American Economic Review 2019, 109(8): 2693–2727
https://doi.org/10.1257/aer.20171445

Who Pays for the Minimum Wage?[†]

By Peter Harasztosi and Attila Lindner*

This paper provides a comprehensive assessment of the margins along which firms responded to a large and persistent minimum wage increase in Hungary. We show that employment elasticities are negative but small even four years after the reform; that around 75 percent of the minimum wage increase was paid by consumers and 25 percent by firm owners; that firms responded to the minimum wage by substituting labor with capital; and that disemployment effects were greater in industries where passing the wage costs to consumers is more difficult. We estimate a model with monopolistic competition to explain these findings. (*JEL* J23, J24, J31, J38, L13)

Despite several decades of microeconometric evidence, the minimum wage remains a highly controversial policy. On the one hand, opponents argue that the minimum wage makes low-skilled workers worse off as many of them lose their jobs (e.g., Stigler 1946, Neumark and Wascher 2010). On the other hand, proponents insist that the minimum wage has no discernible effect on employment and sometimes has a positive effect on it (e.g., Card and Krueger 1995; Dube, Lester, and Reich 2010). In addition to debating the sign and the size of the employment effects, there is also disagreement on whether the minimum wage is passed on to consumers through higher prices, or whether it is paid by firm owners through lower profits (see, e.g., Aaronson and French 2007 on prices and Draca, Machin, and Van Reenen 2011 on profits).

* Harasztosi: European Commission, Joint Research Centre (JRC), Via Fermi 2749, 21027 Ispra, Italy (email: peter.harasztosi@ec.europa.eu); Lindner: University College London, 30 Gordon Street, London, WC1H 0AX, United Kingdom, and CEP, IFS, IZA, MTA KTI (email: a.lindner@ucl.ac.uk). Thomas Lemieux was the coeditor for this article. We thank David Card, Patrick Kline, and Emmanuel Saez for their continuous guidance throughout the project. We are grateful to three anonymous referees and to Alan Auerbach, Tamás Bátyi, Katalin Bodnár, Michael Best, Stefano DellaVigna, Eric French, Hedvig Horváth, Hilary Hoynes, Larry Katz, János Köllő, Gábor Kézdi, Gábor Kőrösi, Alan Manning, John Mondragon, Carl Nadler, Steve Machin, Balázs Muraközy, Suphanit Piyapromdee, Michael Reich, Balázs Reizer, Ana Rocca, Jesse Rothstein, David Silver, Isaac Sorkin, Peter Spittal, Ferenc Szűcs, Álmos Telegdy, Chris Walters, Danny Yagan, Owen Zidar, and participants in seminars at Bocconi University, Collegio Alberto, Chicago Booth, Duke University, Harris School of Public Policy, Syracuse University, University of Oslo, UC Berkeley, Tinbergen Institute, University of Utah, and in conferences at AEA, Royal Academy of Science, IAB in Nuremberg, IZA in Bonn, for very helpful comments. We also thank the Hungarian Central Statistical Office and IE CERS HAS for their support. Financial support from the Center for Equitable Growth at UC Berkeley, from the European Research Council (ERC-2015-CoG-682349), from the NKFI-125101 grant "Employment of the Low-Skilled in Hungary" and from "Firms, Strategy and Performance Lendület" grant of the Hungarian Academy of Sciences is gratefully acknowledged. All errors are our own. The views expressed are that of the authors only and do not imply a policy position of the European Commission. The authors declare that they have no relevant or material financial interests that relate to the research described in this paper.

[†] Go to https://doi.org/10.1257/aer.20171445 to visit the article page for additional materials and author disclosure statements.

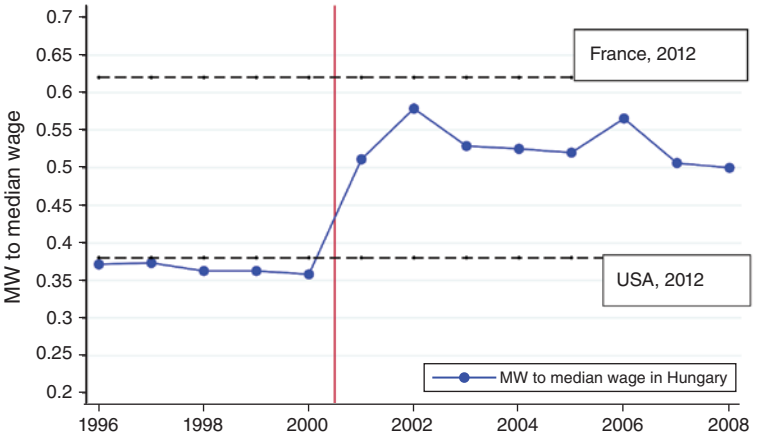

FIGURE 1. MINIMUM WAGE IN HUNGARY

Notes: This figure shows the ratio of the minimum wage to median wage in the private sector for Hungary between 1996 and 2008 (own calculations). The two dashed lines depict the ratio of the minimum wage to the median wage for France and the United States in 2012 (OECD).

In this paper, we present new evidence on the employment effect and the incidence of the minimum wage by exploiting a very large and persistent increase in the minimum wage in Hungary. Figure 1 shows the remarkable recent history of the minimum wage in Hungary. Prior to 2000, the ratio of the minimum wage to the median wage in the country was around 35 percent, comparable to the current ratio in the United States, while two years later the minimum wage rose to 55 percent, a level only slightly below the current minimum wage in France. The apparent size and permanence of this unique policy change allow us to examine responses to a minimum wage increase in a context where firms have strong incentives to restructure their production process or exit the market as soon as possible because the loss in profit from inaction is significant.

We estimate the employment and wage effects of this unique minimum wage increase by exploiting administrative data on firms filing balance sheet statements to the tax authority. We estimate the firm-level relationship between the fraction of workers who earned below the new minimum wage before the reform and the percentage change in employment relative to year 2000, the last year before the minimum wage hike. We find that firms employing only minimum wage workers had 10 percent lower employment four years after the minimum wage hike than firms with no minimum wage workers. This implies that 1 out of 10 low wage workers lost their job as a result of the reform. The divergence in employment between firms with different levels of exposure emerged only after the minimum wage hike, which underlines that the employment changes are driven by the reform and not something else.

At the same time, the average wage at the highly exposed firms increased by 54 percent more than the average wage at firms with no exposure to the minimum wage. Therefore, the employment responses relative to the wage effects are small even for sizable changes in the minimum wage and so the average earnings of low-wage

workers increased considerably. The implied employment elasticity with respect to the workers' *own wage* is −0.18 (SE 0.03), which is at the lower end of the estimates in the literature.

The administrative data combined with the very large minimum wage shock allow us to examine various other margins of adjustment and to assess the incidence of the minimum wage directly. We find a strong positive relationship between changes in total revenue (relative to year 2000) and minimum wage exposure after the reform, while no such relationship was present beforehand. A similar analysis reveals a slight drop in profits after the reform. We calculate that around 25 percent of the increased cost of labor is covered by lower profits, and so paid by the firm owners, and around 75 percent is paid by consumers in the form of higher revenue. Therefore, understanding revenue responses is crucial for the incidence analysis.

The change in total revenue is the product of changes in quantity and prices. To provide direct evidence on the role of price changes, we exploit unique firm-product level data for the manufacturing sector and document that firm-level price indices increased considerably in response to the minimum wage. We also find substantial heterogeneity in responses to the minimum wage across sectors consistent with the role of price pass-through. We show that the revenue effects are smaller (and the disemployment effects are larger) in the tradable, in the manufacturing, and in the exporting sectors. In these sectors, Hungarian firms are more likely to face foreign competitors which are not hit by the minimum wage shock. Thus, the price increase of Hungarian firms leads to a competitive disadvantage and a large fall in output. On the contrary, the minimum wage increased revenue more (and the disemployment effects were smaller) in the non-tradable sector. In this sector, all firms were hit by the minimum wage shock meaning that individual firms could raise their prices without a loss in competitive advantage or a large fall in output.

Understanding responses at various margins also allows us to shed new light on how firms absorbed the minimum wage shock. The main explanations in the literature for the near-zero effect on employment emphasize the importance of labor market frictions (e.g., Dube, Lester, and Reich 2016; Flinn 2010; Bhaskar, Manning, and To 2002; Van Den Berg 2003; Rebitzer and Taylor 1995). However, the increase in prices and the seemingly important role for product market competition highlight the importance of the channels predicted by the standard labor demand model (Aaronson and French 2007). We also document a large increase in capital stock, which suggests that capital-labor substitution, another important margin of adjustment in the standard labor demand model, plays a crucial role.[1]

We propose a simple partial equilibrium model that incorporates these channels of adjustment. To capture the importance of the structure of output market we deviate from the standard labor demand model, which assumes perfect competition, and we introduce market imperfection in the output market. In particular, we assume that monopolistically competitive firms are selling differentiated goods. The model's key predictions are in line with the Hicks-Marshall rule of derived demand

[1] The relatively fast and sizable adjustment also underscores that the minimum wage hike was so radical that firms adjusted their production processes quickly. Since the change in capital stock mainly occurred at firms which existed before the minimum wage hike, such a large response in capital stock is inconsistent with the Putty-Clay model (see Aaronson et al. 2018).

(Hamermesh 1993), which suggests that firm-level responses to the minimum wage depend on (i) the cost share of different factors in production (labor, capital, intermediate goods); (ii) the substitution elasticity between labor and other factors (capital and intermediate goods); and (iii) the relevant output demand elasticity that firms face after a minimum wage hike. However, this latter channel depends on the market structure in our model. In markets where only one firm is hit by the minimum wage shock (e.g., exporting markets), the relevant output demand is the firm-level one which tends to be highly elastic. Alternatively, in markets where all firms are hit by the minimum wage shock, the relevant output demand is the market-level one which tends to be less elastic.

We evaluate the model quantitatively by matching the model predictions to our empirical estimates on the effects of the minimum wage increase on labor, capital, revenue, and materials (intermediate goods and services). The model performs well in explaining the key moments in the data, especially when we allow the underlying parameters to vary by sector. The best fitting model matches the employment and capital responses very closely. Moreover, the predicted price effects in the manufacturing sector are also very close to their empirical counterparts even though we do not use these moments in the estimation. However, the model fit is not perfect. In some cases we over-predict revenue responses and under-predict responses on materials, which underlines some limitations of this arguably simplistic framework.

Nevertheless, finding the best fitting parameters allows us to translate our reduced-form estimates at various margins to easily interpretable structural parameters. We uncover three structural parameters. The substitution elasticity between capital and low wage workers is quite high: it is 3.35 (SE 0.62) four years after the reform. However, this large substitution elasticity has only a limited effect on employment, because the share of capital expenses in firm-level production is only around 8 percent. The second structural parameter is the substitution elasticity between materials and labor, which we estimate to be close to zero (0.03, SE 0.06) even in the medium run. Given that the spending on materials accounts for 74 percent of an average firm's costs, a low level of this elasticity is required for the employment responses to be low.

The output demand elasticity is close to zero (0.11, SE 0.22) in the medium term (four years after the reform). Moreover, there is considerable variation in the estimates across sectors. The output demand is more elastic in the manufacturing and in the tradable sector, where the relevant output demand elasticity is the firm-level one. For instance, in the exporting sector, we find that the output demand elasticity is 3.64 (SE 0.98).[2] By contrast, in the non-tradable sector we find near-zero output elasticities even in the medium run. This suggests that firms in these sectors can pass the minimum wage to consumers because output demand is inelastic.

