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INSTRUMENTAL VARIABLES AND CAUSAL MECHANISMS: UNPACKING THE EFFECT OF TRADE ON WORKERS AND VOTERS

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ABSTRACT

It is often the case that an endogenous treatment variable causally affects an intermediate variable that in turn causally affects a final outcome. Using an Instrumental Variable (IV) identifies the causal effect of the endogenous treatment on both the intermediate and the final outcome variable, but not the extent to which the intermediate variable affects the final outcome. We present a new testable framework in which a single IV suffices to also estimate the causal effect of the intermediate variable on the final outcome. We use this framework to investigate to what extent German voters responded to the labor market turmoil caused by increasing trade with low-wage manufacturing countries. We first establish that import competition increased voters' support for only extreme (right) parties. We then decompose this populist 'total effect' into a 'mediated effect' running through labor market adjustments and a 'direct effect' of trade exposure on voting behavior. We find the total consists of a large populist effect driven by labor markets and a relatively smaller but moderating direct effect. Our approach provides a template that may be useful in a broad range of empirical applications studying causal mechanisms in observational data.

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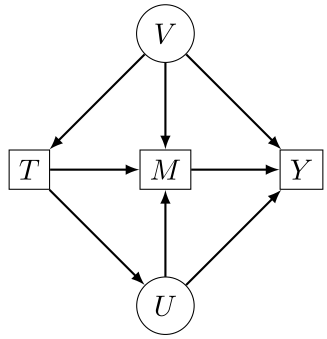
1 Introduction

The nature of international trade has undergone dramatic changes in the past thirty years. Manufacturing trade between high-wage and low-wage countries has risen substantially (Krugman, 2008). This has re-shaped the economy of high-wage countries. A primary consequence of the influx of products from low-wage countries is the loss of manufacturing jobs (Autor, Dorn, and Hanson 2013; Dauth, Findeisen, and Suedekum 2014; Pierce and Schott 2016). At the same time, we observe that trade exposure is associated with growing political polarization and with increasing support for parties that advocate for populist and protectionist agendas (Malgouyres 2014; Feigenbaum and Hall 2015; Autor, Dorn, Hanson, and Majlesi 2016; Jensen, Quinn, and Weymouth 2016; Che, Lu, Pierce, Schott, and Tao 2016).

It is intuitively appealing to assume that labor market adjustments to trade are a main contributor to the observed political backlash. However, the observed correlation among trade, labor market outcomes and voter preferences does not imply causality. There are two main sources of confounding effects that prevent the identification of causal relations. One, unobserved variables may jointly affect the three variables of interest. Two, rising trade exposure may affect an economy through multiple channels that subsequently impact labor markets and politics. We present a testable framework to address this identification problem which allows us to estimate the causal effect of trade-induced labor market adjustments on voting behavior.

Our identification problem is common to a broad class of causal models relying on mediation analysis (Heckman and Pinto, 2015a; Pearl, 2014; Imai, Keele, and Tingley, 2010a). The standard mediation model consists of three observed variables: (1) an endogenous variable T (e.g. trade exposure) that usually denotes a *treatment* status; (2) an intermediate variable M (e.g. labor market adjustments) called *mediator*; and (3) a final outcome Y (e.g. a political outcome). Treatment T causes mediator M which in turn causes outcome Y. A general mediation model allows for selection bias generated by an unobserved variable V that causes T and M and Y. V is a *confounder*. In our empirical context it refers to unmeasured regional characteristics that impact regions' trade exposure, labor markets, and voter preferences. Another source of selection bias arises from an *unobserved mediator* U that is caused by T and causes M and Y. In our empirical analysis, U stands for unmeasured regional economic variables affected by trade. Figure 1 offers a visual representation

Figure 1: Mediation Model with General Confounding Effects



This diagram represents the mediation model with general confounding effects as a Directed Acyclic Graph (DAG). Observed variables are denoted by squares. Unobserved variables are denoted by circles. Variables that share a direct causal relation are connected by arrows. The arrows also inform the the causal direction of this relation.

of the general mediation model.[1]

The general mediation model is hopelessly unidentified. T is endogenous. The observed correlation of T and M (or Y) may arise due to the variation of confounder V instead of the causal effect of T on M (or on Y). M is also endogenous. Finally, V and U induce a correlation between M and Y that could be mistakenly interpreted as the causal effect of M on Y.

To overcome the identification problem, we present a simple framework that employs standard instrumental variables techniques. A large literature in economics uses instrumental variables to evaluate the causal effects of a treatment T on an outcome Y. The primary benefit of instrumental variables is to solve the problem of confounding bias that persists in observational data. A valid instrument Z stands for a variable that causes T and is statistically independent of confounders V. It is well-known that IV allows for the identification of the causal effect of T on the outcome Y (the *total effect*). It also allows for the identification of the causal effect of T on M. Indeed, we may think of M as an intermediate outcome. Our methodological contribution is to show that IV also provides exogenous variation that is useful to evaluate the causal effect of intermediate outcomes on final outcomes.[2] Specifically, we show under which conditions a mediation model with general

[1] The diagram of Figure 1 is called a Directed Acyclic Graph (DAG) which is studied by the literature of Bayesian Networks, e.g. Lauritzen (1996). A DAG is simply a convenient representation of the causal relations defined by the structural equations of a causal model. It does not contain any additional information that is not assessed by its structural equations. See Heckman and Pinto (2015b) for the definition of causal models and further discussions on causality.

[2] A large literature evaluates causal effects on mediation models without instrumental variables (Imai, Keele, and Yamamoto, 2010b; Imai, Keele, Tingley, and Yamamoto, 2011a,b). Identification arises by evoking the strong assumption of no confounding effects.

confounding effects can be identified using a single set of instrumental variables. Our mediation model explicitly allows for the existence of both unobserved confounders and unobserved mediators that bias the results by rendering T and M endogenous. Our identification strategy does not require a second set of instrumental variables for mediator M, which is convenient since this is rarely available in observational data. Instead, we exploit the causal relations between observed and unobserved variables as they are structurally given by our mediation model. We further propose a simple statistic to perform a specification test of the causal model. Finally, we present a step-by-step implementation of the model estimation and the specification test.

Our main empirical contribution is to evaluate the extent to which the recent populist backlash against globalization (Y) is driven by the labor market consequences (M) of trade exposure to low-wage manufacturing countries (T). In a first step, we establish a causal effect of trade exposure on voting behavior. In a second step, we re-affirm the causal effect of trade exposure on local labor market outcomes reported in the literature (Autor et al., 2013; Dauth et al., 2014), showing that trade exposure caused significant labor market turmoil. The third and critical step is to quantify the causal links between these effects in our mediation analysis.

We focus on Germany. Our data combines nationwide changes in sector-specific trade flows with local labor markets' (*Landkreis*') initial industry mix to determine regional trade exposure. Because changes in sectoral trade flows may be driven by unobserved domestic conditions (V), we instrument Germany's changing trade exposure with that of other high-wage countries (Z), following an approach advanced by Autor et al. (2013). This generates a measure of changing trade exposure that is driven by supply changes (productivity or market access increases) in low-wage trading partners, and not by changing domestic conditions in Germany. We focus on the net difference of changes in import competition minus changes in export access. The data is organized as a stacked panel of two first differences for the periods 1987–1998 and 1998–2009, with specific start- and endpoints dictated by national election dates. 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 in 2001 China's accession to the WTO led to another large increase in trade exposure. Taking differences over roughly ten-year intervals makes our results easily comparable with an existing empirical literature on trade and labor markets.

We assess voting behavior Y by looking at the *Landkreis*-level outcomes of federal elections.

Along the entire political spectrum,[3] we find that trade exposure (T) increased voters' support for only the narrow segment of the highly populist extreme right, with no significant effect on turnout, any of the mainstream parties, small parties, or the far left.

We corroborate our regional-level results with individual worker-level data using Germany's Socioeconomic Panel (SOEP). The SOEP is unique among attitudinal socio-economic surveys in having had a panel structure since the 1980s, and in that it surveys party preferences as well as workers' industry and educational background. This allows us to mirror the set-up of our main analysis, relating decadal changes in workers' stated party preferences to changes in their home region's trade exposure over the same time. The individual-level results closely replicate our main results. Particularly, they confirm that trade exposure increases voters' support of extreme-right parties. It is this pro-populist response to trade that we focus on in the remainder of our empirical analysis. In the individual level analysis we can also decompose the main finding by worker type. We find that the effects on right-wing party support are entirely driven by low-skill workers employed in manufacturing, i.e. those most affected by labor market turmoil caused by increasing international trade.

We then apply the same *Landkreis*-level specification again to probe the extent to which trade exposure (T) causes labor market adjustments (M). Our results re-affirm previous studies on this topic: We find that increasing import competition has had large negative effects on manufacturing's employment share, manufacturing wages, and total employment, as well as causing an increase in unemployment. Decomposing the effects by period provides additional reduced form evidence for a relationship between labor market adjustments M and voting behavior Y. Since German labor markets were substantially less regulated in 1998–2009 than in 1990–1998 (Dustmann, Fitzenberger, Schönberg, and Spitz-Oener, 2014), labor market outcomes were far more responsive to trade shocks in the second period. This finding is exactly mirrored in the voting outcomes, where trade exposure affects voting for the extreme right only in the second period.

All these symmetries in the empirical patterns strongly suggest that trade exposure changes voting behavior *because* it affects labor markets but they cannot quantify the importance of this mechanism. We thus apply our proposed method to distinguish between the *direct* causal effect

[3]Election outcomes are divided 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).

of trade exposure on far-right voting and its *indirect* or *mediated* effect that works through labor market mechanisms. In practice, this boils down to performing three straightforward TSLS estimations and then testing whether the causal model we postulate is empirically sound. Perhaps surprisingly, we find that the direct and indirect effects of trade on populist voting work in opposite directions. The indirect effect of trade exposure as it is mediated by labor market adjustments is almost twice as large as the total effect on extreme right party votes. This means that the labor market disruptions caused by trade have had even greater consequences in the political arena than just the inspection of trade effects on voting and labor markets alone could have suggested. Conversely, trade exposure net of its labor market effects had a moderating effect on voters, i.e. it decreased extreme-right party support. An implication is that increasing trade integration is politically moderating if there are no negative labor market consequences. A possibly explanation is that the fracturing of global supply chains and the trade in intermediate products increases the need for coordination and teamwork with foreign producers which may be politically moderating; see (Antras, 2015).[4] Lastly, we test and confirm that the data complies with our model assumptions.

Our substantive focus relates our paper to three strands of the political economy literature. First we relate to a number of recent studies on the relationship between trade and politics. Most of these papers are on U.S. politics and thus focused on either the changing positions of local politicians in response to their congressional district's trade exposure (Feigenbaum and Hall, 2015; Autor et al., 2016) or on the vote-shares of the only two large parties (Jensen et al., 2016; Che et al., 2016). By contrast, the paper closest to ours is Malgouyres (2014) who similarly applies a local labor market identification strategy to find a positive effect of local import competition on the Front National's vote share in France. Second, our paper speaks to a broader literature on the effects of economic shocks on a range of political outcomes including an incumbent's reelection chances (Bagues and Esteve-Volart 2014, Jensen et al. 2016), turnout (Charles and Stephens, 2013), and stated voter preferences for redistribution (Brunner, Ross, and Washington 2011, Giuliano and Spilimbergo 2014). Third, our paper relates to an earlier political science literature on trade and political cleavages (Rogowski, 1987). This literature focuses on political cleavages along factor

[4]Another possible explanation may be that as the international exchange of goods and services grows the cultural exchange across borders increases as well, in turn decreasing the attractiveness of nationalist agendas.

(e.g. occupation) or industry lines and either studies self-reported party preferences amongst voters in survey data (Scheve and Slaughter, 2001) or legislators' voting records on certain types of legislation (Hiscox, 2002). See Rodrik (1995) for an extensive survey. Our methodical contribution ties this literature more closely to the literature on the effects of international trade on local labor markets (Autor et al., 2013; Dauth et al., 2014; Pierce and Schott, 2016).

In the following, section 2 explains our mediation model and lays out our identification approach. Section 3 describes how all variables are measured, discusses the space- and time-dimension of our data, and provides descriptive statistics on the core variables. Section 4 presents the results. First, it documents the causal effect of trade exposure on voting behavior, including a micro-level analysis of workers in the GSOEP. Then, it decomposes trade effects on voting behavior and labor market adjustments. Eventually, we apply our mediation model to unpacking the causal links between trade exposure, labor market adjustments and voting behavior. Section 5 concludes.

2 A General Mediation Model

This section derives the mediation model to be implemented in the subsequent empirical analysis. We will employ this model to identify both the direct effect of trade exposure on voting behavior and its indirect effect on voting behavior mediated by labor market adjustments. We first describe the model in general terms because our identification strategy is applicable to a broad range of empirical applications characterized by an endogenous treatment, the presence of a valid instrument, and the observation of both an intermediate and a final outcome.

The simplest mediation model consists of three observed variables T, M, Y and three statistically independent error terms $\epsilon_T, \epsilon_M, \epsilon_Y$. Causal relations are defined by the following equations:

$$T = f_T(\epsilon_T)\ ,\ M = f_M(T, \epsilon_M)\ ,\ Y = f_Y(T, M, \epsilon_Y). \tag{1}$$

Input variables of functions f_T, f_M, f_Y are said to cause their respective output variables. Thus T causes M and Y while M causes Y. Neither functions f_T, f_M, f_Y nor error terms $\epsilon_T, \epsilon_M, \epsilon_Y$ are observed. We use the notations $supp(T), supp(M), supp(Y)$ for the support of T, M, Y respectively. Figure 2 displays Model (1) as a *Directed Acyclic Graph* (DAG).

Figure 2: Mediation Model without Confounding Variables

$$
\begin{tikzcd}
\boxed{T} \arrow[r] \arrow[rr, bend right] & \boxed{M} \arrow[r] & \boxed{Y}
\end{tikzcd}
$$

Fixing is a key concept in causal analysis. Fixing is defined as the causal operation that assigns a value to an argument of a structural equation. It is used to generate counterfactual variables. For instance, Model (1) renders three counterfactual variables. The counterfactual mediator $M(t)$ is generated by fixing the argument T of function f_M to a value $t \in supp(T)$, that is, $M(t) = f_M(t, \epsilon_M)$. The counterfactual outcome $Y(t)$ for T fixed at t is given by $Y(t) = f_Y(t, M(t), \epsilon_Y)$ and the counterfactual outcome Y when T is fixed at t and M is fixed at m is given by $Y(t,m) = f_Y(t, m, \epsilon_Y)$. We refer to Heckman and Pinto (2015b) for a detailed discussion on the fixing operator, counterfactual outcomes and causal models.

We are interested in identifying the effect of T on outcome Y, but most importantly, we are interested in identifying the mechanism M though which T causes Y. This task is often referred to as mediation analysis and it requires the identification of all three counterfactual variables $Y(t)$, $M(t)$, $Y(m,t)$. Robins and Greenland (1992) examine the case of a binary treatment $supp(T) = \{t_0, t_1\}$ and define three primary causal parameters in mediation analysis: the *total*, *direct* and *indirect* effects.

$$
\begin{array}{llcll}
\text{Total Eff. :} & ATE & = & E(Y(t_1) - Y(t_0)) & \equiv \; E(Y(t_1, M(t_1)) - Y_i(t_0, M(t_0))), \\
\text{Direct Eff. :} & ADE(t) & = & E(Y(t_1, M(t)) - Y(t_0, M(t))) & \equiv \; \int E\Big(Y(t_1, m) - Y(t_0, m)\Big) dF_{M(t)}(m), \\
\text{Indirect Eff. :} & AIE(t) & = & E(Y(t, M(t_1)) - Y(t, M(t_0))) & \equiv \; \int E\Big(Y(t, m)\Big)\Big[dF_{M(t_1)}(m) - dF_{M(t_0)}(m)\Big],
\end{array}
$$

where $F_{M(t)}(m)$ stand for the cumulative probability distribution of counterfactual mediator $M(t)$. ATE is the average causal effect of T on Y. $ADE(t)$ is the causal effect of T on Y when we hold the distribution of M fixed at $M(t)$. $AIE(t)$ is the causal effect of T on Y induced by the change in the distribution of the mediator M. The total effect ATE can be expressed as the sum of direct and indirect effect as:

$$
\begin{aligned}
ATE &= E(Y(t_1, M(t_1)) - Y_i(t_0, M(t_0))) \\
&= \Big(E(Y(t_1, M(t_1))) - E(Y(t_0, M(t_1)))\Big) + \Big(E(Y(t_0, M(t_1)) - Y_i(t_0, M(t_0)))\Big) = ADE(t_1) + AIE(t_0) \\
&= \Big(E(Y(t_1, M(t_1))) - E(Y(t_1, M(t_0)))\Big) + \Big(E(Y(t_1, M(t_0)) - Y_i(t_0, M(t_0)))\Big) = AIE(t_1) + ADE(t_0).
\end{aligned}
$$

Model (1) has no confounding variables. That is to say that model (1) assumes no unobserved variable that jointly causes T, M and Y. This implies that variables T, M are independent of counterfactual outcomes, that is, $T \perp\!\!\!\perp \big(Y(t), M(t)\big)$ and $M \perp\!\!\!\perp Y(t,m)$.[5] These relations need to be satisfied in order to be able to express expected counterfactual outcomes in terms of observed conditioned expectations. We illustrate this fact for the counterfactual outcome $Y(t)$. The observed outcome Y can be expressed as:

$$Y = \sum_{t \in supp(T)} Y(t) \cdot \mathbf{1}[T = t],$$

where $\mathbf{1}[T = t]$ is an indicator function that takes value one if $T = t$ and zero otherwise. If $T \perp\!\!\!\perp Y(t)$ holds then $E(Y(t)) = E(Y(t)|T = t)$ also holds and we can express $E(Y(t))$ as:

$$E(Y(t)) = E(Y(t)|T = t) = E\left(\sum_{t \in supp(T)} Y(t) \cdot \mathbf{1}[T = t]|T = t\right) = E(Y|T = t).$$

The expectation $E(Y|T = t)$ can be evaluated from observed data and thereby $E(Y(t))$ is said to be identified.

