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The Decline of Manufacturing Employment and the Rise of the Far-Right in Austria

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Abstract

In recent decades right-wing populist parties have experienced increased electoral success in many western democracies. This rise of the far-right, which is strongly built on the support of the working class, coincides with a sharp decline of the manufacturing sector. This paper analyzes the contribution of this manufacturing decline to the rise of the Austrian far-right. Overall the decline in manufacturing employment has strongly contributed to this rightward shift in the political landscape, with the manufacturing decline explaining roughly 43% of the observed increase in far-right vote-shares between 1995 and 2017. This effect is entirely driven by increases in natives unemployment rates, which increased considerably due to the manufacturing decline. Regarding the influences of the forces underlying the manufacturing decline, namely international trade and automation technologies, suggests that both forces contributed in roughly equal parts to this development.

JEL classification: D72, F14, J21, J23, O14, R23

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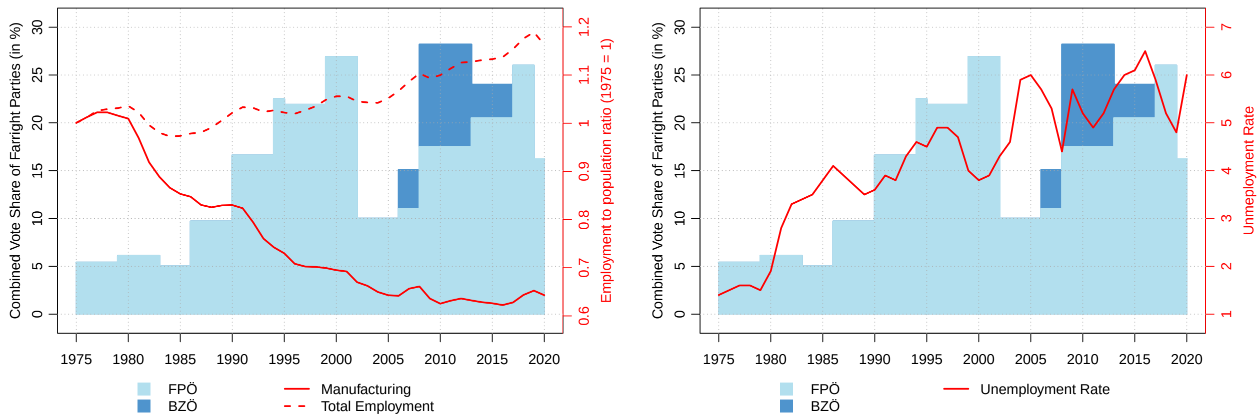
1 Introduction

Fueled by the removal of international trade barriers and the rise of automation technologies, most developed economies across the globe have experienced a sharp decline in the share of manufacturing employment. While employment and economic welfare as a whole generally increased over time, this unprecedented shift in the structure of labor demand has generated a cleavage between the winners and losers of automation and globalization, with those on the losing side experiencing a drastic decline in their economic status and social well-being. Correspondingly a vast literature has linked the manufacturing decline to the erosion of the middle class and increased polarization in the labor market (Autor and Dorn, 2013), increases in wage inequality (Gould, 2019), increases in drug use and mortality rates (Pierce and Schott, 2020) or declining marriage and fertility rates among prime aged men (Autor, Dorn, and Hanson, 2019).

This large and persisting shift in the structure of labor demand also coincides with the rise of far-right populist parties in many western democracies. Figure 1 depicts this graphically for the case of Austria, while Figure A1 shows that the same pattern emerges in practically all countries where far-right populist parties emerged during the last decades. Here the emergence and subsequent rise of far-right parties coincides with a sharp decline in the fraction of the overall population employed in the manufacturing sector. As is shown in panel b of Figure 1, this decline of manufacturing employment also coincides with a steady increase in unemployment, which highlights that the shocks underlying the manufacturing decline have not only led to a shift towards service employment but have also adversely affected the employment prospects of those workers most reliant on employment within manufacturing. Historically it was precisely this type of heavily affected, working class voters whose increasing electoral support formed the basis of the rise of the Austrian far-right (Pelinka, 2002). This notion is also confirmed when looking at data from the European Social Survey (ESS) in Table A1 in the Appendix, as far-right populist parties find much stronger support among voters who are more reliant on employment in the manufacturing sector, more often work in blue collar occupations, are more likely to be unemployed, posses lower skill levels

Figure 1: Far-right Voting and the Decline of Manufacturing Employment

(a) Employment/population ratio (b) Unemployment rate



Notes: The employment-to-population ratios in Panel (a) are calculated using employment data from EU-Klems (March 2007 and 2023 releases) and OECD population data (as in Appendix Figure A1). The seasonally adjusted unemployment data used in Panel (b) comes from the AMECO-database from EuroStat. To make the evolution of the employment-to-population ratios for manufacturing employment and total employment directly comparable, both are rescaled to have a value of one in 1975. Between 1975 and 2020 the share of the population employed in manufacturing declined by around 36% (from 16.9 to 10.8 percentage points), while the overall employment-to-population ratio increased by around 16% (from 56.8 to 66.1 percentage points). The relationship between the non-rescaled manufacturing employment-to-population ratio and far-right voting is presented in Figure A1 (panel A) in the Appendix. Vote-shares are stacked for the two parties making up the far-right camp in Austria (the Austrian Freedom Party, FPÖ and the Alliance For The Future Of Austria, BZÖ, see Section 2).

and are more likely to be male. Importantly this pattern not only emerges for Austria but also for practically all European countries where far-right populist parties are a relevant part of the political spectrum. Hence, Austria appears to be a rather typical case in terms of the observed simultaneity in the rise of the far-right and the decline of the manufacturing sector (Figure A1) as well as the demographic and labor market characteristics of the far-right voters base (Table A1). The Austrian far-right, in particular the Austrian Freedom Party (FPÖ), was however among the first right wing populist parties that saw wide spread electoral success already during the mid 1980s, while the rise of similar parties in the rest of Europe largely only commenced in the mid to late 1990s.[1] The FPÖ thus played a major role in establishing and defining the far-right populist movement and its political brand, which in subsequent years established itself in practically all of Europe, and with the victory of Donald Trump in the 2016 US presidential elections also spread to the United States.

