Who Pays for the Minimum Wage?*

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Job Market Paper

January 2015

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 at most a small negative effect on employment, and so the main 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. Contrary to theoretical models that attribute the small employment effects of minimum wage changes to monopsonistic wage setting, we find no evidence that the rise in the minimum wage led to lower profitability among low-wage employers. Instead, we find that the costs of the minimum wage were largely passed through to consumers.

*We are extremely grateful for David Card, Patrick Kline and Emmanuel Saez for their continuous guidance throughout the project. We would like to thank Alan Auerbach, Tamás Bátyi, Katalin Bodnár, Stefano DellaVigna, Hedvig Horváth, Hilary Hoynes, János Köllő, Álmos Telegdy, Gábor Kézdi, Gábor Kőrösi, John Mondragon, Carl Nadler, Michael Reich, Balázs Reizer, Ana Rocca, Jesse Rothstein, David Silver, Ferenc Szűcs, Chris Walters, Danny Yagan, Owen Zidar for very helpful comments. Financial support from the Center for Equitable Growth at UC Berkeley is gratefully acknowledged. All errors are our own.

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1 Introduction

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

In this paper we present new evidence on the distributional impacts of a higher minimum wage, based on worker- and firm-level evidence 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. This large step-like increase in the minimum wage makes it possible to implement and test a variety of relatively credible difference-in-difference style estimators. Moreover, the apparent permanence of the new higher level allows us to address the concern that many of the minimum wage increases analyzed in the recent labor economics literature are only temporary (Sorkin, 2013).[1]

[1] Reynolds and Gregory (1965) and Castillo and Freeman (1990) study the impacts of imposing the US

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

A limitation of this simple analysis is that other factors may have affected aggregate labor demand in Hungary in 2001. While our reading of the evidence is that this is unlikely to be the case, we go on to implement a grouped version of the bunching estimator which exploits differences in the fraction of workers who were earning less than the new minimum wage across demographic groups and regions, and allows us to control for aggregate trends.[3] Again, we estimate that the group-level excess mass (bunching) generated by the minimum wage is very close to the mass of workers earning below the new minimum wage. The implied estimates of the labor demand elasticity facing the group who were previously earning less than the new minimum wage can rule out elasticities larger than -0.4 from our various specifications.

The result that the minimum wage increase pushed up wages with at most small employment effects raise the question: who paid for the wage increase? In the second part of the

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.

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

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

paper, we scrutinize firm-level behavior to understand the incidence of the minimum wage. Combining the employer-employee data with a longitudinal firm-level corporate tax dataset we calculate firm level exposure to the minimum wage, measured by the fraction of workers paid below the new minimum wage prior to the reform. We then estimate how outcomes (such as employment, payroll, profits, etc.) vary with this exposure variable.

First, we show that the minimum wage had a large positive effect on the labor costs of the firms. However, the firm-level estimates on labor costs are slightly lower than the increase implied by our bunching estimates. The gap is due to the fact that some firms cut other forms of labor costs (e.g., subsidized meals) in response to the wage increase. We calculate that the increase in workers' earnings is mitigated by about 20% through cuts to non-cash benefits. Second, we show that accounting profit did not decline in response to the minimum wage increase. Instead, we found that our sales measure did increase substantially as a result of the minimum wage hike indicating that firms passed through the effect of the minimum wage to consumers. Finally, we look at whether firm responses vary by firm-level characteristics. Our most interesting finding is that firms operating in elastic product demand markets (exporting and manufacturing) suffered much bigger losses in employment than firms in more inelastic product demand (non-exporting and service) markets.

Our findings are hard to reconcile with many models in the literature that attempt to explain close to zero employment effects in the literature. First, a monopsonistic model of the labor market (Manning 2003, Card and Krueger 1995) and the search and matching model proposed by Flinn (2010) could explain a small negative effect on employment in response to a large change in the minimum wage, but they also predict a large negative effect on profits. But we find no effect on profits. Second, an efficiency wage model of the labor market, e.g. Rebitzer and Taylor (1995), would predict that disemployment effects are small in markets with elastic product market and large in markets with inelastic demand.[4] But we find that

[4]Rebitzer and Taylor's (1995) model argues that the minimum wage can raise workers' unobserved effort and so production. If the product demand is elastic, the output market can absorb the newly produced good without deteriorating prices. Therefore, the production expansion will increase sales, which could be used to pay for the minimum wage. However, if the product demand is inelastic, the production increase can decrease market prices so that both firm owners and workers are worse off.

markets with elastic product demand suffer the most from the minimum wage increase.

Instead, we show that our results are consistent with a standard neoclassical model with competitive firms and four inputs (capital, labor, intermediate goods and goods purchased for resale). 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 the range of the previous the literature that goes from 0.36 in Chirinko et. al (2011) to 1.25 in Karabarbounis and Neiman (2014). Moreover, we find that our estimates imply a product demand elasticity of 0.24 (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 .

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

Our paper relates to several branches of the minimum wage literature. The first part of the paper relates to the literature on inequality and minimum wage. Many papers demonstrated that the minimum wage has a substantial effect at the bottom of the wage distribution

(DiNardo, Fortin and Lemieux 1996, Lee 1999, Autor, Manning and Smith 2014). We go further here, and use the change in the distribution to identify the employment effects of the minimum wage. Meyer and Wise (1983) were the first to propose this idea. However, their implementation was criticized by Card and Krueger (1995) and Dickens et al. (1998), since their results strongly relied on the functional form assumptions they made. We extend Meyer and Wise (1983) in two important ways that address these criticisms. First, we use wage distributions from before and after the minimum wage increase to provide a more credible counter-factual for the shape of the wage distribution. Second, we use the actual *number* of workers to calculate the excess mass (bunching) rather than the fraction of workers, so we explicitly account for lost employment arising from the imposition of the minimum wage.

The second part of the paper is related 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.

The papers proceeds as follows. In Section 2, we describe the institutional context of the minimum wage raise in Hungary. In Section 3 we estimate the effects on workers using bunching evidence. In Section 4 we scrutinize firm-level responses to the minimum wage hike. In Section 5 we present a simple model that can explain our findings, and we conclude in Section 6.

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.

The right-wing government announced on April 6th, 2000 that they would raise the minimum wage from 25,500 HUF to 40,000 HUF on January 2001. The main reasons mentioned by government officials were to alleviate income differences, to raise government revenue and to diminish tax evasion (Cserpes and Papp 2008). The government (including the prime minister) also pledged to increase further the minimum wage in 2002. Keeping their promise, a year later, the minimum wage was raised to 50,000 HUFs. In 2002 the right-wing party lost the election and the new socialist government decided to keep the minimum wage at 50,000 HUF.[6] However, they exempted the minimum wage from personal income taxes to appeal to their working class voters. Figure 1 summarizes the evolution of the minimum wage in relation to the median wage in the private sector between 1996 and 2008.[7]

The economy around the minimum wage hike was stable. The Appendix Figure A-1 Panel (a) depicts the evolution of real GDP growth, which was 4-5% around 2000. The employment-to-population ratio and the unemployment rate over time is shown in Panel (b). The growth in the employment-to-population ratio slowed down after 2001 and the fall in unemployment

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

[7] Public sector wages were raised by 50% between 2001 and 2003. This increased the median earnings in the public sector, while it has only a small spillover effect on the private sector (Telegdy 2014). Looking at the minimum wage to median wage in the private sector captures this spillover effect, but helps us abstracting away from the salary increase in the government sector.

rate stopped, albeit at very low level, around 2001. The presence of pre-trends in the key labor market variables makes it difficult to draw any inference from aggregate data about the effect of the minimum wage increase. For our analysis, we rely on disaggregated data sources to cleanly identify the effects of the minimum wage increase, net of any aggregate trends.[8]

Tax evasion is not uncommon in Hungary. There are two basic forms of evading taxes: unreported employment and under-reporting of earnings (grey employment). Unreported employment is estimated to be 16-17% of the total employment between 2001 and 2005 (Elek, Köllő, Reizer and Szabó 2011). The minimum wage increase might push some formal workers to the informal sector. In datasets where we cannot observe informal employment (e.g. in the corporate income tax data), we will over-estimate the true effect on (total) employment. Given that we find a very small effects on employment this channel cannot be large. The second channel for avoiding taxes is under-reporting earnings. Tonin (2006) and Elek et. al. (2011) shows that some workers reporting earnings at the minimum receives some of their earnings under the table. Throughout the paper we do many robustness checks to show that our results are not contaminated by tax evasion. In particular, our results hold for looking at only large firms and firms paying substantial amount of corporate taxes.

2.2 Data

We use two main data sources. The Wage Survey (WS) of the National Employment Service is a linked employer-employee data set comprising observations on over 100,000 individuals in about 8,000 private businesses employing at least 5 workers.[9] Firms between 5-20 workers are randomly selected from the census of enterprises. Individual data on each employee working at the firm as of May 31st are reported for these firms. Larger firms, employing more than 20 workers, are supposed to report data for the Wage Survey. Response rate is very high for

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

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

firms with more than 300 employees (≈90%), while it is lower (60%) for the firms between 20-300 employees.[10] Firms responding to the survey report information on a roughly 10% 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.[11] The survey contains detailed information on wages, job characteristics, and workers' demographic and human capital variables. Due to the complex sampling design for the Wage Survey, observations are weighted.[12] While we can link firms in the Wage Survey over time, there is no individual ID for linking workers over time.

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

For the firm level analysis we mainly use the Corporate Income Tax Data (CIT). This data contains information on firm's balance-sheet and income statements and so it allows us to assess firms' income and cost structure, wages and personnel costs and material expenses. One key advantage of the Corporate Income Tax Data is that it contains information on the universe of firms with double book-keeping and so it is possible to follow firms over-time.

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

[11] 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.

[12] 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.

However, the CIT does not contain information on worker-level wages. Therefore, we use the Wage Survey to calculate the firm-level exposure to the minimum wage. We calculate FA_i, fraction of workers below the 2002 minimum wage, for each firm in which we observe at least 5 workers in the Wage Survey. If we observe a firm in more than one year before 2000, we take the most recent FA_i measure. We omit sectors that are heavily regulated and/or have unreliable balance sheet information. In particular, we leave out agriculture, mining, tobacco, oil and refining, energy sectors, water and air transport, telecommunication and finance and government related sectors, such as education. In the final sample we have 5696 firms, which is 4% of the annual firm population. Given the sampling structure of Wage Survey we over-sample larger firms. To make our sample representative at 1-digit and by firm size we weight our regressions.[13]

Table 4 summarizes the firm level sample. The first column shows the unweighted means of the main variables, while the second the weighted one. Since large firms are over sampled in the Wage Survey, the weights change the composition of the sample substantially. However, our results are robust to not weighting the regressions. The fraction of affected in the last row measures the exposure to the minimum wage. For an average firm, 36% of the workers earn less than the 2002 minimum wage. This number is higher than the exposure at the worker level, which is 10%, because large firms with low exposure employ more workers.

3 Effect on Workers

3.1 Framework

In this section we propose a new approach to estimate the employment effect of the minimum wage that relies on the earnings 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 may sometimes

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

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 is generated.[16] Hence, the amount of "bunching" in the wage distribution at and above the minimum wage will be a nonparametric indicator of the number of jobs are being preserved.

This basic 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. Comparing the number of jobs affected by the minimum wage to the excess number of jobs can be used to identify the employment effect of the minimum wage.[17]

To formalize this idea first we define the below mass in the following way:

$$BM(MW^{new}) \equiv Emp^{old}\left[w < MW^{new}\right] - Emp^{new}\left[w < MW^{new}\right]$$

The first part of this formula is the number of jobs earning below the new minimum wage in the *old* wage distribution. Therefore, this formula represents the number of jobs for whom the minimum wage binds. The second part is the number of jobs in the *new* earning distribution

[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).

[16]The effect of the minimum wage with and without spill-over effects is explained in Appendix Figure A-2.

[17]As it is highlighted on Figure 2 the employment effects of the minimum wage can be also estimated by looking at the change in the number of workers in non-employment. However, non-employment is also affected by jobs destroyed (or created) at the upper-tail of the earning distribution. As long as changes in the upper-trail is not caused by the minimum wage looking at the change in non-employment can be misleading. The approach proposed here has the advantage that it attributes the employment effect of the minimum wage to job changes that occur around the new minimum wage.

that earn below the new minimum wage. This latter is zero under perfect compliance to the minimum wage, but in the data some, albeit a small, fraction of the workers will earn in that range.

The excess mass is defined in the following way

$$EM(MW^{new}, \bar{W}) \equiv \left(Emp^{new}\left[w < \bar{W}\right] - Emp^{new}\left[w < MW^{new}\right]\right) - \\ - \left(Emp^{old}\left[w < \bar{W}\right] - Emp^{old}\left[w < MW^{new}\right]\right)$$

The first part is the number of workers earning between MW^{new} and $\bar{W}$ in the *new* earnings distribution, while the second part is the same in the *old* earnings distribution. Throughout the text we use the 2000 earning distribution as the *old* earning distribution. For notational simplicity when we refer to the timing of $BM(MW^{new})$ or $EM(MW^{new}, \bar{W})$ we always refer to the new earnings distribution.

The ratio of the excess mass to the below mass is the fraction of workers who bunch at and above the new minimum wage:

$$B = \frac{EM(MW^{new}, \bar{W})}{BM(MW^{new})}$$

Whether B is smaller or larger than one tells us the employment effect of the minimum wage. If the minimum wage increase has a negative effect on employment, then B will be less than one. We measure the employment effect of the minimum wage by the following formula:

$$EmpEffect = B - 1 = \frac{EM(MW^{new}, \bar{W})}{BM(MW^{new})} - 1 \tag{1}$$

This formula estimates the fraction of jobs for which the minimum wage binds and are destroyed. Using the definitions of $BM(MW^{new})$ and $EM(MW^{new}, \bar{W})$ the employment effect can be rearranged to

$$EmpEffect = B - 1 = \frac{Emp^{new}\left[w < \bar{W}\right] - Emp^{old}\left[w < \bar{W}\right]}{BM(MW^{new})}$$

The equation shows the importance of $\bar{W}$ in our estimation. If $\bar{W} = \infty$, then all changes in employment at all earnings level are attributed to the minimum wage. This case is equivalent to studying the effect of the minimum wage on non-employment. However, if we set $\bar{W} < \infty$, then the employment changes above that threshold are not attributed to the effect of the minimum wage. Setting $\bar{W}$ therefore imposing more structure to the estimation, but can help if the employment changes above the threshold would bias the estimation.

The appropriate level of $\bar{W}$ depends on what we think about the exogeneity of the minimum wage shocks and their effects on employees. If the researchers have access to a large sample of random shocks in minimum wage, then there is no need to restrict the estimation with setting $\bar{W}$ less then infinity. On the other hand, if we lack random variation in minimum wage or only a few minimum wage shocks are observed, then imposing more structure with lowering $\bar{W}$ can improve the estimation by excluding changes in the number of jobs that are not likely to be related to the minimum wage.

