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Not so far east? the impact of Central-Eastern European imports on the Brexit referendum

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ABSTRACT

We explore the effect of the UK's economic links with Central-Eastern Europe and China in the case of the Brexit referendum. First, replicate and independently verify the Colantone and Stanig findings (2018a) on the effect of globalization on Brexit. Then, we extend their original analysis and demonstrate that although both Chinese and Central-Eastern European imports were significant causal determinants of the referendum's outcome, exposure to Central-Eastern European imports was up to three times more important than exposure to Chinese imports. This may be due to cultural correlates of trade. Our analysis reveals that differences in media coverage between Central-Eastern European and Chinese economic news, as well as migrant population shares from these two regions, might have played a decisive role. Overall, the article also suggests that the import-shock method, as currently applied in the literature, is ill-suited to compare shocks with diverse origins.


KEYWORDS Brexit; euroscepticism; import shock; instrumental variables; trade

Introduction

Since the early 1970s, the world has witnessed an unprecedented wave of internationalisation and growth in the political, cultural, and commercial realms. Led by China and its unparalleled growth in exports, globalisation's primary economic characteristic was an explosion in global trade, especially between high- and low-income countries. In the European Union (EU), the expansion of market liberalisation was coupled with eastbound geographical expansion, which resulted in the rapid integration of many former Warsaw Pact countries.

While globalisation has undoubtedly brought material advantages to Western European countries, those gains have been perceived as being unevenly distributed; many European countries have experienced, since

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2012, a staggering rise in support for communitarian, antiglobalist, anti-European parties, culminating with the 2016 British referendum to leave the EU. In fact, concerns over national sovereignty are in the background of many explanations for the outcome of the Brexit referendum (Rudolph, 2020), from reclaiming control of borders to contain migration (Goodwin & Milazzo, 2017) to backlash against globalisation (Colantone & Stanig, 2018a) and deindustrialisation (Becker et al., 2017).

Yet, it is reasonable to expect that *if* deindustrialisation and global economic integration had some causal effect on voting on Brexit, *then* regional economic integration within the EU's single market (Colantone & Stanig, 2018a; Los et al., 2017) might have had an even larger impact. If so, an explanation must be provided as to whether voters meaningfully distinguish between sources of economic shocks.

To explore these effects, we replicate and independently verify the results by Colantone and Stanig (2018a), who had found a strong effect of Chinese imports on Brexit, and then extend their original analysis to assess the effect of Central-Eastern European (CEE) imports. After determining that the CEE shock appears larger than the Chinese shock, we explore whether voters' apparent capability to differentiate between these two sources of economic disruption is plausibly explained by mechanisms that are linked to trade but not economic in nature. To do so, we consider moderating effects of cultural factors, namely media salience and migrant population composition. Our results lend credibility to the intuition that the public is somewhat able to differentiate between sources of trade, even though we cannot rule out that the difference between import effects may be an artifact attributable to the method itself.

All in all, our results suggest that the CEE import effect appears to be larger than the effect attributable to imports from China. The cultural correlates of trade may be responsible for this differentiation. We show that differences in media coverage between CEE and Chinese economic news, as well as different demographics in the migrant population, may have played a decisive role. While the key contribution of this article is not methodological (and others have explored these limitations in shift-share instruments; Borusyak et al., 2018), we show that the import-shock approach is ill-suited to studying the chain of transmission from economic processes to labour market shocks to political decisions when the source of the labour market disruption is uncertain. When this is the case, identifying underlying mechanisms that may lead to differentiation – in this article, migration and news coverage – becomes paramount.

Trade and brexit: theoretical perspectives

As domestic manufacturing industries cannot compete on price with imports from low-income countries, import competition can lead to lower wages and

higher unemployment through various channels (Rigby et al., 2017). These negative labour market outcomes may trigger economic voting, which at its core suggests that incumbent politicians tend to benefit from a positive economic climate during their tenure but are also blamed for negative economic circumstances (Lewis-Beck & Stegmaier, 2019). Attitudes towards European integration have often been related to economic performance (see, for instance, Gabel & Palmer, 1995). More specifically, EU research has shown that negative economic outcomes can lead to political polarisation and a shift towards more populist, protectionist, and radical positions (Hernández & Kriesi, 2016). On the one hand, right-wing parties might have capitalised on a cultural rather than merely economic backlash to globalisation (Norris & Inglehart, 2019) since the reduction of external barriers increases society's cultural diversity. On the other hand, trade exposure fosters (right-wing) populist voting (Hays et al., 2019) since import competition harms the national labour market, which in turn fosters economic inequality (Barone & Kreuter, 2019).

