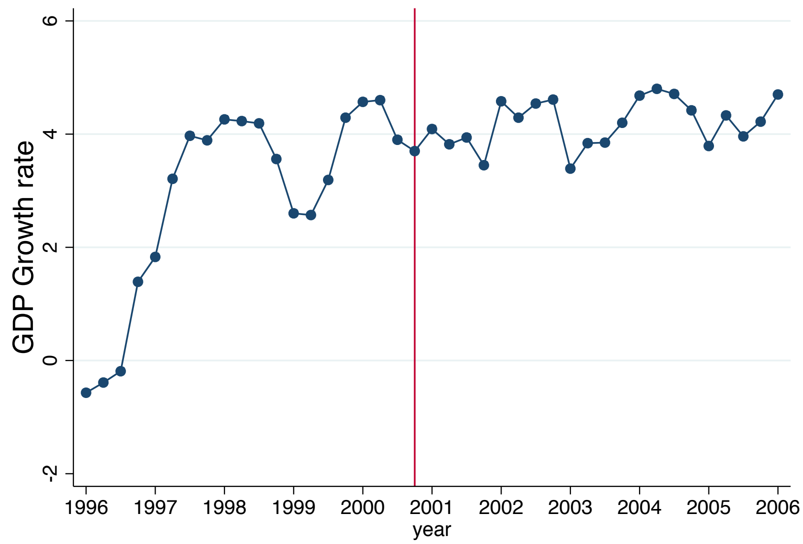
Notes: This figure shows the ratio of the minimum wage to median wage in the private sector for Hungary between 1996 and 2008 (our own calculations). The two dashed lines depict the ratio of the minimum wage to the median wage for France and the U.S. in 2012 (OECD). The graph shows the large and permanent increase in the minimum wage that occurred after 2000.

Figure 1.2: The effect of the minimum wage on (hourly) earnings

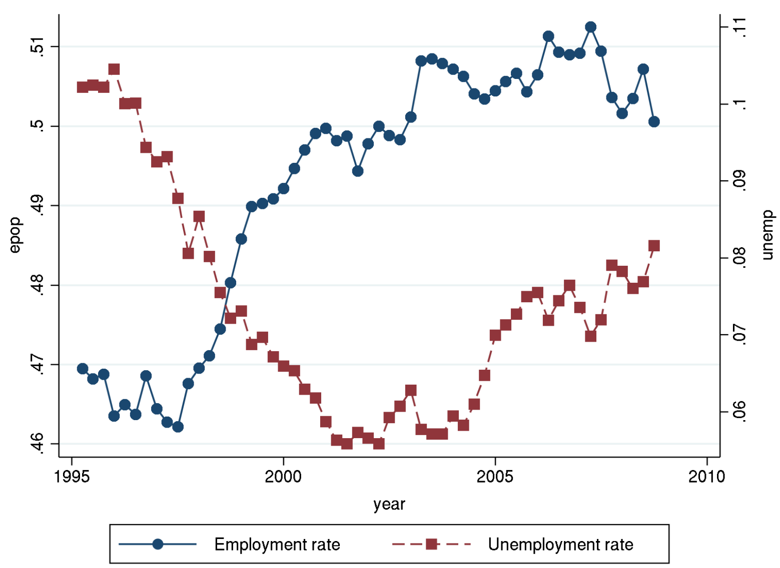


Notes: The effect of the minimum wage on the (frequency) distribution of hourly earnings is depicted here. The blue bar at zero represents workers without jobs before the introduction of the minimum wage, while the blue solid line show the earnings distribution. The number of workers below the minimum wage is denoted by BM. The introduction of the minimum wage can affect these workers in two ways: they get laid off or they get a pay raise. Workers getting the pay raise generate an excess mass, or "bunching", in the new earnings distribution (red dashed line) compared to the old earnings distribution (blue solid line). If the minimum wage spills over to higher wages, then the earnings distribution above the minimum wage is also affected. The vertical dashed black line is $\bar{W}$, the highest pre-reform wage that experiences spillovers. The excess of number of workers (bunching) relative to the number of workers below the new minimum wage (BM in the figure) can be used to estimate the employment effect of the minimum wage.

Figure 1.3: Macroeconomic Trends



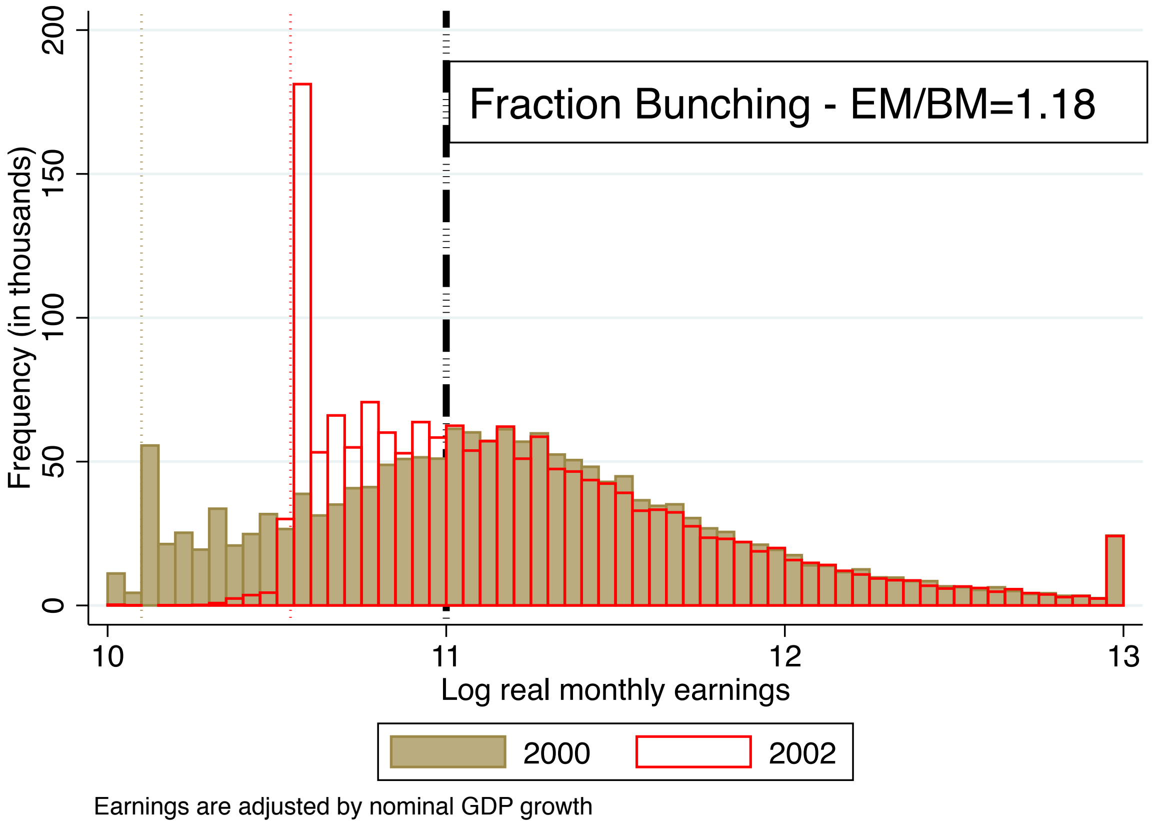
(a) GDP growth



(b) Labor market trends

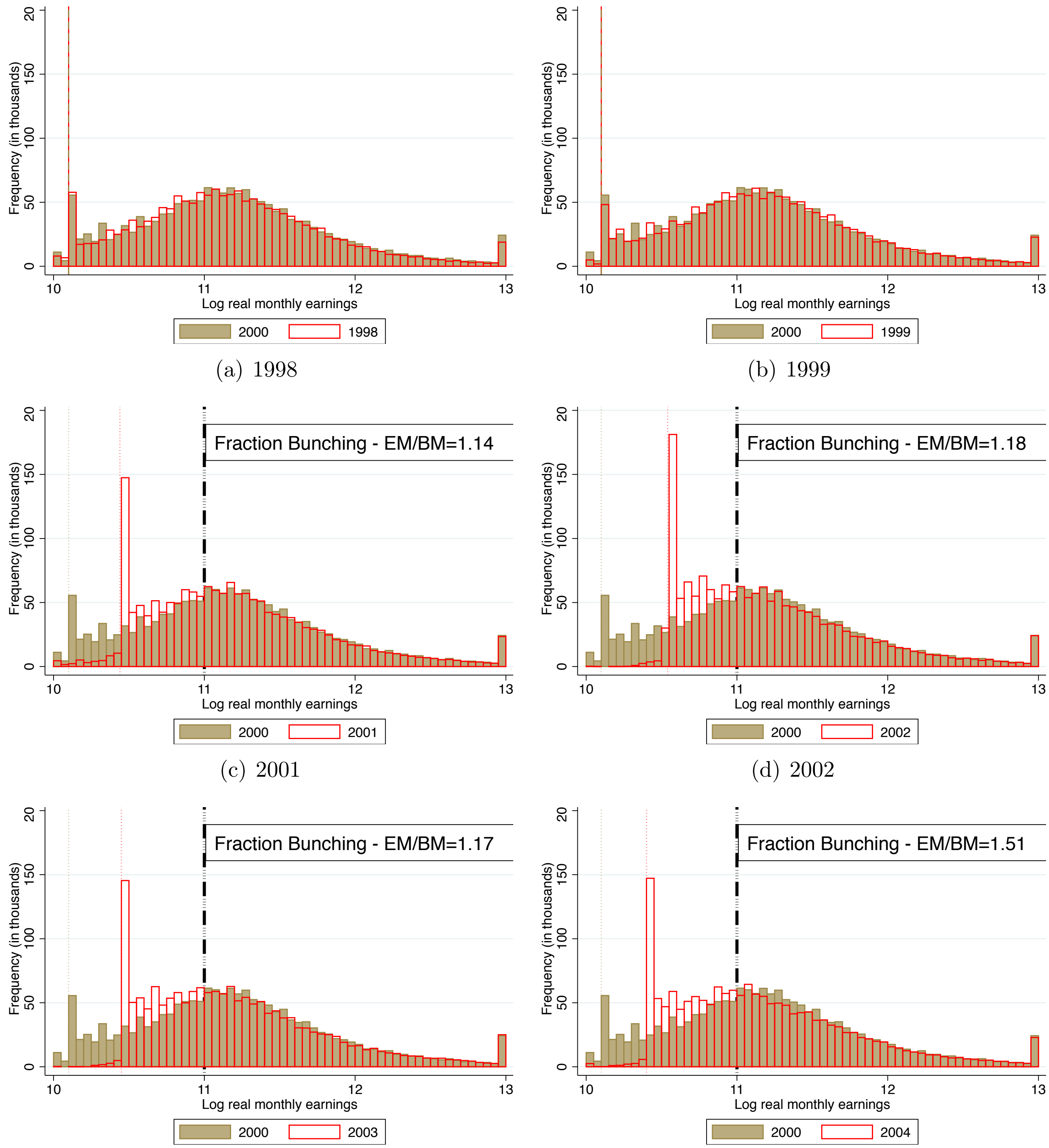
Notes: Panel (a) shows the seasonally adjusted GDP growth rate between 1996 and 2006 in Hungary. The data was obtained from the Hungarian Central Statistical Office. The graph shows that the GDP growth was stable around the examined period. Panel (b) shows the evolution of the employment to population rate and the unemployment rate between 1996 and 2006. There are trends in the employment-to-population and unemployment rates before the reform, therefore, it is hard to use the aggregate data for inference.

Figure 1.4: Log earnings distribution in 2000 and in 2002



Notes: The frequency distribution of monthly log earnings in 2000 (1 year before the minimum wage hike), and in 2002 (2 years after the minimum wage hike) are depicted here. The red bars show the earning distribution in 2002, while the brown filled bars bars display the same in 2000. The dotted brown (red) dashed line is at the bar in which the minimum wage located in 2000 (2002). The vertical dotted dash black line shows the $\bar{W}$ that we use for calculating the excess number of jobs. The graph demonstrates that the minimum wage increase generated an excess mass (bunching) in the 2002 earnings distribution. The size of bunching relative to the number of jobs below the 2002 minimum wage is reported in the top right corner. The estimated fraction is above one indicating that the minimum wage has a positive effect on employment.

Figure 1.5: Evolution of log earnings distributions over time



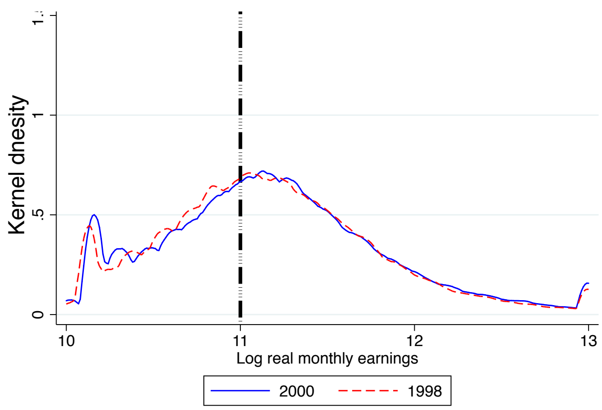
(a) 1998 (b) 1999

(c) 2001 (d) 2002

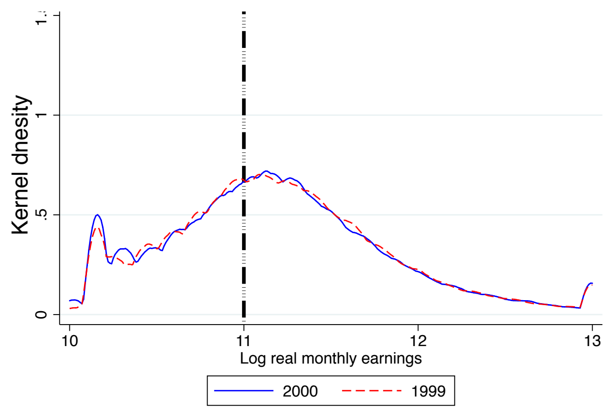
(e) 2003 (f) 2004

Notes: The distribution of monthly log earnings over time is shown here. Each panel shows the earnings distribution in year t (red bars) compared to 2000 earnings distribution (brown filled bars). The graphs show the vertical dotted dash black line $\bar{W}$, while the dotted vertical lines (brown in 2000, red in other years) show the bar where the minimum wage is located in the earnings distribution. As we predicted in Figure 1.2 an excess mass shows up in the distributions after 2000. The excess mass over the below mass (fraction bunching) is reported in the top left corner. This fraction is larger than one in each year after the minimum wage hike. This suggests that the minimum wage had a positive effect on employment.

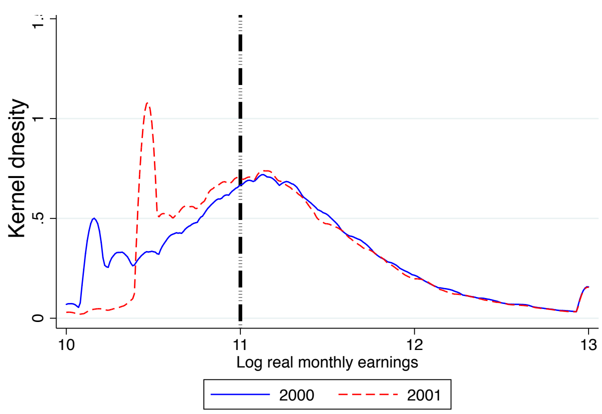
Figure 1.6: Evolution of kernel densities over time



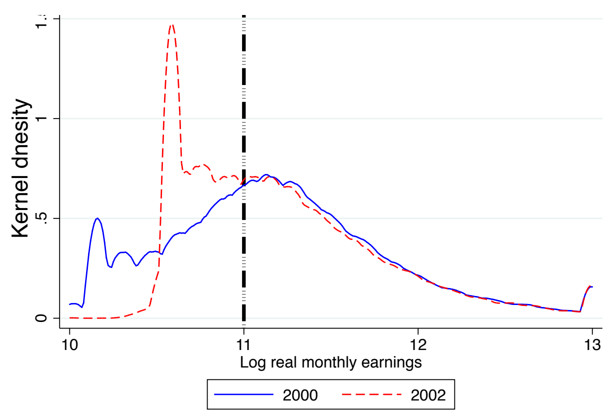
(a) 1998



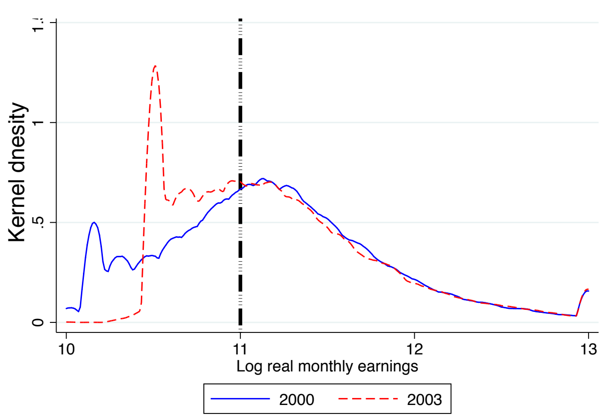
(b) 1999



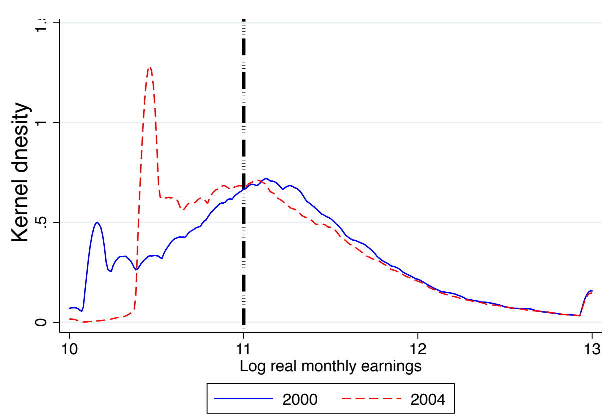
(c) 2001



(d) 2002



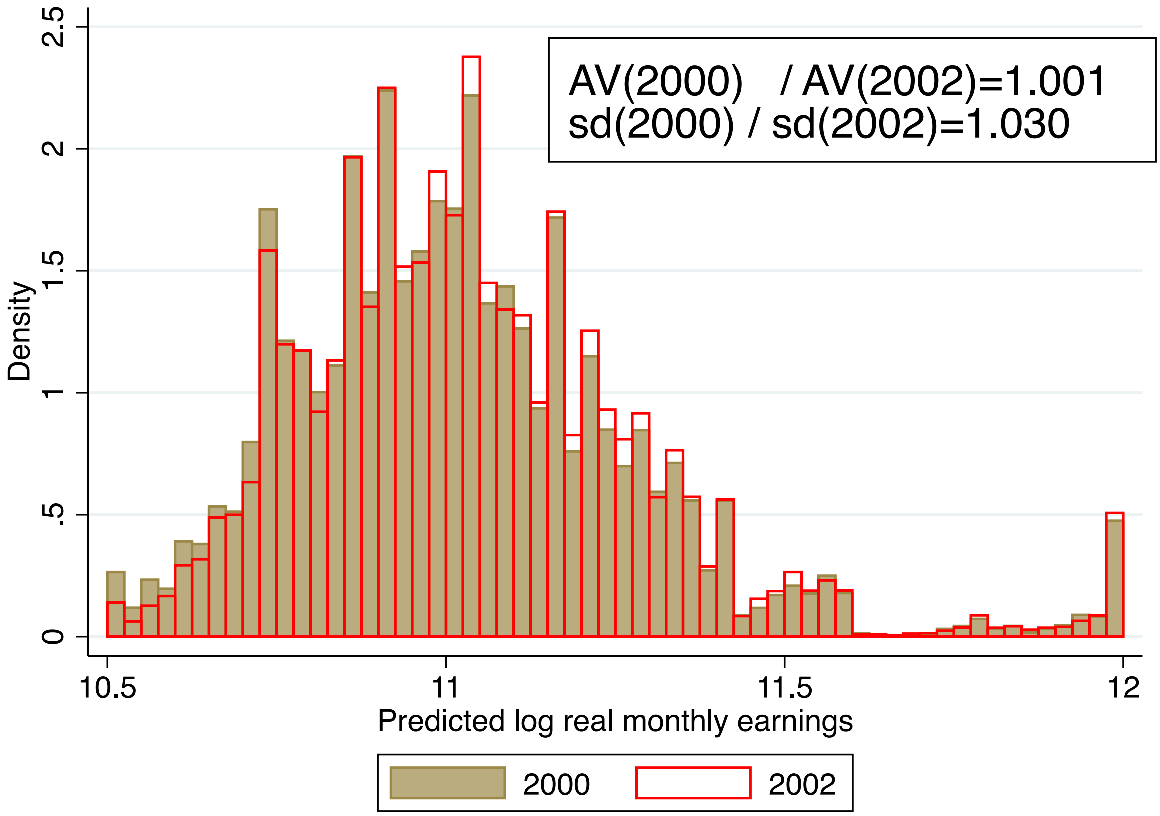
(e) 2003



(f) 2004

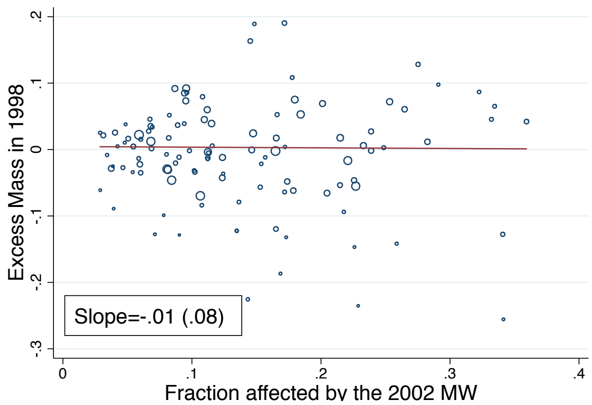
Notes: The kernel density of monthly log earnings over time is shown between 1998 and 2004 (red dashed line) relative to 2000 (blue line). The vertical dotted dash black lines show $\bar{W}$.

Figure 1.7: Predicted Earnings Distribution in 2002 and 2000

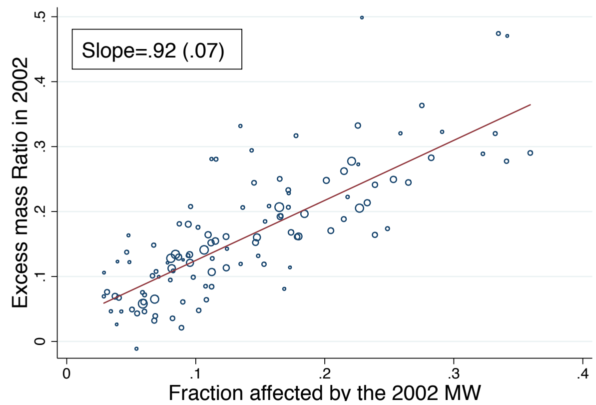


Notes: This figure shows the (density) earning distributions predicted by observables (age, age square, sex, education, region) in 2000 (brown solid bars) and in 2002 (red solid bars) for jobs that earned less than $\bar{W}$. In both years we use the relationship between observables and the earnings in 2000. The differences between the 2002 predicted value and the 2000 predicted value uncover the effect of changes in observables in the earnings distribution. The ratio of means (first line) and the standard deviation (second line) between 2002 and 2000 are reported in the top right corner. This ratio is close to one indicating that the two earnings distributions are very similar and so the observables characteristics in jobs that earned less than $\bar{W}$ in 2002 and in 2000 are very similar.

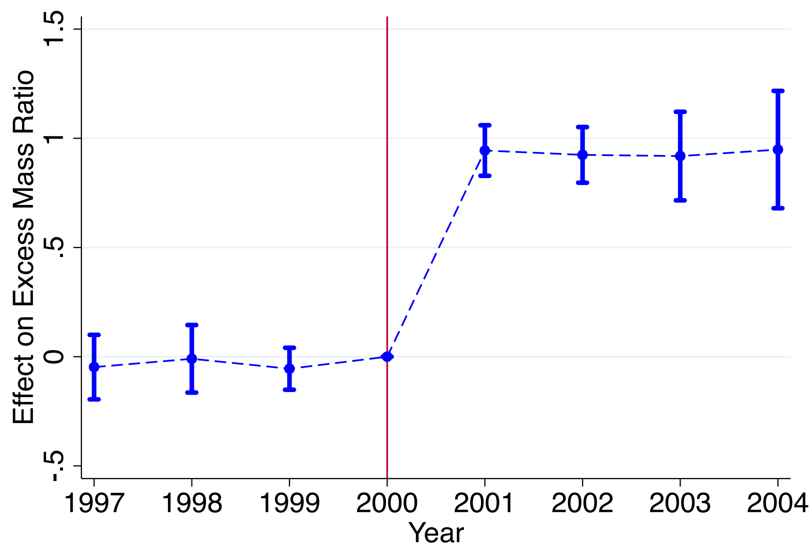
Figure 1.8: Group-level relationship between excess mass and fraction affected



(a) Placebo Test



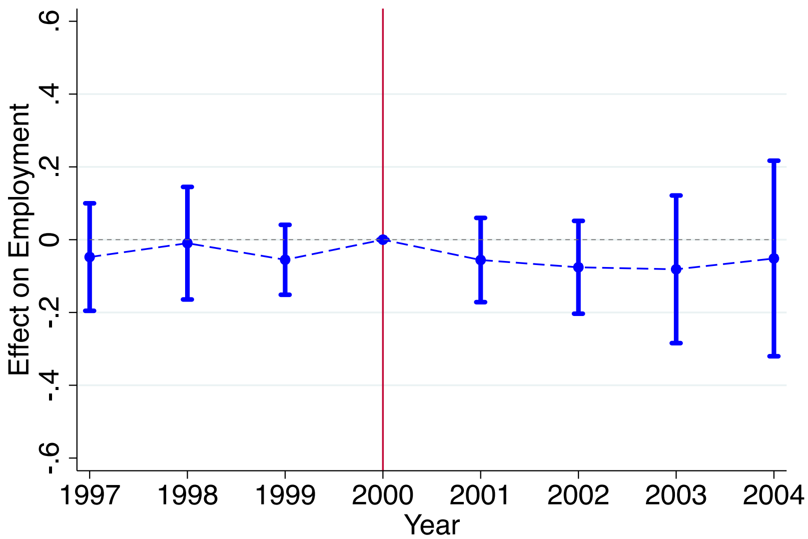
(b) Main Results



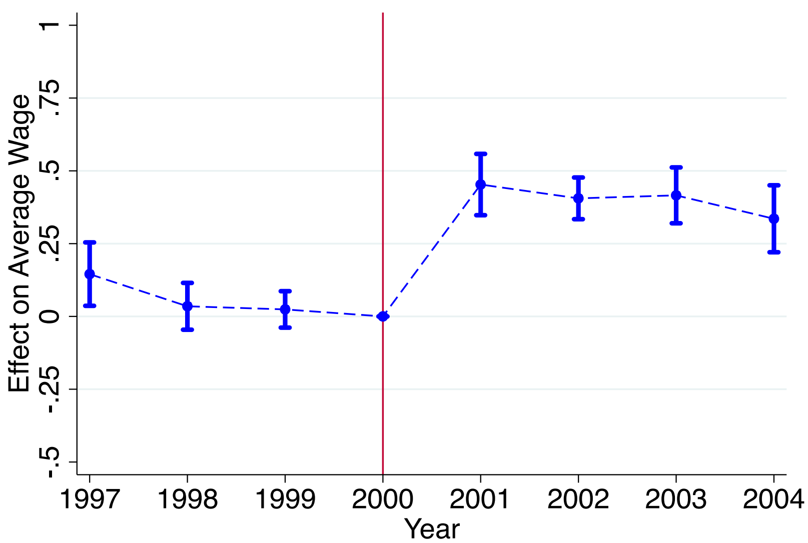
(c) Relationship over time

Notes: This graph depicts the group-level relationship between Excess Mass ratio (excess number of workers divided by employment in 2000) and the fraction of workers affected by the minimum wage. Panel (a) shows the scatter plot of the group-level relationship between Excess Mass in 1998 and fraction of workers affected by the 2002 minimum wage. The solid red line is the linear fit (weighted by employment in 2000) and its slope is shown in the bottom left corner. Panel (b) shows the scatter plot of the group-level relationship between the excess mass in 2002 and the fraction of workers affected by the 2002 minimum wage. The solid red line is the linear fit (weighted by employment in 2000) and its slope is shown in the top left corner. Panel (c) depicts the relationship between the Excess Mass and the fraction of workers affected by the minimum wage over time. The pre-2000 years show the slope of a regression, where the dependent variable is the Excess Mass in year t and the independent variable is the fraction of workers affected by the 2002 minimum wage. The post-2000 years show the slope of the regression of Excess Mass in year t on the fraction of workers affected by year t minimum wage.

