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THE IMPACT OF PAYROLL TAX REDUCTIONS ON EMPLOYMENT AND WAGES: A NATURAL EXPERIMENT USING FIRM LEVEL DATA

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Abstract

Past evidence on the incidence of payroll tax subsidies on employment and wages for disadvantaged workers has been quite mixed. Therefore, this paper makes use of a unique panel of firm level data and a natural experiment to analyze the incidence of wage subsidies on full-time manual workers and pre-tax wages. Using a number of straightforward evaluation estimators we find that employment subsidies increased full-time manual employment and pre-tax wages. Moreover, we find that employment subsidies have increased employment but not wages by more in low-wage exporting industries. This is line with a textbook description of labor markets where it is predicted that the incidence of employment subsidies on employment and wages is larger the more elastic is product and therefore labor demand and where the employment effect is larger and the wage effect is smaller the more elastic is labor supply because of a binding minimum wage.

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I. Introduction

Economists writing about the impact of globalization or recent technological change have highlighted the decreasing demand for less-skilled employment in developed economies (Freeman [2006], Autor, Levy and Murnane [2003], Goos and Manning [2007]). An interesting question thus is how developed economies can redistribute the gains from globalisation or technological change to assure that also disadvantaged workers fare well in face of current changes in relative factor demand.

Market incentives for increased educational investment and skill upgrading can play some role in alleviating the growing inequality and labor market prospects of the less-skilled. But the process of supply adjustment can take many years, and many disadvantaged individuals face financial and informational barriers to pursuing further education and training. Therefore, wage subsidies to private employers have often been proposed by economists as a relatively flexible and efficient method to improve the relative earnings and employment of the less-skilled (for example, see Phelps [1994]).

However, not much research attention has focused on evaluating micro demand-side policies to offset the decline in demand for less-skilled workers. This paper therefore makes use of a natural experiment to analyze the effects of payroll tax exemptions targeted at manual workers in the late 1990s in Belgium, generally known as the "Maribel subsidies". Given a unique panel of firm level data with information about whether or not a firm received subsidy

and, if so, the amount of subsidy received in any given year, we are able to examine the impact of employment subsidies on employment and wages in various ways. We find that employment subsidies have had a positive impact on manual employment and a positive but smaller impact on pre-tax wages. Moreover, we find that employment subsidies have increased employment but not wages by more in low-wage exporting industries. This is line with a textbook description of labor markets where it is predicted that the incidence of employment subsidies on employment and wages is larger the more elastic is product and therefore labor demand and where the employment effect is larger and the wage effect is smaller the more elastic is labor supply because of a binding minimum wage.

The remainder of the paper is organized as follows. The next section describes the theoretical motivation and briefly surveys the existing related literature. Section III describes the firm level data together with the employment subsidy system. Section IV presents the micro-econometric evidence and Section V concludes.

II. Theoretical Motivation and Related Literature

II. A. Theoretical motivation

Employment subsidies are defined as a per-period lump-sum reduction in employer social security contributions for each manual worker employed at

the firm. Given payroll taxes are proportional, the pre-tax ($w_{pre-tax}$) and post-tax ($w_{post-tax}$) wages are therefore given by:

$$(1) \qquad w_{post-tax} = w_{pre-tax}(1+t) - \tau$$

with t the marginal payroll tax and τ the employment subsidy paid to the firm. In this setting the marginal tax rate is not affected by the introduction of the subsidy, but the average tax rate becomes decreasing in the pre-tax wage.

Figure 1 provides the simplest possible framework to analyze the impact of the employment subsidy. An increase in employment subsidies shifts the unconditional labor demand curve out thereby increasing employment and the pre-tax wage. It is also straightforward to see that the increase in employment is larger the more elastic are labor supply and demand. Similarly, the increase in the pre-tax wage is larger the more elastic is labor demand and the more inelastic is labor supply. Besides an increase in labor demand, also labor supply can shift following an increase in employment subsidies. For example, if reservation wages are indexed to prices rather than wages, labor supply increases following an increase in employment subsidies.[1] Consequently, the increase in the pre-tax wage will therefore be smaller and the increase in employment larger.

It follows immediately from equation (1) that, when labor markets are competitive, the structure of taxation does not matter. That is, what matters

[1] That is, employment subsidies will shift the labor demand curve out, thereby increasing real market wages. So, if real reservation wages are constant, the increase in the real market wage induces an unambiguous positive substitution effect to labor supply.

for the labor demand response is by how much the employment subsidy decreases total costs independent of how exactly subsidies are being paid to employers. More generally, the nature of taxation should not matter in any model where each individual firm takes the equilibrium wage as given, such as a wide range of incentive wage models (e.g. the assumption that higher wages bring about more effort from employees, the assumption that higher wages attract better quality workers or the assumption that higher wages prevent workers from shirking (Pissarides [1998])).

However, the structure of the tax system does matter when each firm no longer is a wage taker. For example, when workers are organized in trade unions and wages are determined after a bargain between the firm and the union. To see this, consider Figure 1 again. Now an increase in employment subsidies will shift the labor demand curve out, just as before. But also the wage bargaining curve will directly shift out as a response to an increase in subsidies. The intuition is that more employment subsidies make the payroll tax system more progressive which implies unions perceive the labor demand curve to become more elastic thereby moderating their wage demands.[2]

In sum, employment subsidies are expected to increase manual employment in subsidized firms. This employment effect will be larger the more elastic are labor demand and supply. Pre-tax wages for subsidized

[2] Moreover, under particular assumptions about bargaining and production technologies, it is easy to show that the shift in the wage bargaining curve following an increase in Maribel subsidies results in a decrease rather than increase in pre-tax wages. For example, Lockwood and Manning [1993] show this is the case for right-to-manage bargaining assuming isoelastic labor demand.

workers are also expected to increase the more elastic is labor demand and the more inelastic is labor supply but the wage incidence of employment subsidies could be mitigated if also labor supply increases because worker's outside options become relatively less interesting or because unions moderate their wage demands.

