Who Pays for the Minimum Wage?*

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Abstract

This paper analyzes the effects of a large (~60%) and persistent increase in the minimum wage instituted in Hungary in 2001. 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. Our estimates imply that the higher minimum wage had a small negative effect on employment, and so the primary effect was pushing up wages. We then use data on a large panel of firms to evaluate the economic incidence of the minimum wage increase. We show that firms highly exposed to the minimum wage experienced a substantial increase in their total labor cost. We also find that firms' profits are not affected, while their sales increased in response to the minimum wage. Exploiting a unique dataset on producer prices we also show that firms in the manufacturing sector responded to the minimum wage by raising output prices. This evidence indicates that firms passed through the effect of the minimum wage to consumers.

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1 Introduction

Despite several decades of microeconometric evidence, the minimum wage remains a highly controversial policy. On the one hand, opponents argue that minimum wage makes low-skilled workers worse off as many of them lose their jobs (e.g., Stigler, 1946; Neumark and Wascher, 2010). On the other hand, proponents insist that an increase in the minimum wage does not just raise wages but it also has a positive effect on the employment of low-wage workers (e.g., Card and Krueger, 1995; Dube et al., 2010). While a large negative or a clear positive effect on employment would simplify the welfare assessment of minimum wage policies, the existing empirical literature has not find a strong support for any of these stark cases. Instead, recent meta-analysis of minimum wage studies concluded that the effect of the minimum wage is likely to be small negative, many times cannot be distinguished from zero (CBO,).

In the presence of small employment effects, the welfare implication of the minimum wage also depends on its effect on other outcomes. If minimum wage have a large effect on wages even a small employment effect might be tolerable. Moreover, the redistributional consequences of the minimum wage also depend on the profit, the revenue and the price responses.

In this paper, we present new evidence on the effect of a the minimum wage on these outcomes by exploiting a very large and persistent increase in the minimum wage from Hungary. Figure 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. 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]

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. In the first part of the paper we focus on the employment and the wage effects of the minimum wage. Beyond implementing the standard group-level regressions used to evaluate minimum wage shocks in the U.S. context (e.g. Card, 1992b), and the firm-level regressions used by Machin et al. (2003) in the UK, we also propose a new approach that relies on the cross-sectional wage distribution.

Figure 2 summarizes the key idea of the underlying approach. Following an increase in the

[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. Moreover, Kertesi and Köllő (2004) studied the employment effects of the 2001 raise in the minimum wage in Hungary. Although they use different methods and datasets, many of their estimates are close to ours.

minimum wage, jobs previously earning less than the new minimum will either be destroyed or will increase into compliance by generating an excess mass (i.e., "bunching") at or just above the minimum.[2] In practice, firms may sometimes shift pay at affected jobs above the minimum by upgrading the task content of the position, but they are unlikely to shift such jobs to the very top of the wage distribution. Hence, the amount of "bunching" in the wage distribution at and slightly above the minimum wage is a nonparametric indicator that jobs are being preserved.[3]

Figure 2 also highlights that the effect of the minimum wage can also be calculated by estimating the change in non-employment (dashed red rectangle). What is the advantage of using the bunching estimator proposed here then? We show that shocks affecting only the upper tail of the wage distribution or aggregate shocks can cause a substantial bias in changes of the non-employment, while this bias is much lower for the bunching estimator.

To compare our estimates to the standard group-level regressions we also implement a grouped version of the bunching estimator which exploits differences in the fraction of workers who had been earning less than the new minimum wage across demographic groups and regions. We show that the grouping version of the bunching estimator alleviates the biased caused by non-zero correlation between employment growth and the fraction of affected workers. Moreover, because shocks affecting the upper tail of the distribution do not affect the bunching estimator, the effect of minimum wage is more precisely estimated.

Our various estimations techniques and data sources point to similar employment effects. For instance, estimates comparing highly exposed and less exposed firms' responses to the minimum wage indicate that firms fired 8.7% (s.e. 5.8%) of the directly affected workers, while our bunching estimate, indicate the minimum wage led to laying off 6% (s.e. 3%) of the workers directly affected by the minimum wage change. These estimates translates to a -0.21 (s.e. 0.14) employment elasticity with respect to the minimum wage, and to a -.16 (s.e. 0.08) employment elasticity with respect to 1% increase in wages. These estimates, therefore, indicate that the main effect of the minimum wage was pushing up wages.

The large effect on wages and the small effect on employment indicates that total earnings paid out to the low-wage workers increased substantially. In the second part of the paper, we scrutinize the incidence of the minimum wage by looking at the effect on firm-level outcomes that are rarely examined in the minimum wage literature.[4]

[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 reasoning is analogous to Saez (2010)'s argument that bunching of taxable earnings in the neighborhood of tax kinks is a sign of behavioral responses to tax incentives – here, the bunching indicates that firms are adjusting to the minimum wage by raising wages instead of shedding workers.

[4]Given that the worker-level (e.g. bunching estimator) and the firm-level estimates are very similar on employment, the incidence of minimum wage measured from firm-level regressions are likely to provide an

Using the corporate income tax data from Hungary we show that 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 increase their total labor cost by 20% more. We also 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 by raising prices. Moreover, by exploiting a unique dataset on producer prices in the manufacturing sector we document that firms exposed to the minimum wage, indeed, raise their prices more than the non-minimum wage ones. Our estimates imply that a 1% increase in wages led to a 0.18% (s.e. 0.07) increase in prices in the manufacturing sector.

Passing through the effect of the minimum wage to consumers might be easier in some markets then others. We show that in markets, where a small increase in output price leads to a large loss in market level output (e.g. manufacturing and exporting sectors), firms pass-through the effect of the minimum wage to a lesser extent and the employment losses are larger. On the other hand, in markets where output demand is likely to be more inelastic (e.g. service sector and non-exporting sector), it is easier for firms to pass-through the effect of the minimum wage and so they suffer smaller employment losses.

These results are consistent with a standard neoclassical model with competitive firms and three inputs (capital, labor, intermediate goods). 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 1.08 (s.e. 0.31), which is in the range of previous estimates in the literature going 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 0.24 (s.e. 0.38). 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). This latter is found to be fairly low in some contexts. For instance, Seale et al. (2003) estimated that is between 0.03 and 0.2 for food consumption .

Our results 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). Many papers in this literature find close to zero effects of the minimum wage (Doucouliagos and Stanley, 2009, CBO). However, these papers are 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 a 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.

accurate picture on the general incidence of the minimum wage.

Moreover, our empirical method relates to previous attempts that used 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 before and after the minimum wage increase to provide a more credible counterfactual 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 paper also contributes to the literature on estimating the effect of minimum wage changes on 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 the large and permanent increase in the minimum wage forces firms to re-optimize quickly and so we are more likely to capture long-term responses here.

Our paper is also related to handful of papers that examine the effect of the minimum wage on prices (see Lemos, 2008. These estimates often rely on strong structural assumptions and provide imprecise estimates (Aaronson and Eric 2007, Aaronson, French and MacDonald 2008). Therefore, while the estimates in the literature are indicative of the sign of the price changes, the extent of price pass-through has not yet been examined.

Finally, this paper is also related to the literature that looks at the incidence of the minimum wage. MaCurdy (2015)(MaCurdy 2014) examines the incidence of the minimum wage under the assumption that there is no employment effect and the wage cost increase is fully passed through to the consumers. Our estimates confirm the validity of these assumptions as we found that the employment effects are relatively small, and a large part of the minimum wage is passed-through to the consumers.

The papers proceeds as follows. In Section 2, we describe the institutional context of the minimum wage raise in Hungary. In Section 3 we explain our empirical strategy, and the details of the bunching estimator. In Section 4, we present the results on the employment effects of the minimum wage. In Section 6 we present the results on sales, profits and prices. In Section 7 we discuss the implications of our results to a simple competitive model. In Section 8 we conclude.

2 Institutional Context and Data

2.1 Institutional Context

The minimum wage in Hungary is negotiated annually by a national-level tripartite council — a consultative body that consists of unions, employer's associations and the government.[5] Upon failing to reach conclusions, the government is authorized to decide unilaterally, an authority which they invoked every year between 1998 and 2002.

Before 2000 the norm was raising the minimum wage a few percent above the inflation rate. However, on April 6th, 2000 the right-wing government announced that they would raise the minimum wage from 25,500 HUF to 40,000 HUF on January 2001 and also pledged to increase further the minimum wage in 2002 to 50,000 HUF. This announcement was rather unexepected. None of the major political parties in the previous election, in 1998, was campaigning for radical change in the level of the minimum wage. The purpose of the minimum wage change according to the government officials was to alleviate income differences, to raise government revenue and to diminish tax evasion (Cserpes and Papp 2008). Political commentators, on the other hand, argued that the real purpose was to boost re-election chances in the 2002 parliamentary elections. Figure 1 summarizes the evolution of the minimum wage in relation to the median wage in the private sector between 1996 and 2008. In the Online Appendix we show the minimum wage deflated by nominal GDP growth, or inflation, and we also describe the timing of the reform in more details.

The economy around the minimum wage hike was stable. The evolution of real GDP growth, which was 4% around 2000. 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 inference from aggregate data about the effect of the minimum wage increase. Therefore, in our analysis, we use disaggregated data (e.g. firm-level or group-level regressions) to cleanly identify the effects of the minimum wage increase, net of aggregate trends.

Changes in the policy environment can potentially conteminate our results. While our reading of the evidence is that this is not the case, in the Online Appendix, we list all relevant policy changes and discuss their effects on our results. These policy changes are the following: the expansion of higher education from 1996, the 1999 pension reform, a small one-time subsidy to firms highly exposed to the minimum wage in 2002, exemption of the minimum wage from personal income taxes, and the 50% increase in public sector base wages in 2002. Finally, through-out the paper we assume that estimated effects presented in the paper are real responses. However, in the presence of tax evasion, some of the estimated effects might only affect reporting behavior (Elek, Köllő, Reizer and Szabó 2011). In the Online Appendix we present various robustness checks suggesting that our estimates are unlikely to be driven by changes in reporting behavior.[6]

[5] 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.

[6] These robustness checks include using self-reported survey data, using only occupations and industries that are less prone to tax evasion, and estimating the effects seprataley for large and small firms.

2.2 Data

We use four main data sources in the paper. While in the Online Appendix we provide a detailed explanation on these data sources and define the key variables used in the analysis, here we only briefly describe the data.

The Labor Force Survey (LFS) is a large household sample survey providing quarterly results on self-reported employment status. While the sample covers all workers (e.g. self-employed and worker's at small firms), there is no wage information in the survey.[7] To relate employment status to minimimum wage exposure, therefore, we need to rely on other data sources.

The Structure of Earnings Survey (SES) is a large yearly enterprise survey providing detailed information on worker-level wages, job characteristics, and demographic characteristics. The key advantage of the data is that it can be used to calculate both employment and wages. However, the sample covers only firms with at least 5 workers,[8] so responses to the minimum wage at microenterprises cannot be assessed using the data. The sample design of the SES is the following. 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. All larger firms, employing more than 20 workers, are supposed to report data for the SES. [9] Firms responding to the survey report information on a roughly a random sample of their workers as of May 31st based on workers' date of birth. The sampling is designed to over-sample white-collar workers.[10] Due to the SES's complex sampling design observations are weighted.[11]

The Corporate Income Tax Data (CIT) contains information on firm's balance-sheet and income statements and so it allows us to assess firms' incom and cost structure, wages and personnel costs and material expenses. One key advantage of the Corporate Income Tax Data is that it contains information on the universe of firms with double book-keeping. However, the CIT does not contain information on worker-level wages. Therefore, we use the SES to calculate the firm-level exposure to the minimum wage. We calculate FA_i^{02}, fraction of workers below the 2002 minimum wage, for each firm in which we observe at least 5 workers in the SES.

[7] In the 2nd quarter of 2001 the LFS was supplemented with a questionnaire on wages. In the Online Appendix we show that wage distribution in the labor force survey is very similar to the wage distribution in the Structure of Earnings Survey.

[8] Before 2000, it only covers firms with at least 10 employees.

[9] In spite of obligatory reporting, some companies do not respond to the survey. The statistical office reports that non-response rate is around 90% for larger firms, and 50% per cent for the smallest companies. 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). In the Online Appendix we show that non-response behavior is not related to the employment change at the firms, indicating that there is no selective bias.

[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 used for selecting.

[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.

If we observe a firm in more than one year before 2000, we take the most recent FA_i^{02} measure. In the firm-level analysis we only focus on the manufacturing, the service and the construction sector. We omit sectors that are heavily regulated (e.g. energy), sectors where balance sheet items are hard to interpret (finance) and sectors with excise tax (oil and tobacco). In the final sample we have 5696 firms, 7% of the annual firm population, representing 745 thousand workers, 53% . To make our sample representative we weight our regression results.[12]

Our price data comes from Annual Survey of Industrial Production (ASIP). The ASPI is yearly firm-level survey of manufacturing firms and contain product-level information on the total volume and value of production. We calculate firm-level price changes relative to the previous year using a Laspeyres price index. The base-weights are the revenue share of the product from previous (base) years.[13]

3 Empirical Strategy

3.1 A Distributional Based Approach

We propose a novel approach to estimate the employment effects of the minimum wage that relies on the earning distribution. A binding minimum wage directly affects jobs that would otherwise earn sub-minimum wages. Such jobs can either be destroyed or shifted into compliance with the new minimum wage. Hence the spike at the earnings distribution is a nonparametric indicator that jobs are being preserved. In practice, firms sometimes shift pay at affected jobs above the minimum wage by upgrading the task content of the position.[14] Moreover, firms might also increase wages in jobs that are not directly affected by the minimum wage to preserve the wage structure within firm.[15] As a result the spike at the minimum wage will decrease, but an excess mass above the earnings distribution emerges.

This idea is summarized on Figure 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 2. The graph also show that above $\bar{W}$ the pre- and post ditsribution converges. This assumption captures the idea that wages at the top of the earning distributions are unlikely to be affected by the minimum

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We weight our regressions to make the sample representative by one-digit NACE and firm-size categories (5-20, 20-50, 50-300, more than 300). Moroever our FA_i^{02}, the fraction of workers below the 2002 minimum wage, contains measurment error as for larger firms it is calculated from only a sample of workers. To take into consideration that the measurment errors are varies by firms we multiply the weights by the fraction of workers observed in the SES.

13 The prices change cannot be calculated for products that were not prodcued in the last year. Therfore, we only take the revenue share relataive to all the prodcuts that for price change can be calculated.

14 The upgraded jobs might not employ the same (type of) worker as before. It is possible that some directly effect workers are replaced by workers earning slightly above the minimum wage as in Teulings' (2000) model. Our measure of employment loss, therefore measures only the net jobs destruction.

15 There are many explanations for the spillover of the minimum wage including efficient wages (e.g. Rebitzer and Taylor, 1995) and distance based substitute elasticities(Teulings 2000).

wage.

We can calculate the employment effect of the minimum wage by comparing the number of workers earning below the new minimum wage to the excess number of workers at and above the minimum wage. Formally, we define the bunching estimator of the minimum wage as the following:

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

where $Emp^0\left[MW \le w < \bar{W}\right]$ and $Emp^1\left[MW \le w < \bar{W}\right]$ is the number of workers earning between MW and $\bar{W}$ in the pre and in the post period, respectively, and $Emp^0\left[w < MW\right]$ is the number of workers earning below the new minimum wage. The bunching estimator depends on two things: the level of the new minimum wage MW and $\bar{W}$, the thershold that we .

The role of $\bar{W}$ can be seen more directly from a simple rearrangement of the bunching estimator. Using that $Emp^1\left[w < MW\right] = 0$ if minimum wage is enforced[16], one can easility show that the bunching estimator is equivalent to

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

This formula highlights the this estimator only take into consideration the employment changes occuring below $\bar{W}$. A special case of this estimator is 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 relates to this employment change to the exposure to the minimum wage. 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).

As equation 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 there is a aggregate shock, θ, as well, and so the total employment would be $Emp^0\left(1+\theta\right)$ in the absence of the minimum wage shock. Moreover, assume that the employment loss of the minimum wage is a linear function of the number of people directly exposed to the minimum wage $pEmp^0\left[w < MW\right]\left(1+\theta\right)$[17]. Under these assumption the new employment level is going to be the following:

$$Emp^1 = Emp^0\left(\theta+1\right) - p\left(\theta+1\right)Emp^0\left[w < MW\right]. \tag{2}$$

[16]In practice some workers will be below the new minimum wage, because of measurment error or non-perfect compliance. In the Structure of Earning Survey only a small fraction (1-2%) earn subminimum wages.

[17]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 $Emp^0\left[w < MW\right](1+\theta)$ and not simply on $Emp^0\left[w < MW\right]$.

The next Lemma derives what the bunching estimator will look like under this assumption:
Proposition 1. *Suppose that the employment process follows 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.

Proposition 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 ($F^0(w < MW) = .1$), 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.

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, but we also explore alterantive wage adjustments such . 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 density converges to the pre-reform density distribution. We also show the main results with alternative thresholds, including $\bar{W} = \infty$.

