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The Employment Effects of Nearly Doubling the Minimum Wage – The Case of Hungary

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BY

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Abstract

The effect of minimum wages on employment has been a matter of debate for more than a decade. Apart from a few cases (Puerto Rico, Indonesia, Columbia) the empirical works analysed the aftermaths of minor increases in the minimum wage, and yielded mixed results. Hungary 2000-2002 provides a unique opportunity to look at the effects of an exceptionally large minimum wage hike in a relatively developed market economy. Unexpectedly, the country's right-wing government increased the statutory minimum by 96 per cent (XX per cent in real terms) in only two steps between December 2000 and January 2002. The paper looks at the short-run effects of the first hike (57 per cent). It finds that increasing the minimum wage significantly reduced employment in the small firm sector and adversely influenced the jobloss and job finding probabilities of low-wage workers. The effects appear to be stronger in low-wage segments of the market, and depressed regions, where the minimum wage bites deeper into the wage distribution.

Keywords: Minimum Wage, Transition

JEL Classification: J3, P3

KERTESI GÁBOR – KÖLLŐ JÁNOS

A MINIMÁLBÉR DUPLÁJÁRA EMELÉSÉNEK FOGLALKOZTATÁSI KÖVETKEZMÉNYEI – A MAGYARORSZÁGI ESET ÉRTÉKELÉSE

Összefoglaló

A minimálbér-emelések kedvezőtlen foglalkoztatási következményeivel kapcsolatos szokásos előrejelzések érvényességét az elmúlt évtizedben számos kutató megkérdőjelezte. A legújabb empirikus mérési eredmények is meglehetősen ellentmondásosak. Néhány harmadik világbeli ország (mint Puerto Rico és Indonézia) példájától eltekintve, ahol nagymérvű minimálbér-emelésekre került sor, és a foglalkoztatás-csökkenéssel kapcsolatos hagyományos előrejelzések is igazolódtak, igen vegyes eredményeket találunk. Kisebb mértékű minimálbér-emelések jó részénél nem sikerült a foglalkoztatás csökkenését kimutatni. A 2001. és a 2002. évi magyarországi minimálbér-emelések, amelyek a bázisévhez képest egy év alatt nagyjából a duplájára növelték a minimálbér szintjét, kivételes lehetőséget biztosítanak arra, hogy egy nagymérvű foglalkoztatáspolitikai beavatkozás következményeit tanulmányozhassuk. A tanulmány a minimálbér-emelés 2001. januári, első hullámának (57 %-os emelés) rövid távú foglalkoztatási következményeit vizsgálja, a leginkább érintett népesség, a kisvállalati szektor alacsony bérű foglalkoztatottjaira összpontosítva. A tanulmány legfontosabb következtetései szerint a minimálbér-emelés lényeges mértékben növelte a munkaerőköltségeket, és csökkentette a foglalkoztatás szintjét a kisvállalati szektorban, továbbá előnytelenül befolyásolta az alacsony bérű dolgozók állásban maradási esélyeit, illetve rontotta a korábban alacsony bérű álláskeresők állásba kerülési esélyeit. Ezek a kedvezőtlen következmények annál súlyosabbak, minél nagyobb volt az alacsony bérű foglakoztatottak aránya, illetve minél rosszabb volt az eredeti foglalkoztatási helyzet egy régióban.

1. Introduction

In January 2001 the Hungarian government increased the statutory minimum wage from Ft 25,500 to Ft 40,000. One year later the minimum was set at Ft 50,000. The two hikes increased the minimum wage-average wage ratio from values around 28-29 per cent in 1994-2000 to 39 per cent in 2001 and 43 per cent in 2002. A minimum wage increase of this magnitude is unprecedented in contemporary OECD practices albeit similarly radical adjustments did occur in developing countries including Indonesia *(Rama,* 2000; *Alatas* and *Cameron,* 2003) and Puerto Rico (*Freeman* and *Castillo-Freeman*, 1991). The shock hitting the Hungarian labour market was amplified by the fact that the minimum wage regulations cover all employment contracts without exemptions for young workers, small firms, backward regions, or low-wage industries – a common practice in EU member states as discussed in *Dolado* et al. (1996) and elsewhere.

In this paper we study the impact on employment of the first hike. Identifying minimum wage effects when the economy is hit by other exogeneous shocks that may affect low-wage and high-wage workers in different ways, like the recession and the parliamentary elections of 2002 in Hungary, seems to us too difficult in a non-experimental setting. At this stage of the research we restrict the attention to the short-run aftermaths of increasing the minimum wage from Ft 25,500 to Ft 40,000 – a change which came unexpectedly in an otherwise peaceful period of steady economic growth.

Minimum wages in Hungary are less concentrated on teenagers, school leavers, and low-wage industries than in the U.S. or Western Europe. The key dimensions instead are firm size and tenure. About 2/3 of the minimum wage workers are employed in firms with less than 50 employees, and 2/3 of them spent no more than 5 years with the firm. (Only 1/8 is employed by larger firms *and* have tenures exceeding 5 years.)[1] These peculiarities suggest that in order to trace minimum wage effects one has to pay particular attention to the small firm sector on the one hand, and the entry and exit portals of internal labour markets on the other. Accordingly, we shall study the evolution of employment in small firms (Section 4b); flows out of employment (Section 4c); and exit to jobs from unemployment (Section 4d). This will be preceeded by an attempt to assess the magnitude of the shock (Section 2); confronting the declared aims of the minimum wage hike with theoretical expectations elaborated in the recent literature (Section 3); and

[1] These data are drawn from the 2001. 2nd quarter wave of the Labour Force Survey (LFS) and will be discussed in more detail later.

an overview of descriptive statistics on employment (Section 4a). Section 5 concludes.

Since the analytical parts work with three data sets of different character the methodological issues will be discussed within the relevant sections 4b-4d. The data used there and elsewhere come from a variety of sources introduced in a Data Appendix. The paper is basically concerned with the employment effects but contains a Supplement discussing spillover wage effects.

The almost general agreement on the risks of minimum wage legislation gradually dissolved in the last two decades (*Brown,* 1999). The widely accepted 'stylized fact' that high minimum wages kill jobs was called into question by contradicting empirical findings in the 1990s. This case study has not much to add to the ongoing theoretical and methodological dispute but may be of interest to researchers seeking *in vivo* tests of the theoretical predictions. The question of whether nearly doubling the minimum wage had or had no effect on employment in Hungary is, first of all, important for the local community and as such is admittedly non-academic. We think, however, that in the current state of the international debate it bears relevance for a wider public.

2. THE MAGNITUDE OF THE MINIMUM WAGE SHOCK

In 1989, when Hungary's last communist government introduced a statutory minimum wage it amounted to 34.6 % of the average wage, a level well below the EU average but slightly higher than that of Spain, the laggard within the EU. (*Figure 1*). The minimum related to gross monthly earnings net of overtime and shift pay, bonuses, rewards, and premia; was legally binding; and covered all employers and full-time employees. The fundamentals did not change since then. In 1990-1998 adjustments were negotiated annually by a national-level tripartite council and entered into effect in the annual budgets. Under the right-wing cabinet of 1998-2002 the minimum wage was set unilaterally by the government.

During a decade of transition the relative value of the minimum wage was almost constantly falling: in 2000 it amounted to only 29.1% of the average wage.[2] The two consecutive hikes brought the ratio back to the OECD

[2] On similar developments in other Central and East European countries see Standing and Vaughan-Whitehead (1995).

range though with 38.6% in 2001 and 43.7% in 2002 it still lagged behind the average.

The minimum wage-average wage ratio, also known as the Kaitz index (if weighted with coverage as in *Kaitz*, 1970) tells only one aspect of how the minimum wage relates to the 'market wage'. An equally telling indicator is the fraction of workers paid at or near the minimum. *Figure 2* reveals three remarkable features of how this ratio developed over time. The ratio was explicitly low in international comparison until at least 1997: less than 1% in 1989 and 3% in 1997. While the average wage/minimum wage ratio was falling in 1991–2000 the fraction of workers paid near the minimum was rising clearly suggesting that, with the build-up of a sizeable low-wage population, even a falling minimum wage could become effective.[3] Most importantly, the data depict the fundamental change which came in 2001–2002 when the two hikes increased the fraction of employees paid near the minimum from 5% to 12.1% and 17.3%, respectively. While in 2000 Hungary was located in the lower part of the OECD range (countries having similar ratios in the early 1990s were Austria, Belgium, the Netherlands, Denmark, and the US) in only two years it shifted to the position of a heavy outlier with an exceptionally high fraction of minimum-wage workers.

The price shock caused by the radical reform is difficult to assess since there are at least four factors at work. The immediate effect depends on the fraction of workers whose earnings should rise, the gap between their wages and the new minimum, compliance with the law, and the rigidity of relative wages which may induce further instantaneous (and unintended) adjustment in the lower tail of the wage distribution. A lower-bound estimate of the immediate wage shock can be defined as:

[3] The growth of low-wage employment can be easily demonstrated with several indicators: between 1989 and 2000 the d1/d9 ratio of gross earnings fell from 0.32 to 0.2; the d1/median ratio decreased from 0.59 to 0.48; the fraction of workers paid less than 2/3 of the median grew from 16% to 26%, their average earnings fell from 48% to 38% of the national average. The fact that even in 2000 only 20 in 100 workers earning less than 2/3 of the median were paid at or near the statutory minimum wage clearly suggested the dominance of market forces in this process.The data referred are based on the Wage Survey (WS). Alternatively the paradox could be explained by aggravating imperfections and/or a general move from competition to monopsony or analogous market structures. In a competitive labour market workers with marginal product below the minimum wage are simply not employed so there should be no spike in the wage distribution at or near the minimum.

$$(1) \qquad \omega = \frac{w^{*}F + w_H(1-F)}{w_F F + w_H(1-F)}$$

where F is the fraction of workers with sub-minimum wages, w_F is their average wage at the moment of the hike, w_H is the average wage of other workers, and w* is the new minimum wage. The formula measures the size of the wage shock at the moment of the minimum wage hike if all sub-minimum wages are raised to the level of the new minimum and there is no further instantanous wage and employment adjustment. We prefer ω to the customarly used F as the latter ignores valuable information on the pre-hike earnings level of low-wage workers while, in fact, both measures rely on the same assumptions.

We estimated ω for several groups of labour using the large individual data set of the Wage Survey (WS, conducted in May each year) which covers firms employing more than 5 workers and the public sector. *Table 1* presents the mean ω-s for the interactions of 5 age groups, 3 educational levels, and 4 groups of regions.[4]

Under the assumptions discussed earlier we can estimate that the minimum wage hike of 2001 caused a shock of 2.33 per cent to average monthly base wages. (The second wave implied a hypothetical average wage growth of 1.78 per cent.) The ω-s varied in a wide range depending on skill, age, and region: the estimates are 1 per cent for secondary and higher qualification and 6 for 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 with primary or vocational education who lived in depressed regions (3rd and 4th quartiles) were expected to rise by as much as 9.7–16.7 per cent at the moment of the hike.

Compliance with the law

Checking whether the increased minima were actually paid to workers is essential in a country where non-compliance with the state regulations has been traditionally strong. Firms openly setting a sub-minimum base wage for their full-time employees face a high risk of detection and punishment.

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[4] 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 cent 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.

Accordingly, open non-compliance was infrequent as shown in *Table 2* . The proportion of full-time workers paid below the new minimum wage in May 2001 was 1.9 per cent as reported by *firms* in the WS. 1.4 per cent of the full-time *workers* interviewed in the UI Exit to Jobs Survey (EJS), and 3.6 in the Labour Force Survey (LFS), reported gross monthly earnings below Ft 40,000 in April-June, respectively.[5] These percentages are upper-bound estimates since unpaid leave and other disturbances can temporarily result in sub-minimum monthly earnings.

There are hidden ways of rescuing the regulations, however. Some firms may employ their workers full-time but register them as part-time and pay them subminimum monthly wages. The fact that the fraction earning sub-minimum wages within *all* wage earners including part-timers was only 5.5 per cent (LFS) and 2.6 per cent (EJS) in April-June 2001 suggests that these practices were of marginal importance (*Table 2*).

Second, firms may fraudulently lay off their workers and contract with them as 'trade partners'. The magnitude of this kind of manipulation cannot be assessed in general but the EJS provides information on a subsample of low-wage workers. As shown in *Table 3*, only 1.5 per cent of the low-wage UI recipients who found a job in April 2001 expected to earn their labour income as a contract fee, as opposed to 64.7 per cent receiving a fixed salary, and 33.8 per cent paid an hourly wage. Only a single person in a sample of 3,157 newly hired low-wage workers expected to earn a contract fee lower than Ft 40,000.

Third, and most importantly, firms can increase the base wage and reduce side payments. 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 base wages if firms comply with the regulations.

The validity of this assumption can be first checked using the grouped data introduced in *Table 1*. The descriptive regressions $\Delta\ln(\text{compensation})=b\Delta\ln(\omega)+gX+u$ presented in *Table 4* suggest that 1 per cent difference in the minimum wage shock was associated with nearly 1 per cent difference in the change of actual compensation between May 2000 and 2001, which-

[5] 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)

ever definition of ’compensation’ was used. A dummy for secondary and higher education was included in the equations to account for the fact that the proportion of side payments within earnings fell in this particular category of labour implying faster growth of base wages (but not of earnings) compared to ω.[6]

A more precise account of offsets can be given using firm-level or industry-level data. The effect on labour demand of higher minimum wages depends on the shock to real labour costs rather than real wages. The cost effect can be partly or fully offset by cutting the non-wage components of employee compensation, improvements in workers’s productivity, or a wedge between the growth of producer and consumer prices. We use data from the financial records of 20,601 firms in 2000 and 21,722 firm in 2001 (FR) to show that labour costs were also strongly (nearly equiproportionally) affected by the minimum wage increase even if we account for some of the above mentioned possibilites.