II. B. Related macroeconomic literature

Pissarides [1998] simulates the general equilibrium incidence of taxation using the different models discussed above. Assuming that the marginal payroll tax is 40 percent of the pre-tax wage, he finds that a lump-sum subsidy set at 20 percent of the pre-tax wage decreases unemployment with 4 percentage points and increases the pre-tax wage with 13 percent if unemployment benefits are indexed to prices and not wages and labour markets are assumed competitive. In case unemployment benefits are indexed to wages and not prices, the impact on pre-tax wages is similar but unemployment decreases by less than a percentage point. Similar effects are found assuming incentive wages though the decrease in unemployment is generally larger because the "non-shirking condition" is modelled to be more elastic than the labour supply curve. In the presence of unions, an employment subsidy set at 20 percent of the pre-tax wage decreases the unemployment rate with 6 percentage points and increases the pre-tax wage with 12 percent if unemployment benefits are indexed to prices and not wages. If unemployment

benefits are indexed to wages and not prices, the effect on the pre-tax wage is similar but unemployment only decreases with 1 percentage point.

Turning to the empirical evidence, Figure 2 uses OECD data to show the relationship between employment and payroll taxes in various developed economies. Countries, such as Belgium, France and Germany with a payroll tax burden of more than 40 percent of pre-tax wages are characterized by lower employment rates. However, Nickell [2003] surveys eleven studies using cross-sections or panel data for a number of OECD countries to examine the employment impact of taxes. In these studies, the estimated long-run impact on labor market participation of a 10 percentage point rise in the tax wedge varies from -7.5 percentage points to 5.5 percentage points with a midpoint of -0.5. He also provides a similar discussion for estimates of the impact of taxes on wages and also here the evidence is quite mixed. In conclusion, Nickell (2003) ascribes the wide fluctuations in estimates of the incidence of taxation to variations in other variables included in the labor demand equation that are also correlated with tax differences.

II. C. Related microeconomic literature

An alternative approach is to move beyond time-series and cross-country variation to find payroll tax changes which had differential effects within a country over time. For example, Katz [1996] reviews the evidence on the effectiveness of wage subsidy programs in the US and provides some evidence on the employment effects of the Targeted Jobs Tax Credit that

operated in the US from 1979 to 1994. He finds that subsidies related to net changes in employment (sometimes conditional on training and retaining requirements) have increased employment and wages for the group of targeted workers. However, Katz [1996] also argues that despite the substantial experience of advanced nations with payroll tax subsidies, the lack of formal evaluation evidence leaves much uncertainty concerning the likely impacts of such policies.

In answer to this, Gruber [1997] uses firm level panel data to analyze the privatisation of social security in Chile during the early 1980s when the average payroll tax fell from 30 percent to 5 percent over a six year period. He finds that the incidence of payroll tax reductions was fully on wages, with no effect on employment. To explain this finding, Gruber conjectures that either labor supply is very inelastic or that -labor supply might have shifted inwards if the reduction in payroll taxation was paid for by a decrease in other benefits related to work such as retirement benefits or compensation for workplace injuries.

This study differs from Gruber [1997] in two important ways. First, Gruber does not observe whether or not a firm actually received a subsidy and, if so, the amount of subsidy received. This implies that the reduction in payroll taxes that is only due to a change in the tax system has to be imputed at the firm level in some way. This paper does not have this problem since we directly observe subsidies received by each participating firm. Second, the policy change examined in Gruber [1997] applied to all workers in all firms in

all sectors. In contrast, this paper estimates the incidence of employment subsidies exploiting a number of sources of variation in program intensity using various straightforward estimators.

III. Institutional Background and Data

III.1. Institutional Background

The history of "Maribel" employment subsidies that were implemented in Belgium is summarized in Table 1. Maribel I stated that employers were entitled to a reduction of 6.17 percentage points in employer contributions for each full-time manual worker employed in the private sector, except for companies in electricity, gas and water as well as financial intermediation. In 1983, Maribel I was transformed from a proportional into a lump-sum subsidy and higher subsidies were given to smaller firms. In 1993, Maribel I was replaced by Maribel II/III though this change only affected a number of industries. In particular, higher subsidies were granted to "target industries", i.e. exporting as well as transport industries. Mid 1997, Maribel II/III was substituted for Maribel IV. Maribel IV no longer explicitly targeted specific industries but made the subsidy to increase with the fraction of manual workers employed at the firm. Finally, Maribel subsidies came to an end in the second quarter of 1999. From 1999 until 2004, employer tax exemptions have been gradually converging towards a harmonised system of proportional and lump-sum reductions for manual as well as non-manual labor.

Due to data constraints explained below, it is the variation imposed by Maribel II/III and Maribel IV that will be used in this paper. Table 2 therefore shows the annual lump-sum subsidies per full-time manual worker for Maribel II/III. The table shows that for the first 5 manual workers in small companies (i.e. less than 20 employees), the subsidy was 37 200 BEF for target industries and 12 000 BEF for non-target industries (except firms in excluded industries)[3]. For all other manual workers in target and non-target industries, the subsidy amounted to 33 748 BEF and 7 500 BEF respectively.