Aggregate earnings distribution might be contaminated by aggregate shocks. Therefore, we estimate the relationship between the 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 (23-30, 30-40, 40-50, 50-55), workers' gender, and workers' education (less than high school, high school or above).[18] We run the following group-level regression:

$$\frac{Emp_g^t \left[MW \leq w < \bar{W}\right] - Emp_g^{2000} \left[MW \leq w < \bar{W}\right]}{Emp_{2000,g}} = \alpha + \beta^B \frac{Emp_g^{2000} \left[MW^t < \bar{W}\right]}{Emp_{2000,g}} + \varepsilon_g \tag{3}$$

We divide our key variables with $Emp_{2000,g}$ to adjust for heteroskedasticity and we also

[18]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.

weight the regressions by $Emp_{2000,g}$.[19] 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 (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 ($\triangle logMW$). In our set-up, it is not straightforward to claculate this, as all group of workers hit by the same minimum wage shock. Therefore, we calculate instead the relationship between the percent change in employment ($\triangle logEmp$) and in wages induced by the minimum wage increase ($\triangle logW$). This relationship is more relevant for welfare assessment as it directly estimate the benefits of minimum wage increase (measured by increase in wages) relative to the potential cost (loss in employment) (see Lee and Saez (2002)). This strategy is also followed a handful of recent papers (e.g., Dube et al., 2010), and in Figure 13 we summarizes these estimates.

We calculate the relationship between $\triangle logMW$ and $\triangle logEmp$ in the following way. First we estimate the employment effects of the minimum wage using $\beta^B - 1$ from equation (3). Then we estimate, β^{AW}, the group level relationship between the excess mass ratio, $\frac{BM_g(MW^t)}{Emp_{2000,g}}$, and the change in group level average wage. The ratio of $\beta^B - 1$ and β^{AW} will give us the employment elasticity with respect to the minimum wage. We also claculate the relationship between $\triangle logMW$ and $\triangle logEmp$ for a hypotehtical group with 25% of directly affected workers[20] to make our coimparision the literature more transparent.

3.2 Firm-level estimates on employment and other outcomes

We estimate the employement effect of the minimum wage by directly assessing firms behavior. We follow closely (Machin, Manning and Rahman 2003) who 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

$$\frac{y_{it} - y_{i2000}}{y_{i2000}} = a_{st} + \beta_t FA_i^{02} + \gamma_t X_{it} + \varepsilon_{it} \tag{4}$$

[19] 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.

[20] The literature in the U.S. tend to focuse on teenage workers. Currently, 25% of teenagers earning at th minimum wage.

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 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. [21]

We estimate equation 4 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).[22] 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[23] with respect to wages (cost of labor) we divide the estimated β_t from equation 4 by the β_t selection corrected average wage (cost of labor). The standard errors are calculated from 1000 bootstrap replications.

Finally we also explore heterogenous responses to the minimum wage. We run the following regressions:

$$\frac{y_{it}-y_{i2000}}{y_{i2000}} = \alpha_t + \beta_t^1 FA_i + \beta_t^2 FA_i * SubGroup_i + SubGroup_i + \varepsilon_{it} \tag{5}$$

$Subgroup_i$ 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.

3.3 Firm-level estimtates on total labor cost, total revenue, profits and prices

We also explore the effect of the minimum wage to other outcomes. For most of these outcomes (e.g. sales, labor cost, output prices) we estimate equation 4. However, for profits, (where we have negative values) we cannot interpret the percentage change in the minimum. Therefore, we estimate the following equation:

$$\frac{y_{it}-y_{i2000}}{\frac{1}{3}\sum_{k=1997}^{1999} Sales_k} = a_{st} + \beta_t FA_i + \gamma_t X_{it} + \varepsilon_{it} \tag{6}$$

The right hand side is the same as for equation 4. The left hand side measures firm-level profitability[24] relative to the average sales in the pre-period. Similar to our previous analysis we include all firms in the regression, regardless of their survival through the sample period.

[21] 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.

[22] In particular, the change in average wage (cost of labor) was imputed to be 100% for firms that die. Then we estimate equation 4 with a least absolute deviation (LAD) on the total sample.

[23] 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.

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

4 Results

4.1 Effect on employment and wages

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.

Aggregate earnings distribution. Figure 3 shows the effect of the minimum wage on the (frequency) distribution of monthly earnings. 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. We also show in the appendix that the main results are very similar if we use hourly wages in the after 1999 sample. The Figure shows the 2002 (red empty bar) and 2000 (brown solid bar) earnings distribution. 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. As we predicted in our theoretical description on Figure 2 this could happen if the minimum wage pushes up earnings even for those who are not directly affected. The spillover effect on the wage distribution is quite large and fades out slowly.[25] 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 4 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 size of the excess mass (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 2, we see that both the excess mass (bunching) and below mass 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

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

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 in Appendix Figure A-3 where we report the kernel densities.[26] 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 A-4 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 lies on the top of each other indicating the lack of substitution between workers based on observable characteristics.

Group level analysis. Since aggregate earnings distribution might be contaminated by aggregate shocks we estimate group level regressions proposed in equation (**??**). In Figure 5 Panel (a) we show the scatter plot between demographic-region group-level Excess Mass Ratio in 1998 (excess number of jobs in 1998 divided by employment in 2000) and Below Mass Ratio in 2002 (the below the 2002 minimum wage mass divided by employment in 2000). 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 at the group level as well. Panel (b) shows that there is a strong relationship between Excess Mass Ratio and Below Mass Ratio after the minimum wage hike. The point estimate for the fraction of bunchers is larger than one in 2002 indicating that increase in minimum wage raised employment. However, standard errors are not small enough to rule out negative effects on employment. Panel (c) on Figure 5 shows the relationship between Excess Mass Ratio and the Below Mass Ratio over time for the whole period. As in the histograms, the timing of the reform is strongly visible in the evolution of excess mass.

In Table 2 we report the main results. Column (1) shows the regression behind our main specification is shown in Figure 5. Panel (b). In the last but one row we report the effect of the minimum wage on the percentage change in employment ($\beta^B - 1$), while in the last row the labor demand elasticity. The effect on employment is positive as discussed in the previous paragraph and with the standard errors we can rule out larger than -0.3 labor demand elasticity with respect to wages.

[26] 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.

Figure summarizes the key results of this section. Panel (a) transforms the results shown in Figure 5 Panel (c) 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 Ratio and the Below Mass Ratio 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 use the formula shown in equation (2): the estimated effect on Excess Mass Ratio minus one. The estimated effects of the minimum wage are negative in 2001 and positive later on. The lower bound of the estimates show that at most 20% of the workers earning below the minimum wage were laid off.

In Panel (b) we show the effect of the minimum wage increase on wages. For the pre-2000 years the relationship between average wage change and Below Mass Ratio in 2002 are shown. In the post minimum wage years the relationship between average wage change at year t and Below Mass Ratio in year t 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 demand elasticity. In Figure 13 we compare our estimates for the labor demand elasticity to the one estimated in the previous literature. The dashed vertical line is at -.2, which shows our preferred estimate based on this section and the firm-level evidence. The bunching estimate from 2001 is very close to that level, while for latter years the point estimates are somewhat higher. However, the lower bounds of our estimates are very close to each other and can be used to rule out medium sized negative effects on employment.

Now we turn to check the robustness of our results. Table 2 columns (2)-(8) explore alternative specifications of Equation **??**. Results using hourly earnings are reported in column (2). The estimated coefficient on Excess Mass Ratio is very similar to our main specification. This is not surprising given that 95% of the employees work in full-time in Hungary. In Column (3) we also include firms between 5-10 employees in the sample. The employment estimates is positive here, but we cannot reject that the effect of the minimum wage is zero. In Column (4)-(5) we look at how changing $\bar{W}$ affects our results. The estimates in the three collumns are very similar to the benchmark estimates. The most interesting case is Column 5, where we set $\bar{W}$ to be infinity. Here we find smaller (-12% instead of +7%) and more imprecise (s.e. is 0.347 vs. 0.173) estimates. Remember, when $\bar{W}$ is set to be infinity, we are simply estimating the effect of the minimum wage on the employment changes at all earning levels.

To understand the benefits of lowering $\bar{W}$ in Appendix Figure 6(b) we compare the estimated effect on employment for $\bar{W} = \infty$ (Panel (a)) and for our main specification with $\bar{W} = 11$ (Panel (b)). There are a few things worth noting here. First, the two estimates are not statistically different from each other in most years (except for 1997 and 2004). Second, 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. Third, the placebo estimates for $\bar{W} = \infty$ shows 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 column (4) and (5) of Table 3 we explore alternative ways for locating the wage distribution over time. In column (4) we use nominal GDP growth in the private sector, but the estimated coefficient stays the same. In column (5) 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.[27] Finally in Column (6) we show that our results are robust to including firms with 5-10 employees.

Interpreting the estimated effects in the previous literature. Our estimates imply a close to zero labor demand elasticity, with a lower bound of -.3. This finding is in harmony with the large literature on minimum wage that finds close to zero effect on employment, sometimes positive (Doucouliagos and Stanley 2009). However, our result demonstrate that the close to zero effect of the minimum wage holds for large and permanent changes in the minimum wage as well.

So far we have focused on estimating the relationship between employment change ($\triangle logEmp$) and the wage 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). We found that a 60% increase in the minimum wage leads to a $+3 \pm 14\%$ change in the employment of workers for whom the minimum wage binds. Therefore, a 10% increase in the minimum wage leads to $+0.5 \pm 2.3\%$ change in the affected population. Assume that 25% of the workers are affected by the minimum wage. If 25% experience an $+0.5 \pm 2.3\%$ change in their employment, while the remaining 75% do not have any loss in employment, the group level change in employment will be $+0.12 \pm .5\%$. 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, $+0.12 \pm .5\%$, is smaller than that, however it should be noted that our sample population is the working-age population rather than teenagers.

Now we turn and look at the effect on employment. We start our firm-level analysis by investigating the effect of minimum wage increase on average wage and average labor cost. These two differ in the presence of non-cash benefits such as subsidized meals, transport and culture. In Figure 7 Panel (a) we show the estimated β_t-s from equation 4 for two outcomes: average wage and average total labor cost. The blue (solid) line shows the β_t for average wage. Remember, β_t is the coefficient of FA_i, the fraction of workers for whom the minimum wage binds in 2002. Therefore β_t shows the difference in cumulative growth (relative to 2000) between a firm with all its workers below the 2002 minimum wage and a firm with none of its workers below the 2002 minimum wage. Figure 7 shows that average wage declines in highly exposed firms relative to non-exposed ones before 2000. However, in 2001, the first year after

[27] 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 **??**, 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.

the minimum wage hike, firms with 100% exposure experience a 32% increase in their average wages, a gap that widens to 44% by two years after the reform. By four years after the reform, the gap dissipates to 37%. This pattern closely follows the path of the minimum wage depicted on Figure 1.

The red (dashed) line in Figure 7 Panel (a) shows the effect on total labor cost. The estimated β_t for average labor cost closely follows the estimated β_t for average wage before 2000. On the other hand, after 2000 the effects on average labor cost are always lower. For instance, in 2002, the estimated effect is 36% for total labor cost (instead of a 44% for average earnings). It appears that firms are able to mitigate some of the burden of the minimum wage by cutting back on other non-wage labor costs. This is shown more directly in Figure 7 panel (b), where we investigate the effect of FA_i on the share of non-financial remuneration in total compensation.[28] The graph clearly shows that firms cut back non-financial compensation (by 4 percentage points) after the minimum wage was increased.

Our analysis on average labor cost highlights that the rise in earnings overstates the "true" change in workers remuneration. We calculate that the change in workers' remuneration is about 80% of the change in earnings. However, average labor cost still increased by a large amount (30%) at highly exposed firms.

Figure 8 panel a) depicts results for employment effects. Remember, we include in our analysis the firms that die as well. Therefore, the results presented here include firms' extensive margin (closing) and intensive margin (lay-off) decisions. The estimated β_t for employment shows that highly exposed and not-exposed firms had very similar employment trends before the minimum wage hike. However, after 2000 firms with higher exposure to minimum wage experience of quantitatively small and marginally significant disemployment effects. For instance, the point estimate in 2002 indicate that 0.56 out of 10 affected workers lost their job as result of the minimum wage hike. Even though the sign of the point estimate is different, the firm-level employment effect is within the confidence interval of the bunching estimates.

Another interesting finding is that the short-term employment effect (0.4 workers out of 10 in 2001) is slightly smaller than the long-term effect (0.7 workers out of 10 in 2003).[29] Finally, in Appendix Figure A-6 we present non-parametric binned scatter plots of the relationship between percentage change in employment and FA_i for the years 1998, 2001, 2002 and 2003. The graph shows that the relationship estimated in 8 Panel (a) is close to linear.

On Figure 8 Panel (b) we plot the selection-corrected average labor cost. This graph differs from the average labor cost graphs presented on Figure 7, because it takes into account that firms that died do not have information on average labor cost. We compute the selection corrected average wage by following Johnson et al. (2000).[30] The key identification assumption of this procedure is that firms that died would have been above the conditional median of the wage change. Dividing the estimates from 8 Panel A or with Panel B gives us back the labor demand elasticity with respect to labor cost.[31] For instance, the estimates from 2000 imply

[28]In particular, we run the following regression
$\frac{NonFinanRenum_t}{TotalLaborCost_t} - \frac{NonFinanRenum_{2000}}{TotalLaborCost_{2000}} = a_{st} + \beta_t FA_i + \gamma_t X_{it} + \varepsilon_{it}$

[29]The point estimates for the bunching analysis does not show increasing negative effect on employment over time. However, the standard errors for the bunching estimation increases over time, which makes it difficult to compare short term and long term effects.

[30]In particular, the change in average firms who die was imputed to be 100%. Then we estimate equation 4 with a least absolute deviation (LAD) on the total sample.

[31]There are two fundamentally two different ways to interpret firm level demand elasticity. If firms operating

that the labor demand elasticity with respect to labor cost is -.2 in 2002. [32]

In Figure 9 we summarize the effect of minimum wage on total labor cost. Again the non-exposed and highly exposed firms had very similar trends before the minimum wage hike, but this breaks after 2000. The increase in total labor cost is large, though it is less then the increase in average wage. This gap comes from two sources. First, as we showed, the increase in labor cost is smaller than the increase in earnings because firms cut non-financial remuneration in response to the minimum wage. This explains approximately half of the gap. Second, at the firm-level we found a small disemployment effect, which decreases total spending on labor.

To inspect the incidence of the minimum wage we look at other outcomes as well. In Figure 11 Panel (a) we show results of equation (**??**) for labor cost. The effect is very similar to percentage changes depicted on Figure 9 Panel (a): highly exposed and non-exposed firm have similar trend before 2000 and exactly at the timing of the reform labor costs rise at the exposed ones. In Panel (b) we show the effect on profits. Before the reform there are no large differences in profits and we also do not see any changes after 2000. Remember, these regressions include firms that died as well, and so the stability of profits is not driven by selection. In Appendix Figure A-7 we present the non-parametric binned scatter plots of the relationship between changes in profit ratio (profit over average sales between 1997 and 2000) and 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 11 Panel (b) is close to linear.

We augment this analysis by estimating heterogeneous treatment effects for firms with high and low pre-reform profitability. This helps us to focus on firms that could potentially finance the increase in labor costs. In Figure 12 Panel (a) and Panel (b) we show the results on labor costs and operating profits for firms whose average profitability between 1997 and 2000 was above median. The average pre-reform profit margin for these firms is 7.52%, and the average increase in labor cost ratio (labor cost over average sales) as result of the minimum wage hike is around 4.2% (see. Figure 12 Panel (a)). Therefore, these firms could pay for the minimum wage easily, but we do not find any evidence for that. The operating profit shown in Panel (b) is stable before and after the minimum wage hike even for firms having substantial profit before the minimum wage hike.

All pieces of evidence show that the minimum wage did not reduce operating profits. This indicates that the labor cost increase caused by the minimum wage was passed on to consumers. In Figure 11 Panel (c) we show results of equation (**??**) for sales. We subtract COGFR from sales to focus the production process of the firm.[33] The figure highlights that

on the same market, but have different share of minimum wage workers, then the minimum wage hike hits only some firms on the market. In this case the demand elasticity is the individual level demand elasticity. On the other hand, if firms operating on the same market has the same share of minimum wage workers, the the minimum wage hits all workers on the market. In that case what our estimates uncover is the market level labor demand. elasticity. Individual level labor demand elasticity are always larger than market level elasticities. Given that the estimated elasticity that we found here are quite low we interpret the estimated elasticities as market level demand elasticity.

[32]Note that this elasticity is not exactly the same as the one we calculated for the bunching part. Here we calculated the demand elasticity with respect to *labor cost*, while in the first part (bunching evidence) we calculated the demand elasticity with respect to *earnings*. The firm-level estimates indicated that the demand elasticity with respect to wages is -0.16, which is within the lower bound of the bunching estimates (-0.28).

