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Trade Exposure and the Decline in Collective Bargaining: Evidence from Germany

Abstract

We analyze the effect of the increase in trade exposure induced by the rise of China and the transformation of Eastern Europe on collective bargaining coverage of German plants in the period 1996–2008. We exploit cross-industry variation in trade exposure and use trade flows of other high-income countries as instruments for German trade exposure. We find that increased import exposure has led to an increase in the probability of German plants leaving industry-wide bargaining agreements, accounting for about one fifth of the overall decline in the German manufacturing sector. The effect is most pronounced for small and medium-sized plants.

JEL-Codes: F160, J510.

Keywords: international trade, import competition, collective bargaining.

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1 Introduction

One of the most important changes in the institutional landscape of many advanced countries' labor markets has been a strong decline in unionization and collective bargaining coverage. Panel (a) of Figure 1 shows the development of two key indicators in this respect in the OECD countries. The first is union density, measured as the share of union members in total employment, and the second is the collective bargaining coverage rate, which refers to the share of workers covered by a collective bargaining agreement and, hence, centralized wage negotiations. Between 1990 and 2010, union density declined from 26 to 18 percent and collective bargaining coverage dropped from 43 to 35 percent. It is evident from Panel (b) that there is substantial variation across countries, reflecting their different institutional settings.[1] However, the general pattern is quite pervasive, with 18 out of the 24 countries (with the available data) experiencing a decline in the collective bargaining coverage rate, 3 a constant rate and only 3 an increase.

The consequences of this decline in unionization and collective bargaining coverage have been widely discussed in the related literature, where the considered outcome variables include wage inequality (e.g. DiNardo et al., 1996; Card et al., 2004), demographic employment patterns (Bertola et al., 2007), macro performance (Mitchell and Erickson, 2005), or the extent of international outsourcing (Lommerud et al., 2009).

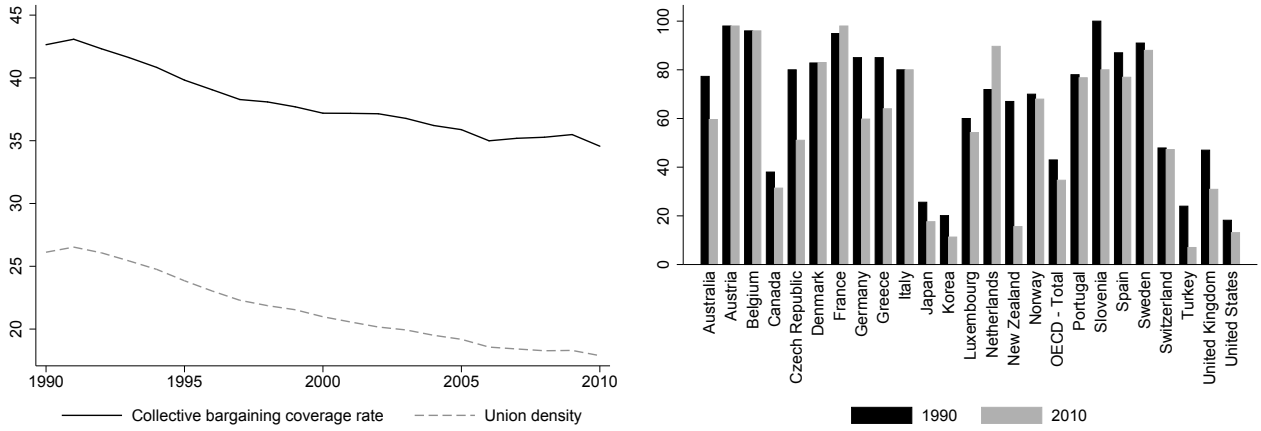
The reasons for this decline are less clear, however. Acemoglu et al. (2001) posit that skill-biased technical change has been an important driver. Their argument is that, by increasing the outside option for skilled workers, technical change undermines the coalition among skilled and unskilled workers in the formation of unions. Another explanation is structural change in advanced economies (Hirsch, 2008), the idea being that unionization declines due to employment reallocation from unionized to non-unionized industries.

In this paper, we focus on an alternative explanation: globalization, and in particular increased exposure to low-wage country import competition. Moreover, we explore changes in bargaining regimes at the level of the employer, instead of looking at the formation of unions at the workers' side. This fits the institutional setting in Germany, the focus of our analysis.

Germany is an interesting case in point, for two main reasons. First, Germany experienced a strong decline in collective bargaining coverage in the recent past. From 1996 to 2008, our period of analysis, the share of establishments covered by a collective bargaining agreement (CBA) fell from 58 to 35 percent.[2] The major part of this decline was driven by a decrease in industry-wide collective agreements, which implied a decentralization of the wage setting process from the industry level to the level of the individual firm. This remarkable shift has been hinted at as a source of increased German competitiveness (Dustmann et al., 2014), but also as a major driver of rising wage inequality (Dustmann et al., 2009; Antonczyk et al., 2010; Baumgarten et al., 2018; Biewen and Seckler, 2017). The second reason is that Germany's trade exposure increased greatly during the same period

[1] A good overview of the different institutional settings is given in OECD (2017).

[2] These numbers are based the IAB Establishment Panel. Details on this data set are provided in Section 4. The numbers differ from the ones obtained from the OECD in Figure 1, Panel (b), because the latter are based on the share of workers (as opposed to establishments) covered by a collective agreement.



(a) OECD-wide trends (in %) **(b)** CB coverage rate across countries (in %)

Sources: OECD (2017), online data appendix at `https://doi.org/10.1787/empl_outlook-2017-8-en`, accessed June 26, 2019; and OECD.Stat (`https://stats.oecd.org/Index.aspx?DataSetCode=CBC`, accessed June 26, 2019.

Notes: Panel (b) includes all OECD countries with available data for the years 1990 and 2010 (or the closest possible year within a range of ±2 years). The stated values for 2010 refer to 2008 for Belgium, 2009 for France and Norway, and 2011 for Luxembourg and Sweden.

Figure 1. Trends in collective bargaining coverage and unionization

of time. Much of it has been due to two major globalization shocks which originated in predominantly labor-abundant countries with substantially lower wages than Germany: on the one hand the rise of China and its accession to the World Trade Organization (WTO) in 2001 and on the other hand the fall of the Iron Curtain and the subsequent transformation of the former socialist countries into market economies. Over the period from 1996 to 2008, our period of analysis, German imports from China and Eastern Europe – in the following referred to as "the East"[3]– grew by more than 300 percent (from 42 billion euros in 1996 to 182 billion euros in 2008, measured in constant year-2000 terms).[4]

In this paper, we explore to what extent the decline in industry-wide CBAs can be causally attributed to the rise in import competition from China and Eastern Europe. Our hypothesis is that an increase in import penetration on final goods markets is likely to induce some firms to opt out of collective wage agreements, both because particularly smaller, less productive firms find it increasingly difficult to pay union wages and because workers are more willing to accept an opting-out decision if their employers face a credible threat of going bankrupt or downsize production.[5] For our analysis, we exploit variation in

[3]The East covers China and the following Central and Eastern European countries: Bulgaria, Czech Republic, Hungary, Poland, Romania, Slovakia, Slovenia, the former USSR or its succession states Russian Federation, Belarus, Estonia, Latvia, Lithuania, Moldova, Ukraine, Azerbaijan, Georgia, Kazakhstan, Kyrgyzstan, Tajikistan, Turkmenistan, and Uzbekistan.

[4]Of course, Germany also increased its export volumes to China and Eastern Europe over the respective time period. Figure 2 in Section 4 illustrates the evolution of German import and export volumes with respect to both regions.

[5]That globalization and the firm's choice of leaving industry-wide collective bargaining might be linked, is also backed up by anecdotal evidence. For example, the agricultural machinery producer Amazone from Northern Germany left industry-wide collective bargaining in 2012, claiming that it wanted to become "more flexible and

changes in import (and export) exposure both across three-digit industries as well as over time and relate them to establishments' changes of their bargaining regime. To identify a causal relationship, we apply the instrumental variable strategy pioneered by Autor et al. (2013) and instrument trade flows between Germany and the East by trade flows between the East and other high-income countries.

Our results can be summarized as follows. We find that an increase in import competition from China and Eastern Europe over the period from 1996 to 2008 induces establishments to leave centralized collective bargaining regimes. In terms of magnitude, we conclude that the increase in import exposure explains about 19 percent of the overall decline in industry-wide CBAs in the German manufacturing sector. In contrast, we do not find, on average, any effect of increased export opportunities on establishments' decisions to change their collective bargaining status. Furthermore, an analysis of heterogeneous effects reveals that it is the small and medium-sized plants that react most strongly to an increase in import penetration.

Our paper contributes to the literature in two main ways. First, we analyze a new dimension of how firms react to increased import competition. This channel is important, since changes in collective bargaining coverage have been shown to influence a host of economic and labor market outcomes. We therefore shed light on an indirect, albeit important link between international trade and labor market outcomes that, to our knowledge, has been largely neglected in the existing literature. Our second, more general contribution is that we stress the endogeneity of labor market institutions. Most existing trade models treat labor market institutions as exogenous. To the extent that international trade changes bargaining regimes, this exogeneity assumptions is challenged and should lead to modified trade models that treat labor market institutions as endogenous.