Given the sampling procedure of the WS we can not reliably measure ω on the firm level so the data were aggregated to the 4-digit industry level. The question addressed is how *wage costs* (wages + taxes) and *total pecuniary employee compensation costs* (wages + taxes + other payments to persons) measured in real terms were affected by ω holding productivity growth and some other factors (potentially affecting wages and compliance with the law) constant.

In addition to wage costs (earnings *including* bonuses, and premia) and social security contributions total compensation comprises ‘other payments to persons’ containing contract fees, honoraria and miscellanous casual payments. The three items accounted for 64.0%, 24.4%, and 20.6% of the total compensation in 2000. Allocating the tax burden proportionately we get that pre-tax earnings amounted to 84.6 % of the pre-tax total compensation. An ambiguity in what should be considered ’labour cost’ stems from the fact that ’other payments’ apparently include components representing profit-sharing rather than indispensable payments for the services of labour.

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[6] 2002 was an election year which started with generous pay increase in the public sector with high-skilled employees acquiring the largest gains. The total earnings of public sector employees with secondary and higher education grew by 17.7% as opposed to 11.5% received by their business sector counterparts and a grand mean of 11%.Furthermore, the recession reaching Hungary in the Fall of 2001 may have had non-trivial wage effects loosening the linkage between the second minimum wage shock and actual wage outcomes. Even so, once high skills are controlled for, the hypothesis of the elasticity of the wage with respect to ω being equal to 1 could not be rejected.

Furthermore, ‘other payments’ are often directed at persons who are not employed by the firm. We believe that wage cost is a better approximation of labour cost but shall also consider the effect of ω on total compensation.

Since productivity is partly driven by employment adjustment that, in turn, is influenced by the wage we estimate the simultaneous equations system:

$$w_i = \beta_0 + \beta_1\omega_i + \beta_2(q/n)_i + \beta_3 FU_i + \beta_4 X + u_i \tag{2}$$

$$n_i = \alpha_0 + \alpha_1 q_i + \alpha_2 w_i + \alpha_3 Z_i + v_i \tag{3}$$

where *w* is wage cost or total compensation cost, ω is the minimum wage shock on the industry level, *q* and *n* stand for output and employment, FU denotes unionisation, and *X* and *Z* comprise controls.[7] Monetary aggregates were discounted using the producer price index available on the 4, 3 or 2 digit levels (35 distinct values). The symbols denote log changes between 2000 and 2001 on a year-on-year basis. Equation (2) relates the change in labour costs to productivity and the minimum wage shock. Equation (3) is a conditional labour demand equation based on the assumption that employment is affected by output (q) and the cost of labour (w) on the short run. The parameter of interest is β_1 and the expectation is β_1=1 under full compliance and no offsets. In the 3sls estimation employment and wages (and hence productivity) were treated as endogeneous.

Table 5 presents the descriptive statistics for the sample and *Table 6* contains both sure and 3sls estimates for models with wage costs and total compensation costs as the measures of w. The parameters of ω in the wage cost equations are highly significant with parameter values of 0.94 in the sure estimation and 1.00 in the 3sls model. The coefficients of ω in the total compensation model are lower with 0.87 in the sure and 0.95 in the 3sls models suggesting that firms more severly affected by the minimum wage increase may have cut some components within ’other payments’.

Allowing endogeneity has an impact on the coefficients of productivity in the wage equations, and labour costs in the employment equations, but the key parameter β_1 is weakly affected. Our interpretation for the former is that the sure estimates for productivity capture employment adjustment on the margin, with little effect on efficiency, rather than rent sharing. This can be the case for instance if adjustment affects auxliary jobs without major influence on the productivity of the remaining operations. The lower wage elasticities of demand (-0.26 significant at the 0.036 level and –0.14

[7] Unionisation is measured as the fraction of workers covered by collective wage agreements in 1998. This is the latest figure available from *Neumann* (2002) and *Kertesi* and *Köllő* (2002) both based on National Labour Centre data.

significant only at the 0.25 level in the wage cost and total compensation models, respectively) may be explained by the correlation between output and employment cuts on the one hand and wage growth on the other. The average wage tends to rise when output and employment are cut and the average quality of the labour force improves. This correlation is ignored in the sure estimates where employment is assumed to respond to exogeneous increases in w holding q constant – this leads to an overestimation of the wage effect.

If β_1 is indeed measuring compliance it is expected to increase as we move from low to high levels of unionisation. To test this hypothesis we estimated model (2)-(3) by interacting the minimum wage shock variable with unionisation by including $\Delta\ln(\omega)$, FU, and $\Delta\ln(\omega)\times$FU to the wage setting equations. Indeed, as shown in the bottom panel of *Table 6* the elasticity of the wage cost (WC) and total compensation cost (TC) with respect to the minimum wage shock (denoted with σ henceforth) increased with FU though the interaction terms were weakly significant. At low levels of unionisation (0.11, the P25 value of FU) $\sigma^{WC} = 0.83$ and $\sigma^{TC} = 0.72$. At the mean FU of 0.41 $\sigma^{WC} = 1.04$ and $\sigma^{TC} = 1.00$. At high levels (0.68, the P75 level of FU) $\sigma^{WC} = 1.21$ and $\sigma^{TC} = 1.24$. It seems that non-unionised sectors found easier to alleviate the cost shock by cutting some side payments while the highly organised ones experienced more severe wage spillover effects.

The model also has a message about the aggregate employment effect of the minimum wage increase – we shall come back to it in Section 4.1. We do not engage in a detailed account of wage evolutions and spillover effects either - a brief analysis if presented in the Supplement. (The main finding is that while the workers directly affected by the minimum wage increase were found in the 1st-16th percentile of the wage distribution wages grew faster than the average up to the 40th percentile.) At this point of the analysis the important finding is that, albeit with variations across sectors, the first minimum wage hike was certainly effective causing an unexpected and severe shock to the Hungarian labour market.

3. EXPECTED MINIMUM WAGE EFFECTS

The government's motives to radically increase the minimum wage have never been systematically explored so the observers are free to build their own hypotheses from fragments of speeches and interviews. The stereotype of general support on the political left and opposition on the right does definitely not help in this case. The hikes were decided by a right-wing government explicitly committed to increasing the relative 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)

A couple of economic arguments seem to have emerged from the political disputes, however. Among them were that higher minimum wages were 'required' by the EU to rule out unfair competition and promote social cohesion. Though this claim have never been documented it is credible and also familiar from the Indonesian and Puerto Rican cases where similar pressures on the part of the US and the trade organisations played an important role.

The main line of the argument layed elsewhere. It was repeatedly argued by the prime minister and other government officials that while the effect of the minimum wage increase on labour demand should be negligible it stimulates work effort, leads to higher productivity, makes it easier to hire additional workers, and by widening the gap between benefits and wages creates proper incentives for paid employment, labour force participation, and job search. The arguments were typically presented in popular form: the minimum wage will 'restore the prestige of work', combat the idleness of benefit recipients, 'whiten the black economy', and so on. However, behind the slogans it is not so difficult to recognise some key arguments of the 'new economics of the minimum wage' providing support to such expectations.

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 labour supply curve is that of a local monopsony. Since the firm is the only buyer on the market it can 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 increased too, the firm's marginal expenditure on labour (ME_L) curve is steeper than its supply curve. Employment is set at the point where ME_L equals the marginal revenue product while the wage is set at the lowest level compatible with that level of employment given the supply curve. A minimum wage increase can effectively decrease the firm's marginal expenditure on labour and lead to a concomittant increase in wages and employment at the cost of the monopsony rent. A 'too high' minimum wage hike, however, may shift ME_L upwards in the vicinity of the current employment level and result in a loss of jobs.

The modern theories developed to understand why the employment effects are often small or even positive are generalisations of the monopsony model in several ways. The motivation to develop such models rooted in empirical findings from the 1980s and 1990s calling into question that a high minimum wage kills jobs. In fact, the theoretical models of a positive employment effect were developed many years before the supporting evidence was available. In a partial equilibrium model incorporating the supply side and labour turnover *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.[8] Among the pioneering works mentioned in *Brown'*s (1999) overview is the model of *Pettengil* (1981) predicting positive employment effect under efficiency wage setting. The search friction models of *Mortensen* (1988) and *Burdett* and *Mortensen* (1989) were also ready for use before a series of studies including *Card* (1992*a,b), Katz* and *Krueger* (1992) and *Card* and *Krueger* (1994) opened new chapter in the study of minimum wages.

These studies summarised in *Card* and *Krueger* (1995) called the attention to the benefits from carefully designed quasi-experiments where an appropriate control for unrelated disturbances is achieved by an appropriate experiment desighn rather than sophisticated and therefore vulnerable econometrics. Empirically, these papers together with *Machin* and *Manning* (1994) *Dolado* et al. (1996) and others showed weak, zero, or even positive effect of increasing the minimum wage while the time-series results from this period also suggested much weaker effects than previously (*Brown,* 1999). This challenge gave new impetus to both the empirical and the theoretical research of minimum wages and also affected the political debate over the issue.

It was recognised that the logics underlying the benchmark monopsony model can be applied to a wide variety of market structures. While single-employer towns are indeed rare quite many firms can be the only buyer of certain skills in the local labour market thus the monopsony model can hold without any major modification. Mobility costs provide a degree of monopsony power to nearly all enterprises. A multitude of firms can be supply-

[8] 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.

constrained by search frictions - inasmuch as a minimum wage increase 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). If workers respond to an increase of the minimum wage by increasing their effort as in the efficiency wage models of *Rebitzer* and *Taylor* (1991) wages, productivity and employment can rise. A higher minimum wage enforces the managers to search for well-functioning incentive schemes, something they were not so deeply interested to do before, and in this sense the minimum wage hike can be interpreted as a cause of higher employment. Distortions on the labour market can also drive the outcome far from the competitive predictions. In the monopsonistic competion model of *Bashkar* and *To* (1999) the direction of change depends on the share of fixed costs with higher shares predicting an increase in employment and vice versa. In a model of dual wage determination with minimum wages set by the government and other wages negotiated in a Nash-bargain *Cahuc* et al (2001) find positive employment effect under the condition that low-wage and hogh-wage workers are highly substitutable.

The competitive theory has not been overthrown by these theoretical innovations – neither were its predictions discredited by the above-mentioned 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 Columbia; *Carneiro* (2000) in Brazil; *Freeman* and *Castillo-Freeman* (1991) in Puerto Rico; *El Hamidi* and *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 in magnitude, restricted to certain segments of the market, but definitely not positive or zero.

The Hungarian government's assuption of no effect on demand, or demand effect fully offset by the reduction of search frictions and increase in effort, were thus rather brave ones. The available evidence on the wage elasticity of labour demand suggested that firms are responsive to wages particularly in the low-wage segment of the market. Estimating dynamic conditional labour demand equations for homogeneous labour *Kőrösi* (1998, 2000) found relatively low but significantly negative short-run elasticities during the transition period. His preferred specification suggested short-run wage elasticities of between –0.55 and –0.65 in 1992-95 but only –0.31 and –0.33 in 1996-97. Estimates for heterogeneous labour are available in an earlier work by the authors of this paper. The model in *Kertesi* and *Köllő* (2002b) distinguished four factors of production (unskilled and two types of skilled labour plus capital) and assumed optimal choice under translog technology. The repeated cross-section estimation of the optimal cost share equations for firms employing at least 300 workers provided rather high own-wage elasticities for 1996-1999. The mean elasticities were –0.8 for skilled and –1.4 for unskilled labour. Given these elasticities, for employment to increase after doubling the minimum wage the supply side should be highly responsive in order to offset the fall in demand. The forthcoming sections try to measure up how this bold experiment suceeded.

4. EMPLOYMENT EFFECTS

4.1. DESCRIPTIVE STATISTICS

The minimum wage increase of January 2001 coincided with a sudden break in the growth of aggregate employment as shown in *Figure 3*. The dotted line depicts seasonally adjusted employment in the non-agricultural private sector in 1998–2002.[9] The path of employment growth prior to 2001 ccould be precisely approximated with a quadratic form ($4672*t-28.5*t^2$, R^2=0.98) indicated by the solid curve. Employment growth was gradually slowing down in the period considered with the monthly growth rates falling from 0.027% in 1998–1999 to 0.018% in 2000. Had this trend continued in 2001-2002, as depicted by the extrapolated part of the curve, aggregate employment should have grown further by 2.8%. The deviation of employment from its preceding path starting from January 2001 (month 36, indicated by the vertical line) is easy to observe in the graph.

This remains true if we consider the path of employment *relative to GDP*. Prior to the first quarter of 2001 the economy followed a path at which 1% growth of GDP was associated with 0.5 % growth of employment as shown in *Figure 4*. 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 sepa-

[9] 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.

rating the years (with larger distances indicating faster growth) while the *relation* between employment and GDP is captured by the *slope* of the fitted line. For lack of monthly GDP data we now turn to quarterly figures. The economy was slowing down in 2001 (as well as in 2000 relative to 1999) but a moderate *fall* of employment in that year (-0.2 per cent) was clearly at odds with the experience of the preceding years. Had the economy remained on its path followed in 1998-2000 employment should have risen by about 1.7 % in 2001 and 1.8% in 2002, at the given rates of GDP growth.