[33]Not that this measure is still include intermediate goods such as materials used for production and energy expenses.

the Sales Ratios have similar trend before 2000, but this trend breaks exactly at the timing of the reform. Moreover, the increase in this sales measure is just the same in magnitude as the increase in the total labor cost shown in Figure 11 Panel (a). This indicates that firms were able to increase the margin that they earn on goods they produced, and they spent the extra revenue from that to finance their increased labor cost. In Panel (b) of Figure 9 we estimate equation 4 for sales. As it is clear from the figure that sales increases substantially in response to the minimum wage.

Robustness. In Table 6 we summarize the graphical results presented up to this point. In the first four columns we present our main results. In Columns (1) and (2) we look at percentage changes between 2000 and 2002. In Columns (1), (3) and (5) we run equation 4 without any controls, while in Columns (2), (4) and (6) we show that controlling for firm level characteristics in 1997 (export share in 1997 its square term) and industry dummies do not affect the results.

Panel A shows the results of regressions where the average wage is the outcome variable and we adjust for selection. The estimated coefficient in column (4) indicates that a firm with 100% of workers below the minimum wage experienced 44% higher average wage increase than a firm with no exposure to the minimum wage. These effects are only slightly affected by including control variables or industry effects. In Panel B we report the effect on average labor cost. As we showed on Figure 7 the increase is lower for average labor cost than for average wage (36% instead of 44%) because firms cut back non-cash benefits in response to the minimum wage. In Panel (C) we detect a small negative effect on employment that is statistically significant. The point estimate suggests that 0.5 out of 10 workers laid-off as a result of the minimum wage increase. The employment effects are slightly larger in 2003, 0.7 out of 10 affected workers are laid off, indicating that firms need time to adjust for the minimum wage changes. The fact that the effects on average wages are large, but on employment are small indicate that firm-level labor costs must increase substantially. We show in Panel D that labor cost increases by 20% at firms with high exposure (100%) relative to non-exposed (0%) firms.

In Table 5 Panel E and F we transform our estimates on employment and selection corrected average wages to labor demand elasticity. One key advantage of the firm-level data is that we can separate the labor demand 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 the labor demand elasticity with respect to the cost of labor ($\Delta logLC$), which measures 1% increase in *cost of labor* induced by the minimum wage ($\Delta logW$) on employment ($\Delta logEmp$). The results in Panel E and F show that the labor demand elasticity with respect to cost of labor is approximately 25% larger than with respect to wages (12% vs. 15% in 2002 and 23% vs. 19% in 2003).

In Table 6 we look at the effect of the minimum wage on other outcomes. In Panel A we show that value added increases substantially in response to the minimum wage indicating that firms owners are unlikely to pay for the minimum wage. In Panel B we show that materials. We also calculate that a 1% increase in cost of labor increases net sales by .2%. This is a very large change given that the share of labor in net sales for a representative firm is 23%. In Panel C we report the effect of the minimum wage on sales. Columns (5) and (6) highlight that sales decreased at highly exposed firm relative to non-exposed ones, though this decrease is insignificant. However, this trends breaks after 2000 and so 1% increase in cost of labor leads to a 13% in 2002 sales and 6% in 2003 one. Columns (5) and (6) highlight that highly exposed firms have somewhat different trends in capita[34]l than non-exposed firms. However, the point estimate suggests that capital increased in response to the minimum wage increase indicating the presence of capital-labor substitution in production.

Treatment effect heterogeneity. Table 8 explores the treatment effect heterogeneity of the minimum wage (focusing on the changes between 2002 and 2000), while Table 9 reports the corresponding placebo tests (the changes occur between 2000 and 1998). Each column represents one of our key variables. The horizontal panels explore heterogeneous responses to the minimum wage hike by various subgroups. In particular, we estimate equation (3) in the following way:

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. In Table 9 we examine this by looking at the changes between 2000 and 1998. Again, we find evidence in support of common pre-trends. However, notable exception is the firms with more than 20 employees (in 1997) and some outcomes for the manufacturing firms.

In Table 7 Panel C we show estimates by employment size in 1997. As we already noted these results should be treated cautiously, because the presence of pre-trend for firms with more than 20 employees. However, the point estimate suggests that small firms experience a slightly larger employment loss (0.75 vs. 0.59 out of 10 workers), but these differences are not statistically different. In Panel D we also examine firms' reactions by the share of non-financial remuneration in their total labor cost. In Figure 7 we show that firms mitigate the increase in earnings by cutting back non-financial remuneration. Therefore, we would expect that firms with larger shares of non-financial remuneration are more protected from the minimum wage shock. Consistent with that, firms having higher non-financial remuneration before 2000 experience less increase in their average labor cost, and also less severe employment losses. One important implication of this finding is that industries and countries where non-financial remuneration plays larger parts of wage compensation might have relatively smaller employment effects in response to the minimum wage. However, the crowding out of non-financial remuneration might lead to substantial distortions.

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 elavated cost of firms were paid out from an increase in sales instead of a decrease in firm's 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 face with inelastic demand.

In neoclassical model of firms the effect of the wage increase on employment is determined by the Hicks–Marshall rules of derived demand. These rules connects the labor demand elasticity with the substitution between labor and other inputs and the product demand elasticity. More specifically, let us suppose that firms have a 4-factor production function:

[34]Real capital is calculated with the perpetual inventory method, which sums up series of real investments over the life cycle of the 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.

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

where Y is output, L is low skilled labor, H is high 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 earns the minimum wage. Since our empirical estimates show the effect of the minimum wage on firms this makes it

Our empirical sections we estimated the effects of the minimum wage on firms This makes it easier to connect the firm-level evidence, where we only observe the changes in total employment, to the data. However, as we showed earlier, we did not find evidence for the presence of labor-labor substitution (see Appendix Figure A-4 for the details). We assume that F have constant return to scale (CRS) in the four 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):

$$\frac{\partial logL}{\partial logw} = \underbrace{-s_L\eta}_{\text{scale effect}} \quad \underbrace{-s_K\sigma_{KL}}_{\substack{\text{substitution}\\ \text{between K and L}}} \quad \underbrace{-s_M\sigma_{ML}}_{\substack{\text{substitution}\\ \text{between M and L}}} \tag{7}$$

where s_L is the share of labor in output, s_K is the share of capital expenses in production, s_I is the share of intermediate goods used in the production, s_M is the share of intermediate goods purchased only for resale, η is the *market-level* product demand elasticity firms face, σ_{KL} is the substitution between capital and labor, and σ_{IL} and σ_{ML} is the substitution between intermediate goods and labor. The first part of equation (7) is the scale effect. When a competitive firm is hit by a wage increase, it must raise prices to survive. If the production function have 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 cut 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 became more costly firms will substitute labor with other inputs. The second part show 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 and the fourth part of the equation (6) is the substitution between labor and intermediate and purchased goods for resale, respectively.

Equation (7) highlights that the importance of scale effects and the substitution effects depends on the factor shares. Table 5 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%. Expenses on intermediate goods are another 45%, while cost of goods purchased for resale is 30% for a representative firms. This indicates that the low labor demand elasticities can only be consistent with with the Hicks–Marshall rules of derived demand if σ_{IL} and σ_{ML} are sufficiently low.

Is a low value of these two key elasticities in line with existing estimates? The substitution between purchased goods for resale, σ_{ML}, is not well understood in the literature.[35] However, it is hard to imagine how buying more purchased goods for resale could be used as a substitute of labor and so this substitution elasticity is likely to be close to zero. On the other hand, many studies have attempted to estimate the substitution between intermediate goods and labor. For instance, elasticity of substitution between energy expenses and labor 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 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_{IL} in the manufacturing is 0.3, with alternative specifications vary between -0.2 to 0.9. A more recent estimate by Atalay (2014) found 0.05 using all industries in their estimation.[36] 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 over-estimates 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 firm-level evidence can be used to uncover the relevant empirical elasticities of the neoclassical model. For that we use the following theoretical moments (derivation in Hamermesh, 1993). The relationship between changes in sales and increase in labor cost is the following

$$\frac{\partial logpY}{\partial logw} = \underbrace{s_L}_{\text{price effect}} \quad \underbrace{-s_L\eta}_{\text{scale effect}} \tag{8}$$

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%.

As we discussed in the previous section sales is less precisely estimated then net sales (which is sales net of cost of goods purchased for resale). It can be shown that the effect on cost of goods for resale is the following:

$$\frac{\partial log\left(pY - c_s M\right)}{\partial logw} = \frac{1}{(1-s_M)}s_L - s_L\eta - \frac{s_M}{(1-s_M)}s_L\sigma_M \tag{9}$$

Moreover, the neoclassical model also predicts how under inputs effected by the minimum

[35]Most micro level evidence on the substitution between intermediate inputs and labor focus on the manufacturing sector. However, cost of goods purchased for resale plays a minor role in that sector.

[36]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.

wage:

$$\frac{\partial logK}{\partial logw} = s_L(-\eta + \sigma_{KL}) \tag{10}$$

$$\frac{\partial logp_I I}{\partial logw} = s_L(-\eta + \sigma_{IL}) \tag{11}$$

$$\frac{\partial logc_s M}{\partial logw} = s_L(-\eta + \sigma_{ML}) \tag{12}$$

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 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 10 shows the estimated parameters for 2002 and for 2003. In these estimates we assume that the elasticity of substitution between goods purchased for resale and labor is zero. In principle one could estimate this parameter as well, however that increases the standard errors substantially. Columns (1) and (4) show the empirical moments. Column (2) and Column (5) use only three moments in the estimations: Equation (7), the labor demand elasticity; Equation (9) the effect on net sales, and Equation 10 the effect on capital; 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 (1) and Column (3) show the empirical moments from 2002 and 2003, while Column (2) and (4) show the corresponding estimates. The estimated substitution between capital and labor is 0.72 (s.e. 0.25) in 2002 and 1.06 (s.e. 0.30) in 2003. Both of these estimates are in the range of the previous estimate in the literature that goes from 0.36 in Chirinko et. al (2011) to 1.25 in Karabarbounis and Neiman (2014). The estimated product demand elasticity is 0.14 (s.e. 0.27) in 2002 and 0.24 (s.e. 0.37) in 2003. This estimate is lower than the usual range for the uncompensated product demand elasticity (1.5-0.5). However, since 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 in the range of 0.1-02, which is in line with the existing estimates in the literature.

In Columns (5) and (8) of Table 10 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. On the other hand 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 (0.84, s.e. 0.54) is very close to the estimates for Armington elasticity in the trade literature using high frequency data (Blonigen and Wilson 1999, Reinert and Roland-Host 1992), but 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 (0.086), but it is consistent with some estimates in the literature . Moreover, the fairly low output demand elasticity is also consistent with the findings of 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 is not likely to be a Hungarian or a research-design specific result.

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. Only a small part (20%) of this wage increase was offset by cutting back non-financial remuneration. 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 in higher output prices. Hence, our empirical results indicate that the ultimate incidence of the minimum wage fell on the consumers.

Using the firm-level data we were also able to rule out many competing explanations that attempt to explain the close to zero effect of the minimum wage. In particular, we did not find supporting evidence for the presence of monopsonistic wage setting (Manning 2003, Card and Krueger 1995), of the search and matching model (Flinn 2010) and of the efficiency wage models (Rebitzer and Taylor 1995). The common feature of these models is that introducing a minimum wage can improve the allocation of resources, since there exist some frictions or distortions beforehand. Therefore, our results presented here 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, as it is showed here, is an important channel empirically. To incorporate consumer pass-through in their framework one needs to take into consideration general equilibrium effects of the minimum wage. Thus, one important future direction is to add general equilibrium considerations in 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 more costly than in the U.S. where low-wage workers are concentrated in the service sector (Dube, Lester and Reich 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.

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Table 1: Descriptive Statistics - Wage Survey

	(1)	(2)	(3)	(4)	(5)
	unweighted	weighted			
	mean	mean	sd	min	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 2: Group-Level Relationship between Excess Mass and Below Mass

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Changes between 2000 and 2002								
Fraction Affected	0.912***	0.923***	1.053***	0.852***	0.842***	0.822***	0.921***	0.875***
	(0.0558)	(0.0559)	(0.0693)	(0.0611)	(0.0653)	(0.101)	(0.0566)	(0.0517)
%△ in Emp in pop	0.226***	0.204***	0.277***	0.162***	0.289***	0.566***	0.227***	0.211***
	(0.0467)	(0.0473)	(0.0592)	(0.0455)	(0.0553)	(0.0969)	(0.0465)	(0.0472)
Constant	0.0314***	0.0259***	0.0419***	0.0272***	0.0366***	0.0101	0.0340***	0.0183**
	(0.00732)	(0.00726)	(0.0131)	(0.00761)	(0.00849)	(0.0186)	(0.00734)	(0.00717)
R-squared	0.730	0.749	0.688	0.716	0.669	0.489	0.724	0.749
Implied %△ in Emp	-0.087	-0.081	0.052	-0.145	-0.156	-0.176	-0.076	-0.125
	0.058	0.058	0.075	0.063	0.068	0.102	0.06	0.052
Implied Emp Elast wrt. Minimum wage	-0.036	-0.034	0.022	-0.06	-0.065	-0.074	-0.032	-0.052
	0.024	0.024	0.031	0.026	0.029	0.042	0.025	0.022
Implied Emp Elast wrt. Wage	-0.215	-0.209	0.14	-0.367	-0.39	-0.436	-0.187	-0.322
	0.141	0.15	0.196	0.149	0.163	0.259	0.147	0.127
Panel B: Placebo Test: Changes between 1998 and 2000								
Fraction Affected	-0.00420		-0.101	0.00491	-0.0549	0.121	0.0377	0.0233
	(0.0779)		(0.0884)	(0.0683)	(0.0884)	(0.125)	(0.0822)	(0.0735)
%△ in Emp in pop	-0.0826		-0.0694	-0.0551	-0.112	-0.423***	-0.0883	-0.0854
	(0.0649)		(0.0731)	(0.0530)	(0.0706)	(0.119)	(0.0677)	(0.0663)
Constant	0.00468		-0.0635***	-0.00140	0.00965	-0.0356	0.0156	0.0110
	(0.0109)		(0.0165)	(0.00946)	(0.0120)	(0.0225)	(0.0118)	(0.0111)
R-Squared	0.013		0.020	0.009	0.022	0.114	0.015	0.013
Nobs	112	112	112	112	112	112	112	112
Thershold, W	11	11	11	10.8	11.2	INF	11	11
Firm-Size	Emp>10	Emp>10	Emp>5	Emp>10	Emp>10	Emp>10	Emp>10	Emp>10
Wage Adjustment	nominal GDP	nominal GDP	nominal GDP	nominal GDP	nominal GDP	nominal GDP	nominal GDP (private)	75th percent
Earnings	monthly	hourly	monthly	hourly	monthly	monthly	monthly	monthly

Table 3: Descriptive Statistics - Corporate Income Tax Data

	(1)	(2)	(3)	(4)	(5)
	unweighted	weighted			
	mean	mean	sd	p25	p75
Employment	142.1	47.8	418.4	10	24
Average Earnings (HUF in thousnad)	161.98	50.6	474.9	4.7	22.0
Average Labor Cost (HUF in thousnad)	249.1	78.2	707.5	7.8	34.5
Share of non-financial renumeration	0.10	0.11	0.11	0.04	0.13
Average value added (HUF in thousnad)	403.2	127.9	1195.3	9.9	55.9
Profitability (EBIT/SALES)	0.04	0.03	0.06	0	0.07
Fraction affected	0.26	0.36	0.38	0	0.75
Number of firms	5596	5596	5596	5596	5596

Note: This table presents descriptive statistics of firm-level 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 4: Descriptive Statistics - Firm-Level Cost Shares

	Number of Firms	Value Added			Materials and Other Expenses	
		Labor Cost	Capital Expenses		Materials	Cost of Goods for Resale
			EBIT	Depreciation		
Manufacturing (food, textile)	1429	0.183	0.029	0.024	0.479	0.265
Manufacturing (chemicals, metals)	876	0.220	0.039	0.025	0.616	0.095
Manufacturing (machines)	590	0.251	0.046	0.025	0.544	0.113
Construction	704	0.180	0.039	0.022	0.699	0.047
Retail and wholesale	1158	0.095	0.025	0.017	0.216	0.642
Transportation and hotels	506	0.187	0.026	0.039	0.534	0.199
Other services	333	0.254	0.043	0.039	0.481	0.134
All	5596	0.173	0.033	0.025	0.464	0.291

Note: This table shows the cost shares 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 5: Firm-Level Evidence, Effect on Employment

	(1)	(2)	(3)	(4)	(5)	(6)
	Main results				Placebo estimates	
	Change from 2000 to 2002		Change from 2000 to 2004		Change from 1998 to 2000	
Panel A: Change in selection corrected wage						
Fraction Affected	0.456***	0.459***	0.363***	0.371***	-0.033**	-0.030**
	[0.019]	[0.017]	[0.0222]	[0.0235]	[0.013]	[0.012]
Constant	0.021**		0.022**		0.0519***	
	[0.009]		[0.009]		[0.008]	
Panel B: Change in selection corrected cost of labor						
Fraction Affected	0.372***	0.379***	0.290***	0.282***	-0.016	-0.016
	[0.018]	[0.018]	[0.0216]	[0.0229]	[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*	-0.0653**	-0.0595*	0.0132	0.0169
	[0.0226]	[0.0242]	[0.0292]	[0.0310]	[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***	0.155***	0.371***	-0.0182	-0.0115
	[0.0265]	[0.0287]	[0.0324]	[0.0235]	[0.0216]	[0.0229]
Constant	-0.120***		-0.173***		-0.100***	
	[0.0138]		[0.0150]		[0.0118]	
Panel E: Implied Employment Elasticity with respect to the wage						
Fraction Affected	-0.119	-0.096	-0.180	-0.160		
	[0.049]	[0.054]	[0.064]	[0.068]		
Panel F: Implied Employment Elasticity with respect to the cost of labor						
Fraction Affected	-0.146	-0.119	-0.225	-0.211		
	[0.060]	[0.067]	[0.081]	[0.088]		
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

Note: Table shows the estimated results from equation 4 under different specifications. The first four columns show our main results, Columns (1)-(2) show the effect of fraction of workers earning below the 2002 minimum wage on the percentage change between *2002 and 2000,* while Column (3) and (4) for changes between *2004 and 2000.* We also report the implied 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 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 for the details).

