Globalization and Its (Dis-)Content: Trade Shocks and Voting Behavior*

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Abstract

We identify the causal effect of trade-integration with China and Eastern Europe on voting in Germany from 1987 to 2009. Looking at the entire political spectrum, we find that only extreme-right parties respond significantly to trade integration. Their vote share increases with import competition and decreases with export access opportunities. We unpack mechanisms using reduced form evidence and a causal mediation analysis. Two-thirds of the total effect of trade integration on voting appears to be driven by observable labor market adjustments, primarily changes in manufacturing employment. These results are mirrored in an individual-level analysis in the German Socioeconomic Panel.

Keywords: Trade Integration, Voting Behavior, Local Labor Markets
JEL Codes: D72, F16, F69

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1 Introduction

The past three decades have seen a dramatic change in the nature of international trade, signified by a secular rise in manufacturing trade between high-wage and low-wage countries (Krugman (2008)). This rise of low-wage manufacturing has caused sizable reductions in industrialized countries' manufacturing and employment (Pierce and Schott (2012), Autor, Dorn, and Hanson (2013), Dauth, Findeisen, and Suedekum (2014)). We study the causal effect of this increased trade integration on voting behavior, and find that trade exposure exclusively affects one narrow segment of the entire political spectrum: the extreme right. Decomposing this effect, we find that import competition leads to political radicalization while export access leads to moderation. Using a unified framework to estimate the effect of trade-integration on both changes in voting and local labor market adjustments, we find that labor markets respond strongly to trade exposure, with import competition having negative and export access having positive labor market effects. Where labor market responses are larger, political responses are larger too, suggesting the two are related. A formal mediation analysis decomposes the extent to which trade affects voting behavior through its effect on labor markets, and suggests that at least 65 percent of the voting response is explained by trade exposure's effects on labor markets.

Germany is a particularly interesting case to study in the given context because its multi-party system covers the full political spectrum from far left to extreme right.[1] German data also allows us to combine administrative data on the outcomes of national elections with measures of regional import competition and export access, as well as changing labor market conditions, across 408 local labor markets (*Landkreise*). Thus, we observe changes in trade exposure, the resulting labor market adjustments, and changes in voting behavior, all at the same subnational level. We organize our data as a stacked panel of two first differences for the periods 1987–1998 and 1998–2009. This is done for two reasons: first, each of the two periods includes a large exogenous shock to the global trading environment. In 1989, the fall of the Iron Curtain opened up the Eastern European markets, and China's accession to the WTO in 2001 led to another large increase in trade exposure.[2] Second, taking differences over roughly ten-year intervals (with specific start and end

[1] Over our study period, we observe more than 50 parties running for election, including several on the far left and the extreme right, which makes party votes a meaningful representation of the electorate's range of political preferences.

[2] Our first-differences analysis does not require any sharpness in trade shocks around the 1989 and 2001 dates. Important for us is only that both periods contain large shifts in the global trading environment.

points determined by national election dates) means our results can be easily compared with an existing literature on trade and labor markets that is based on data from the decadal U.S. census.

Our identification strategy combines nationwide changes in sector-specific trade flows with local labor markets' initial industry structure to determine regional trade exposure. One concern with our approach is that changes in sectoral trade flows can be driven by unobserved domestic conditions rather than by productivity increases in China and Eastern Europe. For example, imports in a sector may increase because of booming domestic demand for intermediate inputs in that same sector. Likewise, rising exports may reflect weakening domestic demand. To address this concern, we follow the approach in Autor et al. (2013) and instrument Germany's changing trade exposure to China and Eastern Europe with that of other countries at similar levels of development. This gives us a measure of changing trade exposure that is driven by supply changes in China and Eastern Europe, and not by changing domestic conditions in Germany. We consider the effects of changes in both import competition and export access, but focus primarily on their net difference, which we refer to as *NetExposure*.

We divide voting outcomes into changes in the vote-share of (i) four mainstream parties: the CDU, the SPD, the FDP and the Green party, (ii) extreme-right parties, (iii) far-left parties, (iv) other small parties, and (v) turnout (see Falck, Gold, and Heblich (2014)). We find that *NetExposure* has no effect on any of the mainstream parties, small parties, turnout, or the far left. The only part of the political spectrum that responds is the extreme-right parties' vote share, which increases significantly with *NetExposure*. Our preferred estimate suggests that a one-standard-deviation increase in *NetExposure* causes an increase in the vote-share of extreme-right parties that amounts to roughly 20 percent of the extreme-right's overall per-decade gains. When we separately consider import competition and export access, we find significant effects working in opposite directions: import competition raises the extreme right's vote-share while export opportunities lower it. Again, we do not find systematic patterns for any of the other political outcomes.

Next, we probe the extent to which the observed trade effect on voting is driven by labor market adjustments. For this purpose, we consider changes in seven labor market outcomes: the share of manufacturing employment in total employment, manufacturing and non-manufacturing wages, total employment, local population size, the unemployment rate, and the share of high-skilled workers. We find strong evidence that increasing trade exposure induces labor market

turbulence. For the subsample of our labor market outcomes that overlaps with the outcomes investigated in Dauth et al. (2014), we estimate effects of similar magnitude.

Splitting the results by period and decomposing trade exposure into import competition and export access suggests a tight connection between voting responses and labor market adjustments to trade: Firstly, the effects of trade exposure on voting and on our main labor market outcomes are both much more pronounced in the second period. A likely explanation for this is that German labor markets underwent substantial deregulation in the late 1990s, which allowed firms to react more flexibly to trade shocks in the 1998–2008 period (Dustmann, Fitzenberger, Schönberg, and Spitz-Oener (2014)). Secondly, decomposing trade exposure shows that import competition radicalizes voters and negatively impacts labor markets, while export access moderates voters and positively impacts labor markets. Thirdly, we see a symmetry in the relative magnitude of the export access and import competition effects on voting responses and on labor market adjustments. In both cases, the moderating/positive effects are about one-quarter larger. We interpret this finding through the lens of Germany's labor market institutions.

We can use statistical mediation analysis to more formally assess to what extent the effect of trade exposure on voting is mediated by labor market adjustments (Imai, Keele, and Yamamoto (2010), Heckman, Pinto, and Savelyev (2013), Heckman and Pinto (2015)). Specifically, we estimate the average causal mediation effect (ACME) of trade-induced labor market adjustments on voting behavior. To reduce the dimensionality of possible mediating channels, we first extract the principal components (PCs) of the seven labor market outcomes under consideration. We determine two PCs with eigenvalues above one. Together, these explain more than 70 percent of the variation observed in local labor market outcomes. One PC is characteristic of regions specialized in manufacturing production, and it is strongly negatively affected by trade integration. This PC's ACME explains about 65 percent of the overall baseline effect on the extreme right's vote share. The other PC primarily characterizes urban agglomeration, is largely unaffected by increasing trade exposure, and therefore plays no role in mediating the effect of trade exposure on far-right voting. We perform a formal sensitivity analysis to examine the robustness of our findings to confounding effects. Specifically, we show that the identifying assumptions would need to be strongly violated for labor market adjustments not to explain any of the effect of trade integration on voting.

Because the main results average over local labor markets, they do not tell us whether it is pri-

marily individuals who are personally affected by trade integration that turn towards the extreme-right parties. In the final section, we exploit individual-level data from the German Socioeconomic Panel (SOEP) to distinguish between the relative importance of individuals' own trade exposure vs. their local labor markets' overall trade exposure in explaining changes in voting behavior. We define local labor market shocks as before, and measure individuals' trade exposure by combining their start-of-period sector of employment with nationwide changes in sector-specific trade flows. Controlling for initial voting preferences and a range of socio-economic controls, we find individual-level results that are strikingly close to the aggregate results. For both measures of trade exposure, it is again voting for the extreme right that systematically responds to trade exposure, and we again find both radicalizing and moderating effects. Interestingly, the effect of local labor markets' overall trade exposure is three times larger than the effect of individuals' personal trade exposure. At least in the context we study, voters therefore appear to adjust their voting behavior in large part because of what they observe in their socio-economic environment as opposed to voting only based on their own economic situation.

This paper contributes to the literature on the effects of economic shocks on voting, which, among other findings, has uncovered effects on incumbents' reelection chances (Bagues and Esteve-Volart (2014)), turnout (Charles and Stephens (2013)), and stated voter preferences for redistribution (Brunner, Ross, and Washington (2011), Giuliano and Spilimbergo (2014)). Our contribution is threefold: First, we study effects on the full range of political attitudes, from far-left to extreme-right, using actual election data as opposed to stated preferences. Second, we identify the effect of trade integration, one of the biggest economic shocks over the past three decades. Third, we formally and informally unpack the mechanisms underlying our core findings in a way that is novel to this literature. In studying the transmission from shocks to economic adjustments to political responses in one integrated estimation framework and at the same spatial unit of analysis, our paper also connects the political economy literature to a long-standing literature on local labor markets (Bartik (1991), Katz and Murphy (1992), Glaeser and Gyourko (2005)).

Moreover, our paper relates to a long-standing literature on trade integration and political cleavages (Rogowski (1987)). This literature focuses on political cleavages along factor (e.g. occupation) or industry lines and either studies self-reported voting behavior in survey data (Scheve and Slaughter (2001)) or legislators' voting records as proxies for their district's voter preferences

(Hiscox (2002)). Its core finding is that import competition creates a demand for protective tariffs (see Rodrik (1995) for an extensive survey). We differ from this literature's approach in our focus on local labor markets rather than occupations or industries, and in using election data as opposed to survey or roll-call data. Focusing on a country whose multiparty system covers the full spectrum of political preferences, we are the first to document and quantify not only a protectionist reaction to trade integration but also a shift towarsd the extremes of the political spectrum.

In the remainder, section 2 lays out the identification strategy and shows how regional trade shocks are measured. Section 3 provides background information on Germany's political system and party spectrum. Section 4 discusses the space- and time-dimension underlying our analysis, and provides descriptive statistics on all core variables. Section 5 presents the core results on voting. In section 6, we focus on mechanisms, studying the nexus of trade exposure, labor market adjustments, migration, and voting. In section 7, we conclude by discussing the potential consequences of the estimated voting effects in the German data and how they relate to other contexts.

2 Estimation Framework and Regional Trade Exposure

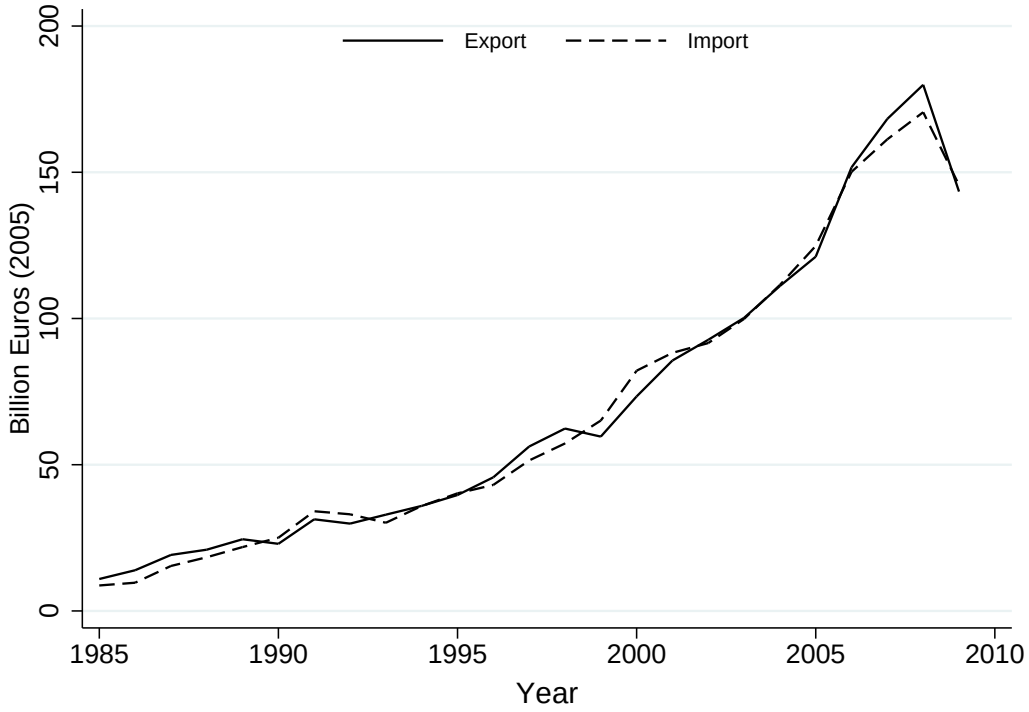
Figure 1 shows the strong increase in German trade with China and Eastern Europe over the observation period. Both imports and exports roughly tripled from 1987 to 1998 (from about 20 billion to about 60 billion Euros each),[3] and then tripled again from 1998 to 2008, before falling off somewhat in 2009 due to the global financial crisis. Our focus is on the net effect of this increasing trade exposure on subnational local labor markets.

As in Autor et al. (2013), we define net exposure as increased import competition net of added export market access, i.e. $\Delta NetExposure_{Git} = \Delta ImportExposure_{Git} - \Delta ExportExposure_{Git}$ where subscript G denotes Germany, i denotes local labor markets ('*Landkreis*'), and t denotes the period. Leaving the discussion of fixed effects, controls, and the error term for later, we are interested in estimating the effect of trade exposure on voting behavior and economic adjustments in a regression like

$$\Delta y_{it} = \alpha + \beta_0 \Delta NetExposure_{Git}, \tag{1}$$

[3] Throughout the paper, we report values in thousands of constant-2005 Euros using exchange rates from the German *Bundesbank*.

Figure 1: Germany's Trade Exposure to China and Eastern Europe



Notes: Aggregate trade flows between Germany and China plus Eastern Europe based on UN Comtrade Data.

where outcomes Δy_{it} in the core regressions stand for changes in voting behavior. When investigating mechanisms in section 6, Δy_{it} can also stand for labor market adjustments. In the course of analysis, we also decompose the net effect of trade exposure into separate effects of import competition and export market access in a regression like

$$\Delta y_{it} = \alpha + \beta_1 \Delta ImportExposure_{Git} + \beta_2 \Delta ExportExposure_{Git}. \tag{2}$$

To measure local labor market i's exposure to national trade flows, we map industry-specific national shocks to local labor markets based on i's initial industry composition. The change in German imports, ΔM_{Gejt}, from a trading partner e in industry j in period t can thus be mapped to local labor market i using industry j's relative importance in i. For region i, j's relative importance is given by $\sum_j \frac{L_{ijt}}{L_{jt}} \frac{1}{L_{it}}$. Intuitively, sector j receives more weight if region i's national share of that sector $\frac{L_{ijt}}{L_{jt}}$ is high, but a lower weight if i's overall workforce L_{it} is larger. We thus define import

competition as

$$\Delta ImportExposure_{Git} = \sum_j \frac{L_{ijt}}{L_{jt}} \frac{\Delta M_{Gejt}}{L_{it}} \tag{3}$$

and correspondingly export access as

$$\Delta ExportExposure_{Git} = \sum_j \frac{L_{ijt}}{L_{jt}} \frac{\Delta X_{Gejt}}{L_{it}}. \tag{4}$$

Variation in trade exposure as we define it above comes from both the relative importance of import-intensive and export-intensive industries and from the relative overall importance of manufacturing employment in a region. Because we are interested in the former and not the latter, we condition all our specifications on the regions' initial share of manufacturing employment.