The contribution of the minimum wage increase to the slow-down of employment growth is difficult to assess. Some priors can be based on the industry-level results for 2001 presented in *Table 6*. We found that the variations in the growth of actual labour costs were equiproportional to the exogeneous variations implied by the minimum wage increase (ω), therefore the wage elasticities of the employment equations could be directly used to calculate an employment effect. Depending on which concept is accepted as the relevant measure of labour costs the wage elasticities of labour demand were –0.27 and –0.14 (the latter being weakly significant). The 57 per cent increase of the minimum wage was estimated to raise the average wage by 2.33 per cent therefore the implied immediate employment loss could be between 0.63 and 0.32 per cent. Even if we accept the seemingly unrelated regressions estimates of the wage elasticity of employment (about –0.4) the implied effect should be slightly below 1 per cent.

The aggregate effect at constant output can be interpreted as a downward shift in the trend of employment growth already touched upon in Figure 3. *Figure 6* shows the graph redrawn under the assumptions that labour costs are best approximated by wage costs and their effect on employment is best captured by the 3sls estimates. Three vertical lines indicate the middle of 2000 and 2001 with the date of the minimum wage increase in the centre. Aggregate employment was expected to approach the new trend by about June-July 2001 – the mid-point of the period considered in our estimates using annual data. Employment was indeed very close to this level in the middle of 2001 but continued to move away from the growth path characteristic of the previous years.

The increase in the minimum wage may have destroyed jobs in the micro-firm sector and the public sector excluded from the estimates with the former having a 14 per cent share in employment and 23 per cent share in low-wage employment, and the latter having a 25 per cent share in both total and low-wage employment.[10] We think that a 0.63 per cent rate of job loss should be regarded as an upper bound estimate for the two left-out sectors. Therefore, with the aggregate data at hand we can only give an upper-bound guesstimate for the contribution of the minimum wage hike that predictably did not exceed 0.63 per cent affecting less than 23,000 jobs on the short run. The longer-run effect and the contribution of the minimum wage increase to a slow-down of GDP growth requires an extended period of observation and new, preferably panel, data.[11]

A minor aggregate effect does not exclude that some categories of labour were severely hurt. The grouped data introduced in *Tables* 1 and 4 suggest that a category's exposure to the minimum wage shock was closely related to its subsequent change of employment. This is shown in *Figure 5* where the 60 groups were plotted by their exposure to the minimum wage increase (ω) and change of employment between the 4th quarters of 2000 and 2001. The slopes of the best-fitting lines were then estimated for all groups and 40 unskilled groups.[12] Both the weighted and the unweighted regressions are presented in *Table 7*. The weighted LFS data suggest employment elasticities with respect to ω of –0.45 for all groups and –1.29 for unskilled groups. The unweighted estimates are higher but follow similar patterns in indicating larger differences *within* unskilled labour.

The patterns observed in 2000-2001 cannot be attributed to secular trends. As shown in *Table 8,* the groups exposed to strong minimum wage shock in 2001 actually 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 low-wage groups experienced a rise in their relative employment probabilities.[13] An-

[10] The calculation is based on the LFS Supplementary Survey of 2001 April-June. 'Low-wage' denotes a gross monthly wage below Ft 40,000.

[11] In the industry panel of 2000-2001 the direct effect of ω on output appears to be insignificant in OLS regressions with or without controls.

[12] Unlike in Figure 5, the LFS-based regressions have employment *levels* on the left hand, controlled for change in the number of working age adults who do not attend school as full-time students and do not recieve old-age pension. This is required because the rotation of the LFS sample leads to random variations in the size of the groups observed and therefore their *levels* of 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 had to be dropped. The number of old-age employees observed in the 12 region-education cells ranged between 3 and 71 in 2000 4th quarter, for instance.

[13] The equations include a dummy to control for the fact that the employment of workers older than 55 increased substantially in 1999-2000 when the retirement age was increased by one year (61 for men and 57 for women).

other argument against interpreting the observed correlations as minimum wage effects refers to the potentially non-neutral impact of the recession following 11 September 2001. However, similarly to the aggregate statistics presented earlier, the grouped LFS data suggest that low-wage employment started to fall immediately after the minimum wage hike. This is supported by the calculations underlying *Table 9*. The employment ratios of the 60 groups, normalised to their 2000. 4th quarter levels and expressed in logs, were regressed on ln(ω) in panels comprising quarters 1, 1-2, 1-3 and 1-4, respectively. The estimation was repeated for 40 groups of unskilled labour. The coefficients of ω were weakly affected by the extension of the panel period in both models.

The descriptive statistics reviewed in this section seem to show that employment was adversely affected by the first minimum wage hike. Aggregate employment did not remarkably fall in absolute terms in 2001 but deviated from its path followed in the preceding years. Groups exposed to stronger shock had less favourable employment records compared to other groups and their own past experience. The data revealed particularly large differences within unskilled labour by age and region. The question of causality remains open, however. The possibility that employment evolutions were driven by shocks other than the minimum wage increase can not be ruled out using aggregate or grouped data. Finding the locus for a deeper analysis is the last preparatory step addressed before we start.

Most empirical studies of the minimum wage effect concentrate on youth employment and low-wage industries in both the US and Europe. We shall deviate from this tradition because the data summarised in Table 10 and 11 hint at more important dimensions in the Hungarian context. The logit model of *Table 10* estimates the probability that a worker observed in the WS in May 2000 earned less than Ft 38,685, that is, was presumably affected by the minimum wage increase six months later. All the explanatory variables are dummies and the coefficients are expressed as odds ratios. The parameters on personal characteristics and industrial affiliation depict a familiar picture: females, young and unskilled workers, those employed in high-unemployment regions are more likely to earn subminimum wages, and so do employees in the light industry, trade, hotels and restaurants, road transport, and services. No, or very few, low-wage workers are found in petroleum mining and refining, banking, R&D, public transport, and the tobacco industry.[14]

[14] An extraordinary parameter (or=70.9) on Insurance signals that the agents are typically paid the minimum wage as the fixed part of their remuneration.

Budapest and the regions around Lake Balaton, Hungary's major tourist zone, have relatively high fractions of low-wage workers. In these cases we suspect that many of these employees are only registered as minimum-wage workers and paid partly in cash.

More important than this is what the firm-level variables suggest. The fraction of low-wage workers sharply increases as we move from the reference category of large firms employing more than 3,000 workers. The odds ratios are 3.0 for medium sized firms (50-300 workers), and as high as 6.6 and 12.4 for the two categories of small firms, with 21-50 and 5-20 employees respectively. Similarly important is the firm's productivity level. Starting from the most productive ¼ of firms the odds ratios jump from 1 to 1.6, 3.1 and 7.8 in the 2^{nd}-4^{th} quartiles. The inclusion of firm size and productivity to the model increases its pseudo-R^2 from .18 to .35, and a model with only these two variables has a better fit (.24) than a model with the remaining 63 variables.

Table 11 based on the WS and the LFS Supplementary Survey of April-June 2001 looks at the composition of minimum wage workers in 2001, a few months after the first hike. The composition of low-wage employment by gender, education, age, and experience is surprisingly balanced. Women are slightly over-represented with 54 and 56 per cent shares in the two data sets. The vast majority (¾) of the minimum wage workers are prime-age (25-54); only 1/5 is under 25; and teenagers account for less than 2 per cent. Half of the minimum wage workers have more than 20 years of experience and only less than 10 per cent entered the labour market 0-4 years before the survey date. Workers with primary, vocational, and secondary/higher education have roughly 30, 40, and 30 per cent shares, respectively. Married couples without children (38 per cent) and young adults living with their parents (19 per cent) form large groups of the minimum wage workers but 35 per cent are parents of one or several children.

By contrast, a high concentration of minimum wage workers can be observed in jobs with short tenures. About 20-25 per cent of the minimum wage workers have tenures (t) shorter than a year; 38 per cent have $t<2$, and 60 per cent have $t<5$ (while only 4.4 per cent of them spent less than 5 years on the labour market).

Tenure also has a strong impact on the *probability* of being paid at or below the minimum wage. In simple byvariate probits $P(w<1.05w^*)=\Phi$ (tenure, experience) estimated with data of the LFS Supplementary Survey sample the marginal effects turn out to be -.028 for tenure and .003 for experience. In univariate estimates the marginals are -.024 and -.0007, respectively.[15]

Finally, *Table 11* also calls the attention to the importance of small firms in the low-wage segment of the labour market. About 15-20 per cent of the minimum wage workers are employed in micro-firms with less than 5 employees. Excluding this category, for which further data are not available, we get that *half* of the minimum wage workers work in small firms with 5-20 employees as opposed to their 20 per cent share in employment.

We conclude from these data that the study of minimum wage effects should primarly focus on small and/or low-income firms on the one hand, and the entry and exit portals of internal labour markets on the other. If anywhere, a 'too high' minimum wage is expected to reduce employment in the small firm (low-income) sector, and exert influence on the flows between employment and non-employment. These, rather than teenage employment or the low-skilled labour market in general seem to be the adequate fields for the empirical ivestigation.

Data availability restricts us to address only three questions within this broad area. In section 4.2. we analyse a short panel of 1,818 small firms drawn from the 2000 and 2001 waves of the WS to see how firms' exposure to the minimum wage hike affected their wages, output, and employment in 2001. Section 4.3. compares the jobloss probabilities of workers who are paid exactly the minimum wage with those earning slightly more than that. At this aim we follow 16,429 individuals observed in the LFS Supplementary Survey of 2001 2nd quarter. Finally, in Section 4.4. we compare the flows from insured unemployment to employment of low-wage and high-wage workers using a panel of monthly data from 171 labour offices covering January 1998 – March 2002.

4.2. Employment in small firms 2000–2001

In analysing the impact of the 2001 minimum wage hike on small-firm employment we share the assumptions describing the imperfectly competitive firm. Our enterprises employing 5-20 workers hardly behave as monopsonies; are likely to face highly (albeit not infinitely) elastic labour supply and product demand curves. The minimum wage hike forced the vast majority of these firms[16] to choose between paying more to their low-wage

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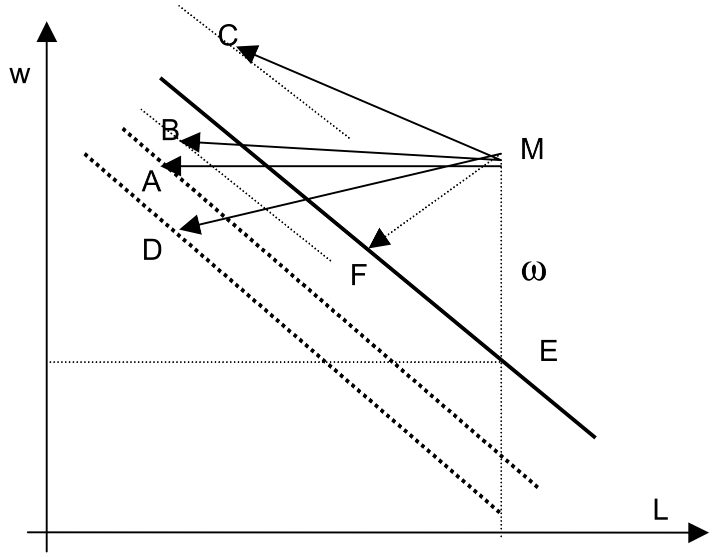
[15] The estimations are available on request.

[16] Prior to the 2001 minimum wage increase 25.6 per cent of the small firms had no directly affected workers at all (F=0); 17 per cent had F=1; while 57.6 per cent of them

workers or dismissing them with or without additional layoffs affecting high-wage employees.

Chart 1: **Responses to a minimum wage shock**



The firm's response to a given shock affects the average wage, employment, and output simultaneously as shown in *Chart 1*. 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 labour is homogeneous in terms of skills wage dispersion may arise as a result of on-the-job training (individual productivity differentials) or Becker-Stigler type bonding. When the minimum wage is increased some workers are affected and the increase of the average wage by ω drives the firm out of its pre-hike equilibrium denoted with E. What happens afterwards 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 insists on its bonding scheme, only an employment effect will work that will drive the firm from M to A.[17] In other cases the firm will substitute low-productivity (low-wage) for high-productivity (high-wage) workers – this will result in $\Delta w>\omega$, a growth in productivity, while output and employment will be affected by both substitution and scale effects (M → B, M→ C). If low-wage and high-wage labour are complements the firm can also react to the minimum wage shock by dismissals biased against high-wage workers – this can result in $\Delta w<\omega$

had some low-wage workers (F=47 per cent on average). Author's calculation from the WS, May 2000.

[17] The firm can sell at higher prices but faces a downward sloping demand curve.

and a major fall in output. (M→ D). Finally, the firm can choose to cut the unregulated components of employee compensation and achieve $\Delta w<\omega$ without productivity loss, at least on the short run (M→ F). On the long run it risks losing its low-wage workers to other firms.