Table 6: Firm-Level Evidence, Effect on Sales, Profits and Prices

	(1)	(2)	(3)	(4)	(5)	(6)
	Main results				Placebo estimates	
	Change from 2000 to 2002		Change from 2000 to 2004		Change from 1998 to 2000	
Panel A: Change in selection corrected wage						
Fraction Affected	0.456***	0.459***	0.363***	0.371***	-0.033**	-0.030**
	[0.019]	[0.017]	[0.0222]	[0.0235]	[0.013]	[0.012]
Constant	0.021**		0.022**		0.0519***	
	[0.009]		[0.009]		[0.008]	
Panel B: Change in selection corrected cost of labor						
Fraction Affected	0.372***	0.379***	0.290***	0.282***	-0.016	-0.016
	[0.018]	[0.018]	[0.0216]	[0.0229]	[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*	-0.0653**	-0.0595*	0.0132	0.0169
	[0.0226]	[0.0242]	[0.0292]	[0.0310]	[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***	0.155***	0.371***	-0.0182	-0.0115
	[0.0265]	[0.0287]	[0.0324]	[0.0235]	[0.0216]	[0.0229]
Constant	-0.120***		-0.173***		-0.100***	
	[0.0138]		[0.0150]		[0.0118]	
Panel E: Implied Employment Elasticity with respect to the wage						
Fraction Affected	-0.119	-0.096	-0.180	-0.160		
	[0.049]	[0.054]	[0.064]	[0.068]		
Panel F: Implied Employment Elasticity with respect to the cost of labor						
Fraction Affected	-0.146	-0.119	-0.225	-0.211		
	[0.060]	[0.067]	[0.081]	[0.088]		
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

Note: Table shows the estimated results from equation 4 under different specifications. The first four columns show our main results, Columns (1)-(2) show the effect of fraction of workers earning below the 2002 minimum wage on the percentage change between *2002 and 2000*, while Column (3) and (4) for changes between *2004 and 2000*. We also report the implied 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 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 for the details).

Table 7: Effect on Firm-level Outcomes by Sectors

	(1) Cost of Labor	(2) Employ-ment	(3) Labor Cost	(4) Sales	(5) Materials	(6) Profit	(7) Capital	(8) Price
Panel A: Change between 2000 and 2002								
Panel A1: All firms								
All firms	0.366***	-0.0474*	0.252***	0.0749***	0.0488*	0.00281	0.0410	
obs.=5459	[0.0185]	[0.0242]	[0.0287]	[0.0267]	[0.0270]	[0.00476]	[0.0344]	
Panel A2: by industry								
service	0.362***	-0.0355	0.263***	0.0812**	0.0513	-0.00146	0.0422	
obs.=2894	[0.0228]	[0.0293]	[0.0347]	[0.0323]	[0.0323]	[0.00553]	[0.0422]	
manufacturing	0.378***	-0.0828**	0.220***	0.0564	0.0414	0.0154*	0.0374	0.0749***
obs.=2565	[0.0273]	[0.0405]	[0.0480]	[0.0459]	[0.0480]	[0.00901]	[0.0538]	(0.0203)
Panel B: Change between 2000 and 2004								
Panel B1: All firms								
All firms	0.282***	-0.0595*	0.158***	0.0257	0.00653	0.00458	0.0480	
obs.=5459	[0.0229]	[0.0310]	[0.0347]	[0.0296]	[0.0295]	[0.00501]	[0.0409]	
Panel B1: by industry								
service	0.278***	-0.0519	0.168***	0.0219	0.00264	0.00378	0.0556	
obs.=2894	[0.0284]	[0.0382]	[0.0428]	[0.0359]	[0.0357]	[0.00591]	[0.0501]	
manufacturing	0.292***	-0.0819*	0.128**	0.0368	0.0180	0.00692	0.0254	0.0842***
obs.=2565	[0.0336]	[0.0483]	[0.0543]	[0.0502]	[0.0505]	[0.00933]	[0.0647]	(0.0299)
Panel C: Change between 1998 and 2000								
Panel C1: All firms	0.0378**	-0.0169	0.0115	0.00380	0.0123	-0.0143***	-0.0458	
All firms	[0.0163]	[0.0221]	[0.0229]	[0.0274]	[0.0275]	[0.00501]	[0.0342]	
obs.=5459								
Panel C1: by industry								
service	0.0334*	-0.0227	0.00729	-0.0166	-0.00376	-0.0138**	-0.0813*	
obs.=2894	[0.0197]	[0.0243]	[0.0277]	[0.0316]	[0.0333]	[0.00589]	[0.0421]	
manufacturing	0.0508*	-0.0482	0.0241	0.0500	0.0596	-0.0157*	0.0597	-0.0195
obs.=2565	[0.0272]	[0.0314]	[0.0384]	[0.0445]	[0.0460]	[0.00934]	[0.0521]	(0.0282)

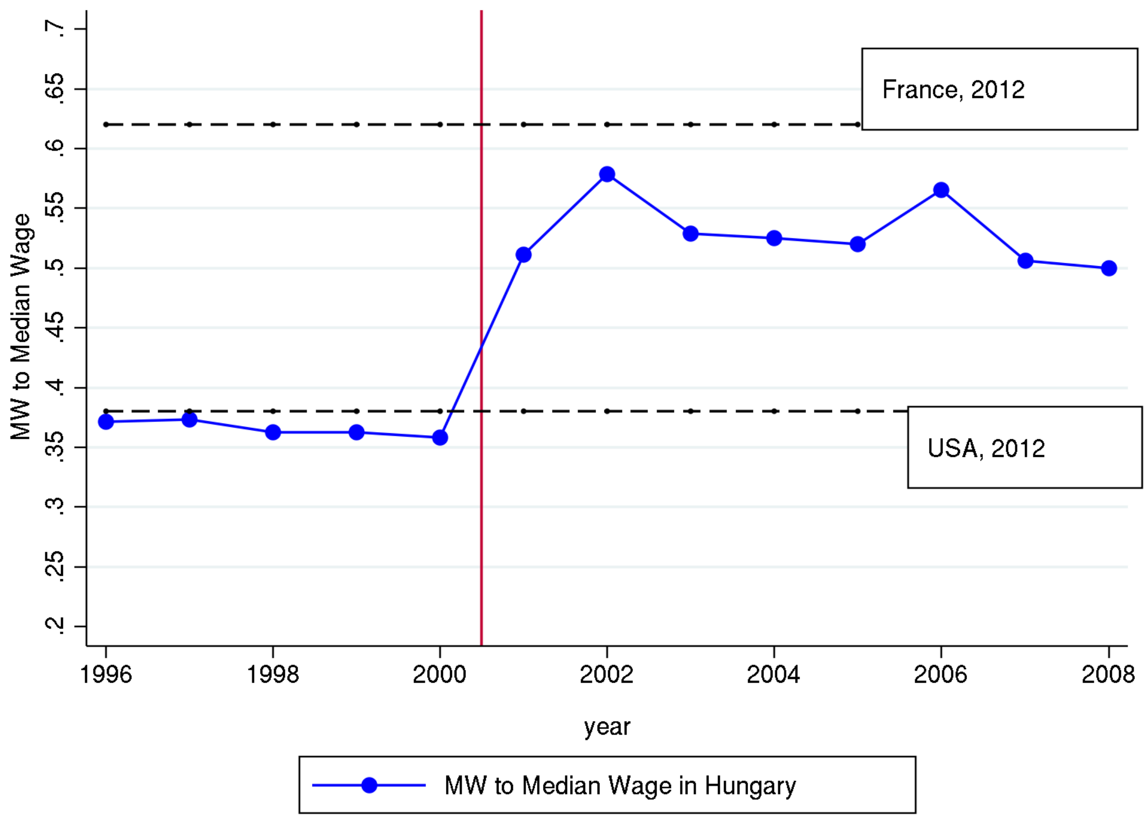
Table 8: Estimating the neoclassical model in 2002 and 2003

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All Firms in 2002		All Firms in 2003		Exporting Firms in 2003		Non-Exporting Firms in 2003	
	Empirical moments		Empirical moments		Empirical moments		Empirical moments	
Output Demand Elasticity		0.14		0.24		0.84		0.086
		[0.27]		[0.37]		[0.54]		[0.90]
Substitution between Capital and Labor		0.72		1.06		1.1		1.17
		[0.25]		[0.30]		[0.51]		[0.71]
Substitution between Intermediate Goods used for Production and Labor		0.14		0.25		0.22		0.10
		[0.12]		[0.17]		[0.27]		[0.32]
Predicted Moments								
Labor demand (Eq 7)	-0.15	-0.15	-0.23	-0.23	-0.34	-0.3402	-0.16	-0.16
	[0.05]		[0.06]		[0.14]		[0.14]	
Net Sales (Eq 9)	0.20	0.23	0.20	0.24	0.02	0.11	0.26	0.28
	[0.06]		[0.08]		[0.14]		[0.13]	
Capital (Eq 10)	0.1	0.1	0.15	0.14	0.05	0.04	0.19	0.19
	[0.1]		[0.14]		[0.15]		[0.20]	
Intermediate Goods used for Production (Eq 11)	0.01	0	0.01	0.002	-0.01	-0.11	0.02	0.003
	[0.06]		[0.08]		[0.13]		[0.19]	
Goods purchased for resale (Eq 12)	0.01	-0.02	0.05	-0.0411	-0.078	-0.14	0.05	-0.0146
	[0.12]		[0.16]		[0.20]		[0.21]	
Moments used		5		5		5	5	
Goodness of fit		0.26		0.87		1.25		0.11

Standard Errors Reported in the Brackets

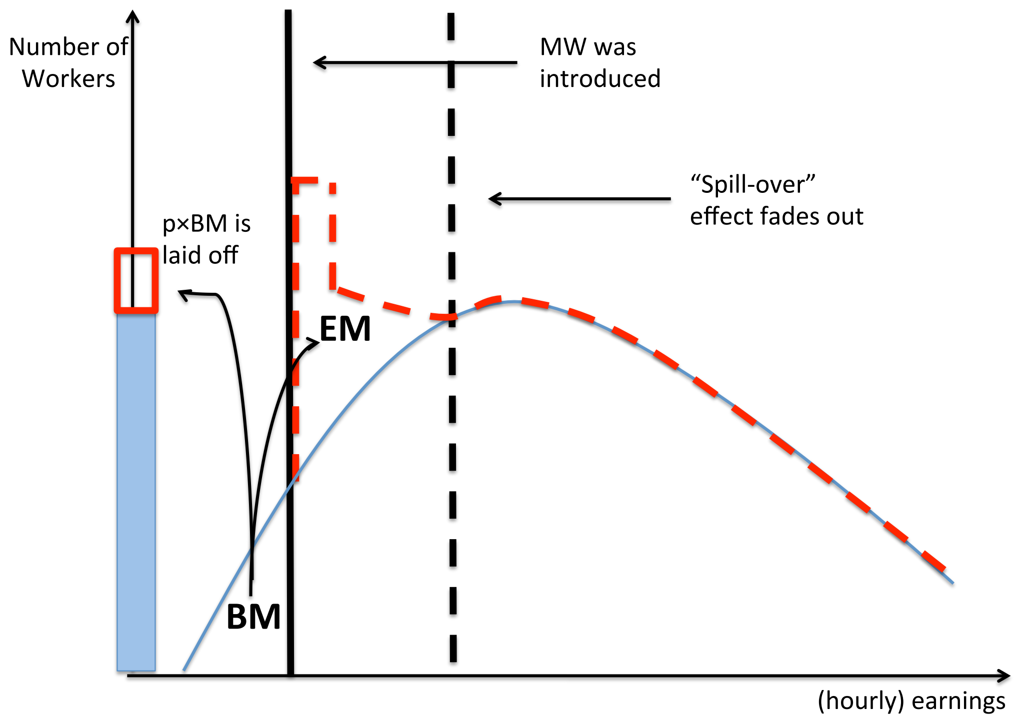
Note: We estimate the parameters of the neoclassical model presented in Section 5 with a minimum-distance estimator. The theoretical moments can be found in Section 5. Column (1) and Column (4) shows the empirical moments in 2002 and in 2003, respectively. Column (1)-(4) show results for all firms, while Column (5)-(8) for small firms.

Figure 1: Minimum wage in Hungary



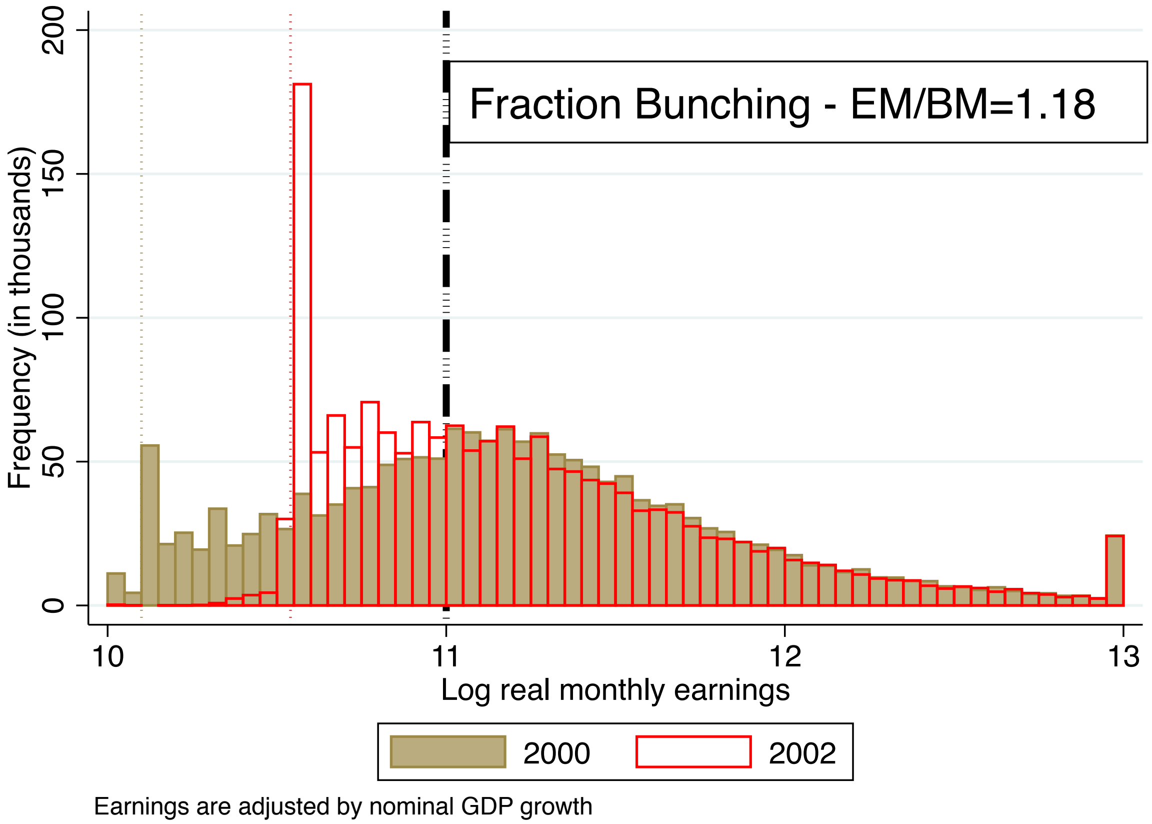
Notes: This figure shows the ratio of the minimum wage to median wage in the private sector for Hungary between 1996 and 2008 (own calculations). The two dashed line depicts the ratio of the minimum wage to the median wage for France and the U.S. in 2012 (OECD). The graph shows the large and permanent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