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

Aggregate earnings distribution might be contaminated by changes in the sample composition or because of aggregate shocks. Therefore, we estimate the relationship between the 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, worker's age in four categories (22-30, 30-40, 40-50, 50-60), worker's gender, and worker's education (less than high school, high school or above).[18] We run the following group-level regression:

$$\frac{EM_g\left(MW^t, \bar{W}\right)}{Emp_{2000,g}} = \alpha + \beta^B \frac{BM_g\left(MW^t\right)}{Emp_{2000,g}} + \varepsilon_g \tag{2}$$

We divide our key variables with $Emp_{2000,g}$ to adjust for heteroskedasticity and we also 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 Ratio at year t. The right hand side regressor, the below mass based on year t minimum wage divided by employment in 2000, and we will call it the Below Mass Ratio at year t. Note that the below mass ratio is just the share of workers (jobs) for whom the minimum wage binds. Also we often refer to that variable as the fraction of jobs affected by the minimum wage.

The parameter β^B in equation (2) estimates the fraction of bunchers. The key identification assumption here is that the grouping-level excess mass ratio 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 Ratio in the pre-minimum wage hike years and the Below Mass Ratio in 2002.

Most studies in the minimum wage literature focus on the relationship between the percent change in employment ($\triangle logEmp$) and in the minimum wage ($\triangle logMW$). However, this definition does not take into consideration that the fraction of jobs for which the minimum wage binds might differ across years, research designs and level of aggregation.[20] In this paper, following a handful of recent papers (e.g. Dube et al., 2010), we focus on the relationship

[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 with the same rate after 2000. This large shift in teenage employment related to the rapid expansion of the higher education around that time.

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

between the percent change in employment ($\triangle logEmp$) and in wages induced by the minimum wage increase ($\triangle logW$). In the standard competitive model with binding minimum wage this would measure the labor demand elasticity. Focusing on that elasticity is also more relevant from the welfare point of you as it is recently highlighted by Lee and Saez (2002). In Figure 12 we summarizes some of the estimates in the literature including our estimates from this part and the part using firm-level evidence.

We calculate the relationship between $\triangle logW$ and $\triangle logEmp$ in the following way. First we estimate the employment effects of the minimum wage using $\beta^B - 1$ from equation (2). 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 labor demand elasticity. We calculate the standard errors by bootstrapping.

3.2 Results

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.[21] 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 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.[22] 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

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

[22]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.

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 (2). 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. In Panel A, we estimate the relationship between the Below Mass Ratio in 2002 and the change in average wage. The effect on average wage is significant both in statistical and economic terms. The point estimate in Column (1) tells us that a (hypothetical) group with all workers earning below the 2002 minimum wage would experience a 43% increase in the minimum wage. This number is slightly lower than the percentage increase in the minimum wage in 2002, 57%, because workers earning between the old and the new minimum wage experience a smaller increase than that. In Panel B we report the relationship between Excess Mass Ratio and Below Mass Ratio. 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.

Figure 6 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 12 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)-(4) explore alternative specifications of Equation 2. Results using hourly earnings are reported in column (2), Panel (b). 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 columns (4) and (5) we report estimation results for large firms (more than 50 employee) and small firms (less than 50 employees) separately. The employment loss at large firms is very close to our benchmark specifications. Since large firm are less likely to engage in tax evasion activities (Kleven, Kundsen, Kreiner, Pedersen and Saez 2011), this suggest that our results are not driven by wage under-reporting. The estimated coefficient for small firms is slightly higher than for the large firms, but the difference is not statistically significant.

In Table 3 we explore further robustness checks. In the first three columns of Table 3 we look at how changing $\bar{W}$ affects our results. Column (1) with a lower threshold ($\bar{W} = 10.9$) is almost the same as our benchmark specification ($\bar{W} = 11$). When we move up $\bar{W}$ to a higher level ($\bar{W} = 11.1$) the relationship between excess mass and below mass gets slightly weaker and less precise. This is even more striking for column 3, 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.[23] 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.

[23] 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 2, 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.

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.

4 Effect on Firms

In the previous section we showed that the minimum wage increase had large positive effects on earnings, with at most small negative effect on employment. This indicates that firms level-expenses on low-skilled workers increases in response to the minimum wage, and so their total labor cost increased substantially. The increase in earnings must come at the expense of other actors in the economy. To understand the effects on firm owners or consumers we scrutinize firm-level responses to the minimum wage.[24]

[24]There are both advantages and disadvantages of examining firm level responses. The key advantage is that we can directly estimate what happens to profits and thus producer surplus. If some firms experience a substantial decrease in their profits, then it is a sign that firm owners bear the burden of the incidence. However, firm-level evidence might easily over-estimate the true effect of the minimum wage in the presence of reallocation. If some firms exposed to the minimum wage cut back their production (and so their employment), other firms with less exposure might be able to produce and sell more. These firms might hire some of the workers who were previously laid off at highly exposed firms. This reallocation would lead to an over-estimation of the employment effects, and under-estimation of the profit effects. However, as we show later, our results

4.1 Framework

Our goal here is to estimate the effect of the minimum wage on firms by comparing highly exposed and non-exposed firms behavior from four years before and four years after the minimum wage hike. To do that we estimate different versions of the following regression

$$\frac{y_{it} - y_{i2000}}{y_{i2000}} = a_{st} + \beta_t FA_i + \gamma_t X_{it} + \varepsilon_{it} \tag{3}$$

where the left hand side is the percentage change in outcome y between year 2000 and year t. 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 (the firm level estimates for $\frac{BM_i(2002)}{Emp_{i2002}}$), while X_{it} is a set of firm characteristics in 1997 of the analysis (export share and its square in 1997). We restrict our sample to firms that had at least 3 employees between 1997 and 2000. [25]

4.2 Results

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 3 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

(e.g. on employment responses) are very close to (and within the confidence interval) of the worker-level estimates presented in the previous section and so this reallocation problem must be small in our setting.

[25] 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 the before 2000 years as well. We also do not allow firms to die in the before sample, but results are not sensitive to that assumption.

2000. However, in 2001, the first year after 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.[26] 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.

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

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).[27] 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).[28] 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.[29] For instance, the estimates from 2000 imply that the labor demand elasticity with respect to labor cost is -.2 in 2002. [30]

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

[27]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.

[28]In particular, the change in average firms who die was imputed to be 100%. Then we estimate equation 3 with a least absolute deviation (LAD) on the total sample.

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

[30]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).

gap. Second, at the firm-level we found a small disemployment effect, which decreases total spending on labor.

However, the effect on labor cost is still very large: by 2002, total labor costs had grown by 23% at highly exposed firms in response to the minimum wage. This large increase in labor cost indicates that models that predict small employment effects and small increases in labor cost in response to the minimum wage falls to explain the results presented here. For instance, Dube et. al. (2014) show that in Burdett-Mortensen type of search model the minimum wage shock in wages can be absorbed by cutting back firing and hiring costs. This could happen if worker turnover decreases in response to the minimum wage. Even though we do not directly observe turnover, the fact that total spending on labor substantially increased indicates that the cost saving on this margin cannot be too large.

The elevated level of labor cost must be financed from somewhere. In the presence of rent sharing between firm owners and workers, minimum wage might simply reallocate the rents. In that case we would see small (in some cases positive) employment effects in response to the minimum wage, but large decline in profits. This idea is formalized in many labor market models with monopsonistic wage-setting (Manning 2003) or in Flinn's (2006) search and matching model. This latter model is particularly interesting, since it is able to generate a similar earnings distribution that we see in the data. Moreover, the model also predicts that firms would be willing to keep many of the jobs below the new minimum wage, since they still get some, albeit a lower, share of the match surplus.

To inspect the incidence on firm owners, we look at the effect on profits. Since some firms make negative profits, it is hard to interpret the percentage change here. Instead we divide the change in outcome with average sales between 1997 to 2000. In particular, we estimate the following equation:

$$\frac{y_{it} - y_{i2000}}{\frac{1}{4}\sum_{k=1997}^{2000} Sales_k} = a_{st} + \beta_t FA_i + \gamma_t X_{it} + \varepsilon_{it} \tag{4}$$

The right hand side is the same as for equation (3). The left hand side is the change in outcome y relative to the average sales between 1997 to 2000. This measures firm-level profitability 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. We focus here on two outcomes: operating profits[31] and total labor costs.

In Figure 10 Panel (a) we show results of equation (4) 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 10 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 11 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 11 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.

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

This indicates that the labor cost increase caused by the minimum wage was passed on to consumers. In Figure 10 Panel (c) we show results of equation (4) for net sales (sales net of cost of goods purchased for resale (COGFR)). We subtract COGFR from sales to focus the production process of the firm.[32] The figure highlights that the Net 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 10 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 3 for net sales. As it is clear from the figure, sales net COGFR increases substantially in response to the minimum wage.

In Appendix Figure A-8 we present direct evidence on sales. In Panel (a) we show estimates for equation (3) when the outcome variable is sales. There is a small declining trend in sales before the minimum wage hike that makes it difficult to interpret these results directly. However, it is apparent that this trend breaks in 2000, which suggests that firms responded to the minimum wage hike by increasing their sales. In panel (b) we show the evolution of cost of goods purchased for resale (COGFR). Again, the pre-reform trend in COGFR makes it hard to interpret the results, however, it suggests that COGFR did not decline substantially in response to the minimum wage increase.

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 3 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

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

and we adjust for selection. The estimated coefficient in column (3) 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 6 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 7 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 net sales (sales net of cost of goods purchased for resale) increases by 6% at highly exposed firms.

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. In Panel D and E we report the effects on intermediate goods and the effects on COGFR, respectively. None of these variables affected by the minimum wage change substantially. Finally in Panel F we show the effect of the minimum wage on capital[33]. Columns (5) and (6) highlight that highly exposed firms have somewhat different trends in capital 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:

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

$Subgroup_i$ 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.

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

[33]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.

pre-trends. However, notable exception is the firms with more than 20 employees (in 1997) and some outcomes for the manufacturing firms.

Table 8 Panel A repeats the results for the whole sample already reported in Table 6 or in Column (2) in Table 7. In Panel B we compare effects across two broad sectors, manufacturing and services. While the effect on average labor costs are similar in these two sectors, manufacturing firms have larger employment responses (.9 (s.e. 0.31) out of 10 affected workers) than the service sector (0.4 (s.e. 0.24) out of 10 worker). In Table 7 Panel B we compare exporters to non-exporters. Exporting firms hit by the minimum wage shocks lay off 1 (s.e. 0.4) out of 10 affected workers, while non-exporting ones only lose fifth of it: 0.2 (s.e. 0.23). These large differences in employment effects indicate that the labor demand elasticity with respect to the cost of labor in the exporting sector is substantially larger in absolute value (-.25, s.e. 0.8) than in the non-exporting sector (-.13, s.e. 0.8)[34]. This suggests that the effect of the minimum wage is highly unequally distributed: in some markets disemployment effects are substantial, while negligible in others.

The fact that exporting firms experience substantial job loss can be explained in a neoclassical model with competitive firms. Firms selling their products on the export market face high competition and an highly elastic output demand curve. Therefore, these firms cannot raise their prices to finance the labor cost increase. On the other hand, firms operating in the non-exporting market or in the service sector likely to face an inelastic output demand curve. Service sector firms can therefore pass through the effect of the minimum wage to consumers. In the next section we formalize this idea. [35]

The presence of heterogenous responses by exporting status also suggests that the productivity gains caused by the "shock therapy" of the minimum wage cannot be large. For

[34]In 2003 the employment effects are (-.34, s.e. 0.14) than in the non-exporting sector (-.16, s.e. 0.14). Results on the elasticities by sub-groups are not reported, but available for request.

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

instance, Rebitzer and Taylor's (1995) model argues that the minimum wage can raise workers' unobserved effort and so production output. In most cases the output increase can be sold in the exporting market without substantially deteriorating output prices. Thus sales will increase, which could be used to finance the minimum wage. However, in the non-exporting markets, the increase in output will flood the markets. This can drive prices down , decrease sales and so, finally, firms must lay-off some of their workers. The productivity increase caused by the minimum wage hence predicts the disemployment effects are smaller in the exporting markets (where the output demand elasticity is small) than in the non-exporting ones (where it is large). However, we find the opposite of that.

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.

Tax Evasion. Finally, we also investigate whether our results are affected by tax evasion. Firms evading taxes often report zero profits in Hungary. We have already shown that our results hold for highly profitable firms and so firms reporting unrealistically low levels of profits do not contaminate the evidence presented here (see Figure 11). Another potential proxy for tax evasion (or avoidance) is taking many tax deductions. In the last Panel of Table 7 we examine our key results by firms effective tax rate (measured by paid taxes divided by operating profits). High effective tax rates are a signal for non-cheating as these firms do not use tax optimizing techniques extensively. Since we find that low effective tax rate firms are very similar to high effective tax rate firms in response to the minimum wage, we conclude that our results are not driven by tax evasion.[36]

5 Theoretical Framework

In the previous sections we have shown that the minimum wage hike in Hungary had on average at most a small negative effect on employment. Models with friction such as the monopsonistic wage setting (Manning 2003, Card and Krueger 1995), Flinn's search and matching model , or efficiency wages (Rebitzer and Taylor 1995) are proposed to explain the small negative effect on employment. We showed that simple version of these models cannot explain the observed behavior in the data. First, the monopsonistic wage setting and the search and matching model would predict a decline in profit in response to the minimum wage, but we do not find that in the data. Finally, the efficiency wage model would predict that markets with elastic product demand, such as the exporting sector, would suffer less employment loss than markets with inelastic product demand. Contrary to that, we found the opposite: the employment losses are much larger in the exporting sector. In the next section we show that a simple neoclassical model can be reconciled with these findings if the product demand elasticity that firms face with is low and the substitution between labor and intermediate goods is sufficiently low.

5.1 Neoclassical Model

In neoclassical models with competitive firms the effect of the wage increase on employment is determined by the Hicks–Marshall rules of derived demand. These rules connects the labor

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

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:

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

where Y is output, L is labor, K is capital and I is intermediate goods used for production purposes (includes energy and goods used for production), while M is the goods purchased for resale. It is worth noting that we only have one type of labor here. 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

$$pY = wL + rK + p_I I + c_s M$$

where w is the average wage, r is the rate of return on capital, p_I the cost of intermediate goods, while c_s is the cost of goods purchased for resale.

