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ABSTRACT

Fighting "Low Equilibria" by Doubling the Minimum Wage? Hungary's Experiment*

In January 2001 the Hungarian government increased the minimum wage from Ft 25,500 to Ft 40,000. One year later the wage floor rose further to Ft 50,000. The paper looks at the short-run impact of the first hike on small-firm employment and flows between employment and unemployment. It finds that the hike significantly increased labor costs and reduced employment in the small firm sector; and adversely affected the job retention and job finding probabilities of low-wage workers. While the conditions for a positive employment effect were mostly met in depressed regions spatial inequalities were amplified rather than reduced.

JEL Classification: J38, P23, R23

Keywords: minimum wages, transition, regional labor markets

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1. INTRODUCTION

A decade of transition to the market economy brought Hungary's employment ratio, similarly to most other transforming countries, down from one of the highest in the world to one of the lowest in Europe. There have been substantial inequalities behind the low aggregate rates. While the metropolitan areas and most of the highly industrialised zones of Central and Eastern Europe gradually recovered from the transformational recession the region still has a belt of low-employment, low-wage provinces along the former Soviet border and spots of similar districts within each country. The employment prospects of low-educated people, and the Roma community in general, remained faint all over the region. Most regions and occupations losing jobs also experienced falling relative wages during the transition.

One reason why an array of demand-side employment policies ranging from tax holidays to investment and wage subsidies fail to exert substantial influence on the depressed labor markets can be that the problems, in fact, lie on the supply side. In a low-employment, low-wage environment the net gains from searching and working are modest as the fixed costs of search and work are relatively high, and the welfare benefits (most of them regionally unadjusted and many of them flat rate) provide relatively generous support to the non-employed. Household strategies and lifestyles relying on social transfers, home production, and casual jobs are gaining ground and provide alternatives to employment in the formal sector. Since in most depressed regions business density is particularly low, and only a few industries are present, more firms can use their monopsony power to restrain wages. The labor market may get to a bad equilibrium characterised by low levels of labor force participation and employment, low wages, little job search, frictions and incentive problems.

By widening the gap between wages and income while unemployed the government can potentially break a low equilibrium of this type but the policies aimed at this end such as the cuts of benefits, restrictions placed on the hidden economy, and increases in the minimum wage are particularly risky. If mis-diagnosed, a depressed labor market may react to such treatments producing even more frictions, falling employment, and growing poverty. The Hungarian government in office between 1998 and 2002 made two attempts at breaking what it diagnosed as a low equilibrium state primarily explained by supply-side deficiencies. Following cuts in the unemployment benefits in January-May 2000[1] the minimum wage was doubled in two steps starting from January 1, 2001. In this paper we look at the short-run consequences of the first hike of 57 %.[2]

Section 2 gives a brief overview of the arguments for a potentially positive employment effect. Section 3 discusses the magnitude of the shock to the labor market and justifies the choice of fields for a deeper analysis. Section 4 presents descriptive statistics suggesting that the overall effect of the hike was most probably negative with the strongest influence expected in the small-firm sector and marginal (exit and entry portal) jobs.

Section 5 shows that 43.5 % of the workers employed in small firms (5-20 employees) were directly affected by the minimum wage hike, and average wages ought to have increased by 11.4 % overnight to close the gap opened by the new minimum wage. The elasticity of the average wage with respect to this gap ranged between 0.65 and 0.77 suggesting higher levels of compliance in high-unemployment regions. Given an estimated wage elasticity of employment of –0.41 in our preferred specification of a simultaneous equations model this implied elasticites of employment with respect to the gap between –0.26 and –0.31 suggesting more severe employment cuts in depressed regions. The results from alternative specifications are similar. Large firms more exposed to the minimum wage shock also had less favourable employment records in 2001 but the estimates were not statistically significant in the small sample available for the analysis. Data on medium sized firms are not available.

[1] The maximum duration of UI was shortened from 12 to 9 months, and the UA benefit available for UI exhausters on a means-tested basis was replaced by a social benefit (SB) administered by local governments. The restrictive eligibility conditions of SB (participation in public works whenever called, the real estate of the recipients mortgaged) led to a two-digit fall in the probability of benefit receipt among the UI exhausters without remarkable effect on their job finding rates (Galasi and Nagy 2001b). Likewise, the cutting of UI eligibility had no measurable impact on the exit to jobs rate of the recipients (Galasi and Nagy 2001a).

Section 6 compares workers paid the minimum wage to those paid marginally above the minimum wage by means of a discrete time duration model of jobloss using quarterly Labor Force Survey (LFS) data. Minimum wage workers were twice as likely to lose their jobs 4-12 months after the hike than their marginally better paid counterparts. This result relates to workers who spent a minimum of two years and an average of seven years in their jobs prior to the hike. The risk of jobloss was unrelated to local unemployment in the control group but positively related among minimum wage workers.

Section 7 estimates the job finding probabilities of the low-wage unemployment insurance benefit (UI) recipients relative to the low-skilled recipients using grouped data from 172 labor offices and 54 months in 1998-2002. Depending on specification the finding is that the relative exit rate of the low-wage unemployed fell by 7-9 percentage points in 2001 and further 2-3 percentage points in January-July 2002 with the effects being similar across regions.

Taken together, the results yield support to the view that a huge increase in the minimum wage comes at the cost of jobs. The finding that depressed regions were equally or even worse affected than others underlines the importance of the `classic' demand-side reactions even in environments where the conditions of a positive effect are met.

2. MINIMUM WAGES AND EMPLOYMENT

Most, if not all, models calling into question the conventional wisdom of negative employment effects of the minimum wage abandon the assumption of an infinitely elastic supply curve facing the firm. The benchmark model assuming a positively sloped labor supply curve is that of a local monopsony. Since the firm is the only buyer it can only hire additional workers by increasing the wage. If, as generally assumed, the marginal worker's wage can be increased only if the wages of other workers are also increased the

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[2] The data for the study of the second hike are not available as yet. Furthermore, the identification of minimum wage effects seems much more difficult because of the shocks caused by the world-wide recession and Hungary's parliamentary elections of 2002.

firm's marginal expenditure on labor curve is steeper than its supply curve. Employment is set at the point where marginal expenditure equals marginal revenue while the wage is set at the lowest level compatible with that level of employment given the supply curve. A hike in the minimum wage can decrease the firm's marginal expenditure on labor and lead to a concomitant increase in wages and employment at the cost of the monopsony rent. A 'too high' minimum wage hike, however, may shift the marginal expenditure regime upwards in the vicinity of the current employment level and result in a loss of jobs.[3]

The modern theories developed to understand why the employment effects can be negligible or even positive are generalisations of the monopsony model in several ways. Early models of a positive employment effect were developed many years before any supporting evidence was available. In a partial equilibrium model Mincer (1976) showed that depending on how the turnover rate and the elasticities of demand and supply relate to each other employment can increase, and unemployment can fall, as a result of a minimum wage hike.[4] The efficiency wage theorem and the search friction models of Mortensen (1988) and Burdett and Mortensen (1989) were also widely known before a series of seminal empirical papers including Card (1992a,b), Katz and Krueger (1992) and Card and Krueger (1994, 1995) opened new chapter in the study of minimum wages.

These studies, together with Machin and Manning (1994) and Dolado et al. (1996) in Europe, found weak, zero, or even positive effects on employment. The time-series evidence from this period also suggested weaker negative impact than before (Brown, 1999). This challenge gave new impetus to both the empirical and the theoretical research and also affected the political debate over minimum wages.

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[3] The discussion and graphic presentation of the argument are found in labor economics textbooks including Ehrenberg and Smith (2002).

[4] For employment to be higher and unemployment lower $s>\eta>\sigma$ should hold where σ is the turnover rate, and η and s are the demand and supply elasticities respectively.

The logics underlying the benchmark monopsony model can indeed be applied to a variety of market structures. While single-employer towns are infrequent many firms can be the sole buyer of certain skills in the local labor market. Even in large, open markets mobility costs provide nearly all enterprises with a degree of monopsony power. A multitude of firms can be supply-constrained by search frictions - inasmuch as a higher minimum wage reduces these frictions by encouraging job search and promoting competition for job openings it can have positive impact on employment even if some firms go bankrupt. (Ahn and Arcidiacono, 2003). Wages, productivity and employment can simultaneously rise if workers respond by increasing their effort as predicted in the efficiency wage models of Rebitzer and Taylor (1991) and others.[5] Distortions on the labor market can also drive the outcome far from the competitive prediction. In the monopsonistic competition model of Bashkar and To (1999) the direction of change depends on the share of fixed costs within labor costs with higher shares predicting an increase in employment and vice versa. In a model of dual wage determination with unskilled wages set by the government and skilled wages negotiated in a Nash-bargain Cahuc et al. (2001) find positive employment effect under the condition that unskilled and skilled workers are highly substitutable.

The competitive theory has not been overthrown by these theoretical innovations – nor was its central prediction discredited by the available empirical findings. The Card-Krueger results were themselves subject to criticism by Neumark and Wascher (1992) and others, and a whole array of papers found significant negative impact of higher minimum wages including Deere, Murphy and Welch (1996) and Neumark and Wascher (2002) in the US, Abowd, Kramarz and Margolis (1999) in a US-France comparison, Bell (1997) and Maloney and Mendez (2003) in Colombia, Carneiro (2000) in Brazil, Castillo-Freeman and Freeman (1991) in Puerto Rico, El Hamidi and

[5] A difficult point to explain in the efficiency wage models is why the parties wait for the government instead of increasing the lowest wages - once they all gain from it. It might be argued that a higher minimum wage enforces managers to search for well-functioning incentive schemes, something they were not deeply interested to do before, and in this sense the minimum wage hike can indeed be interpreted as a *cause* of higher employment and productivity.

Terrell (1997) in Costa Rica (for the upper tiers of the industrial minimum wage/average ratios), Pereira (1999) in Portugal (for teenagers); Rama (2000) and Alatas and Cameron (2003) in Indonesia (for small firms). The effects found in these papers are often small and restricted to certain segments of the market but they lend support to the orthodox approach while a similarly massive body of evidence confirming the predictions of the 'new economics of the minimum wage` is hard to find in the current empirical literature.