Therefore this paper investigates the effect of the manufacturing decline on the rise of the far-right

[1]The sole exception here is the french Front Nationale which also rose to political relevance during the mid 1980s (see Figure A1)

in Austria. For this I use detailed employment data from the Austrian social security records combined with detailed data on regional election outcomes. This broad data basis allows to track regional employment changes specifically in the manufacturing industries and relate those to changes in regional electoral outcomes. To isolate the causal effect of changes in manufacturing employment on the vote-share of the far-right I apply an instrumental variable strategy that relies on variation in industry level employment trends in other European countries for identification. Since industry level employment trends in other countries are plausibly unrelated to unobserved regional confounders in Austria, this identification strategy allows to estimate the effects of the manufacturing decline independent of the simultaneously occurring effects of immigration, and thus to isolate the contribution of the manufacturing decline to the observed rise of the Austrian far-right.

The results of the analysis show that declines in manufacturing employment lead to pronounced increases in the vote-share of far-right parties. Overall the estimated effect explains around 43% of the observed increase in far-right voting during 1995-2017. Comparing the size of this effect to existing results from the literature on the electoral effect of immigration suggests that the contribution of the manufacturing decline to the rise of the far-right is only slightly smaller than the contribution of immigration. While immigration thus certainly is the stronger factor, the manufacturing decline has made a strong contribution to the rise of the far-right in Austria. Unpacking the mechanisms of this effect by applying a causal mediation analysis reveals that the entirety of the electoral effect of the manufacturing decline operates through increases in natives' unemployment rates, highlighting that it is indeed the adverse impact on natives' labor market prospects that is driving the results.

Looking at the impacts of trade and technology - two forces which strongly shaped the manufacturing decline - shows that both factors have an increasing effect on far-right vote shares, which operates primarily through their adverse effects on manufacturing employment. Both increases in rising trade exposure (measured by the change in net-import exposure from China and the former Eastern Block) and industrial robotization generally lead to declines in manufacturing employment and increase far-right voting. The aggregate effect of net-imports however masks strong effect heterogeneity with respect to import and export exposure. Regarding imports and exports separately reveals that, while they have roughly equal sized negative (imports) and positive (exports) effects on manufacturing employment, the effects on far-right vote shares are highly asymmetric. Here the increasing effect of rising imports is much larger than the decreasing effect of rising exports. Hence increased trade exposure appears to increase far-right voting even when the employment gains from increased exports offset employment losses due to increased import-competition.

Overall both trade and robotization have had increasing effects on far-right vote shares, whereby the the vast majority of both effects is mediated by their adverse impact on manufacturing employment. With regards to the magnitude of these effects, both factors have made similar sized contributions, as the effect of the manufacturing decline on the increase in far-right voting is explained in roughly equal parts by increases in trade exposure and industrial robotization.

This paper relates to a growing literature investigating the determinants of the electoral success of the far-right. A prominent strand of this literature ascribes this rise of the far-right to the simultaneously occurring increase in immigration. For example, Halla, Wagner, and Zweimüller (2017) provide evidence that immigration has been a major contributing factor behind the rise of the far-right in Austria, with similar results being documented for Denmark (Dustmann, Piil Damm, and Vasiljeva, 2019), France (Edo et al., 2019), Italy (Barone et al., 2016) or Switzerland (Brunner and Kuhn, 2018), among others. Steinmayr (2021) shows that during the refugee wave of 2015 mere exposure to refugees increased far-right voting, while actual contact had a decreasing effect. While immigration certainly is an important factor behind the rise of openly xenophobic far-right parties, its impact is likely not fully independent of the manufacturing decline. As the influx of medium to low skilled immigrants in particular increases labor market competition between natives and immigrants in a declining manufacturing sector, opposition of native workers of similar skill levels towards immigration is likely to increase (Mayda, 2006).

A related literature has recently provided evidence for an increasing effect of economic shocks on electoral support for the far-right. This literature, which was largely motivated by the surge in far-right voting in the wake of the Great Recessions, shows that general employment shocks (Dehdari, 2021), austerity measures (Fetzer, 2019), job loss and unemployment (Algan et al., 2017; Margalit, 2013) or financial crises in general (Funke, Schularick, and Trebesch, 2016) benefit the far-right. While these types of labor market shocks are mostly temporary in nature, the manufacturing decline represents arguably the largest structural shift western labor markets have experienced in recent decades. Hence this paper adds to this literature, by examining the effects of this large and persistent structural shift in labor demand on far-right voting.

This paper most closely relates to the literature examining the separate impacts of trade shocks (Dippel et al., 2022; Autor et al., 2020; Rodrik, 2018; Colantone and Stanig, 2018a, 2018b, Margalit, 2011) and recently also of automation (Anelli, Colantone, and Stanig, 2019, 2021, or Frey, Berger, and Chen, 2018), which has shown that the forces underlying the manufacturing decline did increase support for far-right populist parties. As these studies however only focus on trade and technology respectively, they only examine the reduced form relationship between the causes of the manufacturing decline and the rise of the far-right, where the actual mechanism at play - the structural decline of manufacturing employment itself - is not regarded directly. Hence these studies only tell part of the story, and consequently are unable to assess the full extent to which the declining manufacturing sector has contributed to the rise of the far-right. Lastly this paper contributes to this literature by highlighting that, while the employment effects of rising import- and export-exposure are roughly symmetric, the electoral effects are highly asymmetric, with the increasing effect of imports strongly dominating the offsetting effects of exports.

The rest of this paper is structured as follows: Section 2 provides a brief overview over the recent history of the Austrian far-right. Section 3 presents the used data sources, while Section 4 discusses the estimation of the effect of manufacturing employment on far-right voting and presents the main results of the analysis. Section 5 analyses the relative contributions of trade and technology to the overall manufacturing effect, while Section 6 benchmarks the size of the estimated effects against the observed increase in far-right voting and the contribution of immigration. To assess the validity of the empirical strategy, Section 7 presents a variety of robustness checks. Lastly, Section 8 briefly summarizes the results and concludes.

2 Background: The Austrian Far-Right

While most European countries did not experience the rise of the far-right until the late 1990s/early 2000s, the Austrian Freedom Party (FPÖ) was among the first far-right parties in Europe that found wide spread electoral success already during the 1980s (see Figure A1 in the Appendix). Up to this point the FPÖ was regarded as a right-liberal party. This drastically changed with the German-nationalistic wing of the party, lead by the up-and-coming Jörg Haider, taking over the party in 1986. This led to a sharp reorientation of the party towards far-right ideas and rhetoric. Core of this ideological re-orientation under Jörg Haider was a strong anti-immigration stance. This lead to the party initiating the "Anti-Foreigners Referendum" in 1993 which, among other things, demanded an immediate immigration stop, and was supported by 7.35% of voters. Building on this radical anti-immigration stance, the FPÖ - for the first time - found strong support in the working class, which up to this point was seen as the core-base of the Social Democrats (Pelinka, 2002). This movement of the working class towards the far-right continued during the 1990s, and saw the FPÖ rise to become one of the most important political forces in Austria. This trend culminated in the FPÖ reaching a vote-share of almost 27% in the 1999 elections (Figure 1). After this remarkable success in the 1999 national election, the FPÖ became part of the Austrian government which was led by the conservative Austrian People's Party (ÖVP). The inclusion of the far-right FPÖ in the Austrian government marked the first time since 1945 that an openly far-right party rose to power in any western European country.[2] Consequently it was met with widespread opposition and even caused the European Union to impose economic sanctions on Austria.