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

$$\frac{\partial logL}{\partial logw} = \underbrace{-s_L\eta}_{\text{scale effect}} \quad \underbrace{-s_K\sigma_{KL}}_{\substack{\text{substitution}\\ \text{between K and L}}} \quad \underbrace{-s_I\sigma_{IL}}_{\substack{\text{substitution}\\ \text{between I and L}}} \quad \underbrace{-s_M\sigma_{ML}}_{\substack{\text{substitution}\\ \text{between M and L}}} \tag{6}$$

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 (6) 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 (6) 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.[37] 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

[37] 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.

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.[38] 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

$$\frac{\partial logpY}{\partial logw} = \underbrace{s_L}_{\text{price effect}} \quad \underbrace{-s_L\eta}_{\text{scale effect}} \tag{7}$$

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

[38]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.

cost of goods for resale is the following:

$$\frac{\partial log\left(pY - c_s M\right)}{\partial logw} = \frac{1}{\left(1 - s_M\right)} s_L - s_L \eta - \frac{s_M}{\left(1 - s_M\right)} s_L \sigma_M \tag{8}$$

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

$$\frac{\partial logK}{\partial logw} = s_L(-\eta + \sigma_{KL}) \tag{9}$$

$$\frac{\partial logp_I I}{\partial logw} = s_L(-\eta + \sigma_{IL}) \tag{10}$$

$$\frac{\partial logc_s M}{\partial logw} = s_L(-\eta + \sigma_{ML}) \tag{11}$$

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 $m\left(\xi\right)$ 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 $\left(m\left(\xi\right) - \hat{m}\right)' W \left(m\left(\xi\right) - \hat{m}\right)$, 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 (6), the labor demand elasticity; Equation (8) the effect on net sales, and Equation 9 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. 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, one of the most important implications of 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 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 much 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) unweighted mean	(2) weighted mean	(3) sd	(4) min	(5) max
Panel A - Whole sample					
Male	0.59	0.60	0.49	0	1
Education: high school or more	0.49	0.43	0.49	0	1
Age	40.01	39.97	10.00	23	60
Log earnings*	11.22	11.22	0.66	10.12	13.13
Number of obsevation	110,274	110,274	110,274	110,274	110,274
Panel B - Earn below the 2002 minimum wage					
Male	0.57	0.53	0.50	0	1
Education: high school or more	0.29	0.24	0.42	0	1
Age	38.46	38.48	9.99	23	60
Log earnings*	10.29	10.30	0.18	10.12	10.58
Number of obsevation	20,069	20,069	20,069	20,069	20,069
Panel C -Earn between the 2002 minimum wage and 1.5 times of that					
Male	0.55	0.55	0.50	0	1
Education: high school or more	0.31	0.25	0.43	0	1
Age	39.62	39.50	9.98	23	60
Log earnings*	10.80	10.81	0.12	10.58	11
Number of obsevation	22,116	22,116	22,116	22,116	22,116

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

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

Table 2: Group-Level Relationship between Excess Mass and Below Mass - Main Results

Panel A

VARIABLES	(1) $\Delta AvgWage_{2002-2000}$	(2) $\Delta AvgWage_{2002-2000}$	(3) $\Delta AvgWage_{2002-2000}$	(4) $\Delta AvgWage_{2002-2000}$
BM2002/Emp2000	0.429***	0.452***	0.336***	0.586***
	(0.037)	(0.040)	(0.052)	(0.055)
Constant	-0.038***	-0.038***	-0.012*	-0.110***
	(0.007)	(0.007)	(0.007)	(0.023)
Observations	112	112	112	112
R-squared	0.455	0.458	0.218	0.639
Wage	monthly	hourly	monthly	monthly
Firm size	all	all	Emp>50	Emp<50

Panel B

VARIABLES	(1) EM2002/Emp2000	(2) EM2002/Emp2000	(3) EM2002/Emp2000	(4) EM2002/Emp2000
BM2002/Emp2000	1.03	1.04	1.03	1.13
	(0.070)	(0.071)	(0.109)	(0.101)
Constant	0.028	0.021	0.005	0.073
	(0.009)	(0.009)	(0.007)	(0.034)
Observations	112	112	112	112
R-squared	0.687	0.704	0.571	0.592
Wage	monthly	hourly	monthly	monthly
Firm size	all	all	Emp>50	Emp<50
% Change Employment	.03	.04	.03	.13
	(0.070)	(0.071)	(0.109)	(0.101)
Implied labor demand elasticity (wrt. wage)	.07	.10	.11	.21
	(.171)	(.173)	(.363)	(.193)

Robust standard errors in parentheses
Groups are created based on demographics (sex, education, age) and region
Groups are weighted by employment in 2000
For Panel A: *** p<0.01, ** p<0.05, * p<0.01 testing that the parameters are zero

Note: Panel A shows the group-level relationship between average wage change (between 2002-2000) and the below mass ratio in 2002 (below the 2002 minimum wage mass divided by employment in 2000). Panel B shows the group-level relationship between excess mass ratio in 2002 (excess mass in 2002 divided by employment in 2000) and the below mass ratio in 2002 (see equation 2 in the main text). The last but one row in Panel B shows the implied percentage change in employment in response to the minimum wage increase. A positive number means jobs were created, while a negative number means jobs were destroyed. The last row in Panel B shows the ratio of the employment effect and the wage change. The standard errors are calculated by bootstrapping. Column (1) shows our benchmark specification, column (2) shows the results with hourly wages, column (3) and (4) by firm-size.

Table 3: Group-Level Relationship between Excess Mass and Below Mass - Alternative Specifications

VARIABLES	(1) EM2002/Emp2000	(2) EM2002/Emp2000	(3) EM2002/Emp2000	(4) EM2002/Emp2000	(5) EM2002/Emp2000	(6) EM2002/Emp2000
BM2002/Emp2000	1.027	1.01	0.938	1.040	0.976	1.02
	(0.068)	(.079)	(0.128)	(0.070)	(0.062)	(.066)
Constant	0.018	0.032	0.012	0.031	0.018	.028
	(0.009)	(0.011)	(0.021)	(0.009)	(0.008)	.066
Observations	112	112	112	112	112	112
R-squared	0.723	0.622	0.346	0.681	0.706	0.68
$\bar{W}$	10.9	11.1	INF	11	11	11
Small firms (5-10) incl	no	no	no	no	no	yes
Earnings adjusted by	nominal GDP	nominal GDP	nominal GDP	nominal private GDP	75th percentile	nominal GDP
% Change Employment	.02	.01	-.06	.03	-.02	.02
	(0.068)	(0.079)	(0.128)	(0.070)	(0.062)	(.066)
Implied labor demand elasticity (wrt. wage)	.07	.01	-.12	.11	-.061	.09
	(.170)	(.199)	(.347)	(.171)	(.159)	(.190)

Robust standard errors in parentheses
Groups are created based on demographics (sex, education, age) and region
Groups are weighted by employment in 2000

Note: This table shows the group-level relationship between excess mass ratio in 2002 (excess mass in 2002 divided by employment in 2000) and the below mass ratio in 2002 (below the 2002 minimum wage mass divided by employment in 2000). The last but one row shows the implied percentage change in employment in response to the minimum wage increase. A positive number means jobs were created, while a negative number means jobs were destroyed. The last row shows the ratio of the employment effect and the wage change. The standard errors are calculated by bootstrapping. Column (1)-(3) explore different thresholds of $\bar{W}$, while Column (4)-(5) different adjustment of earnings. Column (6) include firms with 5-10 employees.

Table 4: 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 5: 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 6: Firm-Level Evidence, Labor Demand Elasticity

	(1)	(2)	(3)	(4)	(5)	(6)
	Main results				Placebo estimates	
	Change from 2000 to 2002		Change from 2000 to 2003		Change from 1998 to 2000	
VARIABLES						
Panel A: Change in average wage						
Fraction Affected	0.457***	0.448***	0.393***	0.379***	-0.0300***	-0.0245**
	[0.0152]	[0.0142]	[0.0151]	[0.0147]	[0.0104]	[0.0101]
Constant	0.00382		-0.00269		0.0460***	
	[0.00603]		[0.00613]		[0.00487]	
Panel B: Change in average labor cost						
Fraction Affected	0.365***	0.361***	0.312***	0.300***	-0.0184*	-0.0185*
	[0.0152]	[0.0160]	[0.0163]	[0.0166]	[0.0109]	[0.0103]
Constant	0.00973		0.000317		0.00760	
	[0.00601]		[0.00693]		[0.00514]	
Panel C: Change in employment						
Fraction Affected	-0.0615***	-0.0565***	-0.0740***	-0.0700***	0.00409	0.00840
	[0.0184]	[0.0199]	[0.0209]	[0.0223]	[0.0168]	[0.0178]
Constant	-0.123***		-0.169***		-0.0906***	
	[0.00871]		[0.00978]		[0.00754]	
Panel D: Change in total labor cost						
Fraction Affected	0.243***	0.235***	0.168***	0.164***	-0.0236	-0.0177
	[0.0224]	[0.0242]	[0.0247]	[0.0263]	[0.0180]	[0.0193]
Constant	-0.113***		-0.172***		-0.0817***	
	[0.00954]		[0.0107]		[0.00851]	
Panel E: Implied Labor Demand Elasticity with respect to wage						
Fraction Affected	-0.138***	-0.129***	-0.184***	-0.180***		
	[0.036]	[0.039]	[0.048]	[0.052]		
Panel F: Implied Labor Demand Elasticity with respect to average labor cost						
Fraction Affected	-0.168***	-0.155***	-0.229***	-0.233***		
	[0.046]	[0.051]	[0.063]	[0.069]		
Observations	5,596	5,596	5,596	5,596	5,596	5,596
industry	no	yes	no	yes	no	yes
controls	no	yes	no	yes	no	yes

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

Note: Table shows the estimated results from equation 3 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* for selection corrected average wage (Panel A), for selection corrected average labor cost (Panel B), for employment (Panel C), and for total labor cost (Panel D). Columns (3) and (4) shows the changes in the same variable between *2003 and 2000*. The average labor cost is observed only for firms that survived, and so we correct for this selection (see the text for the details). In Panel E and D we report the labor demand elasticity implied by our estimates with respect to wages, and the cost of labor, respectively. Standard errors 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).

Table 7: Effect on Firm Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Main results				Placebo estimates	
	Change from		Change from		Change from	
VARIABLES	2000 to 2002		2000 to 2003		1998 to 2000	
Panel A: Change in value added						
Fraction Affected	0.198***	0.193***	0.175***	0.168***	0.019	0.034
	[0.0403]	[0.0443]	[0.0436]	[0.0460]	[0.0379]	[0.0408]
Implied Elasticity	0.540***	0.539***	0.534***	0.552***		
	[0.095]	[0.110]	[0.130]	[0.144]		
Panel B: Change in net sales						
Fraction Affected	0.075***	0.072***	0.070***	0.063***	0.013	0.014
	[0.0237]	[0.0256]	[0.0256]	[0.0270]	[0.0221]	[0.0240]
Implied Elasticity	0.200***	0.198***	0.224**	0.201***		
	[0.052]	[0.056]	[0.072]	[0.078]		
Panel C: Change in sales						
Fraction Affected	0.0358*	0.0489**	0.012	0.021	-0.031	-0.033
	[0.0205]	[0.0225]	[0.0222]	[0.0236]	[0.0211]	[0.0227]
Implied Elasticity	0.0960*	0.133	0.038	0.066		
	[0.049]	[0.055]	[0.066]	[0.074]		
Panel D: Change in intermediate goods used for production						
Fraction Affected	0.004	0.004	0.004	0.003	-0.004	-0.018
	[0.0236]	[0.0254]	[0.0262]	[0.0278]	[0.0225]	[0.0240]
Implied Elasticity	0.01	0.009	0.011	0.007		
	[0.052]	[0.056]	[0.070]	[0.076]		
Panel E: Change in cost of good for resale						
Fraction Affected	-0.017	0.003	-0.005	0.016	-0.055*	-0.049
	[0.0316]	[0.0349]	[0.0332]	[0.0358]	[0.0329]	[0.0354]
Implied Elasticity	-0.0465	0.007	-0.015	0.053		
	[0.105]	[0.121]	[0.143]	[0.164]		
Panel F: Change in Capital						
Fraction Affected	0.0405	0.0376	0.0452	0.0434	0.0455*	0.0442
	[0.0266]	[0.0287]	[0.0294]	[0.0314]	[0.0265]	[0.0282]
Implied Elasticity	0.11	0.101	0.145	0.142		
	[0.089]	[0.099]	[0.124]	[0.140]		
Observations	5,596	5,596	5,596	5,596	5,596	5,596
industry	no	yes	no	yes	no	yes
controls	no	yes	no	yes	no	yes

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

Note: Table shows the estimated results from equation 3 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 *2003 and 2000*. We also report the implied elasticities with respect to the cost of labor. Standard errors 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 8: Firm Level Evidence - Main Results, Treatment Effect Heterogeneity

	(1) Average Labor cost	(2) Emplyoment	(3) Value added	(4) Net Sales	(5) Sales	(6) Intermediate goods	(7) Capital
Panel A: All firms							
	0.361***	-0.0565***	0.193***	0.0720***	0.0489**	0.004	0.0376
obs= 5596	[0.0160]	[0.0199]	[0.0443]	[0.0256]	[0.0225]	[0.0254]	[0.0287]
Panel B: by industry							
service & construction	0.343***	-0.0435*	0.176***	0.0845***	0.0524*	0.003	0.0375
obs= 2975	[0.0192]	[0.0248]	[0.0541]	[0.0323]	[0.0276]	[0.0316]	[0.0360]
manufacturing	0.395***	-0.0892***	0.235***	0.0405	0.0402	0.001	0.0380
obs= 2618	[0.0310]	[0.0314]	[0.0744]	[0.0385]	[0.0374]	[0.0401]	[0.0438]
Panel C: by trade							
non-exporters	0.347***	-0.0287	0.243***	0.102***	0.0771***	0.006	0.0526
obs= 3083	[0.0180]	[0.0234]	[0.0502]	[0.0301]	[0.0264]	[0.0297]	[0.0339]
exporters	0.422***	-0.0991**	0.0646	0.0121	-0.0171	-0.002	0.0285
obs= 2618	[0.0364]	[0.0391]	[0.0940]	[0.0450]	[0.0410]	[0.0461]	[0.0539]
Panel D: by size							
expl<20	0.306***	-0.0759***	0.187***	0.0866***	0.0575**	0.005	0.0251
obs= 1930	[0.0178]	[0.0252]	[0.0554]	[0.0323]	[0.0281]	[0.0321]	[0.0365]
empl>20	0.293***	-0.0587**	0.161***	0.00341	-0.00318	-0.004	0.0466
obs=3666	[0.0165]	[0.0268]	[0.0603]	[0.0324]	[0.0308]	[0.0331]	[0.0386]
Panel E: by share of non-cash benefit in labor cost							
below median	0.419***	-0.0947***	0.193***	0.0624*	0.0338	0.003	0.0304
obs= 2783	[0.0203]	[0.0265]	[0.0597]	[0.0353]	[0.0299]	[0.0350]	[0.0385]
above median	0.301***	-0.00892	0.193***	0.0848**	0.0685**	0.004	0.0494
obs= 2810	[0.0221]	[0.0281]	[0.0609]	[0.0337]	[0.0316]	[0.0336]	[0.0404]
Panel F: by effective taxrate							
below median	0.362***	-0.0238	0.182**	0.0630	0.0624*	0.004	0.0514
obs= 2830	[0.0265]	[0.0322]	[0.0761]	[0.0402]	[0.0348]	[0.0398]	[0.0474]
above median	0.364***	-0.0460*	0.229***	0.102***	0.0646**	0.005	0.0574*
obs= 2763	[0.0192]	[0.0236]	[0.0500]	[0.0313]	[0.0271]	[0.0314]	[0.0334]
Controls	yes	yes	yes	yes	yes	yes	yes
Industry dummies	yes	yes	yes	yes	yes	yes	yes

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

Note: This table shows estimates of equation 5 for our benchmark specification (Table 6 and Table 7, Column (2)). Columns (1)-(7) show the effect of fraction affected on percentage change in different outcomes between *2002 and 2000*. In each regression we control for the share of export in sales in 1997 and its square term, and 2-digit NACE (industry codes). Results are weighted to make the sample representative (see the text for the details).