A remaining concern with this empirical strategy is that both $\Delta ImportExposure_{Git}$ and $\Delta ExportExposure_{Git}$ may be driven by Germany-specific demand or supply shocks in certain sectors. If M_{Gejt} and domestic output in sector j are substitutes–the case if both are final products competing for German consumers–domestic productivity gains in sector j could drive down ΔM_{Gejt}. If, by contrast, imports largely reflect intermediate inputs, then M_{Gejt} and domestic output in sector j are complements, and domestic productivity gains will drive up ΔM_{Gejt}. Similar concerns apply to the export side. This poses a problem, because changes in domestic productivity could simultaneously affect local trade exposure, local voting behavior, and local labor market conditions. To overcome this problem, we follow the approach in Autor et al. (2013) and instrument Germany's imports ΔM_{Gejt} (and exports ΔX_{Gejt}) with other similar, high-wage Western economies' imports and exports from and to the same trading partner.[4]

A last endogeneity concern arises if the anticipation of future import competition or export opportunities were already reflected in contemporaneous employment, in which case there would be a simultaneity problem. To account for simultaneity, we additionally lag the initial employment share in sector j and region i and the initial workforce by one decade and denote this lag by the

[4]We choose the same countries as Dauth et al. (2014) to instrument German im- and exports: Australia, Canada, Japan, Norway, New Zealand, Sweden, Singapore, and the United Kingdom. This set of countries excludes Eurozone countries, because their demand- and supply conditions are likely correlated with Germany's.

subscript $t-1$. As instruments, we thus define

$$\Delta ImportExposure_{Oit} = \sum_j \frac{L_{ijt-1}}{L_{jt-1}} \frac{\Delta M_{Oejt}}{L_{it-1}}, \tag{5}$$

and equivalently $\Delta ExportExposure_{Oit}$, as well as $\Delta NetExposure_{Oit}$ as the former minus the latter.

3 Background on German Politics 1987 to 2009

The German Election System: Since the end of WWII, Germany has had a multiparty party system, with the two largest parties–the Christian Democratic Union (CDU) and the Social Democratic Party of Germany (SPD)–forming coalitions with either the Free Democratic Party (FDP) or the Greens (*Bündnis 90/Die Grünen*) during our observation period (1987 to 2009).[5] German elections are based on the principle of proportionality. The main vote cast for a party is the "second vote" (*Zweitstimme*), which we focus on. The overall number of parliamentary seats is determined in proportion to a party's share of the second vote.[6] Parties further have to surpass a 5 percent minimum threshold to be represented in federal parliament. However, this does not mean that small parties do not capture any votes. Small parties that failed to pass the 5 percent threshold still captured about 11 percent of the total votes in our election data.

The Political Party Spectrum in Germany: We always classify the CDU, the SPD, the FDP, and the Greens as established parties. The conservative CDU and the social-democratic SPD are the dominant parties in Germany, in terms of both membership and votes obtained. For our period of analysis, one of those two parties was always in power. The liberal FDP participated in governments led by the CDU. The Greens are, for ideological reasons, usually the SPD's preferred coalition partner. On the extreme right of the political spectrum, three parties have regularly run in federal elections. The National Democratic Party of Germany (NPD - *Nationaldemokratische Partei Deutschlands*), founded in 1964, the Republicans (REP - *Die Republikaner*), founded in 1983, and the German People's Union (DVU - *Deutsche Volksunion*), founded in 1987 (and merged with

[5] In this paper, we will always report votes for the *CDU* and its Bavarian subsection Christian Social Union (CSU) as combined CDU votes and refer to it as the CDU.

[6] Voters can also elect individual candidates on a first-past-the-post basis. Ironically, this second ballot is called the "primary vote" (*Erststimme*). In every election district, the candidate who wins the majority of primary votes is directly elected to parliament. However, electoral law ensures that this has no major effect on the overall distribution of seats, which is determined exclusively by proportionality votes.

the NPD in 2011).[7] They all follow neo-Nazi ideologies, are anti-democratic, polemicize against globalization, and agitate against immigrants and foreigners. All three have been monitored by the German Federal Office for the Protection of the Constitution (*Verfassungsschutz*). None of these extreme-right parties has ever passed the 5 percent hurdle required to enter Germany's national parliament, and it is unthinkable that any mainstream party would ever form a coalition with them (see Art (2007)). On the far left of the political spectrum, there are around 10 parties and factions that are often related with each other. Besides the Left party (*Die Linke*) and its predecessors, the Party of Democratic Socialism (PDS) and Labour and Social Justice The Electoral Alternative (WASG), three branches have been dominant: Successors to the Communist Party of Germany, which had been outlawed in 1956, e.g., the German Communist Party (DKP) and the Communist Party of Germany (KPD); Leninist, Stalinist, and Maoist organizations like the Marxist-Leninist Party of Germany (MLPD); and Trotskyist organizations such as the Party for Social Justice (PSG). Like the parties on the extreme right, these far-left parties are regularly monitored by either the Federal Office for the Protection of the Constitution or its state-level equivalents. We classify other parties that ran for elections but do not fit the above categories as small parties (see Falck et al. 2014).

4 The Data

We provide a detailed description of data sources in Online Appendix B and focus here on describing the spatial and time dimension of the data, and on documenting the key variation. Our spatial units of observation are counties (*Landkreise*) as a representation of local labor markets (see Dauth et al. 2014). The federal election of 1987 marks our starting point. This is before the fall of the Iron Curtain, in 1989, and Germany's subsequent reunification, in 1990. We focus on federal elections for two reasons: First, the election years for state and local elections vary, and we would therefore lack a shared start and end point for the first-difference analysis if we used these. Second, the focus on federal elections ensures that all types of parties–classified as mainstream, small, far-left, and extreme-right–are represented in all elections.

To allow comparison with the existing literature, we would ideally want to study decennial

[7] In Online Appendix A, we provide a history of these three parties. See also comprehensive work by Stöss (2010) or Mudde (2000).

changes. However, we are tied to election dates and therefore study two periods of 11 years, 1987 to 1998 and 1998 to 2009, instead.[8] We further restrict our sample to include only West Germany in the first period. A first consideration is political: Even if we shifted our analysis to let the first period begin in 1990, the year of East Germany's first free elections, many small, far-left, or extreme-right parties could not be observed in East Germany, so that a 1990–1998 comparison is for the East German districts more or less equivalent to a 1998 cross-sectional analysis of all party outcomes except the CDU-votes and the SPD-votes (see also Online Appendix A). A second consideration is economic: Under the Socialist regime, almost all East German firms were state-owned until 1990. They were only slowly privatized by the *Treuhandanstalt* ("Trust Agency") after reunification, a process that lasted well into the mid-1990s and provided some shield against international competition. For the newly privatized East German firms, integration with the global economy was far less important than integration with West Germany. Including East Germany in the sample therefore only appears reasonable in the second period, when its integration into the German economy was more or less complete.[9]

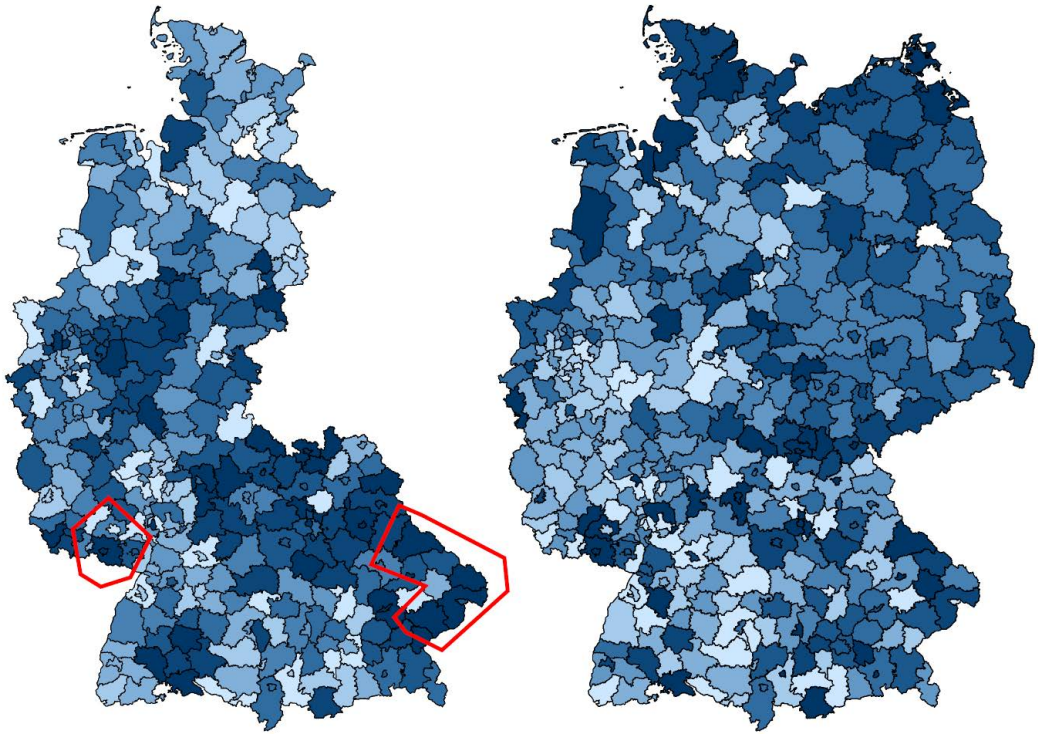
Figure 2 shows the spatial dispersion of our key regressor, $\Delta NetExposure_{Git}$. A first observation is that there appears to be little auto-correlation in the trade exposure measure between the two periods (i.e. regions that are equally dark or light in both periods). This partly reflects the changing source of trade competition over time. While we consider trade flows from Eastern Europe and China in both periods, Eastern Europe imposes the dominant shock on German local labor markets in 1987–1998, while the shock from China dominates in the period 1998–2009 (Dauth et al. (2014)). A second observation is that shocks are spatially dispersed and not clustered by state, reflecting Germany's diverse pattern of industrial production. Third, the patterns we observe are consistent with our knowledge of the spatial dimension of structural change in Germany over the past two decades. For the two circled regions in figure 2, we provide a detailed narrative in Online Appendix C, where we explore the nexus of import competition, structural decline in manufacturing, and changes in voting behavior.

Table 1 shows the broad patterns in our data. In the regressions, these broad patterns will

[8] Federal elections took place in 1987; in December 1990 (after reunification, on October 3); and in 1994, 1998, 2002, 2005, and 2009.

[9] For the same reasons, Dauth et al. (2014) exclude the East German regions in the 1990s from their replication of Autor et al. (2013) for German labor markets.

Figure 2: $\Delta NetExposure_{Git}$ in 1987–1998 (Left), and 1998–2009 (Right)



Notes: Trade Shocks mapped into 322 West German counties for 1987–1998 (left) and into 408 German counties for 1998–2009 (right). The two circled regions are being discussed in Online Appendix C by the way of example.

Table 1: The Core Variables in 1987–1998 and in 1998–2009

	(1)	(2)	(3)	(4)	(5)	(6)
	Period 1 (1987-1998), N=322			Period 2 (1998-2009), N=408		
percentile:	25th	median	75th	25th	median	75th
Regressors:						
Δ NetExposure_{Git}	-0.264	0.068	0.521	-1.222	-0.663	-0.144
instrumented Δ NetExposure_{Git}	-0.068	0.143	0.402	-1.150	-0.574	-0.113
Voting Outcomes:						
Δ Turnout	-0.034	-0.020	-0.012	-0.167	-0.128	-0.095
Δ Vote Share CDU/CSU	-9.234	-7.659	-5.730	-4.493	-2.258	0.620
Δ Vote Share SPD	4.120	6.472	8.248	-19.904	-17.936	-16.079
Δ Vote Share FDP	-2.933	-2.188	-1.467	6.942	8.459	9.820
Δ Vote Share Green Party	-1.779	-1.282	-0.616	2.513	3.673	4.770
Δ Vote Share Extreme-Right Parties	1.520	2.086	3.099	-1.525	-1.021	-0.478
Δ Vote Share Far-Left Parties	0.677	0.908	1.165	5.688	7.078	8.373
Δ Vote Share Small Parties	1.211	1.487	1.796	0.716	1.514	2.525
Economic Outcomes:						
Δ Share Manufacturing Employment	-4.505	-2.686	-0.987	-1.732	-0.711	0.593
Δ log(Mean Manufacturing Wage)	0.104	0.122	0.147	-0.008	0.022	0.051
Δ log(Mean Non-Manufacturing Wage)	0.086	0.102	0.117	-0.093	-0.071	-0.046
Δ log(Total Employment)	-0.067	0.001	0.081	-0.110	-0.044	0.021
Δ Share Unemployment	0.492	1.259	1.983	-2.138	-1.234	-0.650
Δ log(Total Pop)	0.058	0.099	0.133	-0.046	0.000	0.033
Δ Share High Skilled	0.336	0.558	0.881	0.163	0.437	0.801

Notes: Period one (1987–1998) is for West German labor markets only, N = 322. Period two (1998–2009) is for West plus East German labor markets, N = 408. The numbers for 1998–2009 do not change substantively if we drop the East. The table displays the 25th percentile, median, and 75th percentile of three sets of variables: regressors, voting outcomes, and economic outcomes.

largely get absorbed by period-by-region fixed effects. Accordingly, we consider table 1 primarily as background information, which may be best viewed through the lens of a standard account of the German economy's development. This development was characterized by prolonged economic stagnation from reunification through the late 1990s, followed by an equally prolonged export and productivity boom until the beginning of the Euro-crisis (Dustmann et al. (2014)). Table 1 is organized in the following way: Each row presents the distribution of one variable, sliced into its 25th percentile, median, and 75th percentile. Columns 1–3 do this for the period 1987–1998, and columns 4–6 for the period 1998–2009. $\Delta NetExposure_{Git}$ is defined in units of 1,000 € per worker in constant 2005 prices. A comparison of columns 1–3 and 4–6 shows that trade integration was relatively balanced between import competition and export access in 1987–1998, with an average $\Delta NetExposure_{Git}$ of just 66 € per worker. In 1998–2009, trade integration was more export-heavy, with changes in export access exceeding changes in import competition by on average 660 € per worker.[10] The table further shows substantial variation in political trends across the two periods. From 1987 to 1998, the established parties saw on average a 4.7 percentage point reduction in their share of the popular vote, while small parties and the extreme right gained. From 1998 to 2009, what appears like an additional reduction in established parties' vote shares is in fact entirely driven by the SPD breaking with its own left faction, which subsequently merged with the far left to form the new party *Die Linke*, which gets classified as far left in our data. See section 3 for more details. By contrast, the extreme right lost part of its gains from the previous decade. Turning to labor markets, the decline in manufacturing was much more pronounced in 1987–1998 than in 1998–2009. Furthermore, unemployment was rising in 1987–1998, but falling in 1998–2009. In summary, the overall patterns in 1987–1998 are changes in import competition and export access that roughly balanced out, economic stagnation, and political radicalization. This was followed by increased export access, economic stabilization, and political stabilization in 1998–2009.

