# Trade Liberalization and Labor Market Institutions*


Leonardo Baccini
*McGill University*

Mattia Guidi
*University of Siena*

Arlo Poletti
*University of Trento*

Aydin Yildirim
*World Trade Institute*



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**Abstract**

While the firm-level distributional consequences of market liberalization are well understood, previous studies have paid only limited attention to how variations in domestic institutions across countries affect the winners and losers from opening up to trade. We argue that the presence of coordinated wage bargaining institutions, which impose a ceiling on wage increases, and state-subsidized vocational training, which creates a large supply of highly skilled workers, generate labor market frictions. Upward wage rigidity, in particular, helps smaller firms weather the rising competition and increasing labor costs triggered by trade liberalization. We test this hypothesis using a firm-level dataset of European Union countries, which includes more than 800,000 manufacturing firms between 2003 and 2014. We find that, for productive firms, gains from trade are 20 percent larger in countries with liberal market economies than they are in coordinated market economies. Symmetrically, less productive firms in coordinated market economies experience significantly lower revenue losses compared to liberal market economies. We show that both the presence of an institutionalized wage ceiling and the availability of subsidized vocational training are key mechanisms for reducing the reallocation of revenue from unproductive to productive firms in coordinated market economies compared to liberal market economies. In line with our theory, we find that wages and employment in liberalized industries increase differentially across both types of labor markets. Finally, we provide suggestive evidence that trade liberalization triggers a differential demand for redistribution at the individual level across different labor markets, which is in line with our firm-level analysis.



***Keywords:*** trade liberalization, trade agreements, labor institutions, heterogeneous firms, wage.

***JEL Classification:*** F13, F14, F16.



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# 1 Introduction

The notion that open international markets make societies better off has been increasingly contested in recent years in many advanced economies. Central to these discussions of the merits of trade liberalization is a widespread popular perception that globalization has generated greater wealth for a small group of individuals and firms but has made most citizens worse off. These perceptions are in line with recent research in international trade that has found that the benefits of trade liberalization are indeed highly concentrated in the hands of a few "superstar" exporting firms. These studies show that firm-level differences in size and productivity can account for their divergent export performance, which in turn explains why trade liberalization can unevenly reallocate profits across firms.[1] Thus, critics of globalization in the political arena and the recent literature both depict trade liberalization as a policy choice that concentrates wealth in the hands of the few, at the expense of the many.[2]

At the same time, there are significant differences in the extent to which protectionist sentiments increase and affect the political debate across countries. While in some countries popular concerns over the welfare effects of trade liberalization are widespread and have generated marked protectionist responses by elected representatives, opposition to trade liberalization has been much less intense in other countries. This observation underscores the importance of assessing if, and eventually how, domestic institutional factors influence the distribution of the welfare effects of trade liberalization in different societies. The comparative political economy literature has long noted that different domestic institutional setups can affect the distributive consequences and politics of trade in systematic ways.[3]

This paper contributes to the expanding literature on firm heterogeneity and trade politics by incorporating domestic institutional differences into the analysis of the competitive dynamics generated by trade liberalization. We build on Melitz's (2003) model of international trade regarding the determinants of the effect of trade liberalization on firms' performance, and on Iversen and Soskice's (2010) model regarding institutional differences between liberal market economies (LMEs) and coordinated market economies (CMEs). The joint effect of coordination in wage bargaining (which tends to equalize wages within sectors) and state-subsidized vocational training (which creates a large supply of highly skilled workers) in CMEs leads to *wage compression* – i.e., the difference between wages in high- vs. low-productivity firms is smaller in CMEs than in LMEs. When trade liberalization kicks in, an implication of CMEs' commitment to wage compression is upward wage rigidity, even in the case of an expansion of labor demand due to increasing exports. Lower wages imply lower production costs, which help keep unproductive firms more competitive in CMEs than in LMEs. In turn, this generates a reallocation of revenue from the least to the most productive firms, which is weaker in CMEs than in LMEs.

We test our argument using the Amadeus firm-level dataset on European Union (EU) countries, which includes information on more than 800,000 manufacturing firms between 2003 and 2016. Since

[1] Baccini et al. 2017; Bernard et al. 2003; Kim 2017; Melitz 2003; Osgood et al. 2016.

[2] Kim and Osgood 2019; Osgood 2018.

[3] Betz 2017; Hays 2009; Kono 2009; Katzenstein 1985; Milner and Kubota 2005; Rogowski 1987.

Amadeus does not include the entire universe of EU firms, we pay particular attention to ensuring that sampling issues do not affect our empirical strategy. To measure the occurrence of trade liberalization, we rely on *de jure* tariff cuts implemented by the EU with trade partners in all preferential trade agreements (PTAs) signed after 1995. Our identification strategy boils down to a triple difference-in-differences estimation in which the distributional effect of firms' productivity and tariff cuts varies across labor market institutions. Importantly for our purposes, both LMEs and CMEs face the *same* preferential tariffs, which helps mitigate concerns about the endogeneity of tariff cuts. Since countries differ with respect to labor market institutions *and* several other characteristics, we control for a large number of potential confounders – such as compensation policies, welfare, access to credit, innovation, and migration flows – in interaction with productivity and tariff cuts in order to identify the effect of wage bargaining systems.

We find that, for productive firms, gains from trade are 20 percent larger in countries with liberal market economies (e.g. the United Kingdom) than they are in coordinated market economies (e.g. Germany). Moreover, we show evidence of the mechanisms highlighted in our theory: (1) the presence of both an institutionalized wage ceiling and subsidized vocational training reduce the reallocation effect from the least to the most productive firms after trade liberalization; (2) wages increase significantly more in LMEs than in CMEs as a result of trade liberalization; (3) employment in the liberalized industries increases more in CMEs than LMEs after trade liberalization due to the over-supply of skilled workers.

We complement our firm-level analysis with suggestive individual-level evidence using European Social Survey (ESS) data and a novel geographical measure of trade liberalization weighted on the share of workers employed in unproductive firms, which we geo-located at the level of EU regions. By exploiting the heterogeneous impact of trade liberalization across European regions, we show that preferential liberalization generates a weaker demand for redistribution in CMEs compared to LMEs, given that the gains from trade are more uniform in the presence of wage rigidity. Importantly, this effect is driven by low-educated individuals, who are the likely losers from trade openness in developed economies. In line with the firm-level analysis, we show that our results hold even when we account for other characteristics – e.g., the size of the welfare system – that correlate with labor market institutions.

Our paper contributes to several lines of research. First, a number of empirical articles have documented selection and market share reallocation effects caused by trade liberalization.[4] Recent studies have pointed out that a few large productive firms enjoy the lion's share of the benefits from trade liberalization at the expense of smaller, less productive firms.[5] We show that domestic institutions affect gains from trade, and that labor market frictions make the benefits from trade liberalization more uniform across firms.

Second, the paper adds to a large literature explaining support for globalization in general, and

[4]Amiti and Konings 2007; Pavcnik 2002; Topalova and Khandelwal 2011.

[5]Baccini et al. 2017; Osgood et al. 2016.

trade liberalization in particular.[6] Recent studies have found that trade shocks enhance support for economic nationalism and populism among the losers of globalization.[7] Our firm-level analysis explains the economic micro-foundations of how these policy preferences are formed, by identifying the heterogeneous effects of trade liberalization among firms operating in different types of labor markets.

Third, our paper speaks to the large and important literature on embedded liberalism. Starting with a number of seminal works highlighting the connection between trade openness and government spending for redistributive policies, a flurry of political economy research has investigated the micro-level foundations of this relationship over the years.[8] The key insight of this stream of research is that compensating the losers of globalization helps increase support for the trend.[9] Our findings indicate that domestic institutions, and labor market institutions in particular, complement compensation policies designed to mitigate the backlash against globalization within developed democracies.

Finally, we contribute to the Varieties of Capitalism (VoC) literature[10] by demonstrating how differences in labor market arrangements affect the distributive consequences of trade liberalization. While a large number of prior studies in this literature have noted the significance of various domestic market institutions through which firms arrange their operations, our study is the first to apply these insights to a firm-level analysis of the distributive consequences of trade liberalization and to explore cross-country variations in individual-level responses to trade liberalization.

## 2 Theory

Our theory examines the dynamics linking trade liberalization, productivity, wages and labor market institutions. Our argument builds on two established strands of literature. The first is the recent wave of studies highlighting the relevance of firms as the unit of analysis in explanations of trade policy.[11] These studies highlight how firm-level characteristics can account for their heterogeneity in export performance, why trade liberalization can unevenly reallocate profits across firms, and the growing relevance of firm-level lobbying over trade policy.[12]

Second, we build on the VoC literature, for which the presence (or absence) of strategic coordination mechanisms between firms and employees explains most differences between advanced capitalist

[6] Hainmueller and Hiscox 2006; Mansfield and Mutz 2009; Margalit 2012; Mayda and Rodrik 2005; Owen and Johnston 2017; Scheve and Slaughter 2004; Walter 2010; Walter 2017.

[7] Ballard-Rosa et al. 2017; Colantone and Stanig 2018a, 2018b; Jensen et al. 2017; Margalit 2011.

[8] Cameron 1978; Katzenstein 1985; Rodrik 1998; Ruggie 1982.

[9] Gingrich 2019; Hays et al. 2005; Hays 2009; Margalit 2011; Rickard 2015; Richtie and You 2020; Rudra 2005.

[10] Dean 2016; Hall and Soskice 2001b; Hancké et al. 2007; Iversen and Soskice 2010; Thelen 2012.

[11] Madeira 2016.

[12] Baccini et al. 2017; Bernard et al. 2012; Kim 2017; Osgood et al. 2016.

countries, including their comparative advantage and trade profiles.[13] This literature is based on the conceptual distinction between LMEs, in which "firms coordinate their activities primarily via hierarchies and competitive market arrangements," and CMEs, where firms "depend more heavily on non-market relationships to coordinate their endeavors with other actors and to construct their core competencies."[14] These varieties are characterized by different institutional complementarities; we focus here on cross-country differences in (1) wage bargaining institutions and (2) skill formation and training.

## 2.1 Assumptions

Building on these two streams of research, we make three assumptions. First, only the most productive firms are exporters. Based on the seminal work of Melitz (2003), a number of contributions have shown how firm-level differences in size and productivity can account for heterogeneity in export performance.[15] Exporters face trade costs, including the fixed costs of distribution and servicing, as well as variable costs such as transport, insurance, fees, and tariffs. Only the most productive firms can afford to sustain the fixed and variable costs associated with accessing foreign markets and still profit from trade.[16]

Our second assumption is that CMEs rely on coordinated wage bargaining while LMEs do not. In CMEs, coordinated wage bargaining institutions (e.g., trade unions, employer associations, and government agencies) can strike and enforce wage deals for all firms operating in a sector.[17] This implies that wages and wage caps are imposed on entire industries in CMEs, whereas workers bargain for wages in a decentralized manner at the plant level in LMEs.[18]

Our third assumption is that CMEs ensure a high supply of highly skilled workers due to the presence of publicly-subsidized vocational training systems supervised by employer associations and trade unions. These publicly-subsidized vocational training systems are largely missing in LMEs.[19].

Armed with these assumptions, we first describe differences in the labor market between CMEs and LMEs. We then introduce trade liberalization, explaining how gains from trade differ in the two types of economies.

[13] Hall and Soskice 2001a; Hancké et al. 2007; Manger and Sattler 2019.

[14] Hall and Soskice 2001a, 8.

[15] Bernard et al. 2003; Kim 2017; Melitz and Ottaviano 2008; Osgood et al. 2016.

[16] Bernard et al. 2012.

[17] Iversen 1999; Hopner and Lutter 2014; Pontusson et al. 2002; Rueda and Pontusson 2000; Thelen 2009.

[18] Addison et al. 2007; Hall and Gingerich 2009; Hall and Soskice 2001; Iversen and Soskice 2010.

[19] Iversen and Soskice 2010; Thelen 2004; Thelen 2012.

## 2.2 The labor market in CMEs and LMEs

Iversen and Soskice[20] argue that CMEs have higher wage compression than LMEs as a result of the combined effect of coordinated wage bargaining and state-subsidized training of a skilled workforce. Wage compression means that there is a relatively small gap between wages paid by productive firms to skilled workers and wages paid by unproductive firms to unskilled workers. Unlike Iversen and Soskice, we are not interested in analyzing differences between sectors (traded vs. non-traded), but differences between firms *within* the same sector. We posit that wage compression is different between CMEs and LMEs in the same sector populated by firms with heterogeneous productivity, heterogeneous workers' skills, and therefore heterogeneous wages. It is well documented that manufacturing – the traded sector *par excellence* – includes firms with different levels of productivity in every economy.[21]

Two mechanisms yield the above-mentioned differences between LMEs and CMEs. First, institutions that coordinate wage bargaining tend to equalize wages within sectors, and to produce deals at the industry level that grant moderate (and in any case predictable) wage increases.[22] The second mechanism is that state-subsidized vocational training provides a large supply of very skilled workers to highly productive firms in order to contain shop floor pressure against wage increases.[23]

The effect of these two institutions goes in the same direction. Both wage coordination and vocational training compress the wages of highly skilled workers employed in productive firms, and simultaneously generate upward pressure on the wages of less skilled workers employed in less productive firms.[24] Since these institutions are typical of CMEs but not of LMEs (by assumption), differences in wages between productive and unproductive firms in CMEs are less pronounced than in LMEs. This means that the wages of workers employed in the most productive CMEs should be relatively lower than those of workers employed in firms with similar levels of productivity in LMEs. On the contrary, the wages of workers employed in less productive firms should be relatively higher in CMEs than in LMEs.

One implication of this wage compression is that CMEs have a higher degree of wage rigidity than LMEs.[25] On the one hand, wage compression is assured by wage coordination, which entails well-

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[20] Iversen and Soskice 2010.

[21] See Bernard et al. 2012 and Kim and Osgood 2019 for a thorough overview.

[22] Franzese and Hall 1999; Hall and Soskice 2001; Hall and Gingerich 2009; Hopner and Lutter 2014; Iversen 1999; Iversen and Soskice 2010; Manger and Sattler 2019; Pontusson et al. 2002; Rueda and Pontusson 2000. Differently from Iversen and Soskice (2010), we relax the assumption that CMEs equalize wages between the traded and non-traded sectors, i.e. throughout the economy. Since we only examine variation within manufacturing, it is enough for us to assume that coordination in wage bargaining takes place within each manufacturing industry.

[23] Culpepper and Thelen 2008; Estevez-Abe et al. 2001; Iversen and Soskice 2010; Iversen and Stephens 2008.

[24] Iversen and Soskice 2010.

[25] Babecký et al. 2009; Franz and Pfeiffer 2006; Holden and Wulfsberg 2007. Although VoC scholars do not use the concept of wage rigidity, they all agree that CMEs are much better than LMEs at moderating wages (see Hall and

defined wage settings negotiated by trade unions and business associations. Wages are not permitted to move above or below these levels, at least in the short term. By creating upward wage rigidity, caps on skilled workers' wages help exporters maintain their competitiveness abroad. On the other hand, subsidized vocational training reduces the cost of acquiring skills, which in turn creates upward rigidity due to an over-supply of skilled workers and downward rigidity due to a shortage of unskilled workers (assuming a constant pool of labor). In sum, upward rigidity reduces the cost of labor for exporters, which is the main goal of wage compression.[26]

The dynamics are quite different in LMEs, where the two mechanisms described above (wage coordination and vocational training) are absent; thus wages are more dispersed. Without any form of coordination, wages are free to move in response to market forces. Thus, if a number of firms need a larger number of, say, skilled workers, their wages go up, thus increasing wage dispersion. Moreover, absent publicly subsidized vocational training, the pool of skilled workers is more limited in LMEs than in CMEs. As such, accommodating any shortage of skilled workers and committing to specific wage settings is more difficult in the former than in the latter, especially in the short term. In sum, wages are flexible in LMEs for two main reasons: (1) there is no commitment to wage compression and (2) labor dynamics are determined almost entirely by market forces rather than institutions.

### 2.3 The Effect of Trade Liberalization in CMEs and LMEs

In this section we use Melitz (2003) to explore how CMEs and LMEs respond differently to trade liberalization because of their different labor market institutions. The Melitz model describes an economy in which firm survival, firm profitability, and firms' decisions to export all depend on a single firm characteristic: productivity. Remember that, by assumption, exporting firms are more productive than those that only serve the domestic market. More specifically, the Melitz model identifies two productivity cutoffs in every economy: a domestic market productivity cutoff, $\varphi(d)$, and a foreign market productivity cutoff, $\varphi(x)$, with $\varphi(d) < \varphi(x)$ due to the variable and fixed costs of trade discussed above. Firms with a level of productivity higher than $\varphi(d)$, but lower than $\varphi(x)$, serve only the domestic market. Those with productivity higher than $\varphi(x)$ serve both the domestic and foreign markets. Firms with productivity less than $\varphi(d)$ do not survive in the market and exit. The higher these cutoffs, the fewer firms that are able to survive and to export; thus the remaining firms have higher revenues and profits.

When export tariffs decrease, the variable costs of trade go down as well. This implies that $\varphi'(x) < \varphi(x)$, where $\varphi'(x)$ is the productivity cutoff for exporting *after trade liberalization*. This generates two effects. First, new firms, which were below the foreign market productivity cutoff before trade liberalization, are now able to access foreign markets, as the productivity cutoff is lower (i.e. the extensive margins of trade increase). Second, firms that were already exporting before trade liberalization increase their sales in foreign markets, because lower tariffs allow them to charge more

Soskice 2001, 5). Also, see Culpepper (2008) on how coordinated wage bargaining is mainly aimed at wage moderation.

[26] Iversen and Soskice, 2010.

competitive prices (i.e. the intensive margins of trade increase).

The entry of new firms into foreign markets and the increase of sales by firms that were already exporting raise the general demand for inputs, particularly labor. In turn, this increased demand for labor pushes wages up (and thus raises the cost of labor for firms). Only the most productive firms, which sell large amounts of goods in both domestic and foreign markets, can afford to recoup these rising costs of labor and to keep prices low. Less productive firms are unable to sustain the increasing costs without charging higher prices, which in turn leads them to reduce their sales to the advantage of more productive firms (*reallocation effect*) or, at the extreme, to exit the market (*selection effect*). Thus, through the labor market channel, a reduction in export tariffs leads to a higher domestic market productivity cutoff, i.e. $\varphi(d) < \varphi'(d)$, with $\varphi'(d)$ serving as the domestic market productivity cutoff *after trade liberalization*. The higher the increase in wages, the higher $\varphi'(d)$ is and the larger the distributional consequences of trade liberalization across firms are.[27]

While this effect is at play in both CMEs and LMEs, we expect that they diverge in the extent to which wages are able to rise after trade liberalization due to the differences in their labor market rigidities discussed above.

Figure 1 illustrates this argument. A reduction in the variable costs of trade reduces the foreign market productivity cutoff, increasing the number of exports and the total sales in foreign markets.[28] This increase raises the demand for labor, which shifts to the left, creating an upward pressure on wages (Panel A in Figure 1). At this point, CMEs and LMEs diverge. In LMEs, wages are free to move and labor market frictions are minimal; wages thus rise to the level $W'$. In CMEs, wages are capped at $\overline{W}$ due to the presence of wage coordination and collective bargaining among all firms operating in a given industry. Moreover, the presence of government-subsidized vocational training generates a surplus of skilled workers, which allows the labor supply to expand, shift to the left, and (partially) offset the wage increase generated by the growing labor demand.[29] This combination of institutional

[27]Another possible channel triggering distributional effects across firms is an increase in product market competition resulting from foreign firms accessing the domestic market. Since these competitors are exporters (more productive than domestic firms), they are able to charge lower prices for similar goods, thus eroding unproductive domestic producers' market share. We note that this channel is not operational in Melitz (2003), since his model does not allow direct competition in the same line of products due to his monopolistic characterization of the economy.

[28]The foreign market productivity cutoff should be smaller for CMEs than LMEs, given the lower wages of high-skilled workers, which makes exporters more competitive in CMEs than LMEs. For simplicity, we normalize this cutoff so that it is the same for CMEs and LMEs; this does not affect the logic of our argument.

[29]We conservatively report a single labor supply for both CMEs and LMEs. Yet in practice, CMEs are likely to have a flatter (i.e. more elastic) labor supply curve (i.e., relatively small wage increases are sufficient to accommodate the increasing labor demand) and LMEs a steeper, less elastic labor supply curve, since in the absence of strategic coordination, wages are free to increase as the demand for labor grows (Iversen and Soskice 2010, 609). If this were the case, for the same increase of labor demand, wages would rise more in LMEs than CMEs, an outcome in line with our argument.

devices embedded in the labor market helps adjust the expansion of labor demand and keep wages at the negotiated level $\overline{W}$, with $\overline{W} < W'$. In sum, wages increase less in CMEs than in LMEs after trade liberalization.[30]

Note that more workers are employed in CMEs than in LMEs after trade liberalization, i.e. $L'_{CME} > L'_{LME}$. These workers are disproportionally employed in exporting firms, which experience an expansion of economic activities due to a reduction in the variable costs of trade. This result is consistent with Iversen and Soskice (2010), who argue that the traded sector is larger in CMEs than in LMEs due to wage compression.

These differences in how the labor market adjusts to a rise in demand has important distributional consequences for firms. Figure 1 illustrates that the relationship between wages and revenues is negative for unproductive firms (Panel B) and positive for productive firms (Panel C); the slope is a function of firm productivity. Melitz (2003) finds that these relationships hold in equilibrium, i.e. after the economy adjusts to trade liberalization. Since we assume that a firm's ability to export is a function of its productivity, for simplicity's sake, Panel B can be considered to represent the impact of trade liberalization on the average firm that serves the domestic market only. Panel C represents the impact of trade liberalization on the average exporting firm.[31]

What happens to the revenues of domestic firms given the differences in wage increases between CMEs and LMEs? As illustrated in Panel B of Figure 1, we expect a negative relationship between wages and revenues in domestic firms. These firms suffer from wage increases "imposed" by productive exporters. They have to raise their wages to retain employees and to avoid job poaching, but to remain profitable they must also raise prices, which ultimately leads to a decrease in their revenues. Although this happens in both CMEs and LMEs, our model predicts that the loss of revenue for domestic firms is lower in the former, since the increase in wages is smaller for them.[32] This is represented by the difference between $R^*(d)$ and $R'(d)$, which is lower in CMEs than in LMEs.

For exporting firms, the situation is the opposite (see Panel C of Figure 1): their increased demand for labor drives wage increases. When they expand their workforce, exporting firms increase their sales in both the domestic market (at the expense of less productive firms) and foreign markets, which is why they have a positive relationship between wages and revenues. Again, our model suggests that

[30] While we allow wages to be the same in CMEs and LMEs before trade liberalization, the initial level of wages are different in practice due to wage compression in CMEs. However, our argument does not depend on this initial difference in wages. What matters is that the difference between pre- and post-trade liberalization is smaller in CMEs than LMEs, regardless of wage levels.

[31] In practice, there are firms with different levels of productivity among both CMEs and LMEs, and in both the domestic and foreign markets. Figure B1 in Appendix B shows different curves for different levels of firm productivity.