Economic voting and referenda

While economic voting, tailored as it is to the dynamics of the electoral cycle, is not directly applicable in the context of major referenda on constitutional change, it can still be related to referenda depending on whether economic performance is attributed, fundamentally, to institutional/policy issues or to the economic performance of the powerholders (e.g., Clarke & Kornberg, 1994). In fact, Svensson (2002) and Franklin (2002) differentiate between referenda as a form of second-order elections and referenda focused instead on the contents of the matter at hand. Developing this reasoning further, we differentiate between referenda as *first order* or *second order* (Figure A1.1 in the Appendix). In the case of first-order referenda, economic voting suggests that poor (good) economic performance would lead to rejection of (support for) the institutional status quo. With second-order referenda, poor (good) economic performance would lead to the rejection of (support for) the policy position promoted by the parties in government. Three elements suggest that analysing the Brexit vote as a first-order referendum seems more appropriate. First, the issue of European integration has been highly salient and divisive in the British political debate for many years. The promise of a referendum dates back to at least 2007, when David Cameron pledged to hold a vote on the Lisbon Treaty that was being negotiated at the time. Second, the membership question was not aligned with party cleavages: both major parties in the UK featured sizeable groups supporting or opposing membership. Finally, previous research has shown that the Brexit referendum outcome can be interpreted as backlash against the institutional ties linking the UK to the EU rather than to the fate of the party in power. Hence, we study the Brexit referendum as a first-order referendum: we expect weak economic performance to

generate a backlash against the institutional status quo of globalisation, embodied by the UK membership in the EU.

Globalisation and voting behaviour in the 2016 Brexit referendum

Over the years, a rich body of research has been published examining citizens' voting decisions in the Brexit referendum. Explanations for the electoral decisions are manifold and range from (missing) European identity (Carl et al., 2019), higher media coverage for the 'out' campaign (Khabaz, 2018), and cultural divides within British society (Blick & Salter, 2020), to negative sentiments towards immigration (Goodwin & Milazzo, 2017).

In addition, the effect of economic factors on Brexit voting has been widely studied (Halikiopoulou & Vlandas, 2018). For instance, Los et al. (2017) show that greater trade integration with the EU was associated with a higher share of votes to leave. However, Becker et al. (2017) suggest instead that deindustrialisation, rather than trade, was correlated with the 'leave' outcome. While laudable, both fall short of a causal analysis of trade effects, first introduced by Colantone and Stanig (2018b). They adopt a quasiexperimental approach proposed by Autor et al. (2013) to study the effects of trade shocks on political behaviour, measuring the import exposure of different U.S. regions to China. This approach has since become widespread, applied to multiple countries (Dauth et al., 2014; Dippel et al., 2015), and further extended to explore the link to electoral behaviour (Caselli et al., 2019; Colantone & Stanig, 2018b; Malgouyres, 2017). In this context, Colantone and Stanig (2018a) examine the relationship between the share of leave votes in a given British region and that region's exposure to Chinese imports, finding that regions that were more exposed systematically had more leave votes in the referendum.

Overall, whereas some of these studies introduce economic or cultural phenomena as alternative explanations for the Brexit vote, others study their interplay (Carreras et al., 2019; Hobolt, 2016). Carreras et al. (2019) show that combining individual- and regional-level data on immigration and economic issues is a fruitful way to explain the causes of the Brexit vote. This article replicates and extends the mechanism introduced by Colantone and Stanig (2018a) beyond the Chinese effect and includes CEE countries in the analysis. Showing that the source of economic disruption needs to be correctly identified by looking beyond China, the economic nature of the vote in the Brexit referendum might be explained by how the trade shock interplays with cultural mechanisms, such as migration and news exposure.

Beyond the China effect

As noted, most research on import shocks uses Chinese imports as a proxy for economic globalisation. This is because China's unprecedented boom in

exports over the last 30 years was driven mostly by unexpected internal changes to the nature of the country's economy, which makes it a rare form of natural experiment. However, as Dauth et al. (2014) note, the fall of the Iron Curtain after 1989, and the subsequent integration of many CEE countries into the world economy, constitutes a similarly exogenous event for the EU. Many factors that make China's rise quasixogenous to developments in Western countries also apply to CEE countries: the fall of the Soviet Union was arguably as unexpected as the opening of the Chinese economy; many CEE countries eventually joined the EU, which decreased barriers to trade even more than did China's accession into the World Trade Organization (WTO); and finally, the isolation of many Eastern European countries led to a productivity gap that allowed for rapid catch-up growth similar to what China experienced after its isolation under Mao (Autor et al., 2016; Stephan, 2002).