Because of its simplicity, model (1) is well-suited to introducing the notion of fixing and coun-

[5] Note that $T = f_T(\epsilon_T)$ depends only on ϵ_T, $M(t) = f_M(t, \epsilon_M)$ and $Y(t) = f_Y(t, M(t), \epsilon_Y)$ only depend on ϵ_M, ϵ_Y. But ϵ_T is independent of ϵ_M, ϵ_Y. Thus we can write:

$$\big(\epsilon_Y, \epsilon_M\big) \perp\!\!\!\perp \epsilon_T \;\Rightarrow\; \big(f_Y(t, f_M(t, \epsilon_M), \epsilon_Y), f_M(t, \epsilon_M)\big) \perp\!\!\!\perp f_T(\epsilon_T) \;\Rightarrow\; \big(Y(t), M(t)\big) \perp\!\!\!\perp T$$

On the other hand, $Y(t,m) = f_Y(t, m, \epsilon_Y)$ only depends on ϵ_Y. Thus we can write:

$$\epsilon_Y \perp\!\!\!\perp \big(\epsilon_M, \epsilon_T\big) \;\Rightarrow\; f_Y(t, m, \epsilon_Y) \perp\!\!\!\perp f_M(f_T(\epsilon_T), \epsilon_M) \;\Rightarrow\; Y(t,m) \perp\!\!\!\perp M.$$

A substantial literature on mediation analysis assumes no confounding variables. This literature often evokes the Sequential Ignorability Assumption of Imai et al. (2010a). Online Appendix A shows that Model (1) also implies Sequential Ignorability.

terfactual outcomes presented above. The main drawback of Model (1) is that it assumes no confounding effects. We do, however, face two essential sources of confounding effects in observational data:

1. A general *confounder* V that is an unobserved exogenous variable that causes T, M and Y.

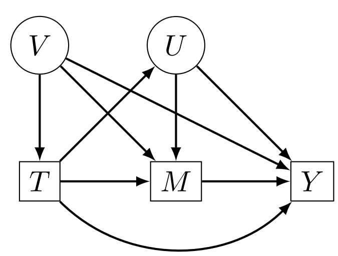
2. The *unobserved mediator* U that is caused by T and causes observed mediator M.

Thus, we need a general mediation model that allows for the presence of both the confounder V and the unobserved mediator M . Such a model is given by:

$$\text{Confounder: } V = f_V(\epsilon_V), \tag{2}$$
$$\text{Treatment: } T = f_T(V, \epsilon_T), \tag{3}$$
$$\text{Unobserved Mediator: } U = f_U(T, V, \epsilon_U), \tag{4}$$
$$\text{Observed Mediator: } M = f_M(T, U, V, \epsilon_M), \tag{5}$$
$$\text{Outcome: } Y = f_Y(T, M, U, V, \epsilon_Y) \tag{6}$$

Unobserved Error terms $\epsilon_V, \epsilon_T, \epsilon_U, \epsilon_M, \epsilon_Y$ are assumed to be statistically independent. This is not a binding constraint because we use unobserved variables V and U to model the dependence structure among variables. Confounder V is called exogenous because it is not caused by any of the variables in the model. Figure 3 represents the general mediation model (2)–(6) as a DAG.

Figure 3: The General Mediation Model with Confounder V and Unobserved Mediator U



It is not surprising that independence relations $T \perp\!\!\!\perp \big(Y(t), M(t)\big)$ and $M \perp\!\!\!\perp Y(t,m)$ do not hold in model (2)–(6). Indeed, unobserved confounder V induces a correlation between the treatment T and the counterfactuals $Y(t) = f_Y(t, M(t), U(t), V, \epsilon_Y)$, $M(t) = f_M(t, U, V, \epsilon_M)$. Confounder V and the unobserved mediator U induce a correlation between M and the counterfactual outcome $Y(t,m) = f_Y(t,m,U(t),V,\epsilon_Y)$. Variables T, M are called endogenous and none of the counterfactual variables $Y(t), M(t), Y(t,m)$ are identified. In particular, $E(Y(t)) \neq E(Y|T=t)$.

A large literature in economics uses instrumental variables (IV) to identify causal effects in the presence of confounders. This motivates us to investigate if IV can be used in a mediation model to identify causal mechanisms in addition to causal effects.

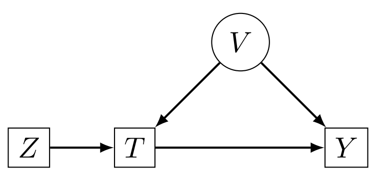
2.1 Adding Instrumental Variables to the Mediation Model

A standard IV model consists of four variables Z, V, T, Y whose causal relations are defined by the following equations:

$$T = f_T(Z,V,\epsilon_T), \quad Y = f_Y(T,V,\epsilon_Y), \quad V = f_V(\epsilon_V), \quad Z = f_Z(\epsilon_Z). \tag{7}$$

The instrumental variable Z causes T and is not caused by the unobserved confounder V. Treatment T is caused by the confounder V and the instrument Z. Outcome Y is caused by confounder V and treatment T. Variables Z, V are exogenous as they are not caused by any variable in the system and error terms are statistically independent. Figure 4 represents the standard IV model as a DAG.

Figure 4: Standard IV Model



The independence relation $Y(t) \perp\!\!\!\perp T$ does not hold in the IV model (7) because the confounder V induces a correlation between $Y(t)$ and T. Nevertheless the IV model renders two properties of the instrument Z that allows for the identification of causal effects:

$$\text{Exclusion Restriction: } Z \perp\!\!\!\perp Y(t) \tag{8}$$

$$\text{IV Relevance : } Z \not\perp\!\!\!\perp T \tag{9}$$

The exclusion restriction (8) is a consequence of the independence between V and Z. The IV relevance states that the instrument Z correlates with treatment T which arises from the causal link between Z and T.[6] This model is sufficient to identify the causal effect of T on Y.

Our goal, however, is to go beyond causal effects and use IV to also identify causal mechanisms. We seek to identify counterfactual outcomes $Y(t), M(t), Y(m,t)$ in a general mediation model with confounding variables. More precisely, we seek for a model that complies with the following list of desirable properties:

1. Allows for confounders and unobserved mediators.
2. Causal Variables T and M are endogenous thus the independence relations $T \perp\!\!\!\perp \big(M(t), Y(t)\big)$ and $M \perp\!\!\!\perp Y(m,t)$ do *not* hold.
3. The model relies on an instrumental variable Z that directly causes T.
4. Even though the instrumental variable Z does not directly cause M, the instrument must be suitable for the identification of three causal relations: (1) $T \to Y$; (2) $T \to M$; and (3) $M \to Y$.
5. The model should *not* require a dedicated instrumental variable that directly causes M because such an additional set of instruments is typically not available in empirical settings.
6. The model must be testable. Specifically, any assumption about the causal relations among observed or unobserved variables that reduces the generality of the mediation model (2)–(6) must be testable.

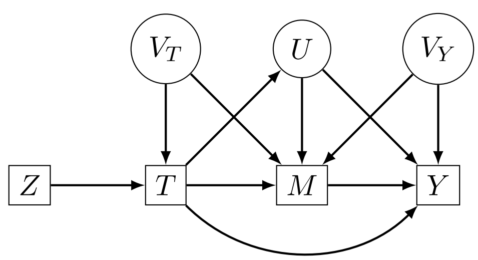
The model defined by Functions (10)–(15) complies with all desirable properties above.

[6] Conditions (8)–(9) are necessary but not sufficient to identify the causal effect of T on Y. There is an extensive literature on the additional assumptions that render the identification of treatment effects. Those assumptions depend on the statistical properties of the instrument Z and the treatment T and functions f_T and f_Y. For example, if T and Z are continuous and functions f_T, f_Y are linear, then causal effects can be evaluated by two-stage least squares. Imbens and Angrist (1994) examine the case of a binary T and identify the Local Average Treatment Effect ($LATE$). Vytlacil (2006) studies the case of categorical T and continuous instruments. Pinto (2015) investigate categorical Z and T and generates choice restrictions using revealed preference analysis. Heckman and Vytlacil (2005) investigate the binary treatment, continuous instruments and assume that the treatment assignment is characterized by a threshold-crossing function. Imbens and Newey (2007); Blundell and Powell (2003, 2004); Altonji and Matzkin (2005) study control function methods that rely on assumptions on the functional forms of f_T, F_Y to generate a variable that controls for the confounding effect of V.

$$\text{Confounders: } V_T = f_{V_T}(\epsilon_{V_T}), \quad V_Y = f_{V_Y}(\epsilon_{V_Y}), \tag{10}$$
$$\text{Instrumental Variable: } Z = f_Z(\epsilon_Z), \tag{11}$$
$$\text{Treatment: } T = f_T(Z, V_T, \epsilon_T), \tag{12}$$
$$\text{Unobserved Mediator: } U = f_U(T, \epsilon_U), \tag{13}$$
$$\text{Observed Mediator: } M = f_M(T, U, V_T, V_Y, \epsilon_M), \tag{14}$$
$$\text{Outcome: } Y = f_Y(T, M, U, V_Y, \epsilon_Y). \tag{15}$$

Unobserved error terms are again assumed to be statistically independent. The mediation model (10)–(15) differs from our general mediation model (2)–(6) in two aspects. First, it decomposes the confounder V in (2) into two unobserved variables: V_T that causes T, M and V_Y that causes Y, M. Second, it adds an instrumental variable Z that is exogenous, that is, it is not caused by any variable in the system. In particular, Z is not caused by confounders V_T, V_Y. Figure 5 represents the mediation model (10)–(15) as a DAG.

Figure 5: Mediation Model with IV, Confounding Variables and Unobserved Mediator



Treatment T and Mediator M are endogenous in the mediation model (10)–(15). Relation $T \perp\!\!\!\perp M(t)$ does not hold due to confounder V_T,; confounder V_Y and unobserved mediator U invalidate $M \perp\!\!\!\perp Y(m,t)$; and the relation $T \perp\!\!\!\perp Y(t)$ is refuted by confounders V_T, V_Y. The model still generates three sets of IV properties for each of the three relevant causal links. Those properties are listed in Theorem **T-1**.

Theorem T-1 *The following statistical relations hold in the mediation model* (10)–(15)*:*

	Targeted Causal Relation	*IV Relevance*		*Exclusion Restrictions*
Property 1	*for* $T \to Y$	$Z \not\perp\!\!\!\perp T$	*and*	$Z \perp\!\!\!\perp Y(t)$
Property 2	*for* $T \to M$	$Z \not\perp\!\!\!\perp T$	*and*	$Z \perp\!\!\!\perp M(t)$
Property 3	*for* $M \to Y$	$Z \not\perp\!\!\!\perp M\|T$	*and*	$Z \perp\!\!\!\perp Y(m)\|T$

Proof P-1 *See Appendix A*

Theorem **T-1** states that our model renders three exclusion restrictions. Property 1 implies that instrument Z can be used to evaluate the causal effect of T on Y. Property 2 implies that Z also allows us to evaluate the causal effect of T on M. These two relations arise from the fact that Z directly causes T and does not correlate with the unobserved confounders. M can be interpreted as another outcome caused by T. Property 3 implies that instrument Z can be used to evaluate the causal relation of M on Y if (and only if) conditioned on T. Indeed, while $Z \perp\!\!\!\perp Y(m)|T$ holds, $Z \perp\!\!\!\perp Y(m)$ does not.

Property 3 arises from the fact that T is caused by both Z and V_T. Unobserved confounder V_T and the observed instrument Z are unconditionally statistically independent. However, conditioning on T induces a correlation between Z and V_T. But V_T causes M and does not (directly) cause Y. Thus, conditioned on T, Z affects M (via V_T) and does not affect Y by any channel other than M.

Property 3 may come as a surprise, and it is useful to interpret the property through the lens of our empirical application. We are interested in the chain of causal effects from trade flows with low wage countries (T) to domestic labor market adjustments (M) to changes in voting behavior (Y). Imports , for instance, may increase exogenously because of productivity growth in (or lower trade barriers with) China and Eastern Europe. But they may also increase endogenously because of stagnant domestic productivity growth (V_T), which may have effects on labor markets also. We follow the standard approach of using other high-wage countries' changing industry-specific imports (exports) from (to) China and Eastern Europe as instruments (Z) for those of Germany (see section 3.4). The argument for this is that Germany's unobserved industry-specific productivity trajectories are unconditionally statistically independent of other high-wage countries' industry-specific changes in trade flows with China and Eastern Europe, i.e. $Z \perp\!\!\!\perp V_T$. However, conditional on Germany's industry-specific changes in trade flows with those countries (T), other high-wage countries' industry-specific changes in trade flows with China and Eastern Europe (Z) should now be positively correlated with German unobserved industry-specific productivity trajectories. For example, if other high-wage countries' imports of Chinese gear wheels increased more than Germany's then the difference is likely partly driven by unobserved productivity growth in German gear wheel manufacturing.

Properties 1 and 2 of **T-1** allow for the identification of counterfactual outcomes $M(t)$ and $Y(t)$ by applying standard IV techniques. A novel feature of our model is the possibility to use Z to identify the counterfactual outcome $Y(m,t)$. Property 3 of **T-1** allows for the identification of the conditional counterfactual $(Y(m)|T = t)$ while Corollary **C-1** states that this conditional counterfactual is equal in distribution to the counterfactual outcome $Y(m,t)$.

Corollary C-1 *In the mediation model* (10)–(15)*, the counterfactual outcome* $Y(m)$ *conditioned on* $T = t$ *is equal in distribution to the counterfactual outcome* $Y(m,t)$*, i.e.,* $(Y(m)|T = t)\overset{d}{=}Y(m,t)$.

Proof P-2 *See Appendix B*

2.2 Model Specification Test

The general mediation model (2)–(6) has only one confounder V that causes T, M and Y (see Figure 3). The IV mediation model (10)–(15) decomposes V into V_T and V_Y (see Figure 5). Multiple confounders do not imply greater generality; indeed, the opposite is true. Confounder V in the general mediation model (2)–(6) jointly causes T and Y, while V_T and V_Y in model (10)–(15) do not. This is a subtle but important distinction.

The assumptions on unobserved variables V_T, V_Y of the adopted mediation model (10)–(15) incur a loss of generality. These assumptions can only be justified if they comply with observed data. Specifically we need a model specification test based on observed data that evaluates the causal restrictions on unobserved confounders. Fortunately the causal assumptions on the unobserved confounders of model (10)–(15) are testable and the test is surprisingly simple.

Our model specification test relies on the fact that the mediation effects in model (10)–(15) are overidentified. The total causal effect of T on Y, that is $E(Y(t))$, can be identified by using Property 1 of **T-1** and proper IV techniques. An alternative method to evaluate $E(Y(t))$ combines Properties 2 and 3 of **T-1** and Corollary **C-1**. Property 2 of **T-1** allows us to identify the distribution of the counterfactual mediator $M(t)$. Property 3 of **T-1** and Corollary **C-1** allow us to identify the distribution of the counterfactual outcome $Y(m,t)$. We can combine these two results and evaluate $E(Y(t))$ by:

$$E(Y(t)) \equiv \int E(Y(t,m))dF_{M(t)}(m). \tag{16}$$

Our goal is to test the loss of generality incurred by assuming no confounding variable that causes both T and Y. Our alternative hypothesis is that there exists an unobserved confounder V that jointly causes T, M and Y (as in the general mediation model (2)–(6)). If the latter hypothesis was true, Properties 1 and 2 of **T-1** would still hold as Z would still be an instrument for T on M and T on Y. As a consequence we could still evaluate the total causal effect $E(Y(t))$ using Property 1 of **T-1**. However, if a confounder V caused T, M and Y then Property 3 would not hold and $E(Y(t))$ could not be identified through Property 3. Thus we can test potential violations of the model assumptions by comparing the total effect $E(Y(t))$ evaluated by Property 1 of **T-1** with the one evaluated through Properties 2 and 3 of **T-1** and Corollary **C-1**. A significant discrepancy between these two values would provide statistical evidence that the assumptions of our model do not hold.