[2]The FPÖ was also part of a Social Democrat led government coalition from 1983 to 1986. During this time the FPÖ was seen as a right-liberal party. The coalition was terminated by the Social Democrats in 1986, after the far-right camp within the FPÖ took over the party.

This involvement of the FPÖ in the Austrian government however was relatively short lived, as internal conflict within the party forced early national elections in 2002. As is illustrated in Figure 1, this led to a dramatic drop in the FPÖs vote share and a secession of parts of the party into the newly formed (and similarly positioned) Alliance For The Future Of Austria (BZÖ). The newly formed BZÖ was led by Jörg Haider, the man responsible for the rise of the FPÖ, while his former party was lead into the 2006 national elections by the young Heinz-Christian Strache, who was politically socialized in German-nationalistic fraternities and had contacts to the Austrian neo-nazi scene (Horaczek and Reiterer, 2009). This split of the Austrian far-right into two parties, however only temporarily hampered the strength of the far-right, which soon returned to (combined) vote-shares of well above 25%. While the BZÖ was a relatively important part of the far-right populist camp in the early years of its existence, its popularity waned after the death of Jörg Haider in late 2008. Consequently the BZÖ dropped out of the Austrian parliament in the 2013 national elections, and the FPÖ regained its status as the sole parliamentary representation of the Austrian far-right.

Following a streak of electoral successes that saw the FPÖ regain its political strength, the FPÖ candidate Norbert Hofer entered the race in the Austrian presidential elections in 2016. He finished the first round of the presidential election in second place and thus entered the runoff election against the center-left candidate Alexander Van der Bellen. While Norbert Hofer lost this runoff election, he gathered a - for a far-right candidate unprecedented - vote-share of 46%.

Under the leadership of Heinz-Christian Strache, and with this remarkable performance in the presidential elections under their belt, the FPÖ again entered a government coalition with the conservative ÖVP in 2017. This second involvement of the far-right FPÖ in the Austrian government wasn't met by nearly as much opposition internationally as compared to their first involvement in 1999, as by 2017 the far-right populist movement has become much more normalized all across Europe. However, similarly to their 1999 involvement in the Austrian government, the 2017 involvement turned out to be rather short-lived, as a large scale corruption scandal in the FPÖ's leadership (known as the "Ibiza-scandal") forced early elections in 2019.

During the Covid-Pandemic the FPÖ (now under the leadership of Herbert Kickl) successfully positioned itself as a Covid-skeptic party. Consequently the party swiftly recovered from the losses caused by the Ibiza-scandal. While the next national elections in Austria are scheduled for the fall of 2024, the FPÖ has risen to the number one spot in national polling (with a pooled vote share of up to 29%). Even though the party has been plagued by numerous large scale scandals, it thus has risen to be one of the most relevant political forces in Austria.

3 Data

This Section presents the data sources used during the analysis. Generally, two types of data-sources are used, whereby changes in manufacturing employment and voting outcomes are measured at the regional level, while data for the construction of the instrumental variable, as well as for the measurement of trade and technology shocks is only available at the industry level.

While most regional data sources are available at the level of all Austrian municipalities, the analysis is performed at the level of 100 clustered commuting zones. This approach is chosen in order to control for the presence of spatial employment spillovers which are caused by commuting patterns. These commuting zones are computed analogously to commuting zones for the US (see Tolbert and Sizer, 1996). As is discussed in much detail in the Online Appendix of Bekhtiar (2022), these commuting zones perform much better in capturing spatial spillover effects than municipalities or political districts.[3] A detailed discussion of the estimation procedure, as well as an evaluation of their performance is available in the Online Appendix of Bekhtiar (2022).

Data on manufacturing employment in Austrian regions comes from the Austrian Social Security Database (ASSD, Zweimüller et al., 2009). The ASSD covers the universe of Austrian private sector employment starting in 1972. It contains detailed information on any firm's industry (according to the NACE Rev. 1.1 and Rev. 2 classifications) and geographical location (at the municipality level).

[3]Political districts are aggregated regional units which are delineated with political rather than labor market considerations in mind.

This allows to compute exact employment changes between elections by industry and geographical location, as well as regional industry structures. Data on regional unemployment rates, as well as the demographic structure of the regional population comes from the Austrian census and the Austrian Labor Market Statitics.[4]

To isolate the causal effect of changes in manufacturing employment on far-right voting, I rely on a Bartik-type instrumental variable.[5] The construction of this instrument additionally requires data on industry level changes in manufacturing employment from other high income countries. This data comes from European Structural Business Statistics (SBS). It is available online at EuroStat, and covers a large number of European countries starting in 1995.

Data on Austrian national elections is publicly available at the Austrian Ministry Of The Interior (BMI). The BMI provides detailed election results at the municipality level. This data includes the exact number of votes cast for any party, as well as the total number of eligible voters. Throughout the analysis I define the vote-share of the Austrian far-right as the combined vote-shares of the Austrian Freedom Party (FPÖ) and the Alliance For The Future Of Austria (BZÖ) (see Section 2).

To measure industry level trade flows I use detailed trade data from the UN-Comtrade database. This data contains the current US-Dollar trade value of imports and exports at the commodity level. These commodities have been crosswalked to the NACE (Rev. 2) 4-digit industries using the `concordance`-package in R (Liao et al., 2020). The trade values have been inflated to 2017 US-Dollars and converted to Euros using the average USD-Euro exchange rate for 2017.

Lastly, data on industry level changes in robotization comes from the International Federation of

[4]The Austrian census is only available for the years 1991, 2001 and 2011. Therefore data points for years between census years have been linearly interpolated. After 2011 the same variables are available on an annual basis in the Austrian Labor Market Statistics. Since these two data sources are compiled using the same register based approach first introduced in the 2011 census, they are internally consistent and directly comparable. Both data sources are available online at the Austrian statitical office Statistik Austria.

[5]This type of instrument has been first proposed by Bartik (1991) and is widely used in the analysis of local labor demand shocks. It is discussed in more detail in section 4.2.