The first column of Table 3 shows how Maribel IV was different from Maribel II/III. Rather than explicitly targeting certain industries, Maribel IV granted lump-sum reductions in payroll taxes that are increasing in the intensity of manual labor at the firm. It also clear from Table 3 that the subsidy was highest for firms employing less than 5 employees and smallest for firms employing more than 10 employees and that, for a given fraction of manual work, firms employing between 5 and 10 employees received some convex combination of both.

In sum, Tables 2 and 3 show that one can expect substantial variation in whether or not a firm received Maribel subsidies both within and between time periods. It is also clear that among the group of subsidised firms at each point in time, the average per-worker subsidy is decreasing in firm size. It is this variation that will be exploited in the analysis below.

[3] BEF stands for Belgian Francs, one Euro = 40.33 BEF.

III.B. Firm level data

The data used in this paper are derived from the balance sheets of Belgian companies contained in *BELFIRST*. Most importantly, since 1996 *BELFIRST* includes the social balance sheet of companies providing panel data information about employment, wages and the amount of employment subsidy received by each firm every year. Because this information is only available from 1996 onwards, we only retrieved data from 1996 up to 1999.

To check the randomness of our *BELFIRST* sample, Table 4 compares administrative data from all social security tax records in 1995 with *BELFIRST* data in 1996. The rows in Table 4 give the fraction of Maribel workers (i.e. full-time manual workers) and the fraction of Maribel subsidies by industry for both data sources. This comparison suggests that the *BELFIRST* data are representative except for the single industry of health and social work. Moreover, Table 4 shows that Maribel II/III workers are largely employed in manufacturing industries and that exporting industries in manufacturing receive higher subsidies on average. This is also true to some extend for companies in "transports and communication" given that the transport sector also is a targeted industry under Maribel II/III. Table 4 shows that Maribel workers are also concentrated in construction and wholesale and retail industries but that those industries received lower average subsidies compared to targeted companies. Finally the bottom row of Table 4 suggests that about 50 percent of all subsidized firms are included in our data set.

Table 5 provides some information about the number of subsidized and non-subsidized firms. For each year, Table 5 shows the total number of firms, mean full-time employment and mean full-time manual employment for receivers and non-receivers (i.e. firms with strictly positive full-time manual employment but not receiving a subsidy) in our sample respectively. For the group of subsidized firms, the number of observations increased from 20 635 in 1996 to 32 517 in 1998, which is explained by the extension of Maribel subsidies to firms in non-target industries. The drop in the number of observations between 1998 and 1999 can be explained by the fact that Maribel subsidies ended in June 1999. Importantly, Table 5 also shows that mean full-time employment in subsidized firms was much larger compared to the group of firms that employed manual workers but received no subsidy in the sample. This could be due to the fact that subsidized firms are concentrated in industries with higher average firm size or that it was too expensive for small firms to know about or administer the subsidy.[4] In any case, it will be important to account for these differences in the analysis below.

Finally, Table 6 only considers the group of subsidized firms in *BELFIRST* and the first column compares the average subsidy received per full-time manual worker for the different years (in brackets are standard errors reflecting the variation of average per-worker subsidies across firms). It shows that the average subsidy increased between 1996 and 1998 or 1999 if Maribel

[4] Katz [1996] also finds that, if participation is voluntary, take-up rates in the New Jobs Tax Credit (NJTC) and Targeted Jobs Tax Credit (TJTC) programs are decreasing in firm level employment. For example, Perloff and Wachter [1979] find that only 34 percent of firms in their survey knew of the existence of NJTC although almost all large firms did.

had continued to exist until the end of that year. The final column informs about the fraction of the total subsidy as a percentage of total labor costs, reflecting that on average Maribel subsidies increased from 1.61 percent of the wage bill in 1996 to 2.64 percent in 1998.

IV. Results

IV.A. Main results

One way of assessing the incidence of Maribel subsidies is to compare employment growth between firms that started or stopped receiving Maribel subsidies and firms that did not change their participation status in the Maribel program. Pooling all years and all firms, this suggests the following estimating equation:

$$\log(y_{it}) = \beta_0 + \beta_1 maribel_{it} + \beta_2 'YEAR_t + \alpha_i + \varepsilon_{it} \quad (2)$$

with y_{it} full-time manual employment for firm *i* at time *t*, $maribel_{it}$ a dummy equal to 1 if firm *i* received a subsidy at time *t*, $YEAR_t$ a vector of year dummies, α_i a firm fixed effect and ε_{it} a white-noise error term. An OLS point estimate for β_1 is given in the first column of Table 7 suggesting that firms start (stop) receiving Maribel subsidies increased (decreased) full-time manual employment by 5.7 percent due to the subsidy (standard errors are in brackets). Because the level of and entitlement to the Maribel subsidy changed over time,

column (2) of Table 7 provides an estimate for each year the subsidy was in place using the following estimating equation:

$$(3) \qquad \log(y_{it}) = \beta_0 + \sum_t \beta_{1t} maribel_{it} * year_t + \beta_2 'YEAR_t + \alpha_i + \varepsilon_{it}$$

Point estimates for β_{1t} are given in column (2) and reflect that the employment impact was larger in 1997 compared to 1998 which is due to the fact that Maribel IV became relatively more generous for less competitive non-exporting industries and the fact that minimum wages are more likely to bind in low-paid exporting industries that were targeted by the Maribel II/III regime (note for example that in Table 5 the annual average pre-tax wage paid by subsidized firms in 1997 is smaller than in 1998). Also the lower point estimate for 1999 can be expected since the average annual Maribel IV subsidy was lower due to an end to the program in that year.