This paper contributes to several strands of the minimum wage literature. First, we contribute to the extensive literature on the employment effects of the minimum wage (e.g., see the surveys by Neumark and Wascher 2010 and Card and Krueger 1995). Many papers in this literature find that the effect of the minimum wage is

[2]The output demand elasticity in the exporting sectors identifies the Armington elasticity, i.e., the elasticity of substitution between products of different countries. Our estimates are in line with the findings in the trade literature which have found that the Armington elasticity is close to 1.4 in the short run and to 6.2 in the long run (Ruhl 2008).

close to zero (Doucouliagos and Stanley 2009, CBO 2014). However, these papers are criticized on the basis that they rely on small and temporary shocks for identification (Sorkin 2015, Aaronson et al. 2018) and study only short-term responses (Baker, Benjamin, and Stanger 1999), and so the lack of immediate responses might simply reflect adjustment costs or inertia (Chetty et al. 2011). In this paper, we address these critiques by examining an unusually large and persistent increase in the minimum wage, where costs of inaction or delaying responses are substantial, and show that the effect of the minimum wage is small even for such a large minimum wage change.[3]

This paper also contributes to the literature investigating margins of adjustment to the minimum wage. Previous literature has examined the effect on firm profitability and revenue (Mayneris, Poncet, and Zhang 2018; Hau, Huang, and Wang 2016; Riley and Bondibene 2018; Draca, Machin, and Van Reenen 2011; Allegretto and Reich 2018), on the stock-market value (Card and Krueger 1995, Bell and Machin 2018), on capital stock (Hau, Huang, and Wang 2016), and on output prices in the local service sector (see Lemos 2008 and MaCurdy 2015 for a review). Here we provide a comprehensive study of the different margins adjustment, both across the whole economy and separately by sector. Thanks to the large permanent increase in the minimum wage we have the statistical power to look at the margins of adjustment in sectors where the labor share is low and so a smaller shock to the minimum wage would have only modest impacts on firms (e.g., in the retail sector, see Ganapati and Weaver 2017) or where a smaller minimum wage shock would not be binding (e.g., in the exporting and in the manufacturing sector).

Finally, our paper also contributes to the scant literature on the incidence of the minimum wage. MaCurdy (2015) examines the incidence of the minimum wage in a general equilibrium framework under the assumption that there is no employment effect and the increase in wage cost is fully passed through to the consumers as higher prices. Our estimates suggest that this assumption is inaccurate as only 75 percent of the minimum wage is passed through to consumers, while 25 percent is paid by firm owners. Similarly to MaCurdy (2015), we also examine the consumption of households to better understand who buys the goods produced by minimum wage workers but, contrary to him, we find that rich and poor households spend a very similar fraction of their income on goods produced by minimum wage workers.

The paper proceeds as follows. In Section I, we describe the institutional context of the minimum wage increase and our data. In Section II we present evidence on the employment and wage effects of the minimum wage. In Section III, we estimate various margins of adjustment by firms. In Section IV we present a labor demand model with monopolistic competition on the output market and estimate the key parameters of the model. In Section V we present robustness checks and address the potential threats to identification, and we conclude in Section VI.

[3] Some other studies have exploited very large minimum wage shocks. Reynolds and Gregory (1965) and Castillo-Freeman and Freeman (1990) study the impacts of imposing the US federal minimum wage on Puerto Rico, which was relatively large but occurred over several years. Moreover, Kertesi and Köllő (2004) studied the employment effects of the 2001 raise in the minimum wage in Hungary. Although they use different methods and datasets, many of their estimates are close to ours. Recently, Jardim et al. (2017) examine the short-term impact of the $13 minimum wage in Seattle.

I. Institutional Context and Data

A. *Institutional Context*

The minimum wage in Hungary is negotiated annually by a national-level tripartite council, a consultative body that consists of unions, employers' associations, and the government.[4] If the tripartite council fails to agree, the government is authorized to decide unilaterally.

Before 2000, the minimum wage was typically increased by slightly more than the inflation rate each year. However, on April 6, 2000 the right-wing government announced (and later decided unilaterally) that it would raise the minimum wage from 25,500 HUF to 40,000 HUF in January 2001 and also pledged to increase the minimum wage further to 50,000 HUF in 2002.[5] This announcement was rather unexpected, since the radical increase of the minimum wage had not previously been part of the political discourse.[6] For instance, the unions were demanding a 13 percent increase in minimum wage at the pre-negotiations, so a government proposal to double the nominal minimum wage in two years was above all expectations (Tóth 2001). In fact both unions and employers strongly opposed such a radical change to the minimum wage as they were afraid of the negative consequences for jobs.

Government officials stated that the main objectives of the minimum wage increase were to alleviate income differences, to increase government revenue, and to diminish tax evasion (Cserpes and Papp 2008). Political commentators, on the other hand, argued that the real purpose of such a salient and radical change in minimum wage was to "set the political agenda" and to boost party support.

The main opposition parties did not oppose raising the minimum wage, and so the increase was not reversed after the right-wing government lost the 2002 election. This is highlighted in Figure 1, which summarizes the evolution of the minimum wage in relation to the median wage in the private sector between 1996 and 2008. It is clear that the large increase in the minimum wage between 2001 and 2002 represented a permanent regime-shift.

The Hungarian economy was performing well and there were no dramatic macroeconomic shocks around the time of the reform (see online Appendix Figure A2 for the details). Real per capita GDP growth was around 4 percent before and after the reform. In line with the positive growth rate, aggregate labor market conditions were gradually improving: the employment to population rate increased by 0.5 percent each year between 1997 and 2004 and the unemployment rate fell to 5 percent by 2001 and then remained at this low level. Inflation (CPI) was relatively high (around

[4] The council sets the minimum monthly base earnings (total earnings net of overtime pay, shift pay, and bonuses) for a full-time worker. For part-timers, accounting for only 5 percent of all employees in Hungary, the minimum is proportionally lower.

[5] The exchange rate was 280 HUF/US$ on January 1, 2001, so the monthly base earnings was increased from $91 to $179.

[6] In the previous general election in 1998, none of the major political parties campaigned for increasing the minimum wage. However, by the next general election in 2002, all major parties described their positions on minimum wage in their election manifesto. The increased prominence of minimum wage policies highlight that the governing right-wing parties were successful in setting the political agenda with its radical minimum wage policy.

10 percent in 2000) and it was slowly declining. The exchange rate was also stable around the time of the reform.

Other changes in the policy environment could potentially contaminate our results. While our reading of the evidence is that there were no significant changes that could alter our conclusions significantly, we list all relevant policy changes that we are aware of in online Appendix Section A.6 and discuss their potential effects on our results. These policy changes are the following: the expansion of higher education from 1996, small minimum wage compensation schemes in 2001 and 2002, exemption of the minimum wage from personal income taxes in 2002, and a 50 percent increase in public sector base wages in 2002 (see Telegdy 2018). Moreover, throughout the paper we assume that the estimated effects we report are real responses. However, in the presence of tax evasion, some of the estimated effects may reflect only reporting behavior (Elek et al. 2011). In online Appendix Section A.6.5 we present various robustness checks which suggest that our estimates are unlikely to be driven by changes in reporting behavior.

Finally, it is unlikely that firing and hiring restrictions substantially prevented firms from responding to the increased minimum wage: in the period we examine, the strength of employment protection in Hungary was in the bottom third of OECD countries, at a level similar to Switzerland or Japan (OECD 2004).

B. *Data and Descriptive Statistics*

The main data source in the paper is the Hungarian Corporate Income Tax Data (CIT) that covers the universe of firms with double book-keeping. The data contain information on employment, firms' balance-sheet, and income statements. This panel dataset allows us to follow employment, revenue, profitability, and the cost structure of firms over time. But it does not contain information on worker-level wages.

We observe individual worker-level information for the subset of firms which are in the Hungarian Structure of Earnings Survey (SES).[7] The SES collects detailed information on worker-level wages, job characteristics, and demographic characteristics. For small firms in the survey (with 5 to 20 employees) we observe all workers, while for larger firms (more than 20 employees) we only observe a random sample of workers. Using individual-level wage information, we calculate the firm-level fraction of the workers below the 2002 minimum wage (adjusted by inflation and GDP growth) for the subset of firms with at least five workers in the SES. We say that these workers were directly affected by the increase in the minimum wage.

To maximize the sample size in our analysis we also predict the fraction of workers affected by the increased minimum wage for the firms not in the SES. We extend our sample in the following steps. First, we estimate the relationship between the fraction of workers affected by the minimum wage (observed in the SES) and the average cost of labor (observed in CIT) for the subset of firms included in both datasets. Second, we calculate the predicted fraction of workers affected by the new minimum wage for all firms in the CIT data using the average cost of labor

[7] The survey includes 26 percent of all firms in Hungary, representing 70 percent of all workers as larger firms are over-sampled (see online Appendix Section A.5.2 about the sample design).

Table 1—Summary Statistics of Firm-Level Characteristics in 2000

		All	Manufacturing	Tradable	Non-tradable	Exporting
		(1)	(2)	(3)	(4)	(5)
Average wage (1,000 HUF)	Mean	847	841	845	679	1,090
Average cost of labor (1,000 HUF)	Mean	1,358	1,313	1,316	1,099	1,845
Number of workers	Mean	47.1	78.7	80.7	34.9	145
Revenue (1,000 HUF)/labor	Mean	17,637	11,047	10,327	20,178	18,110
Capital (1,000 HUF)/labor	Mean	3,801	3,560	3,348	3,678	4,659
Export share (export/revenue)	Mean	0.10	0.21	0.25	0.06	0.71
Profitability (profit/revenue)	Mean	0.032	0.032	0.039	0.021	0.047
Depreciation share (depr/revenue)	Mean	0.026	0.038	0.025	0.023	0.027
Labor share (labor cost/revenue)	Mean	0.18	0.23	0.25	0.12	0.26
Material share (material/revenue)	Mean	0.74	0.69	0.67	0.81	0.64
GAP	Mean	0.16	0.14	0.13	0.21	0.090
Fraction affected	Mean	0.43	0.39	0.38	0.54	0.25
	SD	0.36	0.35	0.34	0.35	0.29
	p5	0	0	0	0	0
	p25	0.07	0.05	0.20	0.05	0
	p50	0.37	0.31	0.56	0.30	0.14
	p75	0.77	0.69	0.89	0.66	0.40
	p95	1	1	1	1	0.89
Observations		19,485	6,312	4,557	6,196	2,000

Notes: This table shows some summary statistics of the firms in our benchmark sample. All characteristics are evaluated in 2000 (one year before the minimum wage hike). Tradable and non-tradable sectors are defined as in Mian and Sufi (2014). The exporting sector compromises firms that export at least 40 percent of their total revenue. All statistics are weighted by the logarithm of revenue to be consistent with the regression analyses presented in the paper. The *GAP* measures the increase in average wage needed to comply to the 2002 minimum wage.

(observed in the CIT) and the estimated relationship. Third, to reduce noise in the predicted values, we calculate the predicted fraction affected every year between 1997 and 2000 and then we take the average across years.

Our main analysis focuses on the manufacturing, service, and construction sectors. We omit the public sector; agriculture; heavily regulated industries (energy, pharmaceuticals); industries where balance sheet items are hard to interpret (finance and insurance); and industries with special excise tax (oil and tobacco), since our revenue measure includes excise taxes. We focus on firms that existed between 1997 and 2000, and we drop firms with the top 1 percent and bottom 1 percent growth rate between 1997 and 2006. We also drop firms where the average wage per worker is less than 90 percent of the minimum wage in any year between 1997 and 2000. None of these restrictions are crucial, but they reduce the impact of outliers on our estimates. Moreover, in our benchmark specification we omit firms with less than 5 employees.[8] In the final sample we have 19,950 firms, representing around one million workers (or one-third of the total workforce in Hungary).

In Table 1 we report the means of some firm-level characteristics in 2000 by sector. An average firm in our sample employs 47 workers, 10 percent of its revenue is earned from exports, and its profitability is 3.2 percent of total revenue. The labor

[8] We exclude these micro-enterprises from our analysis for two reasons. First, the relationship between firm-level fraction affected and average cost of labor was estimated on a sample of firms with at least 5 employees, since the SES does not cover smaller firms. Therefore, the prediction of fraction affected for micro-enterprises might be biased. Second, the CIT data are less reliable for the smallest firms because of tax evasion.

share in total production is 18 percent, while the share of materials (intermediate goods and services) is around 74 percent.[9] The fraction of workers affected by the increased minimum wage for an average firm is 43 percent, while the median is 37 percent. The large exposure to the minimum wage is driven by smaller firms where the average cost of labor is often close to the minimum wage. The employment weighted average fraction affected is around 20 percent in our sample. The table also highlights some sectoral differences. Firms in the manufacturing, in the exporting and in the tradable sectors employ more workers on average, have higher labor share in production, and are also less exposed to the minimum wage than those in the non-tradable sectors.

