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ABSTRACT

Hit by the Silk Road: How Wage Coordination in Europe Mitigates the China Shock*

Coordination in collective wage setting can constrain potential monopoly gains to unions in non-traded-goods industries. Countries with national wage coordination can thus stabilize overall employment against fluctuations and shocks in the world economy. We test this theory by exploring within-country variation in exposure to competition from China in 13 European countries. Our causal estimates demonstrate that in countries with uncoordinated wage setting, regions with higher import exposure from China experienced a marked fall in employment, while countries with wage-coordination experienced no such employment effects. We test our main mechanism against other explanations, and show that our findings are robust to alternative measures of wage coordination, industry classifications, and trade exposure.

JEL Classification: F16, F66, J51, J60

Keywords: wage-coordination, employment, globalization, China-shock

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1 Introduction

Global shocks do not have the same employment effects in all countries. China's new position as a global supplier of manufacturing goods reduced employment in Europe, but not to the same extent everywhere. Likewise, the aftermath of the Great Recession demonstrated a huge variation in how employment patterns changed in different countries. The start of the Corona pandemic led to a new China shock in the opposite direction – and again we should expect to see different employment effects across countries.

The divergent labor market responses to shocks and higher competition have renewed the interest in wage determination and bargaining institutions. Comparing wage setting systems across countries, OECD (2018) finds that coordinated systems are associated with higher employment and lower unemployment than fully uncoordinated systems. The organization does not find adverse productivity effects in systems with coordination relative to systems without.

In this paper we provide evidence that coordinated wage setting has mitigated the impact of the fiercer global competition that has recently hit European economies. Coordinated wage setting contributes to a reduction in wage gaps as well as to an expansion of employment, suggesting that there is a prize rather than a price of equality in the form of higher employment levels (Barth & Moene, 2016). Our basic claim is that coordination of collective wage bargaining stabilizes employment against fluctuations in the world economy. It enables the majority of workers to achieve a greater share of the potential gains from globalization.

Historically, institutions that facilitate coordination in wage bargaining have been on the rise when economies face challenges of globalization and large negative shocks (Wallerstein & Western, 2000). The experience of the small open economies in northern Europe is instructive. Each union in these countries was initially strong within its own trade, but weak in its ability to collaborate across trades. This weakness became particularly evident during the world crisis in the 1930s. When foreign demand collapsed, workers in the exporting sectors, as for instance the militant metal workers, had to take large wage reductions in order to stem the decline in employment. The equally militant construction workers came under no such pressure, in large part because their activities were less exposed to foreign competition. Construction workers also produced inputs to exporting firms, and some of them even worked in the export sector as well as in home construction. All this implied that higher wages in construction would raise the costs in the export sector, which would threaten the jobs of metal workers even further.

To prevent sheltered unions (as the unions in the non-tradeable sector were called) from obtaining higher wage gains at the cost of the workers in the export industry, the union movement tried to coordinate the wage setting and internalize some of the indirect effects. In Scandinavia this was done – with the support of the employers – after the Basic agreement between the national associations of unions and employers in 1935 in Norway and in 1938 in Sweden. The peak associations introduced "solidarity negotiations" where wage setting at the industry level is replaced by direct negotiations over pay by the national associations of unions and employers. The change started in the 1930s, but became institutionalized first in the 1950s as a form of pattern negotiations where overall wage setting became more in line with the conditions in the traded-good sector (Moene & Wallerstein, 1995). In the same period also other European countries, but far from all, introduced a similar coordination between unions.[1] Today it therefore exists a considerable variation in the system of wage setting across countries with a corresponding variation in the level of coordinated wage-setting that we exploit below.

Wage-coordination cushions overall employment from fluctuations in the world market that spread via domestic input-output linkages. Input-output linkages generate spillovers and induce high correlations between the demand for labor in different sectors. Manufacturers of consumption goods typically depend on material inputs from other industries, machinery, business services, utilities, travel services, and goods transportation. Investments in manufacturing firms depend on the price of capital goods and construction. Every private sector relies on infrastructure and government services, financed by taxes on employment, sales, and profits. All this creates interdependence between unions in different industries, and unions that care about their own employment may gain by coordinating across industries to internalize such spillovers. The key mechanism is wage moderation in the non-tradeable industries where unions and employers have market power to raise both wages and output prices. Unions and employers in tradeable industries in contrast, must adhere to international prices. Wage moderation in the non-tradable sector, we argue, gives smaller wage differentials and higher profits in all sectors, and tends to disperse localized shocks across industries and regions.

To test this hypothesis, we use data from 13 European countries during the period of China's entrance into the world market. We measure wage-coordination using the coordination index provided by Jelle Visser in the ICTWSS data base.[2] This index measures the dimensions of bargaining systems that best captures our notion of wage coordination across sectors. The same measure is recently used for instance by OECD (2018) to measure wage-coordination that "helps negotiators internalize the macroeconomic effects of the terms set in collective agreements" and "keeping wage increases in the non-tradable sector in line with what can be afforded by the tradeable sector" (ibid. p. 79). We first show, descriptively, that manufacturing employment had a more modest decline in countries with wage-coordination than in countries with uncoordinated wage systems, even though their exposure to China was equally high. Coordinating countries also

[1] See for instance Visser (2016 a,b), Calmfors & Driffill (1988), Elvander (1988), Freeman (1988), Moene, Wallerstein, & Hoel (1993), and Ross & Hartman (1960). Crisis and unemployment have had a strong influence on the extent of wage coordination and centralization of collective bargaining. Unemployment is more likely to induce more coordination if there is some level of collective bargaining in the first place, as discussed in Wallerstein & Western (2000). See also Chaison (2018), Moene (2015) and Katz (1993).

[2] The Institutional Characteristics of Trade Unions, Wage Setting, State Intervention and Social Pacts database, see data description for details.

had a more moderate growth in wage levels. The stylized pattern that we describe is consistent with our hypothesis that wage-coordination mitigates negative effects from globalization.

In the main empirical analysis, we explore within-country variation in exposure to China. We follow the now famous shift-share approach of Autor, Dorn, & Hanson (2013) and link initial employment composition with trade flows going from China to Europe. Regions initially specialized in the production of goods that China started to export were relatively more exposed to higher import competition. To give our estimates a causal interpretation we instrument actual imports by trade flows from China to other rich economies outside Europe. Our main interest is to explore whether the effect of the China shock was different for countries with wage-coordination than for countries with uncoordinated wage systems.[3]

The analysis reveals an interesting pattern. We find no employment effects of higher import penetration in countries with wage-coordination. In contrast, we find strong negative effects in countries with uncoordinated wage systems. The effects are mainly due to a reduction in manufacturing employment. The baseline estimate is that a 1000 euro rise in import exposure (per worker) leads to a reduction in manufacturing employment as a fraction of the population of 1.5 percentage points.

Is this a large effect? We can compare it with the total employment change. Our predicted trade-induced employment-decline amounts to around one fifth of the actual decline in manufacturing employment during 2000 to 2008. We can also compare our estimates with those reported in previous research. The trade-induced employment decline for countries with uncoordinated wages is for example stronger than what is found for the US by Autor *et al.* (2013) and others (Autor, Dorn, Hanson, & Song, 2014; Acemoglu, Autor, Dorn, Hanson, & Price, 2016; Pierce & Schott, 2016; Asquith, Goswami, Neumark, & Rodriguez-Lopez, 2019; Bloom, Handley, Kurman, & Luck, 2019).

Overall, our findings help resolve the puzzling variations in employment effects of the China-shock found in different European countries. For instance, Dauth, Findeisen, & Suedekum (2014) and Balsvik, Jensen, & Salvanes (2015) find very small employment effects in Germany and Norway, both countries with a coordinated wage bargaining; Donoso, Martín, & Minondo (2015) find large negative employment effects in Spain, a country with "uncoordinated wage setting".

Can the pattern that we find be explained by differential growth in export to China? Clearly, trade goes both ways. Dauth *et al.* (2014) find that German regions specialized in export-oriented industries experienced employment gains and lower unemployment. To test for such effects, we

[3]We approach this by adding an interaction term between import exposure and a measure of wage coordination. Beach & Lopresti (2019) use a similar approach to investigate whether the effects of the China shock on crime in the US depend on the generosity of unemployment insurance.

add a discussion that takes export into account, and experiment with different measures of import exposure. The main findings are not sensitive to these changes in measurement. The import of intermediates in production raises similar concerns. As discussed by Autor *et al.* (2016), offshoring may raise the productivity of workers (Grossman & Rossi-Hansberg, 2008) or lower the price of intermediates (Auer *et al.*, 2013), and thus yield positive employment effects in the importing countries. While such effects were not detected in Auer *et al.* (2013), evidence from many countries may be more susceptible to variations in the share of intermediates, and thus we use imports in final goods in our main analysis. Again, the main findings are not sensitive to these changes in measurement.

Another key question is whether our results are driven by wage coordination in itself or by some other country-level variable correlated with coordination. We assess this by investigating several other country-level characteristics, such as union density, employment protection legislation and average education attainments. We conduct two tests to explore whether these characteristics explain our findings. In the first we weight our regressions in such a way that the group of countries with wage coordination matches those with uncoordinated wage systems. In the second test we add an extra interaction term between import exposure and the additional country variables. Both tests suggest that the different employment effect is due to wage-coordination, and not any other (observed) country-level characteristic.

Some recent papers have raised concerns with shift-share approaches like the one we use. Addressing some of the most relevant concerns, we first show that our estimates are robust to controlling for "pre-treatment" trends in manufacturing employment. Adao, Kolesár, & Morales (2019) argue that conventional standard errors might be biased due to residual correlation across regions with similar manufacturing structure. We show that our results are robust to standard errors that account for this type of correlation.

Borusyak, Hull, & Jaravel (2018) and Goldsmith-Pinkham, Sorkin, & Swift (2018) transform the standard shift-share design into numerically equivalent specifications, where the former relies on exogeneity of the import shocks while the latter relies on exogeneity in the initial employment shares. We calculate so-called Rotemberg weights (Goldsmith-Pinkham *et al.*, 2018) to determine which of these quasi-experimental designs fit best to our application. The exercise shows that our estimates are identified primarily by the import shocks and not the employment shares, which suggests that our results should be seen through the "shock" view of Borusyak *et al.* (2018) rather than the "share" view of Goldsmith-Pinkham *et al.* (2018). That finding is reassuring, as exogeneity in the import shocks are much more plausible in our setting.

Finally, our paper adds to the long string of literature exploring wage setting in small open economies. This literature focuses on the role of wage policies to retain full employment and prevent inflation. It started with the work of Odd Aukrust in the 1960s and by Edgren, Faxen, &

Odhner (1969).[4] Nymoen (2017) surveys the comprehensive literature and provides informative long-run estimates of the suggested relationships.

The remainder of the paper is structured as follows. In Section 2 we articulate our main argument through a simple model. In Section 3 we describe our data, provide motivating empirical patterns and discuss our empirical strategy. We present our empirical results and the robustness analysis in Section 4 and conclude in Section 5.

2 The mechanism

What do unions care about? We follow the traditional view in labor economics that each union has a welfare function that depends positively on the wage of its members and the employment level of the sector that the union covers.[5] Normally, however, the union contract only specifies the wage levels, while employers keep the right to manage employment levels.

With a demand for labor that is downward-sloping in the producer real wage, the union must trade-off higher wage aspirations and lower employment levels. But, since there are potentially indirect cost and demand effects of wage contracts, each union does not necessarily face the real trade-off between pay and jobs. Alone, a union can realistically hope to achieve improvements in real wages only at the expenses of profits and employment elsewhere in the economy.

There are several links that lead to indirect effects. Supplying inputs creates cost links via endogenous output prices. Employment and earnings in each sector also affect total demand and create demand-links between wages in one sector and profits and employment in others. The type of links that are most dominant varies with the competitive position that is typical of the sector. Most importantly, it depends on whether the sector is sheltered from foreign competition, or not.

