

The German Trade Shock and The Rise of the Neo-Welfare State in Early 20th Century Britain¹

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Abstract

A neo-welfare state emerged in Britain in the first decade of the 20th century. We study the international origins of this transformation, examining the effects of rising German import competition on economic and political development in late 19th and early 20th century Britain. Employing a shift-share measure of local exposure to German imports, we present evidence that rising imports caused worse labor market outcomes as measured by vagrancy and the share of workers in unskilled jobs in the period 1880-1910. We also find that rising imports led to a decrease in support for the Conservative Party in national elections after 1900, by which time the Liberal Party had signaled its support for welfare state reforms. We suggest that trade's effect on support for a more expansionary state is driven by two mechanisms: the German trade shock increased assessments of how volatile employment is in a market economy and it changed elite beliefs about the deservingness of the poor, transforming vagrants into the unemployed. We show that rising imports increased local newspaper references to trade and imports, increased references to social reform in Liberal campaign manifestos, and increased references in local newspapers to terms associated with attributing bad labor market outcomes to market forces relative to terms associated with blaming the poor for bad outcomes.

1 Introduction

Seminal contributions by Cameron (1978) and Rodrik (1998) advanced the idea that openness to international trade led to the expansion of government spending. Cameron emphasized how specialization in trade led to industrial concentration which in turn strengthened the role of unions in policymaking. Rodrik argued that trade increased economic volatility, and that state spending from both government consumption and social insurance could help limit the negative consequences of these disruptions. This compensation theory became not only central to understanding variation in the size of government and the growth of the welfare state (see also Adserà and Boix (2002); Mares (2005)), but also foundational to Ruggie (1982)'s embedded liberalism argument that open markets were politically possible because states limited their distributional consequences in part through the welfare state and other forms of government spending (Hays, Ehrlich and Peinhardt, 2005; Hays, 2009; Kurtz and Brooks, 2008; Mansfield and Rudra, Forthcoming). To be sure, critics and the authors themselves pointed out that even if openness increased the demand for government, it might also create a race to the bottom that constrained the ability of states to meet the new demands of their citizens (Rodrik, 1997; Rudra, 2002). Nonetheless, this critique does not conflict with the main idea that openness increases the demand for government.

The empirical record of the last two decades raises questions about the relationship between openness and the demand for a greater role for government in the economy. China's integration with the world economy, and policy liberalization around the globe increased exposure to trade in many countries. The political response has been varied both within and between countries. In some cases, the re-

action has been a turn leftward consistent with compensation theory. But in others, the response has been characterized by skepticism about the role of government in the economy, a preference for protectionist trade and restrictionist immigration policies, and a turn toward more authoritarian and nationalist values (Margalit, 2019; Che et al., 2016; Colantone and Stanig, 2018*b,c,a*; Hays, Lim and Spoon, 2019; De Vries, Hobolt and Walter, 2020; Milner, 2021; Broz, Frieden and Weymouth, 2020; Ballard-Rosa et al., Forthcoming; Gidron and Hall, 2017, 2020). While some have argued that this reaction in part reflects unmet demand, with compensation theory helping to explain variation in the extent of the right-wing populist reaction (Colantone and Stanig, 2018*c*, 2019), it is clear that increased openness is not necessarily accompanied by rising demand for government.¹ Other research finds that import competition leads voters to punish incumbent legislators (Jensen, Quinn and Weymouth, 2017).

This poses an important set of research questions about what accounts for variation in how voters respond to increased openness. Compensation theory was developed with reference to the global economy in the second half of the twentieth century and most empirical work evaluating the framework studies the same period. To understand when and why voters react differently to increased openness, we need to broaden the empirical record investigating the question. The large and expanding literature on the China shock is starting to compile exactly such a record. In this paper, we study the “golden age” of globalization from 1880 to 1910 in Great Britain and specifically investigate the economic and political consequences of the surge in

¹Adserà and Boix (2002) and Mares (2005) provide early accounts of heterogeneous policy responses to increased trade integration. See Iversen and Cusack (2000) for a critique of compensation theory that takes a skeptical view of the quantitative importance of globalization in generating labor market risks that would merit a policy response.

German imports that accompanied Germany’s industrialization and integration with the world economy. This case is critical for understanding the scope conditions of compensation theory as it investigates the political responses of a different era of globalization and evaluates the role of openness in the origins as opposed to the expansion of the welfare state.

We estimate the effects of the German trade shock on economic and political outcomes in England and Wales from 1880 to 1910 using parliamentary constituencies as the unit of analysis. We measure the change in import penetration at the local level using the empirical strategy developed by Autor, Dorn and Hanson (2013). Specifically, we construct a shift-share change in import penetration per worker measure of local exposure to German imports based on 94 industries using national-level trade data by product and local measures of occupations allocated to each constituency. We examine the effects of this variable on labor-market disruption using census micro-data at the constituency level, and on the vote shares of different parties. To further understand the political response to the German trade shock, we use data from the British Newspaper Archive on the text of 480 newspapers, which we geocode and link to parliamentary constituencies. We use this source to measure local concerns about trade and immigration as well as local beliefs about the deservingness of the poor. Finally, we also measure local concerns from references in candidate campaign manifestos collected by Laura Bronner and Daniel Ziblatt including references to social reform and attitudes about the unemployed.

Our estimation strategy examines the effects of within-constituency changes in imports per worker on our measures of labor market outcomes, voting for particular parties, and the prevalence of different issues in newspapers and campaign

manifestos. We estimate first-difference and fixed effects regressions and control for non-linear trends related to pre-shock manufacturing activity. Estimates from these regressions can be interpreted causally within the difference-in-differences framework. The key identifying assumption is that apart from the effects of changes in imports, constituencies with greater employment in affected industries would have followed similar trajectories to constituencies with less employment in those industries.

We present evidence that rising imports caused worse labor market outcomes as measured by vagrancy and the share of workers in unskilled jobs in the period 1880-1910. We also find that rising imports led to a decrease in support for the Conservative Party in national elections after 1900, by which time the Liberal Party had signaled its support for welfare state reforms. The key findings are that the German trade shock had a negative effect on local labor markets in Britain and the political response was a shift away from the Conservative Party toward left-of-center parties, mostly toward the Liberals. This result is inconsistent with voters demanding protectionism in response to the trade shock. After 1900 the Liberals still unambiguously favored free trade while the Conservative Party was divided with some party leaders advocating protective tariffs.

We find evidence that trade elevated xenophobic concerns, but this mechanism is not driving our main voting results. We find that German import competition was correlated with references in Conservative candidate campaign manifestos to immigrants, aliens, and Jews and that trade shocks were correlated with xenophobia as measured by local newspaper references to foreigners. But this effect should have on balance favored the Conservative Party who were expressing the concern and had

implemented restrictionist immigration policies in the 1905 Aliens Act, and so this effect cannot account for our main voting results. We also find no evidence that the results are driven by incumbency.

Given that the timing of when the trade shock favored the Liberals coincided with the Liberals' embrace of social reform, this result is broadly consistent with compensation theory. We further present evidence that trade shocks are correlated with increased references to social reform in Liberal candidates' campaign manifestos, which bolsters the interpretation that greater support for Liberal candidates reflected a demand for the emerging neo-welfare state.

We suggest that there were two mechanisms at work in trade's effect on the demand for more government. First, as argued by Rodrik (1998), the German trade shock increased assessments of how volatile employment is in a market economy and as result increased the demand for government policies that would smooth these cycles. We show that rising imports increased local newspaper references to trade and imports in addition to Liberal candidate references to social reform. Second, we find evidence suggesting that the trade shock changed elite beliefs about the deservingness of the poor, transforming "vagrants" into the "unemployed." A range of social scientific work on support for the welfare state emphasizes that the more individuals believe that bad economic outcomes are due to a lack of effort or some other defect on the part of the worker, the less favorably they view the welfare state (see e.g. Piketty (1995); Fong (2001); Alesina and Angeletos (2005)). For much of the history of capitalism up to the 20th century, moral failing was a dominant account of poverty. We show that trade shocks are positively associated with the use of neutral terms like "unemployment" relative to morally-charged terms like "pauperism" and

“vagrancy.”

This paper makes three main contributions. First, it provides evidence that the golden age of globalization contributed to demands for welfare state development at the origins of the neo-welfare state in contrast to previous work primarily focused on the post World War II and contemporary periods. As such, the paper builds on Mares (2005)’s cross-country study of unemployment insurance during the interwar period and provides an out-of-sample test of compensation theory with a research design that supports a causal interpretation. This contribution is complementary to Barnes (2020)’s recent work arguing that the shared interests in free trade of elites and labor led to more progressive tax policies prior to World War I in Europe generally and in the United Kingdom specifically. Barnes (2020)’s argument is not about compensation, in that she emphasizes shared interests in free trade driving some elites to compromise on progressive taxation that workers already were demanding. Nonetheless, both her study and ours argue that the international origins of the neo-welfare state have been neglected in prior research.

Second, the paper introduces a new mechanism for the compensation effect of globalization: negative trade-induced labor market outcomes are less likely to be attributed to the failings of the unemployed and government spending on the deserving poor is viewed more favorably by voters. This connects compensation theory to a large empirical literature on public support for redistributive policies.

Third, the emerging literature studying the political consequences of China’s integration into the world economy suggests heterogeneous responses, leading to increased demand for compensation in some cases, authoritarianism and economic nationalism in others. This paper applies similar methods to Germany’s integration

into the world economy to construct a richer empirical record that might help identify the conditions which make some political reactions to globalization more likely than others.

The rest of the paper proceeds as follows: we first describe the economic and political environment in late 19th and early 20th century Britain that witnessed dramatic increases in German imports, significant economic change, and the emergence of new cleavages in British politics over the regulation of capitalism and the formation of a neo-welfare state. We then describe the new constituency-level historical data that we have constructed to study the effect of rising German imports on labor market outcomes, election results, and local economic and political concerns expressed in newspapers and campaign manifestos. Next, we outline our empirical strategy and present our main results on the effect of the German trade shock on labor market outcomes and election results. We then present our analysis exploring the mechanisms underlying the relationship between rising imports and vote choice. We conclude by discussing the implications of the findings for the literatures on globalization, size of government, and redistributive politics.

2 German Trade and British Political Economy in the Late 19th and Early 20th Century

Before analyzing the within-constituency effects of German imports on economic change and demand for the neo-welfare state, it is natural to ask whether at the national level rising imports from Germany were accompanied by the expansion of social spending.

Figure 1 reports UK imports from Germany from 1880 to 1910. Our data come from the *Annual Statement of the Trade of the United Kingdom*. At this time, Germany shipped its products directly from German ports but also through Belgium and the Netherlands. Our data source assigns the country that the good is shipped from as the origin of the import whether or not the good was produced there. Consequently, we count imports from Belgium and the Netherlands as German imports as well as shipments directly from Germany. The figure indicates an almost doubling of German imports from 1880 to 1910.

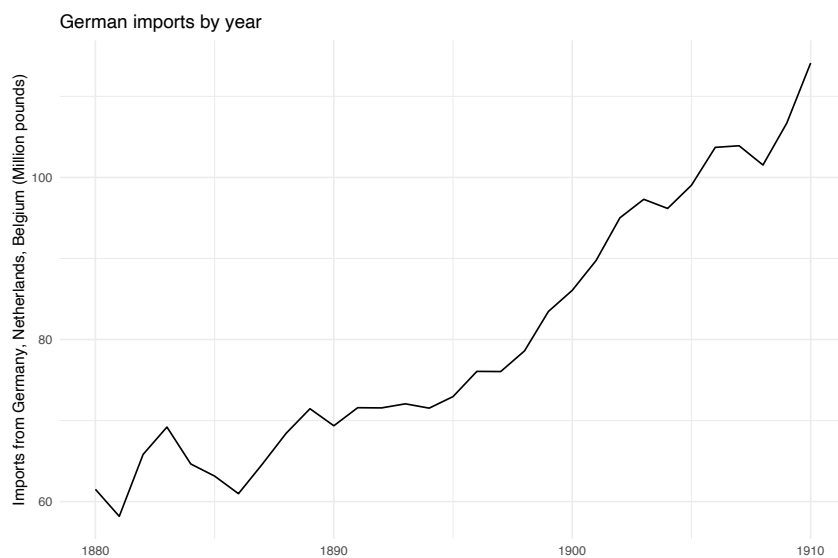


Figure 1: UK imports from Germany, 1880–1910

During most of this period, there were only modest changes in German and UK trade policies. Germany generally had high tariffs while the UK maintained free trade. The increase in German imports reflected the country’s rapid industrialization, especially after 1890, comparative advantage, and declining transportation costs. Figure 2 breaks down the increases in imports by product categories.