These similarities may also have important implications for the study of the Brexit leave vote. For one, the economic effects of the accession of CEE countries and the ensuing import shock were potentially even more important to the outcome of Brexit than the Chinese shock (Dauth et al., 2014). Furthermore, EU membership has always been more than a mere calculus on costs and benefits of trade: voters may be more sensitive to European trade because they perceive that they pay a 'sovereignty' price for it. If so, then the politicisation potential inherent to labour displacement associated with imports from Eastern Europe should be high. Furthermore, voters may perceive Chinese competition as part of a global process they can hardly influence or stop. In contrast, the reversibility of EU integration opened spaces of contestation that made labour displacement induced by EU trade more politically salient than its Chinese counterpart.¹

In sum, while the effect of Chinese economic competition is prominently featured in Colantone and Stanig (2018a), it is reasonable to expect that intra-European trade would have affected voting behaviour to a similar, if not larger, extent than Chinese trade. By building a CEE import shock, and by comparing it with our own replication of Colantone and Stanig (2018a) Chinese import shock, we test this specific expectation:

H1: CEE imports had a significant causal effect on the share of leave votes in a given region in the 2016 Brexit referendum.

H2: The effect of CEE imports was greater than the effect of Chinese imports in determining the leave vote share.

Next, to make sense of these possible differences in direct effects, we consider moderating aspects which might have indirectly affected citizens' voting behaviour, leading voters to differentiate between trade pressure arising from CEE countries and pressure originating in China. Our intuition

is that differences in voters' sensitivity to the origin of their economic problems are possibly mediated by their direct experience (e.g., through the presence of a migrant population) and/or by the salience of the problem in the media. These effects would be in the direction reported by Carreras and colleagues, linking voters' economic and migration attitudes to Brexit vote outcomes (2019). While much of the econometric exploration of the Chinese effect is focused on trade, in the specific case of European integration trade is accompanied by a larger institutional and cultural footprint arising from the pooling of national sovereignty. If such pooling of sovereignty is seen as a cost (Rudolph, 2020), then this cost is higher for EU membership than for WTO membership. The inflow of migrants and labour from the new member states may have made the British electorate more sensitive to changes in the labour market associated with EU-related trade. While the direct effect of migration and labour competition on imports is relatively small, large migrant populations may have affected British voters' sensitivity to trade shocks originating in different parts of the world.

Furthermore, in the months preceding the Brexit vote, coverage of the political, cultural, and economic relationship bonding the UK to the EU, as well as coverage of EU trade and migration issues in the news, remained much higher than coverage of similar issues in relation to China from 2000 to 2016 (Appendix 2). Voters not only have been more sensitive to EU news due to the referendum campaign but also have been exposed to much more coverage of EU-related economic news than China-related news for a prolonged period. Relatedly, while many pro-Brexit politicians made a case for opening the UK to world trade, they invariably depicted trade and labour competition from Eastern Europe as unfair. Figure A1.2 in appendix summarises such logic.

While the lack of data prevents us from testing specific hypotheses regarding the mechanisms that would lead to differentiated propagation of import shocks from the labour market to voting behaviour, we can still provide an aggregate-level exploration of the potential effects of migration and the impact of news exposure. To this end, Section 4.2 compares the import shocks' interactions with news exposure and the composition of the migrant population.²

Data and methods

The construction of import shocks and instrumental variables

To capture the effects of trade on the domestic labour market, we used NUTS 3 data. Autor et al. (2013) suggest that the variation in import exposure between regions can be identified based on the different specialisations of local labour markets. Intuitively, those regions where employment is

heavily concentrated in manufacturing-based industries will be more exposed, given that lower-income countries hold a significant comparative advantage in manufacturing driven by structurally lower wages. Moreover, those regions that focus on a manufactured good for which the specialisation of the low-wage trading partner is particularly high will be more exposed than regions that focus on less competitive manufactured goods.

To test our main hypotheses, we select CEE countries that joined the EU to build a dedicated import-shock variable. To ensure comparability between the Chinese and CEE import shocks, we replicate Colantone and Stanig's (2018a) findings by creating our own version of the Chinese import-shock variable, used along with the original import-shock variable provided in their replication dataset. Hence, we add four new import-shock variables: (1) our independent replication of the Chinese exposure variable, (2) the associated instrument for the Chinese import shock, (3) a new import-shock variable measuring exposure to CEE imports, and (4) the associated instrument for the CEE import shock. Like Colantone and Stanig (2018a) we construct the variable to measure the exposure to CEE imports as follows:

$$import_shock_east_{it} = \sum_k \frac{L_{ik}}{L_i} * \frac{\Delta IMP\ east_{kt}}{L_k}, \quad (1)$$

where $\Delta IMP\ east_{kt}$ is the change in CEE imports in industry k at time t , L_k is the total (national) employment in industry k , L_{ik} is the employment in manufacturing-oriented industry k in region i , and L_i the total employment in region i . The import shock of a given region is then determined using the sum of individual exposures in each industry k over a period of five years. In practice, CEE imports are defined as the aggregate imports from the 2004 and 2007 EU enlargements.³ As in Colantone and Stanig (2018a) research, the final import-shock variable for each NUTS 3 region is then calculated as the average of the individual five-year import shocks.⁴ In the equation, an increase in imports in manufacturing industry k is first split equally over all UK workers in said industry. The regional impact of the shock is determined by the share of workers in a given region i who are employed in industry k $\left(\frac{L_{ik}}{L_i}\right)$. As Autor et al. (2013) intended, this means that the shock is larger for those regions where the proportion of manufacturing jobs is larger (so $\sum_k \frac{L_{ik}}{L_i}$ is larger), as well as those regions where the concentration of jobs in the industries that are experiencing heavy import competition from the exporting country is higher.