Closest to our paper is a short paper by Carluccio et al. (2016) that analyzes the link between firm-level importing/offshoring as well as exporting and the propensity to sign a firm-level collective agreement in France. Contrary to our findings, their results indicate that exporting is positively related to a firm's probability of signing a firm-level agreement while importing/offshoring has no significant effect. Our paper differs from theirs in important respects. Most importantly, the institutional setting is different in France and Germany. In France, industry-level wage agreements define wage floors by occupations that are binding for *all* firms in the specific industry, and firm agreements are optional and on top of these industry agreements. Therefore, Carluccio et al. (2016) interpret these firm-level agreements as a way of additional rent sharing. In contrast, in Germany, industry-wide collective agreements with their implied wage floors are only binding for the firms that sign them. Firms are free to pay more than than the negotiated wage floors regardless. We explicitly address the issue of *leaving* industry-wide collective bargaining and think of leaving firms as the ones seeking more downward wage (and general employment) flexibility. Based on these differences in the institutional set-up, we also have a different mechanism

better positioned for global competition" (authors' translation; source in German: `http://www.wochenblatt.com/landwirtschaft/nachrichten/amazone-verlaesst-arbeitgeberverband-4871.html`, last accessed June 26, 2019). Needless to say, these claims need to be taken with a grain of salt as globalization might be an easy scapegoat for an unpopular decision, but they further motivate a systematic analysis based on representative data.

in mind and choose a different source of variation. Looking at the question through a heterogeneous-firm lens, we expect the firms most likely to suffer from increased foreign competition – and hence potentially most inclined to leave collective bargaining – to be the smallest and least productive ones in an industry, and these are usually not the ones that engage in international trade themselves. Hence, we measure import (and export) exposure at the industry level as opposed to the firm or plant level.

In addition, our analysis relates to the following strands of the literature. First, we add to the empirical literature that analyzes the causal effects of import competition (from low-wage countries) on different margins of firm adjustment, such as employment growth and survival (Bernard et al., 2006), innovation (Bloom et al., 2016; Autor et al., 2019), skill upgrading (Mion and Zhu, 2013), and changes in the product mix (Bernard et al., 2010).

Within this strand of the literature, our identification strategy builds on the influential study by Autor et al. (2013) and their follow-ups. In their analysis of the US labor market consequences from increased Chinese import competition, they account for unobserved shocks that simultaneously affect imports and labor market outcomes by using trade flows from China to other high income countries as an instrument for US trade exposure, thereby only exploiting the "supply shock element" of Chinese import competition. They find severe negative effects on US manufacturing employment. An excellent overview of follow-up and related research is given in Autor et al. (2016). Dauth et al. (2014) apply the same empirical strategy to the German context. They, however, do not only focus on China, but also consider trade with nearby Eastern Europe, whose rise and opening-up resembles the one of China in several respects. Moreover, Dauth et al. (2014) investigate the effects induced by both increased import competition and rising export opportunities. Taking both channels into account, they find that trade with China and Eastern Europe has contributed to retaining employment in the manufacturing sector in Germany.

In their initial paper, Autor et al. (2013) measure trade exposure at the level of local labor markets by apportioning national industry-level trade flows to regions according to their share of national industry employment so that they are able to capture both the direct effect on import-competing employers and indirect effects in the surrounding geographic area. In this paper, however, we measure trade exposure at the industry level as in Autor et al. (2014) or Dauth et al. (2016), thereby focusing on the first-order direct effect on employers in the import-competing (or export-oriented) industry. Relying on industry-level trade flows has further advantages in our context. On the one hand, in the German institutional setting, employers and unions are usually also organized at the level of industries. On the other hand, we avoid the potential pitfalls of using a shift-share type of instrument that have recently been highlighted in a number of papers (Goldsmith-Pinkham et al., 2018; Borusyak et al., 2018; Adao et al., 2018).

Second, our research relates to a more specific literature that deals with firm or establishment determinants of the choice of the bargaining regime (e.g. Schnabel et al., 2006) and with reasons to leave centralized wage bargaining (Kohaut and Schnabel, 2003) in Germany. This literature, however, does not explore the role of increased trade exposure, nor does it aim at the causal identification of effects. Further contributions that theoretically model the endogenous choice of different bargaining regimes at the level of the firm are

discussed in Section 3.

Finally, at a more general level, this paper also speaks to the trade-and-institutions literature as summarized in Nunn and Trefler (2014). A large part of this literature analyzes the role of domestic institutions as a source of comparative advantage, where some papers have indeed focused on labor market institutions. Egger et al. (2015b) show that differences in unionization rates across countries can be a source of comparative advantage and, therefore, shape trade patterns. Other labor market institutions that have been considered in this context are worker monitoring capabilities (Costinot, 2009), labor market protection regulations (Tang, 2012), and overall labor market flexibility (Cuñat and Melitz, 2012). We focus on the other direction of causality and analyze to what extent trade shapes domestic labor market institutions.[6]

The rest of the paper is organized as follows. Section 2 discusses the institutional background and the system of industrial relations in Germany. Section 3 discusses the theoretical background and formulates the working hypothesis. The data employed in our analysis is introduced in Section 4. Section 5 describes our empirical approach, before the results are discussed in Section 6. Section 7 concludes.

2 Institutional background

The German system of industrial relations is based on the principle of *autonomy of wage bargaining*, which is rooted in Article 9 III of the German constitution. It implies that the right to negotiate over wages and working conditions is assigned only to the labor market parties, that is employers, employer associations, and trade unions. The principle of wage bargaining autonomy guarantees that the process of industrial relations is independent of the government or the political process. Different from many other countries, industrial relations are therefore based on contracts and mutual agreements and are not rooted in legislation.[7] Moreover, Germany has introduced a statutory minimum wage only after our period of analysis (in 2015).

Collective agreements are negotiated either at the region-industry level or at the firm level. They typically cover arrangements with respect to wages, working hours, and other aspects of working conditions. Collectively bargained wages generally act as minimum

[6] In this context, Dreher and Gaston (2007) have analyzed the importance of globalization, in a much broader sense, on deunionization in a macroeconomic cross-country setting. Our analysis, however, departs from their contribution in several important respects. First, we analyze the relationship between trade exposure and the decline in collective bargaining within one country, exploiting variation across industries and plants. This way, we circumvent any issues relating to (unobserved) cross-country heterogeneity, which might hinder the proper identification of parameters. In addition, by focusing on establishments, our unit of analysis corresponds to the economic entity which actually decides on the bargaining regime, at least in Germany, and we are able to control for many establishment-level characteristics. Second, and relatedly, we focus on collective bargaing coverage as opposed to union density, where the former is the relevant variable for determining how many workers are covered by centralized wage negotiations. Third, we focus on a clearly defined aspect of globalization, increased trade exposure to low-wage countries. Fourth, our empirical approach allows us to establish causal effects.

[7] An exception are so called "Allgemeinverbindlichkeitserklärungen". In exceptional cases, the government can declare a collective agreement legally binding for all firms in an industry, including employers and employees that originally were not covered. Since the legal requirements for these government-extended collective agreements are quite high, they play only a minor role today. In 1996, (2010) 4.1% (1.5%) of all collective agreements were declared generally binding. The majority of these apply to the construction sector. See Bispinck (2012) and Schulten and Bispinck (2013) for further information.

wages. Payments above the union wage – the so-called wage cushion or wage drift – are common (Dustmann and Schönberg, 2009). On average, wages in establishments covered by collective agreements are higher than in uncovered ones (e.g. Guertzgen, 2016), but also less responsive to firm-specific quasi-rents (Guertzgen, 2009). As a result, wage dispersion is more compressed in the covered sector. In line with this evidence, Hirsch and Mueller (2018) find that collective bargaining coverage is associated with a positive wage premium at the first decile of the wage premium distribution, but not at the ninth decile.

A special feature of the German institutional setting is that the recognition of trade unions is at the discretion of the firm. This implies that collective contracts cover only workers that are employed in firms that recognize the relevant collective agreement – and this is generally true whether the worker is a union member or not.[8] On average, collective agreements in Germany are formed for a period of 22 months.[9] During this period, a duty not to engage in industrial disputes holds.

Firms that once have recognized a collective contract can choose to opt out at their own discretion. They can do so by leaving the respective employer association or by becoming a so-called "OT member" (OT = "ohne Tarif", i.e. without collective agreement), which gives employers the possibility to be a member of the association without being covered by the collective agreement, thereby still benefiting from other services. If a firm leaves its association or changes membership, this does not instantly terminate the collective agreement. Instead, the employer has to stick to the agreement until it ends ("Nachwirkungsfrist"). During this period, however, the collective agreement does not apply anymore to new hires, nor is the firm obliged to honor newly negotiated wage increases. After the expiration of an agreement, new employment and wage contracts can be formed. Over time, a firm therefore may be able to lower wage costs and increase employment flexibility by opting out of a collective agreement.

Over the last couple of years so-called opening or hardship clauses have also gained importance (see Brändle et al., 2011 and Bispinck et al., 2010). These clauses allow firms to *temporarily* deviate from some collectively agreed standards in times of economic difficulties. The firm is, however, expected to return to the general conditions of the original agreement after a predefined period. Another characteristic feature of the German system of industrial relations is that a considerable fraction of firms not officially bound by an (industry-wide) collective agreement states that it is "oriented" toward one and follows its basic features. Since the fraction of self-declared orienting establishments has increased over time, Addison et al. (2016) ask whether the erosion thesis is "overblown". However, according to their results, orientation and true coverage seem to be imperfect substitutes at best. First, the increase in orientation is substantially smaller than the decrease in coverage of industry-wide collective agreements. Second, both in the cross-section and when following establishments that leave collective bargaining over time, wages in orienting establishments are lower than in establishments covered by industry-wide agreements. Finally, orientation is not legally

[8] From a legal perspective, a collective agreement is only binding for union members of a firm. However, the employer generally extends the agreement conditions to all (comparable) workers to weaken workers' incentives to become a union member. Due to this practice, collective bargaining coverage rates are generally higher than union membership rates (see Fitzenberger et al., 2013).