In sectors that are somewhat sheltered from foreign competition, unions have implicit market power over the output price. A higher nominal wage therefore does not raise the producer real wage much, compared to the one-to-one rise in the producer real wage in the traded goods sector with a given world market price. In other words, the wage setting in non-traded-goods industries has a strong impact on the price of the domestic supply of inputs to traded-goods industries. Higher wages in non-traded-goods industries can therefore reduce employment and income in the traded-goods industries.

Similarly, since the activities in the traded goods industries affect the extent of the market for

[4] Aukrust's work was first published in English in Aukrust (1977).

[5] See Farber (1986) for an overview of the early literature.

sheltered industries via demand links, expanding employment and production in traded goods sector might have considerable effects on the possibility of high wages and employment in the non-traded-good sector.

The link that is most important in our case is that from non-traded to traded goods industries. We concentrate attention on three implications:

1. The strength of the unions in the sheltered non-traded goods sector depends in part on how sensitive the output price is to wage increases. The more output price goes up after a given wage increase, the stronger the union. The stronger this implicit monopoly power of the sheltered unions, the higher the price increase for each wage rise – and the lower the corresponding rise in the producer real wage and, hence, the corresponding reduction in employment in the sheltered industries.
2. Higher output prices in the sheltered sector mean higher costs to traded-goods producers and thus a lower ability to compete on the world market. Accordingly, the stronger the implicit monopoly power of unions in non-traded industries, the weaker the position of unions in the traded goods industries. Employment and wages in traded-goods industries become lower and their workers become more vulnerable to changes in the world market.
3. Wage coordination across trades can internalize more of the indirect effects. By mitigating the implicit monopoly gains to sheltered unions, wage differentials become smaller. The moderation of the high sheltered wages improves overall employment in both the sheltered and the traded-goods sector. Wage moderation in the sheltered sector also stabilizes employment towards abrupt changes in the global economy.

These general effects follow as long as the wage coordination takes place between workers who are each others complements and who are willing to coordinate wage setting. To be clear we now provide a simple illustration in a setting with one sheltered and one traded-goods sector with one union in each sector.

An illustration: The non-traded-goods sector has subscript s (sheltered) and the exposed traded-goods sector subscript e (exposed). The two unions have preferences equal to the earnings of potential union members N_i with a wage w_i, employment n_i, and an outside option z_i, given by: $u_i(w_i, n_i) = n_i w_i + (N_i - n_i)z_i$. The implicit cost of a job loss is $(w_i - z_i)$. Coordinating, unions care about workers in the other sector by a weight $\beta_i \leq 1$ on their interests (most likely in the the expectation that the other union does likewise): $v_i = u_i(w_i, n_i) + \beta_i u(w_j, n_j)$ for $j \neq i$. Maximizing v_i with resepct to w_i yields the generic first order condition:

$$n_i = -\left[(w_i - z_i)\, dn_i/dw_i\right] - \beta_i \left[(w_j - z_j) dn_j/dw_i\right], \quad \text{for } i (\neq j) = e, s. \tag{1}$$

Outputs are produced by labor n_i according to concave production functions $x_i(n_i)$.

Each firm is a price taker in the labor market and in the output market. Part of the s-output goes as inputs in a fixed proportion α to the output x_e in the e-sector. The value added in the e-sector has a net unit price $p_e = (P_e - p_s\alpha)$, where P_e the fixed world-market price. Profits in the two sectors are $\pi_i = p_i x_e - w_i n_i$, for $i = e, s$. The demand for labor is $\arg\max_{n_i} \pi_i \equiv f_i(w_i/p_i)$, which by assuming a quadratic approximation to the production function is $f(w_i/p_i) = a_i - b_i(w_i/p_i) = n_i$ (with a and b positive). Finally, with a downward-sloping demand for s-goods the equilibrium price p_s is an increasing function of the wage w_s. We approximate the producer real wage in the s-sector $w_s/p(w_s)$ by $(1/\theta)(w_s + q)$, where θ captures the implicit market power of the s-union[6] (how sensitive the price p_s is to wage rises), while q captures a level effect.

No coordination means that w_s and w_e are determined by the first order conditions in (1) with $\beta_i = 0$, which combined with the demand for labor give us the solutions:

$$w_s^* = [z_s - q + \theta a_s/b_s](1/2) \tag{2}$$

$$n_s^* = [a_s - b_s(z_s + q)/\theta)](1/2) \tag{3}$$

$$w_e^* = [z_e + (P_e - \alpha p(w_s))\,(a_e/b_e)](1/2) \equiv w_e^*(w_s) \tag{4}$$

$$n_e^* = [a_e - \frac{b_e z_e}{P_e - \alpha p(w_s)}](1/2) \equiv n_e^*(w_s) \tag{5}$$

The expressions (2) and (3) show how the s-union benefits from a higher market power θ. The wage goes up with θ, a general result, while a higher θ, in our specification, also leads to higher employment in the non-traded-goods sector. In the traded-goods sector, in contrast, the union has no power over the net output price. From (4) and (5) we see that w_e and n_e becomes lower the higher is w_s. Hence, the stronger the position of the s-union, the weaker the position of the e-union and the lower its employment and wage. From (4) and (5) we also see that a reduction in the world price P_e, leads to lower wages and employment.

Coordination means that w_s and w_e are determined by the first order condition (1) with $\beta_i > 0$ combined with the demand for labor. The solutions, $\hat{w}_s$ and $\hat{n}_s$ for the non-traded-goods sector, and the corresponding solutions for the traded goods sector, $\hat{w}_e$ and $\hat{n}_e$, can implicitly

[6]The price of sheltered sector output is thus $p(w_s) = \theta w_s/(w_s + q))$.

be expressed by:

$$\hat{w}_s = w_s^* - \beta_s \alpha h \tag{6}$$

$$\hat{n}_s = n_s^* + \beta_s (b_s/\theta)\alpha h \tag{7}$$

$$w_e^* = w_e^*(\hat{w}_s) \tag{8}$$

$$n_e^* = n_e^*(\hat{w}_s) \quad \text{where} \tag{9}$$

$$h = \frac{(\hat{w}_e - z_e)(\theta/2b_s)\hat{w}_e p'(\hat{w}_s)}{[P_e - \alpha p(\hat{w}_s)]^2}(1/2) > 0 \tag{10}$$

The basic difference to the case without coordination is that the costs of wage increases for the s-sector go up, leading to wage moderation in the sheltered industries according to (6). This wage moderation leads again to employment expansion in the non-traded-goods sector, according to (7), as well as in the traded-goods industries, according to (9).

Using these expressions, we also see that a drop in the world price, P_e, now reduces employment less (maybe not at all) since the value of h goes up as P_e declines. Hence, the more severe the decline in P_e, the more wage coordination compensates for the shock. The level of w_s declines and as a result employment expands in both sectors.

3 Data and identification

To test this mechanism we need detailed information about changes in employment and which sectors that are hit by the China-shock and which that are not. In this section we describe our data sources and provide motivating empirical patterns, dependent on how wage bargaining is organized. We also describe our empirical approach.

3.1 Data sources and measurement

Our analysis focuses on the period 2000 to 2008, and we use data from the following 13 European countries: Austria, Belgium, France, Germany, Greece, Ireland, Italy, Netherlands, Norway, Portugal, Spain, Sweden and the United Kingdom.

Most of our data are extracted from the Eurostat Regional Database. The *Structural Business Statistics* annual employment data for manufacturing. The published data, however, is incomplete for some countries, and in these cases we supplement with national sources (see Appendix A for details). In total, we have the dis-aggregated employment data for 187 labor market regions, so-called NUTS 2. The Eurostat Database also provides information on population size, total employment and average education attainments at the same level of aggregation. In addition, we extract some data on wages from the Eurostat, which we supplement with micro data from the *Structure of Earnings Survey.* The geographical identifiers in both of these data sets, however, are at a higher level than NUTS 2. Therefore we do not use these data in our main empirical analysis.

We get information on international trade from the United Nations Commodity Trade Statistics (Comtrade). This data source provides annual trade flows for over 170 countries, by commodity and trade partner. We use the 4-digit SITC product codes, which gives us about 1100 commodity groups. We convert the trade flows into 2007 euros. We then match trade flows with disaggregated manufacturing employment data using harmonized industry and product classifications. The World Bank provides the correspondence between SITC product codes and NACE industry codes. Using this correspondence list, we are able to unambiguously match 93 percent of all commodity groups to industries. The Structural Business Statistics provides annual employment data at NUTS 2 level for 14 different manufacturing industries at the 2-digit NACE level. We use these disaggregated regional employment levels as weights in calculating measures of trade exposure at the NUTS 2 level. The rest of the commodities are linked to more than one NACE code. For these ambiguous cases we make use of the 5-digit SITC trade data and compute the share of trade by NACE codes within each 4-digit commodity groups. We then choose the NACE code with the highest share, separately for import and export.[7] Finally, we allocate commodities into final consumption goods, intermediates and capital goods using the United Nations' *Broad Economic Categories.* As discussed in the introduction, we use imports into final consumption goods in our main specifications below, but also provide robustness analyses with intermediates included.

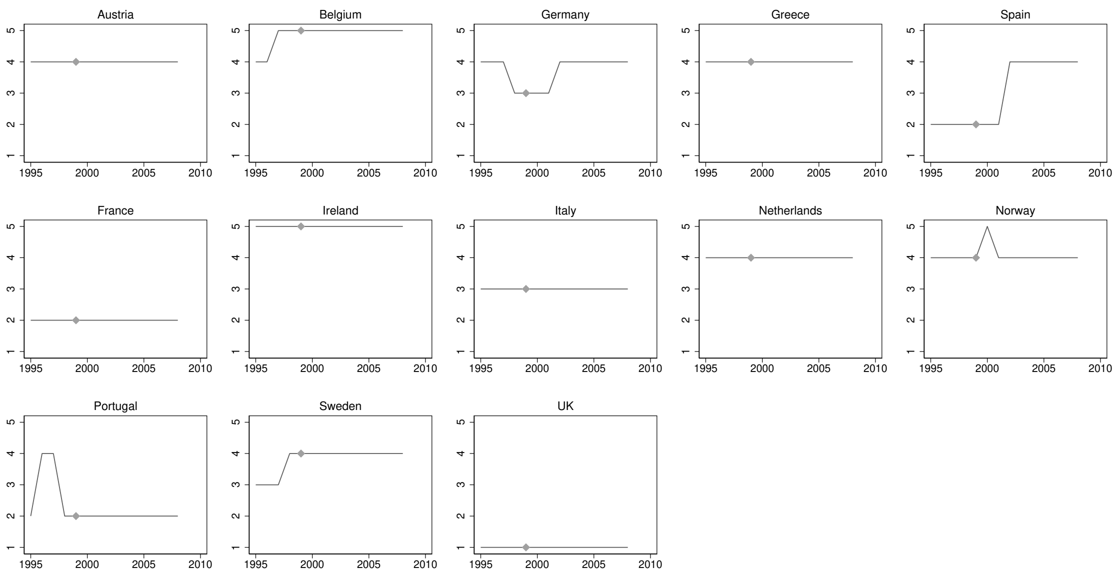
We extract country-level labor market characteristics from the databank compiled by Jelle Visser (Visser, 2016). This dataset includes information on e.g. union density, employment protection legislations, wage-setting coordination and much more. The coordination index takes five values:

1. Fragmented wage bargaining, confined largely to individual firms or plants
2. Mixed industry and firm-level bargaining, weak government coordination through minimum wage setting or wage indexation
3. Negotiation guidelines based on centralized bargaining
4. Wage norms based on centralized bargaining by peak associations with or without government involvement
5. Maximum or minimum wage rates/increases based on centralized bargaining

[7]The reason for why we do not *only* use the 5 digit data, is that they have a lot of missing commodities (the sum of trade in 5-digit commodities is much lower than the sum of trade in 4-digit commodities.