The Hungarian government's motives to radically increase the minimum wage were presented in popular form (the hike will 'restore the prestige of work', combat `living on benefits', 'whiten the black economy', and so on) but the political slogans actually drafted some key arguments of the new economics of the minimum wage. It was repeatedly argued in interviews and press briefings that a higher minimum wage stimulates work effort, leads to higher productivity, makes the hiring of additional workers easier, and by widening the gap between benefits and wages creates proper incentives for paid employment and job search. [6]

The forces to be offset by these mechanisms were by no means negligible. The available evidence suggested that Hungarian firms were responsive to labor costs particularly in the low-wage segment of the market. Estimating differenced conditional labor demand equations for homogeneous labor Kőrösi (1998, 2000) found relatively low but significantly negative short-run elasticities of between –0.55 and –0.65 in 1992-95 and –0.31 and –0.33 in 1996-97. A translog cost function model estimated in repeated cross-sections by Kertesi and Köllő (2002b) yielded rather high own-wage elasticities of –0.8 for skilled and –1.4 for unskilled labor on average in 1996-1999.[7] A positive employment effect offseting the shock to labor demand

[6] The stereotype of general support on the political left and opposition on the right does definitely not help in understanding the case. The hikes were decided by a right-wing government explicitly committed to increasing the welfare of the middle class and promoting the competitiveness of domestic businesses including exporters - an unusual candidate for aggressive minimum wage policies. At least the first hike was opposed by the largest trade union federation of socialist orientation (MSZOSZ) worried about the potentially adverse employment effects (Berki, 2003). Alleged EU guidelines were repeatedly mentioned but have never been documented. The claim is nevertheless credible and familiar from the Indonesian and Puerto Rican cases where pressures on the part of the US and other trade partners played important role.

[7] Unlike a differenced dynamic model the cross-section translog estimates are long-run elasticities with the 'long run' lasting from one equilibrium state to another.

could be expected, if anywhere, in the country`s most depressed regions characterised by low participation rates, low search intensity, high benefits relative to wages, high shares of the informal economy, relatively high travel-to-work costs, and more frequent occurance of monopsony settings.

3. **THE MAGNITUDE OF THE MINIMUM WAGE SHOCK**

The mandatory minimum wage, introduced in 1989 by Hungary's last communist government, relates to gross monthly earnings net of overtime pay, shift pay and bonuses, is legally binding and covers all employment contracts. For part-timers accounting for only 3.5 % of all employees the minimum is proportionally lower. In 1990-1998 the adjustments were negotiated annually by a national-level tripartite council and entered into effect in the annual budgets while under the cabinet of 1998-2002 the wage floor was set unilaterally by the government.

At its introduction the minimum amounted to 34.6 % of the average wage, a level deep below the European average and slightly higher than that of Spain, the laggard within the EU. (Compare with Dolado et al. 1996). During a decade of transition the relative value of the minimum was falling and reached a low of 29% in 2000. This trend was broken by the two hikes bringing the index up to 39% in 2001 and 43% in 2002 – levels still lower than the EU average but higher than those of the UK, Spain or Portugal.

An alternative indicator of how the minimum wage relates to the 'market wage' is the fraction of workers paid at or near the mandatory wage floor. This ratio was slowly increasing from less than 1% in 1989, 3% in 1997 and 5% in 2000.[8] The ratio jumped to 12.1% in 2001 and 17.3% in 2002 - while before the hikes Hungary was located in the lower part of the OECD range together with Austria, Belgium, the Netherlands, Denmark, and the US in only twelve months it shifted to the position of a heavy outlier with an extraordinarily high fraction of minimum-wage workers.

[8] The ratios relate to the share of full-time employees paid 95-105 % of the minimum wage in firms employing more than 5 workers in 1999-2002, 10 workers in 1995-98, and 20 workers in 1989-95. The data are calculated using the Wage Survey (WS henceforth). The opposite movement of the Kaitz index and the share of minimum wage workers was explained by the build-up of a sizeable low-wage population.

Both the welfare and cost effects of a minimum wage hike depend on its influence on average wages rather than the percentage increase of the minimum itself. A benchmark indicator of a firm's or occupation's exposure to the minimum wage hike can be defined as:

$$(1) \quad \omega = [w^{*}F + w_{H}(1-F)] \big/ [(w_{F}\, F + w_{H}(1-F)]$$

with F denoting the fraction of workers paid below the new minimum wage at the moment of the hike, w_F being their average wage, w_H standing for the average wage of other workers and w^* for the new minimum wage. The formula measures the average wage gap to be filled on the day of the hike under the assumption that all sub-minimum wages are raised to the level of the new minimum and there is no further instantaneous wage and employment adjustment. As such, ω is a hypothetical benchmark that does *not* measure the actual response of average earnings but serves as a useful starting point for the study of actual evolutions. We prefer ω to the customarily used F as the latter ignores valuable information on the pre-hike earnings level of low-wage workers while, in fact, both indicators rely on the same assumptions in measuring exposure.

The minimum wage hike was estimated to cause an immediate shock of 2.33% to the economy-wide average monthly base wage at January 1, 2001 under the conditions mentioned above. Calculating exposure for the interactions of 5 age groups, 3 educational levels, and 4 quartiles of the 150 micro-regions by unemployment we get that group-level exposure varied in a wide range between 0.3% and 16.7% with the estimates being 1 % for workers with secondary and higher qualification and 6 % with primary or lower education; 1 % for workers older than 45 and 6.1 % for those under 25; 1.7 % for the 'best' ¼ of regions and 3.6 % for the least fortunate quartile. The average wages of workers under 35 years of age with primary or vocational education who lived in the `worse' half of the regions were expected to rise by as much as 9.7-16.7 % at the moment of the hike. The dispersion of the group-level ω-s is shown later. [9]

Table 1: Compliance with the law – Selected indicators

		Source, date, unit of observation
Paid below the minimum wage (%)		
Full time employees	1.9	WS, May 2001, payroll data
Full time employees	3.6	LFS, April-June 2001, individuals
Full time employees	1.4	EJS, April 2001, individuals
All employees	5.5	LFS, April-June 2001, individuals
All employees	2.6	EJS, April 2001, individuals
Paid as a subcontractor (%)	1.5	EJS, April 2001, individuals
Elasticities with respect to ω		
∂(base wage)/ ∂ ω	0.96	WS, May 2001/May 2000 60 interactions of age×education×region [1]
∂(earnings)/ ∂ ω	1.00	WS, May 2001/ May 2000[2] 60 interactions of age×education×region
∂(earnings+taxes)/∂ω	1.00	FR, 2001/2000, 432 industries [3]
∂(all payments to persons +taxes)/∂ω	0.95	FR, 2001/2000, 432 industries [4]

Notes:
1) OLS estimates from a model wherein the log change of the group's average base wage was regressed on group-level ln(ω) and a dummy for higher education background.
2) Earnings include overtime pay, shift pay, and bonuses
3) 2sls estimates from a two-equations system composed of a wage equation (RHS variables are the log change in productivity, fraction unionised, mean regional unemployment, and sector dummies) and an employment equation (RHS variables are the log change of output, the minimum wage shock indicator ω, share of small firms in the industry, and sector dummies). Monetary aggregates were discounted using industry-level PPI (32 distinct values). Wages, employment and hence productivity were assumed to be endogenous.
4) Other payments include per diem, honoraria and various casual pecuniary benefits that can also be paid to persons who are not counted as employees.

Checking whether the increased minima were actually paid is essential in a country where non-compliance with the state regulations has been traditionally wide-spread. The indicators collected in *Table 1* unequivocally suggest high levels of compliance. The proportion of full-time workers paid below the new minimum in May was only 1.9 % according to payroll data reported in the WS. Likewise, only 1.4 % of the full-time workers interviewed

[9] The data were taken from the WS. Since our wage observations related to May we spoke of sub-minimum wages if a worker's wage was lower than w*/(1+r) where r was the rate of wage inflation between May and the time of the minimum wage increase.On the basis of the monthly wage data available at the Central Statistical Office we set *r* at 0.32 % per month between May and November 2000. The data on monthly earnings in December are severely affected by year-end premia and bonuses on the one hand, and year-end holidays on the other, and were therefore disregarded.

in the UI Exit to Jobs Survey (EJS), and 3.6 of the respondents of the Labor Force Survey (LFS), reported gross monthly earnings below Ft 40,000 in April-June 2001.[10] These are upper-bound estimates since unpaid leave and other disturbances can temporarily result in sub-minimum monthly earnings.

There are hidden ways of escaping the regulations, however. First, firms may employ their workers full-time but register them as part-time and pay sub minimum monthly wages. The fact that the fraction earning sub-minimum wages within *all* wage earners including part-timers was only 5.5 % in the LFS and 2.6 % in the EJS suggests that these practices were of minor importance. Second, firms may fraudulently lay off their workers and contract with them as 'trade partners'. According to the EJS only 1.5 % of the low-wage UI recipients who found a job in April 2001 concluded a business contract with the employer as opposed to 64.7 % receiving a fixed salary and 33.8 % paid an hourly wage. Third, and most importantly, firms can increase the base wage and reduce some side payments exempt from the regulations. The pecuniary offsets, however, unveil in comparisons of base wages with broader concepts of worker compensation. Most side payments, especially shift pay and overtime pay, are set as percentages of the base wage therefore regular monthly earnings are expected to rise at approximately the same rate as do base wages if the firms comply with the regulations. As shown in the bottom of the table, the estimates of the elasticity of earnings and labor costs with respect to ω (using grouped and industry-level data) fall close to unity.