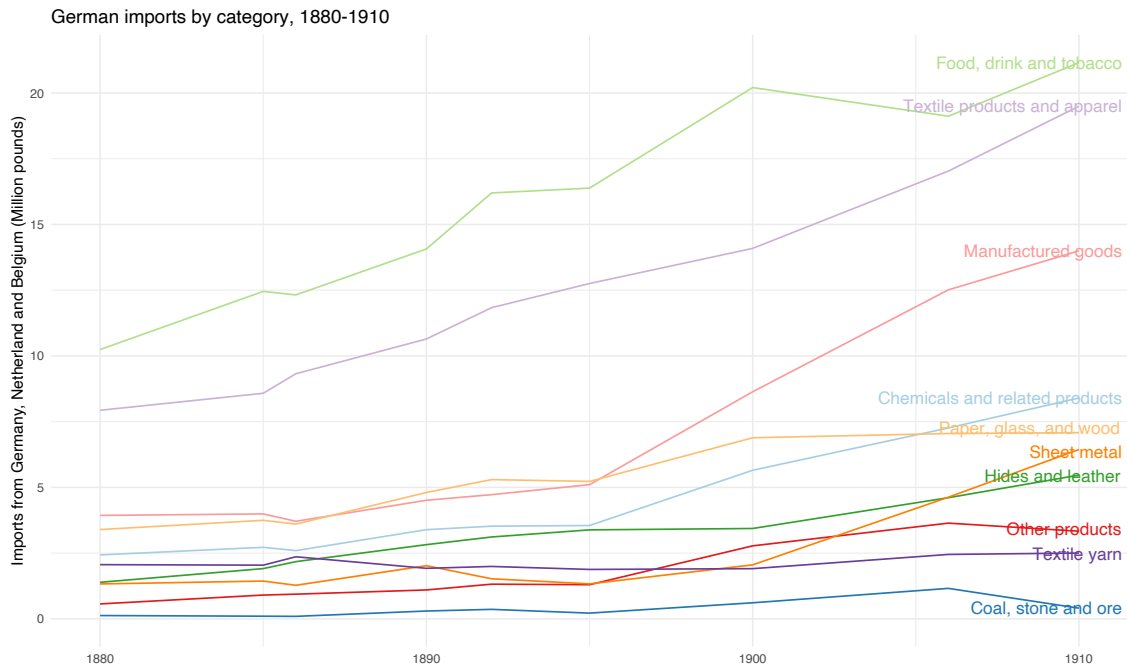


Figure 2: UK imports from Germany in decade and election years, by category

Figures 1 and 2 suggest that the magnitude of the shock was economically significant. Below we provide a new analysis assessing the economic effects of the shock. But for context, it is important to note that contemporaries thought German imports were important. They were in fact one of a number of indicators that suggested relative economic decline in the Victorian era and explaining this decline as well as identifying what to do about it was an obsession of businessmen and economists of the period (McCloskey and Sandberg, 1971). An 1896 book drawing attention to the prevalence of imports “Made in Germany” ran through six editions (Minchinton, 1975). The book warned “The industrial supremacy of Great Britain ... is fast turning into a myth” (Williams, 1896, 1). In a 1903 speech, Joseph Chamberlain, a leading advocate of protectionism, warned that in the face of foreign

competition “Sugar has gone; silk has gone; iron is threatened; wool is threatened; cotton will go ... Do you think, if you belong at the present time to a prosperous industry, that your prosperity will be allowed to continue?” (Chamberlain, 1914, 177).

Were these rising imports accompanied by greater social spending? Figure 3 reports data from Boyer (2019) combining spending on poor relief and spending on pensions in the United Kingdom. It records a steady increase in social spending starting in the 1890s through the mid-1900s followed by a dramatic increase for the remainder of that decade and leading up to World War I. This increase reflected the Liberal Party running and winning in 1906 on a platform committed to social reform and free trade. The data capture only a fraction of the legislation enacted in this period that could be viewed as, in part, serving a compensatory purpose. The Liberals passed the Workmen’s Compensation Act of 1906, the Old-Age Pensions Act of 1908, the Labour Exchange Act of 1909, the National Insurance Act of 1911 as well as other legislation that would address directly and indirectly some of the costs associated with increased import competition. It is, of course, impossible to tell from these aggregated data whether greater social spending was at least partially a response to increased trade. The remainder of the paper seeks to determine the nature of this relationship.

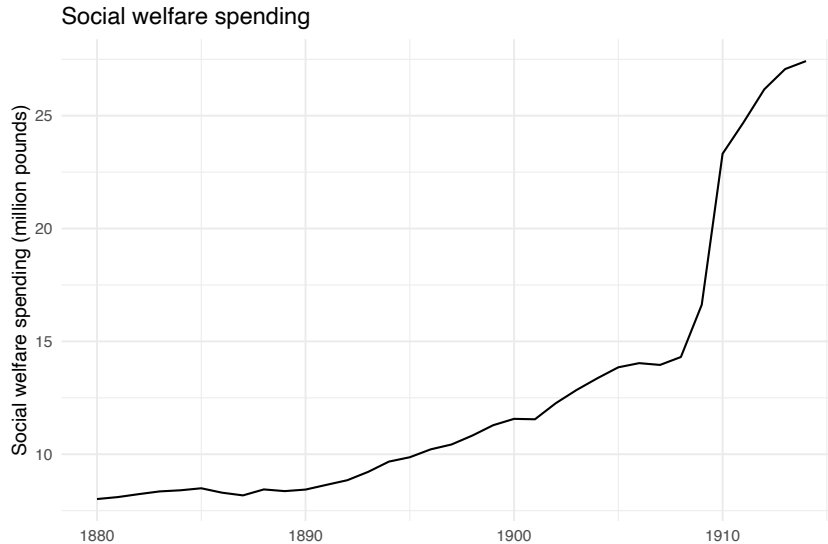


Figure 3: Social welfare spending, 1880–1914

3 Data

3.1 Trade and Labor Market Outcome Data

We estimate the effects of the German trade shock on economic and political outcomes in England and Wales, using parliamentary constituencies as the unit of analysis. We measure the change in import penetration at the local level using the empirical strategy developed by Autor, Dorn and Hanson (2013), that is, we compute

$$\Delta IPW_{it} = \sum_j^n \frac{L_{ij}}{L_i} \frac{\Delta M_{jt}}{L_j}$$

where L_{ij}/L_i is the share of employment in industry j in constituency i in the base year, 1881. $\Delta M_{jt}/L_j$ is the change in imports for industry j in year t , relative to total employment in that industry in 1881. We index the change in imports relative to

different years in different specifications: in long first-difference specifications, ΔM_{jt} is the change in imports relative to the previous period, in other models which use constituency fixed effects we index relative to the first year used in the analysis, although the choice of years does not affect the coefficients estimated.

We use the full-count 1881 census of England and Wales (Schürer and Higgs, 2014) to compute the sizes and distributions of different industries, and combine this with product-level data on imports from the *Annual Statement of the Trade of the United Kingdom*. Occupational categories in the 19th-century census contain a high degree of specificity about industries, distinguishing, for instance, “Ironfounders” from “Iron clasp, buckle, and hinge makers” and “brass founders.” We group occupational categories and product-level import data into 94 industries, with the aim of identifying the finest level of variation present in both the trade statistics over the total period and the occupational categories.

British parliamentary constituencies do not coincide with administrative units, which has previously prevented scholars from computing economic variables at the constituency level. We resolve this problem by allocating parishes—the finest level of aggregation in the census—to constituencies. For the 1881 census we use crosswalk files constructed by Jusko (2017), who manually assigned parishes to constituencies, based on contemporary reports by the boundary commission and maps. For other years we first link the census data to a consistent GIS based on parishes in the 1851 census (Satchell et al., 2016), using crosswalk files constructed by Day (2016). We then assign parishes to constituencies using shapefiles from Project (2004). Where parishes fall into multiple constituencies, we weight the fraction assigned to each constituency by the fraction of the parish falling into that

constituency multiplied by the relative population density of the constituencies.²

We compute two measures of the economic effects of the trade shock—the percentage of vagrants and the percentage employed in unskilled occupations—at the constituency level, using full-count data from the 1881, 1891, 1901 and 1911 censuses. We classify vagrants as those whose occupation was listed as “No specified occupation – vagrants, unemployed.” This measure plausibly captures labor-market disruption, in the form of increased unemployment, and the unemployed migrating in search of work. Using the limited time-series data collected by Poor Law administrators, Boyer (2019, 111–112) finds that rates of vagrancy and unemployment closely tracked one another.

We classify unskilled occupations using the *Seventy-fourth Annual Report of the Registrar General*, 1913, which allocated census occupations to eight social classes. The percentage of people in occupations in class 5 (“occupations including mainly unskilled men,” p. xli) has been used in the historical geography literature to measure poverty at the local level (Gregory, Dorling and Southall, 2001). The fraction employed in unskilled jobs would plausibly increase in response to import competition if there was a reduction in higher-skilled employment, shifting workers into lower skilled jobs. Using a contemporary measure of status prevents us from making anachronistic classifications, due to the valence of job titles varying over time. For instance, “Builders” are classified in the Registrar General’s report as performing a managerial task, as opposed to their unskilled laborers.

²The fraction of a parish assigned to constituency i is

$$\frac{s_i d_i}{s_i d_i + \sum_{j \neq i} s_j d_j}$$

where s_i is the fraction of the parish physically located in constituency i , and d_i is the population density of constituency i . We calculate population density using the published census reports.

We additionally use full-count census data to compute two important control variables: 1881 manufacturing share, and share of immigrants. In many of our regression specifications we control for 1881 manufacturing employment interacted with year dummies in order to separate the effects of the German trade shock from time-variant effects related to manufacturing. We compute this measure of 1881 manufacturing employment using the fraction of people employed in secondary occupations — those in which raw materials were converted into finished products — according to the classification system developed by Wrigley (2010), using lookup tables to the census occupations provided by Bennett et al. (2017). Figure 4 shows the geographic distribution of import competition in 1910, with and without this control.

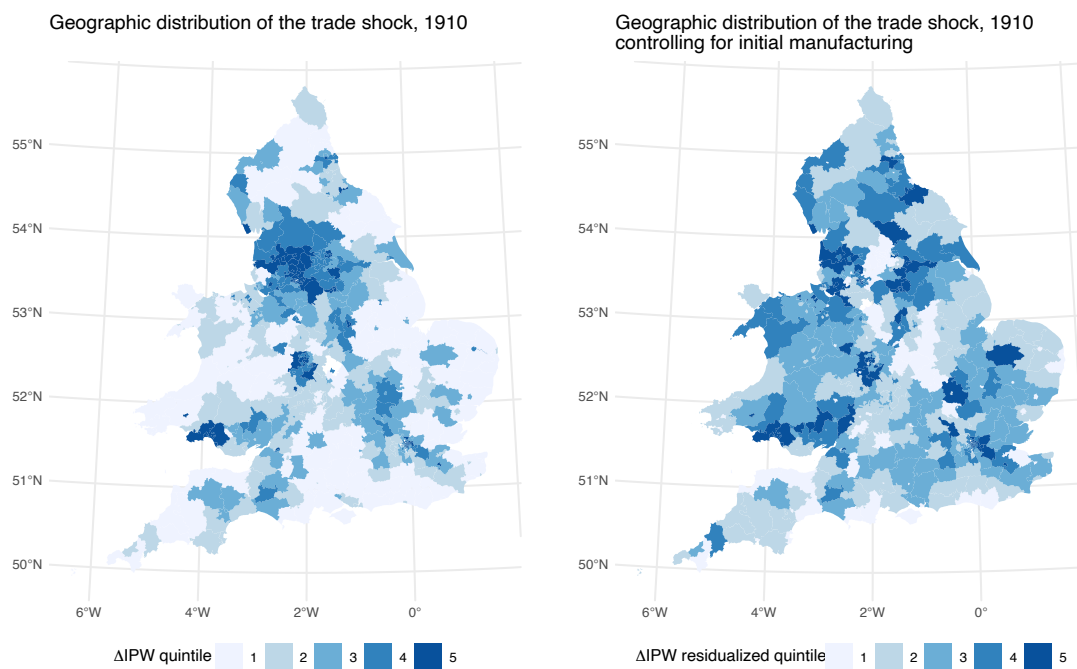


Figure 4: Geographic distribution of change in German imports per worker, 1885–1910

We examine whether the German trade shock increased xenophobia. A regression of xenophobia on the trade shock could however be biased, if, for instance, areas affected by the shock happened to have more immigrants, and the prevalence of immigration-related issues at a national level changed over time. In regressions examining the effect of the trade shock on xenophobia, we control for the 1881 share of immigrants—specifically, the fraction of the population recorded in the 1881 census as not being born in Britain or Ireland—interacted with year dummy variables. This approach allows for the effect of immigration on anti-immigrant sentiment to vary by year. It is also preferable to controlling for the share of immigrants in the year in question, which may itself be affected by anti-immigrant sentiment, and thus would be a bad control. We exclude Irish immigrants as Ireland was legally part of the domestic British Isles and so Irish immigrants were not considered Aliens and were not subject to anti-immigration legislation.

3.2 Election Data

Our primary measure of the political effects of import competition is the share of the vote won by Conservative and Unionist parliamentary candidates. We use data from Eggers and Spirling (2014), and compute the share of the vote won by different parties in the eight general elections from 1885 to 1910. Constituency boundaries and the electoral franchise were consistent over this period. The franchise was also relatively broad: around two-thirds of adult men could vote. Exclusion was somewhat arbitrary, based primarily on residency criteria, leading one historian to conclude, after analyzing ward-level population and voter records, that “the overall occupational structure [of the franchise] does not differ vastly from what one would have expected

from a fully inclusive franchise” (Brodie, 2004, 52). We additionally compute three measures of the trade shock’s effects on incumbency: the share of the vote won by incumbent MPs in a given constituency in a given year, the share of the vote won by incumbent parties—that is, parties representing that constituency in the previous parliament—in a given constituency in a given year, and the election-to-election change in the vote share won by the governing party.

3.3 Newspaper Measures of Local Concerns

We use data from the British Newspaper Archive to estimate the prevalence of different local concerns. The British Newspaper Archive is a project aiming to digitize the British Library’s extensive historical newspaper collections. Over the 1885–1910 period, the British Newspaper Archive contains text for 480 newspapers, which we geocode and link to parliamentary constituencies.³ We compute the number of references to specific terms made in a given year by a given newspaper, divide by the number of issues of the newspaper in the British Newspaper Archive in that year, and then subtract the mean and divide by the standard deviation of that variable to aid interpretation. Our intuition in using these measures is that if an issue became more prevalent in a given constituency in a given year, one would expect newspapers to devote greater attention to it. We use newspaper fixed effects in all such specifications to control for time-invariant linguistic or topical features of specific newspapers.