Robotics (IFR). The IFR provides a large industry level dataset for many developed countries on the stock of installed industrial robots. This data is collected by the IFR through an annual survey among international robot suppliers which covers around 90% of the global market for industrial robots. This data has been first introduced into the economic literature in the work of Graetz and Michaels (2018), and has since then become the most widely used data source for studying the effects of robotization.

4 Manufacturing Employment & Far-Right Voting

4.1 Estimations

To assess the impact of changes in manufacturing employment on changes in the vote share of far-right parties, I estimate the following equation at the level of Austrian commuting zones:

$$\%\Delta Voteshare_{rt} = \gamma \times \%\Delta Manufacturing_{rt} + C_{rt}\beta + \rho_r + \tau_t + \epsilon_{rt} \tag{1}$$

Here the percentage-change in the far-right vote-share is regressed on the percentage-change in manufacturing employment. The parameter of interest is $\hat{\gamma}$ which captures the elasticity of changes in far-right vote-shares with respect to changes in manufacturing employment. Alternative specifications replace the percentage change in the vote-share with the percentage-point change.

The analysis is performed on the timeframe 1995 to 2017. Since in Austria early elections are the norm rather than the exception, and the time between elections thus varies, I partition the data into 4 sub-periods (1995-2002, 2002-2008, 2008-2013 and 2013-2017). Due to the timing of the election dates between 1995 and 2017, the sub-periods cannot be defined such that each period is of equal length. Therefore all variables that measure changes are re-scaled to 5-year differences.[6]

[6] Partitioning the data like this is motivated by data constraints. The year 1995 is chosen as starting point, because the SBS data used to construct the Bartik instrument is not available in prior years (see Section 4.2). Similarly, the year 2002 is chosen as starting point for the second panel period, because the IFR-robotics data (used in Section 5) is

All estimations are weighted by the start-of-period size of the local population eligible to vote.

To control for heterogeneity in regional characteristics all estimations contain a vector of control variables C, as well as commuting zone fixed effects ρ_r. Additionally period fixed effects τ_t are included to capture unobserved election specific determinants of voting outcomes like incumbent effects or the collapse of the far-right vote-share after internal conflict within the FPÖ caused a split of the far-right camp into two parties (see Section 2). The vector of controls C consists of several distinct variable types, aiming to capture regional heterogeneity with respect to industry structure, economic performance, regional political conditions, the demographic structure of the electorate, past employment trends and size and changes in the local immigrant population. These controls are generally pre-determined at the start of period t to capture the initial conditions of a commuting zone before the ensuing change in manufacturing employment occurs.

To control for regional differences in industry structure, which partly determine regional employment trends and might have compositional effects on voting outcomes, I include detailed employment shares of several sub-industries of the manufacturing and non-manufacturing sectors. These employment shares are measured at the beginning of each period and include the share of regional employment in the manufacturing of food, consumption goods, industrial goods and capital goods (for the manufacturing sector), as well as construction, utilities, personal services and business services (for the non-manufacturing sector). Employment in the primary sector (agriculture and mining & quarrying) serves as the baseline category and is thus excluded.[7]

The second set of included controls aims at capturing start-of-period differences in regional economic and political conditions. For this I include start-of-period values of the regional unemployment rate (decomposed by the respective contributions of natives and immigrants) as well as the logarithm of the gross regional product. To measure regional political conditions at the start of period t, I include measures for the composition of the regional governments at the level of

not available at the industry level for Austria in prior years.

[7] These employment shares are defined analogously to the work of Dauth et al. (2021).

the Austrian federal states. These controls are constructed as the share of a commuting zone's population living in a federal state with the local governor or vice-governor belonging either to the Austrian People's Party (ÖVP) or the Austrian Freedom Party (FPÖ). For commuting zones that are entirely contained within one of Austrias nine federal states, these variables are simple dummies, while for commuting zones that stretch across federal state borders, they measure the corresponding fractions of the population. The only other party that governed on the federal state level during the sample period is the Austrian Social Democrat Party (SPÖ) which serves as baseline.

In a third step, I include controls for the demographic characteristics of the regional electorate. For this I include the start-of-period share of natives differentiated by gender, four age groups (16-29, 30-49, 50-64 and 65+) and three educational groups (high-, medium- and low-education) in a commuting zone's overall population. Additionally this set of controls includes the share of a commuting zone's native voting age population living in urban areas, central rural areas and remote rural areas to control for the degree of urbanization.[8]

If employment trends are highly persistent over time, it is conceivable that part of the estimated effect for the contemporaneous change in manufacturing employment reflects persisting effects of past employment changes (see Jaeger, Ruist, and Stuhler, 2018). To control for this possibility, I include the first lag of the percentage-change in manufacturing and non-manufacturing employment into the set of control variables.

Lastly, I include controls for the size and development of the immigrant population. These controls include the shares of high-, medium- and low-skilled immigrants in a commuting zone's overall population, as well as the percentage-change of these education specific migrant-shares. While the included migrant shares are pre-determined with respect to the change in manufacturing employment occurring during the ensuing period, the percentage-change in those migrant shares occurs simultaneously with the change in employment. Because of this simultaneity between the

[8]Urban and (central and remote) rural areas are defined according to the official Urban-Rural classification published by the Austrian Statistical Agency.

explanatory variable of interest (the percentage-change in manufacturing employment) and the percentage-changes in the migrant shares, these controls might be regarded as ’bad controls’ in the sense of Angrist and Pischke (2008), as they could be regarded as being an outcome themselves. However, as a vast literature has shown, changes in immigration are a prime driver of the rise of the far-right (see Halla, Wagner, and Zweimüller, 2017 and Steinmayr, 2021 for results for Austria). Therefore these controls are nevertheless included to (i) account for changes in immigration and (ii) check for the stability of the estimates with respect to the inclusion of these controls.

4.2 Identification Strategy

Since equation 1 is specified in percentage changes, the estimate $\hat{\gamma}$ directly captures the elasticity of the far-right vote share with respect to changes in manufacturing employment. Estimating equation 1 solely via OLS is however unlikely to result in unbiased estimates for $\hat{\gamma}$, and thus likely fails in isolating the causal effect of changes in manufacturing employment on far-right voting.