Figure 1.9: Effect on Employment and on Average Wage



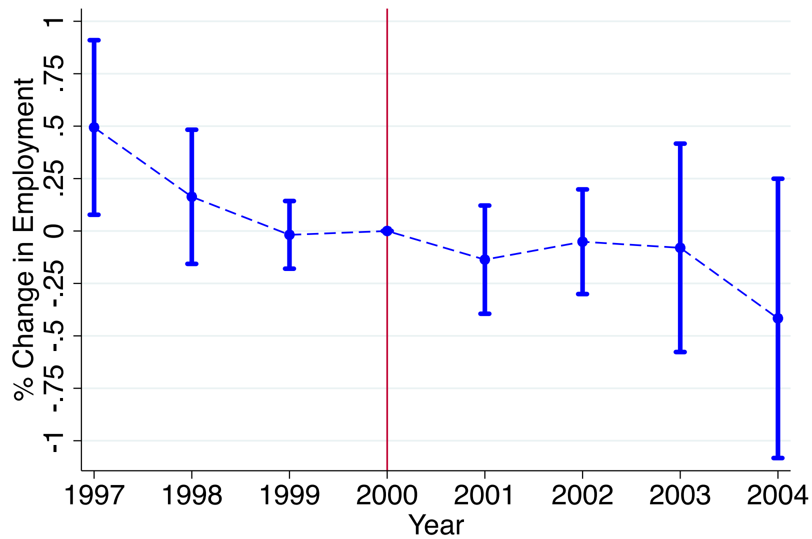
(a) % Change in Employment



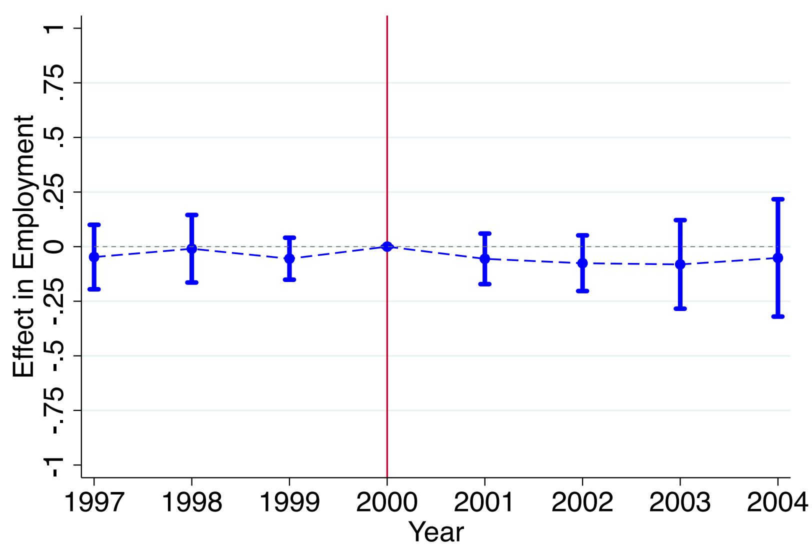
(b) % Change in Average Earnings

Notes: Figure 1.9 summarizes the main results of our bunching analysis. Panel (a) transforms the results shown in Figure 1.8 Panel (c) into a percentage change in employment. For the post minimum wage years (after 2000), we take the estimated effect on Excess Mass and subtract one from it. For the pre-2000 years we report the relationship between the Excess Mass and the fraction of workers affected by the 2002 minimum wage. This test uncovers whether there are any changes in employment in the relevant earnings range before the minimum wage hike. In Panel (b) we show the relationship between the change in the average wage and the fraction of workers affected by the minimum wage. In the years leading up to the reform, the relationship between the change in average wage and the fraction of workers affected by the 2002 minimum wage is plotted. In the post-reform years the relationship between the change in average wage at year t and the fraction of workers affected by the year t minimum wage is reported. The ratio of Panel (a) and Panel (b) gives us the labor demand elasticity (with respect to wage).

Figure 1.10: Comparison of main specification and the standard approach



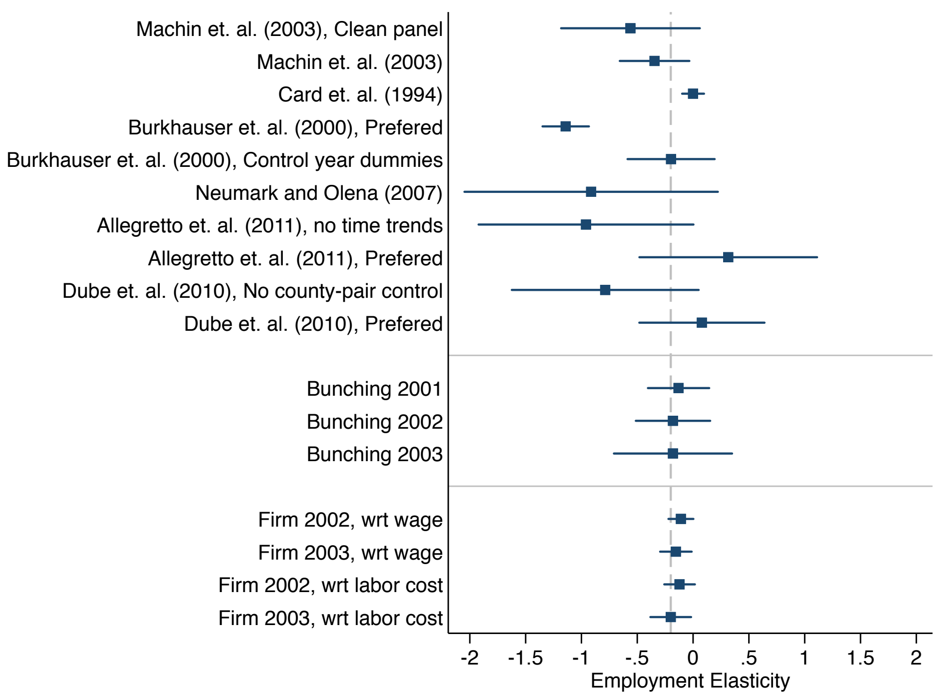
(a) Standard Approach, $\bar{W} = \infty$



(b) Main Specification, $\bar{W} = 11$

Notes: Panel (a) shows the implied percentage change in employment when $\bar{W}$ is set to infinity. As we described in the text, this specification is equivalent to estimating the group-level relationship between the fraction affected by the minimum wage and the change in employment. In Panel (b) we show the estimated effects using the bunching approach (see Figure 1.8 Panel (c) for the details). This specification is equivalent to estimating the relationship between the fraction of workers affected by the minimum wage and the change in employment below $\bar{W}$. Comparing Panel (a) with Panel (b) reveals the advantage of using the bunching approach here. In Panel (a) there is a relationship between exposure to the minimum wage and the changes in employment even before the minimum wage hike. However, this relationship disappears once we trim out the high earners as is shown in Panel (b).

Figure 1.11: Labor Demand Elasticity in the literature and in this paper



Notes: This figure summarizes the minimum wage implied employment elasticities estimated in the literature and the ones estimated in this paper. The dashed vertical line shows our preferred estimates for the employment elasticity, which is -0.2. In cases where employment elasticity was not directly reported we used the delta method to obtain the standard errors.

Chapter 2

Who Pays for the Minimum Wage?

with Péter Harasztosi

2.1 Introduction

In Chapter 1 we documented that the main effect of the minimum wage hike in Hungary was pushing up wages, while the effect on employment was limited. These results raise the question: who paid for the wage increase? In models where firms have market power in the labor market, firm owners can earn substantial profit on low-paid workers (Manning, 2003; Card and Krueger, 1995). These models predict that (a large) increase in the minimum wage lowers profits and so the incidence of the minimum wage falls more heavily on the firm-owners. As opposed to this, firms operating in competitive markets do not have "extra" resources to finance the minimum wage increase. Having no other choice, these firms must raise their output prices and pass through the effect of the minimum wage to consumers. In Chapter 2, we assess the relevance of these models by scrutinizing firm-level responses to the minimum wage change. In particular, by comparing highly exposed to less exposed firms, we estimate the effect of the minimum wage on employment, total labor cost, profits and sales.

We start our analysis by estimating the effect of the minimum wage on average wages and employment at the firm-level. We find that the minimum wage has a large positive effect on wages, and a small, however, statistically significant negative effect on employment. Both of these estimates are within the confidence interval of the results presented in Chapter 1. We also show that the average cost of labor increases less than average wages. This difference arises because firms cut non-wage benefits (e.g., subsidized meals, firing and hiring costs) in response to the minimum wage. We calculate that the increase in workers' earnings is mitigated by about 20% through cuts to non-wage benefits. Taking this effect into consideration we find that a 1% wage increase induced by the minimum wage leads to -0.2% (s.e. 0.09) decrease in employment.

The limited employment response to the increase in the cost of labor indicates that firms' total labor cost must increase in response to the minimum wage. We show that this is indeed the case. Total labor cost at highly exposed and non-exposed firms follows a parallel trend before the minimum wage hike, and this trends breaks exactly at the timing of the reform: highly exposed firms experience a substantial increase in their total labor cost. We also scrutinize the effect on profits and sales. We show that accounting profit did not decline in response to the minimum wage increase, while sales increased substantially. This indicates that firms passed through the effect of the minimum wage to consumers. Finally, we look at whether the effect of the minimum wage varies according to firm-level characteristics. Our most interesting finding is that firms operating in elastic product demand markets (exporting and manufacturing) suffered much bigger losses in employment than firms in more inelastic product demand (non-exporting and service) markets.

We explain these findings in the standard competitive model, where firms have three inputs (capital, labor, intermediate goods and services). Assuming constant returns to scale, the product demand elasticity and the elasticity of substitution between labor and other inputs determine the employment responses and the effect on sales. We estimate the model with a minimum-distance estimator, matching the estimated empirical firm-level responses to the predictions of the model. Our estimates imply an elasticity of substitution between capital and labor of 0.33 (s.e. 0.46), which is the range of the previous the literature that goes from 0.36 in Chirinko et. al (2011) to 1.25 in Karabarbounis and Neiman (2014). Moreover, we find that our estimates imply a product demand elasticity of 0.46 (s.e. 0.47). This demand elasticity is smaller than the conventional estimates for the uncompensated demand elasticity often used for calibrations (see Aaronson and French, 2007). However, in our framework, where the increase in the minimum wage increases the purchasing power of workers, the compensated demand elasticity is a more appropriate concept to use (Harberger, 1962). The latter is found to be fairly low in some contexts. For instance, Seale et al. (2003) estimated that it is between 0.03 and 0.2 for food consumption. Finally, we find that the elasticity of substitution between labor and intermediate goods is 0.16 (s.e. 0.12). This estimate is also within the range of the existing estimates (see Atlay, 2014).

The key prediction of the competitive model is that the disemployment effect of the minimum wage is larger in markets where firms face an elastic labor demand. Consistent with this, we find that exporting and manufacturing firms experienced larger disemployment effects. Moreover, our model is also in line with a handful of papers analyzing the price pass-through effect of the minimum wage (see Lemos, 2008 and MaCurdy, 2014). These papers find a strong positive relationship between prices and changes in the minimum wage, just as the neoclassical model predicts (Aaronson and Eric, 2007; Aaronson et al., 2008).

This chapter relates to the small number of papers estimating the relationship between minimum wage changes and firm profitability. Card and Krueger (1995) found no effect on stock market outcomes in the U.S. while Darca et al. (2011) found a significant negative effect on firm profitability in the U.K. Both of these papers looked at considerably smaller changes in the minimum wage. Since capital is costly to adjust, small shocks might only uncover short term responses. One virtue of our set-up is that since the large and permanent increase in the minimum wage forces firms to re-optimize quickly, we are more likely to capture long-term responses here.

This chapter proceeds as follows. In Section 2, we describe the corporate income tax data that we mainly use in this Chapter. In Section 3 we describe our empirical strategy, while in Section 4 we discuss the result. Section 5 introduces a competitive model of firms that can explain our findings, and we also estimate the key parameters of this model, while Section 6 concludes.

2.2 Data

In addition to the Wage Survey described in Chapter 1, in this Chapter we use the Corporate Income Tax Data (CIT) from Hungary. This data contains information on firms' balance-sheet and income statements and so it allows us to assess firms' income and cost structure, wages and personnel costs and material expenses. One key advantage of the CIT data is that it contains information on the universe of firms with double book-keeping and, therefore, allows us to follow firms over time. However, the CIT does not contain information on worker-level wages. Therefore, we use the Wage Survey to calculate firm-level exposure to the minimum wage. We measure exposure by calculating the fraction of workers who earn below the 2002 minimum wage in the most recent pre-reform year with at least 5 workers observed.

We omit sectors that are heavily regulated and/or have unreliable balance sheet information. In particular, we leave out agriculture, mining, tobacco, oil and refining, energy sectors, water and air transport, telecommunications and finance and government related sectors, such as education. We also omit state owned firms. In the final sample, we have 5459 firms which constitute 4% of the annual firm population. Given the sampling structure of the Wage Survey we over-sample larger firms. To make our sample representative at 1-digit and by firm size we weight our regressions. [1] Moreover, for large firms we do not observe all workers in the Wage Survey which induces a measurement error in our exposure variable, FA_i. To alleviate the potential bias caused by this we adjust the weights by multiplying them with the proportion of workers observed in the Wage Survey.

Table 2.1 summarizes the firm-level sample. The first column shows the unweighted means of the main variables, while the second the weighted ones. Since large firms are over-sampled in the Wage Survey, the weights change the composition of the sample substantially. However, our results are robust to not weighting the regressions. The fraction of affected workers in the last row measures the exposure to the minimum wage. For an average firm, 36% of the workers earn less than the 2002 minimum wage. This number is higher than the exposure at the worker level, which is 10%, because large firms with low exposure employ more workers.

Table 2.2 shows the key balance sheet items relative to sales by industry for our main sample in 2000. The labor cost is only 15% of total sales and there is substantial variation across industries (8% in retail vs. 20% in manufacturing). The largest share of expenses (~70%) is related to intermediate goods used for production or purchased for resale. The average profit margin, measured by the ratio of earnings before interest and taxes (EBIT) to sales, is 3.3% of sales, which is close to the European average (3.8%, see. Table 1, Panel B in Lyandres et. al., 2013).

2.3 Empirical Strategy

We start our analysis by estimating the effect of the minimum wage on average wage, average cost of labor and employment. Our empirical strategy compares highly exposed and non-exposed firms four years before and four years after the minimum wage hike. We estimate different versions of the following regression model

$$\frac{y_{it} - y_{i2000}}{y_{i2000}} = a_{st} + \beta_t FA_i + \gamma_t X_{it} + \varepsilon_{it} \tag{2.1}$$

where the left hand side is the percentage change in outcome y between year 2000 and year t. We winsorize the percentage change, $\frac{y_{it}-y_{i2000}}{y_{i2000}}$, at -1 (-100%) and +1 (+%100). The right

[1] Our weights make the sample representative at employment size categories (5-20, 20-50, 50-300, more than 300) and at one-digit NACE industry level

hand side of this regression consists of the following variables: a_{st} is 2-digit NACE industry effects, FA_i is the fraction of workers for whom the 2002 minimum wage binds, while X_{it} is a set of firm characteristics. In our benchmark regression we control for export share and its square in 1997. We restrict our sample to firms that had at least 3 employees between 1997 and 2000. [2]

We estimate equation 2.1 for employment and total labor cost by imputing -1 (-100% decline) for firms that died. However, for these firms we do not observe average wage and average cost of labor. Therefore, we show the effect on these variables by restricting the sample for those firms that survived until 2004. However, firms that die might not be randomly selected. To deal with that issue we compute the selection corrected average wage by following Johnson et al. (2000).[3] The key identification assumption of this procedure is that firms that died would have been above the conditional median of the wage change.

To calculate the elasticities[4] with respect to wages (cost of labor) we divide the estimated β_t from equation 2.1 by the β_t selection corrected average wage (cost of labor). The standard errors are calculated from 1000 bootstrap replications.

To inspect the incidence of the minimum wage, we look at the effect on profits and sales. Since firms sometimes make negative profits, it is hard to calculate the percentage change here. Instead, we divide the change in outcome with average sales between 1997 to 1999.[5] In particular, we estimate the following equation:

$$\frac{y_{it} - y_{i2000}}{\frac{1}{3}\sum_{k=1997}^{1999} Sales_k} = a_{st} + \beta_t FA_i + \gamma_t X_{it} + \varepsilon_{it} \tag{2.2}$$

The right hand side is the same as for equation 2.1. The left hand side is the change in outcome y relative to the average sales between 1997 to 2000. Similar to our previous analysis, we include all firms in the regression, regardless of their survival through the sample period. We focus here on outcomes such as total labor cost, operating profits,[6] total labor cost, sales and materials. To calculate the elasticities with respect to wages (cost of labor) we divide the estimated β_t from equation 2.2 by the mean of the dependent variable, $\frac{y_{it} - y_{i2000}}{\frac{1}{3}\sum_{k=1997}^{1999} Sales_k}$, and by the β_t of the selection corrected average wage (cost of labor) . The standard errors are calculated from 1000 bootstrap replications.

Finally, we also explore the presence of heterogeneous responses by estimating the following regression model:

$$\frac{y_{it} - y_{i2000}}{\frac{1}{3}\sum_{k=1997}^{1999} Sales_k} = \alpha_t + \beta_t^1 FA_i + \beta_t^2 FA_i * SubGroup_i + SubGroup_i + \varepsilon_{it} \tag{2.3}$$

[2] For firms born after 2000 we cannot observe FA_i, the fraction of workers for whom the minimum wage binds. Therefore we decided not to allow firm birth in in the years before 2000 either.

[3] In particular, the change in average wage (cost of labor) was imputed to be 100% for firms that die. Then we estimate equation 2.1 with a least absolute deviation (LAD) on the total sample.

[4] The elasticity is the effect of a 1% minimum wage induced increase in wages (or cost of labor) on the percentage change in different outcomes such as employment.

[5] Since the reform was announced in March 2000, the year 2000 might be affected by the minimum wage. Therefore, we decided not to use that year when we calculating the average sales. However, the results are robust to use year 2000 sales as well. Also we decided to use three years average to minimize the measurement error in the outcome variable.

[6] Operating Profit is the earnings before interest and taxes. Formally, $EBIT = Sales - MAT - TotalLaborCost - OtherExp - Depreciation$

$Subgroup_i$ is a dummy variable indicating which group the firm belongs to. We report parameter β_t^1 and the sum of parameters β_t^1 and β_t^2 with the appropriate standard errors.

2.4 Results

2.4.1 Effect on Employment and Cost of Labor

We start our analysis by investigating the effect of the minimum wage on average wage, average cost of labor, and employment. In Table 2.3, Panel A we show the estimated coefficient from equation 2.1 for percentage change in average wage. Column (1) shows that firms where 100% of the workers earned below the new minimum wage in pre-reform years, experienced a 44% higher average wage increase between 2000 and 2002 than a firm with no workers below the minimum wage. These effects are only slightly affected by including control variables or industry effects. In Column (3) and (4) results on changes between 2000 and 2003 are reported. The effect on average wage is slightly lower here (39% vs. 45%), which matches well the pattern of the minimum wage showed in Chapter 1, Figure 1.1. In the last two columns of Table 2.3 we show the changes in average wages between 1998 and 2000 to test the presence of different trends in the pre-reform years. The estimated coefficients are 3.3% (3% without controls). Even though this is a statistically significant difference, it is very small relative to the observed post-reform effects.

In Panel B we report the effect on the average cost of labor. This differs from the average wage because the cost of labor includes non-wage benefits (e.g. subsidized food, firing and hiring costs) and social security contributions. The coefficients of the fraction of workers affected by the minimum wage are consistently lower for average cost of labor than for the average wage (e.g. in column (1) 37% vs. 45%). This indicates that firms cut back their non-wage benefits to mitigate the cost increase caused by the minimum wage.

To get a better overview on the relationship between fraction affected and average wage (average cost of labor), in Figure 2.1, Panel (a) we plot the estimated coefficients for each year between 1997 and 2000. The graph confirms the findings in Table 2.3. First, the effect of fraction affected is negligible in the pre-reform years. Second, the effect on average wages matches extremely well the evolution of the minimum wage shown in Chapter 1, Figure 1.1.

In Table 2.3 Panel C we show that the minimum wage has a small negative effect in employment that is statistically significant. The point estimates in column 2 suggests that 4.6 (6.3) out of 100 workers were laid-off by 2002 (2003) in response to the minimum wage. The last two column in Table 2.3 shows that exposure to the minimum wage is not related to employment changes in the pre-reform years. This supports our identification assumption that highly exposed and non-exposed firms would have behaved similarly in the absence of the minimum wage shock. In Figure 2.1, Panel (b) we show the estimated effects over time. Again, the graphs show that except for 1997, the employment changes are very similar in the pre-reform years. Another interesting findings is that the short-term employment effects (3 workers out of 100 in 2001) are smaller than the long-term effects (6 workers out of 100 in 2003).[7] Finally, in Figure 2.2 we present non-parametric binned scatter plots of the relation-

[7]The point estimates for the bunching analysis presented in Chaper 1 do not show an increasing negative effect on employment over time. However, the standard errors for the bunching estimation increase over time, which makes it difficult to compare short term and long term effects.

ship between the percentage change in employment and the fraction of affected workers for the years 1998, 2001, 2002 and 2003. The graph show that the relationship estimated in 2.1 Panel (a) is close to linear.

In Table 2.3 Panel E and F, we transform our estimates to employment elasticities. One key advantage of our data is that we can separate the employment elasticity with respect to wages, which measure the effect of a 1% increase in *wage* induced by the minimum wage ($\Delta logW$) on employment ($\Delta logEmp$), from employment elasticity with respect to the cost of labor , which measures 1% increase in *cost of labor* induced by the minimum wage ($\Delta logCoL$)) on employment ($\Delta logEmp$). The results in Column 2 and Column (3) highlight that employment elasticity is between 0.10 (in 2002) and 0.15 (in 2003) with respect to wages and between .12 (in 2002) and .20 (in 2003) with respect to cost of labor. Therefore, the elasticities are 20% larger with respect to cost of labor than with respect to wages.

A key limitation of our regression approach is that we do not observe the fraction of workers affected by the minimum wage for firms born after 2000. Therefore, our regressions are restricted to firms that existed in 2000 and so we implicitly rule out the effect of the minimum wage on entry rate. To get a rough idea on the importance of this channel, we estimate the relationship between exposure to the minimum wage and firm entry rate at the 2-digit NACE industry-level. In Figure 2.3 we plot the exposure to the minimum wage (in 2000) and the entry rate for each year between 1998 and 2003. The graphs shows no indication of changes in the entry rate after 2000.

2.4.2 Effect on Sales and Profits

The large effect on wages and the limited effect on employment implies that total labor cost must have increase. In Table 2.4 Panel D, we show that this is indeed case. The estimated coefficients of fraction affected highlight that firm-level labor costs increased by 25% (17%) between 2000 and 2002 (2003) in response to the minimum wage. Moreover, the last two columns of Table 2.3 Panel D show again that the parallel trend assumption holds in the pre-reform years.

The elevated level of labor cost must be financed from somewhere. To inspect who bears the incidence of the cost increase, we estimate the effect of the minimum wage on the different items of firms' balance sheets using equation 2.2. The main results are shown in Table 2.4. Panel A presents the results on total labor cost based on equation 2.2. The key difference here relative to the previous paragraph, where we showed results using equation 2.1, is that we use the change in total labor cost relative to average sales in the pre-reform years instead of using the percentage change. The estimated coefficients show that firms highly exposed to the minimum wage experienced a 3% (2.4%) points bigger increase in their labor cost to average sales ratio between 2000 and 2002 (2003) than non-exposed ones. This effect comes from two sources. First, as we have shown in the previous paragraph, labor cost increased by 25% (17%) by 2002 (2003). Second, the share of labor expenses to total sales is around 17% in our sample, which translates the 25% (17%) increase in labor cost to an $25\% * .15 \approx 3\%$ ($17\% * .15 \approx 2.4\%$) point change in labor cost relative to average sales. This latter calculation also highlights that the key reason beyond the small percentage point increase in labor cost to sales ratio is that expenses on labor only constitute a small part (17%) of total expenses. However, this also indicates that even a large wage shock will have a moderate effect on firms behavior.

In Table 2.4 Panel B the effects on sales (revenue) are reported. Sales relative to pre-reform average sales increased by 4.6% between 2000 and 2002 in response to the minimum wage. The increase in sales is larger the the increase in labor cost (3% for labor cost vs. 4.6% for sales), but since the effect on sales is noisly estimated, the difference is not statistically different from zero. Moreover, the effect on sales is quite a bit lower in 2003 (1.5%) than in 2002.

In Table 2.4. Panel C we report the estimates on profits. Contrary to the results on sales, profits do not seem to decrease in response to the minimum wage. If anything, profits slightly increased, but the effects are never significantly different from zero. The last two columns in the table highlights that there is a significant albeit small changes in profits between 1998 to 2000. However, even if we compare the changes after 2000 relative to pre-2000, only 1/3 of the labor cost increase can be explained by shrinking profits. In Figure 2.5 we also present the non-parametric binned scatter plots of the relationship between changes in profit ratio (profit over average sales between 1997 and 2000) and the fraction of workers for whom the minimum wage binds (FA_i) for year 1998, 2001, 2002, and 2003. The graphs show that the relationship estimated on 2.4 Panel (b) is close to linear.

In Table 2.4 Panel D we scrutinize the effect on intermediate goods and services. Even if sales are constant, firms can spend more on labor related expenses, if spening on intermediate goods and services decrease. This situation can arise if firms cut back their production and use less materials. The effect on intermediate goods is very close to zero in 2000 (1%) and goes negative in 2003 (-1%). This indicates that after 2002 the elevated level of labor cost was partly financed from the lower spending on materials.

In Figure 2.4 we summarizes the key results of this section. We plot the estimated coefficients of fraction of affected workers from equation 2.2 for each year between 1997 and 2004. Panel (a) shows that firms' total labor cost (relative to average sales) evolves parallel before 2000, but this trend breaks afterwards: total labor cost increases substantially in response to the minimum wage. The graph also highlights that the pattern of labor cost closely matches the evolution of minimum wage depicted in Chapter 1, Figure 1.1. Panel (b) in Figure 2.4 shows the effects on profit. Even though profits at the minimum wage firms increased slightly from 1999 to 2000, the plot shows clearly that the labor cost increase is not financed from lower profits. On the contrary, Figure 1.1 Panel (c) highlights that sales increased considerably in the first 3 years after the minimum wage hike, indicating that firms financed the minimum wage from sales, at least in the short-term. In the medium-term (e.g. four years after) the sales differences between highly exposed and non-exposed firms dissipates. However, Figure 1.1 Panel (d) shows that intermediate goods shrank considerably between 2000 and 2004 indicating that the elevated labor cost were financed out of the declining expenses on the intermediate goods and services.

Finally, we also examine the effect of the minimum wage hike on capital in Table 2.4 Panel E. The effect on capital stock[8] is always very close to zero, indicating that firms do not readjust their stock of capital in response to the minimum wage.

[8]Real capital is calculated with the perpetual inventory method which sums up a series of real investments over the life cycle of firms. The real investment is computed as the change in fixed and immaterial assets plus depreciation deflated by 2 digit NACE industry investment price indices provided by the Statistical Office.

2.4.3 HeterogeneousResponses

Table 2.5 explores the heterogeneity in responses to the minimum wage change. Since some of the variables in balance sheets are noisily estimated (e.g. sales, materials), to minimize the measurement error we compare the average of year 2002 and year 2003 to year 2000 when we calculate the change in outcome variables relative to sales. Each column represents one of our key variables, while the horizontal panels explore heterogeneous responses to the minimum wage hike by various subgroups. All of our results are only indicative of a minimum wage hike effect if the affected and unaffected firms do not show different behavior before 2000. Table 2.6 reports the placebo tests that scrutinize the changes between 2000 and 1998. For most variables and specifications we find no indication of different pre-trends.

Table 8 Panel A repeats the results for the whole sample already reported in Table 2.5 or in Column (2) in Table 2.5. In Panel B we compare effects across two broad sectors, manufacturing and services. While the effect on average labor costs are similar in these two sectors, manufacturing firms have larger employment responses (9 (s.e. 3.1) out of 100 affected workers) than the service sector (4 (s.e. 2.4) out of 100 workers). In Table 2.5 Panel B we compare exporters to non-exporters. Exporting firms hit by the minimum wage shocks lay off 10 (s.e. 4) out of 100 affected workers, while non-exporting ones only lose a fifth of it: 2 (s.e. 2.3). These large differences in employment effects indicate that the labor demand elasticity with respect to the cost of labor in the exporting sector is substantially larger in absolute value (-.3, s.e. 0.2) than in the non-exporting sector (-.12, s.e. 0.2).