While an elasticity of 1.0 does correspond to the expectation under full compliance, no pecuniary offsets, and no strong spillover effects it may actually result from other scenarios. Therefore compliance is further tested in *Figure 1* based on matched wage observations from May 2000 and 2001. In lack of any panel data on wages we created a quasi-panel of individuals observed in the 2000 and 2001 waves of the WS. Though individuals cannot

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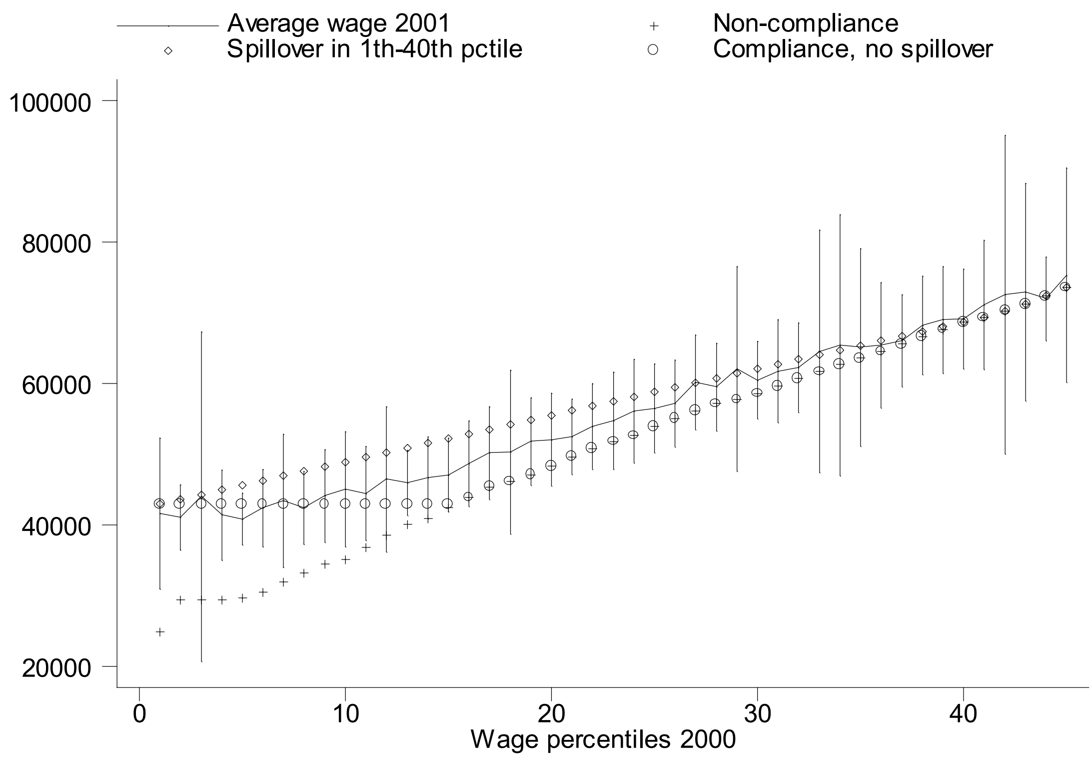
[10] The bias from not distinguishing between base wages and earnings is predictably minimal as these fall close to each other at the lower tiers of the wage distribution. The average earnings and base wages of workers earning less than Ft 40,000 in May 2001 were Ft 35,025 and Ft 34,736, respectively. (WS)

be identified across waves of the WS one can try to match those with the same firm ID, plant location, gender, year of birth, level of education and four-digit occupational code. Excluding multiple matches we got a panel of 52,057 full-time employees with information on their wages. For this sample we could calculate the mean and standard deviation of the May 2001 wages along the percentiles of the May 2000 wage distribution – these are indicated by the connected curve and the vertical splines in Figure 1.

Figure 1: Average wages in May 2001 in the 1st-40th percentiles of the May 2000 wage distribution – Comparison with predictions from three scenarios

WS Individual Panel 2000-2001, N=52,057



Sum of squared residuals$\cdot 10^{-6}$ in the 1st-40th percentiles:
Non-compliance 47.3, spillover 9.03, compliance with no spillover 7.49.

Actual wages are compared to predictions from three scenarios. Each assumes that wages grew by the product of inflation and GDP growth in the 40th-100th percentiles – this assumption predicts actual wage increases almost perfectly in the upper tiers of the distribution. Scenario 1 marked with ”+” assumes that the minimum wage hike had no effect at all, wages grew by the main rule all along the distribution. Scenario 2 marked with circles makes the assumptions underlying ω: wages below Ft 40,000 in January 2001 were upgraded but other wages were not affected. Finally, Scenario 3 marked with diamonds assumes that wages in the 1st percentile were upgraded, the 40th-100th percentiles were unaffected, while in the 2nd-39th percentiles growth was also influenced by a spillover effect. (The rates were approximated with linear interpolation). The data clearly reject the assumption of no effect – Scenario 1 crudely under-estimates the rate of wage growth in the lower tail of the distribution. Scenario 2 slightly under-estimates wage growth in the 1st-40th percentiles and the opposite holds for Scenario 3. The sum of errors is lowest with Scenario 2 assuming full compliance and minor short-run spillover effects. The best fitting scenario, generating predictions very close to the observed data, would be a mixed one assuming minimum wages in percentiles 1-10, no effect above the 40th percentile and successively lower rates of spillover in percentiles 11-40.

The wage evolutions would certainly deserve closer inspection but for this paper, concerned with the sign of the employment effect, the important result is that the first minimum wage hike was certainly effective causing an unexpected and severe shock to the Hungarian labor market.

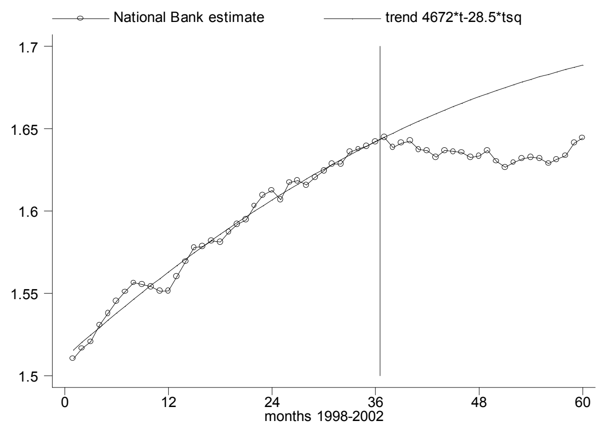
6. CHANGES OF EMPLOYMENT – DESCRIPTIVE STATISTICS

This section presents descriptive statistics raising the conjecture that the minimum wage hike of 2001 came at the cost of jobs. *Figure 2* indicates a sudden break in the growth of aggregate employment in January 2001. The dotted line depicts seasonally adjusted employment in the non-agricultural private sector in 1998-2002.[11] The path of employment growth prior to 2001 could be precisely approximated with a quadratic form indicated by the solid curve.[12] Employment growth was gradually slowing down during the observed period with the monthly growth rates falling from 0.027% in 1998-1999 to 0.018% in 2000. Had this slow-down continued in 2001-2002, as

[11] Though the LFS results are published quarterly the data allow the calculation of monthly employment levels. The data used here were seasonally adjusted at the National Bank of Hungary. The authors are greatful to Barnabás Ferenczi of the Bank for sharing the adjusted series. The seasonally adjusted quarterly figures relating to the whole economy, as published by the Central Statistical Office, depict a similar path of employment.

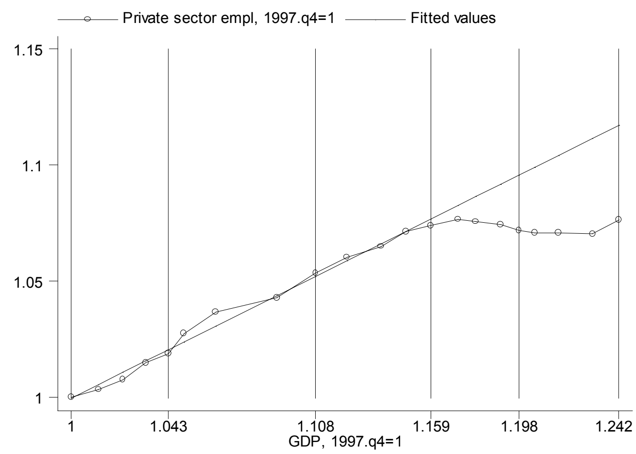
depicted by the extrapolated part of the curve, aggregate employment should have grown further by 2.8% in January-December 2001 when it actually decreased by 0.2 %.

Figure 2
Seasonally adjusted monthly employment 1998-2002
LFS, adjusted by the National Bank, agriculture and the public sector excluded, Head counts, million



1998 1999 2000 2001 2002

Figure 3
GDP and seasonally adjusted quarterly employment 1998-2002.
LFS and National Accounts, agriculture and the public sector excluded
1997 4th quarter = 1.



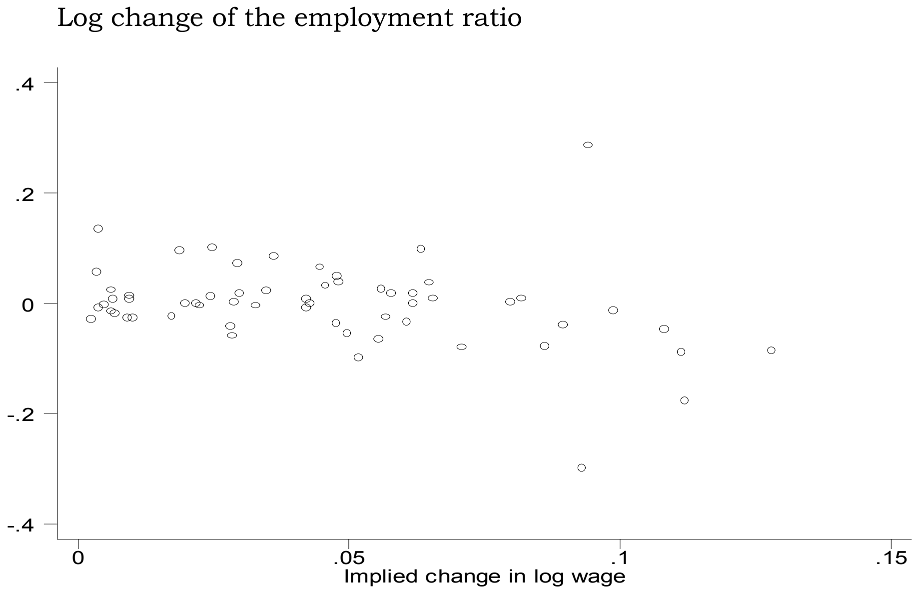
1998 1999 2000 2001 2002

This remains true if we consider the path of employment relative to GDP. Since the start of employment's recovery from the transformational recession the economy followed a path at which 1% growth of GDP was associated with 0.5 % growth of employment as shown in *Figure 3*. The chart has GDP on the horizontal axis and employment on the vertical axis both normalized to their 1997 4th quarter levels. The rate of GDP growth is captured by the distance between the vertical lines separating the years while the relation between employment and GDP is captured by the slope of the fitted line. For lack of monthly GDP data we turn to quarterly figures. The economy was slowing down in 2001 but a moderate *fall* of employment after the minimum wage hike was clearly at odds with the experience of the preceding years. Even with the slow-down of growth employment would have been expected to rise by about 1.7 % in 2001 and 1.8% in 2002 at the given rates of GDP growth.