³In cases where city newspapers would have catered to multiple constituencies—for instance, the *Manchester Guardian* would reflect opinion in Manchester, and not just one particular Manchester parliamentary constituency—we aggregate the shock variable at the city level.

3.4 Other Data

We additionally use an unpublished dataset of parliamentary candidates’ manifestos compiled by Laura Bronner and Daniel Ziblatt. From the late 19th century onwards, candidates could distribute one leaflet for free via Royal Mail, in order to inform voters of their views. Bronner and Ziblatt collect and digitize manifestos for all parliamentary candidates in general elections from 1892 to 1910. We use this data in a similar way to the newspaper data. We divide the number of references to a given term by the number of words in the manifesto, and then standardize that measure.⁴

4 Empirical Framework

4.1 Model specification

Our estimation strategy examines the effects of within-constituency changes in imports per worker on a set of outcome variables: labor market distress, voting for particular parties, and the prevalence of different issues in newspapers and campaign manifestos. We use two main model specifications. For the economic outcome variables, using decadal data from the census, we estimate regressions of the form

$$\Delta Y_{it} = \beta_1 \Delta IPW_{it} + \mathbf{X}'_{it} \beta_2 + \gamma_t + \varepsilon_{it}$$

where ΔY_{it} is the change in a given outcome variable in constituency i relative to the previous census, ΔIPW_{it} is the change in the trade shock measure relative to the

⁴These candidate communications should be interpreted in the context that voting was, nonetheless, by this time party-centered (Cox, 1984; Dewan, Meriläinen and Tukiainen, 2020).

Table 1: Summary statistics

Variable	N	Mean	SD	Min	Max
<i>Constituency</i>					
Manufacturing share 1881	464	0.202	0.093	0.069	0.474
Immigrant share 1881	464	0.009	0.011	0.001	0.132
<i>Constituency x Industry</i>					
Industry share	44,080	0.011	0.087	0.000	0.966
<i>Constituency x census year</i>					
Manufacturing share	1,852	0.162	0.079	0.056	0.430
Vagrant share	1,852	0.006	0.005	0.000	0.024
Unskilled jobs share	1,852	0.058	0.020	0.012	0.194
Average economic status	1,852	48.044	1.645	43.752	53.317
<i>First difference constituency x census year</i>					
ΔIPW_t	1,389	0.724	0.787	-1.226	8.498
$\Delta \ln$ vagrant share	1,389	0.019	2.239	-6.257	4.625
$\Delta \ln$ unskilled jobs share	1,389	0.014	0.130	-0.963	0.600
Δ Average economic status	1,389	0.610	0.514	-1.463	2.890
<i>Constituency x election year</i>					
ΔIPW_{1885}	3,133	0.945	1.262	-1.600	11.154
Conservative vote share	3,133	0.497	0.112	0.000	1.000
Liberal vote share	3,133	0.473	0.138	0.000	1.000
Labour vote share	3,133	0.047	0.147	0.000	0.817

previous census, γ_t is a year fixed effect, and \mathbf{X}'_{it} is a vector of controls. We estimate these models in stacked first differences, consistent with other economic studies of the effects of trade shocks (Autor, Dorn and Hanson, 2013).

We estimate the majority of regressions with political dependent variables in levels. This practice is consistent with empirical studies of the effects of trade shocks on voting (Colantone and Stanig, 2018*b,c*; Feigenbaum and Hall, 2015). We are interested in the effects of long-term changes in import penetration, not those of year-to-year variation. This focus makes 10-year census-to-census first-differences appropriate, but election-to-election first-differences inappropriate. One would expect the change in imports per worker relative to 1885 to affect voting in 1895, but one would not necessarily expect the change in imports per worker relative to 1892 to affect voting in that year.⁵ We estimate regressions of the form

$$Y_{it} = \beta_1 \Delta IPW_{it} + \mathbf{X}'_{it} \beta_2 + \gamma_t + \delta_i + \varepsilon_{it}$$

Where Y_{it} is some political outcome variable, ΔIPW_{it} is the change in imports per worker for constituency i in year t relative to the start year, \mathbf{X}'_{it} is a vector of controls, γ_t is a year fixed effect, and δ_i a constituency fixed effect. Note that the differenced dependent variables and constituency fixed effects account for time-invariant confounders.

⁵While there are theoretical reasons for favoring the specification in levels, the particular specification choice is not important for our results. As a robustness check we estimate the main voting regressions using long election-to-election differences: 1885–1892, 1892–1900, and 1900–1910, and obtain similar results.

4.2 Identification

Estimates from these regressions can be interpreted causally within the difference in differences framework. While our measure of imports per worker is computed according to a shift-share formula, our identification strategy does not rely on the use of exogenous variation in the form of exports from Germany to a third party. Goldsmith-Pinkham, Sorkin and Swift (2020) argue that shift-share designs rely on the assumption that the initial shares used to construct the shift-share variable are exogenous to the outcome variable. This assumption is more plausibly satisfied in research designs like ours which control for unit fixed effects, and for which the equivalent identifying assumption is that these shares are exogenous to changes in the outcome variables. Thus for our estimates to be interpreted causally, one must believe that, apart from the effects of changes in imports, constituencies with greater employment in affected industries would have followed similar trajectories to constituencies with less employment in those industries.

We address this assumption in three ways. First, we include controls for initial manufacturing interacted with year dummies across all our specifications. We thus allow more industrial constituencies to follow different non-linear trajectories to less industrial constituencies, and implicitly compare constituencies affected by German imports in a given year to less-affected industrial constituencies. Second, we follow the procedure outlined by Goldsmith-Pinkham, Sorkin and Swift (2020) to identify the industry-year combinations for which our estimated coefficients are most sensitive to mis-specification, and show that our results are robust to controlling for these initial industry shares interacted with year dummies, and to controlling for the first 3 principal components of the 1881 industry shares interacted with year

dummies. These robustness checks suggest it is unlikely that differential trends relating to specific industries or clusters of industries are driving our results. Third, we employ traditional difference-in-differences robustness tests: controlling for constituency time trends in appropriate specifications, and in others we control for leads of the trade shock measure.

The shift-share design is important to our empirical strategy as an accounting method, and as a way to avoid bias from post-treatment economic changes. It is important to emphasize that our primary use of the Autor, Dorn and Hanson (2013) trade shock formula is simply to measure the incidence of import competition at the local level. Using the 1881 industry shares, as opposed to subsequent shares, has the additional benefit of separating our measure of exposure to German imports, from changes in local economies that may themselves be affected by German imports.

4.3 Standard Errors

There are two potential problems with using standard errors clustered at the constituency level. First, misallocation of parishes to parliamentary constituencies, or local spillover effects may induce spatial autocorrelation in the error terms of our regression. We account for this possibility by conservatively clustering our standard errors at the county level, rather than at the more granular constituency level. Second, Adão, Kolesár and Morales (2019) note that in shift-share designs, conventional standard errors fail to account for correlation in the error structure between units with similar shares. Borusyak, Hull and Jaravel (2018) show that one can avoid this autocorrelation problem by aggregating the relevant variables to the industry level, in the same way that one can avoid problems with within-cluster correlations

by aggregating to the level of the cluster. They show that the “exposure robust” standard errors from such an aggregation are asymptotically equivalent to those proposed by Adão, Kolesár and Morales (2019). In the appendix, we re-estimate all the regressions in the main body of the article using the aggregation method proposed by Borusyak, Hull and Jaravel (2018).

5 Economic Consequences of the German Trade Shock

We first examine the effects of German import competition on labor market disruption. Table 2 reports the results of stacked first-difference regressions in which the dependent variables are the log share of vagrants in a constituency, and the log share of people employed in unskilled jobs. Import competition was associated with negative outcomes in local labor markets: the fraction of vagrants increased, as did the share of people employed in unskilled jobs. This evidence is consistent with a theoretical account in which German imports cause reductions in employment in import-affected industries, pushing workers either out of the labor force entirely—into the vagrants category—or into unskilled jobs. Models (1) and (2) suggest a 1 pound increase in imports per worker was associated with a 15% relative increase in vagrancy, (5) and (6) suggest such an increase was associated with a roughly 1.5% relative increase in the share of employment in unskilled jobs.⁶ These results are robust to the inclusion of controls for 1881 manufacturing interacted with year

⁶As an additional robustness test of the economic effect of the trade shock, we report results in Table A-2 showing a negative effect on the average economic status of constituency occupations as measured by the HISCAM Project (Lambert et al., 2013).

dummies, and to the addition of constituency-specific time trends, which make it more plausible that the parallel trends assumption holds. Additionally, in Appendix B we show that these results are robust to controlling for initial shares in key industries interacted with year dummies, and to controlling for the first three principal components of the matrix of 1881 industry shares, which account for 84% of the variance in those shares, interacted with year dummies.

6 Political Responses to the German Trade Shock

We now examine the effects of German import competition on political outcomes. We find that import competition reduced vote share for the Conservative Party, and increased it for the Liberal and Labour parties, but only after 1900. Table 3 documents the main electoral effects, regressing the Conservative and Unionist share of the vote on ΔIPW over different periods. While there was essentially no association between import competition and vote share for the Conservative Party over the entire 1885–1910 period (1 and 2), the association between these variables varied over the period. From 1885–1900, we find a positive correlation between imports per worker and Conservative vote share. While the positive coefficient in model (3) could be taken as evidence that German imports increased vote share for the more protectionist party, we are wary of drawing strong conclusions from this result. Adding controls for initial manufacturing shares interacted with year dummies results in a smaller and statistically insignificant coefficient in model (4), suggesting that the effect in model (3) may be picking up changes in voting patterns in industrial areas unrelated to the trade shock. We find stronger evidence for a negative effect of the trade shock on Conservative vote share in the 1900–1910

Table 2: Effects of import competition on local economies

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\Delta \ln \% \text{ vagrants}$			$\Delta \ln \% \text{ unskilled jobs}$				
ΔIPW_t	0.150*** (0.032)	0.143*** (0.038)	0.077*** (0.028)	0.373*** (0.086)	0.014*** (0.005)	0.018*** (0.006)	0.016** (0.007)	0.017*** (0.004)
Controls	x	x	x	x	x	x	x	x
Initial Mf x year			x				x	
Constituency trends				x				x
Observations	1,389	1,389	1,389	1,389	1,389	1,389	1,389	1,389
R ²	0.860	0.883	0.893	0.900	0.022	0.118	0.121	0.585
Adjusted R ²	0.860	0.882	0.893	0.849	0.020	0.113	0.115	0.374

Note:

*p<0.1; **p<0.05; ***p<0.01

Stacked first difference estimates, at the constituency level, for 1880–1890, 1890–1900, 1900–1910. All models include year fixed effects. (2)–(4) and (6)–(8) add controls for lagged manufacturing employment, lagged fraction in unskilled jobs, lagged fraction of vagrants, and lagged average economic status; (3) and (7) include 1880 manufacturing employment interacted with year dummy variables, (4) and (8) include constituency fixed effects, which adjust for constituency-specific time trends. Standard errors clustered by county in parentheses.

period. In model (5), we find that a 1 pound increase in imports per worker was associated with a roughly 2 percentage point decrease in Conservative vote share over this period. In 15% of constituency races from 1900–1910, the difference between the Conservative and Liberal or Labour vote share was smaller than this difference. This effect is robust to the addition of manufacturing by year controls, and to the addition of time-varying controls for specific industries, and for the 1881 industry shares PCA (Table A-9). Tables A-6 and A-7 switch the dependent variable from Conservative vote share to vote share for other parties, and confirm the pattern of results from the Conservative vote shares. Import competition was associated with increased vote share for the Labour party, and, post-1900, with increased vote share for the combined Liberals and Labour, and for the Liberals in seats uncontested by Labour.⁷

In Table 4 we show the results of equivalent first-difference regressions of Conservative vote share on import competition for the 1885–1892, 1892–1900 and 1900–1910 waves. These long-difference regressions provide a closer political analogue to our regressions examining the effects of import competition on labour market outcomes, but at the cost of arbitrarily throwing out a large fraction of our data. While we are more confident in the results from the regressions in levels, we note that results from either method are quantitatively similar. In models (5) and (6), we add leads of the shock variable, examining the additional effect of the shock over the next decade on Conservative voting, once controlling for the current-period

⁷One consideration is whether variation in union activity across constituencies is either driving or moderating these results. In Table A-10, we address these questions using data on unionization by county. We find some evidence that the effect of the import competition on support for the Conservatives was stronger in more unionized areas, but note that this evidence is not consistent across specifications. Controlling for unionization interacted with period dummy variables attenuates our coefficients somewhat, but does not change their substantive or statistical interpretation.

Table 3: Effects of import competition on voting

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ΔIPW_{1885}	-0.003 (0.004)	-0.003 (0.002)	0.014** (0.006)	0.008 (0.006)				
ΔIPW_{1900}					-0.021*** (0.006)	-0.016*** (0.005)	-0.018*** (0.004)	-0.018*** (0.005)
Years	All	All	1885-1900	1885-1900	1900-1910	1900-1910	1900-1910	1900-1910
Initial MF x election		x		x		x		x
Matched panel							x	x
Observations	3,133	3,133	1,860	1,860	1,578	1,578	684	684
R ²	0.709	0.712	0.720	0.723	0.834	0.837	0.792	0.796
Adjusted R ²	0.657	0.661	0.626	0.629	0.765	0.768	0.720	0.723

Note: *p<0.1; **p<0.05; ***p<0.01

Constituency-level fixed effects regression, dependent variable is share of the vote for the Conservative Party. All models include constituency and election fixed effects, (2) and (4), (6), and (8) add manufacturing employment in 1880 interacted with election dummies. (7) and (8) use a panel matched on Conservative vote share in 1885, 1892, and 1900. Standard errors clustered by county in parentheses.

shock. Our estimated coefficients for the future shock are very close to zero and not statistically significant. This finding is consistent with the parallel trends assumption holding, in that it suggests that areas differentially affected by decade-specific changes in the trade shock were not trending in different directions prior to the shock.