The fact that exporting firms experience a larger loss than non-exporting ones is consistent with the standard neoclassical model of firms. Firms selling their products on the export market face fierce competition and a highly elastic output demand curve. Therefore, these firms cannot raise their prices to finance the labor cost increase. On the other hand, firms operating in the non-exporting market or in the service sector are likely to face an inelastic output demand curve. Service sector firms can therefore pass through the effect of the minimum wage to consumers. In the next section we formalize this idea. [9]

The presence of heterogeneous responses by exporting status also suggests that the productivity gains caused by the minimum wage cannot be large. Suppose firms hit by the minimum wage shock are suddenly able to increase their productivity and sell more on the market. In that case, the employment effect of the minimum wage depends on the output demand elasticity that firms face with. In the exporting market firms can increase output without substantially deteriorating prices. Thus sales will increase, which could be used to finance the minimum wage. However, in the non-exporting markets, the increase in output will flood the markets. This can drive prices down, decrease sales and so, finally, firms must lay-off some of their workers. The productivity increase caused by the minimum wage hence predicts that the disemployment effects are smaller in the exporting markets (where the output demand elasticity is small) than in the non-exporting ones (where it is large). However, we find the opposite of that.

[9] Kovács (2011) argued that the exchange rate appreciation in early 2000 put manufacturing firms under pressure. We control for industry fixed effects and export share in 1997 (and its square term) to control out the effect of this shock. We also tried to control for the full export dynamics before 2000 by controlling for export share and its square term in 1997, 1998, 1999 and 2000, but the results are not affected by these additional controls. The robustness of our results indicate that our results are not driven by the exchange rate appreciation.

In Table 7 Panel D we show estimates by employment size in 1997. As we already noted, these results should be treated cautiously, because highly exposed and non-exposed firms behave differently at firms with more than 20 employees in the pre-reform years. However, the point estimate suggests that small firms experience a considerably larger employment loss (7.5 vs. 0 out of 100 workers). In Panel E we also examine firms' reactions by the share of non-wage benefit in their total labor cost. In Figure **??** we showed that firms mitigate the increase in earnings by cutting back non-financial remuneration. We would expect that firms with larger shares of non-wage benefits are more protected from the minimum wage shock. Consistent with that, firms having higher non-wage benefits in the pre-reform years experience less increase in their average cost of labor, and also less severe employment losses. One important implication of this finding is that in industries and in countries where non-wage benefits play a larger role in wage compensation, the minimum wage might have relatively smaller employment effects. However, crowding out of non-wage benefits might ultimately distort workers' welfare.

Finally, we also investigate whether our results are affected by tax evasion. Firms evading taxes often use tax deductions excessively . In the last Panel of Table 7 we examine our key results by firms effective tax rates (measured by paid taxes divided by operating profits). High effective tax rates are a signal for non-cheating as these firms do not use tax optimizing techniques extensively. However, these two groups of firms respond very similarly to the minimum wage, which indicates a limited role for tax evasion in explaining our results.[10]

2.5 Implications

The previous section showed that the minimum wage hike in Hungary had a small negative effect on employment, and firms exposed to the minimum wage experienced a large increase in their labor cost. The elevated cost of firms were paid out from an increase in sales instead of a decrease in firms' profits. In this section we show that these results are consistent with the standard competitive labor market model, where firms hit by the minimum wage shock are faced with inelastic demand.

In the neoclassical model of firms the effect of the wage increase on employment is determined by the Hicks–Marshall rules of derived demand. These rules connect the labor demand elasticity with the substitution between labor and other inputs and product demand elasticity. More specifically, let us suppose that firms have a 3-factor production function:

$$Y = F(L, K, M)$$

where Y is output, L is low skilled labor, K is capital and M is intermediate goods used for production (includes energy, materials used for production, and goods purchased for resale). It is worth noting that we only have one type of labor here, and we implicitly assume that all workers at the firm earn the minimum wage. Remember, our empirical estimates uncover the effect of the minimum wage when 100% of the workers earn sub-minimum wages in the pre-reform years. Therefore, having only low-wage workers in the model makes it easier to connect our empirical estimates with the model. Moreover, the presence of high-skilled labor

[10]The group of low tax payers contains a wide range of firm types (low profits due bad management, false reporting of incomes, tax exemption granted for large investments), thus we do not focus on them.

would be relevant only in the presence of labor-labor substitution, for which we did not find evidence in Chapter 1 (see Figure 1.7).

We assume that F have constant returns to scale (CRS) in the three inputs. Operating sales of the firms can be expressed in the following way:

$$pY = wL + rK + p_M M$$

where w is the average wage, r is the rate of return on capital, p_M the cost of intermediate goods.

Under perfect competition it can be shown that the labor demand elasticity (with respect to the cost of labor) has the following form (see Hamermesh (1993) for the derivation):

$$\frac{\partial logL}{\partial logw} = \underbrace{-s_L\eta}_{\text{scale effect}} \quad \underbrace{-s_K\sigma_{KL}}_{\substack{\text{substitution}\\ \text{between K and L}}} \quad \underbrace{-s_M\sigma_{ML}}_{\substack{\text{substitution}\\ \text{between M and L}}} \tag{2.4}$$

where s_L is the share of labor in output, s_K is the share of capital expenses in production, s_M is the share of intermediate goods and services used in the production, η is the *market-level* product demand elasticity firms face, σ_{KL} is the substitution between capital and labor, and σ_{ML} is the substitution between intermediate goods and services and labor. The first part of equation (2.4) is the scale effect. When a competitive firm is hit by a wage increase, it must raise prices to survive. If the production function has CRS, it turns out that the price increase will be related to the labor cost share in the output, which is s_L. The price increase of s_L cuts back market level demand by $s_L\eta$, where η is the elasticity of demand with respect to output prices.

The second and the third part of equation (6) is the substitution effect between labor and other inputs: when labor becames more costly, firms will substitute labor with other inputs. The second part shows the substitution between capital and labor. This substitution will depend on the Allen-partial elasticity of substitution between capital and labor, formally $\sigma_{KL} = \frac{\partial log\frac{K}{L}}{\partial log\frac{r}{w}}$, and the share of capital in production, s_K. The third apart of the equation (6) is the substitution between labor and intermediate goods services.

Equation (2.4) highlights that the importance of scale effects and substitution effects depends on the factor shares. Table 5 shows the share of these inputs by broad industry categories. The labor cost is only 17% of total sales, while spending on capital is another 5% for a representative firm. The rest is spending on intermediate goods and services. This indicates that the low labor demand elasticities can only be consistent with the Hicks–Marshall rules of derived demand if σ_{ML} is sufficiently low.

Is a low value of this key elasticity in line with the existing estimates? Even though many studies attempted to estimate the substitution between intermediate goods and labor the evidence is still inconclusive.[11] For instance, elasticity of substitution between energy expenses and labor was found to be around 0.3-0.7 (Berndt and Wood, 1975; Hamermesh, 1993), however, only a small portion, 2-3%, of intermediate goods are related to energy expenses (Basu

[11] Moreover, most of the empirical studies estimate the substitution elasticity between intermediate goods and services using data from the manufacturing sector. However, this substitution elasticity might be substantially lower in the service sector, where intermediate goods (e.g. the goods purchased for resale) are often very hard to substitute with labor.

and Fernald, 1997; Hamermesh, 1993). Overall estimates on the elasticity of substitution between materials and labor are often found to be much smaller. Bruno's (1984) benchmark estimates for σ_{ML} in the manufacturing is 0.3, with alternative specifications varying between -0.2 to 0.9. A more recent estimate by Atalay (2014) found 0.05 using all industries in his estimation.[12] Moreover, Bernd and Wood (1979) and Basu (1995) pointed out that in the presence of varying capital and labor utilizations these estimates are likely to overestimates the true elasticity of substitution between material and labor. Therefore, a low elasticity of substitution between intermediate goods and labor can be reconciled with existing empirical estimates.

Our empirical estimates can be used to uncover the parameter values of the model presented here. For that we use the following (theoretical) moments (derivation in Hamermesh, 1993). The relationship between changes in sales and an increase in labor cost is the following:

$$\frac{\partial logpY}{\partial logw} = \underbrace{s_L}_{\text{price effect}} \quad \underbrace{-s_L\eta}_{\text{scale effect}} \tag{2.5}$$

The first part of this formula is the price effect: when a competitive firm is hit by an increase in its total labor cost, it will raise its prices by s_L. The second part (ηs_L) comes from the decrease in quantity demanded when prices rise (i.e. the movement along the market-level product demand curve when the price increases). If the output prices increase by s_L% the market demand falls by ηs_L%.

Moreover, the neoclassical model also predicts the effect of the minimum wage on capital:

$$\frac{\partial logK}{\partial logw} = \underbrace{-s_L\eta}_{\text{scale effect}} \quad \underbrace{-s_L\sigma_{KL}}_{\text{substitution effect}} \tag{2.6}$$

The first part of equation 2.6 comes from the scale effect: as the price increases in the competitive market, firms' output decreases. This leads to a decrease in the usage of all inputs used in the production including capital. The second part is the substitution effect. As the cost of labor increases, firms substitute labor with other inputs. This substitution depends on the share of labor, s_L, and the substitution between capital and labor, σ_{KL}.

Finally, the effect on intermediate goods and services is very similar to the effect on capital:

$$\frac{\partial logc_s M}{\partial logw} = s_L(-\eta + \sigma_{ML}) \tag{2.7}$$

Estimation. We estimate this model with a minimum-distance estimator, matching the empirical elasticities presented in Table 6 and Table 7 to the parameters of this model. Denote by $m(\xi)$ the vector of moments predicted by the theory as a function of the parameters ξ, and by $\hat{m}$ the vector of observed moments. The minimum-distance estimator chooses the parameters $\hat{\xi}$ that minimize the distance $(m(\xi) - \hat{m})' W (m(\xi) - \hat{m})$, where W is a weighting matrix. As a weighting matrix, we use the diagonal of the inverse of the variance-covariance matrix. Hence, the estimator minimizes the sum of squared distances, weighted by

[12] Atalay (2014) in Appendix F finds a plant level elasticity of substitution between materials and value added is between 0.45-0.8 in the manufacturing sector. The discrepancy between his main estimates and the one presented in the Appendix F might be because the elasticity of substitution is substantially lower in the service sector.

the inverse variance of each moment. Under standard conditions, the minimum-distance estimator using weighting matrix W achieves asymptotic normality, with estimated variance $(\hat{G}'W\hat{G})^{-1}(\hat{G}'W\hat{\Lambda}W\hat{G})(\hat{G}'W\hat{G})^{-1}/N$, where $\hat{G} \equiv N^{-1}\sum_{i=1}^{N}\nabla_{\xi}m_i(\hat{\xi})$ and $\hat{\Lambda} \equiv Var[m(\hat{\xi})]$ Wooldridge (2010). We calculate $\nabla_{\xi}m(\hat{\xi})$ numerically in Matlab using an adaptive finite difference algorithm.

Table 2.7 shows the estimated parameters for different specifications. The odd columns show the empirical moments, while the even columns the estimated parameters and the predicted moments. We use four moments in the estimations: Equation (2.4), the employment elasticity; Equation (2.5), the sales elasticity, and Equation 2.6 the capital elasticity; and Equation 2.7, the intermediate goods and services elasticity; and estimate three parameters: the market-level output demand elasticity and the elasticity of substitution between capital and labor and the substitution between intermediate goods used for production and labor.

Column (2) and (4) shows the estimates for 2002 and 2003. The estimated substitution elasticity between capital and labor is 0.33 (s.e. 0.46) in 2002 and 0.34 (s.e. 0.64) in 2003. The estimates in the previous literature go from 0.36 in Chirinko et. al (2011) to 1.25 in Karabarbounis and Neiman (2014), and therefore, our estimate is at the lower end of this range. The estimated product demand elasticity is 0.09 (s.e. 0.36) in 2002 and 0.46 (s.e. 0.47) in 2003. This estimate is outside of the the uncompensated product demand elasticities often used for calibration (0.5-1.5). However, since the minimum wage hike increases workers' purchasing power, the compensated elasticity might be a more appropriate concept to use in this case (Harberger, 1962). This latter is often found to be fairly low in many contexts (e.g. 0.03-0.2 for food consumption in Table 6 of Seale et al. 2003). Finally, our estimates on the substitution between intermediate goods and labor is 0.2 (s.e. 0.09) in 2002 and 0.16 (s.e. 0.12) in 2003, which is in line with the existing estimates in the literature. In Columns (6) and (10) of Table 2.7 we report the estimated parameters when changes between the average of 2002 and 2003 and 2000 is considered. The estimated parameters for this specification lies between 2002 and 2003 except for the substitution between capital and labor, which is somewhat lower (0.15 instead of 0.34).

In Columns (8) and (10) of Table 2.7 we show estimates separately for the exporting and non-exporting sector for 2003. The substitution between capital and labor is very similar in the two sectors. However, there is a substantial difference between the implied output demand elasticity: in the exporting sector the output demand is substantially larger than in the non-exporting one. The estimated output demand elasticity in the exporting sector (0.82, s.e. 0.87) is very close to the estimates for Armington elasticity that is estimated in the trade literature using high frequency data (Blonigen and Wilson, 1999; Reinert and Roland-Host, 1992). However, it is lower than the elasticity found in cross sectional studies (Ruhl, 2008). The output demand elasticity in the non-exporting sector is fairly low (0.21), but it is consistent with some estimates in the literature(Seale et al., 2003). Moreover, the fairly low output demand elasticity is also consistent with the findings of the minimum wage literature that often documents large effect on prices, but no effect on employment (MaCurdy, 2014). This implies that our low estimates for output demand elasticity are not likely to be a Hungarian or a research-design specific result.

2.6 Discussion and Conclusion

This paper investigated the economic effects of a large and persistent increase in the minimum wage in Hungary. Most firms responded to the minimum wage by raising wages instead of destroying jobs. Hence, the higher minimum wage in Hungary redistributed substantial resources to workers. We also showed that profitability did not decline among low-wage employers. Instead, the minimum wage increase was passed on to the consumers. Hence, our empirical results indicate that the ultimate incidence of the minimum wage fell on the consumers.

The evidence presented here is hard to reconcile with the monopsonistic wage setting (Manning 2003, Card and Krueger 1995), or the search and matching models (Flinn 2010) that would predict a large negative effect on profits in response to the large increase in the minimum wage analysed in this paper. A common feature of these models is that the minimum wage can improve resource allocation by introducing another distortion in addition to the existing ones. Therefore, one of the most important implications of this chapter is that the minimum wage is unlikely to improve efficiency.

However, the minimum wage might be an effective tool for redistribution if it can reallocate resources from the rich to the poor without large efficiency losses. This idea is explored in Lee and Saez (2002), who show that the introduction of a minimum wage can be welfare improving in a simple neoclassical model even in the presence of optimal taxes and transfers. One limitation of their analysis is that they do not consider the possibility of passing through the minimum wage to consumers, which is an important channel empirically. To incorporate consumer pass-through in their framework one needs to take into consideration the general equilibrium effects of the minimum wage. Thus, one important future direction is to add general equilibrium considerations to our partial equilibrium model presented in Section 5.

Our findings also indicate that the employment effect of the minimum wage varies across industries and potentially across countries. For instance, industries (or countries) where firms have more leeway to cut back non-cash benefits might experience lower employment losses. However, in these countries the cost of the minimum wage might come from the distorted compensation structures. Moreover, in countries where low-wage jobs are concentrated in manufacturing (e.g. Germany) raising the minimum wage can be much more costly than in the U.S. where low-wage workers are concentrated in the service sector (Dube et al., 2010).

Finally, the evidence presented here can justify sector-specific minimum wage polices, present in some European countries such as Germany and Austria. If the minimum wage has the largest negative employment effects in the manufacturing sector, then setting a lower minimum wage there could lead to less of a negative employment effect overall. Targeted minimum wage polices may be get the best of both worlds: increase wages, where it is possible, but save jobs, where it is not.

Tables

Table 2.1: Descriptive Statistics - Corporate Income Tax Data

	(1)	(2)	(3)	(4)	(5)
	unweighted	weighted			
	mean	mean	sd	p25	p75
Employment	110.5	13.9	55.5	10	95
Average Earnings (HUF in thousnad)	132.11	13.0	78.3	6.8	92.9
Average Labor Cost (HUF in thousnad)	204.0	20.0	119.8	10.5	142.3
Share of non-financial renumeration	0.10	0.11	0.11	0.05	0.12
Average value added (HUF in thousnad)	429.3	43.0	356.6	18.1	240.4
Profitability (EBIT/SALES)	0.03	0.03	0.07	0	0.06
Fraction affected	0.26	0.41	0.39	0	0.50
Number of firms	5459	5459	5459	5459	5459

Note: This table presents the descriptive statistics of the Corporate Income Tax data, 2000 wave. The fraction affected statistics comes from the Wage Survey (see the details in the text). This table shows the summary statistics of our main sample used for the firm-level analysis. The first column shows the unweighted means of the main variables, Column (2) to (3) show the weighted statistics. Weights are created to make the sample representative at size categories (5-20, 20-50, 50-300, more than 300) and 1-digit industry level. As we described in the data section, we over-sample large firms, which necessitates the use of sample weights in columns (2) and (3).

Table 2.2: Descriptive Statistics - Balance Sheet Items

	Number of Firms	Value Added			Intermediate Goods	
		Labor Cost	Capital Expenses		Materials	Cost of Goods for Resale
			EBIT	Depreciation		
Manufacturing (food, textile)	1397	0.154	0.027	0.025	0.425	0.292
Manufacturing (chemicals, metals)	863	0.200	0.041	0.027	0.585	0.094
Manufacturing (machines)	574	0.216	0.057	0.024	0.468	0.127
Construction	682	0.152	0.045	0.022	0.658	0.045
Retail and wholesale	1150	0.081	0.026	0.015	0.183	0.605
Transportation and hotels	472	0.151	0.024	0.039	0.490	0.187
Other services	321	0.230	0.044	0.038	0.428	0.125
All	5459	0.145	0.033	0.024	0.413	0.296

Note: This table shows the key balance sheet items relative to sales by industry for our main sample in 2000. Value added is the sum of labor cost, depreciation and operating profit (EBIT). Results are weighted to make the sample representative (see the text for the details).

Table 2.3: Effect on the Cost of Labor and Employment

	(1)	(2)	(3)	(4)	(5)	(6)
	Main results				Placebo estimates	
	Change from 2000 to 2002		Change from 2000 to 2003		Change from 1998 to 2000	
Panel A: Change in average wage						
Fraction Affected	0.456***	0.459***	0.396***	0.402***	-0.033**	-0.030**
	[0.019]	[0.017]	[0.019]	[0.021]	[0.013]	[0.012]
Constant	0.021**		0.022**		0.0519***	
	[0.009]		[0.009]		[0.008]	
Panel B: Change in average cost of labor						
Fraction Affected	0.372***	0.379***	0.311***	0.313***	-0.016	-0.016
	[0.018]	[0.018]	[0.020]	[0.019]	[0.013]	[0.011]
Constant	-0.002		-0.010		0.00897	
	[0.009]		[0.010]		[0.008]	
Panel C: Change in employment						
Fraction Affected	-0.0551**	-0.0462*	-0.0685***	-0.0635**	0.0132	0.0169
	[0.0226]	[0.0242]	[0.0256]	[0.0271]	[0.0211]	[0.0221]
Constant	-0.120***		-0.161***		-0.116***	
	[0.0129]		[0.0141]		[0.0117]	
Panel D: Change in total labor cost						
Fraction Affected	0.256***	0.253***	0.178***	0.176***	-0.0182	-0.0115
	[0.0265]	[0.0287]	[0.0292]	[0.0312]	[0.0216]	[0.0229]
Constant	-0.120***		-0.173***		-0.100***	
	[0.0138]		[0.0150]		[0.0118]	
Panel E: Employment Elasticity with respect to the wage						
Fraction Affected	-0.119**	-0.098*	-0.173***	-0.154**		
	[0.054]	[0.057]	[0.068]	[0.072]		
Panel F: Employment Elasticity with respect to the cost of labor						
Fraction Affected	-0.145**	-0.121*	-0.217***	-0.199**		
	[0.065]	[0.070]	[0.086]	[0.093]		
Observations	5,459	5,459	5,459	5,459	5,459	5,459
industry	no	yes	no	yes	no	yes
controls	no	yes	no	yes	no	yes

Robust standard errors in brackets, *** p<0.01, ** p<0.05, * p<0.1

Note: Table 2.3 shows the estimated results from equation 2.1 under different specifications. The first four columns show our main results, Columns (1)-(2) show the effect of the fraction of workers affected by the minimum wage on the percentage change between *2002 and 2000* for average wage (Panel A), for average cost of labor (Panel B), for employment (Panel C), and for total labor cost (Panel D). Columns (3) and (4) show the changes in the same variables between *2003 and 2000*. The average labor cost is observed only for firms that survived, and so we correct for this selection (see the text for the details). In Panel E and D we report the employment elasticity with respect to wages, and the cost of labor, respectively. Standard errors are bootstrapped. Columns (5)-(6) show a placebo test: the effect of the fraction of workers earning below the 2002 minimum wage on the changes between *2000 and 1998*. Columns (1), (3) and (5) show results without any control variables, Columns (2), (4) and (6) control for the share of export in sales in 1997 and its square term, and 2-digit NACE (industry codes). Results are weighted to make the sample representative (see the text).

Table 2.4: Effect on Sales and Profits

	(1)	(2)	(3)	(4)	(5)	(6)
	Main results				Placebo estimates	
	Change from 2000 to 2002		Change from 2000 to 2003		Change from 1998 to 2000	
Panel A: Change in total labor cost						
Fraction Affected	0.0309***	0.0295***	0.0247***	0.0236***	0.0009	0.0016
	[0.0031]	[0.0032]	[0.0038]	[0.0038]	[0.0028]	[0.0029]
implied elasticity	0.456***	0.441***	0.440***	0.423***		
	[0.050]	[0.054]	[0.072]	[0.075]		
Panel B: Change in sales						
Fraction Affected	0.0338 *	0.046**	0.0145	0.0208	0.001	-0.0014
	[0.0196]	[0.021]	[0.022]	[0.0233]	[0.0225]	[0.0241]
implied elasticity	0.094*	0.126**	0.048	0.068		
	[0.059]	[0.064]	[0.075]	[0.082]		
Panel C: Change in profits						
Fraction Affected	0.0021	0.0029	0.0075	0.0055	0.0108 **	0.0142 ***
	[0.0045]	[0.0047]	[0.0046]	[0.0048]	[0.0048]	[0.005]
implied elasticity	0.136	0.170	0.602	0.431		
	[0.337]	[0.356]	[0.404]	[0.428]		
Panel D: Change in intermediate goods and services						
Fraction Affected	-0.0002	0.0103	-0.0199	-0.0117	-0.0052	-0.0102
	[0.0161]	[0.0172]	[0.0181]	[0.0192]	[0.0183]	[0.0195]
implied elasticity	0.0001	0.038	-0.089	-0.054		
	[0.065]	[0.071]	[0.083]	[0.090]		
Panel E: Change in capital						
Fraction Affected	-0.0012	0.0018	-0.0022	0.0013	0.0062	0.0077
	[0.0065]	[0.0066]	[0.008]	[0.0083]	[0.0066]	[0.0068]
implied elasticity	-0.012	0.016	-0.029	0.010		
	[0.075]	[0.078]	[0.111]	[0.117]		
Observation	5459	5459	5459	5459	5459	5459
Industry	no	yes	no	yes	no	yes
Controls	no	yes	no	yes	no	yes

Robust standard errors in brackets, *** p<0.01, ** p<0.05, * p<0.1

Note: Table 2.4 shows the estimated results from equation 2.2 under different specifications. The first four columns show our main results, Columns (1)-(2) show the effect of the fraction of workers affected by the minimum wage on the percentage change between *2002 and 2000,* while Columns (3) and (4) for changes between *2003 and 2000.* We also report the elasticities with respect to the cost of labor. Standard errors of the elasticities are bootstrapped. Columns (5)-(6) show a placebo test: the effect of the fraction of workers affected by the minimum wage on the changes between *2000 and 1998.* Columns (1), (3) and (5) show the results without any control variables, Columns (2), (4) and (6) control for the share of export in sales in 1997 and its square term, and 2-digit NACE (industry codes). Results are weighted to make the sample representative (see the text for the details).

Table 2.5: Heterogeneous Responses to the Minimum Wage

	(1) Cost of Labor	(2) Employ-ment	(3) Labor Cost	(4) Sales	(5) Materials	(6) Profit	(7) Capital
Panel A: All firms							
All firms	0.344***	-0.055**	0.026***	0.037*	0.004	0.005	0.001
obs.=5459	[0.014]	[0.024]	[0.003]	[0.021]	[0.017]	[0.004]	[0.007]
Panel B: by industry							
service	0.320***	-0.042	0.025***	0.041	0.007	0.003	-0.003
obs.=2894	[0.022]	[0.029]	[0.004]	[0.025]	[0.021]	[0.005]	[0.009]
manufacturing	0.341***	-0.090**	0.031***	0.027	-0.004	0.011	0.016
obs.=2565	[0.026]	[0.038]	[0.007]	[0.036]	[0.029]	[0.009]	[0.013]
Panel C: by trade							
non exporter	0.338***	-0.046*	0.027***	0.037	0.003	0.006	-0.002
obs.=3928	[0.016]	[0.027]	[0.004]	[0.023]	[0.019]	[0.005]	[0.008]
exporter	0.417***	-0.122**	0.024**	0.031	0.009	0	0.031
obs.=1531	[0.045]	[0.058]	[0.010]	[0.053]	[0.046]	[0.014]	[0.022]
Panel D: by size							
empl. <20	0.343***	-0.074***	0.022***	0.031	0.003	0.005	-0.001
obs.=1916	[0.018]	[0.027]	[0.004]	[0.024]	[0.020]	[0.005]	[0.008]
empl. >=20	0.347***	0.002	0.044***	0.057	0.006	0.008	0.017
obs.=3513	[0.023]	[0.044]	[0.007]	[0.036]	[0.029]	[0.007]	[0.014]
Panel E: by share of non-wage benefit in total labor cost							
below median	0.350***	-0.100***	0.024***	0.024	-0.013	0.003	0.005
obs.=2733	[0.021]	[0.032]	[0.005]	[0.028]	[0.023]	[0.006]	[0.010]
above median	0.285***	-0.001	0.027***	0.051*	0.022	0.007	-0.002
obs.=2726	[0.021]	[0.035]	[0.005]	[0.031]	[0.026]	[0.006]	[0.010]
Panel F: by effective taxrate							
below median	0.325***	-0.015	0.034***	0.058**	0.017	0.002	-0.001
obs.=2747	[0.024]	[0.034]	[0.005]	[0.028]	[0.023]	[0.006]	[0.010]
above median	0.355***	-0.043	0.025***	0.053*	0.018	0.01	0.01
obs.= 2712	[0.025]	[0.028]	[0.004]	[0.029]	[0.024]	[0.006]	[0.010]

Robust standard errors in brackets, *** p<0.01, ** p<0.05, * p<0.1

Note: This table shows estimates of equation 2.3. Columns (1)-(7) show the effect of the fraction workers affected by the minimum wage on the percentage change between *the average of year 2002 and year 2003* and year 2000 for different outcomes and for different sub-groups. In each regression we control for the share of export in sales in 1997 and its square term, and 2-digit NACE (industry codes). Results are weighted to make the sample representative (see the text for the details).