[32] In Melitz (2003), unproductive firms' wages increase proportionally with the rise in productive firms' wages in both CMEs and LMEs, because the model assumes workers are homogenous. Further developments of the New New Trade Theory model workers' heterogeneity (Helpman et al. 2010), which does not change the main empirical implications of our theory.

**Figure 1:** The effect of trade liberalization in CMEs and LMEs: domestic vs. exporting firms

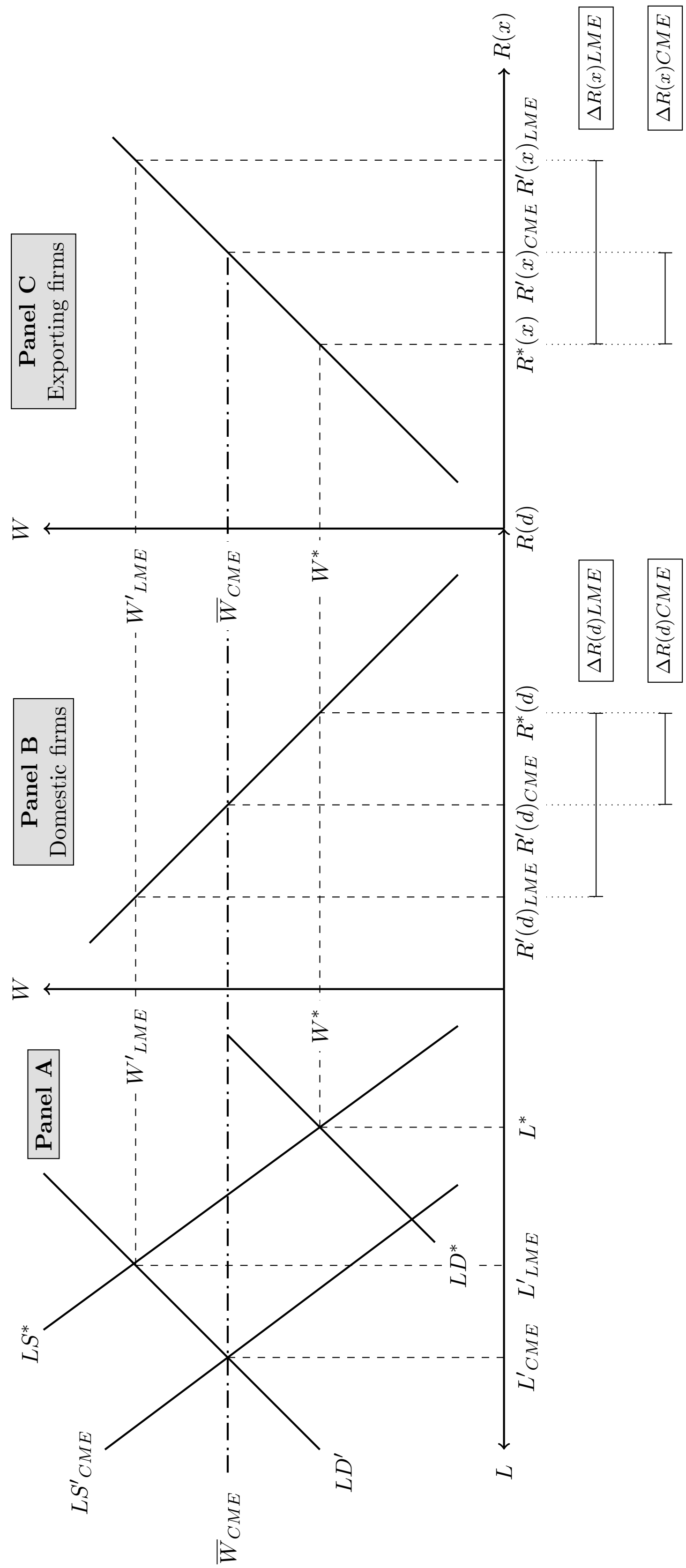
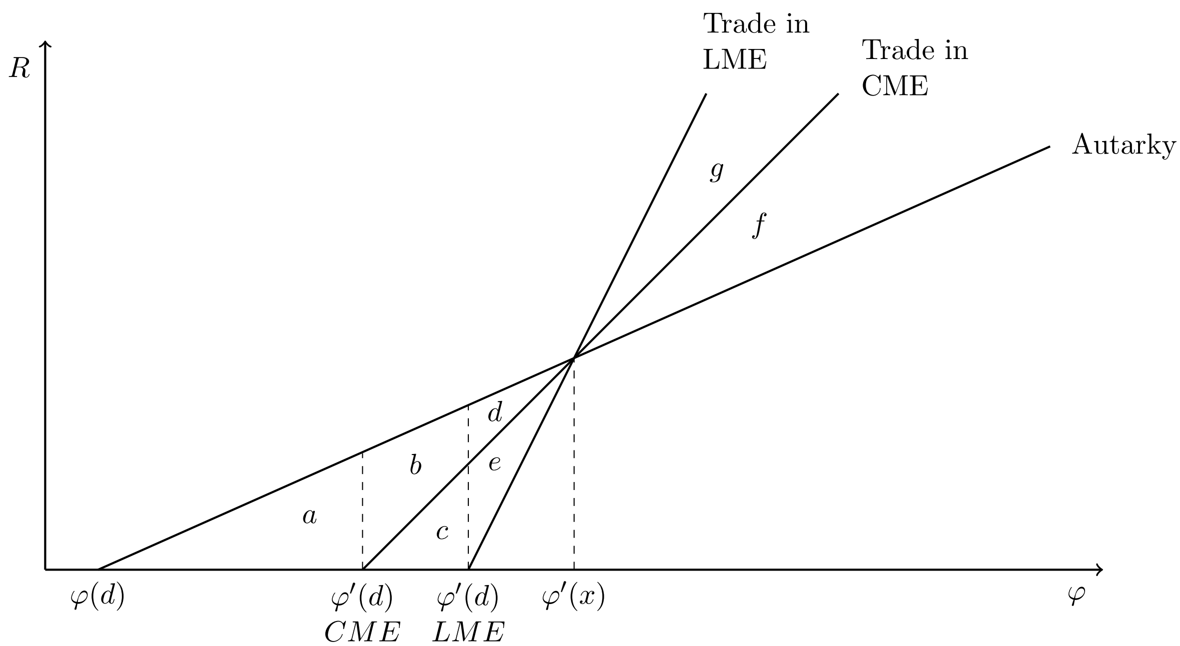


**Figure 2:** Selection and reallocation effect in CMEs and LMEs




the consequences of a wage increase are different in LMEs and CMEs. In LMEs, where wages can be raised freely, domestic firms experience a substantive reduction in their sales or are forced to exit the market altogether; thus few productive exporters experience a large increase in their revenues after trade liberalization. In CMEs, this dynamic is mitigated by domestic institutions that limit wage rises. As a result, the increase in revenues for exporters is lower in CMEs than in LMEs.

Building on Melitz,[33] Figure 2 summarizes the differences in selection and reallocation effects between CMEs and LMEs. Remember from Figure 1 that, since wages are more rigid in CMEs than in LMEs, they increase less in the former than in the latter due to trade liberalization, which mitigates the distributional consequences of tariff reductions. Figure 2 shows that, after trade liberalization, the domestic market productivity cutoff increases more in LMEs than CMEs. Moreover, the slope of trade liberalization is steeper in LMEs than in CMEs, meaning that shifts in the productivity cutoff to the right generate larger revenue increases for exporters in LMEs than in CMEs. This is because the higher increase in the cost of labor experienced by LMEs compared to CMEs reduces the number of unproductive firms more in LMEs than CMEs. Thus, for the same productivity cutoff, there are fewer firms in LMEs than in CMEs; for the surviving firms, revenues are higher in LMEs than in CMEs.[34]

This set-up affects both the selection and reallocation effects. The selection effect is weaker in CMEs than in LMEs, since the domestic market cutoff productivity after trade liberalization is lower

[33] Melitz 2003, 1715.

[34] As in Melitz (2003), the aggregate revenues are unchanged before and after trade liberalization. Thus, this exercise is only able to explain the distributional consequences of trade liberalization and not the aggregate welfare effect.

in CMEs than in LMEs ($\varphi'(d)_{CME} < \varphi'(d)_{LME}$). The selection effect impacts all firms placed in $a$ in CMEs, and all firms placed in $a + b + c$ in LMEs. Thus, there are more surviving firms in CMEs than in LMEs after trade liberalization. The reallocation effect is weaker in CMEs than LMEs. In CMEs, firms placed in $d$ survive trade liberalization, but experience losses, i.e. their revenue after trade liberalization shrinks compared to their revenue under autarky.[35] In LMEs, firms placed in $d+e$ experience losses from trade, i.e. their revenue after trade liberalization is lower than under autarky. Therefore, for the same level of productivity, losers from trade liberalization that remain in the market experience larger reductions in revenue in LMEs than they do in CMEs.

On the contrary, firms placed in $f$ experience gains from trade in CMEs, i.e. their revenue after trade liberalization increases compared to their revenue under autarky. In LMEs, all firms placed in $f+g$ experience gains from trade. Thus, for the same level of productivity cutoff, winners score higher revenue in LMEs compared to CMEs after trade liberalization (all firms placed in $g$). Combining these two effects, Figure 2 shows that the distributional consequences of trade liberalization are more severe in LMEs than in CMEs.[36]

In sum, we derive the following key empirical implication: *the reallocation effect between low- and high-productivity firms should be significantly higher in LMEs than in CMEs.* In other words, we expect that while revenues increase more for productive than for unproductive firms after trade liberalization, this effect is more pronounced in LMEs than in CMEs.

## 3 Data

We test our argument using a reduced-form approach. In this section we describe our sample and main variables.

**Sample** We test the empirical implications of our argument on a large number of firms from EU countries from 2003 to 2014. Firm-level data come from the Amadeus database provided commercially by the Bureau Van Dijk. The data-gathering process was performed following best practices in terms of downloading methodology and cleaning procedures.[37] Our baseline sample includes more than 800,000 manufacturing firms operating in the (up to) 28 EU countries. To analyze the distributional consequences of trade liberalization, our unit of observation is the firm-industry-country-year.

[35] In CMEs, firms placed in $c$ also experience shrinking revenues, but they would have exited the market in an LME. Thus, for these firms, the distributional effect of trade liberalization is more favourable in CMEs than in LMEs. Note that since more firms survive in CMEs compared to LMEs, the average revenue of domestic firms in the former should be lower than in the latter, as shown in Figure 2.

[36] According to Melitz (2003, 1711), $\varphi'(x)$ increases proportionally with $\varphi'(d)$, and so $\varphi'(x)$ should increase more for LMEs than CMEs. For simplicity, we keep $\varphi'(x)$ the same in both CMEs and LMEs, which does not affect the logic of our argument.

[37] Kalemli-Ozcan et al. 2017.

The key advantage of using this database is that it includes firms from all EU countries. This provides us with the variation in labor institutions that we need to test our argument. Moreover, it includes several firm-level characteristics, which is crucial for building our measure of productivity and other important controls. Furthermore, the database includes a large number of firms of different sizes and levels of productivity, which operate in many industries at the NAICS 4-digit level. This heterogeneity allows us to exploit variation across firms and across tariff cuts.

The Amadeus database also has three shortcomings. First, Kalemli-Ozcan et al.[38] show that there are significant changes in its coverage over time and across countries. Second, the dataset does not include the universe of firms in each EU country. It over-represents large, productive firms at the expense of small, unproductive firms. Third, the database does not systematically collect longitudinal firm-level data. Because the sample does not include the entire universe of firms, a firm $f$ may be present in 2006 but not in 2007, either because it exited the market or because it was not surveyed. Hence, our data are repeated cross sections.[39] We return to these points below.

**Dependent variable** Our dependent variable is the logarithm of the revenue of firm $f$ in industry $i$ in country $c$ in year $t$. We use this variable to proxy for the gains from trade, which allows us to quantify the distributional consequences of trade liberalization. According to Melitz (2003), revenue increases proportionally with firm productivity after tariffs are reduced (known as the reallocation effect). Other proxies capture the distributional consequences of trade liberalization. An obvious candidate would be firm exit, which captures the selection effect (i.e., productive firms should exit less frequently than unproductive firms after trade liberalization). However, our repeated cross-sectional data are not suitable for measuring firm exit. Another option would be to rely on profit rather than revenue. However, we opt for revenue because its coverage is substantively better in our data.

**Independent variables** We use the interaction between three independent variables to test our argument. First, to measure firm productivity we use a standard measure of total factor productivity ($TFPR$) using the Solow residuals. We calculate $TFPR$ for each firm-year by regressing the firm-level log of revenue on firm-level physical assets, employment, year, 4-digit industry, and country fixed effects. The residuals of this regression, which might also be negative, are our time-varying measures of firm productivity. We rescaled this variable so that it has only positive values.

The second independent variable is a measure of trade liberalization, which we constructed by creating an original dataset containing preferential tariff concessions made by EU partner countries in all PTAs signed between 1995 and 2014. For all PTAs, we extracted tariff schedules, each of which contains around 5,000 tariff lines at a highly disaggregated level. All PTAs contain at least two tariff schedules, one for the EU with its trade partner, and one for its trade partner with the EU. Our data

[38] Ibid.

[39] Financial data for surveyed companies are retained for a rolling period of 8 years. When a new year of data is added, the oldest year is dropped, meaning only the most recent data for each company are available.

are at the Harmonized Commodity Description and Coding System (HS) 6-digit level. To merge the tariff data with the Amadeus database, we use available crosswalks from HS 6-digit to NAICS 4-digit.

The data were compiled from two sources. We gathered tariff data for the year prior to the PTA's entry into force from the World Integrated Trade Solution (WITS) dataset, which relies on data reported by customs administrations. We then added information on tariff concessions from the officially negotiated tariff schedules listed in the appendices of the PTAs. Our tariff data enjoys significantly better coverage than that of the WITS, as documented in Baccini et al. (2018). Moreover, our tariffs are *de jure* and not applied, which should mitigate endogeneity concerns. We treat *de jure* tariffs as instruments for applied tariffs in reduced form. Moreover, all EU countries face the same tariffs. Thus, the endogeneity of tariff cuts would be an issue for us if and only if preferential tariff cuts are affected by the preferences of large productive firms in LMEs (e.g. Estonia or the UK), which anticipate gains from trade, more than by the preferences of large productive firms in CMEs (such as Belgium or Germany).

Our data include preferential tariffs (PRFs) from the entry into force of a PTA until the end of the implementation period, since not all tariffs go to zero in the year of ratification. In other words, we capture the phasing-out period for each product at the 6-digit level. Importantly, we collected data for the average tariff (most-favored nation, MFN) that existed before the entry into force of each PTA's agreement. That allows us to capture the tariff cut (i.e., MFN–PRF) implemented by EU trade partners in each 6-digit product for each year. In line with Melitz (2003) and our argument, we rely on export tariff cuts since they raise real wages by increasing exporters' demand for labor. We label this variable $\Delta\tau$.[40]

The third independent variable, *CME*, measures the degree to which wages are coordinated within an economy. This variable comes from the ICTWSS database[41] and is based on Kenworthy's (2001) index of coordination in wage bargaining. This variable measures "the degree of intentional harmonization observed in the wage-setting process"[42] – that is, the extent to which the rest of the economic actors follow the wage settings determined by the major players (peak-level union and employer confederations, unions, and employer associations of influential sectors, such as metal manufacturing).

The variable is ordinal, ranging from 1 ("Fragmented wage bargaining, confined largely to individual firms or plants"[43]) to 5 ("Maximum or minimum wage rates/increases based on centralized bargaining,"[44]); it captures the level of actual market competition between firms on salaries. In countries scoring 1 (e.g., the UK), each firm can freely increase salaries to attract workers. The more

[40] Appendix A explains how we build our measure of tariff reduction. Figures A1 and A2 show the distribution of tariff cuts by industry and over time.

[41] Visser 2016. Database on Institutional Characteristics of Trade Unions, Wage Setting, State Intervention and Social Pacts in 51 countries between 1960 and 2014, available at: `http://uva-aias.net/en/ictwss`.

[42] Ibid., 76.

[43] Ibid., 76.

[44] Ibid., 76.

a country has a wage-setting dynamic that limits this tendency (formally or informally), the more we expect the relocation effect to be constrained. For instance, Germany scores 4 in this variable, implying that "wage norms are based on centralized bargaining by peak associations with or without government involvement."[45] Since our argument implies a dichotomous distinction between CMEs and LMEs, our *CME* dummy takes a value of 0 if Visser's original variable scores 1 and a value of 1 if Visser's variable scores 2–5, which all imply some degree of wage coordination.

We include two additional variables from the ICTWSS database to test the mechanism. The first variable, *Wage Cap*, captures the mechanism related to the presence of a wage cap $\overline{W}$: it scores 1 if a country negotiates agreements that contain a norm or ceiling regarding maximum wage rises. The second variable, *Subsidies for VT*, is coded 1 if a country negotiates agreements containing concessions regarding employment policies that include subsidies for vocational training. This variable captures the second mechanism related to the presence of a surplus of skilled workers, which allow the labor supply to expand to keep wages low.

The key independent variable of interest is the triple interaction between firm productivity, tariff cuts, and coordinated wages. To test our mechanisms, we also interact *TFPR* and $\Delta\tau$ with *Wage Cap* and *Subsidies for VT*. As is customary, we also include the double interaction terms and each variable alone in our model specification, unless these terms are absorbed by the fixed effects. The correlation between the three terms of the interaction is always very low: $\rho \approx 0$.

# 4 Empirical Strategy

We use a triple difference-in-differences approach to identification. We compare the evolution of (the log of) revenue across industries and firms according to the degree of trade liberalization and firm productivity in countries with different labor market institutions. Firm productivity varies across firms, but does not vary over time. In other words, firms enter the dataset with a given level of productivity, which is assumed to be exogenous and remains constant.[46] Note that the distribution of firm productivity is remarkably similar across different labor market institutions (see Figures B2 and B3 in Appendix B).[47]

Tariff cuts vary across industries and over time, but not across countries, as all EU countries face the same preferential tariffs. $\Delta\tau$ distinguishes between treated (industries that face tariff cuts at some point) and control (those that never face tariff cuts) industries. In our setting, the intensity of the

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[45] Ibid., 76.

[46] Our results hold if we allow productivity to vary over time (results are available upon request). These models require accepting a further identification assumption: Firm productivity does not change differentially in countries with different labor market institutions as a result of trade liberalization, a point that we address below.

[47] These figures show that the large majority of firms included in Amadeus are large and thus productive. Small, unproductive firms are under-represented in the dataset, as demonstrated by the long tail on the left of the distribution. This implies that our models *underestimate* the reallocation effect.

treatment varies, since industries face different degrees of trade liberalization. $\Delta\tau$ also captures the difference between pre- and post-treatment effects, since tariff cuts vary over time. In our setting, the treatment (i.e., tariff cuts) occurs at different points in time for each industry; the same industry may experience several treatments (i.e., several tariff cuts) over time. The over-time variation comes almost exclusively from $\Delta\tau$ in our estimates.[48]

Furthermore, we let the variable *CME* vary across countries and over time. While there is no provision that forces EU countries to change their labour market regulations in PTAs and while these labour market institutions are sticky and hard to change, we concede that they may be affected by globalization, though the empirical evidence is not conclusive.[49] We note that relatively few countries changed labor market institutions during our time span. This third component of the triple interaction term provides us with different slopes of the combining effect of *TFPR* and $\Delta\tau$ on firm revenue across labor market institutions.

More formally, we estimate the following baseline model:

$$\begin{aligned} Revenue_{fict} = \beta_0 + \beta_1 TFPR_{fic} + \beta_2 \Delta\tau_{it} + \beta_3 TFPR_{fic} \times \Delta\tau_{it} + \beta_4 TFPR_{fic} \times CME_{ct} + \\ \beta_5 \Delta\tau_{it} \times CME_{ct} + \beta_6 TFPR_{fic} \times \Delta\tau_{it} \times CME_{ct} + \mathbf{X_{fict}}\gamma' + \mathbf{W_{ict}}\eta' \\ + \delta_{ct} + \delta_i + \epsilon_{fict}, \end{aligned} \tag{1}$$

where revenue is the dependent variable, and *TFPR*, $\Delta\tau$, *CME*, and their interactions are the main independent variables. $\beta_0$, $\beta_1$, ... $\beta_6$, $\gamma$, and $\eta$ are the coefficients. The key coefficient of interest is $\beta_6$, which we expect to be negative. $\delta_{ct}$ and $\delta_i$ are country-year and industry fixed effects, respectively. Year fixed effects capture and control for overall trends in firms' revenue. Country-year fixed effects net out time-variant differences across countries, whereas industry fixed effects net out time-invariant differences across industries. Since we include $\delta_{ct}$, we are unable to estimate the coefficient of *CME*, which is absorbed by the fixed effects. $\epsilon_{fict}$ accounts for all residual determinants of the dependent variable.

The matrix $\mathbf{X_{fict}}$ includes standard firm-level controls. We control for firm size (logged number of employees) as well as firm age (number of years it has operated in the market) and firm age squared. The matrix $\mathbf{W_{ict}}$ includes industry-level controls – MFN tariffs, the (log of) labor–capital ratio, and market concentration, measured using the Herfindahl-Hirschman index of revenue.[50]

We run ordinary least squares (OLS) regressions with standard errors clustered at the country-year level. Because our dataset includes more than 800,000 private and public firms for a period of over

[48] Table B1 in Appendix B shows that no firm- or industry-level covariate predicts preferential tariff cuts, i.e. tariff cuts do not seem endogenous to firm- and industry-level characteristics.

[49] Potrafke, 2013. In the conclusion, we discuss how productive firms in CMEs have incentives to weaken wage coordination to increase their gain from trade. If the probability of successfully changing labour market institutions in CMEs depends on firm's performance and on the size of tariff reduction, this may create upward bias in our estimates.

[50] Descriptive statistics are reported in Table B2 in Appendix B.

10 years, we have more than 4 million observations in our baseline models. Note that the Amadeus database reports only the main industry in which firms operate, i.e. each firm appears in the data only once in each year.

There are three main concerns about our identification strategy. First, since the Amadeus database changes firms' coverage over time and across countries, we need to make sure that our estimates are not purely an artifact of sampling issues. To address this concern, we use Kalemli-Ozcan et al.'s[51] data on how much of Eurostat's employment data is covered in the Amadeus data on the manufacturing sectors in EU countries. We correlate this data with our variable *CME* for total employment and employment broken down by firm size. The Amadeus coverage displays a low correlation with labor market institutions, which implies that bias in a firm's coverage is not driving the results of the triple difference-in-differences estimation. We also run some of the models with weights from Kalemli-Ozcan et al.'s data.[52]

Second, industries that are implementing trade liberalization may have been on a different trend than those facing no tariff cuts. Support for the parallel trend assumption comes from the fact that our results are robust to the inclusion of industry specific time trends.[53]

The third and most important threat to the identification strategy comes from variables that are correlated with *CME*, since CMEs and LMEs differ in several ways in addition to their labor markets. Indeed, country-level characteristics may be responsible for the differential effect of firms' productivity and tariff cuts on revenue. For instance, if countries adopting the euro are correlated with, say, CMEs, the monetary mechanism could be mediating the effect of firm productivity and tariff cuts on firm performance. To address this concern, we identify a large number of country-level variables and interact them with *TFPR* and $\Delta\tau$. We then include them with our key triple interaction terms in our models. If the results remain unchanged, this would allow us to safely rule out the possibility that these confounders invalidate our identification strategy.