However, simply introducing the import shock in a regression model is insufficient because the model could suffer from endogeneity. To enable causal conclusions about the impact of economic integration (global and

European) on Brexit voting, the variable must capture only the exogenous effects that come with CEE integration into European trade. Endogeneity could occur via omitted variable bias if imports are correlated with political leanings (Colantone & Stanig, 2018a). Hence, to maintain the quasineutral experiment to measure the impact of globalisation, a correction is needed. As established in the literature, we circumvent these issues by constructing an instrument that is included as part of a two-stage least-squares regression and acts to isolate the exogenous effect of Chinese and CEE productivity increases. Equation 2a describes our instrumental variable:

$$instrument_east_{it} = \sum_k \frac{L_{ik}}{L_i} * \frac{\Delta IMP\ east_{other\ kt}}{L_k}. \quad (2)$$

The instrument is constructed in the same way as the *import_shock_east* variable but uses imports from CEE countries to a combination of other developed nations, instead of to the UK.⁵ Like the import-shock variable itself, the final instrument uses the average of 5 five-year changes in imports. For the reasons outlined above, we also replicate the Chinese import shock and the instruments used by Colantone and Stanig (2018a, appendix 3). However, while they use Chinese exports to the US as the basis for the instrument, we aim to ensure full comparability between the two import shocks; thus, the replication of their instrument is based on Chinese exports to the same six countries used for the CEE instrument.

Data

The dependent variable is the share of leave votes in the 2016 referendum on EU membership, at NUTS3 level. Our main independent variables are the following: the original Colantone and Stanig (2018a) import shock obtained from their replication dataset (*import shock CS*), our reconstruction of the Chinese import shock (*import shock China*), and our dedicated CEE import shock (*import shock east*). Appendix 2.1 discusses the construction of these two variables in greater detail. Since the import shock for a given region is partially determined by the overall degree of labour market specialisation in manufacturing industries and therefore constant for both the Chinese and CEE import shocks, we observe considerable overlap in the regional distribution of the two import shocks, which share an important component. The strength of the two import shocks especially varies in Southern England, Northeast England, and certain areas in Eastern England, due to different specialisations within the manufacturing industry and to different imports coming from China and CEE countries.

We explore the potential mechanisms explaining differences in effects between the Chinese and CEE import shocks by looking at differences in

news exposure and in the composition of the migrant population. Our rationale for including these variables, discussed in the previous section, is that if a differentiated impact is observable between the import shocks, this may be due to differences in voters' sensitivity to economic integration with the EU or China, which in turn may depend on voters' direct experience and news salience. The specific construction of these indicators is provided in Appendix 2.2.

In addition to the main independent variables, we include several control variables. These are sourced directly from the Colantone and Stanig (2018a) replication dataset. The first is a measure for the degree of fiscal cuts to the region, which is intended to control for the relationship between government spending and voting. The second control variable is the share of cancer patients treated over 62 days, which acts as a measure of the quality of service by the National Health Service; the third variable measures the growth in public employment in a given region. The fourth control variable measures the region's change in relative income versus the median region. As such, this variable is effectively a measure of the comparative economic performance of a region and should therefore block a large portion of the effect of the import shock.

Models

Using this dataset, we run a series of fixed-effects (NUTS 1) regression models. In general, we run each specification twice, comparing a model with (replicated) Chinese import shock with a model with the CEE import shock as the main independent variables. Like Colantone and Stanig (2018a), we include dummy variables for the specific NUTS 1 region to which the NUTS 3 regions belong. The underlying motivation for this decision is that there are certain unobserved region-specific characteristics that lead to a clustering of the import shocks on a NUTS 1 level.

We begin with a set of simple fixed-effects linear regressions with neither controls nor instruments (Table 5.1 in appendix). Specification A1 uses the original import-shock data from Colantone and Stanig (2018a) replication dataset. Specification A2 introduces our own version of the Chinese import-shock, while specification A3 introduces our CEE import shock. Accordingly, the independent variable 'shock' takes three forms, depending on which of the three shocks is used: the Colantone and Stanig (2018a) original China shock variable, our autonomous replication of the Chinese shock variable, or our new CEE shock. The base model is shown in Equation 3; the variable '*Shock*' in Equation 3 should be understood as any of the three. Our unit of observation is the NUTS 3 region (i); the baseline specification includes NUTS 1 fixed effects (r).

$$\text{Leave Share}_i = \beta_0 i + \beta_1 \text{Shock}_i + FE_{(i)r} \quad (3)$$

Our baseline estimates are reported in Table 1. Models B1 and B2 are IV estimations of Equation 3, where the shocks are instrumented as described in

Section ‘The Construction of Import Shocks and Instrumental Variables’. This IV-only regression allows us to assess the strength of the instruments and potential endogeneity concerns. We then extend the analysis by adding extra controls (Models B3 and B4), which are versions of B1 and B2 controlling for migration.