Table 2.6: Heterogeneous Responses to the Minimum Wage, Placebo tests

	(1) Cost of Labor	(2) Employ-ment	(3) Labor Cost	(4) Sales	(5) Materials	(6) Profit	(7) Capital
Panel A: All firms	-0.016	0.0169	0.0016	-0.0014	-0.0102	0.0142***	0.0077
All firms	[0.011]	[0.0221]	[0.0029]	[0.0241]	[0.0195]	[0.005]	[0.0068]
obs.=5459							
Panel B: by industry							
service	-0.016	0.022	0.002	0.006	-0.001	0.014**	0.012
obs.=2894	[0.017]	[0.026]	[0.003]	[0.029]	[0.024]	[0.006]	[0.008]
manufacturing	-0.021	0.002	-0.001	-0.025	-0.038	0.016*	-0.005
obs.=2565	[0.014]	[0.039]	[0.006]	[0.040]	[0.031]	[0.009]	[0.012]
Panel C: by trade							
non exporter	-0.017	0.023	0.002	0.000	-0.01	0.016***	0.009
obs.=3928	[0.013]	[0.024]	[0.003]	[0.026]	[0.021]	[0.005]	[0.007]
exporter	0	-0.029	-0.003	-0.01	-0.009	-0.002	-0.001
obs.=1531	[0.018]	[0.060]	[0.010]	[0.069]	[0.053]	[0.015]	[0.020]
Panel D: by size							
empl. <20	-0.015	0.011	-0.001	-0.007	-0.012	0.014**	0.004
obs.=1916	[0.015]	[0.024]	[0.003]	[0.027]	[0.022]	[0.006]	[0.008]
empl. >=20	-0.023	-0.063	-0.001	-0.017	-0.035	0.017*	0.020*
obs.=3513	[0.015]	[0.044]	[0.007]	[0.041]	[0.033]	[0.010]	[0.012]
Panel E: by size of non financial remuneration in labor cost							
below median	-0.021	-0.010	-0.004	0.009	-0.003	0.018**	0.008
obs.=2733	[0.015]	[0.030]	[0.004]	[0.033]	[0.026]	[0.007]	[0.009]
above median	-0.022	0.04	0.007	-0.012	-0.015	0.009	0.008
obs.=2726	[0.021]	[0.031]	[0.004]	[0.033]	[0.028]	[0.007]	[0.010]
Panel F: by effective taxrate							
below median	-0.005	0.041	0.008*	0.01	-0.002	0.009	0.006
obs.=2747	[0.017]	[0.029]	[0.004]	[0.032]	[0.026]	[0.007]	[0.009]
above median	-0.040*	0.024	-0.001	-0.034	-0.027	0.004	0.023**
obs.= 2712	[0.023]	[0.033]	[0.004]	[0.035]	[0.028]	[0.007]	[0.010]

Robust standard errors in brackets, *** p<0.01, ** p<0.05, * p<0.1

Note: This table shows the **placebo estimates** based on equation 2.3. Columns (1)-(7) show the effect of the fraction of workers affected by the minimum wage on the percentage change between *1998 and 2000* for different outcomes and for different sub-groups. In each regression we control for the share of export in sales in 1997 and its square term, and 2-digit NACE (industry codes). Results are weighted to make the sample representative (see the text for the details).

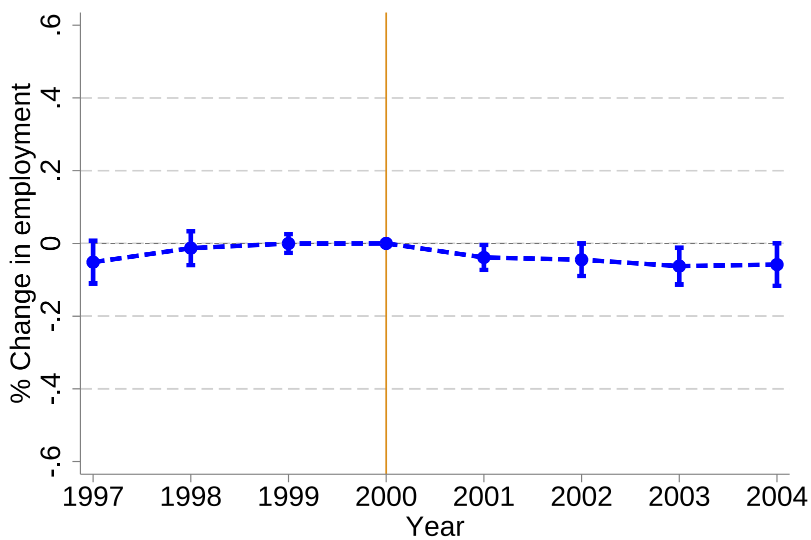
Table 2.7: Estimating the neoclassical model in 2002 and 2003

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	All Firms in 2002		All Firms in 2003		All Firms in 2002&2003		Exporting Firms in 2002&2003		Non-Exporting Firms in 2002-	
	Empirical moments		Empirical moments				Empirical moments		Empirical moments	
Output Demand Elasticity		0.09		0.46		0.26		0.82		0.21
		[0.36]		[0.47]		[0.36]		[0.87]		[0.39]
Substitution between Capital and Labor		0.33		0.34		0.15		1.65		0
		[0.46]		[0.63]		[0.46]		[0.99]		[0.59]
Substitution between Intermediate Goods used for Production and Labor		0.20		0.16		0.14		0.15		0.13
		[0.09]		[0.12]		[0.09]		[0.26]		[0.11]
Moments									0	0
Labor demand (Eq 2.4)	-0.12	-0.19	-0.20	-0.22	-0.15	-0.16	-0.29	-0.30	-0.12	-0.13
	[0.01]		[0.01]		[0.01]		[0.02]		[0.01]	
Sales (Eq 2.5)	0.13	0.15	0.07	0.09	0.10	0.13	0.07	0.12	0.10	0.13
	[0.06]		[0.08]		[0.07]		[0.07]		[0.07]	
Intermediate Goods (Eq 2.6)	0.04	0.02	-0.05	-0.05	0.01	-0.02	0.02	-0.03	0.01	-0.01
	[0.06]		[0.09]		[0.07]		[0.08]		[0.1]	
Capital (Eq 2.7)	0.02	0.04	0.01	-0.02	0.01	-0.02	0.23	0.23	-0.03	-0.04
	[0.07]		[0.11]		[0.07]		[0.13]		[0.11]	
Goodness-of-fit statistic		3.03		0.81		4.23		0.18		0.31
(probability value)		0.05		0.30		0.02		0.86		0.61

Note: We estimate the parameters of the competitive model presented in Section 5 with a minimum-distance estimator. The theoretical moments can be found in Section 5. Columns (1), (3), (5), (7), (9) show the empirical moments, while Columns (2), (4), (6), (8), (10) display estimates of the key parameters and the predicted moments. Standard errors are reported in the bracket.

Figure 2.1: Effect on Workers' Remuneration and Employment

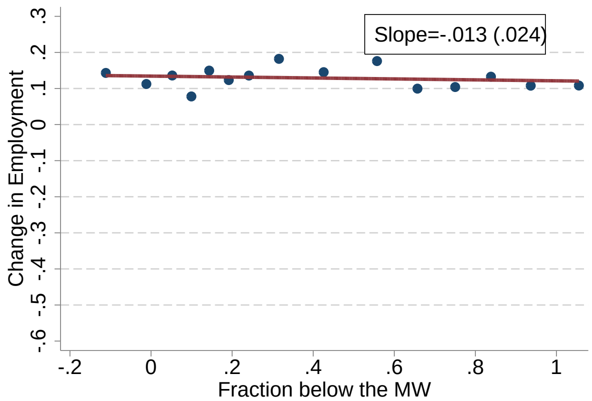
(a) Effect on Average Wage and Cost of Labor



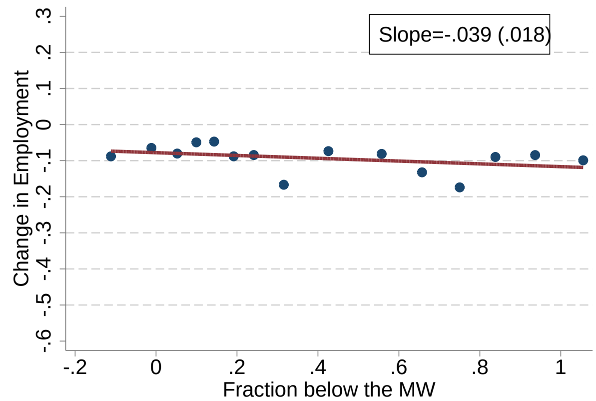
(b) Effect on Employment

Notes: Figure 2.1 Panel (a) shows the estimated firm-level relationship between the fraction of workers affected by the minimum wage and the percentage change (relative to 2000) in average earnings (beta coefficients with their confidence intervals from equation (2.1), blue dotted-dashed line) and in average cost of labor (beta coefficients from equation (2.1), red dashed line) over time. Both lines indicate that remuneration increased at highly exposed firms after 2000. The average earnings line is above the average cost of labor line indicating that the increase in earnings was mitigated by cutting back non-wage benefits. Panel (b) shows the estimated firm-level relationship between the fraction of workers affected by the minimum wage and the percentage change (relative to 2000) in employment (beta coefficients from equation (2.1)). The ratio of Panel (a) and Panel (b) gives us the labor demand elasticity. Panel (b) includes firms that died in the regression.

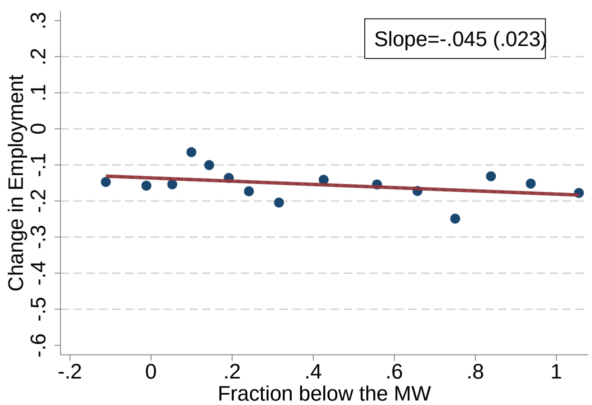
Figure 2.2: Effect on Employment over time



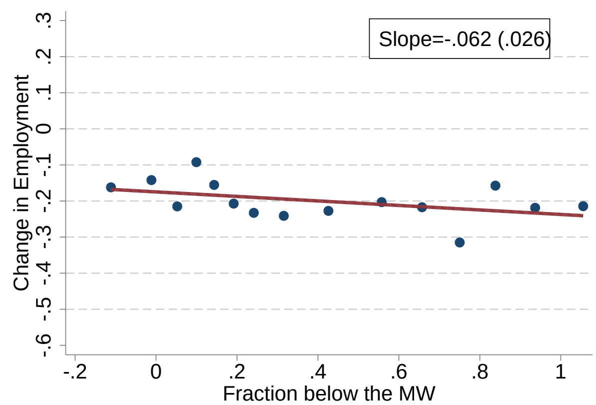
(a) 1998



(b) 2001



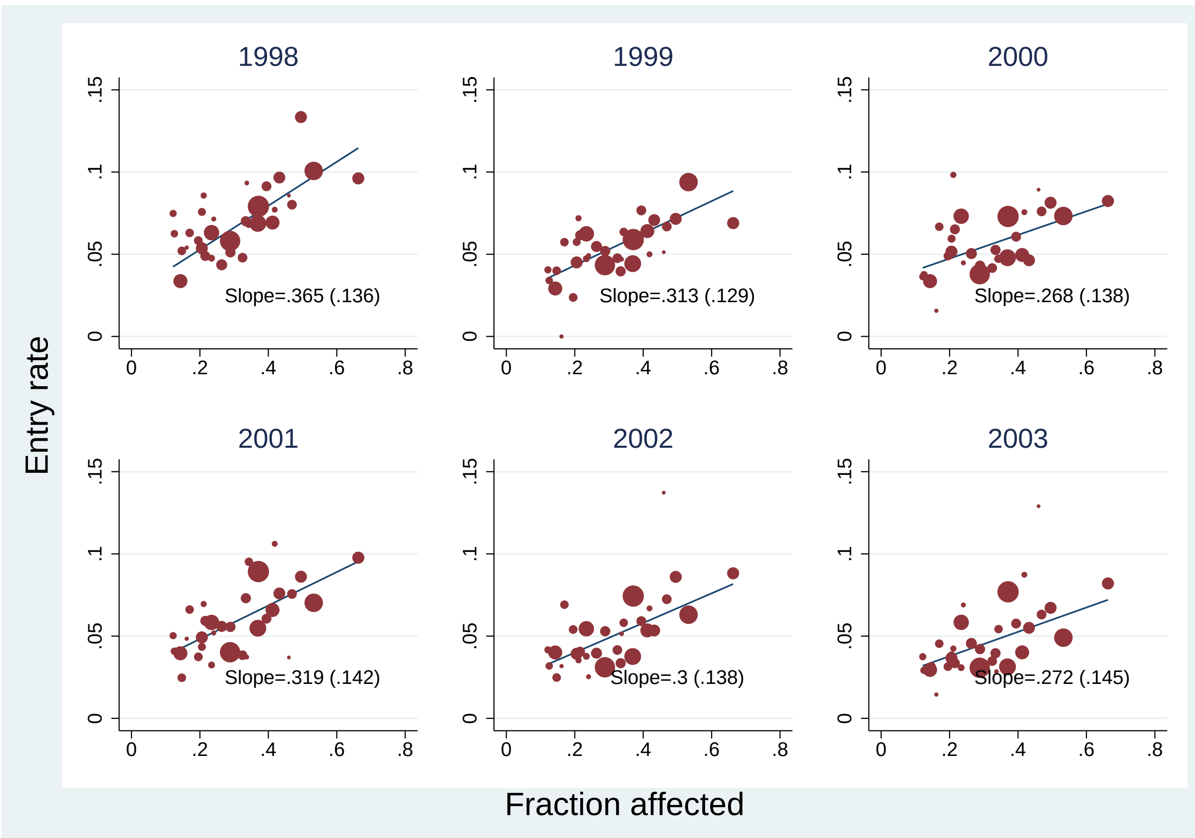
(c) 2002



(d) 2003

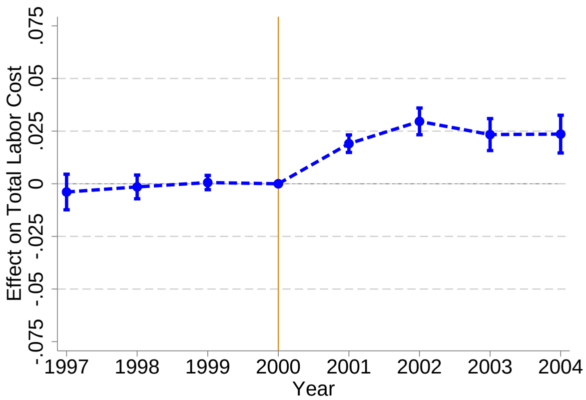
Notes: These figures present non-parametric binned scatter plots of the relationship between the fraction of workers affected by the 2002 minimum wage and the cumulative growth relative to 2000. This is the non-parametric version of equation (2.1). The red solid line is the linear fit, while the slope of this fit is reported in the top right corner.

Figure 2.3: Effect on Firms Entry

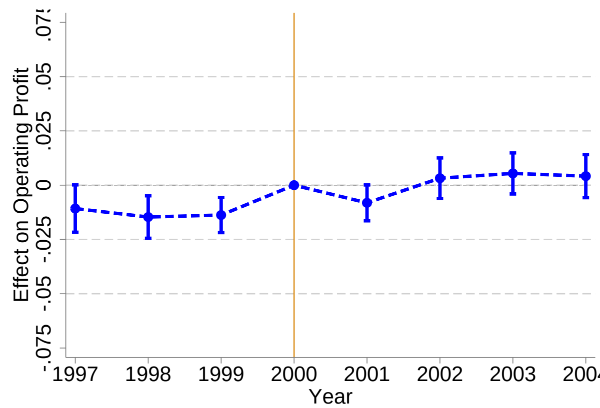


Notes: This figure shows the relationship between exposure to the minimum wage and firms entry at the two digit industry level. Each scatterplot relates the share of new firms in a two-digit industry to the fraction of workers affected by the minimum wage in that sector. In each graph the fitted regression line is the outcome from a corresponding OLS weighted by the number of firms in the sector. The regression slope along with the standard errors are indicated in the right bottom corner of each year from 1998 to 2003.

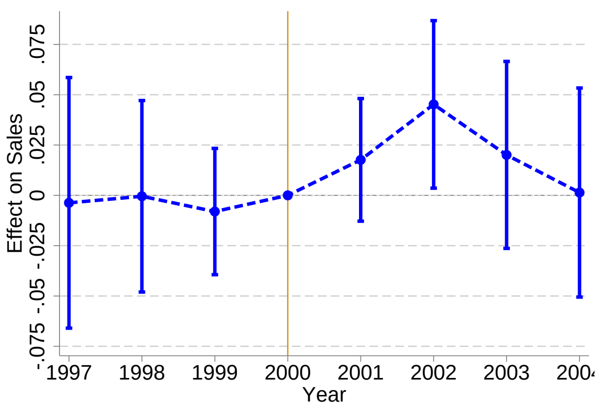
Figure 2.4: The Incidence of the Minimum Wage



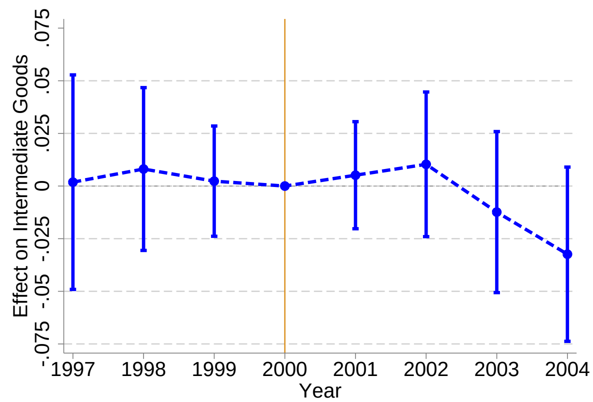
(a) Effect on Total Labor Cost



(b) Effect on Profit Margin



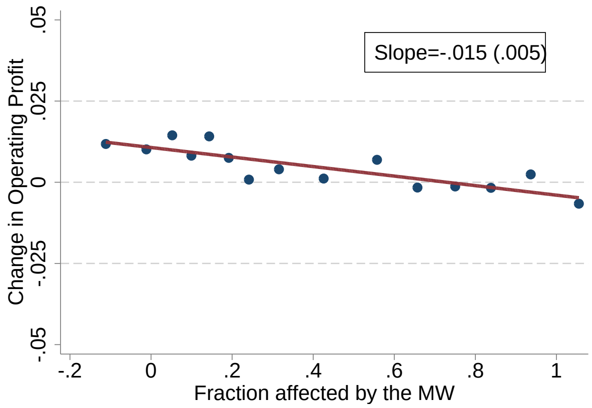
(c) Effect on Sales



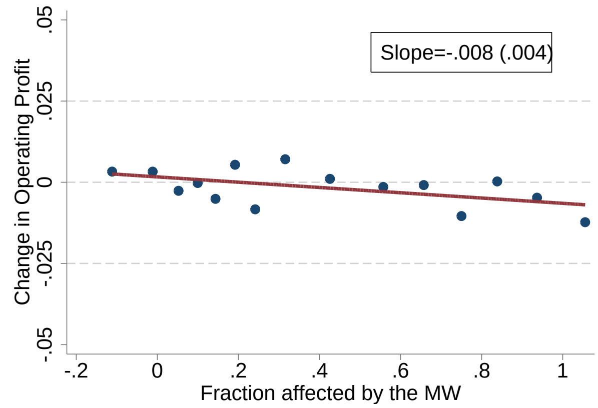
(d) Effect on Intermediate Goods and Services

Notes: Figure 2.4 Panel (a) shows the results from a firm-level regression of changes (relative to 2000) in total labor cost relative to the average sales on the fraction of workers affected by the minimum wage (beta coefficients from equation (2.2)). The graph shows that firm-level expenses increased substantially at highly exposed firms after the minimum wage hike. Panel (b) depicts effects on profits, Panel (c) on sales (revenue), Panel (d) on intermediate goods and services. Both Panel (a) and Panel (b) include firms that died in the regression. Controls and industry dummies are also included in the regressions.

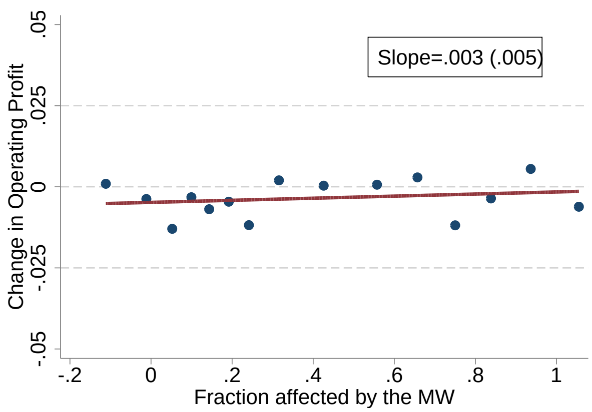
Figure 2.5: Effect on Profit over time



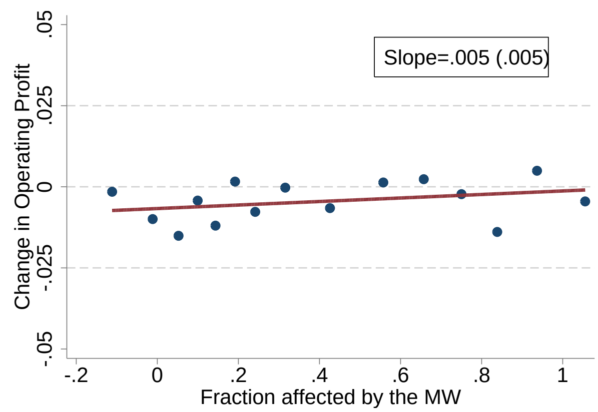
(a) 1998



(b) 2001



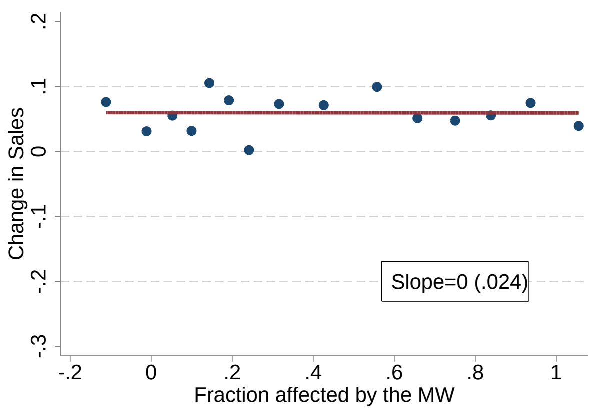
(c) 2002



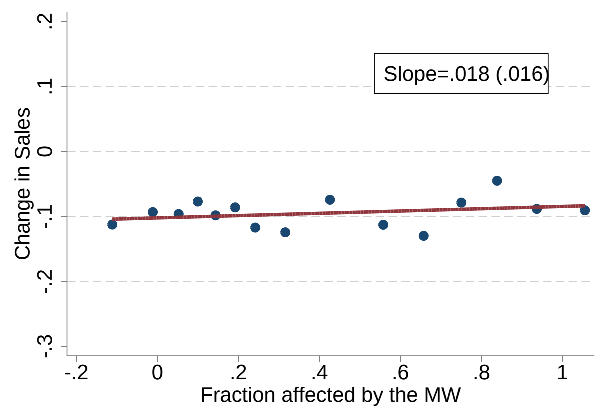
(d) 2003

Notes: These figures present the non-parametric binned scatter plots of the relationship between the fraction of workers affected by the 2002 minimum wage and the change in operating profits (operating profits divided by the average sales between 1997 and 1999). This is the non-parametric version of equation (2.2). The red solid line is the linear fit, while the slope of the fit is reported in the top right corner.

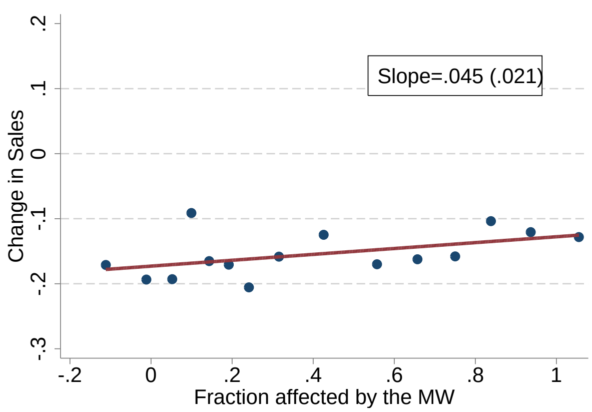
Figure 2.6: Effect on Sales over time



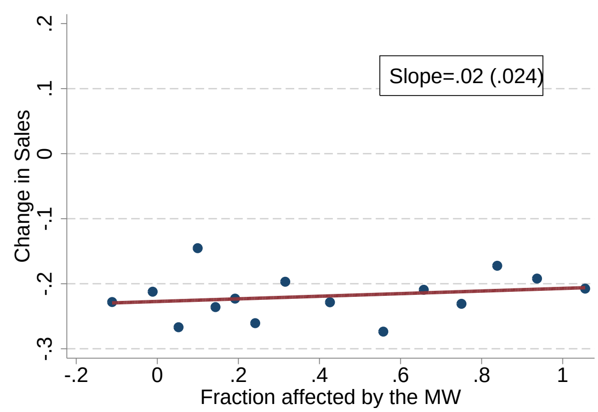
(a) 1998



(b) 2001



(c) 2002



(d) 2003

Notes: These figures present the non-parametric binned scatter plots of the relationship between the fraction of workers affected by the 2002 minimum wage and the change in sales (sales divided by the average sales between 1997 and 1999). This is the non-parametric version of equation (2.2). The red solid line is the linear fit, while the slope of the fit is reported in the top right corner.

Chapter 3

Reference-Dependent Job Search: Evidence from Hungary

with Stefano DellaVigna, Reizer Balázs and Johaness Schmieder

3.1 Introduction

Unemployment insurance programs in most Western countries follow a common design. The benefits are set at a constant replacement rate for a fixed period, typically followed by lower benefits under unemployment assistance. In such systems, the hazard rate from unemployment typically declines from an initial peak the longer workers are unemployed, then surges at unemployment exhaustion, and declines thereafter. This has been shown in a variety of settings, such as Germany Schmieder et al. (2012a), Austria Card et al. (2007a), Slovenia van Ours and Vodopivec (2008), Hungary Micklewright and Nagy (1999) or France Le Barbanchon (2012).[1]

It is well-known that a basic job search model a' la Mortensen (1986) and van den Berg (1990) is unable to match this pattern. This model predicts an increasing exit hazard up until benefit expiration, with a constant exit rate thereafter. To match the time path of the hazards, job search models add unobserved heterogeneity among workers. More productive workers are more likely to find a job initially, leading to a decrease in the hazard over time as the workers still unemployed are predominantly of the less productive type. Apart from heterogeneity, researchers have proposed that the spike at UI exhaustion might be explained by storable job offers (Boone and van Ours 2012), as well as sanctions imposed by the UI agencies (Cockx et al. 2013).

In this chapter, we propose, and test empirically for, a behavioral model of job search which can account for this time path of unemployment, and other job search patterns. We propose that workers have reference-dependent preferences over their utility from consumption. As in prospect theory Kahneman and Tversky (1979), workers are loss-averse with respect to payoffs below the reference point. Further, we assume that the reference point is given by consumption in the recent past.

To fix ideas, consider a reference-dependent worker who was just laid off. For simplicity, assume, as we do in the paper, that the worker has no savings and that in each period she

[1]The evidence for the United States is more limited, due to the lack of administrative data, with Katz and Meyer (1990) reporting a sharp spike but with small sample sizes., while others, such as Fallick (1991) do not find such a spike. Card, Chetty and Weber (2007b) provide a careful discussion of the evidence on spikes and highlight the importance of distinguishing the exit hazard from UI from the exit hazard from non-employment (that is into employment). While the exit hazard into employment shows less of a spike than the exit from UI, it is nevertheless quite pronounced in many papers relying on large and high quality administrative datasets.

consumes the benefits.[2] Because the unemployment benefits are significantly lower than the previous wage, this worker finds the new state of unemployment particularly painful given the loss aversion, and works hard to search. Over the weeks of unemployment, however, the reference point shifts as the individual adapts to the lower consumption level, and the loss aversion is thus mitigated. Hence, the worker's search effort decreases. As the end of the UI benefits draws near, the worker, if still unemployed, anticipates the loss in consumption due to the exhaustion of the benefits, and searches harder. This force is at work also in the standard model, but it is heightened by the anticipation of the future loss aversion. If the worker does not find a job before UI expiration, the worker once again slowly adjusts to the new, lower benefit level. Hence, the hazard for unemployment for this reference-dependent worker decreases from the initial peak, increases at exhaustion, then decreases again. Hence, the hazard displays the same qualitative pattern as in the data, even in absence of unobserved heterogeneity.

Still, the two models are impossible to distinguish using the aggregate time path of exit from unemployment. As we discussed above, the standard model can also fit this path if one allows for unobserved heterogeneity, a plausible assumption. How would one test then for reference dependence in job search?

We sketch a simple model which highlights three robust predictions of the reference-dependent job search model which are not shared by the standard model, even with unobserved heterogeneity. Consider two UI systems, both of which have the same benefit level after some period T (say, from a second social insurance tier, such as welfare benefits or unemployment assistance). The first UI system however offers a constant benefit path, while the second one offers high initial benefits (up to T_1), while lower benefits between T_1 and T (Figure 3.1 a). The standard model predicts that, starting from period T, the hazard rate in the two systems would be the same, as the future payoffs are identical (Figure 3.1 b). Furthermore, the hazard rate before period T will be higher in the system withtwo step benefits given the moral hazard. Allowing for unobserved heterogeneity would alter the plot qualitatively, but the qualitative predictions above would hold.