[10] Dauth et al. (2014) explore this finding in detail, and show that trade integration with Eastern Europe was primarily associated with intra-industry trade in final products, i.e., Eastern European final products displaced German final products in German markets. By contrast, trade with China–which was more dominant in period two–was primarily inter-industry, i.e., Chinese imports displaced imports from other countries rather than German production.

5 Results

5.1 Core Results

We estimate the following equation:

$$\Delta y_{it} = \alpha_1 + \beta_1 \Delta \mathit{NetExposure}_{Git} + X'_{it}\gamma_1 + \tau^1_{tr} + \epsilon_{it} \tag{6}$$

Outcome y_{it} either refers to turnout or to the vote-share of incumbent, small, extreme-right, or far-left parties along the political spectrum. To account for the endogeneity of trade exposure, we instrument for $\Delta \mathit{NetExposure}_{Git}$ with similar other countries' lagged import and export exposure to China and Eastern Europe, as defined in equation (5).[11] There are 408 districts (*Landkreise*) in our data, 86 of which are in East Germany and therefore dropped in period one. The total number of observations in our stacked panel is therefore 730 ($= (408 - 86) + 408$) counties. For the fixed effects τ_{tr}, we follow (Dauth et al. (2014)) and consider four regions (North, West, South, and East Germany) that are comparable to U.S. Census divisions. Each of Germany's 16 states (*Bundesländer*) is fully contained inside one of these four regions. The regional fixed effects are time-varying, to allow changes in voting behavior in 1987–1998 and 1998–2009 to follow different trends. This is clearly appropriate, given the broadly different trends displayed for the two periods in table 1. Doing so further accounts for the fact that East Germany is not included in the panel from 1987–1998. Standard errors ϵ_{it} are clustered at the level of 96 commuting zones defined by the Federal Office for Building and Regional Planning (BBR)).

Table 2 presents our baseline results. Each cell reports results from a different regression. Rows specify different outcome variables, and columns refer to different regression specifications. Results for the coefficients on all control variables are reported in Online Appendix D, table 2. In our least conservative specification (column 1 of table 2), we consider start-of-period manufacturing as the only control. As discussed in section 2, we always control for a region's start-of-period manufacturing share in employment because it inherently drives part of the variation in $\Delta \mathit{NetExposure}_{Git}$ without being our focus. In column 2, we add controls for the structure of the

[11] We estimate the first stage equation

$$\Delta \mathit{NetExposure}_{Git} = \alpha'_1 + \lambda_1 \Delta \mathit{ImportExposure}_{Oit} + \lambda_2 \Delta \mathit{ExportExposure}_{Oit} + X'_{it}\gamma'_1 + \tau'_{tr} + \epsilon'_{it}. \tag{7}$$

workforce, i.e., the start-of-period employment share that is college educated, foreign born, or female.

It is a feature of the German economy that some regions are dominated by one specific industry. In such regions, individual firms (e.g. Daimler-Benz, Volkswagen, or Bayer) are likely to have political bargaining power, and as a result politicians may help buffer trade shocks to limit adverse employment effects. In column 3, we account for this by including a control for the employment share in the largest sector, along with separate controls for the employment share in car manufacturing and the chemical industry. The latter account for those industries' outstanding importance for the German economy. In column 4, we add start-of-period vote-shares for all party outcomes and turnout. Finally, in column 5, we include controls for start-of-period values of the share of the population that is of retirement age or unemployed. This is the most conservative specification, and our preferred one. In it, a one-standard-deviation increase in $\Delta NetExposure_{Git}$ (1,350 €) increases the extreme-right vote share by 0.12 ($0.09 \cdot 1.35$) percentage points, roughly 28 percent of the average per-decade increase of 0.43 percentage points during the 22 years we study. Column 6 reports the results from our preferred specification as beta coefficients to facilitate comparison between the effects on political outcomes.

The effects are broadly consistent across all five specifications, though we see that the stepwise inclusion of controls reduces the effect size. Our findings suggest no effect on turnout, and looking at reactions across the political spectrum, we see no significant effects on established, small, or far-left parties in our preferred specification in column 5. The only segment of the party spectrum that responds consistently to trade shocks across all specifications is the vote-share of extreme-right parties.[12] Looking at the beta coefficients reported in column 6, we see that the estimated effects for all parties except the extreme right are not only insignificant but also small compared to the effect on extreme-right parties. For a better understanding of potential biases, we present corresponding OLS estimates in table 1 in Online Appendix D. A comparison between IV and

[12]However, the coefficient for the liberal FDP shows a marginally insignificant t-statistic of 1.58, and for turnout we see a t-statistic of 1.22. The latter indicates that turnout might increase with trade exposure. This would complement Charles and Stephens (2013), who find that positive economic shocks decrease voter turnout. One possible explanation for the positive though marginally insignificant effect on votes for the liberal FDP is that regions hit by a trade shock may face increasing demand for redistribution or government intervention in markets (Rodrik (1995)). As a result, those who do not approve such policies may choose to vote for the (market) liberal FDP. Based on our reading of German politics, we take this as a hint for possible polarization, if the economically liberal FDP became an attractive choice for voters who position themselves against growing anti-globalization sentiments in their region.

Table 2: Effect of $\Delta NetExposure_{Git}$ on Voting

	(1) Baseline IV	(2) + Structure IV	(3) + Industry IV	(4) + Voting IV	(5) +Socio IV	(6) Standard. IV
Δ Turnout	0.002	0.003	0.004	0.002	0.002	0.036
	(0.939)	(1.192)	(1.455)	(1.095)	(1.223)	(1.223)
Established Parties:						
Δ Vote Share CDU/CSU	-0.128	-0.130	-0.180	-0.062	-0.066	-0.016
	(-0.744)	(-0.808)	(-0.993)	(-0.475)	(-0.501)	(-0.501)
Δ Vote Share SPD	-0.020	0.004	-0.006	-0.011	-0.009	-0.001
	(-0.129)	(0.030)	(-0.039)	(-0.090)	(-0.073)	(-0.073)
Δ Vote Share FDP	0.215***	0.176**	0.170**	0.109	0.119	0.022
	(2.788)	(2.384)	(2.197)	(1.377)	(1.583)	(1.583)
Δ Vote Share Green Party	-0.132**	-0.055	-0.030	-0.025	-0.018	-0.006
	(-2.294)	(-1.309)	(-0.612)	(-0.551)	(-0.413)	(-0.413)
Non-established Parties						
Δ Vote Share Extreme-Right Parties	0.118***	0.099***	0.113***	0.086**	0.089**	0.044**
	(3.370)	(3.118)	(2.845)	(1.980)	(2.055)	(2.055)
Δ Vote Share Far-Left Parties	-0.037	-0.078	-0.080	-0.068	-0.092	-0.024
	(-0.289)	(-0.643)	(-0.639)	(-0.588)	(-0.859)	(-0.859)
Δ Vote Share Other Small Parties	-0.015	-0.017	0.013	-0.028	-0.024	-0.018
	(-0.391)	(-0.458)	(0.327)	(-0.687)	(-0.564)	(-0.564)
First Stage:						
Δ Import Exposure_{Git}	0.225***	0.234***	0.221***	0.220***	0.220***	0.220***
	(8.220)	(8.350)	(7.816)	(7.966)	(7.971)	(7.971)
Δ Export Exposure_{Git}	-0.211***	-0.212***	-0.208***	-0.201***	-0.202***	-0.202***
	(-8.519)	(-8.251)	(-8.065)	(-7.660)	(-7.568)	(-7.568)
F-Stat. of excluded Instruments	43.81	43.64	40.15	38.77	38.21	38.21
Period-by-region F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Observations	730	730	730	730	730	730

Notes: Each cell reports results from a separate instrumental variable regression. Column 1 controls only for start-of-period manufacturing. Column 2 adds controls for the structure of the workforce (share female, foreign, and high-skilled). Column 3 adds controls for dominant industries (employment share of the largest industry, in automobiles, and chemicals). Column 4 adds start-of-period voting controls. Column 5 adds socioeconomic controls at the start of the period (population share of unemployed individuals, and individuals aged 65+). This is our preferred specification. Finally, Column 6 presents our preferred specification with standardized outcome variables to facilitate comparison. The data is a stacked panel of first-differences at the county level. The panel comprises 322 districts in West Germany, observed in 1987–1998 and 1998–2009, and 86 districts in East Germany, observed in 1998–2009. We drop three city-states (Hamburg, Bremen, and Berlin in the East). All standard errors are clustered at the level of 96 commuting zones. All specifications include region-by-period fixed effects. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

OLS estimates for the effect on extreme-right parties shows that the OLS coefficient is consistently smaller that the IV coefficient. This result is in line with our concern that imports partly reflect domestic sectoral demand shifts. For example, though booming domestic production may increase demand for intermediate input imports, it is unlikely to have the same political consequences as import competition.

The observed patterns in table 2 raise the question why the extreme right captures the anti-globalization vote, especially since the anti-globalization movement is often associated with the political left. Sommer (2008, p. 312) describes the German Left's inability to capture this vote:

> "In opposing globalization, left-wing organizations usually criticize an unjust and profit-oriented economic world order. [They] do not reject globalization per se but rather espouse a different sort of globalization. In contrast, the solutions proposed by the extreme right keep strictly to a national framework. The extreme right's claim, therefore, that it is the only political force that opposes globalization fundamentally [...] rings true."

By contrast, Germany's extreme-right has marketed itself as a viable anti-globalization alternative, while attempting to tie this in with their ideology of a hidden Jewish world conspiracy. The following excerpt from the NPD's candidate manual illustrates how to articulate the party's stance on globalization:

> "Globalization is a planetary spread of the capitalist economic system under the leadership of the Great Money. This has, despite by its very nature being Jewish-nomadic and homeless, its politically and military protected location mainly on the East Coast of the United States" (Grumke (2012, p. 328))

The observed patterns may of course not generalize to other institutional contexts where the platforms and histories of "left" and "right" are different, or where established extreme-right parties do not exist. There are, however, similarities with other countries. In France, for instance, the left found it equally hard to form a coherent position against globalization (Sommer (2008), Arzheimer (2009)), while the far right around Marie LePen's *Front National* has successfully captured French anti-globalization sentiment. The sociological literature even suggests an empirical

regularity (Mughan and Lacy (2002)). For example, Mughan, Bean, and McAllister (2003) argue that

> "the achievement of right-wing populists has been to [..] compound this fear by holding that the liberalized international economy makes it harder for workers to find as secure and well-paying a job as their current one, should they become unemployed."

5.2 Subsample Results

In table 3, we do by-period breakdowns and split $\Delta NetExposure_{Git}$ into $\Delta ImportExposure_{Git}$ and $\Delta ExportExposure_{Git}$. For this purpose, we focus on our preferred specification from column 5 in table 2. We start with by-period regressions. In Panel A, we report separate outcomes for the 1987–1998 period. In Panel B, we report estimates for the 1998–2009 period, which includes the regions of East Germany. By 1998–2009, Eastern Germany's former state-owned companies were re-privatized and the political party structure had had time to establish. Nevertheless, lingering traits from the Communist era continued to make East Germany a special case. To account for that, Panel C presents specifications where we drop East Germany even in 1998–2009.

The subsample estimations are in line with the main results in that we do not find consistent patterns for any part of the political spectrum other than the extreme right. Further, they suggest that the result for extreme-right parties is primarily driven by period two (Panel B) and West Germany (Panel C). This finding is consistent with Germany's recent political history. In the 1980s and early 1990s, the extreme right was more focused on immigration than on globalization. At that time, the German constitution guaranteed an absolute right to asylum, resulting in large immigrant inflows. In 1992, Germany admitted almost 70 percent of all asylum seekers registered in the European Community. Faced with increasing pressure from extreme-right parties, the German parliament amended Article 16 of Constitution in December 1992, which paved the way for more restrictive asylum laws in 1993. After that, extreme-right parties had to settle new political goals. At this time, they spotted the discontent of globalization as an opportunity to propagate a strong national state. For example, the 1997 NPD party program began polemizing against "nation-stateless predator capitalism", the "global dictatorship of big money", and the "zionist lobby" as a driving force of globalization (Stöss (2010, pp.40-42)).

Table 3: Decomposing the Core Results

	(1) Turnout IV	(2) CDU/CSU IV	(3) SPD IV	(4) FDP IV	(5) Green Party IV	(6) Right IV	(7) Left IV	(8) Small IV
Panel A: Period 1								
Δ Net Exposure$_{Git}$	0.000	-0.298	0.320	0.013	-0.003	-0.025	-0.001	-0.007
	(0.013)	(-1.159)	(1.558)	(0.150)	(-0.030)	(-0.243)	(-0.041)	(-0.105)
Panel B: Period 2								
Δ Net Exposure$_{Git}$	0.000	-0.115	-0.173	0.076	0.081	0.071*	0.058	0.003
	(0.080)	(-0.704)	(-1.072)	(0.821)	(1.142)	(1.696)	(0.360)	(0.044)
Panel C: Period 2, West only								
Δ Net Exposure$_{Git}$	0.002	-0.095	-0.161	0.083	0.110	0.084**	-0.023	0.001
	(0.514)	(-0.542)	(-0.987)	(0.886)	(1.342)	(2.078)	(-0.187)	(0.018)
Panel D: Period 1								
Δ Import Exposure$_{Git}$	0.001	-0.180	0.499*	-0.032	-0.143	-0.119	-0.031	0.006
	(0.267)	(-0.634)	(1.828)	(-0.321)	(-1.035)	(-0.885)	(-1.527)	(0.082)
Δ Export Exposure$_{Git}$	0.001	0.468	-0.060	-0.079	-0.200	-0.111	-0.044	0.024
	(0.352)	(1.245)	(-0.173)	(-0.612)	(-1.308)	(-0.757)	(-1.159)	(0.332)
Panel E: Period 2								
Δ Import Exposure$_{Git}$	0.000	-0.061	-0.181	0.042	0.064	0.074*	0.077	-0.015
	(0.133)	(-0.387)	(-1.068)	(0.450)	(0.907)	(1.750)	(0.428)	(-0.255)
Δ Export Exposure$_{Git}$	-0.002	-0.253	0.225	0.153	0.037	-0.094*	-0.184	0.116
	(-0.392)	(-1.119)	(0.975)	(1.135)	(0.454)	(-1.933)	(-0.664)	(1.458)
Panel F: Period 2, West only								
Δ Import Exposure$_{Git}$	0.002	-0.037	-0.183	0.054	0.095	0.088**	0.001	-0.019
	(0.522)	(-0.216)	(-1.048)	(0.578)	(1.183)	(2.099)	(0.010)	(-0.320)
Δ Export Exposure$_{Git}$	-0.002	-0.297	0.311	0.109	-0.010	-0.108**	-0.138	0.133*
	(-0.508)	(-1.225)	(1.325)	(0.855)	(-0.109)	(-2.100)	(-0.618)	(1.771)

Notes: The table reports subsample estimations of the voting outcomes. Panels A–B separate the effect of $\Delta NetExposure_{Git}$ into period one (1987–1998) and period two (1998–2009). Panel C additionally drops 86 East German counties. Panels D–F repeat the by-period specifications and additionally distinguish between import and export effects. All specifications include identical controls to our preferred specification in Table 2, Column 5. Standard errors are clustered at the level of 96 commuting zones. *** p<0.01, ** p<0.05, * p<0.1.