[12] We have a technical and a substantive reason not to include the pre-1998 period in the time series. First, seasonally adjusted monthly data are not available for 1992-97. Second, and more importantly following almost a decade of deep crisis employment started to increase in 1998.

The data on the age×education×region interactions introduced earlier suggests that a group's employment record in 2001 was closely related to its exposure to the minimum wage shock (*Figure 4*). The slopes of the best-fitting lines across the scatter plot were estimated for all groups and 40 unskilled groups.[13] The weighted data suggested employment elasticities with respect to ω of –0.45 for all groups and –1.29 for unskilled groups with the unweighted estimates being slightly higher in absolute value.

Figure 4: Change of employment and the minimum wage shock in 60 groups
2000. 4th quarter - 2001. 4th quarter. Data: LFS



The patterns observed in 2000-2001 were clearly at odds with previous experience. As shown in *Table 2,* the groups exposed to strong minimum wage shock in 2001 had average or better than average employment records in the preceding years. In 1998-1999 the group level ω-s (as of 2000) and employment change were uncorrelated while in 1999-2000 the low-wage groups experienced a rise in their relative employment probabilities.

Another argument against interpreting the observed correlations as minimum wage effects, tentatively either, refers to the potentially non-

[13] The regressions have employment levels on the left hand, controlled change in the number of working age adults who do not attend school and do not recieve old-age pension. This is required because the rotation of the LFS sample leads to variations in the size of the groups observed and therefore employment. The coefficients of this variable are close to unity as expected under random fluctuations in group size. For lack of sufficient observations workers above the retirement age were dropped. The equations include an age dummy to control for the effect of increasing the retirement age by one year on 1999-2000.

neutral impact of the recession following September 11, 2001. However, similarly to the aggregate statistics the grouped data suggest that low-wage employment started to fall immediately after the minimum wage hike. This can be tested by regressing the employment ratios of the groups on ω in panels comprising quarters 1, 1-2, 1-3 and 1-4, respectively and showing that the coefficients were weakly affected by the extension of the panel period (The results are available on request).

Table 2 : Employment and exposure to the 2001 minimum wage increase
Dependent: log change of employment.

	1998-1999		1999-2000		2000-2001	
	All	Unskilled	All	Unskilled	All	Unskilled
ln(ω)	.2106	-.0081	.5051	-.3450	-.4195	-1.234
	(0.68)	(0.02)	(2.33)	(0.87)	(1.95)	(3.22)
Δln(POP)	1.1565	1.2291	1.1175	.9714	.9940	.9125
	(13.11)	(10.17)	(24.3)	(8.92)	(13.42)	(9.37)
Age>54	.1436	.1434	.2159	.0671	.0586	.0440
	(3.57)	(2.55)	(8.38)	(3.31)	(2.33)	(1.2)
Constant	-.0053	.0198	-.0013	.0185	.0055	.0620
Adj R^2	.7811	.7352	.9153	.8283	.7616	.7584
Goups	60	40	60	40	60	40

OLS regressions, t-values in brackets. Data on employment: LFS 1998-2001 Q4. Data on exposure: WS 2000. Employment is defined on ILO-OECD grounds. The groups are weighted with their population size in the base period. ω denotes exposure of the group to the minimum wage increase in 2001. *POP* stands for the working age population less old-age pensioners and students in 1999-2001, and the working age population in 1998-99. Due to change in the registration of student status in 1999 the definition used later was not applicable.

The tables and charts presented in this section raise the suspicion that the minimum wage hike adversely affected employment but they obviously do not identify this effect. For a deeper analysis we chose fields with an eye on the potential effects, macro-economic relevance, and data availability. Most empirical studies of the minimum wage concentrate on youth employment and low-wage industries in both the US and Europe but we deviate from this tradition because the data summarised in appendix tables B1 and B2 hints at more important dimensions.

The vast majority of the Hungarian minimum wage workers are 25-54 year old with only 20 % being younger than 25 and a mere 2 % being teenager. Half of the minimum wage workers have more than 20 years of experience. By contrast, a high concentration of minimum wage workers is observed with short tenures: about 20-25 % of the minimum wage workers had tenures shorter than one year; nearly 40 % worked less than 2 years and 60 % had less than 5 years with the firm - while only 4.4 % spent less than 5 years on the labor market. Estimates from data sets recording both experience and tenure also suggest that the probability of low wages is affected much stronger by tenure than experience.

The differences by personal characteristics and industrial affiliation depict the familiar picture. Women and low-skilled workers are slightly over-represented among the minimum wage workers. The young and those employed in high-unemployment regions are more likely to earn low wages, and so do the employees of the light industry, trade, hotels and restaurants, road transport, services and insurance. [14] No, or very few, low-wage workers are found in petroleum mining and refining, banking, research and development, public transport, and the tobacco industry. Budapest and the regions around Lake Balaton, Hungary's major tourist zone, have relatively high fractions of low-wage workers. These are, however, not the most important dimensions: firm size and the firms's level of productivity have much stronger impact. The pseudo-R^2 of a logit model explaining the occurence of sub-minimum wages in 2000 (Table A1) for instance increases from 0.18 to 0.35 by the inclusion of these two indicators and a model with only these two variables has a better fit (0.24) than a model including gender, experience, education, region and industry.

We conclude from these data that the study of minimum wage effects should primarily focus on the small-firm and/or low-income sector of the economy, and the flows between short-tenured 'entry-portal jobs' and non-employment. These, rather than teenage employment or the low-skilled labor market in general seem to be the adequate fields for the empirical investigation.

5. EMPLOYMENT IN SMALL FIRMS 2000-2001

When the minimum wage is increased the actual changes of wages (Δw) are expected to exert stronger than usual influence on employment given a truly exogeneous variation in wage adjustment. This effect can be captured by comparing the α_2 coefficients over time of conditional labor demand equations similar to (2) with *L*, *q* and *w* standing for employment, real output and real average wages, and ***X*** containing controls. This is a useful tool at the researcher`s disposal when no direct information is available on firms' exposure to the minimum wage increase. As an example see Kim and Taylor (1995) on Califoria`s retail trade industry.

$$\Delta \ln(L)_i = \alpha_0 + \alpha_1 \Delta \ln(q)_i + \alpha_2 \Delta \ln(w)_i + \alpha_3 X_i + v_i \qquad (2)$$

More reliable models become available when the researcher has information on *F,* ω, or some other indicators of exposure. Machin, Manning and Rahman (2003) estimate an equation analogous to (2) by instrumenting *w* with their 'shock to the average wage' variable, a close relative to our ω, and treating other firm-level variables as exogeneous.

The impact of minimum wages via average wages can be explicitly modeled and incorporated into a system of equations in several ways. If the regressors in a wage equation similar to (3) are assumed to be exogeneous (3) can be substituted to (2) to estimate $\partial L/\partial \omega = \alpha_2 \cdot \beta_1$, a parameter capturing the combined effect of compliance and the wage elasticity of demand for labor.

$$\Delta \ln(w)_i = \beta_0 + \beta_1 \ln(\omega)_i + \beta_3' Z_i + u_i \qquad (3)$$

There is a strong case, however, for leaving the question of exogeneity open when one works with firm-level average wages. Consider the simplest case when ω measures the shock to a firm employing workers with a particular type of skill that is the only input. Even though labor is homogeneous in terms of skills wage dispersion may arise as a result of individual productivity differentials or Becker-Stigler type bonding. What happens after a minimum wage hike depends on the nature of wage dispersion on the one hand, and complementarities and substitution on the other. If all workers are equally productive but wages differ because of bonding, and the firm

[14] This simply indicates that the agents are paid the minimum wage as the fixed part of their remuneration.

insists on its bonding scheme, only an employment effect is at work. In other cases the firm substitutes low-productivity (low wage) for high-productivity workers – this results in $\Delta w>\omega$, a growth in productivity, while output and employment is affected by both substitution and scale effects. If low-wage and high-wage labor are complements the firm can also react to the minimum wage shock by dismissals biased against high-wage workers – this can result in $\Delta w<\omega$ and a fall in output. Finally, the firm can choose to cut the unregulated components of the compensation package and achieve $\Delta w<\omega$ without productivity loss, at least on the short run. (On the long run it risks losing its low-wage workers to other firms.) In order to cope with endogeneity and some other factors affecting compliance and employment we write a simultaneous equations system (4-5) with *q*, *L*, and *w* treated as potentially endogenous.

$$\Delta \ln(w)_i = \beta_0 + \sum_{j=1}^{4} \beta_{1j}\{\ln(\omega)_i \cdot U_{ij}\} + \beta_2 \pi_i^0 + u_i \qquad (4)$$

$$\Delta \ln(L)_i = \alpha_0 + \alpha_1 \Delta \ln(q)_i + \alpha_2 \Delta \ln(w)_i + \alpha_3 \ln(K/L)^0{}_i + \alpha_4 X_i + v_i \qquad (5)$$

where $\Delta ln(w)$ is the log change of the PPI-adjusted labor cost, ω is our benchmark of the shock to the average wage also discounted using the PPI, the U_j-s are dummies for region quartiles by unemployment, *L* stands for employment, $\Delta ln(q)$ is the PPI-adjusted log change of value added; $(K/L)^0$ and π^0 are the base-period capital-labor ratio and profit respectively, and $\boldsymbol{X}$ comprises industry and region dummies.