Table 9: Firm Level Evidence - Placebo estimates, Treatment Effect Heterogeneity

	(1) Average Labor cost	(2) Emplyoment	(3) Value added	(4) Net Sales	(5) Sales	(6) Intermediate goods	(7) Capital
Panel A: All firms							
	-0.0185*	0.008	0.034	0.013	-0.033	-0.018	0.044
obs= 5596	[0.010]	[0.017]	[0.040]	[0.024]	[0.022]	[0.024]	[0.028]
Panel B: by industry							
service & construction	-0.018	0.014	0.064	0.053*	-0.008	0.005	0.078**
obs= 2975	[0.014]	[0.021]	[0.050]	[0.029]	[0.028]	[0.029]	[0.0354]
manufacturing	-0.015	-0.006	-0.042	-0.086**	-0.095***	-0.079**	-0.042
obs= 2618	[0.013]	[0.030]	[0.066]	[0.037]	[0.036]	[0.037]	[0.0433]
Panel C: by trade							
non-exporters	-0.010	0.036*	0.035	0.033	-0.010	0.010	0.067**
obs= 3083	[0.011]	[0.020]	[0.047]	[0.028]	[0.026]	[0.028]	[0.0329]
exporters	-0.037*	-0.021	0.113	0.024	-0.027	-0.028	0.030
obs= 2618	[0.021]	[0.032]	[0.081]	[0.044]	[0.043]	[0.043]	[0.0539]
Panel D: by size							
expl<20	-0.015	0.010	0.039	0.040	-0.011	0.008	0.038
obs= 1930	[0.014]	[0.021]	[0.051]	[0.030]	[0.029]	[0.030]	[0.0363]
empl>20	-0.016	-0.108***	-0.048	-0.111***	-0.121***	-0.133***	0.027
obs=3666	[0.011]	[0.026]	[0.053]	[0.0310]	[0.030]	[0.031]	[0.0372]
Panel E: by share of non-financial remuneration in labor cost							
below median	-0.020	-0.016	0.005	0.010	-0.037	-0.021	0.034
obs= 2783	[0.012]	[0.023]	[0.057]	[0.032]	[0.030]	[0.031]	[0.0379]
above median	-0.015	0.026	0.055	0.013	-0.028	-0.013	0.061
obs= 2810	[0.017]	[0.024]	[0.058]	[0.032]	[0.031]	[0.034]	[0.0399]
Panel F: by effective taxrate							
below median	-0.019	0.049*	0.052	0.049	0.001	0.022	0.055
obs= 2830	[0.015]	[0.029]	[0.075]	[0.039]	[0.037]	[0.038]	[0.0471]
above median	-0.014	0.014	0.020	0.012	-0.032	-0.017	0.079**
obs= 2763	[0.012]	[0.021]	[0.042]	[0.028]	[0.026]	[0.029]	[0.0323]
Controls	yes	yes	yes	yes	yes	yes	yes
Industry dummies	yes	yes	yes	yes	yes	yes	yes

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

Note: This table shows the **placebo estimates** based on equation 5. Columns (1)-(7) show the effect of fraction affected on percentage change between *2002 and 2000* for different outcomes. In each regression we control for the share of export in sales in 1997 and its square term, and 2-digit NACE (industry codes). Results are weighted to make the sample representative (see the text for the details). Results are weighted to make the sample representative (see the text for the details).

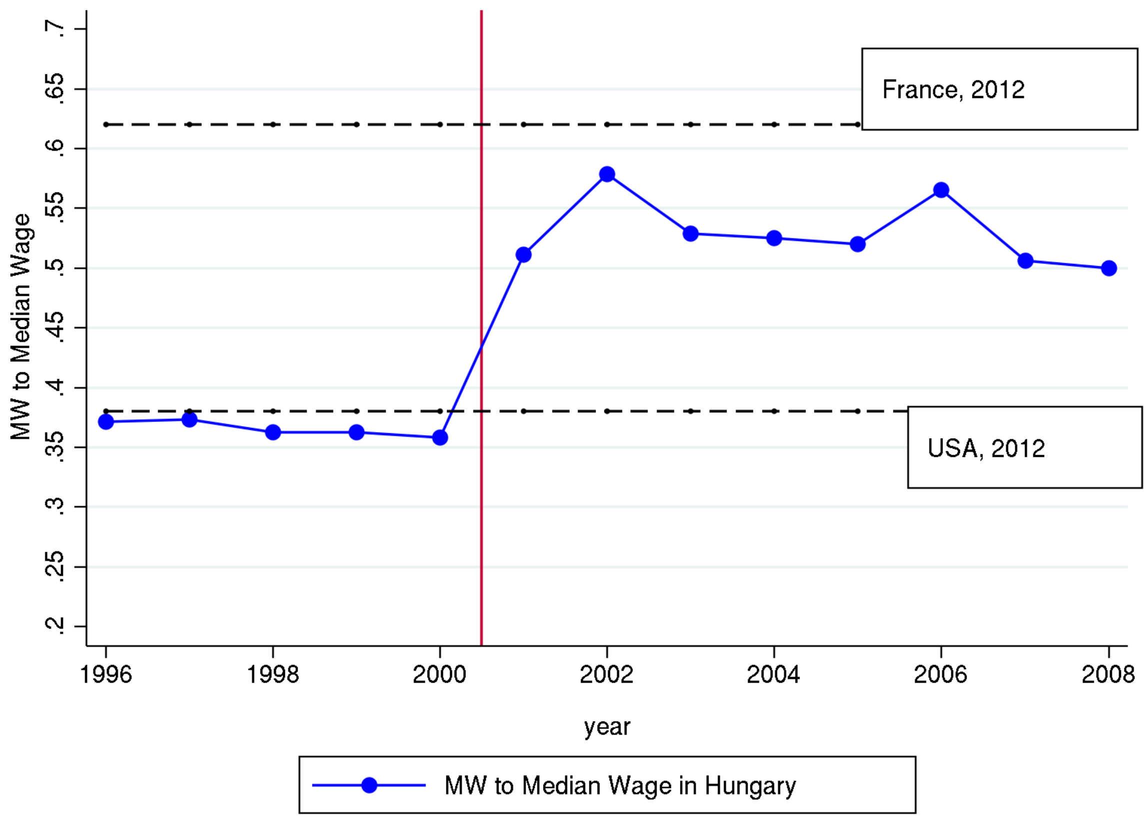
Table 10: Estimating the neoclassical model in 2002 and 2003

	(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

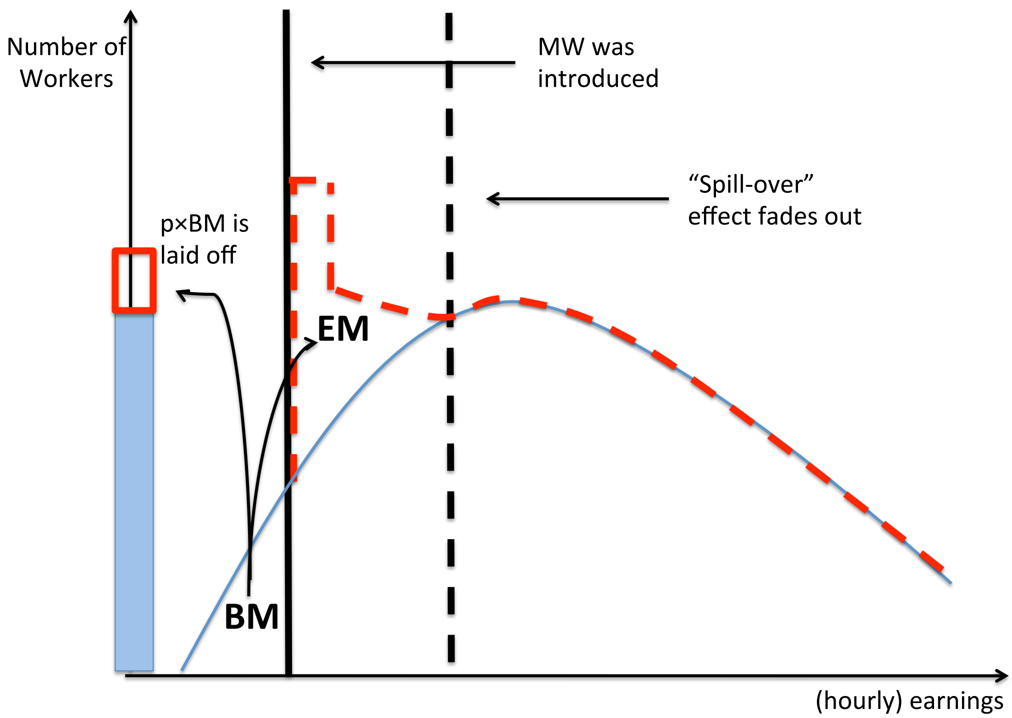
Note: We estimate the parameters of the neoclassical model presented in Section 5 with a minimum-distance estimator. The theoretical moments can be found in Section 5. Column (1) and Column (4) shows the empirical moments in 2002 and in 2003, respectively. Column (1)-(4) show results for all firms, while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

Figure 1: Minimum wage in Hungary



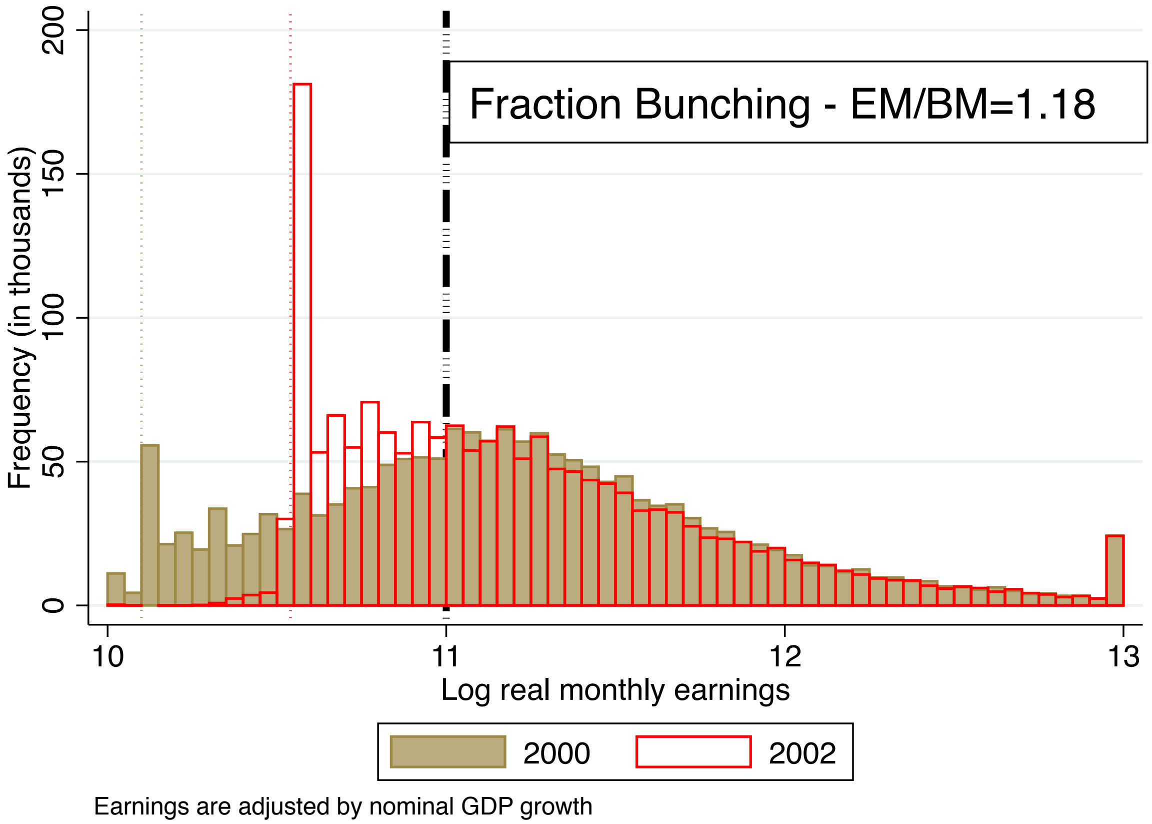
Notes: This figure shows the ratio of the minimum wage to median wage in the private sector for Hungary between 1996 and 2008 (own calculations). The two dashed line depicts the ratio of the minimum wage to the median wage for France and the U.S. in 2012 (OECD). The graph shows the large and permanent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