For a subset of manufacturing firms in the CIT data we also have information on product-level prices from the Hungarian Annual Survey of Industrial Production (ASIP). We use these data to calculate firm-level Laspeyres price indices.

II. Employment Effects of the Minimum Wage

Identification Strategy.—We estimate the employment effects of the minimum wage by comparing the evolution of key outcome variables at firms with many workers affected by the minimum wage increase to those firms with few affected workers. We closely follow Machin, Manning, and Rahman (2003) and Draca, Machin, and Van Reenen (2011) and estimate regression models of the following form:

$$\frac{y_{it} - y_{i2000}}{y_{i2000}} = \alpha_t + \beta_t FA_i + \gamma_t X_{it} + \varepsilon_{it}\,, \tag{1}$$

where the left-hand side is the percentage change in outcome y between year 2000, the final full calendar year before the minimum wage increase, and year t.[10] This specification allows time effects and the impact of firm characteristics, γ_t, to vary flexibly over time.

We winsorize the percentage changes, $(y_{it} - y_{i2000})/y_{i2000}$, to take values between the first and the ninety-ninth percentile in each year. We include firms that shut down in the analysis as they experienced a 100 percent decline in their employment (and other outcomes). In the main analysis, we measure exposure to the minimum wage, FA_i, by calculating the fraction of workers for whom the 2002 minimum wage binds, while in Section VI we explore robustness to alternative measures of exposure such as the "wage gap" measure. As we describe in the data section, we predict FA_i from the average cost of labor (observed in CIT) before the minimum wage hike. The regression specification above assumes a linear relationship between exposure to the minimum wage and various outcomes. In online Appendix Figures A5 and A8 we show that nonparametric estimates of the responses we present here are indeed approximately linear.

[9] Such a large role for intermediate goods and services at the firm-level is not unusual in European countries (see online Appendix Table A1).

[10] The minimum wage hikes were announced in year 2000, so it is possible that outcomes in 2000 were already affected. However, we do not detect any unusual changes in year 2000 and so any anticipation effects must be small.

We restrict our sample to firms that existed between 1997 and 2000. We estimate robust standard errors[11] and we use the logarithm of the revenue in 2000 as weights in our regressions.[12] In our benchmark regression we control for firm age, the legal form of organization (e.g., limited liability company, publicly traded, etc.), and the following variables and their squares: average export share between 1997 and 2000; average profitability between 1997 and 2000; the average share of labor between 1997 and 2000; average depreciation rate between 1997 and 2000; the average share of wage cost in total labor cost between 1997 and 2000; and the average industry level import exposure between 1997 and 2000. In Section V we also explore including three-digit NACE industry dummies.

The key identification assumption in this difference-in-differences type of regression is that changes at firms with fewer minimum wage workers are a valid estimate of the counterfactual for firms with many affected workers and so these firms would follow a parallel trend in the absence of the minimum wage increase. While this assumption cannot be tested directly, we test whether the parallel trends assumption holds before the minimum wage hike. Reassuringly, we cannot reject the presence of differential trends in most specifications.

There are various limitations of the approach presented here. First, β_t identifies responses relative to the untreated firms (which requires the Stable Unit Treatment Value Assumption (SUTVA)). Second, equation (1) only shows the effect on existing firms. In Section V we return to these issues and show that these shortcomings are unlikely to affect our key results.

Employment Effects.—The estimates for employment from regressions of equation (1) are summarized in panel A of Table 2. Columns 1 and 2 show the effects of the minimum wage two years after the minimum wage hike. The point estimate in column 1 indicates that the employment declines by 7.8 percent (SE 0.8 percent) more at firms where 100 percent of the workforce is directly affected by the minimum wage relative to firms where there are no exposed workers. Remember, in our analysis we also include firms which shut down. Therefore, the results presented here reflect firms' decisions on both the extensive margin (closing) and intensive margin (layoffs). The estimated employment loss is slightly smaller (7.6 percent, SE 1.0 percent) if we control for the rich set of observable characteristics described above (see column 2).

In columns 3 and 4 we examine the "medium-term" employment effects by estimating employment changes between 2000 and 2004. The specification with controls shows that employment is around 10.0 percent (SE 1.2 percent) lower at firms with 100 percent exposure relative to a firm with no exposure. This highlights

[11] Using the predicted FA_i instead of the actual FA_i can potentially bias the estimates of our the standard errors. In online Appendix Section A.5.3 we show that the bias from predicting FA_i is negligible.

[12] Most papers in the minimum wage literature do not use weights in firm-level regressions (Machin, Manning, and Rahman 2003; Draca, Machin, and Van Reenen 2011; Hau, Huang, and Wang 2016; Kertesi and Köllő 2004; Mayneris, Poncet, and Zhang 2018). A notable exception is Card and Krueger (1994) who report estimates from regressions using employment weights. In our case, using the level of employment or the level of revenue as weights would be problematic as the distributions of these variables are highly skewed (e.g., the employment has a Pareto tail with $\alpha = 1.5$) and so the mean and the variance of the weights are not finite. Therefore, the central limit theorem does not hold in the level-weighted regressions. To avoid using highly skewed weights we use the logarithm of revenue as weights.

TABLE 2—EMPLOYMENT AND WAGE EFFECTS

	Main changes between 2000 and 2002		Main changes between 2000 and 2004		Placebo changes between 2000 and 1998	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Change in firm-level employment						
Fraction affected	−0.078	−0.076	−0.093	−0.100	−0.003	0.002
	(0.008)	(0.010)	(0.012)	(0.012)	(0.008)	(0.009)
Constant	−0.050		−0.105		0.046	
	(0.005)		(0.007)		(0.005)	
Observations	19,485	19,485	19,485	19,485	19,485	19,485
Employment elasticity with respect to *MW* (directly affected)	−0.11	−0.10	−0.15	−0.15		
	(0.01)	(0.01)	(0.02)	(0.02)		
Panel B. Change in firm-level average wage						
Fraction affected	0.53	0.58	0.48	0.54	−0.02	−0.03
	(0.01)	(0.01)	(0.01)	(0.01)	(0.003)	(0.01)
Constant	0.08		0.16		−0.08	
	(0.002)		(0.01)		(0.001)	
Observations	18,415	18,415	16,980	16,980	19,485	19,485
Employment elasticity with respect to wage	−0.15	−0.13	−0.20	−0.18		
	(0.02)	(0.02)	(0.03)	(0.03)		
Panel C. Change in firm-level average cost of labor						
Fraction affected	0.47	0.49	0.41	0.43	−0.03	−0.04
	(0.01)	(0.01)	(0.01)	(0.01)	(0.003)	(0.01)
Constant	0.04		0.10		−0.04	
	(0.001)		(0.002)		(0.001)	
Observations	18,415	18,415	16,980	16,980	19,485	19,485
Employment elasticity with respect to cost of labor	−0.17	−0.16	−0.22	−0.23		
	(0.02)	(0.02)	(0.03)	(0.03)		
Controls	No	Yes	No	Yes	No	Yes

Notes: This table shows the firm-level relationship between the fraction of workers exposed to the minimum wage and the change in employment (panel A), the change in average wage (panel B), and the change in average cost of labor (panel C). The cost of labor includes wages, social security contributions, and non-wage labor expenses. The estimates are based on equation (1). The employment changes include both extensive margin (closing) and intensive margin (layoff) decisions. Columns 1 and 2 show the short-term effects (the change between 2000 and 2002), columns 3 and 4 the medium-term changes (changes between 2000 and 2004). Columns 5 and 6 test for the presence of preexisting trends by looking at the effect on "placebo" changes, which equal to the year 1998 outcome minus the year 2000 outcome. Columns 1, 3, and 5 show the raw correlations, while columns 2, 4, and 6 show the estimates when we control for firm age, the legal form of organization (e.g., limited liability company, publicly traded, etc.), and the following variables and their squares: average export share between 1997 and 2000; average profitability between 1997 and 2000; the average share of labor between 1997 and 2000; average depreciation rate between 1997 and 2000; the average share of wage cost in total labor cost between 1997 and 2000; and the average industry level import exposure between 1997 and 2000. We winsorized all outcome variables at 1 percent and 99 percent levels for each year. Robust standard errors are reported in parentheses.

that the medium-term employment effects are somewhat larger than the short-term effects (10.0 percent versus 7.6 percent), but 76 percent of the total employment loss occurred within two years of the minimum wage hike. This pattern is also underscored in panel A of Figure 2, where we plot the evolution of the relationship between fraction affected and the change in employment over time.

How do these estimates relate to the previous literature? In panel A of Table 2 we report the employment elasticity with respect to the minimum wage for the

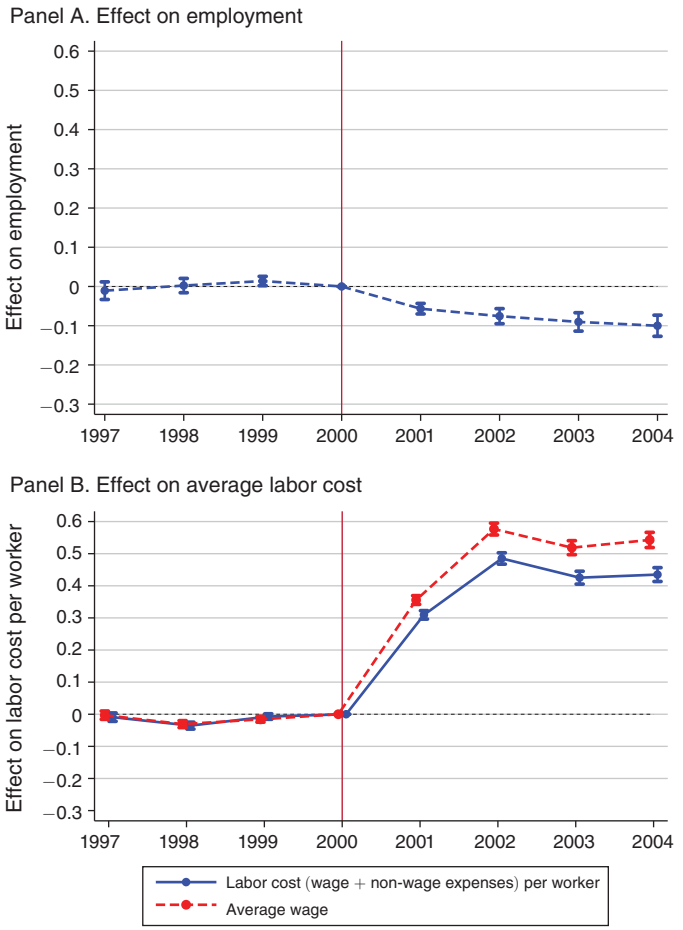

FIGURE 2. EFFECT ON EMPLOYMENT AND ON WAGES

Notes: This figure shows the relationship between changes in different outcome variables and the fraction of workers affected by the minimum wage hike over time (beta coefficients with its 95 percent confidence intervals from equation (1)). Panel A shows the effects on changes in employment. The employment changes include both extensive margin (closing) and intensive margin (layoff) decisions. Panel B shows the effect on firm-level average wage (total wage bill per worker) and average labor cost (total labor cost per worker). The cost of labor includes wages, social security contributions, and non-wage labor expenses. The ratio of panel A and panel B determines the employment elasticity. Controls are included in the regressions.

directly affected workers ($\%\Delta Emp/\%\Delta MW$). To compare these estimates to the ones reported in existing literature, we need to take into account that elasticities are often reported for a particular group of workers (e.g., teenagers or restaurant workers). As a result, we need to multiply our estimates for the *directly affected workers* by the fraction of workers directly affected in that particular group. For instance, to compare our results to the estimates in the US literature that mainly focus on teenage workers, we need to multiply our estimates by 0.25, which is the share of directly affected teenagers in the United States in 2012. Therefore, our estimates in panel A imply that the teen employment elasticity is between -0.02 and -0.04 depending on the specification considered. These employment elasticities are an order of magnitude smaller than the range of -0.1 to -0.3 suggested by Neumark

and Wascher (2010) or Brown (1999), but are in line with some recent meta-analyses in the literature (Belman and Wolfson 2014, CBO 2014, Doucouliagos and Stanley 2009, MaCurdy 2015).