2.3 The IV Mediation Model Under Linearity

This section examines the IV mediation model (10)–(15) under linearity. We present the relevant formulas for causal mediation effects, explain how to evaluate the model and show how to implement the model specification test. Let the structural equations (12)–(15) be defined as:

$$\begin{aligned}
T &= \xi_Z \cdot Z + V_T + \epsilon_T, \\
U &= \zeta_T \cdot T + \epsilon_U, \\
M &= \varphi_T \cdot T + \varphi_U \cdot U + \delta_Y \cdot V_Y + \delta_T \cdot V_T + \epsilon_M, \\
Y &= \beta_T \cdot T + \beta_M \cdot M + \beta_U \cdot U + V_Y + \epsilon_Y,
\end{aligned}$$

where $\beta_T, \beta_M,\ \beta_U, \varphi_T,\ \varphi_U, \delta_Y,\ \delta_T, \xi_Z, \zeta_T$ are scalar coefficients and $Z, V_T, V_M,\ \epsilon_T, \epsilon_U, \epsilon_M, \epsilon_Y$ are mean-zero mutually independent variables.

The counterfactual mediator variable M when T is fixed at $t \in supp(T)$, that is, $M(t)$ is given by:

$$\begin{aligned} M(t) &= \Lambda_T^M \cdot t + \epsilon_{M(t)} \\ \text{where } \Lambda_T^M &= \left(\varphi_T + \varphi_U \cdot \zeta_T\right) \end{aligned} \tag{17}$$

$$\text{and zero-mean Unobserved Error Term } \epsilon_{M(t)} = \varphi_U \cdot \epsilon_U + \delta_Y \cdot V_Y + \delta_T \cdot V_T + \epsilon_M.$$

The counterfactual outcome $Y(t)$ is given by:

$$\begin{aligned} Y(t) &= \Lambda_T^Y \cdot t + \epsilon_{Y(t)} \\ \text{where } \Lambda_T^Y &= \Big(\beta_T + \beta_M\left(\varphi_T + \varphi_U \zeta_T\right) + \beta_U \zeta_T\Big) \end{aligned} \tag{18}$$

$$\text{and zero-mean Unobserved Error Term } \epsilon_{Y(t)} = \beta_M \delta_T \cdot V_T + \beta_M \cdot \epsilon_M + \beta_U \cdot \epsilon_U + (1 + \beta_M \delta_Y) \cdot V_Y + \epsilon_Y.$$

The counterfactual outcome $Y(m,t)$ when T is fixed at $t \in supp(T)$ and M is fixed at $m \in supp(M)$ is given by:

$$\begin{aligned} Y(m,t) &= \Pi_M^Y \cdot m + \Pi_T^Y \cdot t + \epsilon_{Y(m,t)} \\ \text{where } \Pi_T^Y &= \Big(\beta_T + \beta_U \zeta_T\Big) && (19) \\ \Pi_M^Y &= \beta_M && (20) \end{aligned}$$

$$\text{and zero-mean Unobserved Error Term } \epsilon_{Y(m,t)} = \beta_U \cdot \epsilon_U + V_Y + \epsilon_Y.$$

We are interested in identifying the causal coefficients Λ_T^M, Λ_T^Y that are associated with counterfactuals $M(t), Y(t)$ and Π_T^Y, Π_M^Y that are associated with counterfactual $Y(m,t)$.[7] While Λ_T^Y gives the total effect of T on Y, the other coefficients allow for decomposing this causal effect into a direct and an indirect effect. The direct effect of T on Y is given by Π_T^Y while the indirect (mediated) effect is given by the product of multiplying Π_M^Y by Λ_T^M (See Online Appendix A.1 for a detailed derivation.)

All causal parameters of the mediation analysis can thus be evaluated through the following three TSLS estimations:

[7] In Appendix C, we express the observed variables T, M, Y in terms of only external variables Z, V_T, V_Y and error terms.

1) The TSLS estimation of the effect of T on Y using Z as instrument evaluates Λ_T^Y (second stage coefficient of T gives total effect of T).

2) The TSLS estimation of the effect of T on M using Z as instrument evaluates Λ_T^M (second stage coefficient of M to be multiplied with Π_M^Y).

3) The TSLS estimation of the effect of M on Y conditioned on T using Z as instrument evaluates both Π_T^Y (second stage coefficient of T gives direct effect of T) and Π_M^Y (second stage coefficient of M multiplied with Λ_T^M gives indirect effect of T on M).

The TSLS of T on Y that uses Z as instrument is defined by the following equations:

$$\text{First Stage: } T = \Gamma^T + \Gamma_Z^T \cdot Z + \epsilon^T;$$

$$\text{Second Stage: } Y = \Gamma^Y + \Gamma_T^Y \cdot T + \epsilon^Y.$$

$$\text{Estimand } \hat{\Gamma}_T^Y \text{ identifies: } \operatorname{plim}(\hat{\Gamma}_T^Y) = \frac{\operatorname{cov}(Z,Y)}{\operatorname{cov}(Z,T)} = \frac{\big(\beta_T + \beta_M(\varphi_T + \varphi_U\zeta_T) + \beta_U\zeta_T\big)\xi_Z}{\xi_Z} = \Lambda_T^Y.$$

The TSLS of T on M is defined by the following equations :

$$\text{First Stage: } T = \Gamma^T + \Gamma_Z^T \cdot Z + \epsilon^T;$$

$$\text{Second Stage: } M = \Gamma^M + \Gamma_T^M \cdot T + \epsilon^M.$$

$$\text{Estimand } \hat{\Gamma}_T^M \text{ identifies: } \operatorname{plim}(\hat{\Gamma}_T^M) = \frac{\operatorname{cov}(Z,M)}{\operatorname{cov}(Z,T)} = \frac{(\varphi_T + \varphi_U\zeta_T)\cdot\xi_Z}{\xi_Z} = \Lambda_T^M.$$

The TSLS of M on Y conditioned on T that uses Z as instrument is defined by the following equations:[8]

[8] Unconditional on T, the exclusion restriction $Z \perp\!\!\!\perp Y(m)$ does not hold, according to **T-1**. The TSLS of M on Y that uses Z as instrument would return:

$$\frac{\operatorname{cov}(Z,Y)}{\operatorname{cov}(Z,M)} = \frac{(\varphi_T + \varphi_U\zeta_T)\cdot\xi_Z}{\big(\beta_T + \beta_M(\varphi_T + \varphi_U\zeta_T) + \beta_U\zeta_T\big)\cdot\xi_Z},$$

which does not have a causal interpretation.

$$\text{First Stage: } M = \Gamma^{M|T} + \Gamma_Z^{M|T} \cdot Z + \Gamma_T^{M|T} \cdot T + \epsilon^{M|T};$$

$$\text{Second Stage: } Y = \Gamma^{Y|T} + \Gamma_M^{Y|T} \cdot M + \Gamma_T^{Y|T} \cdot T + \epsilon^{Y|T}.$$

$$\text{Estimand } \hat{\Gamma}_M^{Y|T} \text{ identifies: } \operatorname{plim}(\hat{\Gamma}_M^{Y|T}) = \frac{\operatorname{cov}(T,Z) \cdot \operatorname{cov}(T,Y) - \operatorname{cov}(T,T) \cdot \operatorname{cov}(Z,Y)}{\operatorname{cov}(M,T) \cdot \operatorname{cov}(T,Z) - \operatorname{cov}(T,T) \cdot \operatorname{cov}(Z,M)} \tag{21}$$

$$= \frac{\operatorname{cov}(V_T,V_T) \cdot \beta_M \cdot \delta_T \cdot \xi_Z}{\operatorname{cov}(V_T,V_T) \cdot \delta_T \cdot \xi_Z} = \beta_M = \Pi_M^Y;$$

$$\text{Estimand } \hat{\Gamma}_T^{Y|T} \text{ identifies: } \operatorname{plim}(\hat{\Gamma}_T^{Y|T}) = \frac{-\big(\operatorname{cov}(M,Z) \cdot \operatorname{cov}(T,Y) - \operatorname{cov}(M,T) \cdot \operatorname{cov}(Z,Y)\big)}{\operatorname{cov}(M,T) \cdot \operatorname{cov}(T,Z) - \operatorname{cov}(T,T) \cdot \operatorname{cov}(Z,M)} \tag{22}$$

$$= \frac{\operatorname{cov}(V_T,V_T) \cdot \delta_T \cdot \xi_Z \cdot \big(\beta_T + \beta_U \cdot \zeta_T\big)}{\operatorname{cov}(V_T,V_T) \cdot \delta_T \cdot \xi_Z} = \big(\beta_T + \beta_U \cdot \zeta_T\big) = \Pi_T^Y.$$

See Appendix D for a detailed derivation of Equations (21)–(22).

While the total effect Λ_T^Y of T on Y has a causal interpretation if the standard IV assumptions (8)–(9) hold, model assumptions (10)–(15) have to hold to allow a causal interpretation of its direct effect Π_T^Y and its indirect effect $\Pi_M^Y \cdot \Lambda_T^M$. These assumptions can be tested using Equation (16). Under linearity, this model specification test implies the following relation among identified parameters:

$$E(Y(t)) \equiv \int E(Y(t,m)) dF_{M(t)}(m) \qquad \Rightarrow \Lambda_T^Y \cdot t = \Pi_T^Y \cdot t + \Pi_M^Y E(\Lambda_T^M(t));$$

$$\Rightarrow \Lambda_T^Y = \Pi_T^Y + \Pi_M^Y \cdot \Lambda_T^M \tag{23}$$

If our model assumptions are met, then (23) must hold. Thus our model specification test is a test of the hypothesis $H_0 : \Lambda_T^Y = \Pi_T^Y + \Pi_M^Y \cdot \Lambda_T^M$. If the hypothesis is rejected, we would conclude that the model assumptions do not hold. Otherwise we would accept the model specifications, that is to say that we would accept the causal links of the unobserved confounders V_T and V_Y. As a consequence, we would have identified the causal mechanism behind the causal effect of T on Y.

3 Data Construction

It is well documented that the rise of China and Eastern Europe had pronounced effects on manufacturing employment in high-wage countries. This paper studies the political consequences of this development. In Germany imports from *and* exports to China and Eastern Europe roughly tripled over the period 1987 to 1998 (from about 20 billion to about 60 billion Euros each),[9] and again tripled between 1998 and 2009. At the same time, we observe an increase in political polarization and in labor market adjustments. To identify trade-induced labor market adjustments as causal mechanism behind an effect of trade exposure on voting behavior, we implement the mediation framework developed in section 2. We need the following variables to test the extent to which trade-induced labor market adjustments caused changes in voting behavior: *Treatment* T_{it} is our measure of a local labor market's trade exposure. *Mediators* M_{it} are labor market variables, and *Final Outcome* Y_{it} refers to voting outcomes. Finally, we use other countries' trade exposure to construct Z_{it} as an *Instrument* for T_{it}. We now explain how we measure these variables.

3.1 Trade Exposure (Treatment T_{it})

To measure changes in a local labor market i's trade exposure T at time t, we follow Autor et al. (2013) and calculate

$$T_{it} = \sum_j \frac{L_{ijt}}{L_{jt}} \frac{\Delta Trade_{Gjt}}{L_{it}}. \tag{24}$$

The intuition is straightforward: local labor market i's composition of employment L across 157 manufacturing industries j at the beginning of period t determines its exposure to changes in German ('G') national industries' trade flows $Trade_G$ over the following decade. Changes in trade flows can refer to (i) stronger import competition (ΔIM_{Gjt}), (ii) better export opportunities (ΔEX_{Gjt}), or (iii) a net measure of trade exposure ($\Delta IM_{Gjt} - \Delta EX_{Gjt}$). 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. Our units of observation are counties (*Landkreise*) as a representation of local labor markets and we consider decadal changes $\Delta Trade_{Gjt}$. We link sectoral employment data L_{ij}, which the *Institut für Arbeitsmarkt- und Berufsforschung* (IAB) reports in standard international

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

trade classification (SITC), to the UN Comtrade trade data using the crosswalk described in Dauth et al. (2014).

3.2 Labor Market Variables (Mediator M_{it})

Our labor market data stem from the *Institut für Arbeitsmarkt- und Berufsforschung* (IAB)'s Historic Employment and Establishment Statistics (HES) database (see Bender, Haas, and Klose 2000 for a detailed description). The HES is collected for social insurance purposes, and includes information on daily wages, a range of socio-demographic variables (such as educational attainment, gender, and age) and the industry, occupation, and place of work for all German workers subject to social insurance.[10] From the individual-level data we aggregate up to the *Landkreis* level to match our voting data. We consider decadal changes in six *Landkreis*-level variables describing labor market adjustments: total employment, manufacturing industries' share of total employment, manufacturing and non-manufacturing wages, unemployment, and finally total population size (with the latter two being provided by the German Statistical office). To summarize them more concisely, we calculate the principal components of these labor market variables and use these as our measures of labor market adjustments M_{it} in our mediation analysis. Online Appendix B provides additional information on data sources and variable construction.

3.3 Voting (Final Outcome Y_{it})

To measure how trade integration affects voting behavior, we focus on party-votes in federal elections in Germany (*Bundestagswahlen*).[11] Due to its at-large voting system Germany, like most continental European countries, has consistently had a multi-party system that spans the full spectrum from far-left to extreme-right parties. There are four parties that we label 'established' in that they were persistently represented in parliament over the 25 years we study. There is also a large number of small parties that run for election. The average vote share of these small parties is far below

[10] Civil servants and self-employed individuals are not included in our database. Furthermore, we choose to exclude workers younger than 18 or older than 65 and we exclude all individuals in training and in part-time jobs because their hourly wages cannot be assessed.

[11] The party vote, called (*Zweitstimme*), mainly determines a party's share of parliamentary seats. German voters also cast a second vote for individual candidates, called (*Erststimme*). This vote for individuals affects the very composition of party factions in the parliament, but has no significant influence on their overall parliamentary share. Moreover, the decision on individual candidates might be strategic. We thus follow Falck et al. (2014) and focus on the party vote.

the 5% threshold of party votes needed to enter the federal parliament.[12] We collected these data to create a novel dataset of party vote shares at the county level. We group the small parties into three categories: far-left parties, extreme-right parties, and a residual category of other small parties. Altogether, we consider eight *Landkreis*-level voting outcomes as the set of final outcomes Y_{it}: changes in the vote-share of each of the four mainstream incumbent parties; changes in the aggregate vote shares of each of the far-left, extreme-right and other small parties; and finally, changes in voter turnout. Online Appendix C provides additional information on the data sources and more details on the variable construction.

3.4 The Instruments Z_{it}

There are three potential sources of bias which our measure of trade exposure (T_{it}) suggested in (24) may be vulnerable to. First, variation in trade exposure is a composite effect of the relative importance of trade-intensive industries *and* the relative importance of manufacturing employment in a region. Since the former determines region i's exposure to trade with low-wage countries while the latter might independently affect labor-market and voting outcomes, we condition on region i's initial share of manufacturing employment in all our regressions. Second, changes in trade flows might be the result of German demand or supply shocks. In this case, domestic productivity shocks would 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 from (exports to) China or Eastern Europe, ΔIM_{Gjt} (ΔEX_{Gjt}), with the average imports from (exports to) a set of similar high-wage economies 'O', ΔIM_{Ojt} (ΔEX_{Ojt}).[13] Third, reverse causality may bias our estimations if the anticipation of future import competition or export opportunities affected contemporaneous employment. To account for this, we lag the initial employment share in sectors j and regions i and the initial workforce by one decade and denote this lag by the subscript $t-1$. Combining the second and third argument, we

[12]This threshold is not binding if a party wins at least three seats through the vote for individual candidates (*Erststimme*). During our period of analysis, this occurred once in 1994. The individual candidates of the party PDS won 4 seats by *Erststimme*. As a result, the party received 30 seats in total, according its 4.4% of party votes (*Zweitstimme*) received.