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



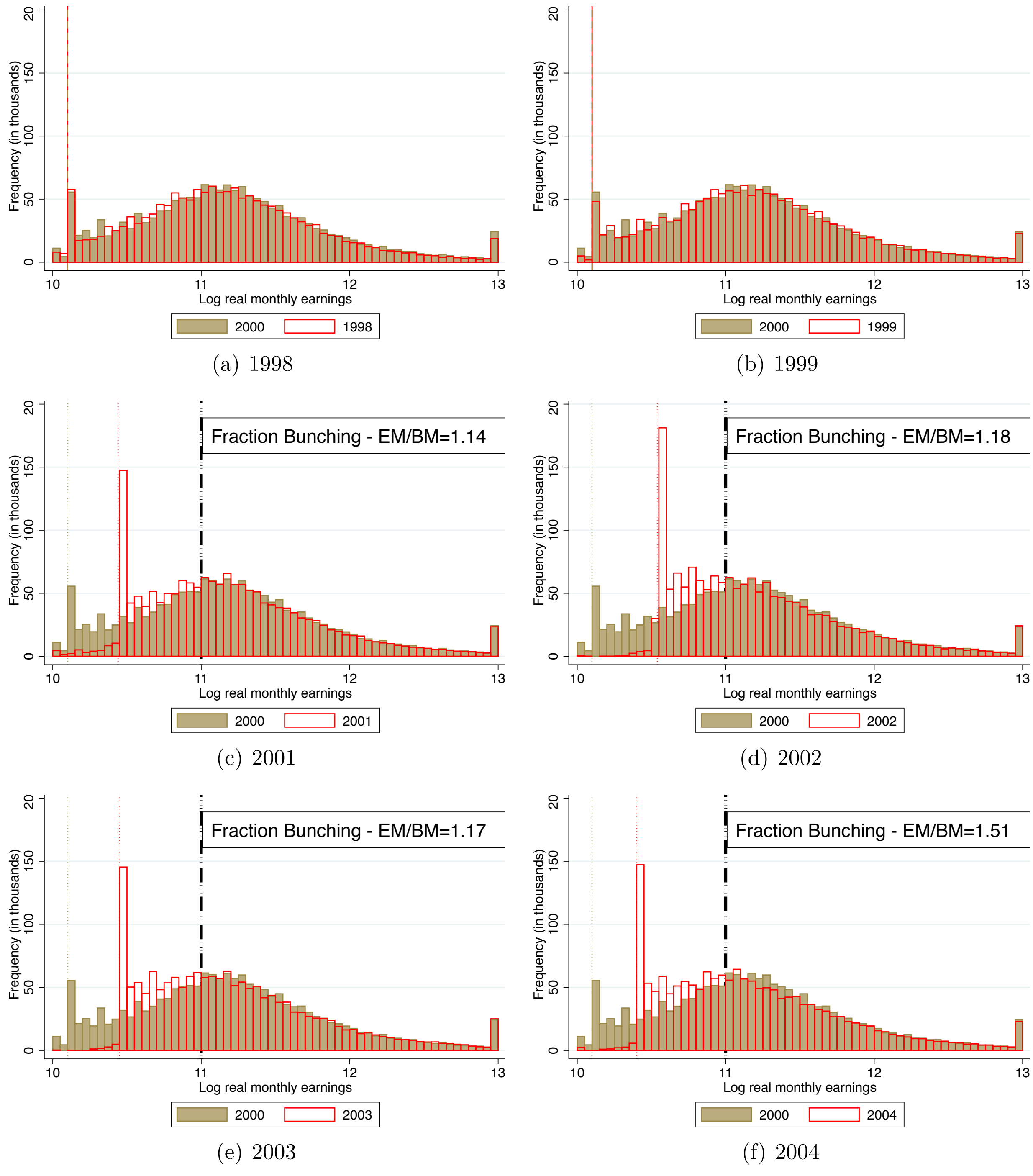
Notes: The effect of the minimum wage on the (frequency) distribution of hourly earnings is depicted here. The blue bar at zero represents workers without a job before the introduction of the minimum wage, while the blue solid line show the earnings distribution. The mass of workers below the minimum 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

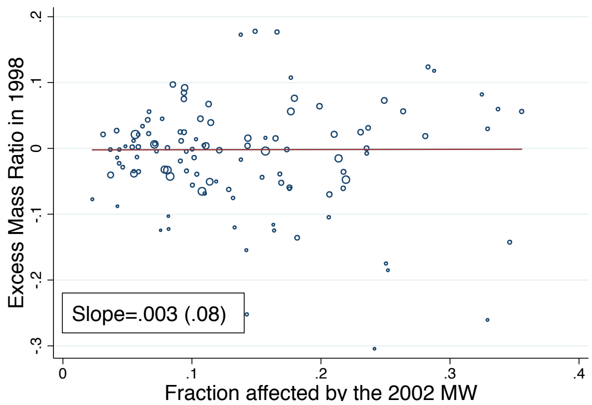
(d) 2002

(e) 2003

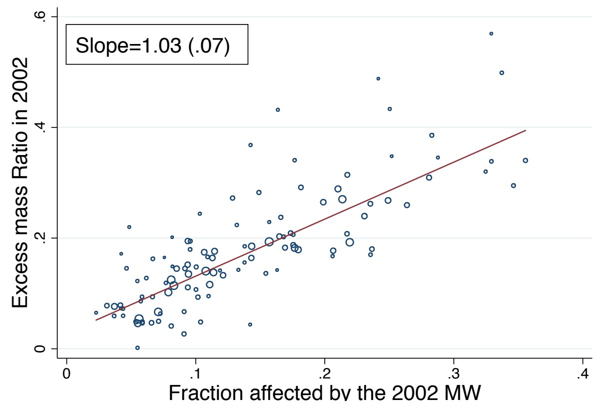
(f) 2004

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.

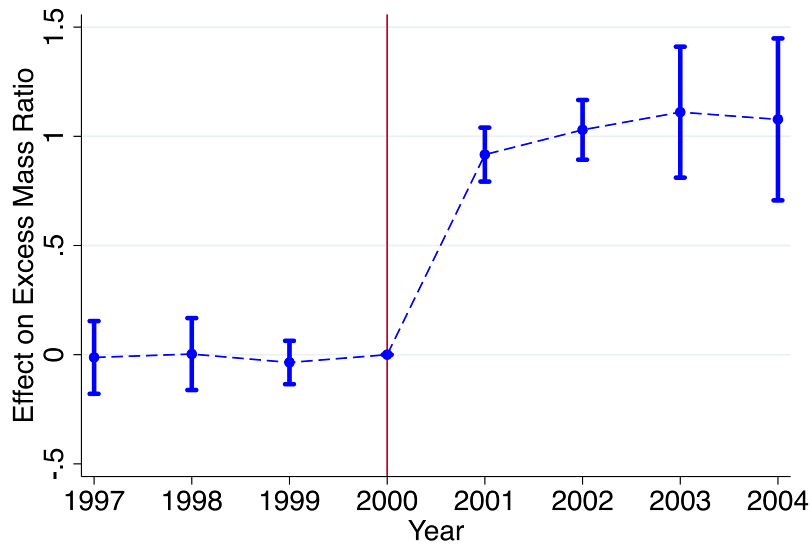
Figure 5: Group-level relationship between excess mass and below mass



(a) Placebo Test



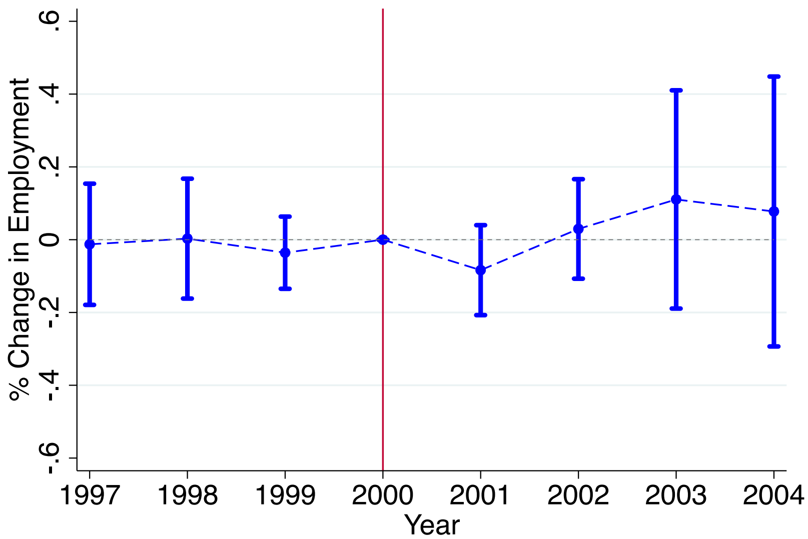
(b) Main Results



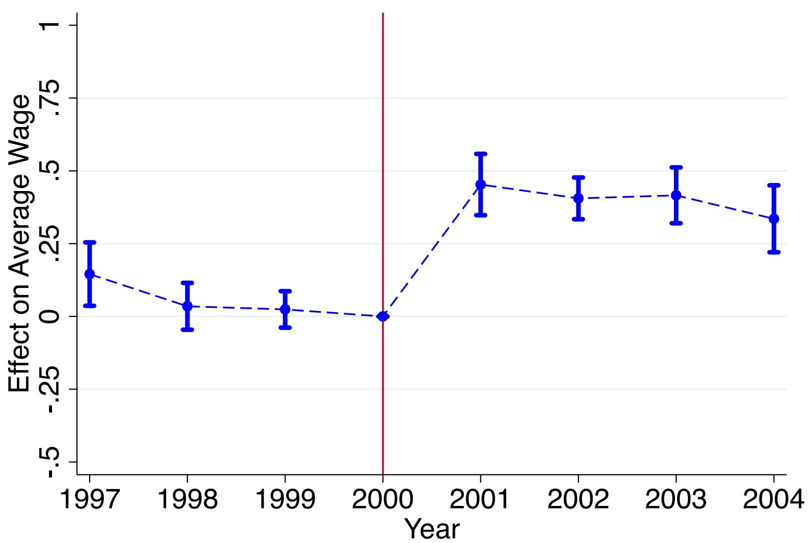
(c) Relationship over time

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



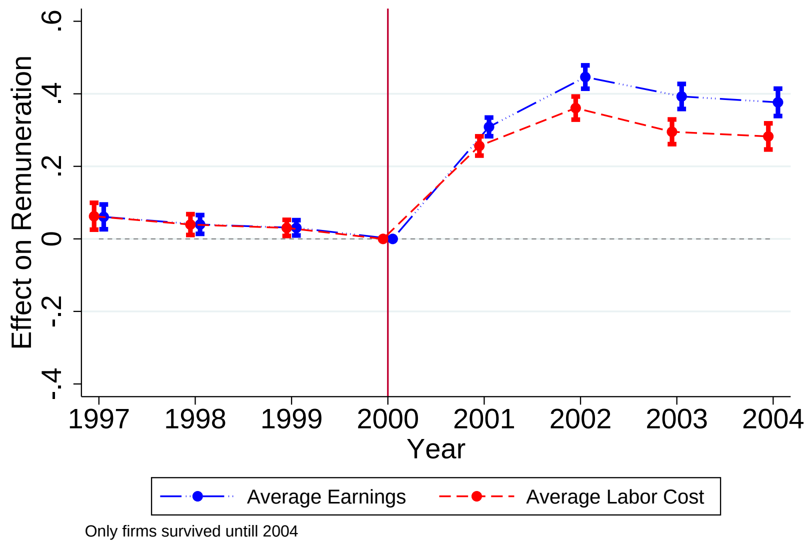
(a) % Change in Employment



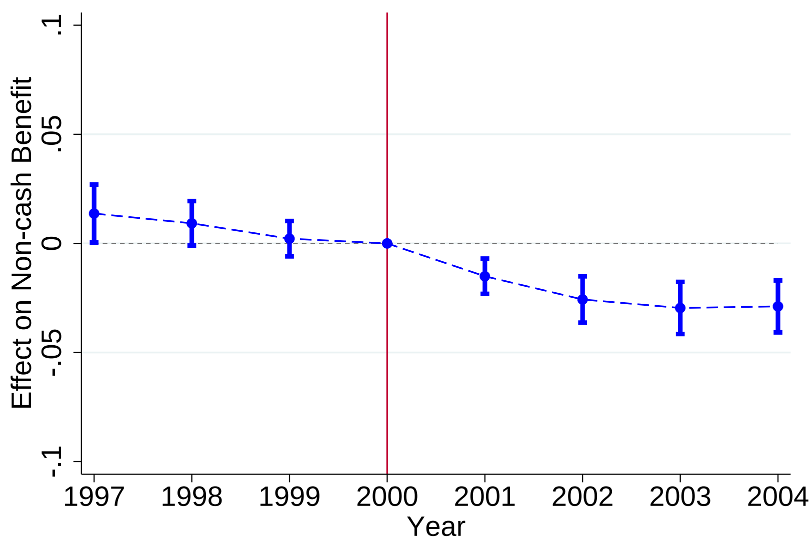
(b) % Change in Average Earnings

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 (1): 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



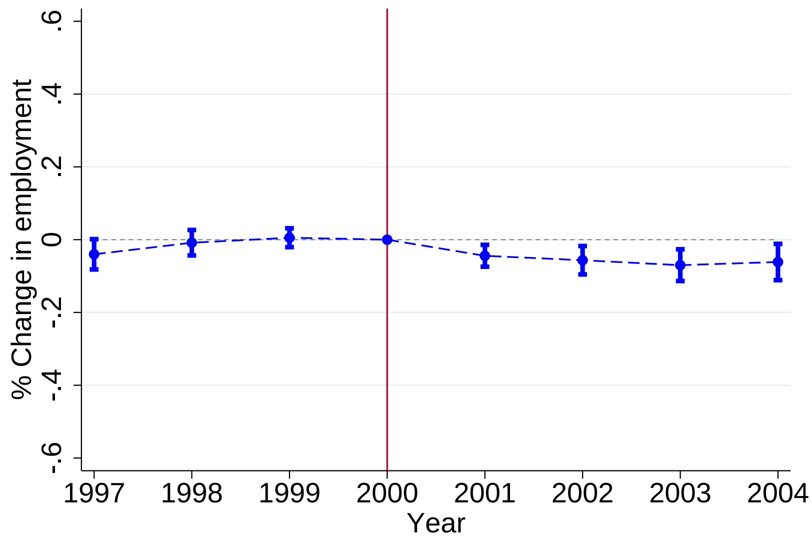
(a) Effect on Average Earnings and Average Labor Cost



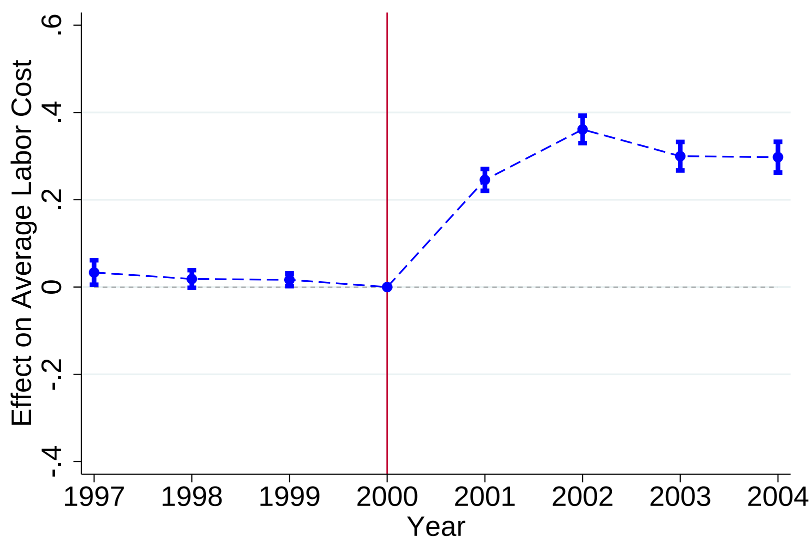
(b) Effect on Non-Cash Benefits

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 (3), 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 (3), 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