Two points should be noted. First, columns 5 and 6 in Table 2 and panel A of Figure 2 show the relationship between exposure to the minimum wage and employment preceding the minimum wage hike. Consistent with a causal interpretation of our estimates, the pre-reform changes in employment are close to zero and the negative relationship emerged exactly at the timing of the reform.

Second, as we mentioned before, we show in online Appendix Figure A5 that the nonparametric relationship between employment changes and exposure to the minimum wage is linear. Notice that such a linear relationship is hard to reconcile with the presence of imperfect competition and monopsony power in the labor market. If firms face upward-sloping labor supply curves, we would expect that small minimum wage shocks would have a small or even positive effect on employment, but for large shocks the effect should be negative (Manning 2003). Therefore, the presence of monopsonistic competition would predict that the relationship between minimum wage exposure and the disemployment effects should be nonlinear, contrary to our empirical findings.

Effects on Wages and the Cost of Labor.—The size of the employment effect should be compared to the wage effects (Machin, Manning, and Rahman 2003). We investigate the effect on wages by estimating the firm-level relationship between fraction of affected workers and the percentage change in the wage for an average worker using equation (1). Since we can only calculate wage changes for the firms that survived, we restrict the sample to those.[13]

We use two concepts of remuneration in this section: wages and cost of labor. This latter differs from the former because it does not only include wages, but employer's social security contributions and non-cash employment benefits as well. In panel B of Table 2 we report the effect of the minimum wage on wages. This is what most papers in the literature estimate and so it is useful for comparing our results to existing studies. In panel C of Table 2 we report estimates on the average cost of labor. This measure is what firms take into account when they make their firing and hiring decisions.

Columns 1 and 2 in panel B of Table 2 highlight that the minimum wage had a very large and statistically significant effect on average wages in the short run. For instance, column 2 in panel B shows that firms with 100 percent fraction affected experienced a 58 percent (SE 1 percent) increase in their average wage relative to those with no affected workers in 2002. The increase in cost of labor was around 15 percent less at 49 percent (see panel C). The lower impact on labor cost simply reflects that the wage increase is compared to a higher base which includes social security contributions and non-cash benefits. We find no indication that firms tried to offset the wage increase by cutting non-wage benefits.

[13] Firms' closure might not occur at random. In online Appendix Table A2 we report estimates in which we correct for nonrandom exit by following Johnson, Kitamura, and Neal (2000). The results are very similar to those presented here.

Columns 3 and 4 of Table 2 show that the effect on average wage is slightly lower in the medium term, because the real value of minimum wage was somewhat lower in 2004 than in 2002 (see Figure 1). For instance, the wage effects are 54 percent (SE 1 percent) in the medium term. Panel B in Figure 2 shows the evolution of wage effects and cost of labor effects over time. Two points should be noted. First, before the reform there is no clear relationship between fraction affected and the change in either the wage or the cost of labor, while both the wage and the cost of labor increased dramatically at highly exposed firms after the reform. Second, the time pattern of the wage increase (with the effect highest in 2002 before dropping slightly in 2003) closely resembles the evolution of the minimum wage in Figure 1. This implies that the changes in wages are likely to be related to the minimum wage change and not to something else.

Figure 2 also highlights the size of the wage and cost of labor effects (panel B) relative to the employment effects (panel A). To make these two comparable, we use the same scale in both panels. The large differences in the percentage changes in wage and employment highlight that the wage effect of the minimum wage dominates the employment responses. This is also reflected in the employment elasticities with respect to the wage, i.e., ratios of the (estimated) percentage change in employment and the (estimated) percentage change in wages that we report in panel B of Table 2, and the employment elasticities with respect to the cost of labor that we report in panel C. The short-run elasticity for wages is around -0.13 (SE 0.02), while the medium-run is slightly higher (-0.18, SE 0.03). The employment elasticity with respect to labor cost is slightly higher (in absolute terms) at -0.16 (SE 0.02) two years after, and -0.23 (SE 0.03) four years after the minimum wage hike. These elasticities are at the lower end of the literature but lie within the 95 percent confidence intervals of most previous estimates.[14] Moreover, the precision of our estimates is an order of magnitude smaller than previous estimates, even though many papers in the literature do not calculate robust standard errors. The relatively small standard errors are the consequence of the uniquely large and persistent minimum wage shock considered here. The magnitude of the reform delivers a large and precisely estimated effect on wages (e.g., 58 percent with 1 percent SE in 2002). When we divide the employment effects by this precisely estimated wage effect, the standard errors on our employment elasticities remain small.

It is also worth mentioning that, while the employment elasticities gradually increase (in absolute value) up to 2003, they stabilize after. The employment elasticity with respect to labor cost is -0.22 in 2003, which is almost the same as the 2004 estimates (-0.23). This suggests that the medium-term employment elasticity also reflects long-term responses. Moreover, the quick convergence in the elasticities underscores that firms responded quickly to the radical change in the minimum wage.

[14] In online Appendix Figure A7 we contrast our employment elasticities with the findings in the previous literature.

III. Firms' Margins of Adjustment and the Incidence of the Minimum Wage

The previous section shows that the minimum wage increase had a large positive effect on real wages and a small negative effect on employment. The simple consequence of this finding is that the income of low wage workers increased in response to the minimum wage. However, this income gain must be paid for by others in the economy. In this section we examine behavioral responses of firms at various margins in order to better understand who bears the incidence of the minimum wage.

A. *Firms' Margins of Adjustment*

Labor Cost.—We first document the effect of the minimum wage on total labor cost, which is a proxy for total income collected by workers. Again we estimate the relationship between the fraction of workers affected by the minimum wage and the change in total labor cost four years before and four years after the minimum wage increase using equation (1). Panel A of Table 3 shows the estimated coefficients, while Figure 3 plots them over time. Figure 3 (and also column 3 of Table 3) shows that firms with different levels of minimum wage exposure follow a parallel trend before the minimum wage hike. However, this trend broke exactly in 2001, when the minimum wage was raised. The increase in labor cost at firms where 100 percent of the workers earned below the new minimum wage was 32.5 percent (SE 1.3 percent) and 23.8 percent (SE 2.0 percent) higher two and four years after the minimum wage hike relative to a firm with no workers below the new minimum. This large increase in firms' labor cost is in line with our previous findings on wages and employment.

Revenue.—We examine the effect on revenue in panel B of Table 3 and in panel B of Figure 3. The relationship between the minimum wage and revenue exhibits a similar pattern to the labor cost. Highly exposed and less exposed firms follow a parallel trend before the reform, but this trend breaks exactly at the time of the reform. Total revenue increased by 6.6 percent (SE 1.3 percent) more at highly exposed firms two years after the hike and by 3.6 percent (SE 1.8 percent) four years after. The considerable increase in revenue suggests that a part of the labor cost increase was financed by consumers.

Price.—Is the increase in revenue caused by higher output or by higher prices? We examine the effect of the minimum wage on prices in the manufacturing sector where we have access to firm-product level price data for a large sample, covering around 50 percent of firms. We construct a firm-level Laspeyres price index by weighting product-level price changes by the product's revenue share in the firm's output portfolio, and then we estimate the effect of the minimum wage on this price index using equation (1).[15] Column 1 of Table 4 shows the raw correlation between fraction affected and the change in output prices. The estimates show that prices increased by 7.4 percent (SE 2.4 percent) in the short term and by 13.4 percent (SE

[15] See online Appendix Section A.5.5 for further details on how we construct our price index.

Table 3—Firms' Margins of Adjustment

	Main changes between 2000 and 2002	Main changes between 2000 and 2004	Placebo changes between 2000 and 1998
	(1)	(2)	(3)
Panel A. Change in total labor cost			
Fraction affected	0.325	0.238	−0.031
	(0.013)	(0.020)	(0.009)
Panel B. Change in revenue			
Fraction affected	0.066	0.036	−0.020
	(0.013)	(0.018)	(0.015)
Panel C. Change in materials			
Fraction affected	0.049	0.021	-0.008
	(0.014)	(0.019)	(0.019)
Panel D. Change in capital			
Fraction affected	0.148	0.270	−0.006
	(0.034)	(0.054)	(0.015)
Panel E. Change in profits (*relative to revenue in 2000*)			
Fraction affected	−0.011	−0.008	0.006
	(0.003)	(0.004)	(0.004)
Observations	19,485	19,485	19,485
Controls	Yes	Yes	Yes

Notes: This table shows the firm-level relationship between the fraction of affected workers and the percentage change in various outcomes (see equation (1)). The first two columns show our main results by looking at changes between 2000 and 2002 (short-term) and changes between 2000 and 2004 (medium-term). The output changes include both intensive margin and firm closure responses. Column 3 tests for the presence of preexisting trends by looking at the effect on "placebo" changes, which equal to the year 1998 outcome minus the year 2000 outcome. Regressions are weighted by the logarithm of revenue in 2000. We control for firm age, the legal form of organization (e.g., limited liability company, publicly traded, etc.), and the following variables and their squares: average export share between 1997 and 2000; average profitability between 1997 and 2000; the average share of labor between 1997 and 2000; average depreciation rate between 1997 and 2000; the average share of wage cost in total labor cost between 1997 and 2000; and the average industry level import exposure between 1997 and 2000. We winsorized all outcome variables at 1 percent and 99 percent levels for each year. Robust standard errors are reported in parentheses.

4.5 percent) in the medium term. Controlling for observable characteristics (column 2) slightly decreases the estimates to 4.3 percent (SE 2.8 percent) in the short term and 10.8 percent (SE 4.8 percent) in the medium term. Panel C also reports the relationship between exposure to the minimum wage and prices in the periods before the minimum wage reform, and we do not find evidence for preexisting trends.[16]

The red dashed line in Figure 4 plots the estimated coefficients from column 2 of Table 4 over time. The graph provides further support for the findings in Table 4. It demonstrates the absence of a relationship between the minimum wage and price changes before the reform and the emergence of a large and significant positive price effect after the minimum wage hike. The figure also suggests that the price

[16] Raising prices is likely to be the hardest in the manufacturing sectors, where firms face foreign competition. Therefore, even though we cannot assess the price effects outside of manufacturing, it is likely that price increase played a key role in other sectors as well.

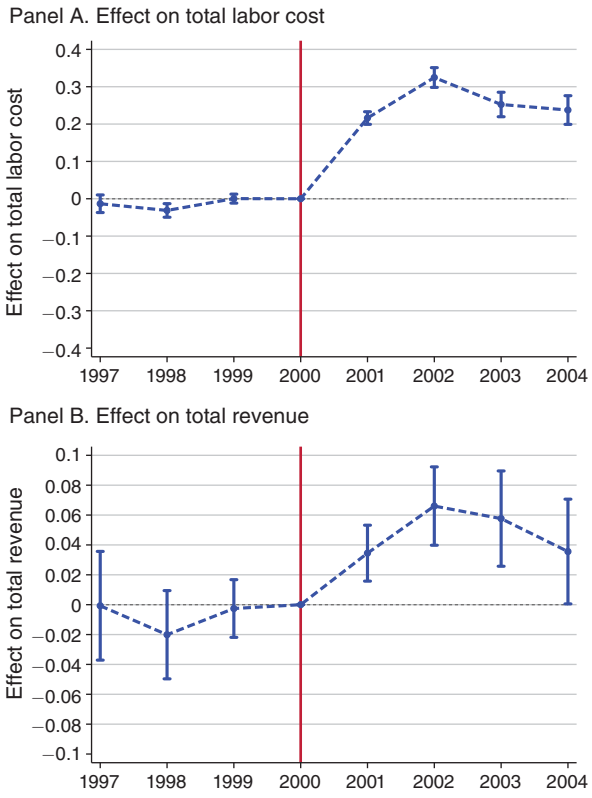

FIGURE 3. EFFECT ON TOTAL LABOR COST AND ON REVENUE

Notes: This figure shows the relationship between changes in different outcome variables and the fraction of workers affected by the minimum wage hike over time (beta coefficients with its 95 percent confidence intervals from equation (1)). Panel A shows the effects on changes in total labor cost, while panel B on changes in total revenue. Both panel A and panel B show regression results which include firms' extensive (firm closure) and intensive margin responses. Controls are also included in the regressions.

responses to the minimum wage occur gradually as it takes time for firms to adjust their prices.