Wages are assumed to grow faster given ω in profitable firms able to share their income with their employees. A higher level of compliance is expected in depressed regions where failures to pay the new minimum wage would menace with the quitting of core workers. Therefore the minimum wage shock variable is interacted with regions allowing ω having different coefficients in high- and low-wage environments with the β_{1j}-s expected to rise as we move from low to high-unemployment regions. Employment is assumed to respond to output and wages uniformly across regions as suggested by Kőrösi's (2000) estimates. The equation includes the base period capital-labor ratio under the assumption that capital intensive firms are less likely to react with dismissals on the short run. The 18 region dummies control for supply shifts and 10 industry dummies allow for demand shocks unobserved in Δq, and changes in technology.

The Durbin-Wu-Hausman test rejects the exogeneity of labor costs but not of output. (The null of the test is that the residuals from regressions with $\Delta ln(q)$ and $\Delta ln(q)$ on the LHS and all instruments on the RHS have zero coefficients in the employment equation). With Δq treated as exogenous the system passes both the overidentification and the exclusion restrictions tests allowing the estimation with 3sls. (The test statistics are presented together with the estimation results).

The estimates may also be affected by measurement error that is generally difficult to control, this paper being no exception. Let y denote nominal sales with p and P standing for firm-level and industry-level prices so that $\Delta ln(p)=\Delta ln(P)+\Delta ln(\pi)$. While in the structural equation we have $\Delta ln(q)=\Delta ln(y)-\Delta ln(p)$, and a similar expression for real wages, in an estimable equation the observed nominal changes are discounted with $\Delta\ln(P)$ and the residual becomes $u=\nu+(\alpha_1+\alpha_2)\Delta ln(\pi)$. For ω to be a valid instrument $E(u\omega)=0$ is required, which may not be the case if the within-industry variations in price movements are strongly correlated with the level of wages and hence ω. Since industrial shocks are quite often non-neutral the IV does not certainly mitigate the impact of measurement bias.[15] Fortunately, the economy was free of major shocks until the end of 2001.

Data. The data on annual average employment, labor costs and output were taken from the year-end financial reports (FR) of firms employing 5-20 workers. The sample was drawn from the population of enterprises interviewed in the 2000 and 2001 waves of the WS. A unique advantage of this sub sample is that firms employing 5-20 workers report individual data

[15] This is less of a problem in the Machin-Manning-Rahman paper since they analyse a homogeneous sample of residential care homes at the time the minimum wage was reintroduced in the UK.

on each and every employee within the WS thus allowing a precise measurement of ω.[16]

Sample selection. In each cross-section wave of the WS small firms are randomly selected within strata formed by four-digit industries. Given the target population of small firms and the sampling quota the expectation was that about 350 firms could be followed in a short panel. In fact, the number of small enterprises observed in 2000 *and* 2001 amounted to 2,008. This regrettably calls into question the alleged independence of the cross-section samples but fortunately provides us with a sizable longitudinal sample drawn from a populace of firms heavily exposed to the minimum wage shock. Out of the 2,008 firms 1,818 had all the variables required for the estimation.

Table 3: Small firm panel 2000-2001 - Probits of sample selection

Sample	Dependent variable = 1	Number of employees	Fraction low-wage	Lossmaker in 2000	Pseudo R2	Nobs
Small firms observed in 2000	Also observed in 2001	.0012 (2.43)	-.1074 (4.96)	-.1239 (5.93)	.0209	2,874
Small firms observed in both 2000 and 2001	Has complete data	.0036 (2.51)	-.0099 (0.60)	-.0581 (3.17)	.0166	2,008

*) The table shows the marginal effects

The probits in *Table 3* check how the estimation sample was selected from the base-period population of 2,874 small firms observed in. Firms also observed in May 2001 were larger, generated profit in the base period; and had fewer workers paid below the new minimum wage. The dropouts were predictably hit harder by the minimum wage hike therefore our model underestimates the extent and potentially adverse implications of the minimum wage shock. The estimation sample within the panel is also biased for larger firms and profit makers but does not systematically differ from the rest of the sample in terms of the base-period fraction of low-wage workers.

Results. The descriptive statistics of the estimation sample are presented in *Table A1* of the Data Appendix. The median firm had 13

[16] As discussed in the Data Appendix all firms above this size category are obliged to fill in the WS questionnaire but they are expected to provide individual data on only 10% sample of their workers.

employees of which 5 was paid below the new minimum wage, and was hit by an average wage shock of 11.2 %. *Table 4* gives an overview of changes between 2000 and 2001 broken down by the size of the minimum wage shock. Real labor costs grew and employment fell as a function of exposure.

Table 4: Small firms in the estimation sample – Performance in 2000-2001

Characteristics in 2000			Mean log change 2000-2001				
Minimum wage shock (ω, percent)	Fraction low-wage	Mean ω	Average wage	Labor cost/PPI	Employ-ment	Output	Number of firms
0	0	0	.125	.062	.045	.046	468
0-10	.274	.032	.158	.091	-.007	-.034	632
10-25	.741	.166	.279	.177	-.054	-.007	319
> 25	.959	.359	.399	.305	-.090	-.032	399
All firms	.435	.119	.224	.146	-.020	-.017	1,818

Table 5 presents the 3sls estimates The wage setting equation suggests that the elasticities of real labor costs with respect to the minimum wage shock ranged between 0.66 and 0.77 with high-unemployment regions having slightly higher elasticities. Generally we find a lower level of compliance than earlier, using grouped or industry-level data on the whole economy. Base-period profits have the expected sign. The labor demand equation suggests an output elasticity of 0.25 and a labor cost elasticity of –0.41. The implied elasticities of employment with respect to ω range between -0.27 and -0.32 depending on region.

Results from the alternative specifications are similar. Estimating the labor demand equation after substituting (4) to (5) yields $\partial L/\partial\omega = \alpha_2 \cdot \beta_1 = -0.31$. The single equation IV with ω as the only instrument for w yields $\partial L/\partial w = -0.43$. In these cases, too, the estimates for the interactions are higher in 'bad regions'. In a reduced form employment equation estimated with OLS $\partial L/\partial w = -0.03$ reflecting strong attenuation bias due to the neglected endogeneity. Adding ω results in zero coefficient for w and $\partial L/\partial\omega = -0.33$ reinforcing that employment was affected by the variations in exposure rather than the variations of w at given levels of exposure.

Table 5: 3sls estimates of employment and wages in small firms 2000-2001

	Coeff	St. error
Dependent: log change in real labor cost		
Log minimum wage gap × 1st region quartile	0.6554***	0.0537
Log minimum wage gap × 2nd region quartile	0.7071***	0.0674
Log minimum wage gap × 3rd region quartile	0.7629***	0.0678
Log minimum wage gap × 4th region quartile	0.7703***	0.1049
Profit 2000	0.0003**	0.0001
Constant	0.1247	
Chi-sq	305.861	0.0000
Dependent: log change of employment		
Log change of output	0.2522***	0.0242
Log change of labor cost	-0.4089***	0.1029
Fixed assets/worker 2000	0.0006*	0.0004
Industry dummies (10)	Yes	0.0794[4]
Region dummies (18)	Yes	0.3322[4]
Constant	0.1299	
χ^2	140.125	0.0000
Specification tests		
Exogeneity of labor cost (P>|t|)[1]	0.001	
Exogeneity of output (P>|t|)[1]	0.272	
Overidentification ($P(\chi^2)$)[2]	0.051	
Exclusion restrictions (P>F)[3]	0.002	

Durbin-Wu-Hausmann test. 2) Sargant test 3) Joint significance of the excluded exogenous variables. 4) F-test of joint significance. The cases are weighted with base-period employment.
Significant at the ***) 0.01 level **) 0.05 level *) 0.1 level

What do these estimates tell about the magnitude of the minimum wage effect? A low-wage firm (ϖ=.36) with 20 employees located in a low-unemployment region was estimated to lose 1.9 jobs as a result of the minimum wage hike while its counterpart operating in a high-unemployment area lost 2.4 jobs. The differences in case of 10-25 % share of low-wage workers (ϖ=.165) were lower with implied losses of 0.6 and 0.7 jobs. At the average shock and elasticity the loss amounted to 0.7 jobs. Firms with 5-20 workers had a combined employment of 328,000 in the base period according to the available statistics. The results thus suggest that the minimum wage hike eliminated about 12,000 small-firm jobs mostly in the depressed regions – a huge loss in the Hungarian context.[17]

Contrast with previous experience. When a national minimum wage is increased the variations in firm's exposure is entirely determined by variations in their *level* of wages. If low wages in year *t* are generally conducive to employment cuts in year *t+1* this linkage is captured in our model as a `minimum wage effect'. Indeed, low wages may result from poor firm performance indicative of forthcoming employment cuts, or signal lags in the process of wage adjustment so that the periods of low wage levels are followed by periods of fast wage growth and employment cuts. The results in *Table 6* show that the changes of employment were unrelated to the level of wages and the share of low-wage workers in 1999-2000 unlike in the period of the minimum wage hike.[18]

Table 6: Base period wages and employment growth, univariate OLS

Dependent variable: log change of employment	1999-2000		2000-2001	
Base period log average wage	-0.014	0.40	0.056	3.49
Fraction low-wage in 2000 (w<Ft 38,685 → 1.-16. percentiles)			-0.121	4.51
Fraction low-wage in 1999 (1.-16. percentiles → w<Ft 34,953)	0.004	0.06		

Data source: FR 1999, 2000, 2001. Number of firms 1.046 in 1999-2000 and 1,818 in 2000-2001

Puzzles. Readers familiar with the empirical literature on labor demand may find our output elasticity estimate of 0.25 suspiciously low. In fact, this result is consistent with those of Kőrösi (2002) who estimated short-run output elasticities of between 0.29 and 0.35 for 1996-99 using a differenced model and firm-level data. It is also consistent with the intuition that many small firms in services, trade, or tourism may have difficulties to adjust the number of workers to the fluctuations in turnover. The question of how labor productivity was raised in many hard-hit low-wage enterprises seems a harder nut to crack. (Real sales per worker grew by –1.3, -0.2, 4.3 and 8.8 % on average as we move from low to high levels of ω). We do not address the topic in this paper concerned with wether employment fell or rose following the minimum wage hike. The preliminary investigation suggests no shift from wage to non-wage costs (indicative of outsourcing) but the composition of the workforce did change in favor of more skilled workers in the most affected group of firms. The data base used here provides no information on

[17] In these calculations we take into account that the direct impact of ω on q was isignificant as suggested by a parameter of 0.009 (0.14) in the first-stage regression.

hours but the LFS data do not indicate any growth in working hours between 2000 and 2001. The possibility that increased effort and better incentives also played a role, as proposed in efficiency wage models, can not be excluded.[19]

8. THE JOBLOSS RISKS OF MINIMUM WAGE WORKERS, 2001

A minimum wage hike decided at a government office randomly divides the low-wage population into two parts. Workers whose pre-hike wages were just above the new minimum are likely to have similar human capital endowments and occupational characteristics to those who earned just below the line but their employers have no straightforward motivation to fire them as they are continued to be paid at their marginal products. These workers can also be indirectly affected because of wage spillovers or because the firm's demand falls for the whole category of labor they belong to. Still there is likely to be a difference in the jobloss probabilities of those directly affected and those who are not, or only indirectly, influenced. Following this line of reasoning in this section we study how wages affected the jobloss hazards of full-time employees interviewed in the LFS Supplementary Survey of 2001 2nd quarter, the only wave since 1993 when respondents were asked about wages.