The reference dependence model makes three qualitatively different predictions (Figure 3.1 c). First, right after period T the hazard in the second system would be higher because the loss in consumption relative to the recent benefits is larger. Second, this difference would attenuate over time and ultimately disappear as the reference point adjusts to the lower benefit level. Third, the hazard rate in the first UI system increases already *in advance of* period T, in anticipation of the future loss aversion.

We evaluate a change in the Hungarian unemployment insurance system which is ideally suited for a test of the above predictions. Before November 2005, the Hungarian system featured a constant replacement rate for 270 days, followed by lower unemployment assistance benefits. After November 2005, the system changed to a two-step unemployment system: benefits are higher in the first 90 days, but lower between days 90 and 270, compared to the pre-period (Figure 3.2). Importantly, there was no major change in the unemployment assistance system taking place after 270 days. As such, this UI set-up corresponds to the hypothetical case outlined above when evaluated around 270 days.

An additional feature of the set-up simplifies the evaluation of the pre- and post-regime.

[2] A hand-to-mouth consumption rule is approximately accurate if workers are highly impatient, as our estimates suggest. In ongoing work, we aim to estimate a model which includes a consumption-savings decision.

Differences in total benefits paid out could complicate the evaluation of the UI change given that they could lead to differences in the selection of workers still unemployed at 270 days. Yet, an important feature of the Hungary reform is that the total amount of benefits paid out to individuals unemployed up until day 270 remains about the same after the reform. Hence, differences in savings and in selection in the pre- and post- period are likely to be relatively small, allowing for a more straightforward comparison. We then evaluate the reform by comparing the hazard rates in the year before and after the reform.

The impact of the reform on hazard rates is strikingly in line with the predictions of the reference-dependent model. In the period immediately preceding the 270-day exhaustion of benefits, the hazard rate in the pre-period rises above the hazard rate in the post-period, despite the fact that benefits are higher in the pre-period. In the months following the exhaustion, the hazard rate in the pre-period remains higher, and then it ultimately converges to the post-period level after a couple months. The observed pattern around the exhaustion is consistent with the anticipation of, and then the direct effect of, the higher loss in consumption for individuals in the pre-reform period. The ultimate convergence between the two hazards indicates, in this interpretation, the timing of the reference point adjustment.

While we focused so far on the hazard rate around the exhaustion of benefits, we observe a similar spike in the hazard at 90 days in the post-period, corresponding to the first step down in benefits. Similarly to the pattern observed around day 270, the surge in hazard disappears after 3-4 months. However, notice that the spike itself in this period can be explained by the standard model.

We present several robustness checks of the policy evaluation. First, we show that controlling for a broad set of observable controls barely affects the estimated hazards. Second, we show that differential ways to control for a contemporaneous introduction of a re-employment bonus has a minimal effect on the results. (And in any case this change is unlikely to have any effect for individuals still unemployed after 200 days) Third, we present an event study analysis of the changes in the hazards showing that the breaks in the hazards occur immediately in the quarter of introduction of the reform, and do not appear to reflect previous trends.

In the final part of the paper, we structurally estimate a model of job search with optimal search effort and unobserved heterogeneity of cost of search.[3] Since the reference-dependent model embeds the standard model, we compute the best fit both with and without allowing for reference dependence. We estimate the model with a minimum-distance estimator, matching the empirical hazard rates from unemployment in the pre- and post-period to the predictions of the model.

The best estimate for the standard model does a relatively good job of fitting the hazard rate path in the first 200 days. In particular, it matches qualitatively the spike in the post-hazard at 90 days, and the later decrease given a substantial degree of estimated heterogeneity in costs of search. The standard model, however, is unable to capture the observed behavior leading up to, and following, the exhaustion of benefits. In particular, as discussed above, the hazard rates from period 270 on in the pre- and post-period are predicted to be almost identical, counterfactually.

The best estimate of the reference dependence model captures the spike at 90 days and the

[3]The model does not currently allow for a reservation wage choice, and assumes that consumption equals the benefits. We aim to relax both assumptions. In preliminary estimates, allowing for a reservation wage choice has little effect on the results.

subsequent decrease, similar to the standard model (and with a closer fit). Importantly, this behavioral model also captures key features of the data which the standard model does not fit: the increase in hazard in the month prior to the expiration of benefits in the pre- period, the spike at 270 days, the decrease thereafter, and the ultimate convergence of the hazard between the pre- and post-period after a few months. The fit of the model is not perfect: the model underfits the spike at 270 days and the difference in hazards in the following two months. Still, it captures most of the qualitative features which the standard model does not fit at all. Interestingly, the reference dependent model, even when estimated without allowing for any unobserved heterogeneity, still provides a better fit of the data than the standard search model with heterogeneity. In this latter comparison, the reference-dependent model fits better despite having fewer parameters.

Turning to the point estimates, the model estimates that the weight on the gain-loss utility is at least as large as the weight on consumption utility, indicating an important role for loss aversion in job search.[4] The estimates also indicate that the reference point is updated quite slowly, as an average over the income over the past 240 days. This is one of very few estimates of the speed of updating in backward-looking reference-point models (see also Post et al., 2008).

We examine alternative specifications of the model. For the reference-dependent model, we allow for different levels of gain and loss utility and for an alternative process of reference-point updating, leading to similar results. We also allow for different specifications of the utility functions and for estimation of the discount factor, neither of which alters the qualitative results. Third,, we compare the estimates of the reference-dependent model to the estimates of a habit-formation model a la Campbell and Cochrane (1999). This latter model, like the reference-dependent model, induces a temporarily high marginal utility of income following a benefit cut. The habit-formation model indeed fits the data similarly to the reference-dependent model, although the fit is not quite as good. We also show that incorporating job acceptance decisions and reservation wages does not quantitatively alter our resutls. Finally we show, that the estimates are quite robust to using alternative samples of unemployed workers.

The chapter relates to the literature on job search and the design of unemployment insurance. This literature has mainly focused on the impact of the maximum duration and level of benefits, often using the estimated elasticities to gauge the welfare consequences of unemployment insurance (e.g. Chetty 2008, Kroft, Notowidigdo 2010, Schmieder, von Wachter, and Bender 2012). We evaluate a different type of reform: rather than changing the level or duration of benefits, the reform in Hungary changed the time path of the benefit schedule, keeping the overall payments approximately constant. While the theoretical literature of optimal unemployment insurance (e.g. Hopenhayn and Nicolini 1997, Pavoni 2007) has argued that benefits that gradually decline over the unemployment spell are likely optimal, we are not aware of research that has evaluated reforms that change the time path without also greatly increasing or reducing the generosity of the UI system.

The chapter also contributes to a small literature on behavioral labor economics, including work on gift exchange between employer and employee (Akerlof, 1982, Fehr, Kirchsteiger,

[4]The model allows for gain utility as well. Given that the unemployment benefits never increase over the unemployment spell, the gain utility applies to the utility of reemployment, not to the utility of unemployment. Such gain utility does not alter the path of exit from unemployment substantially.

and Riedl, 1993 and Gneezy and List, 2006), horizontal pay equity (Kahneman, Knetsch and Thaler, 1986; Card, Mas, Moretti, and Saez, 2012), and target earnings in labor supply (starting from Camerer et al., 1997). More relatedly, within job search, DellaVigna and Paserman (2005) consider the impact of present-bias while Spinnewijn (2013) examines the role of overconfidence. We show that a reference-dependent model of job search make unique predictions which are not shared by these other models.

The chapter also relates to the behavioral literature on reference dependence. Evidence of reference dependence comes from a number of settings including insurance choice Sydnor (2010); Barseghyan et al. (2013), labor supply Fehr and Goette (2007), domestic violence Card and Dahl (2011), goal setting Allen et al. (n.d.), and tax elusion Engström et al. (2013); Rees-Jones (2013). Across most of these settings, the reference point is the status-quo, or the forward-looking expectation (as in Koszegi and Rabin, 2006). In this chapter, the set-up with varying payoffs allows us to estimate the speed of updating of a backward-looking reference point as in Bowman, Minehart, and Rabin (1999); the only other example we are aware of is Post et al. (2008). This chapter is also part of a growing literature on structural behavioral economics which aims to identify the underlying behavioral parameters Laibson et al. (2007); Conlin et al. (2007); DellaVigna et al. (2012).

The chapters proceeds as follows. In Section 2, we present a simple model of job search and reference dependence. In Section 3 we present the institutional details and the data for the Hungary unemployment insurance reform, which we evaluate in Section 4. In Section 5 we present the structural estimates, and we conclude in Section 6.

3.2 Model

In this section we present a simple discrete-time model of job search with reference dependent preferences. We build on the job search intensity model presented in Card, Chetty, and Weber 2007a by adding a reference dependent utility function in consumption with backward looking reference point.

Each period a job seeker decides on how much effort $s_t \in [0,1]$ to put into searching for a job, which represents the probability of receiving a job offer at the end of period t and thus of being employed in period $t+1$. Search costs are given by the function $c(s_t)$ each period and we assume that the twice continuously differentiable function $c(s)$ is increasing and convex, with $c(0)=0$ and $c'(0)=0$.

The individuals receive unemployment benefits b_t if unemployed at period t, and they consume their income. Hence, the utility from consumption in period t for an unemployed person is $v(b_t)$. The novel aspect is the fact that the reference-dependent individual has, in addition to consumption utility $v(b_t)$, also gain-loss utility. Following the functional form of Koszegi and Rabin (2006), flow utility in each period is

$$u(b_t|r_t) = \begin{array}{ll} v(b_t) + \eta\left[v(b_t) - v(r_t)\right] & \text{if} \quad b_t \geq r_t \\ v(b_t) + \eta\lambda\left[v(b_t) - v(r_t)\right] & \text{if} \quad b_t < r_t \end{array} \tag{3.1}$$

where r_t denotes the reference point for consumption in period t. The utility consists of the consumption utility $v(b_t)$ and in addition of the gain-loss utility $v(b_t) - v(r_t)$. Whenever the consumption is on the gain side relative to the reference point ($b_t \geq r_t$), the individual derives gain utility $v(b_t) - v(r_t) > 0$, which receives weight η. Whenever the consumption is on the loss side relative to the reference point ($b_t < r_t$), the individual derives loss utility $v(b_t) - v(r_t) < 0$, with weight $\lambda\eta$. The parameter $\lambda \geq 1$ captures the loss aversion, the fact that the marginal utility of consumption is higher on the loss side than on the gain side. This reference-dependent utility function builds on prospect theory (Kahneman and Tversky, 1979) without, for simplicity, modelling either diminishing sensitivity or probability weighting. Notice also that the standard model is embedded as the special case for $\eta = 0$.

The second key set of assumptions is the determination of the reference point $r_{t.}$ Unlike in the recent literature on forward-looking reference points (Koszegi and Rabin, 2006 and 2007), but in the spirit of the literature on habit formation and of the older tradition on backward-looking reference points (Minehart, Bowman, and Rabin, 2001), we assume that the reference-point is a weighted average of past income over the N preceding periods:[5]

$$r_t = \frac{1}{N} \sum_{k=t-N}^{t-1} y_k.$$

To gain perspective on the impact of reference dependence on the marginal utility of consumption, consider the impact on utility of a small, permanent cut in benefits from b to $b - \Delta b < b$, taking place in period T. Assume that for the previous T periods, with $T > N$, benefits were constant, so that the reference point r_T equals b and utility in period $T-1$ equals $v(b)$ (there is no gain-loss utility in steady-state). Then in period T the utility changes to $v(b - \Delta b) + \eta\lambda [v(b - \Delta b) - v(b)]$. The short-term change in utility $u(b_t|r_t)$ is, up to a linear approximation, equal to $(1 + \eta\lambda) \Delta b v'(b)$. Over time, however, the reference point adjusts to ultimately equal $b - \Delta b$ so that the utility after N periods equals $v(b - \Delta b)$. Hence, the long-term change in utility equals just $\Delta b v'(b)$, while $\eta\lambda$ captures the additional short-term utility response to an income loss.

The reference point at time t depends on income in the past N period. For unemployed workers the reference point in period t is given by the benefit path (and by the pre-unemployment wage). For workers who have found a job, the reference point depends on how many periods prior to t a worker found a job. To make this distinction explicit, let's denote r_t the reference point in period t if the individual was unemployed (at least) until period $t-1$, and let's denote $r_{t|j}$ the reference point of an individual in period t who started a job in period j. Note that $r_{t|t} = r_t$.

Turning to the job search decision, each period when unemployed, the worker chooses the search effort s_t to maximize the following value function:

$$V_t^U = \max_{s_t \in [0,1]} u(b_t|r_t) - c(s_t) + \delta \left[s_t V_{t+1|t+1}^E + (1 - s_t) V_{t+1}^U \right] \tag{3.2}$$

where V_{t+1}^U is the continuation payoff from being unemployed in period $t+1$ and $V_{t+1|t+1}^E$ is the continuation payoff of being employed in period $t+1$ conditional on finding a job that starts in period $t+1$. Writing the value function of employment conditional on the time when a person starts a job becomes relevant below, since the time when a person finds a job will determine the path of the reference point in future periods.

[5] In the estimation below we also consider alternative ways of reference point formation, such as an AR(1) process.

We assume that individuals hold a job with wage w forever after finding a job, with the wage w larger than the benefits b_t at any period. As such, V^E_{t+1} is given by

$$V^E_{t+1|t+1} = \frac{v(w)}{1-\delta} + \eta \sum_{i=1}^{N} \delta^i \left[v(w) - v\left(r^{t+1}_{t+i}\right)\right].$$

The first term in $V^E_{t+1|t+1}$ is the standard term from receiving consumption utility of $v(w)$ forever, while the second term consists of the gain term, where the reference point will adjust over time. Notice that the second term disappears after N periods, since by then the reference point $r_{t+N+1|t+1} = w$. We solve the model by backward-induction starting from a point $\bar{T}$ after which we assume that search effort is stationary (at least N periods after an individual finds a job).

Equation (3.2) for the case of interior solution implies that the optimal search s_t satisfies

$$c'(s^*_t) = \delta \left[V^E_{t+1|t+1} - V^U_{t+1}\right]. \tag{3.3}$$

Given our assumptions above we can define the inverse of the first derivative of the cost function: $\mathcal{C}(.) = c'^{-1}(.)$, so that we can solve for s^*_t:

$$s^*_t = \mathcal{C}\left(\delta \left[V^E_{t+1|t+1} - V^U_{t+1}\right]\right) \tag{3.4}$$

To highlight the predictions of the model and to contrast it with the standard model, we consider a specific reform of an unemployment insurance system that closely corresponds to our empirical setting in Hungary. Suppose the UI system has two possible levels in the UI benefit path. For the first T_1 periods benefits are paid at a level of b_1, then they change to a second level b_2 until T and afterwards may drop to a final lower second tier (such as social assistance) with benefits $\underline{b}$. If there is no step-down in this UI system, this would be captured by $b_{constant} = b_1 = b_2$, which corresponds to the UI system in many countries including the US, and is illustrated as the blue solid line in Figure 3.1 (a).

Now consider a reform that front-loads the UI benefit path, by raising benefits b_1 in the first T_1 periods and reducing them in the periods T_1 to T, as indicated by the red dashed line in Figure 3.1 (a). Also assume that the total amounts of benefits paid under the old and the new regime for an unemployed individual who is unemployed for at least T periods is identical (that is the initial increase is exactly offset by the drop between T_1 and T) such that:

$$b_1 T_1 + b_2(T - T_1) = b_{constant} T \tag{3.5}$$

The benefit level in the second tier $\underline{b}$ in the periods following T is unaffected. Note that if equation (3.5) holds, then we have $\frac{\partial b_2}{\partial b_1} = -\frac{T_1}{T_2 - T_1}$. We compare how optimal search effort s^*_t is affected by a marginal increase in b_1 subject to the constraint (3.5) in the standard and the RD model. We express the results in terms of $\frac{ds^*_t}{db_1} = \frac{\partial s^*_t}{\partial b_1} - \frac{T_1}{T_2 - T_1}\frac{\partial s^*_t}{\partial b_2}$, where the total derivative takes the implied adjustment of b_2 into account and thus captures the full movement of the benefit path.

The following proposition states how the benefit increase will affect search effort in the periods $t \geq T$, that is after benefits are exhausted.[6]

[6]Note that search effort in period t is not affected by UI benefits in period t, since the individual will only start a job found in period t in period $t+1$. Thus search effort s_t corresponds to the exit hazard from unemployment in period $t+1$: $s_t = h_{t+1}$.

Proposition 2. *Consider a shift in the benefit path that front-loads the benefits.*

a) In the standard model ($\eta = 0$), the search effort in all periods after T is unaffected: $\frac{ds^*_{T+i}}{db_1} = 0$, *for* $i = 0, 1, ...$

b) In the reference-dependent model ($\eta > 0$ and $\lambda \geq 1$) search effort (weakly) increases temporarily in the first N periods after T, and remains constant in later periods: $\frac{ds^*_{T+i}}{db_1} \geq 0$, *for* $i = 0, 1, ...N-1$ *and* $\frac{ds^*_{T+i}}{db_1} = 0$, *for* $i = N, N+1, ...$ *Furthermore, if the adjustment speed N of the reference point is shorter than T, then the inequality is strict:* $\frac{ds^*_{T+i}}{db_1} > 0$, *for* $i = 0, 1, ...N-1$

The first part is straightforward from equation (3.4). In the standard model, the search decision depends exclusively on future benefits and wages, and the reform leaves unaffected the benefits past period T.

In the reference-dependent model, instead, past benefits may affect current search effort through the reference point. Taking the derivative of equation (3.4) with respect to b_1 we get:

$$\frac{ds^*_t}{db_1} = \left(\frac{dV^E_{t+1|t+1}}{db_1} - \frac{dV^U_{t+1}}{db_1} \right) \mathcal{C}' \left(\delta \left[V^E_{t+1|t+1} - V^U_{t+1} \right] \right) \tag{3.6}$$

The second part on the right hand side $\mathcal{C}' \left(\delta \left[V^E_{t+1|t+1} - V^U_{t+1} \right] \right)$ is always positive, so the sign of $\frac{ds^*_t}{db_1}$ is determined by the first part. To see that the first part is also positive, notice that

$$\frac{dV^E_{t+1}}{db_1} = -\eta \frac{dv(r_{t+1})}{db_1} + \delta \frac{dV^E_{t+2|t+1}}{db_1} \tag{3.7}$$

and

$$\frac{dV^U_{t+1}}{db_1} = -\lambda\eta \frac{dv(r_{t+1})}{db_1} + \delta \left(s_{t+1} \frac{dV^E_{t+2|t+2}}{db_1} + (1 - s_{t+1}) \frac{dV^U_{t+2}}{db_1} \right). \tag{3.8}$$

.For $t + 2 = T + N + 1$, i. e. a person one period before his reference point reaches the new steady state, we have that $\frac{dV^E_{t+2|t+2}}{db} = \frac{dV^U_{t+2}}{db} = 0$ and in that case clearly $\frac{dV^E_{t+1|t+1}}{db_1} - \frac{dV^U_{t+1}}{db_1} = -\eta \frac{dv(r_{t+1})}{db_1} + \lambda\eta \frac{dv(r_{t+1})}{db_1} = (\lambda - 1)\eta v'(r_{t+1}) \frac{dr_{t+1}}{db_1} \leq 0$. The intuition is simply that the change in benefits decreases the reference point in period $t+1$ (whether employed or unemployed) and thus increases the utility flow. But since loss utility is larger than gain utility, the value of unemployment changes more and thus the gap between $V^E_{t+1|t+1}$ and V^U_{t+1} decreases, thus reducing the returns to searching for a job.

For $t + 2 < T + N + 1$, but still $t + 2 \geq T + 1$, we also have to consider the second term on the right hand sides of equations (3.7) and (3.8). We provide a formal proof in the appendix to show that the second term in (3.8) is larger than the term in (3.7) which implies part b) in Proposition 1. The intuition is that while the part $\frac{dV^E_{t+2|t+1}}{db_1}$ is positive (thus increasing incentives to search), the part in equation (3.8) is always larger due to the fact that changes in the reference point for someone who does not find a job in period $t+1$ has a lower reference point in period $t+2$ and the concavity of $v(.)$ as well as the fact that any utility coming from future time spend in unemployment is lower due to $\lambda > 1$.

Thus in the periods after benefits are exhausted ($t > T$), the benefit path change has starkly different effects on search effort: In the standard models, search effort will not be affected, while in the RD model search effort will decrease for the next N periods. In the periods before benefits are exhausted, the standard model predicts an increase in search effort, since the value of unemployment is decreased, while the value of employment is unaffected. In the RD model the predictions are ambiguous and depend on the exact parameters: On the one hand the increase of b_1 will tend to increase the reference point in periods that are close enough to the first benefit step that the increase in b_1 has a larger impact on the reference point than the decrease in b_2. This would tend to increase search effort in periods prior to T where N is suffienctly long and t close enough to T_1. For shorter N and t being close to T, the decrease in b_2 will dominate, reducing the reference point and thus reducing search effort. In addition to the changing incentives coming from the gain-loss part of the utility function, there is also the direct part, that a decrease in benefits b_2 will reduce the utility from unemployment and thus tend to increase search effort. Which of these effects dominate in the pre-T periods depends on the exact values of the parameters.

The predictions of the standard model are highlighted in Figure 3.1 (b). The optimal search effort will increase under both regimes up until period T, and then plateau at a constant level after period T, since the two regimes have identical benefits moving forward. Moreover, the hazard rate for regime 1 is lower than the one for regime 2, given that the benefits are frontloaded in regime 1, thus reducing the future value of staying unemployed and increasing the incentives to find a job..

The optimal search effort under reference-dependence is quite different and shown in (c). First, the search effort at period T is substantially higher under the first regime, since individuals experience a sharp drop in consumption and thus (for $N < T$) experience significant loss utility due to their high reference point. The difference in hazards persists but in attenuated form in the subsequent period, until it fully goes away after N periods, which is the time length afer which the reference point has been full updated to the new benefit level. After this point, there is no more of a difference.

In addition, loss aversion generates a difference in hazards in anticipation of future losses. Namely, in the last few period before period T, for sufficiently large loss aversion λ, the hazard is actually higher under regime 1 compared to regime 2, despite the fact that regime 1 has more generous benefits (in sharp contrast to the standard model). This reflects the fact that the reference-dependent agents anticipate the future loss, and this anticipation is stronger under regime 1. To the extent that this force is stronger than the usual direct benefit effect, we observe the pattern in the graphs.

While we do not include savings in this model, note that if individuals could save, then the standard model would actually predict a decrease in search effort after benefits are exhausted relative to a regime without a UI benefit increase. This is because some of the additional UI benefits will be saved thus increasing the value in unemployment and reducing the pressure to find a job. Thus even savings would not alter the insight from the model, that an increase in search effort (after T) in response to an increase in b would strongly support the RD model over the standard model.

3.3 Data and Institutions

3.3.1 Unemployment Insurance in Hungary

Hungary had a generous unemployment insurance system in the period we examine. The UI insurance had a two tiered structure. In the first tier, potential duration and benefit amount depended on past UI contribution.[7] The maximum potential duration, which was obtained after around 4 years of contribution, was 270 days,[8] while the benefit was calculated based on the earnings in the previous year. After all the benefit had been exhausted in the first tier, "unemployment assistance" (UA) benefits could be claimed in the second tier. The benefit amount in this tier was the same for everybody, while the potential duration depended on age.

On May 30th, 2005 the Hungarian government announced a comprehensive reform of the unemployment insurance system.[9] The main goal of the new UI regulation was to speed up transition from unemployment to employment. To achieve this goal, the government changed the benefit calculations formula in the first tier, but did not alter the way potential duration and earnings base were calculated. Before the reform, the benefit in the first tier was constant with a replacement rate of 65% and with minimum and maximum benefit caps. After the reform, a two-step benefit system was introduced. The length of the first step was half of the potential duration in the first tier, and at most 91 days. In the first step, the replacement was lowered to 60%, but both the minimum and maximum benefit caps were increased substantially. For most UI claimants these changes meant higher benefits than under the old schedule. On the other hand, in the second tier everybody received the new minimum benefit amount. In practice, most UI claimants received lower benefits in this tier than before. The benefit formula changes are summarized in Figure 3.2.

The most prominent change occured for those who had 270 days eligibility (four years of UI contributions before lay-off) and had base year earnings above the new benefit cap (that is, they earned more than 114,000HUF ($570) per month in 2005). The old and new benefit schedules are summarized on Figure 3.3 for this group. In the first tier, the potential duration is 270 days both before and after. In the old system, the benefits were constant in the first tier. On the other hand, under the new rules, benefits increased substantially in the first 91 days, but decreased afterwards. An important feature of the reform for this particular group is that the benefit increase in the first 91 days is almost the same as the benefit decrease between 90 and 270 days. Therefore, the expected benefit pay-out for individuals who were unemployed for 270 days is very similar under the two benefit schedules.

Even though the main element of the reform was the new benefit formula, there were other changes that occured at the same time. Most notably, a reemployment bonus scheme was introduced as well. The bonus amount was 50% of the remaining total first tier benefits. However, claiming the bonus was not without costs. First, if the bonus was claimed, then the

[7] Every worker in the formal sector must pay a UI contribution. In 2005, employers contributed 3% to the UI fund, while employees 1%. There is no experience rating of UI benefits in Hungary.

[8] More specifically, potential benefit in the first tier was calculated as UI contribution days in the last 4 years divided by 5, but at most 270 days.

[9] The reform was part of a wider government program (called "100 steps"). Policies related to the labor market and unemployment insurance (such as reemployment bonus and training polcies) are discussed latter. In addition to that VAT and corporate income tax were decreased from January 1st.

entitlement for the unused benefit days was nulled. This could be very costly for risk-averse agents or for those who could only find an insecure job. Second, the bonus could only be claimed after the date of first tier benefit exhaustion. In practice, this meant substantial hussle costs, since UI claimants had to show up one more time in the local UI office and fill out the paper work. Given the presence of these costs, it is not surprising that the take-up rate of reemployment bonus was only 6%. In our main analysis, we focus on the pattern of the hazard that should not be affected by the presence of the reemployment bonus. Moreover, as a robustness check we show that the pattern of the hazard and our results are not sensitive to dropping the reemployment bonus users from our sample.[10]

In addition to the introduction of the reemployment bonus, there were other minor changes that are relevant for our analysis. First, those who claimed UI benefit before February 5th, 2005 faced a shorter[11], but somewhat higher, benefit in the second tier.[12] To avoid the complications that this change caused we only focus on those who claimed their benefits after February 5th, 2005. Second, there were some minor changes in financing training programs.[13] Howevever, participation in training programs was very low (less than 5%) in our sample and our results are robust to dropping these claimants.

Those who exhausted benefits in both tiers and were still unemployed could claim means tested social assistance. The duration of social assistance is indeterminate, while the amount depends on family size, family income, and wealth. In most cases social assistance benefits are lower than the second tier UI benefit level. [14]

3.3.2 Data

We use administrative data[15] that contains information on the social security contributions for roughly 4 million individuals between January 2002 and December 2008. Every Hungarian citizen who was older than 14 and younger than 75 in 2002 and who was born on even days of months was selected into our sample. Therefore, the sample represents roughly half of the Hungarian population. Information on UI claims from February 2004 to December 2008 were

[10]Lindner and Reizer (2014) investigate the reemployment bonus in detail and show that it does not affect the shape of the hazard function.

[11]Before the reform, the potential duration in the second tier was 270 days above age 45 and 180 days below 45. Those who claimed UI after February 5th, 2005 were eligible for 180 days above age 50 and 90 days below 50 in the second tier.

[12]The change in the duration and benefit level in the second tier was introduced at November 1st, 2005 at the same time as other changes. However, it affected everybody who claimed second tier (UA) benefits after November 1st, 2005. A UI claimant who claimed her benefits after Ferbraury 5th, 2005 and had 270 days potential eligibility, could only claim second tier benefits (UA) after November 1st, 2005. Therefore, claimants between February 5th, 2005 and November 1st, 2005 are under the old benefit system in the first tier, but face with the same second tier (UA) insurance scheme, see Figure 3.4.

[13]Unemployed participating in training programs received the so-called income substituting benefit. Before November 1st, 2005 this amount was 22,200HUF ($111) or 44,400HUF ($222), depending on household characteristics and type of training. This benefit was payed in excess of the UI. After November 1st, the benefit was 34,200HUF ($171) for everybody. However, the UI benefit was suspended during training. Although ee can only observe training participation after November 1st, 2005 aggregate data show that the probability of participation in training programs remained constant throughout this periodFrey (2009).

[14]For large families, social assistance can be more generous than UI. However, social assistance cannot be claimed before all other benefits have been exhausted in the UI system.