We identify the following confounders: innovation, corruption, electoral system, migration, level of unemployment, and access to credit. We also include other variables that capture the market structure and could act as additional confounders: social welfare expenditure, government expenditure, size of the service sector, fiscal capacity, foreign direct investment (FDI) outflows (and inflows), and Eurozone membership. In theory, any of these variables could mediate the effect of trade liberalization and firm productivity on firm performance.[54] Before interacting these variables with *TFPR* and $\Delta\tau$, we begin by noting that their correlation with *CME* is generally quite low (see Table C1 in Appendix C). Then, we include each of these (potential) confounders on the right-hand side of our main model and, as a very conservative test, all triple interaction terms at the same time. While we do our best to include

[51] Kalemli-Ozcan et al. 2017.

[52] We use the data for the total sample reported in Tables 6.5-6.8, and average values over the entire time span (Kalemli-Ozcan et al.'s 2017, 42-47).

[53] The results are robust to including quadratic industry-country specific time trends (available upon request).

[54] Appendix C reports the descriptions and sources of these variables.

all the potential confounders, we acknowledge that we cannot completely rule out the possibility of omitted observables and unboservables. In short, given our research design, our identification strategy requires stronger assumptions than it would require in an experimental setting.

## 5 Results

**Main findings** Table 1 reports our main results. Model 1 includes the triple interaction term among *TFPR*, $\Delta\tau$, and *CME*; the coefficient is negative and significant as expected. This implies that the reallocation effect is weaker in CMEs than in LMEs. That is, productive firms increase their revenue less in CMEs than in LMEs as a result of preferential liberalization. Models 2 and 3 report the mechanisms related to the two institutions highlighted in our theory: wage ceiling and subsidized vocational training. The triple interaction terms among *TFPR*, $\Delta\tau$, and *Wage Ceiling* and among *TFPR*, $\Delta\tau$, and *Subsidized VT* are both negative and significant, in line with our theory. These findings imply that the reallocation effect is weaker when these two institutions are in place in the case of trade liberalization. Note that the triple interaction term among *TFPR*, $\Delta\tau$, and *CME* remains negative and significant in Models 2 and 3.

Models 4-6 show the same model specifications as in Models 1-3, but they also include industry-specific trends. The results are virtually the same and the coefficients of interest are similar across different model specifications. Importantly, the coefficient of the double interaction term between *TFPR* and $\Delta\tau$ remains positive and significant in each model specification, adding plausibility to the results.

Figure 3 illustrates the effect of the triple interaction term, reporting the marginal effect of tariff cuts for different levels of firm productivity across CMEs and LMEs. It shows that the marginal effect is significantly more elastic for LMEs than for CMEs. In other words, the increase in revenue is significantly larger for productive firms in LMEs than for those in CMEs as a result of preferential trade liberalization. Concretely, the elasticity of the marginal effect is 20 percent larger in LMEs than it is in CMEs. Moreover, Figure 3 shows the distribution of the moderator, *TFPR*, distinguishing between CMEs and LMEs. There is no concern regarding a lack of common support for the moderator, since the distribution of firms is similar for both CMEs and LMEs and they both have firms for each value of *TFPR*. This is not surprising, given that we rely on a very large number of observations.

While we have provided evidence that labor market institutions mediate the distributional consequences of trade liberalization, concerns may remain that country-level characteristics other than wage coordination are responsible for this mediating effect. Given the nature of the triple difference-in-differences analysis, these confounders are a threat to our identification strategy if and only if they correlate with *CME* and they impact firm performance differentially in industries facing tariff cuts *and* based on firm productivity. To sharpen our identification strategy, we include the triple interaction term of *TFPR*, $\Delta\tau$, and a large number of country-level (potential) confounders together with our key triple interaction term, i.e., $TFPR \times \Delta\tau \times CME$.

Table 2 displays the results of these tests. Model 1 includes the triple interaction term with

**Table 1:** Reallocation effect in CMEs and LMEs

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | OLS | | | | | |
| | ln(Revenue) | | | | | |
| Δτ | -0.369*** | -0.369*** | -0.371*** | -0.364** | -0.364** | -0.367** |
| | (0.141) | (0.141) | (0.141) | (0.142) | (0.142) | (0.142) |
| TFPR | 0.419*** | 0.419*** | 0.419*** | 0.421*** | 0.421*** | 0.420*** |
| | (0.020) | (0.020) | (0.020) | (0.020) | (0.020) | (0.020) |
| TFPR*Δτ | 0.010*** | 0.010*** | 0.010*** | 0.010** | 0.010*** | 0.010** |
| | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) |
| Δτ*CME | 0.351** | 0.348** | 0.358** | 0.347** | 0.344** | 0.354** |
| | (0.141) | (0.141) | (0.141) | (0.142) | (0.142) | (0.142) |
| TFPR*CME | -0.063** | -0.060* | -0.044 | -0.064** | -0.061* | -0.045 |
| | (0.032) | (0.034) | (0.034) | (0.031) | (0.033) | (0.034) |
| **TFPR*Δτ*CME** | **-0.009**** | **-0.009**** | **-0.010**** | **-0.009**** | **-0.009**** | **-0.010**** |
| | **(0.004)** | **(0.004)** | **(0.004)** | **(0.004)** | **(0.004)** | **(0.004)** |
| Δτ*Wage Ceiling | | 0.098** | 0.091** | | 0.095** | 0.088** |
| | | (0.044) | (0.043) | | (0.044) | (0.044) |
| TFPR*Wage Ceiling | | -0.028 | -0.043 | | -0.029 | -0.044 |
| | | (0.053) | (0.053) | | (0.053) | (0.053) |
| **Δτ*TFPR*Wage Ceiling** | | **-0.003**** | **-0.003**** | | **-0.003**** | **-0.002**** |
| | | **(0.001)** | **(0.001)** | | **(0.001)** | **(0.001)** |
| Δτ*Subsidies for CVT | | | 0.083*** | | | 0.076*** |
| | | | (0.020) | | | (0.022) |
| TFPR*Subsidies for VT | | | 0.121*** | | | 0.120*** |
| | | | (0.037) | | | (0.037) |
| **Δτ*TFPR*Subsidies for VT** | | | **-0.002***** | | | **-0.002***** |
| | | | **(0.001)** | | | **(0.001)** |
| Constant | -8.435*** | -8.568*** | -8.929*** | 458.193*** | 157.523*** | 637.468** |
| | (0.820) | (0.898) | (0.959) | (24.895) | (50.269) | (252.098) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Country-year fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry-specific Trends | No | No | No | Yes | Yes | Yes |
| Observations | 4,053,929 | 4,032,150 | 3,918,518 | 4,053,929 | 4,032,150 | 3,918,518 |
| R-squared | 0.766 | 0.767 | 0.775 | 0.766 | 0.767 | 0.775 |

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

Note: OLS with standard errors clustered at the country-year level in parentheses. The unit of observation is firm-industry (4-digit NAICS)-country-year. The outcome variable in all models is the log of revenue. Sources: Amadeus dataset and Visser (2016).

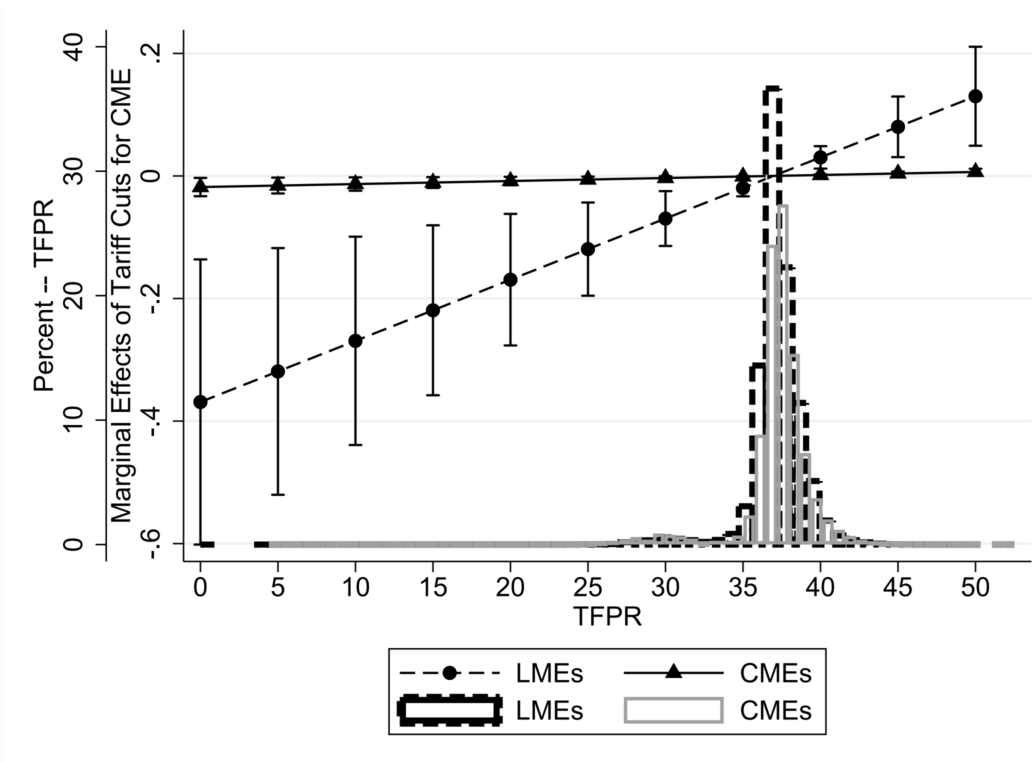
**Table 2:** Identification test

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | OLS | | | | | | | |
| | ln(Revenue) | | | | | | | |
| $\Delta\tau$ | -0.403*** | -0.386*** | -0.394*** | -0.300** | -0.342*** | -0.670 | -0.434*** | -0.920 |
| | (0.143) | (0.146) | (0.135) | (0.144) | (0.124) | (0.601) | (0.160) | (1.004) |
| TFPR | 0.396*** | 0.407*** | 0.481*** | 0.384*** | 0.228*** | -0.073 | 0.386*** | -2.037*** |
| | (0.018) | (0.024) | (0.023) | (0.062) | (0.048) | (0.292) | (0.049) | (0.573) |
| TFPR*$\Delta\tau$ | 0.011*** | 0.011*** | 0.011*** | 0.008** | 0.009*** | 0.017 | 0.012*** | 0.026 |
| | (0.004) | (0.004) | (0.004) | (0.004) | (0.003) | (0.016) | (0.004) | (0.027) |
| $\Delta\tau$*CME | 0.387*** | 0.362** | 0.388*** | 0.311** | 0.297** | 0.392** | 0.362** | 0.280** |
| | (0.142) | (0.142) | (0.137) | (0.134) | (0.125) | (0.169) | (0.151) | (0.131) |
| TFPR*CME | -0.054* | -0.061** | -0.044 | -0.046 | -0.004 | 0.047 | -0.054* | 0.175*** |
| | (0.028) | (0.029) | (0.029) | (0.035) | (0.036) | (0.049) | (0.028) | (0.055) |
| **TFPR*$\Delta\tau$*CME** | **-0.010*** | **-0.010** | **-0.011*** | **-0.008** | **-0.008** | **-0.011** | **-0.010** | **-0.008** |
| | **(0.004)** | **(0.004)** | **(0.004)** | **(0.004)** | **(0.003)** | **(0.005)** | **(0.004)** | **(0.004)** |
| Constant | -9.685*** | -9.239*** | -9.217*** | -9.183*** | -9.982*** | -9.038*** | -9.190*** | -10.321*** |
| | (0.643) | (0.654) | (0.722) | (0.669) | (0.634) | (0.720) | (0.650) | (0.407) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Country-year fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Innovation | Yes | No | No | No | No | No | No | No |
| Corruption | No | Yes | No | No | No | No | No | No |
| Electoral system | No | No | Yes | No | No | No | No | No |
| Migration | No | No | No | Yes | No | No | No | No |
| Unemployement | No | No | No | No | Yes | No | No | No |
| Market structure | No | No | No | No | No | Yes | No | No |
| Access to credit | No | No | No | No | No | No | Yes | No |
| All | No | No | No | No | No | No | No | Yes |
| Observations | 4,053,923 | 4,053,923 | 4,053,923 | 4,029,281 | 4,053,923 | 3,217,580 | 4,044,624 | 3,212,603 |
| R-squared | 0.766 | 0.766 | 0.766 | 0.767 | 0.767 | 0.801 | 0.766 | 0.805 |

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

Note: OLS with standard errors clustered at the country-year level in parentheses. The unit of observation is firm-industry (4-digit NAICS)-country-year. The outcome variable in all models is the log of revenue. Sources: Amadeus dataset, Visser (2016), WGI, WDI, and ILO.

**Figure 3:** The effect of tariff cuts on firm revenue for different levels of firm productivity in CMEs and LMEs




Note: The predictions are plotted from Model 1 in Table 1. LME includes countries with "fragmented wage bargaining, confined largely to individual firms or plants." CME include countries with "mixed industry and firm-level bargaining, weak government coordination through MW setting or wage indexation," "negotiation guidelines based on centralized bargaining," "Wage norms based on centralized bargaining by peak associations with or without government involvement," and "Maximum or minimum wage rates/increases based on centralized bargaining." The histogram shows the distribution of *TFPR* for both CMEs and LMEs. 99% C.I.

innovation. Model 2 includes the triple interaction term with the level of corruption.[55] Models 3–5 include the triple interaction term with, respectively, a dummy capturing a proportional represenation electoral system, the share of migrants, and level of unemployment. Model 6 includes triple interaction terms with country-level variables capturing the market structure: social welfare expenditure, size of the service sector, fiscal capacity, FDI outflows (and inflows), and the membership of the Eurozone. Model 7 includes interaction terms with variables capturing firms' ease of access to credit. Model 8 includes all of the above triple interaction terms. Our main coefficient of interest remains negative

[55]The results are similar if we use other measures of the quality of institutions such as rule of law, government effectiveness, and regulatory quality. These variables are highly correlated with one another, which is why we do not include all of them at the same time.

and statistically significant. Importantly, its magnitude remains the same across model specifications. In sum, there is no evidence that these confounding factors are driving our results. That said, it is fundamentally very difficult to isolate the effect of CMEs from other country-level characterestics (observables and unobservables), which correlate with labour market institutions.

**Other mechanisms** We also explore two other mechanisms at play, as highlighted in our theory. First, we show that wages increase differentially across labor institutions as a result of trade liberalization. In particular, we use real wage data, collected by the ILO, for all EU countries between 2002 and 2008.[56] Importantly for us, the ILO wage data vary across industries.[57] We run a model with the first differences of wages as the outcome variable and the interaction between $\Delta\tau$ and *CME* as key independent variables. We also include country, industry, and year fixed effects. Figure 4 shows the effect of the interaction term graphically, which supports the argument that wages are stickier in CMEs than in LMEs in the case of tariff cuts. In fact, after trade liberalization hourly wages increase by about ten percent in LMEs, whereas they remain unchanged in CMEs.[58] This finding validates the claims that we make with respect to the y-axis in Panel A of Figure 1: Wages are more rigid in CMEs than LMEs in the case of an increase in labor demand triggered by trade liberalization.[59]

Second, we show that employment in industries facing tariff cuts increases differentially more in CMEs than LMEs. In particular, we regress labor share at the industry level on the interaction between $\Delta\tau$ and *CME*. We also include country, year, and industry fixed effects. Moreover, we control for market concentration, capital/labor ratio, and MFN tariffs as well as their interaction with *CME*.[60] Figure 5 shows that CMEs employ a higher share of workers than LMEs in those industries that face tariff cuts, a result in line with Iversen and Soskice.[61] This finding validates the claim made with respect to the x-axis in Panel A of Figure 1: The supply of skilled labor increases more in CMEs than LMEs to accommodate the increase in labor demand and to contain the upward pressure on wages after trade liberalization.[62]

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[56] We focus on the post-Euro period. The ILO wage data does not cover the period after 2008.

[57] We use available crosswalks to merge the ISIC 88 Rev 3 industries, which the ILO uses, with the NAICS 4-digit industries of our dataset.

[58] EU PTA partners are usually small, less-developed economies with limited capacity to absorb a large amount of EU exports. Paired with the fact that the labor supply is particularly elastic in CMEs, which facilitates capping wages, this may explain why wages remain unchanged after reducing preferential tariffs.

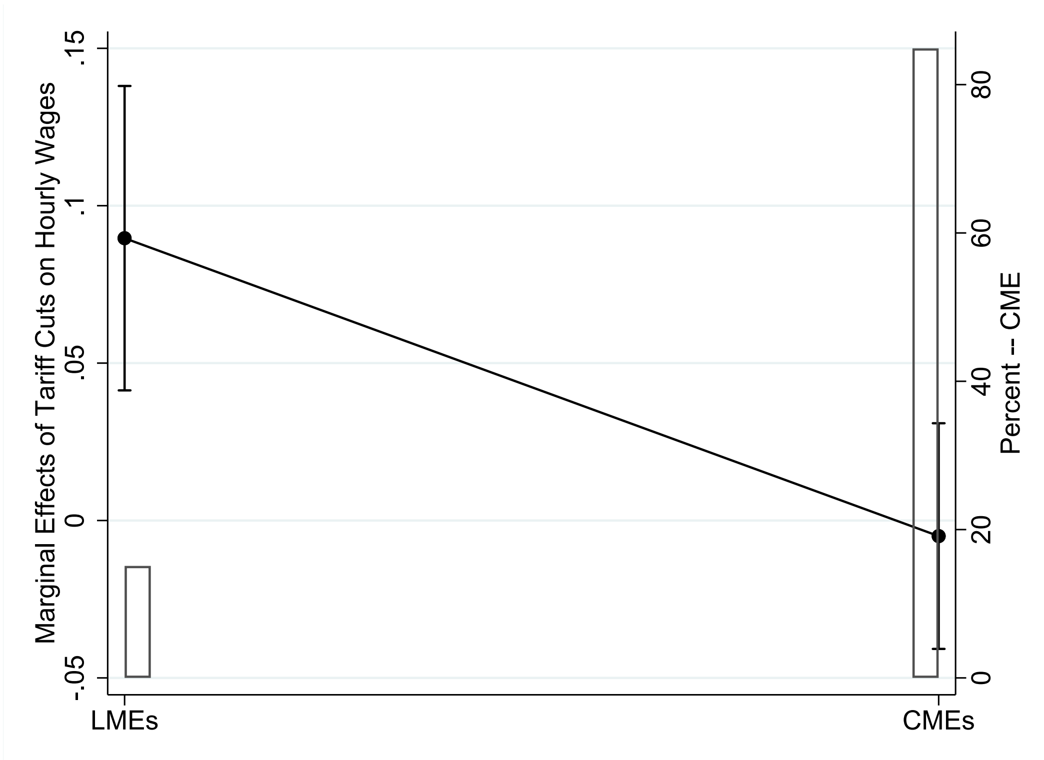
[59] Appendix D reports another test showing that costs of employees at the firm-level increase differentially across labor institutions as a result of trade liberalization.

[60] Since our outcome is a ratio between 0 and 1, we rely on a fractional response model. We weight observations by (the log of) number of employees so that industries with only a few firms are not driving the results.

[61] Iversen and Soskice 2010. The effect is significant only when tariff cuts are relatively sizable, which makes sense given that EU trade partners are generally small economies absorbing a relatively low amount of EU exports.

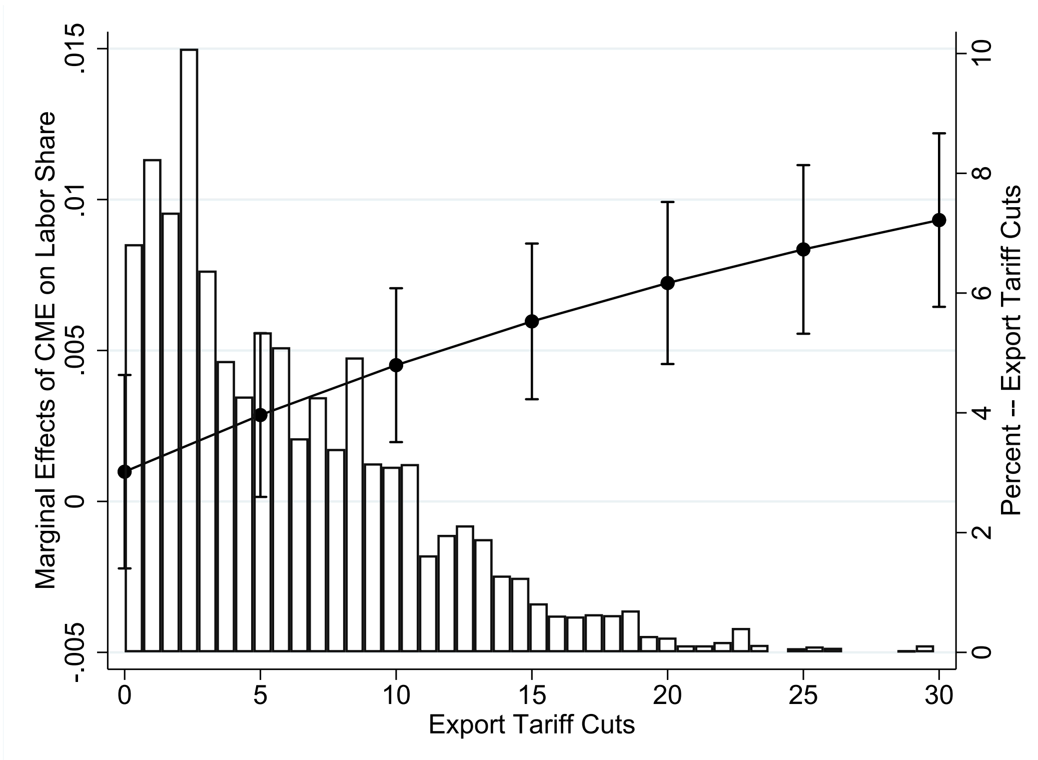
[62] In Appendix D, we also show that a reduction in preferential tariffs increases the intensive margins of trade for the

**Figure 4:** The effect of tariff cuts on wages in CMEs and LMEs




Note: The outcome variable is the first difference of hourly real wages in (constant) US dollars. The graph shows the marginal effect of export tariff cuts on wages for CMEs and LMEs. The model includes country, industry, and year fixed effects. OLS regression with robust standard errors are clustered at the country level. The histogram shows the distribution of *CME*. 90% C.I.

**Figure 5:** The effect of tariff cuts on employment at the industry level in CMEs and LMEs




Note: The outcome variable is labor share at the industry level. The graph shows the marginal effect of *CME* on industry's employment. The model includes country-year and industry fixed effects. Fractional response models with robust standard errors are clustered at the country level. The histogram shows the distribution of $\Delta\tau$. 90% C.I.