We distinguish a *treatment group* (workers who were paid exactly the new minimum wage) from a *control group* (those who earned slightly more than that) and estimate the two group's jobloss probabilities in 2001 using a discrete time duration model. Our approach is similar to that of Currie and Fallick (1996) and Abowd et al. (1997) both comparing workers paid the minimum wage with those earning just above the limit.

Model. As shown in Jenkins (1995) by choosing the quarterly employment spells of individuals as the units of observation the exit hazard

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[18] Data for firms employing 5-10 workers are only available since 1999. The short panels built for firms with 11-20 workers in 1997-98 and before are too small for a similar kind of analysis (contain only about 100 firms).

[19] The sampling rule of the WS does not allow a similar study of medium or large firms because they reoprt individual data on a 10 % sample of their staff - insufficient for calculating F or ω except for enterprises employing 500 or more, thus reporting data on 50 or more, workers. Low-wage large firms had better than average employment records in 1999-2000 and worse than the average in 2000-2001 but the estimates are not significant at conventional levels within the small samples of 337 and 332 firms, respectively.

from a stock sample can be estimated with a logit model augmented with a baseline hazard function f(t):

$$h(t) = Prob(t < \tau < t+1) = L(\alpha X + \beta w + \gamma T) + f(t) \qquad (6)$$

where t and τ denote time spent in the job, X stands for individual and environmental characteristics, *w* denotes a set of wage level dummies, and *T* represents calendar time.

Sample restrictions. Workers in marginal jobs change employer frequently so they they tend to have high jobloss probabilities and low wages at any given point of time. In order to minimise the influence of this correlation we estimate model (6) for workers who spent at least two years in their jobs prior to the survey date. (The treated and the controls spent 6.7 and 7.3 years in their jobs on average.) Workers were followed by the end of 2001. The reason of not following them for 5 quarters, the longest possible period allowed by the LFS design is that the second minimum wage shock exposed the control group to the same type of risk that hit the treatment group in 2001. The analysis is restricted to full-time employees. After these restrictions the estimation sample contains 22,315 quarterly employment spells.

The wage brackets. The wage data relate to gross monthly earnings as reported by the respondents, or estimated from the net figure by the CSO.[20] We distinguished between workers paid 90-110 cent of the minimum wage (*treatment)* and those earning 110-125 % (*control*), and three other categories earning even higher wages. [21] The brackets were chosen to maximise the distance between the treatment and control groups in terms of exposure to the minimum wage increase. Data from the WS-based individual panel introduced in section 3 suggested that the maximum distance is achieved by setting the brackets at 90-110 and 110-125 % - in this case the estimate is

[20] The gross figure is what labor contracts include in Hungary.
[21] Workers earning less than Ft 36,000 were excluded from the estimation sample because this category apparently includes many workers planning to retire. Furthermore, we observed high wage mobility between this and other brackets suggesting that sub-minimum wages are often explained by temporary reasons.

that 83.6 % of the treatment group was affected but only 54.4 % of the controls was unaffected. The control group is thus far from being first-best but we are not deeply concerned with it because, since the vast majority of the misclassified workers are found in the control group, the model underestimates the treatment effect [22]

Results. There was a large and statistically significant difference between the members of the treatment and control groups in their probability of becoming unemployed in the 2nd -4th quarters of 2001 as shown by the parameters of 1.05 (3.00) versus 0.15 (0.31) significantly different from each other at the 0.04 level (*Table 7*).

Table 7: Exit from employment 2001 2nd-4th quarters
Discrete time duration model, multinomial logit form

Left employment for:	Unemployment		Non-participation	
	Coefficient	Z	Coefficient	Z
Male	-.0948512	-0.31	-.5614574	-3.10
Age	.5115863	3.39	-.3338472	-6.75
Age squared	-.0063266	-3.38	.0041778	7.01
Unskilled blue collar.	-.1559254	-0.32	-.4750061	-1.20
Semi-skilled blue collar	.1277137	0.33	.0850755	0.34
Skilled blue collar	.2456568	0.64	-.004839	-0.02
Unemployment (log)	-.0166451	-0.08	.3708437	2.54
Public sector	-.9144718	-1.65	-.0598691	-0.22
Union member	-.7294738	-1.82	.1420791	0.63
Tenured job	-.3426703	-0.62	-.6559291	-2.08
Wage Ft 36,000-44,000 (treatment)	1.059692	3.00	.1078196	0.44
Wage Ft 44,000-50,000 (control)	.1494378	0.31	.0600268	0.19
Wage Ft 75,000-100,000	-.5535763	-1.14	-.4572246	-1.63
Wage Ft >100,000	-.0494438	-0.10	-.3114691	-0.97
2001 4th quarter	.3108904	1.09	.3152385	1.79
Exp (-tenure in years)	4.424675	2.61	-.265705	-0.09
Constant	-15.56376	-5.06	2.867735	2.50
Nobs	22,315			
-log likelihood	1302.12			
Pseudo R^2	.0525			
F-test b_treatment=b_control (unemployment)	4.13 (.0421)			
F-test b_treatment=b_control (non-participation)	0.02 (.8906)			
Coefficients from an alternative specification:				
Wage Ft 36,000-44,000 (treatment) * U	3.967194	2.13	3.643163	2.37
Wage Ft 44,000-50,000 (control) * U	-1.366391	-0.38	2.348137	1.27
Wage Ft 50,000-75,000 * U	-3.870989	-0.93	3.903593	2.68
Wage Ft 75,000-100,000 * U	-10.57837	-1.56	-.7622896	-0.29
Wage > Ft 100,000 * U	-8.755421	-1.55	3.209584	1.20

Reference categories are white collars, wage Ft 75,000-100,000, tenure>18 months.
Standard errors adjusted for clustering by individuals
Data source: LFS 2001 2nd quarter Supplementary Survey, LFS 2001 3rd and 4th quarters <epanel38.dta>

22. It might also be mentioned at this point that the second minimum wage hike that became a credible promise/threat by the autumn of 2001 also biases the observed treatment effect downwards.

While the exit to non-participation hazards were equal in the two groups minimum wage workers were much more more likely to lose their jobs after at least two years of uniterrupted tenure *and* try to get back to work through active job search.

The exit to unemployment hazards increased with regional unemployment *within* the minimum wage group while at higher wages the regional differences were negligible. However, in this case the equality of the coefficients can be rejected only at the 0.09 level, while the parameters for exit to non-participation are statistically equal.

The estimated quarterly outflow to unemployment rates of a 25 year old male worker with 5 years of tenure were 0.243 and 0.119 % in the treatment and the control groups, respectively. These rates suggest rather long prospective tenures. For sake of illustration we can estimate the fraction staying in their jobs for the rest of their career ($(1\text{-h})^{160}$ given a retirement age of 65 and assuming constant hazard). This yields 67.5 and 82.6 % in the control and treatment groups, respectively.

The sensitivity of the results to compositional differences between the treatment and control groups seem minimal. For workers with at least two years of tenure the exit to unemployment logit has only three significant parameters: the wage, age, and tenure. The average age of workers in the treatment (control) groups were 39.2 (40.0) years, and the average tenure was 6.67 (7.33) years. The predicted exit to unemployment rate setting aother variables at their default and unemployment at zero was 0.0167 in the treatment group. Using the average age and tenure of the control group the estimate is practically unchanged (0.0168) while the prediction for the control group is 0.0068, less than half of the treatment group's exit rate.

The admittedly small but statistically significant effects encourage us to conclude that the minimum wage workers, most of them paid above their marginal product right after the minimum wage increase, had higher probability of becoming unemployed in July-December 2001 than their observationally similar counterparts paid marginally higher wages. We also find weak evidence that the minimum wage workers of depressed regions were worse off than their counterparts in other districts.

9. OUTFLOWS FROM UNEMPLOYMENT 1998-2002

The competitive theory of the minimum wage predicts a fall in the job finding probabilities of those unemployed who were paid below the new minimum wage prior to losing their jobs. Whether the actual outcome was predominantly shaped by this classic demand-side effect, or by more complex mechanisms offsetting the adverse impact of the minimum wage shock, is tested using a panel comprising 172 labor offices and 54 months from January 1998 to June 2002.

For each office and month we know the number of low-wage and high-wage workers in the UI stock at the beginning of the month and their exit to job rates (h^{LW} and h^{HW}) during the month. The same information is available for low-skilled and high-skilled workers (h^{LS} and h^{HS}).[23] The return to comparing low-wage and high-wage workers is clearly minimal as these groups largely differ in terms of skill levels and exposure to economic shocks. In order to get closer to a sensible comparison we study how the exit rates of *low-wage* workers related to the exit rates of *low-skilled* workers before and after the minimum wage hike.