[15]The dataset is requested and cleaned by the Institute of Economics - Hungarian Academy of Sciences.

merged to the data. We also observe basic information used by the National Employment Service, in particular, the starting and ending date of the UI benefit spells and the earnings base that is used for benefit calculations.

In this chapter we only focus on UI claimants who are eligible for the maximum potential duration (270 days) in the first tier. The reason for this is that we would like to avoid the complications caused by varying potential duration. In addition to that we restrict our sample to those who are older than 25 years and younger than 49 years. We drop the older population, since specific rules were applied close to retirement. Moreover, we identify as our main sample UI claimants with high earnings base, since our goal is to explore the variation showed in Figure 3.3. To construct a consistent sample over time, we focus on the unemployed whose earnings base was above the 70th percentile of the earnings base distribution of the UI claimants in the given year. In 2005, a UI claimant at the 70th precentile earned 100,800 HUF ($504).[16]

3.3.3 Descriptives

Our empirical analysis focuses on how search behavior of UI claimants was affected by the reform in November 2005. We construct two comparison groups of workers who entered UI just before or just after the reform, since the claiming date determined under which regime an individual was. Due to the change in unemployment assistance in February 2005, we use all UI claimants between February 5th, 2005 and October 15, 2005 (to avoid getting too close to the reform) as our pre-reform group. In order to get a comparable post-reform group that shows similar seasonal patterns, we take UI entrants in the same date range (February 5 to October 15) in 2006 as our comparison group. Figure 3.4 shows the timing of the two comparison groups, as well as highlights the range for which our data is available. For robustness checks, we will later show results using data in the earlier and later ranges as well. Table 3.1 shows basic descriptives for the two groups. The basic demographic characteristics are almost unchanged. Age at time of claiming, education and log earnings in the years 2002 - 2004 are very similar. We also find that the waiting period (the number of days between job loss and the time of claiming UI benefits) is almost identical across the two groups, indicating that people towards the end of our before sample were not trying to delay UI claiming dates in order to become eligible to the new regime.[17]

As we mentioned before, after 2005 the Hungarian government also introduced a reemployment bonus scheme. The take-up rates are quite low in the post period (and by default zero in the pre period). Below we present careful robustness checks to address the possibility that this bonus may have affected our results.

[16]Our results are robust to alternative earnings thresholds over time. For example, we estimated our main specifications for those whose (real) earnigs base was above 114,000 HUF ($570) and obtained virtually the same results.

[17]Appendix Figure A-1 shows the unemployment rate and GDP growth rate around the two periods in Hungary. The unemployment rate was quite stable at around 7.5 percent during and after the two sample periods. GDP growth was also stable during the sample periods, only slowing down at the beginning of 2007. Below we show extensive robustness checks, showing that our results are not driven by changes in the economic environment that occurred later and that the shape of the hazard rates are in fact very stable over time except for the exact point when the UI policy changes.

3.4 Reduced Form Results

3.4.1 Estimating Hazard Plots

In this section, we evaluate the impact of the reform on the exit rates from unemployment. We focus in particular on the hazard rates around the exhaustion point at 270 days, which is where the models make the most distinct predictions. We estimate the hazard rates with a linear probability model separately for each 15 day period, indexed by t, after entering unemployment insurance:

$$I(t_i^* = t | t_i^* \geq t) = \beta_{0,t} + \beta_{1,t} POST_i + X_i \gamma + \epsilon_{it}, \tag{3.9}$$

where i indexes individuals and t_i^* represents the duration of unemployment of individual i. The left hand side is an indicator for individual i finding a job in period t, conditional on still being unemployed at the beginning of the period. The variable $POST_i$ is an indicator for individual i claiming benefits in the post-reform period, while X_i is a matrix of control variables. The equation is estimated separately for each period t on the sample of individuals who are still unemployed at time t (that is conditional on $t_i^* \geq t$). The estimates for $\beta_{0,t}$ are estimates for the hazard function in the pre-period, while the estimates for $\beta_{1,t}$ represent the shift of the hazard function between the before and after period. In our baseline estimates we do not control for any observables X_i, and instead show results controlling for X_i as additional specifications.

Note that these hazard functions should not be viewed as consistent estimates on the individual level, but rather as estimates of the average hazard function in the population before and after the reform. While the natural experiment, assuming the CIA holds, identifies the causal effect of the reform on the average hazard function in the population, the shape of this average hazard function is potentially affected by either behavioral responses (true duration dependence) or by changes in selection patterns that are due to the reform. While we address differential selection in our reduced form results section, by comparing how observables vary throughout the unemployment spell, as well as by comparing the estimated hazard function controlling and not controlling for observables, an important aspect of our structural estimation below will be to explicitely model the potential of unobserved heterogeneity affecting these hazard functions.

3.4.2 Main Result

Figure 3.5 (a) shows the estimates of equation (3.9) for each t with no control variable. The blue line represents the coefficient estimates of $\beta_{0,t}$- the estimated hazard function in the before period - while the red line represents the estimates $\beta_{0,t} + \beta_{1,t}$ - the estimated after period hazard. Vertical lines between the two periods indicate that the difference between the two lines is statistically significant at the conventional 5% level.

The exit hazard from unemployment in the pre-reform period shows a familiar pattern for a one-step unemployment system. The exit hazard falls in the first months after entering UI, and then it increases as it approaches the exhaustion point of UI benefits (at 270 days). After this exhaustion point, it falls and spikes again as people exhaust the second tier benefits, unemployment assistance, at 360 days. The hazard rate then decreases monotonically after this point, as unemployed people are only eligible for welfare programs.

The exit hazard changes substantially after the introduction of a two-step unemployment insurance system. The hazard rate increases at 90 days, at the end of the higher unemployment insurance benefit, and remains elevated compared to the pre-reform period for the following 2.5 months. By 180 days, the pre- and post-reform hazards have converged back, and both hazards increase at the exhaustion of the UI benefits at 270 days. Importantly, though, the post-reform hazard increases significantly less, and the pre-reform hazard remains significantly higher for the 2 months following the UI exhaustion. Finally, by 360 days, the end of the unemployment assistance, the two hazards have once again converged back together.

The most striking difference occurs around day 270, when in the pre-reform period the exit hazard remains significantly higher after the UI exhaustion point (270 days) relative to the after period. As we discussed above, this difference in hazards is hard to reconcile with the standard model: from day 270 onwards, the benefit levels are identical in the pre- and post-period, and in addition the total amount of benefits received up to day 270 is also almost identical. Hence, as we discussed in Section 3, in the standard model we would expect similar hazards (even with heterogeneity, as we show below).

The difference in hazards instead fits nicely with the reference-dependent model: the workers in the pre-reform period experience a larger drop-off in benefits around day 270, inducing a spike in loss utility and thus an increase in the value of search. The persistence for 2-3 months of the higher hazard suggests that it takes a substantial amount of time for the reference point to adjust to the new level. Furthermore, the increase in hazard in the pre-period happens already in anticipation of benefit expiration at day 270, consistent with the reference-dependent model.

While we focus mainly on the hazard rate around day 270 because it leads to the most distinct predictions, the observed patterns around day 90 are also consistent with reference dependence. The spike in the hazard at 90 days in the post-period, corresponding to the first step down in benefits, disappears after 3-4 months, consistent once again with loss utility relative to slowly-adjusting reference points. However, the spike itself in this period could also be explained by the standard model in the presence of unobserved heterogeneity, as we show further below.

In order to see how the reform affected the total amount spent in unemployment in the two groups, Figure 3.5 (b) shows the estimated survival function for the two groups. We obtain these estimates using a variant of equation (3.9), where we estimate the equation again pointwise for all t but including the whole sample and taking $P(t_i^* \geq t)$ as the outcome variable. This provides natural non-parameteric estimates of the survival function, as well as whether differences between the two are pointwise significant. The survival functions diverge after 90 days, with lower survival probabilities in the after group than in the before group. This difference persists until around 300 days, after which the two lines converge and the difference disappears. Since the expected duration in unemployment is simply the integral over the survival function from 0 onwards, the expected unemployment duration is significantly reduced in the after period. It is striking that even though the reform made the UI system more generous on average (since short term unemployed received more benefits, while the long-term unemployed received about the same overall), the expected duration actually decreased.

3.4.3 Robustness Checks

The results presented so far do not control for demographic characteristics. Even though the differences in demographics between the pre- and the post- period are quite small (Table 1), they could potentially explain differences in the hazard patterns over time if the demographic impacts on the hazard rates are large. Thus, we re-estimate equation (3.9) controlling for a rich set of observable characteristics, where we allow these characteristics to have arbitrary effects on the hazard function at each point, the only restriction being that the effect is the same in the before and after period. As Figure 3.6 (a) shows, controlling for observables has virtually no effect on the differences between the two hazard rates, implying that they cannot explain our findings. Alternatively we also used propensity score reweighting to estimate the hazards in the pre- and post-period, holding the observables constant over time and obtained almost identical results (not shown).

A separate concern regards the introduction of the reemployment bonus in November 1st, 2005. While the take-up rate of the bonus was just 6% in our sample, it is likely to affect the hazard rate in the post-reform period, especially in the first 90 days. One way to check for potential impacts of this is to drop all individuals that received a reemployment bonus and estimate our baseline specification on this restricted sample. Figure 3.6 (b) shows that the results are virtually unchanged.

In order to assure that the differences in the hazard rates are in fact due to the reform in the UI system and not simply the result of some general trend, we exploit the fact that we have additional data from 2004 and after 2006. First we estimated two 'placebo' tests for whether there are differences in the year 2 years before the reform and the year 1 year before the reform, using the same estimation strategy as before. We report these results in Appendix Figure A-2 (a), revealing that the hazard rates are virtually unchanged between 2004 and 2005. There is a small difference right after the 270 line, which is expected due to the reduction in unemployment assistance in February 2005, leading to a slight increase in the hazard at this point in 2005. Similarly Appendix Figure A-2 (b) shows that there are virtually no differences between the hazards 1 and 2 years after the reform, again indicating that the differences between our before and after period line up nicely with the reform and thus are likely due to the reform.

We explore the timing further by plotting time-series graphs of the exit hazards over specific intervals. Figure 3.7 (a) shows the evolution over time of the exit hazard between 30 and 90 days (red line) and between 90 and 150 days (black line). Each dot indicates the average hazard for each 3-month period between 2004 and 2007, with quarter 1 indicating the first 3-month period after the reform. Prior to the reform, the hazard at 90-150 days is smaller than the hazard at 30-90 days, consistent with the patterns in Figure 5. Subsequent to the reform introducing a step down of benefits after 90 days, the pattern abruptly changes. Already in the first quarter after the reform, the hazard at 90-150 days increases sizeably, becoming similar to the hazard at 30-90 days, a pattern that remains largely similar over the next 6 quarters. The figure provides little evidence of previous trends, suggesting that the changes in hazards are indeed a causal effect of the reform.

Figure 3.7 (b) provides parallel evidence for the hazard at 210-270 days versus at 270-330 days. In the quarters pre-reform, the hazard at 270-330 days is significantly higher than the hazard at 210-270 days, a pattern that changes abruptly with the first quarter following the reform. The time-series plots again indicate a change that is coincidental with the reform and not due underlying trends or changes in the macroeconomic environment.

3.5 Structural Estimation

Set-up. We use the model of Section 2, imposing four additional assumptions, some of which we relax later. First, we assume that the search cost function has a power form: $c(s) = ks^{1+\gamma}/(1+\gamma)$. This form implies that the parameter γ is the inverse of the elasticity of search effort with respect to the net value of employment. To see this, recall that the first-order condition of search effort (equation 3.3) is $c'(s^*) = v$, where we denote with v the net value of employment (that is, the right-hand-side of equation 3.3)). Given the parametric assumption, this yields $s^* = (v/k)^{1/\gamma}$, and the elasticity of s^* with respect to v is $\eta_{s,v} = (ds/dv)\, v/s = 1/\gamma$.

Second, we assume for most of the estimates a log utility function, $v(b) = \ln(b)$, but we also generalize to a power utility function, $v(b) = b^{1+\rho}/(1+\rho)$ which admits log utility as a special limit case, as well as to a linear utility function. Third, we assume that reemployment wages are constant over the UI spell and they are equal to past wages. In our main estimation we set past wages (and so reemployment wages) equal to the median earnings in our sample, which is 135,000 HUF ($675), but explore alternative assumptions below.

Fourth, we allow for a three-point heterogeneity among the unemployed workers in the cost of search. Thus, we estimate five parameters: three levels of cost of search k_{high}, k_{med}, and k_{low}, with the assumption $k_{high} \geq k_{med} \geq k_{low}$, as well as the probability at the start of the unemployment spell of low-cost types, p_{low}, and the probability of the medium-cost types, p_{med}.

The vector of parameters ξ that we estimate for the standard model are: (i) the three levels of search cost k_{high}, k_{med}, and k_{low}, and the two probability weights p_{low} and $p_{med;}$ (ii) the search cost curvature γ. For the reference-dependent model, we estimate in addition: (iii) the loss aversion parameter λ; and (iv) the number of (15-day) periods N over which the backward-looking reference point is formed.[18] Notice that the weight on the gain-loss utility η is set to 1 rather than being estimated; thus, the loss-aversion parameter λ can be interpreted also as the overall weight on the losses. The reason for this assumption is that over the course of the unemployment spell the individual is always on the loss side since the benefits are always (weakly) lower than the reference point. Hence, it is difficult to estimate a separate weight on gain utility and loss utility.[19]

Estimation. To estimate the model, we use a minimum-distance estimator. Denote by $m(\xi)$ the vector of moments predicted by the theory as a function of the parameters ξ, and by $\hat{m}$ the vector of observed moments. The minimum-distance estimator chooses the parameters $\hat{\xi}$ that minimize the distance $(m(\xi) - \hat{m})' W (m(\xi) - \hat{m})$, where W is a weighting matrix. As a weighting matrix, we use the diagonal of the inverse of the variance-covariance matrix. Hence, the estimator minimizes the sum of squared distances, weighted by the inverse variance of each moment.[20] To calculate the theoretical moments, we use backward induction. First

[18] In the estimations tables we report the speed of adjustment in days, which is just N*15.

[19] In principle, the weight on gain utility η could be separately identified since gain utility affects the utility of reemployment, but the remployment utility does not allow for precise identification of η, as we show in robustness checks below.

[20] As robustness check below, we alternatively use the identity matrix as a weighting matrix.

we compute numerically the steady state search and steady state value of being unemployed using a hybrid bisection-quadratic interpolation method, pre-implemented in Matlab as the fzero routine. Then going backward we analytically calculate the searching effort and the value of being unemployed in each period.

Under standard conditions, the minimum-distance estimator using weighting matrix W achieves asymptotic normality, with estimated variance $(\hat{G}'W\hat{G})^{-1}(\hat{G}'W\hat{\Lambda}W\hat{G})(\hat{G}'W\hat{G})^{-1}/N$, where $\hat{G} \equiv N^{-1}\sum_{i=1}^{N}\nabla_{\xi}m_i(\hat{\xi})$ and $\hat{\Lambda} \equiv Var[m(\hat{\xi})]$ **?**. We calculate $\nabla_{\xi}m(\hat{\xi})$ numerically in Matlab using an adaptive finite difference algorithm.

Moments. As moments $m(\xi)$ we use the 15-day hazard rates from day 15 to day 540. We include in the estimation the respective moments from both the pre-reform and post-reform period leading us a total of 35*2=70 moments. We do not use the hazard from the first 15 day period, since it would require modelling search on the job.

Identification. While the parameters are identified jointly, it is possible to address the main sources of identification of individual parameters. The cost of effort parameters k_j are identified from both the level of search intensity and the path of job search over time. This is clearest in the standard model, where the heterogeneity in the parameters is needed, for example, to explain the decay in the hazard after day 360, when benefits remain constant and thus, in absence of heterogeneity, the hazard would be constant in the standard model (but not in the reference-dependent model). The search cost curvature parameter, γ, is identified by the responsiveness of the hazard rate to changes in earnings since $1/\gamma$ is the elasticity of search effort with respect to the (net) value of finding a job. Once again, this is clearest in the standard case. The increases in hazard once the benefits decrease identify the elasticity, and as such the curvature parameter.

Turning to the reference-dependence parameters, for a given value of η (fixed to 1 in the benchmark specification), the parameter λ denotes the extent of the loss utility. A major component to identification for this parameter is the extent to which the hazard for the pre-group is higher both before and after day 270, in response to a larger loss. Remember that instead the standard model has essentially identical hazards from day 270 onwards. The loss parameter is, of course, also identified by the response to other changes in the benefits, such as at 90 days in the post- period. The parameter N is identified by the speed with which the losses are reabsorbed into the reference point. The main source of identification is the fact that the hazard is higher in the pre-period after day 270, but it converges again after 3-4 months. Similarly, the speed of convergence of the hazard after day 90 contributes, similarly suggesting several months of adjustment.

Estimates. We first present report the benchmark estimates, under the assumption that all unemployed workers are eligible for welfare payments following the end of the unemployment insurance period (after 360 days). Figure 3.8 (a) presents the fit for the standard model with 3-type heterogeneity. The model fits quite well the surge in hazard around day 90 in the post-period, and the decreasing path of the hazard in the first 200 days. The fit is also reasonably good for the period from day 400 on. However, the fit between days 250 and 400 is poor. As discussed above, the standard model predicts that the hazard rates for the pre- and post- period should be almost exactly the same after day 270. As such, the model misses both the sharp difference in hazard between day 260 and day 360, as well as the spikes at both 260 and 360 days.

In Column (1) of Table 2 we present the point estimates. The estimates for k indicate substantial heterogeneity, with k varying from the high-cost type at $\hat{k}_{high} = 235$ to the low cost type at $\hat{k}_{low} = 91$. The estimated ex-ante share of the high-cost type is very small, at $1 - .458 - .538 = .004$, ensuring that even after 300 days there is enough heterogeneity in the population left to reproduce the declining pattern of the hazard for long durations (400 days+). The estimate of the cost elasticity $\hat{\gamma} = .11$ indicates a high elasticity of search effort to incentives, needed in order to fit the large increase at 90 days in response to the different benefit levels. We also report the standard goodness of fit (GOF) measure $(m(\xi) - \hat{m})' W (m(\xi) - \hat{m})$, which allows to compare the model fit across different specifications.

In comparison, Figure 3.8 (b) displays the fit of the reference-dependent model with three types (and thus two more parameters compared to the standard model). The fit in the first 250 days is very good, though it was quite good also for the standard model. But, as anticipated, the model does much better for longer durations, when the standard model fits poorly. In particular, the model fits better the surge in the hazard rate in the pre-period in anticipation of the benefit cut after 270 days (which is larger in the pre period than in the post period), as well as the elevated level for the following three months, compared to the pre-period. Then the model tracks quite well the period following the exhaustion of unemployment assistance (after 360 days).

The fit of the reference-dependent model, while clearly superior to the standard model, is certainly not perfect. The most striking aspect of the data which the model does not capture is the very large spike on day 270 for the pre-period; storable offers may play a role in this case. In addition, the reference-dependent model under-fits the difference in hazards between the pre- and post-period after day 270.

Column (2) presents the point estimates. This model, which has two extra parameters, has a substantially better fit (GOF of 172 versus 243). The reference-dependence parameters are quite precisely estimated. The weight on loss utility is estimated at $\hat{\lambda} = 1.7$ (s.e. .2), indicating a substantial role for gain-loss utility. The estimate for the adjustment speed of the reference point N indicates a long duration of adjustment, $\hat{N} = 255$ (s.e. 34) days. The slow adjustment of the reference point is consistent with the duration of a few months before the spikes in hazard taper down, both after the benefit drop at 90 days in the post period, and after the benefit drop at 270 days. The estimates of the auxiliary parameters – the cost levels and the curvature of the cost of search function – are relatively comparable to the ones for the standard model.

A fair objection to the better fit of the reference-dependent model is that it has two extra parameters. Thus, in Column (3) we show the fit of the reference dependent model with no heterogeneity in costs, and thus only 4 parameters compared to 6 parameters for the standard model. Interestingly, this bare-bones model fits the data better than the standard model (goodness of fit of 217.6 compared to 243.1). As Figure 3.9 (a) shows, the qualitative fit is almost as good in this model as in the reference-dependent models with unobserved heterogeneity.

Appendix Figures A-6 and A-7 show some of the key model components for the benchmark standard estimates (Column (1) of Table 3.2) and the benchmark reference-dependent model (Column (2) of Table 3.2)). Panels a and b of Figure A-6 display the flow utility for unemployed workers. In the standard model (panel a), it follows the step down in the benefits, with the size of the later steps accentuated by the curvature of the utility function. In the reference-dependent model (panel b), the flow utility captures also the intensity of the loss relative to the reference point. In the pre-period (dotted blue line), for example, the flow utility of unemployment is particularly low at the beginning given the large loss relative to the pre-unemployment wage (which is the reference point then), and then it increases all the way to day 270 despite constant benefits because of adaptation in the reference point. Panels c and d show the value of unemployment for the low-cost type. In the standard model, the value of unemployment is always decreasing given that benefits never increase over time. In the reference-dependent case, instead, the value of unemployment actually increases most of the time reflecting the importance of reference-point adaptation. Panels a and b of Appendix Figure A-7 show that the value of employment is constant in the standard case, but increasing in the reference-dependent case. The increase occurs because over time the reference point declines and hence obtaining a job becomes more attractive because of the gain utility from finding a job. This increase in the value of employment is monotonic and nearly linear, unlike the pattern for the value of unemployment, and hence does not contribute much to the explanation of the patterns in the hazard. Finally, Panel c shows the path for the reference point.

In Columns (4)-(6) of Table 3.2 and and corresponding Figures 3.8 (c) and 3.8 (d) we consider the parallel results for an alternative benchmark. Given that take-up of welfare is low in the data, we assume that workers are not eligible for welfare, and allow instead for home production of consumption (also capturing spousal earnings), which enters the consumption function. The alternative assumptions improve somewhat the fit of the reference-dependent model, allowig it to fit better the increase in take-up at 360 days (Figure 8(d)), while the fit of the standard model is slightly worse. In the rest of the chapter, we present results for the first benchmark which, if anything, favors the standard model.

Alternative Reference-Dependent Models. In Table 3.3 we consider variants of the benchmark reference-dependent model (Column (2) in Table 3.2), reproduced in Column (1). First, we explore an alternative updating of the reference point. Instead of defining the reference-point as the average of past income over the N preceding periods, we assume the the reference point follows an AR(1) process:

$$r_t = \rho r_{t-1} + (1-\rho)b_t = (1-\rho)\sum_{i=1}^{\infty}\rho^i b_{t-i}$$

This updating rule has longer "memory" and adjusts more smoothly than the benchmark reference point, with the speed of adjustment captured by ρ.[21] Column (2) of Table 3.3 shows the estimated speed of adjustment ρ=0.83, which implies slower adjustment (half-life is 56.5 days) than in the benchmark case. The estimates for the other parameters such as λ and γ are close to the benchmark estimates. The goodness of fit with AR(1) updating, though, (188.4) is not quite as good as the benchmark estimates (172.6). Figure 3.9 (d) highlights that the AR(1) model does not fit quite as well the moments between 270 and 360 days.

Next, we disentangle the role played by gain and loss utility in the estimates. So far, we have arbitrarily set the gain utility parameter, η, to 1, thus fixing the gain utility at a set level, while estimating the weight on loss utility, $\eta\lambda$. In Columns (3) and (4) we examine the role of gain and loss utility by including only one at a time in the model. In Column (3) we assume

[21]When we implement this estimate we assume that the memory of the AR(1) update goes back to 1050 days (or 70 15-day period).

no gain utility when workers get a job, but still estimate the loss utility weight $\eta\lambda$. The fit of the model is almost as good as the standard one, and the estimated speed of updating of the reference point is nearly the same (though not the estimated loss aversion). In Column (4), we do the complementary exercise of not allowing for loss utility while unemployed, while modelling gain utility. This model does much worse and is unable to reproduce the difference in hazards past 270 days. This indicates the key role played by loss utility.

We present a parallel take on this result in the next three columns. Columns (5) and (6) report the estimates setting, respectively, a value of η of 0.2 and of 5. 3.9 Interestingly, as the (assumed) weight on gain utilityη increases, the estimated λ decreases, holding the product$\eta\lambda$, which is the weight on loss utility, at comparable (though not constant) levels. The goodness of fit is slightly better for $\eta = 5$ (168) than for the benchmark case (172.6) or for $\eta = 0.2$ (175), but, as it is shown in Figure 3.9 (b) and in Figure 3.9 (c), the predicted hazards are virtually the same as in the benchmark case. Along similar lines, in Column (7) we fix the loss aversion estimate to 1 and obtain comparable estimates.

Finally, in Column (8) we consider a model related to the reference-dependent one: a habit-formation model a la Campbell and Cochrane (1999). We replace our reference dependent utility function (defined in Equation (3.1)) with the following one:

$$v(b_t, r_t) = log(b_t - zr_t),$$

where z captures the responsiveness of the utility function to changes in the habit stock, while r_t is calculated the same way as before, but reinterpreted as a measure of habit stock.[22] This model, which embeds the standard model for $z = 0$, is similar to the reference-dependent model in that it induces a temporarily high marginal utility of income following a benefit cut. The habit-formation model indeed fits the data better than the standard model (204.6 in the habit model vs 243.1 in the standard model), although its performance lags behind the reference dependence model (172.6), as also Figure 3.9 (d) shows.

Robustness. In Table 3.4 we consider the robustness of the benchmark estimates of the standard model and of the reference-dependent model, first to alternative specifications of the utility function (Columns (1) to (5)) and then to alternative estimation methods (Columns (6) to (9)). We consider alternative curvatures of the utility function: linear utility (Column (1)), a CRRA utility function with relative risk aversion parameter of 0.5 (Column (2)), and CRRA utility function with relative risk aversion parameter of 2 (Column (3)). The best performing specification for the standard model is the linear utility in Column (1), but the improvement compared to our benchmark specification is negligible.[23] For the reference-dependent model the goodness of fit of these alternative models is slightly inferior compared to our benchmark specification, but the difference in fit is small and the estimates for key reference-dependence parameters, λ and N, are similar, suggesting that the curvature of the utility function is not crucial in explaining the observed hazard rates.

Then, in Column (4) we modify the cost of effort function by allowing for a linear time

[22] Observe that for low levels of b_t and high level of z this function is not defined. To avoid this problemCampbell and Cochrane (1999) made z a non-linear function of $b_t - r_t$. For simplicity we treat z as a parameter instead and we check in the optimum whether our utility function is defined for the relevant b_t and r_t.

[23] In the standard model we were able to estimate the relative risk aversion parameter and we found that the best performing CRRA utility function is close to linear utility (results are not reported).

trend in the baseline cost factor k. This allows for skill depreciation or conversely learning to search better. This additional parameter leaves the fit of the reference-dependent model unaffected, but it improves to some extent the fit of the standard model, though the fit after day 270 remains essentially the same.

For the final utility parameter, we consider in Column (5) the discount factor δ. The optimal estimate suggests a high rate of impatience, but the quality of the fit does not vary substantially, suggesting that time discounting does not play a critical role in the estimates.

In the next specifications we consider variants to the estimation procedure. In Column (6) we use the identity matrix to weight the moments in the minimum distance estimator and in Column (7) we use the moments estimated after controlling for observables (shown in Figure 3.6 (b)). Though the goodness of fit cannot be compared to the previous estimates, the qualitative conclusions are the same as before: the reference-dependent model fits substantially better than the standard model and the reference-dependence parameters, λ and N remain comparable to the benchmark specifications.

In Column (8) we vary the type of moments used. Instead of using the estimated hazard rates in each 15-day period, we use the estimated (unconditional) probability of exiting unemployment in each 15-day period. The advantage of this alternative procedure is that we can use the full variance-covariance matrix for weights. Once again, while the measures of fit are not comparable, the pattern of the results is very similar.

Finally, in Column (9) we explore the role played by the spikes in periods 270 and 360. One may worry that the spiked play a disproportional role in the identification given the quadratic distance measure used in the minimum distance estimator. When we re-estimate the model without using such moments. we find once again similar patterns indicating that the results are not driven by the spikes.

Unobserved Heterogeneity. In Table 3.5, we return to examining the role played by the type of unobserved types in the estimates. So far, we have mostly assumed three types in the cost of search parameter k, capturing different levels in the ability to generate offers. We now examine the role of varying both the number of types and the type of heterogeneity. In the first 5 columns, we vary the number of cost types from 2 types (Column (1)) all the way to 6 types (Column (5)). The results for the reference-dependent model are clear: there is a minor improvement in fit going from 2 to 3 types, but the additional improvements in allowing additional types are basically nil. Indeed, estimates of the reference-dependent model with more than 3 types have trouble converging and lead, when converged, to essentially the same fit as the benchmark specification. The result that the 2-cost type fits almost as well as the 3-type model is important because the 2-type reference-dependent model has the same number of parameters as the benchmark standard model, and thus allows for a clean comparison of the goodness of fit.