As is discussed in much detail in Goldsmith-Pinkham, Paul, and Swift (2020), the prime source of endogeneity concerns in this type of setting stems from the idiosyncratic regional component of employment growth. To more clearly illustrate this notice that region r's employment growth rate in industry i (for any period t) can be decomposed into a national industry growth rate component $\%\Delta Employment_{it}$ and a region specific residual $\tilde{g}_{irt}$ capturing idiosyncratic local determinants of employment growth:

$$\%\Delta Employment_{irt} = \%\Delta Employment_{it} + \tilde{g}_{irt} \tag{2}$$

Here the first part of the sum in expression 2 captures industry wide trends that are the same for all regions in Austria and the second part $\tilde{g}_{irt}$ captures the influence of region specific factors. While the national industry growth rate $\%\Delta Employment_{it}$ in expression 2 is by definition unrelated to (observed or unobserved) regional factors, the idiosyncratic regional component $\tilde{g}_{irt}$ is likely to be influenced by unobserved regional confounders. For example $\tilde{g}_{irt}$ may be correlated with political incentives to settle a firm in region r, regional agglomeration economies or regional demand shocks that might themselves be influenced by the regional political climate, hence causing an endogeneity problem.

To address this issue, I instrument the percentage change in manufacturing employment with a variant of the Bartik instrument. This type of instrumental variable aims at isolating the industry specific component of employment growth. It has been proposed by Bartik (1991) and was popularized in the economic literature by Blanchard and Katz (1992) and Autor and Duggan (2003). The Bartik instrument is built on an accounting identity that states that the aggregated growth rate of employment can be expressed as a weighted sum of industry level growth rates, whereby the relative sizes of each industry (measured by its share in overall employment) serve as weights:

$$\%\Delta Employment_{rt} = \sum_{i} \frac{Employment_{irt}}{Employment_{rt}} \times \%\Delta Employment_{irt} \tag{3}$$

Building on the accounting identity in equation 3 the Bartik instrument is constructed by replacing the industry level growth rates $\%\Delta Employment_{irt}$ in region r by the industry level growth rates in other geographical regions. While the classical Bartik instrument (as originally proposed in Bartik, 1991) calculates these industry level growth rates from regions within the same country, I construct the instrument using industry-employment changes from other high income countries. This broadly follows the intuition behind shift-share instruments used in the literature on trade- and technology shocks, which generally relies on industry level variation in other high income countries for identification, and has the straight forward appeal that employment changes in other countries are much more likely to be exogenous to regional voting behavior in Austria, as opposed to employment changes in other Austrian regions.[9]

[9] Data on employment changes by 3-digit manufacturing industry for the computation of $\%\Delta Employment^{OtherCountries}$ come from the Structural Business Statistics (SBS) from EuroStat, and is available from 1995 onward. The industry-employment growth rates $\%\Delta Employment^{OtherCountries}$ are computed as averages over all countries for which sufficient data is available. EuroStat censors data-points in the SBS data, whenever the number of reporting firms in a given country-industry-year cell is too small to guarantee anonymity.

To further remove the instrument from the accounting identity in equation 3, and to thus avoid mechanical correlations between the instrument and the explanatory variable, the employment shares used to project these industry growth rates onto the regional level are lagged 15 years into the past.[10] The construction of the Bartik instrument is outlined in equation 4:

$$Bartik_{rt}^{IV} = \sum_{i} \frac{Employment_{irt-15}}{Employment_{rt-15}} \times \%\Delta Employment_{it}^{OtherCountries} \tag{4}$$

To ensure that the instrument in equation 4 isolates variation in manufacturing employment changes, I restrict the set of industries from which the instrument is constructed to all 3-digit manufacturing industries, such that $i \in$ Manufacturing Industries and $\sum_i \frac{Employment_{irt}}{Employment_{rt}} = 1$. Since the exposure shares used to construct the instruments thus sum up to one, this further ensures that conventional period fixed effects are able to isolate within-period variation.[11]

Recently, several papers have thoroughly discussed under which conditions Bartik-type instrumental variables are able to plausibly isolate the causal effect of employment growth. This work

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While the SBS-data in principle covers all member-states of the European Union, very small countries are thus not usable due to a very large number of censored data points. Similar issues arise for Poland and the United Kingdom, where entire years are missing. These countries have been removed. Additionally Germany is removed because Austria shares very strong trade-linkages with the German economy. The final sample of countries in the SBS used to construct $\%\Delta Employment^{OtherCountries}$ consists of Belgium, Czechia, Finland, France, Hungary, Italy, Netherlands, Norway, Portugal, Spain and Sweden. Since the SBS-data changes the used industry classification in 2008, the periods 1995-2002 and 2002-2008 use the NACE Rev. 1.1 industry classification, while the periods 2008-2013 and 2013-2017 use the NACE Rev. 2 classification.

[10]While many studies relying on Bartik-type instruments uniformly lag the exposure shares to the same base year, I opt to lag the exposure shares for each panel period by 15-years. Doing this leads to a stronger first stage. As is discussed in Borusyak, Hull, and Jaravel (2022), this updating of exposure shares is valid in this application since equation 1 is specified in stacked differences (see Borusyak, Hull, and Jaravel, 2022, page 196). Alternative specifications with fixed exposure shares are presented in robustness checks in Section 7.

[11]As is shown in Borusyak, Hull, and Jaravel (2022), conventional period-fixed effects require some adjustments in settings where the exposure shares are incomplete, i.e. do not sum to one. See Section 5 for more details.

has shown that the primary source of endogeneity concerns relates to the idiosyncratic regional component of employment growth ($\tilde{g}_{irt}$ in equation 2), which may be correlated with unobserved regional confounders. The instrument in equation 4 thus must be orthogonal to this region specific component of employment growth. Recent papers by Adao, Kolesár, and Morales (2019) and Borusyak, Hull, and Jaravel (2022) argue that this condition is satisfied when the industry level employment growth rates used to compute the instruments $\%\Delta Employment_i^{OtherCountries}$ are plausibly unrelated to unobservables at the regional level. This *exogenous shocks* condition is both necessary and sufficient for identification in Bartik-type settings. The employment shares used to project the employment shocks onto the regional level are thus explicitly allowed to be endogenous (Borusyak, Hull, and Jaravel, 2022). The exogenous shocks condition thus requires that the growth rates of manufacturing employment in other European countries are only correlated with regional manufacturing growth via common industry level trends and are uncorrelated with idiosyncratic regional determinants of employment growth.

As is briefly mentioned above, I construct the Bartik instrument from industry level employment changes in other European countries, while the instrument as originally proposed by Bartik (1991) relies on changes in other regions of the same country. Intuitively the exogenous shocks condition appears to be much more plausible when computing $\%\Delta Employment_i^{OtherCountries}$ from other high income countries, since employment growth in other countries is geographically much more removed from any Austrian region, as opposed to employment growth in other Austrian regions. This is especially the case for endogeneity concerns that relate to the presence of spatial spillover effects. While such spillovers would contaminate the Bartik-instrument when constructed from employment changes in the same country, this would not be the case when constructing it from other European countries.