[13]We choose the same countries as Dauth et al. (2014) to instrument German imports 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. See Dauth et al. (2014) for a discussion of this selection.

derive the instrument

$$Z_{it}^{IM} = \sum_j \frac{L_{ijt-1}}{L_{jt-1}} \frac{\Delta IM_{Ojt}}{L_{it-1}}, \tag{25}$$

and we calculate Z_{it}^{EX} analogously using ΔEX_{Ojt}.

3.5 Space- and Time-Dimension

All the data for our main empirical analyses is observed at the county (*Landkreis*) level. We observe 408 counties in our data, 86 of which are in East Germany. Indeed, it is a unique feature of the German data that it allows assessing trade exposure (T_{it}), local labor market adjustments (M_{it}), and voting behavior (Y_{it}) in the same spatial unit. We organize our data as stacked panel of first differences between election dates. Accordingly, we deviate slightly from studying decennial changes, and instead study two periods of 11 years, 1987 to 1998 (period 1) and 1998 to 2009 (period 2). Our analysis starts with the federal election of 1987, i.e. before the fall of the Iron Curtain in 1989 and Germany's subsequent reunification in 1990. We thus exclude East-German counties from the first period of analysis,[14] which gives us 730 ($= (408 - 86) + 408$) observations in total. Table 1 provides descriptive statistics for our main variables. The table 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 Period 1 from 1987–1998, and columns 4–6 for Period 2 from 1998–2009. T_{it} 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 exposure was relatively balanced between import competition and export access in Period 1, with an average T_{it} of just 68 € per worker. In Period 2, trade exposure was more export-heavy, with changes in export access exceeding changes in import competition by on average 663 € per worker.[15] Online Appendix B Figure 1 illustrates the spatial dispersion of the net exposure measure in our data.

[14]If we let the first period begin with first democratic election in East Germany in 1990, we could not observe many of the small parties we observe otherwise, since it took time for them to build up party organizations in the East. Thus, a 1990-1998 comparison is for East German districts more or the less equivalent to a 1998 cross-sectional analysis for most but the major parties. Moreover, it took time to privatize the state-owned enterprises dominating the East German economy, casting doubt on the reliability of our measure of trade exposure for East German labor markets in period 1. Since Berlin cannot unambiguously be classified as East or West, we drop this city state from the sample. To homogenize the sample, we also drop the other two city states, Hamburg and Bremen.

[15]Dauth et al. (2014) explore this finding in detail, and show that trade exposure with Eastern Europe –the dominant shock in period one– 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.

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:						
T_{it}	-0.264	0.068	0.521	-1.222	-0.663	-0.144
instrumented T_{it}	-0.068	0.143	0.402	-1.150	-0.574	-0.113
M_{it} (Labor Market 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
Y_{it} (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

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.

Looking at the labor market outcomes, we find evidence of economic stagnation in Period 1. Most importantly, we see a decline in the share of manufacturing employment across all regions concurrent with increasing unemployment. Indeed, Germany was considered "the sick man of Europe" during the 1990s. The period of stagnation was followed by an equally prolonged export and productivity boom. Following Gerhard Schröder's electoral victory in 1998, Germany's inflexible labor market institutions underwent substantial reforms, see (Dustmann et al., 2014). In the course of these reforms, we observe important changes in the behavior of trade unions and employers' associations. Wage policies became more moderate and firms and local labor union chapters were now allowed to deviate from collective bargaining agreements to flexibly adopt to local labor market conditions.[16] As a result of these reforms, the decline in manufacturing employment slowed down and unemployment decreased during Period 2. Furthermore, we observe more moderate or even negative wage growth in this period.

Finally, the table shows substantial variation in political trends across the two periods. From 1987 to 1998, established parties saw an average 4.7 percentage point reduction in their share of the popular vote, while small parties and the extreme right saw an increasing vote share. From 1998 to 2009, the main parties CDU and SPD as well as the extreme-right parties lost electoral support.[17]

In summary, 1987–1998 saw changes in import competition and export access that roughly balanced out, economic stagnation and an increase in support for the extreme right. This was followed by increased export access, economic stabilization, and political moderation in Period 2. Period-by-region fixed effects will largely absorb these secular trends in our empirical analyses, as well as accounting for the unbalanced panel that arises from not considering East German counties in Period 1.

[16] A perusal of the *OECD Labour Market Policies and Institutions Indicators Database* nicely illustrates this regulatory change. On the core 'strictness of employment protection' index, Germany stayed in a tight band between 3.13–3.25 throughout Period 1, but this measure then dropped rapidly to an average of 1.46 during Period 2. See `www.oecd.org/employment/emp/employmentdatabase-labourmarketpoliciesandinstitutions.htm`

[17] The large decrease in SPD vote share reflects the party breaking with its left wing, which subsequently merged with the socialist party PDS to form the new party *Die Linke*. In our data, *Die Linke* is classified as far left. See section Online Appendix C for more details.

4 Empirical Results

Our empirical analysis is organized in the following way. Section 4.1 evaluates the total causal effect of trade exposure (T_{it}) on voting behavior (Y_{it}). Section 4.1.1 explores this effect in more detail using individual level data from the GSOEP. Section 4.2 examines the effect of trade exposure on six labor market variables, and aggregates this information into principal components (M_{it}). Section 4.2.1 decomposes trade effects on labor market outcomes and voting outcomes by period, providing reduced form evidence for a relationship between the two effects. Section 4.3 implements the mediation analysis outlined in section 2 to identify to what extent labor market adjustments M_{it} mediate the effect of T_{it} on Y_{it}. This section decomposes the total effect of trade on voting into an 'indirect' effect mediated by labor market adjustments and a residual 'direct' effect. We also verify the assumptions underlying our mediation model by testing (23).

4.1 Estimating the Total Effect of T on Y

We estimate the following Second Stage equation using TSLS:

$$Y_{it} = \Gamma_T^Y \cdot T_{it} + \Gamma_X^Y \cdot X_{it} + \epsilon_{it}^Y \tag{26}$$

Treatment T_{it} represents trade exposure as defined in (24). Outcome Y_{it} denotes changes in voting behavior in county i over period t. Specifically, these are changes in turnout, and changes in the vote-shares of incumbent, small, extreme-right, and far-left parties. Together, these vote shares cover the entire political spectrum. Γ_T^Y is our estimator for the total effect of trade exposure on voting, see (18). X_{it} denotes a selection of control variables. These are i's start-of-period manufacturing employment share; the start-of-period employment share that is college educated, foreign born, or female; the employment share in the largest sector;[18] along with separate controls for the employment share in car manufacturing and the chemical industry;[19] start-of-period vote-shares for all parties; voter turnout, start-of-period unemployment rate, and population-share of retirement age.

[18] 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.

[19] The latter account for those industries' outstanding importance for the German economy.

$\Gamma_X^Y \cdot X_{it}$ further includes a set of period-specific region fixed effects (North, West, South, and East Germany) with the regions being comparable to U.S. Census divisions (Dauth et al., 2014).[20] The regional fixed effects are time-varying to allow for different trends in voting behavior over the periods 1987–1998 and 1998–2009, as evident from the descriptive statistics in section 3.5.

The First Stage equation is

$$T_{it} = \Gamma_{IM}^T \cdot Z_{it}^{IM} + \Gamma_{EX}^T \cdot Z_{it}^{EX} + \Gamma_X^T \cdot X_{it} + \epsilon_{it}^T, . \tag{27}$$

Instruments Z_{it}^{IM} and Z_{it}^{EX} are defined in (25). Control variables X_{it} are the same in the first and second stage. 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 the start-of-period manufacturing employment share as the only control. We always control for a region's start-of-period manufacturing share in employment because it inherently drives part of the variation in T_{it}; see the discussion in 3.4. In column 2, we add controls for the structure of the workforce, i.e., the start-of-period employment share that is college educated, foreign born, or female. In column 3, we account for the disproportionate regional employment share of some firms 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. In column 4, we add start-of-period vote-shares for all party outcomes and turnout. Finally, in column 5, we add the start-of-period unemployment rate and the population-share of retirement age. This is the most conservative specification, and our preferred one. In this specification, a one-standard-deviation increase in T_{it} (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 election outcomes.

[20] Each of Germany's 16 states (*Bundesländer*) is fully contained inside one of these four regions.

Table 2: Effect of T_{it} 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:						
Z^{IM}_{it}	0.225***	0.234***	0.221***	0.220***	0.220***	0.220***
	(8.220)	(8.350)	(7.816)	(7.966)	(7.971)	(7.971)
Z^{EX}_{it}	-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: (*a*) Each cell reports results from a separate instrumental variable regression. The data is a stacked panel of first-differences at the *Landkreis* level. Each regression has 730 observations, i.e. 322 *Landkreise* in West Germany, observed in 1987–1998 and 1998–2009, and 86 *Landkreise* in East Germany, observed only in 1998–2009. We drop three city-states (Hamburg, Bremen, and Berlin in the East). (*b*) All specifications include region-by-period fixed effects. 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. (*c*) The bottom panel presents the first stage results. It reports coefficients for only the two instruments, but includes the full set of controls from the top-panel. (*d*) 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.

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.[21] 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 of Online Appendix D. A comparison between IV and OLS estimates for the effect on extreme-right parties shows that the OLS coefficient is consistently smaller than the IV coefficient. This result is in line with our concern that trade exposure partly reflects domestic sectoral demand shifts.[22]

Overall, the estimated total effect of trade exposure on support for the extreme right is small in absolute terms. To some extent, this small coefficient may be due to measurement error. This is because election results are observed at the *place (Landkreis) of residence* while the labor market data that we use to calculate T_{it} are reported at the *place (Landkreis) of work*. The imperfect overlap of the spatial units is likely attenuating our estimates toward zero. From 1999 on, labor market data are reported at the place of work *and* the place of residence. In Online Appendix D.2, we replicate our main result for the period 1999-2009 with *place-of-residence* data only to gauge the attenuation from combining *place-of-residence* voting data with *place-of-work* employment data. We find that the trade effect on the extreme-right is around 50 percent larger when we use *place-of-residence* labor market data, see Online Appendix D.2 (table 3). However, even a 50 percent larger effect remains small in absolute terms. The plausible explanation for this finding is that Germany did not have a populist leader (or party) with broad appeal like Marine Le Pen in France, Nigel Farage in the UK,

[21]However, the coefficient for the market-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 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.

[22]For example, booming domestic production may increase demand for intermediate input imports, but this is unlikely to have the same political consequences as import competition.

or Donald Trump in the U.S. during our study period. All anti-globalization parties at the right fringe were extremist parties with neo-Nazi ties and associations to the *Third Reich* which made them anathema to most Germans. The coefficient size is thus specific to the political context, and our focus is therefore not on the magnitude of the effect of trade exposure on voting behavior but on the causal mechanisms underlying it.

4.1.1 Individual-Level Evidence

In this section, we test whether our regional-level results can be confirmed at the individual level. We do this using the GSOEP, an annual household survey that started in 1984 (GSOEP, 2007). The GSOEP is unique amongst attitudinal socio-economic surveys in its long-run panel structure.[23] Importantly, we observe individuals in local labor markets. As a result, we can associate individual workers w with their local labor market i's trade exposure (T_{it}), instrument T_{it} with Z_{it} as before, and add the same set of regional controls.[24] This allows us to track decadal changes in individuals' party preferences in a way that mirrors our main local labor market analysis.[25] In addition, we can control for individual characteristics including age, educational attainment, and gender. For our purpose, the relevant GSOEP question asks: "*If there was an election today, who would you vote for?*" We translate this question into a series of dummies that reflect the full party spectrum also observed in table 2, e.g. one dummy if the individual would you vote for the CDU, one if the individual would vote for the SPD, etc.[26] For each party, we aggregate individuals' self-reported voting intentions into a decadal cumulative share of years in which a respondent answered in the affirmative. Based on this, we calculate Y^P_{wt}, as $(\frac{\text{\# years that } w \text{ would vote for party } P}{\text{\# years } w \text{ answered the question}})_{wt}$

We prefer measuring the outcome as a cumulative share for the whole period over a first difference approach that merely relies on individuals' answer at the beginning and the end of the period. Moreover, respondents do not answer all questions in every year, which would increase the number of missing observations in a first difference specification. By contrast, with a cumulative share we simply sum up the instances in which a question was answered in the affirmative

[23]The General Social Survey for example only added a panel component in 2008.

[24]We also face the same attenuation bias as before, with trade exposure being measured at the place of work but individual voting intentions at the place of residence.

[25]Because the SOEP only started to ask about voting intentions for the full party spectrum in 1990 we use the time windows 1990-1998 and 1998-2009, which implies a slightly shorter Period 1 compared to our main results.

[26]There is no question on turnout in the GSOEP.

and divide by the number of years where we observe an answer. As a result, we obtain about three times as many 'person-decade' observations using the share measure than with the first-difference measure. For each party P, the dependent variable is a share between 0 and 1 for individual w in time period t and we separately estimate

$$Y^P_{wt} = \gamma^Y_{Y-1} \cdot Y^P_{wt-1} + \gamma^Y_T \cdot T_{it} + \gamma^Y_X \cdot X_{it-1} + \epsilon_{wt}. \tag{28}$$

for each party outcome.

With a slight abuse of notation, Y^P_{wt-1} controls for w's survey response to the same question in the base year. X_{it} refers to the same set of regional controls for the base-year as in table 2. Our focus is on estimating γ^Y_T, the effect of region i's trade exposure T_{it} on a resident worker w's reported party support.

Table 3 reports the results. Across rows it mimics closely our main table 2, except that there is no turnout measure in the GSOEP. Every coefficient in table 3 reports the estimate of γ^Y_T from a separate regression. T_{it} is always instrumented as before, although we do not report the first stage regressions again. Column 1 includes period and regional fixed effects as well as the regional economic controls from table 2, the most important one of which is a region's baseline manufacturing employment share. We also add region i's base-year socio-economic and voting controls X_{it-1} from table 2 for each period. To better gauge magnitudes, column 2 reports the same specification with standardized outcomes.

Regional trade exposure shifts individuals' preferences to the extreme right, though the effect is marginally insignificant with a t-stat of 1.619. In the individual-level data there is stronger evidence of a reduction in preference for the established left-wing party, the SPD, which comes out much less clearly in the aggregate results in table 2. No other party across the entire spectrum shows a response that is close to being significant. The discussion in section 3.5 suggests that the effect of trade shocks on labor markets should be more pronounced in the second period, when companies were more flexible to react. We therefore report the results separately by period in columns 3 and 4. It turns out that both the extreme right and SPD results are driven entirely by period 2, i.e. after Germany's labor markets were de-regulated. We will also find this at the regional level, see table 5.

Table 3: Individual-Level Analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All Controls	Standardized	1990-1998	1998-2009	High-Skill	Low-Skill & Manuf.	Low-Skill & Not Manuf.	Low-Skill & Manuf., 1998-2009	Low-Skill & Not Manuf., 1998-2009
Established Parties:									
Would Vote CDU/CSU	0.001	0.003	-0.025	0.002	-0.007	-0.013	0.008	-0.006	0.003
	(0.292)	(0.292)	(-0.743)	(0.227)	(-0.278)	(-0.794)	(0.827)	(-0.350)	(0.257)
Would Vote SPD	-0.008*	-0.016*	0.027	-0.019**	-0.013	-0.011	-0.017*	0.001	-0.022**
	(-1.901)	(-1.901)	(0.761)	(-2.217)	(-0.460)	(-0.400)	(-1.930)	(0.031)	(-2.352)
Would Vote FDP	0.001	0.005	-0.038	0.015	-0.018	0.011	0.007	0.002	0.021
	(0.459)	(0.459)	(-0.720)	(1.177)	(-0.420)	(0.664)	(0.568)	(0.116)	(1.431)
Would Vote Green Party	0.003	0.012	0.019	0.016	0.070	0.025	0.002	0.007	0.007
	(1.000)	(1.000)	(0.409)	(1.295)	(1.474)	(0.909)	(0.152)	(0.363)	(0.565)
Non-Established Parties:									
Would Vote Extreme-Right Parties	0.003	0.023	0.029	0.028*	0.010	0.083**	0.006	0.088**	0.016
	(1.619)	(1.619)	(0.735)	(1.802)	(0.875)	(2.206)	(0.475)	(2.013)	(1.035)
Would Vote Far-Left Parties	-0.001	-0.007	-0.008	-0.005	-0.051	0.019	-0.009	0.026	-0.010
	(-1.059)	(-1.059)	(-0.670)	(-0.751)	(-1.358)	(1.356)	(-1.055)	(1.579)	(-1.043)
Would Vote Other Small Parties	-0.001	-0.007	0.018	-0.012	0.005	-0.026	-0.003	-0.048*	-0.001
	(-0.642)	(-0.642)	(0.340)	(-1.072)	(0.182)	(-1.053)	(-0.190)	(-1.674)	(-0.042)
Period-by-region F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,669	9,669	3,694	5,975	1,348	2,199	6,122	1,168	3,817

Notes: (*a*) Each cell in this table reports on a separate regression. An observation is an individual w over a period t, where we consider 1990–1998, and 1998–2009, closely mirroring the local labor market results. Each row reports on survey responses to a different question. For every question/outcome y the left-hand-side variable is the share $\frac{\text{\# years that } w \text{ would vote for party } y}{\text{\# years } w \text{ answered the question}}_{it}$. The reported coefficient in all cells is the IV coefficient of regional trade exposure T_{it}. (*b*) Column 1 is the baseline specification which includes period and four region fixed effects as well as all the regional economic, voting and demographic controls from table 2, and individuals' base-year stated political preferences. This is the full set of controls included in all columns. To better gauge magnitudes, columns 2–9 standardize all outcomes by their mean. Columns 3–4 split the sample by period (3,694 + 5,975= 9,669). The results are driven entirely by period 2, i.e. after Germany's labor markets were de-regulated. No part of the political spectrum responds in period 1. In period 2, SPD support is reduced in response to trade exposure and support for the extreme right goes up. In columns 5–7, we break the sample by individuals' skill as well as by whether they are employed in the manufacturing sector (1,348 + 2,199 + 6,122 = = 9,669). High-skill workers (column 5) do not appear to change their political support at all in response to trade exposure. Column 6 shows that it is the population most affected by trade exposure – low-skill manufacturing workers – that drives the effects on the far right. Interestingly, the mainstream-left results are driven by low-skill service workers in column 7. We conjecture that this may be because they experience increased competition from laid-off low-skill manufacturing workers. In columns 8–9, we focus on the second period, which again sharpens the results from columns 6–7. (*c*) Standard errors are clustered at the region level. *** p<0.01, ** p<0.05, * p<0.1.