[9] See `http://www.boeckler.de/wsi-tarifarchiv_4832.htm`, last accessed June 26, 2019.

binding and may be "a weak policy tool as firms can withdraw from the terms set in the agreement at any time or just pick-and-choose the elements of the agreement they like" (OECD, 2017, p. 144).

3 Theoretical background and working hypothesis

Although there is a long-standing literature on the effects of trade (liberalization) on labor market outcomes in the presence of unionization (e.g. Brander and Spencer, 1988; Bastos and Kreickemeier, 2009), it offers little guidance for our empirical analysis. This is because in these models, unionization is a fixed country and/or industry characteristic and thus exogenous.[10]

More relevant for our analysis therefore is a strand of literature which considers the endogenous choice of wage bargaining regimes – without focusing on globalization effects, however. While some papers deal with the endogenous formation of unions on the worker side (e.g. Acemoglu et al. 2001), most relevant are those papers which consider the endogenous choice of the bargaining regime on the employer's part, which fits the institutional setting in Germany (cf. Section 2).

One example is Jimeno and Thomas (2013). They consider an economy with search and matching frictions, firm-specific productivity shocks and firm-level and sector-level bargaining. Part of their analysis is about an *efficient-opting-out* scenario where all firms are ex-ante covered by a centralized wage regime, but firms and workers can mutually agree to move from sector-level bargaining to firm-level bargaining. In this framework, the productivity threshold below which workers are laid off is higher in the centralized than in the decentralized regime, as the wages paid by firms do not respond to firm-specific (negative) productivity shocks. This implies that the least productive firms which cannot afford to pay collective wages opt out of CBAs. Their employees accept the opting-out since they would otherwise lose their jobs.

In another paper, Baumann and Brändle (2017) establish a link between the extent of collective bargaining coverage and the degree of productivity dispersion within an industry. In their model, the choice of the bargaining regime is based on the tradeoff between wage costs and fixed transaction/negotiation costs, which are assumed to be higher under decentralized bargaining. They show that a more dispersed productivity distribution among firms leads to lower collective bargaining coverage, as a uniform wage becomes unattractive for a larger fraction of firms. They further predict that less productive firms are most likely to follow a fully decentralized wage regime, as for them the burden of a uniform wage is greatest.[11]

Hirsch et al. (2014) put forward the argument of a "hide effect", according to which high productivity firms self-select into centralized wage regimes since it allows them to hide behind less productive firms. The underlying idea is that under a decentralized regime, firms pay wages according to their own productivity, while under centralized bargaining,

[10] In the model of Bastos and Kreickemeier (2009), deunionization is modeled as a decrease in the share of unionized sectors, but again exogenous with respect to trade liberalization.

[11] The transaction-cost savings argument is also central to the paper by Capuano et al. (2014), which essentially arrives at the same prediction.

the wage is based on an industry average or some other aggregate reference point.

Thus, despite highlighting different mechanisms, all of these models share the prediction that the smallest, least productive firms will prefer decentralized wage bargaining. In contrast, larger, more productive firms will opt for centralized wage regimes.[12]

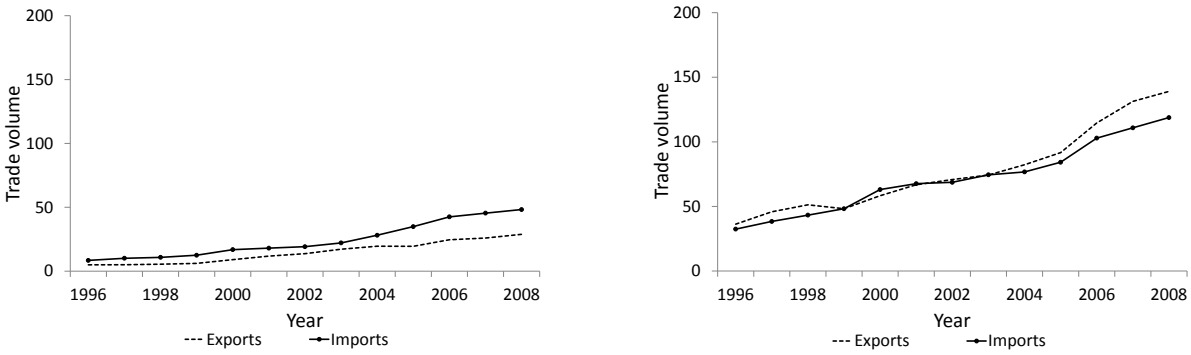
As outlined above, these models remain silent about the effects of increased trade exposure or globalization. We argue, however, that the models are nevertheless informative regarding the likely impact of increasing trade exposure, as the latter will affect the size (and profit) distribution within an industry, with direct implications for firms' choices of their bargaining regime. Workhorse heterogeneous-firm trade models such as Melitz (2003) predict that the largest, exporting firms expand and the smaller, purely domestic firms contract in reaction to increasing trade liberalization.

Thus, we expect increased import exposure to induce some firms – in particular the smaller, less productive firms – to opt out of a centralized collective bargaining regime in order to lower wage costs and increase employment flexibility. Likewise, we expect employees to be more likely to accept an opting-out decision if they face a credible threat of losing their jobs. In contrast, increased export exposure should benefit the largest firms and thus lead to less exits from or even more entry into centralized bargaining agreements among this subset of firms.[13]

We should note that the mechanism we have in mind relates primarily to increases in final goods trade exposure. We acknowledge however that the rise of the East also implies an increase in the opportunities of offshoring, i.e. the threat of relocating (parts of) the production abroad, which constitutes an alternative mechanism through which firms and employees might be influenced in their decision to agree upon leaving a centralized bargaining regime. The reasoning in Dustmann et al. (2014) refers to this second channel. They argue that German firms were able to increase their bargaining position significantly after the transformation of Central and Eastern Europe since this development made it credible that German firms might relocate production. Although we are not going to test this second channel explicitly, we will discuss to what extent our results might capture this alternative mechanism.

[12]Only the model by Baumann and Brändle (2017) deviates slightly in this respect. While they do share the prediction that the least productive firms opt for fully decentralized wage bargaining, they also predict that the most productive firms will be subject to firm-level collective bargaining. The latter prediction, however, is based on the assumption that unions can force these firm-specific agreements upon the firm, at the expense of some fixed costs.

[13]The mechanism we have in mind is related to, but also different from the one proposed by Do and Levchenko (2009). They analyze theoretically the relationship between international trade and the quality of economic institutions – modeled as costs of firm entry – in a setting with heterogeneous firms. They assume that political (lobbying) power of firms is directly linked to firm size and that larger firms prefer to set higher costs of entry, ceteris paribus, to reduce competition. As trade leads to a more unequal distribution of firm size, it shifts political power towards larger firms, which can in turn lead to worse institutions (i.e. higher entry costs). The difference with respect to our setting is that the institution in Do and Levchenko (2009) applies to all firms. In the case of collective bargaining, however, while it is conceivable that larger firms can tilt the terms of collective agreements in their favor, smaller firms have the possibility to opt out.



(a) China **(b)** Eastern Europe

Source: German Statistical Office.

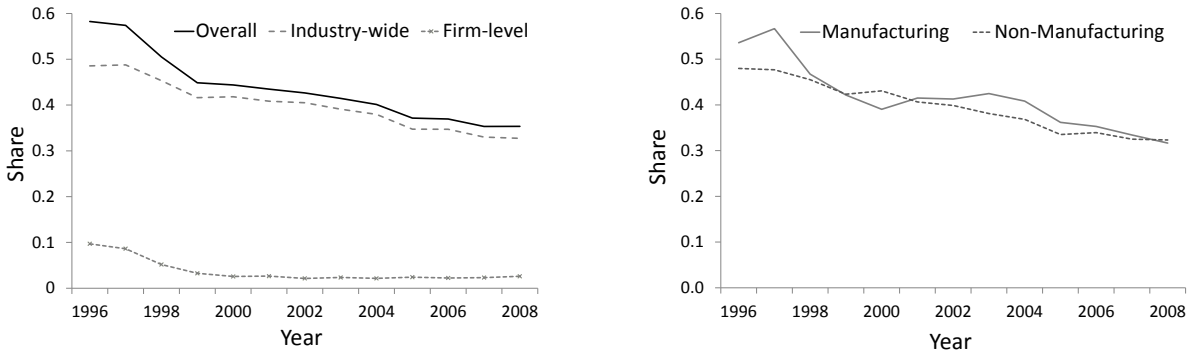
Figure 2. German trade volumes in (year-2000) billion euros, 1996–2008

4 Data

Trade Data For our analysis, we use data on international trade from the BACI Database, which is provided by the CEPII and builds on the UN Comtrade Database. It contains detailed bilateral trade statistics for more than 200 countries and 5,000 products. We aggregate the product-level trade data to the three-digit industry level (in the WZ93 classification, which is equivalent to NACE Rev.1). Trade values are converted into year-2000 euros using exchange rates and consumer price indices supplied by the German Bundesbank and the German Statistical Office, respectively. Figure 2 shows the evolution of German import and export volumes from/to China and Eastern Europe in the aggregate. It can be seen that the German economy experienced a sizable increase in trade volumes with respect to both regions. Dauth et al. (2014) discuss in detail that this increase in trade exposure was much larger than with respect to any other German trading partner in the world, making it the major trade shock that hit the German economy during the last two decades – and one which originated primarily from low-wage countries.