Instead, allowing for additional types in the standard model keeps improving the fit, but at a decreasing rate. Increasing the number of types from 2 to 3 lead to a large improvement of fit, and the gain from 4 types (Column (3) is sizable. There are then further gains to moving to 5 and 6 types, but the gains are smaller. Indeed, even the model with 6 types does worse in terms of fit than the reference-dependent model, despite having 14 compared to 8 parameters. In particular, the versions of the standard model with many types capture very well the behavior up to day 270, but do not do anywhere nearly as well in capturing the post-day 270 behavior.

Next, we consider alternative types of the unobserved heterogeneity. In Columns (6) and (7) we allow for heterogeneity in the reemployment wage with two different sets of assumptions. In both cases, we pin down the 3 types of reemployment wages using the data, and estimate the probabilities of the three types. In Column (6), we take the 5th, 50th, and 95th percentile in the reemployment wage in the data, while in Column (7), we take the 10th, 50th, and 90th percentile. Under both assumptions, we estimate also one cost parameter k and one curvature parameter γ. The results indicate that the reference-dependent model does about equally well under these specifications, while the standard model does significantly worse. The standard model cannot fit the behavior in the data if we take as given the heterogeneity in reemployment wages from the data. We explore this further in the later section on reservation wage choices.

Finally, in Column (8) we consider the implication of allowing for heterogeneity in the curvature parameter γ instead. This alternative form of heterogeneity improves the fit of the reference-dependent model significantly, while the fit of the standard model is worse than in the standard specification. We conclude that alternative specifications of the heterogeneity do not generally help by much the fit of the standard model, or hurt it significantly. Instead, the reference-dependent model fits quite similarly under these alternative assumptions. This should not be surprising given that we presented evidence that the reference-dependent model in fact fits quite well even without any heterogeneity.

Reservation wages. So far we take the reemployment wage as fixed for each individual so that the unemployed accept every job offer and have job search effort as a choice variable.[24] While this is consistent with a growing literature documenting a small role of reservation wages for job search dynamics (e.g. Card, Chetty, and Weber 2007, Schmieder, von Wachter, and Bender 2014, Krueger and Mueller 2013), traditionally job search has been modelled using both reservation wages and search effort thus allowing workers to reject jobs that do not offer a high enough wage. In order to test whether reservation wages would change our conclusions, we incorporate job acceptance decisions through a reservation wage into our model and reestimate this expanded model using additional moments based on individual reemployment wages. In this expanded model, individuals draw job offers from a (stationary) log-normal wage offer distribution and decide whether or not to accept it. Solving this model requires solving for an optimal reservation wage path and search effort path using backwards induction.

We set the standard deviation of the wage offer distribution at 0.5, close to the standard deviation of the actual reemployment wages. As an additional parameter we solve for the mean of the wage offer distribution. In order to estimate this model we also use the reemployment wage path, i.e. the average reemployment wage of people exiting in period t after entering the UI system, in the pre- and post periods as addional moments. Since we have 35 reemployment wage moments in the pre- and post period, this adds 70 additional moments for a total of 140, which are used in the minimum distance estimator. At this point we estimate the model using linear utility functions, with log-utility being work in progress.

Table 3.6, Column (1) shows the estimates for the standard model incorporating reservation wages, while Column (2) shows the corresponding results for the reference dependent model. The mean of the wage offer distribution is close - slightly lower - to the average reemployment

[24] In the baseline model there is a single reemployment wage for everyone. In the models in Table 5, columns (6) and (7) there are 3 different types of individuals who each face a different fixed reemployment wage.

wage in the sample. The reference dependent model now implies slightly lower, but still sizable loss aversion, with $\lambda = 1.1$ and a somewhat faster adjustment speed of around 174 days, compared to the benchmark model. While the goodness of fit statistics are not directly comparable to the previous models because of the additional moments, the RD model performs clearly better than the standard model.

Appendix Figure A-5 shows the empirical moments (hazard rates and reemployment wage path) for both models together with the corresponding simulated moments from the model estimates. While we use the reemployment wage paths, those moments are quite noisy (no doubt due to the sample being relatively small compared to the variance in wages). Thus most of the identification still comes from the hazard rates with a limited role for the reemployment wage path. Since we use a linear utility function the fit of the RD model is slightly worse than when we use a log utility function (see Table 3.4). Nevertheless, with reservation wages we still obtain the basic result that the RD model fits the empirical moments quite a bit better than the standard model and can capture the observed crossing in the hazard rates around the 270 day mark quite easily.

Alternative Samples. So far we focused on individuals with pre-unemployment income sufficiently high such that they qualify for the highest possible UI benefit levels before and after the reform, thus yielding the cleanest natural experiment. One strength of our setting however is that we can compare whether other demographic groups that experienced different rule changes also display the same behavior and whether we obtain similar parameter estimates if we estimate our model on this subgroup of the population. Figure 2 highlights two alternative pre-unemployment income samples that we use. These are individuals with 75,000 to 85,000 HUF and 85 to 114,000 HUF. While they were not affected by the cap on the first step UI benefits after the reform, they nevertheless experienced the introduction of a two step UI system.

Figure 3.10 shows the corresponding actual hazard plots (the moments) and the simulated hazards from the estimated standard and the RD models for the two groups. Since both groups had lower earnings prior to becoming unemployment, their UI benefits in the post-period over the first 90 days are lower than in our main sample while benefits between 90 and 270 days are unaffected. Thus there is a much smaller drop-off in benefits after 90 days, which is reflected in the absence of a clear spike in the post-period in Figure 3.10 (a) and (c) at 90 days. There is however still a clear difference in the size of the benefit drop at 270 days between the before and after period, both for the medium and the low earnings base group and we still see a much larger spike in the hazard rate for the before period at 270 days and then a smaller one at 360 days. In particular the hazard rates of the before and after period still cross just before the 270 days mark, as would be expected with reference dependent preferences if people search harder in anticipation of the large loss-utility occurred after the 270 day mark. The basic pattern is thus still in line with our main sample and consistent with reference dependent preferences.

Table 5 Columns (3) and (4), as well as Columns (6) and (7)show the results for estimating our baseline standard and RD model on these two alternative samples. For both samples the standard model again provides a substantially worse fit than the RD model (the goodness of fit being 165.8 vs 148.1 for the first sample and 110.3 vs. 94.4 for the second sample). It is also noteworthy that we obtain almost the same estimates for the adjustment time for the reference point of 270 and 277 days as in the benchmark model. Similarly the estimates suggest very similar values for λ, the gain-loss utility parameter. The fact that the estimates are so similar even though based on different samples and somewhat different natural experiment, is quite reassuring for our main estimates. Of course it is somewhat expected that the reference dependent model would be able to fit the data at least slightly better, since it allows for two additional free model parameters to be fitted thus offering more flexibility. In order to have a comparison of the reference dependent model with the same number of parameters as the standard model, we also estimated our model on the medium earnings base, however setting N and λ at the values that we obtain from our benchmark estimates (i.e. 255 and 1.73). Thus we estimate the same parameters as in the standard model (that is all the search cost parameters), while assuming the ame utility function across different samples. The estimates are shown in Columns (5) and (8). For both the medium and low earnings base samples, using the loss aversion parameter and adjustment speed parameter from our benchmark estimates, yield almost the same fit as when the parameters are allowed to differ across sample. For example in the medium earnings base sample, holding N and λ at the benchmark values decreases the goodness of fit from 148.1 to 149.7, which is still much better than for the standard model. The same holds for the low earnings base sample.[25]

3.6 Discussion and Conclusion

In the previous section, we provided evidence that a model with reference-dependent preferences can explain qualitative features of the hazards which a standard model has a hard time fitting. The model itself builds on one of the most robust behavioral deviations from the standard model, reference dependence, and uses a natural candidate for a backward-looking reference point.

An important implication of the results above is that they open the door to potential redesigns in unemployment insurance policies. In particular, the evidence suggests that it is possible to design simple quasi-revenue-neutral transitions to two-steps systems which speed exit out of unemployment. While more evidence is needed to fully assess such UI designs, they open the door for a qualitative redesign of unemployment systems which typically instead involve only a one-step decrease. We should be clear though that we have not presented a full welfare analysis of such plans, which is beyond the scope of this chapter.

Turning to some caveats, we want to stress an important limitation of the above analysis. The model at this point makes the stark assumption that individuals in each period consume their income. We make this assumption of hand-to-mouth consumers for computational reasons: incorporating a consumption-savings model with backward-looking reference-dependent preferences is computationally difficult, especially with slowly-updated reference points, as the

[25] Finally we also estimated our model using all three pre-unemployment income groups jointly. For this we used 70 moments from each of the three groups, thus 270 in total. We allowed for different cost parameters and shares of the different unobserved search cost groups, across the three income groups. This captures the fact that individuals in different income groups are likely different along other dimensions and face different job prospects. We do however restrict the parameters of the utility functin to be the same across the three groups, in particular the search cost elasticity parameter γ as well as the RD parameters λ and N. In this joint model, the RD model still provides a substantially better fit than the standard model. What is most striking is that even in this joint estimation we still obtain very similar estimates for the gain-loss parameter $\lambda = 2.04$ and for the adjustment period $N = 255$, suggesting that both estimates are quite robust to different samples and specifications.

evidence suggests. In the light of a consumption-savings model, one can interpret the current set-up as the approximate solution for an individual with high impatience and therefore no assets, since this individual would optimally choose to essentially consume hand-to-mouth. The high rates of discounting implied by the current estimates is not inconsistent with this scenario. In ongoing work we are exploring ways to incorporate endogenous saving decisions into the model using a backwards looking reference point that is only based on past income (and not consumption). Preliminary results indicate that this does not help the model fit of the standard model, but likely appears consistent with the reference dependent model with sufficiently low savings and discount rates.

Tables

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

	before	after	diff	t-stat
Percent Women	41%	46%	5.2%	5.75
	(0.006)	(0.006)		
Age in Years	36.8	36.9	0.06	0.47
	(0.1)	(0.1)		
Imputed Education (years)	12.83	13.00	0.17	4.20
based on occupation	(0.028)	(0.031)		
Log Earnings in 2002	11.55	11.52	-0.03	-3.56
	(0.006)	(0.006)		
Log Earnings in 2003	11.70	11.68	-0.03	-2.72
	(0.005)	(0.007)		
Log Earnings in 2004	11.79	11.78	-0.01	-1.37
	(0.007)	(0.007)		
Waiting period*	31.1	32.0	0.84	1.18
	(0.47)	(0.51)		
Reemployment bonus claimed	0.000	0.059	0.059	19.81
	(0)	(0.003)		
Participate in training	N.A.	0.042		
		(0.003)		
Inconsistent observations	0.024	0.022	0.022	-0.75
	(0.002)	(0.002)		
Number of observations**	6305	5562		

Notes:
Participation in training programs was not recorded prior to 2006.
* number of days between jobb loss and UI claim.
** for log earnings in 2002; 2003; 2004 there are some missing values.

Table 3.2: Structural Estimation of Standard and Reference Dependent Model

Models:						
		Benchmark I			**Benchmark II**	
	(1) Standard	(2) RD 3-type	(3) RD 1-type	(4) Standard	(5) RD 3-type	(6) RD 1-type
Parameters of Utility Function						
Utility function $v(.)$	log(b)	log(b)	log(b)	log(b)	log(b)	log(b)
Loss aversion λ		1.73	3.16		2.19	6.3
		(0.22)	(0.24)		(0.236)	(0.66)
Gain utility η		1	1		1	1
Adjustment speed of reference point N		255.0	200.9		237	199
in days		(34.4)	(14.1)		29.0	(18.4)
Discount factor (15 days) δ	0.99	0.99	0.99	0.99	0.99	0.99
Parameters of Search Cost Function						
Elasticity of search cost γ	0.11	0.06	0.12	0.32	0.080	0.28
	(0.02)	(0.01)	(0.02)	(0.14)	(0.023)	(0.05)
Search cost for high cost type k_{high}	235.5	227.0	246.4	137.2	101.0	155.3
	(10.6)	(4.3)	(5.6)	(29.0)	(41.7)	(45.3)
Search cost for medium cost type k_{med}	193.6	186.0		87.9	80.3	
	(4.2)	(11.7)		(12.7)	(33.5)	
Search cost for low cost type k_{low}	91.2	83.2		16.4	35.3	
	(5.0)	(18.7)		(8.5)	(20.6)	
Share of low cost UI claimant	0.458	0.09		0.40	0.11	
	(0.04)	(0.04)		(0.05)	(0.05)	
Share of medium cost UI claimant	0.538	0.37		0.60	0.7777	
	(0.04)	(0.16)		(0.05)	(0.0434)	
Non-labor/UI income				331.5	290.6	291.2
				(62.7)	110.4	(79.7)
Model Fit						
Number of moments used	70	70	70	70	70	70
Number of estimated parameters	6	8	4	7	9	5
Goodness of Fit	243.1	172.6	217.6	250.2	154.4	231.9
Heterogeneity in search-cost	yes	yes	no	yes	yes	no
Non-market income estimated	no	no	no	yes	yes	yes

Notes:
The table shows parameter estimates for the standard and the reference dependent search model. Estimation is based on minimum distance estimation, using the hazard rates in the pre- and post-reform periods as the moments. Standard errors for estimated parameters in parentheses.

Table 3.3: Alternative Specifications for Structural Estimation of Reference Dependent Model and Habit Formation Model

Models:	**Benchmark I 3-types**	**AR(1) Updating**	**No Gain Utility**	**No Loss Utility**	**Alternative Eta**		**Fix $\lambda = 1$ Estim. η**	**Habit Formation**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Parameters of Utility Function								
Utility function $v(.)$	log(b)	log(b)	log(b)	log(b)	log(b)	log(b)	log(b)	log(b-z*rf)
Loss aversion λ	1.73	1.94	1.2	0	6.58	0.84	3.03	
	(0.22)	(0.37)	(0.2)		(1.11)	(0.04)	(0.63)	
Gain utility η	1	1	0	2.77	0.2	5	1	
				(0.90)				
Adjustment speed of reference point N in days	255.0		240	15.0	240.0	300.0	270	120.0
	(34.4)		(26.3)	(7.5)	(27.09)	(44.72)	(38.8)	(38.76)
Habit formation parameter z								0.38
								(0.04)
AR(1) parameter		0.83						
		(0.03)						
Implied half life of AR(1) process		56.5						
Elasticity of search cost γ	0.06	0.06	0.07	0.14	0.07	0.08	0.07	0.12
	(0.01)	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)
Model Fit								
Number of moments used	70	70	70	70	70	70	70	70
Number of estimated parameters	8	8	8	8	8	8	8	8
Goodness of Fit	172.6	188.4	175.8	227.0	175.0	168.0	169.7	204.6
Heterogeneity in cost	yes	yes	yes	yes	yes	yes	yes	yes

Notes:
The table shows parameter estimates for the reference dependent search model and the habit formation model. Estimation is based on minimum distance estimation, using the hazard rates in the pre- and post-reform periods as the moments.
Standard errors for estimated parameters in parentheses.

Table 3.4: Estimating Standard and Reference Dependent Model under Alternative Specifications for Utility Function, Search Cost and Estimation Methods

Models:									
	Robustness on Utility Function					**Statistical Robustness**			
	Linear Utility	CRRA Utility	CRRA Utility	Time Varying Search Cost	Estimated δ	Weighting Matrix Identity	Moments with Controls	Probability moments	Estimation without Spikes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Standard Model									
Utility function $v(.)$	b	$\frac{b^{(1-\kappa)}}{(1-\kappa)}$	$\frac{b^{(1-\kappa)}}{(1-\kappa)}$	log(b)	log(b)	log(b)	log(b)	log(b)	log(b)
Util. function parameter κ		0.5	2						
Discount factor (15 days) δ	0.99	0.99	0.99	0.99	0.91	0.99	0.99	0.99	0.99
					(0.05)				
Elasticity of search cost γ	0.05	0.07	0.23	0.04	0.69	0.10	0.10	0.23	0.14
	(0.007)	(0.011))	(0.045)	(0.00)	(0.28)	(0.015)	(0.015)	(0.05)	(0.03)
Time varying search cost				0.0020					
Number of estimated parameters				(0.0002)					
Goodness of Fit	239.3	240.4	251.8	210.2	238.1	0.0034*	219.7*	254.1*	170.5**
Number of estimated parameters	6	6	6	7	7	6	6	6	
Reference Dependent Model									
Loss aversion λ	1.95	1.97	1.37	1.73	3.47	1.93	1.68	1.96	1.60
	(0.15)	(0.19)	(0.17)	0.50	(0.56)	(0.14)	(0.21)	(0.24)	(0.24)
Adjustment speed of reference	211.9	240.0	270.0	255	240.0	255.0	255.0	270	262
point N in days	(17.5)	(24.3)	(43.3)	38.9	(21.0)	(26.2)	(34.2)	(43.7)	(47.1)
Discount factor (15 days) δ	0.99	0.99	0.99	0.99	0.91	0.99	0.99	0.99	0.99
					(0.02)				
Elasticity of search cost γ	0.04	0.05	0.09	0.06	0.52	0.07	0.06	0.11	0.07
	(0.005)	(0.007)	(0.012)	0.01	(0.13)	(0.008)	(0.008)	(0.02)	(0.01)
Time varying search cost				0.0000					
Number of estimated parameters				(0.0011)					
Goodness of Fit	179.8	175.3	173.7	172.6	164.1	0.0027*	146.8*	216.2*	115.0**
Number of estimated parameters	8	8	8	9	9	8	8	8	8
Heterogeneity in cost	yes	yes	yes	yes	yes	yes	yes	yes	yes
Number of moments used	70	70	70	70	70	70	70	70	66

Notes:
The table shows parameter estimates for the standard and the reference dependent search model. Estimation is based on minimum distance estimation, using the hazard rates in the pre- and post-reform periods as the moments.
Standard errors for estimated parameters in parentheses.
* These are the SSE with the identity weighting matrix and alternative moments respectively and are not directly comparable to the goodness of fit statistics in the other columns. *∗ These SSE correspond to the reduced number of moments (that is not including the spikes). The comparable SSE from the standard model (that is also excluding the spike moments) are 176.5 and 122.0 respectively.

Table 3.5: Performance of Standard and Reference Dependent Model using Alternative Types of Heterogeneity

Models:	2 cost types	3 cost types	4 cost types	5 cost types	6 cost types	Heterogeneity Wages 95-50-05	Heterogeneity Wages 90-50-10	Heterogeneity seach cost elasticity γ
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Standard Model								
Elasticity of search cost γ	0.37	0.109	0.051	0.041	0.037	0.3604	0.3506	3-types
	(0.13)	(0.018)	(0.007)	(0.005)	(0.004)	(0.0291)	(0.1704)	
Goodness of Fit	334.3	243.1	201.5	194.2	188.1	315.4	338.4	271.0
Number of estimated parameters	4	6	8	10	12	4	4	6
Reference Dependent Model								
Loss aversion λ	2.13	1.73	*	*	*	2.12	2.01	1.93
	(0.23)	(0.22)				(0.110)	(0.109)	0.15
Adjustment speed of reference point N in days	247.4	255.0	*	*	*	251.8	253.2	91.6
	(27.5)	(34.4)				(19.3)	(21.1)	6.4
Elasticity of search cost γ	0.08	0.06	*	*	*	0.6844	0.5962	3-types
	(0.01)	(0.01)				(0.0008)	(0.0008)	
Goodness of Fit	178.3	172.6	*	*	*	172.9	178.5	152.2
Number of estimated parameters	6	8	10	12	14	6	6	8
Number of types	2	3	4	5	6	3	3	3
High wage						1150	869	
Medium wage						423	423	
Low wage						188	234	
Number of moments used	70	70	70	70	70	70	70	70

Notes:
The table shows parameter estimates for the standard and the reference dependent search model. Estimation is based on minimum distance estimation, using the hazard rates in the pre- and post-reform periods as the moments.
Standard errors for estimated parameters in parentheses.
* The reference dependent model does not converge with more than 3 types, indicating that additional types are not identified and do not improve the fit.

Table 3.6: Estimation of Standard and Reference Dependent Model

Samples:		
	Reservation Wage	
	Standard Model	Ref. Dep. Model
	(1)	(2)
Parameters of Utility Function		
Utility function	linear	linear
Loss aversion λ		1.11
		(0.16)
Adjustment speed of reference point N in days		174
		(17.9)
Discount factor (15 days) δ	0.99	0.99
Elasticity of search cost γ	0.10	0.10
	(0.03)	(0.04)
Mean of wage offer distribution (in log)	5.96	5.95
	(0.02)	(0.02)
Standard deviation of wage offer distribution	0.5	0.5
Model Fit		
Number of Moments	70	70
Number of estimated parameters	7	9
Goodness of Fit	404.9	353.6
Heterogeneity in cost	yes	yes

Notes:
The table shows parameter estimates for the standard and the reference dependent search model. Estimation is based on minimum distance estimation, using the hazard rates in the pre- and post-reform periods as the moments.
Standard errors for estimated parameters in parentheses.

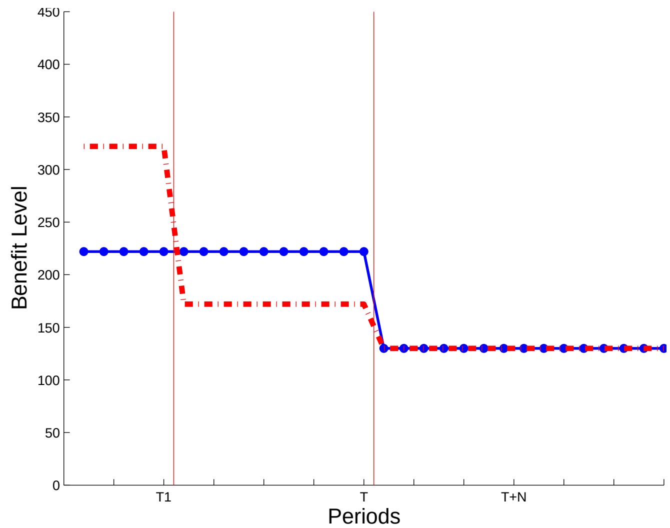
Table 3.7: Performance of RD and Standard Model on Alternative Samples

Samples:								
	Benchmark Sample		Pre-UI Income Medium Earnings Base			Pre-UI Income Low Earnings Base		
	Standard Model	Ref. Dep. Model	Standard Model	Ref. Dep. Model	Ref. Dep. Model	Standard Model	Ref. Dep. Model	Ref. Dep. Model
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Parameters of Utility Function								
Utility function	log(b)	log(b)	log(b)	log(b)	log(b)	log(b)	log(b)	log(b)
Loss aversion λ		1.73		1.97	1.73		1.66	1.73
		(0.216)		(0.364)			(0.353)	
Gain utility η		1		1	1		1	1
Adjustment speed of reference point N in days		255		270	255		277	255
		(34.4)		(62.3)			(77.2)	
Elasticity of search cost γ	0.11	0.06	0.18	0.10	0.12	0.07	0.07	0.07
	(0.018)	(0.009)	(0.043)	(0.024)	(0.022)	(0.013)	(0.016)	(0.010)
Model Fit								
Number of Moments	70	70	70	70	70	70	70	70
Number of estimated parameters	6	8	6	8	6	6	8	6
Goodness of Fit	243.1	172.6	165.8	148.1	149.7	110.3	94.4	94.8
Heterogeneity in cost	yes	yes	yes	yes	yes	yes	yes	yes

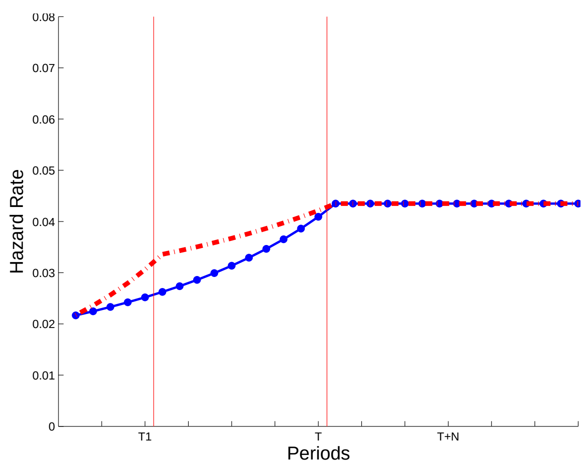
Notes:
The table shows parameter estimates for the standard and the reference dependent search model. Estimation is based on minimum distance estimation, using the hazard rates in the pre- and post-reform periods as the moments.
Standard errors for estimated parameters in parentheses.

Figures

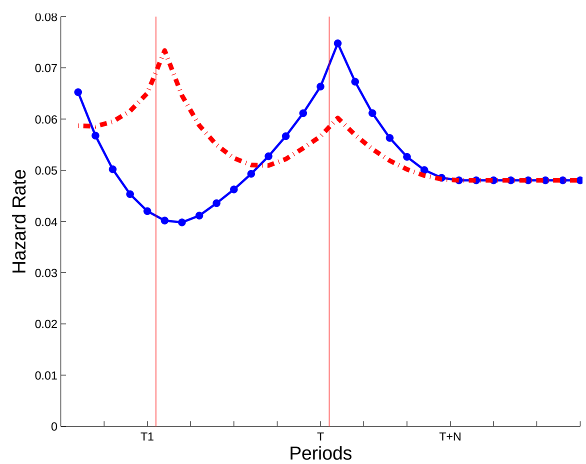
Figure 3.1: Model Simulations of the Standard and the Reference-Dependent model



(a) Benefits



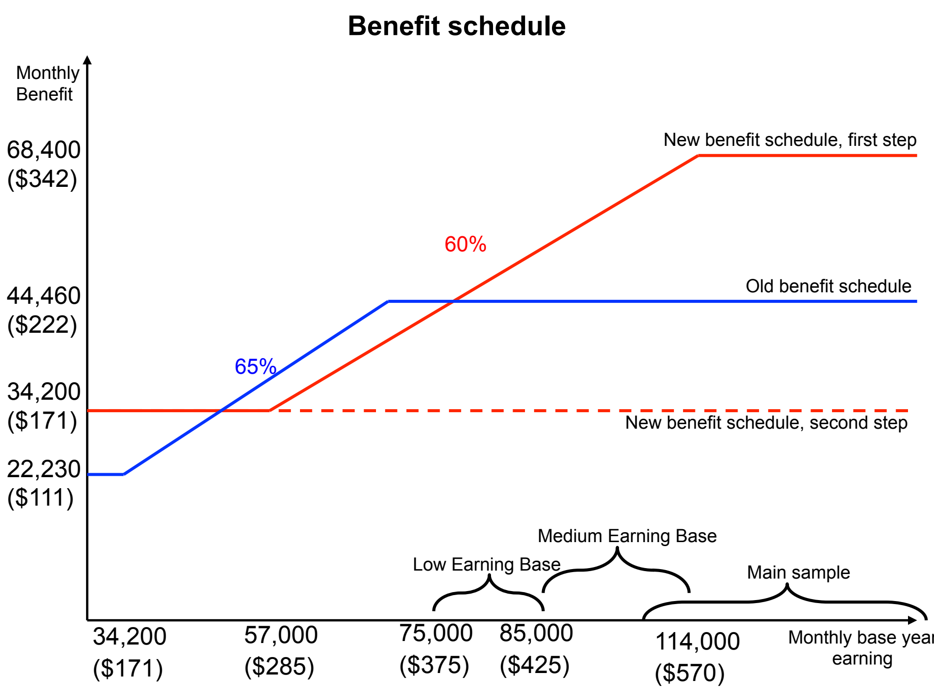
(b) Standard Model



(c) Reference-Dependent Model

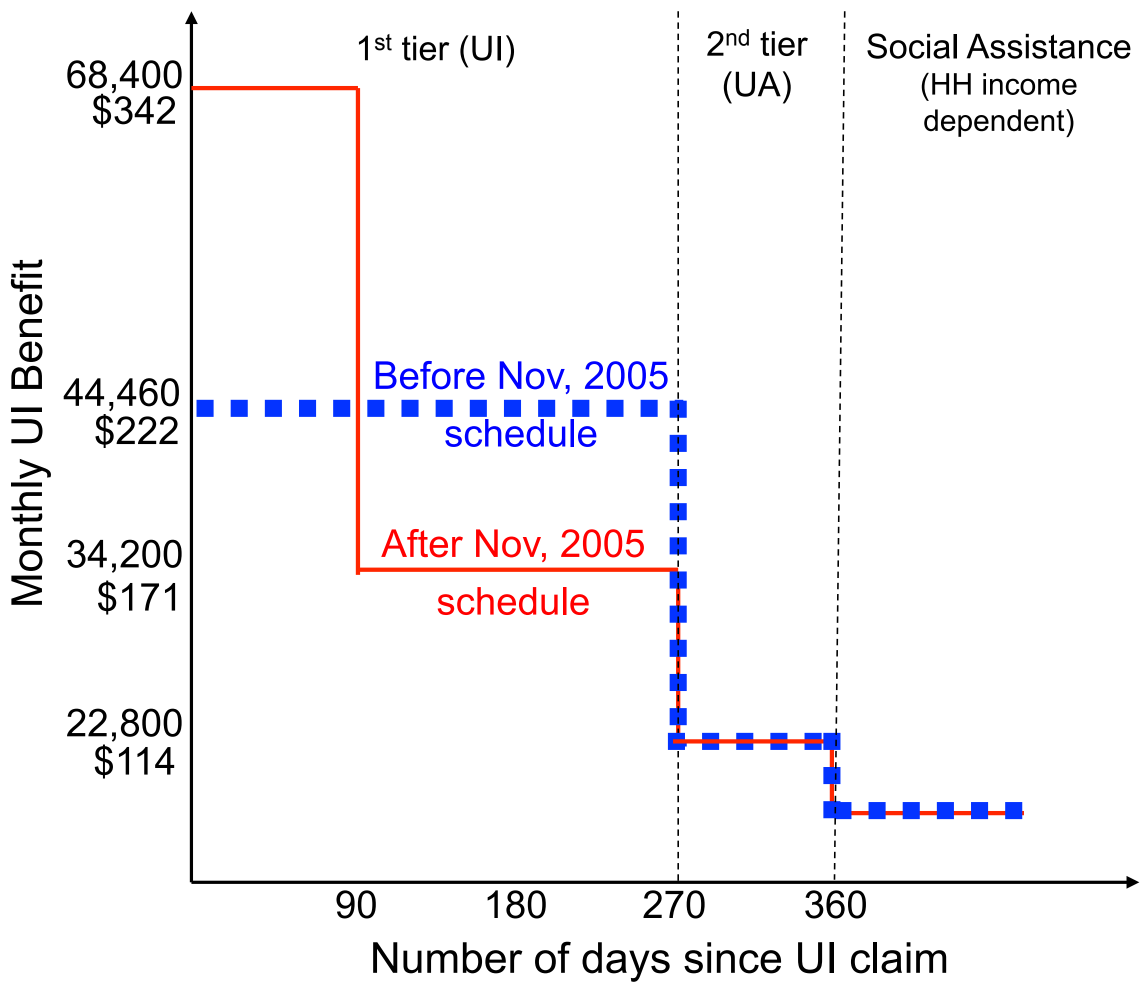
Notes: Panel (a) shows two benefit regimes, both of them having a step-down benefit system. In the first step benefits are higher in the regime represented by squared blue line than in the regime represented by red solid line. In the second step benefits drops to the same level. Panel (b) shows the hazard rates predicted by the standard model (with $k = 130$, $\gamma = 0.6$, $w = 555$, $\delta = 0.99$) while Panel (c) the prediction of the reference-dependent model (with $k = 160$, $\gamma = 0.6$, $w = 555$, $\delta = 0.99$, $\lambda = 2$, $N = 10$).