Columns (3) to (6) of Table 7 repeat the analysis done in columns (1) and (2) after splitting up the experimental group into firms that start participating and firms that stop participating in the program. Point estimates for both experimental groups are statistically significant and the employment impact of Maribel subsidies seems to be independent of the direction of change in program status. This is important since it suggests that firms participating in the Maribel program are not characterized by higher employment trend growth that would bias upwards the point estimates in

columns (1) to (4) and bias downwards the point estimates in columns (5) and (6).[5]

Table 8 repeats the analysis done in Table 7 using the logarithm of the mean pre-tax wage paid at the firm as the dependent variable. Though, due to data limitations, the pre-tax wage is calculated as the mean wage for all full-time workers in the firm (not just full-time manual workers), three conclusions can be drawn from Table 8. First, the wage incidence of Maribel subsidies is smaller at around 2 percent but point estimates are statistically significant. Second, point estimates are larger for 1998 compared to 1997. This is consistent with the smaller employment effects for 1998 compared to 1997 found in Table 7 and suggests that labor supply for non-target industries is less elastic. Some evidence in support of this hypothesis is given by that point estimates in Table 8 for firms that stop receiving subsidies are smaller compared to firms that start receiving subsidies, especially in 1997.

Though one could conclude from the evidence presented in Tables 7 and 8 that Maribel subsidies have increased full-time manual employment and wages, an important question is to what extend these gains have come at the "displacement" of other types of labor. Table 9 therefore experiments with alternative employment measures that were not directly affected by the subsidy. Columns (1)-(2) and (3)-(4) use the number of full-time managers and other white-collar full-time employment as the dependent variables

[5] Also note that the analysis already partially controls for the presence of different trend growth between firms receiving and firms not receiving Maribel subsidies since the control group also contains firms that receive subsidies in any two consecutive periods.

respectively. Though point estimates are positive, they are not always statistically significant (also due to relatively small sample sizes) and in any case smaller to those reported in the first column of Table 7. The explanation for the absence of any displacement effect can be twofold. First, point estimates in Table 9 can capture that Maribel receivers would have increased overall employment even in the absence of the subsidy. If this would be the case, the point estimates of Table 9 would have to be subtracted from the point estimates in Table 7 to find the true employment impact of Maribel subsidies for full-time manual workers. A second possible explanation for the absence of any displacement effect is that different types of labor are complements rather than substitutes in production and that Maribel subsidies also increased the employment measures reported in Table 9. There are two reasons why one would favor this second hypothesis. First, since it was argued above that it is unlikely that point estimates in Table 7 are biased due to a differential trend in employment growth between participating and non-participating firms, the point estimates in Table 9 would have to capture idiosyncratic factor demand shocks (rather than secular differences in employment trend growth) at the firm level that are correlated with program participation. Second, in line with this reasoning are the somewhat higher point estimates in Table 9 for temporary employment (though they are only marginally statistically significant) since adjustment costs for temporary employment are significantly less compared to permanent workers in Belgium.

In sum, simple difference-in-differences estimates suggest that employment subsidies have increased full-time manual employment with 5 to 8 percent and pre-tax wages with 1 to 3 percent depending on the amount of per-worker subsidy received and the distribution of employment subsidies across sectors that differ in their product market competitiveness and the presence of a binding minimum wage. Moreover, no displacement effects could be found suggesting that other types of labor inputs are complements rather than substitutes in production.

IV.B. Robustness checks

The higher average employment sizes for subsidy receivers compared to non-subsidized firms reflected in Table 5 suggests Maribel receivers and non-receivers could also differ in other dimensions omitted from equations (2) and (3) that are also correlated with program participation. One way to look at this is to compare employment changes between receivers and non-receivers given equal initial full-time manual employment for each year of the program. To this end, columns (1), (4) and (7) of Table 10 use the following estimating equation for each *t*=1997, 1998 or 1999:

$$\log(y_{it}) = \beta_0 + \beta_1 maribel_{it} + \beta_2 \log(y_{it-1}) + \varepsilon_{it} \quad (4)$$

with y_{it} full-time manual employment for firm *i* at time *t*, $maribel_{it}$ a dummy equal to 1 if firm *i* received a subsidy at time *t* and y_{it-1} full-time manual employment for firm *i* at time *t-1* and ε_{it} a white-noise error term. OLS point

estimates for β_1 are 7.1 percent for 1997, 6.5 percent for 1998 and 5.3 percent for 1999. These point estimates are in line with those reported in column (2) of Table 7 and, if anything, are somewhat larger due to lower percentage employment growth for larger firms in general. To test the robustness of this matching estimate, columns (2), (5) and (8) add a dummy for whether or not a firm was also participating in other employment programs while receiving Maribel subsidies. Interestingly, the coefficient on participation in other employment programs is positive, statistically significant and particularly large in 1998. Though the coefficient on the incidence of Maribel subsidies seems positively correlated with participation in other employment programs, point estimates remain positive and statistically significant ranging from 5.5 percent in 1997, 5.2 percent in 1998 to 2.6 percent in 1998. Finally, columns (3), (6) and (9) also add 2-digit industry dummies to equation (4) in order to compare the employment changes between firms in the same industry without any significant impact on the estimated difference-in-differences for Maribel participation.

Table 11 repeats the analysis in Table 10 using the logarithm of the mean pre-tax wage in each firm as the dependent variable in (4). Point estimates are larger compared to those reported in the second column of Table 8 suggesting that high-wage firms have lower percentage wage growth in

general.[6] For the full specification, point estimates are between 1.7 percent in 1999, 4.7 percent in 1997 to 5.2 percent in 1998. All in all, point estimates presented in Tables 10 and 11 are in line with the hypothesis that Maribel subsidies have increase full-time manual employment and wages and the estimates suggest that, if anything, the fixed-effects estimates presented in Tables 7 and 8 are biased downwards.