In Panels D–F, we repeat the by-period breakdowns but decompose $\Delta NetExposure_{Git}$ into $\Delta ImportExposure_{Git}$ and $\Delta ExportExposure_{Git}$.[13] Focusing on right-wing party vote shares in column 6, panels D–F show that the *radicalizing* effect from increasing import competition is contrasted by a *moderating* effect of export opportunities. Interestingly, the moderating effect (in absolute terms) is about one-quarter larger than the radicalizing effect in period 2. We conjecture that this asymmetry is the result of Germany's labor market reforms in the late 1990s (Dustmann et al. (2014)). The new labor market institutions allowed for highly decentralized and flexible labor contracts that could absorb adverse employment effects from, for example, trade shocks. We revisit this interpretation in table 6 when we break up labor market adjustments in an analogous way. It is also worth noting that Panel D shows the vote shares of the social-democratic SPD increasing in import competition. This is the flipside of our discussion of Germany's recent political history. In the early 1990s, anti-globalization attitudes were primarily a hallmark of the left. It was after the 1998 elections, when the SPD committed to a free-market agenda, that the extreme-right suceeded in capturing the anti-globalization votes.

How does the evidence presented in table 3 relate to the descriptive statistics presented in table 1, which showed that extreme-right parties gained vote share in period one but lost vote share in period two? Our results suggest that the right-wing gains in period one were driven by reasons other than trade (likely by immigration), while the decline in period two may well be at least partly explained by trade integration: The extreme right appears to have staked its agenda on the damaging effects of globalization just at the time when these effects became less pronounced in Germany.

In table 4 we address the problem that our election results are measured at place-of-residence while our employment data are available only at place-of-work for the entire 1987–2009 period. We expect the induced measurement error to attenuate our estimates toward zero. To assess this, we exploit employment data at place-of-residence that are available from 1999 on and recalculate $\Delta NetExposure_{Grt}$ with the initial industry composition $\frac{L_{rjt}}{L_{jt}}$ and overall employment L_{rt} at place of residence r (instead of place of work) in the initial period $t = 1999$. Because this period-two start year differs from the results reported so far, and because we cannot lag employment in our

[13] These are instrumented by $\Delta ImportExposure_{Oit}$ and $\Delta ExportExposure_{Oit}$, i.e. we have two regressors and two instruments.

Table 4: Place of Work and Place of Residence

4.A: 1999–2009 data for "Place of Residence"

	(1) Turnout IV	(2) CDU/CSU IV	(3) SPD IV	(4) FDP IV	(5) Green Party IV	(6) Right IV	(7) Left IV	(8) Small IV
~ Panel B: Period 2								
Δ Net Exposure$_{Git}$	0.004	-0.123	-0.165	-0.025	0.117	0.124*	-0.029	0.102
	(1.082)	(-0.722)	(-0.815)	(-0.193)	(1.338)	(1.912)	(-0.140)	(1.251)
~ Panel C: Period 2, West only								
Δ Net Exposure$_{Git}$	0.004	-0.061	-0.069	-0.007	0.083	0.148**	-0.180	0.085
	(0.920)	(-0.342)	(-0.358)	(-0.049)	(0.822)	(2.147)	(-0.992)	(0.996)
~ Panel E: Period 2								
Δ Import Exposure$_{Git}$	0.005	0.055	-0.172	-0.033	0.123	0.138**	-0.162	0.050
	(1.160)	(0.273)	(-0.730)	(-0.237)	(1.463)	(1.976)	(-0.671)	(0.599)
Δ Export Exposure$_{Git}$	-0.008	-0.678*	0.196	0.061	-0.146	-0.188*	0.624	0.130
	(-1.210)	(-1.787)	(0.480)	(0.271)	(-1.227)	(-1.959)	(1.526)	(1.050)
~ Panel F: Period 2, West only								
Δ Import Exposure$_{Git}$	0.006	0.141	-0.112	-0.033	0.098	0.160**	-0.295	0.040
	(1.052)	(0.650)	(-0.459)	(-0.216)	(0.999)	(2.017)	(-1.329)	(0.434)
Δ Export Exposure$_{Git}$	-0.010	-0.767*	0.243	0.113	-0.145	-0.198	0.652*	0.102
	(-1.258)	(-1.874)	(0.561)	(0.477)	(-1.021)	(-1.623)	(1.686)	(0.770)

4.B: 1999–2009 data for "Place of Work"

	(1) Turnout IV	(2) CDU/CSU IV	(3) SPD IV	(4) FDP IV	(5) Green Party IV	(6) Right IV	(7) Left IV	(8) Small IV
~ Panel B: Period 2								
Δ Net Exposure$_{Git}$	0.002	-0.049	-0.182	0.093	0.109*	0.080*	-0.088	0.038
	(0.685)	(-0.424)	(-1.358)	(1.232)	(1.674)	(1.948)	(-0.569)	(0.685)
~ Panel C: Period 2, West only								
Δ Net Exposure$_{Git}$	0.004	-0.059	-0.146	0.076	0.100	0.088**	-0.117	0.058
	(0.991)	(-0.496)	(-1.142)	(1.010)	(1.361)	(1.970)	(-0.864)	(0.970)
~ Panel E: Period 2								
Δ Import Exposure$_{Git}$	0.002	0.093	-0.162	0.033	0.094	0.089**	-0.131	-0.017
	(0.651)	(0.711)	(-0.995)	(0.415)	(1.616)	(2.018)	(-0.722)	(-0.290)
Δ Export Exposure$_{Git}$	-0.003	-0.395	0.120	0.092	-0.061	-0.110*	0.221	0.132
	(-0.513)	(-1.526)	(0.468)	(0.705)	(-0.730)	(-1.901)	(0.799)	(1.502)
~ Panel F: Period 2, West only								
Δ Import Exposure$_{Git}$	0.003	0.101	-0.170	0.009	0.096	0.091*	-0.122	-0.006
	(0.831)	(0.717)	(-1.023)	(0.110)	(1.417)	(1.801)	(-0.746)	(-0.092)
Δ Export Exposure$_{Git}$	-0.003	-0.415	0.216	0.122	-0.088	-0.098	0.132	0.131
	(-0.535)	(-1.444)	(0.816)	(0.921)	(-0.853)	(-1.423)	(0.545)	(1.373)

Notes: The table reports results from regressions of voting outcomes on $\Delta NetExposure_{Git}$. In 4.A, $\Delta NetExposure_{Git}$ is defined using start-of-the-period employment at *place of residence*. Because *place of residence* only becomes available in 1999 instead of 1998 as in the baseline *place of work* results, 4.B replicates the baseline results when start-of-the-period employment structure is measured in 1999. The panel-labels in 4.A and 4.B refer to panels in Table 3. All specifications include identical controls to our preferred specification in Table 2, Column 5. Standard errors are clustered at the level of 96 commuting zones. *** p<0.01, ** p<0.05, * p<0.1.

instrument due to the absence of employment data at place-of-residence before 1999, we cannot simply compare period-two results for place-of-residence to the place-of-work results reported so far.

Table 4.A reports the place-of-residence results. In table 4.B, we replicate the place-of-work results from Panels B–C of table 3 with $t = 1999$. These place-of-work specifications closely resemble the baseline specifications. Comparing Tables 4.A and 4.B, we find between 55 percent ($0.124/0.08 = 1.55$) and 68 percent ($0.148/0.088 = 1.681$) larger effects in the place-of-residence specifications. At the place of residence, a one-standard-deviation increase in $\Delta NetExposure_{Git}$ (1,110 €) increases extreme-right votes by between 0.14 ($0.124 \cdot 1.11$) and 0.16 ($0.148 \cdot 1.35$) percentage points, which is up to thirty-seven percent (compared to twenty-eight percent at place of work) of the average per-decade increase of 0.43 percentage points during the 22 years we study.

6 Mechanisms

6.1 Local Labor Market Evidence

Some of the preceding discussion already suggested that labor market adjustments are likely to be a key transmission channel from trade exposure to changes in voting behavior. We now investigate more thoroughly to what extent the observed effects on voting are driven by labor market adjustments. To this end, we first investigate labor market effects of trade shocks in this section, before we integrate labor-market and election effects of trade shocks into a single estimation framework in section 6.2. We begin with re-running regression (6) for seven different labor market outcomes. This relatively large set of outcomes provides a good representation of the range of possible labor market turbulences. The results are displayed in table 5, which is structured in exactly the same way as table 2, with each cell reporting a different regression specification. Column 1 is our baseline specification; column 2 adds structural characteristics of the workforce, i.e., the employment shares of female, foreign, and high-skilled workers; column 3 adds controls for the employment share in the largest industry, along with controls for the employment shares in the automobile and chemical sector; column 4 adds voting controls; and finally, our preferred specification, in column 5, also includes socio-economic controls for the unemployment share and the share of individuals

over age 65.[14] Column 6 reports the results from our preferred specification as beta coefficients to facilitate comparison with the effects on political outcomes. We present corresponding OLS results in Online Appendix D, table 3. Consistently with our results on voting outcomes, we find that the labor market effects are more pronounced in the IV. Again, this suggests the endogeneity concern that imported inputs into Germany's supply chain can increase because of booming domestic production. As before, we focus on the coefficients on trade exposure and report the coefficients on all controls in Online Appendix D, table 4.

In the first row of table 5, the outcome is changes in the share of manufacturing employment in total employment. This has been the key variable in previous research on the labor market effects of the rise of China and Eastern Europe. We use this specification to cross-validate our data and, reassuringly, we find effects that closely resemble previous findings reported for German data in Dauth et al. (2014).[15] Import competition has a significant and negative effect on manufacturing employment. In our preferred specification, in column 6, a one-standard-deviation increase in $\Delta NetExposure_{Git}$ (€1,350) decreases the share of manufacturing employment by around 1 percent ($-0.75 \cdot 1.35$), roughly three-quarters of Germany's average by-decade decrease of 1.3 percent over the period.

We also find evidence that trade exposure implies wage cuts in manufacturing industries. However, these cuts in manufacturing wages are economically very small: a one-standard-deviation increase in net imports decreases manufacturing wages by 0.7 percent.[16] If all laid-off manufacturing workers found non-manufacturing jobs in the same region, we should perhaps see wage reductions outside of manufacturing. But we do not. Instead, we see that a one-standard-deviation increase in net imports increases the unemployment rate by 0.15 percent and depresses total employment growth by about 3 percent ($e^{-0.024 \cdot 1.35} - 1 = -0.0319$).

We are also concerned about migration, because it could have a purely compositional effect on

[14] In tables 2 and 5, we run two separate two-staged least squares systems that share the same instrument. Because of this, we use the exact same set of controls in both tables, adding some potentially irrelevant social and voting controls to the labor market specifications in columns 4–5 of table 5. As a result of this minor simplification, there are no efficiency gains from estimating the two equations jointly in *seemingly unrelated regressions* (SUR) (Wooldridge (2002, p. 143-146)).

[15] Some differences arise because (i) our period-windows differ slightly–being determined by national election dates–and (ii) we add more controls, and (iii) because of minor differences in the way we constructed our data (see Online Appendix B.3 for details). Despite these small differences, our results are qualitatively the same.

[16] By contrast, in the U.S. data, import competition appears to depress non-manufacturing wages but not manufacturing wages (Autor et al. (2013), Table 7), suggesting more downward wage rigidity in U.S. than in German manufacturing.

Table 5: Effect of $NetExposure_{Git}$ on Labor Markets and Demographics

	(1) Baseline IV	(2) + Structure IV	(3) + Industry IV	(4) + Voting IV	(5) +Socio IV	(6) Standard. IV
Δ Share Manufacturing Employment	-0.440**	-0.618***	-0.738***	-0.745***	-0.755***	-0.247***
	(-1.979)	(-3.098)	(-3.601)	(-3.677)	(-3.745)	(-3.745)
Δ log(Mean Manufacturing Wage)	-0.006**	-0.005**	-0.006**	-0.005**	-0.006***	-0.083***
	(-2.496)	(-2.145)	(-2.466)	(-2.501)	(-2.592)	(-2.592)
Δ log(Mean Non-Manufacturing Wage)	-0.005***	-0.002*	-0.002	-0.001	-0.001	-0.015
	(-2.864)	(-1.666)	(-1.027)	(-0.785)	(-0.808)	(-0.808)
Δ log(Total Employment)	-0.023***	-0.024***	-0.025***	-0.025***	-0.024***	-0.207***
	(-2.853)	(-3.131)	(-3.203)	(-3.239)	(-3.295)	(-3.295)
Δ Share Unemployment	0.076	0.097	0.076	0.084	0.110*	0.060*
	(1.100)	(1.540)	(0.918)	(1.031)	(1.694)	(1.694)
Δ log(Total Population)	-0.009***	-0.007***	-0.006**	-0.005**	-0.004*	-0.050*
	(-3.108)	(-2.903)	(-2.381)	(-2.254)	(-1.852)	(-1.852)
Δ Share High Skilled	-0.253***	-0.184**	-0.143**	-0.133**	-0.139**	-0.156**
	(-3.448)	(-2.404)	(-2.434)	(-2.323)	(-2.555)	(-2.555)
First Stage:						
Δ Import $Exposure_{Git}$	0.225***	0.234***	0.221***	0.220***	0.220***	0.220***
	(8.220)	(8.350)	(7.816)	(7.966)	(7.971)	(7.971)
Δ Export $Exposure_{Git}$	-0.211***	-0.212***	-0.208***	-0.201***	-0.202***	-0.202***
	(-8.519)	(-8.251)	(-8.065)	(-7.660)	(-7.568)	(-7.568)
F-Stat of excluded Instruments	43.81	43.64	40.15	38.77	38.21	38.21
Period-by-region FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	730	730	730	730	730	730