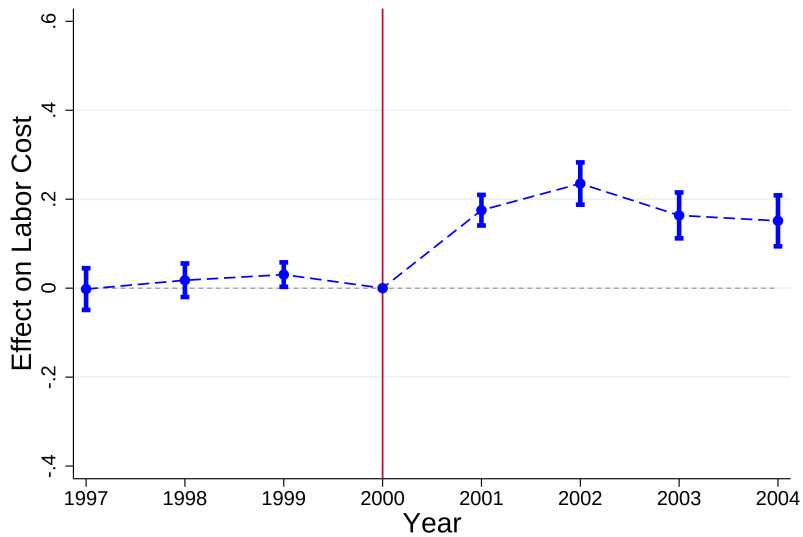
(a) Effect on Employment



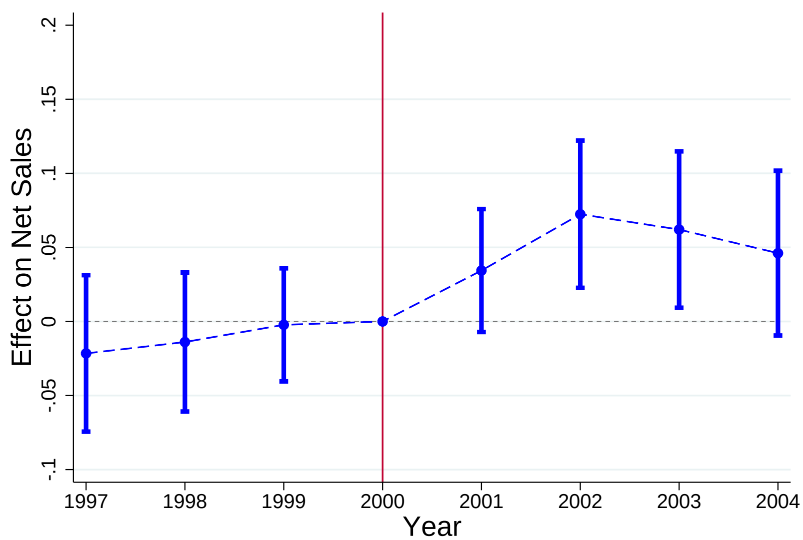
(b) Effect on (Selection-Corrected) Average Labor Cost

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 (3). 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 (3), 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 Net Sales



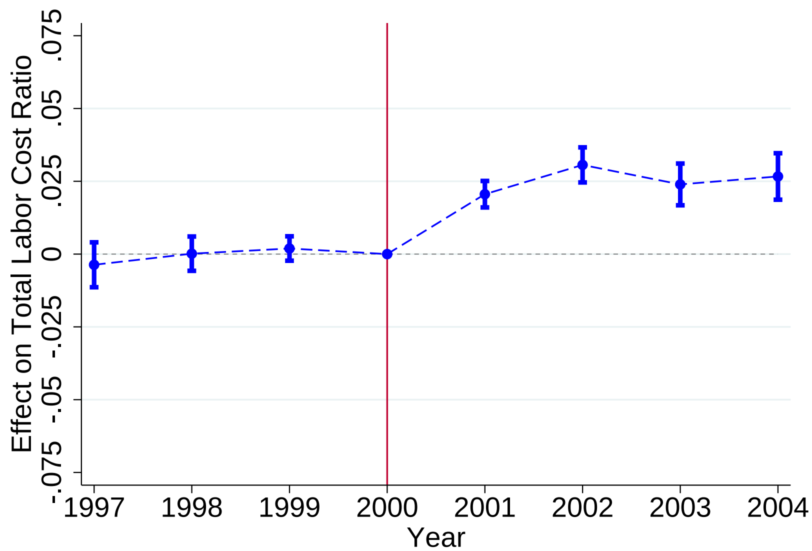
(a) Effect on Total Labor Cost



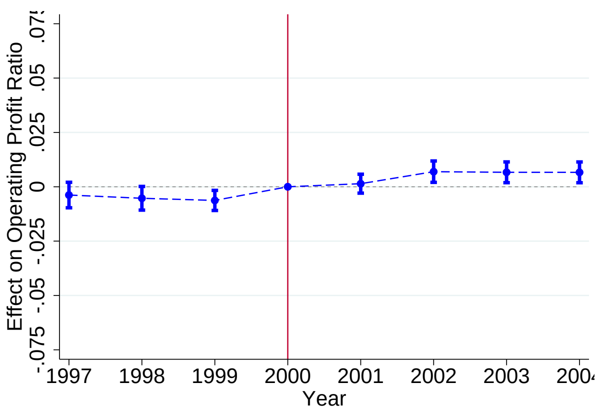
(b) Effect on Net Sales

Notes: Figure 10 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 3). 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 Net Sales on fraction affected by the minimum wage (beta coefficients with its confidence intervals from equation 3). Net Sales is Sales net of Cost of Goods Purchased for Resale (COGFR). We subtract COGFR to focus on the firm's production process. It is clear that net sales substantially increase in response to the minimum wage. Both Panel (a) and Panel (b) include firms that died in the regression. Controls and industry dummies are also included in the regressions.

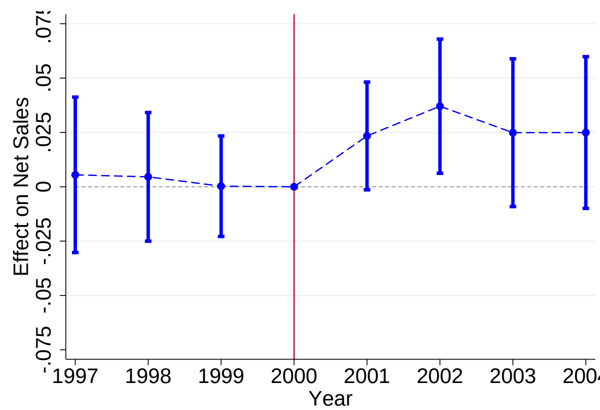
Figure 10: Effect on Profit Margin. Total Labor Cost Ratio and Net Sales Ratio



(a) Effect on Total Labor Cost Ratio



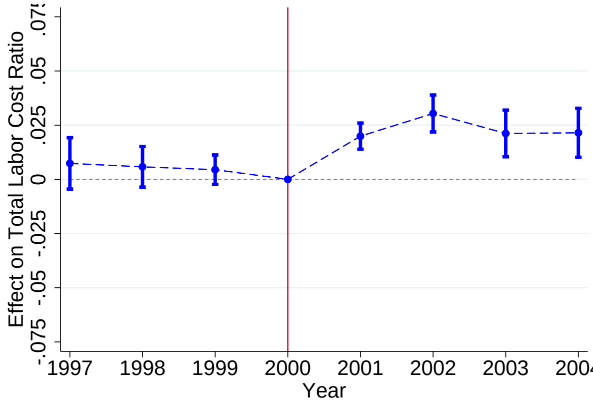
(b) Effect on Operating Profit Ratio



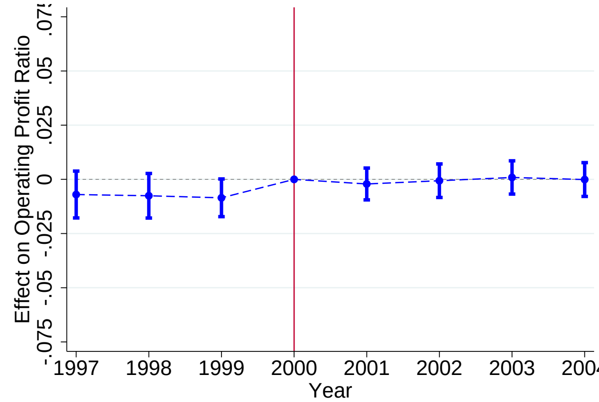
(c) Effect on Net Sales Sales Ratio

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 (4)). 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 (4)). 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 Net Sales (beta coefficients with its confidence intervals from equation (4)). Net Sales is sales net of costs goods purchased for resale divided by the average sales between 1997 to 2000. 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.

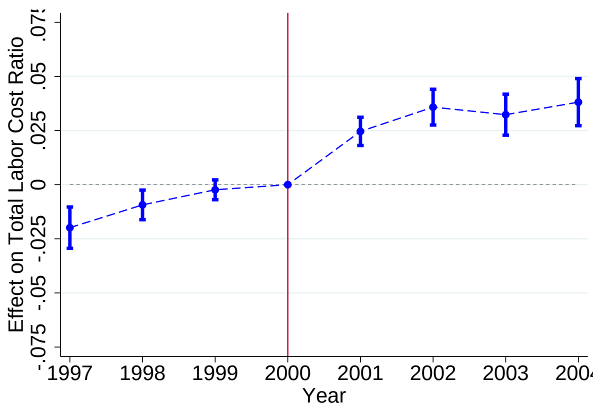
Figure 11: Effect on Profits by Profitability Before the Minimum Wage Hike



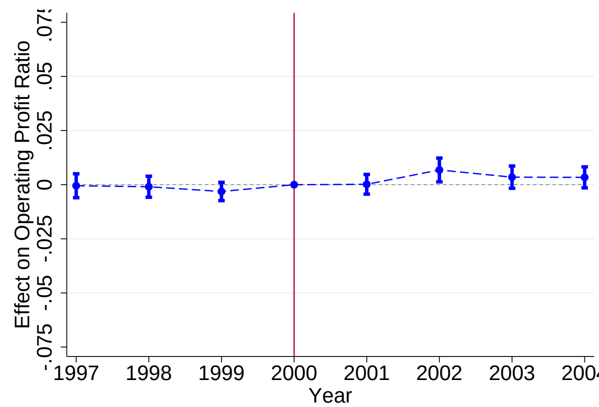
(a) High Profit Before 2000, Labor Cost Ratio



(b) High Profit Before 2000, Profit Ratio



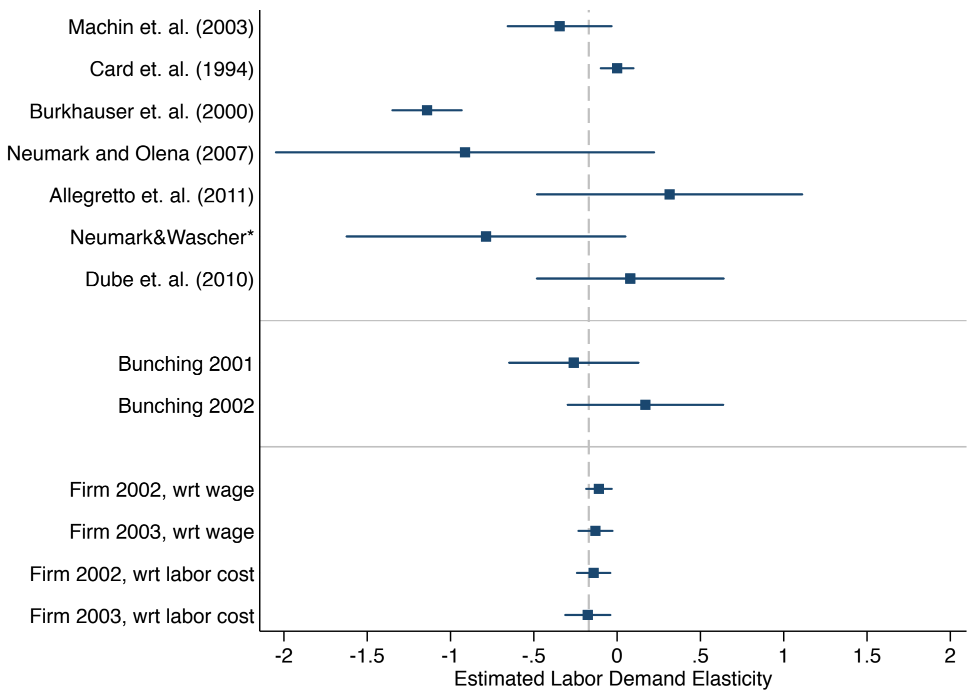
(c) Low Profit Before 2000, Labor Cost Ratio



(d) Low Profit Before 2000, Profit Ratio

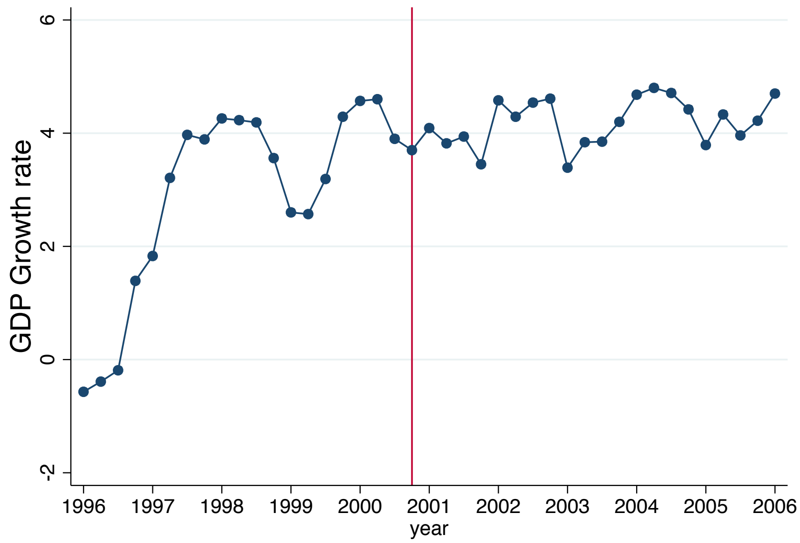
Notes: Figure 12 shows the results presented on Figure 11 by profitability before the minimum wage hike. High profitability firms have above median average profit margin (profit over sales) between 1997 and 2000. The average profit margin in the high profitability group is 7.2%, while in the low profitability 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 12: Labor Demand Elasticity in the literature and in this paper