Establishment Data Our establishment-level data are based on the IAB Establishment Panel (EP), which is provided by the Research Data Centre of the Institute for Employment Research (IAB).[14] The EP is a stratified random sample of all German establishments which employ at least one worker covered by social security. Strata are defined over three dimensions: regions, industries, and size classes. Appropriate weights are provided to ensure the representativeness of the data. The EP started in 1993 with 4,265 establishments in West Germany. East German establishments were included from 1996 onwards. After taking

[14]More precisely, this study uses the Linked-Employer-Employee Data (LIAB) [cross-sectional model 2 1993–2010 (LIAB QM2 9310)] from the IAB, which combines the EP with social security records on individual workers (see below). Data access was provided via on-site use at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB) and subsequently remote data access. See Ellguth et al. (2014) for further details on the EP and Heining et al. (2013) for a detailed description of the LIAB data.



(a) All CBA types, whole economy **(b)** Industry-wide CBAs by economic sector

Source: IAB Establishment Panel. Notes: Sampling weights are employed.

Figure 3. Decline in collective bargaining coverage in Germany, 1996–2008

in several waves of additional establishments, the sample size increased to about 16,000 in 2008. Although participation is voluntary, the response rate of repeatedly interviewed establishments is high, amounting to about 80 percent. We augment the data from the EP with selected variables from the Employment Statistics. The latter are administrative social security records of all workers who were employed in one of the establishments as of the 30th of June of a given year. We aggregate these worker-level data to the establishment level. As a consequence, we drop establishments from the EP that cannot be linked to the social security data. We also drop establishments where the reporting unit changed over time since we will exploit the panel dimension in our analysis and changes in the reporting unit might be associated with spurious changes in the bargaining regime and the control variables.

The resulting data set is very rich. For our analysis at hand, information regarding the collective bargaining regime of an establishment is most important, which is available from 1996 onwards (1995 for West Germany). It is surveyed every year in the EP and distinguishes between collective agreements at the industry level, the firm level, and no collective agreement. Other important establishment-level variables for our analysis are the industry affiliation of the plant at the three-digit level, the region of the workplace, establishment size (in terms of employees and sales), establishment age, its legal form, information on whether it has a works council, whether it is part of a larger group, its export behavior and its technological status as well as information about its workforce composition. Figure 3 displays the evolution of collective bargaining coverage in Germany. It becomes apparent that, from the mid 1990s onwards, there was a strong decline in collective bargaining coverage rates. Most of this development was due to a decrease of industry-wide agreements. While in 1996 about 49 percent of all establishments were covered by an industry-wide contract, this share declined to only 33 percent in 2008 (Panel (a)). The share of plants covered by firm-level contracts declined from about 10 to 3 percent over the same time period. Moreover, the decline in CBAs is not (primarily) driven by shifts in the sectoral employment share, but

also found within sectors. The manufacturing sector shows the strongest decline, where the share of plants covered by an industry-wide agreement declined from 54 percent to 32 percent over the period of analysis (Panel (b)). While the observed decline is partly driven by establishment turnover, with newly founded establishments being less likely to follow collective agreements than older (and exiting) ones (Card et al., 2013), to a great extent it reflects previously covered establishments leaving collective agreements. We will provide evidence on the latter when discussing our dependent variable.

We combine both data sets by harmonizing industry and product classifications and merging trade flows to the EP. We restrict our analysis to the years from 1996 (which is the first year we observe information on CBAs for all the plants in the panel) to the year 2008 (to ensure that our analysis is not confounded by the global economic crisis). Moreover, we focus on the manufacturing sector since this is where we have detailed trade information for. We end up with 1,563 (1996) to 2,926 (2008) plants each year in our unbalanced raw panel data.

Supplementary Data In our analysis, we normalize trade volumes with total employment. For the industry-level employment data, we rely on the Sample of Integrated Employment Biographies (SIAB), a two-percent random sample of administrative social security records. For our empirical analysis, we keep observations for the 30th of June of every year and aggregate the individual-level data to the three-digit industry level (and multiply the values by 50). To supplement our main analysis, we also make use of the 1995 input-output-table from the German Statistical Office. We exploit this source to distinguish final goods imports from overall imports. A final data source which we consult for a robustness exercise is the number of migrants from China and Eastern Europe per German district and year. Details on the construction of the latter two data sets can be found in the Appendix.

5 Empirical approach

For our empirical strategy, we closely follow previous work by Autor et al. (2013, 2014) and Dauth et al. (2014, 2016). In particular, we make use of the fact that the productivity rise in China as well as the fall of the Iron Curtain and the subsequent transformation of the former socialist countries happened quickly and to a large extent exogenously from the point of view of Germany. Moreover, this increase in trade exposure affected Germany and many other countries alike.

In order to investigate how the rise in trade exposure to China and Eastern Europe affected German plants in adjusting their collective bargaining regime, we relate changes in a plant's bargaining status to changes in import and export exposure to the East. We first discuss how we measure the exogenous increase in trade exposure, before we turn to the exact definition of changes in the collective bargaining status at the establishment level. We elaborate on the exact empirical specification at the end of this section.

Trade exposure to the East Our main measure of trade exposure is the change in import penetration for each three-digit industry j of the manufacturing sector between t

and $t + \Delta$, which is defined as:

$$\Delta IM_{jt}^{East} = \frac{\Delta IM_{jt}^{D \leftarrow East}}{E_{j,95}}, \tag{1}$$

where $\Delta IM_{jt}^{D \leftarrow East}$ corresponds to the total change in German imports from the East in industry j between t and $t + \Delta$. We normalize trade volumes with total employment in industry j in the pre-sample period, $E_{j,95}$. Our measure of import exposure, ΔIM_{jt}^{East}, therefore captures the per-capita change in imports for industry j between t and $t + \Delta$.[15] One concern regarding the use of (1) in our empirical analysis is that it might also capture domestic shocks that affect both firm-level outcomes and German import demand. To extract only the supply-driven component in (1), we instrument for German import exposure by using the change in imports of other high-income countries vis-a-vis the East, constructing the following variable:

$$\Delta IMO_{jt}^{East} = \frac{\Delta IM_{jt}^{Other \leftarrow East}}{E_{j,95}}, \tag{2}$$

where $\Delta IM_{jt}^{Other \leftarrow East}$ now corresponds to changes in total import flows of industry j from the East to other high-income countries between t and $t + \Delta$.[16] Moreover, in the denominator, we fix employment as of the year 1995 to address potential issues of reverse causality if employment reacted in anticipation of future trade exposure. Note that we construct both measures ((1) and (2)) also with respect to export exposure:

$$\Delta EX_{jt}^{East} = \frac{\Delta EX_{jt}^{D \rightarrow East}}{E_{j,95}}, \tag{3}$$

and

$$\Delta EXO_{jt}^{East} = \frac{\Delta EX_{jt}^{Other \rightarrow East}}{E_{j,95}}. \tag{4}$$

Changes in collective bargaining We measure the collective bargaining status of plant i at time t with a dummy variable, CB_{it}, that indicates whether the establishment recognizes an industry-wide collective agreement or not. We consider this information to be the most relevant for our analysis since most of the variation in collective bargaining coverage rates relates to industry-wide as opposed to firm-level agreements (see Figure 3).

[15] Table 8 in the Appendix lists the industries that experienced the largest increase in import penetration per worker.

[16] This instrumental variable approach has been developed by Autor et al. (2013) and applied to Germany by Dauth et al. (2014). We follow the latter and consider the following countries as instrument countries: Australia, New Zealand, Japan, Singapore, Canada, Sweden, Norway, and the UK. The underlying assumption is that those countries are similarly affected by the rise of the East while industry demand shocks across Germany and those other high-income countries are largely uncorrelated.

Table 1. Changes in collective bargaining coverage at the establishment level (in %)

ΔCB_{it}	N	CBA exit -1	No change 0	CBA entry +1
1996–1999	797	18.23	76.37	5.41
1999–2002	1010	3.75	87.11	9.14
2002–2005	1763	8.13	87.51	4.36
2005–2008	1772	9.06	87.44	3.50

Source: IAB Establishment Panel, regression sample. Notes: Sampling weights are employed.

Furthermore, switching from an industry-level agreement to either a firm-level agreement or no collective agreement at all captures the idea of an increasing decentralization of the wage setting process, which we are after. As our dependent variable, we define the change in the collective bargaining status at the establishment level between t and $t + \Delta$, ΔCB_{it}. It can take the value 0 if establishment i has not changed its bargaining status, -1 if the plant has left an industry-wide agreement, and $+1$ if the establishment has joined such an agreement between t and $t + \Delta$.[17, 18]

When choosing the interval length Δ, we aim to find the optimal balance between two different objectives. On the one hand, Δ should not be too small since we have to take into account that establishments might react to a change in economic conditions only with a certain time lag. Moreover, we need to consider that establishments that are willing to leave a collective agreement are still bound by the latter until it expires. Thus, the probability of observing a change in the plant's bargaining regime increases with the length of Δ. On the other hand, Δ should not be too large since panel attrition and the ensuing reduction in sample size as well as sample selectivity would increasingly compromise the analysis. For example, if we chose $\Delta = 12$ (from 1996 to 2008), we would only observe 211 plants in our sample, preventing a reasonable analysis. Taking this trade-off into account, we opt for three-year windows. Since our analysis covers the years 1996 to 2008, we end up with four stacked three-year differences: 1996–99, 1999–02, 2002–05, and 2005–08. Tables 1 and 2 show descriptive statistics of the dependent variable as well as the trade measures for the respective time periods.