Notes: Each cell reports results from a separate instrumental variable regression. Column 1 controls only for start-of-period manufacturing. Column 2 adds controls for the structure of the workforce (share female, foreign and high-skilled). Column 3 adds controls for dominant industries (employment share of the largest industries, automobiles, and chemicals). Column 4 adds start-of-period voting controls. Column 5 adds socioeconomic controls at the start of the period (population share of unemployed individuals and individuals aged 65+). This is our preferred specification. Finally, Column 6 presents our preferred specification with standardized outcome variables to facilitate comparison. The data is a stacked panel of first-differences at the county level. The panel comprises 322 districts in West Germany, observed in 1987–1998 and 1998–2009, and 86 districts in East Germany, observed in 1998–2009. We drop three city-states (Hamburg, Bremen, and Berlin in the East). All standard errors are clustered at the level of 96 commuting zones. All specifications include region-by-period fixed effects. For outcomes in logs, the table reports on a semi-elasticity: For example, a one-standard-deviation increase in $\Delta NetExposure_{Git}$ (€1,350) decreased total employment by about 3 percent, ($e^{-0.024 \cdot 1.35} - 1 = -0.032$). *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Unpacking by Sub-Samples: Labor Market Outcomes; Equivalent to Table 3

	(1) Manuf	(2) log(mean Manuf. Wages)	(3) log(Non-Manuf. Wages)	(4) log(Total Empl.)	(5) Share Unempl.	(6) log(Pop.)	(7) Share High Skilled
	IV	IV	IV	IV	IV	IV	IV
Panel A: Period 1							
Δ Net Exposure$_{Git}$	-0.324	0.007**	0.005*	-0.027	0.079	-0.006	0.005
	(-0.896)	(2.563)	(1.662)	(-1.600)	(0.883)	(-1.341)	(0.114)
Panel B: Period 2							
Δ Net Exposure$_{Git}$	-0.400**	-0.002	0.001	-0.011	-0.079	-0.001	-0.021
	(-2.350)	(-0.607)	(0.433)	(-1.311)	(-1.112)	(-0.502)	(-0.551)
Panel C: Period , West only							
Δ Net Exposure$_{Git}$	-0.572***	-0.005	-0.001	-0.019**	-0.055	-0.002	-0.014
	(-3.069)	(-1.406)	(-0.337)	(-1.990)	(-0.823)	(-0.905)	(-0.412)
Panel D: Period 1							
Δ Import Exposure$_{Git}$	-0.791**	0.006*	0.006*	-0.044**	0.202**	-0.008	-0.001
	(-2.076)	(1.760)	(1.812)	(-2.435)	(2.286)	(-1.295)	(-0.011)
Δ Export Exposure$_{Git}$	-0.353	-0.009**	-0.004	0.001	0.099	0.003	-0.013
	(-0.907)	(-2.018)	(-0.865)	(0.079)	(0.755)	(0.528)	(-0.210)
Panel E: Period 2							
Δ Import Exposure$_{Git}$	-0.432**	-0.004	-0.001	-0.012	-0.077	-0.002	-0.018
	(-2.462)	(-1.143)	(-0.346)	(-1.344)	(-1.080)	(-0.569)	(-0.466)
Δ Export Exposure$_{Git}$	0.623***	0.016***	0.015***	0.013	0.061	0.003	0.006
	(2.723)	(3.384)	(3.434)	(1.381)	(0.664)	(0.785)	(0.095)
Panel F: Period 2, West only							
Δ Import Exposure$_{Git}$	-0.607***	-0.007*	-0.003	-0.019**	-0.058	-0.002	-0.014
	(-3.135)	(-1.827)	(-1.084)	(-1.985)	(-0.860)	(-0.859)	(-0.391)
Δ Export Exposure$_{Git}$	0.807***	0.018***	0.017***	0.018*	0.080	0.001	0.015
	(3.331)	(3.758)	(3.905)	(1.688)	(0.934)	(0.452)	(0.261)

Notes: The table reports subsample estimations of the voting outcomes reported in Table 5, Column 5. Panels A–B separate the effect of $\Delta NetExposure_{Git}$ into Period 1 and Period 2. Panel C additionally drops 86 East German counties. Panels D–F repeat the by-period specifications and also distinguish between import and export effects. All specifications include region fixed effects. Standard errors are clustered at the level of 96 commuting zones. *** p<0.01, ** p<0.05, * p<0.1.

voting. For instance, more mobile workers may also be more politically moderate voters. What speaks against the migration mechanism is the small and only marginally significant effect on total population growth. A one-standard-deviation increase in $\Delta NetExposure_{Git}$ would decreases a region's total population by about half a percent ($e^{-0.004 \cdot 1.35} - 1 = -0.005$). We do, however, find some evidence for compositional changes in the workforce. A one-standard-deviation increase in net imports decreases the percentage share of high-skilled workers by just under 0.2 percent ($-0.139 \cdot 1.35$). The decrease in high-skilled workers is consistent with the migration literature that finds a positive relationship between educational attainment and mobility (see Bauernschuster, Falck, Heblich, Suedekum, and Lameli (2014)). One important aspect of the individual-level analysis in section 6.3 is therefore to show that individuals do actually change their voting behavior in response to trade exposure.

Table 6 reports results that are analogous to table 3. We split our sample by period and further decompose $\Delta NetExposure_{Git}$ into $\Delta ImportExposure_{Git}$ and $\Delta ExportExposure_{Git}$. The asymmetry we found for extreme-right voting in table 3 is also present here. As discussed in section 5.2, Germany introduced far-reaching labor market liberalization in the late 1990s. Our interpretation is that the asymmetry between period one and period two responses to trade integration reflects vastly different labor market characteristics in the two periods. Specifically, we see that in 1987–1998, under Germany's rigid old labor laws, wages did not adjust downward with increasing import competition. In fact, import competition had a small but positive effect on wages. Instead of wages adjusting downward in response to competition, it was manufacturing employment and total employment that fell and unemployment that increased. In 1998–2009, by contrast, wage effects have the expected sign, and the negative employment effects of import competition are offset by positive employment effects of export access.

Table 3 showed that the moderating effects of export access are larger in absolute terms than the radicalizing effects of import competition. Correspondingly, table 6 shows that the positive labor market effects of export access are larger than the negative ones of import competition. In fact, for manufacturing's share in employment the relative size of the coefficients ($\frac{-0.607}{0.807}$) is practically the same as that for extreme-right voting in table 3. We view this symmetry between tables 3 and 6 as strongly suggestive of a causal link between trade integration, labor market adjustments, and voter radicalization. In the following, we investigate this link more formally and estimate how much of trade integration's effect on voter radicalization is being mediated by observed labor market adjustments.

6.2 Mediation Analysis on Principal Components

This section formally assesses how much of the effect of $\Delta NetExposure_{Git}$ on voting in table 2 can be explained by the observable labor market adjustments reported in table 5. The fundamental identification problem is that even though we have exogenously determined variation in voting outcomes in table 2 and exogenously determined variation in labor market adjustments in table 5, the relationship between these two effects is not clear. For example, exogenous variation in $\Delta NetExposure_{Git}$ may also cause labor market adjustments that are unobserved, such as increases in perceived job uncertainty and anxiety about the future (Scheve and Slaughter (2001)). If these

unobserved effects are not statistically independent of the observed effects in table 5–in a sense that we precisely define below–then the effect of $\Delta NetExposure_{Git}$ on voting can be wrongly attributed to the observed labor market adjustments.

The econometric mediation analysis we conduct below does not fully resolve this issue. However, it precisely defines the identifying assumptions under which we can estimate the *average causal mediation effect* (ACME) of an observed mediating channel (Imai et al. (2010), Heckman and Pinto (2015)), as well as allowing us to undertake a sensitivity analysis that maps violations of the identifying assumptions into changes in the estimated ACME. The ACME is identified under two assumptions that are referred to as 'sequential ignorability' (Imai et al. (2010)). Using the 'Holland-Rubin potential outcomes' notation of causal inference, where the value of the mediating variable is $M_i(1)$ if treatment equals unity and $M_i(0)$ if treatment status is zero[17], the formal sequential ignorability conditions are:

$$\begin{aligned} \{Y_i(t', m), M_i(t)\} \perp\!\!\!\perp T_i | X_i = x \\ Y_i(t', m) \perp\!\!\!\perp M_i(t) | T_i = t, X_i = x. \end{aligned} \tag{8}$$

The first assumption is the standard one that treatment status is ignorable conditional on controls, i.e. there are no unobserved confounding variables that change with $\Delta NetExposure_{Git}$ (T_i), and affect voting (Y_i) or labor market outcomes (M_i). The second assumption states that the mediator is ignorable conditional on treatment status and controls. This condition requires that no *unobserved* variables affect both voting (Y_i) and labor market adjustments (M_i) (Imai, Keele, Tingley, and Yamamoto (2011)) after conditioning on observable variables that do affect labor market adjustments and voting. While we consider this strong assumption to be reasonable in our context, we will also relax it in the following.

The traditional approach to mediation analysis is to separately estimate a set of linear structural equations

$$\Delta y_{it} = \alpha_1 + \beta_1 T_{it} + \lambda_1^T X_{it} + \epsilon_{1i}, \tag{9}$$

$$M_{it} = \alpha_2 + \beta_2 T_{it} + \lambda_2^T X_{it} + \epsilon_{2i}, \tag{10}$$

[17]The notation conventionally assumes binary treatment, and we adhere to it for notational convenience. However, the analysis does not require a binary treatment. In our case, a high value of $\Delta NetExposure_{Git}$ would correspond to the binary treatment usually assumed.

$$\Delta y_{it} = \alpha_3 + \beta_3 T_{it} + \gamma M_{it} + \lambda_3^T X_{it} + \epsilon_{3i}, \tag{11}$$

and to derive the ACME from equations (10) and (11) as the product of $\hat{\beta}_2 \cdot \hat{\gamma}$ (Baron and Kenny (1986), MacKinnon (2008)), with equation (9) being redundant if (10) is substituted into (11). Under the assumption of sequential ignorability, and as long as the system is linear, the methodology proposed in Imai et al. (2010) is numerically equivalent to the traditional approach, and $\hat{\beta}_2 \cdot \hat{\gamma}$ is an unbiased estimate of the ACME. The advantage of the Imai et al. (2010) framework in a linear regression setting like ours is therefore not the procedure to estimating the ACME per se, but rather the transparency of the underlying identifying assumption of sequential ignorability and–most importantly–the ability to conduct formal sensitivity analysis to examine how violations of the identifying assumptions affect the findings.

Because table 5 contains seven measures of labor market adjustments as potential mediators, we first reduce the dimensionality of the data. This is done via principal component analysis (PCA). The appeal of PCA for our purpose is twofold: First, PCA combines any number of measures of labor market adjustment into aggregated principal components (PCs) that condense labor market conditions into their key characteristics. Second, these principal components are by construction orthogonal to each other so that we can conduct separate mediation analyses for each PC. A choice needs to be made of how many principal components to consider. The standard "Kaiser-Guttman" criterion is to analyze only principal components with an eigenvalue larger than 1. In our data, two PCs pass this criterion. The second column of table 7 shows that these first two principal components together explain 70 percent of the variation in the data.

As statistical constructs, principal components are best interpreted through the lens of their underlying variables' factor loadings, which indicate how strongly every labor market outcome relates to each PC. The first principal component's factor loadings are positive for changes in wages, total population, and unemployment. The second principal component's factor loadings are strongly positive for changes in manufacturing's share of employment and changes in total employment, and negative for unemployment. How should these PCs be interpreted? The urban agglomeration literature offers a highly plausible interpretation: Duranton and Puga (2005) point out that regional specialization has increasingly become "functional" over the last decades, as opposed to "sectoral" in the times before. Functional specialization implies a tendency for head-

quarters and business services to cluster in large cities and for manufacturing plants to cluster in smaller cities. Indeed, Bade, Laaser, and Soltwedel (2003) find a strong trend towards functional specialization in German data from 1976–2002. This implies that regions with smaller cities are those that are likely to be most affected by trade integration because these are Germany's manufacturing centers. Using a classification of German counties (the so-called *siedlungsstrukturelle Kreistypen*) provided by the Bundesamt für Bauwesen und Raumordnung (BBR) (2003) that differentiates urban centers, urbanized regions (i.e. small cities and manufacturing hubs), and rural regions, we can test if this interpretation matches our results.

In unreported regressions of the two principal components on these classification dummies, we find strong support for our interpretation of the PCs. The first PC is significantly positively correlated with urban centers and negatively correlated with urbanized regions and rural regions. The second PC is significantly positively correlated with urbanized regions and negatively correlated with urban centers and rural regions. The first PC thus appears to characterize regions that transitioned towards agglomerated urban centers with increasing shares of white-collar workers, higher wages, and population growth. The second principal component appears to characterize regions specialized in manufacturing production.

Another perspective on the principal components emerges from the asymmetry in labor market responses across the two periods, as reported in table 5. Unreported by-period correlations reveal that both principal components correlate strongly with almost all the indicators of labor market adjustments in both periods (correlation coefficients always have the same sign as factor loadings in table 7). However, there is one exception is that the first principal component absorbs all of the data variation in wages in the first period. This is important because table 5 Panel D showed that wages responded in the "wrong" way to trade integration in the first period, which we argued reflects Germany's highly regulated labor markets at that time, and correspondingly there was no effect on far-right voting in table 2 Panel D. It is beyond the scope of this paper to fully explain the wage responses to trade integration in table 5 Panel D, or to fully disentangle these from the patterns of urban agglomeration discussed above. We restrict ourselves to noting that both the first-period wage responses and the urban agglomeration forces are entirely captured in the *first* principal component of labor market adjustments, whereas the second principal component appears to be strongly related to manufacturing employment, i.e. the type of labor

Table 7: Principal Component Analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Principal Components		Factor-Loadings				
	Eigenvalue	Eigenvalue - Proportion	Δ Share Manuf. Empl.	Δlog(Mean Manuf. Wage)	Δ log(Mean Non-Manuf. Wage)	Δ Log(Total Empl.)	Δ Share Unempl
1st Princ.Comp.	3.399	0.486	-0.2712	0.4469	0.4674	0.2615	0.4429
2nd Princ.Comp.	1.470	0.210	0.5825	0.0079	-0.1454	0.6715	-0.2481

Notes: Following the "Kaiser-Guttman" criterion, we retain and analyze PCs with an eigenvalue above 1. The first column shows the eigenvalues of the two principal components we retain. The second column shows the share of total data variation they explain. Together, the two PCs explain over 70 percent of the variation in the data ($0.486 + 0.210$). Moving on to factor loadings, the first PC is associated with changes in total population and wages, as well as with unemployment. The second PC is strongly associated with changes in the manufacturing share of employment and in total employment.

market where the impact of trade integration is likely to be the most pronounced.

We now estimate the ACMEs of the two principal components. The identifying conditions require that treatment with trade integration is exogenous. Of course, this identifying condition was required all along and we continue to instrument $\Delta NetExposure_{Git}$ as before with $\Delta ImportExposure_{Oit}$ and $\Delta ExportExposure_{Oit}$. Under the sequential ignorability assumption, we can estimate (10) and (11) independently and calculate the ACME as $\hat{\beta}_2 \cdot \hat{\gamma}$. We estimate this linear structural equation model using generalized method of moments and assume independent moment equations. For all estimations, we employ the same control variables X_{it} of our preferred specification so that the $\hat{\beta}_2^j$ in specification (10)–for principal components $j = \{1, 2\}$–are estimated in the same specification as for the separate labor market outcomes in column 5 of table 5.