Finally, we run a series of IV estimations with a full set of controls, reported in Table 5.3 in the Appendix: Models C1 and C2 add controls for the quality of public services, the growth of public employment, and the size of fiscal cuts. Models C3 and C4 (Table 3) are fixed-effects regressions that include only the change in relative income versus the median regional income as a control variable, and Models C5–C8 test, respectively, the interaction effects of the two import shocks with migration composition and news exposure. The table with the estimates is reported in Appendix 5.3, while [Figures 1 and 2](#) graphically summarise the results.

Results and discussion

Main results

[Table 1](#) reports our baseline IV models. We first verified that our independent replication of Colantone and Stanig (2018a) is robust (see also the discussion in Appendix 5 and the associated table).

Table 1. Determinants of Leave Share in NUTS 3 region, IV estimations.

Models	(B1)	(B2)	(B3)	(B4)
Independent variables				
Instrumented Chinese shock (**)	10.329*** [3.985]		9.676*** [3.503]	
Instrumented CEE shock (**)		35.177*** [10.907]		33.457*** [7.827]
Share of migrant population (% of total population)			-0.468*** [0.150]	-0.485*** [0.151]
New migrant arrivals (% of total working-age population)			-0.152 [0.659]	-0.082 [0.650]
Constant	54.438*** [2.371]	51.892*** [2.592]	57.274*** [2.218]	54.831*** [2.035]
Fixed Effects (NUTS1)	Yes	Yes	Yes	Yes
IV	Yes	Yes	Yes	Yes
Observations	167	167	167	167
R-squared	0.58	0.586	0.651	0.659
Endogeneity Chi-sq.	0.048	0.688	-	-
Rkf	777.9	585.4	728.8	616.5

*Units of the independent variables given in brackets. Robust standard errors (clustered at NUTS 2 level) in brackets; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

**Import shocks are measured as import shocks units, computed as indicated in Equation 1a. Roughly, they can be interpreted as ‘dollars of import exposure per worker’.

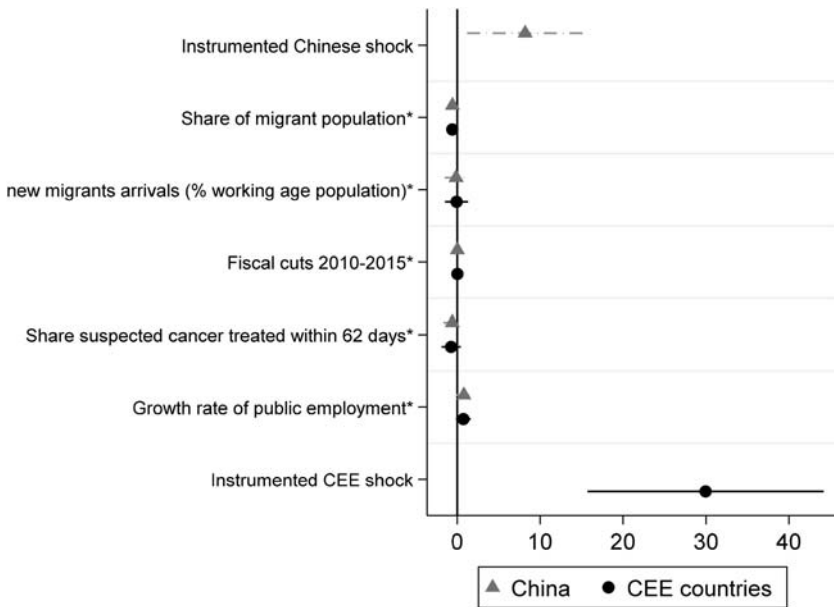


Figure 1. Comparing Effects.

Note: Fixed effects omitted. This figure is the result of two separate IV estimates plotted on a single figure. Estimates can be found in Table 5.3 of the online appendix, Models C1 and C2. Figure A6 in the Appendix provides equivalent results for a model where both variables are jointly instrumented in the same model. Control variables marked with a * are included to allow comparison with Colantone and Stanig (2018a), and are sourced from their replication dataset.