Our results suggest that the trade shock increased the share of the vote for left-of-center parties in the 1900–1910 period, but was associated with a mild shift away from those parties in the preceding period. These differential trends may suggest that our estimates for the 1900–1910 period constitute a lower bound: if certain constituencies were trending towards the Conservatives from 1885 to 1900, and then reversed direction, the effect of the trade shock relative to a continued trend towards the Conservatives would be larger than the effect we estimate. However, a plausible concern is that our estimates for 1900–1910 reflect some form of mean-reversion after an outsized shift to the Conservatives. As an additional robustness check we use matching to create a panel of constituencies following a similar trend in Conservative voting from 1885–1900. We divide constituencies into two groups according to the incidence of the 1900–1910 trade shock, and then match on 1885, 1892, and 1900 Conservative vote share. We discard pairs which differ by more than 0.1 standard deviations in 1900 Conservative vote share, and apply a looser cutoff to the 1885 and 1892 vote shares. The idea is not to use matching to provide causal inferences within a selection-on-observables framework, but rather to create a panel which more plausibly satisfies the parallel trends assumption. Replicating the 1900–1910 difference in differences regressions of Conservative vote share on import competition in Table 3, models (7) and (8), we find a slightly smaller, but comparable and statistically

Table 4: First-difference effects of import competition on voting

	(1)	(2)	(3)	(4)	(5)	(6)
ΔIPW_t	0.018** (0.007)	0.016** (0.008)	-0.027*** (0.009)	-0.020*** (0.007)	0.017 (0.010)	0.017 (0.011)
ΔIPW_{t+1}					0.001 (0.007)	-0.002 (0.009)
Years	1885-1900	1885-1900	1900-1910	1900-1910	1885-1900	1885-1900
Initial Mf x election		x		x		x
Observations	712	712	578	578	712	712
R ²	0.015	0.017	0.072	0.086	0.015	0.017
Adjusted R ²	0.013	0.011	0.069	0.079	0.011	0.010

Note: *p<0.1; **p<0.05; ***p<0.01

Constituency-level stacked first-difference regressions, for waves 1885-1892, 1892-1900, 1900-1910 (note there were two elections in 1910). Dependent variable is change in share of the vote for the Conservative Party. All models include constituency and election fixed effects, (2), (4) and (6) add controls for manufacturing employment in 1880 interacted with election dummies. Standard errors clustered by county in parentheses.

significant effect, of -1.8 percentage points. Figure 5 shows this strategy, comparing the average Conservative vote shares over time between constituencies more and less affected by the 1900–1910 trade shock: while the matched constituencies follow the same trajectory prior to 1900, they subsequently diverge, and Conservative support falls more sharply in constituencies affected worse by the trade shock.

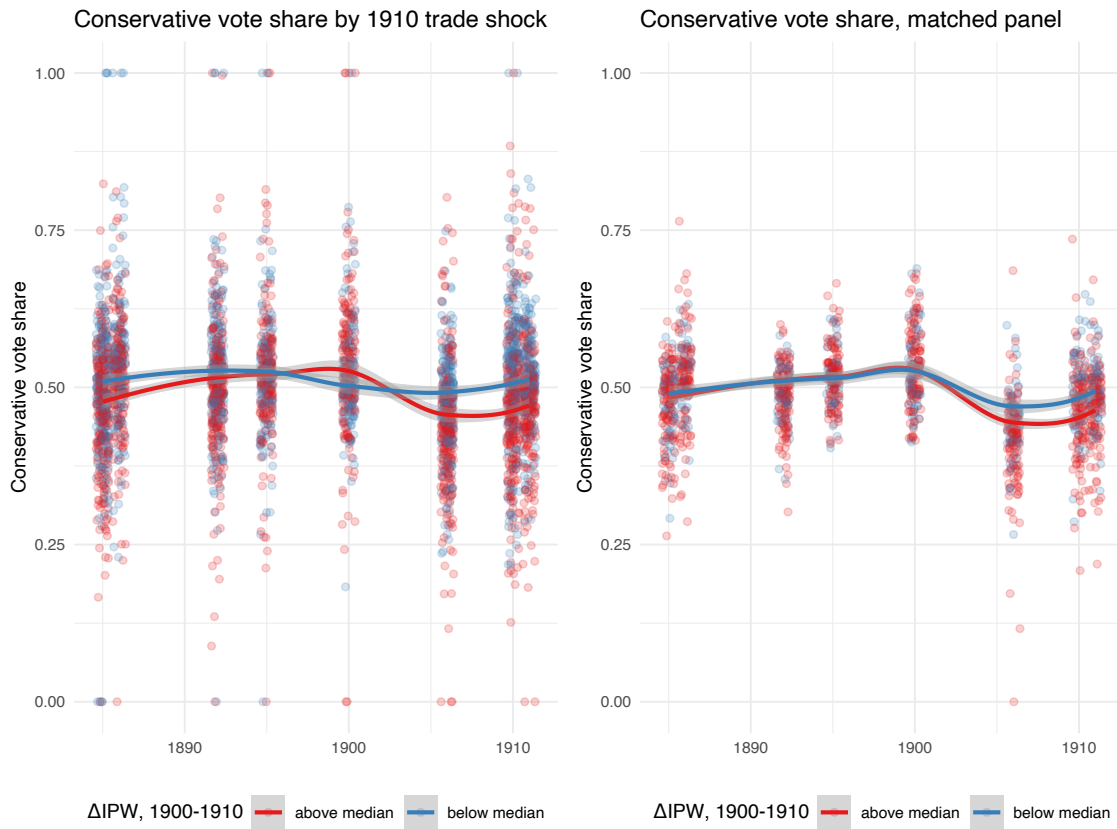


Figure 5: Conservative vote share by 1910 Δ IPW, with matched panel

As illustration of this matching process, consider the following matched pair. Henley, in Oxfordshire, and Norfolk South Western, were both fairly agricultural constituencies (20% and 24%, respectively were employed in agriculture in 1881), with similar manufacturing shares (11.4% and 10.5% in 1881). They both experi-

enced a gradual drop in Conservative support over the 1885–1900 period, in Henley from 53.7% in 1885 to 51.2% in 1900, in Norfolk South Western from 52.0% in 1885 to 50.4% in 1900. Yet their paths diverged after 1900. The constituencies differed in that Norfolk South Western had higher employment in the lace industry, which experienced a sharp increase in German imports after 1900. Both constituencies shifted further away from the Conservatives in 1906, but the shift was sharper in Norfolk South Western, to 44.3% as opposed to 47.0% of the vote in Henley. While the Conservative vote recovered in Henley in 1910, in a series of elections fought on issues of class and landowning privileges, to 59.1% in December, the recovery in Norfolk was much weaker, at 47.3% of the vote.

7 Interpretation

Having established that the German trade shock increased support for left-of-center parties after 1900, we now discuss mechanisms which may account for this effect. Prior research has emphasized a number of different mechanisms through which trade-induced economic change may influence voting behavior (Margalit, 2019). The most straightforward potential effect is that voters who are negatively impacted by increased trade want less trade and turn to protectionist candidates and parties (Che et al., 2016). Our main finding, that Conservatives post-1900 were electorally harmed by the trade shock, rules out the idea that import competition led to increased protectionism in this case. The Liberals were unified in support of free trade while the Conservatives at this time were split on trade with important figures in the party such as Joseph Chamberlain advocating for preferential tariffs to protect British industry.

In this section, we focus on evaluating three alternative voter reactions: negative trade shocks reduce support for incumbents, economic decline from trade induces value change, and the compensation effect. We find little support for the incumbency effect theory. Our results do suggest that the trade shock increased anti-immigrant sentiment. Nonetheless, this mechanism cannot explain the shift away from the Conservative party in trade-affected areas because Conservatives were more likely to share these anti-immigrant views. Finally, we find considerable evidence consistent with the compensation theory.

7.1 Incumbency and Economic Voting

There is little evidence that an anti-incumbency effect accounts for our results. Table 5 examines the effect of import competition on three different measures of incumbency. In models (1) and (2) the dependent variable is the share of the vote won by incumbent MPs; in (3) and (4) it is the share of the vote won by the local incumbent party, that is, the party that won the constituency in the last election. Models (5) and (6) are estimated in first differences, and examine changes in the vote won by the nationally-incumbent party.⁸ Coefficients across these specifications are close to zero. The one statistically significant result, in model (3), suggests that if anything there was a positive incumbency effect for local parties. It is important to emphasize that these models only control for constituency and year fixed effects, the former of which are differenced out in models (5) and (6), and that the marginally significant result in model (3) does not hold up in model (4), which is restricted to the post-1900 period. We are thus hesitant to interpret the positive coefficient in

⁸For context, the Liberal Party was the incumbent government for the elections held in 1886 and 1910 (both). The Conservative Party was the incumbent in the other elections.

model (3) as evidence of an increased positive incumbency effect, and conclude that the trade shock had no effect on support for incumbents.

Table 5: Effects of import competition on incumbency

	MP		Local Party		National Party	
	(1)	(2)	(3)	(4)	(5)	(6)
ΔIPW_{1885}	0.002 (0.007)		0.010* (0.005)			
ΔIPW_{1900}		0.009 (0.025)		0.001 (0.007)		
ΔIPW_t					0.004 (0.003)	-0.0004 (0.004)
Years	All	1900-1910	All	1900-1910	All	1900-1910
Observations	3,133	1,578	3,133	1,578	2,025	1,098
R ²	0.336	0.434	0.500	0.486	0.230	0.183
Adjusted R ²	0.219	0.198	0.412	0.272	0.228	0.181

Note: *p<0.1; **p<0.05; ***p<0.01

Constituency-level regressions, (1)–(4) are estimated in levels and include constituency and year fixed effects, (5) and (6) in stacked first-differences, and include year fixed effects. For (1) and (2) the dependent variable is the share of the vote won by incumbent MPs, for (3) and (4), the share of the vote won by incumbent parties at the local level, for (5) and (6), the change in voteshare by the nationally-incumbent party. Standard errors clustered by county in parentheses.

7.2 Xenophobia

We next consider a different potential effect of the German trade shock: increased xenophobia. In the 1900s the British government began to regulate immigration. The Conservative government in 1905 introduced the Aliens Act, which defined cat-

egories of undesirable immigrants and gave the state power to exclude them. The act mainly excluded Jewish immigrants from Eastern Europe. We examine the association between import competition and demand for immigration restriction using both Conservative parliamentary candidates' appeals, and newspaper coverage. We employ the same empirical setup as in the previous sections, regressing standardized measures of references to specific terms on ΔIPW , and control in addition for the initial immigrant population interacted with year dummies. Table 6 reports the results of the regressions using Conservative campaign manifestos. Import competition was associated with increased references to immigrants, aliens, and Jews, results consistent with Conservative candidates prioritizing immigration restriction in trade-affected constituencies.

The context in which these terms were used was often pejorative. The Conservative candidate in Aston Manor in 1906 argued that the Conservatives deserved credit for “the Aliens Act, which prevents the infection of our cities by criminal, diseased and pauper aliens.” Conservative candidates often linked immigration restriction to trade restriction. The 1906 Conservative candidate in Norwich noted of the Aliens Act “That useful measure deals with undesirable foreigners. The unfair competition by foreign goods remains to be dealt with in a similar manner.” Table 7 reports the results of equivalent regressions using news coverage. News coverage of immigration increased with the trade shock, although the language of such coverage was different from the language used by Conservative parliamentary candidates. There was a weak and statistically insignificant effect of the trade shock on newspaper references to “aliens,” a technical terms used mainly in the manifestos in conjunction with the 1905 act, but a robust effect on references to “foreigners.”⁹

⁹We also considered whether the trade shock had a more directed impact on attitudes about

Table 6: Effect of local trade shocks on references to immigration in Conservative campaign manifestos

	“immigrant” (1)	(2)	“alien” (3)	(4)	“jew” (5)	(6)	“foreigner” (7)	(8)	All (9)	(10)
ΔIPW_{1885}	0.059* (0.033)	0.087*** (0.031)	0.084* (0.050)	0.215*** (0.060)	0.069* (0.038)	0.095* (0.056)	-0.006 (0.065)	0.025 (0.075)	0.061 (0.071)	0.165** (0.080)
Initial immigrants x year	x	x	x	x	x	x	x	x	x	x
Initial Mf x year		x		x		x		x		x
Observations	2,679	2,679	2,679	2,679	2,679	2,679	2,679	2,679	2,679	2,679
R ²	0.183	0.183	0.296	0.306	0.217	0.218	0.325	0.326	0.343	0.348
Adjusted R ²	0.008	0.007	0.145	0.157	0.050	0.049	0.181	0.180	0.203	0.207

Note: *p<0.1; **p<0.05; ***p<0.01

Manifesto-level regressions. Dependent variable is number of uses of specified term relative to total length of manifesto, by Conservative candidates, standardized. All models include constituency and election fixed effects. Standard errors clustered by county in parentheses.