So far the analysis has only used information about whether or not a firm received Maribel subsidies. However, we also observe the total subsidy each participating firm received each year throughout the duration of the Maribel program. Given the variation in per-worker subsidies documented in Tables 2 and 3, consider the following estimating equation:

$$\log(y_{i1998}) = \beta_0 + \beta_1 \log(subsidy_{i1998}) + \beta_2 \log(y_{i1997}) + \varepsilon_{i1998} \quad (5)$$

with y_{i1998} (y_{i1997}) full-time manual employment or the mean pre-tax wage for firm i in 1998 (1997) and $subsidy_{i1998}$ the average per-worker subsidy firm i received in 1998. The problem with estimating (5) using OLS, however, is that the error term could not be independently and identically distributed. For example, measurement error in total subsidies will bias OLS estimates of β_1 downwards.

To this end, Table 12 applies a 2SLS estimator using as first-stage predictions for Maribel subsidies the variation in program intensity

[6] This is also true in our data. A regression of mean pre-tax wage growth onto the logarithm of beginning-of-period mean pre-tax wages gives a point estimate of -0.271 with a standard error of 0.002.

documented in Table 3 together with full-time manual and total employment as well as the fraction of full-time manual workers employed at each participating firm in 1997. Columns (1) and (3) of Table 12 report Maribel IV elasticities of full-time manual employment and mean pre-tax wages respectively. Both elasticities are positive and statistically significant suggesting that a doubling of per-worker subsidies increases full-time manual employment with 3.7 percent and pre-tax wages with 1.3 percent on average. Finally, in columns (2) and (4) of Table 12, the first-stage controls are added linearly to equation (5) and Maribel coefficients are identified through the discontinuities in program generosity documented in Table 3. Adding the first-stage controls to equation (5) does not change elasticity estimates which are given by 3.3 percent for full-time manual employment and 1.5 percent for pre-tax wages.

To see that the Maribel elasticities estimated in Table 12 are roughly in line with the point estimates presented in Tables 7 and 8, first assume the government would double its total Maribel payments by starting to subsidize an equally large number of non-receivers in 1998. Difference-in-differences estimates in Tables 7 and 8 would then suggest an increase in full-time manual employment with 5.4 percent and an increase in pre-tax wages with 2.8 percent of newly subsidized firms. Alternatively, suppose the government would double the per-worker subsidy paid to all firms already receiving Maribel IV subsidies thereby also doubling total Maribel expenditures. Point estimates in Table 12 would then suggest an increase in full-time manual employment with

3.7 percent and an increase in pre-tax wages with 1.3 percent of all participating firms.

IV.C. The incidence of Maribel subsidies by industry

A final robustness test is given by point estimates in Table 13. Given that even within industries some firms received subsidies and others did not, Table 13 provides difference-in-differences estimates for each of eight industries using the following estimating equation:

(6)

$$\log(y_{ijt}) = \beta_0 + \sum_j \beta_{1j} maribel_{ijt} * industry_j + \beta_2 'YEAR_t + \alpha_i + \varepsilon_{it}$$

with y_{it} full-time manual or the average pre-tax wage in firm *i*, industry *j* at time *t*. The first column of Table 13 shows that the increase in full-time manual employment was highest for manufacturing firms (8.6 percent) and firms in transport and communication (9.2 percent). The higher point estimates for manufacturing firms as well as firms in transport and communication can be explained by the higher average per-person subsidies in those industries targeted by the Maribel II/III regime. But the higher point estimates could also be explained by the more elastic factor demands due to more competitive output markets for exporting manufacturing and transport and communication industries. One way of testing for this hypothesis more formally is to regress the logarithm of full-time manual employment on a dummy for Maribel subsidies as well as its interaction with a measure of import penetration for a

number of sub-industries in manufacturing. Doing this gives a point estimate of 3.7 percent for the coefficient on the Maribel dummy and 7.1 percent for the coefficient on its interaction with a measure of import penetration. The difference-in-differences estimates for average pre-tax wages are reported in column (2) of Table 13. All point estimates are positive but not all are statistically significant and none are statistically significantly different from the other (excluding mining and quarrying). Given the relatively high point estimates for full-time manual employment in manufacturing and transport and communications, this suggests that labor supply is relatively more elastic in those industries, a fact that could partially be explained by more low-wage jobs in these sectors and therefore a more binding minimum wage. But we leave it to future exploration to test this hypothesis more formally.

V. Conclusions

This paper made use of a natural experiment to analyze the effects of payroll tax exemptions targeted at manual workers in the late 1990s in Belgium, generally known as the "Maribel subsidies". Given a unique panel of firm level data with information about whether or not a firm received subsidy and, if so, the amount of subsidy received in any given year, this paper has shown that employment subsidies have increased full-time manual employment with 5 to 8 percent and pre-tax wages with 1 to 3 percent without much evidence for "displacement" effects for other workers. Moreover, we have argued that

employment subsidies have increased employment but not wages by more in low-wage exporting industries. This is line with a textbook description of labor markets where it is predicted that the incidence of employment subsidies on employment and wages is larger the more elastic is product and therefore labor demand and where the employment effect is larger and the wage effect is smaller the more elastic is labor supply because of a binding minimum wage. Hopefully these insights will provide some guidance towards policies aimed at improving the employment and income prospects of those workers in developed nations most hurt by the current changes in relative factor demand.