As can be seen, we observe between 3.8 and 18.2 percent of establishments leaving industry-wide collective bargaining agreements each period. At the same time, between 3.5 and 9.1 percent of plants are observed to enter CBAs. Although these numbers refer to our restricted sample of establishments that can be observed at the beginning and the end

[17] Since we want to capture permanent changes in a plants' bargaining status, we impute the CBA variables, when an establishment reports a different status only for one year (and switches back to the previous status directly in the next period). In such a case, we ignore the switch and adjust the CBA variable. We provide details on this imputation in the Appendix. Generally, our results are not sensitive to this adjustment.

[18] An alternative strategy could have been to measure the dependent variable not at the plant, but at the industry level. However, at the level of disaggregate industries, cell sizes would become too small and representativeness can no longer be ensured. As the Research Data Center points out (see Fischer et al., 2008, p.21), representative aggregate figures can only be obtained at the aggregation level of the variables that define the sampling strata. Since the strata industry dimension is quite aggregated – in 1996 there are only 16 strata industries and only 3 of them are in the manufacturing sector –, a meaningful industry-level analysis is not feasible. It is however possible to conduct a valid establishment-level analysis since one can explicitly control for the strata-defining variables, and thereby for the sampling probability, in the regression. See Fischer et al. (2008) for a detailed discussion.

Table 2. Changes in import and export exposure

	N est.	N ind.	mean	sd
1996–1999				
Change in imports	797	80	2.37	5.26
Change in imports, instrumented	797	80	4.03	10.35
Change in exports	797	80	0.89	2.53
Change in exports, instrumented	797	80	1.12	4.63
1999–2002				
Change in imports	1010	84	2.57	6.50
Change in imports, instrumented	1010	84	7.53	17.26
Change in exports	1010	84	1.81	2.81
Change in exports, instrumented	1010	84	1.76	5.12
2002–2005				
Change in imports	1763	87	1.85	4.35
Change in imports, instrumented	1763	87	4.96	13.48
Change in exports	1763	87	2.25	2.85
Change in exports, instrumented	1763	87	2.75	6.37
2005–2008				
Change in imports	1772	87	3.84	11.86
Change in imports, instrumented	1772	87	4.08	12.38
Change in exports	1772	87	5.21	5.58
Change in exports, instrumented	1772	87	2.36	6.50

Notes: Trade exposure measured according to equations (1) to (4), combining trade data from the BACI Database and employment data from the Sample of Integrated Employment Biographies (SIAB). The table shows the values for our regression sample, measured in 1000 (year-2000) euros per worker. Sampling weights are employed. N est. gives the number of establishments and N ind. the number of distinct 3-digit manufacturing industries.

of the respective intervals, the net change matches quite well the overall decline in CBAs that we observe in the entire sample (see Figure 2, Panel (b)). This holds true both for the entire period and the different intervals, with the exception of the interval 1999–2002 when the population average coverage rate stayed fairly flat while we observe net entry into industry-wide agreements in our regression sample. Recall that discrepancies between the total decline and the one observed in our regression sample mainly arise because we only capture within-establishment changes and not the ones arising from establishment turnover.

With respect to trade exposure, it becomes apparent that we see an increasing growth in the average export penetration over time in our regression sample. Starting with a change in exports per capita of about 890 euros between 1996 and 1999, it rises to more than 5,000 euro in the period from 2005 to 2008. In contrast, changes in import exposure are, on average, more stable over time, fluctuating between 1,850 euros per worker and 3,840 euros per worker across the 3-year intervals. While the average change in imports per capita exceeds the average change in exports per capita in the two earlier intervals, this trend reverses in the latter two intervals. Note that these average changes mask substantial heterogeneity across narrow industries, which we exploit in our empirical analysis.

Empirical specification To analyze the effect of the rise in trade exposure on establishment-level responses with respect to their bargaining regime, we relate the dependent variable,

ΔCB_{it}, at the establishment level to changes in our import and export measures, ΔIM_{jt}^{East} and ΔEX_{jt}^{East}, at the industry level. More specifically, we estimate variants of the following regression model:

$$\Delta CB_{it} = \beta_0 + \beta_1 \Delta IM_{jt}^{East} + \beta_2 \Delta EX_{jt}^{East} + \beta_3 X_{it} + \delta_r + \gamma_k + \mu_t + \epsilon_{it}, \tag{5}$$

where i denotes the firm, j the three-digit industry, and t the time period. We control for a rich set of start-of-period establishment-level characteristics, X_{it}, that have been identified to be relevant for the choice of the bargaining regime (Kohaut and Schnabel, 2003 and Schnabel et al., 2006). These are: establishment size (10 categories) and age (dummy indicating whether the establishment is younger than 6 years), information on the existence of a works council and the plant's legal form, information on whether the plant is a single-unit plant, whether it engages in exporting, information on its technological status (dummy indicating whether the plant reports to have a technological status that is above the industry average), the share of low-educated workers and the share of employees with fixed-term contracts. We further include the start-of-period collective bargaining status to account for the fact that, by construction, initially covered plants can only remain covered or exit (but not enter), while initially uncovered plants can only remain uncovered or enter (but not exit). We report descriptive statistics of plant-level control variables in Table 9 in the Appendix. Moreover, we add a full set of time dummies (μ_t) as well as regional (federal state, δ_r) and two-digit industry dummies (γ_k). Given that we consider a specification in first differences, these dummies capture region-specific and industry-specific time trends, respectively. The main coefficients of interest are β_1 and β_2. Estimated by OLS, they would capture the causal effect of rising trade exposure on plant-specific changes in the bargaining regime only if ΔIM_{jt}^{East} and ΔEX_{jt}^{East} were fully exogenous. Since this is unlikely, we instrument these measures as outlined above. Note that the direction of the endogeneity bias is a priori unclear. If a negative cost shock (e.g. due to past bargaining "mistakes"), leads to both increased opting-out of collective bargaining as well as an increase in imports, OLS estimation would lead to a downward bias in the import exposure coefficient. In contrast, a positive productivity or demand shock that leads to both more imports as well as less opting-out, would lead to an upward bias of the OLS estimate. For statistical inference, we cluster standard errors at the three-digit industry level to account for serial correlation of the error term within industries, both in the cross-section and over time.

6 Results

We now report regression results pertaining to different variants of regression equation (5). We start with our baseline results, then explore heterogeneous effects along different dimensions, before subjecting our results to a series of robustness checks.

Baseline results Columns (1) and (2) of Table 3 report the OLS results of specification (5), where the former only has the baseline control variables – the strata-defining sets of dummy variables for region, industry, and establishment size class as well as year dummies and a dummy for initial collective bargaining coverage – and the latter adds the full

set of establishment control variables. It can be seen that a rise in import competition from the East is associated with a significant increase in establishments' probability of leaving industry-wide bargaining agreements. In contrast, an increase in export exposure is not correlated with a plant's change of the collective bargaining regime. With respect to the included control variables, the results suggest that single-unit establishments are more likely to leave centralized wage agreements. Perhaps surprisingly, exporting plants are found to be more likely to exit from collective agreements, too. This is in line with previous evidence by Capuano et al. (2014) according to which exporting establishments are less likely to be covered by collective bargaining conditional on plant size. A (speculative) explanation that we already alluded to earlier could be that the threat of relocating parts of the production abroad is arguably more credible for exporting than for purely domestic plants, given that the former are already globally connected. On the other hand, the results show that plants with a works council and those that are sole proprietorships or partnerships (as opposed to belonging to corporations) are less likely to opt out of centralized bargaining regimes. With respect to workforce composition, we do not find a significant relationship between the share of low-skilled and fixed-term workers, respectively, and the probability of changing the bargaining regime. Finally, we find – as expected – that establishment size is negatively related to the probability of leaving industry-wide collective agreements.[19]

We now turn to the results of the IV estimation (cf. columns (3) and (4)). The key results of the first-stage regression are provided at the bottom of Table 3 and indicate that the instrument for our import measure is strong, with an F-test statistic above the common rule of thumb of ten. This does not hold true for the instrument of export exposure, which shows a first-stage F-test statistic of only 7.34 and 7.44, respectively.[20, 21] We therefore need to exert some caution when interpreting the results pertaining to exporting. Turning to our second-stage results, the import coefficient in columns (3) and (4) is again negative and statistically significant. It is even larger in absolute terms than the corresponding OLS estimate. This suggests that the OLS coefficient is rather (upward) biased towards zero, which could either be due to measurement error or to unobserved demand shocks that cause both increased imports as well as less pressure to leave industry-wide bargaining agreements. Note that we still do not find any effect related to the export channel, the point estimate actually being zero. In terms of economic magnitude, our IV estimate implies that an increase in import penetration from the East by 1,000 euros per worker increases

[19] We include dummies for 10 establishment size groups. Results are not shown for the sake of space, but they are available upon request.

[20] We report Sanderson-Windmeijer F-tests for multiple endogenous variables (Sanderson and Windmeijer, 2016), which are constructed along the lines of the suggestion by Angrist and Pischke (2009).