**Robustness checks** We corroborate our main findings with tests tackling other identification issues. In particular, we show that our results hold (1) if we endogenize tariff reduction and (2) if we account for other potential confounders, i.e. labor flexibility and automation. Moreover, we explore the effect of other types of tariff cuts, i.e. import and input tariffs, on the reallocation effect among firms in different labor markets (see Appendix E for details).

Moreover, we implement a battery of robustness checks to test the sensitivity of our findings to issues related to diagnostics of the interaction term, choice of sample, and model specification. Details of these tests and the corresponding results are reported in Appendix F.

## 6 Gains From Trade and Attitudes Towards Redistribution

So far we have shown that gains from trade are more uniformly distributed among firms in CMEs than they are in LMEs, and that labor market frictions help unproductive firms weather trade liberalization. In short, the same unproductive firm is better off in a CME than in an LME after trade liberalization, since CMEs have labor market institutions that tame the uneven distributional effects of globalization. In addition to this firm-level analysis, we offer suggestive evidence in support of our argument by testing the effect of preferential trade liberalization on individual attitudes towards redistribution. This part analysis builds on seminal contributions claiming that government compensation policies help mitigate the winner-takes-all effect of trade openness.[63]

A core assumption in this political economy literature is that economic interests are key sources of individual-level preferences regarding redistribution. Insurance models of redistribution imply that citizens' preferences for redistribution are a function of their exposure to labor market risks, especially as reflected in actual or threatened unemployment and their related actual or potential income losses.[64]

These models' underlying logic suggests an additional empirically observable implication of our argument. To the extent that trade liberalization generates a greater reallocation effect in LMEs than in CMEs, i.e. a greater number of unproductive firms either lose market share or exit the market altogether, actual or potential income losses for individuals employed in unproductive firms should be greater in LMEs than in CMEs after trade liberalization. Extending the logic of insurance models of redistribution to our argument allows us to derive the additional expectation that trade liberalization should trigger individual-level support for redistribution more in LMEs than in CMEs, especially for workers employed in unproductive firms. In short, labor institutions complement compensation policies in reducing individuals' concerns about inequality generated by globalization.[65]

most productive firms, in line with our theory.

[63] Cameron 1978; Katzenstein 1985; Rodrik 1998; Ruggie 1982.

[64] Alt and Iversen 2016; Cusack et al. 2006; Iversen and Soskice 2001; Rehm 2009; Rehm 2011; Thewissen and Rueda 2019.

[65] Hays et al. 2005; Gingrich 2019; Margalit 2011; Rickard 2015; Rudra 2005.

**Model** We use ESS data covering several EU countries with six waves from 2004 to 2014 to test the effect of preferential liberalization on individual attitudes toward redistribution.[66] Importantly for us, this data reports the geographic location of each respondent at the level of NUTS-2 regions. An important limitation of our analysis is that we lack data on personal exposure to trade liberalization; our data is only on regional exposure. In other words, we cannot tell if a given individual has been directly affected by trade liberalization. It is, however, reasonable to assume that the negative consequences of trade liberalization (firms reducing their revenues, workers losing their jobs) have aggregate consequences. Thus, we assume that people living in a certain area are either directly or indirectly affected, or at least aware of others who are affected.

Following previous studies,[67] we use respondents' level of agreement with the following statement to capture preferences regarding redistribution: *The government should take measures to reduce differences in income levels.*[68] Figure H1 in Appendix H shows the geographical distribution of this variable.

Our main independent variable measures the magnitude of trade liberalization in a specific industry $i$ weighted on the share of manufacturing of industry $i$ in a NUTS-2 region $r$. To build our independent variable, we geocoded all the firms used in the previous analysis at the level of a NUTS-2 region.[69] This Bartik variable is similar to the ones used by Colantone and Stanig.[70] More formally, this variable is built using the following equation:

$$PRF\ Liberalization_{crt} = \sum_j \frac{L_{rjf}}{L_r} \times \frac{\Delta\tau_{jt}}{Import_{cj}}, \tag{2}$$

where $c$ indexes countries, $r$ NUTS-2 regions, $j$ industries, $f$ firms, and $t$ years.

$\frac{\Delta\tau_{jt}}{Import_{cj}}$ is the yearly change in preferential tariff cuts in country $c$ and industry $j$. In order to back out the region-specific trade shock, we take the weighted sum of the change in tariff cuts per worker across industries, where the weights capture each industry's relative importance in a given region. Specifically, the weights are defined as the ratio of the number of workers in region $r$ and industry $j$ over the total number of workers in the region.

The important difference with respect to previous studies is given by the index $f$. We are interested in the share of employees in region $r$ and industry $j$ working in unproductive firms, who we expect to lose from trade liberalization proportionally more than very productive firms. Thus, the numerator of $\frac{L_{rjf}}{L_r}$ measures the share of workers in firms belonging to the lowest $10^{th}$ percentile of the productivity

[66] To match the time span of the firm-level analysis, we drop the first (2002) and last (2016) ESS waves.

[67] Rehm 2009; Walter 2017; Wren and Rehm 2013.

[68] Answers were scored on a five-point scale from "strongly disagree" to "strongly agree"; we recode it as a dummy (1 = agree or strongly agree) because the relevant variation is between those in favor of and those against redistribution policies, whereas there is limited variation across the five-point scale.

[69] Details of the geocoding procedure are provided in Appendix G.

[70] Colantone and Stanig 2018a.

distribution. The underlying logic is as follows: Larger preferential liberalization shocks are attributed to regions characterized by larger shares of workers employed in unproductive firms, who should lose disproportionally more from tariff cuts than those employed in more productive firms.[71]

The unit of analysis is respondent-NUTS-2 region-country-ESS wave. Since ESS waves are every other year, we take the biyearly sum of the equation 2. Armed with these dependent and independent variables, we estimate the following baseline model:

$$
\begin{aligned}
Redistribution_{prcw} = {} & \gamma_0 + \gamma_1 PRF\ Liberalization_{r(p)cw} + \gamma_2 CME_{cw} \\
& + \gamma_3 PRF\ Liberalization_{r(p)cw} \times CME_{cw} \\
& + \mathbf{X_{prcw}} + \mathbf{X_{prcw}} \times CME^{'}_{cw}\eta \\
& + \mathbf{X_{prcw}} \times PRF\ Liberalization^{'}_{r(p)cw}\zeta \\
& + \mathbf{Z_{cw}} \times PRF\ Liberalization^{'}_{r(p)cw}\theta + \delta_{cw} + \delta_i + \epsilon_{prcw},
\end{aligned}
\tag{3}
$$

where $p$ indexes people responding to the ESS, $r$ NUTS-2 regions, $c$ indexes countries, and $w$ waves. The function $r(p)$ maps respondent $p$ to its NUTS2 region $r$. $\gamma_0$, $\gamma_1$, $\ldots$ $\gamma_4$, $\eta$, $\zeta$, and $\theta$ are the coefficients. The key coefficient of interest is $\gamma_3$, which we expect to be negative.

Moreover, $\delta_{cw}$ and $\delta_i$ are country-wave and industry (in which respondents are employed) fixed effects, respectively. Country-wave fixed effects capture and control for time-varying country-level characteristics. Industry fixed effects net out time-invariant differences across the industries in which respondents are employed. $\epsilon_{prcw}$ accounts for all residual determinants of the dependent variable. Since $CME_{cw}$ varies across countries and over time, we are unable to estimate its coefficient, which gets absorbed by country-wave fixed effects.

Furthermore, the matrix $\mathbf{X_{prcw}}$ includes standard individual-level variables. First and most importantly, the literature's key finding is that individuals who invest in acquiring specific skills, a feature often associated with CMEs, tend to be more supportive of redistribution.[72] Thus, following Alt and Iversen (2017), we control for a variable that captures skill specificity.[73] Second, we control for education level, gender, and ideology. Each of these controls is interacted with *CME* and *PRF Liberalization*.

In addition, the matrix $\mathbf{Z_{cw}}$ includes all the potential confounders of CMEs described in the previous analysis in interaction with *PRF Liberalization*. Since these confounders only vary across countries and over time, we are unable to estimate their coefficient alone, because it gets absorbed by country-wave fixed effects.[74]

We employ a difference-in-differences empirical strategy in which the treatment (*PRF Liberaliza-*

[71] Figure H2 in Appendix H shows the geographical distribution of this variable.

[72] Alt and Iversen 2017; Cusack et al. 2006; Iversen and Soskice 2001; Rehm 2009.

[73] More details on how the skill specificity variable is built are provided in Appendix H.

[74] Descriptive statistics are reported in Table H1 in Appendix H.

**Table 3:** Support for redistribution

| | (1) | (2) | (3) |
|---|---|---|---|
| | OLS | | |
| | Support for Redistribution | | |
| | All Sample | Low Education | High Education |
| PRF Liberalization | 0.003 | 0.004 | -0.000 |
| | (0.003) | (0.003) | (0.004) |
| **PRF Liberalization*CME** | **-0.003*** ** | **-0.003** ** | **-0.001** |
| | **(0.001)** | **(0.001)** | **(0.003)** |
| Constant | 0.891*** | 0.872*** | 0.780*** |
| | (0.025) | (0.026) | (0.045) |
| Observations | 120,904 | 100,366 | 20,538 |
| R-squared | 0.116 | 0.104 | 0.156 |
| Country-wave fixed effects | Yes | Yes | Yes |
| Industry fixed effects | Yes | Yes | Yes |
| Individual controls | Yes | Yes | Yes |
| Confounders | Yes | Yes | Yes |

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

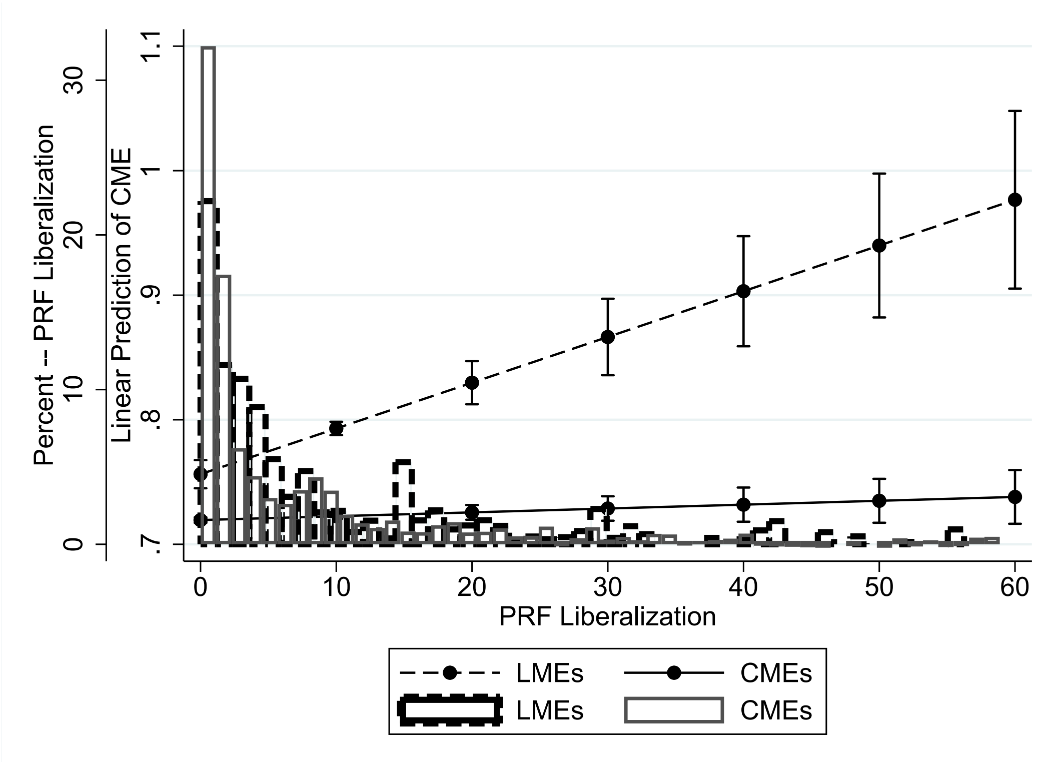
Note: OLS with robust standard errors clustered by country in parentheses. The unit of observation is respondent-region-country-wave. The outcome variable in all models is a dummy scoring 1 if respondents answer "strongly agree" or "agree" with the following statement: *The government should take measures to reduce differences in income levels. Low Education* implies no college degree. Sources: Amadeus dataset, Visser (2016), and ESS (2018).

*tion*) varies in intensity across regions and over time and is interacted with labor market institutions. We run OLS regressions with robust standard errors clustered at the country level.[75] We have about 20,000 respondents per wave for a total of approximately 120,000 observations. All regressions are run with population size weights.

**Results** Table 3 reports the main results. The coefficient of the interaction between the instrument in the equation 2 and *CME* is negative and significant as expected in Model 1, which includes the entire sample. In a nutshell, we observe that the demand for redistribution in CMEs is weaker than in LMEs in the cases of preferential liberalization that affect a large share of workers employed in unproductive

[75] We use OLS regressions for two reasons: (1) we are using a difference-in-differences strategy, which requires a linear estimator and (2) due to the incidental parameter problem. The results are similar if we use logistic regressions (results are available upon request).

**Figure 6:** The effect of tariff cuts on support for redistribution in CMEs and LMEs




Note: The figure plots predictions from Model 1 in Table 3. The outcome variable in all models is a dummy scored 1 if respondents "strongly agree" or "agree" with the following statement: *The government should take measures to reduce differences in income levels.* The graph shows the linear predictions of $\Delta\tau$ for CMEs and LMEs. The histogram shows the distribution of $\Delta\tau$ for both CMEs and LMEs. 90% C.I.

firms. The coefficient of the instrument of PRF liberalization alone is positive and significant, which indicates that trade liberalization increases support for redistribution regardless of labor institutions.

Models 2 and 3 show the effect of heterogeneity based on individuals' education. The key finding is that support for redistribution is stronger in LMEs than in CMEs after trade liberalization, especially among individuals with a low level of education – i.e., with no college degree. In short, these effects are more pronounced among low-skilled individuals, who are more likely to work in unproductive firms and for whom the income gap with high-skilled workers increases after trade liberalization. While the inference is ecological (as we cannot directly observe whether the respondents work in productive or unproductive firms), these results are consistent with our firm-level analysis and our argument about the differential effect of trade liberalization between labor markets.

To better interpret the results, Model 1 plots the effect of the interaction term – i.e., the linear predictions of *CMEs* for different values of *PRF Liberalization*. The main findings are twofold. First, support for redistribution is always lower in CMEs than in LMEs, with or without trade liberalization. This result is in line with the fact that there is wage compression and thus smaller wage inequality in

CMEs.[76] Second, while trade liberalization does not affect attitudes toward redistribution in CMEs, as evidenced by the flat line, it increases support for redistribution by about 20 percent in LMEs. This is the key finding of the individual level analysis.[77]

In summary, while suggestive, our individual-level analysis confirms the firm-level results. The distributional consequences of trade liberalization are less severe when wages are more rigid , reducing upward pressure on wages, especially among high-skilled workers. In turn, demand for redistribution is weaker in CMEs than in LMEs.

## 7 Conclusion

This paper explores the distributional consequences of trade liberalization across different types of labor market institutions. The main findings are twofold. In the firm-level analysis, we show that the reallocation effect is weaker in CMEs than in LMEs. That is, the revenue of productive firms increases proportionally less in CMEs than in LMEs. This effect is driven by smaller wage increases in CMEs compared to LMEs due to upward wage rigidity, which we documented in our analysis. In the individual-level analysis, we find suggestive evidence that the demand for redistribution is weaker in CMEs compared to LMEs due to the impact of trade liberalization on unproductive firms.

Our analysis has three important and timely policy implications. First, our findings indicate that some labor market institutions mitigate the winner-takes-all effect produced by trade liberalization, thus producing more uniform gains from trade. While trade liberalization is akin to increasing the market power of a few large corporations,[78] some countries are less prone than others to producing "superstars," given the presence of labor market frictions. This is a positive consequence of labor market frictions, which have often been blamed for high unemployment and sluggish economic growth. In other words, upward wage rigidity helps compensate the losers from globalization.

Second, our findings suggest that large and competitive firms should be in favor of removing labor market coordination mechanisms that constrain their ability to reap the full potential of trade liberalization. For instance, the domestic politics of Germany's labor market policies during the so-called Hartz reforms of 2003–2005 supports this view.[79] Although many traditional mechanisms of labor market coordination remain in place, Germany's labor market policy has undergone substantial liberalization, in line with the preferences and demands for more liberal labor market policies vocally

[76] Iversen and Soskice 2010. The caveat here is that we are unable to account for pre-existing inequality. Moreover, while we account for a host of counfounders capturing economic and political differences between CMEs and LMEs, we cannot rule out the possibility of having omitted other variables capturing norms, regulatory policies, and industrial structure, which may also explain some of these differences.

[77] Figure H3 displays the marginal effect of Model 2 (related to low-educated individuals), which is almost identical to Figure 6. However, there is no effect for highly educated respondents (Figure H4).

[78] Osgood et al. 2016.

[79] Paster 2017.

advocated by Germany's employers' associations through the oft-cited public relations campaign New Social Market Initiative.[80] These dynamics suggest that labor market coordination mechanisms that are typical in CMEs may become the target of firms that view labor frictions as constraints on growth potential.

Finally, our paper warns that weakening labor market coordination would have important implications for inequality for both firms and workers. Indeed, our results show that coordinated labor market institutions mediate the effect of trade liberalization on people's concerns about differences in income levels. Some have argued that the current backlash against globalization in developed countries is partly triggered by extensive job losses in manufacturing due to competition from emerging economies.[81] Our findings thus indicate that trade openness does not affect all countries in the same way. In particular, we document that variation in labor institutions leads to discrepancies in concerns about inequality once trade liberalization kicks in.

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[80] Ibid.

[81] Colantone and Stanig 2018a, 2018b.

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# Appendix A

## Preferential Tariff Cuts

We build our tariff cut variable ($\Delta\tau$) following the steps below:

1. We have data on preferential tariffs at the HS 6-digit level for all the PTAs signed by the EU post-1995. For each product, we know preferential tariffs for time zero, i.e., year of ratification, and for all subsequent years until preferential tariffs go to zero (up to 22 years). In other words, we know the phase-out tariff period for each product for each PTA.

2. For each product at the 6-digit level, we know the MFN tariff, which we use as baseline to calculate the tariff cut.

3. We create a variable $PRF$ that captures the level of PRF tariff for each product for each PTA in each year. This variable takes into account the phase-out tariff period. For instance, if a PTA is ratified in 2000, $PRF$ of product $i$ includes the level of PRF tariff from 2000 to 2021.

4. We create a tariff cut variable for each product and for each PTA. Tariff cut is the difference between MFN and $PRF$ in the year of ratification and it is the inverse of the first difference of $PRF$, i.e., *PRF lagged-PRF*, in subsequent years. In other words, to calculate the tariff cut, we use MFN as baseline for the first year in which PRF tariffs kick in and the PRF tariffs of the previous year in subsequent years in which a PTA is in force.

5. We create a variable capturing proportional tariff cuts, i.e., $\frac{MFN-PRF}{MFN}$, in the first year and $\frac{PRF\ lagged-PRF}{PRF\ lagged}$, following the same procedure as in 4.

6. We create weighted tariff cuts and weighted proportional tariff cuts dividing tariffs by import value. We then follow the same procedures as in 4 and 5.

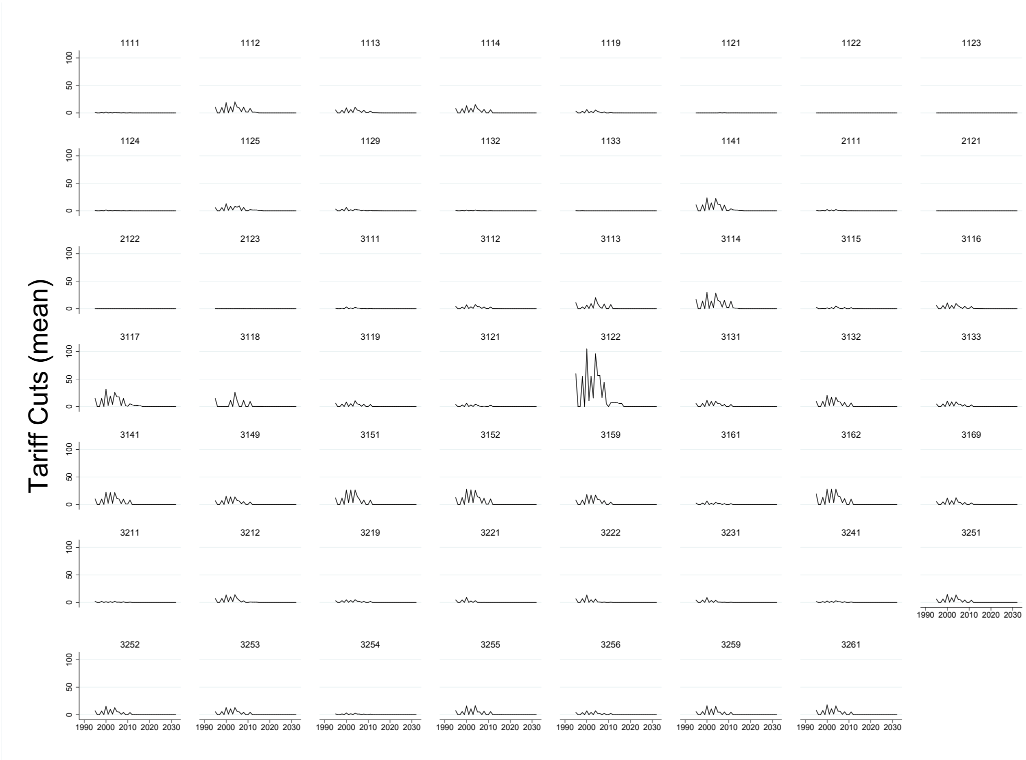
7. We sum all the tariff cuts (weighted and not) across all EU PTAs for a given product $i$ in a given year $t$. That gives us our measure of preferential trade liberalization.

8. We take the average value of proportional tariff cuts (weighted and not) across all EU PTAs for a given product $i$ in a given year $t$.