We also explore further robustness checks related to the price effects in Table 4. In the short term, we have more firms with price data than in the medium term (3,252 in 2002 and 2,541 in 2004), because some firms shut down or otherwise leave the survey over time. In columns 3 and 4 we examine whether the differential short- and long-term price changes are caused by changes in the sample composition and we find no indication for that.

What is the effect on quantity sold? The blue solid line in Figure 4 plots the size of the revenue effects in the manufacturing sector. The figure highlights that the revenue change, which measures the change in the product of price and quantity, is lower than the price change after 2002. This implies that the quantity fell in response to the minimum wage after 2002. Such a drop in output and increase in prices are not in line with the basic predictions of the monopsony model (Aaronson and French 2007), since in those models a minimum wage induces firms to hire more, which eventually leads to higher output and lower prices.

Table 4—Effect on Firm-Level Price Index in the Manufacturing Sector

	All firms		Exists between 2000 and 2004	
	(1)	(2)	(3)	(4)
Panel A. Change between 2000 and 2002 (short-term)				
Fraction affected	0.074	0.043	0.083	0.061
	(0.024)	(0.028)	(0.026)	(0.032)
Constant	0.16		0.15	
	(0.01)		(0.01)	
Observations	3,252	3,252	2,541	2,541
Panel B. Change between 2000 and 2004 (medium-term)				
Fraction affected	0.134	0.108	0.134	0.108
	(0.045)	(0.048)	(0.045)	(0.048)
Constant	0.28		0.28	
	(0.02)		(0.02)	
Observations	2,541	2,541	2,541	2,541
Panel C. Placebo change between 2000 and 1998				
Fraction affected	−0.001	−0.015	−0.008	−0.032
	(0.019)	(0.022)	(0.023)	(0.027)
Constant	−0.10		−0.10	
	(0.01)		(0.01)	
Observations	2,640	2,640	1,822	1,822
Controls	No	Yes	No	Yes

Notes: This table shows the firm-level relationship between the fraction of affected workers and the Laspeyres price index in the manufacturing sector. The price index was calculated from the Producer Price Survey (see the details in the text). Columns 1–2 include all firms for which price changes can be calculated, while columns 3–4 restrict the sample to the firms which existed between 2000 and 2004. Panel A shows the short-term effects (change between 2000 and 2002), panel B the medium-term (change between 2000 and 2004), while panel C checks for preexisting trends by looking at the effect on "placebo" changes, i.e., the year 1998 price-level minus the year 2000 price-level. Columns 2 and 4 control for firm age, the legal form of organization (e.g., limited liability company, publicly traded, etc.), and the following variables and their squares: average export share between 1997 and 2000; average profitability between 1997 and 2000; the average share of labor between 1997 and 2000; average depreciation rate between 1997 and 2000; the average share of wage cost in total labor cost between 1997 and 2000; and the average industry level import exposure between 1997 and 2000. We winsorized the price changes at level 1 percent and 99 percent levels for each year. Regressions are weighted by the logarithm of revenue in 2000. Robust standard errors are reported in parentheses.

Materials.—In Table 3 we also examine the effect on materials (intermediate goods and services). Even though adjustment on that margin is often overlooked in the literature, it is in an important factor as spending on materials is around 74 percent of total revenue (see Table 1).[17] Total spending on materials increased in the short term (4.9 percent, SE 1.4 percent),[18] while in the medium term the effect on materials is smaller and insignificant (2.1 percent, SE 1.9 percent). Both

[17] Online Appendix Table A1 shows that the share of materials in production is generally high across Europe: it is around 66 percent in Western Europe and 72 percent in Eastern Europe.

[18] The positive effect on materials can be explained by substitution between labor and materials or by a differential increase in the price of the intermediate goods. This latter can emerge if the suppliers of a minimum wage firm tend to be other minimum wage firms and so all firms raise prices throughout the supply chain.

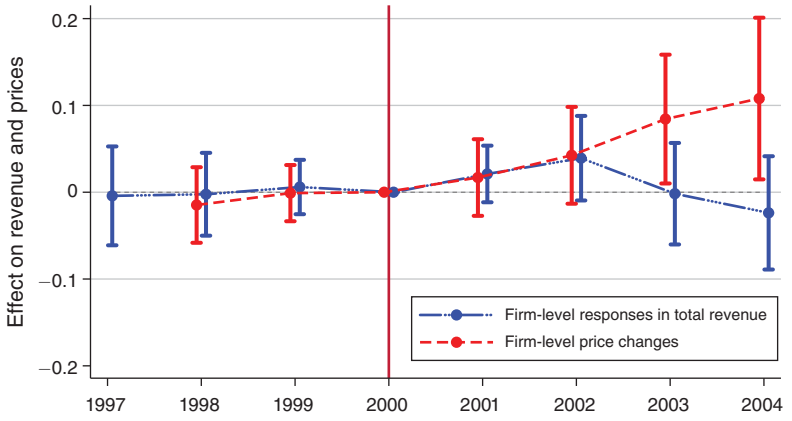

FIGURE 4. EFFECT ON PRICE INDEX AND REVENUE IN THE MANUFACTURING SECTOR

Notes: The figure shows the relationship between changes in different outcome variables and the fraction of workers affected by the minimum wage hike over time (beta coefficients with its 95 percent confidence intervals from equation (1)). The red dashed line shows firm-level price changes, while the blue solid line shows the revenue changes. The revenue changes include extensive (firm closure) and intensive margin responses. Since price data are available only for the manufacturing sector, we restrict our analysis to that sector. Controls are also included in the regressions.

the short-term and long-term estimates on materials are lower than the increase in revenue, and so the increase in revenue net of materials was considerable.

Capital.—In panel D of Table 3 we explore the effect on capital.[19] Apart from a recent study on the effect of the minimum wage in China (Hau, Huang, and Wang 2016), existing literature has not examined capital responses to the minimum wage. The point estimates show a large and significant positive effect on the capital stock both in the short and in the medium term. The capital stock had increased by almost 27.0 percent (SE 5.4 percent) within four years of the minimum wage hike. This suggests that capital-labor substitution is an important margin of adjustment.

Profits.—In panel E we estimate the effect on accounting profits (Earnings before Interest and Taxes). Column 1 shows that profits (relative to the revenue in 2000)[20] fell by 1.1 percentage point (SE 0.3) at highly exposed firms in the short run (within two years of the reform). This change is around 30 percent of the average profitability in 2000, which was 3.2 percent (see Table 1). The medium-term profit reduction was slightly less at 0.8 percentage points (SE 0.4), which is around 20 percent of

[19] We calculated the capital stock using the perpetual inventory method (see the details in Békés and Harasztosi 2013). We use the investment flows from 1992 (or the year of establishment for firms established later). In the initial period we take the value of fixed assets as investments. In later periods investments is the sum of depreciation and the change in tangible fixed assets. To turn nominal values into real ones, we use sector level investment deflators from the Central Statistics Office of Hungary. The perpetual inventory method has an unfortunate shortcoming that it does not take into account rented capital. If a firm rents machinery, office space, or cars, such items appear as material costs.

[20] Since profit can be negative or zero in the base year (in year 2000), we use $\Delta Profit/Revenue_{i2000}$ as an outcome variable in equation (1).

the average yearly profit level. This suggests that part of the wage increase is paid by firm owners.

B. *Incidence of the Minimum Wage*

Our estimates above can be used to assess the incidence of the minimum wage. Our starting point is the following accounting identity:

$$Profit \equiv Revenue - Material - LaborCost - Depr - MiscItems,$$

where *Depr* is depreciation expenses, while *MiscItems* includes minor accounting items (e.g., accrual deferrals). This equation leads to the following expression:[21]

$$\frac{\Delta LaborCost}{Revenue_{2000}} = \underbrace{\frac{\Delta Revenue}{Revenue_{2000}} - \frac{\Delta Material}{Revenue_{2000}} - \frac{\Delta MiscItems}{Revenue_{2000}}}_{\text{Consumers Pay}} \underbrace{- \frac{\Delta Depr}{Revenue_{2000}} - \frac{\Delta Profit}{Revenue_{2000}}}_{\text{Firm Owners Pay}}. \tag{2}$$

The equation above highlights that the change in labor cost (relative to the revenue in 2000) can be decomposed into two parts. The first part shows the revenue change net of material (and other) expenses, and so it captures the value added in production. This is the amount that firms receive from consumers in exchange of the value they created.[22] The value added is divided between firm owners and workers. The second part shows the effect on firm owners, which is the sum of profits and depreciation expenses. Three points should be noted.

First, our definition of firm owners' incidence is standard in the rent sharing literature (see, e.g., Card, Cardoso, and Kline 2016; Guiso, Pistaferri, and Schivardi 2005). Second, for simplicity we abstract away from the fact that part of the profits are paid to the government through taxes and not to the firm owners. Third, even if the level of accounting profits and depreciation expenses depend on whether the firm rents capital or directly owns it, the change in profit and depreciation will reflect the incidence on firm owners independently of the ownership structure.

We assess the incidence of the minimum wage by estimating the effect of the reform on various items in equation (2). We report the point estimates in Table 5. The 32.5 percent increase in total labor cost in 2002 (panel A in Table 3) translates into a 3.8 percentage point increase when we measure it relative to revenue in year 2000 and not to its own value in year 2000. The change in revenue net of materials and miscellaneous items equals to 2.8 percentage points in 2002 and this is the part that is paid by consumers. The remaining 1.0 percentage point is paid from a 1.1 percentage point reduction in profits, partly offset by a 0.1 percentage point increase in depreciation. As a result around 74 percent of the cost increase is paid

[21] This equation can also be derived from the perfectly competitive neoclassical model. Using the envelope theorem, it can be shown that the first-order effect on profit is equal to the change in revenue as a result of higher prices minus the wage bill increase (see Ashenfelter and Smith 1979 who show a special case where prices are fixed).

[22] If firm produces final goods (or services) these consumers are individuals, while if it sells intermediate goods the consumers are other firms.

Table 5—Incidence of the Minimum Wage

	Changes between 2000 and 2002 (1)	Changes between 2000 and 2004 (2)
Change in total labor cost relative to revenue in 2000	0.038	0.021
Change in revenue relative to revenue in 2000(Δ *Revenue*)	0.066	0.036
Change in materials relative to revenue in 2000 (Δ *Material*)	0.033	0.014
Change in miscitems relative to revenue in 2000 (Δ *MiscItems*)	0.005	0.005
Incidence on consumers (Δ *Revenue* $-$ Δ *Material* $-$ Δ *MiscItems*)	0.028	0.017
Change in profits relative to revenue in 2000 (Δ *Profit*)	−0.011	−0.008
Change in depreciation relative to revenue in 2000 (Δ *Depr*)	0.001	0.003
Incidence on firm owners ($-\Delta$ *Profit* $-$ Δ *Depr*)	0.010	0.005
Fraction paid by consumers (percent)	74	77
Fraction paid by firm owners (percent)	26	23

Notes: This table assesses the incidence of the minimum wage hike. We estimate the relationship between fraction affected and various balance sheet items in equation (2). We also report the *Incidence on consumers*, which is the revenue minus the sum of materials and other items. The *Incidence on firm owners* is the sum of profits and depreciation multiplied by −1. The *Fraction paid by consumers* is calculated by taking the ratio of the *Incidence on consumer* and the *Change in total labor cost*, while the *Fraction paid by firm owners* is the ratio of *Incidence on firm owners* and the *Change in total labor cost*. Column 1 shows the change between 2000 and 2002 (short-term), while column 2 shows the change between 2000 and 2004 (medium-term). In each regression we control for firm age, the legal form of organization (e.g., limited liability company, publicly traded, etc.), and the following variables and their squares: average export share between 1997 and 2000; average profitability between 1997 and 2000; the average share of labor between 1997 and 2000; average depreciation rate between 1997 and 2000; the average share of wage cost in total labor cost between 1997 and 2000; and the average industry level import exposure between 1997 and 2000. We winsorized all outcome variables at 1 percent and 99 percent levels for each year. Regressions are weighted by the logarithm of revenue in 2000.

by consumers while 26 percent paid by firm owners. Column 2 shows that the medium-term incidence is very similar to the short-term incidence: around 77 percent of the labor cost increase is paid by consumers and 23 percent by firm owners.