Two things are clear even from this basic example. First, the identification of the impact of the minimum wage increase on small firms requires a model accounting for the endogeneity of wages, employment, and output. The firm's choice over its responses depends on substitution and complementarities that we can not directly observe but the changes in the composition of the labour force and hence productivity can be indirectly taken into account through incorporating output to the model. Second, in order to capture the relation between ΔL and ω we need to control for offsets. We assume that offsets are less likely with profitable firms able to share their income with their employees; if quits put the firm's ongoing capital investments at risk; and in a low-wage environment where failures to pay the new minimum wage menace with the quitting of core workers. Therefore we estimate a two-equations system:

$$c_i = \beta_0 + \beta_1(\omega_i \cdot U_i) + \beta_2 K_i + \beta_3 \pi_i^0 + \beta_4' X_i + \beta_5 Y_i + u_i \qquad (4)$$
$$L_i = \alpha_0 + \alpha_1 q_i + \alpha_2 c_i + \alpha_3 Z_i + \alpha_4 Y_i + v_i \qquad (5)$$

where c is log change of the PPI-adjusted labour cost, K is log change of the capital stock; π^0 denotes profit in the base period, L is employment, q is the PPI-adjusted log change in value added; and X, Z, and Y denote exogeneous controls. The wage setting equation is controled for region effects (X, 17 dummies), the employment equation includes the base period capital-labour ratio (Z, under the assumption that capital intensive firms are less likely to react with dismissals on the short run), and a dummy for Lake Balaton. Ten industry dummies (Y) are included into the system as additional exogoneous regressors. The minimum wage shock is interacted with dummies for the 1^{st}-4^{th} quartiles of micro-regions by unemployment allowing ω to have different coefficients in high- and low-wage environments. $\boldsymbol{\beta}_1$ is a parameter vector with the coefficients expectedly rising as we move towards high-unemployment regions. In high-unemployment regions a large fraction of unskilled labour, including core workers, were affected by the minimum wage hike therefore we expect stronger compliance. We estimate the system with three-stage least squares regressions treating L, q, and c as endogeneous.

Data

As an exception to the general sampling rule of the Wage Survey firms employing 5-20 workers are expected to report data on each of their employees. The firms are randomly selected within four-digit industries.[18] In this particular size category, covering 20 per cent of aggregate employment in the enterprise sector, we can precisely measure the fraction of low-wage workers within the firm and also have hope to observe at least some enterprises before and after the minimum wage increase.

Given the Wage Survey's target population of small firms and a sampling quota of roughly 12% we would expect that about 350 firms could be followed in a short panel. In fact, the number of small enterprises observed in both 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.

The probits in *Table 12* check how the estimation sample was selected from the base-period population of 2,874 small firms observed in the 2000 wave of the WS. Firms also observed in 2001 tend to be larger, generating profit in the base period; and have less workers paid below the new minimum wage. The dropouts were presumably 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.

Table 13 presents the descriptive statistics of the estimation sample. The median firm had 13 employees, of which 5 was paid below the new minimum wage, and was hit by an average wage shock of 11.2 per cent. *Table 14* gives an overview of changes between 2000 and 2001 broken down by the size of the minimum wage shock (zero, 0-10, 10-25, and over 25 per cent). Real labour costs grew by 6.2, 9.1, 27.9 and 30.5 per cent, respectively, while employment changed by +4.5, -0.7, -5.4 and –9 per cent.

Results

Table 15 presents the estimates of equations (4)-(5) based on unweighted and weighted (with base-period employment) data. We refer to the un-

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

weighted results. The wage setting equation suggests that the elasticities of real labour costs with respect to the minimum wage shock ranged between 0.6 and 0.9 with high-unemployment regions having higher elasticities. Growth in the capital stock and base-period profits also have the expected sign though the latter is not significant.[19] The labour demand equation suggests an output elasticity of 0.33 and a labour cost elasticity of –0.37.

Firms by Lake Balaton had larger employment losses presumably because many of them took their minimum wage workers off the payroll and continued to pay them in cash or on contract basis – a common practice in this tourist zone where firms themselves have substantial intakes in cash. Capital-intensive firms reduced employment less than others.

What can be told about the magnitude of the minimum wage effect using these estimates? The impact can be approximated with $\beta_{1j}\cdot\alpha_2$ for region quartile j. In high-unemployment regions an 1 per cent average wage growth implied by the minimum wage hike reduced employment by 0.33 per cent while the elasticities were –0.28 and –0.22 in the 2^{nd}-3^{rd} and the 1^{st} quartiles. (The weighted estimates are very close to these values except in the 4^{th} quartile where it is reduced to –0.29.) A low-wage firm (F>0.25, ϖ=.36) with 17 employees, located in a low-unemployment region was estimated to lose 1.2 jobs as a result of the minimum wage hike while its counterpart operating in a high-unemployment area lost 2 jobs. The differences in case of 10-25 per cent share of low-wage workers (ϖ=.165) were obviously lower with implied losses of 0.6 and 0.9 jobs. At the average shock (ϖ=.119) and elasticity ($\beta_{1.}\cdot\alpha_{2=}$-.282) the loss amounted to 0.57 jobs. Firms with 5-20 workers had a combined employment of 328,00 in the base period. Our estimates suggest that the minimum wage hike eliminated about 11,000 jobs in this sector – a huge loss in Hungarian context.[20]

The employment losses of low-wage small firms in 2001 were at odds with their previous employment histories. In a similar panel of 1,046 small firms observed in the 1999 and 2000 waves of the WS the log change of employment regressed on F (fraction low-wage in May 2000) yields a parameter of 0.0875 (1.49) while a similar univariate regression yields -.1063 (4.01) in the 2000-2001 panel.[21]

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[19] The system was also estimated with Zellner's seemingly unrelated regressions with almost identical result in the wage setting equation.

[20] 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.

[21] The data and the results are available on request.

A puzzle to be solved before accepting these results is the apparent deviation of changes in output from changes in employment, and the nature of productivity growth, in small firms hit by strong minimum wage shock. In the most affected group of firms productivity grew by 8.8 per cent in a year. (In the other three groups it changed by –1.3, -0.2, and 4.3 per cent as we move from low to high ω-s). How can we explain this 'miracle'? The most popular answer to the question is that these firm took their workers off the payroll and continued to pay them in cash or kind, or by purchasing their 'business services' on the market. However, in this case their material costs and other, unspecified costs should have increased. This was not the case, as shown in *Table 14* – the share of non-wage costs in total costs actually decreased by 1.8 percentage points in the most affected group. By contrast, we find evidence that the composition of employment changed in favour of more educated and non-manual workers in these enterprises (*Table 16*). *Table 17* furthermore suggests that more affected firms employed more high-wage workers in 2001 than expected on the basis of their wage distribution of 2000. Whether a 2.2 percentage points drop in the share of low-skilled workers, a 3 percentage points growth in the share of white collars, and a 4.8 percentage points increase in the share of high-wage workers can explain a nearly nine per cent increase of output per worker remains an open question. The possibility that increased effort and better incentives played a role, as proposed in efficiency wage models, can not be excluded.

The sampling rule of the WS does not allow a comparison with larger firms (the number of individual observations is insufficient for calculating F or ω) except for enterprises employing more than 500, thus reporting data on 50 or more, workers. As shown in *Table 18* the low-wage large firms had better than average employment records in 1999-2000, and worse than average in 2000-2001 but the estimates are not significant at conventional levels in the small samples of 337 and 332 firms, respectively.

4.3. THE JOBLOSS RISKS OF MINIMUM WAGE WORKERS, 2001

Textbook competitive theory predicts that the firm will no longer employ its workers directly affected by the introduction of a biting minimum wage. There are many reasons why this is not the case in practice ranging from the costs of dismissals to monopsony power. Nevertheless the workers directly affected by a minimum wage hike are at exceptionally high risk of jobloss in a competitive labour market since their employers are obliged to pay them more than their original wages adjusted to their marginal revenue product.

A minimum wage hike decided somewhere at a government office desk randomly divides the low-wage population into two parts. Workers whose pre-hike wage was 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 disemploy them as they are continued to be paid at their marginal product. These workers can also be indirectly affected because of wage spillovers (the firm's insistence to its system of relative wages) or because the firm's demand falls for the whole category of labour 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 utilise information on the wages of 16,429 full-time employees interviewed in the LFS Supplementary Survey of 2001 2nd quarter. We distinguish workers who were paid exactly the new minimum wage (treatment group) from those who earned slightly more than that (control group), 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.

As shown in *Jenkins* (1995) by chosing the quarterly employment spells of workers belonging to the risk group as the units of observation the exit hazard from a stock sample can be estimated with logit augmented with a baseline hazard function f(t):

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

where t and τ denote time spent in the job, X stands for individual and enviromental characteristics, w denotes a set of wage level dummies, and q represents calendar time. In a discrete time duration model each individual contributes to the sample likelihood with as many spells of observed outcome as he/she spent in the risk group after the date of stock sampling. Censored observations are those with an unknown outcome due to dropout from the rotating panel. Censoring is achieved via exclusion of the spell from the estimation sample. The baseline hazard is customarily captured by duration dummies or assuming a functional form of the distribution of completed spells (usually exponential or *Weibull*).

Among the Xs we include gender, age, job characteristics, union membership, type of work contract, local unemployment, and industry dummies. A set of dummies stand for the wage of the observed individuals in April-

June 2001. For reasons presented later we distinguish between workers paid in a 10 per cent range of the minimum wage, those earning 110-125 per cent of the minimum, and three other categories earning even higher wages. Calendar time is captured by quarter dummies and f(t) is also approximated with quarterly duration dummies in the first version of the model where all workers are followed by the end of 2001.

The reason of not following the sample for the longest possible period allowed by the LFS design (5 quarters) is that the second minimum wage shock exposed our control group to the same type of risk that hit the treatment group in 2001.

The motivation to estimate two versions of the model originates in the conjecture that particularly low wages and high jobloss probabilities mutually depend on each other in the low-wage, high-mobility segment of the labour market. Workers in marginal jobs change employer rather frequently, flow in and out of employment, tend to have short tenures at any given point of time, and earn low wages. In order not to confuse the causal effect with this sort of spurious correlation we also estimate model (6) for workers who spent at least 2 years in their jobs prior to the survey date.

To distinguish between flows to unemployment versus non-participation we estimate multinomial logits with duration dummies, treating tenures longer than18 months as the reference (whole sample) and assuming exponential baseline hazard (tenure>2 years). [22]

Before we come to data description and estimation a few paragraphs should be devoted to justify our choice of the treatment and control groups. At this aim we use a quasi-panel of individuals covering full-time employees observed in the 2000 and 2001 waves of the Wage Survey. Individuals cannot be directly identified across waves but one can try to match workers employed by the same firm in year t and t+1 who have the same gender, year of birth, level of education (9 grades), and 4-digit occupational code. In this way 52,057 workers observed in 2000 could be identified in the 2001 sample. For reasons explained by the WS survey design the panel is biased for small-firm employees that we do not correct in the forthcoming rough calculations. The important point we wish to check using this panel is how the minimum wage workers of 2001, and those earning slightly more, were recruited from the employed population of 2000. For each worker we know his/her gross earnings of May 2001 and thus whether or not he/she was di-

[22] Allowing flexible baseline hazard by using year dummies results in drop-outs from the estimation sample because of failures completely determined in some duration categories.

rectly affected by the minimum wage increase. [23] As shown in *Table 19* as much as 83.6 per cent of the treatment group was estimated to be affected but only 54.4 per cent of the population analogous to our control group was unaffected. The control group is thus far from being the ideal one but we are not deeply concerned with it because, since the vast majority of the misclassified workers are found in the control group, our model underestimates the treatment effect and thus provides *a fortiori* results. [24] The fact that the second minimum wage hike became a credible promise (threat) by the Autumn of 2001 also biases the observed treatment effect downwards.

Data

We use the LFS Supplementary Survey of 2001 April-June – the only wave since 1993 when respondents were asked about wages. A total of 22,416 employees provided wage data. We accepted the gross wage figure as reported by the respondents, or estimated from the net figure by the CSO.[25] Five wage categories were distinguished: below thousand Ft 36, 36-44 (treatment), 44-50 (control), 50-75, 75-100, and over 100. Setting a relatively broad bracket for the treatment group is explained by the fact that the reported LFS figures relate to total monthly earnings subject to both random and permanent variations around the base wage.[26]

Workers were followed in the 3rd and 4th quarters of 2001. The Hungarian LFS is a rotating panel with 1/6 of the sample leaving the survey each quarter. Excluding part-timers (workers who customarily work less than 6 hours a day), and the spells ending in dropout, we got to an estimation sample of 28,315 quarterly spells for all workers and 22,315 spells for workers with a tenure longer than two years. The descriptive statistics are presented in *Table 20*. A total of 1.68 and 1.1 per cent of the observed spells ended in exit from employment in the two samples. The treatment groups comprised 18.7 and 15.2 per cent, respectively, while the control

[23] Those earning less than Ft 38,685 in May 2000 are assumed to have been affected. Workers were assigned to the treatment and control groups on the basis of their total gross earnings in May 2001 – the variable available in the LFS of April-June 2001.

[24] Reducing the proportion of misclassified workers by shifting the wage bracket (now 110-125 per cent) upwards seems to us a mistaken strategy – what is gained in terms of more precise classification is likely lost in terms of comparability.

[25] The gross figure is what labour contracts include in Hungary.

[26] Workers earning below Ft 36,000 were excluded from the estimation sample because this category includes many workers planning to retire. Furthermore, the WS Individual Panel showed high mobility between this and other wage brackets suggesting that sub-minimum wages are often explained by temporary reasons.

groups contained 9.8 and 9.3 per cent. Workers in the two samples spent 5.9 and 7.3 years in their jobs on average.