9. We merge the dataset with an NAICS 4-digit variable to merge the tariff data with the Amadeus database.

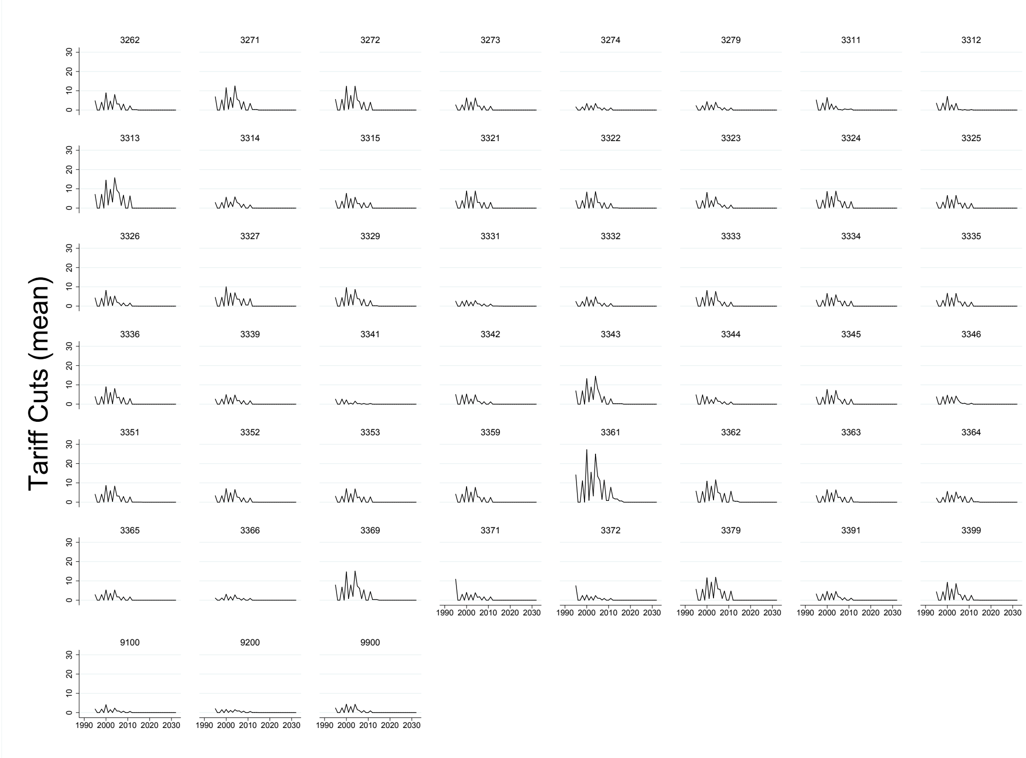
10. We take the average value of all our measures of tariff cuts (proportional and not, weighted and not) in each year to move from HS 6-digit to NAICS 4-digit. Note that we did not sum the tariff cut in this case because there are different numbers of 6-digit products in 4-digit industries.

**Figure A1:** Tariff cuts by industry and time (part 1)




Note: Source: Baccini et al. (2018).

**Figure A2:** Tariff cuts by industry and time (part 2)

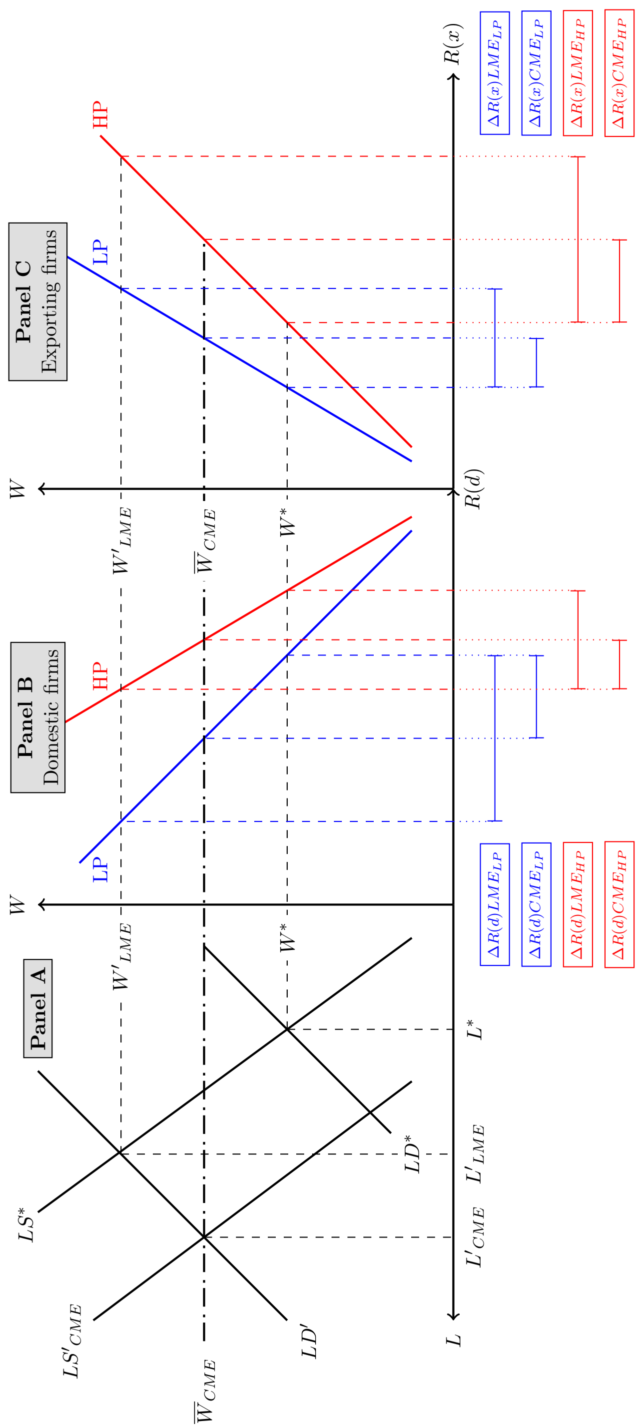



Note: Source: Baccini et al. (2018).

# Appendix B

## Figure Supporting the Theory

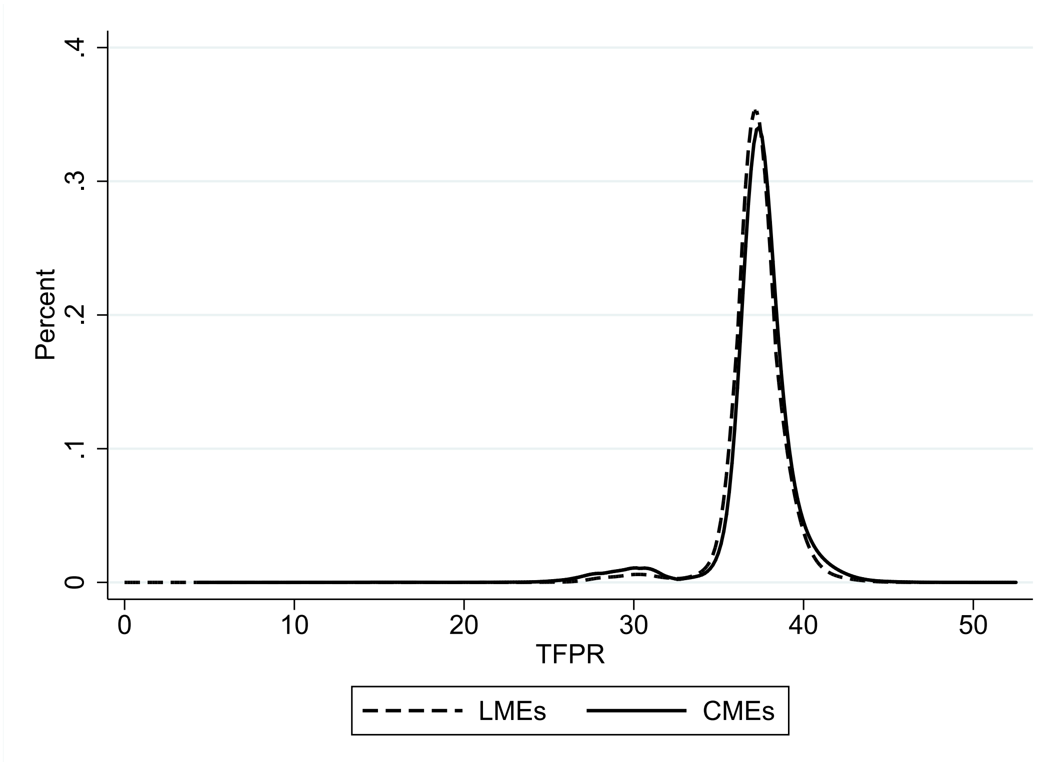
**Figure B1:** The effect of trade liberalization in CMEs and LMEs: domestic vs. exporting firms




Note: Blue curves refer to firms with higher productivity (HP), red ones to firms with lower productivity (LP).

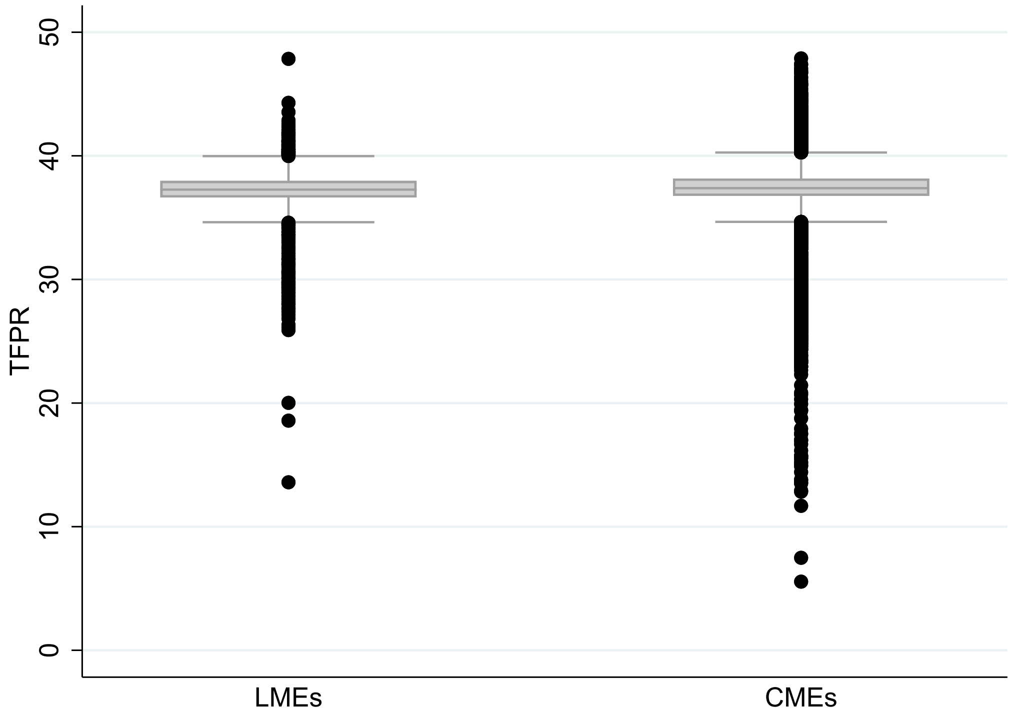
## Descriptive Statistics (firm-level analysis)

**Figure B2:** Kernel Density Estimate of TFPR by Labor Institutions




Note: Sources: Amadeus dataset and Visser (2016).

**Figure B3:** Box Plot of TFPR by Labor Institutions




Note: Sources: Amadeus dataset and Visser (2016).

**Table B1:** Explaining preferential tariff cuts

| | OLS | | |
|---|---|---|---|
| | PRF Tariff Cut | | |
| | (1) | (2) | (3) |
| Revenue | 0.0005 | 0.0244 | 0.0007** |
| | (0.000) | (0.028) | (0.000) |
| CME | -0.0090 | -0.0150 | 0.0066 |
| | (0.010) | (0.344) | (0.006) |
| Revenue*CME | -0.0002 | -0.0045 | -0.0003* |
| | (0.000) | (0.008) | (0.000) |
| TFPR | 0.0001 | 0.0152 | 0.0002 |
| | (0.000) | (0.023) | (0.001) |
| TFPR*CME | -0.0000 | -0.0125 | -0.0001 |
| | (0.000) | (0.010) | (0.000) |
| MFN | 0.0066 | 1.3342** | -0.0144 |
| | (0.012) | (0.557) | (0.022) |
| MFN*CME | 0.0018* | 0.0604 | 0.0015* |
| | (0.001) | (0.041) | (0.001) |
| HHI | 0.0144 | -3.9913 | 0.0329** |
| | (0.009) | (3.277) | (0.015) |
| HHI*CME | -0.0008 | 3.3440 | -0.0034 |
| | (0.003) | (3.272) | (0.004) |
| K/L | -0.0008 | -0.0025 | -0.0002 |
| | (0.001) | (0.094) | (0.001) |
| K/L*CME | 0.0004 | 0.0149 | 0.0000 |
| | (0.000) | (0.040) | (0.000) |
| Age | -0.0004 | 0.1944 | 0.0047* |
| | (0.002) | (0.191) | (0.002) |
| Age*CME | -0.0001 | -0.1050 | -0.0011 |
| | (0.001) | (0.081) | (0.001) |
| Age2 | 0.0000 | -0.0131 | -0.0002 |
| | (0.000) | (0.012) | (0.000) |
| Age2*CME | 0.0000 | 0.0067 | 0.0000 |
| | (0.000) | (0.005) | (0.000) |
| Size | -0.0007 | -0.0393 | -0.0019 |
| | (0.001) | (0.068) | (0.002) |
| Size*CME | 0.0004* | 0.0032 | 0.0008 |
| | (0.000) | (0.030) | (0.001) |
| Constant | 0.0069 | 34.2276*** | 0.0264 |
| | (0.029) | (3.912) | (0.019) |
| Observations | 4,053,929 | 4,053,929 | 4,032,150 |
| R-squared | 0.466 | 0.124 | 0.572 |
| Controls | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| Country FE | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes |

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

Note: OLS with robust standard errors clustered by country in parentheses. Unit of observation is firm-industry (4-digit NAICS)-country-year. The outcome variable in all models is $\Delta\tau$. Sources: Amadeus dataset, Baccini et al., (2018), and Visser (2016).

**Table B2:** Descriptive statistics

| Variable | Obs | Mean | Std. Dev. | Min | Max | Source |
|---|---|---|---|---|---|---|
| Ln(Revenue) | 4,053,929 | 13.70 | 2.87 | -19.57 | 29.35 | Amadeus |
| TFPR | 4,053,929 | 37.37 | 1.84 | 0 | 49.67 | Amadeus |
| CME | 4,053,929 | 0.90 | 0.30 | 0 | 1 | Visser |
| MFN | 4,053,929 | 3.84 | 3.88 | 0 | 47.26 | Baccini et al |
| HHI | 4,053,929 | 0.06 | 0.10 | 0 | 1 | Amadeus |
| K/L | 4,053,929 | 11.72 | 1.82 | -17.74 | 26.07 | Amadeus |
| Firm Age | 4,053,929 | 9.09 | 1.84 | 1 | 10 | Amadeus |
| Firm Age2 | 4,053,929 | 85.97 | 26.43 | 1 | 100 | Amadeus |
| ln(Labour) | 4,053,929 | 2.27 | 1.34 | 0.69 | 13.25 | Amadeus |
| Labour Flexibility | 2,846,018 | 2.54 | 0.62 | 1.10 | 4.42 | OECD |
| Union Density | 2,897,046 | 26.47 | 16.69 | 6.53 | 77.71 | Visser |
| Centralization | 2,470,583 | 0.35 | 0.10 | 0.10 | 0.88 | Visser |
| Government Intervention | 4,032,150 | 2.97 | 0.95 | 1.50 | 5.00 | Visser |
| Ext | 4,042,895 | 1.49 | 1.26 | 0 | 3 | Visser |
| Sector | 3,956,669 | 1.34 | 0.73 | 0 | 2 | Visser |
| Unauthority | 3,934,890 | 0.34 | 0.14 | 0 | 0.80 | Visser |
| Cfauthority | 3,934,890 | 0.37 | 0.16 | 0 | 0.70 | Visser |
| Corruption | 4,053,929 | 0.81 | 0.79 | -0.58 | 2.23 | WB |
| PR | 4,053,929 | 0.90 | 0.30 | 0 | 1 | WB |
| Migration | 4,029,283 | 8.10 | 4.60 | 0.70 | 18.00 | UN |
| Social Expenditure | 3,218,385 | 25.07 | 3.68 | 11.00 | 31.90 | WDI |
| Service | 4,053,929 | 62.44 | 8.67 | 42.48 | 77.81 | WDI |
| Tax/GDP | 4,053,929 | 19.51 | 4.43 | 1.50 | 51.11 | WDI |
| FDI | 4,053,129 | 0.95 | 1.28 | -0.06 | 7.68 | WDI |
| Euro | 4,053,929 | 0.61 | 0.49 | 0 | 1 | Authors |
| Private Credit | 4,044,630 | 96.22 | 44.64 | 0.19 | 253.26 | WDI |
| Bank Credit | 4,044,630 | 96.16 | 44.62 | 0.19 | 253.15 | WDI |
| Financial Credit | 4,044,630 | 136.10 | 63.26 | 0.23 | 316.61 | WDI |
| Unemployment | 4,053,929 | 12.28 | 5.50 | 2.92 | 22.67 | WDI |
| Export Tariff | 4,053,929 | 7.16 | 14.56 | 0 | 1764.91 | Baccini et al |
| Wage ceiling | 4,032,150 | 0.05 | 0.21 | 0 | 1 | Visser |
| Subsidies for VT | 3,918,518 | 0.07 | 0.25 | 0 | 1 | Visser |
| Import Tariff | 4,053,929 | 11.61 | 67.84 | 0 | 1764.91 | Baccini et al |
| Input Tariff | 4,032,150 | 0.35 | 0.49 | 0 | 8.04 | Baccini et al |
| Automation | 3,211,758 | 11.94 | 14.62 | 0 | 56.03 | Acemoglu & Restrepo |

# Appendix C

## Confounders

The variables that we analyze as possible confounders in the empirical analysis are the following.

**Innovation** The logic is that innovation may help productive firms to navigate trade liberalization more than unproductive firms. If innovation is significantly higher in LMEs, this could be a potential alternative channel that explains our results. We rely on number of patents (by residents) to measure innovation, as well as on share of firms that spend on R&D, researchers in R&D (per million people), and technicians in R&D (per million people). Data come from the WDI. The time span is between 1960 and 2016.

**Corruption** The logic is that corruption may create additional fixed or variable costs for firms, especially when competition increases due to trade liberalization. These additional costs are more likely to be supported by productive firms rather than unproductive firms. In turn, this creates uneven gains from trade. If corruption correlates with labor market frictions, it may be a confounder. We rely on a measure of control of corruption by the Worldwide Governance Indicators (WGI) of the World Bank (Kaufmann et al. 2010). The time span is between 1996 and 2016.[82]

**Electoral system** The logic is that different types of electoral systems provide different incentives from politicians to support different types of firms. For instance, it may be that majoritarian systems raise incentives to remunerate large, productive firms more than proportional systems do. If this also happens during episodes of trade liberalization, electoral systems may be a confounder. Data on electoral systems come from the Database of Political Institutions 2017 (Cruz, Keefer, and Scartascini 2018).

**Migration** If migrants, especially economic migrants, move to CMEs from LMEs in the case of trade liberalization, the supply of labor would increase in CMEs more than in LMEs. This may reduce the increase of wages in ways that have nothing to do with labor market institutions. We use the international migrant stock as a percentage of the total population (both sexes). Data are available for all the countries in the sample, 2003 to 2016. Data come from the United Nations and are available at `http://www.un.org/en/development/desa/population/migration/data/index.shtml`.

**Unemployment** The logic is that (pre-trade-liberalization) high-level unemployment reduces the increase of wages after trade liberalization. In turn, this may help unproductive firms in the case of increasing competition due to tariff cuts. If unemployment correlates with labor market frictions, it may be a confounder. We rely on a measure of unemployment collected by the ILO and available through the WDI. The time span is between 1960 and 2016.

[82] Results are similar if we use other variables capturing the quality of governance, e.g., rule of law and regulatory quality.

**Market structure** We include social expenditure and government expenditure. Data are from the OECD and the WDI respectively, and are available from 1990 to 2016. Moreover, we include the size of the service sector, amount of taxes over GDP, and amount of FDI outflows. Data are from the WDI and are available from 1960 to 2016. Finally, we include a dummy for countries that adopted the Euro. Data come from `https://europa.eu/european-union/about-eu/money/euro_en`. All these variables can mitigate (e.g., social expenditure) or magnify (Euro) the reallocation effect. Therefore, they are all potential confounders.

**Access to credit** In countries in which access to credit is easy, firms can weather the increasing competition triggered by trade liberalization better than in countries in which firms face credit constraints. In particular, easy access to credit can help small, unproductive firms.[83] To capture access to credit we rely on the following variables: (1) domestic credit to private sector by banks (% of GDP); (2) domestic credit provided by financial sector (% of GDP); and (3) domestic credit to private sector (% of GDP). Data come from the WDI and are available from 1960 to 2016.[84]

[83]For a review of the literature on trade liberalization and access to credit, see Foley and Manova (2015).

[84]Results are similar if we use variables capturing access to credit from the Enterprise Survey of the World Bank. We do not rely on these variables in the main analysis because data start from 2006.

**Table C1:** Correlations of confounders

| | CME |
|---|---|
| CME | 1 |
| Corruption | **0.48** |
| Unemployment | **0.11** |
| Electoral system | **0.34** |
| Migration | **0.17** |
| Innovation | **0.13** |
| Social expenditure | **0.33** |
| Services (%GDP) | **0.25** |
| Tax (%GDP) | **0.20** |
| FDI outflows | **0.65** |
| Euro | **0.17** |
| Private credit | **0.21** |
| Bank credit | **0.21** |
| Financial credit | **0.30** |

Note: Sources: WGI (WB 2018), Database of Political Institutions (2017), UN (2018), ILO (2018), WDI (2018), OECD (2018).

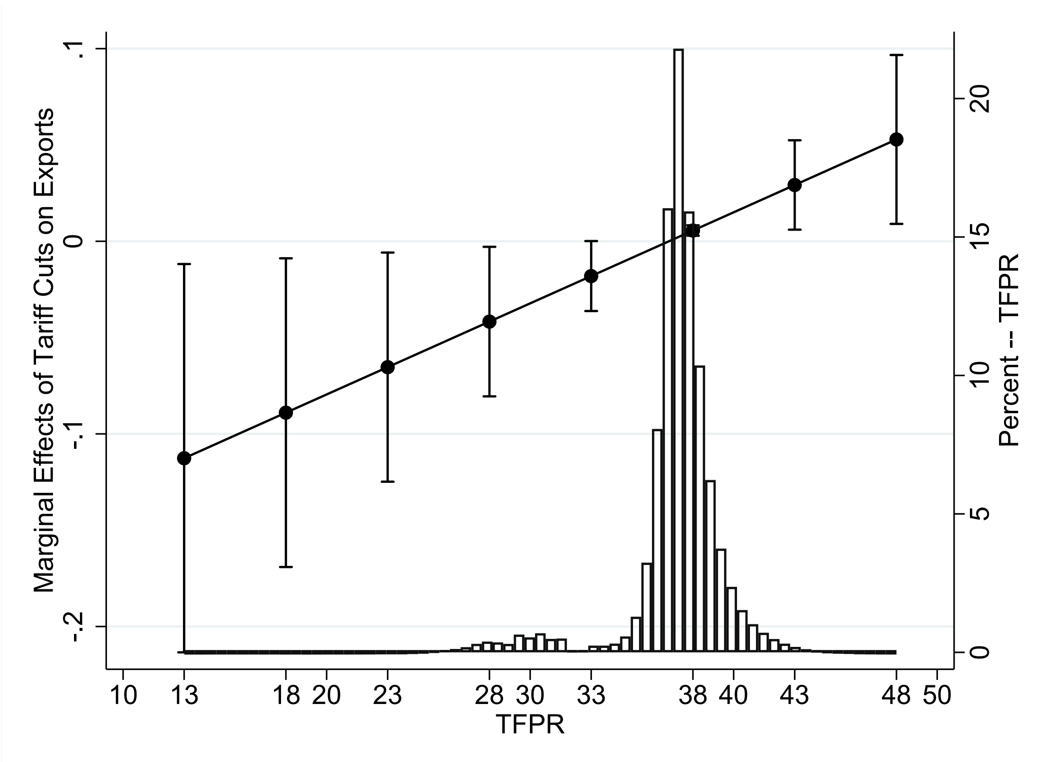
# Appendix D

## Mechanisms

Table D2 reports another test at the firm level, indicating that the cost of labor increases more for LMEs than CMEs after trade liberalization. We use firm-level data capturing the cost of employees over revenue as the dependent variable. We are unable to use the first differences of variable capturing the cost of employees over revenue, since our data are repeated cross-sectionally. Assuming that workers' (other-than-wage) benefits do not change differentially between CMEs and LMEs as a result of tariff reduction, this should be a good proxy for wages. We run models with this variable as outcome and the interaction among *CME*, $\Delta\tau$, and *TFPR* as key independent variables. Results are shown in Table D2. Model 1 shows the results of the baseline model, whereas Model 2 includes industry-specific trends. The coefficient of the main interaction is negative and significant in both models, as expected. All in all, these findings validate the claims that the cost of labor increases differentially more in LMEs than CMEs after trade liberalization.