[21] In general, our first-stage F statistics are lower than the ones of other papers making use of similar types of instruments (e.g. Dauth et al., 2014). A likely reason is that we use a fairly demanding specification for identification, where we restrict attention to the manufacturing sector only and exploit variation across three-digit industries (in the cross section and over time) while controlling for two-digit industry fixed effects. For comparison, Autor et al. (2013) and Dauth et al. (2014) only include dummies for four higher-order regions in their (baseline) local labor market-level regressions. Indeed, if we drop the two-digit industry fixed effects, the first-stage F statistics increase substantially, while the point estimates in the second stage are very similar (but more precisely estimated). We nevertheless prefer the specification with two-digit industry fixed effects for two main reasons. First, it allows us to control for (possibly confounding) trends across broader industry groups. Second, and as already discussed, since the industry (even though at a more aggregate level) is one dimension that defines the sampling strata, we thereby account for the sampling probability of establishments.

Table 3. Effects of trade exposure on changes in CBAs: baseline results

Dependent variable: Change in industry-wide collective bargaining status at the plant level
ΔCB_{it} takes the value -1 for plants leaving collective agreements

	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS	(5) 2SLS	(6) 2SLS
Import exposure	−0.0017***	−0.0018***	−0.0031**	−0.0031**	−0.0032*	
	(0.0006)	(0.0006)	(0.0015)	(0.0015)	(0.0016)	
Export exposure	−0.0006	−0.0007	−0.0002	−0.0000		
	(0.0008)	(0.0008)	(0.0019)	(0.0018)		
Net import exposure						−0.0026**
						(0.0011)
Collective agreement (0/1)	−0.2833***	−0.3017***	−0.2835***	−0.3021***	−0.3021***	−0.3022***
	(0.0165)	(0.0158)	(0.0163)	(0.0156)	(0.0156)	(0.0156)
Works council (0/1)		0.0764***		0.0770***	0.0770***	0.0767***
		(0.0141)		(0.0140)	(0.0140)	(0.0139)
Young plant (0/1)		−0.0135		−0.0141	−0.0141	−0.0144
		(0.0152)		(0.0151)	(0.0151)	(0.0150)
Plant age: missing (0/1)		0.0108		0.0108	0.0108	0.0109
		(0.0212)		(0.0209)	(0.0209)	(0.0210)
Single plant (0/1)		−0.0502***		−0.0502***	−0.0502***	−0.0496***
		(0.0140)		(0.0139)	(0.0139)	(0.0139)
Sole proprietor / partnership (0/1)		0.0222*		0.0215*	0.0215*	0.0222*
		(0.0116)		(0.0114)	(0.0114)	(0.0114)
Exporter (0/1)		−0.0178**		−0.0177**	−0.0177**	−0.0176**
		(0.0090)		(0.0089)	(0.0089)	(0.0088)
Technology above average (0/1)		0.0043		0.0041	0.0041	0.0040
		(0.0113)		(0.0112)	(0.0112)	(0.0112)
Share of low-skilled workers		−0.0173		−0.0167	−0.0167	−0.0168
		(0.0165)		(0.0163)	(0.0163)	(0.0163)
Share of fixed-term workers		0.0023		0.0022	0.0022	0.0026
		(0.0482)		(0.0481)	(0.0481)	(0.0483)
N	5342	5342	5342	5342	5342	5342
R2	0.15	0.16	0.15	0.16	0.16	0.16
First stage, import exposure						
Import (other countries)			0.2841***	0.2839***	0.2417***	
			(0.0604)	(0.0603)	(0.0696)	
Export (other countries)			−0.1656***	−0.1653***		
			(0.0602)	(0.0599)		
Net import (other countries)						0.2992***
						(0.0385)
Sanderson-Windmeijer F-test			18.23	18.25	12.05	60.54
First stage, export exposure						
Import (other countries)			−0.0122	−0.0124		
			(0.0243)	(0.0242)		
Export (other countries)			0.1410***	0.1414***		
			(0.0435)	(0.0433)		
Sanderson-Windmeijer F-test			7.34	7.44		

Notes: Regressions are based on four stacked 3-year windows from 1996 to 2008. Establishment controls are measured at the start of each window. All regressions include a constant and dummy variables for 10 plant-size groups, the region (federal state) of the workplace, the 2-digit industry, and the time period. First-stage regressions include the same set of control variables as the corresponding second stage. Standard errors (clustered at the 3-digit industry level) are given in parentheses. *p<0.10, **p<0.05, ***p<0.01.

Table 4. Heterogeneous effects: plant size

Dependent variable: Change in industry-wide collective bargaining status at the plant level ΔCB_{it} takes the value -1 for plants leaving collective agreements			
	(1) Small	(2) Medium	(3) Large
Import exposure	-0.0034*	-0.0043*	-0.0024
	(0.0018)	(0.0025)	(0.0019)
Export exposure	-0.0034	-0.0017	0.0070*
	(0.0041)	(0.0036)	(0.0041)
Establishment controls	YES	YES	YES
Estimation method	2SLS	2SLS	2SLS
N	2571	1275	1496
R2	0.20	0.18	0.12
First-stage Sanderson-Windmeijer F-test			
Imports	6.85	9.51	38.71
Exports	5.44	7.01	8.27

Notes: Plant size groups are defined as follows: small: < 50 workers; medium: ≥ 50 and < 200 workers; large: ≥ 200 workers. All regressions include a constant, a full set of establishment-level control variables as in Table 3 and complete sets of dummy variables for 10 plant-size groups, the region (federal state) of the workplace, the 2-digit industry, and the time period. First-stage regressions include the same set of control variables as the corresponding second stage. Standard errors (clustered at the 3-digit industry level) are given in parentheses. *p<0.10, **p<0.05, ***p<0.01.

(decreases) a plants' probability of leaving (joining) industry-wide bargaining agreements by 0.31 percentage points. A back-of-the-envelope calculation therefore suggests that a cumulative increase in import exposure of 13,300 euros per worker, which corresponds to the manufacturing-wide increase over the entire period, yields a change in collective bargaining coverage of 4.13 percentage points. Put into perspective, this amounts to 19 percent of the observed total decline in industry-wide collective bargaining coverage in the manufacturing sector from 1996 to 2008 (cf. Figure 3).

One concern might be that the estimation results for importing and exporting could suffer from a high collinearity between these two variables. However, we obtain very similar results to our baseline specification if we consider only imports or net imports as our instrumented trade shock variables. The respective results are shown in columns (5) and (6) of Table 3.

Heterogeneous effects We now turn to heterogeneous effects and analyze which types of establishments react strongest – in terms of changing their bargaining regime – to the increase in trade exposure. Following our reasoning in Section 3, we expect the import effect to be strongest for smaller, less productive establishments. The mechanism we have in mind is that increased import competition puts a particular pressure on the least competitive plants in an industry. Thus, for them, the wage floor imposed by centralized collective bargaining agreements should become increasingly binding, leading to growing exit rates. Likewise, we expect their employees to be more likely to accept an opting-out decision if these establishments face the credible threat of going bankrupt or the need to downsize production.

Unfortunately, obtaining proper measures of productivity is difficult with the data at

hand. First of all, information on establishment sales is missing for a non-negligible – and probably non-random – fraction of establishments. Second, information on the capital stock is not available. Although imputation procedures have been proposed in the literature, they would certainly give rise to measurement error. Therefore, we focus on heterogeneous effects by establishment size. Note that heterogeneous-firm trade theory (Melitz, 2003) postulates that firm size (in terms of revenues or employment) is a power function of firm productivity.[22] We estimate the regression model (5) separately for three different plant-size groups: small (< 50 workers), medium-sized (≥ 50 and < 200 workers), and large (> 200 workers).

Results are shown in Table 4. Consistent with our prior expectations, we find that in particular small (< 50 workers) and medium-sized (≥ 50 and < 200 workers) plants are likely to leave an industry-wide agreement in response to an increase in low-wage import competition. For large establishments, this effect is smaller in magnitude and not significant. At the same time, we find a positive and statistically significant effect of a change in export exposure for large establishments only, suggesting that an increase in export opportunities makes large plants less likely to opt out of a collective agreement. This is in line with the notion that in particular large and highly productive plants benefit from increased export opportunities. This makes it less likely that they leave centralized collective agreements. We interpret these heterogeneous effects by establishment size as supportive of the notion that we are indeed capturing effects of final goods trade as opposed to offshoring (or threats to relocate production). In the latter case, we would expect in particular larger establishments to leave centralized wage bargaining, as these are the ones that can credibly threat to relocate production.[23] That is, however, not what we find in the data. Importantly, this does not mean that the offshoring channel is not important or does not exist, but it does not seem to be what we pick up with our import exposure variable.

With respect to further heterogeneous effects at the plant level, one might expect that the low-skill intensity of the workforce matters for the impact of increased trade exposure on the probability of changing the bargaining regime. There are two reasons for this conjecture. First, low-skill-intensive establishments might be the ones that suffer the most from increased competition from low-wage countries. Second, collective agreements are generally more binding for low-skilled than for high-skilled workers (see Dustmann and Schönberg, 2009), which would increase the incentives of plants that employ a relatively large share of low-skilled workers to leave collective agreements. To shed light on this dimension of heterogeneity, we split the sample in two groups at the median low-skill intensity (using the ratio of low-skilled workers relative to high skilled workers) and repeat the analysis for both groups. Table 5 displays the results. Different to our presumption, however, we find that the two respective groups of plants are affected very similarly, showing almost identical point estimates and the same level of statistical significance.

One possible explanation could be that, while plants with a high share of low-skilled

[22] As an alternative, we have also constructed a mark-up measure as sales over total costs, assuming that more productive establishments have larger mark-ups (e.g. Melitz and Ottaviano, 2008). Although sample size is somewhat reduced due to the aforementioned missing values in the sales variable, results are similar to the ones relying on establishment size. Results are available upon request.