Results

The results on the whole sample presented in *Table 21* show that males, prime age and skilled workers, public sector employees, union members, and those in tenured jobs were less likely to leave employment in July-December 2001. The baseline hazard for unemployment was falling until about 9 months spent in the job and was basically flat at longer tenures.

The parameters of our interest are 3.23 (2.75) for the treatment group and 1.95 (1.13) for the control group with respect to exit to unemployment. In the case of exit to non-participation the parameters were 6.21 (6.85) and 4.49 (3.23). Though the members of the treatment group were more likely to leave employment the difference between them and the controls were statistically insignificant as shown by the F-tests for the equality of the parameters at the bottom of the table.

In *Table 22* the dummies for the wage categories are interacted with regional unemployment rates under the expectation that a stronger shock to the labour market of depressed regions resulted in higher outflows from employment. Here again, the unemployment-related differentials in the jobloss probabilities of minimum wage workers seem to be larger than those between the members of the control group but the coefficients do not differ from each other at conventional levels of significance.

The results for workers with at least two years of tenure – our prefered specification - are presented in *Tables 23 and 24*. In this case we observe large and statistically significant differences between the treatment and control groups with respect to exit to unemployment. The respective parameters are 1.05 (3.00) versus 0.15 (0.31) significantly different at the 0.04 level. The hazards of flows to non-participation are statistically equal in the two groups.

The regional differences in exit to unemployment *within* the minimum wage group also seem larger than those in the control group though 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 rates for a 25 year old male worker with 5 years of tenure are 0.243 and 0.119 per cent in the treatment and the control groups, respectively. Both of these rates suggest very long prospective tenures, lasting longer on average than the time until retirement. The fraction of group members staying in their jobs for the rest of their career (calculated as $(1\text{-h})^{40}$ given a retirement age of 65 and assuming constant hazard) is estimated to be 95.3 and 90.7 per cent 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 all 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 differences 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 found weak evidence that the regional differences in the outflow rates of minimum wage workers were larger than in other wage categories.

4.4. Outflows from unemployment 1998–2002

A minimum wage hike is expected to reduce the job finding probabilites of unemployed workers who were paid below the new minimum wage prior to losing their jobs. Inasmuch as their earnings reflected the employer's evaluation of their marginal product they will not be demanded at a significantly higher wage.

This, however, is only one side of the coin. A higher minimum wage makes employment more attractive for the non-employed and encourage them to look for jobs more actively. The worker's low pre-unemployment wage is a wrong signal but only one of the signals the employer takes into consideration at the hiring decision. The expected reduction of voluntary quits and search frictions, or a prospective increase in work effort, may make the employment of low-wage workers profitable for the firm after a minimum wage hike.

Whether the job finding probabilites of low-wage workers were predominantly shaped by the classic demand-side effect, or by more complex mechanisms offsetting the adverse impact of the minimum wage shock, is the third question addressed in detail. At this aim we use a panel comprising 171 labour offices and 51 months from January 1998 to March 2002.

For each office and month we have an estimate of 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 during the month. The same information is available in a breakdown by educational levels (but we do not know the composition of skill groups by wage levels and vice versa).

The return to comparing the exit to job rates of low-wage and high-wage workers is clearly minimal as these groups sharply differ in terms of skill levels and can be differently affected by aggregate, regional, or industrial shocks. In order to get closer to what we believe a sensible comparison we shall 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.

This choice can be justified by data from the NLC Exit to Jobs Survey of April 2001. The survey covered 9,502 UI recipients finding a job of which 78 per cent was low-skilled (had no certified secondary school education) and 50 per cent was low-wage (earned less than the median prior to unemloyment).[27] The crosstabulation of these two attributes suggested that the vast majority (81.4 per cent) of the low-wage workers were low-skilled but only half of the low-skilled (48.8 per cent) were low-wage.[28] The exit rate of the *whole* low-wage group (h^{LW}) relative to the exit rate of the *whole* low-skilled group (h^{LS}) can therefore be considered a crude approximation of the wage-level specific job finding rate ($h^{LW}|LS$) within the unskilled group. We expect the h^{LW}/h^{LS} ratio to fall after the minimum wage increase as far as its aftermaths are dominated by demand-side effects. We study the evolution of the h^{LW}/h^{LS} ratio by estimating equation (7).

$$\ln\left(h^{LW}/h^{LS}\right)_{it} = \beta \ln U_{it} + \lambda MD + \gamma YRD + c_i + v_{it} \qquad (7)$$

where h 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 expectation is $\beta<0$ because it is more difficult for low-wage workers to find jobs when the market is depressed and vice versa. 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 unskilled groups.[29] These fixed effects are captured by c_i.

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[27] Previous earnings were discounted using the average wage index.

[28] If the wage level is inferred from the benefit, as will be done in the forthcoming sections, the respective proportions are 82.1 and 56.7 per cent.

[29] 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

We can suspect that $E(U_{it}v_{it})\neq 0$. Equation (7) is controled for seasonality and aggregate and regional shocks by the inclusion of the month and year dummies and regional unemployment. However, some sort of regional shocks may exert particularly strong impact on the h^{LW}/h^{LS} ratios. When whole plants are closed or opened employers screen their workers/applicants more carefully than they usually do and while doing so interpret low-wages as a signal of low productivity. Since both closures and openings tend to *decrease* the h^{LW}/h^{LS} ratio but push the unemployment rate to opposing directions the sign of the correlation between v and U is *a priori* indeterminate. (If screening is particulary strong in cases of closures, which is likely to be the case, the correlation will be negative). Allowing for the possibility that U and v are correlated U_{it} was instrumented with its t-1 and t-2 values.[30]

Similarly to *Deere, Murphy* and *Welch* (1996) we focus on the coefficients of the year dummies in identifying minimum wage effects. The expectation is that prior to the minimum wage hike the year effects were weak and similar in magnitude but there was a significant break in 2001. We also expect that the relative exit rate of low-wage workers fell more in high-unemployment regions. Therefore at the second step we interact a dummy for the post-hike period with dummies for regional unemployment to allow γ to differ by regions.

Data

The unemployment insurance (UI) register has the unique advantage of containing data on the unemployed workers' pre-unemployment wage levels. The labour 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 increased the costs of data collection far beyond the resources at our disposal. We recourse to the NLC Exit to Jobs Survey of April 2001 again to show how pre-unemployment earnings and benefits were related. As shown in *Table 25* workers recieving lower than average benefits typically earned less than the median before unemploy-

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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.

[30] It is worth noting that there is no straightforward link between the flows of the UI system and unemployment. In 2000, the UI stock accounted for only 47% of the stock of ILO-unemloyment. As a result of poor targeting less than 20% of the ILO-unemployed received benefit in that year. (MT 2001 227-230).

ment, and vice versa. (The 2001 March value of the median pre-unemployment wage was just equal to Ft, 40,000, the new minimum wage). In this data set 92.3% of the recipients could be correctly classified as 'low-wage' or 'high-wage' on the basis of the benefit.

The evolution of the quarterly relative exit to job rates of low-wage workers are shown in *Table 26*. Given that the relative job finding probabilities display strong seasonality the table is organised by years and quarters. Comparing the quarterly figures columnwise suggests that the relative exit rate of low-wage workers fell by 7-8% in 2001–2002 compared to job seekers with primary and/or vocational qualification. Consistently with the aggregate figures and the LFS data analysed in section 4.1. we observe that the shift took place immediately after the first minimum wage hike. It should be noted that the outflow rate of low-wage workers did not change remarkably in absolute terms. Similarly to the aggregate data the UI figures display a sudden break in an improving trend and this was particularly the case with the low-wage UI recipients.

The changes between 2000 and 2001 were not driven by a few outliers: the annual *relative* exit to job rate of low-wage workers fell in 75% of the offices representing 81.5% of the UI stock in the base period.

Results

The estimation results are shown in *Table 27* (versions A and B). In 2 per cent of the cases the exit rate of low-wage workers were zero – in version A these cases were excluded and in version B the zeros were replaced assuming the outflow of ½ person. The qualitative results are identical.

The estimates support that the h^{LW}/h^{LS} ratios negatively correlated with the unemployment rate though the respective parameters are weakly significant. More importantly, the results suggest that the job finding probability of the low-wage unemployed relative to the unskilled dropped by 9 percentage points in 2001 and further 5 percentage points in January-March 2002.

In *Table 28* the pairwise equality of the year effects are tested using the coefficients from version B. The parameters for 1999 and 2000 are pairwise equal, those for 2001 and 2002 are sharply different from each of the year effects of 1998–2000, and 2001 and 2002 are also different though at a lower significance level. 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 -.0845 (9.38) suggesting that the h^{LW}/h^{LS} ratio fell from 99.1 to 91.1 per cent.

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 capture the long-term differences in the h^{LW}/h^{LS} ratios they do not control for pthe expected regional variations in the *changes* of the ratio when the minimum wage increases. In a low-wage region most 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} much stronger than h^{LS} so the ratio falls substantially. The bias in the estimated coefficients of the interactive terms therefore leads to an underestimation of the effect hitting the low-wage regions. [31]

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 per cent of the working age non-employed excluding students and pensioners received UI at the eve of 2001 – a small and non-randomly selected minority.[32] 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 labour market.

5. CONCLUSIONS

Every piece of information we could analyse in this paper suggested that the Hungarian government’s decision to increase the minimum wage by 57 per cent in 2001 implied a loss of employment opportunities. Aggregate employment did not remarkably fall in absolute terms but deviated from its path followed in preceding years, as soon as January 2001. This remained true when the relation of employment growth to GDP growth was considered. Had the economy remained on its path followed in 1998–2000 employment should have risen by about 1.7 % in 2001 at the given rate of

[31] It is worth emphasising at this point that the change in the exit rates were clearly wage-specific rather than skill-related. Estimating equation (7) with h^{LS}/h^{HS} on the left hand (HS:high-skilled) yields a coefficient of 0.0076 (0.51) for 2001-2002. The contrast between low-wage and high-wage UI recipients was particularly strong as suggested by a coefficient of -.1739 (6.20) for the ‘regime dummy’ in an equation with h^{LW}/h^{HW} on the left hand

[32] Author’s calculation from LFS 2000 4th quarter.

GDP growth. It actually fell by 0.4 per cent. Groups exposed to stronger shock had less favourable employment records compared to other groups and their own past experience.

The effect of minimum wages on employment was expected to be strongest in small firms and marginal jobs. We found that small firms exposed to stronger shock lost more jobs indeed: one per cent average wage increase implied by the minimum wage hike reduced their level of employment by 0.22-0.33 per cent depending on region. The employment records of the losers were at odds with their development in previous years. Similarly to the papers by *Rama* (2000) and *Alatas* and *Cameron* (2003) on the Indonesian case, the closest analouge 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. For lack of adequate data we could not study the impact on medium-sized firms. The small firm sector we could analyse in detail lost about 3 per cent, or 11,000 jobs, in a year.

In view of a strong linkage between low pay and short tenures we made attempts at analysing how the flows between employment and unemployment were affected. We found that workers paid at the minimum wage after the hike (most of them paid below this level beforehand) were more likely to become unemployed in the 2nd-4th quarters of 2001 than tehir observationally similar counterparts paid marginally above the minimum wage. This finding related to workers who had a stable job for more than 2 years prior to the minimum wage hike, excluding the possibility that the results were driven by the coincidence of low wages and job hopping. Analysing a panel of UI outflows covering 1998–2002 we found that the job finding probability of low-wage UI recipients (most of them unskilled) markedly deteriorated from the 1st quarter of 2001 onwards compared to the whole unskilled population recieving UI. The aggregate effect of the changes in the observed flows would be difficult to summarise in a single indicator. The findings are qualitative and suggest that low-wage workers found more difficult to keep their jobs and find new ones after the minimum wage hike.

The government's declared assumption that a large minimum wage increase has negligible effect on labour demand while it exerts strong positive influence on search frictions and incentives, and hence employment, was apparently mistaken. This paper observed the net effect of demand and supply reactions and as such was unable to seperate the two sides. However, in case the post-hike evolutions were dominated by monopsony, search friction, or efficiency wage mechanisms the high-unemployment, low-wage regions ought to have experienced more favourable employment records. Our findings suggested that these regions were equally or even worse affected than others.

Our analysis can be subject to criticism for its old-fashioned 'differential treatment' approach. In a non-experimental setting it is difficult to disentangle the minimum wage effect from unrelated disturbances, such as shifts in demand for low-wage versus high-wage workers due to technological change or structural shocks. We responded to this challenge by chosing a short and otherwise peaceful period for the study of the aftermaths, and by controling the estimates for such distrubances when it was possible with the data at hand.

There is no general agreement in the recent literature as to how a minor increase in the minimum wage affects employment but most analyists agree that a major hike is likely to eliminate jobs. Where the frontiers between 'minor' and 'major' are is hard to answer in general – it seems that Hungary's experiment of 2001 is an example for the latter.