Once we dig deeper into what types of workers are driving the observed patterns we find distinctive results. In columns 5–7 we split the sample by skill as well as by whether an individual works in manufacturing, i.e. whether their employment sector is more heavily exposed to trade competition.[27] Both the extreme right effect and the SPD effect are entirely driven by low-skill workers, while high-skill workers do not respond at all.[28] Splitting the low-skill sample into manufacturing and non-manufacturing employment, we see that the extreme-right response is entirely driven by low-skill workers in manufacturing sectors, i.e. those likely to be most exposed to competition from low-wage countries. For this subpopulation the effect is also much larger. By contrast and most interestingly, the reduction in the change in the SPD's vote share is entirely driven by low-skill *non*-manufacturing workers. A possible interpretation is that low-skill workers in the service sector are affected by competition from laid-off manufacturing workers, or that laid-off manufacturing workers had to accept unattractive jobs in the service sector. In either case, they might blame the SPD-induced labor market reforms, such that trade exposure would only indirectly affect their changing party support. Columns 8–9 show that this pattern is again driven by the second period. In summary, the individual level evidence confirms our main findings of a political backlash to increasing international trade, and additionally shows that those who are most likely to experience adverse labor market effects are the ones who respond the most to increasing trade exposure.

4.2 Estimating the Effect of Trade Exposure T on Labor variables M

We now turn to estimating equation

$$M_{it} = \Gamma_T^M \cdot T_{it} + \Gamma_X^M \cdot X_{it} + \epsilon_{it}^M, \tag{29}$$

again using TSLS. M_{it} are the six labor market outcomes: manufacturing's employment share, total employment, manufacturing and non-manufacturing wages, unemployment and total population size. Γ_T^M is our estimator for the effect of trade exposure on these labor market variables,

[27] In an earlier working paper, we focused on comparing the effect of individuals' trade exposure due to their industry of employment relative to their regions' trade exposure (Dippel, Gold, and Heblich, 2015). However, we have come to the conclusion that individuals' industry of employment is measured too coarsely in the GSOEP to draw strong conclusions about the relative importance of these two types of trade exposure.

[28] The GSOEP reports skills as educational attainment according to the 'ISCED-1997' classification, where 'high' means some college.

see (17). Trade exposure T_{it}, control variables X_{it}, and instruments Z_{it} in equation (29) are the same as the ones used to estimate the effect of trade on voting behavior in (26). We only replace the political outcomes Y_{it} in (26) with the labor market variables M_{it} of (29).

The results are displayed in table 4, which is structured in exactly the same way as table 2. Each cell reports the result from 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.[29] Column 6 reports the results from our preferred specification as beta coefficients to facilitate comparison with the effects on voting outcomes.

We closely replicate results that have already been established in Autor et al. (2013), Dauth et al. (2014) and Pierce and Schott (2016): Trade exposure has a significantly negative effect on manufacturing employment. In our preferred specification, in column 5, a one-standard-deviation (1,350 €) increase in trade exposure decreases the share of manufacturing employment by around one percentage point, i.e. roughly three-quarters of Germany's average by-decade decrease of 1.3 percent over the period. Trade exposure also implies small but significant wage cuts in manufacturing industries. This is in contrast to U.S. data, where 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. manufacturing. Moreover, trade exposure increases unemployment and depresses total employment. Lastly, there is a small but significant negative effect on total population.[30] As before, Online Appendix D reports corresponding OLS results (table 4), and the coefficients of all controls (table 5).

Our key question of interest is to what extent labor market adjustments mediate the causal

[29] In tables 2 and 4, 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 4. 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).

[30] One might be concerned that the trade effects on extreme right party support could be driven by selective out-migration of trade exposed individuals. However, the population effect is small ($e^{-0.004 \cdot 1.35} - 1 = -0.005$), and the previous section's individual analysis confirms that individuals exposed to trade to indeed change their voting behavior.

Table 4: Effect of Trade Exposure T_{it} on Labor Market Adjustments

	(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)
1st Prinicpal Component	-0.105**	-0.050	-0.045	-0.032	-0.021	-0.011
	(-2.108)	(-1.313)	(-1.150)	(-0.903)	(-0.679)	(-0.679)
2nd Prinicpal Component	-0.265***	-0.301***	-0.328***	-0.324***	-0.322***	-0.271***
	(-2.894)	(-3.526)	(-3.667)	(-3.696)	(-3.755)	(-3.755)
First Stage:						
Z^{IM}_{it}	0.225***	0.234***	0.221***	0.220***	0.220***	0.220***
	(8.220)	(8.350)	(7.816)	(7.966)	(7.971)	(7.971)
Z^{EX}_{it}	-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: (*a*) Each cell reports results from a separate instrumental variable regression. The data is a stacked panel of first-differences at the *Landkreis* level. Each regression has 730 observations, i.e. 322 *Landkreise* in West Germany, observed in 1987–1998 and 1998–2009, and 86 *Landkreise* in East Germany, observed only in 1998–2009. We drop three city-states (Hamburg, Bremen, and Berlin in the East). (*b*) All specifications include region-by-period fixed effects. 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. For outcomes in logs, the table reports on a semi-elasticity: For example, a one-standard-deviation increase in T_{it} (€1,350) decreased total employment by about 3 percent, ($e^{-0.024 \cdot 1.35} - 1 = -0.032$). (*c*) The bottom panel presents the first stage results. It reports coefficients for only the two instruments, but includes the full set of controls from the top-panel. (*d*) 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.

relationship between trade exposure and voting behavior. Accordingly, we are most interested to what extent labor market adjustments as a whole act as causal mechanism. The framework in section 2 allows for the identification of the mediating effect of a single mechanism. We therefore aim to reduce the dimensionality of the labor market data to obtain a concise measure of labor market adjustments. A natural way of doing so is to conduct a principal component analysis (PCA). PCA is appealing for two reasons: First, PCA combines all observed labor market variables into aggregated principal components (PCs) that condense labor market adjustments into their key characteristics. Second, these principal components are by construction orthogonal so that they can be investigated separately one at a time. The standard "Kaiser-Guttman" criterion suggests to analyze principal components with an eigenvalue larger than 1. In our data, the second PC of the labor market variables observed has an eigenvalue of 1.415 followed by a bif drop in the third PC's eigenvalue to 0.6085. We interpret this as natural break and consider the first two PCs in our analysis. Together, the two PCs explain about 80 percent of the variation in the labor market data. See Appendix E table 7 for details.

As statistical constructs, principal components are best interpreted through the lens of their factor loadings, which indicate how strongly every labor market outcome relates to each PC. In Appendix E, table 7 reports the factor loadings of all six outcomes on the first two principal components. 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 the share of manufacturing employment and changes in total employment, and negative for changes in unemployment. The urban agglomeration literature offers a highly plausible interpretation for these factor loadings. Duranton and Puga (2005) point out that regional specialization has increasingly become "functional" as opposed to "sectoral" over the last decades, implying a tendency for headquarters and business services to cluster in large cities and for manufacturing plants to cluster in smaller cities, a trend that appears to be clearly borne out in Germany (Bade, Laaser, and Soltwedel, 2003). The second principal component appears to capture changes in manufacturing, with positive coefficients indicating easing on the labor markets, and negative coefficients indicating tensions. The first principal component captures changes in functional specialization and urban agglomeration, with positive coefficients indicating an increase in urban agglomeration that comes with increasing population, wages, and unemployment, and

Table 5: Decomposing the Results by Period and Imports vs Exports

5.A: Labor Market Outcomes/Mediators

	(1) Manuf	(2) log(mean Manuf. Wages)	(3) log(Non-Manuf. Wages)	(4) log(Total Empl.)	(5) Share Unempl.	(6) log(Pop.)	(7) 1st Principal Comp'nt	(8) 2nd Princip. Comp'nt
Period 1								
T_{it}	-0.324	0.005*	-0.000	-0.027	0.079	-0.006	-0.003	-0.252
	(-0.896)	(1.662)	(-0.011)	(-1.600)	(0.883)	(-1.341)	(-0.068)	(-1.435)
Period 2								
T_{it}	-0.400**	0.001	-0.002	-0.011	-0.079	-0.001	-0.017	-0.141*
	(-2.350)	(0.433)	(-1.144)	(-1.311)	(-1.112)	(-0.502)	(-0.441)	(-1.651)
Period 2, West only								
T_{it}	-0.572***	-0.001	-0.002	-0.019**	-0.055	-0.002	-0.032	-0.230**
	(-3.069)	(-0.337)	(-0.945)	(-1.990)	(-0.823)	(-0.905)	(-0.742)	(-2.453)

5.B: Final Voting Outcomes

	(1) Turnout	(2) CDU/CSU	(3) SPD	(4) FDP	(5) Greens	(6) Right	(7) Left	(8) Small
Period 1								
T_{it}	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)
Period 2								
T_{it}	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)
Period 2, West only								
T_{it}	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)

Notes: The table reports subsample estimations. Panel A reports on the same eight labor market outcomes in table 4. Panel B reports on the same eight political outcomes in table 2. Every result reported in table 5 is from a TSLS estimation that breaks treatment into separate import competition and export access effects, instrumented with Z_{it}^{IM} and Z_{it}^{EX}, defined in (25). Every panel additional reports the results for three separate sub-samples: period 1 (1987–1998) and period 2 (1998–2009), and period 2 without the 86 East German districts. The sample sizes are 322, 408, and 322 respectively. 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.

negative coefficients indicating a decrease.[31] At the bottom of the Second Stage panel of table 4, we report the effect of T_{it} on these two principal components of labor market adjustments. It is evident that trade exposure has no bearing on the first PC but is a very important driver of the second PC.

4.2.1 Subsample Results for the Effect of Trade T on Labor M and Voting Y

The discussion in section 3.5 suggests that the effect of trade shocks on labor markets should be more pronounced in the second period, when companies were more flexible to react. We found

[31] In unreported regressions, we do indeed find the first PC to be significantly positively correlated with urban centers and negatively correlated with urbanized regions and rural regions. On the contrary, the second PC is significantly positively correlated with urbanized regions and negatively correlated with urban centers and rural regions.

some evidence for this pattern in the individual results in table 3. This motivates us to decompose the effect of trade exposure on local labor markets by period in this section. Panel A of table 5 reports the same six labor market outcomes plus their principal components as table 4, estimated separately for Period 1 (1987–1998), and Period 2 (1998–2009), as well as for West Germany only in Period 2. Panel B of table 5 similarly decomposes the same eight political outcomes as reported in table 2. The sample sizes are 322, 408, and 322 respectively.

Comparing the three sub-panels of panel A shows evidence for increasing flexibility in labor markets between Period 1 and Period 2. This is nicely reflected by the core result for manufacturing employment in column 1 and by the second principal component of labor market adjustments reported in column 8 of Panel A. In period 1, import competition also has counter-intuitive effects on wages, hinting at regulatory rigidities. The observed contrast between periods is not driven by the inclusion of East German regions in period 2. In fact, the contrast between the two periods is more pronounced once we focus on West Germany. Panel B shows that voting responses to trade were strongest when labor markets were least regulated. Combining the evidence, table 5 suggests that trade exposure had the biggest effect on both voting and labor market adjustments in the second period in West Germany, i.e. when labor markets were most deregulated and subject to market forces. We interpret this symmetry as "reduced form evidence" for the important role of labor markets as mediators in the transmission from trade shocks to voting responses. However, without additional econometric structure, it is not possible to infer on the causality of the labor market mechanisms.

4.3 Mediation Analysis

In this section, we implement the mediation model outlined in section 2. The aim is to identify the causal mechanism underlying the effect of trade on voting behavior. To this end, we decompose the *total effect* of trade exposure on voting (estimated by $\hat{\Gamma}_T^Y$ in table 2), into an 'indirect' or 'mediated' effect that works through labor markets and a residual 'direct' effect.

Regarding the final outcome Y_{it}, we restrict ourselves to the vote share of extreme right parties, i.e. the only significant voting response to trade observed in the data. For the mediator M_{it} we focus on the second principal component of the six labor market outcomes, i.e. the principal component causally affected by trade. We have already estimated the effect of trade exposure on labor market adjustments $\hat{\Gamma}_T^M$ reported in table 4. To decompose $\hat{\Gamma}_T^Y$ into a direct and an indirect effect of trade exposure on voting, we need to additionally estimate $\hat{\Gamma}_M^{Y|T}$ and $\hat{\Gamma}_T^{Y|T}$. The direct effect of trade on voting will be given by $\Gamma_T^{Y|T}$, and the indirect or mediated effect will be given by multiplying $\Gamma_M^{Y|T}$ with $\hat{\Gamma}_T^M$.

As shown in section 2.3, we estimate the following second stage equation:

$$Y_{it} = \Gamma_T^{Y|T} \cdot T_{it} + \Gamma_M^{Y|T} \cdot M_{it} + \Gamma_X^{Y|T} \cdot X_{it} + \epsilon_{it}^{Y|T}. \tag{30}$$

The First Stage now takes the form

$$M_{it} = \Gamma_{IM}^{M|T} \cdot Z_{it}^{IM} + \Gamma_{EX}^{M|T} \cdot Z_{it}^{EX} + \Gamma_T^{M|T} \cdot T_{it} + \Gamma_X^{M|T} \cdot X_{it} + \epsilon_{it}^{M|T}. \tag{31}$$

(30) differs from the second stage equation (26) used before in that we are interested in the causal effect of the mediator M on outcome Y (instead of T on Y or T on M). (31) differs from the first stage equation (27) used before in that trade exposure T is now included. (See section 2.3.)

Table 6 reports the TSLS estimates of (30). Estimate $\hat{\Gamma}_M^{Y|T}$ gives the effect of labor market adjustments M_{it} on extreme right party vote shares Y_{it}. M_{it} is the second principal component of six observed labor market outcomes, standardized to have a mean of zero an a standard-deviation of one. Decreases of M_{it} broadly indicate worsening labor markets for manufacturing workers. See Appendix E table 7 for the six labor market outcomes' factor loadings. $\hat{\Gamma}_M^{Y|T}$ suggests that a one-standard deviation decrease in the principal component increases the extreme right's vote share by 0.492 (column 5).

The effect of trade-induced labor market adjustments on voting, that is the mediated effect of T on Y, can be derived by multiplying $\hat{\Gamma}_M^{Y|T}$ by $\hat{\Gamma}_T^M$. This is equivalent to combining table 4 column 5 with table 6 column 5. The implied magnitude of the indirect effect $\hat{\Gamma}_M^{Y|T} \cdot \hat{\Gamma}_T^M$ is 0.1584 $(-0.492 \cdot -0.322)$. A one-standard deviation increase in trade exposure T_{it} (1,350 €) induces labor market adjustments which in turn increase the extreme-right vote share by 0.213 $(0.1584 \cdot 1.35)$ percentage points, i.e. roughly 50 percent of the average per-decade increase of 0.43 percentage points during the 22 years we study. This effect is highly significant. Using Generalized Method of Moments (GMM) estimations, we find a z-statistic of 2.3 and a p-value of 0.022 for the product $\hat{\Gamma}_M^{Y|T} \cdot \hat{\Gamma}_T^M$.