Overall, the results indicate that there was a large difference in the impact of the two shocks on Brexit (Models B1 and B2). Within a given NUTS 1 region, a one standard deviation increase in exposure to imports from China per worker (approximately 199 dollars of exposure per worker), is predicted to raise the share of leave votes by 2.05%.⁶ Moreover, a one standard deviation increase in import exposure per worker from CEE countries (only approximately 87 dollars) is predicted to raise the share of leave votes by 3.05%.⁷ The difference is clearer if we examine the effect on an absolute basis: An additional 100 dollars in import exposure per worker increased the predicted share of leave votes by about 1% for Chinese imports and about 3.5% for CEE imports. Appendix A7 illustrates this difference further, replicating Colantone and Stanig (2018a) thought experiment on Wales. This remained consistent across all specifications. Including migration as a control (Specifications B3 and B4 in Table 1) had little impact on the effects of either import shock.

Specifications C1 and C2 (Table 5.3 in Appendix) show that the effects of both the Chinese and CEE import shocks are robust and statistically significant even when a variety of control variables are included in the regression. Figure 1 compares Models C1 and C2 graphically.

Similarly, in Models C3 and C4 we include changes in real income since Colantone and Stanig (2018a) show that these are effectively a measure of the relative economic performance of a region. As one would expect, both coefficients do decrease but remain statistically significant.

First, we expected CEE imports to have a significant effect on the share of leave votes. Second, we expected CEE imports to have a larger effect than Chinese imports. Overall, the results corroborate both hypotheses. The CEE import shock effect is statistically significant throughout all models, even in Specification C4, where the inclusion of a control for relative income change should have blocked some of the effect. However, this blocking effect was larger for the Chinese import shock than for the CEE one. This outcome is in line with previous findings by Dauth et al. (2014) and Dippel et al. (2015) – who examined the impact of Eastern European imports on the German labour market and voting behaviour, respectively – but it provides a novel view of the specific Brexit case. The results also show that voting behaviour was more sensitive to CEE imports than Chinese imports. In this context, it is important to distinguish between the size of the import shock, as measured by the import shock variable, and the effect. Although the total size of the Chinese import shock is larger – as measured by the average of the regional import shocks – the effect for a given import quantity is much smaller. Overall, this outcome suggests that the CEE import shock was undoubtedly a more significant determinant of Brexit.

Exploring the difference in main effects

The fundamental question that remains unanswered at this point is, ‘What can explain the difference in the impacts of the two import shocks?’ A possible interpretation is strictly methodological, and we suggest two mechanisms that could explain the variation: news exposure and migration share.

We partially attempt to explore the differentiated impact of migration as a form of moderator since others have shown that economic and cultural factors interact (Carreras et al., 2019). For instance, while Goodwin and Milazzo (2017) report clear evidence for the effect of migration on Brexit (higher migration correlates with higher shares of leave votes), our results (in line with Colantone & Stanig, 2018a) suggest⁸ that areas with higher migration shares are associated with lower leave vote shares but that migration interacts with economic factors. This may seem reasonable since migrants tend to concentrate in economically successful, densely populated areas that also tend to have lower shares of leave votes. This assumption is in line with findings by Nicoli and Reinl (2019), who show that migration interacts negatively with population density and positively with unemployment in predicting voting for Eurosceptic parties and Hartevelde et al. (2018), who suggest that perceptions of and news on migration correlate with support for nationalist agendas.

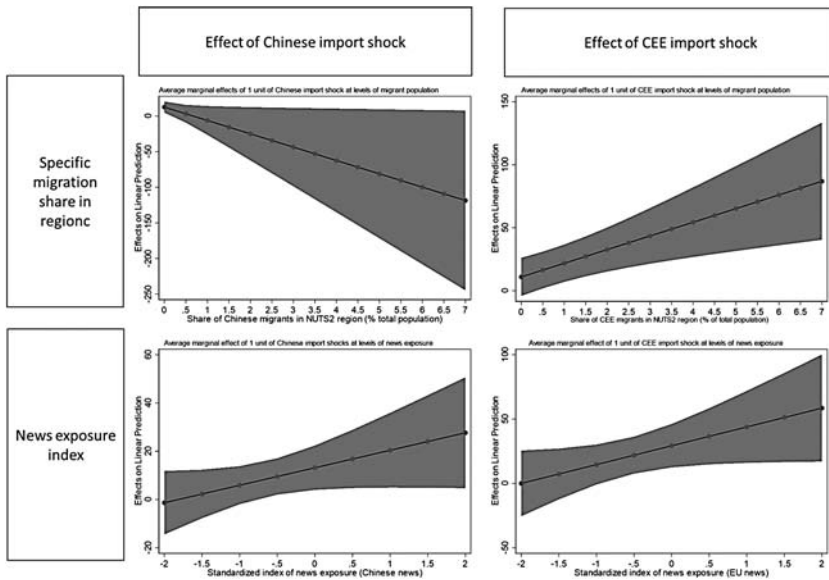


Figure 2. Interaction Effects.