Model. The procedure we follow is closest in spirit to that in Deere, Murphy and Welch (1996) analysing teenage employment after increasing the US federal minimum wage. We estimate equation (7):

$$\ln(h^{LW})_{it} = \beta_1 \ln(h^{LS})_{it} + \beta_2 \ln(U)_{it} + \beta_3 MD + \beta_4 YRD + c_i + v_{it} \qquad (7)$$

where h_{it} is the exit rate at office *i* month *t*, *LW* and *LS* refer to low-wage and low-skilled workers, respectively, and *MD* and *YRD* are month and year dummies. The long-run averages of the office-level h^{LW}/h^{HW} ratios can differ depending on the typical duration of unemployment of the low-wage and

[23] \`Skilled\` workers are those with secondary and higher education. \`Low-wage\` workers are those receiving lower than average unemployment benefit (see further details in the text). No information is available on the number of low-wage and high-wage workers *within* skill categories and *vice versa*.

unskilled groups – the resulting time-invarying fixed effects are captured by the c_i-s.[24] The expectations are β_1=1, $\beta_2 \leq 0$ (it is more difficult for low-wage workers to find jobs when the market is depressed) and $\boldsymbol{\beta_4}$=[0] unless some unexpected shocks divert the h^{LW}/h^{HW} ratio from its long-run average. Prior to the minimum wage hike the year effects are expected to fall close to zero but a significant break is anticipated in 2001. The relative exit rate of low-wage workers may have fallen more (or less) in high-unemployment regions – this is tested by interacting a dummy for the post-hike period with regional unemployment to allow $\boldsymbol{\beta_4}$ to differ across provinces.

The equation has to be instrumented for obvious endogeneity on the one hand, and possible correlation between the residual and h^{LS} on the other. Some sort of regional shocks may exert particularly strong impact on h^{LS} relative to h^{LS}. When whole plants are closed or opened employers screen their workers/applicants more carefully and while doing so they interpret low-wages as a signal of low productivity – this establishes a link between the changes of h^{LS} and the residual. The sign of the correlation is a priori unclear since h^{LW} is expected to rise *less* when h^{LS} is rising, and fall *more* when h^{LS} is falling. We instrument h^{LS} with its t-1 period value.[25]

Distinction between high and low wage workers. The labor offices record the recipients' earnings in the four calendar quarters preceding their current unemployment spells. Since the benefits are earnings-related they also provide an indirect measure and we use them as a proxy of the wage. Though pre-unemployment earnings are known they relate to different time periods - computing the present value of past earnings case by case would have enormously increased the costs of data collection. Data from the EJS show, however, that the benefit is indeed a good proxy of the wage: 98.7 % of the workers receiving lower than average benefits earned less than the

[24] The mean benefit divides the population of UI recipients to fractions of varying size depending on the regions' wage level. The difference in the skill endowments of the median recipient and the median low-wage recipient tends to be smaller in low-wage regions, which provides an explanation for the regional fixed effects. Regional differences in the share of seasonal low-wage industries add a further component to c_i.

[25] Further complications might arise from the fact that the composition this month's inflows have an impact on the composition of next months' stock. We neglect this feedback because job finds account for less than 1/3 of the total outflows from the UI stock and the latter is also largely affected by the inflows. It is also worth noting

median wage prior to unemployment, and 87.2 % of the high-benefit recipients had higher than median wages. Altogether, 92.3% of the UI recipients could be correctly classified as 'low-wage' or 'high-wage' on the basis of the benefit.

Low-wage versus low-skilled workers. The available data suggest that the exit rate of *all* low-wage workers (h^{LW}) relative to the exit rate of *all* low-skilled workers (h^{LS}) can be considered a crude approximation of the wage-level specific job finding rate ($h^{LW}|LS$) within the unskilled group. In particular, the EJS suggested that as much as 81.4 % of the low-wage workers were low-skilled but only 48.8 % of the low-skilled were low-wage.[26]

Table 8: The exit to job rate of low-wage UI recipients 1998- 2002
Panel estimates using monthly data from 172 labor offices, January 1998 – June 2002

Dependent: log exit rate of the low-wage recipients	FE - IV Missing values replaced		FE - IV Cases with missing values dropped		FE Missing values replaced	
Log exit rate of low-skilled	1.0242	17.13	0.9560	15.96	0.8120	105.51
Regional unemployment	-0.0191	0.64	-0.0224	0.82	-0.0444	2.70
1999	-0.0199	1.80	-0.0199	1.97	-0.0274	2.68
2000	-0.0062	0.48	0.0051	0.41	0.0267	2.59
2001	-0.0883	5.88	-0.0742	5.26	-0.0451	4.36
2002	-0.1173	6.56	-0.0960	5.83	-0.0712	5.43
Constant	-0.0150	0.007	-0.2346	1.08	-0.5988	21.79
Within R2	0.7190		0.7363		0.7409	
Overall R2	0.7773		0.7846		0.7818	
Number of observations	9116		8975		9116	
Wald chi2	738744		890437		F=1502.44	
F-test. H_0: β_1=1	0.16	0.6857	0.47	0.4909	591.96	0.0000
Alternative specifications:						
(i)						
2001-2002 dummy	-0.0871	8.42	-0.0778	8.07	-0.0536	7.35
(ii)						
2001-2002 × 1st quartile	-0.0863	5.31	-0.0782	5.37	-0.0589	4.12
2001-2002 × 2nd quartile	-0.0548	3.15	-0.0563	3.60	-0.0441	3.09
2001-2002 × 3rd quartile	-0.0967	5.69	-0.0873	5.63	-0.0605	4.18
2001-2002 × 4th quartile	-0.0992	5.21	-0.0819	4.58	0.0521	3.59

Notes. The unemployment rates relate to the area served by the office and defined as the ratio of registered unemployment to the working age population. The Budapest office areas are treated as one unit.

Results. The evolution of the quarterly relative exit to job rates of low-wage workers are shown in *Table A3*. The estimation results of equation (7) are shown in *Table 8*. In 2 % of the cases the exit rate of low-wage workers were

that there is no straightforward link between the flows of the UI system and unemployment. In 2000 less than 20% of the ILO-unemployed received UI. (MT 2001 227-230).

[26] If the wage level is inferred from the benefit, as happens in this section, the respective proportions are 82.1 and 56.7 %.

zero – in one version these cases were excluded and in the other the zeros were replaced assuming the outflow of ½ person. The qualitative results are identical.

In the fixed effects model β_1 falls short of unity while in the IV it does not significantly differ from the expectation of $\beta_1=1$. When unemployment increases the relative exit probability of the low-wage recipients falls but this effect is not significant at conventional levels. The month effects (not displayed) hint at changes in the composition of the low-wage unemployed pool over the year.[27] Most importantly, the results suggest that the job finding probability of the low-wage unemployed relative to the unskilled dropped by 7-9 percentage points in 2001 and further 2-3 percentage points in January-June 2002.

In *Table 9* the pair-wise equality of the year effects are tested using the coefficients from version B. The parameters for 1998-2000 are not significantly different from zero and each other. Those for 2001 and 2002 are strongly different from zero and any of the previous year effects. 20001 and 2002 are different at the .95 but not at the .99 level of significance. Treating the pre- and post-hike periods as different regimes by estimating the same equation with a dummy for 2001-2002 provides a coefficient of -.087.

Table 9. F-test for the equality of year effects

	1999	2000	2001	2002
1998	3.25	0.22	34.5**	43.1**
1999		0.89	17.8**	27.3**
2000			58.5**	63.1**
2001				4.66*

Significant at the **) 0.01 *) 0.05 level

Interacting this 'regime dummy' with dummies for the four quartiles of regional unemployment (treating the h^{LW}/h^{LS} ratio of all regions in 1998-2000 as the reference) yields statistically equal parameters for all regions. In evaluating this result one has to take into account that while the fixed effects

[27] The sum of the β_3 coefficients is above zero. Presumably, the reason is that during the Fall and Winter when unskilled job opportunities are scarce many young, low-wage unemployed return to employment from `unemployment holiday`. This increases h^{LW} relative to a falling h^{LS}.

capture the long-term differences in h^{LW} relative to h^{LS} they do not control for the regional variations in the *changes* of the two exit rates in response to a wage shock. In low-wage region more unskilled workers are low-wage therefore h^{LW}/h^{LS} changes little when h^{LW} falls. In high-wage regions a wage-related shock affects h^{LW} stronger than h^{LS} so the ratio falls substantially. Id this bias exists it leads to an underestimation of the effect hitting the low-wage regions.

The UI register is incapable of providing a full picture on how the job finding probabilities of the jobless were changing after the minimum wage hike. Only 14 % of the working age non-employed population (excluding students and pensioners) received UI at the eve of 2001 – a small and non-randomly selected minority. Unfortunately, the LFS provides no data on the previous wages of the non-employed, preventing the researchers from a comprehensive study of outflows from non-employment. We see no reason to assume, however, that the robust changes observed with the insured unemployed are specific to this particular segment of the labor market.

10. CONCLUSIONS

Every bit of information we could analyse suggested that the Hungarian government's decision to increase the minimum wage by 57 % in 2001 implied a loss of employment opportunities. Despite the brutal price shock the immediate effect did not seem dramatic, however. Similarly to Rama (2000) and Alatas and Cameron (2003) analysing Indonesia – the country providing the closest analogue to Hungary's minimum wage experiment - we found no significant link between exposure to the minimum wage hike and subsequent employment change with large firms. In the same time the small firm sector lost about 3 % of its jobs in less than a year, and the job retention and job finding probabilities of low-wage workers deteriorated. The depressed regions were more severely affected despite their conditions that favour a positive employment effect. The results underline the relevance of the classic framework in predicting minimum wage effects.

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Data appendix

FR – Financial Reports

The FR data contain the tax sheets of enterprises, collected by the Ministry of Finance. The sample used in this paper is restricted to firms observed in the WS. The reports include a full account of assets and liabilites and of annual intakes and costs including the annual average number of employees, wages and taxes, sales revenues, material and other costs, and depriciation. The firms can be identified across waves. The descriptive statistics of the estimation sample are presented in Table A1.