Because principal components are by construction orthogonal, we apply the analysis separately for each of the two principal components. Table 8 shows the results of our mediation analysis. Columns 1 and 3 show the results of estimating equation (10) for each of the two principal components. Columns 2 and 4 show the results of estimating equation (11), i.e., the effect of $\Delta NetExposure_{Oit}$ on the change in the vote-share of extreme-right parties, while additionally controlling for either principal component. For each principal component mediator, the ACME is the product of the effect of the exogenous regressor on the principal component and the effect of the principal component on the outcome. For the second principal component (columns 3–4), the point estimate of the ACME is $0.060 = -0.333 \cdot -0.180$. The share of the total effect of predicted $\Delta NetExposure_{Git}$ that works through this second principal component is simply the ratio of its

Table 8: Mediation Analysis

Dependent:	(1) 1st Principal Component	(2) Δ Vote Share Far-Right Parties	(3) 2nd Principal Component	(4) Δ Vote Share Far-Right Parties
Δ Net Import Exposure$_{Git}$	-0.054*	0.091**	-0.333***	0.024
(Instrumented)	(-1.77)	(2.15)	(-3.61)	(0.57)
Principal Component		0.027		-0.180***
(1st in col 2, 2nd in col 4)		(0.38)		(-4.37)
ACME of Principal Comp.	-0.001		0.060***	
	[-0.009 , 0.006]		[0.021 , 0.099]	
% of Tot Eff mediated	-1.12		67.22	
Rho at which ACME = 0	0.020		-0.214	

Notes: The ACME is calculated as the product of the effect of the exogenous regressor on the mediator and the effect of the mediator on the outcome, i.e., $0.06 = -0.333 \cdot -0.180$. The percentage of the total effect that is mediated by the PC equals the ACME divided by the total effect. All standard errors are clustered at the level of 96 commuting zones. The top panel reports t-statistics, the bottom panel reports confidence intervals below the point estimates.

ACME and the total effect, which is 0.089, the estimated coefficient in column 5 of table 2. About 67 percent ($\frac{0.060}{0.089}$) of the total effect of trade integration on the extreme right's vote share is explained by changes in the second principal component. By contrast, the first principal component is barely significantly affected by $\Delta NetExposure_{Git}$ in column (1), and does not explain extreme-right voting in column (2). As a result, the confidence interval of its ACME includes zero. Our interpretation is that the irrelevance of the first principal component as a mediator is due to the two features discussed above. Namely, it captures a process of urban agglomeration in general and the positive wage responses to trade integration in 1987–1998.

How sensitive is the 67 percent estimate of the mediation channel to violations of the identifying assumptions? We address this question in a formal sensitivity analysis that calculates how the ACME would change for a given degree of violation of the identifying assumptions. Violations of the sequential ignorability assumption imply a correlation between ϵ_{2i} in (10) and ϵ_{3i} in (11), i.e. $\rho \neq 0$. The sensitivity analysis examines how the ACME differs from $\hat{\beta}_2 \cdot \hat{\gamma}$ for a given degree of correlation between these error terms.[18] As shown in the bottom of table 8, the correlation coefficient between the two error terms would have to be -0.2230 for *none* of the effect of

[18]This is related to Conley, Hansen, and Rossi (2012) who also compute different values of an unidentified sensitivity parameter to assess how violations of the exclusion restriction of a "plausibly exogenous" instrument affect the results of an instrumental variable strategy.

trade integration on extreme-right voting to be explained by labor market adjustments.[19] At a correlation coefficient of 0.093, the *entire* effect of $\Delta NetExposure_{Git}$ on extreme-right voting would be explained by labor market turmoil. Online Appendix D figures 1 and 2 show how the ACMEs of both principal components vary with violations of sequential ignorability.

6.3 Worker-level Evidence

Because we have so far measured trade exposure's effect averaged over local labor markets, our results up to here do not tell us whether individuals turn towards the extreme-right parties because they are personally affected by trade integration or because they see their socio-economic environment being affected by trade integration. In this section, we use individual-level data from the German Socio-Economic Panel (SOEP) to differentiate between individuals' personal exposure to trade integration and their indirect exposure via adjustments in the local labor market environment. We are able to separate both effects because the two measures of trade exposure–to be defined below–are practically uncorrelated, with a correlation coefficient of only 0.05.

The SOEP is an annual household survey that started in 1984 (GSOEP (2007)). Since 1990, the SOEP contains consistent information on individual employment by two-digit industry code, and, most importantly, an annual survey question about individual voting intentions that allows us to distinguish between (intended) votes for established parties, far-left parties, extreme-right parties, and other small parties in the same way as before. We again consider two periods. Relative to before, period one is shortened to 1990–1998 because the survey question on voting intentions was not asked before 1990. Period two covers 1998–2009 as before. Exposure to local labour market shocks is measured in the same way as before using the trade shock $\Delta NetExposure_{Grt}$, i.e. with 1987 as initial year.[20] The results are not sensitive to redefining the initial year to be 1990. Similar to Autor, Dorn, Hanson, and Song (2014), we measure individual i's trade exposure in industry j as

$$TradeExposure_{Git} = \frac{\text{Imports}_{jt}^{\text{G}\leftarrow\text{East}}}{\text{Employment}_{jt}} - \frac{\text{Exports}_{jt}^{\text{G}\leftarrow\text{East}}}{\text{Employment}_{jt}} \tag{12}$$

where $\text{Imports}_{jt}^{\text{G}\leftarrow\text{East}}$ and $\text{Exports}_{jt}^{\text{G}\leftarrow\text{East}}$ refer to national trade flows in industry i between Ger-

[19] To give a sense for how large this correlation is, it is in absolute terms roughly equal to the positive correlation coefficient between our two main labor market outcomes, changes in total employment and in manufacturing's share of employment.

[20] Subscript r now refers to the county as relevant local labor market and subscript i denotes individuals.

many and China plus Eastern Europe. Trade flows are expressed in €1,000 at 2005 prices and normalized to be in per-worker terms. Following the same logic as before, we instrument this measure with the same industry's measure of trade flows between other countries that are similar to Germany and China plus Eastern Europe, using the same set of eight other countries as before. Also as before, we lag the initial industry affiliation by one period when calculating the instruments, to 1989 and 1997, respectively.[21] We instrument $\textit{TradeExposure}_{Git}$ with $\frac{\text{Imports}_{jt}^{\text{O}\leftarrow\text{East}}}{\text{Employment}_{jt-1}}$ and $\frac{\text{Exports}_{jt}^{\text{O}\leftarrow\text{East}}}{\text{Employment}_{jt}}$, and $\Delta\textit{NetExposure}_{Git}$ is instrumented with $\Delta\textit{ImportExposure}_{Oit}$ and $\Delta\textit{ExportExposure}_{Oit}$ exactly as before.

Our outcome of interest is the cumulative number of years that an individual expressed support for a given established party or group (left, right, or small). The specific question we code asks, "*if there was an election today, who would you vote for?*" Because respondents do not always respond to every question in every year, we normalize this cumulative number (which takes on values 1–8 in period one and 1–11 in period two) by the cumulative number of years where a respondent states a preference. For each party or party-grouping j, this gives us a measure $\textit{Cumulative Political Support}_{it}^{j}$ that varies between 0 and 1 for every individual in each of the two periods. Our core regression is

$$\textit{Cumulative Political Support}_{it}^{j} = \alpha^{j}\textit{TradeExposure}_{Git} + \beta^{j}\Delta\textit{NetExposure}_{Grt} + \gamma^{j}X_{it-1} + \epsilon_{it}. \quad (13)$$

As before, the data is organized as a stacked panel. There are 3,332 'person-decade' observations. A number of factors limit the observations to that number. First, we need to observe an individual over a full period, i.e., from either 1990–1998 or 1998–2009 or both. Individuals who enter or exit the SOEP inside one of these windows are dropped. Second, individuals who are unemployed or not in the workforce in either baseline year are excluded from the data. This amounts to roughly half of all observations, reflecting Germany's labor force participation rate of roughly 50 percent. Third, a sizeable portion of employed individuals do not state their industry of employment. Fourth, among those who do, only about one-third are employed in manufacturing, and can be assigned a measure of trade exposure as defined in equation (12).

[21] In the local labor market results, every observation is assigned a specific value of $\Delta\textit{NetExposure}_{Git}$ since shocks were measured for each labor market separately. By contrast, at the individual level, the number of different values of $\textit{TradeExposure}_{Git}$ is only 27 per period because shocks are defined at the industry-of-employment level and the SOEP has a coarse 2-digit industry classification that leaves us with no more than 27 manufacturing sectors.

Table 9: Individual-Level Analysis

	(1) CDU/CSU IV	(2) SPD IV	(3) FDP IV	(4) Green Party IV	(5) Right IV	(6) Left IV	(7) Small IV
Panel A:							
Trade Exposure$_{Git}$	0.000	0.001	0.000	-0.003	0.001*	-0.000	0.001
	(0.45)	(0.76)	(0.23)	(-1.5)	(1.86)	(-0.57)	(1.12)
Δ Net Exposure$_{Grt}$	0.004	0.003	0.001	-0.004	0.003**	0.003*	-0.001
	(0.66)	(0.33)	(0.46)	(-1.31)	(2.01)	(1.92)	(-0.59)
Panel B:							
Trade Exposure Imp$_{Git}$	0.000	0.001	-0.000	-0.003	0.001*	-0.000	0.001
	(0.62)	(0.65)	(-0.50)	(-1.58)	(1.68)	(-0.14)	(1.11)
Trade Exposure Exp$_{Git}$	0.002	0.019**	-0.002	-0.006*	-0.002	-0.000	-0.001
	(0.30)	(2.08)	(-0.83)	(-1.85)	(-0.91)	(-0.18)	(-0.24)
Δ Imp Exposure$_{Grt}$	0.022	-0.043	-0.001	0.000	0.017**	0.012	-0.002
	(0.81)	(-1.31)	(-0.13)	(0.03)	(2.02)	(1.45)	(-0.59)
Δ Exp Exposure$_{Grt}$	-0.014	0.007	-0.000	0.007	-0.010**	-0.008*	0.002
	(-0.84)	(0.29)	(-0.11)	(0.98)	(-2.06)	(-1.73)	(0.76)

Notes: The outcome in this table is *Cumulative Political Support*$_{it}^{party}$, the proportion of years in a given period (i.e., 1990–1998 or 1998–2009), that an individual expressed support for a party of a given spectrum. Because the shock is defined over two time windows, baseline controls are measured for 1990 and 1998 respectively. We control for reported initial party support, age, whether foreign-born, number of years in current job, and educational attainment. The number of observations is 3,332 individual-decade pairs. We include individual fixed effects, and standard errors are clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1.

If individuals' political preferences are primarily shaped by their personal exposure to a local labor market shock, the α^j should be significantly different from zero for at least some parts j of the political spectrum. If, on the other hand, changes in political preferences are primarily shaped by voters' socio-economic environment, i.e. the impact of local labor markets' trade exposure on their neighbors, we should see larger estimates of β^j.

Panel A of table 9 reports the results. For ease of comparison we report beta-coefficients, i.e. we report on the effect of a one-standardized-deviation change in either measure of trade exposure. We do not observe turnout in the SOEP, but otherwise consider the same outcomes as before. The effect of *TradeExposure*$_{it}$ on individuals' voting behavior exhibits strikingly similar patterns to our core results in table 2. We see no significant effects on voting for any of the established parties, the left or other small parties. Only voting for the extreme-right parties responds. A one-standard-deviation increase in *TradeExposure*$_{Git}$ raises the cumulative probability that an individual would vote for the extreme-right by a little over 0.1 percent. While this effect is small in absolute terms, it corresponds to over twenty percent of the extreme right's overall support in the

SOEP; as the mean of *Cumulative Political Support*$_{it}$ is 0.49 percent (0.0049) for the extreme right. The local labor market measure of trade exposure also affects voting, and the effects are again concentrated on the extreme right, with some effect also on the far left. A one-standard-deviation increase in Δ*NetExposure*$_{Git}$ raises the cumulative probability that an individual would vote for the extreme-right by a little over 0.3 percent. Panel B of table 9 repeats the exercise of splitting up the trade effects into import exposure and export access in a way that is analogous to table 3. Again, we find similar results on the individual level. Both measures of import exposure have radicalizing effects, while both measures of export access have moderating effects. Remarkably, the environment's trade exposure has a much larger effect on extreme-right voting than an individual's personal trade exposure, in fact almost three times larger in Panel A ($0.0034/0.0012 = 2.83$). The evidence therefore suggests that at least in this context voters adjust their voting behavior in large part because of what they observe in their socio-economic environment, and only to a lesser degree because of their own economic situation being affected.

7 Discussion & Conclusion

To the best of our knowledge, this is the first paper to causally identify the effects of trade integration on voting behavior. We find a significant positive effect of increasing trade exposure on the vote shares of extreme-right parties, while other parties' vote shares are not affected. We find similar effects of individual-level trade exposure on individual election decisions. We show that about two-thirds of the effect of trade-integration on voting is explained by labor-market adjustments.

While theory and empirical evidence mostly agree that trade integration has positive aggregate welfare effects, it also creates distributional frictions between its winners and losers. We expand this notion by investigating the political externalities of increasing trade integration. Broadly interpreted, our results suggest that for a country faced with disproportionate increases in import competitions over export access–for example a country whose trading partners are experiencing positive supply shocks but not expanding their own consumption–the success of trade integration could undermine its own political support if enough individuals experienced negative shocks and cast joint anti-globalization votes.

While in the German context our focus is on votes for extreme-right parties, a broader in-

terpretation of our results is as an increase in the anti-globalization vote. A political backlash also does not require anti-globalization parties to ascend to power. For the case of Germany, the comparatively small increase in the absolute number of extreme-right votes clearly rules out this possibility. However, the German example also illustrates that mainstream parties can be pulled in by anti-globalization sentiment. For example , competition from extreme-right parties in the early 2000s made the governor of a CDU-led state come up with the election slogan "our children instead of Indian immigrants" (*Kinder statt Inder*) as direct opposition against work visas for foreign high-skilled workers. This illustrates that competition for votes on the extremes can lead to a less tolerant climate in a society as a whole.

On a positive note, our results not only suggest that increased import competition radicalizes voters. They also show that better export access has moderating effects. Indeed, in the specific context of Germany we find the moderating effects of export access to exceed the radicalizing effects of import competition. To some extent this is due to the fact that local labor markets in our data ran "trade surpluses" (as measured by $\Delta NetExposure_{it}$) on average. To a larger extent, it is due to the fact that the moderating effect of one extra Euro of export access is larger than the radicalizing effect of the marginal Euro of import competition. Our interpretation–informed by the similar relative magnitudes of labor market adjustments–is that Germany's very flexible labor market arrangements after labor market deregulation in the late 1990s allowed labor unions to be most flexible in those regions where adverse shocks hit the hardest. Both Germany's balance of trade and its labor market institutions help explain why Germany–unlike many European countries–has not seen a secular rise trade integration coincide with a secular rise in the popularity of its extreme-right parties over the past two decades.