Estimates can be found in Models C5–C8 in Table 5.3 of the online appendix. Note that in dy/dx marginal estimation, the size of the effect pertains to the effect on y attributable, at certain levels of the moderator of interest, to the increase of the independent variable of interest of one full unit. As discussed in footnote 7, a full unit is equivalent to about 1,000 dollars per worker of import exposure.

We treat the specific share of Chinese (or CEE) migration, as well as the news exposure index, as a moderator of the effect of the import shock, and we therefore examine interaction effects. This analysis is exploratory because we cannot consider migration share and news exposures fully exogenous. Table 5.3 in the Appendix reports the estimates for Models C5–C8, while Figure 2 provides a snapshot of these effects.

As expected, the effect of the CEE import shock increases as the CEE population share increases in the constituency, while the opposite effect is seen for the Chinese import shock (which is however seldom statistically significant). Furthermore, the impact of both import shocks increases as news exposure increases, and although the effect is statistically significant for both shocks, the effect of the same unitary increase in import exposure at a given level of news exposure is about twice as large for CEE countries than it is for China.

Admittedly, these results (unrealistically) imply that the individuals affected by the global competition in the UK would be able to differentiate fully between the sources of their economic struggle. However, voters are not completely ignorant when it comes to differentiating between factors that have influenced them on a personal level. For example, Margalit (2011) finds that manufacturing workers who lost their job due to offshoring were twice as likely to vote against the incumbent politician in the next

election compared those who lost their jobs due to other factors, such as domestic competition. This indicates that the effect of negative economic outcomes on voting behaviour is not an unconscious process that influences voting in a uniform manner, regardless of the source. If we grant voters some degree of insight into the source of the import shock, CEE imports would have a greater impact on voting behaviour in a referendum directly deciding on the UK's EU membership and trade relationship with Eastern Europe.

Limitations

While providing novel evidence on the impact of European economic integration on the Brexit vote, this article faces several limitations. First, we look only at trade in goods, focusing therefore on manufacturing and deindustrialisation, as Colantone and Stanig (2018a) and Becker et al. (2017) do. Hence, we exclude trade in services, for which regionalised data are less readily available. This methodological choice is perhaps less problematic for China, but the CEE shock came with the eastward expansion of the single market, inclusive of services. Second, we focus on real imports, and we therefore disregard the dynamics of exports. Thus, the analysis does not consider the (possibly positive) effects of exports on domestic labour markets. In other words, we assess the net effect of trade on industrial employment based on gross imports only. Since the UK has historically run a trade deficit with both China and CEE countries, one would expect the effects on the labour market to be negative on aggregate, which would make the omission of export data in this research paper less of an issue. Nonetheless, an analysis that includes exports – for example, by calculating the net imports (imports – exports) as Dippel et al. (2015) do – would allow for more accurate conclusions.

Thus, while this article demonstrates the *negative* effect that globalisation and European integration have had on the voting behaviour of the *losers* of globalisation, it – along with other articles adopting the same method – fails to account for any *positive* effect present among the *winners* of globalisation. This means that our results concern the effect of *imports* themselves but not trade openness overall. All else equal, it is plausible that – without the additional trade in services brought about by European integration (and here, unaccounted for) – some areas of the UK would have been more Euro-sceptic than they have been in reality.

Thus, while this article shows the *negative* effect of globalisation and European integration on the voting behaviour of the *losers* of globalisation, it fails to account for any *positive* effect present among the *winners* of globalisation. This means that our results concern the effect of *imports* themselves but not trade openness overall. All else equal, it is plausible that some areas of the UK would have been more Eurosceptic than they have been in reality.

Moreover, it is unclear to what extent the findings are generalisable regarding the overall process of globalisation, in terms of both geographical distribution and integration processes. The UK has always constituted a peculiar element of the European system, and it has a long history of viewing European integration with suspicion. Whether the effects of globalisation and European economic integration would be as disruptive in other member states as they were in the UK remains disputable.

Finally, as with most shift-share approaches, this article's methodology suffers from problems related to the incomplete chain of causality from macroeconomic development to individual behaviour. Without an appropriate individual-level dataset on individual exposure to different sources of shock, we cannot explore further the individual-level dynamics of the vote, as Colantone and Stanig (2018a) do while investigating a single shock. Without a long individual panel series on political behaviour, the import shock approach faces a hard limitation in having to connect individual-level behaviour with aggregate data; it therefore suffers, from potential ecological fallacy.