Figure 2: The effect of the minimum wage on (hourly) earnings



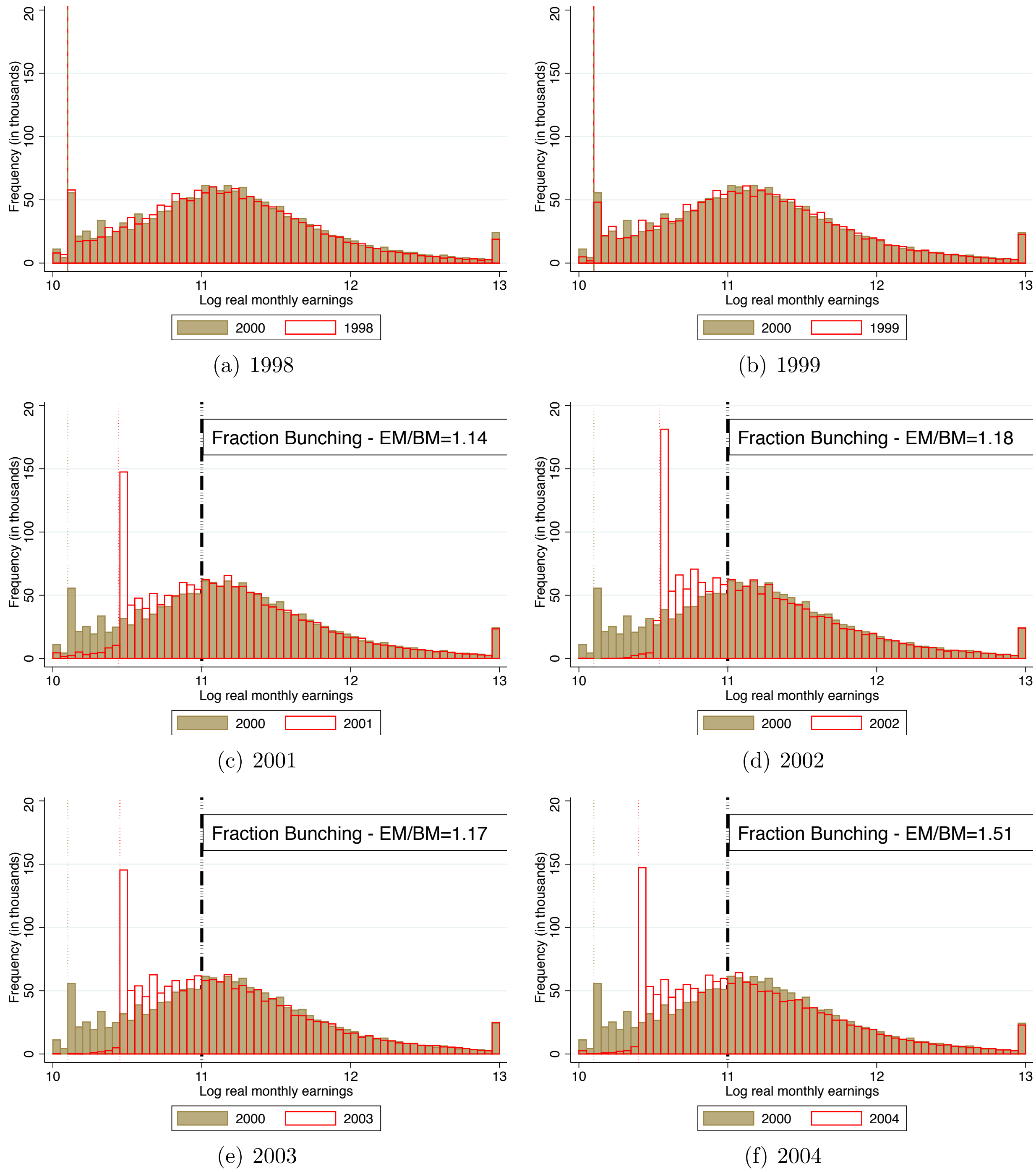
Notes: The effect of the minimum wage on the (frequency) distribution of hourly earnings is depicted here. The blue bar at zero represents workers without a job before the introduction of the minimum wage, while the blue solid line show the earnings distribution. The mass of workers below the minimum wage 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) compare 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 highest pre-reform wage that experiences spillovers from the new minimum is $\bar{W}$. The size of the excess mass (bunching) relative to the below mass (BM in the figure) can be used to estimate the employment effect of the minimum wage.

Figure 3: 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 shows the earning distribution in 2002, while the brown filled bars 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 mass. 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 below the minimum wage mass reported in the top right corner. The estimated fraction is above one indicating that the minimum wage has a positive effect on employment.

Figure 4: Evolution of log earnings distributions over time



(a) 1998

(b) 1999

(c) 2001

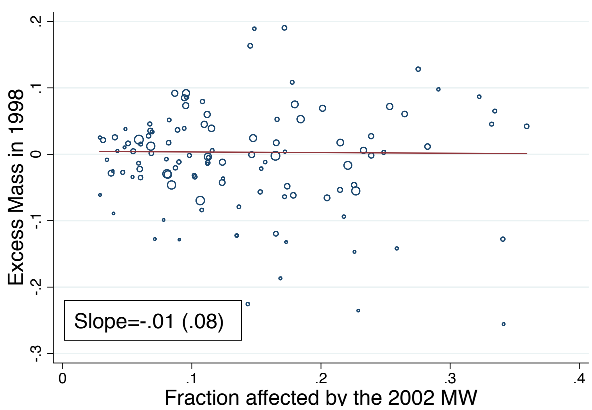
(d) 2002

(e) 2003

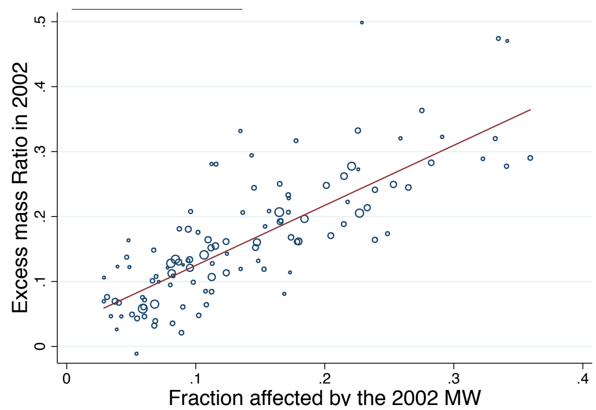
(f) 2004

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 shows the vertical dotted dash black line $\bar{W}$, while the dotted vertical lines (brown in 2000, red in other years) show the bar in where the minimum wage is located in the earnings distribution. As we predicted on Figure 2 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 minimum wage had a positive effect on employment.

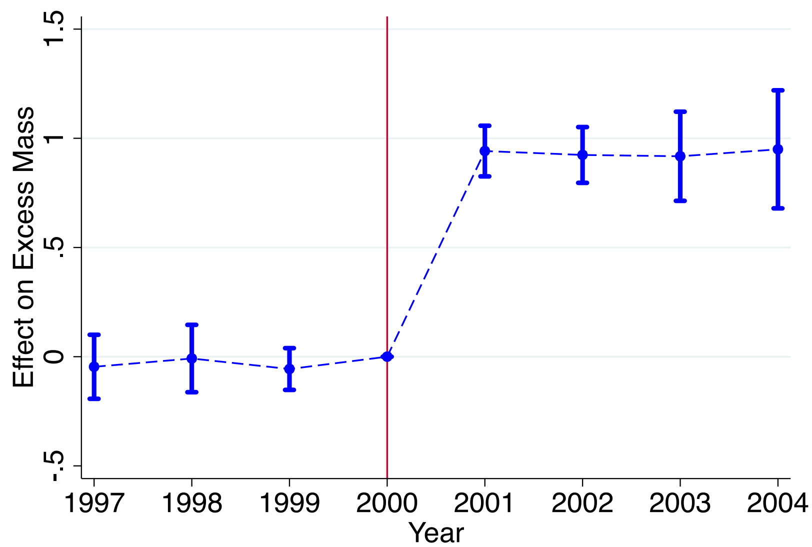
Figure 5: Group-level relationship between excess mass and below mass



(a) Placebo Test



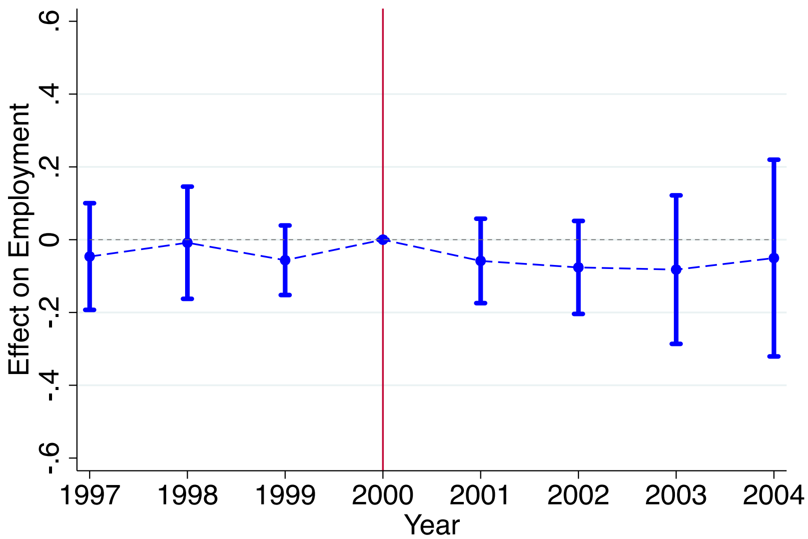
(b) Main Results



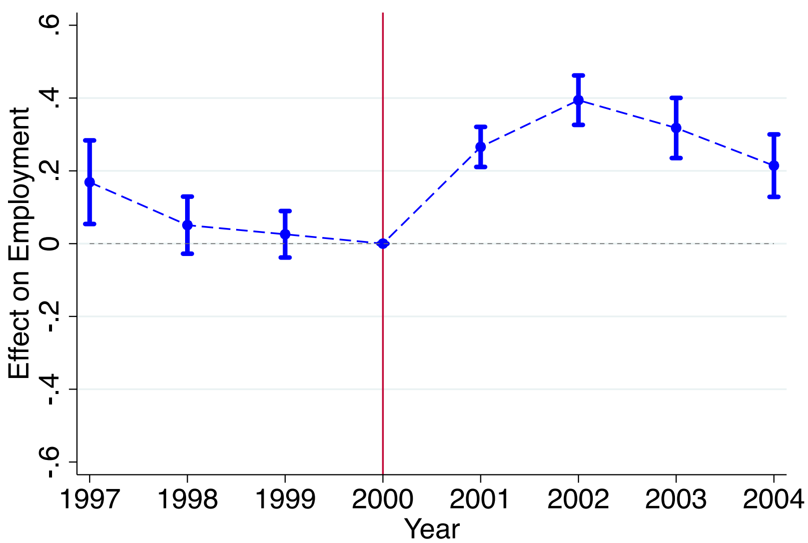
(c) Relationship over time

Notes: This graph depicts the group-level relationship between the excess mass ratio (excess mass divided by employment in 2000) and the below mass ratio in 2002 (below the 2002 minimum wage mass divided by employment in 2000). Panel (a) shows the scatter plot of the group level relationship between excess mass ratio in 1998 and the below mass ratio in 2002. 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 ratio in 2002 and the below mass ratio in 2002. 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 Ratio and the Below Mass Ratio over time. The pre-2000 years show the slope of a regression, where the dependent variable is the Excess Mass Ratio in year t and independent variable is the Below Mass Ratio in 2002. The post-2000 years show the slope of the regression of Excess Mass Ratio in year t on Below Mass Ratio in year t. The graphs demonstrate that an excess mass shows up in the earnings distribution exactly when the minimum wage was raised.

Figure 6: Effect on Employment and on Average Wage



(a) % Change in Employment

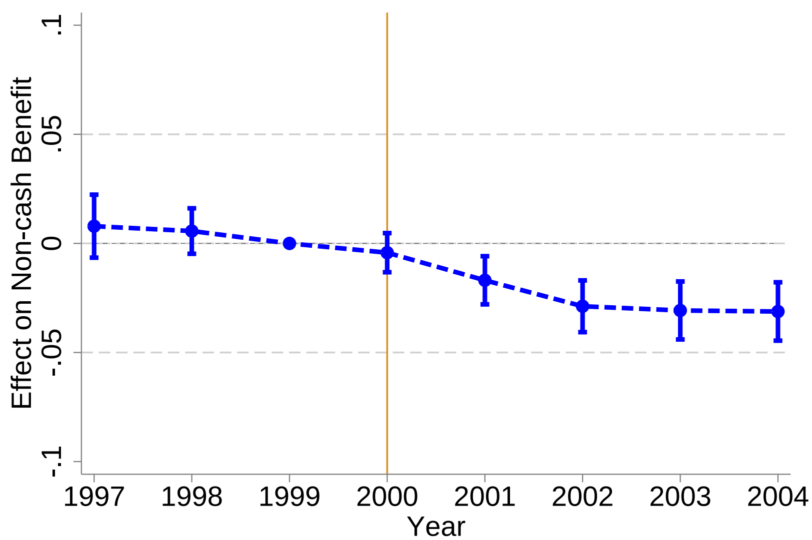


(b) % Change in Average Earnings

Notes: Figure 6 summarizes the main results of our bunching analysis. Panel (a) transforms the results shown in Figure 5 Panel (c) into a percentage change in the jobs affected by the minimum wage hike. For the post minimum wage years (after 2000) we use the formula shown in equation (**??**): the estimated effect on Excess Mass Ratio minus one. For the pre 2000 years we report the relationship between the Excess Mass Ratio and the Below Mass Ratio in 2002. 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 change in average wage and Below Mass Ratio in 2002. In the years leading up to the reform the relationship between change in average wage and Below Mass Ratio in 2002 is plotted. In the post reform years the relationship between change in average wage at year t and Below Mass Ratio in year t is reported. The ratio of Panel (a) and Panel (b) gives us the labor demand elasticity (with respect to wage).

Figure 7: Effect on workers' remuneration

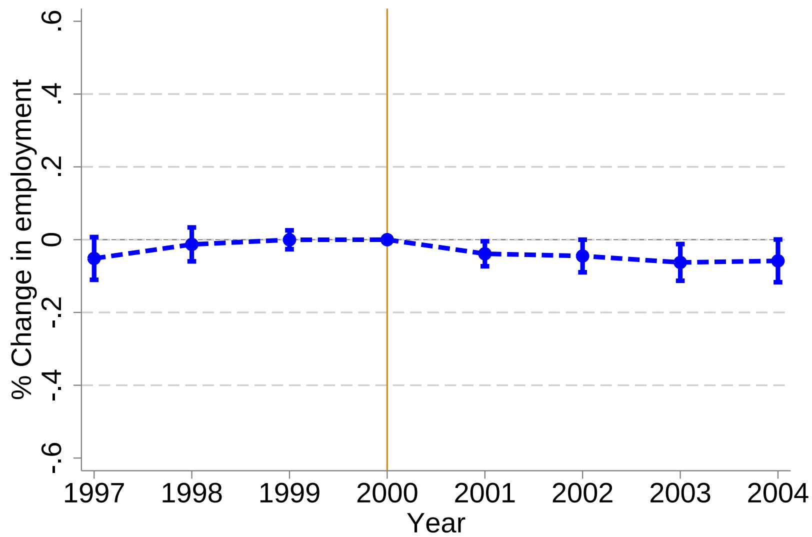
(a) Effect on Average Earnings and Average Labor Cost



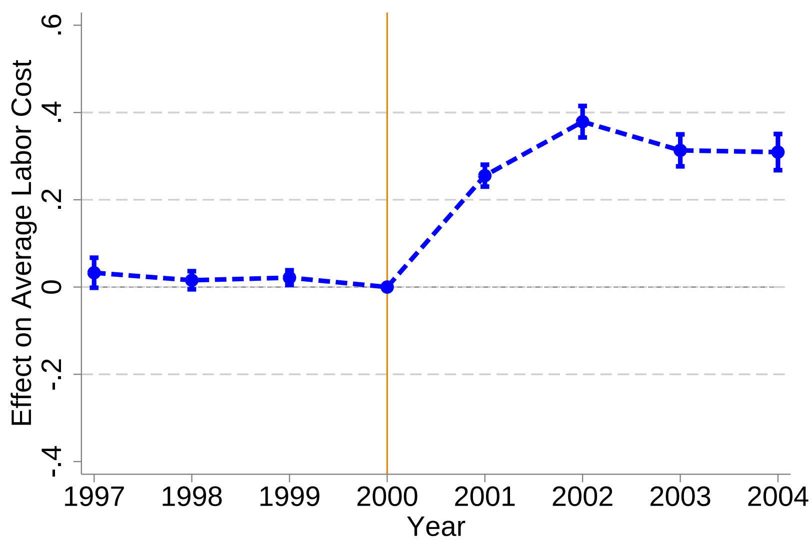
(b) Effect on Non-Cash Benefits

Notes: Figure 7 shows the firm level relationship between fraction affected by the minimum wage and workers' remuneration. Panel (a) shows results from a firm-level regression of percentage change (relative to 2000) in average earnings on fraction affected by the minimum wage (beta coefficients with its confidence intervals from equation (4), blue dotted-dashed line) and from a firm-level regression of percentage change (relative to 2000) in average labor cost on fraction affected by the minimum wage (beta coefficients with its confidence intervals from equation (4), red dashed line). Both lines indicate that average remuneration increased at highly exposed firms after 2000. The average earnings line is above the average labor cost line indicating that the increase in earnings was mitigated by cutting back non-cash benefits. In Panel (b) we test this more directly. Panel (b) depicts the effect of fraction affected on share of non-cash benefits in total labor cost (relative to 2000) over time. We see that the share of non-cash benefits declines after 2000. Controls and industry dummies are included in the regressions.