Table 7: Effect of local trade shocks on newspaper coverage of immigration

	“immigrant” (1)	(2)	“alien” (3)	(4)	“jew” (5)	(6)	“foreigner” (7)	(8)	(9)	All (10)
ΔIPW_{1885}	0.054*	0.045 (0.039)	0.037 (0.027)	0.029 (0.033)	0.068* (0.036)	0.028 (0.037)	0.118*** (0.025)	0.116*** (0.029)	0.123*** (0.024)	0.111*** (0.029)
Initial immigrants x year	x	x	x	x	x	x	x	x	x	x
Initial Mf x year	x	x	x	x	x	x	x	x	x	x
Observations	2,365	2,365	2,365	2,365	2,365	2,365	2,365	2,365	2,365	2,365
R ²	0.652	0.655	0.705	0.705	0.804	0.805	0.779	0.781	0.798	0.799
Adjusted R ²	0.561	0.563	0.627	0.627	0.752	0.753	0.721	0.722	0.745	0.746

Note: *p<0.1; **p<0.05; ***p<0.01

Newspaper-level regressions. Dependent variable is number of uses of specified term per newspaper issue, standardized. All models include newspaper and year fixed effects. For newspapers in cities, ΔIPW is calculated at the city-, not constituency-level. (9) and (10) use mentions of all four terms. Standard errors clustered by county in parentheses.

This evidence suggests that the German trade-shock did increase xenophobia. But this effect cannot explain our main results showing that the trade shock decreased support for the Conservative Party. As shown here, the shock pushed Conservative candidates to make xenophobic appeals and it was the Conservative government that enacted the Aliens Act. On average, this mechanism was apparently not important enough to make the trade shock advantage Conservatives. It does raise the possibility that the trade shock polarized voters in a way that resonates with Autor et al. (2017)’s analysis of the China shock and US voting behavior, but we leave this question for future research and focus on why the German trade shock on balance pushed voters toward left-of-center parties after 1900.

7.3 Voter Concern about German Trade

The compensatory mechanism can operate with or without voters explicitly connecting trade to poor economic outcomes. Voters could simply observe greater economic volatility or high unemployment and demand that the state take a more active role in the economy without connecting trade as a cause of the economic outcomes. That said, evidence that import competition is correlated with increased attention to trade at the local level explicitly maps the trade shock to voter concerns and makes a compensatory explanation of our electoral results as a response to trade more plausible. We regress a standardized measure of the per-issue references to different trade-related terms on ΔIPW , with newspaper and year fixed effects, and

foreigners that was focused on Germany. We find that newspapers were more likely to reference “Germany” and an index of German terms including “germany,” “kaiser,” “teuton,” “prussia,” and “fatherland” but that this concern did not induce greater reference to words associated with the navy or military organizations, which in turn might indicate a more aggressive set of foreign policies toward Germany (see Tables A-11 and A-28). We find weaker evidence of this same pattern in the manifesto data (see Tables A-12 and A-29).

time-varying manufacturing controls. Table 8 shows the results of these regressions. Over the whole period, import competition was associated with increased references in newspapers to trade and imports. The coefficient magnitudes suggest a 1 pound increased in imports per worker was associated with a 0.1 standard deviation increased in coverage, comparable to the results from the economic and voting regressions. The effect is driven by the 1900–1910 period (models (3), (4), (7), (8)). This result is plausible given the timing of the German trade shock: imports accelerated in the mid 1890s and grew steadily in the 1900s. One might not expect a strong political response after five years of heightened import competition, but one would expect such a response after fifteen. This result also adds credence to our political finding of null results prior to 1900, but strong results after.

7.4 Support for the Neo-Welfare State

An important piece of evidence that favors the compensation mechanism is that import competition increased support for the Liberal party after it adopted a program of increased redistribution and state intervention in the economy. The contents of parliamentary candidates' appeals provide additional evidence that import competition led to increased demands for compensatory policy. By 1900 power in British politics was centralized in the cabinet, and voters endorsed parties rather than specific candidates (Cox, 1987). Candidates could however tailor their appeals to local demand, by choosing which issues to prioritize. We regress a normalized measure of references to specific policy-related terms in Liberal manifestos on ΔIPW . We focus on three terms, “social reform,” which was used to refer to broadly to social policy, “poor law,” the punitive system of welfare which Liberal governments in the 1900s

Table 8: Effects of import competition on newspaper references to trade

	“import”			“trade”				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ΔIPW_{1885}	0.089*** (0.031)	0.100*** (0.034)			0.146*** (0.034)	0.111*** (0.033)		
ΔIPW_{1900}			0.198*** (0.048)	0.192*** (0.056)			0.253*** (0.048)	0.206*** (0.062)
Years	All	All	1900–1910	1900–1910	All	All	1900–1910	1900–1910
Initial Mf x year		x		x		x		x
Observations	2,365	2,365	962	962	2,365	2,365	962	962
R ²	0.778	0.779	0.859	0.860	0.786	0.789	0.875	0.878
Adjusted R ²	0.721	0.722	0.752	0.753	0.731	0.733	0.781	0.785

Note: *p<0.1; **p<0.05; ***p<0.01

Newspaper-level regressions. Dependent variable is number of uses of specified term per newspaper issue, standardized. All models include newspaper and year fixed effects. For newspapers in cities, ΔIPW is calculated at the city-, not constituency-level. Standard errors clustered by county in parentheses.

promised to reform, and “labour exchange,” a proposed policy to deal with unemployment due to economic fluctuations. These policies sought to address hardships endured by adult unemployed workers, those affected by import competition. Table 9 shows a consistent positive association between import competition and Liberal candidates mentioning these phrases.

The link between the trade shock and support for the welfare state can run through two channels. It can directly increase support for the welfare state by causing people to update their belief about the risk of unemployment. If one’s expected probability of being unemployed increases, one would support unemployment insurance more. Import competition can also indirectly increase support for the welfare state by changing beliefs about the unemployed. People may support welfare for the deserving poor, but be reluctant to support programs which also benefit shirkers. An exogenous increase in unemployment could change people’s beliefs about the types of people being benefitted by welfare. International trade seems a strong candidate for just this sort of exogenous factor because it has a foreign origin that may be more credibly viewed as outside the control of those affected by it.

There is qualitative evidence, in debates about unemployment, that such a shift in attitudes occurred in early 20th century Britain. Beveridge (1910), later the architect of the welfare state, argued that unemployment, “the problem of the adjustment of the supply of labour and the demand for labour” (p. 4) was the product of technical change, “fluctuations of industrial activity” (p. 13), and the need for excess labour for industries to hire in boom periods. While acknowledging that the least productive workers may be more likely to be unemployed, Beveridge noted that “The best and most regular of workmen may in a changing world find

Table 9: Effect of local trade shocks on references to social reform in Liberal campaign manifestos

	“social reform” (1)	(2)	“poor law” (3)	(4)	“labour exchange” (5)	(6)	All (7)	(8)
ΔIPW_{1885}	0.094** (0.037)	0.062* (0.034)	0.071** (0.032)	0.067** (0.026)	0.079*** (0.026)	0.088*** (0.027)	0.124*** (0.035)	0.101*** (0.031)
Initial Mf x year	x	x	x	x	x	x	x	x
Observations	2,121	2,121	2,121	2,121	2,121	2,121	2,121	2,121
R ²	0.314	0.320	0.268	0.271	0.457	0.458	0.362	0.365
Adjusted R ²	0.126	0.131	0.067	0.069	0.309	0.307	0.187	0.188

Note:

*p<0.1; **p<0.05; ***p<0.01

Manifesto-level regressions. Dependent variable is number of uses of specified term relative to total length of manifesto, by Liberal candidates, standardized. All models include constituency and election fixed effects. Standard errors clustered by county in parentheses.

himself exceptionally unemployed. New method and new machines often render whole classes of labour useless. ... good, bad and indifferent alike are thrown upon the market” (p. 142). The prevalence of unemployment was thus distinct from the moral character of the unemployed. The concept of “unemployment” as distinct from vagrancy entered common usage at this time. This sharp break can be seen in Figure 6, which plots references to “unemployment,” “vagrancy,” and “pauperism” in the *Times* newspaper over the period.



Figure 6: References to unemployment, vagrancy, and pauperism in the *Times*

This attitudinal shift was linked to the incidence of the trade shock. Table 10 examines the link between import competition and the use of terms related to this new concept of unemployment in newspapers. It shows the results of newspaper-level regressions in which the dependent variable is the number of references to “unemployment,” “employment” and the “unemployed,” minus the number of references to “pauper”(s), “pauperism,” “vagrant”(s), and “vagrancy.” Positive coefficients across

specifications suggest that coverage of the economic effects of the trade shock focused on the morally-neutral phenomenon of unemployment, not morally-charged notions of vagrancy and pauperism. In the appendix we employ a more principled approach, and use natural language processing methods to identify terms connected with the new concept of unemployment relative to older notions of pauperism. We find a similar effect of import competition on newspaper usage of terms connected to this new concept of unemployment in Table A-13.

Table 10: Effects of import competition on newspaper references to unemployment, vagrancy, and pauperism

	(1)	(2)	(3)	(4)
ΔIPW_{1885}	0.095** (0.036)	0.073* (0.039)		
ΔIPW_{1900}			0.204*** (0.063)	0.170** (0.077)
Years	All	All	1900–1910	1900–1910
Initial Mf x year		x		x
Observations	2,365	2,365	962	962
R ²	0.706	0.709	0.791	0.794
Adjusted R ²	0.630	0.633	0.632	0.636

Note: *p<0.1; **p<0.05; ***p<0.01
Newspaper-level regressions. Dependent variable is the number of references to “unemployed,” “unemployment,” and “employment,” minus the number of references to “vagrants,” “vagrancy,” “pauper,” and “pauperism,” standardized. All models include newspaper and year fixed effects. For newspapers in cities, ΔIPW is calculated at the city-, not constituency-level. Standard errors clustered by county in parentheses.

8 Conclusion

We examine the economic and political effects of rising German imports in late 19th and early 20th century Great Britain. We find that the German trade shock increased the prevalence of vagrancy and employment in low-skilled occupations during the full study period of 1880 to 1910 and decreased electoral support for the Conservative Party after 1900. We note that the timing of when exposure to increasing imports had a differential effect on voting patterns coincides with when the Liberal Party started to advocate social reforms and investment in Britain's neo-welfare state. We provide evidence that trade shocks were correlated with Liberal candidate manifesto mentions of social reform, bolstering our interpretation that the left-of-center shift in trade-impacted constituencies reflects increased demand for social welfare spending. Our results suggest this compensation mechanism was driven by two considerations: the German trade shock increased assessments of how volatile employment is in a market economy and therefore how much social insurance was optimal and it changed elite beliefs about the deservingness of the poor, transforming vagrants into the unemployed, which in turn increased support for welfare state development.

These results on trade and the origin of the neo-welfare state resonate with a large literature on compensation theory including Cameron (1978) and Rodrik (1998) as well as research on the role of embedded liberalism in establishing the liberal international order after World War II (Ruggie, 1982). It is notable that some of the more recent research on the political consequences of China's integration with the world economy shows some political responses that are also left-of-center as we show here (Che et al., 2016). But a great deal of this research records a response to

trade that is more protectionist and skeptical of government's role in the economy (Margalit, 2019; Colantone and Stanig, 2018*b,c*; Hays, Lim and Spoon, 2019; Milner, 2021).

What accounts for this variation across individuals, regions, countries, and time periods in the political effects of openness? Future research is needed to construct a compelling comprehensive answer to this question. A speculative answer that focuses on the context of British politics in the first decade of the 20th century might emphasize three key points of contrast to the political economy setting of 21st century advanced industrial democracies. First, progressive reforms in the 20th century promised to have a relatively significant marginal impact because they were added to a minimal state and promised to ameliorate some of the worst aspects of laissez-faire capitalism. Second, the 21st century context was one in which the state was perceived to have failed to set policies that ensured that the gains from globalization were widely shared while at the end of 19th and early 20th century the idea that the state was responsible for such outcomes was just beginning to take hold. Third, differences in income levels in the two periods may have influenced the weight of labor market costs and consumer benefits associated with increased trade. Free trade in early 20th century Britain was first and foremost associated with cheaper food prices and this was central to Liberal Party arguments against proposed protectionism and in favor of social reform to deal with labor market dislocation. While consumer considerations are certainly relevant in the modern context and have been shown to be important in attitudes about trade in the developing world (Baker, 2003), it is not clear that they have same political resonance in contemporary debates in developed democracies.