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Online Appendix

to

"Globalization and Its (Dis-)Content: Trade Shocks and Voting Behavior"

Online Appendix A Background on Germany's Extreme-Right Parties

Online Appendix A.1 The Extreme-Right in West Germany

There is a strong sense of historical cultural roots and their time-persistence when it comes to explaining votes for far-right parties in Germany today. Mocan and Raschke (2014) use state-level survey aggregates from the ALLBUS, a general population survey for Germany, to show that people who live in states that had provided above-median support of the Nazi party in the 1928 elections have stronger anti-semitic feelings today. Voigtländer and Voth (2015) use the same data to show that the effects of historical antisemitic attitudes on today's political attitudes was amplified for the cohorts that grew up during Nazi Germany's indoctrination programs in 1933–1945.

Having said that, there is substantial time-variation in the popularity of the far-right in Germany. The NPD, the oldest of the three major right-wing parties we consider, was founded in 1964 as the successor to the German Reich Party (DRP). Its goal was to unite a number of fragmented far-right parties under one umbrella. Between 1966 and 1968, the NPD was elected into seven state parliaments, and in the 1969 federal election it missed the 5 percent minimum threshold by just 0.7 percentage points. Afterwards, support for the NPD declined and it took the NPD more than 25 years to re-enter state parliaments in Saxony (2004) and Mecklenburg-Western Pomerania (2006). In both states, the party got reelected in the subsequent elections, in 2009 and 2011, respectively. In 2001, the federal parliament brought in a claim to the German Constitutional Court to forbid the NPD due to its anti-constitutional program. The claim was turned down in 2003 because the NPD's leadership was infiltrated by domestic intelligence services agents, which caused legal problems. On December 7th 2015, the German Constitutional Court opened new proceedings aimed at the prohibition of the NPD.

The DVU was founded by publisher Gerhard Frey as an informal association in 1971. Frey published far-right newspapers such as the German National Newspaper (DNZ) and a number of books with the goal of mitigating Germany's role in WWII. His reputation as a publisher of far-right material helped Frey to become an influential player in the German postwar extreme right scene (Mudde (2000)). In 1986, Frey took it one step further starting his own far-right party German List (*Deutsche Liste*). After some name changes, the party became known as German People's Union (DVU) from 1987 on. Since its foundation, the DVU got parliamentary seats in the state assemblies of Brandenburg (1999, 2004), Bremen (1991, 1999, 2003, 2007), Schleswig-Holstein (1992), and Saxony-Anhalt (1998). In 2010, the DVU merged with the NPD.

The Republicans (Die Republikaner) were founded in 1983 as an ultraconservative breakaway from the Christian Democratic Union (CDU) and the Christian Social Union of Bavaria (CSU). Under their leader, Franz Schönhuber (who also ran as a candidate for the DVU and NPD in his later political career), the party moved further to the extreme right by propagating a xenophobic view on immigrants, and particularly asylum seekers. Compared to the NPD and DVU, the Republicans were considered to be less openly extreme right which helped it secure votes from the ultraconservative clientele. The REP got parliamentary seats in Berlin's senate (1989) and the state parliament of Baden-Wuerttemberg (1992, 1996).

Online Appendix A.2 The Extreme-Right in East Germany after the Reunification

In the first decade after reunification, only the two mainstream parties, CDU and SPD, were able to establish themselves regionwide in East Germany next to the Party of Democratic Socialism (PDS),

the successor of the Socialist Unity Party (SED), which had been ruling the German Democratic Republic till its collapse.

During this time Smaller parties were struggling to put a party infratructure into place in East Germany. Accordingly, while all three extreme-right parties tried to establish themselves in East Germany after reunification, they did not gain major political attention until the late 1990s (Hagan, Merkens, and Boehnke (1995)). At the same time, we saw some of the worst excesses of far-right crime in East Germany in the early 1990s, when migrants' and asylum seekers' residences were set on fire and a mob of people from the neighborhood applauded. Research by Krueger and Pischke (1997) suggests that neither unemployment nor wages can explain these incidences of extreme-right-driven crime from 1991 to 1993. It is more likely that the sudden increase in the number of immigrants and asylum seekers caused these xenophobic excesses in the early 1990s.

In the mid-1990s, the initial euphoria of reunification passed and East German labor markets experienced stronger exposure to international competition. East Germany now faced almost twice as much unemployment as West Germany, and this economic malaise caused feelings of deprivation that often transformed into violent crime against immigrants. Militant right-wing groups declared "nationally liberated zones" in East Germany where foreigners were undesired. In line with that, Lubbers and Scheepers (2001) find that unemployed people have been more likely to support extreme right parties in Germany, and Falk, Kuhn, and Zweimüller (2011) find a significant relationship between extreme-right crimes and regional unemployment levels over the years 1996–1999.[22] The story goes that the political heritage of the GDR may have preserved ethnic chauvinism, which, in in combination with subsequent economic hardship, provided a fertile ground for extreme-right parties. Consequently, far-right parties had turned East German regions into their strongholds by the beginning of the 2000s.

[22]Note that Falk et al.'s (2011) findings do not necessarily contradict Krueger and Pischke (1997) who find no relationship between unemployment and extreme-right-driven crimes. It may very well be that the motivation for crimes changed over the 1990s.

Online Appendix B Data Sources

Online Appendix B.1 Election Data

We focus on federal elections (*Bundestagswahlen*) because the timing of state elections (*Landtagswahlen*) and local elections (*Kommunalwahlen*) varies widely across German regions. Federal elections took place in 1987, in December 1990 after the reunification on October 3, and in 1994, 1998, 2002, 2005, and 2009. We define the first-period outcomes as changes in the vote-share from 1987 to 1998, and second-period outcomes as changes from 1998 to 2009. Election outcomes are observed at the level of 412 districts (*'Landkreis'*) in Period 2 and 322 West German districts in Period 1.

The average vote share of extreme-right parties is persistently below 5 percent in both periods. This presented a major challenge for our data collection, since official election statistics do not report all votes shares below the 5 percent minimum threshold separately by party. To extract information on extreme-right parties form this residual category, we had to contact the statistical offices of the German states and digitize some results from hard copies. By doing so, we have generated a unique data set that provides detailed insight into Germany's political constellation and allows us to create a precise measure of spatial variation in preferences for extreme-right parties. This measure eventually allows us to extend existing studies on spatial variation of extreme-right activities and partisanship that were typically bound to the state level (Falk et al. (2011), Lubbers and Scheepers (2001)) or limited in their time horizon (Krueger and Pischke (1997)) to a new level of detail.

Online Appendix B.2 Trade Data

Our trade data stem from the U.N. Commodity Trade Statistics Database (Comtrade). The database provides information on trade flows between country pairs, detailed by commodity type. As in Dauth et al. (2014), we express all trade flows in thousands and convert them to 2005 Euros. To merge four-digit SITC2 product codes with our three-digit industry codes, we use a crosswalk provided by Dauth et al. (2014), who themselves employ a crosswalk provided by the U.N. Statistics Division to link product categories to NACE industries. In 92 percent of the cases, commodities map unambiguously into industries. For ambiguous cases, we use national employment shares from 1978 to partition them to industries. In this way, we end up with 157 manufacturing industries (excluding fuel products), classified according to the WZ73 industry classification.

Online Appendix B.3 Labor Market Data

We obtain information on local labor markets from two different sources. Information on employment, education, and the share of foreigners stems from the Social Security records in Germany.[23] Based on the Social Security records, we calculate the trade exposure measures for local labor markets, the share of high-skilled workers (with a tertiary degree), foreign workers, workers in the automobile or chemical industry, and wages. For the years before 1999, social security data are recorded at the place of work only. After 1999, place-of-work and place-of-residence information is available.

The remaining variables are provided by the German Federal Statistical Office. These variables include the overall population, the female population share, the population share of individuals

[23] See Bender, Haas, and Klose (2000) for a detailed description of the data from the Institute for Employment Research (IAB). For an additional description of the regional distribution of wages across German municipalities, see Falck, Heblich, and Otto (2013)

of working age (aged 18 to 65), the population share of individuals older than 65, and the unemployment rate, which is calculated by dividing the number of unemployed individuals by the working-age population.

Online Appendix C Structural Decline and Extreme-Right Voting: Examples

The left panel of figure 2 shows two circled regions–South-Eastern Bavaria and Western Palatine–for which we provide more details on how import competition led both to structural decline in manufacturing and to changes in local political attitudes. The circled districts in South-Eastern Bavaria–all bordering Austria or the Czech Republic in the so-called Dreiländereck–are (from southwest to northeast): Rottal-Inn, Passau (with the city of Passau visible in the middle), Freyung-Grafenau, Regen, and Cham. The region is known as traditional manufacturing region specialized in glass products and wood products. These labor-intensive industries were all hit hard by rising international competition which triggered a period of structural change. Today, only a few important players like Nachtmann Crystal A.G. and Schott A.G. have survived this tumult while the vast majority of small firms have disappeared. The years of structural change saw increasing unemployment and an exodus of young and skilled workers, which left the local labor market in tatters. At the same time, the region was known for right-extremist activities that attracted international attention with the near-fatal attack on Passau's police chief in 2008, which was supposedly carried out by neo-Nazis. As reported in the *New York Times* (2009), the police chief "has been known for his hard line against the extreme right, but earned the particular enmity of neo-Nazi groups after ordering the opening of the grave of a prominent former Nazi, Friedhelm Busse, after his death last July. Mr. Busse was buried with a flag bearing a swastika, which is outlawed in Germany, and the police removed the flag as evidence."

The second region highlighted in our map is Southwest-Palatine (*Südwestpfalz*), a region that was characterized by shoe and leather manufacturing firms. Increasing trade integration was a big shock to this region, centered as it was on traditional labor-intensive manufacturing industries. Today, the region–with its two main cities, Pirmasens and Zweibrücken–is considered to be one of the structurally weakest regions in West Germany; it experienced significant outmigration of young and skilled workers. Over the 1990-2006 period, Pirmasens saw a 14 percent decline in population and its unemployment rate in 2005 was at about 20 percent. A study commissioned by the Friedrich Ebert Foundation (Hafeneger and Schönfelder (2007)) investigated (among others) the case of Pirmasens and conducted interviews with local politicians to help define strategies against right-extremist parties in local parliaments. The interviews suggest that the *Republikaner*, who were represented in the city parliament, tried to mobilize voters by explicitly linking the social hardships observed to excessive globalization. In our data, Southwest-Palatine is in the top decile of negatively shocked districts in both periods. In 1987–1998, $\Delta \widehat{NetExposure}_{Git} = 3.62$, while in 1998–2009, $\Delta \widehat{NetExposure}_{Git} = 4.25$ in thousands of constant 2005 Euros per worker. Consistent with this, extreme-right parties increased their vote-share from 1.3 percent in 1987 to 3.45 percent in 2009.

Online Appendix D Robustness and Further Results

Online Appendix D Table 2 reports the coefficients on all controls in our core table 2. The initial share of manufacturing is significantly associated with increases in the extreme-right vote-share over time. In line with that, unreported specifications show that omitting the initial manufacturing share considerably increases the estimated effect of $\Delta NetExposure_{Git}$ on extreme-right voting. While not our focus, this relationship suggests that general structural decline and economic depression provide fertile grounds for extreme-right parties (Arzheimer (2009)). Regions with more educated workers and higher female labor force participation are less prone to shift right. Older demographics appear more prone to vote right, a finding that corroborates qualitative evidence (Stöss (2010)). Finally, high initial vote shares for extreme-right parties imply a reversion in the data, perhaps indicating cyclicality, where past swing voters to the right tend to swing back toward the mainstream. Online Appendix D table 4 reports coefficients on all control variables in the same way for table 5 in the paper.

Online Appendix D table 1 and table 3 are the OLS versions of the paper's tables 2 and 5, respectively.

Figures 2 and 1 show the sensitivity of the mediation effects reported in table 8.

Online Appendix Table 1: OLS Version of Table 2

	(1) Baseline OLS	(2) + Structure OLS	(3) + Industry OLS	(4) + Voting OLS	(5) +Socio OLS	(6) Standard. OLS
Δ Turnout	0.004***	0.003***	0.004***	0.003**	0.003**	0.040**
	(2.932)	(2.669)	(3.059)	(2.337)	(2.430)	(2.430)
Established Parties:						
Δ Vote Share CDU/CSU	-0.081	-0.093	-0.113	-0.062	-0.067	-0.016
	(-1.015)	(-1.204)	(-1.423)	(-0.963)	(-1.020)	(-1.020)
Δ Vote Share SPD	-0.037	-0.035	-0.044	0.061	0.062	0.005
	(-0.416)	(-0.399)	(-0.471)	(0.884)	(0.929)	(0.929)
Δ Vote Share FDP	0.094**	0.114***	0.105**	0.081*	0.088**	0.016**
	(1.971)	(2.672)	(2.398)	(1.805)	(2.097)	(2.097)
Δ Vote Share Green Party	0.046	0.034	0.063*	0.062*	0.068**	0.024**
	(1.221)	(1.016)	(1.755)	(1.835)	(2.042)	(2.042)
Non-established Parties						
Δ Vote Share Extreme-Right Parties	0.038*	0.042**	0.036	-0.009	-0.004	-0.002
	(1.703)	(1.963)	(1.522)	(-0.483)	(-0.240)	(-0.240)
Δ Vote Share Far-Left Parties	-0.108*	-0.105	-0.109	-0.138**	-0.153**	-0.039**
	(-1.669)	(-1.565)	(-1.597)	(-2.159)	(-2.491)	(-2.491)
Δ Vote Share Other Small Parties	0.048	0.042	0.062**	0.003	0.007	0.005
	(1.586)	(1.439)	(2.186)	(0.138)	(0.259)	(0.259)
Period-by-region FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	730	730	730	730	730	730

Notes: T-statistics reported, standard errors are clustered at the level of 96 commuting zones, *** p<0.01, ** p<0.05, * p<0.1.