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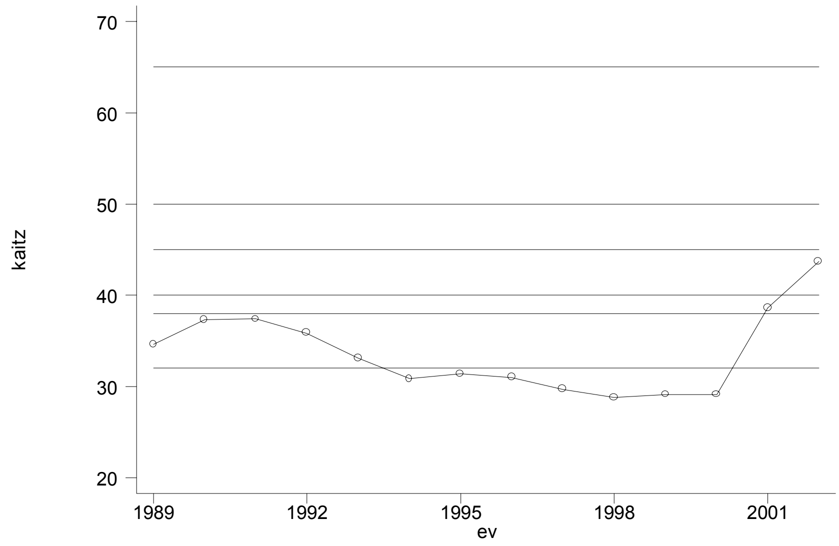
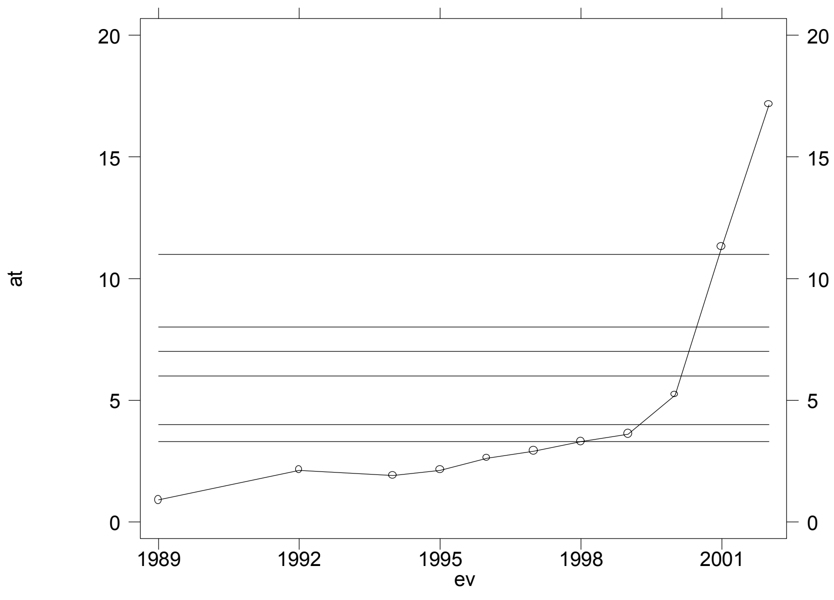
FIGURES

Figure 1: **The minimum wage-average wage ratio 1989–2002**



Source: 1998-2001: Munkaerőpiaci Tükör 2002. 2002: Wage Survey
Lines: selected OECD countries 1992–93 (*Dolado* et al 1996)

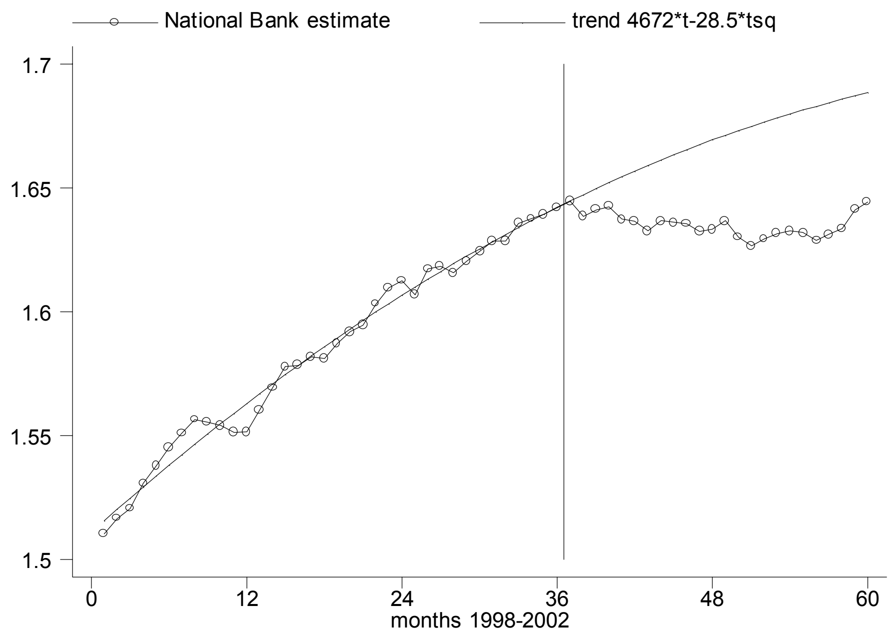
Figure 2: **Workers paid near the minimum wage 1989–2002**



Near: ±5 % range.
Source: 1989–2001: Munkaerőpiaci Tükör 2002. , 2002: Wage Survey
Lines: selected OECD countries 1992-93 (*Dolado* et al 1996)

Figure 3: **Employment in 1998-2002**

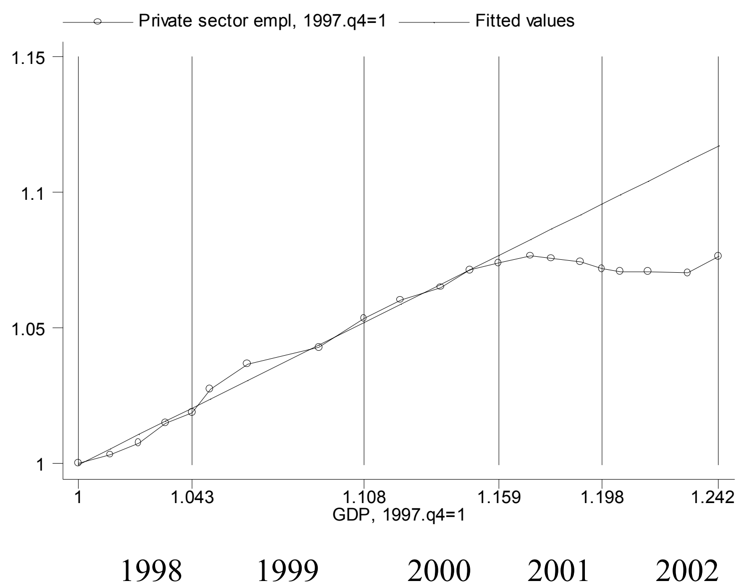
(Seasonally adjusted monthly levels, million, agriculture and the public sector excluded)



Source: LFS, adjusted by the National Bank of Hungary.

Figure 4: **Quarterly GDP and seasonally adjusted employment in 1998–2002**

The public sector and agriculture excluded. 1997.q4 = 1.



Sources: Employment-National Bank. GDP – CSO, www.ksh.hu/stadat

Figure 5: **Illustration of the estimated aggregate employment effect**
(Seasonally adjusted monthly employment levels, million, agriculture and the public sector excluded)

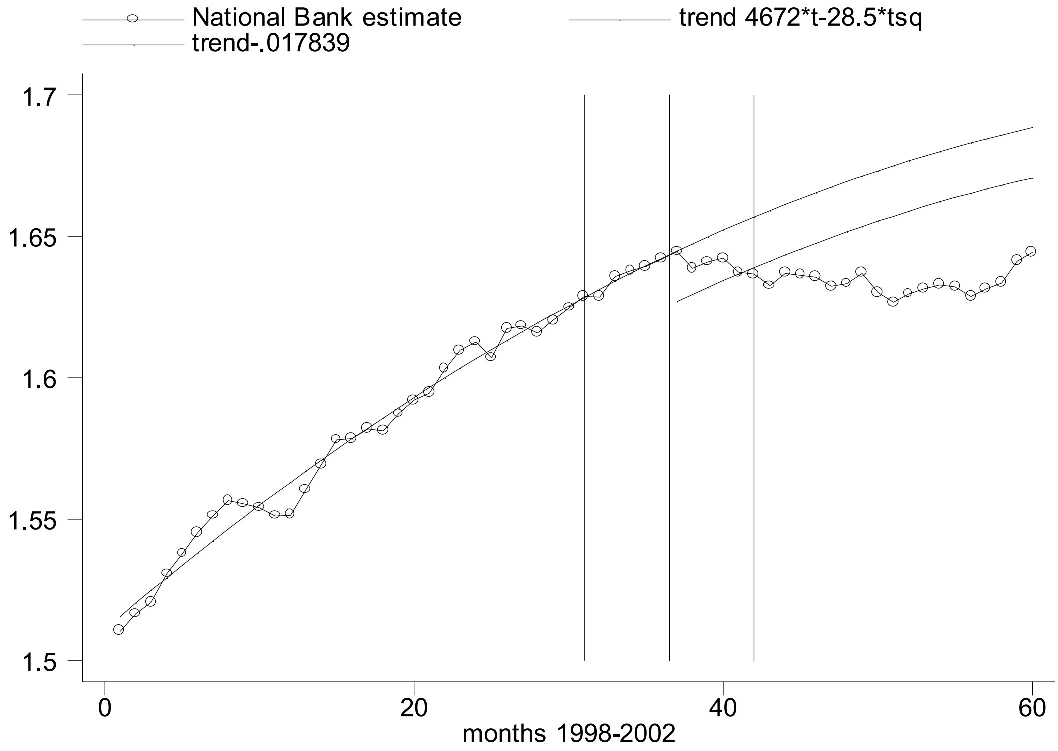
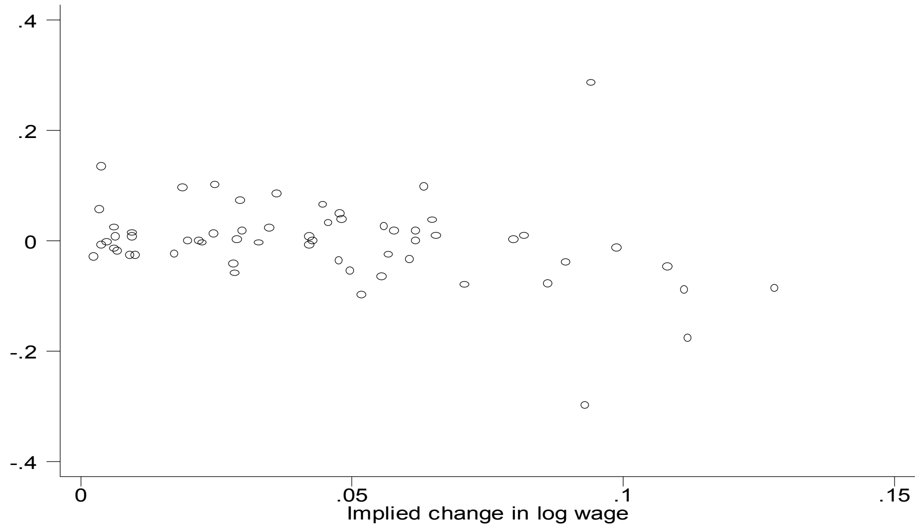


Figure 6: **Change of employment and the minimum wage shock in 60 groups**
Before: 2000. 4th quarter. After: 2001. 4th quarter. *Source:* LFS

Log change in the
employment ratio



TABLES

Table 1: **Fraction low-wage and ω in May 2000**

(A) Fraction low-wage (Base wage<Ft 38,685, per cent)

Regions	15-24 old	25-34 old	35-44 old	45-54 old	55 & older	Total
Primary						38.4
1st	34.7	30.3	31.9	28.0	33.1	
2nd	45.3	35.5	34.1	36.6	38.6	
3rd	57.9	46.8	44.3	43.3	42.4	
4th	58.5	52.9	54.5	49.9	50.6	
Vocational						27.5
1st	32.7	29.6	21.0	18.2	14.8	
2nd	42.3	29.8	24.0	21.2	17.7	
3rd	51.3	35.4	30.3	23.8	19.0	
4th	52.5	39.4	33.6	22.9	22.5	
Sec, high						11.3
1st	22.9	12.8	9.2	6.7	5.8	
2nd	28.9	16.0	9.9	6.7	5.2	
3rd	35.9	17.7	9.9	7.5	5.4	
4th	38.1	18.7	9.6	6.7	4.3	
Total	36.0	24.5	20.1	17.6	16.2	21.7

(B) Estimated ω (per cent)

Regions	15-24 old	25-34 old	35-44 old	45-54 old	55 & older	Total
Primary						6.0
1st	7.1	5.3	5.0	4.2	5.4	
2nd	10.2	7.2	6.2	5.5	6.2	
3rd	13.2	9.7	7.9	6.9	7.0	
4th	16.7	13.1	10.1	8.8	10.3	
Vocational						4.1
1st	5.5	4.3	3.0	2.3	1.9	
2nd	8.7	4.9	3.6	2.8	2.5	
3rd	12.8	6.4	4.8	3.9	3.2	
4th	12.4	7.4	5.1	3.4	2.8	
Sec, high						1.0
1st	2.4	0.9	0.6	0.4	0.4	
2nd	4.3	1.4	0.8	0.5	0.4	
3rd	6.5	2.2	1.0	0.6	0.5	
4th	5.8	2.1	0.9	0.6	0.3	
Total	6.1	2.6	2.1	1.0	1.0	2.3

(C) Shares in full-time employment

Age	15-24 old	25-34 old	35-44 old	45-54 old	55 & older	Total
	10.0	24.0	26.6	31.4	7.9	100.0
Education	Primary	Vocational	Sec,high			
	20.1	30.7	49.2			100.0
Regions	1st	2nd	3rd	4th		
	46.5	20.8	21.8	10.8		100.0

Data *source:* Author's calculation from the Wage Survey, May 2000. Nobs= 179,177

Table 2: **Open non-compliance**

The proportion of workers paid below the new minimum wage

Source:	Wage Survey	Labour Force Survey	UI Exit to Jobs Survey
Date:	May 2001	April-June 2001	April 2001
Wage concept	Gross base wage	Earnings*	Gross earnings
Reported by:	Firm	Worker	Worker
Full-time (>36 hours a week)	1.92	3.63	1.37
All workers	n.a.	5.50	2.57
Nobs	182,263	22,416	8,811

*) Gross earnings<Ft 40,000 if gross wages were reported. Net earnings<Ft 30,800 if net wages were reported

Table 3: **Low-wage* UI recipients finding a job in April 2001**

Source of income in the new job	Head	Per cent	Per cent earning less than Ft 40,000 in new job
Fixed monthly salary	2,043	64.7	4.8
Hourly wage	1,067	33.8	5.3
Contract fee	47	1.5	2.1
Total	3,157	100.0	5.0

Source: UI Exit to Jobs Survey, April 2001.