Who are these consumers buying the goods produced by the minimum wage workers? While we are not able to connect firm-level production to purchases by final consumers, we can assess consumption at the industry level. Following MaCurdy (2015) we calculate the industry-level cost shares of minimum wage workers. To take into account that some firms produce intermediate goods and services we use input-output linkages across industries to assess the exposure to the minimum wage at the industry level. We then calculate the share of each consumer's spending on various industries using the Hungarian Household Budget Survey from the year 2000 and, based on that, the minimum wage content of their consumption. Figure 5 shows the nonparametric relationship between household income and the minimum wage content of consumption for each household income decile. The figure highlights that the consumption spending of richer households contains a smaller fraction of the goods produces in industries exposed to the minimum wage, although the relationship is weak. While around 4.5 percent of the consumption is related to minimum wage workers in the lowest decile, it is 4.2 percent in the highest decile.[23]

[23] MaCurdy (2015) assumes that the effect of a price increase on consumers is the same across sectors. Nevertheless, we find that consumers are more responsive to firm-level price changes in the tradable sector than in the non-tradable sector, which suggests that consumers can substitute between minimum wage producers and

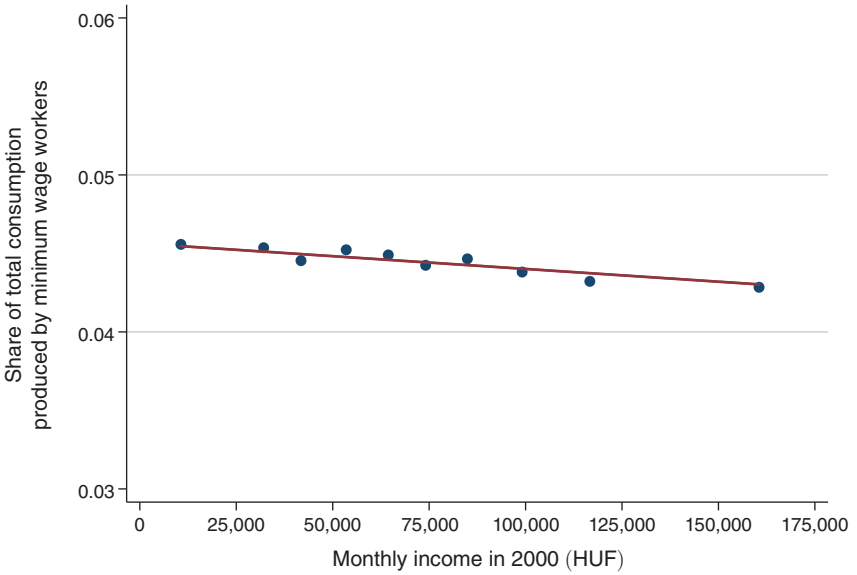

FIGURE 5. THE RELATIONSHIP BETWEEN HOUSEHOLD INCOME AND THE MINIMUM WAGE CONTENT OF CONSUMPTION

Notes: This figure shows the relationship between minimum wage content of consumption and household income. The blue dots show the share of consumption produced by minimum wage workers for each household decile and the red line is the linear fit. We calculate the minimum wage content of consumption following MaCurdy (2015); see online Appendix Section A.8 for details.

Since the minimum wage raised income of low wage workers, while the higher output prices are more or less equally shared among consumers, our evidence underscores that the minimum wage is an effective redistributive policy.

C. *Heterogeneous Responses to the Minimum Wage*

Figure 6 explores heterogeneity in the responses to the minimum wage increase. We focus here on the employment elasticity with respect to labor cost (panel A) and the revenue elasticity with respect to labor cost (panel B). We present results on other outcomes in online Appendix Tables A4 and A5.

The top rows in both panels of Figure 6 show the benchmark estimates on the medium-term responses (changes between 2000 and 2004). Rows 2–6 shows the effects by industries. We estimate regression equation (1) separately for each of the following sectors: manufacturing, service, tradable and non-tradable, exporting. The exporting sector comprises firms that export at least 40 percent of their total revenue. We classify sectors to tradable and non-tradable categories following Mian and Sufi's (2014) procedure. The tradable sector consists of those industries where the value of imports or exports exceeds 10 percent of total revenue in that industry. The

non-minimum wage producers more easily in the tradable than in the non-tradable sector. In the online Appendix we show that even if we take into consideration that the price change in the tradable sector impacts consumers less than the price change in other sectors, the relationship between household income and the consumption content of minimum wage remains quite similar.

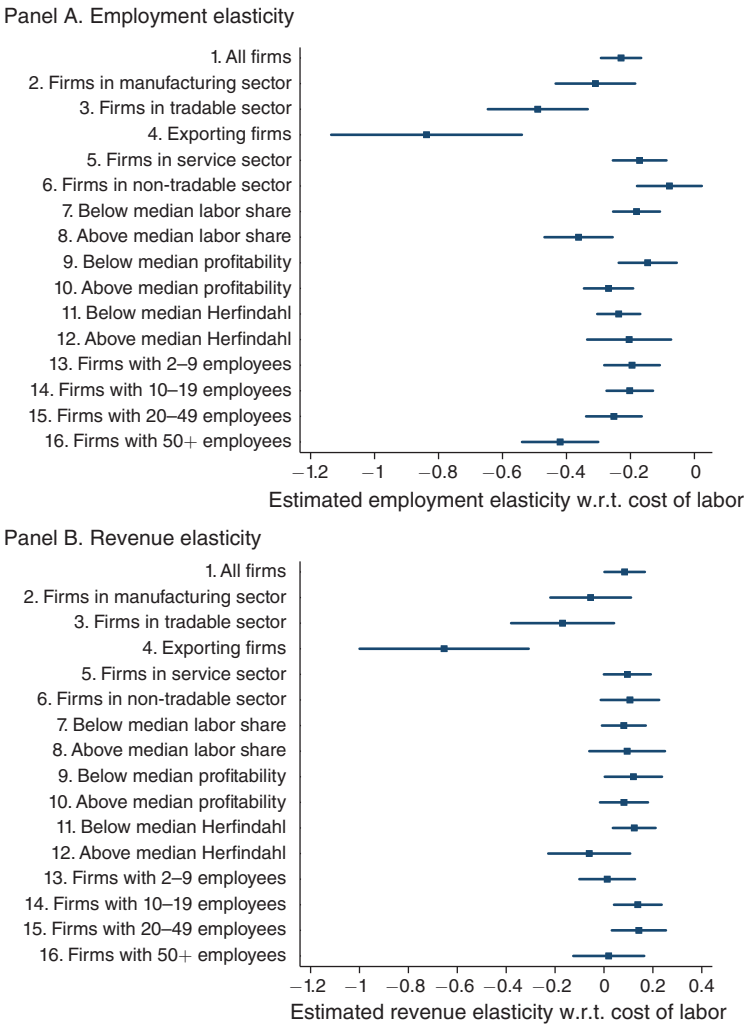

FIGURE 6. HETEROGENEOUS RESPONSES TO THE MINIMUM WAGE

Notes: Panel A shows the point estimates and 95 percent confidence intervals of the medium-term employment elasticities with respect to the cost of labor for various subgroups. Panel B shows the same for the revenue elasticity with respect to the cost of labor. We calculate the medium-term elasticities by estimating a separate regression for each subgroup. Medium-term elasticities are based on employment (panel A) and revenue (panel B) changes between 2000 and 2004. Controls are also included in each regression. The confidence intervals are calculated based on bootstrapped standard errors.

non-tradable sector consists of the retail and the catering sectors and those industries where firms are not geographically concentrated. In the online Appendix we describe the procedure and list the classification for each industry in detail.

Panel A highlights that the disemployment effects are considerably larger in the tradable and in the exporting sectors than in the non-tradable or service sectors. For instance, the employment elasticity in the exporting sector is -0.84, while in the non-tradable sector it is only -0.12 and statistically indistinguishable from zero. As a result, the benefits of the minimum wage are not shared equally across sectors.

Low wage workers in the tradable sector face large disemployment risks, which dampen the effects of the pay increase, but the minimum wage has a clear positive effect on earnings in the local service and non-tradable sectors.

Rows 2–6 in panel B explore the revenue effects across sectors. The revenue effects are negative in sectors where the disemployment effects are larger (e.g., the exporting or tradable sectors), while they are positive in the sectors where the employment changes are close to zero. Such a relationship between employment and revenue responses can emerge if the pressure to raise prices is similar across sectors, but consumers' responses to such a price change differ. In the tradable sector, any price increase will lead to competitive disadvantage relative to foreign companies which were not affected by the minimum wage reform. This causes a large loss in output and a negative effect on revenue. At the same time, in the non-tradable sector raising prices is less problematic as most competitors are also hit by the same cost shock. Therefore, any decrease in output demand following a price increase will be more limited. This can explain why we find a positive revenue effect in the non-tradable sector.

The differences between non-tradable and tradable sectors are hard to reconcile with models emphasizing the productivity enhancing effect of the minimum wage (Mayneris, Poncet, and Zhang 2018; Riley and Bondibene 2018; Hau, Huang, and Wang 2016) or models relying on monopsony power in labor markets (Bhaskar, Manning, and To 2002; Manning 2003). The common prediction in these models is that firms absorb the minimum wage shock by increasing their total production. Under the productivity enhancing explanation, firms produce more using the same amount of workers. In monopsony models production is increased since firms respond to the minimum wage by hiring more workers. Nevertheless, whatever the reason behind the increase in production, the prediction on the revenue effects is the same: revenue should increase in sectors where output demand is elastic, and fall in sectors where it is inelastic. For instance, firms in the exporting sector can easily sell the extra production without a substantial price impact. As result, we would expect positive revenue responses. At the same time, in the non-tradable sector, flooding the market with excess production would lead to price reductions, and so we would expect a fall in revenue. The revenue responses that we see in the data clearly contradict these predictions.

Figure 6 also explores heterogeneity in the effect of the minimum wage across various firm characteristics. We assign firms based on their characteristics between 1996 and 2000 and then estimate separate regressions for each group.[24] Rows 7 and 8 in Figure 6 show that the disemployment effect is larger for firms with a higher labor share. This evidence is in line with the role of pass through as firms with larger share of labor in production need to raise prices more to cover their labor cost.[25]

[24] An alternative approach would be to examine the effect of all relevant characteristics at the same time by running one regression with interaction of various firm characteristics. We do this exercise in the online Appendix and the responses are very similar to the ones presented here in most cases. One notable exception is the profits results, where we find no significant differences once we take into account that more profitable firms tend to be in the exporting sector and tend to be larger.

[25] We also find that the revenue responses are similar at low and at high labor share firms. The lack of differences in revenue can emerge in the model presented in Section IV if the output demand elasticity of low labor share firms is smaller than that of the high labor share ones.

Rows 9 and 10 show the difference between highly profitable and less profitable firms. The disemployment effects are slightly larger for profitable firms, which reflects that these firms tend be in the exporting sector. Rows 11 and 12 highlight that responses to the minimum wage are not related to market concentration measured by the Herfindahl index at the four-digit level. Finally, rows 13 to 16 highlight that the minimum wage reduces employment (and revenue) at larger firms more than at smaller ones. These differences partially reflect that fact that larger firms tend to be in the exporting and in the tradable sectors.

IV. A Hicks-Marshall Style Analysis of the Minimum Wage

The evidence provided so far underlines the importance of output demand in understanding responses to the minimum wage. This is in stark contrast with standard explanations for responses to the minimum wage which mainly focus on labor market frictions. In this section, we present a simple model with imperfect competition on the output market that is consistent with our key empirical findings. Then we assess this model quantitatively by estimating the key parameters using a method of moment estimation. The key advantage of the estimation is that it allows us to translate the "reduced-form" responses to easily interpretable structural parameters.