[23] There is strong evidence that the most productive and largest firms within an industry self-select into offshoring (as summarized in, e.g., Egger et al., 2015a).

Table 5. Heterogeneous effects: low-skill intensity

Dependent variable: Change in industry-wide collective bargaining status at the plant level ΔCB_{it} takes the value -1 for plants leaving collective agreements		
	(1) Low-skill intensity below median	(2) Low-skill intensity above median
Import exposure	-0.0035*	-0.0033*
	(0.0021)	(0.0018)
Export exposure	-0.0001	-0.0002
	(0.0031)	(0.0023)
Establishment controls	YES	YES
Estimation method	2SLS	2SLS
N	2743	2599
R2	0.16	0.18
First-stage Sanderson-Windmeijer F-test		
Imports	7.29	29.71
Exports	5.12	9.76

Notes: Low-skill intensity is defined as ratio of low-skilled workers to high-skilled workers. All regressions include a constant, a full set of establishment-level control variables as in Table 3 and complete sets of dummy variables for 10 plant-size groups, the region (federal state) of the workplace, the 2-digit industry, and the time period. First-stage regressions include the same set of control variables as the corresponding second stage. Standard errors (clustered at the 3-digit industry level) are given in parentheses. *p<0.10, **p<0.05, ***p<0.01.

employees might be more strongly affected by low-wage country competition, they might also be confronted with a stronger opposition from their workforce if they want to leave centralized bargaining, as collective wages are binding for a larger fraction of employees.

Robustness checks In the following, we run several robustness checks, which are summarized in Table 6. First, we restrict our analysis to single-unit plants only (shown in column (1)), thereby taking into account that decisions on collective agreements are usually formed at the firm as opposed to the plant level. The estimated effect of an increase in import penetration on firms' propensity to opt out of industry-wide collective agreement is still positive and significant and even slightly larger in absolute terms than in our baseline specification with the full sample. This suggests that our establishment-level analysis approximates the decisions taken at the firm level reasonably well.

Next, we focus on the risk set of establishments that could potentially leave collective bargaining and restrict our sample to plants that have been covered by a industry-wide collective agreement at the beginning of each time window. In this specification our dependent variable, ΔCB_{it}, can therefore only take the values -1 for CBA leavers and 0 for CBA stayers. Moreover, the number of observations reduces significantly (from 5,342 in the baseline to 2,569 in this specification). Column (2) shows the results. We estimate roughly similar coefficients as in our baseline specification, albeit with less statistical precision. We take this as supporting evidence for the validity of our symmetry assumption which is implicit in our baseline estimation, i.e. the assumption that the explanatory variables have a symmetric effect on the probabilities of leaving and joining, respectively, industry-wide bargaining agreements. Comfortingly, we also obtain qualitatively the same results regarding

Table 6. Robustness checks I

	(1) Single-unit plants	(2) Initially covered plants	(3) Final goods only	(4) Panel survival
Dependent variable:	Change in industry-wide CBA			Panel survival
Import exposure	-0.0037**	-0.0035	-0.0032*	0.0009
	(0.0016)	(0.0022)	(0.0017)	(0.0009)
Export exposure	-0.0011	0.0030	-0.0001	0.0015
	(0.0020)	(0.0039)	(0.0017)	(0.0044)
Establishment controls	YES	YES	YES	YES
Estimation method	2SLS	2SLS	2SLS	2SLS
N	3899	2569	5342	9227
R2	0.17	0.11	0.16	0.03
First-stage Sanderson-Windmeijer F-test				
Imports	17.55	43.52	10.00	17.92
Exports	9.98	7.92	6.27	10.04

Notes: All regressions include a constant, a full set of establishment-level control variables as in Table 3 and complete sets of dummy variables for 10 plant-size groups, the region (federal state) of the workplace, the 2-digit industry, and the time period. First-stage regressions include the same set of control variables as the corresponding second stage. Standard errors (clustered at the 3-digit industry level) are given in parentheses. *p<0.10, **p<0.05, ***p<0.01.

the heterogeneous effects by establishment size when restricting the sample to single-unit plants and to establishments initially covered by a collective agreements, respectively (see Table 10 in the Appendix).

As a further robustness check, we restrict attention to final goods imports. One might think that imported intermediates have a different effect on German firms, potentially leading to lower marginal costs and less pressure to leave collective bargaining. To distinguish final goods imports from intermediate goods imports, we rely on the German input-output table from the pre-sample period 1995. Specifically, we calculate the share of imports originating from a given two-digit industry abroad that is not used as an input in any (other) two-digit industry (similar to Autor et al., 2013 and Dauth et al., 2014). We apply this share to all three-digit industries within a given two-digit industry. The instruments are adjusted accordingly. As this transformation is less straightforward for our measure of export exposure, we leave the latter unchanged. Restricting attention to final goods imports hardly affects the regression results (column (3)).

Given our regression set-up, one further concern could be related to panel attrition. Although the average response rate of the survey is quite high (about 80 percent), we still lose establishments over our three-year windows. Panel attrition could lead to biased estimates if it is not random but correlated with our main variables of interest. In particular, we would expect to underestimate our main effect (the β_1 coefficient) if an increase in import penetration is positively related to panel attrition, implying that such an increase induces some (low-productivity) establishments not only to exit a collective agreement but also to disappear from the panel. We analyze this possibility by regressing a dummy variable of plant survival (which takes the value 1 if a plant observed in t is still in the panel in

Table 7. Robustness checks II: migration

Dependent variable: Change in industry-wide collective bargaining status at the plant level ΔCB_{it} takes the value -1 for plants leaving collective agreements				
	(1) Baseline	(2) Excluding border districts	(3) Controlling for migration	(4) Controlling for migration
Import exposure	-0.0031**	-0.0030*	-0.0032**	-0.0032**
	(0.0015)	(0.0016)	(0.0015)	(0.0015)
Export exposure	-0.0000	-0.0002	-0.0000	0.0000
	(0.0018)	(0.0018)	(0.0018)	(0.0018)
Level of migrants / total workforce			-0.4722	
			(0.6240)	
Change in level of migrants / total workforce				0.8737
				(0.9385)
Establishment controls	YES	YES	YES	YES
Estimation method	2SLS	2SLS	2SLS	2SLS
N	5342	4847	5342	5342
R2	0.16	0.16	0.16	0.16
First-stage Sanderson-Windmeijer F-test				
Imports	18.25	18.04	18.30	18.67
Exports	7.44	7.14	7.43	7.58

Notes: In Column (3) we account for the level of migrants relative to the total workforce at the district level. We consider migrants in the age of 18 to 65 from all countries that are considered as "the East" in our analysis. The total number of the workforce at the district level refers to the year 1995 and is held fix. In Column (4) we control for the change in the level of migrants over period Δ relative to the total number of workers at the district level. All regressions include a constant, a full set of establishment-level control variables as in Table 3 and complete sets of dummy variables for 10 plant-size groups, the region (federal state) of the workplace, the 2-digit industry, and the time period. First-stage regressions include the same set of control variables as the corresponding second stage. Standard errors (clustered at the 3-digit industry level) are given in parentheses. *p<0.10, **p<0.05, ***p<0.01.

$t + \Delta$ and 0 otherwise) on all covariates of our model.[24] Column (4) of Table 6 shows the results. As can be seen, both trade shock coefficients are close to zero and not statistically significant. We therefore conclude that panel attrition does not confound our analysis.

Our final robustness check relates to migration, which is one potentially confounding factor that we have not addressed so far. That is, not only did German trade exposure to the East grow substantially during our period of analysis, but the German labor market also attracted many migrants, especially from Central and Eastern Europe.[25] Arguably, this migration inflow could also have influenced German employers and employees to agree upon leaving collective wage regimes, given that the implied increase in labor supply might have affected the bargaining position of establishments vis-a-vis their incumbent workers.

Note, however, that this potential channel would only be a threat to our identification strategy if migration did not only influence the establishments' choice of the bargaining regime, but was also correlated with trade flows between the East and our instrument countries. One potential mechanism for this to be the case could be, for example, that workers from comparative-advantage sectors in the Eastern countries migrate to Germany

[24] Unfortunately, it is not feasible to run a true selection model since this would require a valid exclusion restriction, which is difficult to come by in the setting at hand.

[25] Although Germany restricted the freedom of movement of citizens from the new EU member states, a sizable number of migrants already arrived during the 2000s (see e.g. Haug, 2004 and Elsner and Zimmermann, 2013).

seeking employment in the same sectors or in those regions where these sectors predominate. In this scenario, trade flows from the East to other countries would go hand in hand with migration from the East to Germany. To rule this mechanism out, we consider the following robustness checks. First, we exclude all plants from our analysis that are located in a district at the eastern border of Germany (column (2)). Since migration is generally concentrated at regions which are relatively close to the home country (for this distance pattern of migration from Central and Eastern European countries to Germany, see e.g. Lehmann and Nagl, 2018), we thereby exclude those plants that are arguably most strongly affected by migration from the East. Second, we use the full sample, but include additional controls to account for the number of migrants from the East at the district level. Specifically, we either control for the start-of-period share of migrants from the East in total employment (column (3)) or for the change in the share of migrants in total employment over our three-year period (column (4)).[26] Note that we do not aim at a causal identification of the migration channel itself, which would require us to use proper instruments for the district-specific migration flows, but focus on how the inclusion of these variables affects the estimated trade exposure effects. The results, which are shown in Table 7, show that neither the exclusion of plants at the border nor the control for migrants at the district level affects our main results. We therefore conclude that the trade effect we capture in our analysis is not confounded by simultaneous migration flows to Germany.