Methodological implications

So far, throughout the article, we have assumed that the import-shock method to measure shocks is reliable. However, the analysis carried out here suggests that the method itself may be marked by an 'original sin'. While a full methodological critique of the import-shock approach is beyond the scope of this article, this section addresses the main methodological implication of our findings. Our intuition is that the import-shock approach, in the form it is currently adopted in the literature (including in this article) is, by construction, ill-suited for any comparative analysis. This is because, in any given country, different import shocks share an identical component capturing the underlying regional use of labour: $\sum_k \frac{L_{ik}}{L_i}$. This, in turn, means that any import shock from any source will be multiplied by the same domestic amount. Hence, shocks from different sources will be highly correlated. In our case, the correlation is very high: 0.71. In turn, this may mean that, insofar as alternative instruments are valid, either process could explain the same changes in the underlying use of labour. Hence, other explanations notwithstanding, the largest expected per-unit effect is likely attributable to the *smallest* shock successfully instrumented. If so, then it would not be surprising to identify a larger effect of one unit of CEE shock than of one unit of Chinese shock: since the former is smaller than the latter in absolute terms and they attempt to explain the same change, the change per unit is larger for CEE countries than for China. In fact, it is important to note that, even though the Chinese shock has been prominently featured in the literature so far, this appears to be an issue of timing rather

than underlying processes: there is no real way of fully tearing these effects apart without considering interaction effects, as we tentatively start to do. In our perspective, therefore, the shift-share approach could fail in distinguishing between sources of exposure; instead, it likely captures the effect of exposure to declining industries more generally.

As this paper is not a methodological contribution, we cannot provide formal proof that this intuition is correct. However, until the doubts cast here are addressed by advances in the methodological literature, readers should treat past, present, and future contributions using the methods (including ours) with appropriate care.

Conclusion

This paper adds to the growing literature examining the effects of economic globalisation (in the form of trade) on voting behaviour. The results of this study support earlier findings by Los et al. (2017), in that intra-European trade, in the form of CEE imports, was a significant determinant of the outcome of the Brexit referendum. Our main contribution pertains to the relative sizes of these effects: while the results also confirm previous research that identified Chinese imports as an important driver, the impact per unit of Eastern European imports was substantially larger. To study the underlying mechanisms behind these different effect sizes, we additionally tested for moderating effects of media salience and migrant population composition. Our findings support the assumption that citizens indeed have an ability to distinguish between different sources of trade. However, we cannot completely reject the possibility that the identified difference between Eastern and Chinese imports might be due to the import shock method itself.

Overall, opening to international trade is bound to produce winners and losers. This article shows that losers or perceived losers are likely to react to increases in import competition, and that other factors—like news exposure or migrant communities—may modulate voters' perceptions of competition.

Since our model only focuses on imports of goods, without assessing exports and services, our results seem to suggest that the reaction of the 'losers' of integration is stronger than the support of 'winners'. This effect could depend on different patterns of mobilisation, whereby challengers of the status quo have stronger mobilisation incentives than the status quo's defenders. However, it may also depend on uneven distribution of gains from trade, which might be the case if the country overall benefitted from trade but the median citizen perceived herself as losing out. In fact, while some benefits of integration and globalisation, such as lower prices for consumers, are spread broadly across society, other benefits, such as those that accrue from increased export opportunities, are spread unevenly. The current

set-up is ill-suited for discriminating between these phenomena, and further research is needed to explore them.

In sum, the effect of globalisation on voting behaviour can differ greatly depending on which economies the high-income country trades with. With Brexit, the difference in impact between China and CEE countries is likely explained by one or both of the following explanations: (1) CEE imports had a greater impact on the labour market due to the type of goods that the CEE countries specialise in; (2) voters – especially those working in manufacturing industries – were, on some level, cognisant of the origin of their economic struggle and were therefore more likely to vote in favour of leaving the EU (and reducing intra-EU trade) in those areas that experienced greater import competition from CEE countries. We explored two potential reasons that this might be the case, namely differentiated exposure to migrant communities and different news coverage. Although further work is needed to assess the exact mechanisms at work behind these interactions, both suggest that Chinese imports were simply not as relevant to Brexit since the referendum revolved around questions regarding European integration.

Notes

1. There might also be compositional effects: Dauth et al. (2014) find that due to import substitution, Eastern European imports had a stronger impact on German labour markets than did Chinese ones.
2. Including these interactions in our model prevents us from interpreting the results in a causal way; since it is possible that migrant distribution over the territory and news exposure could be endogenous to trade exposure.
3. Bulgaria, Czech Republic, Hungary, Poland, Romania, Slovakia, Slovenia, Estonia, Latvia, and Lithuania.
4. All import shock variables and instruments are divided by 1,000 for easier interpretation. The value of almost all import shocks is between 0 and 1. A 0.1 point increase in the import shock variable represents a 100 dollar increase in the import exposure per worker, and a 1 point increase represents an exposure of 1,000 dollar per worker.
5. These other nations are Australia, Canada, Japan, New Zealand, Sweden, and Singapore, based once again on Dauth et al. (2014).
6. Standard deviation of the instrumented Chinese shock (0.199) multiplied by its coefficient (10.329).
7. Standard deviation of the CEE shock (0.0867) multiplied by the coefficient (35.177).
8. One should be careful in interpreting the coefficients of these control variables because the two variables have a correlation coefficient of 0.945 and therefore suffer from severe multicollinearity.

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