Table 6: Effect of M_{it} and T_{it} on Y_{it}

	(1) Baseline IV	(2) + Structure IV	(3) + Industry IV	(4) + Voting IV	(5) +Socio IV	(6) Standard. IV
Second Stage:						
$\mathbf{M}_{it}$	-0.473**	-0.585**	-0.534**	-0.504***	-0.492***	-0.244***
	(-2.038)	(-2.144)	(-2.406)	(-3.138)	(-2.900)	(-2.900)
$\mathbf{T}_{it}$	-0.057	-0.075	-0.062	-0.091***	-0.086***	-0.043***
	(-1.187)	(-1.413)	(-1.410)	(-2.942)	(-2.632)	(-2.632)
First Stage:						
Z^{IM}_{it}	0.002	-0.022	-0.028	-0.036*	-0.034*	-0.034*
	(0.088)	(-0.963)	(-1.319)	(-1.777)	(-1.787)	(-1.787)
Z^{EX}_{it}	0.053**	0.056**	0.069***	0.071***	0.070***	0.070***
	(2.216)	(2.466)	(3.238)	(3.477)	(3.442)	(3.442)
$\mathbf{T}_{it}$	-0.057	-0.075	-0.062	-0.091***	-0.075	-0.075
	(-1.187)	(-2.392)	(-1.410)	(-2.942)	(-1.407)	(-1.407)
Period-by-region FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	730	730	730	730	730	730

Notes: (*a*) Each cell reports results from a separate instrumental variable regression. The data is a stacked panel of first-differences at the *Landkreis* level. Each regression has 730 observations, i.e. 322 *Landkreise* in West Germany, observed in 1987–1998 and 1998–2009, and 86 *Landkreise* in East Germany, observed only in 1998–2009. We drop three city-states (Hamburg, Bremen, and Berlin in the East). (*b*) All specifications include region-by-period fixed effects. 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 the same specification with standardized outcome variables. (*c*) The bottom panel presents the first stage results for (31). It reports coefficients for only the two instruments and T_{it}, but includes the full set of other controls as before. (*d*) 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.

The direct effect of T_{it} on Y_{it} that is unrelated to labor market adjustments is given by estimate $\hat{\Gamma}_T^{Y|T}$ reported in Table 6. It turns out that this direct effect on voting behavior is in fact moderating. Disregarding labor market adjustments, increasing trade exposure significantly *decreases* the vote share of extreme right parties. The estimated effect of -0.086 (column 5) implies that the direct effect is practically as large in absolute terms as our estimate $\hat{\Gamma}_T^Y$ of the total effect of 0.089 reported in table 2.

Our mediation analysis thus shows that the total effect of trade exposure on support for populist parties consists of a large radicalizing effect that runs through labor market mechanisms and a moderating direct effect that runs through other channels. This result is perhaps surprising. On the one hand, the fact that $\hat{\Gamma}_M^{Y|T} \cdot \hat{\Gamma}_T^Y$ is larger than $\hat{\Gamma}_T^Y$ means that the labor market consequences of trade have stronger political consequences than a simple inspection of the effects of trade on voting ($\hat{\Gamma}_T^Y$) and the effects of trade on labor markets ($\hat{\Gamma}_T^M$) would have implied. On the other hand, a moderating direct effect of trade exposure is interesting and novel in its own right.[32]

We can only speculate about the reasons for the moderating direct effect of trade exposure but a plausible explanation would be related to the rapidly growing amount of trade in intermediates. As trade in intermediates as a share of total trade has sky-rocketed over the past 25 years, importing increasingly means integrating foreign suppliers and products into the domestic production chain. See Antras (2015) and the many references therein. The resulting exposure to and coordination with foreign specialized suppliers, be it within-firms (through off-shored but vertically integrated suppliers) or across firms, may have politically moderating effects since workers get more exposure to different cultures. Specifically, it might very well have rendered the nationalist agenda propagated by populist parties less attractive. We cannot shed further light on this question here but we believe that the cultural and political effects of the cross-border integration of supply chains are promising avenue for future research.

The interpretations above are only valid if the model assumptions explained in section 2 hold. Testing this is conveniently easy and does not even require a separate table. We just have to

[32]Intuitively, one may have thought about reasons for another radicalizing effect even in the absence of immediate labor market consequences, e.g. anxiety about the future due to increasing international trade (Mughan and Lacy, 2002; Mughan, Bean, and McAllister, 2003).

conduct the specification test suggested in section 2.3

$$H_0 : \Gamma^{Y}_{T} = \Gamma^{Y|T}_{T} + \Gamma^{Y|T}_{M} \cdot \Gamma^{M}_{T},$$

which is based on our GMM estimates. If our assumptions were met, the total effect of trade exposure on voting behavior, evaluated by (27)–(26), would have to be equal to the sum of its direct and its indirect effect, evaluated by (31)–(30). The p-value of this test is 0.835, which gives us no reason to reject the hypothesis that the causal model defined by Functions (10)–(15) and represented by Figure 5 is the correct one. We thus conclude that we have indeed identified the causal mechanism underlying the causal effect of trade on voting.

5 Discussion & Conclusion

While theory and empirical evidence mostly agree that trade liberalization has positive aggregate welfare effects, trade liberalization creates distributional frictions between its winners and losers. Depending on how the losers are compensated, this might have political consequences. To improve our understanding of this relationship, we study the political effects of increasing international trade. We find that higher import competition with low-wage countries increases the support for political populists.

When we try to understand the underlying mechanisms, we face a common empirical problem: Even though we can causally identify the total effect of the treatment (T_{it}) on voting as the final outcome (Y_{it}) *and* on labor markets as a proposed mechanism (M_{it}), we cannot easily identify how much of the total effect works through the observed mechanism.

We develop new methodology to overcome this limitation and find the political response to trade exposure to be caused by an economic mechanism. Specifically, the populist response to trade exposure that is mediated by observable labor market adjustments is larger than the total effect. This is because this 'mediated effect' is partly offset by a moderating 'direct effect,' implying that international trade would have moderating effects on voters, if just the workers could be fully shielded from any negative labor market effects.

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Appendix A Proof of Theorem T-1

Proof P-1 *Model primitives are that error terms $\epsilon_T, \epsilon_U, \epsilon_M, \epsilon_Y$ and exogenous variables Z, V_T, V_Y are mutually independent. It is useful to express the treatment T and counterfactuals $Y(t)$, $M(t)$ and $Y(m)$ in terms of these variables:*

$$
\begin{aligned}
T &= f_T(Z, V_T, \epsilon_T) && (32)\\
U(t) &= f_U(t, \epsilon_U), && (33)\\
M(t) &= f_M(t, U(t), V_T, V_Y, \epsilon_M)\\
&= f_M(t, f_U(t, \epsilon_U), V_T, V_Y, \epsilon_M) && (34)\\
Y(t) &= f_Y(t, M(t), U(t), V_Y, \epsilon_Y)\\
&= f_Y(t, f_M(t, f_U(t, \epsilon_U), V_T, V_Y, \epsilon_M), f_U(t, \epsilon_U), V_Y, \epsilon_Y) && (35)\\
Y(m) &= f_Y(T, m, U, V_Y, \epsilon_Y)\\
&= f_Y(T, m, f_U(T, \epsilon_U), V_Y, \epsilon_Y) && (36)
\end{aligned}
$$

There are two non-independence relations to be proven: $Z \not\perp\!\!\!\perp T$ and $Z \not\perp\!\!\!\perp M|T$. Equation (32) implies $Z \not\perp\!\!\!\perp T$. It remains to prove that $Z \not\perp\!\!\!\perp M|T$. But $M = f_M(T, U, V_T, V_Y, \epsilon_M)$, is a function of V_T. Thus it suffices to prove that $Z \not\perp\!\!\!\perp V_T|T$. According to equation (32), conditioning on $T = t$ is equivalent to conditioning on the values of V_T, Z, ϵ_T such that $f_T(Z, V_T, \epsilon_T) = t$. This induces a correlation between Z and V_T. Thus while $Z \perp\!\!\!\perp (V_T, \epsilon_T)$ holds, $Z \perp\!\!\!\perp (V_T, \epsilon_T)|(T = t)$ does not. This implies that $Z \not\perp\!\!\!\perp V_T|T$. There are three exclusion restrictions to be proven: (1) $Z \perp\!\!\!\perp Y(t)$; (2) $Z \perp\!\!\!\perp M(t)$; and (3) $Z \perp\!\!\!\perp Y(m)|T$. According to equation (35), $Y(t)$ is a function of exogenous variables $\epsilon_U, V_T, V_Y, \epsilon_M, \epsilon_Y$, thus:

$$\Big(\epsilon_U, V_T, V_Y, \epsilon_M, \epsilon_Y\Big) \perp\!\!\!\perp Z \;\Rightarrow\; Y(t) \perp\!\!\!\perp Z.$$

According to (34), $M(t)$ is a function of exogenous variables $\epsilon_U, V_T, V_Y, \epsilon_M$, thus:

$$\Big(\epsilon_U, V_T, V_Y, \epsilon_M\Big) \perp\!\!\!\perp Z \;\Rightarrow\; M(t) \perp\!\!\!\perp Z.$$

Model primitives imply that $(Z, V_T, \epsilon_T) \perp\!\!\!\perp (\epsilon_U, V_Y, \epsilon_Y)$ But according to (32), T is a function of exogenous variables (Z, V_T, ϵ_T), thus $(T, Z) \perp\!\!\!\perp (\epsilon_U, V_Y, \epsilon_Y)$, which also implies that $Z \perp\!\!\!\perp (\epsilon_U, V_Y, \epsilon_Y)|T$. Moreover, according to (36), $Y(m)$ is a function of exogenous variables $(\epsilon_U, V_Y, \epsilon_Y)$, thus:

$$Z \perp\!\!\!\perp (\epsilon_U, V_Y, \epsilon_Y)|(T = t) \;\Rightarrow\; Z \perp\!\!\!\perp f_Y(t, m, f_U(t, \epsilon_U), V_Y, \epsilon_Y)|(T = t) \;\Rightarrow\; Z \perp\!\!\!\perp Y(m)|(T = t).$$

Appendix B Proof of Corollary C-1

Proof P-2 *We need to prove that $P(Y(m) \le y|T = t) = P(Y(m, t) \le y)$ for $y \in supp(Y)$. It is useful to express counterfactual $Y(m, t)$ as a function of exogenous variables:*

$$
\begin{aligned}
Y(m, t) &= f_Y(t, m, U(t), V_Y, \epsilon_Y)\\
&= f_Y(t, m, f_U(t, \epsilon_U), V_Y, \epsilon_Y) && (37)
\end{aligned}
$$

Moreover model primitives imply that:

$$\Big(\epsilon_U, V_Y, \epsilon_Y\Big) \perp\!\!\!\perp \Big(Z, V_T, \epsilon_T\Big). \tag{38}$$

According to Equations (36), we have that:

$$\begin{aligned}
P(Y(m) \leq y | T = t) &\equiv P(f_Y(t, m, U, V_Y, \epsilon_Y) \leq y | T = t), \\
&\equiv P(f_Y(t, m, f_U(t, \epsilon_U), V_Y, \epsilon_Y) \leq y | f_T(Z, V_T, \epsilon_T) = t), \\
&= P(f_Y(t, m, f_U(t, \epsilon_U), V_Y, \epsilon_Y) \leq y), \\
&\equiv P(Y(t, m) \leq y),
\end{aligned}$$

where the third equality comes from the independence relation (38).

Appendix C T, M, Y in Terms of External Variables

We can write the observed variables T, M, Y in terms of external variables Z, V_T, V_Y and error terms $\epsilon_T, \epsilon_U, \epsilon_M, \epsilon_Y$ as:

$$\begin{aligned}
T &= \xi_Z \cdot Z + V_T + \epsilon_T \\
M &= \big(\varphi_T + \varphi_U \zeta_T\big) \cdot \xi_Z \cdot Z \\
&\quad + \big(\varphi_T + \varphi_U \zeta_T + \delta_T\big) \cdot V_T \\
&\quad + \delta_Y \cdot V_Y \\
&\quad + \big(\varphi_U \zeta_T + \varphi_T\big) \cdot \epsilon_T + \varphi_U \epsilon_U + \epsilon_M \\
Y &= \Big(\beta_T + \beta_M \big(\varphi_T + \varphi_U \zeta_T\big) + \beta_U \zeta_T\Big) \cdot \xi_Z \cdot Z \\
&\quad + \Big(\beta_T + \big(\varphi_T + \varphi_U \zeta_T + \delta_T\big)\beta_M + \beta_U \zeta_T\Big) \cdot V_T \\
&\quad + \big(1 + \beta_M \delta_Y\big) \cdot V_Y \\
&\quad + \big(\beta_T + \big(\varphi_T + \varphi_U \zeta_T\big)\beta_M + \beta_U \zeta_T\big) \cdot \epsilon_T + \big(\beta_M \varphi_U + \beta_U\big) \cdot \epsilon_U + \beta_M \cdot \epsilon_M + \epsilon_Y.
\end{aligned}$$

Appendix D Detailed Derivation of The IV Mediation Model under Linearity

The Two-stage Least Square estimation of M on Y conditional on T that uses Z as instrument is defined by the following equations when variables are centered at zero:

$$\begin{aligned}
\text{First Stage: } M &= \Gamma^{M|T} + \Gamma_Z^{M|T} \cdot Z + \Gamma_T^{M|T} \cdot T + \epsilon_4; \\
\text{Second Stage: } Y &= \Gamma^{Y|T} + \Gamma_M^{Y|T} \cdot M + \Gamma_T^{Y|T} \cdot T + \epsilon_5.
\end{aligned}$$

The TSLS estimator can be expressed as:

$$\begin{aligned}
&\big[\Gamma_M^{Y|T}, \Gamma_T^{Y|T}\big]' = \big([MT]'\mathbf{P}_{[ZT]}[MT]\big)^{-1} \big([MT]'\mathbf{P}_{[ZT]}Y\big) \\
&\text{where } \mathbf{P}_{[ZT]} = [ZT]\left([ZT]'[ZT]\right)^{-1}[ZT]'.
\end{aligned}$$

Thus we can express estimators $\left[\Gamma_M^{Y|T}\ ,\ \Gamma_T^{Y|T}\right]'$ as:

$$\begin{aligned}
\left[\begin{array}{c} \hat{\Gamma}_M^{Y|T} \\ \hat{\Gamma}_T^{Y|T} \end{array}\right] &= \left(\left[\begin{array}{cc} M'Z & M'T \\ T'Z & T'T \end{array}\right] \cdot \left[\begin{array}{cc} Z'Z & T'Z \\ T'Z & T'T \end{array}\right]^{-1} \cdot \left[\begin{array}{cc} M'Z & T'Z \\ M'T & T'T \end{array}\right]\right)^{-1} \\
&\cdot \left(\left[\begin{array}{cc} M'Z & M'T \\ T'Z & T'T \end{array}\right] \cdot \left[\begin{array}{cc} Z'Z & T'Z \\ T'Z & T'T \end{array}\right]^{-1} \cdot \left[\begin{array}{c} Z'Y \\ T'Y \end{array}\right]\right), \\
&= \left(\left[\begin{array}{cc} M'Z & M'T \\ T'Z & T'T \end{array}\right] \cdot \left[\begin{array}{cc} T'T & -T'Z \\ -T'Z & Z'Z \end{array}\right] \cdot \left[\begin{array}{cc} M'Z & T'Z \\ M'T & T'T \end{array}\right]\right)^{-1} \\
&\cdot \left(\left[\begin{array}{cc} M'Z & M'T \\ T'Z & T'T \end{array}\right] \cdot \left[\begin{array}{cc} T'T & -T'Z \\ -T'Z & Z'Z \end{array}\right] \cdot \left[\begin{array}{c} Z'Y \\ T'Y \end{array}\right]\right).
\end{aligned}$$

Thus we have that:

$$\begin{aligned}
\left[\begin{array}{c} \operatorname{plim}\left(\hat{\Gamma}_M^{Y|T}\right) \\ \operatorname{plim}\left(\hat{\Gamma}_T^{Y|T}\right) \end{array}\right] &= \left(\left[\begin{array}{cc} \operatorname{cov}(M,Z) & \operatorname{cov}(M,T) \\ \operatorname{cov}(T,Z) & \operatorname{cov}(T,T) \end{array}\right] \cdot \left[\begin{array}{cc} \operatorname{cov}(T,T) & -\operatorname{cov}(T,Z) \\ -\operatorname{cov}(T,Z) & \operatorname{cov}(Z,Z) \end{array}\right] \cdot \left[\begin{array}{cc} \operatorname{cov}(M,Z) & \operatorname{cov}(T,Z) \\ \operatorname{cov}(M,T) & \operatorname{cov}(T,T) \end{array}\right]\right)^{-1} \\
&\cdot \left(\left[\begin{array}{cc} \operatorname{cov}(M,Z) & \operatorname{cov}(M,T) \\ \operatorname{cov}(T,Z) & \operatorname{cov}(T,T) \end{array}\right] \cdot \left[\begin{array}{cc} \operatorname{cov}(T,T) & -\operatorname{cov}(T,Z) \\ -\operatorname{cov}(T,Z) & \operatorname{cov}(Z,Z) \end{array}\right] \cdot \left[\begin{array}{c} \operatorname{cov}(Z,Y) \\ \operatorname{cov}(T,Y) \end{array}\right]\right).
\end{aligned}$$