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Table 1: History of Maribel

	Period	Type of subsidy	Total subsidy is based on
Maribel I	01/1983-09/1993	lump-sum subsidy per manual worker	number of manual workers, firm size
Maribel II/III	09/1993-06/1997	lump-sum subsidy per manual worker	number of manual workers, firm size, industry
Maribel IV	06/1997-04/1999	lump-sum subsidy per manual worker	number of manual workers, firm size, fraction of manual workers
Grouped employer tax exemptions	04/1999-01/2004	proportional subsidy for all employees	wage, number of manual and non-manual workers

Table 2: Maribel II/III

	Manual workers		Non-manual workers
	First 5 in small firms (<20 employees)	Other manual workers	
Target industries	37 200	33 748	0
Other industries (not excluded)	12 000	7 500	0
Excluded industries	0	0	0

Notes: Amounts are in thousand Belgian Francs (BEF), one Euro=10.33 BEF.

Table 3: Maribel IV

	Manual workers		Non-manual workers
	First 5 in small firms (<10 employees)	Other manual workers	
X<0.66	34 000+20 000*X	20 000+20 000*X	0
X>=0.66	34 000+20 000*0.66	20 000+20 000*0.66	0
Excluded industries	0	0	0

Notes: Amounts are in thousand Belgian Francs (BEF), one Euro=10.33 BEF. X measures the fraction of non-manual workers.

Table 4: Comparing Belfirst with administrative data

	Number of Maribel workers		Maribel subsidy	
	Belfirst (1996)	RSZ (1995)	Belfirst (1996)	RSZ (1995)
Mining and quarrying	0.62	0.34	0.67	0.49
Manufacturing	51.94	50.62	73.59	74.44
Electricity, gas and water	0.01	0.00	0.01	0.00
Construction	17.49	15.91	7.22	6.50
Wholesale and retail trade	12.95	10.96	7.40	4.88
Hotels and restaurants	2.92	3.85	1.04	1.62
Transport and comm..	7.17	6.85	7.19	7.96
Financial intermediation	0.03	0.01	0.02	0.01
Business services	5.48	6.48	2.28	2.24
Public administration	0.00	0.00	0.00	0.00
Education	0.00	0.00	0.00	0.00
Health and social work	0.05	3.14	0.02	1.07
Public services	1.27	1.80	0.49	0.74
Private households	0.00	0.01	0.00	0.00
Total	*430 101*	*788 908*	*8 688*	*18 053*

Notes: Maribel subsidies are in million BEF.

Table 5: Subsidized and non-subsidized firms in Belfirst

	Subsidized firms			Non-subsidized firms		
	Number of firms (1)	FT manual employment (2)	Pre-tax wage (x1000 BEF) (3)	Number of firms (4)	FT manual employment (5)	Pre-tax wage (x1000 BEF) (6)
1996	20 635	18.7	799.85	13 003	4.78	792.83
1997	27 644	19.04	804.90	9 869	4.90	754.29
1998	32 517	15.46	822.37	9695	5.28	760.91
1999	22 827	20.66	852.65	23 852	4.12	809.99

Notes: Numbers reported in columns (2), (3), (5) and (6) are means.

Table 6: Firm level Maribel subsidies in Belfirst

	Subsidy per manual worker	Total subsidy as % of total labor costs
1996	19 724 (12 682)	1.61 (1.83)
1997	24 183 (10 390)	2.15 (2.26)
1998	29 784 (9 561)	2.64 (2.88)
1999	15 840 (12 658)	1.33 (2.31)

Notes: Reported numbers are means and standard are in brackets.

Table 7: Difference-in-difference estimates of the impact of Maribel subsidies on log(full-time manual employment) using all years

	All participants		Start participating		Stop participating	
	(1)	(2)	(3)	(4)	(5)	(6)
Received Maribel	0.057	-	0.083	-	0.073	-
	(0.004)		(0.010)		(0.005)	
Received Maribel in 1997	-	0.079	-	0.090	-	0.110
		(0.008)		(0.011)		(0.009)
Received Maribel in 1998	-	0.054	-	0.082	-	0.075
		(0.006)		(0.012)		(0.007)
Received Maribel in 1999	-	0.049	-	0.065	-	0.064
		(0.005)		(0.013)		(0.005)
corr(FE, regressors)	*0.348*	*0.333*	*0.330*	*0.320*	*0.405*	*0.390*
Number of observations	*104 250*	*104 250*	*90 262*	*90 262*	*93 764*	*93 764*
Number of firms	*44 967*	*44 967*	*43 089*	*43 089*	*44 030*	*44 030*

Notes: Data are taken from Belfirst. All estimates are fixed effects estimates controlling for firm fixed effects and year dummies. Reported standard errors are robust standard errors.

Table 8: Difference-in-difference estimates of the impact of Maribel subsidies on log(pre-tax wage) using all years

	All participants		Start participating		Stop participating	
	(1)	(2)	(3)	(4)	(5)	(6)
Received Maribel	0.017	-	0.029	-	0.013	-
	(0.002)		(0.006)		(0.003)	
Received Maribel in 1997	-	0.010	-	0.021	-	0.002
		(0.005)		(0.007)		(0.006)
Received Maribel in 1998	-	0.028	-	0.043	-	0.022
		(0.004)		(0.008)		(0.005)
Received Maribel in 1999	-	0.016	-	0.041	-	0.013
		(0.002)		(0.008)		(0.003)
corr(FE, regressors)	*0.044*	*0.045*	*0.072*	*0.085*	*0.033*	*0.032*
Number of observations	*103 343*	*103 343*	*89 480*	*89 480*	*92 909*	*92 909*
Number of firms	*44 598*	*44 598*	*42 725*	*42 725*	*43 631*	*43 631*

Notes: Data are taken from Belfirst. All estimates are fixed effects estimates controlling for firm fixed effects and year dummies. Reported standard errors are robust standard errors.