In most of our analysis we make use of a simple binary variable to capture wage coordination. We set this binary variable equal to unity if the country has a coordination index of three or more, which implies negotiation guidelines, wage norms, or bounds on wage-rates or -increases based on centralized bargaining by peak organizations. To validate that our cut-off is empirically meaningful, we provide evidence from flexible specifications using all five values of the index below. Figure 1 displays the raw coordination index by country and year. The dots in the figure mark the baseline period, which we use for our binary classification. We thus classify the wage bargaining to be coordinated in the following nine countries: Austria, Belgium, Germany, Greece, Ireland, Italy, Netherlands, Norway and Sweden.

FIGURE 1: Degree of wage coordination



Source: The Institutional Characteristics of Trade Unions, Wage Setting, State Intervention and Social Pacts database. See Visser (2016).

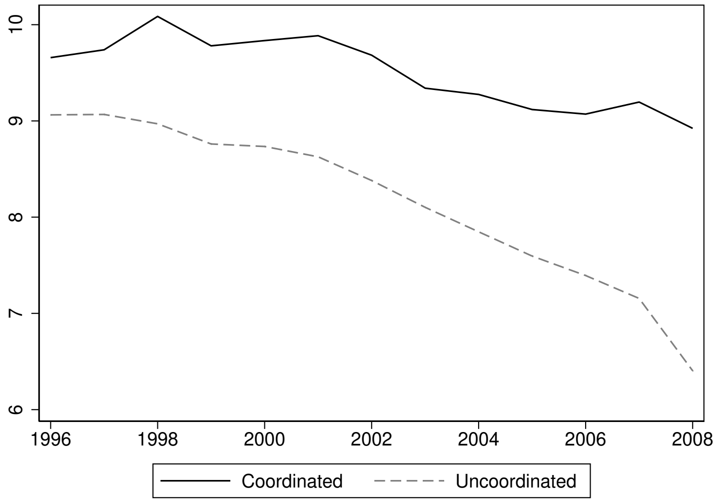
3.2 Descriptive statistics and stylized patterns

Before we proceed we present three descriptive patterns that motivate our empirical investigation.

A. The decline in manufacturing employment has been more modest in those European countries with wage-coordination.

In Figure 2 we plot manufacturing employment relative to working age population (15-74) for the years 1996 to 2008. As can be seen, the employment ratio fell somewhat for countries with wage-coordination, but much more so for those without. From 2000 to 2008 the employment rate fell by more than 2 percentage points for these countries, while it fell by less than 1 percentage point in countries with wage-coordination.

Figure 2: Manufacturing employment as a fraction of working age population



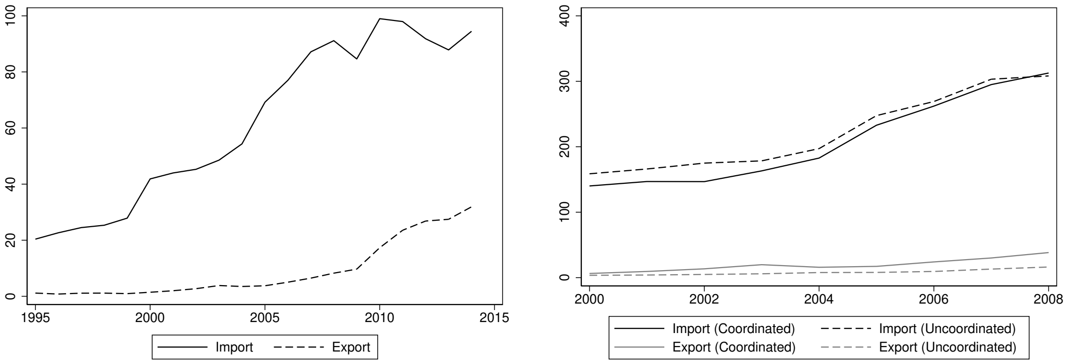
The graph is based on country-level employment figure from the *Structural Business Statistics*. Countries are weighted according to population size.

B. Countries with wage-coordination have been equally exposed to China as countries with uncoordinated wage system.

The decline of manufacturing employment coincides with the entrance of China into the world economy. In the left panel of Figure 3, we present per capita trade flows of final consumption goods. The flow of imports from China to the European countries in our sample increased substantially, especially during the period 2004 to 2008. Exports to China also increased, but much less, leading to an increasing trade deficit in Europe. In the right panel of Figure 3 we zoom in on our main study period and group countries based on wage-coordination. The rise in imports is very similar across the two groups of countries, although slightly steeper for countries with wage-coordination.

In Table 1 we show how the rise in imports allocate to manufacturing industries. Manufacturing of textiles and manufacturing n.e.c. are the two dominant industries. The latter industry produces furnitures, and importantly for this application, sports goods and toys. Textiles make up a slightly higher share of the increase in imports for countries with uncoordinated wage systems than for those with wage-coordination, while the share of manufacturing n.e.c. is somewhat lower. Still, the overall composition is very similar. The top five manufacturing industries make up the majority of the increased imports: 87.2 percent for countries with wage-coordination and 88.7 percent for those with uncoordinated systems. The employment shares for the same industries are also very similar in the two groups of countries. The descriptive statistics thus suggest that exposure to China was the same.

FIGURE 3: Trade in final consumption goods with China (per capita)



The figure presents trade flows of final consumption goods, in constant 2007 euros and in per capita terms. The left graph is based on the EUR/USD exchange rate from 1999 for years before the introduction of the euro.

TABLE 1: Manufacturing industries: Share of total import increase of final goods and employment

	Coordination		Uncoordinated	
	% of Δ Import	% of Empl.	% of Δ Import	% of Empl.
	(1)	(2)	(3)	(4)
Textiles and textile products	38.3	1.4	45.3	1.5
Manufacturing n.e.c.	26.9	0.9	22.8	1.0
Leather and leather products	10.7	0.3	10.2	0.3
Machinery and equipment n.e.c.	6.8	2.2	7.5	1.3
Electrical and optical equipment	4.5	2.0	3.0	1.7
Food products, beverages and tobacco	2.9	1.9	2.2	2.3
Transport equipment	2.5	1.8	2.2	1.3
Rubber and plastic products	2.6	0.8	1.5	0.9
Basic metals and fabricated metal	1.6	2.7	1.5	2.3
Other non-metallic mineral products	1.4	0.8	1.4	0.8
Pulp, paper and paper products	1.0	1.3	1.3	1.4
Chemicals, chemical products and manmade	0.7	1.1	1.0	1.0
Wood and wood products	0.2	0.6	0.3	0.5
Coke, refined petroleum products	0.0	0.1	0.0	0.1

Column (1) and (3) display per cent of changes in import from 2000 to 2008. Column (2) and (4) present per cent of employment in 1999.

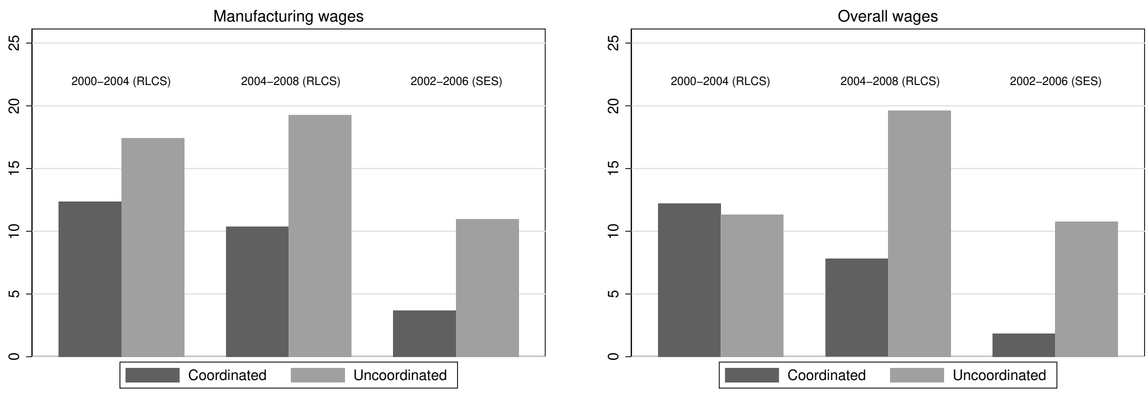
C. Wage growth has been more moderate in countries with wage-coordination.

The left panel of Figure 4 presents estimates of wage growth for the manufacturing sector. The first set of bars plot wage growth per employee in full-time equivalents for the periods 2000-2004 and 2004-2008, using data from the *Labour Costs Survey* (LCS). Nominal wages rose much more rapidly in countries with uncoordinated wage system, especially from 2004 to 2008. During this period, wage levels rose by around 20 percent in these countries, which amounts to about twice the increase of countries with wage-coordination.[8]

The estimates from the LCS do not account for worker qualification, and we have already seen that employment in manufacturing fell more rapidly in countries with uncoordinated wage systems. If this decline disproportionally affected workers with low qualifications it could potentially explain the larger increase in average wages. To explore this, we make use of micro data from the *Structure of Earnings Survey* (SES). We use this data for 2002 and 2006 and from the following six countries: Belgium, France, Greece, Netherlands, Spain and the United Kingdom. With this data we are able to adjust for worker qualification by a standard Mincer wage regression (see Appendix B for details). In the graph we present wages for a permanent full-time employed man with upper secondary education. The estimates confirm that wage growth was much more moderate in countries with wage-coordination.

The right panel of Figure 4 presents corresponding wage estimates for all NACE sectors combined. The figure suggests that wage moderation in countries with coordination was not confined to the manufacturing sector.

FIGURE 4: Nominal wage growth in percent



The left graph shows wages per employee in full-time equivalents, extracted from the *Labour Costs Survey.* The data cover the following countries: Austria, France, Germany, Greece, Italy, Netherlands, Portugal, Spain and the United Kingdom. The numbers in the figure are derived as weighted averages of NUTS 1 estimates.
The right graph is based on micro data from the *Structure of Earnings Survey* for the following countries: Belgium, France, Greece, Netherlands, Spain and the United Kingdom. The numbers in the figure apply for a permanent full-time employed man with upper secondary education.

[8]Note that the LCS does not cover the Belgium, Ireland, Norway and Sweden.

To sum up, despite equal exposure to China, manufacturing employment declined more modestly in countries with wage coordination than in others during the period of China's entrance into the world market. At the same time countries with wage coordination had much lower wage growth. These stylized patterns are consistent with our theory of wage-coordination as a mitigator of the negative effects from globalization.

There is clearly a limit to how much we can infer from this type of descriptive statistics, as countries could have been hit by shocks other than the inflow of inexpensive Chinese imports. In the next section we describe our empirical approach of how we isolate the effects from the China shock.

3.3 Empirical setup

Like Autor *et al.* (2013), we first define changes in import exposure, as:

$$\Delta Import\ exposure_{it} = \sum_j \frac{L_{ijt}}{L_{jt}} \frac{\Delta Import_{jt}}{L_{it}}, \tag{11}$$

where $\Delta Import_{jt}$ is the total change in import from China to Europe in industry j during time period t to $t+1$. The term L_{ijt}/L_{jt} denotes region i's share of the total employment in industry j at time t, while L_{it} represents the total employment in region i. The measure in (11) thus apportion imports of different commodities to regions based on their share of total employment.