These equations generate can be rewritten into the following expressions:

$$\begin{aligned}
\operatorname{plim}\left(\hat{\Gamma}_M^{Y|T}\right) = &- \frac{\operatorname{cov}(T,T) \cdot \left(\begin{array}{c} \operatorname{cov}(Z,Y)\Big(\operatorname{cov}(M,T)\operatorname{cov}(T,Z) - \operatorname{cov}(M,Z)\operatorname{cov}(T,T)\Big) + \\ \operatorname{cov}(T,Y)\Big(\operatorname{cov}(M,Z)\operatorname{cov}(T,Z) - \operatorname{cov}(M,T)\operatorname{cov}(Z,Z)\Big) \end{array}\right)}{\left(\begin{array}{c} +\operatorname{cov}(M,T)^2 \cdot \operatorname{cov}(T,Z)^2 \\ -2 \cdot \operatorname{cov}(M,T) \cdot \operatorname{cov}(M,Z) \cdot \operatorname{cov}(T,T) \cdot \operatorname{cov}(T,Z) \\ +\operatorname{cov}(M,Z)^2 \cdot \operatorname{cov}(T,T)^2 \end{array}\right)} \\
&- \frac{\operatorname{cov}(M,T) \cdot \operatorname{cov}(T,Y) \cdot \Big(-\operatorname{cov}(T,Z)^2 + \operatorname{cov}(T,T)\operatorname{cov}(Z,Z)\Big)}{\left(\begin{array}{c} +\operatorname{cov}(M,T)^2 \cdot \operatorname{cov}(T,Z)^2 \\ -2\operatorname{cov}(M,T) \cdot \operatorname{cov}(M,Z) \cdot \operatorname{cov}(T,T) \cdot \operatorname{cov}(T,Z) \\ +\operatorname{cov}(M,Z)^2 \cdot \operatorname{cov}(T,T)^2 \end{array}\right)},
\end{aligned}$$

which can be simplified to:

$$\operatorname{plim}\left(\hat{\Gamma}_M^{Y|T}\right) = \frac{\operatorname{cov}(T,Y)\operatorname{cov}(T,Z) - \operatorname{cov}(T,T)\operatorname{cov}(Z,Y)}{\operatorname{cov}(M,T)\operatorname{cov}(T,Z) - \operatorname{cov}(M,Z)\operatorname{cov}(T,T)}$$

$$\text{plim}\left(\hat{\Gamma}_T^{Y|T}\right) = \frac{\text{cov}(M,T)\cdot\left(\begin{array}{c} +\text{cov}(Z,Y)\Big(\text{cov}(M,T)\,\text{cov}(T,Z) - \text{cov}(M,Z)\,\text{cov}(T,T)\Big) \\ +\text{cov}(T,Y)\Big(\text{cov}(M,Z)\,\text{cov}(T,Z) - \text{cov}(M,T)\,\text{cov}(Z,Z)\Big)\end{array}\right)}{\left(\begin{array}{c} +\text{cov}(M,T)^2\,\text{cov}(T,Z)^2 \\ -2\,\text{cov}(M,T)\,\text{cov}(M,Z)\,\text{cov}(T,T)\,\text{cov}(T,Z) \\ +\text{cov}(M,Z)^2\,\text{cov}(T,T)^2\end{array}\right)}$$

$$-\frac{-(\text{cov}(T,Y)\cdot\left(\begin{array}{c}\Big(-\text{cov}(T,Z)^2+\text{cov}(T,T)\,\text{cov}(Z,Z)\Big)\cdot \\ \left(\begin{array}{c}+\text{cov}(Z,Z)\,\text{cov}(M,T)^2 \\ -2\,\text{cov}(T,Z)\,\text{cov}(M,T)\,\text{cov}(M,Z) \\ +\text{cov}(T,T)\,\text{cov}(M,Z)^2\end{array}\right)\end{array}\right)}{\left(\begin{array}{c}-\text{cov}(Z,Z)\,\text{cov}(M,T)^2\,\text{cov}(T,T)\,\text{cov}(T,Z)^2 \\ +\text{cov}(M,T)^2\,\text{cov}(T,Z)^4 \\ +2\,\text{cov}(Z,Z)\,\text{cov}(M,T)\,\text{cov}(M,Z)\,\text{cov}(T,T)^2\,\text{cov}(T,Z) \\ -2\,\text{cov}(M,T)\,\text{cov}(M,Z)\,\text{cov}(T,T)\,\text{cov}(T,Z)^3 \\ -\text{cov}(Z,Z)\,\text{cov}(M,Z)^2\,\text{cov}(T,T)^3 \\ +\text{cov}(M,Z)^2\,\text{cov}(T,T)^2\,\text{cov}(T,Z)^2)\end{array}\right)}$$

which can be simplified to:

$$\text{plim}\left(\hat{\Gamma}_T^{Y|T}\right) = \frac{-(\text{cov}(M,Z)\,\text{cov}(T,Y) - \text{cov}(M,T)\,\text{cov}(Z,Y))}{\text{cov}(M,T)\,\text{cov}(T,Z) - \text{cov}(M,Z)\,\text{cov}(T,T)}$$

Thus we are interested in computing the four covariance formulas below:

1. $-\big(\,\text{cov}(M,Z)\cdot\text{cov}(T,Y) - \text{cov}(T,M)\cdot\text{cov}(Y,Z)\big)$
2. $\text{cov}(T,M)\cdot\text{cov}(T,Z) - \text{cov}(T,T)\cdot\text{cov}(M,Z)$
3. $\text{cov}(T_M * \text{cov}(T_Z - \text{cov}(T,T)\cdot\text{cov}(M_Z)$
4. $\text{cov}(T,M)\cdot\text{cov}(T,Z) - \text{cov}(T,T)\cdot\text{cov}(M_Z)$

The relevant covariances to compute the formulas above are:

$$\begin{aligned}\text{cov}(T,Z) &= \xi_Z; \\ \text{cov}(Y,Z) &= (\beta_T + \beta_M(\varphi_T + \varphi_U\zeta_T) + \beta_U\zeta_T)\xi_Z; \\ \text{cov}(M,Z) &= (\varphi_T + \varphi_U\zeta_T)\xi_Z;\end{aligned}$$

And also:

$$\begin{aligned}\text{cov}(T,T) &= \xi_Z^2\cdot\text{cov}(Z,Z) + \text{cov}(V_T,V_T) + \text{cov}(\epsilon_T,\epsilon_T); \\ \text{cov}(T,Y) &= (\beta_T + \beta_M(\varphi_T + \varphi_U\zeta_T) + \beta_U\zeta_T)\xi_Z^2\cdot\text{cov}(Z,Z) + (\beta_T + (\varphi_T + \varphi_U\zeta_T + \delta_T)\beta_M \\ &\quad + \beta_U\zeta_T)\cdot\text{cov}(V_T,V_T) + \big(\beta_T + (\varphi_T + \varphi_U\zeta_T)\beta_M + \beta_U\zeta_T\big)\cdot\text{cov}(\epsilon_T,\epsilon_T); \\ \text{cov}(T,M) &= (\varphi_T + \varphi_U\zeta_T)\xi_Z^2\cdot\text{cov}(Z,Z) + (\varphi_U\zeta_T + \varphi_T)\cdot\text{cov}(\epsilon_T,\epsilon_T) + (\varphi_T + \varphi_U\zeta_T + \delta_T)\cdot\text{cov}(V_T,V_T);\end{aligned}$$

The covariance formula $-(\mathrm{cov}(M,Z)\cdot\mathrm{cov}(T,Y)-\mathrm{cov}(T,M)\cdot\mathrm{cov}(Y,Z))$ can be expressed as:

$$\begin{aligned}
&-\big(\mathrm{cov}(M,Z)\cdot\mathrm{cov}(T,Y)-\mathrm{cov}(T,M)\cdot\mathrm{cov}(Y,Z)\big)=\\
&=\xi_Z(\mathrm{cov}(Z,Z)\cdot(\varphi_T+\varphi_U\zeta_T)\xi_Z^2+\mathrm{cov}(V_T,V_T)\cdot(\delta_T+\varphi_T+\varphi_U\zeta_T)\\
&+\mathrm{cov}(\epsilon_T,\epsilon_T)\cdot(\varphi_T+\varphi_U\zeta_T))\cdot(\beta_T+\beta_U\zeta_T+\beta_M(\varphi_T+\varphi_U\zeta_T))\\
&-\xi_Z(\varphi_T+\varphi_U\zeta_T)\cdot\left(\begin{array}{c}\mathrm{cov}(Z,Z)\cdot(\beta_T+\beta_U\zeta_T+\beta_M(\varphi_T+\varphi_U\zeta_T))\xi_Z^2\\+\mathrm{cov}(\epsilon_T,\epsilon_T)\cdot(\beta_T+\beta_U\zeta_T+\beta_M(\varphi_T+\varphi_U\zeta_T))\\+\mathrm{cov}(V_T,V_T)(\beta_T+\beta_U\zeta_T+\beta_M(\delta_T+\varphi_T+\varphi_U\zeta_T))\end{array}\right),
\end{aligned}$$

which simplifies to:

$$-\Big(\mathrm{cov}(M,Z)\cdot\mathrm{cov}(T,Y)-\mathrm{cov}(T,M)\cdot\mathrm{cov}(Y,Z)\Big)=\mathrm{cov}(V_T,V_T)\cdot\delta_T\cdot\xi_Z\cdot(\beta_T+\beta_U\cdot\zeta_T).$$

The covariance formula $\mathrm{cov}(T,M)\cdot\mathrm{cov}(T,Z)-\mathrm{cov}(T,T)\cdot\mathrm{cov}(M,Z)$ can be expressed as:

$$\begin{aligned}
&\mathrm{cov}(T,M)\cdot\mathrm{cov}(T,Z)-\mathrm{cov}(T,T)\cdot\mathrm{cov}(M,Z)=\\
&=-\xi_Z(\varphi_T+\varphi_U\zeta_T)\cdot(\mathrm{cov}(Z,Z)\cdot\xi_Z^2+\mathrm{cov}(\epsilon_T,\epsilon_T)+\mathrm{cov}(V_T,V_T))\\
&+\xi_Z\cdot\left(\begin{array}{c}\mathrm{cov}(Z,Z)\cdot(\varphi_T+\varphi_U\zeta_T)\xi_Z^2+\\\mathrm{cov}(V_T,V_T)\cdot(\delta_T+\varphi_T+\varphi_U\zeta_T)+\\\mathrm{cov}(\epsilon_T,\epsilon_T)\cdot(\varphi_T+\varphi_U\zeta_T)\end{array}\right),
\end{aligned}$$

which simplifies to:

$$\mathrm{cov}(T,M)\cdot\mathrm{cov}(T,Z)-\mathrm{cov}(T,T)\cdot\mathrm{cov}(M,Z)=\mathrm{cov}(V_T,V_T)\cdot\delta_T\cdot\xi_Z.$$

The covariance formula $\mathrm{cov}(T,Z)\cdot\mathrm{cov}(T,Y)-\mathrm{cov}(T,T)\cdot\mathrm{cov}(Y,Z)$ can be expressed as:

$$\begin{aligned}
&\mathrm{cov}(T,Z)\cdot\mathrm{cov}(T,Y)-\mathrm{cov}(T,T)\cdot\mathrm{cov}(Y,Z)=\\
&=-\xi_Z\cdot\Big(\mathrm{cov}(Z,Z)\cdot\xi_Z^2+\mathrm{cov}(\epsilon_T,\epsilon_T)+\mathrm{cov}(V_T,V_T)\Big)\cdot\Big(\beta_T+\beta_U\zeta_T+\beta_M\cdot(\varphi_T+\varphi_U\zeta_T)\Big)\\
&+\xi_Z\left(\begin{array}{c}\mathrm{cov}(Z,Z)\cdot(\beta_T+\beta_U\zeta_T+\beta_M(\varphi_T+\varphi_U\zeta_T))\xi_Z^2+\\\mathrm{cov}(\epsilon_T,\epsilon_T)\cdot(\beta_T+\beta_U\zeta_T+\beta_M(\varphi_T+\varphi_U\zeta_T))+\\\mathrm{cov}(V_T,V_T)\cdot(\beta_T+\beta_U\zeta_T+\beta_M(\delta_T+\varphi_T+\varphi_U\zeta_T))\end{array},\right)
\end{aligned}$$

which simplifies to:

$$\mathrm{cov}(T,Z)\cdot\mathrm{cov}(T,Y)-\mathrm{cov}(T,T)\cdot\mathrm{cov}(Y,Z)=\mathrm{cov}(V_T,V_T)\cdot\beta_M\cdot\delta_T\cdot\xi_Z$$

The covariance formula $\mathrm{cov}(T,M)\cdot\mathrm{cov}(T,Z)-\mathrm{cov}(T,T)\cdot\mathrm{cov}(M_Z)$ can be expressed as:

$$\begin{aligned}
&\mathrm{cov}(T,M)\cdot\mathrm{cov}(T,Z)-\mathrm{cov}(T,T)\cdot\mathrm{cov}(M_Z)=\\
&=-\xi_Z(\varphi_T+\varphi_U\zeta_T)\cdot\Big(\mathrm{cov}(Z,Z)\cdot\xi_Z^2+\mathrm{cov}(\epsilon_T,\epsilon_T)+\mathrm{cov}(V_T,V_T)\Big)\\
&+\xi_Z\left(\begin{array}{c}\mathrm{cov}(Z,Z)\cdot(\varphi_T+\varphi_U\zeta_T)\xi_Z^2+\\\mathrm{cov}(V_T,V_T)\cdot(\delta_T+\varphi_T+\varphi_U\zeta_T)+\\\mathrm{cov}(\epsilon_T,\epsilon_T)\cdot(\varphi_T+\varphi_U\zeta_T)\end{array}\right),
\end{aligned}$$

which simplifies to:

$$\text{cov}(T, M) \cdot \text{cov}(T, Z) - \text{cov}(T, T) \cdot \text{cov}(M_Z) = \text{cov}(V_T, V_T) \cdot \delta_T \cdot \xi_Z$$

Appendix E Principal Component Analysis

Table 7: Principal Component Analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Principal Components		Factor-Loadings					
	Eigen-value	Eigen-value: Proportion	Δ Share Manuf. Empl.	Δlog(Avg. Manuf. Wage)	Δ log(Avg. Non-Manuf. Wage)	Δ Log(Total Empl.)	Δ Share Unempl	Δ log(Total Pop)
1st Princ.Comp.	3.288	0.548	-0.284	0.452	0.484	0.248	0.460	0.456
2nd Princ.Comp.	1.415	0.236	0.606	0.026	-0.098	0.702	-0.215	0.291

Notes: Following the "Kaiser-Guttman" criterion, we retain and analyze PCs with an eigenvalue above 1. (The third PC had an eigenvalue of 0.609.) 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 almost 80 percent of the variation in the data ($0.548 + 0.236$). 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.

Online Appendix

to

"Beyond Causal Effects: Using Instrumental Variables to Identify the Effect of Trade on Workers and Voters"

Online Appendix A The Sequential Ignorability Assumption

A large literature on mediation analysis relies on the Sequential Ignorability Assumption **A-1** of Imai et al. (2010b) to identify mediation effects.

Assumption A-1 *Sequential Ignorability (Imai et al., 2010b):*

$$\big(Y(t',m), M(t)\big) \perp\!\!\!\perp T|X \tag{39}$$
$$Y(t',m) \perp\!\!\!\perp M(t)|(T,X), \tag{40}$$

where X denotes pre-intervention variables that are not caused by T, M and Y such that $0 < P(T = t|X) < 1$ and $0 < P(M(t) = m|T = t, X) < 1$ holds for all $x \in supp(X)$ and $m \in supp(M)$.