Moreover, we show that the reallocation effect is indeed triggered by increasing trade activities from the most productive firms. In particular, we regress firms' exports over revenue on the interaction between *TFPR* and $\Delta\tau$. We also include all the controls as in the main models as well as country-year and industry fixed effects. Figure D1 shows that exports over revenue increases after trade liberalization only for the most productive firms. This finding validates the claim that a reduction in preferential tariffs increases the intensive margins of trade for the most productive firms. We also find no effect of PTAs on the extensive margin of trade (see Table D1, Model 1, in Appendix D).

**Figure D1:** The effect of tariff cuts on exports for different levels of firm productivity




Note: The outcome variable is exports over revenue. The graph shows the marginal effect of export tariff cuts on exports for different levels of firm productivity. The model includes country, industry, and year fixed effects. OLS regression with robust standard errors are clustered at the country-year level. The histogram shows the distribution of *TFPR*. 90% C.I.

**Table D1:** Mechnanisms: Trade, Wages, and Employment

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | OLS | | | FracReg |
| | Extensive Margins | Intensive Margins | Hourly Wages | Labor Share |
| CME | | | 1.178*** | -0.259 |
| | | | (0.323) | (0.447) |
| Δτ | -0.004 | -0.174* | 0.090*** | -0.027*** |
| | (0.002) | (0.094) | (0.027) | (0.008) |
| TFPR | 0.019*** | 0.096* | | |
| | (0.001) | (0.056) | | |
| Δτ*CME | | | **-0.095**** | **0.026***** |
| | | | **( 0.035)** | **(0.008)** |
| Δτ*TFPR | **0.000** | **0.005*** | | |
| | **(0.000)** | **(0.003)** | | |
| Constant | -0.929 | -51.682*** | -0.118 | -4.401*** |
| | (7.467) | (3.562) | (0.443) | (0.745) |
| Observations | 537,291 | 535,334 | 354 | 22,157 |
| R-squared | 0.470 | 0.360 | 0.412 | 0.109 |
| Controls | Yes | Yes | No | Yes |
| CountryYear FE | Yes | Yes | No | No |
| Industry FE | Yes | Yes | Yes | Yes |
| Country FE | No | No | Yes | Yes |
| Year FE | No | No | Yes | Yes |

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

Note: OLS regressions (Models 1, 2, and 3) and fractional response model (Model 4). Robust standard errors are clustered at the country-year level. Unit of observation is firm-industry (4-digit NAICS)-country-year in Models 1 and 2 and industry-country-year in Models 3 and 4. The outcome variable is the log of revenue in Models 1 and 2, hourly wages in Model 3, and labor share in Model 4. Sources: Amadeus dataset, Baccini et al. (2018), ILO (2016), and Visser (2016).

**Table D2:** Wages and Cost of Labor

| | (1) | (2) |
|---|---|---|
| | OLS | |
| | Cost of Labour | |
| $\Delta\tau$ | -2.735** | -2.757** |
| | (1.185) | (1.197) |
| TFPR | -1.527*** | -1.535*** |
| | (0.454) | (0.458) |
| TFPR*$\Delta\tau$ | 0.067** | 0.068** |
| | (0.031) | (0.031) |
| $\Delta\tau$*CME | 2.692** | 2.715** |
| | (1.181) | (1.192) |
| TFPR*CME | 1.506*** | 1.515*** |
| | (0.467) | (0.471) |
| **TFPR*$\Delta\tau$*CME** | **-0.066**** | **-0.066**** |
| | **(0.031)** | **(0.031)** |
| Constant | -22.429*** | -3,463.642*** |
| | (6.261) | (990.747) |
| Controls | Yes | Yes |
| CountryYear FE | Yes | Yes |
| Industry FE | Yes | Yes |
| Trends | No | Yes |
| Observations | 3,735,589 | 3,735,589 |
| R-squared | 0.015 | 0.015 |

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

Note: OLS with robust standard errors clustered at the country-year level in parentheses. Unit of observation is firm-industry (4-digit NAICS)-country-year. The outcome variables are wages and cost of employees over revenue. Sources: ILO, Amadeus dataset, Baccini et al. (2018), and Visser (2016).

# Appendix E

## Additional Evidence

**Instrumenting tariffs** To further dissipate concerns that endogeneity of wage bargaining institutions is responsible for our findings, we implement an instrumental variable (IV) approach. Let us explain the logic of our IV strategy. Let's assume that wage bargaining institutions are endogenous to globalization. In other words, it may be that governments implement a set of policies that are related to one another under the pressure of globalization. Hence, when governments implement trade liberalization, they are also likely to reform the labor market in a more liberal way. In other words, let $G$ be government. We may be worried that $G \rightarrow \Delta\tau \rightarrow CME$. Thus, if we find an instrument $I$, which is orthogonal to $G$, we can then claim that $I \rightarrow G \not\rightarrow \Delta\tau \not\rightarrow CME$.

To instrument EU tariff cuts, we rely on tariff cuts implemented by trade competitors of the EU. Indeed, it is well-known that major trade entities compete with each other for preferential market access (Manger 2009, Baccini and Dür 2012). Thus, preferential tariff cuts in the same industries are similar among trade competitors. Since governments of EU countries have little to say on trade policies implemented by trade competitors, preferential tariff cuts implemented by EU trade competitors should prune the endogeneity from EU preferential tariff cuts, at least the endogeneity coming from the role of $G$.

We use preferential tariff cuts implemented by Australia, Canada, China, Japan, South Korea, and the US. More specifically, we build a synthetic measure of tariffs cuts implemented by these trading partners to minimize the difference with EU tariff cuts. By building a synthetic measure across different trading partners, we avoid the risk of relying on only one trading partner to instrument EU tariff cuts. This should further reduce concerns about a possible violation of the exclusion restriction. We label this variable *Z*. To instrument each double interaction with $\Delta\tau$ and the triple interaction term, we interact *Z* with *TFPR* and *CME*. Armed with these instruments, we estimate the following models in the first stage:

$$\begin{aligned}\Delta\tau_{ict} = \gamma_0 + \gamma_1 TFPR_{fic} + \gamma_2 Z_{it-1} + \gamma_3 TFPR_{fic} \times Z_{it-1} + \gamma_4 TFPR_{fic} \times CME_{ct} + \\ \gamma_5 Z_{it-1} \times CME_{ct} + \gamma_6 TFPR_{fic} \times Z_{it-1} \times CME_{ct} + \mathbf{X_{fict}}\gamma^{'} + \mathbf{W_{ict}}\eta^{'} + \delta_{\mathbf{ct}} + \delta_{\mathbf{i}} + \epsilon_{\mathbf{fict}},\end{aligned} \tag{4}$$

$$\begin{aligned}\Delta\tau_{ict} \times TFPR_{fic} = {} & \gamma_0 + \gamma_1 TFPR_{fic} + \gamma_2 Z_{it-1} + \gamma_3 TFPR_{fic} \times Z_{it-1} \\ & + \gamma_4 TFPR_{fic} \times CME_{ct} + \gamma_5 Z_{it-1} \times CME_{ct} \\ & + \gamma_6 TFPR_{fic} \times Z_{it-1} \times CME_{ct} + \mathbf{X_{fict}}\gamma^{'} + \mathbf{W_{ict}}\eta^{'} \\ & + \delta_{ct} + \delta_i + \epsilon_{fict},\end{aligned} \tag{5}$$

$$
\begin{aligned}
\Delta\tau_{ict} \times CME_{ct} = \gamma_0 + \gamma_1 TFPR_{fic} + \gamma_2 Z_{it-1} + \gamma_3 TFPR_{fic} \times Z_{it-1} + \gamma_4 TFPR_{fic} \times CME_{ct} + \\
\gamma_5 Z_{it-1} \times CME_{ct} + \gamma_6 TFPR_{fic} \times Z_{it-1} \times CME_{ct} + \mathbf{X_{fict}}\gamma^{'} + \mathbf{W_{ict}}\eta^{'} \\
+ \delta_{ct} + \delta_i + \epsilon_{fict},
\end{aligned} \tag{6}
$$

$$
\begin{aligned}
\Delta\tau_{ict} \times TFPR_{fic} \times CME_{ct} = \gamma_0 + \gamma_1 TFPR_{fic} + \gamma_2 Z_{it-1} + \gamma_3 TFPR_{fic} \times Z_{it-1} \\
+ \gamma_4 TFPR_{fic} \times CME_{ct} + \gamma_5 Z_{it-1} \times CME_{ct} \\
+ \gamma_6 TFPR_{fic} \times Z_{it-1} \times CME_{ct} + \mathbf{X_{fict}}\gamma^{'} + \mathbf{W_{ict}}\eta^{'} \\
+ \delta_{ct} + \delta_i + \epsilon_{fict},
\end{aligned} \tag{7}
$$

We then plug each of the instrumented variable into our main equation 1 and estimate the following model in the second stage:

$$
\begin{aligned}
Revenue_{fict} = \beta_0 + \beta_1 TFPR_{fic} + \beta_2 \widehat{\Delta\tau_{it-1}} + \beta_3 TFP\widehat{R_{fic} \times} \Delta\tau_{it-1} + \beta_4 TFPR_{fic} \times CME_{ct} + \\
\beta_5 \Delta\tau_{it-\widehat{1 \times C}}ME_{ct} + \beta_6 TFPR_{fic} \times \widehat{\Delta\tau_{it-1}} \times CME_{ct} + \mathbf{X_{fict}}\gamma^{'} + \mathbf{W_{ict}}\eta^{'} \\
+ \delta_{ct} + \delta_i + \epsilon_{fict},
\end{aligned} \tag{8}
$$

Our results hold with the IV estimates and diagnostics show no concerns about weak instruments or under-identification (Table E1, Model 1). In Model 2, we rely on two synthetic instruments: (1) one including the minimum distance between EU tariff cuts and tariff cuts implemented by Australia, Canada, and the US ($Z_1$); (2) one including the minimum distance between EU tariff cuts and tariff cuts implemented by China, Japan, and South Korea ($Z_2$). Even in this case, we interact $Z_1$ and $Z_2$ with *TFPR* and *CME*. Since we have more instruments than instrumented variables, we can test the over-identification assumption as a necessary (but not sufficient) validation of the exclusion restriction. The Hansen J statistic is not significant, i.e. there is no concern about over-identification, and our main results remain unchanged.

**Labor flexibility** We use measures of labor flexibility pertaining to the strictness of regulation of both individual dismissals and collective dismissals, as well as the strictness of regulation of the use of fixed-term and temporary work agency contracts. High values imply a flexible labor market, i.e., it is easy to dismiss workers and to rely on temporary contracts. Data come from the OECD (2016) and are available for all OECD countries over time. We interact these measures of flexibility with $\Delta\tau$ and *TFPR*. While this triple interaction is never significant, the coefficients of our main variables are unchanged (Table E2).

**Automation** We use the data on automation from Acemoglu and Restrepo (2019). The data are from the US, since we are concerned about automation being a function of trade liberalization, which would make automation a bad control. The data are from 1993 and do not vary over time. We use a crosswalk to match SIC industries, which are in the original automation data, to NAICS 4-digit industries, which are in our firm-level dataset. While the coefficient of automation alone is absorbed by industry fixed effects, we are able to estimate the effect of automation by interacting it with firm productivity and labor market institutions (double and triple interaction terms). The triple interaction term among automation, firm productivity, and labor market institutions is positive and significant (Table E3). Importantly, our main results hold even when we include this alternative channel.

**Other labor market institutions** While wage coordination is among the most important institutional features of varieties of capitalism (see Hall and Gingerich 2009; Guardiancich and Guidi 2016), there are other characteristics of the labor market that may be relevant to mediating the distributional consequences of trade liberalization. To address these concerns, we identify other variables from the ICTWSS database: government intervention, authority of unions over affiliates, mandatory extension of collective agreements, sectoral organization of employment relations, authority of unions over local branches, union density, measure of centralization of wage bargaining, and minimum wage. The variables that we analyze as alternative measures of labor market frictions follow. All of them are taken from the ICTWSS database (Visser 2016).

**Government intervention in wage bargaining** An ordinal variable ranging from 1 to 5, measuring the degree to which the government influences wage bargaining, where 1 means no intervention whatsoever and 5 means that the government "imposes private sector wage settlements, places a ceiling on bargaining outcomes or suspends bargaining" (Visser 2015).

**Authority of unions over their affiliates** A proxy measuring the authority of confederations over sectoral or local branches. This variable combines information on whether the confederation is routinely involved in consultation with the government, controls the appointment of affiliates' leaders, is involved in negotiation of the affiliates' wage agreements, has a fund for official strikes, and can veto strikes by affiliates.

**Mandatory extension of collective agreements** Mandatory extension of collective agreements to non-organized employers.

**Sectoral organization of employment relations** An ordinal variable measuring how institutionalized are the relationships between employers and unions at the sectoral level. The possible values are 0 (no institutionalization), 1 (medium institutionalization), and 2 (strong institutionalization).

**Authority of unions over their local branches** Authority of unions over local branches. Additive measure.

**Union density** The percentage of union members out of the total number of employed and salaried workers.

**Centralization of wage bargaining** A composite index that combines information about the predominant level at which wage bargaining takes place, the frequency or scope of additional enterprise bargaining, the possibility of renegotiation of contractual provisions at lower levels, the articulation of enterprise bargaining, and the possibility to derogate to national- or sector-level agreements.

**Minimum wage** National minimum wage is set by agreement.

We interact each of the aforementioned variables with *TFPR* and $\Delta\tau$. Because these variables tend to be highly colinear, we do not include all of them at the same time and we do not include them together with our main triple interaction term. Including these variables leaves our results unchanged (Table E4). Three out of seven triple interactions are significant and have the expected negative sign. More specifically, government intervention in wage bargaining weakens the reallocation effect as well as the authority of confederation over its affiliates and mandatory extension of collective agreements to non-organized employers. These results confirm that labor market frictions help unproductive firms to reduce uneven distributional consequences of trade liberalization through imposing a wage ceiling.

**Different tariff cuts** In the main analysis, we have mostly focused on export tariff cuts. However, there are two other types of tariff cuts, which may be exploited. First, import tariff cuts, i.e., tariff cuts implemented by the EU, increase imports and, in turn, raise competition for domestic firms. In turn, this may reduce prices and so real wages. We build import tariff cuts in the same way as we build export tariff cuts (see Appendix B). Second, input tariff cuts reduce firms' costs of production and, in turn, increase their sales due to cheaper, more competitive goods. In turn, this increases the demand for labor and so wages. To build our measure of input tariffs, we follow Topalova and Khandelwal (2011). Formally, input tariff cuts are given by the following:

$$\text{Input Tariff Cut}_{jt} = \sum_s \text{a}_{js} \times \text{ Import Tariff Cuts}_{st}$$

where $a_{js}$ is the share of input s in the value of output j. Data of share of input come from Input-Output (I-O) tables of EU countries. We use baseline values in 2000, which are available at the 4-digit level.[85]

The effect of other types of tariffs, i.e. import tariff cuts and input tariff cuts. In particular, we rerun the model described in equation 1, replacing export tariff cuts with import tariff cuts and input tariff cuts. Table E5 reports the results of this test. It turns out that import tariff cuts generate no differential reallocation effect between CMEs and LMEs, whereas the coefficient of the triple interaction term is significant in the case of input tariffs, which benefit disproportionally large, productive firms. When foreign inputs become cheaper, multinationals reduce their production costs and therefore expand their sales. This increase in economic activities generates a demand for labor

[85] I-O tables are available at `https://www.exiobase.eu/index.php/data-download/exiobase1-year-2000-sample-files`.

and so an upward pressure on wages. CMEs tame this upward pressure better than LMEs, giving relief to smaller, less productive firms. These findings confirm that in the case of trade policies giving advantages to exports and multinationals, gains from trade among firms are even more in CMEs than in LMEs.

**Table E1:** Instrumenting preferential tariff cuts

| | (1) | (2) |
|---|---|---|
| | 2SLS | |
| | ln(Revenue) | |
| $\Delta\tau$ | -0.492*** | -0.475*** |
| | (0.114) | (0.134) |
| TFPR | 0.400 | 0.402*** |
| | (0.029) | (0.026) |
| TFPR*$\Delta\tau$ | 0.013*** | 0.013*** |
| | (0.003) | (0.004) |
| $\Delta\tau$*CME | 0.477*** | 0.459*** |
| | (0.114) | (0.135) |
| TFPR*CME | -0.043 | -0.046 |
| | (0.038) | (0.035) |
| **TFPR*$\Delta\tau$*CME** | **-0.013*****| **-0.012***** |
| | **(0.003)** | **(0.004)** |
| Controls | Yes | Yes |
| CountryYear FE | Yes | Yes |
| Industry FE | Yes | Yes |
| Underidentification test | 16.571*** | 29.639*** |
| Weak identification test | 45.365*** | 45.973*** |
| Hansen J statistic | | 0.845 |
| Observations | 4,053,929 | 4,053,929 |
| R-squared | 0.631 | 0.631 |

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

Note: OLS with robust standard errors clustered at the country-year level in parentheses. Unit of observation is firm-industry (4-digit NAICS)-country-year. The outcome variable is the log of revenue. Sources: ILO, Amadeus dataset, Baccini et al. (2018), and Visser (2016).

**Table E2:** Including Labor Flexibility

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | OLS | | | | | |
| | ln(Revenue) | | | | | |
| Δτ | -0.272*** | -0.277*** | -0.275*** | -0.269*** | -0.274*** | -0.273*** |
| | (0.100) | (0.101) | (0.101) | (0.100) | (0.101) | (0.100) |
| TFPR | 0.540*** | 0.527*** | 0.521*** | 0.541*** | 0.528*** | 0.522*** |
| | (0.056) | (0.056) | (0.056) | (0.056) | (0.056) | (0.056) |
| TFPR*Δτ | 0.007*** | | 0.008*** | 0.007*** | | 0.008*** |
| | (0.003) | | (0.003) | (0.003) | | (0.003) |
| Δτ*CME | 0.145* | 0.137* | 0.135* | 0.140* | 0.132* | 0.131* |
| | (0.080) | (0.079) | (0.079) | (0.079) | (0.079) | (0.078) |
| TFPR*CME | 0.028 | 0.018 | 0.011 | 0.027 | 0.017 | 0.010 |
| | (0.031) | (0.032) | (0.033) | (0.031) | (0.031) | (0.033) |
| **TFPR*Δτ*CME** | **-0.004*** | **-0.004*** | **-0.004*** | **-0.004*** | **-0.004*** | **-0.004*** |
| | **(0.002)** | **(0.002)** | **(0.002)** | **(0.002)** | **(0.002)** | **(0.002)** |
| Δτ*Wage Ceiling | | 0.266*** | 0.266*** | | 0.267*** | 0.267*** |
| | | (0.035) | (0.034) | | (0.035) | (0.035) |
| TFPR*Wage Ceiling | | 0.194*** | 0.201*** | | 0.194*** | 0.201*** |
| | | (0.037) | (0.038) | | (0.037) | (0.038) |
| **Δτ*TFPR*Wage Ceiling** | | **-0.007***** | **-0.007***** | | **-0.007***** | **-0.007***** |
| | | **(0.001)** | **(0.001)** | | **(0.001)** | **(0.001)** |
| Δτ*Subsidies for CVT | | | 0.092*** | | | 0.089*** |
| | | | (0.025) | | | (0.024) |
| TFPR*Subsidies for VT | | | 0.096** | | | 0.096** |
| | | | (0.047) | | | (0.047) |
| **Δτ*TFPR*Subsidies for VT** | | | **-0.002***** | | | **-0.002***** |
| | | | **(0.001)** | | | **(0.001)** |
| Δτ*Labour Flexibility | 0.045 | 0.050 | 0.051 | 0.046 | 0.051 | 0.051 |
| | (0.042) | (0.043) | (0.043) | (0.042) | (0.043) | (0.043) |
| TFPR*Labour Flexibility | -0.059** | -0.052* | -0.048* | -0.058** | -0.052* | -0.048* |
| | (0.028) | (0.028) | (0.029) | (0.028) | (0.028) | (0.029) |
| **Δτ*TFPR*Labour Flexibility** | **-0.001** | **-0.001** | **-0.001** | **-0.001** | **-0.001** | **-0.001** |
| | **(0.001)** | **(0.001)** | **(0.001)** | **(0.001)** | **(0.001)** | **(0.001)** |
| Constant | -10.564*** | -10.309*** | -10.099*** | -287.449* | -285.299* | -285.953* |
| | (0.786) | (0.809) | (0.858) | (164.788) | (164.778) | (164.320) |
| Observations | 2,846,018 | 2,846,018 | 2,846,018 | 2,846,018 | 2,846,018 | 2,846,018 |
| R-squared | 0.809 | 0.809 | 0.809 | 0.809 | 0.810 | 0.810 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| CountryYear FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Trends | No | No | No | Yes | Yes | Yes |

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

Note: OLS with robust standard errors clustered at the country-year level in parentheses. Unit of observation is firm-industry (4-digit NAICS)-country-year. The outcome variable is the log of revenue. Sources: ILO, Amadeus dataset, Baccini et al. (2018), and Visser (2016).