Figure 8: Effect on Employment and on Average Labor Cost



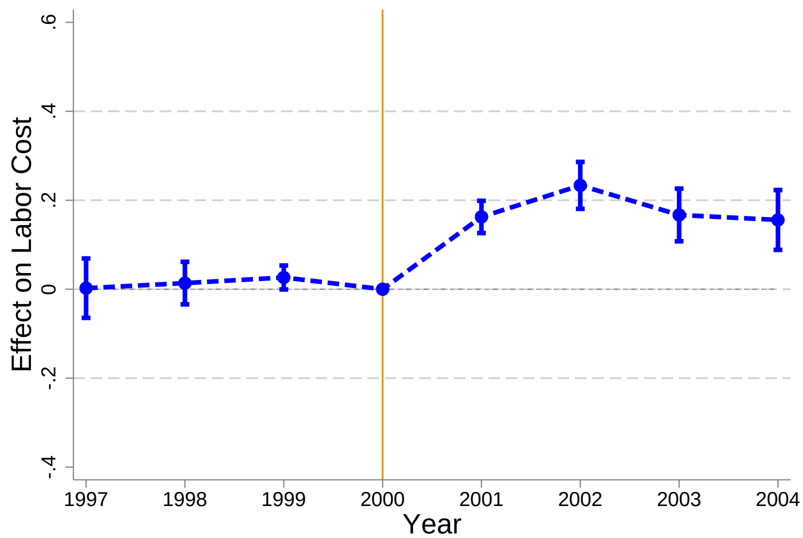
(a) Effect on Employment



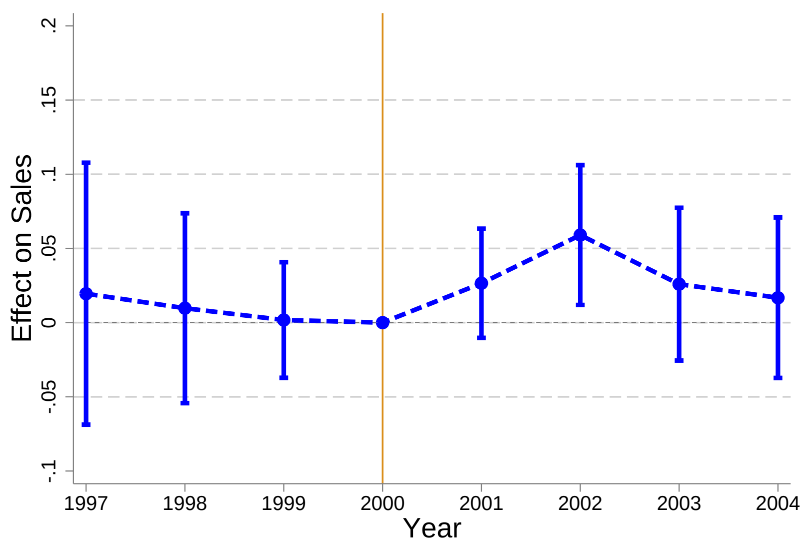
(b) Effect on (Selection-Corrected) Average Labor Cost

Notes: Figure 8 Panel (a) shows the a percentage change in jobs affected by the minimum wage hike at the firm-level. In particular, it depicts results from a firm-level regression of percentage change (relative to 2000) on fraction affected by the minimum wage (beta coefficients with its confidence intervals from equation (4). Panel (b) shows results from a firm-level regression of cumulative growth (relative to 2000) in the selection-corrected average labor cost on fraction affected by the minimum wage (beta coefficients with its confidence intervals from equation (4), see the text for details). Average labor cost is observed only for firms that survived, and so we correct for this selection (see the text for the details). The ratio of Panel (a) and Panel (b) gives us the labor demand elasticity (with respect to labor cost). Both Panel (a) and Panel (b) include firms that died in the regression. Controls and industry dummies are also included in the regressions.

Figure 9: Effect on Total Labor Cost and on Sales



(a) Effect on Total Labor Cost

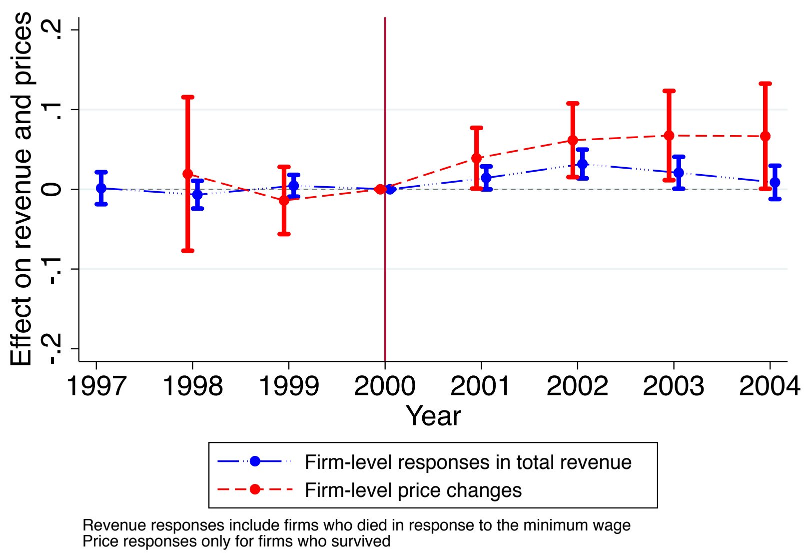


(b) Effect on Sales

Notes: Figure 9 Panel (a) shows results from a firm-level regression of percentage changes (relative to 2000) in total labor cost on fraction affected by the minimum wage (beta coefficients with its confidence intervals from equation 4). It is clear that firm-level expenses increased substantially at highly exposed firms after the minimum wage hike. Panel (b) shows results from a firm-level regression of percentage changes (relative to 2000) in Sales on fraction affected by the minimum wage (beta coefficients with its confidence intervals from equation 4). Both Panel (a) and Panel (b) include firms that died in the regression. Controls and industry dummies are also included in the regressions.

Figure 10: Effect on Prices

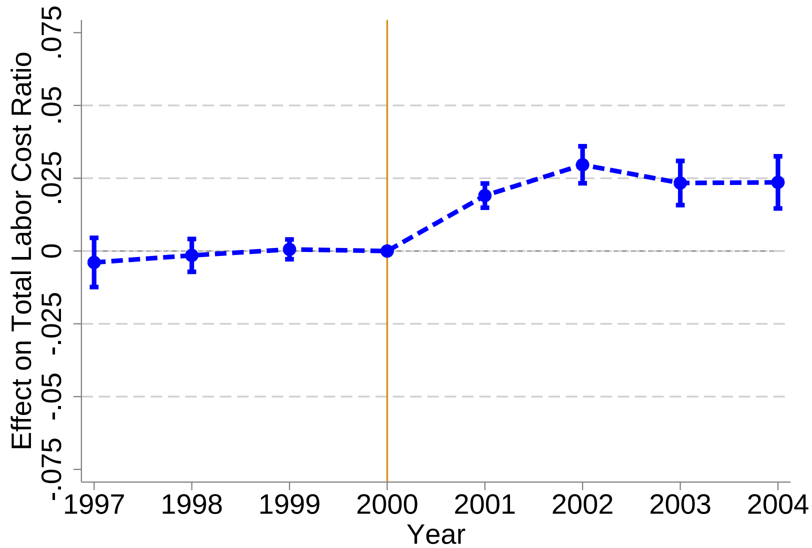
(a) Effect on Firm-Level Prices



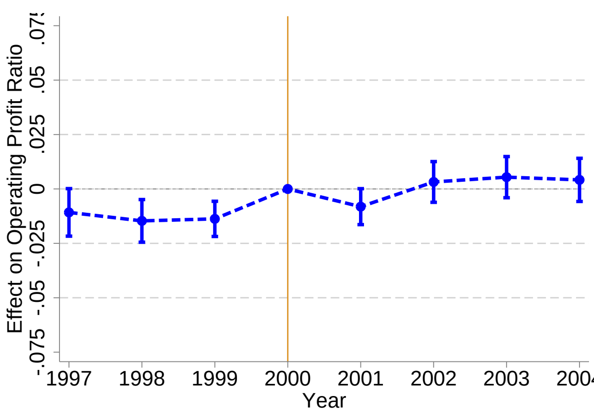
(b) Effect on Prices and Sales

Notes: Figure 10 Panel (a) shows results from a firm-level regression of percentage changes (relative to 2000) in prices on fraction affected by the minimum wage (beta coefficients with its confidence intervals from equation 4). Since price data is only available in the manufacturing sector, we restrict our analysis to that sector. The graph clearly indicates that firm-level prices increased more at highly exposed firms after the minimum wage hike. Panel (b) shows the firm-level price changes and the changes in sales (total revenue) to make the comparison easier. Both Panel (a) and Panel (b) include firms that died in the regression. Controls and industry dummies are also included in the regressions.

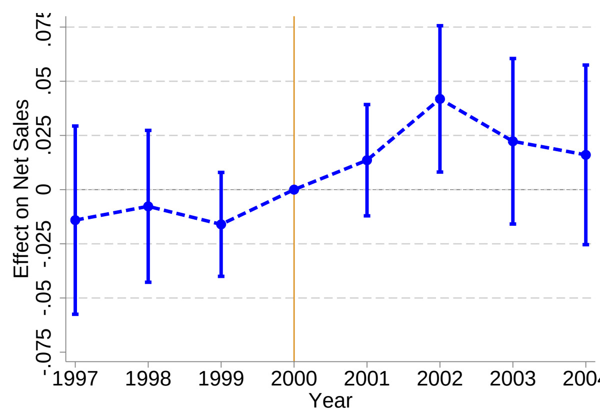
Figure 11: Effect on Profit Margin. Total Labor Cost Ratio and Sales Ratio



(a) Effect on Total Labor Cost Ratio



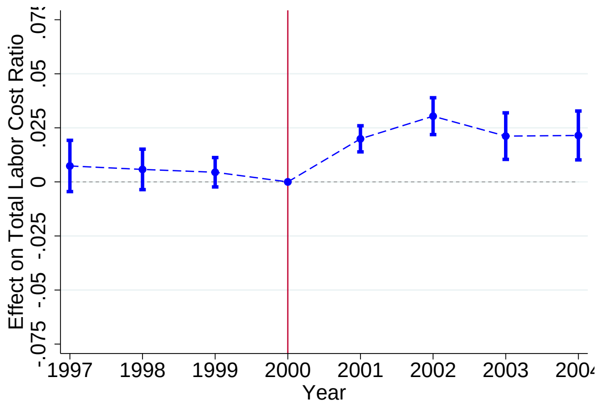
(b) Effect on Operating Profit Ratio



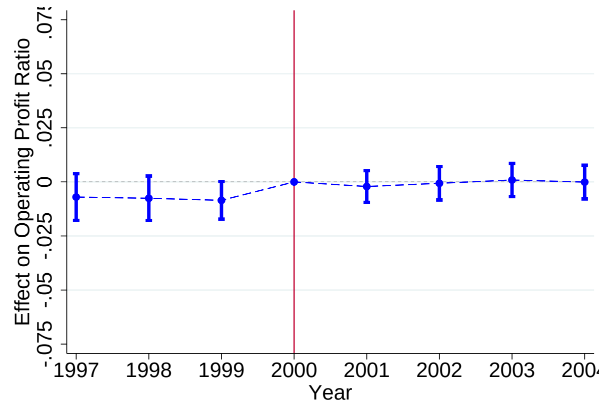
(c) Effect on Sales Ratio

Notes: Figure 11 Panel (a) shows results from a firm-level regression of changes (relative to 2000) in total labor cost ratio on fraction affected by the minimum wage (beta coefficients with its confidence intervals from equation (**??**)). Total labor cost ratio is labor cost divided by the average sales between 1997 to 2000. The graph shows that firm-level expenses increased substantially at highly exposed firms after the minimum wage hike. Panel (b) depicts results from a firm-level regression of changes (relative to 2000) in (operating) profit ratio on fraction affected by the minimum wage (beta coefficients with its confidence intervals from equation (**??**)). Operating profit ratio is operating profit (EBIT) divided by the average sales between 1997 to 2000. The graph shows that profits are not affected by the minimum wage hike. Panel (c) show results from a firm-level regression of changes (relative to 2000) in Sales (beta coefficients with its confidence intervals from equation (**??**)). The graph shows that profits are not affected by the minimum wage hike. Both Panel (a) and Panel (b) include firms that died in the regression. Controls and industry dummies are also included in the regressions.

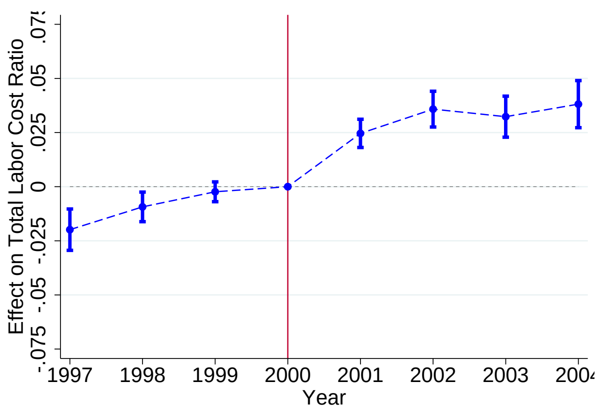
Figure 12: Effect on Profits by Profitability Before the Minimum Wage Hike



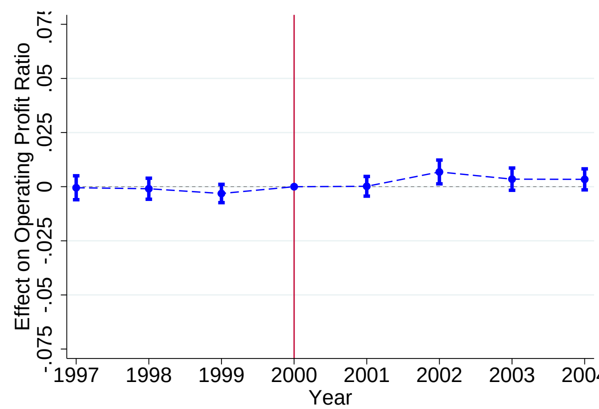
(a) High Profit Before 2000, Labor Cost Ratio



(b) High Profit Before 2000, Profit Ratio



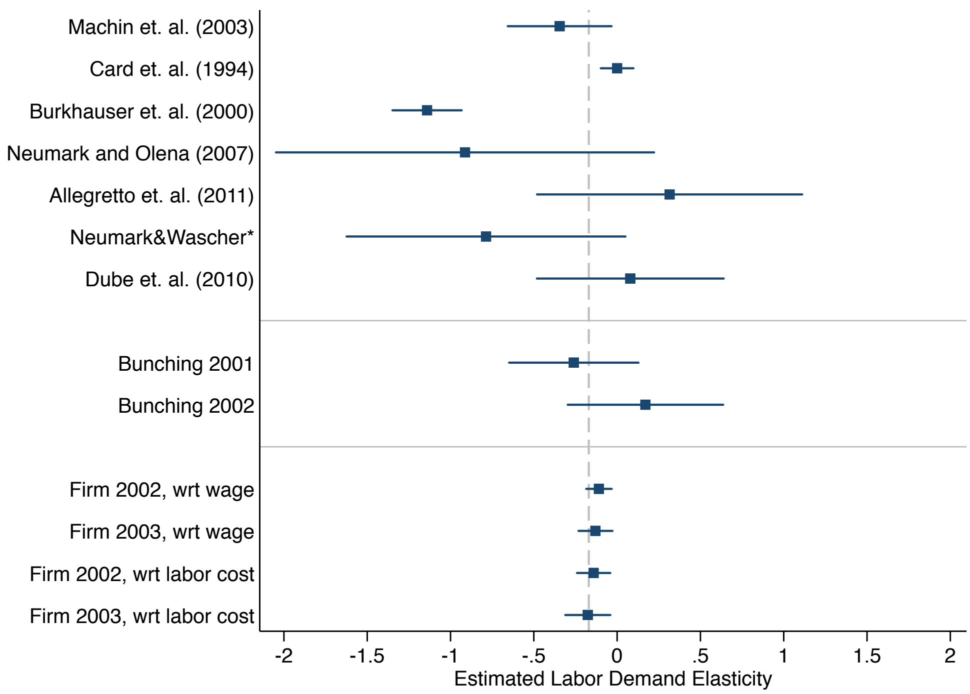
(c) Low Profit Before 2000, Labor Cost Ratio



(d) Low Profit Before 2000, Profit Ratio

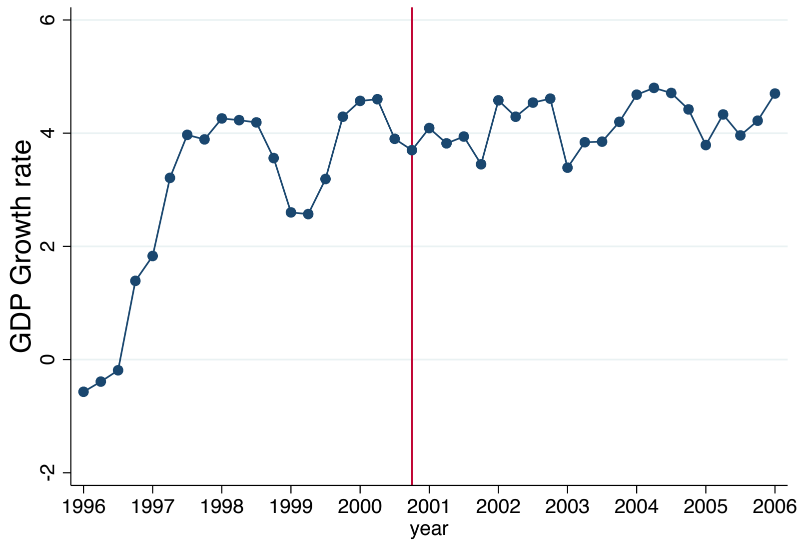
Notes: Figure 12 shows the results presented on Figure 11 by profitability before the minimum wage hike. High profitability firms have above median average profit margin (profit over sales) between 1997 and 2000. The average profit margin in the high profitability group is 7.2%, while in the low profitability group it is 0.5%. Results for firms with high profitability are displayed in Panel (a) and Panel (b). The effect of fraction affected by the minimum wage on labor cost ratio is depicted in Panel (a) and for profit ratio in Panel (b). Panel (a) indicates that total labor cost at highly exposed firms with sizable profits increased substantially as a result of the minimum wage hike. Panel (b) shows that profits at these firms did not decline in response to the minimum wage. This indicates that the effects of the minimum wage were passed on to the consumers even at firms with sizable profits. Results for firms with low profitability are displayed at Panel (c) and Panel (d). The effect of fraction affected by the minimum wage on labor cost ratio is depicted in Panel (c) and for profit ratio in Panel (d). All Panels include firms that died in the regression. Controls and industry dummies are also included in the regressions.