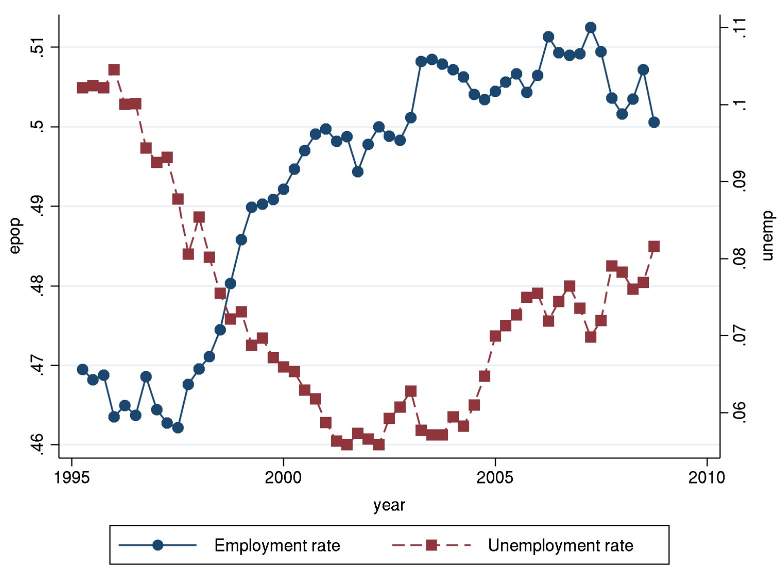


Notes: This figure summarizes the minimum wage implied labor demand elasticities estimated in the literature and the ones estimated in this paper. The dashed vertical line show our preferred estimates for the labor demand elasticity, which is -0.2. Neumark and Wascher* is Dube et. al. (2010) replications. 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



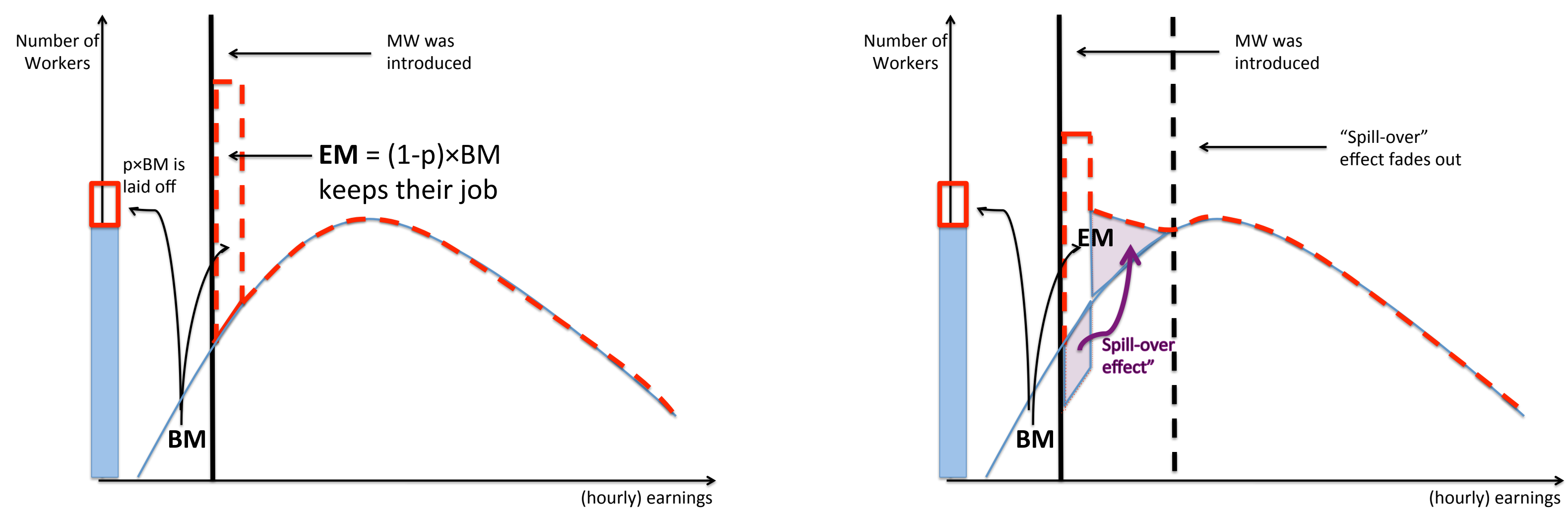
(a) GDP growth



(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

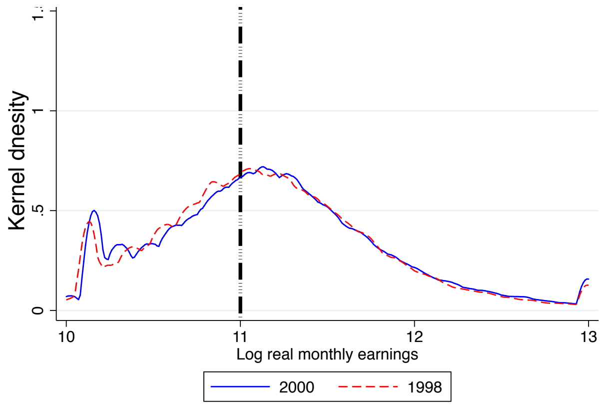


(a) No spillover effects

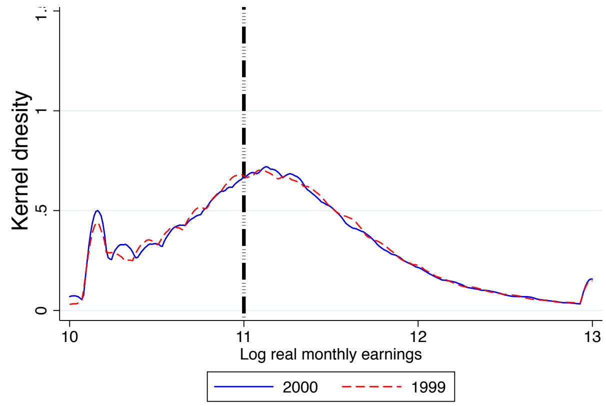
(b) With spill-over effects

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.

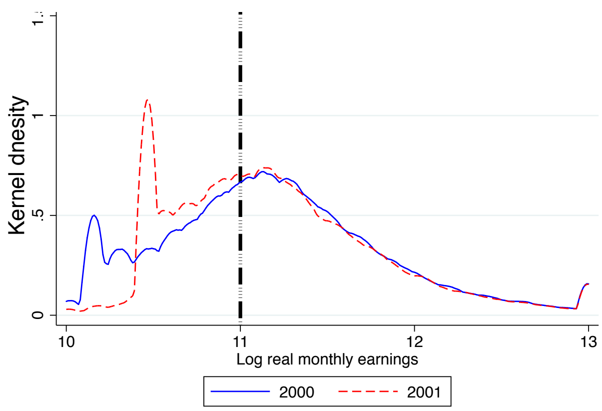
Figure A-3: Evolution of kernel densities over time



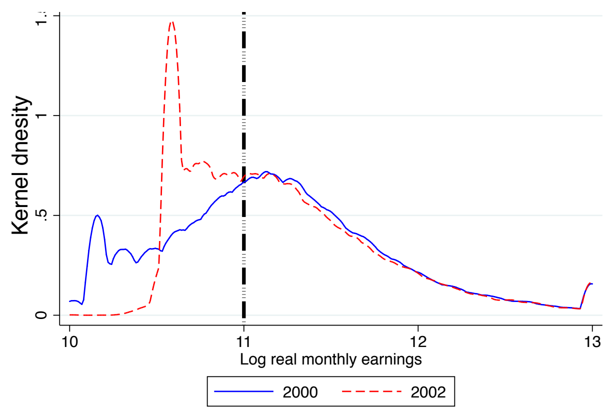
(a) 1998



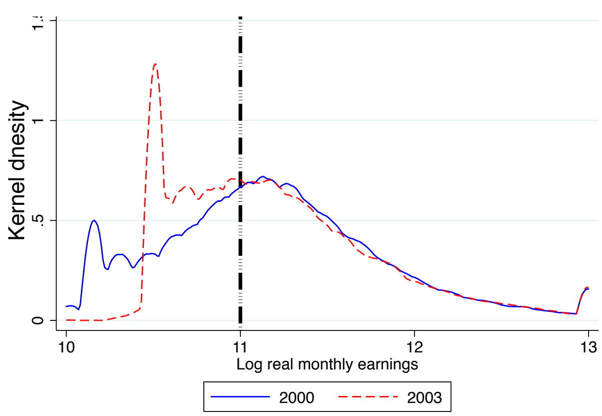
(b) 1999



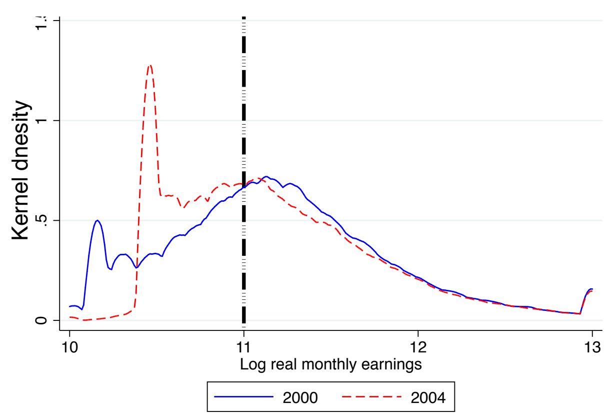
(c) 2001



(d) 2002



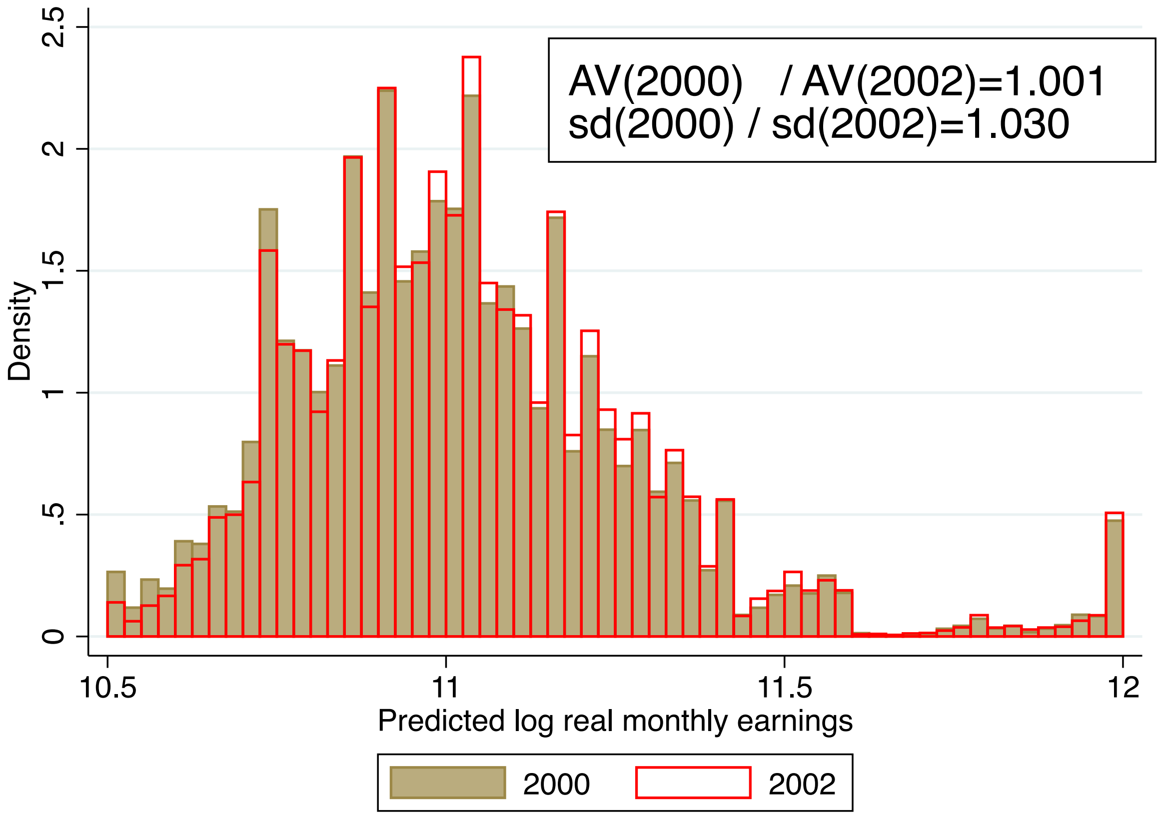
(e) 2003



(f) 2004

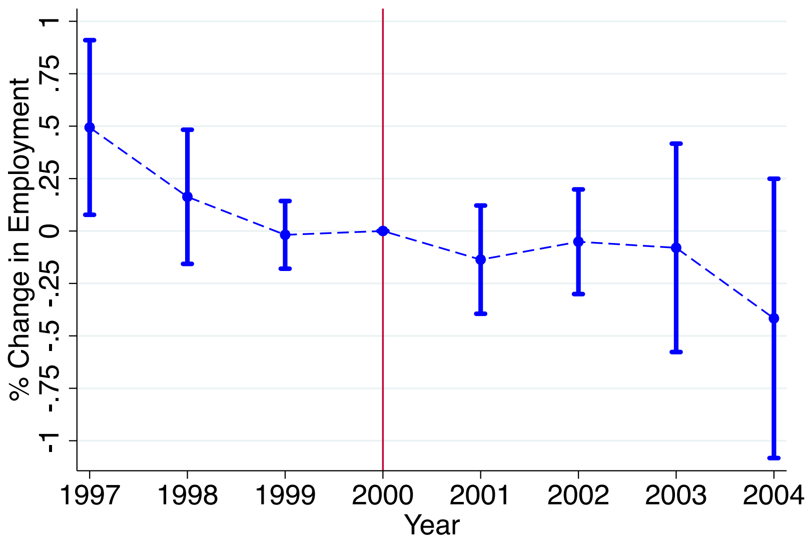
Notes: The kernel density of monthly log earnings over time are shown between 1998 and 2004 (red dashed line) relative to 2000 (blue line). The vertical dotted dash black line show $\bar{W}$.