In our baseline specification we regress the change in regional employment, Y_{it}, between period t and $t+1$, on the change in import exposure over the same period, controlling for start-of-study-period regional characteristics, X_i', and country by period fixed effects, θ_{st}:

$$\Delta Y_{it} = \beta_0 + \beta_1 \Delta Import\ exposure_{it} + X_{it}'\gamma + \theta_{st} + \epsilon_{it}. \tag{12}$$

The variation in import exposure stems from two sources: the size of the overall manufacturing sector and the composition of the manufacturing sector in each region. The empirical strategy seeks to exploit the latter source of variation. We therefore always include the initial share of employment in manufacturing in the regional controls, X_i'.

The main challenge of the specification is the potential endogeneity of regional trade exposure. To tackle this, we use an instrumental variable (IV) strategy similar to Autor *et al.* (2013). We construct the following instrument for every region i:

$$\Delta Import\ exposure_{IV,it} = \sum_j \frac{L_{ij,t-1}}{L_{j,t-1}} \frac{\Delta Import_{jt}^{Other}}{L_{i,t-1}}. \tag{13}$$

This expression differs from (11) in two ways.

First, it replaces changes in actual trade flows from China to Europe with changes in trade flows from China to other high-income countries ($\Delta Import_{jt}^{Other}$). In our baseline specification we use the following high-income countries: Australia, Canada, New Zealand and USA. The intuition is that we see the increased trade flow from China largely as an exogenous supply shock, induced by improved competitiveness of Chinese manufacturing. The supply shock hits the whole world economy, not just Europe. Using import flows to high-income countries outside of Europe as an instrument can therefore identify the exogenous component of Chinese import penetration.

Second, Equation (13) differs from (11) in that it replaces the start-of-period employment structure in each region with the employment structure from a year prior to our estimation period (denoted by the time subscript $t-1$). We do this to tackle potential measurement errors and reverse causality, if firms anticipate future trade exposure and adjust their employment accordingly. In our empirical analysis we use employment data for manufacturing industries and regions one year prior to our study period.[9]

We include an interaction term between import exposure and the degree of wage coordination to identify potential differences in the response to the China shock. Because wage coordination may be endogenous, we use a measure from the year prior to our estimation period. In our main specification, we use a simple binary variable, $Coord_{j,t-1}$, to denote countries with a coordinated wage bargaining. The specification can be thus be written as:

$$\Delta Y_{it} = \beta_0 + \beta_1 \Delta Import\ exposure_{it} + \beta_2(\Delta Import\ exposure_{it} \times Coord_{j,t-1}) + X_i'\gamma + \theta_{st} + \epsilon_{it}. \quad (14)$$

The direct effect of coordination is absorbed in the country fixed effects. Analogus to (13), we construct an instrument for the interaction term as:

$$\Delta Import\ exposure_{IV,it} \times Coord_{j,t-1} = \sum_j \frac{L_{ij,t-1}}{L_{j,t-1}} \frac{\Delta Import_{jt}^{Other}}{L_{i,t-1}} \times Coord_{j,t-1}. \quad (15)$$

Since we include country by period fixed effects in our model, we identify the effect of import exposure from differences between regions within countries. These differences arise because the import shock hits regions differentially according to their industry structure. The heat map in Figure 5 illustrates this variation.

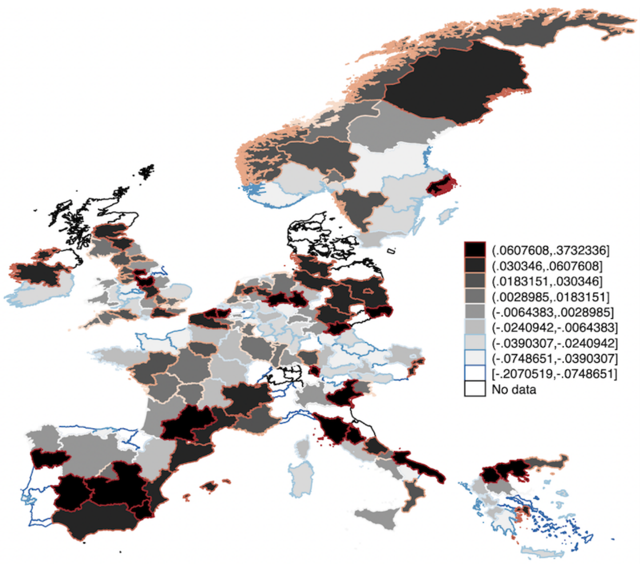
Consider for instance national macro policy responses, or country-wide adaptations by the national confederations of unions or employers, that are identical across industries and regions. Such potential spillovers between regions at the national level are effectively swept out of the estimation together with any other shock at the country level that could potentially be correlated

[9] We are not able to go further back in time due to data constraints.

with the import shock. Thus we are not estimating the total impact of the trade shock. Instead, we provide a clean estimator of the differential effects on regions, and in this way investigate the extent to which coordination across bargaining units at the national level provides insurance against global employment shocks.

Since our model predicts that coordination induces wage moderation in the non-traded sector as well, potential employment effects are spread out across all regions. In terms of Equation (14), this could be translated into a prediction of $\beta_1 < 0$ and $\beta_2 > 0$. If the level of wage moderation is sufficiently high in the non-traded sector, the overall employment effects would be the same regardless of the industry composition of the regions, and $\beta_1 = -\beta_2$.

FIGURE 5: Heat map



The map shows the residual variation in predicted changes in import exposure, after partialling out all regional covariates as well as the country×period fixed effects.

4 Results

This section presents our empirical findings. Our main outcome variable is the four year change in manufacturing employment as a share of the working age population.

4.1 Baseline estimates

We start by presenting our baseline 2SLS estimates. The first stage in these regressions is always strong and coefficients have the expected sign. For brevity, we show the first-stage estimates in Appendix C. The second-stage estimates are presented in Table 2. In the first column we do not include any controls, except the initial manufacturing share. The interpretation of the import exposure coefficient is that a 1000 euro rise in import exposure reduces the manufacturing employment share by 1.482 percentage points in countries with uncoordinated wage systems.

The positive interaction coefficient shows that the decline is much less pronounced in countries with wage-coordination. In fact, the sum of the interaction coefficient and the main coefficient is not significantly differently from zero. In the second column of Table 2 we add controls for the population share of high and medium skilled, as well as the share of female workers. We define medium skilled as those with secondary education (ISCED 3/4) and high skilled as those with tertiary education (ISCED 5A/5B/6 or higher). Adding these controls leaves our main coefficients largely unchanged, although the import exposure coefficient decreases somewhat while the interaction term becomes slightly larger.

TABLE 2: Baseline specification

Dep.var.: Four-year change in emp./working-age pop (in %-points), 2000-2008

	Manufacturing		Non-manufacturing	
	(1)	(2)	(3)	(4)
Δ Import Exposure	-1.482***	-1.408***	-0.960	-0.927
	(0.494)	(0.488)	(0.926)	(0.914)
Δ Import Exposure $\times$ Coordination	1.532***	1.659***	0.743	0.781
	(0.541)	(0.544)	(1.287)	(1.231)
Manufacturing employment share	-0.035***	-0.037***	0.061***	0.063**
	(0.008)	(0.008)	(0.024)	(0.025)
Population share medium skilled		0.024***		-0.009
		(0.009)		(0.040)
Population share high skilled		0.003		0.003
		(0.009)		(0.027)
Female employment share		-0.031**		-0.039
		(0.013)		(0.044)
Observations	366	366	366	366
R^2	0.598	0.612	0.472	0.474
F-stat excluded instruments				
Δ Import Exposure Other	127.3	131.6	127.3	131.6
Δ Import Exposure Other $\times$ Coordination	141.5	143.6	141.5	143.6

Robust standard errors clustered on NUTS 2 are shown in the parentheses. All regressions include country times period fixed effects. 182 observations for the 2000-2004 period, and 184 observations for the 2004-2008 period. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

What is the economic significance of these estimates? One way of illustrating this is to assume, for a moment, that the estimates capture absolute changes and not just relative changes across European regions. We can then compare the predicted trade-induced employment decline with the observed changes during our study period. The average fall in the manufacturing share was 2.3 percentage points between 2000 and 2008 in the four countries with uncoordinated wage systems. Combining the increase in import exposure with the coefficients from Column (2), we can calculate that the rise of Chinese imports contributed to a 0.4 percentage points reduction in the manufacturing share. This is equal to around one-fifth of the total decline in these countries.

Another smell test of our results is to compare them with studies for other countries. Autor *et al.* (2013) estimated a coefficient of -0.6 for the effect of Chinese import penetration on US manufacturing employment. Adjusting for the fact that their analysis is conducted in 2007-US dollars, and not 2007-euros, this coefficient can be converted to -0.8.[10] Thus, their estimates imply a smaller impact in US than what we find for European countries with uncoordinated wage systems. Our results are in line with previous studies of European countries. Dauth *et al.* (2014) and Balsvik *et al.* (2015) find very small employment effects in Germany and Norway, respectively, which is consistent with our result for countries with wage-coordination. In contrast, Donoso *et al.* (2015) estimate large negative employment effects in Spain, a country classified as "uncoordinated" in our analysis. Converted to euros, their baseline estimate is about -2.8, which is twice the effect we estimate for the group of countries with uncoordinated wage bargaining.

In Columns (3) and (4) of Table 2 we use changes in non-manufacturing employment as an outcome variable. The point estimate for the main import exposure coefficient is about two-thirds of that for manufacturing employment, and the interaction term again suggest a more modest effect for countries with wage-coordination. The standard errors are however large and the estimates are far from being statistically significant. Yet, and as a minimum, the estimates clearly reject the idea that the non-manufacturing sector compensated for the job losses in the manufacturing sector.

Overall, the baseline estimates suggest that employment declined significantly due to the rise of Chinese imports, but that wage coordination mitigated the negative effect of higher import penetration. Wage coordination varies at the level of countries, and we only have 13 countries in our sample. Does this mean that we rely on 13 observations in making the above claim? The key is that our dependent and independent variables display variation within coordination level, and this is the variation we use in our estimation procedure. We have 366 observations of regions$\times$years (219 observations with wage coordination and 147 without) that we use to estimate the relationship between import exposure and employment. Our claim is based on comparing the employment effect for each group of coordination, but our inference is based on estimates of both the expected value and statistical uncertainty using within group variation only.

[10] Here we use the average USDEUR exchange rate for 2007 of 1.3705. Note also that Autor *et al.* (2013) study ten-year changes in manufacturing employment.

4.2 Do our results depend on a specific measures of trade exposure?

So far we have only explored trade flows going *from* China *to* Europe. We have also restricted the analysis to trade in final consumption goods. In this section we expand our measure of trade exposure.

The integration of China into the world economy resulted in new market opportunities for European firms (see e.g. Dauth *et al.*, 2014). Since we have ignored exports we might be concerned about an omitted variable bias. In particular one might be concerned if countries with wage-coordination export relative more and if import and export exposure are correlated across regions. To test for this, we formulate an alternative definition of trade exposure and construct a measure of net imports for each industry by subtracting the corresponding export from Europe to China. Following (11), this can be written as:

$$\Delta(Net\ import\ exposure_{it}) = \sum_j \frac{L_{ijt}}{L_{jt}} \frac{(\Delta Import_{jt} - \Delta Export_{jt})}{L_{it}}, \tag{16}$$

where $\Delta Export_{jt}$ is the change in exports in industry j, between period t and $t+1$. As Autor *et al.* (2013), we instrument for this measure using two variables: the instrument for import exposure in (13) and an analogous instrument for exports, using trade flows from the group of other high-income countries to China. The first two columns of Table 3 present estimates based on this specification, using trade in final consumption goods. The regression coefficients are very similar as in our baseline specification, but somewhat larger in magnitude.

The rest of the estimates shown in the table incorporate trade in intermediates and capital goods. This is likely to be particularly relevant for exports, as Europe's export of final consumption goods to China is small. Estimates for net import exposure are shown in Column (3) and (4), while estimates using the baseline import exposure measure are shown in Column (5) and (6). Not surprisingly, the magnitude of the coefficients decrease when we use this broader set of traded goods. Yet, our findings do not change qualitatively. The implied employment effect is even larger in these specifications, since Chinese imports are of much larger magnitude when we include all types of goods.