Figure 13: Labor Demand Elasticity in the literature and in this paper

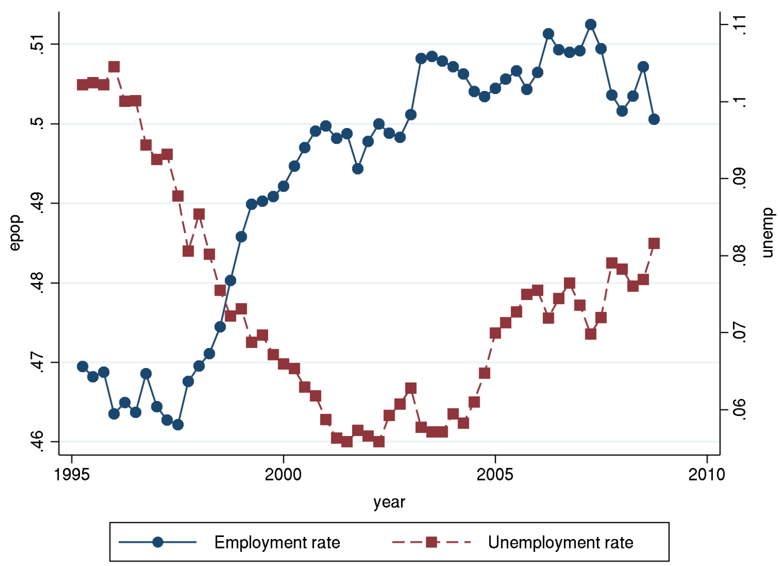


Notes: This figure summarizes the minimum wage implied labor demand elasticities estimated in the literature and the ones estimated in this paper. The dashed vertical line show our preferred estimates for the labor demand elasticity, which is -0.2. Neumark and Wascher* is Dube et. al. (2010) replications. In cases where labor demand elasticity was not directly reported we used the delta method to obtain the standard errors (see the details in Appendix 1).

Figure A-1: Macroeconomic Trends



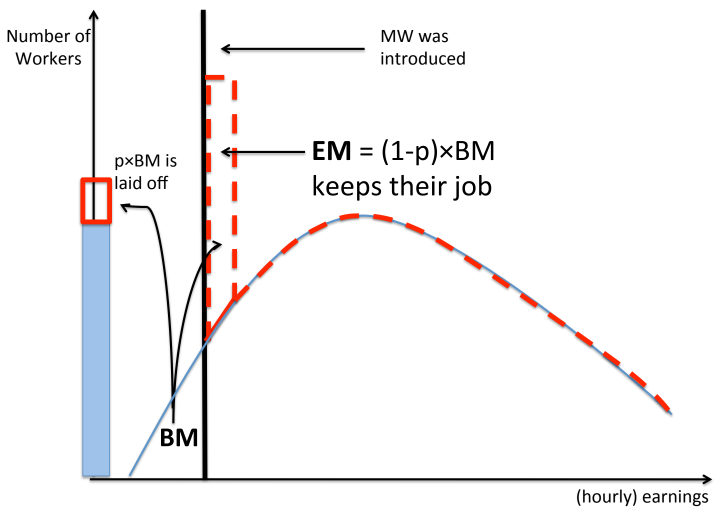
(a) GDP growth



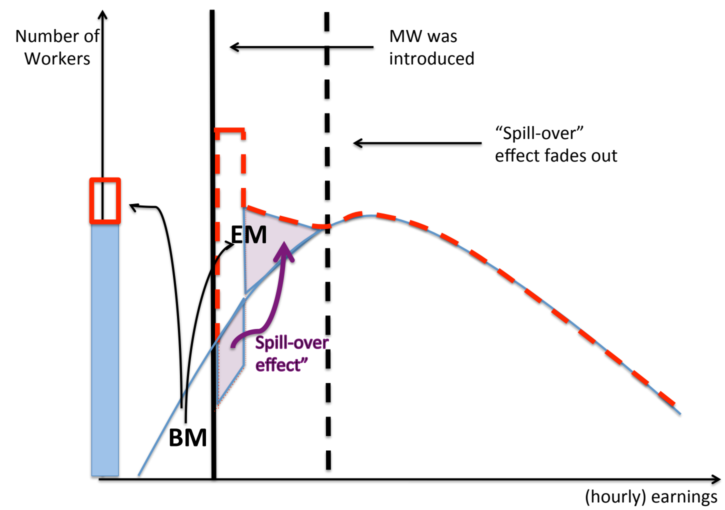
(b) Labor market trends

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 major (red) vertical line indicate the 4th quarter in 2000, the last quarter before the minimum wage hike. The graph shows that the GDP growth was stable around the examined period. Panel (b) shows the evolution of employment to population rate and the unemployment rate between 1996 and 2006 in Hungary. There are trends in employment-to-population and unemployment rate before the reform and so it is hard to use the aggregate data for inference.

Figure A-2: The effect of the minimum wage on (hourly) earnings



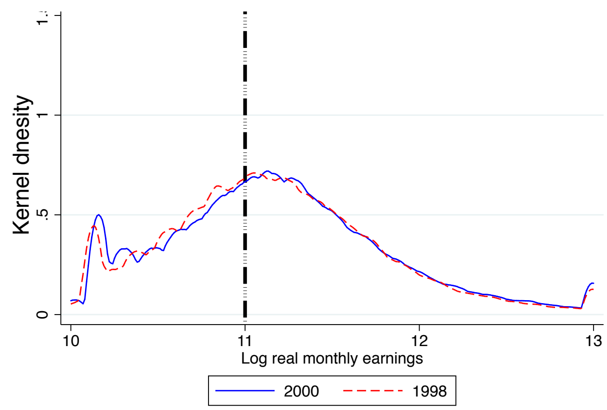
(a) No spillover effects



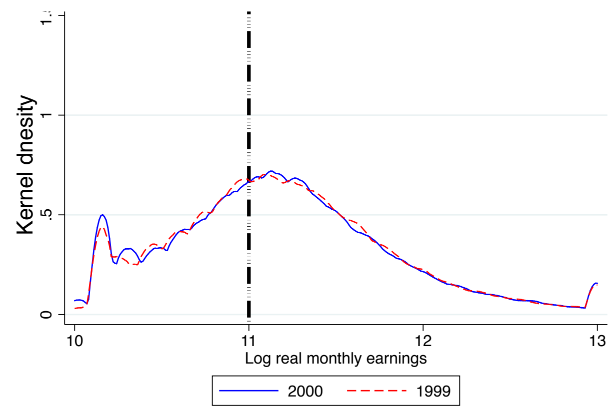
(b) With spill-over effects

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 a job before the introduction of the minimum wage, while the blue solid line shows the earnings distribution. Panel (a) shows the effect in the absence of any spillover effects on wages, while Panel (b) shows the effects with spillover. The introduction of the minimum wage can affect workers earning below the minimum wage in two ways: they get laid off or they get a pay raise. Workers getting the pay raise generate a spike in the earnings distribution. Comparing the size of this spike to the below minimum wage mass can be used to estimate the employment effect of the minimum wage as it is shown in Panel (a). However, if the minimum wage raises the wages of workers who initially earned slightly above the new minimum wage (spillover effect), then the spike at the minimum wage underestimate the true employment effects. This situation is shown at Panel (b). The purple arrow shows that the spillover effect decreases the density at the minimum wage, but increases the density above the minimum wage. This lowers the size of the spike, but creates an excess mass above the minimum wage. Therefore, to calculate the disemployment effects of the minimum wage we need to use the excess mass at and above the minimum wage.

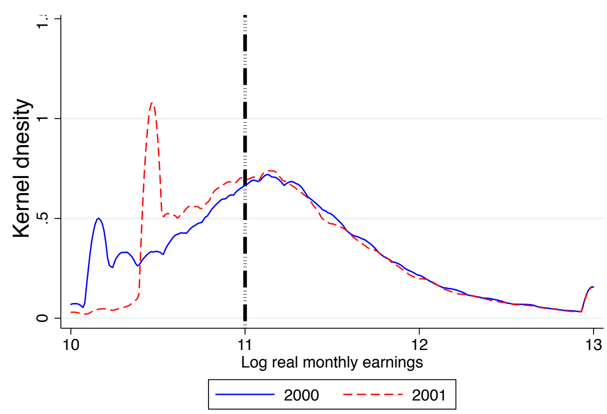
Figure A-3: Evolution of kernel densities over time



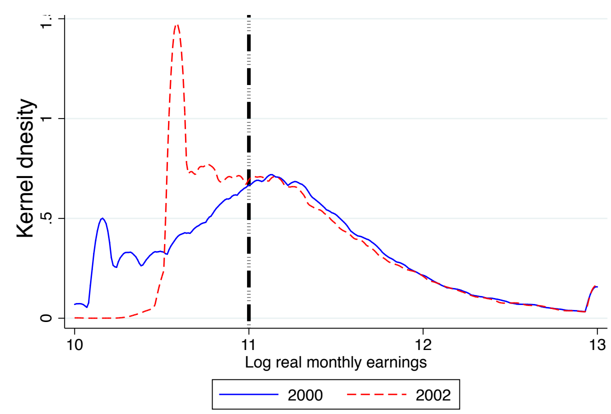
(a) 1998



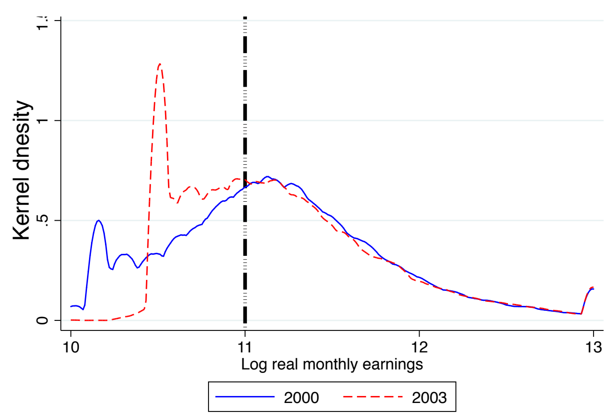
(b) 1999



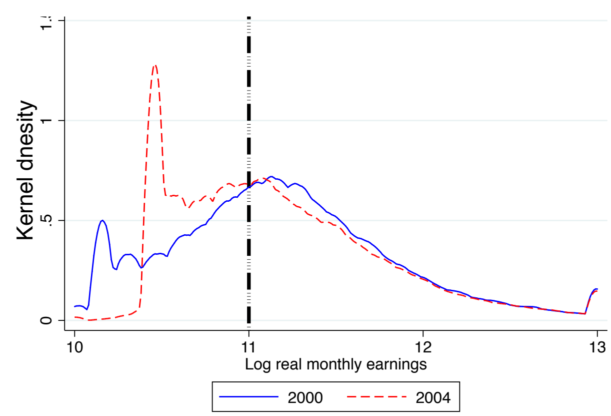
(c) 2001



(d) 2002



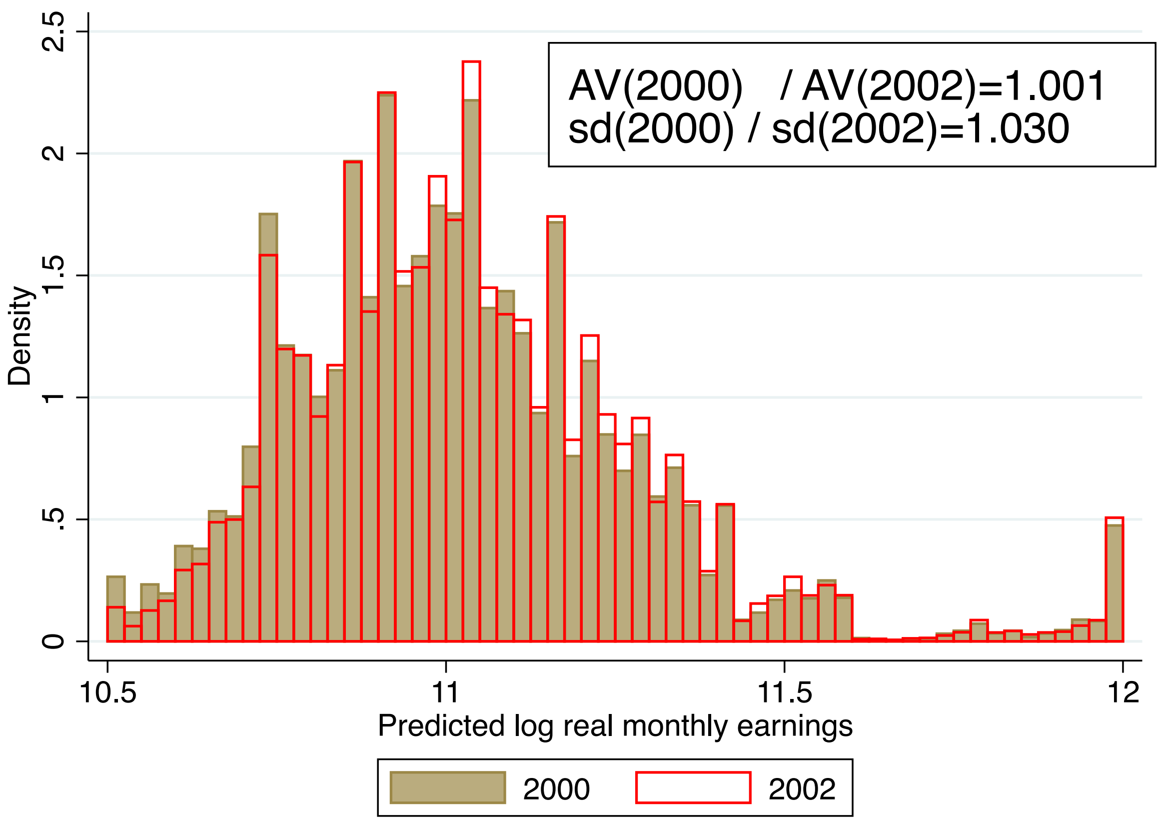
(e) 2003



(f) 2004

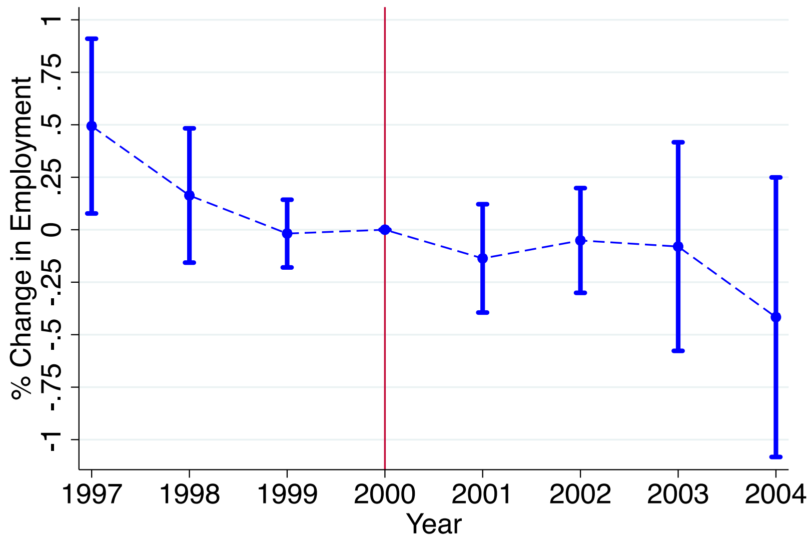
Notes: The kernel density of monthly log earnings over time are shown between 1998 and 2004 (red dashed line) relative to 2000 (blue line). The vertical dotted dash black line show $\bar{W}$.