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

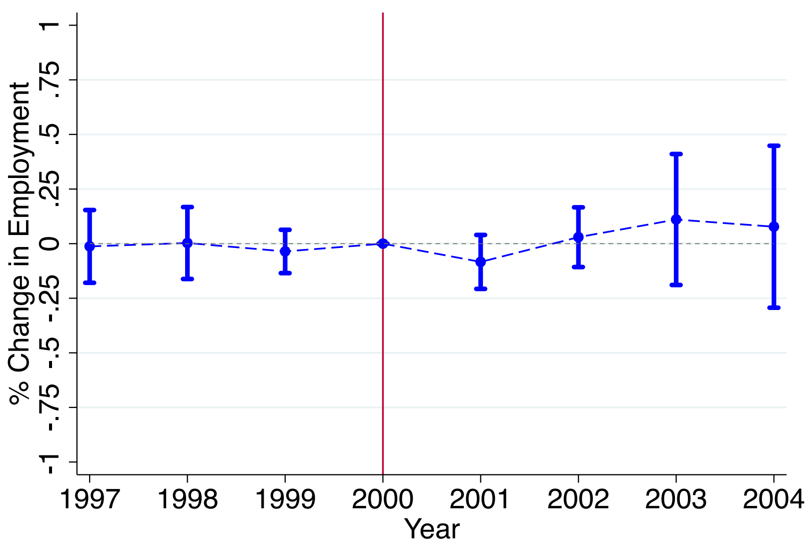


Notes: This figure shows the (density) earning distributions predicted by observables (age, age square, sex, education, region) in 2000 (brown solid bars) and in 2002 (red solid bars) for jobs that earned less than $\bar{W}$. In both years we use the relationship between observables and the earnings 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



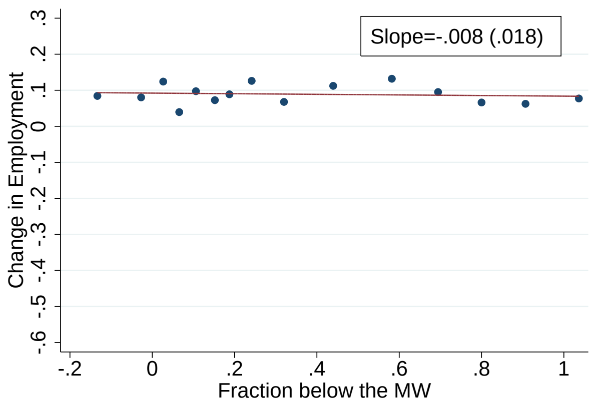
(a) Standard Approach, $\bar{W} = \infty$



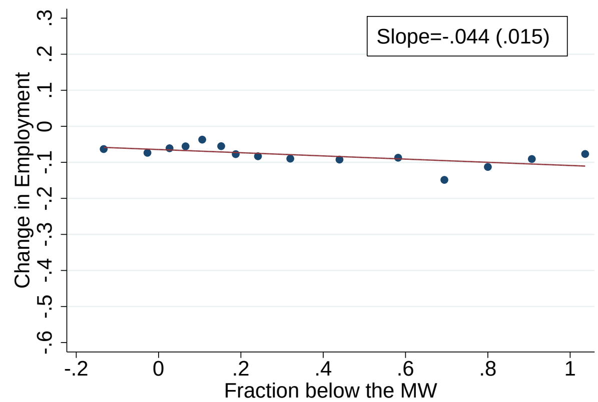
(b) Main Specification, $\bar{W} = 11$

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).

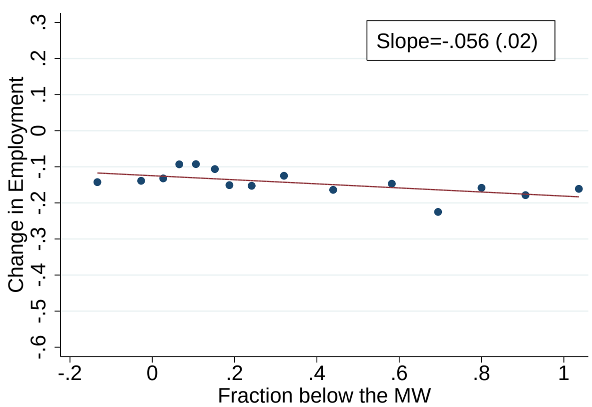
Figure A-6: Effect on Employment over time



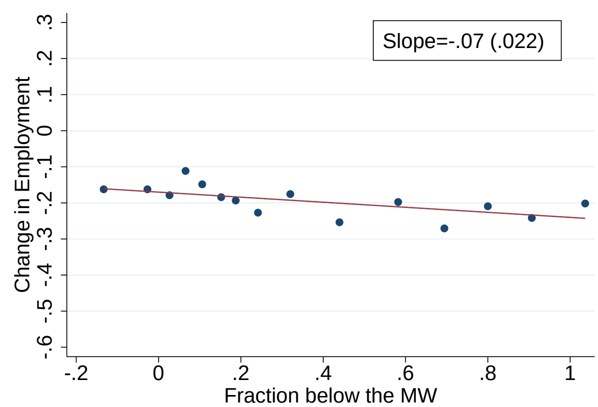
(a) 1998



(b) 2001



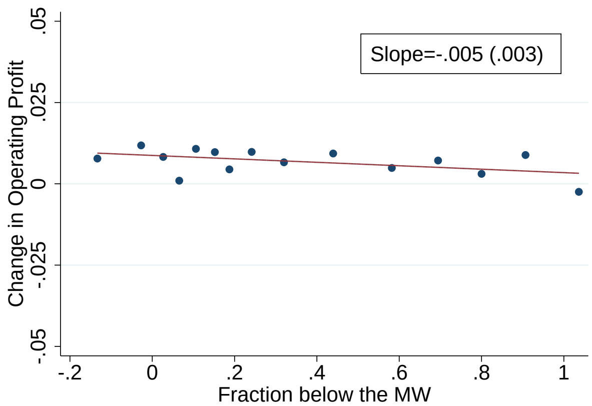
(c) 2002



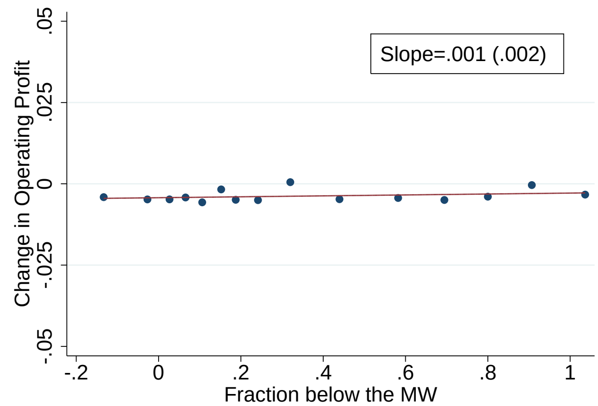
(d) 2003

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 (3). The red solid line is the linear fit, while the slope of this fit reported in the top right corner.

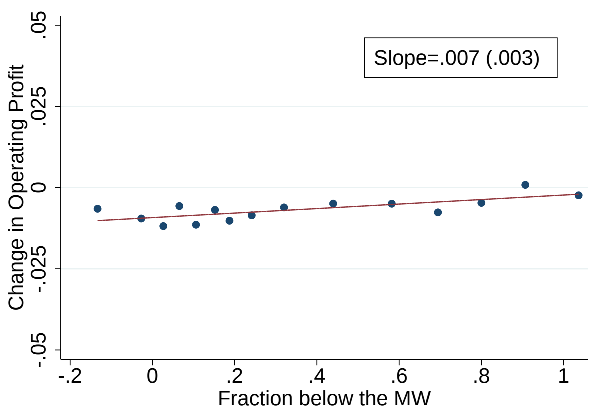
Figure A-7: Effect on Profit over time



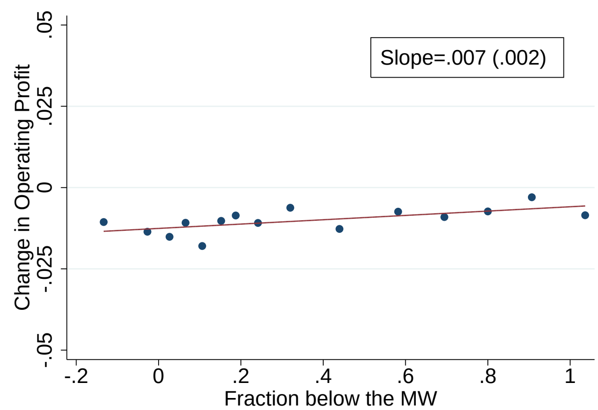
(a) 1998



(b) 2001



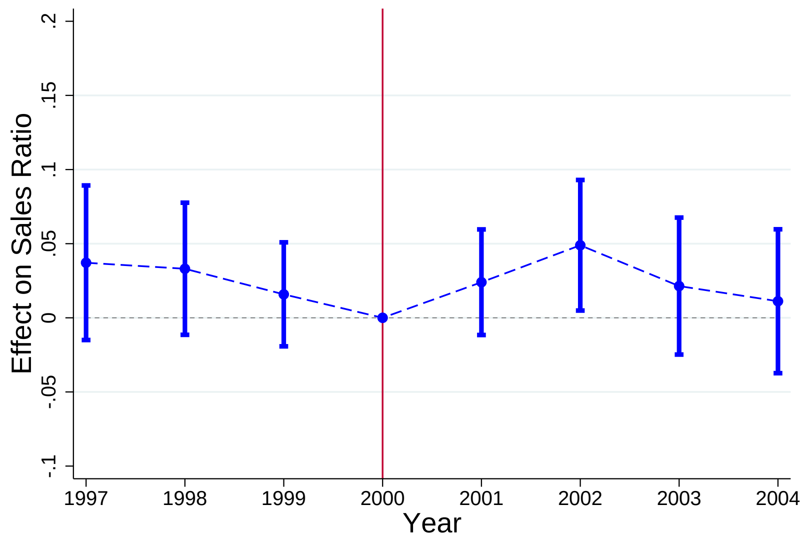
(c) 2002



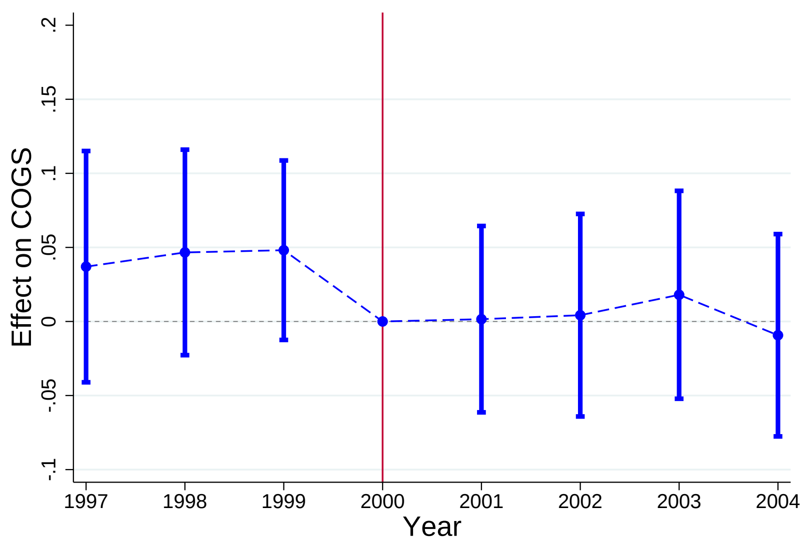
(d) 2003

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 (4). 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 Sales and COGFR



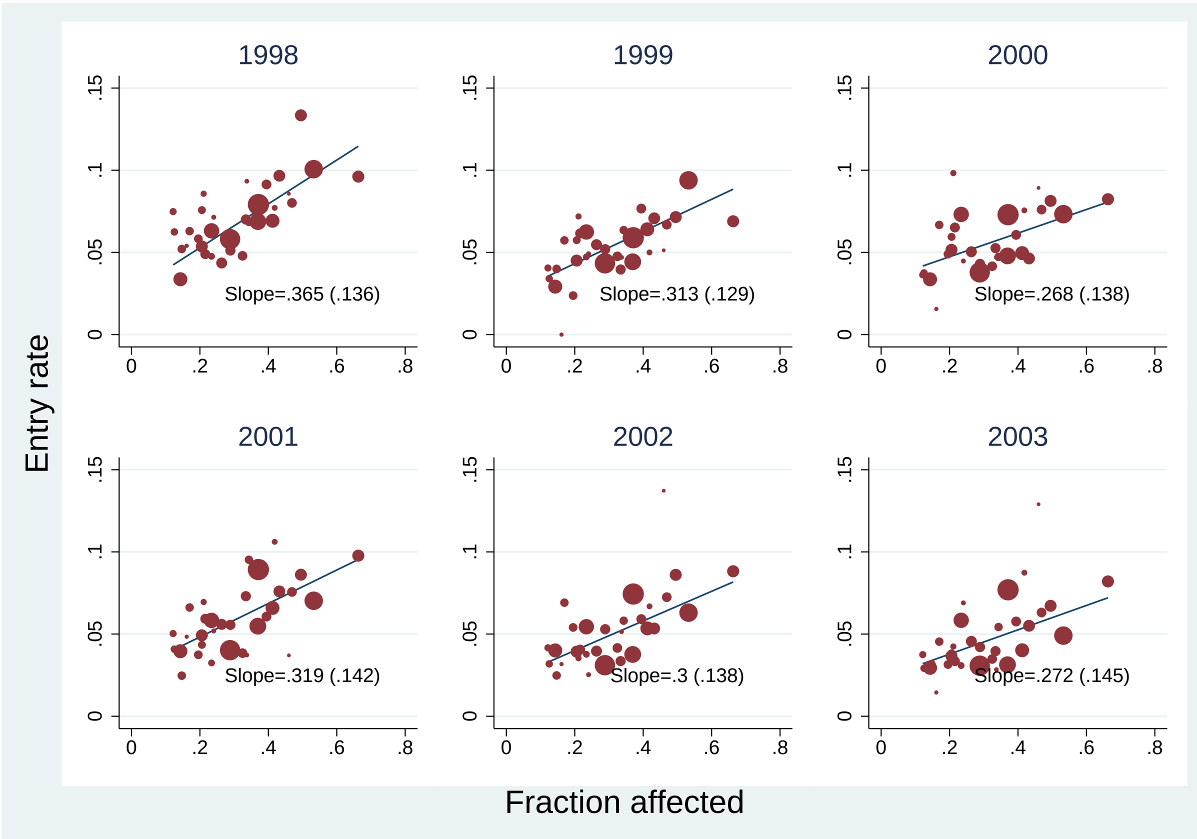
(a) Sales



(b) Cost of Goods Purchased for Resale

Notes: Panel (a) 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 3). We subtract goods for resale to only focus on the production part of the production function. Panel (b) depicts results from a firm-level regression of percentage changes (relative to 2000) in cost of goods for resale on fraction affected by the minimum wage (beta coefficients with its confidence intervals from equation (3)). Cost of goods purchased for resale is goods purchased only for resale and not for production purposes. Expenses on intermediate goods are not affected by the minimum wage change. Both Panel (a) and Panel (b) include firms that died in the regression. Controls and industry dummies are also included in the regressions.

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.