Table A1: Descriptive statistics of the small firm panel (N=1818)

Variable	Mean	Median	Standard deviation
Employment 2000	12.7	13	4.44
Employment 2001	13.6	12	14.30
Value added 2000 (mFt)	227.5	91.5	712.3
Value added 2001 (mFt)	251.3	98.0	891.6
PPI 2000-2001	1.066	1.063	0.025
Average wage 2000 (mFt)	0.824	0.583	0.901
Average wage 2001 (mFt)	0.978	0.700	0.992
Profit 2000 (mFt)	3.27	1	38.3
Assets/worker (mFt) 2000	4.816	1.333	29.1
Fraction low-wage 2000	0.434	0.355	0.392
Minimum wage shock (ω)	0.119	0.043	0.144

WS – Wage Survey

The National Labor Centre's Wage Survey is an annual survey conducted in May 1986, 1989, and each May since 1992. It covers a representative sample of firms and 10% random samples of their workers. In the waves used in this paper the sampling procedure was the following (i) the firm census provided by the CSO serves as the sampling frame (ii) it is a legal obligation of each firm employing more than 20 workers to fill in a firm-level questionaire and provide individual demographic and wage data on a 10 % random sample of the employees. (iii) it is a legal obligation of each budget institution irrespective of size to fill in an insitution-level questionaire and provide individual demographic and wage data on all employees (iii) Firms employing less than 20 workers according to the census are sampled by the NLC in a sampling procedure stratified by 4-digit industries. The firms contacted by the NLC are legally obliged to fill in an firm-level questionaire and provide individual demographic and wage data on all employees. The cases are weighted to ensure representativity. An individual weight (w_1) stands for the number of workers represented by the respondent given the sampling quota within his/her firm. The original survey does not contain information on firm-level non-response. Comparing employment in the target population by 4-digit industry and firm size with the sample a second weight (w_2) was attached to firms by the authors of this paper. The final weights ($w_1 \cdot w_2$) restore representativity under the assumption that non-response is uncorrelated with variables in the calculations. The number of individual observations varied between 180 and 185 thousand in 1999-2001.

LFS – Labor Force Survey

The LFS is a representative quarterly household survey conducted by the Central Statistical Office since 1992. Data are collected about each member of the surveyed households and an 'activity qustionaire' is filled with those aged 15-74. The survey has a rotating panel structure with each quarter 1/6 of the sample dropped after spending 6 quarters in the survey, and replaced with a randomly chosen new cohort. The number of observations varied between 82 and 85 thousand in 1999-2001. The individuals can be identified across waves. The cases are weighted by the CSO to ensure representativity. All calculations in this paper used these weights.

Table A2: Jobloss - Descriptive statistics of the estimation sample

	Mean	St. dev.
Exit to unemployment	0.30	
Exit to non-participation	0.73	
Male	.5245165	.4994098
Age	40.27192	10.36101
Unskilled blue collarl	.0759896	.2649874
Semi-skilled blue collar	.1689385	.374706
Skilled blue collar	.3502658	.4770638
Regional unemployment	.0925442	.0597517
Public sector	.1741024	.379206
Union member	.2502028	.4331394
Tenured job	.9617706	.191754
Wage Ft 36,000-44,000	.1522365	.3592581
Wage Ft 44,000-50,000	.0932583	.2908006
Wage Ft 75,000-100,000	.1919952	.3938781
Wage Ft >100,000	.1718567	.3772643
2001 4th quarter	.434425	.4956924
Tenure in job (years)	7.292149	2.872526
Number of spells	22,315	

LFS Supplementary Survey April-June 2001

The LFS does not collect wage data. In this particular wave respondents working as employees or cooperative members (22,415 out of 30,485 workers employed by the ILO-OECD definition) were asked to tell their last months's gross or net earnings. The gross value of net earnings was calculated by the CSO using PIT tables. We used the gross figures as reported by the CSO and weighted the cases followed in a spell panel with their base period weights of April-June 2001.

NLC Office-level Exit to Jobs Panel 1998-2002

The data base was built in the National Labor Centre in September 2002 using data from Hungary's 175 labor offices . It contains aggregate stock and ouflow to jobs data broken down by the level of education (primary or lower; vocational; secondary and higher), and the level of the benefit (lower or equal/higher than the national mean). The stock figures relate to the first day of the month and the flows relate to the month. Three offices were involved in reorganisation during the period of observation and were dropped from the sample analysed in this paper. The unemployment rates attached to the offices are ILO-OECD counts divided by the population of working age, as estimated by the CSO, in the territory of the office. Job finds exclude entry to public works or other programs for the unemployed. The number of recipients leaving UI for reasons other than job finding is also available.

Table A3. Exit from unemployment to jobs (quarterly means)

	Q1	Q2	Q3	Q4
	Low-skilled (primary or vocational education)			
1998	.074	.063	.056	.034
1999	.066	.063	.055	.036
2000	.071	.076	.063	.047
2001	.080	.082	.069	.041
2002	.077			
	Low-wage (benefit<mean)			
1998	.065	.063	.059	.034
1999	.058	.060	.057	.036
2000	.064	.074	.066	.047
2001	.068	.074	.067	.038
2002	.062			
	Low-skilled = 1			
1998	0.897	0.983	1.047	1.059
1999	0.904	0.970	1.031	1.019
2000	0.895	0.976	1.058	1.007
2001	0.857	0.909	0.969	0.930
2002	0.811			..

*) Benefit<mean. Source: Data provided by the National Labor Centre

NLC EJS – National Labor Centre Exit to Jobs Survey, April 2001

Following a similar survey in April 1994 the NLC interviewed all workers leaving the UI register because of finding a job between March 22 2001 and April 7 2001. The workers were interviewed when they contacted the offices to collect the documents necessary to take up employment. They were saked about their minimum and maximum expected gross monthly earnings in the first months after being hired. The file used in this paper contains the data of 105,957 recipients in the stock on 22 March 2001 and interviews with 9,131 workers finding a job. Of them, 8,811 workers provided wage data. The wage and benefit concepts used in the paper are (i) gross monthly earnings in the four calendar quarters spent in employment prior to the last UI spell adjusted for wage inflation between the time of entry to UI and March 2001. (ii) The mean of the minimum and maximum expected earnings (iii) the monthly value of the pre-tax daily UI benefit assuming 30.5 days a month.

Appendix Tables

Table B1: The probability of sub-minimum wage in May 2000 – Logit
(Full-time employees of firm employing at least 5 workers, budget sector excluded)

Dependent: earned less than 38,685 Ft in May 2000	Odds ratios	Z
Male	0.7341	-15.33
Experience 1-4 years	2.0834	18.12
Experience 5-9 years	1.3192	10.27
Experience 25-35 years	0.6194	-21.84
Experience >35 years	0.6221	-17.13
Primary education	2.8669	40.32
Vocational education	1.6279	21.54
Higher education	0.4054	-21.14
Joined the firm in 1999	1.4367	14.71
Firm size: 5-20 employees	12.3800	42.36
Firm size: 21-50 employees	6.5751	30.86
Firm size: 51-300 employees	3.0361	19.06
Firm size: 301-1000 employees	1.6717	8.48
Firm size: 1001-3000 employees	0.8407	-2.59
Foreign ownership	0.7009	-8.64
Private domestic	1.8482	18.40
No majority owner	2.2570	11.24
Value added/worker <1.39 mFt	7.8319	61.73
Value added/worker 1.39-2.19 mFt	3.0962	33.50
Value added/worker 2.19-4.22 mFt	1.6077	13.95
Regional unemployment 2nd quartile	1.1645	5.68
Regional unemployment 3rd quartile	1.4400	13.65
Regional unemployment 4th quartile	1.5927	14.04
Budapest	1.1829	5.97
Lake Balaton	1.3359	3.99
Industries (6 largest and 6 smallest odds ratios out of 55)		
Insurance	70.8793	38.52
Hotels and restaurants	6.3720	23.39
Forestry	5.3203	13.99
Transport other than railways, airways, and public transport	4.8603	20.09
Services other than business-related, cultural, and public	4.8408	13.93
Textiles	4.3558	17.51
…		
Electric energy	0.8669	-0.81
Research and development	0.3697	-2.22
Tobacco	0.1610	-1.33
Banking	0.1513	-11.28
Public transport	0.0130	-1.96
Petroleum mining and refining (no minimum wage workers)	..	..
Number of observations	119,739	
Pseudo R2	0.3487	
LR chi2 (71)	45224.25	

Reference categories are female; 10-24 years of experience; secondary education; firm size over 3000 employees; state ownership; value added/worker over 4.16 mFt; 1st quartile of micro-regions by unemployment; engineering industry

Table B2: Full-time employees paid near the minimum wage 2001

Percentage shares / Data source:	WS, May	LFS, April-June
Teenagers (under 20)	1.3	1.8
Youths (under 25)	18.6	20.0
Older workers (over 55)	4.5	3.9
Experience<5 years	9.3	4.4
Experience<10 years	27.5	21.6
Experience<20 years	53.5	50.5
Tenure<1 year	approx. 20.6*	25.3
Tenure<2 years	n.a.	38.1
Tenure<5 years	n.a.	60.9
Women	56.0	54.1
0-7 grades	1.0	1.6
Primary school (8 grades)	29.4	29.8
Vocational (without ’maturity’ certificate)	38.6	40.2
Secondary	27.0	25.3
Higher	4.0	3.1
Firm size<5 employees	n.a.	15.8-20.2**
Excluding firms with less than 5 employees:		
5-20 workers	49.9	n.a.
21-50 workers	15.2	n.a.
51-300 workers	18.0	n.a.
>300 workers	7.9	n.a.
Budget institutions	8.9	n.a.

Authors’ calculation from the Wage Survey (May 2001) and the Labor Force Survey (2001. 2nd quarter Supplement). The WS data cover full-time employees of firms employing more than 5 workers. The LFS data cover all employees paid a wage in 2001 2nd quarter. *) The WS records if the worker entered the firm in the preceding year (tenure: 5-17 months). **) The LFS data on firm size are not comparable to the WS data. The <5 category comprises small budget institutions like the local governments or schools of villages in the LFS.

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