Figure A-4: Predicted Earnings Distribution in 2002 and 2000

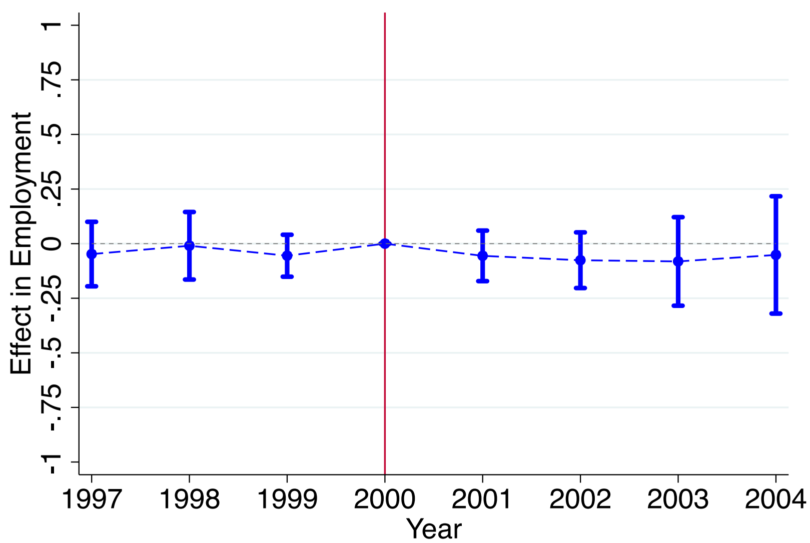


Notes: This figure shows the (density) earning distributions predicted by observables (age, age square, sex, education, region) in 2000 (brown solid bars) and in 2002 (red solid bars) for jobs that earned less than $\bar{W}$. In both years we use the relationship between observables and the earnings in 2000. The differences between the 2002 predicted value and the 2000 predicted value uncovers the effect of changes in observables on the earnings distribution. The ratio of means (first line) and the standard deviation (second line) between 2002 and 2000 is 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 A-5: Comparison of main specification and the standard approach



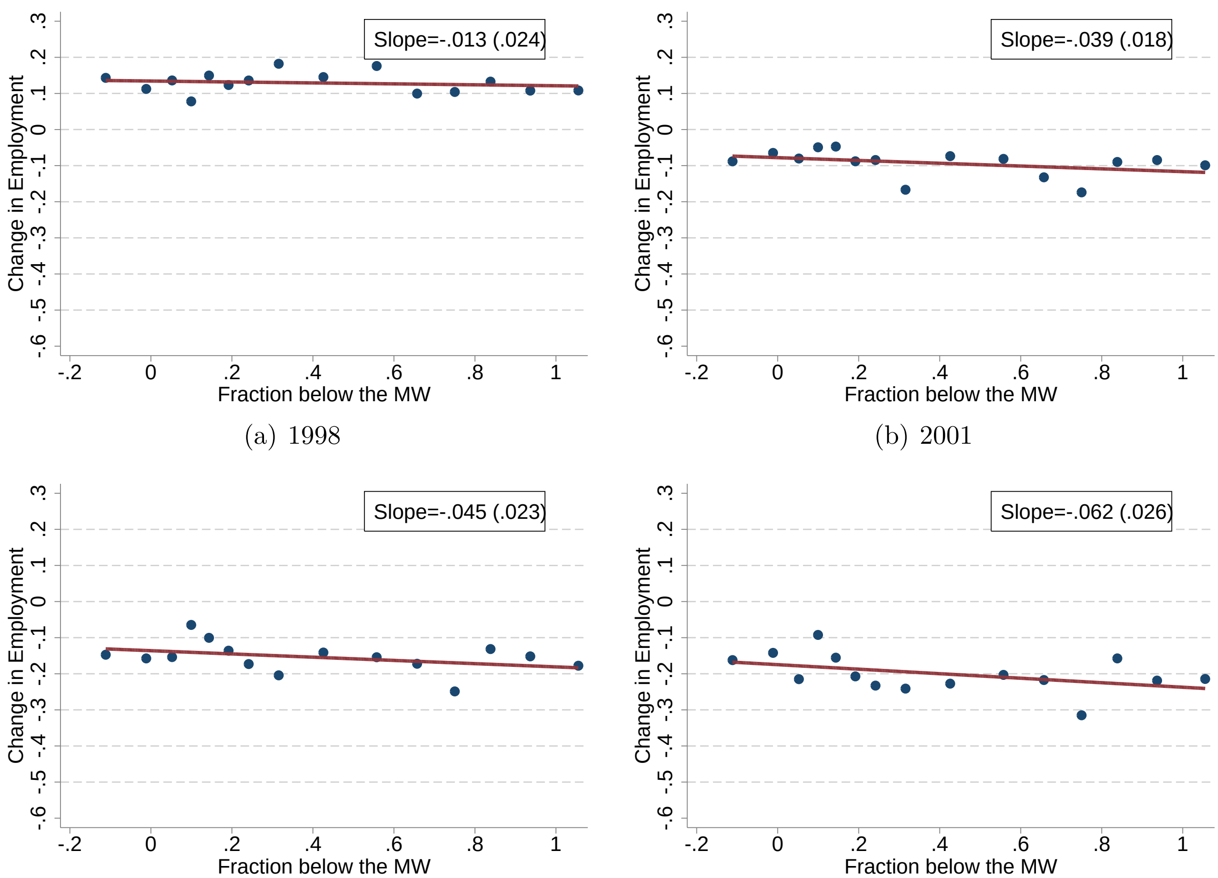
(a) Standard Approach, $\bar{W} = \infty$



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

Notes: Panel (a) shows the effect of the minimum wage, with $\bar{W}$ set to infinity. As we described in the text, this specification is equivalent to estimating the group-level relationship between 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 (5) Panel (a) for the details). This specification is equivalent to estimating the relationship between fraction 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 it is shown in Panel (b).

Figure A-6: Effect on Employment over time



(a) 1998

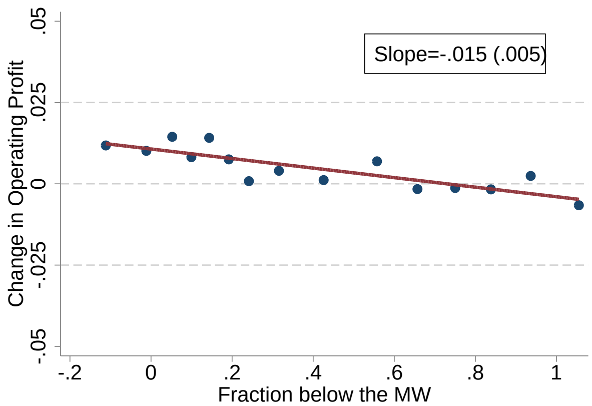
(b) 2001

(c) 2002

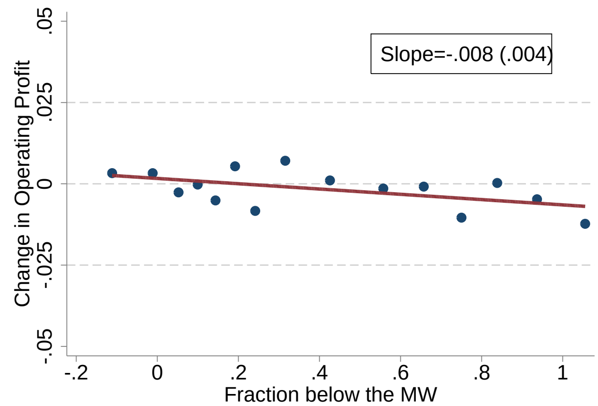
(d) 2003

Notes: These figures present non-parametric binned scatter plots of the relationship between fraction below the 2002 minimum wage and the cumulative growth relative to 2000. This is the non-parametric version of equation (4). The red solid line is the linear fit, while the slope of this fit reported in the top right corner.

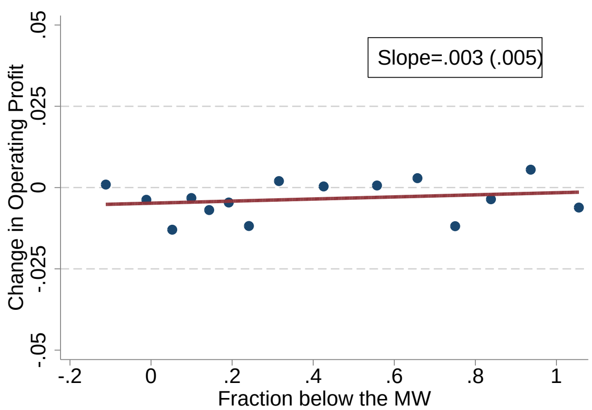
Figure A-7: Effect on Profit over time



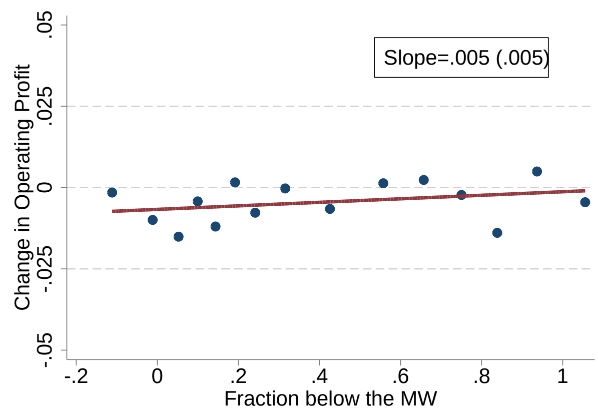
(a) 1998



(b) 2001



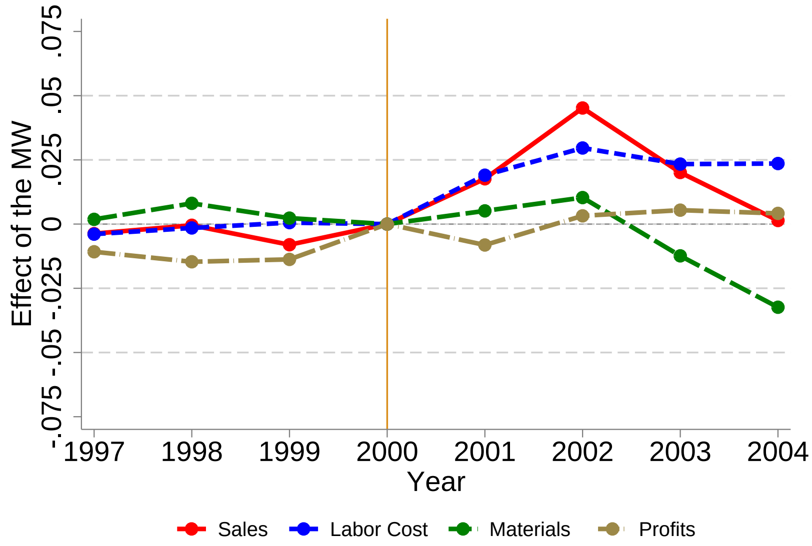
(c) 2002



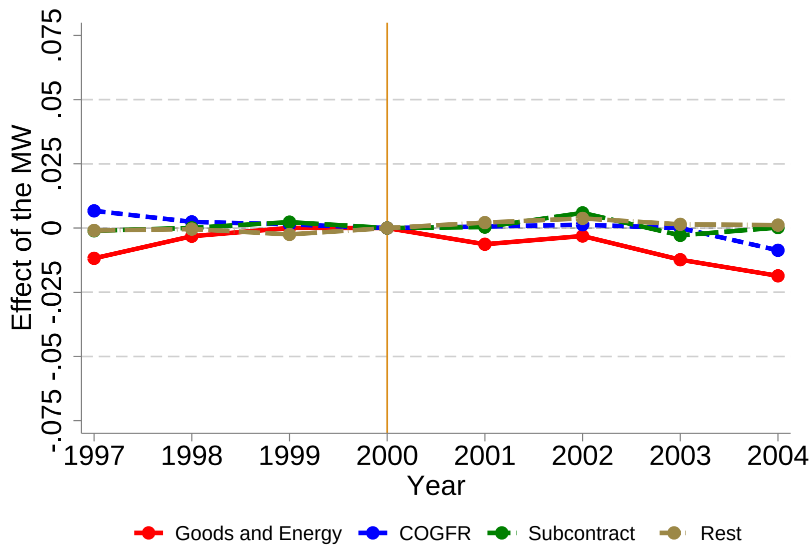
(d) 2003

Notes: These figures present the non-parametric binned scatter plots of the relationship between fraction below the 2002 minimum wage and the change in operating profits ratio (operating profits dividid by the average sales between 1997 and 2000). This is the non-parametric version of equation (**??**). The red solid line is the linear fit, while the slope of this fit reported in the top right corner.

Figure A-8: Effect on the Main Outcomes



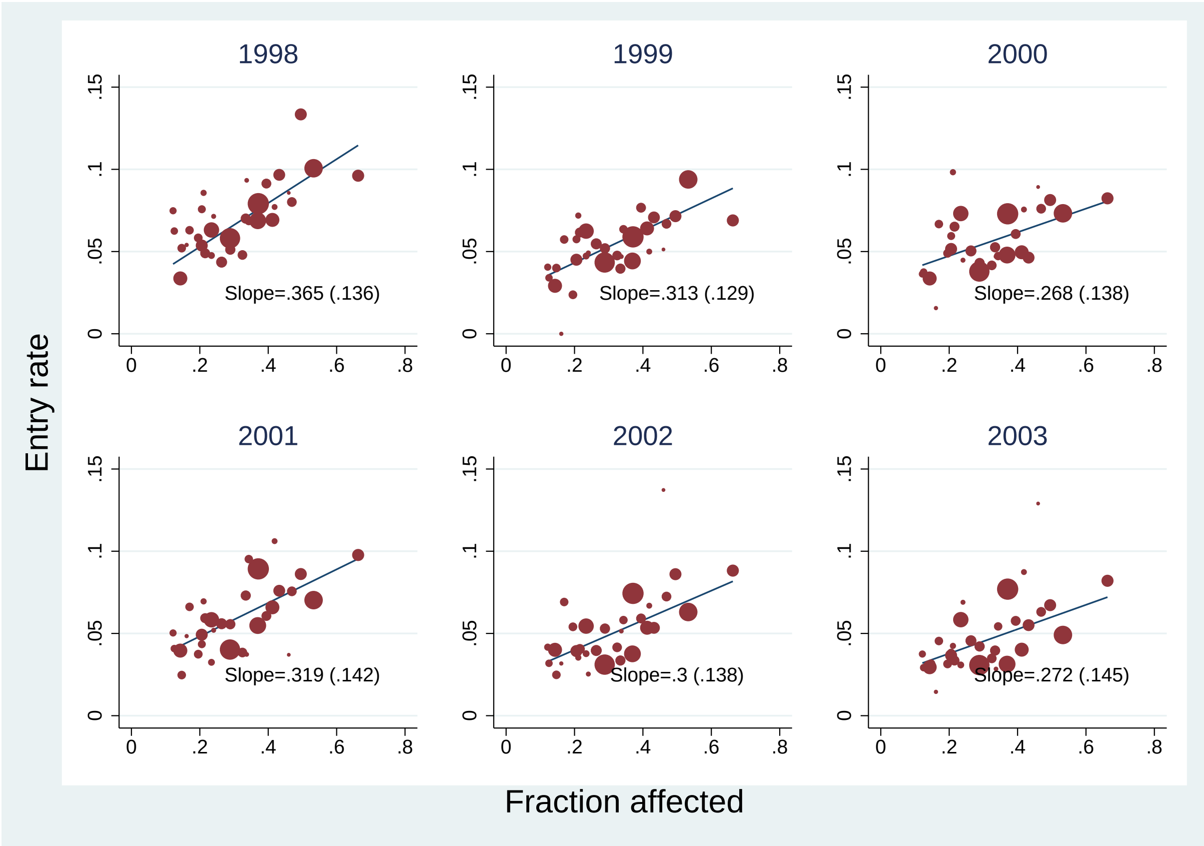
(a) Sales



(b) Cost of Goods Purchased for Resale

Notes: Panel (a) summarizes the estimated effects on our key outcomes. Panel (b) shows the estimated effects on the different parts of material expenses.

Figure A-9: Effect on Firms Entry



Notes: This figure shows the relationship between exposure to the minimum wage and firms entry at two digit industry level. Each scatterplot relates the share of new firms in a two-digit industry to the fraction of affected workers in that sector. In each graph the fitted regression line is the outcome from a corresponding OLS weighted by the number if 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.