*) Low-wage: pre-unemployment gross earnings lower than Ft 40,000 at 2001 March value.

Table 4: **Actual wage growth regressed on the estimated average wage growth implied by the minimum wage increase** (ω, 60 groups, robust regression)

Dependent/Coefficients	b_ln(ω)	HIGH	Constant	F	F-test: b=1
2000-2001					
Δln(base wage)	0.9598	0.0180	0.1283	49.37	0.14
	(9.05)	(2.49)		(0.0000)	(0.71)
Δln(total earnings)	0.9988	0.0073	0.1093	50.73	0.00
	(8.37)	(0.90)		(0.0000)	(0.99)

Units of observation: 60 groups combining 3 educational levels, 5 age groups, and 4 quartiles of 151 micro-regions ordered by their unemployment rates in May 2000.

Before/after: May/May. Data *source*: Wage Surveys 2000, 2001, 2002

Table 5: **Descriptive statistics of the estimation sample of** *Table 6*

	Mean	St. dev.
Minimum wage shock	0.037	0.052
Producer prices (PPI)	1.062	0.028
Log change of (* if PPI-adjusted) :		
• Output*	0.042	0.326
• Employment	-0.043	0.169
• Wage cost*	0.083	0.245
• Total compensation*	0.105	0.242
Share of small firms in employment	0.342	0.289
Unemployment (mean of regional rates)	0.075	0.033
Base period employment	3,466	7,152
Number of industries	432	

Table 6: **The wage effect of the 2001 minimum wage increase**
Estimates of system (2)-(3). Number of observations: 432 industries

Benchmark model

	Sure	3sls	Sure	3sls
	W=Wage cost		W=total compensation	
Minimum wage shock	0.9442	1.0049	0.8650	0.9530
	(7.26)	(7.57)	(5.65)	(6.06)
Productivity	0.2150	0.0492	0.2919	0.0627
	(7.08)	(1.13)	(8.14)	(1.22)
Fraction unionised	0.0451	0.0366	0.0832	0.0771
	(2.56)	(2.02)	(4.01)	(3.59)
Mean log unemployment	0.0091	0.0132	-0.0071	-0.0043
	(0.48)	(0.69)	(0.32)	(0.19)
Constant	0.0863	0.1161	0.0469	0.0783
R2	0.1599	0.1442	0.1300	0.1083
	Employment		Employment	
Output	0.4914	0.4733	0.4956	0.4701
	(7.57)	(17.87)	(19.06)	(17.77)
Wage cost (Total comp.)	-0.4335	-0.2678	-0.4186	-0.1377
	(8.88)	(2.09)	(10.01)	(1.15)
Share of small firms (5-25)	0.0374	0.0350	0.0832	0.0505
	(1.31)	(1.21)	(1.82)	(1.63)
Constant	-0.0407	-0.0512	-0.0341	-0.0600
R2	0.4523	0.4597	0.4530	0.4530

Augmented model: Minimum wage shock interacted with unionisation

	Wage cost 3sls	Total comp. 3sls
Minimum wage shock	0.7682	0.6296
	(3.78)	(1.79)
Minimum wage shock × Fraction unionised	0.6656	0.8953
	(1.57)	(1.79)
Fraction unionised	0.0644	0.1146
	(2.55)	(3.84)
Productivity	0.0446	0.0579
	(1.03)	(1.13)
Mean log unemployment	0.0138	-0.0033
	(0.72)	(0.15)
Constant	0.1119	0.0728
R2	0.1468	0.1120
	Employment	Employment
Output	0.4727	0.4698
	(17.84)	(17.73)
Wage cost (Total employee compensation)	-0.2483	-0.1269
	(1.96)	(1.06)
Share of small firms (5-25)	0.0346	0.0511
	(1.19)	(1.65)
Constant	-0.0522	-0.0612
R2	0.4587	0.4506

Units of observation: 4-digit industries. Year-on-year data 2000/ 2001. The monetary aggregates are PPI-adjusted and are in change of logs. The cases are weighted with employment in the base period. The 3sls equations include 9 sector dummies as additional exogeneous regressors. All data are drawn from the FR except minimum wage shock (WS), fraction unionised (WS 1998) and PPI (CSO-STADAT).

Table 7: **Employment regressed on exposure to the minimum wage increase using grouped data,** 2000–2001

Dependent: log change of employment. Units of observation: 60 (40) groups as in *Table 1.* Data *source:* LFS

	Log minimum wage shock (ω)	Log change of population*	Constant	R2
LFS, change of employment between 2000 4th quarter and 2001 4th quarter				
All groups (60)				
Unweighted	-.7543 (2.51)	.9484 (12.20)	.0311	.7470
*Weighted***	-.4543 (2.04)	.9839 (12.80)	.0006	.7432
Unskilled (40)				
Unweighted	-1.4754 (2.95)	.8607 (8.81)	.0845	.7599
Weighted	-1.2923 (3.29)	.8849 (9.31)	.0684	.7559

Table 8 : **Employment regressed on exposure to the minimum wage increase of 2001 using grouped data,** 1998–2001

Dependent: log change of employment, Data source: LFS. Units of observation: 60 groups (of which 40 unskilled) as in *Table 1.* Weighted with population in the base period. ω = exposure of the group to the minimum wage increase in 2001

	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
R2	.7811	.7352	.9153	.8283	.7616	.7584

POP=1999-2000 and 2000-2001 working age population less old-age pensioners and students. 1998-99: working age population. Due to a change in the registration of student status the definition used in 1999-2001 was not applicable.

Table 9: **Employment ratios regressed on exposure to the minimum wage shock**
Pooled OLS. Sample as in Table 1, Nobs=60x4. Cases weighted with 2000 4th quarter employment.
Dependent: ln(employment ratio in t / employment ratio in 2000 4th quarter)

	2001. Q1	2001. Q2.	2001. Q3.	2001. Q4.
		All groups		
Ln(ω)	-.6726 (2.57)	-.5347 (2.47)	-.6027 (2.47)	-.5793 (2.49)
2nd quarter	-	.0085 (2.23)	.0089 (2.27)	.0087 (2.28)
3rd quarter	-	-	-.0017 (0.28)	-.0019 (0.30)
4th quarter	-	-	-	-.0024 (.040)
Constant	.0229	.0157	.0202	.0193
R^2	.1154	.0957	.0998	.0950
Nobs	60	120	180	240
		Unskilled		
Ln(ω)	-1.0890 2.67)	-.9744 (3.04)	-1.1763 (3.15)	-1.1659 (3.34)
2nd quarter	-	.0177 (2.93)	.0193 (3.02)	.0193 (3.10)
3rd quarter	-	-	.0159 (1.83)	.0158 (1.83)
4th quarter	-	-	-	.0040 (0.44)
Constant	.05289	.0463	.0578	.05725
R^2	.1485	.1565	.1839	.1848
Nobs	40	80	120	160

Table 10: **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 ratio	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

continue

Budapest	1.1829	5.97
Lake Balaton	1.3359	3.99
Agriculture	1.5981	6.64
Forestry	5.3203	13.99
Mining	1.2867	1.19
Food processing	2.2815	11.46
Tobacco	0.1610	-1.33
Textile	4.3558	17.51
Clothing	2.7449	13.41
Leather	2.5164	9.88
Wood	2.2155	8.86
Paper	1.1667	1.03
Printing and publishing	2.3043	8.60
Chemical	1.0828	0.55
Rubber	1.8324	6.70
Nonferrous	1.4232	3.53
Metallurgy	0.9486	-0.35
Metal processing	1.1590	1.85
Office machinery	2.9042	6.66
Electric engines	1.4820	4.45
Telecommunication equipment	1.8337	5.72
Instruments	0.8021	-1.53
Car manufacturing	0.7943	-1.66
Other vehicles	1.3022	1.38
Furniture	2.2223	8.73
Electric energy production and transfer	0.8669	-0.81
Water supply	1.0468	0.31
Construction	2.4155	12.54
Car repair	3.5397	15.88
Wholesale trade	3.0262	15.29
Retail trade	3.5255	18.11
Hotels and restaurants	6.3720	23.39
River and sea transport	2.9350	3.75
Air transport	1.9456	1.17
Transport related services	2.7817	9.77
Railways	1.5941	3.76
Public transport	0.0130	-1.96
Other transport	4.8603	20.09
Mail	1.5194	3.04
Telecommunication	1.3085	1.10
Banking	0.1513	-11.28
Insurance	70.8793	38.52
Real estate	2.0142	7.45
Computing	2.8808	9.55
Research and development	0.3697	-2.22
Business related services	3.0010	15.13
Cultural services	3.3564	11.86
Other services	4.8408	13.93

continue

Number of observations	119,739
Pseudo R2	0.3487
LR chi2 (71)	45,224.25

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 11: **The composition of the workforce paid near the minimum wage 2001**

Percentage share of:	Wage Survey	Labour Force Survey
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
Spouse without children	n.a.	37.8
Spouse with children	n.a.	24.4
Young adult living with parents (no children)	n.a.	19.3
Lonely parent	n.a.	6.6
Lonely	n.a.	4.3
Young adult living with parents (has children)	n.a.	4.1
Other	n.a.	3.5
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 Labour 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 strictly comparable to the WS data. The <5 category comprises small budget institutions (like the local governments or schools of small villages) in the LFS.

Table 12: **Small firm panel** 2000–2001 **– Probits of sample selection**

Sample	Dependent	Number of employees	Fraction low-wage	Lossmaker in 2000	Pseudo R2	Nobs
Small firms observed in 2000	Observed in 2001 (2,008)	.0012 (2.43)	-.1074 (4.96)	-.1239 (5.93)	.0209	2,874
Firms observed in 2000-2001	Has complete data (1,818)	.0036 (2.51)	-.0099 (0.60)	-.0581 (3.17)	.0166	2,008

*) The table shows the marginal effects

Table 13: **Descriptive statistics of the small firm panel** 2000–2001
Year on year data. Source: FR 2000, 2001

Variable	Mean	Median	Standard deviation
Employment 2000	12.7	13	4.44
Employment 2001	13.6	12	14.30
Value added 2000 (mFt)	60.1	24	618.1
Value added 2001 (mFt)	53.3	19	780.3
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
Net value of fixed assets 2000	58.5	16.0	374.57
Net value of fixed assets 2001	69.5	18.0	525.92
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

Table 14: **Small firm panel – Performance in** 2000–2001
Year on year data. Source: FR 2000, 2001

			Mean log (weighted with base-period employment):					
Minimum wage shock (ω)	Fraction low-wage May 2000	Mean ω (-1)	Average wage	Labour cost/PPI	Employment	Output	Fixed assets	Number of firms
0	0	0	.125	.062	.045	.046	.132	468
0-10 %	.274	.032	.158	.091	-.007	-.034	.047	632
10-25 %	.741	.166	.279	.177	-.054	-.007	.148	319
25-100%	.959	.359	.399	.305	-.090	-.032	.119	399
	.435	.119	.224	.146	-.020	-.017	.108	1,818

Table 15: **Small firm panel - 3SLS estimation of equations (4)-(5)**

	Coefficient	Z
Employment equation (log change 2000-2001)		
Log change of output	.3313	3.34
Log change of real labour cost	-.3732	3.77
Fixed assets/worker 2000	.0006	1.60
Lake Balaton	-.2240	2.24
Constant	.0408	2.23
Real labour cost equation (log change 2000-2001)		
Log minimum wage shock × 1st region quartile	.5987	10.01
Log minimum wage shock × 2nd region quartile	.7869	10.07
Log minimum wage shock × 3rd region quartile	.7604	9.58
Log minimum wage shock × 4th region quartile	.8999	7.31
Log change in fixed assets 2000-2001	.0248	2.12
Profit 2000	.0002	1.59
Constant	.0740	5.80
Observations	1,818	
Chi2 employment equation (sign)	33.21 (0.0000)	
Chi2 real labour cost equation (sign)	306.66 (0.0000)	
RMSE employment equation	.4503	
RMSE real labour cost equation	.2564	

The coefficients of 17 region dummies omitted. Labour cost, employment, and output are endogeneous. 10 industry dummies included as additional exogeneous regressors.