4.3 Can other special country-level characteristics explain our results?

Another key question is whether our finding is caused by wage coordination in itself or by some other country-level characteristics correlated with coordination. To explore this, we first present some descriptive statistics for the baseline year, where we separate countries by their level of wage coordination.

Table 3: Alternative measures of trade exposure

Dependent variable: Four-year change in manuf. emp./working-age pop (in %-points), 2000-2008

	Net import exposure				**Import exposure**	
	Manuf.	N-Manuf.	Manuf.	N-Manuf.	Manuf.	N-Manuf.
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Net Import Exposure	-1.457***	-0.965	-1.129**	-1.704		
	(0.517)	(0.899)	(0.439)	(1.272)		
Δ Net Import Exposure × Coordination	1.831***	0.855	1.223***	0.749		
	(0.581)	(1.201)	(0.473)	(1.209)		
Δ Import Exposure					-1.197**	-1.586
					(0.515)	(1.530)
Δ Import Exposure × Coordination					1.014**	0.730
					(0.407)	(1.030)
Observations	366	366	366	366	366	366
R^2	0.612	0.473	0.614	0.471	0.609	0.472
Type of goods	Cons.	Cons.	All	All	All	All

Robust standard errors clustered on NUTS 2 are shown in the parentheses. All regressions include regional controls and country times period fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

In Panel A of Table 4, we present average values using regions as the unit of observation. The overall manufacturing employment share is almost identical in the two groups of countries (and Table 1 shows that the employment shares for the top five importing industries are similar). There are slightly less female employees in countries with coordination and a somewhat lower population share of high skilled. The population share of medium skilled is however much higher, meaning that the fraction of low skilled is lower.

In Panel B of Table 4, we show country-level characteristics. Countries with wage-coordination have much higher union density, higher coverage rate, smaller working age populations and somewhat stronger employment protection legislations. The employment share in high-tech manufacturing is about the same in the two groups of countries.

The descriptive statistics in Table 4 make it clear that countries with wage-coordination differ from those with uncoordinated wage systems on several dimensions. We conduct two types of tests to asses whether these differences can explain our findings. As a first test we weight countries with wage coordination such that they on average match countries with uncoordinated systems on a set of relevant characteristics. We do this by entropy balancing, following the procedure in Hainmueller (2012).[11] Our second test consists of estimating a set of "horse race" regressions. To do this, we interact different country-level variables with import exposure and include them, one-by-one, as additional regressors. To ease presentation, we first standardize the additional variables to mean zero and standard deviation one. The regression estimates for

[11] We weight countries with coordination since they are more numerous, which ease the balancing.

Table 4: Descriptive statistics at baseline

	Uncoordinated (1)	Coordinated (2)	Diff (3)	p-value (4)
Panel A: Regional-level				
Manufacturing employment share	16.1	16.2	0.2	0.83
Female employment share	42.9	41.8	-1.1	0.11
Population share high skilled	21.8	19.9	-1.9	0.09
Population share medium skilled	31.2	45.7	14.6	0.00
Panel B: Country-level				
Union density	20.0	37.1	17.1	0.05
Coverage rate	67.6	71.0	3.4	0.86
Employment protection	2.0	2.6	0.6	0.23
Share high-tech manufacturing	0.9	1.1	0.3	0.38
Working age population (mill.)	33.8	25.2	-8.6	0.43
Observations	147	219		

both tests are shown in Table 5. For brevity, we display the estimates for non-manufacturing employment in Appendix C.

Differences in the share of low-skilled workers do not explain our results. In Column (1) of Table 5 we explore whether our findings could be explained by the relative higher population share of low-skilled in countries with uncoordinated wage systems. Previous research have documented that the China shock affected low-skilled workers relatively more than other workers (Autor *et al.*, 2013, 2014). Even though our estimates are identified from variation *within* countries and time periods, the difference in average education attainments might still be a concern.

In Panel A of Table 5, we therefore weight countries to make sure that the share of low-skilled is the same in the two groups of countries. This amounts to scaling down the influence of observations from countries with wage-coordination and relatively few low-skilled, such as Norway, Sweden and Germany, and scaling up the influence of countries with wage-coordination and relatively more low-skilled, such as Italy and Greece. Doing this, the interaction coefficient decreases somewhat in magnitude, but remains highly significant.

In Panel B of Table 5, we show the second test, which is to add an interaction term between import exposure and (country-level) population shares of low-skilled. The coefficient on the additional interaction is close to zero, while our coefficients of main interest increase in magnitude. Based on the two test we thus conclude that it seems unlikely that our findings are driven by differences in education attainments.

Differences in unionization rates and coverage rates do not explain our results. In Columns (2) and (3) of Table 5 we conduct a similar exercise for union density and coverage rate. The coverage rate measures the fractions of workers in a country covered by a collective

Table 5: Regression with other country-level variables

Dep.var.: Four-year change in emp./working-age pop (in %-points), 2000-2008

	Share low-skilled (1)	Union density (2)	Coverage rate (3)	Empl. protection (4)	High-tech manuf. (5)	Working age pop. (6)	Regional variables (7)
Panel A: Weighted regressions							
Δ Import Exposure	-1.419***	-1.303***	-1.407***	-1.169***	-1.396***	-1.365***	-1.340***
	(0.496)	(0.461)	(0.486)	(0.431)	(0.484)	(0.474)	(0.489)
Δ Import Exposure	1.408**	1.852***	1.632***	1.768***	1.681***	1.775***	1.309**
× Coordination	(0.555)	(0.617)	(0.545)	(0.539)	(0.546)	(0.555)	(0.540)
Observations	366	366	366	366	366	366	366
R^2	0.625	0.625	0.624	0.680	0.625	0.622	0.596
Post-balancing diff							
(Coord. - Uncoord.)	0.0	5.8	1.2	0.0	0.0	0.0	
Panel B: Additional interactions							
Δ Import Exposure	-1.885**	-1.509***	-1.398***	-1.800***	-1.381**	-1.588***	-1.997**
	(0.779)	(0.558)	(0.490)	(0.446)	(0.614)	(0.516)	(0.880)
Δ Import Exposure	2.020***	1.818***	1.688***	1.358***	1.644***	1.800***	2.005**
× Coordination	(0.733)	(0.691)	(0.543)	(0.403)	(0.584)	(0.562)	(0.842)
Δ Import Exposure	0.213	-0.184	0.271	0.715**	0.022	-0.199	
× Additional variable	(0.274)	(0.430)	(0.204)	(0.354)	(0.301)	(0.266)	
Observations	366	366	366	366	366	366	366
R^2	0.612	0.612	0.614	0.617	0.612	0.613	0.618

Robust standard errors clustered on NUTS 2 are shown in the parentheses. All regressions include regional controls and country times period fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

wage bargaining agreement. Since the differences between the two groups are so large in terms of these variables, we are not able to fully balance the sample. Still, given how little the regression estimates change, we are quite confident that difference in unionization and coverage rate cannot explain our findings. If anything, our main interaction term becomes even larger.

Differences in employment protection do not explain our results. In Column (4) of Table 5, we consider employment protection legislation, measured as an index taking values 0 to 6. The index captures the strictness of mandatory rules regulating the contractual relationship between employers and employees. It is plausible that employment would fall less in countries with stricter employment regulations, at least in the short run, simply because it is more difficult to lay off workers. Indeed, the interaction between employment protection and import exposure is positive and significant.[12] Importantly, however, the interaction with wage-coordination remains significant and its magnitude is only slightly lower. Furthermore, weighting based on employment protection has only a very limited impact on our regression.

Differences in the the share of high-tech do not explain our results. In Column (5) of Table 5 we weight and interact using employment shares of high-tech manufacturing. One hypothesis might be that countries with a large high-tech manufacturing sector would be less affected by the China shock. Given the seemingly balance between countries with wage-coordination and uncoordinated systems, it is not surprising however that this variable has little bite on our estimates.

Differences in the size of population do not explain our results. In Column (6) of Table 5 we consider country-level population sizes. Countries with wage-coordination are smaller on average, but this does not seem to explain our findings either.

All differences combined do not explain our results. In Column (7) of Table 5, we include all the regional variables listed in Table 4 at once. The interaction between import exposure and coordination becomes slightly smaller in the weighted regression but it remains highly significant. In Panel B, we add four additional interaction terms, one for each of the regional variables. For brevity, we do not show these coefficients in the table. Note, however, that none of them are statistically significant at a 5 percent level. Our coefficients of interest increases somewhat in magnitude and remains highly significant, despite the quite demanding specification.

In sum, the above results suggest that we can eliminate some plausible stories, other than

[12] Note that this interaction is based on a (standardized) binary variable for employment protection below/above the median. We do this since employment protection is measured as an index without a clear cardinal interpretation. The results are however robust to using the index linearly. Note also that the interaction between employment protection and import exposure is much weaker in a stacked regression using total changes for the period 2000 to 2008. This is consistent with the view the employment protection only has an impact in the short run.

variations in wage-coordination, behind the differential employment effects. Before we draw our final conclusion, however, we should address some further concerns.

4.4 Bartik identification: What drives our estimates?

In this subsection, we address some of the concerns raised by recent papers about Bartik-style instruments (named after Bartik, 1991), like the ones we use.

In a seminal paper, Goldsmith-Pinkham *et al.* (2018) discuss identification focusing on industrial composition. The authors show that Bartik instruments are numerically equivalent to using initial employment shares as separate instruments in a weighted GMM estimation, where the shocks (in our case in Chinese imports) are used to construct the weighting matrix. The authors interpret the equivalence results as implying that the initial industry composition is key for exogeneity of the Bartik instrument. Borusyak *et al.* (2018) derive another equivalence result and show that a standard Bartik shift-share specification can be transformed into a regression at the shock level (in our case the industry level). The authors then show that the shocks could serve as valid instruments even when the employment shares are endogenous.

Which of these quasi-experimental designs fit best to our application? To explore this we calculate so-called Rotemberg weights, as proposed by Goldsmith-Pinkham *et al.* (2018), to determine the relative contribution of the import shocks and the manufacturing industry shares in the estimation. We present the details of the exercise in Appendix D.1. The key take-away is that our estimates are identified primarily from the trade shocks and not the employment shares: changes in import explain 74 percent of the variance in the Rotemberg weights for our main coefficient and 88 percent for the interaction term. This contrasts starkly with the applications discussed in Goldsmith-Pinkham *et al.* (2018), and suggests that our application is best seen through the "shock" view of Borusyak *et al.* (2018) rather than the "share" view of Goldsmith-Pinkham *et al.* (2018).[13] That is reassuring, we suggest, as exogeneity of the import shocks seems much more plausible than exogeneity of the initial employment shares.[14]

Both Borusyak *et al.* (2018) and Goldsmith-Pinkham *et al.* (2018) emphasize that Bartik-style identification could be problematic if there are pre-treatment trends correlated with the treatment variable. In our setting, the concern would be that regions particularly exposed to the China shock, for some reason, were on a different employment trajectory than other regions even before the shock occurred. One could for example worry about technology shocks corre-

[13]Goldsmith-Pinkham *et al.* (2018) find that the trade shocks explain about 20 percent of the identifying variation in Autor *et al.* (2013). In another application on the elasticity of labor supply they find that the shocks (in national growth rates) explain less than 1 percent of the identifying variation.

[14]In terms of the relative importance of the industry shares, we find the largest weights on *Textiles and textile products* and *Manufacturing n.e.c.* This is also reassuring, as these are the two industries with the largest increase in Chinese imports (see Table 1).

lated with exposure to China. To address this we add a pre-trend variable, capturing changes in manufacturing employment during the period 1995 to 1999. This is as far back in time we are able to stretch our employment data.[15] Although the steep rise in trade flows happened after 2000, the increase of Chinese imports was in its small beginning during this period (see Figure 3).