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Online Appendix for “The German Trade Shock and The Rise of the Neo-Welfare State in Early 20th Century Britain”

A Additional information on trade shock measures

Table A-1: Industry categories

Industry	1881 Employment	Δ IPW (1885-1910)
Apparel And Haberdashery	587,889	2.376
Coal Coke And Patent Fuel	381,825	-0.009
Cotton Manufactures	290,772	17.322
Shoes	209,525	0.159
Cotton Yarn	186,136	-0.754
Machinery	172,153	9.483
Wool Manufactures	139,740	5.556
Iron Manufactures	129,884	12.342
Printed Matter	95,949	0.322
Hats	85,334	0.411
Wood Products	83,723	0.800
Sheet Iron And Steel	67,794	51.355
Carriages	55,182	0.254
Silk Manufactures	53,361	-9.635
Wool Yarn	47,485	5.997
Stone	42,543	5.075
Lace	42,406	18.281
China And Earthenware	42,320	6.408
Leather Manufactures	42,015	10.029
Paper	34,895	57.654
Beer	33,438	2.814
Hardware And Cutlery	29,569	35.075

Brass Manufactures	28,273	4.284
Fish	26,667	-1.204
Iron Ore	26,072	0.483
Leather	25,327	37.490
Dairy	24,430	-77.293
Clocks And Watches	23,345	3.241
Glass	21,963	55.714
Art	21,291	15.661
Plaiting Of Straw	16,320	19.085
Chemicals	15,360	77.730
Bristles And Brushes	15,170	27.145
Gloves	14,926	22.578
Implements And Tools	12,859	6.689
Linen	12,850	108.301
Tin Ore	12,807	1.109
Silk Yarn	11,715	26.929
Lead Ore	11,607	0.000
Arms And Ammunition	11,355	-3.686
Slate	10,824	0.000
Cordage	10,716	17.905
Tobacco Manufactures	10,528	4.988
Jewellery	9,257	34.175
Musical Instruments	7,787	29.650
Umbrellas And Sticks	7,363	4.252
Dyes And Paints	7,077	268.612
Skins And Furs	7,071	256.483
Electricals	7,010	198.564
Buttons	5,976	12.485
Meat	5,087	98.880
Soft Drinks	4,809	30.954
Artificial Flowers	4,800	68.442
Oil Seed And Oil Cake	4,790	61.247
Scientific Instruments	4,767	89.464
Alkali	4,634	16.585
Sand Flint Clay Gravel Chalk	4,552	13.875
Chocolate	4,501	133.220
Copper Ore	4,341	1.213

Matches	4,266	31.966
Sheet Copper	4,143	12.086
Toys	4,136	139.194
Copper Manufactures	3,721	81.646
Cement	3,670	19.297
Refined Sugar	3,443	500.000
Candles And Grease	3,395	74.711
Fancy Goods	3,384	137.369
Lamps	3,221	5.059
Tobacco Pipes	3,175	14.261
Embroidery	2,668	500.000
Sheet Lead	2,468	-56.958
Soap	2,445	1.062
Jute Manufactures	2,205	54.536
Mats	1,989	11.924
Sheet Zinc	1,950	500.000
Manure	1,924	120.665
Rubber	1,923	495.493
Feathers	1,807	80.503
Tin	1,602	-9.023
Motor Cars	1,358	500.000
Sheet Gold Silver	1,333	-27.131
Waterproof Goods	962	188.389
Bicycles	949	140.551
Mustard Vinegar Spice Pickle	924	18.902
Hay	902	43.167
Spirits	850	-32.727
Sheet Other Metals	774	-74.398
Silver Ore	682	-3.554
Floor Cloth And Oil Cloth	653	75.790
Jams And Sweets	515	500.000
Glue	399	500.000
Zinc Manufactures	203	500.000
Gold Ore	116	0.000
Gum	107	500.000

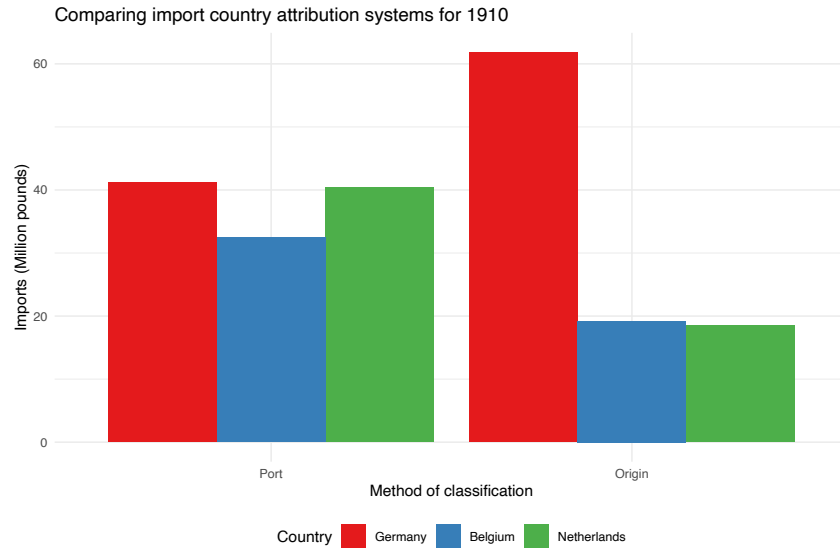


Figure A-1: Comparing value of imports by country according to pre- and post-1908 classification systems

B Additional regressions and robustness checks for economic regressions

As an additional measure, we compute the average economic status of people in the constituency, using occupational titles, and scores from the HISCAM project (Lambert et al., 2013). The HISCAM project uses historical data on the jobs of parents and their children to infer the relative social status of different occupations. The key assumption in constructing these status scores is that children tend to hold similar status jobs to those of their parents, and so if a pair of occupations occur frequently in parent-child pairs, those occupations are likely similar status. We use a version of the scores estimated from 19th century UK parish registers and genealogical data. Regressions using this variable are shown in Table A-2.

Table A-2: Effects of import competition on average economic status

	(1)	(2)	(3)	(4)
ΔIPW_t	-0.067** (0.026)	-0.073*** (0.026)	-0.047** (0.020)	-0.101*** (0.037)
Controls		x	x	x
Initial Mf x year			x	
Constituency trends				x
Observations	1,389	1,389	1,389	1,389
R ²	0.240	0.243	0.306	0.675
Adjusted R ²	0.239	0.240	0.302	0.510

Note: *p<0.1; **p<0.05; ***p<0.01
Stacked first difference estimates, at the constituency level, for 1880–1890, 1890–1900, 1900–1910. Dependent variable is change in average economic status. All models include year fixed effects. (2)–(4) add controls for lagged manufacturing employment and lagged average economic status; (3) includes 1880 manufacturing employment interacted with year dummy variables, (4) includes constituency fixed effects, which adjust for constituency-specific time trends. Standard errors clustered by county in parentheses.

Table A-3: Rotemberg weights for % vagrants regressions

No controls			Controls and Mf x year		
Industry	Year	Weight	Industry	Year	Weight
sheet iron and steel	1910	0.106	sheet iron and steel	1910	0.130
sheet zinc	1910	0.093	sheet zinc	1910	0.121
refined sugar	1900	0.065	refined sugar	1900	0.083
cotton manufactures	1910	0.064	refined sugar	1890	0.073
refined sugar	1890	0.055	sheet zinc	1890	0.063
hardware and cutlery	1910	0.047	refined sugar	1910	0.053
sheet zinc	1890	0.047	hardware and cutlery	1910	0.052
refined sugar	1910	0.042	gloves	1890	0.052
skins and furs	1910	0.037	wool manufactures	1910	0.050
gloves	1890	0.036	skins and furs	1910	0.036
cotton manufactures	1900	0.029	glass	1900	0.031
glass	1900	0.026	sheet copper	1890	0.028
lace	1910	0.023	lace	1910	0.021
sheet copper	1890	0.021	silk manufactures	1900	0.014
wool manufactures	1910	0.012	electricals	1910	0.013
electricals	1910	0.012	chemicals	1910	0.010
linen	1910	0.012	cotton manufactures	1910	0.010
dyes and paints	1910	0.011	linen	1890	0.009
chemicals	1910	0.010	linen	1910	0.009
jewellery	1910	0.010	jewellery	1910	0.009

Table A-4: Rotemberg weights for % unskilled jobs regressions

No controls			Controls and Mf x year		
Industry	Year	Weight	Industry	Year	Weight
sheet iron and steel	1910	0.106	sheet zinc	1910	0.133
sheet zinc	1910	0.093	sheet iron and steel	1910	0.131
refined sugar	1900	0.065	refined sugar	1900	0.072
cotton manufactures	1910	0.064	sheet zinc	1890	0.068
refined sugar	1890	0.055	refined sugar	1890	0.059
hardware and cutlery	1910	0.047	gloves	1890	0.059
sheet zinc	1890	0.047	hardware and cutlery	1910	0.055
refined sugar	1910	0.042	wool manufactures	1910	0.049
skins and furs	1910	0.037	refined sugar	1910	0.043
gloves	1890	0.036	skins and furs	1910	0.034
cotton manufactures	1900	0.029	sheet copper	1890	0.030
glass	1900	0.026	glass	1900	0.028
lace	1910	0.023	lace	1910	0.025
sheet copper	1890	0.021	cotton manufactures	1910	0.019
wool manufactures	1910	0.012	silk manufactures	1900	0.012
electricals	1910	0.012	electricals	1910	0.012
linen	1910	0.012	linen	1890	0.011
dyes and paints	1910	0.011	linen	1910	0.010
chemicals	1910	0.010	jewellery	1910	0.009
jewellery	1910	0.010	silk manufactures	1890	0.009

Table A-5: Robustness checks for economic variables, exposure-robust standard errors

	(1)	$\Delta \ln$ % vagrants		$\Delta \ln$ % unskilled jobs		(8)	
	(2)	(3)	(4)	(5)	(6)	(7)	
ΔIPW_t	0.175*** (0.062)	0.170** (0.077)	0.173*** (0.065)	0.134*** (0.051)	0.017*** (0.007)	0.020*** (0.007)	0.024*** (0.005)
Initial steel x year	x			x			
Initial zinc x year		x			x		
Initial sugar x year			x			x	
Initial shares PCA x year				x			x
First stage F-stat	23.2	15.7	6.4	17.2	23.2	6.4	17.2
Observations	285	285	285	285	285	285	285

Note:

*p<0.1; **p<0.05; ***p<0.01

Stacked first difference estimates, at the constituency level, aggregated to the industry level, for 1880–1890, 1890–1900, 1900–1910. All models include year fixed effects, and controls for lagged share unskilled, lagged manufacturing employment and lagged fraction of vagrants; (1) and (5) include the share of employment in 1881 in sheet iron and steel interacted with year fixed effects, (2) and (6) do the same for employment in sheet zinc, (3) and (7) the same for sugar. (4) and (8) add the first three principal components for the 1881 industry shares interacted with year fixed effects. Standard errors clustered by industry in parentheses.

C Additional regressions and robustness checks for voting regressions

Table A-6: Effects of import competition on voting for different parties

	(1)	Conservative		Labour		Liberal vote share		(8)
		(2)	(3)	(4)	(5)	(6)	(7)	
ΔIPW_{1885}	-0.003 (0.004)	0.014** (0.006)		0.014* (0.008)	-0.016*** (0.005)	-0.026*** (0.009)		
ΔIPW_{1900}			-0.021*** (0.006)				0.008 (0.007)	0.019*** (0.005)
Years	All	1885-1900	1900-1910	All	All	1885-1900	1900-1910	1900-1910
Excluding Labour								x
Observations	3,133	1,860	1,578	3,133	3,133	1,860	1,578	1,336
R ²	0.709	0.720	0.834	0.502	0.533	0.693	0.739	0.811
Adjusted R ²	0.657	0.626	0.765	0.414	0.451	0.590	0.631	0.721

Note: *p<0.1; **p<0.05; ***p<0.01

Constituency-level fixed effects regression, dependent variable is share of the vote for specified party. All models include constituency and election fixed effects. Model 8 excludes elections contested by Labour. Standard errors clustered by county in parentheses.

Table A-7: Effects of import competition on voting for combined Liberals and Labour

	(1)	(2)	(3)	(4)
ΔIPW_{1885}	-0.020*** (0.007)	-0.013 (0.008)		
ΔIPW_{1900}			0.020*** (0.005)	0.015*** (0.005)
Years	1885–1900	1885–1900	1900–1910	1900–1910
Initial MF x election		x		x
Observations	1,860	1,860	1,578	1,578
R ²	0.709	0.713	0.822	0.823
Adjusted R ²	0.611	0.616	0.748	0.748

Note:

*p<0.1; **p<0.05; ***p<0.01

Constituency-level fixed effects regression, dependent variable is combined share of the vote for the Liberal and Labour parties. All models include constituency and election fixed effects, (2) and (4) add the manufacturing employment in 1880 interacted with election dummies. Standard errors clustered by county in parentheses.

Table A-8: Rotemberg weights for post-1900 voting regressions

No controls			Initial Mf x election		
Industry	Year	Weight	Industry	Year	Weight
cotton manufactures	1910	0.095	lace	1906	0.120
cotton manufactures	1911	0.089	sheet iron and steel	1910	0.091
lace	1906	0.085	refined sugar	1910	0.091
sheet iron and steel	1910	0.085	refined sugar	1911	0.089
refined sugar	1910	0.071	sheet iron and steel	1911	0.069
refined sugar	1911	0.069	wool manufactures	1910	0.055
sheet iron and steel	1911	0.063	cotton manufactures	1910	0.045
skins and furs	1910	0.042	skins and furs	1910	0.043
skins and furs	1911	0.040	skins and furs	1911	0.041
hardware and cutlery	1910	0.028	wool manufactures	1911	0.040
hardware and cutlery	1911	0.027	cotton manufactures	1911	0.037
refined sugar	1906	0.016	hardware and cutlery	1910	0.027
linen	1910	0.016	hardware and cutlery	1911	0.025
sheet zinc	1910	0.015	refined sugar	1906	0.021
sheet zinc	1911	0.015	silver ore	1906	0.019
wool manufactures	1910	0.014	sheet zinc	1911	0.018
dyes and paints	1910	0.014	sheet zinc	1910	0.016
linen	1911	0.013	sheet zinc	1906	0.014
dyes and paints	1911	0.012	linen	1910	0.014
silver ore	1906	0.012	electricals	1910	0.012

Table A-9: Robustness checks for post-1900 voting regressions, exposure-robust standard errors

	(1)	(2)	(3)	(4)	(5)
ΔIPW_{1900}	-0.023*** (0.006)	-0.019*** (0.005)	-0.019*** (0.007)	-0.024*** (0.005)	-0.017*** (0.006)
Initial steel x year	x				
Initial cotton x year		x			
Initial sugar x year			x		
Initial lace x year				x	
Initial shares PCA x year					x
First stage F-stat	10.7	14.1	7.8	11.9	13.7
Observations	380	380	380	380	380

Note:

*p<0.1; **p<0.05; ***p<0.01

Constituency-level fixed effects regressions, aggregated to the industry level for exposure-robust standard errors, for 1900–1910. Dependent variable is share of the vote for Conservative candidates. All models include constituency and year fixed effects, (1) includes the share of employment in 1881 in sheet iron and steel interacted with year fixed effects, (2) does the same for employment in sheet zinc, (3) does the same for sugar, (4) does the same for lace. (5) adds the first three principal components for the 1881 industry shares interacted with year fixed effects. Standard errors clustered by industry in parentheses.