In related work Goldsmith-Pinkham, Paul, and Swift (2020) have shown that the exogeneity of exposure shares (i.e. the lagged employment shares in equation 4) is also a sufficient (but not necessary) condition for identification using Bartik-type instruments. This *exogenous shares* condition requires that past industry structures are exogenous to regional unobservables. For analyzing the effect of manufacturing employment changes on voting outcomes this exogenous shares condition however appears somewhat implausible, as it is conceivable that regional industry structures (even when lagged) have an effect on voting outcomes beyond what is captured by the employment-growth channel. For example regional industry structures might affect the composition of the workforce which might directly affect political preferences and voting outcomes, even when manufacturing employment remains stable.

While neither the *exogenous shocks* nor the *exogenous shares* condition can be tested directly (as they essentially mirror a standard exclusion restriction), the papers by Borusyak, Hull, and Jaravel (2022) and Goldsmith-Pinkham, Paul, and Swift (2020) propose several plausibility checks for each respective condition. As I regard the *exogenous shocks* condition as more plausible in this case, I follow the recommendations in Borusyak, Hull, and Jaravel (2022), and conduct several tests to assess the plausibility of shock exogeneity. These plausibility checks are discussed in Section 7.

4.3 Standard Errors

Throughout all estimations I present two different types of standard errors. Firstly, I rely on conventional heteroskedasticity robust standard errors. Secondly, I use clustered standard errors that are specifically tailored to the structure of the used instrumental variables. Since the Bartik instrument for manufacturing employment (but also the instruments for trade and robot exposure in Section 5) are computed as shift-share instruments, I use the shift-share exposure clustered standard errors proposed by Adao, Kolesár, and Morales (2019) (henceforth referred to as AKM standard errors). Adao, Kolesár, and Morales (2019) have shown that the residuals from regressions using shift-share instruments are correlated between regions with similar industry structures (rather than between neighboring regions). They thus propose a procedure that clusters regions according to their industry structures.

Since there is a break in the industry classification in the SBS-data, and thus also in the shocks used in equation 4 to compute the Bartik instrument[12], I compute the AKM-standard errors by clustering according to the NACE Rev. 2 (3-digit) industry structure. Additional to regional clustering, I allow for clustering on the 2-digit industry level (according to Adao, Kolesár, and Morales, 2019, equation 37).

4.4 Main Results

Table 1 presents the estimation results for the effect of changes in manufacturing employment on changes in far-right vote-shares. Panel A presents OLS estimates. In sum, all estimations show a clear and robust negative relationship between manufacturing employment and far-right voting. The OLS estimation in column (1) results in an estimated coefficient of -0.382, which indicates that a 1% decrease in manufacturing employment is associated with a roughly 0.38% increase in the vote-share of the far-right. Including further control variables in columns (2) to (6) leaves this estimate relatively stable, with the specification including full controls in column (6) resulting in a precisely estimated coefficient of -0.294 which is significant at the 1%-level.[13]

Instrumenting the percentage-change in manufacturing employment in panel B of Table 1 leads to an increase in the absolute size of the point estimates. Here the full specification in column (6) indicates an elasticity of -1.181 (panel B). The 2SLS estimations thus confirm a precisely estimated negative relationship between employment changes in the manufacturing sector and far-right voting. These estimates indicate that a 1% decrease in manufacturing employment leads to an increase in far-right voting of between roughly 0.97% (column 1) to 1.18% (column 6).

While all controls that are included in columns (1) to (5) of Table 1 are fixed (and thus pre-determined) at the start of each panel period, the controls included in column (6) (i.e. percentage-change in the share of high-, medium- and low-skilled immigrants) occur simultaneously with

[12]The industry level employment data for other European countries in the SBS uses the NACE Rev. 1.1 (3-digit) classification from 1995 to 2008, and the NACE Rev. 2 (3-digit) classification from 2008 onward.

[13]Since the OLS estimations in Table 1 do not include a shift-share variable (i.e. the Bartik instrument), only conventional robust standard errors are reported here.

Table 1: Manufacturing Employment and far-right voting (1995-2017)

	Dependent Variable: %Δ Voteshare Far-Right Parties					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: OLS Estimations:						
%Δ Manufacturing Emp.:	-0.382	-0.3	-0.274	-0.284	-0.277	-0.294
	(0.149)**	(0.105)***	(0.102)***	(0.103)***	(0.103)***	(0.104)***
Panel B: 2SLS Estimations:						
%Δ Manufacturing Emp.:	-0.967	-1.235	-1.159	-1.325	-1.437	-1.181
	(0.214)***	(0.267)***	(0.401)***	(0.443)***	(0.483)***	(0.418)***
	[0.283]***	[0.241]***	[0.272]***	[0.289]***	[0.285]***	[0.23]***
Panel C: First Stage Estimations:						
Bartik^{IV}:	0.214	0.184	0.155	0.153	0.145	0.157
	(0.015)***	(0.02)***	(0.024)***	(0.023)***	(0.023)***	(0.022)***
	[0.01]***	[0.008]***	[0.009]***	[0.01]***	[0.012]***	[0.011]***
First-Stage F-Statistic:	207.67	85.71	42.46	42.61	39.77	51.3
Period Fixed Effects	x	x	x	x	x	x
Commuting Zone Fixed Effects	x	x	x	x	x	x
Industry Structure	x	x	x	x	x	x
Regional Characteristics		x	x	x	x	x
Demographic Characteristics			x	x	x	x
Lagged employment changes				x	x	x
Migrant shares (by skill groups)					x	x
%Δ Migrant shares						x
Commuting Zones	100	100	100	100	100	100
Periods	4	4	4	4	4	4
Observations	400	400	400	400	400	400

Notes: * < 0.10, ** < 0.05, *** < 0.01. Heteroskedasticity robust standard errors are reported in round brackets, while industry structure clustered standard errors from Adao, Kolesár, and Morales (2019) are reported in square brackets. Units of observation are 100 clustered commuting zones. All specifications include a set of region and period fixed effects, as well as start-of-period employment shares of several sub-industries of manufacturing (production of food products, consumer goods, industrial goods and capital goods), as well as industries outside of manufacturing (utilities, construction, personal services and business services). Regional characteristics control for the start-of-period unemployment rate (decomposed by the respective contribution of natives and immigrants), the logarithm of the gross regional product, as well as start-of-period party affiliation of the local governor and vice governor. Demographic controls include the start-of-period structure of the native voting-age population, as well as the start-of-period degree of urbanization. Lagged employment controls include the first lag of the percentage-changes in manufacturing and non-manufacturing employment. Migrant shares (in start-of-period levels and percentage-changes) are included separately for three skill groups (high-, medium- and low-skilled migrants). All estimations are weighted by the start-of-period native voting-age population.

the percentage-change of manufacturing employment. Notably the inclusion of these simultaneous controls has only a very small (and statistically insignificant) impact on the overall magnitude of the point estimates. Since this picture occurs both in the OLS and 2SLS regressions, this suggests that the location choices of newly arriving immigrants are relatively unrelated to shifts in manufacturing employment. This is consistent with the view of the immigrant-enclave literature (see for example Card, 2001), which shows that immigrants tend to move to areas that already have communities of immigrants from their home country irrespective of regional economic conditions. This relative independence of the change in manufacturing employment and the change in immigration, as indicated by comparing the point estimates in columns (5) and (6) of Table 1, is very reassuring in that the estimations in Table 1 indeed isolate the effect of the manufacturing decline, rather than picking up on simultaneous increases in immigration.