Online Appendix Table 2: Coefficients on Controls in Table 2

	(1) Turnout	(2) CDU/CSU	(3) SPD	(4) FDP	(5) Green Party	(6) Right	(7) Left	(8) Small
Δ NetExposure$_{Git}$	0.002	-0.066	-0.009	0.119	-0.018	0.089**	-0.092	-0.024
	(1.223)	(-0.501)	(-0.073)	(1.583)	(-0.413)	(2.055)	(-0.859)	(-0.564)
Controls Specification 1:								
Empl-share manufacturing $_{-1}$	-0.000	0.023	0.002	0.009	-0.024***	0.017**	-0.010	-0.017**
	(-1.303)	(1.092)	(0.122)	(0.793)	(-2.932)	(2.407)	(-0.776)	(-2.430)
Controls Specification 2:								
Pop-share college-educated $_{-1}$	0.004***	-0.041	0.131**	-0.055	0.156***	-0.093***	-0.146**	0.049
	(2.920)	(-0.811)	(2.538)	(-1.341)	(3.530)	(-5.032)	(-2.197)	(1.544)
Pop-share foreign-born $_{-1}$	0.001	-0.205***	-0.154*	0.156***	-0.008	0.094***	0.095	0.021
	(0.358)	(-3.020)	(-1.820)	(3.820)	(-0.185)	(3.708)	(1.228)	(0.672)
Pop-share female $_{-1}$	0.011***	0.353**	-0.012	0.056	0.160	-0.262***	-0.325***	0.029
	(3.104)	(2.146)	(-0.064)	(0.534)	(1.475)	(-3.083)	(-2.602)	(0.408)
Employm-share in automotive $_{-1}$	-0.000	0.019	-0.038**	-0.001	0.030*	-0.004	-0.002	-0.004
	(-0.045)	(0.629)	(-2.047)	(-0.091)	(1.827)	(-0.353)	(-0.157)	(-0.475)
Employm-share in chemistry $_{-1}$	-0.000	0.036	-0.050***	-0.013	0.017	0.014	-0.004	-0.002
	(-0.955)	(1.214)	(-3.196)	(-0.889)	(0.915)	(0.821)	(-0.199)	(-0.189)
Employment in largest industry $_{-1}$	0.024	-1.807	2.668**	-1.159	-1.649*	0.077	0.739	1.132**
	(0.810)	(-1.090)	(1.982)	(-1.212)	(-1.704)	(0.084)	(0.569)	(2.071)
Controls Specification 3:								
Unemployment-share $_{-1}$	-0.003**	0.061	-0.034	-0.145***	-0.112***	-0.051	0.347***	-0.066***
	(-2.539)	(0.819)	(-0.431)	(-2.897)	(-2.966)	(-1.467)	(3.576)	(-2.652)
Pop-share above age 65 $_{-1}$	-0.005***	-0.113*	-0.077	-0.013	-0.002	0.079***	0.142***	-0.017
	(-3.461)	(-1.661)	(-1.137)	(-0.314)	(-0.053)	(2.598)	(3.215)	(-0.563)
Voter Turnout $_{-1}$	-0.000	0.073***	0.115***	-0.036*	-0.023	-0.016	-0.061*	-0.052***
	(-0.535)	(2.939)	(4.710)	(-1.740)	(-1.562)	(-1.638)	(-1.934)	(-3.519)
CDU/CSU Voteshare $_{-1}$	-0.025***	-0.255	-0.111	0.222	0.004	-0.635***	0.612***	0.163
	(-4.059)	(-0.987)	(-0.506)	(1.217)	(0.033)	(-4.891)	(3.227)	(1.177)
SPD Voteshare $_{-1}$	-0.010**	-0.119	-0.366*	-0.079	0.142	-0.084	0.064	0.441***
	(-2.327)	(-0.502)	(-1.946)	(-0.521)	(1.310)	(-0.993)	(0.284)	(3.561)
FDP Voteshare $_{-1}$	-0.010**	-0.293	-0.392**	-0.024	0.022	-0.089	0.373**	0.403***
	(-2.411)	(-1.334)	(-2.143)	(-0.161)	(0.218)	(-1.041)	(1.977)	(3.328)
Green Party Voteshare $_{-1}$	-0.010**	-0.081	-0.628***	-0.089	0.026	-0.076	0.440**	0.409***
	(-2.381)	(-0.373)	(-3.599)	(-0.588)	(0.262)	(-0.903)	(2.426)	(3.387)
Far-Right Voteshare $_{-1}$	-0.012***	0.120	-0.488***	-0.225	0.007	-0.098	0.359*	0.324***
	(-2.897)	(0.528)	(-2.643)	(-1.491)	(0.072)	(-1.165)	(1.667)	(2.702)
Far-Left Voteshare $_{-1}$	-0.014***	-0.349	-0.321	-0.127	0.059	-0.091	0.468**	0.359***
	(-3.060)	(-1.572)	(-1.625)	(-0.791)	(0.468)	(-1.021)	(2.338)	(2.885)
Period-by-region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	730	730	730	730	730	730	730	730

Notes: T-statistics reported, standard errors are clustered at the level of 96 commuting zones, *** p<0.01, ** p<0.05, * p<0.1.

Online Appendix Table 3: OLS Version of Table 5

	(1) Baseline OLS	(2) + Structure OLS	(3) + Industry OLS	(4) + Voting OLS	(5) +Socio OLS	(6) Standard. OLS
Δ Share Manufacturing Employment	-0.502***	-0.530***	-0.524***	-0.496***	-0.502***	-0.165***
	(-3.348)	(-3.613)	(-3.486)	(-3.289)	(-3.362)	(-3.362)
Δ log(Mean Manufacturing Wage)	-0.003**	-0.003**	-0.004**	-0.003**	-0.003**	-0.051**
	(-2.122)	(-2.213)	(-2.262)	(-2.094)	(-2.152)	(-2.152)
Δ log(Mean Non-Manufacturing Wage)	-0.001	-0.001	-0.001	-0.000	-0.000	-0.004
	(-0.934)	(-1.244)	(-0.853)	(-0.351)	(-0.433)	(-0.433)
Δ log(Total Employment)	-0.013***	-0.012***	-0.011**	-0.009**	-0.009*	-0.075*
	(-3.138)	(-3.066)	(-2.514)	(-2.070)	(-1.919)	(-1.919)
Δ Share Unemployment	0.089*	0.106**	0.095	0.102*	0.125***	0.068***
	(1.659)	(2.078)	(1.617)	(1.732)	(2.674)	(2.674)
Δ log(Total Population)	-0.003*	-0.002*	-0.001	-0.000	0.000	0.006
	(-1.783)	(-1.688)	(-0.807)	(-0.022)	(0.311)	(0.311)
Δ Share High Skilled	-0.136**	-0.138**	-0.096**	-0.085**	-0.089**	-0.100**
	(-2.354)	(-2.439)	(-2.292)	(-2.050)	(-2.279)	(-2.279)
Period-by-region FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	730	730	730	730	730	730

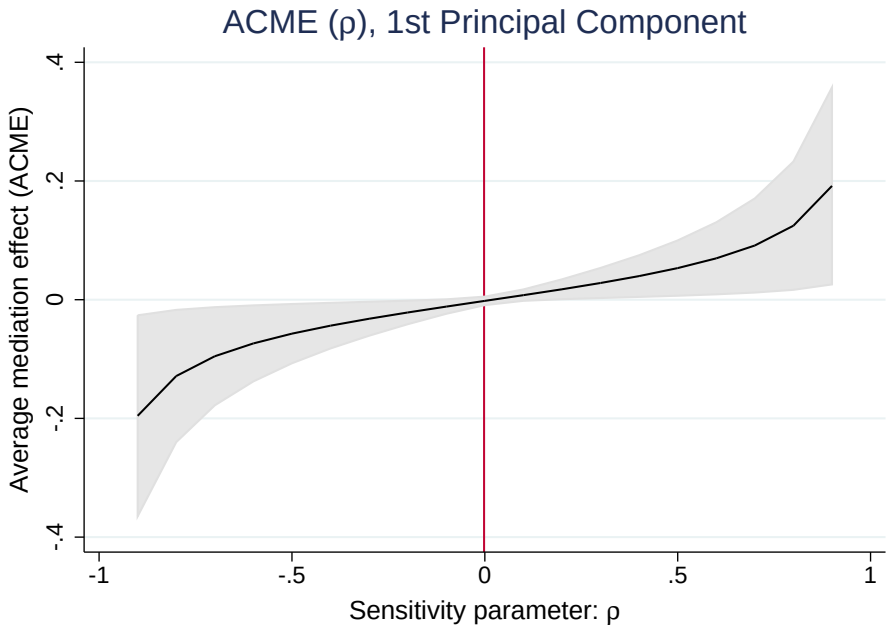
Notes: T-statistics reported, standard errors are clustered at the level of 96 commuting zones, *** p<0.01, ** p<0.05, * p<0.1.

Online Appendix Table 4: Coefficients on Controls in Table 5

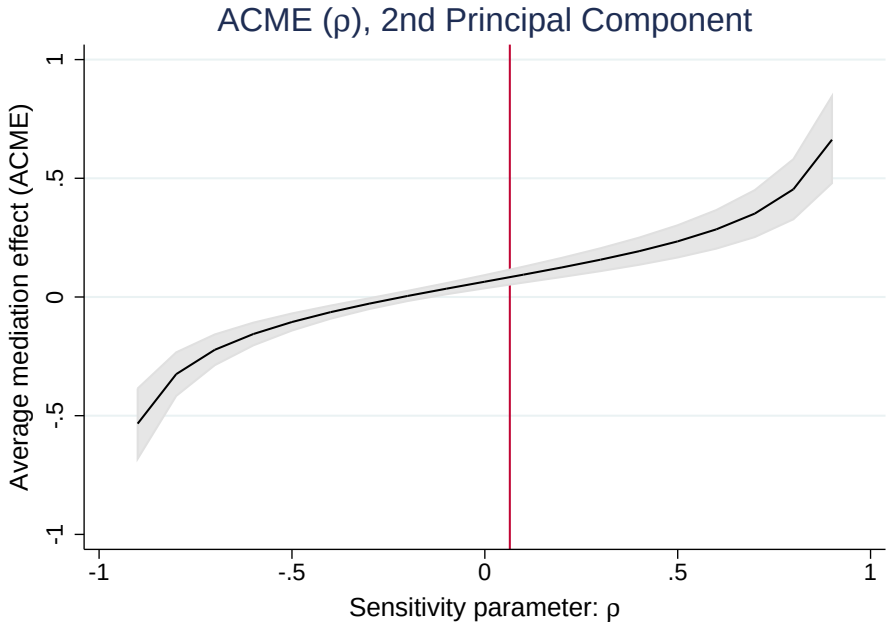
	(1) Manuf	(2) log(Mean Manuf. Wages)	(3) log(Mean Non-Manuf. Wages)	(4) log(Total Empl.)	(5) Share Unempl.	(6) log(Pop.)	(7) Share High Skilled
Δ NetExposure$_{Git}$	-0.755***	-0.006***	-0.001	-0.024***	0.110*	-0.004*	-0.139**
	(-3.745)	(-2.592)	(-0.808)	(-3.295)	(1.694)	(-1.852)	(-2.555)
Controls Specification 1:							
Empl-share manufacturing $_{-1}$	-0.107***	0.001***	-0.001***	-0.001	0.021***	-0.001**	-0.004
	(-4.519)	(2.920)	(-5.459)	(-1.510)	(2.920)	(-2.325)	(-0.625)
Controls Specification 2:							
Pop-share college-educated $_{-1}$	0.067	0.002	0.006***	0.010***	-0.055**	0.006***	0.164***
	(0.850)	(1.631)	(8.164)	(3.876)	(-2.046)	(2.817)	(5.328)
Pop-share foreign-born $_{-1}$	-0.476***	-0.000	0.001	-0.013***	0.156***	-0.004**	0.048**
	(-5.573)	(-0.036)	(1.206)	(-4.329)	(5.373)	(-2.202)	(2.163)
Pop-share female $_{-1}$	-0.062	-0.007**	0.003*	0.005	0.059	0.004	0.048
	(-0.348)	(-2.388)	(1.675)	(0.660)	(0.769)	(1.108)	(0.886)
Employm-share in automotive $_{-1}$	-0.024	-0.001	0.000	0.001	-0.012	0.001	0.032**
	(-0.606)	(-1.524)	(1.259)	(0.965)	(-0.944)	(0.646)	(2.323)
Employm-share in chemistry $_{-1}$	-0.145***	-0.000	0.001*	-0.001	-0.013	0.001	-0.020**
	(-2.713)	(-0.790)	(1.868)	(-0.613)	(-1.179)	(0.785)	(-1.981)
Employment in largest industry $_{-1}$	-0.527	0.018	-0.002	-0.180**	0.601	-0.030	0.492
	(-0.255)	(0.499)	(-0.069)	(-2.009)	(0.965)	(-0.740)	(0.692)
Controls Specification 3:							
Unemployment-share $_{-1}$	0.121	0.003	0.001	-0.010***	-0.374***	-0.013***	0.081***
	(1.484)	(1.575)	(1.001)	(-3.043)	(-7.499)	(-6.321)	(3.684)
Pop-share above age 65 $_{-1}$	-0.036	0.002	-0.002**	-0.018***	0.108***	-0.010***	-0.020
	(-0.586)	(1.420)	(-2.246)	(-6.426)	(3.134)	(-6.665)	(-1.415)
Voter Turnout $_{-1}$	0.051***	-0.000	-0.000	0.000	-0.003	-0.001*	0.000
	(2.753)	(-0.824)	(-0.569)	(0.310)	(-0.267)	(-1.918)	(0.086)
CDU/CSU Voteshare $_{-1}$	-0.276	-0.005	-0.002	-0.007	0.088	-0.000	0.064
	(-1.573)	(-1.559)	(-0.663)	(-0.764)	(0.976)	(-0.014)	(1.269)
SPD Voteshare $_{-1}$	-0.324*	-0.005*	-0.002	-0.010	0.123	-0.001	0.057
	(-1.876)	(-1.800)	(-0.727)	(-1.027)	(1.414)	(-0.251)	(1.150)
FDP Voteshare $_{-1}$	-0.173	-0.006**	0.001	-0.006	0.032	-0.001	0.121**
	(-0.940)	(-2.100)	(0.272)	(-0.614)	(0.362)	(-0.238)	(2.457)
Green Party Voteshare $_{-1}$	-0.487***	-0.002	-0.003	-0.011	0.091	0.002	0.034
	(-2.711)	(-0.493)	(-1.143)	(-1.097)	(0.976)	(0.372)	(0.603)
Far-Right Voteshare $_{-1}$	-0.269	-0.006	-0.001	-0.013	0.231**	-0.003	0.043
	(-1.152)	(-1.591)	(-0.238)	(-1.010)	(2.015)	(-0.520)	(0.578)
Far-Left Voteshare $_{-1}$	-0.383**	-0.011***	-0.004	-0.020**	0.150	-0.001	-0.125**
	(-2.069)	(-2.888)	(-1.545)	(-2.004)	(1.552)	(-0.179)	(-1.974)
Period-by-region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	730	730	730	730	730	730	730

Notes: T-statistics reported, standard errors are clustered at the level of 96 commuting zones, *** p<0.01, ** p<0.05, * p<0.1.

Online Appendix Figure 1: Sensitivity of Mediation Effect of 1nd Principal Component in Table 8



Online Appendix Figure 2: Sensitivity of Mediation Effect of 2st Principal Component in Table 8



Notes: The figures shows sensitivity tests for the average causal mediation effect (ACME) of the two principal components. The first PC (in figure 1) characterizes regions that transitioned towards agglomerated urban centers and the second PC (in figure 2) characterizes regions specialized in manufacturing production. The solid line represents the estimated ACME for different values of the sensitivity parmeter ρ, which measures potential violations of the sequential ignorability assumption. The estimates are enclosed by the 95% confidence interval based on the Delta method. The vertical red line represents the estimated ACME under the assumption of sequential ignorability.