The period 1995 to 1999 is therefore not a pure "pre-period", and it is not unlikely that exposed regions were affected by the China shock already during these years. Hence, the inclusion of the pre-trend variable is not a completely satisfactory test of common pre-trends. With this caveat, we present regression estimates in Appendix D.2. The pre-trend variable drags our coefficients of interest somewhat towards zero, but very modestly, and it does not in any way affect our main conclusions.

Jaeger, Ruist, & Stuhler (2018) raise a related concern and emphasize that Bartik specifications using short time horizons may be misleading if the effect of the treatment takes time to arise. The key argument is that the error term, in such situations, would reflect the ongoing adjustment to previous shocks. Estimates based on short time horizons would hence conflate short- and long-term responses. To address this concern, we estimate a stacked regression using changes in employment during the whole time period 2000 to 2008. Estimates are shown in Appendix D.2. The estimated coefficients are very similar to our baseline, although somewhat larger in magnitude.

The identifying assumption in the framework of Borusyak *et al.* (2018) is that industry-level shocks are as-good-as-randomly assigned, conditional on covariates. To examine this in our application, we test the balance of the industry-level shock with respect to the regional covariates, as suggested by Borusyak *et al.* (2018). We present the details of the exercise in Appendix D.3.

Conditionally on country-time period fixed effects and the initial manufacturing share, we show that the regional covariates (female employment share and the population shares of medium and high skilled) are uncorrelated with the import shock at the industry level. Similarly, we find no significant correlation between the import shock, or the interaction term, and changes in manufacturing employment during the period 1995 to 1999. The results from the balance test thus suggest that the import shock could be seen as close to randomly assigned across industries.

Finally, Adao *et al.* (2019) argue that conventional standard errors may have a downward bias in Bartik regressions due to correlation in industry level shocks across regions. The argument is that residuals may be correlated across regions – even when geographically apart – if they have a similar manufacturing composition. To alleviate this concern, we follow the recommendation of

[15]We still lose some observations. This is the reason for why we do not include pre-trends in our main specification.

Borusyak *et al.* (2018) and estimate our specification at the level of industries. This transformed regression provides statistical inference that accounts for the type of correlation that Adao *et al.* (2019) are concerned with. We provide more details on this in Appendix D.3.[16] The main result is that the industry-level regression yields roughly similar significance levels of our key coefficients as the regional-level regression. We thus conclude that the bias of the standard errors in our baseline regressions is unlikely to be important. In Appendix D.3, we also show that our coefficients of interest remain significant when we cluster standard errors by either NUTS 1, or by countries.[17]

4.5 Further issues

In this section, we present further robustness checks of our main results. We present all regression estimates in Appendix E for brevity.

Is our industry measure too crude? We have already seen that countries with wage coordination have similar employment shares of the major importing industries as countries with uncoordinated wage systems (Table 1). One might still worry, however, that our industry measure is too coarse and that there are differences in the composition of employment *within* each 2-digit NACE industry. To alleviate this concern we make use of employment data for 103 3-digit NACE industries at the country-level. More specifically, we calculate the employment composition of these finer industries within each 2-digit industry and impose the same structure in every region within a country. We then re-calculate the import exposure measure. Our findings are robust to the use of this alternative measure.

Are the categories of wage coordination too rough? To explore more nuanced measures, we split our binary coordination variable into five parts, one for each value one to five. The power to identify separate coefficients for each of these categorises is weaker, but the point estimates suggest very little difference across countries with a coordination index of three or higher. All three coefficients are close to zero. This finding legitimizes the use of the binary variable in our main analysis.

Are our regional controls suitable? In our baseline specification the regional controls are based on values from the start of the study period. We construct the controls in this manner because we worry they might change endogenously due to the import shocks. This is especially a concern for the manufacturing employment share. Yet, as a robustness check we run

[16] Note that we are unable to implement the procedure suggested by Borusyak *et al.* (2018) for our main specification due to the interaction term. We instead apply the procedure to a slightly re-written specification, where we interact all covariates with the wage-coordination measure.

[17] As we only have 13 countries in our sample, we compute wild-bootstrapped p-values when clustering on countries, following the procedure in Cameron, Gelbach, & Miller (2008).

regressions where we control for start-of-*each*-period values of the same covariates. This makes the interaction between import exposure and wage-coordination somewhat smaller only.

Are our result caused by a specific sector or country? To address this question we implement a suggestion by Goldsmith-Pinkham *et al.* (2018) and re-do the whole analysis by dropping one manufacturing sector at a time. Our estimates are very robust to this test, even when removing one of the major import industries. We similarly test whether our findings are caused by a single country. The only case in which the estimates change by some magnitude is when we remove Portugal, which makes the negative employment effect for countries with uncoordinated wage bargaining even stronger.

Are our results due to the specific countries in our instrument? We explore how sensitive our estimates are to the group of countries used to calculate the import exposure instrument by constructing separate instruments for each of the four countries: Australia, New Zealand, Canada and the US. The first-stage regressions using these instruments, one-by-one, are always strong, which supports the view that the rise in imports primarily reflects improved competitiveness of Chinese manufacturing. The second-stage estimates are also strikingly similar no matter which instrument we use.

In sum, our results are robust to plausible alternative measures and specifications.

5 Conclusion

Unions in industries that are sheltered from international competition can reap monopoly gains and raise their wages relative to workers in traded-goods industries. Excessive wage growth in sheltered industries reduces employment not only in the sheltered industries, but also in the traded-goods industries as long as these industries use inputs from sheltered industries.

National coordination in collective wage setting can internalize such indirect effects and thus realize collective gains of higher overall employment by moderating potentially high wages in sheltered industries. The resulting changes in the wage structure can improve overall employment in both sheltered and traded-goods industries in addition to stabilizing overall employment against fluctuations and shocks in the world economy.

It is uncontroversial that globalization can induce huge gains. It is less recognized, however, that these social gains justify, or to be feasible maybe even require, a sharing of the costs of being exposed. Wage coordination can be viewed as collective cost sharing to reap the gains and make them bigger. It can also be viewed as an insurance device that smoothens the income when the economy is exposed to temporary international shocks.

We have tested the basic ingredient of this theory by exploring within-country variation in exposure to China in European countries. We have estimated the causal impacts of differences in wage setting in 13 countries and find that:

a) in countries with uncoordinated wage setting, regions that are exposed to import competition experience a clear fall in employment, mainly due to a reduction in manufacturing employment, while

b) in countries with wage-coordination, regions that are exposed to import competition experience no such fall in employment.

This pattern is consistent with our theoretical view and it is robust to alternative measures of wage coordination, industry classifications, and of trade exposure. We have also tested our main findings against other plausible explanations.

Our results resolve a puzzle in earlier research by explaining why European countries that were equally exposed to competition from China experienced so different employment consequences. The overall lesson we draw is that wage coordination matters. Comprehensive organizations in the labor market enable large groups of workers to reap the gains from globalization that otherwise might have turned out as losses. Wage coordination can provide a majority of workers a greater share of the potential gains from globalization.

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A Data appendix

This section provides background information on our data. Table A1 shows the sources for the employment data for the 14 manufacturing sectors used in the analysis.

Table A1: Manufacturing employment data

Country	Source	No of regions
Austria	Eurostat	9
Belgium	National Bank of Belgium	11
Germany	Eurostat	31
Greece	HSA Statistics Greece	13
Spain	Eurostat	17
France	Eurostat	22
Ireland	Eurostat	2
Italy	Eurostat	19
Netherlands	Eurostat	12
Norway	Statistics Norway	7
Portugal	Eurostat	7
Sweden	Eurostat	8
United Kingdom	ONS	31

Table A2: Manufacturing industries

Industry	NACE-code
Food products, beverages and tobacco	DA
Textiles and textile products	DB
Leather and leather products	DC
Wood and wood products	DD
Pulp, paper and paper products, publishing and printing	DE
Coke, refined petroleum products and nuclear fuel	DF
Chemicals, chemical products and manmade	DG
Rubber and plastic products	DH
Other non-metallic mineral products	DI
Basic metals and fabricated metal	DJ
Machinery and equipment n.e.c.	DK
Electrical and optical equipment	DL
Transport equipment	DM
Manufacturing n.e.c.	DN

B Mincer wage regressions

In Figure 4 we report nominal wage growth for a comparable worker type (gender, age, education, and affiliation) between 2002 and 2006. This section explains the estimation procedure.

We use Eurostat's Structure of Earnings Surveys (SES) for the countries that provide good coverage in both years: Belgium, France, Greece, Netherlands, Spain, and the UK. The data include workers in establishments with 10 or more employees. We estimate a standard Mincer-equation of log hourly wages each year, including the following covariates: indicators for gender, age$<$30, age$>$40, part-time, temporary contract, educational attainment (ISCED lower than upper secondary education, upper secondary education, and post-secondary/tertiary education), and a dummy for the local unit or establishment (NUTS1$\times$NACE2-cell for the UK). We add the constant term and the establishment effect and aggregate by region$\times$industry cell each year. The exponent provides us with a measure of wages by region$\times$industry for a full-time and permanently employed male, between 30 and 39 years of age, and with upper secondary education. Next, we calculate the average wage each year over countries by coordination status, using population weights (age 15-74). The columns in Figure 4 show the difference in average wage between 2002 and 2006 for the coordinated and uncoordinated group of countries, respectively. The results are displayed in the third pair of columns of both panels of Figure 4.

C Additional tables

Table C1: 1. stage estimates, baseline regression

	Δ Import Exposure		Δ Import Exposure × Coordination	
	(1)	(2)	(3)	(4)
Δ Import Exposure Other Countries	2.032***	2.038***	0.136***	0.142***
	(0.178)	(0.179)	(0.047)	(0.046)
Δ Import Exposure Other Countries × Coordination	-0.231	-0.227	1.572***	1.579***
	(0.173)	(0.173)	(0.094)	(0.093)
Manufacturing employment share	-0.004***	-0.004***	-0.002***	-0.002***
	(0.001)	(0.001)	(0.001)	(0.001)
Population share medium skilled		0.000		0.001
		(0.001)		(0.001)
Population share high skilled		-0.000		0.000
		(0.001)		(0.000)
Female employment share		-0.002		-0.002
		(0.002)		(0.001)
Observations	366	366	366	366
R^2	0.929	0.929	0.947	0.948

Robust standard errors clustered on NUTS 2 are shown in the parentheses. All regressions include country times period fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C2: Regression with other country-level variables, Non-Manuf

Dep.var.: Four-year change in emp./working-age pop (in %-points), 2000-2008

	Share low-skilled (1)	Union density (2)	Coverage rate (3)	Empl. protection (4)	High-tech manuf. (5)	Working age pop. (6)	Regional variables (7)
Panel A: Weighted regressions							
Δ Import Exposure	-0.549	-0.885	-0.916	-1.057	-0.900	-0.586	-0.887
	(0.947)	(0.951)	(0.921)	(1.358)	(0.915)	(0.853)	(0.943)
Δ Import Exposure	1.796	-0.210	0.814	0.681	0.657	0.459	1.065
× Coordination	(1.134)	(1.463)	(1.232)	(1.098)	(1.247)	(1.231)	(1.270)
Observations	366	366	366	366	366	366	366
R^2	0.317	0.471	0.432	0.474	0.443	0.478	0.361
Post-balancing diff							
(Coord. - Uncoord.)	0.0	5.8	1.2	0.0	0.0	0.0	
Panel B: Additional interactions							
Δ Import Exposure	-4.802*	-0.037	-0.961	-1.057	-3.406*	-1.752	-4.549
	(2.463)	(1.217)	(0.919)	(1.358)	(1.763)	(1.374)	(2.842)
Δ Import Exposure	3.714*	-0.625	0.688	0.681	2.174	1.429	5.354*
× Coordination	(2.008)	(1.432)	(1.216)	(1.098)	(1.477)	(1.671)	(3.127)
Δ Import Exposure	1.729**	1.626	-0.873	0.237	-1.946**	-0.915	
× Coordination	(0.850)	(1.571)	(0.649)	(1.058)	(0.966)	(0.855)	
Observations	366	366	366	366	366	366	366
R^2	0.479	0.472	0.473	0.474	0.477	0.473	0.476

Robust standard errors clustered on NUTS 2 are shown in the parentheses. All regressions include regional controls and country times period fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

D Identification with Bartik-instruments

D.1 Rotemberg weights

In this section we present our calculations of Rotemberg weights, following Goldsmith-Pinkham *et al.* (2018). The Rotemberg weights can be used to measure the relative importance of industry employment shares and import shocks in determining our estimates. Moreover, the industry-specific weights capture the sensitivity to misspecification when the initial industry composition fails the exogeneity assumption of Goldsmith-Pinkham *et al.* (2018).