Looking at the first stage results in panel C of Table 1 shows that the Bartik instrument is sufficiently strong with the first-stage F-statistic clearly exceeding the commonly applied threshold of 10. Also the instrument appears to be highly relevant, as the first-stage coefficients are very precisely estimated. The first-stage coefficient has the expected sign and is robust to the inclusion of further control variables between columns (1) and (6). The point estimate of the first stage coefficient in the full specification in column 6 indicates that around 16% of observed regional employment growth is explained by industry wide trends common to Austrian commuting zones and the countries used to construct the instrument, with the remainder being explained by either (i) nationwide determinants in Austria (like differences in competitiveness) and (ii) idiosyncratic regional determinants. Figure A2 in the Appendix summarizes the first-stage and reduced form relationships graphically, to investigate the presence of heavy outliers that may be driving the results. This is especially important because the capital city of Vienna accounts for roughly one-fourth of the population in Austria. Panels A and C of Figure A2 show that the 2SLS estimation is not driven by the presence of outliers, while panels B and C show that the 2SLS relationship is also not determined by the capital city Vienna.

4.5 Inter-party dynamics

While Table 1 shows that the Austrian far-right has benefited from the decline in manufacturing employment, Table 2 examines the inter-party dynamics of the electoral effect of the manufacturing decline. Columns (1) to (5) of Table 2 show the reaction of the vote-shares of all parties that consistently participated in each election since 1995, whereby these parties are sorted according to the their position in the political spectrum (according to their average Right-Left Score from the Manifesto Project, see Merz, Regel, and Lewandowski, 2016). These parties are the Communist Party (column 1), the Social Democrats (column 2), the Green Party (column 3), the conservative Austrian People's Party (column 4) and the far-right camp consisting of the Austrian Freedom Party and the Alliance For The Future Of Austria (column 5). Column 6 summarizes all remaining parties (i.e. the parties who did not consistently take part in each election during 1995-2017).[14] Lastly column 6 shows the change in the share of non-voters.

In contrast to Table 1 the estimates in Table 2 use the percentage-point change in the vote-shares of each party (instead of the percentage-change) as dependent variable. To calculate these percentage point changes, the overall eligible population is used as denominator (instead of the actual voter turnout) to be able to also account for movement in and out of vote participation (via the inclusion of the percentage point change in non-voting). This ensures that the estimates in Table 1 add up to

[14]Since these parties do not appear in all of the used elections, no changes in their individual vote shares can be calculated. Therefore these parties are aggregated to a single residual category. In total this category summarizes 25 parties, most of which never managed to gather enough votes to enter the Austrian parliament (for which a minimum vote-share of 4 percentage points is required). The only parties contained in the the 'others' category who were part of the Austrian parliament at some point during 1995-2017 (and thus held some degree of political relevancy) are the liberal NEOS party (from 2013 onward), the populist Team Stronach of billionaire Frank Stronach (part of the parliament between 2013 and 2017) and the left-wing populist PILZ party (who entered the Austrian parliament briefly in the 2017 election but dropped out in 2019). Since the Manifesto Project does not report Right-Left scores for the majority of the fringe parties, no average R/L-score can be computed for this residual category. The parties summarized in this category however are very heterogeneous and are positioned all across the political spectrum.

Table 2: Manufacturing Employment and far-right voting (1995-2017)

		Established Parties					
	(1) Communists	(2) Social Democrats	(3) Greens	(4) Conservatives	(5) Far-right	(6) Other	(7) Non-Voters
Avg. Manifesto Right-Left Score	-21.83	-15.3	-11.71	3.09	7.32		
%Δ Manufacturing Emp.:	-0.014	0.151	0.02	-0.029	-0.272	0.163	-0.019
	(0.004)***	(0.043)***	(0.024)	(0.059)	(0.058)***	(0.044)***	(0.071)
	[0.001]***	[0.03]***	[0.011]*	[0.044]	[0.023]***	[0.011]***	[0.063]
First-Stage F-Statistic:	51.3	51.3	51.3	51.3	51.3	51.3	51.3
Period Fixed Effects	x	x	x	x	x	x	x
Commuting Zone Fixed Effects	x	x	x	x	x	x	x
Regional Characteristics	x	x	x	x	x	x	x
Demographic Characteristics	x	x	x	x	x	x	x
Industry Structure	x	x	x	x	x	x	x
Lagged employment changes	x	x	x	x	x	x	x
Migrant share (by skill groups)	x	x	x	x	x	x	x
%Δ Migrant Shares	x	x	x	x	x	x	x
Commuting Zones	100	100	100	100	100	100	100
Periods	4	4	4	4	4	4	4
Observations	400	400	400	400	400	400	400

Notes: * < 0.10, ** < 0.05, *** < 0.01. Heteroskedasticity robust standard errors are reported in round brackets, while industry structure clustered standard errors from Adao, Kolesár, and Morales (2019) are reported in square brackets. Units of observation are 100 clustered commuting zones. The dependent variables are the percentage-point change in the vote-share of all parties that consistently took place in each national election since 1995. The category 'other' in column (5) summarizes all parties that did not consistently participate in the Austrian parliamentary elections during the sample period. The parties are sorted according to average the Right-Left score from the Manifesto Project (Merz, Regel, and Lewandowski, 2016) for the period 1995-2017, starting with the outmost left party (the Communist Party in column 1) to the far-right (in column 5). To be able to account for movements in and out of electoral participation, the vote shares are constructed with the overall eligible population as denominator (instead of voter turnout). This allows to also regard the change in the share of non-voters in column 7. All specifications include a set of region and period fixed effects, as well as start-of-period employment shares of several sub-industries of manufacturing (production of food products, consumer goods, industrial goods and capital goods), as well as industries outside of manufacturing (utilities, construction, personal services and business services). Regional characteristics control for the start-of-period unemployment rate (decomposed by the respective contribution of natives and immigrants), the logarithm of the gross regional product, as well as start-of-period party affiliation of the local governor and vice governor. Demographic controls include the start-of-period structure of the native voting-age population, as well as the start-of-period degree of urbanization. Lagged employment controls include the first lag of the percentage-changes in manufacturing and non-manufacturing employment. Migrant shares (in start-of-period levels and percentage-changes) are included separately for three skill groups (high-, medium- and low-skilled migrants). All estimations are weighted by the start-of-period native voting-age population.

zero, and allows an interpretation of the results as voter flows between parties and into non-voting.