Figure 3.2: The UI Benefit Schedule Before and After the 2005 Reform in Hungary



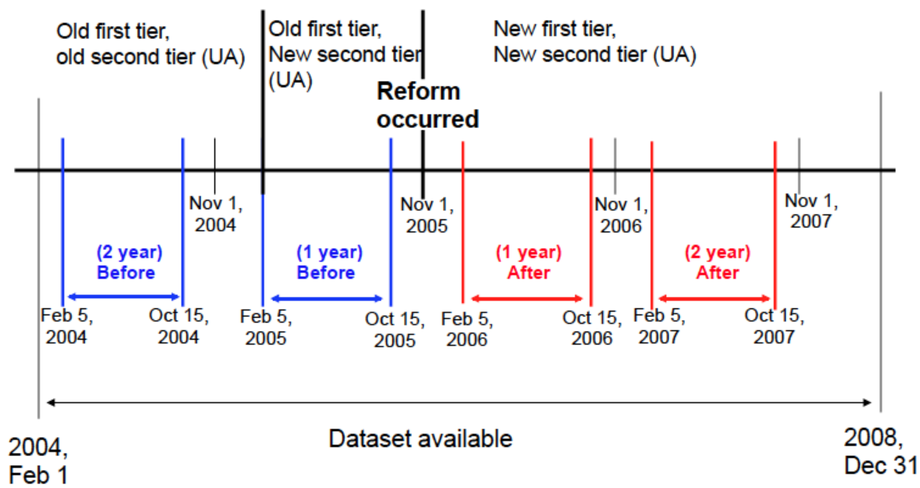
Notes: The figure shows monthly UI benefits in the first tier under the old rule (blue solid line) in the first 90 days under the new rules (red solid line) and between 91-270 days under the new rules (red dashed line) as a function of the monthly base salary. The main sample, defined by being above the 70th percentile of the earnings base distribution of the UI claimants in the given year, denoted by the curly brackets. We also show the sample definitions used for our out of sample analysis (results presented in Table 5): medium earnings base sample is defined by being between the 60th and 78th percentile of the earnings base distribution of the UI claimants in the given year, low earnings base sample is defined by being between the 60th and 78th percentile of the earnings base distribution of the UI claimants in the given year.

Figure 3.3: Benefit Path Change, Main Sample



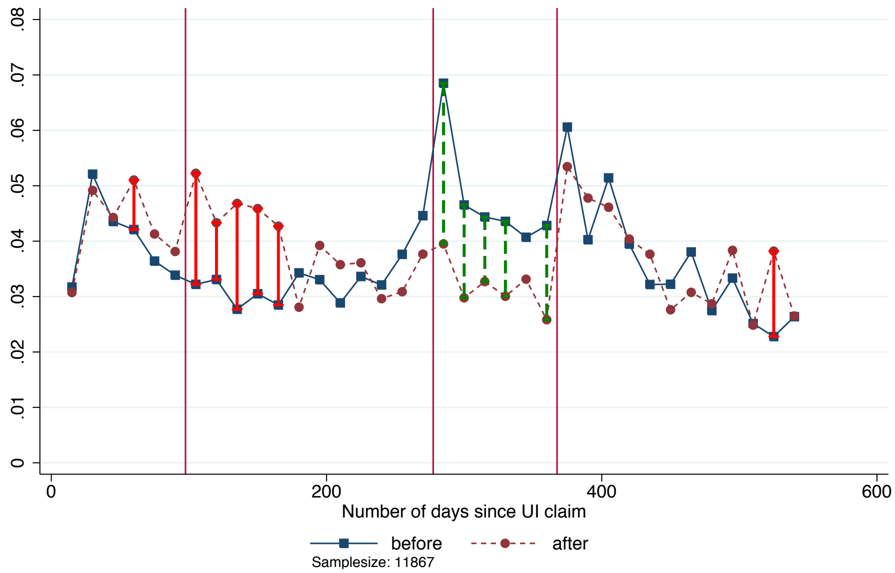
Notes: The figure shows the benefit schedule if UI is claimed on October 31, 2005 (old benefit schedule, dashed blue line) and benefit schedule if UI is claimed on November 1st, 2005 (new benefit schedule, solid red line) for individuals who had 270 potential duration in the first-tier, were less than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

Figure 3.4: Before-After Comparison Groups for Quasi-experiment

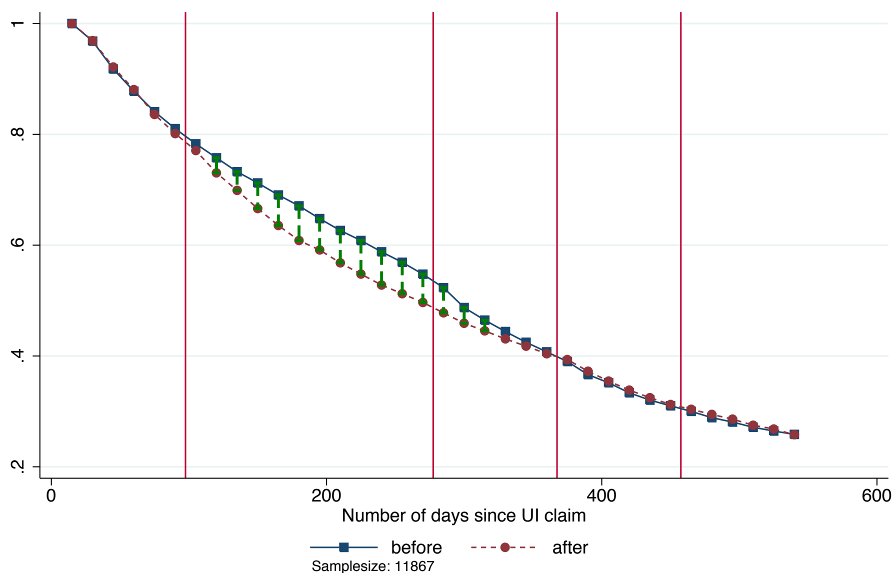


Notes: The figure shows the time frame for which we have access to administrative data on unemployment insurance records, the time of the reform and how we define the before and after periods that we use for our before-after comparison.The timing of the reform was the following: those who claimed UI benefit before February 5th, 2005 faced with the old first tier schedule and old second tier schedule; those who claimed benefit between February 5th, 2005 and October 31th, 2005 faced with the old benefit schedule in the first tier and the new benefit schedule in the second tier; those who claimed benefit after November 1st, 2005 faced with the new benefit schedule in the first tier and the new benefit schedule in the second tier. To avoid complications caused by changes in the second tier, in our main specifications we focus on the (1 year) before sample, claimed UI between February 5th, 2005 and October 15th, 2005, and (1 year) after sample, claimed UI between February 5th, 2006 and October 15th, 2006. We use the (2 year) before sample and the (2 year) after sample to show that the changes in the hazard rates are in line with the timing of the reform. The first tier changes before and after October 31th, 2005 are presented in Figure 2 and Figure 3. The changes in the second tier in February 5th, 2005 were the following: potential duration shortened to 180 days above age 50 and to 90 days below that. Before, it was 270 days above age 45 and 180 days below that. The benefit level was raised slightly from 21,000 HUF ($101) to 22,800 HUF ($114).

Figure 3.5: Empirical Hazard and Survival Rates under the Old and the New Benefit Schedule



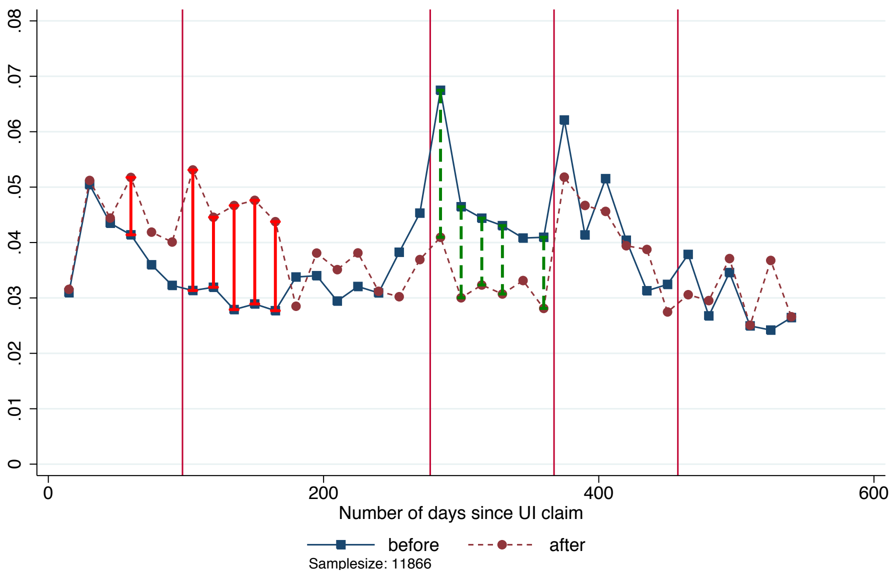
(a) Empirical hazard rates



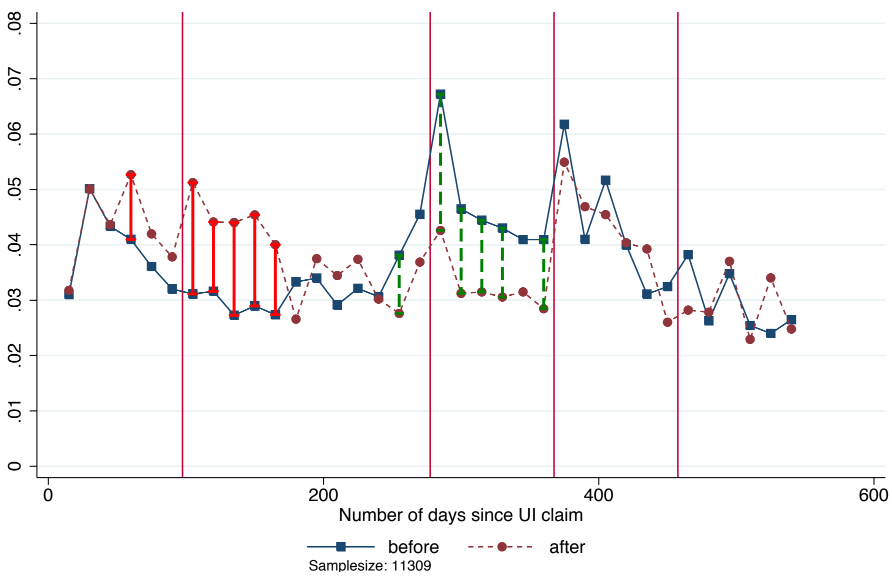
(b) Empirical survival rates

Notes: The figure shows point wise estimates for the empirical hazards, Panel (a), and for the empirical survival rates, Panel (b), before and after the reform. The differences between the two periods are estimated point-wise at each point of support and differences which are statistically significant are indicated with a vertical bar. The three major (red) vertical lines indicate periods when benefits change in the new system. The sample consists of unemployed workers claiming UI between February 5th, 2005 and October 15th, 2005 (before sample) and February 5th, 2006 and October 15th, 2006 (after sample), who had 270 days of potential duration, were 25-49 years old, and were above the 70th percentile of the earnings base distribution of the UI claimants in the given year (See Figure 4 for details).

Figure 3.6: Robustness Checks for change of Hazard rates before and after the reform



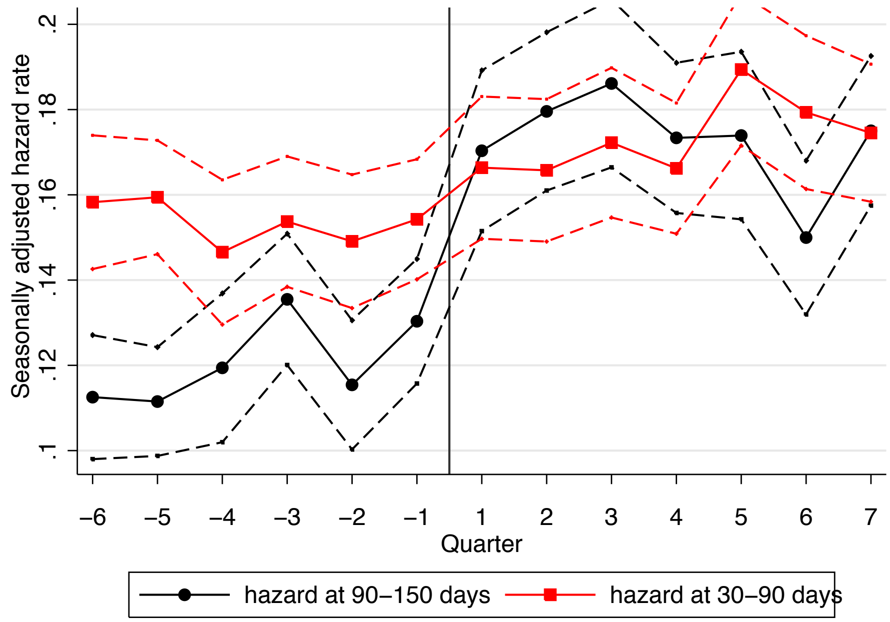
(a) Controlling for observable differences



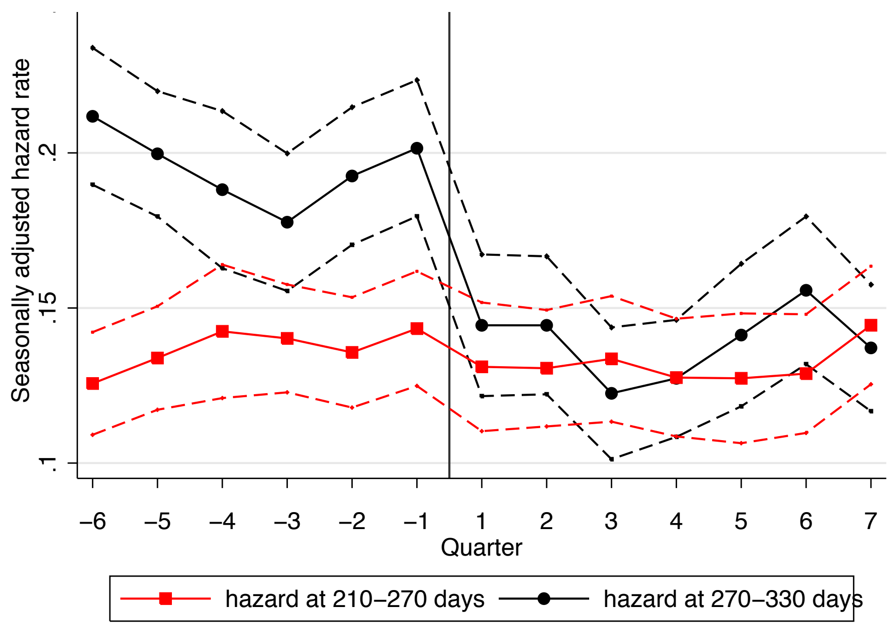
(b) Restricted sample

Notes: The figure shows point wise estimates for the empirical hazards before and after the reform. The differences between the two periods are estimated point-wise at each point of support and differences which are statistically significant are indicated with a vertical bar. The three major (red) vertical lines indicate periods when benefits change in the new system. In Panel (a) we controlled 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 claimed, education, occupation (1 digit) of the last job, log earnings in 2002 and 2003. In Panel (b) in addition to controlling for these control variables we dropped reemployment bonus claimants and those participating in training program (after the reform), see text for the details. The sample is otherwise the same as in Figure 5.

Figure 3.7: Interrupted Time Series Analysis of Exit Hazards



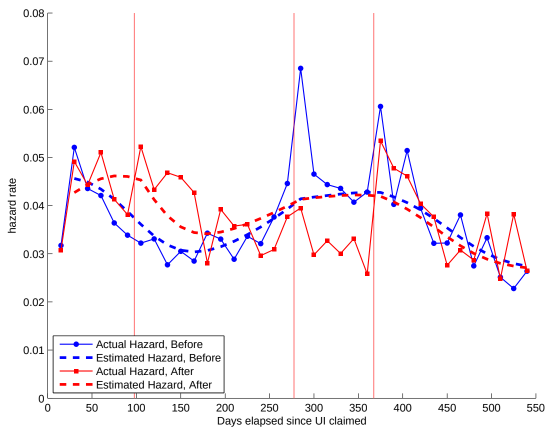
(a) The evolution of the hazard rates between 30 and 150 days



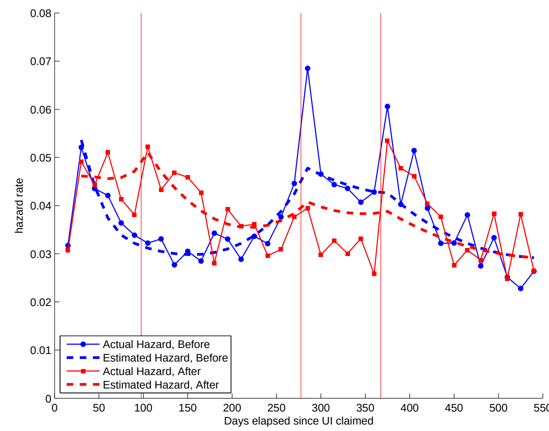
(b) The evolution of the hazard rates between 210 and 330 days

Notes: The figure shows the level of the most important hazard rates 6 quarters before and 7 quarters after the reform. Panel (a) shows the seasonally adjusted hazard rates between 30 and 150 days, while Panel (b) shows the seasonally adjusted hazard rates between 210 and 330 days. The monthly seasonal adjustment of hazard rates takes into consideration the level shift present in the data in November, 2005. The figures highlight that the shift in the hazard plots documented earlier corresponds to the precise timing of the reform. Other sample restrictions are the same as in Figure 5.

Figure 3.8: Structural Estimation of the Standard and the Reference-dependent model

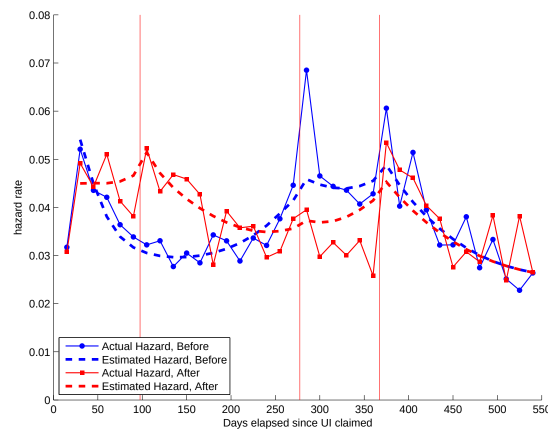


(a) Benchmark I: Standard Model



(b) Benchmark I: Reference Dependent Model

(c) Benchmark II: Standard Model

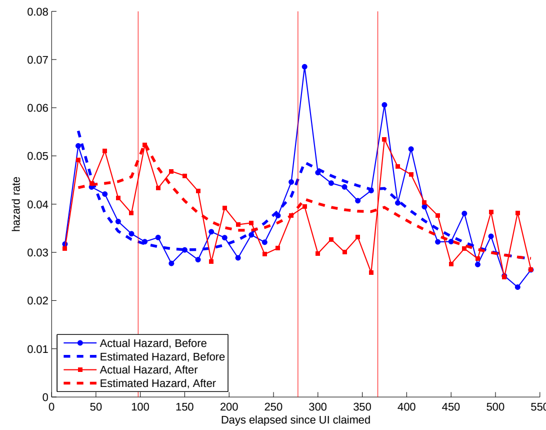


(d) Benchmark II: Reference Dependent Model

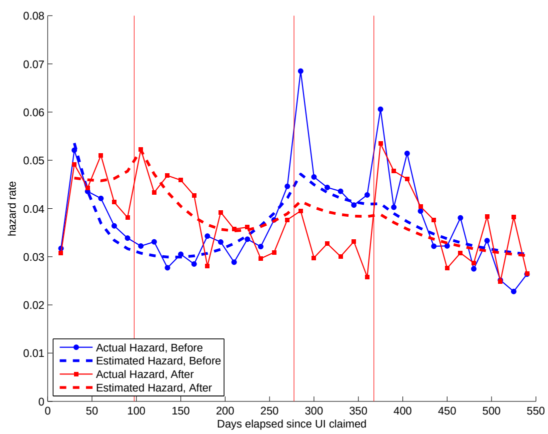
Notes: The figure shows the empirical hazards and the predicted hazards of the standard model, Panel (a) and (c), and of the reference-dependent model, Panel (b) and (d), with three cost types. Panel (a) and (b) contrast the Standard and RD model in the Benchmark I case (column (1) and column (2) in Table 2, respectively), while Panel (c) and (d) are based on the Benchmark II case (column (4) and (5) in Table 2). The three major (red) vertical lines indicate periods when benefits change in the new system.

Figure 3.9: Alternative estimates of the reference-dependent model

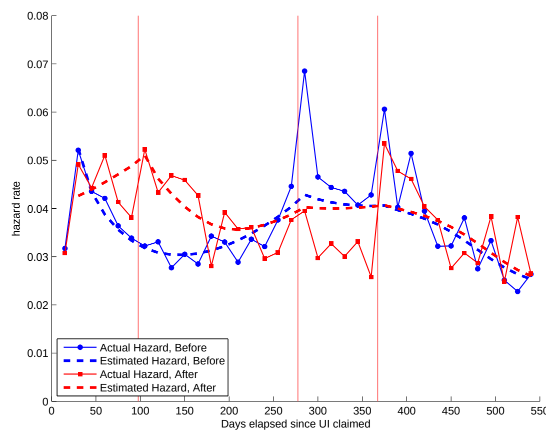
(a) Reference-Dependent Model, no heterogeneity



(b) Reference-Dependent Model, eta=5



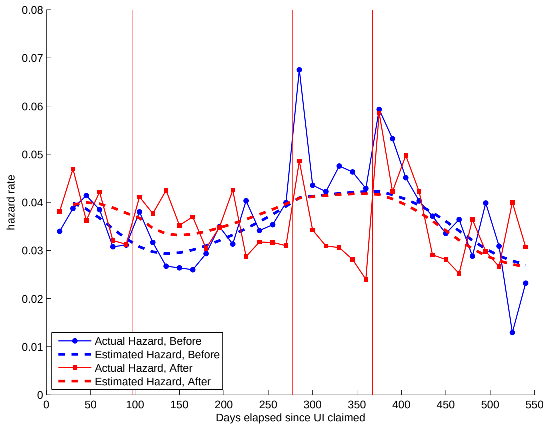
(c) Reference-Dependent Model, AR(1) update



(d) Habit Formation Model

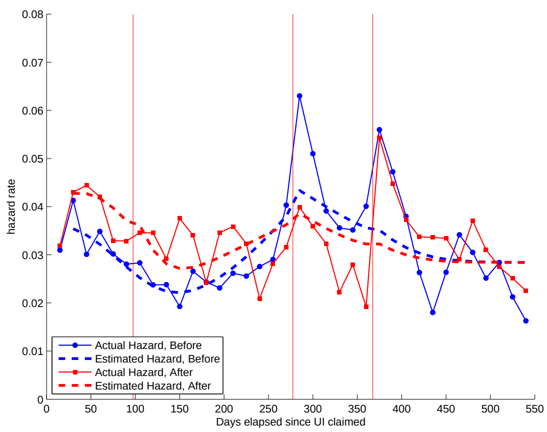
Notes: The figure shows the empirical hazards and the predicted hazards of the alternative versions of the structural estimations. Panel (a) shows the reference-dependent model with no heterogeneity in search cost (column (3) in Table 2). Panel (b), (c) and (d) present estimates with three cost types. Panel (b) shows the reference-dependent model with eta=5 (column (3) in Table 3) and Panel (c) presents the reference-dependent model with AR(1) updating of the reference point (column (4) in Table 3). Panel (d) shows the predictions of the habit formation model (column (8) in Table 3). The three major (red) vertical lines indicate periods when benefits change in the new system.

Figure 3.10: Structural Estimation of the Standard and the Reference-dependent model for groups with alternative earnings base

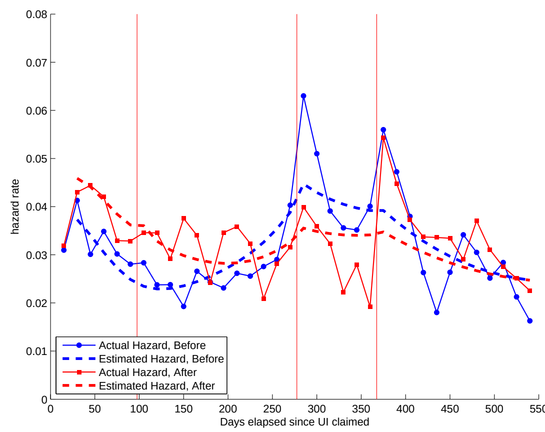


(a) Standard model, Medium Earnings Base

(b) Reference-Dependent Model, Medium Earnings Base



(c) Standard Model, Low Earnings Base



(d) Reference-Dependent Mode, Low Earnings Base

Notes: The figure shows the empirical hazards and the predicted hazards of the UI claimant with alternative earnings base. Panel (a) and Panel (b) present estimates for those whose earnings base were between the 60th and the 78th percentile of the earnings base distribution of the UI claimants in the given year. Panel (a) shows the fit of the standard model (column (3), Table 7) and Panel (b) for the reference-dependent model (column (5), Table 7). Panel (c) and Panel (d) present the results for those whose earnings base were between the 49th and the 60th percentile of the earnings base distribution of the UI claimants in the given year. Panel (a) shows estimates for the standard model (column (5), Table 7) and Panel (b) illustrates the estimates for the reference-dependent model (column (6), Table). All panels present estimations with three cost types. The three major (red) vertical lines indicate periods when benefits change in the new system.

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