Table 9: Difference-in-difference estimates of the impact of Maribel subsidies on other employment measures using all years

	Log(FT managers)		Log(FT non-manual empl excl managers)		Log(PT employment)		Log(temporary employment)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Received Maribel	0.033	-	0.036	-	0.033	-	0.075	-
	(0.027)		(0.006)		(0.008)		(0.053)	
Received Maribel in 1997	-	0.070	-	0.023	-	0.022	-	0.094
		(0.031)		(0.012)		(0.014)		(0.093)
Received Maribel in 1998	-	0.014	-	0.015	-	0.008	-	0.072
		(0.027)		(0.009)		(0.011)		(0.075)
Received Maribel in 1999	-	0.026	-	0.047	-	0.045	-	0.071
		(0.031)		(0.007)		(0.009)		(0.062)
corr(FE, regressors)	*0.045*	*0.048*	*0.042*	*0.050*	*-0.025*	*-0.011*	*0.019*	*0.020*
Number of observations	*6 767*	*6 767*	*52 931*	*52 931*	*51 477*	*51 477*	*7 478*	*7 478*
Number of firms	*3 142*	*3 142*	*23 188*	*23 188*	*24 395*	*24 395*	*3620*	*3620*

Notes: Data are taken from Belfirst. All estimates are fixed effects estimates controlling for firm fixed effects and year dummies. Reported standard errors are robust standard errors.

Table 10: OLS estimates of the impact of Maribel subsidies on log(full-time manual employment) by year

	1997			1998			1999		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Received Maribel	0.071 (0.007)	0.055 (0.008)	0.052 (0.009)	0.065 (0.007)	0.026 (0.008)	0.020 (0.009)	0.053 (0.004)	0.052 (0.005)	0.049 (0.005)
Log(initial FT manual emp)	0.964 (0.002)	0.962 (0.002)	0.957 (0.002)	0.915 (0.002)	0.913 (0.002)	0.899 (0.003)	0.944 (0.002)	0.944 (0.002)	0.939 (0.002)
Participation in other programs	-	0.033 (0.009)	0.041 (0.010)	-	0.075 (0.009)	0.090 (0.009)	-	0.004 (0.005)	0.012 (0.005)
Dummies for 2-digit industry	no	no	yes	no	no	yes	no	no	yes
R-squared	*0.884*	*0.884*	*0.885*	*0.876*	*0.876*	*0.878*	*0.917*	*0.917*	*0.917*
Numb of obs	*27 839*	*27 839*	*27 839*	*33 397*	*33 397*	*33 397*	*38 327*	*38 327*	*38 327*

Notes: Data are taken from Belfirst. Reported standard errors are robust standard errors.

Table 11: OLS estimates of the impact of Maribel subsidies on log(pre-tax wage) by year

	1997			1998			1999		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Received Maribel	0.052 (0.005)	0.045 (0.005)	0.047 (0.006)	0.058 (0.005)	0.045 (0.005)	0.052 (0.006)	0.028 (0.002)	0.021 (0.003)	0.017 (0.003)
Log(initial pre-tax wage)	0.703 (0.003)	0.703 (0.007)	0.657 (0.008)	0.729 (0.003)	0.729 (0.007)	0.688 (0.008)	0.729 (0.003)	0.728 (0.006)	0.696 (0.007)
Participation in other programs	-	0.014 (0.007)	0.013 (0.007)	-	0.022 (0.006)	0.018 (0.006)	-	0.017 (0.004)	0.023 (0.004)
Dummies for 2-digit industry	no	no	yes	no	no	yes	no	no	yes
R-squared	*0.550*	*0.550*	*0.567*	*0.579*	*0.579*	*0.592*	*0.579*	*0.597*	*0.588*
Numb of obs	*27 164*	*27 164*	*27 164*	*32 586*	*32 568*	*32 586*	*37 320*	*37 320*	*38 320*

Notes: Data are taken from Belfirst. Reported standard errors are robust standard errors.

Table 12: 2SLS estimates of Maribel IV elasticities

	Log(FT manual employment)		Log(pre-tax wage)	
	(1)	(2)	(3)	(4)
Log(Maribel subsidy per worker)	0.037 (0.004)	0.033 (0.004)	0.013 (0.001)	0.015 (0.002)
Lagged dependent variable	0.895 (0.003)	0.883 (0.003)	0.705 (0.009)	0.638 (0.010)
First-stage controls	no	yes	no	yes
Number of observations	*27 516*	*27 516*	*27 225*	*27 225*
First-stage R-squared	*0.645*	*0.645*	*0.645*	*0.645*

Notes: Data are taken from Belfirst. First-stage predictions are derived from Table 3. First-stage controls added linearly to the second stage in columns (2) and (4) are initial FT manual employment, initial FT employment, the fraction of manual workers and dummies for 2-digit industries. Reported standard errors are robust standard errors.

Table 13: Difference-in-differences estimates of the impact of Maribel subsidies by industry using all years

	Log(FT manual employment) (1)	Log(pre-tax wage) (2)
Received Maribel	0.057	0.018
	(0.004)	(0.002)
Received Maribel x dummy for		
Mining and quarrying	0.002	0.007
	(0.070)	(0.051)
Manufacturing	0.086	0.017
	(0.010)	(0.005)
Construction	0.047	0.020
	(0.008)	(0.005)
Wholesale and retail	0.049	0.013
	(0.006)	(0.004)
Hotels and restaurants	0.032	0.019
	(0.021)	(0.013)
Transport and communication	0.092	0.021
	(0.020)	(0.011)
Business sercives	0.057	0.020
	(0.020)	(0.012)
Public services	0.029	0.024
	(0.028)	(0.015)
Number of observations	*103 151*	*102 245*
Number of firms	*44 479*	*44 110*

Notes: Data are taken from Belfirst. All estimates are fixed effects estimates controlling for firm fixed effects and year dummies. The industries used in the analyses are taken from Table 4 given a sufficiently large fraction of Maribel employment. Reported standard erros are robust standard errors.