Our empirical setup has two endogenous variables. This contrasts with the applications discussed in Goldsmith-Pinkham *et al.* (2018), which are limited to a single endogenous variable. We follow a similar approach as Bombardini & Li (2020). When calculating the Rotemberg weights for our main import exposure coefficient we include the first-stage predicted values for the interaction term as a control. Similarly, when calculating Rotemberg weights for the interaction term, we add the predicted values for the main import exposure measure. We also add the other controls from our baseline regression.

The Rotemberg weights are presented in Table D1. In Panel A we show that our estimates are identified primarily from the import shocks and not the initial employment shares. In Panel B we similarly show the variance in the Rotemberg weights across the two time periods. The numbers suggest that the second time period is the most important for our estimates. This is reasonable, since most of the increase in imports from China happened during this time period.

In Panel C we display the top five manufacturing industries in terms of Rotemberg weights. The two dominant industries are manufacturing of textiles and manufacturing n.e.c. This is not surprising, given that the rise in imports primarily came in terms of goods produced by these two industries (see Table 1). Note also that the top five Rotemberg industries for the main coefficient correspond to the top five industries in terms of import increases, in the same order.

D.2 Pre-trends and longer time spans

In this section we present regression estimates controlling for pre-trends, and estimates based on longer time spans.

Our pre-trend variable is based on changes in manufacturing employment as a fraction of working age population during the time period 1995 to 1999. This is as far back in time we are able to stretch our data. To keep as much of the sample as possible, we calculate pre-trends using data for 1996 for those with missing data for 1995 and scale them to four-year changes. This adjustment gives us 42 additional observations. We still lose 17 observations as compared to our main analysis. Due to missing data we are not able to construct the pre-trend variable for non-manufacturing employment. Regression estimates for manufacturing employment are shown in the first column of Table D2. The coefficients of interest are slightly smaller in magnitude as compared to our baseline estimates.[18]

We next estimate a stacked regression specification, using changes from 2000 to 2008. Estimates for manufacturing and non-manufacturing employment are shown in Column (2) and (3) of Table D2. The estimated effects are somewhat larger in magnitude, which might suggest that there are some long-term adjustments not captured by our main specification. Still, the differences with our baseline estimates are not large. We thus conclude that the four-year time periods used in the main analysis is not a serious concern.

D.3 Industry-level regressions: Balance test and inference

Borusyak *et al.* (2018) discuss identification with Bartik-type instruments and show that a typical shift-share regression is numerically equivalent to a transformed regression at the shock-level, in our case the manufacturing industry-level. In general, we can write the Bartik-type instrument as $\sum_j s_{ijt} g_{kt}$, where g_{jt} is the shock to industry j at time period t, while s_{ijt} is the exposure to this shock in region i. In our setting, s_{ijt} can be expressed as $\frac{L_{ij,t-1}}{L_{i,t-1}}$, i.e. the employment share of industry j in region i at the initial time period $t-1$. The shock g_{jt} equals $\frac{\Delta Import_{jt}^{Other}}{L_{j,t-1}}$, which denotes the change in imports from China to other countries over total initial employment in industry j. The transformation of Borusyak *et al.* (2018) consists of averaging the outcomes and treatment variables to the industry-level (potentially residualized by covariates), using the exposure shares, s_{ijt}, as weights. The shocks, g_{jt}, can then be used as instruments for the industry-level treatment variable in a regression using the exposure shares as weights. This provides numerically identical estimates as the conventional (region-level) shift-share regression.

We are not able to implement this directly with our baseline specification, where we have common regional covariates and an interaction term between import exposure and wage-coordination. The interaction term is essentially the industry-level shock multiplied with a regional variable, for which we do not have a separate and dedicated instrument at the industry level.[19] We therefore use a slightly re-written specification and interact all regional covariates with the binary wage-coordination measure. With this specification, we can apply the transformation separately for countries with wage-coordination and countries with uncoordinated wage systems. For each group of countries we thus end up with $j \times t$ observations, which we combine into an overall

[18] This difference is not due the smaller sample. Without the pre-trend variable the sample used here gives very similar estimates as our baseline.

[19] This is different from the setup considered in Bombardini & Li (2020), where they interact the shock with another industry-level variable. For that case the framework of Borusyak *et al.* (2018) can easily be extended.

shock-level dataset.[20] Since each industry appear twice in every time period, we always cluster standard errors on industries to allow for residual correlation between these observations.

The identifying assumption in the framework of Borusyak *et al.* (2018) is that the industry-level shocks are as-good-as-random, conditional on covariates. To test the plausibility of this assumption we first test the shock balance with respect to our regional covariates. This type of exercise can also be used to guide the choice of covariates. Each row in the Table D3 presents coefficients and standard errors from separate regressions using industry-level averages as dependent variables, and exposure shares as weights. Before calculating the industry averages, we residualize out country-year fixed effects and the initial manufacturing employment share.[21] The estimates indicate no significant relationships between the import shocks and the regional variables in the table. That is reassuring and suggests that the import shocks are as-good-as-randomly assigned across industries, when conditioned on country-time effects and the size of initial manufacturing sector.

A useful property of the industry-level regression is that it provides standard errors that account for the type of residual correlation discussed in Adao *et al.* (2019). Borusyak *et al.* (2018) therefore refer to these standard errors as *exposure-robust*, and show that they are asymptotically valid in the framework of Adao *et al.* (2019). By comparing the standard errors with those from the regional-level regression we can hence test whether or not the standard errors in the latter regression is likely to be biased due to residual correlation across industries.

The industry-level estimates are presented in the first column of Table D4. In the second column we present the corresponding regional-level regression, where we interact all controls with the coordination variable. Note first that the standard errors in this regional-level regression is somewhat larger than those in our baseline regression, which is reproduced in the third column. This is not surprising, given the more demanding specification with country group-specific controls. Second, the differences between the standard errors of the industry-level and regional-level regressions are not large: the standard error of the main coefficient is somewhat smaller at the industry-level, while the standard error of the interaction term is somewhat larger (the point estimates are the same, that is equivalence results of Borusyak *et al.* (2018)). We thus conclude that the standard errors of our baseline estimates are unlikely to be seriously biased due to residual correlation across regions with similar manufacturing composition.

In the rest of Table D4 we explore how sensitive the standard errors in our baseline specification are to clustering at larger geographical units. In Column (4) we cluster on NUTS 1-regions. This has little impact on the standard errors. In the final column we cluster on countries. This leads to only slightly larger standard errors. However, our sample consists of just 13 countries,

[20] Note that non-manufacturing counts as a separate industry in this setting. We thus have $j = 15$.

[21] The import shock stems from two sources, as discussed in Section 3.3: the size of the overall manufacturing sector and its composition. Our empirical strategy is to explore the latter form of variation, which is why we control for the initial manufacturing share in all our regressions.

which is below the perceived minimum for valid inference. Because of this, we also report wild-bootstrapped p-values clustered at the country level. We construct the p-values following the procedure in Cameron *et al.* (2008).[22] As can be seen, the bootstrap p-values are not much different from the other p-values reported in the table.

Table D1: Rotemberg weights

	Main coefficient (1)		Interaction term (2)
Panel A: Variance across employment shares and import shocks			
Import shock	0.74		0.88
Employment shares	0.26		0.12
Panel B: Variance across years			
2000-2004	0.12		0.26
2004-2008	0.88		0.74
Panel C: Rotemberg weights top five industries			
Textiles and textile products (DB)	0.630	Textiles and textile products (DB)	0.444
Manufacturing n.e.c. (DN)	0.166	Manufacturing n.e.c. (DN)	0.348
Leather and leather products (DC)	0.087	Machinery and equipment n.e.c. (DK)	0.063
Electrical and optical equipment (DL)	0.045	Electrical and optical equipment (DL)	0.034
Machinery and equipment n.e.c. (DK)	0.034	Chemicals, chemical products and fibres (DG)	0.029

Table D2: Pre-trends and 8 year changes

Dep.var.: Change in emp./working-age pop (in %-points), 2000-2008

	Pre-trends	**8 year changes**	
	Manuf.	Manuf.	N-Manuf.
	(1)	(2)	(3)
Δ Import Exposure	-1.263**	-1.769***	-1.815*
	(0.517)	(0.648)	(1.086)
Δ Import Exposure $\times$ Coordination	1.620***	1.769***	1.530
	(0.587)	(0.606)	(1.301)
Observations	349	179	179
R^2	0.629	0.685	0.483

Robust standard errors clustered on NUTS 2 are shown in the parentheses. All regressions include regional controls and country times period fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

[22] We calculate the p-values in the following way. We first run a regression where we impose the coefficient of interest to be equal to zero, and store the residuals and predicted values from this regression. We second compute new outcome variables as the predicted values, adding or subtracting the residual term with a probability of 0.5 of each happening. We do the adding and subtracting by clusters, meaning that each observation within a cluster gets the residual term either added or subtracted. We third run our original regression with this new outcome variable on the left-hand side. Based on this regression, we calculated t-statistics for our coefficient of interest. We repeat the second and third step 999 times. Finally, the wild clustered p-values are calculated as the proportion of times the absolute value of the t-statistic from our original regression is larger than the absolute value of the (1000) t-statistics from the bootstrap procedure.

Table D3: Balance checks

	Δ Import exp.		Δ Import exp. × Coord.	
	Coeff. (1)	SE (2)	Coeff. (3)	SE (4)
Female employment share	0.039	(0.031)	-0.0110	(0.040)
Population share medium skilled	-0.043	(0.068)	0.001	(0.075)
Population share high skilled	-0.040	(0.085)	0.143	(0.101)
Δ Manufacturing share, t-1	-0.015	(0.012)	-0.006	(0.006)

Each row in the table presents coefficients from a separate regression, using the industry-specific weighted average of the listed variables as dependent variables. The regressions are weighted by the exposure weights. Robust standard errors clustered at the industry-level are shown in the parentheses. The number of observations is 60 ($15 \times 2 \times 2$).

Table D4: Industry-level regression and different clusters

Dep.var.: Four-year change in emp./working-age pop (in %-points), 2000-2008

	Covariates interacted with coordination		Baseline specification		
	(1)	(2)	(3)	(4)	(5)
Δ Import Exposure	-1.333***	-1.333**	-1.408***	-1.408***	-1.408**
	(0.278)	(0.522)	(0.488)	(0.527)	(0.606)
	[.000]	[.011]	[.004]	[.008]	[.023]
					{.038}
Δ Import Exposure × Coordination	1.523*	1.523**	1.659***	1.659***	1.659***
	(0.788)	(0.623)	(0.544)	(0.590)	(0.588)
	[.053]	[.015]	[.002]	[.005]	[.005]
					{.004}
Observations	60	366	366	366	366
Cluster	Industry	NUTS2	NUTS2	NUTS1	Country

Robust standard errors are shown in the parentheses. All regressions at the regional level include controls and country times period fixed effects. For the industry-level regression, we residualize out the same covariates before calculate the industry-level average, and the regression is weighted by average exposure. The brackets show p-values, while the curly brackets show bootstrap p-values. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

E Robustness analysis

In this section we present the regression estimates from our robustness analysis.