**Table E3:** Including Automation

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | OLS | | | | | |
| | ln(Revenue) | | | | | |
| Δτ | -0.366*** | -0.365*** | -0.368*** | -0.361** | -0.361** | -0.364** |
| | (0.139) | (0.139) | (0.139) | (0.141) | (0.140) | (0.140) |
| TFPR | 0.434*** | 0.435*** | 0.433*** | 0.435*** | 0.436*** | 0.434*** |
| | (0.022) | (0.022) | (0.022) | (0.022) | (0.022) | (0.022) |
| TFPR*Δτ | 0.010*** | 0.010*** | 0.010*** | 0.010** | 0.010*** | 0.010*** |
| | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) |
| Δτ*CME | 0.347** | 0.344** | 0.355** | 0.343** | 0.340** | 0.351** |
| | (0.140) | (0.140) | (0.139) | (0.141) | (0.141) | (0.140) |
| TFPR*CME | -0.075** | -0.072** | -0.053 | -0.076** | -0.073** | -0.054 |
| | (0.034) | (0.036) | (0.036) | (0.033) | (0.035) | (0.035) |
| **TFPR*Δτ*CME** | **-0.009**** | **-0.009**** | **-0.010**** | **-0.009**** | **-0.009**** | **-0.009**** |
| | **(0.004)** | **(0.004)** | **(0.004)** | **(0.004)** | **(0.004)** | **(0.004)** |
| Δτ*Wage Ceiling | | 0.106** | 0.093** | | 0.103** | 0.090** |
| | | (0.045) | (0.045) | | (0.046) | (0.045) |
| TFPR*Wage Ceiling | | -0.034 | -0.049 | | -0.036 | -0.051 |
| | | (0.052) | (0.053) | | (0.052) | (0.053) |
| **Δτ*TFPR*Wage Ceiling** | | **-0.003**** | **-0.003**** | | **-0.003**** | **-0.002**** |
| | | **(0.001)** | **(0.001)** | | **(0.001)** | **(0.001)** |
| Δτ*Subsidies for CVT | | | 0.095*** | | | 0.088*** |
| | | | (0.020) | | | (0.021) |
| TFPR*Subsidies for VT | | | 0.122*** | | | 0.120*** |
| | | | (0.036) | | | (0.036) |
| **Δτ*TFPR*Subsidies for VT** | | | **-0.002***** | | | **-0.002***** |
| | | | **(0.001)** | | | **(0.001)** |
| TFPR*Automation | -0.003*** | -0.004*** | -0.003** | -0.003*** | -0.004*** | -0.003** |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| Automation*CME | -0.039** | -0.043** | -0.032* | -0.039** | -0.043** | -0.032* |
| | (0.018) | (0.017) | (0.017) | (0.018) | (0.017) | (0.017) |
| **TFPR*Automation*CME** | **0.001**** | **0.001**** | **0.001*** | **0.001**** | **0.001**** | **0.001*** |
| | **(0.000)** | **(0.000)** | **(0.000)** | **(0.000)** | **(0.000)** | **(0.000)** |
| Constant | -8.542*** | -8.700*** | -9.117*** | -162.247*** | 103.448** | 204.364*** |
| | (0.883) | (0.969) | (1.041) | (54.833) | (50.959) | (66.292) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| CountryYear FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Trends | No | No | No | Yes | Yes | Yes |
| Observations | 4,053,929 | 4,032,150 | 3,918,518 | 4,053,929 | 4,032,150 | 3,918,518 |
| R-squared | 0.766 | 0.767 | 0.775 | 0.766 | 0.767 | 0.775 |

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

Note: OLS with robust standard errors clustered at the country-year level in parentheses. Unit of observation is firm-industry (4-digit NAICS)-country-year. The outcome variable is the log of revenue. Sources: ILO, Amadeus dataset, Baccini et al. (2018), and Visser (2016).

**Table E4:** Including other labor market institutions

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | OLS | | | | | | | |
| | ln(Revenue) | | | | | | | |
| **TFPR*Δτ*CME** | **-0.010**** | **-0.008**** | **-0.009**** | **-0.009**** | **-0.010***** | **-0.011***** | **-0.009***** | **-0.010***** |
| | **(0.004)** | **(0.004)** | **(0.004)** | **(0.004)** | **(0.004)** | **(0.003)** | **(0.003)** | **(0.004)** |
| TFPR*Δτ*Govt. Intervention | -0.001 | | | | | | | |
| | (0.001) | | | | | | | |
| TFPR*Δτ*Authority over Affliates | | -0.001 | | | | | | |
| | | (0.004) | | | | | | |
| TFPR*Δτ*Mandatory Extension of Collective Agreements | | | -0.0003** | | | | | |
| | | | (0.000) | | | | | |
| TFPR*Δτ*Sectoral Organiz. | | | | -0.000 | | | | |
| | | | | (0.001) | | | | |
| TFPR*Δτ*Authority over Local Branches | | | | | 0.003 | | | |
| | | | | | (0.004) | | | |
| TFPR*Δτ*Union Density | | | | | | 0.000 | | |
| | | | | | | (0.000) | | |
| TFPR*Δτ*Centralization | | | | | | | 0.012 | |
| | | | | | | | (0.010) | |
| TFPR*Δτ*Minimum Wage | | | | | | | | -0.000 |
| | | | | | | | | (0.000) |
| Constant | -9.275*** | -9.600*** | -9.367*** | -9.414*** | -9.157*** | -10.917*** | -10.431*** | -9.563*** |
| | (0.818) | (0.687) | (0.661) | (0.783) | (0.786) | (0.963) | (0.844) | (0.741) |
| Observations | 4,032,144 | 3,934,884 | 4,042,889 | 3,956,663 | 3,934,884 | 2,897,040 | 2,470,577 | 3,933,830 |
| R-squared | 0.767 | 0.770 | 0.768 | 0.769 | 0.770 | 0.784 | 0.781 | 0.774 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| CountryYear FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Trends | No | No | No | No | No | No | No | No |

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

Note: OLS with robust standard errors clustered at the country-year level in parentheses. Unit of observation is firm-industry (4-digit NAICS)-country-year. The outcome variable is the log of revenue. Sources: ILO, Amadeus dataset, Baccini et al. (2018), and Visser (2016).

**Table E5:** Including other Types of Tariffs

| | (1) | (2) |
|---|---|---|
| | OLS | |
| | ln(Revenue) | |
| $\Delta\tau$ (import) | -0.544** | |
| | (0.238) | |
| $\Delta\tau$ (input) | | 1.416 |
| | | (1.019) |
| TFPR | 0.469*** | 0.494*** |
| | (0.039) | (0.030) |
| CME*TFPR | -0.120*** | -0.149*** |
| | (0.046) | (0.039) |
| $\Delta\tau$ (import)*TFPR | 0.015** | |
| | (0.006) | |
| $\Delta\tau$ (input)*TFPR | | 0.037 |
| | | (0.027) |
| CME*$\Delta\tau$ (import) | 0.040 | |
| | (0.314) | |
| CME*$\Delta\tau$ (input) | | 2.739** |
| | | (1.118) |
| **CME*$\Delta\tau$ (import)*TFPR** | **-0.001** | |
| | **(0.008)** | |
| **CME*$\Delta\tau$ (inpu)*TFPR** | | **-0.073**** |
| | | **(0.029)** |
| Constant | -9.083*** | -8.959*** |
| | (0.773) | (0.760) |
| Observations | 4,053,923 | 4,032,144 |
| R-squared | 0.766 | 0.767 |
| Controls | Yes | Yes |
| CountryYear FE | Yes | Yes |
| Industry FE | Yes | Yes |

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

Note: OLS with robust standard errors clustered at the country-year level in parentheses. Unit of observation is firm-industry (4-digit NAICS)-country-year. The outcome variable is the log of revenue. Sources: ILO, Amadeus dataset, Baccini et al. (2018), and Visser (2016).

# Appendix F

## Robustness Checks

**Interaction term diagnostics** Regarding the interaction term, we follow best practices as recommended by Hainmueller et al. (2019). In particular, we show that our results are not sensitive to nonlinearity issues and there is no concern of lack of common support of our moderating variable, i.e., *TFPR*. Note that we are unable to use the command "interflex" developed by Hainmueller and colleagues (2019), since the command does not extend to triple interaction terms like ours. Thus, in performing these checks, we modify Hainmueller et al.'s tests and extend them to a design like ours including triple interaction terms (rather than double interaction terms). In particular, we implement the following checks:

- We recoded *TFPR* in an ordinal variable with eight values. We recoded the original variable in a eight-value ordinal variable, using the command "binsregselect," which implements a data-driven number of bins selectors using either quantile-spaced or evenly-spaced binning. The command has been recently developed by Cattaneo and colleagues. Using an ordinal variable reduces the probability of lack of common support of the moderator, since there are several observations for both CMEs and LMEs in each category. Results are very similar to the estimates with a continuous *TFPR* (Figure F1).

- We run a binning estimator as suggested by Hainmueller et al (2019: 170-71). To avoid estimating a quadruple interaction terms, which would be difficult to interpret, we estimate two regressions for a bin with low-productivity firms and a bin with high-productivity firms. We avoid estimating a medium category, since there is limited variation in the middle of the distribution of *TFPR*. Using two bins has also the advantage of very conservative test of the lack of common support of the moderator, since two bins include a very large number of observations for both CMEs and LMEs. Crucially, the triple interaction term is negative and significant in both bins. The effect of the triple interaction is larger in the low-productivity bin compared to the high-productivity bin (Table F1). This is in line with Figure 3, in which the largest difference in the linear estimates is for low-productivity firms. In short, the binning estimator shows no concern about nonlinearity or lack of common support of the moderator.

- We re-run our main model using the kernel-based regularized least squares (KRLS), developed by Hainmueller and Hazlett (2014). KRLS allows researchers to tackle regression and classification problems without strong functional form assumptions or a specification search. For our purpose, this estimation technique allows us to check whether nonlinearity issues of the triple interactions are driving our results. Put simply, leaving out an important function of the interaction can result in the same type of omitted variable bias as failing to include an important unobserved confounding variable. Results are shown in Table F2 and are similar to the results of the OLS regressions, reported in Table 1 and Figure 3.[86] In sum, there is no evidence that a nonlinear

[86]For computational reasons, we run the KRLS model on a small subsample of the data. Even with this relatively

interaction effect is responsible for our results.

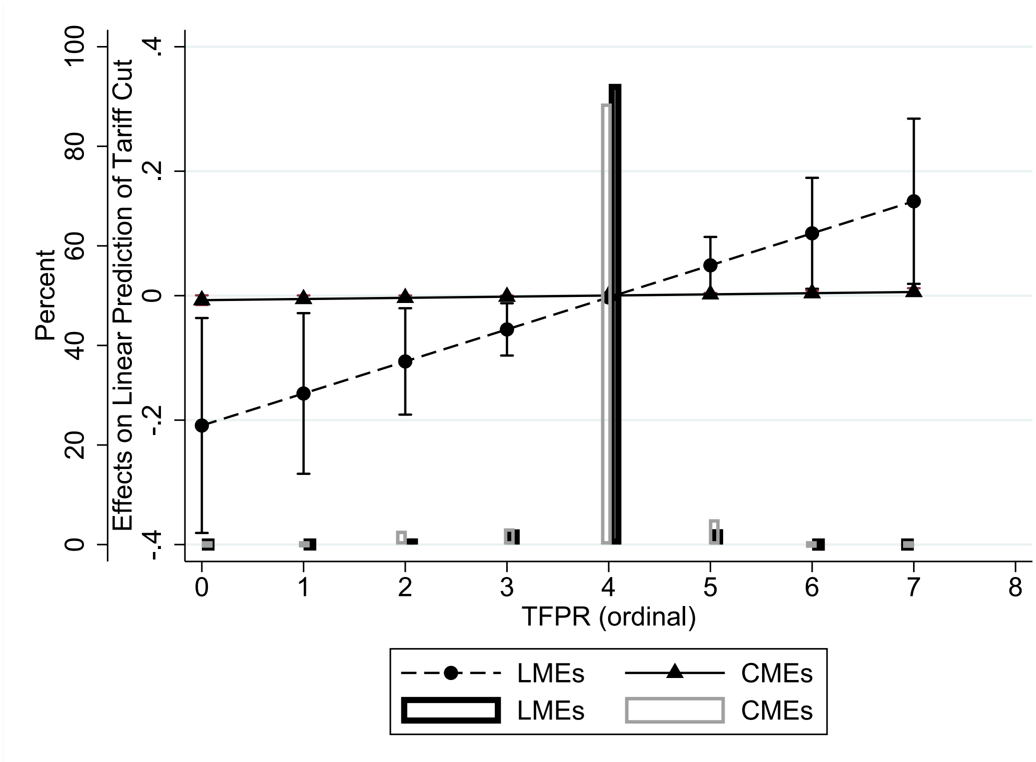
**Sample issues** Regarding sample issues, we show that our results are similar if we run our main models with the aforementioned weights in Kalemli-Ozcan et al. (2017). Moreover, we run our main model for each country to document transparently which countries drive our results. Furthermore, our results hold if we run our main model, dropping one LME at a time. All these tests are reported in Tables F3, F4, and F5.

**Additional model specifications** Regarding model specifications, we show that our results are robust to the inclusion of firm fixed effects. We do not include firm fixed effects in the main model, because our data are repeated cross-sectionally. Note that by including firm fixed effects, we are unable to estimate *TFPR*, which does not change across firms over time. In addition, we show that results are similar when we include preferential tariff cuts prior to 2003 together with $\Delta\tau$. In particular, we use 1995-2003 preferential tariff cuts in interaction with *TFPR* and *CME*. Finally, since our measure of productivity is residuals, there may be concern about our error terms being correlated. To address this issue, we show that our main results are similar if we bootstrap standard errors. All these tests are reported in Tables F6, F7, and F8.

---

low number of observations, the model takes more than 24 hours to run on a powerful computer.

**Figure F1:** The effect of tariff cuts on firm revenue for different levels of firm productivity (ordinal measure) in CMEs and LMEs




Note: The predictions are plotted from Model 1 in Table 1. LME includes countries with "fragmented wage bargaining, confined largely to individual firms or plants. CME includes countries with "mixed industry and firm-level bargaining, weak government coordination through MW setting or wage indexation," "negotiation guidelines based on centralized bargaining," "wage norms based on centralized bargaining by peak associations with or without government involvement," and "maximum or minimum wage rates/increases based on centralized bargaining." The histogram shows the distribution of *TFPR* (ordinal measure) for both CMEs and LMEs. 99% C.I.

**Table F1:** Binning estimator

| | (1) | (2) |
|---|---|---|
| | OLS | |
| | ln(Revenue) | |
| | Bin 1 (low TFPR) | Bin 2 (high TFPR) |
| Δτ | 0.008** | 0.003 |
| | (0.004) | (0.002) |
| TFPR X1 | 0.422*** | |
| | (0.037) | |
| TFPR X1*Δτ | 0.011** | |
| | (0.005) | |
| Δτ*CME | -0.008* | -0.003 |
| | (0.004) | (0.002) |
| TFPR X1*CME | -0.047 | |
| | (0.049) | |
| **TFPR X1*Δτ*CME** | **-0.010**** | |
| | **(0.005)** | |
| TFPR X2 | | 0.297*** |
| | | (0.015) |
| TFPR X2*Δτ | | 0.006*** |
| | | (0.002) |
| TFPR X2*CME | | -0.073*** |
| | | (0.022) |
| **TFPR X2*Δτ*CME** | | **-0.005***** |
| | | **(0.002)** |
| Constant | 4.528*** | 5.205*** |
| | (0.307) | (0.129) |
| Controls | Yes | Yes |
| CountryYear FE | Yes | Yes |
| Industry FE | Yes | Yes |
| Trends | No | No |
| Observations | 2,021,591 | 2,032,338 |
| R-squared | 0.777 | 0.762 |

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

Note: Binning estimator with standard errors clustered at the country-year level in parentheses. Unit of observation is firm-industry (4-digit NAICS)-country-year. The outcome variable in all models is the log of revenue. Sources: Amadeus dataset, Baccini et al. (2018), and Visser (2016).

**Table F2:** Kernel-Based Regularized Least Squares

| | (1) |
|---|---|
| | KRLS |
| | ln(Revenue) |
| $\Delta\tau$ | -0.0004 |
| | -0.001 |
| TFPR | 0.075*** |
| | -0.011 |
| TFPR*$\Delta\tau$ | 0.00003 |
| | (0.00003) |
| $\Delta\tau$*CME | -0.004*** |
| | (0.001) |
| TFPR*CME | 0.0003*** |
| | (0.0001) |
| **TFPR*$\Delta\tau$*CME** | **-0.00009*** ** |
| | **(0.00003)** |
| Controls | Yes |
| CountryYear FE | Yes |
| Industry FE | Yes |
| Observations | 4,053,923 |
| Lambda | 0.054 |
| Tolerance | 0.407 |
| Sigma | 215 |
| Looloss | 509.8 |
| R-squared | 0.985 |

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

Note: KRLS with robust standard errors clustered at the country-year level. Unit of observation is firm-industry (4-digit NAICS)-country-year. The outcome variable in all models is the log of revenue. Sources: Amadeus dataset, Baccini et al. (2018), and Visser (2016).

**Table F3:** Reallocation effect with weighted estimates

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | OLS | | | | | |
| | ln(Revenue) | | | | | |
| $\Delta\tau$ | -0.384** | -0.384** | -0.385** | -0.380** | -0.379** | -0.381** |
| | (0.152) | (0.152) | (0.152) | (0.154) | (0.154) | (0.154) |
| TFPR | 0.413*** | 0.413*** | 0.413*** | 0.414*** | 0.415*** | 0.414*** |
| | (0.020) | (0.021) | (0.020) | (0.021) | (0.021) | (0.020) |
| TFPR*$\Delta\tau$ | 0.010** | | 0.010** | 0.010** | | 0.010** |
| | (0.004) | | (0.004) | (0.004) | | (0.004) |
| $\Delta\tau$*CME | 0.367** | 0.364** | 0.374** | 0.363** | 0.360** | 0.371** |
| | (0.153) | (0.153) | (0.152) | (0.154) | (0.154) | (0.154) |
| TFPR*CME | -0.070** | -0.066* | -0.050 | -0.070** | -0.067* | -0.051 |
| | (0.034) | (0.036) | (0.037) | (0.033) | (0.035) | (0.036) |
| **TFPR*$\Delta\tau$*CME** | **-0.010**** | **-0.010**** | **-0.010**** | **-0.010**** | **-0.010**** | **-0.010**** |
| | **(0.004)** | **(0.004)** | **(0.004)** | **(0.004)** | **(0.004)** | **(0.004)** |
| $\Delta\tau$*Wage Ceiling | | 0.091** | 0.083** | | 0.087** | 0.080** |
| | | (0.041) | (0.040) | | (0.041) | (0.041) |
| TFPR*Wage Ceiling | | -0.023 | -0.039 | | -0.025 | -0.041 |
| | | (0.051) | (0.051) | | (0.050) | (0.051) |
| **$\Delta\tau$*TFPR*Wage Ceiling** | | **-0.003**** | **-0.002**** | | **-0.002**** | **-0.002**** |
| | | **(0.001)** | **(0.001)** | | **(0.001)** | **(0.001)** |
| $\Delta\tau$*Subsidies for CVT | | | 0.077*** | | | 0.070*** |
| | | | (0.022) | | | (0.024) |
| TFPR*Subsidies for VT | | | 0.131*** | | | 0.129*** |
| | | | (0.039) | | | (0.039) |
| **$\Delta\tau$*TFPR*Subsidies for VT** | | | **-0.002***** | | | **-0.002***** |
| | | | **(0.001)** | | | **(0.001)** |
| Constant | -7.987*** | -8.110*** | -8.478*** | 489.891*** | 556.793*** | 18.745*** |
| | (0.873) | (0.970) | (1.057) | (30.975) | (94.609) | (42.355) |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| CountryYear FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Trends | No | No | No | Yes | Yes | Yes |
| Weight | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 4,051,865 | 4,030,086 | 3,916,454 | 4,051,865 | 4,030,086 | 3,916,454 |
| R-squared | 0.761 | 0.762 | 0.771 | 0.762 | 0.763 | 0.772 |

Robust standard errors in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Note: OLS with standard errors clustered at the country-year level in parentheses and weights from Kalemli-Ozcan et al. (2017). Unit of observation is firm-industry (4-digit NAICS)-country-year. The outcome variable in all models is the log of revenue. Sources: Amadeus dataset, Visser (2016), and Kalemli-Ozcan et al. (2017).

**Table F4:** Reallocation effect by country

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | OLS | | | | | | | | | | | | | |
| | ln(Revenue) | | | | | | | | | | | | | |
| | AUT | BEL | BGR | CYP | CZE | DEU | DNK | EST | ESP | FIN | FRA | GBR | GRC | HRV |
| Δτ*TFPR | 0.016*** | 0.004 | 0.005* | -0.017** | 0.003 | 0.004*** | 0.035*** | 0.002 | 0.000** | -0.004 | 0.000* | 0.011*** | 0.005 | 0.004 |
| | (0.005) | (0.004) | (0.003) | (0.008) | (0.003) | (0.001) | (0.010) | (0.002) | (0.000) | (0.003) | (0.000) | (0.003) | (0.004) | (0.005) |
| Constant | -7.253*** | -11.529*** | -14.187*** | -12.281*** | -5.696*** | -10.725*** | -23.849*** | -9.888*** | -9.748*** | -13.757*** | -8.485*** | -6.942*** | -12.075*** | -6.262*** |
| | (1.507) | (1.278) | (1.672) | (4.336) | (1.341) | (0.555) | (2.012) | (0.579) | (0.513) | (1.212) | (0.675) | (2.022) | (1.391) | (0.894) |
| Observations | 24,330 | 146,189 | 232,351 | 810 | 165,732 | 182,317 | 4,152 | 73,765 | 848,850 | 74,545 | 344,662 | 143,098 | 36,856 | 44,747 |
| R-squared | 0.920 | 0.510 | 0.688 | 0.870 | 0.511 | 0.933 | 0.810 | 0.777 | 0.864 | 0.801 | 0.923 | 0.817 | 0.744 | 0.643 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

| | (15) | (16) | (17) | (18) | (19) | (20) | (21) | (22) | (23) | (24) | (25) | (26) | (27) | (28) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | HUN | IRL | ITA | LTU | LUX | LVA | MLT | NLD | POL | PRT | ROM | SWE | SVN | SVK |
| Δτ*TFPR | 0.008** | 0.002 | 0.001*** | 0.008 | 0.015 | 0.018*** | 0.002 | -0.004 | -0.002 | 0.001 | -0.001 | 0.002 | 0.009*** | 0.009*** |
| | (0.004) | (0.006) | (0.000) | (0.005) | (0.016) | (0.007) | (0.009) | (0.003) | (0.001) | (0.002) | (0.003) | (0.002) | (0.002) | (0.003) |
| Constant | -6.978*** | -11.059*** | -11.102*** | -11.000*** | -11.436 | -12.103*** | -18.323*** | -3.785*** | -16.163*** | -8.749*** | -6.889*** | -0.517 | -9.644*** | -13.681*** |
| | (1.053) | (2.186) | (0.489) | (2.145) | (7.381) | (1.383) | (4.437) | (1.195) | (0.716) | (1.455) | (1.511) | (0.499) | (0.760) | (1.422) |
| Observations | 236,654 | 5,818 | 889,828 | 17,096 | 977 | 86,397 | 1,375 | 9,744 | 65,285 | 394,768 | 647,420 | 303,197 | 56,332 | 97,998 |
| R-squared | 0.767 | 0.910 | 0.703 | 0.838 | 0.686 | 0.653 | 0.772 | 0.820 | 0.860 | 0.596 | 0.529 | 0.730 | 0.902 | 0.621 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

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

Note: OLS with robust standard errors clustered at the country-year level in parentheses. Unit of observation is firm-industry (4-digit NAICS)-year. The outcome variable is the log of revenue. Sources: ILO, Amadeus dataset, Baccini et al. (2018), and Visser (2016).