Under Sequential Ignorability **A-1**, it is easy to show that the distributions of conterfactual variables are identified by $P(Y(t,m)|X) = P(Y|X, T = t, M = m)$ and $P(M(t)|X) = P(M|X, T = t)$ and thereby the mediating causal effects can be expressed as:

$$ADE(t) = \int \left(\big(E(Y|T = t_1, M = m, X = x) - E(Y|T = t_0, M = m, X = x, X = x)\big) dF_{M|T=t,X=x}(m) \right) dF_X(x) \tag{41}$$

$$AIE(t) = \int \left(E(Y|T = t, M = m, X = x) \Big[dF_{M|T=t_1,X=x}(m) - dF_{M|T=t_0,X=x}(m) \Big] \right) dF_X(x). \tag{42}$$

Imai, Tingley, Keele and Yamamoto offer a substantial line of research that explores the identifying properties of Sequential Ignorability Assumption **A-1**. See Imai et al. (2011a) for a comprehensive discussion of the benefits and limitations of the sequential ignorability assumption.

The main critics of Sequential Ignorability **A-1** is that it does not hold under the presence of either *Confounders* or *Unobserved Mediators* (Heckman and Pinto, 2015a).

The independence relation (39) assumes that T is exogenous conditioned on X. There exists no unobserved variable that causes T and Y or T and M. For instance, the Sequential Ignorability **A-1** holds for the model defined in (1) because:

$$(\epsilon_Y, \epsilon_M) \perp\!\!\!\perp \epsilon_T \Rightarrow \quad \big(f_Y(t', m, \epsilon_Y), f_M(t, \epsilon_M)\big) \perp\!\!\!\perp f_T(\epsilon_T) \Rightarrow \quad \big(Y(t', m), M(t)\big) \perp\!\!\!\perp T. \tag{43}$$
$$\epsilon_Y \perp\!\!\!\perp \epsilon_M|\epsilon_T \Rightarrow \quad f_Y(t', m, \epsilon_Y) \perp\!\!\!\perp f_M(t, \epsilon_M)|f_T(\epsilon_T) \Rightarrow \quad Y(t', m) \perp\!\!\!\perp M(t)|T, \tag{44}$$

where the initial independence relation in (43) and (44) comes from the independence of error terms.

This assumption is expected to hold in experimental data when treatment T is randomly assigned. The independence relation (40) assumes that M is exogenous conditioned on X and T. It assumes that no confounding variable causing M and Y. Sequential Ignorability **A-1** is an extension of the Ignorability Assumption of Rosenbaum and Rubin (1983) that also assumes that a treatment T is exogenous when conditioned on pre-treatment variables. Robins (2003); Petersen, Sinisi, and Van der Laan (2006); Rubin (2004) state similar identifying criteria that assume no confounding variables. Those assumptions are not testable.

Figure 3 in the paper reveals that Sequential Ignorability **A-1** assumes that: (1) the confounding variable V is observed, that is, the pre-treatment variables X; and (2) that there is no unobserved mediator U. This assumption is unappealing for many because it solves the identification problem generated by confounding variables by assuming that those do not exist (Heckman, 2008).

Online Appendix A.1 Evaluation of Mediation Effects Under Linearity

Consider a change in the treatment variable T denoted by $\Delta(t) = t_1 - t_0$. The Direct and indirect effects can be expressed by:

$$\begin{aligned} ADE(t') &= \Big(\lambda_{YT} \cdot t_1 + \lambda_{YM} \cdot E(M(t'))\Big) - \Big(\lambda_{YT} \cdot t_0 + \lambda_{YM} \cdot E(M(t'))\Big) \\ \therefore ADE &= \lambda_{YT} \cdot \Delta(t) \qquad (45) \\ \text{and } AIE(t') &= \Big(\lambda_{YT} \cdot t' + \lambda_{YM} \cdot E(M(t_1))\Big) - \Big(\lambda_{YT} \cdot t' + \lambda_{YM} \cdot E(M(t_0))\Big) \\ &= \Big(\lambda_{YT} \cdot t' + \lambda_{YM}\lambda_M \cdot t_1\Big) - \Big(\lambda_{YT} \cdot t' + \lambda_{YM}\lambda_M \cdot t_0\Big) \\ \therefore AIE &= \lambda_{YM} \cdot \lambda_M \cdot \Delta(t) \qquad (46) \end{aligned}$$

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 also for friinge 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, Kuhn, and Zweimüller (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.[33] 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 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.

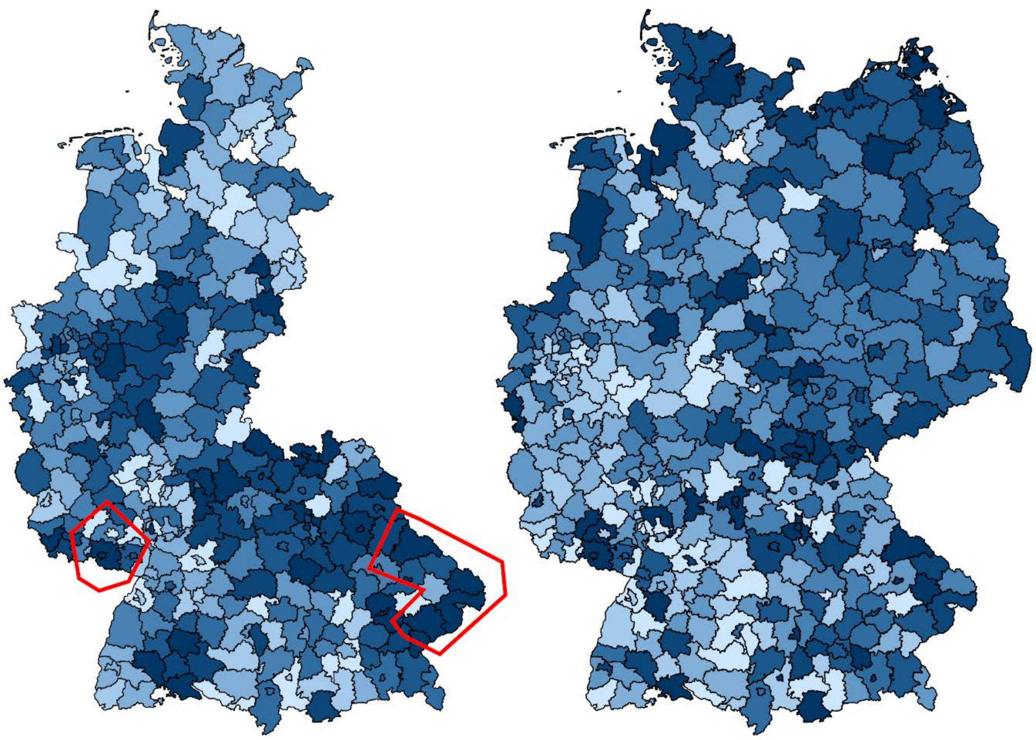
Figure A1 shows the spatial dispersion of our key regressor, T_{it}. A first observation is that there appears to be little auto-correlation in the trade exposure measure between the two periods (i.e. regions that are equally dark or light in both periods). This partly reflects the changing source of 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. The narrative of the two circled regions in Figure A1 illustrates the the nexus of import competition, structural decline in manufacturing, and changes in voting behavior.

The circled region in the south-west of 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, $\widehat{T}_{it} = 3.62$, while in 1998–2009, $\widehat{T}_{it} = 4.25$ 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.

The circled regions in in the south east of the map are located in South-Eastern Bavaria. They are bordering Austria or the Czech Republic in the so-called Dreiländereck. From the southwest to northeast, the districts are: 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

[33] See Bender et al. (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)

Online Appendix Figure 1: T_{it} 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 circles enclose the regions in Palatine (on the left) and Bavaria (on the right).

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 has 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."

Online Appendix C Background on German Politics 1987 to 2009

Online Appendix C.1 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).[34] German elections are based on the principle of proportionality. The main vote, called the "second vote" (*Zweitstimme*), is being cast for parties but not for individual candidates.[35] We will exclusively focus on this party vote. The overall number of parliamentary seats is determined in proportion to a party's share of the second vote. 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.

Online Appendix C.2 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 the NPD in 2011).[36] 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

[34] 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.

[35] Voters can additionally 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 significant effect on the overall distribution of seats, which is determined by the second vote.

[36] In Online Appendix C.4, we provide a history of these three parties. See also comprehensive work by Stöss (2010) or Mudde (2000).

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 German*y (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).

Online Appendix C.3 Stance on Trade and Globalization

Both the large parties CDU and SPD have market-liberal as well as protectionist factions. In comparison, the CDU tends to be more market-friendly. Still, it was a government led by the SPD that implemented substantial labor market reforms in 2003-2005, amongst others decreasing employment protection, unemployment benefits, and establishing a low wage sector in Germany. The smaller FDP explicitly follows a market-liberal agenda, while the Green party focusses on environmental issues. More generally, the political left has traditionally been seen as opposing globalization and capturing the anti-globalization vote.[37] However, this is no unambiguous relationship, as the *The Economist* (July 30th 2016) observes when headlining "Farewell, left versus right. The contest that matters now is open against closed." Throughout Europe, the political left has found it difficult to take a coherent position against globalization in the last two decades, often hampered by internal intellectual conflicts (Sommer 2008, Arzheimer 2009). In contrast, the right and far right successfully attended an anti-globalization agenda (Mughan et al., 2003). For the case of Germany, Sommer (2008, p. 312) argues that "in opposing globalization, the left-wing usually criticizes an unjust and profit-oriented economic world order. [It] does not reject globalization per se but rather espouses 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." The following excerpt from the extreme-right NPD's 'candidate manual' illustrates how Germany's far right rolls protectionist anti-globalization themes into its broader nationalistic, anti-Semitic agenda: "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).[38]

Online Appendix C.4 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-

[37] To some extent this may still be the case. Che et al. (2016) for example argue that trade liberalization with China has turned American voters towards the Democrats, though it seems as if this might have not been true for the 2016 presidential elections.

[38] The bundling of protectionist anti-globalization themes with xenophobic content has also been noted in the 2016 U.S. presidential election, see for example *The Guardian* (March 7th 2016).

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. A second claim to forbid the party, filed on December 7th 2015, was denied by the constitutional judges on January 17th 2017.

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 C.5 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 et al. (2011) find a significant relationship between extreme-right crimes and regional unemployment levels over the years 1996–1999.[39] 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.

[39] 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 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 D Robustness and Further Results

Online Appendix D.1 Additional Results on the Core Table 2

Online Appendix D table 1 presents the OLS results corresponding to the paper's table 2. 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 $\mathbf{T}_{it}$ 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 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
T_{it}	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: Place of Work and Place of Residence

3.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
Period 2								
$\mathbf{T}_{rt}$	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)
Period 2, West only								
$\mathbf{T}_{rt}$	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)

3.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
Period 2								
$\mathbf{T}_{it}$	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)
Period 2, West only								
$\mathbf{T}_{it}$	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)

Notes: This table compares the effect of trade exposure, depending on whether **T** is measured at the *place of residence*, i.e. $\mathbf{T}_{rt}$ where 'r' replaces 'i', or as before at the place of work, $\mathbf{T}_{it}$. *Place of residence* data becomes available in 1999, i.e. one year later than the 1998 election. All specifications include identical controls to our preferred specification in Table 2, Column 5. In each panel, we also show a specification with only the 322 districts in West Germany. Standard errors are clustered at the level of 96 commuting zones. *** p<0.01, ** p<0.05, * p<0.1.

Online Appendix D.2 Place of Work and Place of Residence Results

Here, we replicate our main result for 1999-2009 to gauge the attenuation caused by combining *place-of-residence* voting data with *place-of-work* employment data. We compare the effect of $\mathbf{T}_{rt}$, measured at the Landkreis of residence ('r'), with that of $\mathbf{T}_{it}$, measured as before at the Landkreis of work ('i').[40] Panels A and B of table 3 compare the place-of-residence with the place-of-work results. The place-of-work results for only 1999-2009 in Online Appendix D table 3 panel B are practically identical to those in column 5 of the paper's main table 2. The similarity holds if we exclude East German regions which were not contained in the first period of the sample employed in table 2. When looking at the place-of-residence in panel A, we find the same pattern in that only the vote share of extreme-right parties responds significantly to increasing trade exposure. As expected, the effect is now larger in magnitude. While we cannot extend this exercise to earlier years, table 2 does suggest that the trade effect on the extreme-right may be around 50 percent $(0.124/0.08 = 1.55)$ larger when the trade shock is measured at the place of residence.

Online Appendix D.3 Labor Market Outcomes

Like Table 1 did for the voting regressions, Online Appendix D table 4 presents the OLS results corresponding to the labor market regressions in Online Appendix D table 4. Online Appendix D table 5 reports coefficients on all control variables for table 4 in the paper. .

[40]The estimation for $\mathbf{T}_{it}$ differs from that in our main exercise in two minor respects: One, in the core exercise, period 2 starts in the election year 1998, while here it starts in 1999. Two, we cannot lag employment in our instrument due to the absence of employment data at place-of-residence before 1999.

Online Appendix Table 4: OLS Version of Table 4

	(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)
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 5: Coefficients on Controls in Table 4

	(1)	(2)	(3)	(4)	(5)	(6)
	Manuf	log(Mean Manuf. Wages)	log(Mean Non-Manuf. Wages)	log(Total Empl.)	Share Unempl.	log(Pop.)
$\mathbf{T}_{it}$	-0.755***	-0.006***	-0.001	-0.024***	0.110*	-0.004*
	(-3.745)	(-2.592)	(-0.808)	(-3.295)	(1.694)	(-1.852)
Controls Specification 1:						
Empl-share manufacturing $_{-1}$	-0.107***	0.001***	-0.001***	-0.001	0.021***	-0.001**
	(-4.519)	(2.920)	(-5.459)	(-1.510)	(2.920)	(-2.325)
Controls Specification 2:						
Pop-share college-educated $_{-1}$	0.067	0.002	0.006***	0.010***	-0.055**	0.006***
	(0.850)	(1.631)	(8.164)	(3.876)	(-2.046)	(2.817)
Pop-share foreign-born $_{-1}$	-0.476***	-0.000	0.001	-0.013***	0.156***	-0.004**
	(-5.573)	(-0.036)	(1.206)	(-4.329)	(5.373)	(-2.202)
Pop-share female $_{-1}$	-0.062	-0.007**	0.003*	0.005	0.059	0.004
	(-0.348)	(-2.388)	(1.675)	(0.660)	(0.769)	(1.108)
Employm-share in automotive $_{-1}$	-0.024	-0.001	0.000	0.001	-0.012	0.001
	(-0.606)	(-1.524)	(1.259)	(0.965)	(-0.944)	(0.646)
Employm-share in chemistry $_{-1}$	-0.145***	-0.000	0.001*	-0.001	-0.013	0.001
	(-2.713)	(-0.790)	(1.868)	(-0.613)	(-1.179)	(0.785)
Employment in largest industry $_{-1}$	-0.527	0.018	-0.002	-0.180**	0.601	-0.030
	(-0.255)	(0.499)	(-0.069)	(-2.009)	(0.965)	(-0.740)
Controls Specification 3:						
Unemployment-share $_{-1}$	0.121	0.003	0.001	-0.010***	-0.374***	-0.013***
	(1.484)	(1.575)	(1.001)	(-3.043)	(-7.499)	(-6.321)
Pop-share above age 65 $_{-1}$	-0.036	0.002	-0.002**	-0.018***	0.108***	-0.010***
	(-0.586)	(1.420)	(-2.246)	(-6.426)	(3.134)	(-6.665)
Voter Turnout $_{-1}$	0.051***	-0.000	-0.000	0.000	-0.003	-0.001*
	(2.753)	(-0.824)	(-0.569)	(0.310)	(-0.267)	(-1.918)
CDU/CSU Voteshare $_{-1}$	-0.276	-0.005	-0.002	-0.007	0.088	-0.000
	(-1.573)	(-1.559)	(-0.663)	(-0.764)	(0.976)	(-0.014)
SPD Voteshare $_{-1}$	-0.324*	-0.005*	-0.002	-0.010	0.123	-0.001
	(-1.876)	(-1.800)	(-0.727)	(-1.027)	(1.414)	(-0.251)
FDP Voteshare $_{-1}$	-0.173	-0.006**	0.001	-0.006	0.032	-0.001
	(-0.940)	(-2.100)	(0.272)	(-0.614)	(0.362)	(-0.238)
Green Party Voteshare $_{-1}$	-0.487***	-0.002	-0.003	-0.011	0.091	0.002
	(-2.711)	(-0.493)	(-1.143)	(-1.097)	(0.976)	(0.372)
Far-Right Voteshare $_{-1}$	-0.269	-0.006	-0.001	-0.013	0.231**	-0.003
	(-1.152)	(-1.591)	(-0.238)	(-1.010)	(2.015)	(-0.520)
Far-Left Voteshare $_{-1}$	-0.383**	-0.011***	-0.004	-0.020**	0.150	-0.001
	(-2.069)	(-2.888)	(-1.545)	(-2.004)	(1.552)	(-0.179)
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, *** $p<0.01$, ** $p<0.05$, * $p<0.1$.