In Table E1 we make use of finer employment data for 103 manufacturing industries at the country-level. We calculate the employment share of these industries within each 2-digit NACE industry and impose the same composition for every region within a country. The estimates suggest that our findings are robust to this alternative specification.

Table E2 splits the binary coordination index into four binary variables, one for each index value of one to five. The estimated interaction coefficients between import exposure and each of the three coordination variables of three or higher are very similar and close to zero.

In Table E3 we include time-variant regional covariates. This has little impact on our estimates.

Table E4 explores how sensitive our estimates are to particular manufacturing industries. For each of the top five importing industries we re-calculate our import exposure measure, leaving out imports of goods produced by these particular industries. Doing this, we also reduce the overall magnitude of the import shock. This is especially so when we remove one of the top two industries. To ease the comparison with our baseline estimates we therefore standardize import exposure to mean zero and standard deviation one. The first column reproduces our baseline estimates using this standardization, while the rest of the columns present estimates leaving out one manufacturing industry at a time. The table reveals that our estimates are remarkably robust to this exercise.

In Table E5 we similarly re-do the analysis by dropping each of the 13 countries at a time. The only regression in which the coefficients differ from our baseline by some magnitude is the one where we exclude Portugal. In this regression, the negative employment effect for countries with uncoordinated wage systems become even stronger.

In Table E6 we test how sensitive our results are to the use of different instruments. We calculate four different instruments for import exposure based on imports from China to each of the four countries: Australia, New Zealand, Canada and the US. Are results are very robust to the use of these different instruments.

Finally, Figure E1 presents binned scatter plots of the relationship between predicted change in import exposure and changes in manufacturing employment as a fraction of the working age population, separately for countries with and without wage-coordination.

Table E1: 4-digit NACE industries

Dep.var.: Four-year change in emp./working-age pop (in %-points), 2000-2008

	Manuf. (1)	N-Manuf. (2)
Δ Import Exposure	-1.383***	-0.364
	(0.493)	(0.916)
Δ Import Exposure × Coordination	1.608***	0.243
	(0.564)	(1.322)
Observations	366	366
R^2	0.611	0.473

Robust standard errors clustered on NUTS 2 are shown in the parentheses. All regressions include regional controls and country times period fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table E2: Finer categorizes of coordination

Dep. var.: Four-year change in emp./working-age pop (in %-points), 2000-2008

	Manuf. (1)	N-Manuf. (2)
Δ Import Exposure × Coordination=1	-4.115***	1.227
	(1.580)	(4.452)
Δ Import Exposure × Coordination=2	-1.178***	-1.108
	(0.392)	(0.924)
Δ Import Exposure × Coordination=3	0.334	-0.291
	(0.439)	(1.237)
Δ Import Exposure × Coordination=4	0.064	0.578
	(0.493)	(1.906)
Δ Import Exposure × Coordination=5	-0.040	-0.984
	(0.685)	(3.176)
Observations	366	366
R^2	0.618	0.472

Robust standard errors clustered on NUTS 2 are shown in the parentheses. All regressions include regional controls and country times period fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table E3: Time-variant controls

Dep.var.: Four-year change in emp./working-age pop (in %-points), 2000-2008

	Manuf.		N-Manuf.	
	(1)	(2)	(3)	(4)
Δ Import Exposure	-1.686***	-1.474***	-0.972	-0.618
	(0.557)	(0.532)	(0.954)	(0.824)
Δ Import Exposure	1.497**	1.440**	0.690	0.300
× Coordination	(0.592)	(0.576)	(1.284)	(1.168)
Manufacturing employment share	-0.032***	-0.032***	0.068***	0.074***
	(0.008)	(0.008)	(0.025)	(0.025)
Population share medium skilled		0.035***		-0.013
		(0.010)		(0.038)
Population share high skilled		0.010		0.002
		(0.008)		(0.028)
Female employment share		-0.044***		-0.046
		(0.014)		(0.054)
Observations	366	365	366	365
R^2	0.591	0.615	0.474	0.480

Robust standard errors clustered on NUTS 2 are shown in the parentheses. All regressions include country times period fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table E4: Leaving out one industry at a time

Dep.var.: Four-year change in emp./working-age pop (in %-points), 2000-2008

	Main	DB	DN	DC	DL	DK
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Manufacturing employment						
Δ Import Exposure	-0.213***	-0.185***	-0.161***	-0.200***	-0.172***	-0.182***
	(0.074)	(0.040)	(0.051)	(0.069)	(0.053)	(0.057)
Δ Import Exposure	0.251***	0.277***	0.190***	0.244***	0.211***	0.226***
× Coordination	(0.082)	(0.058)	(0.058)	(0.071)	(0.061)	(0.064)
Observations	366	366	366	366	366	366
R^2	0.612	0.618	0.610	0.610	0.612	0.613
Panel B: Non-manufacturing employment						
Δ Import Exposure	-0.140	-0.143	-0.082	-0.146	-0.125	-0.104
	(0.138)	(0.157)	(0.097)	(0.126)	(0.114)	(0.110)
Δ Import Exposure	0.118	0.195	0.143	0.174	0.192	0.156
× Coordination	(0.187)	(0.184)	(0.143)	(0.154)	(0.149)	(0.151)
Observations	366	366	366	366	366	366
R^2	0.474	0.474	0.474	0.474	0.474	0.474

Robust standard errors clustered on NUTS 2 are shown in the parentheses. All regressions include regional controls and country times period fixed effects. Import exposure is standardized to mean zero and standard deviation one. The estimates in Column (2) to (6) are based on import exposure excluding imports to the manufacturing industry shown in the heading. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table E5: Leaving out one country at a time

Dep.var.: Four-year change in emp./working-age pop (in %-points), 2000-2008

	AT	BE	DE	EL	ES	FR	IE	IT	NL	NO	PT	SE	UK
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Panel A: Manufacturing employment													
Δ Import Exposure	-1.372***	-1.504***	-1.730***	-1.294***	-0.944***	-1.419***	-1.428***	-0.993***	-1.547***	-1.436***	-2.711***	-1.378***	-1.593***
	(0.477)	(0.521)	(0.611)	(0.461)	(0.330)	(0.493)	(0.494)	(0.345)	(0.535)	(0.497)	(0.806)	(0.477)	(0.495)
Δ Import Exposure	1.600***	1.747***	1.716***	1.797***	1.539***	1.679***	1.664***	1.296***	1.782***	1.666***	2.777***	1.692***	1.522***
$\times$ Coordination	(0.547)	(0.584)	(0.654)	(0.538)	(0.427)	(0.578)	(0.550)	(0.428)	(0.593)	(0.553)	(0.822)	(0.539)	(0.560)
Observations	348	344	307	340	332	322	362	330	342	352	355	350	308
R^2	0.619	0.618	0.629	0.626	0.641	0.628	0.604	0.616	0.608	0.607	0.614	0.619	0.471
Panel B: Non-manufacturing employment													
Δ Import Exposure	-0.730	-0.760	-1.614	-0.740	-0.879	-0.189	-0.927	-0.731	-0.815	-0.916	-1.552	-1.042	-1.370
	(0.892)	(0.930)	(1.316)	(0.858)	(0.956)	(0.828)	(0.924)	(0.700)	(0.954)	(0.923)	(2.496)	(0.941)	(1.089)
Δ Import Exposure	1.040	0.994	1.798	0.159	1.514	-0.085	0.785	-0.469	0.846	0.808	1.075	0.712	1.272
$\times$ Coordination	(1.221)	(1.277)	(1.493)	(1.258)	(1.064)	(1.309)	(1.245)	(1.122)	(1.331)	(1.250)	(2.489)	(1.240)	(1.452)
Observations	348	344	307	340	332	322	362	330	342	352	355	350	308
R^2	0.441	0.469	0.358	0.505	0.441	0.553	0.473	0.484	0.477	0.467	0.475	0.479	0.527

Robust standard errors clustered on NUTS 2 are shown in the parentheses. All regressions include regional controls and country times period fixed effects. For each regression, we recalculate import exposure by excluding the country shown in the heading. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

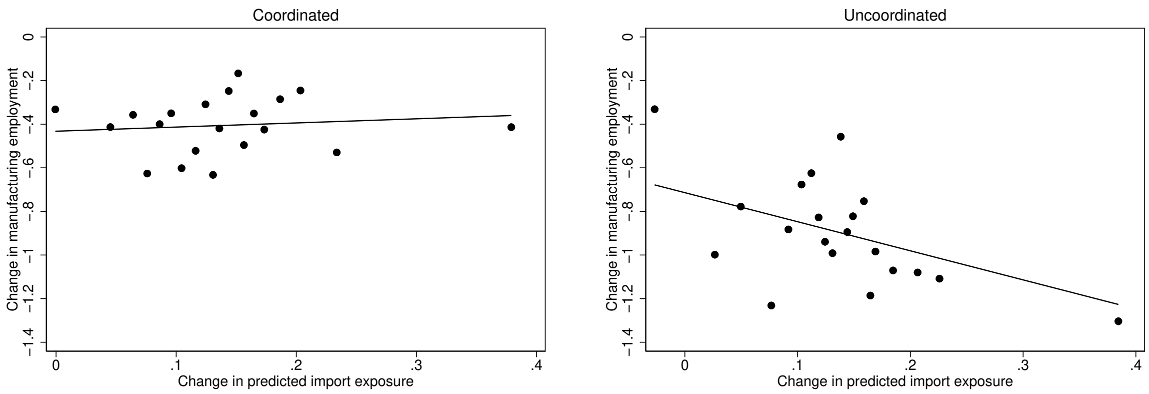
Table E6: Alternative instruments, using trade flows from China to individual countries

Dep.var.: Four-year change in emp./working-age pop (in %-points), 2000-2008

	Manufacturing employment				**Non-Manufacturing employment**			
	AU	NZ	CA	US	AU	NZ	CA	US
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δ Import Exposure	-1.540***	-1.869**	-1.407***	-1.378***	-1.740*	-2.499*	-1.136	-0.697
	(0.558)	(0.834)	(0.466)	(0.493)	(0.981)	(1.417)	(0.970)	(0.950)
Δ Import Exposure × Coordination	1.901***	1.665**	1.765***	1.588***	1.281	1.663	1.098	0.583
	(0.591)	(0.740)	(0.516)	(0.551)	(1.221)	(1.573)	(1.154)	(1.291)
Observations	366	366	366	366	366	366	366	366
R^2	0.611	0.607	0.612	0.612	0.473	0.471	0.474	0.474
F-stat excluded instruments								
Δ Import Exposure Other	109.4	153.4	296.7	90.2	109.4	153.4	296.7	90.2
Δ Import Exposure Other × Coordination	168.2	509.3	342.4	117.6	168.2	509.3	342.4	117.6

Robust standard errors clustered on NUTS 2 are shown in the parentheses. All regressions include regional controls and country times period fixed effects. The instrument for import exposure is based on trade flows from China to each of the countries listed in the heading. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure E1: Binned plot of manufacturing employment versus predicted import exposure



Binned plot of the relationship between predicted change in import exposure and changes in manufacturing employment as a fraction of the working age population. The right panel uses data for countries with uncoordinated wage-setting, and thus correspond to the 2.stage estimate shown in the first row of Column (2) Table D4. The left panel correspondingly uses data for countries with wage-coordination. The plots partial out all regional covariates, as well as the country×period fixed effects.