We consider markets consisting of monopolistically competitive firms in a partial equilibrium framework.[26] The monopolistic competition framework has several advantages. First, our approach allows us to model responses to the minimum wage at the level of both firms and the market. The model makes a distinction between minimum wage shocks that hit only a small subset of firms and shocks that affect all firms in the market equally. Second, in the model firms will set prices above the marginal cost and so they earn positive profits. Third, our model can capture that output prices may increase after a minimum wage hike.

It is easy to show that responses to a change in input prices lead to predictions similar to those described by the Hicks-Marshall rule of derived demand (we provide details in online Appendix Section A.7). When we have three inputs (labor, capital, materials), the elasticity of labor demand with respect to the minimum wage is equal to[27]

$$(3) \qquad \frac{\partial \log l(\omega)}{\partial \log MW} = \underbrace{-s_L \eta}_{\text{scale effect}} - \underbrace{s_K \sigma_{KL}}_{\text{substitution between } K \text{ and } L} - \underbrace{s_M \sigma_{ML}}_{\text{substitution between } M \text{ and } L},$$

where $l(\omega)$ is the low skilled labor demand for the firm producing variety ω, s_L is the share of labor in output, s_K is the share of capital expenses in production, s_M is

[26] It is possible to extend the model to take general equilibrium effects into account, but for simplicity we do not consider that extension in this paper. The key difference in the general equilibrium model is that the market-level output demand elasticity can be interpreted as a compensated demand elasticity instead of an uncompensated one (this point was made by Harberger 1962). If the income effects for the goods produced by the minimum wage workers are positive (normal goods), the uncompensated output demand elasticity will be lower than the compensated one. But if the income effect is negative (inferior goods), the opposite would be true.

[27] In the model a 1 percent increase in minimum wage is associated with 1 percent increase in cost of labor. However, in practice, the 1 percent increase in minimum wage often increases the cost of labor (and the wage) by less than 1 percent. We abstract from this here and use the change in minimum wage and the change in cost of labor interchangeably.

the share of intermediate goods used in the production, and σ_{KL} and σ_{ML} are the partial elasticities between capital and labor and materials and labor. The first part of equation (3) is the well-known scale effect. The model predicts firms will raise their output prices in response to the cost of labor and pass the effect of the minimum wage through to consumers. A key result is that the magnitude of this price response will depend only on the share of labor in the production, s_L. As a result of the price change, output falls and firms must scale back their production, and so they use less labor. The extent of the drop in production depends on the output demand elasticity, η, which is determined by the market structure. If all firms in the market use minimum wage workers, the demand elasticity will depend on the substitution between a market-level composite good and other expenses, which is likely to be small. However, when only a few firms on the market use minimum wage workers, most other firms which do not use minimum wage workers will get a competitive advantage. As a result output falls quite dramatically in the firms affected by the minimum wage increase, and so does employment at these firms.

The second and the third parts in equation (3) show the substitution effects: the possibility of replacing the more expensive labor with other inputs. The second part shows the substitution between capital and labor, while the third part shows the substitution between intermediate goods and labor. This substitution will depend on the Allen-partial elasticity and the share of other inputs in production.

Equation (3) highlights that the disemployment effects of the minimum wage must be negative, but can be quite small under certain parameter values. The importance of scale effects and the substitution effects depend on the factor shares. The labor cost is only 18 percent of total revenue for an average firm, while spending on capital is another 8 percent.[28] Expenses on intermediate goods and services (materials) are around 74 percent. As a result the substitution between intermediate goods and labor, a channel which is often ignored in the literature, potentially plays a crucial role in explaining the disemployment effects caused by the minimum wage.

In the model there is also a tight connection between the price increase and the change in total revenue, $p(\omega)q(\omega)$ as can be seen in the following equation:

$$\frac{\partial \log p(\omega)q(\omega)}{\partial \log MW} = \underbrace{s_L}_{\text{price effect}} - \underbrace{s_L \eta}_{\text{scale effect}}. \tag{4}$$

This equation allows us to translate the observed effect of the minimum wage on revenue into an output effect and a price effect. The key parameters in the employment and revenue equation also determine other outcomes such as demand for capital and intermediate goods:

$$\frac{\partial \log k(\omega)}{\partial \log MW} = s_L(-\eta + \sigma_{KL}), \tag{5}$$

$$\frac{\partial \log m(\omega)}{\partial \log MW} = s_L(-\eta + \sigma_{ML}). \tag{6}$$

[28] The capital share is the sum of profit and spending on capital depreciation.

TABLE 6—METHOD OF THE MOMENTS ESTIMATES USING MEDIUM-TERM RESPONSES

	All firms (1)	Manufacturing (2)	Tradable (3)	Non-tradable (4)	Export (5)
Panel A. Estimated parameters					
Output demand, η	0.11	0.98	1.34	−0.37	3.64
	(0.22)	(0.46)	(0.41)	(0.50)	(0.98)
Capital-labor substitution, σ_{KL}	3.35	2.60	2.34	3.94	4.63
	(0.62)	(1.01)	(0.83)	(1.59)	(2.45)
Material-labor substitution, σ_{ML}	0.03	0	0.01	0	0
	(0.06)	(0.10)	(0.13)	(0.09)	(0.26)
Panel B. Empirical moments					
Employment elasticity	−0.23	−0.31	−0.49	−0.08	−0.84
Revenue elasticity	0.08	−0.05	−0.17	0.11	−0.65
Materials elasticity	0.05	−0.17	−0.26	0.04	−0.73
Capital elasticity	0.62	0.37	0.28	0.70	0.50
Price elasticity		0.25			
Panel C. Moments predicted by the estimated parameters					
Employment elasticity	−0.24	−0.33	−0.51	−0.12	−0.95
Revenue elasticity	0.16	0.003	−0.09	0.12	−0.49
Materials elasticity	−0.01	−0.18	−0.33	0	−0.67
Capital elasticity	0.58	0.29	0.23	0.22	0.1
Price elasticity	0.18	0.23	0.25	0.12	0.18
Share of labor, s_L	0.18	0.23	0.25	0.12	0.18
Share of capital, s_K	0.08	0.07	0.08	0.07	0.08
Share of materials, s_M	0.74	0.70	0.67	0.81	0.74
No. of moments used	4	4	4	4	4
No. of estimated parameters	3	3	3	3	3
SSE	5.64	0.76	1.00	2.20	2.02

Notes: We estimate the parameters of the model presented in Section V using a minimum-distance estimator. In each column we use the empirical moments based on our benchmark estimates with controls. The estimated parameters with standard errors can be found in panel A. Panels B and C report the empirical and the predicted moments, respectively. SSE reports the weighted sum of squared errors.

Estimation.—To identify the key parameters, we estimate the model with a minimum-distance estimator, matching the empirical elasticities of various outcomes with respect to the change in cost of labor to the parameters of this model. We denote by $m(\xi)$ the vector of moments predicted by the theory as a function of the parameters ξ, and by $\hat{m}$ the vector of observed moments. We use four moments: employment elasticity (equation (3)), revenue elasticity (equation (4)), capital elasticity (equation (5)), and materials elasticity (equation (6)). We restrict σ_{KL} and σ_{ML} to be non-negative. The minimum-distance estimator chooses the parameters $\hat{\xi}$ that minimize the distance $\left(m(\xi) - \hat{m}\right)' W \left(m(\xi) - \hat{m}\right)$, where W is a weighting matrix. As a weighting matrix, we use the diagonal of the inverse of the variance-covariance matrix. Hence, the estimator minimizes the sum of squared distances, weighted by the inverse variance of each moment.[29]

Table 6 shows the estimated parameters (panel A) across sectors using our benchmark estimates on medium-term responses (between 2000 and 2004). When all

[29] Under standard conditions, the minimum-distance estimator using weighting matrix W achieves asymptotic normality, with estimated variance $\left(\hat{G}'W\hat{G}\right)^{-1}\left(\hat{G}'W\hat{\Lambda}W\hat{G}\right)\left(\hat{G}'W\hat{G}\right)^{-1}/N$, where $\hat{G} \equiv N^{-1}\sum_{i=1}^{N}\nabla_{\xi} m_i\left(\hat{\xi}\right)$ and $\hat{\Lambda} \equiv \text{var}\left[m\left(\hat{\xi}\right)\right]$ (Wooldridge 2010). We calculate $\nabla_{\xi} m\left(\hat{\xi}\right)$ numerically in Matlab using an adaptive finite difference algorithm.

firms are considered (column 1) we estimate that output demand is quite inelastic (0.11, SE 0.22). This implies that the minimum wage was passed through to consumers without a substantial reduction in output. Nevertheless, that output demand is inelastic on average does not mean that all individual firms can raise their prices without affecting their output: there is substantial heterogeneity across sectors, as highlighted in columns 2 to 5. The output elasticity is quite high in the exporting (3.64, SE 0.98) and in the tradable sectors (1.34, SE 0.41) where we estimate an elasticity that is closer to the firm-level one.[30] This highlights that individual firms cannot really raise prices without a large drop in their output. Conversely, in the local non-tradable sector all firms are hit by the minimum wage and the relevant output demand elasticity is the market-level one. The estimated elasticities are close to zero (−0.37, SE 0.50) which suggests that market-level price changes can be passed through to consumers in those sectors.[31]

Table 6 also reports estimates on the Allen partial elasticities. The estimated substitution between capital and low skilled labor in Table 6 is 3.35 (SE 0.62) and varies little across sectors. These estimates are higher than recent estimates in literature (e.g., Karabarbounis and Neiman 2014 found that capital-labor substitution elasticity is 1.25) although the literature has focused on the substitution between aggregate labor and capital. It is also surprising that the large substitution elasticity between capital and low skilled labor does not generate large disemployment effects. The key reason for this is that the share of capital expenses is only 8 percent of total production at the firm level, and so even a large capital labor substitution has only a small effect on employment. At the same time, the crucial factor in generating a low employment effect is the relatively low substitution between materials and employment, which is close to zero in all specifications.[32]

In Panels B and C of Table 6 we report the empirical and the actual moments. The moments predicted by the optimal parameter values match the moments in the data closely, especially for the employment elasticity and capital elasticity. We also report the predicted price effects, which equals the labor share s_L in that sector. Reassuringly, the estimated price effects in the manufacturing sector (0.23) match the actual price effects (0.25) quite well, even though we do not use that moment in the estimation. However, the model fit is not perfect. The model overpredicts the revenue elasticity and underpredicts the materials elasticity, especially for the specification that estimates one parameter for all firms. Once we move to sector-level analysis (columns 2 to 5), the model fit improves considerably (e.g., the SSE in the manufacturing sector is 0.76 versus 5.64 for all firms). Failing to predict these two moments suggests that our simple model does not capture all relevant aspects of the

[30] The Armington elasticity represents the elasticity of substitution between products of different countries. The short-term Armington elasticity is thought to be close to 1 (Blonigen and Wilson 1999, Reinert and Roland-Host 1992), while the long-term estimates are close to 5 (Ruhl 2008).

[31] MaCurdy's (2015) review concludes that the output demand elasticity in the minimum wage context is likely to be close to zero in the United States. Given that workers work predominantly in the local service sectors (e.g., restaurants or retail) in the United States, our evidence is consistent with that conclusion.

[32] The low elasticity of substitution between intermediate goods and labor is consistent with existing empirical estimates. Bruno's (1984) benchmark estimate for σ_{ML} in the manufacturing sector is 0.3, with alternative specifications producing estimates between −0.2 to 0.9. A more recent estimate by Atalay (2017) found 0.05 using all industries in his estimation. Moreover, Berndt and Wood (1979) and Basu (1996) pointed out that these estimates are likely to overstate the true elasticity of substitution between material and labor in the presence of varying capital and labor utilization.

economy. In particular, the increase in material spending (relative to non-exposed firms) might simply reflect that the price of intermediate goods purchased by minimum wage firms increased relative to the input prices of the non-exposed firms. This can happen, for instance, if minimum wage firms tend to have disproportionately large fraction of suppliers that are also exposed to the minimum wage.[33]

V. Robustness Analysis and Threats to Identification

Robustness.—We examine the robustness of the results to controlling for industry fixed effects, alternative sample selection, and an alternative measure of exposure to the minimum wage. We report the details in online Appendix Tables A7 and A8, but we summarize the most important findings here.