Table A-10: Moderating effect of unions on effect of import competition on voting

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ΔIPW_{1885}	0.009 (0.008)	0.008 (0.009)			-0.001 (0.002)	-0.009 (0.006)				
$\Delta IPW_{1885} \times \% \text{ Union}$	0.189 (0.131)	-0.195 (0.125)								
ΔIPW_{1900}			0.003 (0.014)	0.005 (0.014)			-0.011 (0.008)	-0.025*** (0.006)	-0.015*** (0.005)	-0.013** (0.005)
$\Delta IPW_{1900} \times \% \text{ Union}$			-0.429** (0.205)	-0.385* (0.203)						

Years	All	All	1900-1910	1900-1910	All	All	1900-1910	1900-1910	1900-1910	1900-1910	1900-1910
Union sub-sample	All	All	All	All	All	1H	2H	1H	All	All	All
Union x election										x	x
Initial MF x election		x									
Observations	3,134	3,134	1,578	1,578	1,564	1,570	785	793	1,578	1,578	1,578
R ²	0.704	0.708	0.837	0.839	0.702	0.679	0.827	0.822	0.841	0.841	0.842
Adjusted R ²	0.652	0.656	0.768	0.771	0.647	0.618	0.755	0.744	0.774	0.774	0.775

Note: *p<0.1; **p<0.05; ***p<0.01

Constituency-level fixed effects regression, dependent variable is share of the vote for the Conservative Party. Data on union membership relative to population in 1892 at the county level is taken from Sidney and Beatrice Webb, *The History of Trade Unionism* (London: Longmans, Green and Co., 1896). Models (5) and (7) are estimated for constituencies with above-median unionization, (6) and (8) for constituencies with below-median unionization. Models (9) and (10) replicate regressions from table 3, adding controls for unionization interacted with year dummy variables. All models include constituency and election fixed effects, Standard errors clustered by county in parentheses.

D Additional regressions using news and manifesto data

Table A-11: Effects of import competition on newspaper references to Germany and the naval race

	“germany” (1)	(2)	German terms (3)	(4)	Navy terms (5)	(6)	Militarist groups (7)	(8)
ΔIPW_{1885}	0.083** (0.038)	0.055 (0.033)	0.091** (0.036)	0.062* (0.035)	-0.004 (0.036)	0.009 (0.040)	-0.116* (0.062)	-0.033 (0.048)
Initial Mf x year	x	x	x	x	x	x	x	x
Observations	2,365	2,365	2,365	2,365	2,365	2,365	2,365	2,365
R ²	0.740	0.743	0.758	0.760	0.861	0.861	0.514	0.521
Adjusted R ²	0.673	0.676	0.695	0.697	0.825	0.825	0.389	0.395

Note: *p<0.1; **p<0.05; ***p<0.01

Newspaper-level regressions. Dependent variable is number of uses of specified term per newspaper issue, standardized. All models include newspaper and year fixed effects. For newspapers in cities, ΔIPW is calculated at the city-, not constituency-level. “German terms” are “germany,” “kaiser,” “teuton,” “prussia,” and “fatherland,” “Navy terms” are “navy,” “naval,” “dreadnought,” “battleship,” and “fleet,” “Militarist groups” are “national service league” and “navy league.” Standard errors clustered by county in parentheses.

Table A-12: Effects of import competition on manifesto references to Germany and the naval race

	“germany” (1)	(2)	German terms (3)	(4)	Navy terms (5)	(6)	Militarist groups (7)	(8)
ΔIPW_{1885}	0.038 (0.029)	0.058 (0.042)	0.034 (0.030)	0.054 (0.041)	-0.035 (0.027)	0.004 (0.022)	-0.041* (0.021)	-0.035 (0.024)
Initial Mf x year	x	x	x	x	x	x	x	x
Observations	5,147	5,147	5,147	5,147	5,147	5,147	5,147	5,147
R ²	0.146	0.149	0.145	0.148	0.356	0.358	0.104	0.106
Adjusted R ²	0.060	0.062	0.060	0.062	0.292	0.293	0.015	0.015

Note: *p<0.1; **p<0.05; ***p<0.01

Manifesto-level regressions. Dependent variable is number of uses of specified term relative to total length of manifesto, standardized. All models include constituency, party, and year fixed effects. “German terms” are “germany,” “kaiser,” “teuton,” “prussia,” and “fatherland,” “Navy terms” are “navy,” “naval,” “dreadnought,” “battleship,” and “fleet,” “Militarist groups” are “national service league” and “navy league.” Standard errors clustered by county in parentheses.

Table A-13 examines the link between import competition and the new notion of unemployment in more detail. For models (1) and (2) the dependent variable is a standardized measure of the use of a number of terms which were overused in Beveridge’s analysis of unemployment, relative to other writings supportive of the existing poor law system.¹⁰ Following Gentzkow and Shapiro (2010), we compute a χ^2 measure for each word, which gives the test statistic for the null hypothesis that the probability of the word being used is the same in both corpuses. We then use the χ^2 statistic to identify the most distinguishing terms, and select those terms most overused by Beveridge. The idea is to select terms which distinguish the new concept of unemployment as the product of economic frictions from the old concept of unemployment as the product of character defects. The terms selected by this method refer to industrial dislocation—“fluctuation,” “depression” and “cyclical”—and unemployment, as well as to the economy more broadly, and the industries Beveridge was concerned about, such as the docks. The trade shock was associated with a statistically significant within-newspaper shift towards the use of these terms, which is robust to the inclusion of manufacturing by year controls. The positive coefficient in models (1) and (2) is driven by attention to industrial dislocation and unemployment. This evidence supports the interpretation that updated perceptions of the risk of unemployment led to increased support for the welfare state, in suggesting the trade shock led to increased focus on economic risk. Yet it is also consistent with changing attitudes towards the unemployed: elite newspaper writers responded to an uptick in the prevalence of vagrants and casual laborers by reporting on the disruptive effects of impersonal market forces.

¹⁰The texts in question are Helen Bosanquet’s summary of the Poor Law Report of 1909 (1911), an anonymous criticism of the Poor Law Minority Report (1910), F.C. Montague’s *The Old Poor Law and the New Socialism* (1886), the Poor Law Commissioners’ Report (1834), *Self Help* by Samuel Smiles (1863), and William Dawson’s *The Vagrancy Problem* (1910)

Table A-13: Effects of import competition on newspaper references terms overused in Beveridge’s analysis of unemployment

	Beveridge terms	“fluctuation”	“depression”	“(un)employment”	“exchange”					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ΔIPW_{1885}	0.095*** (0.029)	0.101*** (0.028)	0.124*** (0.034)	0.116** (0.044)	0.137** (0.054)	0.159** (0.074)	0.061* (0.034)	0.062 (0.038)	0.124*** (0.037)	0.167*** (0.041)
Initial Mf x year		x	x	x	x	x	x	x	x	x
Observations	2,365	2,365	2,365	2,365	2,365	2,365	2,365	2,365	2,365	2,365
R ²	0.783	0.785	0.744	0.745	0.764	0.766	0.731	0.733	0.798	0.800
Adjusted R ²	0.727	0.728	0.678	0.678	0.704	0.704	0.661	0.663	0.745	0.747

Note: *p<0.1; **p<0.05; ***p<0.01

Newspaper-level regressions. Dependent variable is number of uses of specified term per newspaper issue, standardized. All models include newspaper and year fixed effects. For newspapers in cities, ΔIPW is calculated at the city-, not constituency-level. “Beveridge terms” refers to terms overused in Beveridge’s *Unemployment: A Problem of Industry*, relative to other contemporary writings supportive of the existing Poor Law system. Terms were selected using the Chi² test statistic proposed by Gentzkow and Shapiro (2010). The terms in question are “fluctuation,” “cyclical,” “dock,” “reserve,” “exchange,” “seasonal,” “depression,” “situation,” “percentage,” “unemployment,” “production,” “unemployed,” “unskilled,” “industrial,” “skilled,” “economic,” “organisation,” “table,” “trades,” “note,” “demand,” “supply,” “applicant,” and “distress.” Standard errors clustered by county in parentheses.

E Regressions using exposure-robust standard errors

Table A-14: Effects of import competition on local economies, exposure-robust standard errors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\Delta \ln$ % unskilled jobs							
ΔIPW_t	0.150*** (0.035)	0.143** (0.057)	0.077* (0.045)	0.373*** (0.096)	0.014** (0.007)	0.018*** (0.006)	0.016** (0.006)	0.017** (0.007)
Controls		x	x	x	x	x	x	x
Initial Mf x year			x				x	
Constituency trends				x				x
First stage F-stat	19.2	16.1	16.7	5.8	19.2	16.1	16.7	5.8
Observations	285	285	285	285	285	285	285	285

Note: *p<0.1; **p<0.05; ***p<0.01

This table replicates the results of Table 2 using the aggregation and standard error calculation methods recommended by Borusyak, Jaravel, and Hull (2018). Stacked first difference estimates, at the constituency level, aggregated to the industry level, for 1880-1890, 1890-1900, 1900-1910. All models include year fixed effects. (2)-(4) and (6)-(8) add controls for lagged manufacturing employment, lagged fraction in unskilled jobs, lagged fraction of vagrants, and lagged average economic status; (3) and (7) include 1880 manufacturing employment interacted with year dummy variables, (4) and (8) include constituency fixed effects, which adjust for constituency-specific time trends. Standard errors clustered by industry in parentheses.

Table A-15: Effects of import competition on voting, exposure-robust standard errors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δ PW	-0.003 (0.004)	-0.003 (0.006)	0.014** (0.006)	0.008 (0.008)	-0.021*** (0.006)	-0.016** (0.006)	-0.018*** (0.005)	-0.018** (0.007)
Years	All	All	1885–1900	1885–1900	1900–1910	1900–1910	1900–1910	1900–1910
Initial MF x election		x		x		x		x
Matched panel							x	x
First stage F-stat	11.5	14.3	5.4	7.2	12.2	13	16.5	15
Observations	760	760	475	475	380	380	380	380

Note: *p<0.1; **p<0.05; ***p<0.01

This table replicates the results of Table 3 using the aggregation and standard error calculation methods recommended by Borusyak, Jaravel, and Hull (2018) Constituency-level variables aggregated up to the industry level, dependent variable is share of the vote for the Conservative Party. All models include constituency and election fixed effects, (2) and (4), (6), and (8) add manufacturing employment in 1880 interacted with election dummies. (7) and (8) use a panel matched on Conservative vote share in 1885, 1892, and 1900. Standard errors clustered by county in parentheses.

Table A-16: First-difference effects of import competition on voting, exposure-robust standard errors

	(1)	(2)	(3)	(4)	(5)	(6)
Shock	0.018*	0.016	-0.027***	-0.020**	0.001	-0.002
	(0.009)	(0.010)	(0.009)	(0.009)	(0.007)	(0.008)
Shock variable	ΔIPW_t	ΔIPW_t	ΔIPW_t	ΔIPW_t	ΔIPW_{t+1}	ΔIPW_{t+1}
Years	1885-1900	1885-1900	1900-1910	1900-1910	1885-1900	1885-1900
Initial Mf x election		x		x		x
First stage F stat	9.9	13	15.8	14.4	12.8	16.9
Observations	190	190	190	190	190	190

Note: *p<0.1; **p<0.05; ***p<0.01

This table replicates the results of Table 4 using the aggregation and standard error calculation methods recommended by Borusyak, Jaravel, and Hull (2018). Stacked first-difference regressions, for waves 1885-1892, 1892-1900, 1900-1910 (note there were two elections in 1910), with constituency-level variables aggregated up to the industry level. Dependent variable is change in share of the vote for the Conservative Party. All models include constituency and election fixed effects, (2), (4) and (6) add controls for manufacturing employment in 1880 interacted with election dummies. Standard errors clustered by industry in parentheses, (5) and (6) control for ΔIPW for the correct period.

Table A-17: Effects of import competition on incumbency, exposure-robust standard errors

	MP		Local Party		National Party	
	(1)	(2)	(3)	(4)	(5)	(6)
Δ IPW	0.002 (0.007)	0.009 (0.025)	0.010** (0.004)	0.001 (0.008)	0.004 (0.004)	-0.0004 (0.004)
Years	All	1900-1910	All	1900-1910	All	1900-1910
First stage F-stat	11.5	12.2	11.5	12.2	17.2	11.5
Observations	760	380	760	380	570	285

Note:

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

This table replicates the results of Table 5 using the aggregation and standard error calculation methods recommended by Borusyak, Jaravel, and Hull (2018). Constituency-level variables aggregated to the industry level, (1)–(4) are estimated in levels and include constituency and year fixed effects, (5) and (6) in stacked first-differences, and include year fixed effects. For (1) and (2) the dependent variable is the share of the vote won by incumbent MPs, for (3) and (4), the share of the vote won by incumbent parties at the local level, for (5) and (6), the change in voteshare by the nationally-incumbent party. Standard errors clustered by industry in parentheses.

Table A-18: Effects of import competition on newspaper references to trade, exposure-robust standard errors

	“import”			“trade”				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δ IPW	0.089*** (0.023)	0.100*** (0.032)	0.198*** (0.058)	0.192** (0.080)	0.146*** (0.028)	0.111*** (0.031)	0.253*** (0.052)	0.206*** (0.067)
Years	All	All	1900–1910	1900–1910	All	All	1900–1910	1900–1910
Initial Mf x year		x		x		x		x
First stage F-stat	7.4	7.2	8.2	7.4	7.4	7.2	8.2	7.4
Observations	665	665	285	285	665	665	285	285

Note: *p<0.1; **p<0.05; ***p<0.01

This table replicates the results of Table 8 using the aggregation and standard error calculation methods recommended by Borusyak, Hull, and Jaravel (2018). Newspaper-level variables aggregated up to the industry level. Dependent variable is number of uses of specified term per newspaper issue, standardized. All models include newspaper and year fixed effects. For newspapers in cities, Δ IPW is calculated at the city-, not constituency-level. Standard errors clustered by industry in parentheses.