**Table F5:** Reallocation effect (dropping one LME at the time)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | OLS | | | | | | | |
| | ln(Revenue) | | | | | | | |
| $\Delta\tau$ | -0.367** | -0.402*** | -0.416*** | -0.372*** | -0.373*** | -0.210*** | -0.372** | -0.373*** |
| | (0.145) | (0.155) | (0.132) | (0.141) | (0.144) | (0.067) | (0.173) | (0.144) |
| TFPR | 0.428*** | 0.403*** | 0.432*** | 0.418*** | 0.418*** | 0.438*** | 0.407*** | 0.418*** |
| | (0.023) | (0.022) | (0.025) | (0.020) | (0.020) | (0.022) | (0.018) | (0.020) |
| $\Delta\tau$*TFPR | 0.010** | 0.011*** | 0.011*** | 0.010*** | 0.010*** | 0.006*** | 0.010** | 0.010*** |
| | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) | (0.002) | (0.005) | (0.004) |
| $\Delta\tau$*CME | 0.348** | 0.384** | 0.398*** | 0.354** | 0.355** | 0.192*** | 0.354** | 0.355** |
| | (0.146) | (0.155) | (0.133) | (0.142) | (0.144) | (0.068) | (0.173) | (0.144) |
| CME*TFPR | -0.072** | -0.047 | -0.077** | -0.063* | -0.062* | -0.082** | -0.051 | -0.062* |
| | (0.034) | (0.033) | (0.036) | (0.032) | (0.032) | (0.034) | (0.031) | (0.032) |
| **CME*$\Delta\tau$*TFPR** | **-0.009**** | **-0.010**** | **-0.011***** | **-0.010**** | **-0.010**** | **-0.005***** | **-0.010**** | **-0.010**** |
| | **(0.004)** | **(0.004)** | **(0.004)** | **(0.004)** | **(0.004)** | **(0.002)** | **(0.005)** | **(0.004)** |
| Constant | -9.031*** | -9.014*** | -9.075*** | -9.019*** | -9.011*** | -8.944*** | -8.954*** | -9.011*** |
| | (0.764) | (0.779) | (0.776) | (0.756) | (0.758) | (0.762) | (0.768) | (0.758) |
| Observations | 3,994,419 | 3,940,839 | 3,877,552 | 4,049,459 | 4,039,509 | 3,993,449 | 3,991,567 | 4,039,509 |
| R-squared | 0.765 | 0.762 | 0.742 | 0.766 | 0.766 | 0.765 | 0.763 | 0.766 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Industry FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Robust standard errors in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Note: OLS with robust standard errors clustered at the country-year level in parentheses. Unit of observation is firm-industry (4-digit NAICS)-country-year. The outcome variable is the log of revenue. Sources: ILO, Amadeus dataset, Baccini et al. (2018), and Visser (2016).

**Table F6:** Reallocation effect with firm fixed effects

| | (1) |
|---|---|
| | OLS |
| | ln(Revenue) |
| $\Delta\tau$ | -0.949*** |
| | (0.228) |
| TFPR*$\Delta\tau$ | 0.026*** |
| | (0.006) |
| $\Delta\tau$*CME | 0.909*** |
| | (0.229) |
| TFPR*CME | 0.361*** |
| | (0.068) |
| **TFPR*$\Delta\tau$*CME** | **-0.024*** ** |
| | **(0.006)** |
| Costant | -8.887*** |
| | (2.313) |
| Observations | 3,941,162 |
| R-squared | 0.881 |
| Controls | Yes |
| CountryYear FE | Yes |
| Industry FE | Yes |
| Firm FE | Yes |

Robust standard errors in parentheses *** p<0.01, ** p<0.05

Note: OLS with standard errors clustered at the country-year level in parentheses and weights from Kalemli-Ozcan et al. (2017). Unit of observation is firm-industry (4-digit NAICS)-country-year. The outcome variable in all models is the log of revenue. Sources: Amadeus dataset and Visser (2016).

**Table F7:** Reallocation effect with pre-2003 tariff cuts

| | (1) | (2) |
|---|---|---|
| | OLS | |
| | ln(Revenue) | |
| Δτ | -0.361** | -0.356** |
| | (0.140) | (0.141) |
| TFPR | 0.418*** | 0.420*** |
| | (0.020) | (0.020) |
| TFPR*Δτ | 0.010** | 0.010** |
| | (0.004) | (0.004) |
| Δτ*CME | 0.343** | 0.339** |
| | (0.140) | (0.141) |
| TFPR*CME | -0.063* | -0.064** |
| | (0.032) | (0.032) |
| **TFPR*Δτ*CME** | **-0.009**** | **-0.009**** |
| | **(0.004)** | **(0.004)** |
| Δτ (pre-2003) | -0.010** | -0.010** |
| | (0.005) | (0.005) |
| Δτ (pre-2003)*TFPR | 0.000** | 0.000** |
| | (0.000) | (0.000) |
| Δτ (pre-2003)*CME | 0.007 | 0.007 |
| | (0.005) | (0.005) |
| **TFPR*Δτ (pre-2003)*CME** | **-0.000** | **-0.000** |
| | **(0.000)** | **(0.000)** |
| Constant | -8.407*** | -161.663*** |
| | (0.824) | (54.665) |
| Controls | Yes | Yes |
| CountryYear FE | Yes | Yes |
| Industry FE | Yes | Yes |
| Trends | No | Yes |
| Observations | 4,053,929 | 4,053,929 |
| R-squared | 0.766 | 0.766 |

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

Note: OLS with standard errors clustered at the country-year level in parentheses and weights from Kalemli-Ozcan et al. (2017). Unit of observation is firm-industry (4-digit NAICS)-country-year. The outcome variable in all models is the log of revenue. Sources: Amadeus dataset and Visser (2016).

**Table F8:** Reallocation effect with bootstrapped standard errors

| | (1) | (2) |
|---|---|---|
| | OLS | |
| | ln(Revenue) | |
| $\Delta\tau$ | -0.372*** | -0.371** |
| | (0.141) | (0.165) |
| TFPR | 0.422*** | 0.422*** |
| | (0.021) | (0.022) |
| TFPR*$\Delta\tau$ | 0.010*** | 0.010** |
| | (0.004) | (0.004) |
| $\Delta\tau$*CME | 0.348** | 0.347** |
| | (0.137) | (0.168) |
| TFPR*CME | -0.073** | -0.073** |
| | (0.029) | (0.035) |
| **TFPR*$\Delta\tau$*CME** | **-0.009**** | **-0.009**** |
| | **(0.004)** | **(0.004)** |
| Constant | -11.509*** | 58.538* |
| | (0.858) | (34.144) |
| Controls | Yes | Yes |
| CountryYear FE | Yes | Yes |
| Industry FE | Yes | Yes |
| Trends | No | Yes |
| Observations | 805,697 | 805,697 |
| R-squared | 0.767 | 0.767 |

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

Note: OLS with bootstrapped standard errors in parentheses. Unit of observation is firm-industry (4-digit NAICS)-country-year. The outcome variable in all models is the log of revenue. Sources: Amadeus dataset and Visser (2016).

# Appendix G

## Geocoding Amadeus

Geocoding Amadeus was performed differently for each country. There is no standardized method, as each Amadeus dataset had different values in terms of the geographic variables. First, we looked at the postal code (zip code) variable. Eurostat provides postal codes to NUTS region tables for each country in the European Union; however, in many cases the matches were geographically inaccurate. The postal code was still useful in some cases, especially in countries with relatively well-documented postal code systems. We then resorted to the region variable provided in Amadeus, which contains the general region in which a firm is located. The entries in the region variable often matched with a NUTS-2 or -3 level name. In most cases, if a country had NUTS-3 names within the region variable, a simple merge was performed. In other countries the region variable was finer in scale, corresponding to local administrative units, which are used by Eurostat to a lesser extent. Again, once the administrative level used in the region variable was identified, a merge was performed.

In the rare case where the region did not match any of the official Eurostat tables, we resorted to official country statistics websites to determine which administrative levels were used. Geocoding based on the region variable covered most of the Amadeus observations, and if a dataset was incomplete, we used a combination of the city and region variables to geocode. This combination was used to prevent any errors which may have arisen due to duplicate city names in certain countries. String matching based on city and region was performed with the help of data from Geonames, a free geographic database which covers all countries and place names (`https://www.geonames.org/`). These datasets contain the relevant administrative boundaries, which often matched Eurostat's NUTS-2 or -3 official names, and again a simple merge was performed.

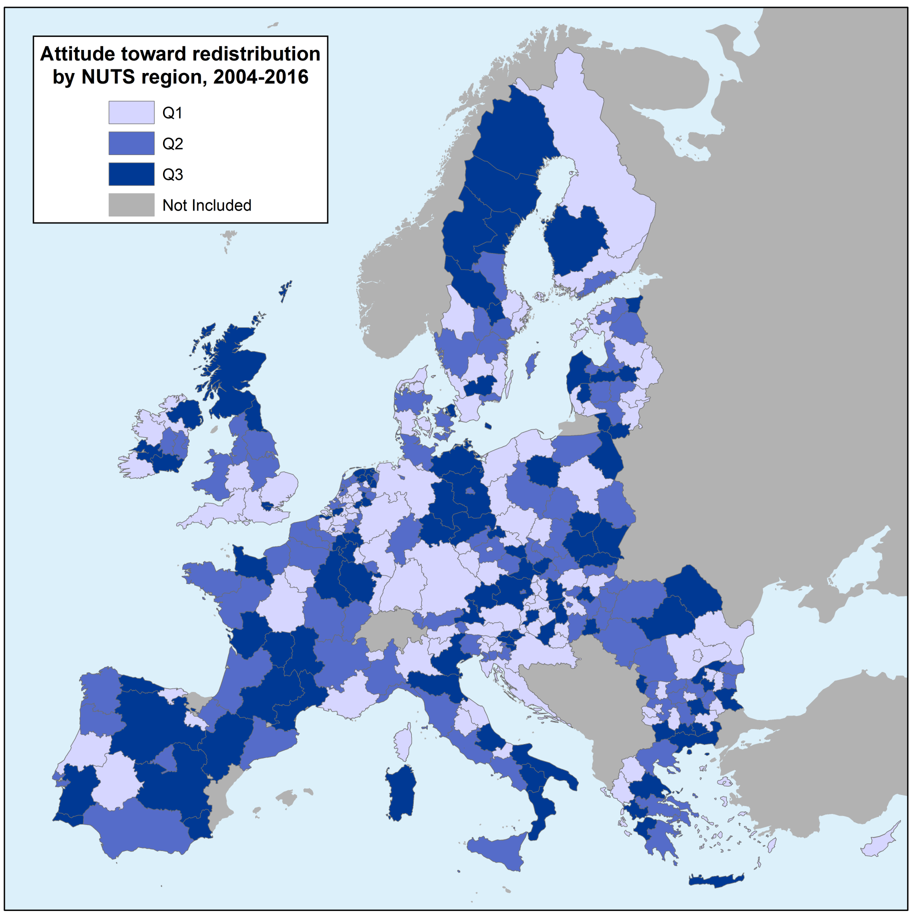
# Appendix H

## Measuring Skill Specificity

The variable *Skill Specificity* is constructed in a few steps. First, the share of lowest level ISCO units within the larger level unit is divided by the share of the surveyed population with that ISCO code. This is then divided by the ISCO skill classification for that ISCO code, which ranges between one and four. Then the measure is standardized. This is done at ISCO one-digit and ISCO two-digit separately, and these measures are then averaged.

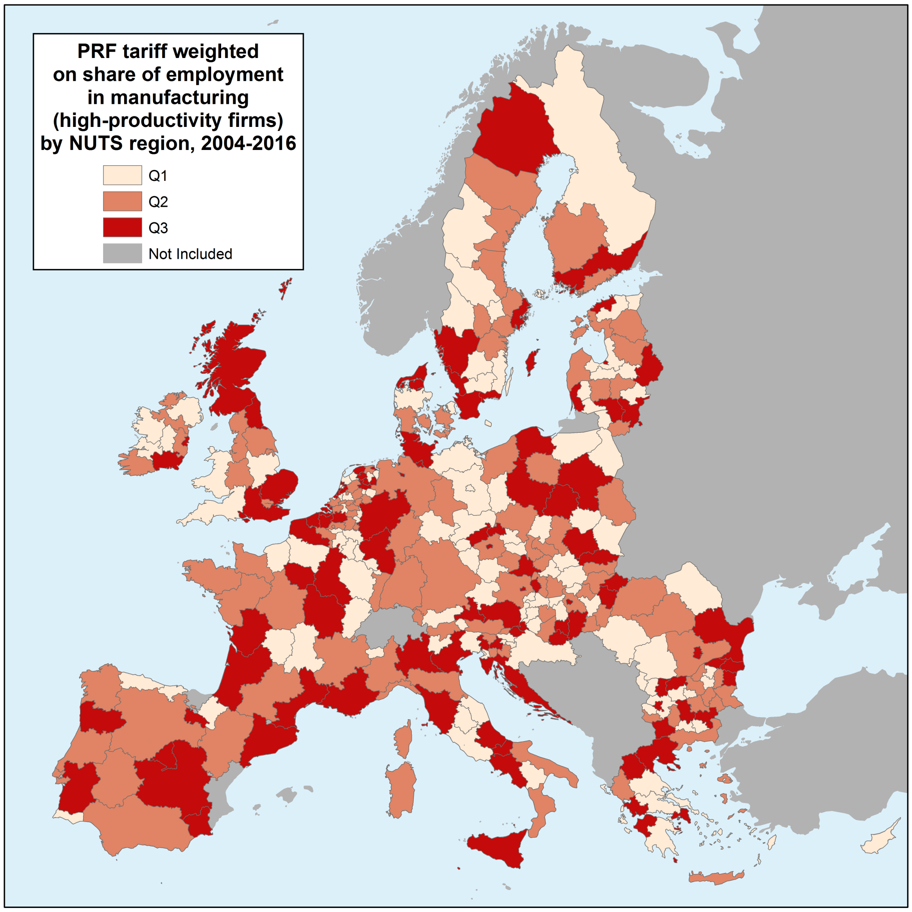
## Additional Figures and Tables (individual-level analysis)

**Figure H1:** Support for redistribution




Note: The variable capturing individual attitudes towards redistribution is a dummy scoring one if respondents answer “strongly agree” or “agree” to the following sentence: *The government should take measures to reduce differences in income levels.* Data are unavailable for ES21, ES53, and ES70. Regions FRA1, FRA2, FRA3, FRA4, FRA5, ES63, ES64, PT20, and PT30 are not shown on the map.

**Figure H2:** Instrument for PRF liberalization

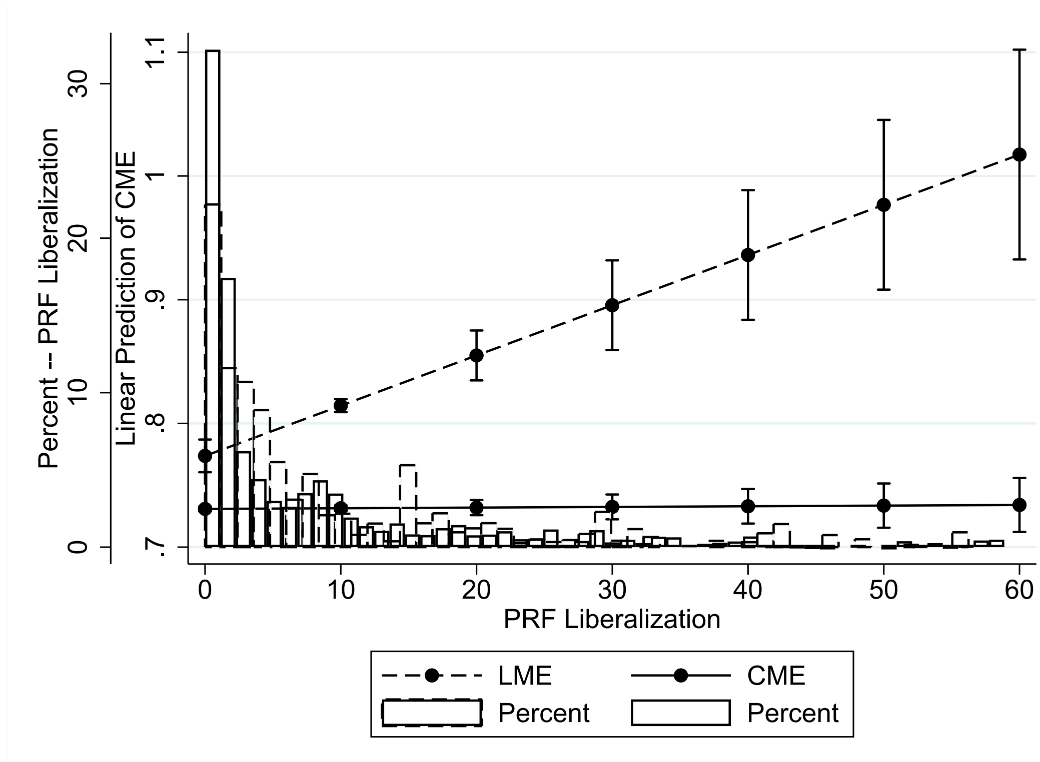



Note: The variable *Instrument for PRF Liberalization* measures preferential tariff cuts weighted on the share of manufacturing workers employed in very productive firms. Data are unavailable for ES21, ES53, and ES70. Regions FRA1, FRA2, FRA3, FRA4, FRA5, ES63, ES64, PT20, and PT30 are not shown on the map.

**Table H1:** Descriptive statistics

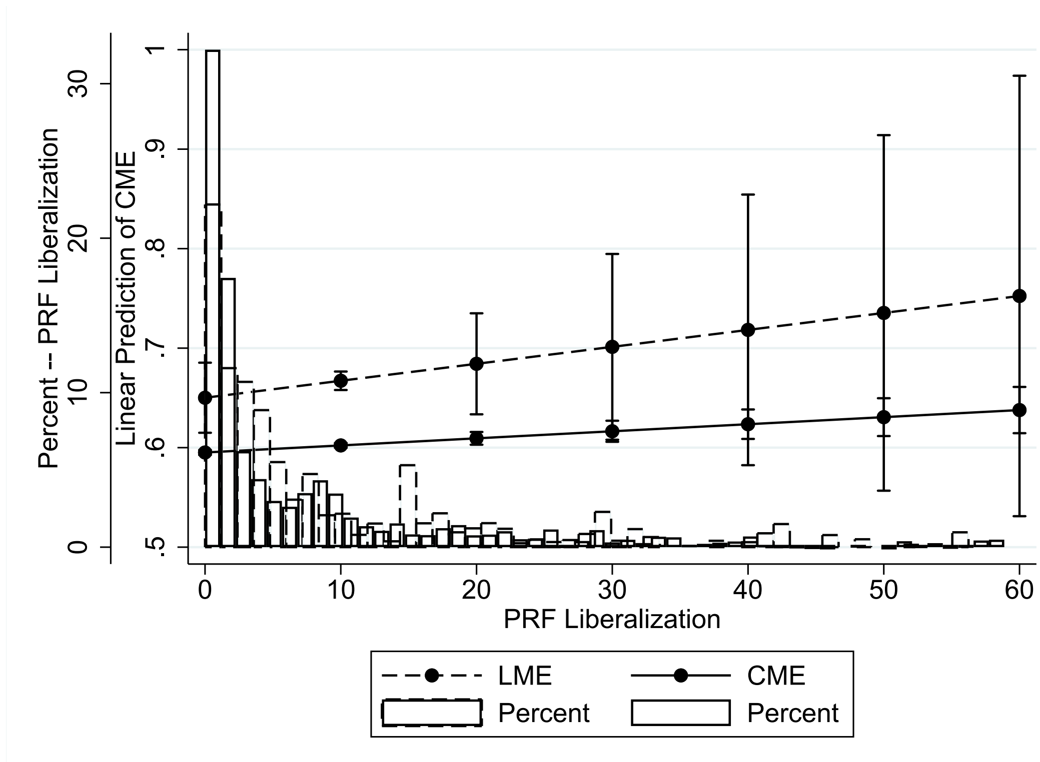
| Variable | Obs | Mean | Std. Dev. | Min | Max |
|---|---|---|---|---|---|
| Support of Redistribution | 120,904 | 0.73 | 0.45 | 0 | 1 |
| PRF Liberalization | 120,904 | 9.55 | 15.11 | 0 | 110.32 |
| Gender | 120,904 | 1.51 | 0.50 | 1 | 2 |
| CME | 120,904 | 0.83 | 0.37 | 0 | 1 |
| Years of Education | 120,904 | 12.72 | 4.06 | 0 | 51 |
| Ideology | 120,904 | 5.01 | 2.19 | 0 | 10 |
| Skill Specificity | 120,904 | 1.19 | 0.61 | 0.40 | 4.90 |
| Patents | 120,904 | 75309.44 | 261793.70 | 0 | 2534918 |
| Corruption | 120,904 | 12.36 | 24.87 | 0 | 239.39 |
| PR | 120,904 | 8.69 | 14.71 | 0 | 110.32 |
| Migration | 120,904 | 99.74 | 183.33 | 0 | 1566.52 |
| Unemployment | 120,904 | 94.90 | 157.14 | 0 | 1264.10 |
| Euro | 120,904 | 6.34 | 12.39 | 0 | 96.66 |
| Private Credit | 120,904 | 987.45 | 1882.29 | 0 | 13721.83 |
| Social Expenditure | 120,904 | 224.17 | 379.80 | 0 | 2901.37 |
| Tax/GDP | 120,904 | 171.58 | 322.43 | 0 | 2962.35 |

**Figure H3:** The effect of tariff cuts on support for redistribution in CMEs and LMEs (low-education)




Note: The predictions are plotted from Model 2 in Table 3. The outcome variable in all models is a dummy scoring one if respondents answer "strongly agree" or "agree" to the following sentence: *The government should take measures to reduce differences in income levels.* The graph shows the linear predictions of $\Delta\tau$ for CMEs and LMEs. The histogram shows the distribution of $\Delta\tau$ for both CMEs and LMEs. 90% C.I.

**Figure H4:** The effect of tariff cuts on support for redistribution in CMEs and LMEs (high-education)




Note: The predictions are plotted from Model 3 in Table 3. The outcome variable in all models is a dummy scoring one if respondents answer "strongly agree" or "agree" to the following sentence: *The government should take measures to reduce differences in income levels.* The graph shows the linear predictions of $\Delta\tau$ for CMEs and LMEs. The histogram shows the distribution of $\Delta\tau$ for both CMEs and LMEs. 90% C.I.

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