7 Conclusion

In this paper we shed light on a hitherto unexplored dimension of establishments' adjustment to increased import competition. Using rich establishment-level data from Germany over the period from 1996 to 2008, we analyze how establishments change their collective bargaining status in response to stronger trade exposure. We show that establishments facing stronger import competition from China and Eastern Europe are more likely to leave industry-wide collective bargaining, thereby contributing to the increased decentralization of wage negotiations. We find that in particular small and medium-sized establishments have reacted to increased import competition in this way. In contrast, and as expected, we do not find any effect of stronger export exposure on establishments' likelihood of leaving industry-wide bargaining.

A back-of-the-envelope calculation suggests that about 19 percent of the entire decline in industry-wide collective bargaining coverage in the German manufacturing sector over the period of analysis from 1996 to 2008 can be explained by the increase in import competition from China and Eastern Europe.

Previous literature has shown that the decline in collective bargaining coverage matters for a wide range of economic and labor market outcomes. Among others, it has been identified as a major source of increasing wage dispersion. In this paper, we have identified an important driving factor of this decline.

At a more general level, the following conclusions can be drawn from our analysis. First, labor market institutions do not only shape comparative advantage and, hence, trade

[26] Details on how we construct the variables at the district level are provided in the Appendix.

patterns, but are themselves endogenous with respect to international trade. Second, the decline in collective bargaining seems to constitute one potentially important channel linking international trade and labor market outcomes – and one that is likely to have long-term consequences. Exploring in detail the link between international trade, the decline in collective bargaining, and labor market outcomes seems to be fruitful avenue for future research.

Also, it would be interesting to compare the German experience to other countries. Our interpretation of the results for Germany is that trade exposure has increased the dispersion of firm profitability within industries, which has made it increasingly difficult to form "one-size-fits-all" collective agreements at the industry level. While there is broad evidence on the differential impact of globalization across heterogeneous firms in terms of sales and profits in many countries (Melitz and Redding, 2014), the reaction pattern in terms of collective bargaining regimes will likely depend on the exact institutional set-up, i.e. on issues such as the predominant level of bargaining (firm, sectoral, or national) and the degree of flexibility of collective agreements.

From a policy perspective, our research gives rise to the question how to react to an increasing erosion of the system of industrial relations in face of rising globalization pressures. One might argue that in the German setting, both unions and policy makers have already reacted to this development: unions by increasingly allowing for wage flexibility within collective agreements, policy makers by introducing a statutory minimum wage to secure a wage floor previously largely upheld by collective agreements.

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Appendix

Input-output data

To differentiate between total imports and final goods imports, we make use of the German input-output table of the year 1995, which is provided by the German Statistical Office. Similar to Autor et al. (2013) and Dauth et al. (2014), we use information on the shares of world imports (by two-digit industry) that are used for consumption or investment rather than as an input by any industry. We multiply these shares with German imports from Eastern Europe and China, applying the same share to all three-digit industries in a given two-digit industry. We adjust the instruments accordingly.

Migration data

Information on the number of migrants at the district level is provided by the German Statistical Office. In particular, we make use of the Central Register of Foreigners. This data set contains information on the stock of migrants by year, nationality, age, and gender at the district level. It covers the period from 1998 to 2014. For the years 1996 and 1997 the number of migrants by nationality, age, and gender is only available at a more aggregate level of German regions. We therefore impute the number of migrants at the district level for these two years, assuming that the distribution of migrants across districts is the same as in 1998. Moreover, we adjust the data taking all regional reforms at the district level into account (Gebietsreformen).

In order to construct our migration control variables, we consider all migrants in the age group 18 to 65 from all countries that are considered "the East" in our analysis (China, Bulgaria, Czech Republic, Hungary, Poland, Romania, Slovakia, Slovenia, the Russian Federation, Belarus, Estonia, Latvia, Lithuania, Moldova, Ukraine, Azerbaijan, Georgia, Kazakhstan, Kyrgyzstan, Tajikistan, Turkmenistan, and Uzbekistan). We normalize the total number of migrants (and the change in the total number of migrants) by the total number of workers at the district level. We extract the latter information from the SIAB data set (Sample of Integrated Labor Market Biographies, provided by the IAB). The denominator is fixed as of the year 1995.

Imputation of collective bargaining status

To ensure that we capture permanent changes in the bargaining status and not only temporary or spurious ones (e.g. short-term exits that are only used to achieve a better bargaining position and which are therefore followed by a reentry in the next period), we explicitly check the data for such patterns and impute the CBA variable in these cases. In particular, we do so if a plant reports to change its bargaining status in two consecutive years, e.g. if a plant reports to be covered in period 0 and period 2 but to be uncovered in period 1 (of course, we equally consider changes in the opposite direction). In such a case, we ignore the first change and impute the CBA variable such that this particular plant is covered in

all three periods.[27] On the entire data set, the imputation affects between 1.04 and 2.65 percent of all observations per year. With respect to the regression windows of our baseline specification, less than 4 percent of changes (ΔCB_{it}) are affected by this procedure. We run our specifications also with the original, non-imputed, CBA variable. Our results are largely stable to this adjustment.

[27] As soon as a plant reports three changes in a row (which is very rarely observed), we do not impute, but only flag these observations and exclude these plants in a robustness check. The whole set of our results remains unaffected.

Table 8. Most affected industries by import exposure per worker

Industry		Change in trade volumes p.w.
177	Manufacture of knitted and crocheted articles	217.56
365	Manufacture of games and toys	177.94
300	Manufacture of office machinery and computers	140.01
323	Manufacture of television and radio receivers, sound or video recording or reproducing apparatus and associated goods	79.95
321	Manufacture of electronic valves and tubes and other electronic components	54.04
152	Processing and preserving of fish and fish products	50.81
274	Manufacture of basic precious and non-ferrous metals	48.06
322	Manufacture of television and radio transmitters and apparatus for line telephony and line telegraphy	42.65
351	Building and repairing of ships and boats	34.85
364	Manufacture of sports goods	34.56
343	Manufacture of parts and accessories for motor vehicles and their engines	34.42
174	Manufacture of made-up textile articles, except apparel	33.87
193	Manufacture of footwear	32.76
314	Manufacture of accumulators, primary cells and primary batteries	28.48
342	Manufacture of bodies (coachwork) for motor vehicles; manufacture of trailers and semitrailers	27.06
316	Manufacture of electrical equipment n.e.c.	27.01
312	Manufacture of electricity distribution and control apparatus	25.92
313	Manufacture of insulated wire and cable	25.19
154	Manufacture of vegetable and animal oils and fats	24.36
192	Manufacture of luggage, handbags and the like, saddlery and harness	22.39

Notes: Changes in import exposure per worker measured according to equation (1), combining trade data from the BACI Database and employment data from the Sample of Integrated Employment Biographies (SIAB). The table shows changes between 1996 and 2008, measured in 1000 (year-2000) euros per worker.

Table 9. Descriptive statistics: establishment-level control variables (pooled)

	N	mean	sd
Industry-wide collective agreement (0/1)	5342	0.463	0.499
Works council (0/1)	5342	0.119	0.324
Young plant (0/1)	5342	0.065	0.246
Plant age: missing (0/1)	5342	0.349	0.477
Single plant (0/1)	5342	0.917	0.276
Sole proprietorship or partnership (0/1)	5342	0.540	0.498
Exporter (0/1)	5342	0.226	0.419
Technology above average (0/1)	5342	0.159	0.366
Share of low-skilled workers	5342	0.306	0.309
Share of fixed-term workers	5342	0.015	0.053

Source: IAB Establishment Panel, regression sample. Notes: The descriptives are pooled across the four stacked 3-year windows from 1996 to 2008 and measured at the start of each window. Sampling weights are employed.

Table 10. Robustness checks: heterogeneous effects by plant size

Dependent variable: Change in industry-wide collective bargaining status at the plant level ΔCB_{it} takes the value -1 for plants leaving collective agreements						
	Single-unit plants only			Initially covered plants only		
	Plant size			Plant size		
	Small	Medium	Large	Small	Medium	Large
	(1)	(2)	(3)	(4)	(5)	(6)
Import exposure	-0.0035*	-0.0047	-0.0025	-0.0088*	-0.0058	-0.0024
	(0.0018)	(0.0028)	(0.0020)	(0.0052)	(0.0042)	(0.0017)
Export exposure	-0.0038	-0.0066	0.0135	-0.0022	-0.0009	0.0081
	(0.0043)	(0.0044)	(0.0095)	(0.0126)	(0.0056)	(0.0062)
Establishment controls	YES	YES	YES	YES	YES	YES
Estimation Method	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
N	2299	937	663	770	638	1161
R2	0.21	0.18	0.16	0.13	0.14	0.09
First stage Sanderson-Windmeijer F-test						
Imports	8.26	15.34	146.12	23.19	9.61	99.95
Exports	8.38	10.88	8.43	10.75	5.38	7.30

Notes: All regressions include a constant, a full set of start-of-period establishment-level control variables as in Table 3 and a complete set of time, region and 2-digit-industry dummies as well as indicators for 10 plant-size groups. First stage regressions include the same set of control variables as the corresponding second stage. Levels of significance: *p<0.10, **p<0.05, ***p<0.01. Standard errors (clustered at the 3-digit industry level) are given in parentheses.