Figure 1: A canonical framework to analyze the impact of Maribel subsidies on employment and wages for different assumptions about the labor market

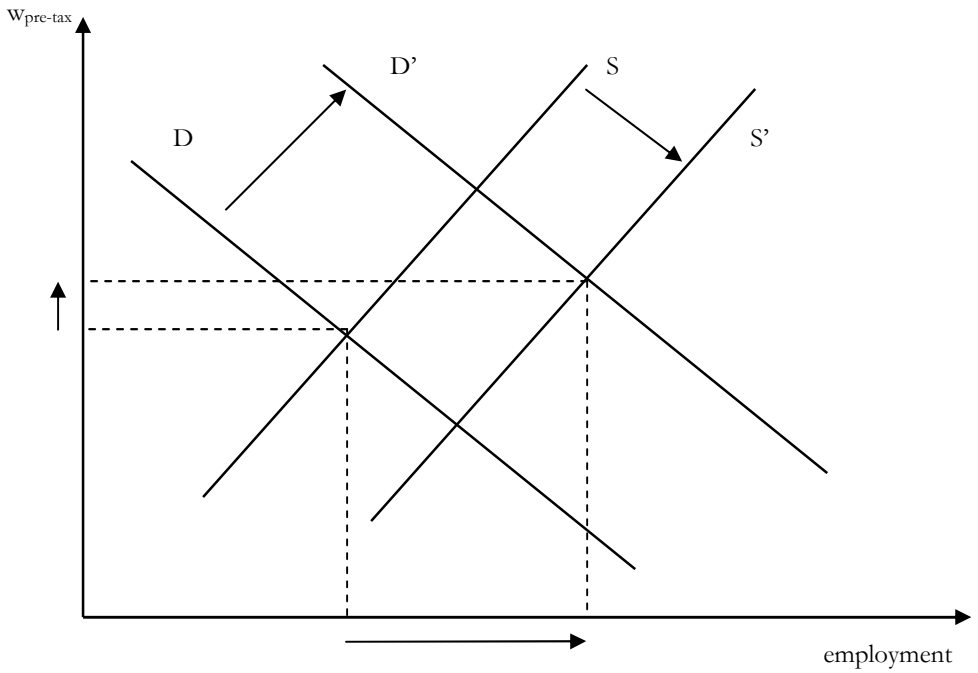
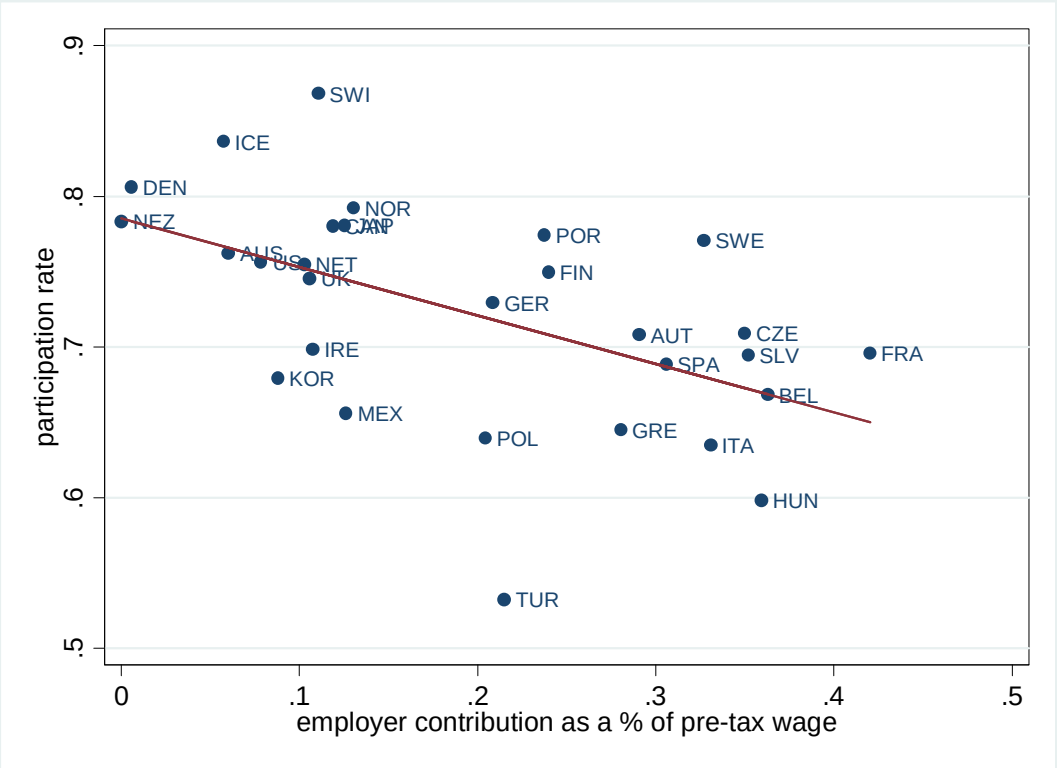


Figure 2: Labor market participation and payroll taxes in OECD countries



Notes: Data are taken from OECD Statistics 2004.