Table 16: **Small firm panel – Changes in the composition of employment**

Minimum wage shock (ω-1)	Fraction low-wage May 2000	Mean shock (ω-1)	Unweighted 2000	Unweighted 2001	Unweighted Change	Weighted 2000	Weighted 2001	Weighted Change
			The share of workers with 0-8 years in school					
0	0	0	9.5	9.2	-0.3	10.4	10.0	-0.4
0-10 %	.274	.032	15.1	14.3	-0.7	15.8	16.7	0.9
10-25 %	.741	.166	16.0	15.7	-0.3	16.9	16.4	-0.5
25-100%	.959	.359	15.7	13.5	-2.2	17.3	15.4	-1.9
Total	.435	.119	13.9	13.1	-0.8	14.8	14.5	-0.3
			The share of blue collar workers					
0	0	0	44.0	45.7	1.2	45.6	49.6	4.0
0-10 %	.274	.032	57.2	56.5	-0.7	59.2	59.5	0.3
10-25 %	.741	.166	63.5	61.5	-2.0	65.0	64.9	-0.1
25-100%	.959	.359	73.4	70.4	-3.0	75.8	73.9	-1.9
Total	.435	.119	52.5	57.5	-1.0	60.0	60.3	0.3

Table 17: **Small firm panel – Changes in the wage distribution**

	May 2000	May 2001		May 2001	May 2001
Minimum wage shock	Fraction high-wage*	Fraction high-wage**	Differ-ence	Fraction paid at the minimum	Fraction paid below the minimum
0	100.0	96.2	-3.8	3.1	0.7
0-10 %	72.6	78.1	5.4	16.9	5.0
10-25 %	7.7	40.9	7.7	49.2	9.9
25-100%	4.8	9.3	4.8	79.1	11.6

*) Paid more than Ft 38,685 (unaffected by the MW increase)
**) Paid more than Ft 40,000

Table 18: **Large firms - Minimum wage shock and employment change**
Data *source:* FR 1999, 2000, 2001

	1999-2000	2000-2001
Output (q)	.5499 (3.15)	.8535 (2.45)
Fraction low-wage (L)	.4817 (1.77)	-.2186 (1.06)
Constant	-.0944	-.1708
AR2	.3494	.2706
Number of firms	337	322

Note: 50 industry dummies and 18 region dummies omitted.

Table 19: **Jobloss – Estimated fraction of workers directly affected by the minimum wage increase in groups analogous to the treatment and control groups**
(Based on the WS Individual Panel 2000-2001, Nobs=52,057)

	Wage in May 2000		
Earnings in May 2001	<Ft 38,685 (affected)	>Ft 38,685 (unaffected)	Total
0.9-1.1 of the minimum wage (treatment)	83.6	16.4	100.0
1.1-1.25 of the minimum wage (control)	45.6	54.4	100.0

Table 20: **Jobloss - Descriptive statistics of the estimation samples**

	All employees		Tenure>24 months	
	Mean	St. dev.	Mean	St. dev.
Exit to unemployment	0.67		0.30	
Exit to non-participation	1.01		0.73	
Male	.5302076	.4990953	.5245165	.4994098
Age	38.49124	10.83934	40.27192	10.36101
Unskilled blue collarl	.0838568	.2771777	.0759896	.2649874
Semi-skilled blue collar	.1885293	.391141	.1689385	.374706
Skilled blue collar	.3455716	.4755625	.3502658	.4770638
Regional unemployment	.0947776	.0607545	.0925442	.0597517
Public sector	.1572019	.3639973	.1741024	.379206
Union member	.2114826	.4083669	.2502028	.4331394
Tenured job	.9218691	.2683821	.9617706	.191754
Wage Ft 36,000-44,000	.1878282	.3905817	.1522365	.3592581
Wage Ft 44,000-50,000	.0982127	.2976072	.0932583	.2908006
Wage Ft 75,000-100,000	.1765264	.381274	.1919952	.3938781
Wage Ft >100,000	.1592627	.3659272	.1718567	.3772643
2001 4^{th} quarter	.4333248	.495543	.434425	.4956924
Tenure in job (years)	5.873398	3.649435	7.292149	2.872526
Number of spells	28,986		22,315	

Table 21: **Exit from employment 2001 2nd-4th quarters – All workers**
Discrete time duration model, multinomial logit form

Left employment for:	Unemployment		Non-participation	
	Coefficient	Z	Coefficient	Z
Male	-.117595	-0.67	-.3045474	-2.30
Age	.1557931	2.55	-.2309851	-6.43
Age squared	-.0021441	-2.64	.0030271	6.58
Unskilled blue collar.	.6803479	2.26	.3473052	1.61
Semi-skilled blue collar	.6760733	2.51	.2858668	1.52
Skilled blue collar	.3759948	1.38	-.14125	-0.75
Unemployment (log)	.2676495	1.95	.6022078	5.14
Public sector	-.8444886	-2.15	-.054331	-0.25
Union member	-.7431471	-2.18	.1054853	0.51
Tenured job	-.8202438	-3.97	-.8207332	-4.91
Wage Ft 36,000-44,000 (TR)	.5755323	2.52	.1882866	1.10
Wage Ft 44,000-50,000 (CO)	.3248732	1.17	.0582941	0.24
Wage Ft 75,000-100,000	-.4710009	-1.25	-.3192486	-1.32
Wage Ft >100,000	.1512703	0.40	-.2651334	-0.94
2001 4th quarter	.5780868	3.38	.5662387	4.23
tenure (months)=1	2.17144	5.54	.9592629	2.64
tenure (months)=2	1.739318	5.83	1.191281	4.27
tenure (months)=3	1.343199	3.90	1.344827	4.16
tenure (months)=4	1.092284	2.98	-.8895244	-1.49
tenure (months)=5	.7046136	1.47	.6797435	1.74
tenure (months)=6	.7299295	1.22	.7083645	1.50
tenure (months)=7	1.457346	2.95	.2920986	0.52
tenure (months)=8	.6158116	1.05	.5323267	1.23
tenure (months)=9	1.106352	2.36	.0749606	0.14
tenure (months)=10	.3999608	0.66	-.4043508	-0.77
tenure (months)=11	1.133729	2.26	.74098	1.88
tenure (months)=12	-.1433495	-0.19	.4246895	1.04
tenure (months)=13	.5709417	0.78	.4262923	0.91
tenure (months)=14	1.068189	1.67	-.4802813	-0.81
tenure (months)=15	.8857183	0.99	-.4650041	-0.65
tenure (months)=16	1.121908	2.27	1.016324	2.14
tenure (months)=17	-.1872222	-0.26	-.7212356	-0.72
tenure (months)=18	1.128604	1.73	.5729359	0.88
Constant	-7.406454	-6.32	1.088524	1.39
Nobs		29,986		
-log likelihood		2224.9		
Pseudo R^2		.1073		
F-test b_treatment=b_control (unemployment)			.92 (.3376)	
F-test b_treatment=b_control (non-participation)			.31 (.5785)	

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>

Table 22: **Exit from employment 2001 2nd–4th quarters – All workers**

Discrete time duration model, multinomial logit form, unemployment interacted with the wage

Left employment for:	Unemployment		Non-participation	
	Coefficient	Z	Coefficient	Z
Male	-.1273354	-0.72	-.3392083	-2.65
Age	.1576268	2.60	-.232152	-6.51
Age squared	-.0021734	-2.69	.0030308	6.64
Unskilled blue collar.	.7610605	2.51	.3958495	1.83
Semi-skilled blue collar	.7513803	2.83	.3661171	1.97
Skilled blue collar	.4415427	1.61	-.0826386	-0.44
Public sector	-.8145393	-2.06	-.0309909	-0.14
Union member	-.746537	-2.19	.0968406	0.47
Tenured job	-.8512091	-4.05	-.8354018	-5.00
Wage Ft 36,000-44,000 (trm) * U	3.233252	2.75	6.212629	6.85
Wage Ft 44,000-50,000 (ctrl) * U	1.958068	1.13	4.492942	3.23
Wage Ft 50,000-75,000 * U	-.4224916	-0.21	5.086063	4.52
Wage Ft 75,000-100,000 * U	-6.258211	-1.52	2.536871	1.27
Wage > Ft 100,000 * U	.5945696	0.16	4.345021	1.89
2001 4th quarter	.5746337	3.36	.5636309	4.20
tenure (months)=1	2.231106	5.63	.9637096	2.63
tenure (months)=2	1.779082	5.98	1.215001	4.38
tenure (months)=3	1.375213	3.99	1.336321	4.09
tenure (months)=4	1.135882	3.12	-.847909	-1.43
tenure (months)=5	.7319302	1.53	.6910063	1.76
tenure (months)=6	.7468139	1.25	.6913502	1.48
tenure (months)=7	1.468803	2.98	.2917725	0.52
tenure (months)=8	.648402	1.11	.5431399	1.25
tenure (months)=9	1.136336	2.44	.0816181	0.15
tenure (months)=10	.4481487	0.75	-.3808861	-0.72
tenure (months)=11	1.167347	2.33	.7527259	1.91
tenure (months)=12	-.1395764	-0.19	.4143309	1.01
tenure (months)=13	.6007123	0.82	.4347663	0.93
tenure (months)=14	1.120376	1.79	-.4780475	-0.80
tenure (months)=15	.9282963	1.06	-.4410902	-0.62
tenure (months)=16	1.168169	2.38	1.043392	2.21
tenure (months)=17	-.1580555	-0.22	-.6775362	-0.68
tenure (months)=18	1.101846	1.69	.5452686	0.84
Constant	-8.017949	-7.37	-.8971664	-1.27
Nobs	28,315			
-log likelihood	2229.2			
Pseudo R^2	.1053			
F-test b_treatment=b_control (unemployment)	0.59 (.4426)			
F-test b_treatment=b_control (non-participation)	1.52 (.2157)			

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>

Table 23: **Exit from employment 2001 2nd–4th quarters – Tenure>24 months**
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 (trm)	1.059692	3.00	.1078196	0.44
Wage Ft 44,000-50,000 (ctrl)	.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)	

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>

Table 24: **Exit from employment 2001 2nd–4th quarters – Tenure>24 months**
Discrete time duration model, multinomial logit form, unemployment interacted with the wage

Left employment for:	Unemployment		Non-participation	
	Coefficient	Z	Coefficient	Z
Male	-.0902772	-0.29	-.6338063	-3.64
Age	.516615	3.40	-.3358542	-6.89
Age squared	-.0064062	-3.39	.0041834	7.14
Unskilled blue collar.	-.064672	-0.13	-.3501494	-0.89
Semi-skilled blue collar	.1677547	0.43	.1940126	0.80
Skilled blue collar	.2699696	0.70	.0871939	0.40
Public sector	-.9174815	-1.65	-.0171159	-0.06
Union member	-.7397677	-1.86	.1267663	0.56
Tenured job	-.3280886	-0.59	-.6645879	-2.10
Wage Ft 36,000-44,000 (trm) * U	3.967194	2.13	3.643163	2.37
Wage Ft 44,000-50,000 (ctrl) * 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
2001 4th quarter	.3097276	1.09	.3139032	1.78
Exp (tenure in years)	4.448773	2.59	-.269338	-0.09
	-15.19633	-4.88	1.607331	1.54
Nobs		22,315		
-log likelihood		1305.6		
Pseudo R^2		.0502		
F-test b_treatment=b_control (unemployment)			2.85 (.0914)	
F-test b_treatment=b_control (non-participation)			0.36 (.5477)	

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>

Table 25: **Exit from unemployment - Benefits related to pre-unemployment earnings**
UI recipients finding a job in April 2001

	Real pre-unemployment earnings (March 2001 value)		
Benefit:	Lower than the median	Higher than the median	Total
Lower than the mean	98.7	1.3	100.0
Higher than the mean	12.8	87.2	100.0

Table 26: **Exit from unemployment – Monthly exit to job rates (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 Labour Centre

Table 27: **Exit from unemployment – The estimation of equation (7)**

Fixed effects IV. January 1998–March 2002

	A) Zero outflows excluded		B) Zero outflows replaced by ½	
Log unemployment	-.0322	1.09	-.0321	1.05
1998	Ref.		Ref.	
1999	-.0248	2.42	-.0259	2.44
2000	-.0086	0.78	-.0103	0.91
2001	-.0915	7.59	-.0915	7.33
2002	-.1430	7.40	-.1426	7.11
Constant	-.1085		-.1071	
Nobs	8,332		8,428	
Groups	172		172	
Within R2	.0821		.0740	

The dependent variable is the log of the ratio of the exit to job rate of low-wage UI recipients to the rate of low-skilled UI recipients. The parameters of the 11 month dummies omitted

Table 28: **Exit from unemployment – F-test for the pairwise equality of year effects**

	1999	2000	2001	2002
1998	5.95 (0.0147)	0.38 (0.3623)	53.78 (0.0000)	50.50 (0.0000)
1999		2.26 (0.1331)	33.52 (0.0001)	38.84 (0.0000)
2000			27.59 (0.0000)	55.88 (0.0000)
2001				8.85 (0.0029)

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.

LFS – LABOUR 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.

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 EJS – NATIONAL LABOUR 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.

NLC OFFICE-LEVEL EXIT TO JOBS PANEL 1998–2002

The data base was built in the National Labour Centre in September 2002 using data from Hungary's 175 labour 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.

WS – WAGE SURVEY

The National Labour 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 per cent 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.