Controlling for 151 three-digit industry dummies in equation (1) has a small impact on our estimates. The medium-term employment elasticity with respect to the minimum wage is −0.19 (SE 0.04) versus −0.23 (SE 0.03) in our benchmark case. The revenue increase is even more prominent when we partial out industry wide shocks, while the profit reduction is smaller. As a result, nearly 100 percent of the incidence falls on consumers once we control for industry fixed effects.[34]

The medium-term employment elasticity with respect to the cost of labor is −0.26 (SE 0.03) for all industries including agriculture, highly regulated industries, and the government sector. When we include small firms the employment elasticity is somewhat smaller (−0.16, SE 0.03) than our benchmark estimate, which reflects that these firms tend to be operating in the local non-tradable sectors.

We also explore using alternative measures of exposure to the minimum wage. We calculate the GAP measure, which is the average wage increase that is needed to comply to the 2002 minimum wage (Card and Kruger 1994; Machin, Manning, and Rahman 2003). Similarly to our fraction affected measure, we first estimate relationship between the GAP and average wage on the sample of firms in the Structure of Earnings Survey (SES) and then predict the GAP measure for all firms. The medium-term elasticity estimate using GAP is −0.19 (SE 0.03) which is very close to the benchmark estimate of −0.22.[35]

Entry Rate.—A potential problem with our firm-level estimates is that we can only define the exposure to the minimum wage for the firms that existed before the minimum wage hike. As a result we dropped new entrants from the sample, which can potentially bias our estimates on employment. However, online Appendix Table A9 and Figure A10 show no indication of a drop (or an increase) in the industry-level entry rate at highly exposed industries relative to industries with less exposure.

[33] The larger predicted revenue effect might reflect a fall in markup that is not allowed if consumers face the standard constant elasticity of substitution (CES) demand function. In the online Appendix we estimate the model with falling markup and we show that the model performance improves with a 70 percent pass-through, though in that case the model underpredicts the actual price changes in the manufacturing sector.

[34] Since using industry fixed effects might also discard some valid identifying information, and also rules out potential changes in the industry structure of the economy, we put more faith in the estimates without industry fixed effects when we discuss incidence of minimum wages.

[35] The GAP estimates from 2002 also allow to assess the extent of within firm-level spillover effects of the minimum wage. The point estimate on wages in 2002 is 1.23 (SE 0.03), which points to substantial spillover effects within firm.

Threats to Identification.—A key identification assumption throughout the paper is that workers and firms with no direct exposure to the minimum wage are unaffected by the minimum wage, the so-called Stable Unit Treatment Value Assumption (SUTVA). There are several reasons why we think this assumption holds in our case. First, even if the minimum wage bites deep into the wage distribution, the minimum wage workers only represent a small fraction of the economy. In our case around 17 percent of the workers are directly exposed to the minimum wage, and their share in the total wage bill is 5.6 percent. Given that one-third of production is related to capital and two-thirds to labor, the cost share of aggregate production hit by the minimum wage is 3.7 percent. This limits the general equilibrium effects of the minimum wage and the potential impacts on the untreated population. Second, any wage or price effect that affects every firm in the same way will be absorbed by changes in the nominal exchange rate in a small open economy. This limits the real consequences of price spillovers on untreated firms.

We also assess the potential violation of the SUTVA assumption empirically. First, we point out that the robustness of the employment estimates to including detailed industry dummies suggests that cross-industry spillover play little role. Second, in online Appendix Section A.1, we show that untreated firms did not behave unusually after the reform: the post-reform employment change at the untreated firms (between 2000 and 2002) was very similar to the pre-reform change (between 1998 and 2000).

Bunching.—The firm-level employment results might overstate the worker-level effects if some workers who are laid-off find jobs at other, less exposed firms. Moreover, the firm-level results might understate the employment consequences as they do not take into account changes in firm entry. While we do not find evidence for changes in entry behavior, we can address these concerns by assessing worker-level employment changes directly. We first examine the evolution of the frequency distribution of monthly earnings over time.[36] Panel A of Figure 7 shows the earnings distribution in 2000 (the last year before the minimum wage hike) and in 2002 (two years after the reform).[37] To normalize the job counts we report the frequencies relative to the total employment in 2000. The logarithm of the minimum wage is raised from the level represented by the brown dashed line (10.1) to the red long-dashed line (10.55), representing a 0.45 log point increase in the minimum wage on the top of nominal GDP growth. This substantial increase in the minimum wage clearly altered the earnings distribution. First, jobs below the 2002 minimum wage disappeared from the earnings distribution, as expected when firms comply with the minimum wage. Second, in 2000 only a small spike was present at the minimum wage. In contrast, a much larger spike appears in the 2002 distribution. Third, we see that additional jobs emerged in the new earnings distribution at and above the new minimum wage.

Panel B shows the difference between the pre- and post-reform distributions. The missing jobs below the minimum wage and the excess jobs above the minimum wage

[36] We use the structure of earnings survey (SES) for this analysis. To ensure the data are consistent over time we restrict the analysis to firms that have at least 10 workers.

[37] To make the wage distributions comparable over time we adjust them by nominal GDP growth. We use the nominal GDP growth for adjustment, and not simply the CPI, because this wage adjustment was better able to match wage growth from the pre-reform years (1996–2000). Moreover, bargaining over wages in Hungary often determined by both expected inflation and expected real GDP growth.

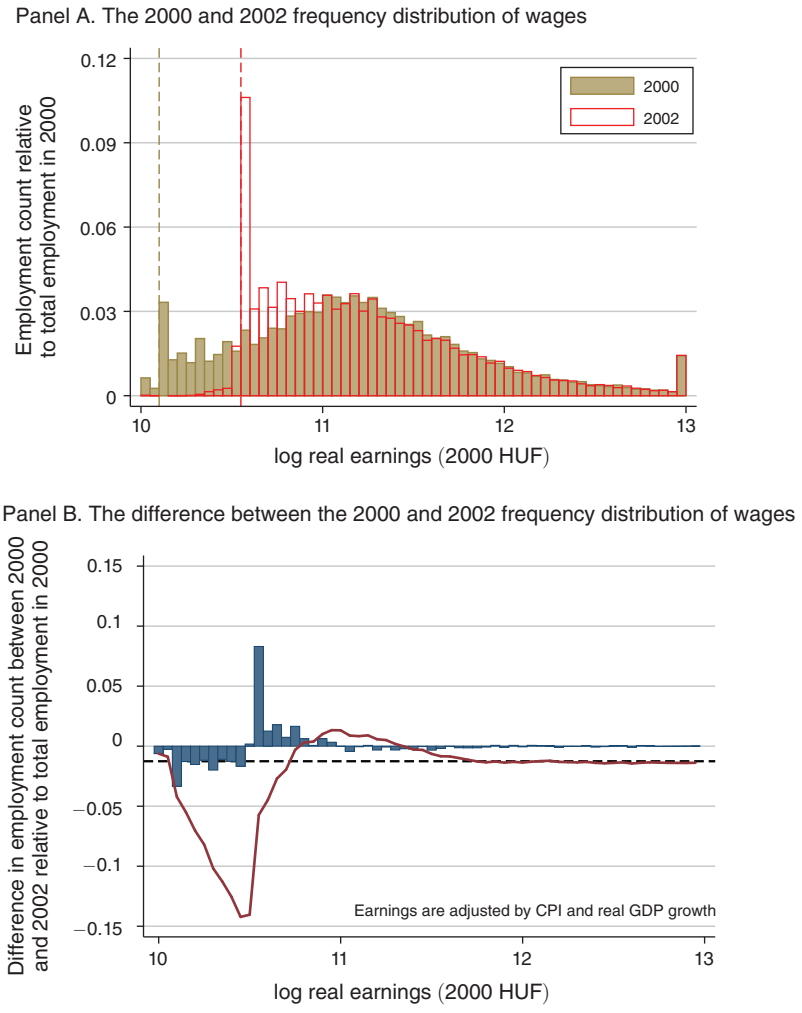

FIGURE 7. FREQUENCY EARNINGS DISTRIBUTION IN 2000 AND 2002

Notes: Panel A shows the frequency distribution of monthly log earnings in 2000 (last year before the minimum wage hike), and in 2002 (two years after the minimum wage hike). The red outlined bars show the earning distribution in 2002, and the brown solid bars show 2000. To make sure that wages are comparable over time we deflate the 2002 earning distribution by the nominal GDP growth. The dotted brown (red) dashed line is at the bar in which the minimum wage is located in 2000 (2002). Panel B depicts the difference between the two wage distributions shown in panel A for each wage bin. The red solid line shows the running sum of employment changes up to the wage bin it corresponds to. The dashed horizontal lines shows the value where 10 percent of the directly affected jobs is destroyed. In both panels we express the number of jobs in terms of year 2000 total employment.

are quite clear. We also report the running sum of employment changes up to each wage bin (red line). The running sum drops to a sizable, negative value just below the new minimum wage, which reflects around 15 percent of pre-treatment employment. The running sum then increases at and above the minimum wage and it goes close to zero before it falls again. Then it converges to a point where 10 percent of the directly affected jobs are destroyed.[38] This is very close to the benchmark firm-level

[38] This point of convergence is around seventy-fifth percentile of the wage distribution, which is very close to what Engbom and Moser (2018) found in Brazil, but substantially larger than recent estimates from the literature

estimates in Table 2 where we found that 10 percent of the jobs destroyed by the minimum wage.

Grouping Estimates.—To provide further evidence on worker-level employment we also implement a grouping estimator in the style of Blundell, Duncan, and Meghir (1998) in online Appendix Section A.2. We assign people to mutually exclusive groups formed from combinations of seven regions, age in five categories, gender, and education. We estimate the relationship between group-level exposure to the minimum wage and the employment to population rate. Our estimates on the implied elasticity with respect to the average wage are in line with the benchmark firm-level estimates, which suggests that our results are robust to using alternative identification strategies.

VI. Discussion and Conclusion

This paper provides a comprehensive assessment of the margins of adjustment used by firms in response to a large and persistent increase in the minimum wage. Most firms responded to the minimum wage by raising wages instead of destroying jobs. Our estimates imply that out of 290,000 minimum wage workers in Hungary, around 30,000 (0.076 percent of aggregate employment) lost their job, while the remaining 260,000 workers experienced a 60 percent increase in their wages. As a result, firms employing minimum wage workers experienced a large increase in their total labor cost which was mainly absorbed by higher output prices and higher total revenue. We also estimated that firms substituted labor with capital and their profits fell slightly. These results suggest that the incidence of the minimum wage fell mainly on consumers.

Given the relatively small effect on employment, our results also suggest that minimum wages can redistribute income from consumers to low-wage workers without large efficiency losses. Our findings also indicate that the optimal level of the minimum wage is likely to vary across industries, cities, and countries. In countries where low-wage jobs are concentrated in the local service sector (such as Germany or the United States) raising the minimum wage is likely to cause limited disemployment effects or efficiency losses. Moreover, in cities where mainly rich consumers enjoy the services provided by low wage workers this redistribution will be from rich to poor. The heterogeneous responses across industries also underline the advantages of sector-specific minimum wage polices used in some European countries such as Italy or Austria. For instance, setting a higher minimum wage in the non-tradable sector than in the tradable sector can push up wages relatively more where it will generate more modest disemployment effects.

using difference-in-differences style estimators (see, e.g., Cengiz et al. forthcoming; Brochu et al. 2017; Autor, Manning, and Smith 2016). Nevertheless, the point of convergence is quite sensitive to the way we adjust the wage distribution over time. For instance, if we adjust the wage distribution by the nominal GDP growth plus 2.5 percent, then the before and after distributions converge at the twenty-fifth percentile (and not at the seventy-fifth percentile as with our benchmark adjustment) where 10 percent of the directly affected jobs are destroyed. Therefore, even if our estimates on the point of convergence are quite sensitive to the particular wage adjustments we use, the employment estimates are unaffected.

Finally, we present new evidence for the key elasticities between low wage workers and other inputs such as capital and intermediate goods. These parameter estimates can be used to evaluate other polices that affect the cost of labor such as taxes and subsides. Our results also suggest that these policies can induce sizable responses in the exporting and in the tradable sectors.

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