Table A-19: Effect of local trade shocks on references to social reform in Liberal campaign manifestos, exposure-robust standard errors

	“social reform” (1)	(2)	“poor law” (3)	(4)	“labour exchange” (5)	(6)	(7)	All (8)
ΔIPW_{1885}	0.094** (0.037)	0.062 (0.042)	0.071*** (0.026)	0.067* (0.036)	0.079** (0.035)	0.088* (0.045)	0.124*** (0.031)	0.101** (0.039)
Initial Mf x year	x	x	x	x	x	x	x	x
First stage F-stat	0.3	9.7	10.3	9.7	10.3	9.7	10.3	9.7
Observations	570	570	570	570	570	570	570	570

Note: *p<0.1; **p<0.05; ***p<0.01

This table replicates the results of Table 9 using the aggregation and standard error calculation methods recommended by Borusyak, Jaravel, and Hull (2018). Manifesto-level data aggregated to the industry level. Dependent variable is number of uses of specified term relative to total length of manifesto, by Liberal candidates, standardized. All models include constituency and election fixed effects. Standard errors clustered by industry in parentheses.

Table A-20: Effects of import competition on newspaper references to unemployment, vagrancy, and pauperism, exposure-robust standard errors

	(1)	(2)	(3)	(4)
Δ IPW	0.095*** (0.027)	0.073* (0.038)	0.204*** (0.074)	0.170 (0.105)
Years	All	All	1900–1910	1900–1910
Initial Mf x year		x		x
First stage F-state	7.4	7.2	8.2	7.4
Observations	665	665	285	285

Note: *p<0.1; **p<0.05; ***p<0.01
This table replicates the results of Table 10 using the aggregation and standard error calculation methods recommended by Borusyak, Jaravel, and Hull (2018). Newspaper-level variables aggregated to the industry level. Dependent variable is the number of references to “unemployed,” “unemployment,” and “employment,” minus the number of references to “vagrants,” “vagrancy,” “pauper,” and “pauperism,” standardized. All models include newspaper and year fixed effects. For newspapers in cities, Δ IPW is calculated at the city-, not constituency-level. Standard errors clustered by industry in parentheses.

Table A-21: Effects of import competition on newspaper references terms overused in Beveridge’s analysis of unemployment, exposure-robust standard errors

	Beveridge terms (1)	(2)	“fluctuation” (3)	(4)	“depression” (5)	(6)	“(un)employment” (7)	(8)	“exchange” (9)	(10)
ΔIPW_{1885}	0.095*** (0.019)	0.101*** (0.026)	0.124*** (0.025)	0.116*** (0.036)	0.137*** (0.032)	0.159*** (0.050)	0.061*** (0.023)	0.062* (0.035)	0.124*** (0.035)	0.167*** (0.046)
Initial Mf x year		x	x	x	x	x	x	x	x	x
First stage F-stat	7.4	7.2	7.4	7.2	7.4	7.2	7.4	7.2	7.4	7.2
Observations	665	665	665	665	665	665	665	665	665	665

Note: *p<0.1; **p<0.05; ***p<0.01

This table replicates the results of Table A-13 using the aggregation and standard error calculation methods recommended by Borusyak, Jaravel, and Hull (2018). Newspaper-level variables aggregated up to the industry level. Dependent variable is number of uses of specified term per newspaper issue, standardized. All models include newspaper and year fixed effects. For newspapers in cities, ΔIPW is calculated at the city-, not constituency-level. “Beveridge terms” refers to terms overused in Beveridge’s *Unemployment: A Problem of Industry*, relative to other contemporary writings supportive of the existing Poor Law system. Terms were selected using the Chi² test statistic proposed by Gentzkow and Shapiro (2010). The terms in question are “fluctuation,” “cyclical,” “dock,” “reserve,” “exchange,” “seasonal,” “depression,” “situation,” “percentage,” “unemployment,” “production,” “unemployed,” “unskilled,” “industrial,” “skilled,” “economic,” “organisation,” “table,” “trades,” “note,” “demand,” “supply,” “applicant,” and “distress.” Standard errors clustered by industry in parentheses.

Table A-22: Effect of local trade shocks on references to immigration in Conservative campaign manifestos, exposure-robust standard errors

	“immigrant” (1)	(2)	“alien” (3)	(4)	“jew” (5)	(6)	“foreigner” (8)
ΔIPW_{1885}	0.094** (0.037)	0.062 (0.042)	0.071*** (0.026)	0.067* (0.036)	0.079** (0.035)	0.088* (0.045)	0.124*** (0.031)
Initial immigrants x year	x	x	x	x	x	x	x
Initial Mf x year	x	x					
First stage F-stat	10.3	9.7	10.3	9.7	10.3	9.7	10.3
Observations	570	570	570	570	570	570	570

Note:

*p<0.1; **p<0.05; ***p<0.01

This table replicates the results of Table 6 using the aggregation and standard error calculation methods recommended by Borusyak, Jaravel, and Hull (2018). Manifesto-level data aggregated to the industry level. Dependent variable is number of uses of specified term relative to total length of manifesto, by Conservative candidates, standardized. All models include constituency and election fixed effects. Standard errors clustered by industry in parentheses.

Table A-23: Effect of local trade shocks on newspaper coverage of immigration, exposure-robust standard errors

	“immigrant” (1)	(2)	“alien” (3)	(4)	“jew” (5)	(6)	“foreigner” (7)	(8)	(9)	All (10)
ΔIPW_{1885}	0.054* (0.028)	0.045 (0.046)	0.037* (0.022)	0.029 (0.034)	0.068*** (0.026)	0.028 (0.044)	0.118*** (0.026)	0.116*** (0.038)	0.123*** (0.025)	0.111*** (0.038)
Initial immigrants x year	x	x	x	x	x	x	x	x	x	x
Initial Mf x year		x		x		x		x		x
First stage F-stat	6.5	5.9	6.5	5.9	6.5	5.9	6.5	5.9	6.5	5.9
Observations	665	665	665	665	665	665	665	665	665	665

Note: *p<0.1; **p<0.05; ***p<0.01

This table replicates the results of Table 7 using the aggregation and standard error calculation methods recommended by Borusyak, Jaravel, and Hull (2018). Newspaper-level variables aggregated to the industry level. Dependent variable is number of uses of specified term per newspaper issue, standardized. All models include newspaper and year fixed effects. For newspapers in cities, ΔIPW is calculated at the city-, not constituency-level. (9) and (10) use mentions of all four terms. Standard errors clustered by industry in parentheses.

Table A-24: Effects of import competition on average economic status, exposure-robust standard errors

	(1)	(2)	(3)	(4)
ΔIPW_t	-0.067** (0.026)	-0.073** (0.032)	-0.047* (0.025)	-0.101** (0.047)
Controls		x	x	x
Initial Mf x year			x	
Constituency trends				x
First stage F-stat	19.2	21.1	21.8	5.8
Observations	285	285	285	285

Note: *p<0.1; **p<0.05; ***p<0.01

This table replicates the results of Table A-2 using the aggregation and standard error calculation methods recommended by Borusyak, Jaravel, and Hull (2018). Stacked first difference estimates, at the constituency level, aggregated to the industry level, for 1880–1890, 1890–1900, 1900–1910. Dependent variable is change in average economic status. All models include year fixed effects. (2)–(4) add controls for lagged manufacturing employment and lagged average economic status; (3) includes 1880 manufacturing employment interacted with year dummy variables, (4) includes constituency fixed effects, which adjust for constituency-specific time trends. Standard errors clustered by industry in parentheses.

Table A-25: Effects of import competition on voting for different parties, exposure-robust standard errors

	(1)	Conservative		Labour		Liberal vote share		(8)
		(2)	(3)	(4)	(5)	(6)	(7)	
Δ IPW	-0.003 (0.004)	0.014** (0.006)	-0.021*** (0.006)	0.014* (0.007)	-0.016*** (0.006)	-0.026*** (0.005)	0.008 (0.014)	0.019*** (0.004)
Years	All	1885-1900	1900-1910	All	All	1885-1900	1900-1910	1900-1910
Excluding Labour								x
First stage F-stat	11.5	5.4	12.2	11.5	11.5	5.4	12.2	13
Observations	760	475	380	760	760	475	380	380

Note: *p<0.1; **p<0.05; ***p<0.01

This table replicates the results of Table A-6 using the aggregation and standard error calculation methods recommended by Borusyak, Jaravel, and Hull (2018). Constituency-level variables aggregated to the shock level, exposure-robust standard errors clustered by industry in parentheses, all models include constituency and election fixed effects. Model 8 excludes elections contested by Labour.

Table A-26: Effects of import competition on voting for combined Liberals and Labour, exposure-robust standard errors

	(1)	(2)	(3)	(4)
Δ IPW	-0.020*** (0.004)	-0.013** (0.005)	0.020*** (0.005)	0.015*** (0.005)
Years	1885–1900	1885–1900	1900–1910	1900–1910
Initial MF x election		x		x
First stage F-stat	5.4	7.2	12.2	13
Observations	475	475	380	380

Note: *p<0.1; **p<0.05; ***p<0.01

This table replicates the results of Table A-7 using the aggregation and standard error calculation methods recommended by Borusyak, Jaravel, and Hull (2018). Constituency-level variables aggregated up to the industry level, dependent variable is combined share of the vote for the Liberal and Labour parties. All models include constituency and election fixed effects, (2) and (4) add the manufacturing employment in 1880 interacted with election dummies. Standard errors clustered by industry in parentheses.

Table A-27: Moderating effect of unions on effect of import competition on voting, exposure-robust standard errors

	(1)	(2)	(3)	(4)	(5)	(6)
Δ IPW	-0.001 (0.008)	-0.009 (0.006)	-0.011 (0.009)	-0.025*** (0.008)	-0.015*** (0.006)	-0.013* (0.007)
Years	All	All	1900–1910	1900–1910	1900–1910	1900–1910
Union sub-sample	2H	1H	2H	1H	All	All
Union x election					x	x
Initial MF x election	x	x	x	x		x
First stage F-stat	9.5	21.4	8.2	7.6	13.8	13.5
Observations	760	760	380	380	380	380

Note: *p<0.1; **p<0.05; ***p<0.01

This table replicates the results of models (5)–(10) of Table A-10 using the aggregation and standard error calculation methods recommended by Borusyak, Jaravel, and Hull (2018) (their method does not allow us to estimate standard errors for variables interacted with the shock). Constituency-level variables aggregated up to the industry level, dependent variable is share of the vote for the Conservative Party. Data on union membership relative to population in 1892 at the county level is taken from Sidney and Beatrice Webb, *The History of Trade Unionism* (London: Longmans, Green and Co., 1896). Models (1) and (3) are estimated for constituencies with above-median unionization, (2) and (4) for constituencies with below-median unionization. Models (5) and (6) replicate regressions from table 3, adding controls for unionization interacted with year dummy variables. All models include constituency and election fixed effects, Standard errors clustered by industry in parentheses.

Table A-28: Effects of import competition on newspaper references to Germany and the naval race, exposure-robust standard errors

	“germany” (1)	(2)	German terms (3)	(4)	Navy terms (5)	(6)	Militarist groups (7)	(8)
ΔIPW_{1885}	0.083*** (0.031)	0.055* (0.032)	0.091*** (0.028)	0.062** (0.031)	-0.004 (0.013)	0.009 (0.017)	-0.116** (0.048)	-0.033 (0.052)
Initial Mf x year		x		x		x		x
First stage F-stat	7.4	7.2	7.4	7.2	7.4	7.2	7.4	7.2
Observations	665	665	665	665	665	665	665	665

Note: *p<0.1; **p<0.05; ***p<0.01

This table replicates the results of Table A-11 using the aggregation and standard error calculation methods recommended by Borusyak, Jaravel, and Hull (2018). Newspaper-level data aggregated to the industry level. Dependent variable is number of uses of specified term per newspaper issue, standardized. All models include newspaper and year fixed effects. For newspapers in cities, ΔIPW is calculated at the city-, not constituency-level. “German terms” are “germany,” “kaiser,” “teuton,” “prussia,” and “fatherland,” “Navy terms” are “navy,” “naval,” “dreadnought,” “battleship,” and “fleet,” “Militarist groups” are “national service league” and “navy league.” Standard errors clustered by industry in parentheses.

Table A-29: Effects of import competition on manifesto references to Germany and the naval race, exposure-robust standard errors

	"germany"		German terms		Navy terms		Militarist groups	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ΔIPW_{1885}	0.038* (0.022)	0.058 (0.036)	0.034 (0.023)	0.054 (0.037)	-0.035 (0.034)	0.004 (0.030)	-0.041** (0.018)	-0.035 (0.025)
Initial Mf x year	x	x	x	x	x	x	x	x
First stage F-stat	9.4	9.4	9.4	9.4	9.4	9.4	9.4	9.4
Observations	570	570	570	570	570	570	570	570

Note: *p<0.1; **p<0.05; ***p<0.01

This table replicates the results of Table A-12 using the aggregation and standard error calculation methods recommended by Borusyak, Jaravel, and Hull (2018). Manifesto-level data aggregated to the industry level. Dependent variable is number of uses of specified term relative to total length of manifesto, standardized. All models include constituency, party, and year fixed effects. "German terms" are "germany," "kaiser," "teuton," "prussia," and "fatherland," "Navy terms" are "navy," "naval," "dreadnought," "battleship," and "fleet," "Militarist groups" are "national service league" and "navy league." Standard errors clustered by industry in parentheses.