As before in Table 1 the estimation result for the effect of changes in manufacturing employment on the vote share of the far-right indicates a negative relationship (Table 2, column 5). Hence declines in manufacturing employment lead to an increase of the vote-share of far-right parties. These vote gains of the far-right primarily come at the expense of the Social Democrats, for whom the point estimate in column (2) indicates that a one percent decline in manufacturing employment leads to 0.151 percentage-point decline in vote-shares. While it cannot be inferred with certainty that these lost voters of the Social Democrats directly moved to the far-right (since other parties also show significant reactions) it is nevertheless interesting that the estimated losses for the Social Democrats account for roughly 56% of the vote gains for the far-right. What can be inferred however (from regarding only the established parties in Table 2) is that the manufacturing decline has contributed to a rightward shift in the Austrian parliament away from the Social Democrats and towards far-right populism. Apart from the Social Democrats other established parties in the Austrian parliament - i.e. the Green party (column 3) and the conservative Austrian People's Party (column 4) - do not show any relevant reactions to the manufacturing decline.

While the inter-party dynamics among the established political parties in columns (2) to (5) thus indicate a clear shift away from the Social Democrats towards the far-right, the vote-share of less established fringe parties (column 6) also declined along with manufacturing employment. These parties (with the exception of the liberal NEOS party who established themselves as a regular part of the Austrian parliament in 2013) regularly position themselves outside of the political mainstream and aim to gain support from voters who are dissatisfied with the political status quo. In some aspects they thus speak to the same anti-establishment sentiments that are core of the far-rights rhetoric. It appears thus rather plausible that this kind of voters is more accessible for the far-rights talking points, especially when economic conditions worsen.

Of all parties in Table 2 who consistently took part in every election since 1995, the Communist Party (column 1) is the only one who never was part of the Austrian parliament, as it never was able to obtain a vote-share exceeding 4 percent. It thus does not possess the type of political relevancy as the other, more established parties in Table 2 do. Nevertheless it is interesting to consider the Communist Party, as it is the only party in Austrian politics that can be considered a far-left party. Including this party thus allows to examine if both extremes of the political spectrum (i.e. the far-left and the far-right) benefited from the manufacturing decline, or if this phenomenon is exclusively concentrated on the far-right. Indeed the estimation result for the Communist Party in column (1) of Table 2 indicates that the far-left also benefited from the manufacturing decline, albeit to a much smaller extent, as the percentage-point gain for the far-right in column (5) is approximately 20 times as large as the percentage point gain of the far-left.

Lastly, the estimation for the effect on the share of non-voters results in a very small and statistically insignificant estimate, suggesting no effect of the manufacturing decline on voter turnout.

4.6 Mechanism: The Manufacturing Decline and the Rise in Unemployment

As is suggested by Figure 1, the decline in manufacturing employment is not simply the mirror image of the simultaneously occurring rise in service employment, but rather coincides with a pronounced increase in unemployment rates. This notion is also confirmed by re-estimating equation 1, using the change in unemployment rates of native workers as dependent variable. These estimations are shown in Table A2 in the Appendix. In sum these estimations result in a robust negative estimated effect, suggesting that declines in manufacturing employment directly lead to increases in unemployment among the Austrian electorate. Here the point estimate for the specification including a full set of controls[15] in column 6 of Table A2 indicates that a one percent decrease in manufacturing employment leads to an increase in natives' unemployment rates by about 0.042 percentage points. This effect is precisely estimated in both standard error definitions.

Since Tables 1, 2 and A2 show that declines in manufacturing employment lead to increases

[15] Since the start-of-period value in unemployment rate is itself part of the percentage-point change in unemployment rates, it is removed from the set of control variables.

in both unemployment rates and far-right vote-shares, it is natural to ask whether the effect on far-right voting operates through the increase in unemployment. This appears to be a plausible mechanism for the voting effect of the manufacturing decline, as unemployment has been shown in the literature to have an increasing effect on far-right voting (see Algan et al., 2017). To investigate this potential mechanism, this section presents the results of a causal mediation analysis which aims at disentangling the direct and indirect effects of changes in manufacturing employment, whereby the indirect effect is mediated through changes in unemployment. In particular I rely on the parametric mediation analysis in single instrument settings from Dippel et al. (2022, 2020). This framework is depicted graphically as a causal diagram in Figure A3 in the Appendix. Here the treatment variable X (the change in manufacturing employment) affects the outcome Y (the change in far-right vote-shares) through two different channels. Firstly it has a direct effect that operates directly from $X \rightarrow Y$. Secondly, the treatment X causally affects a mediator variable M (the change in natives unemployment rates), which in turn has an effect on the outcome Y. This indirect effect thus operates through two causal pathways, namely $X \rightarrow M$ and $M \rightarrow Y$. In this framework the effects of the treatment on the mediator variable $X \rightarrow M$ and the effect of the treatment on the outcome variable $X \rightarrow Y$ are straight forwardly identified through the use of the Bartik-instrument. The identification of the third causal pathway $M \rightarrow Y$ could in principle be achieved by using a separate instrumental variable to introduce exogenous variation in the mediator M (see Frölich and Huber, 2017). It has been shown by Dippel et al. (2022, 2020) that in the absence of such a separate instrumental variable, the causal path $M \rightarrow Y$ can also be identified in the single IV setting, provided a *partial confoundedness* assumption holds.

This partial confoundedness assumption (which is also graphically depicted in Figure A3) requires that the unobserved confounders that cause endogeneity concerns for $X \rightarrow M$ are orthogonal to the unobserved confounders that cause endogeneity in $M \rightarrow Y$. To further investigate whether this partial confoundedness is plausible in this setting, consider the endogeneity concerns in both causal pathways separately.

Starting with the path $X \rightarrow M$ firstly note that changes in the unemployment rate are mechanically determined by changes in employment in both the manufacturing and the non-manufacturing sectors. So any unobserved variable that exclusively shifts manufacturing employment is located upstream to the treatment X on the causal path $X \rightarrow M$. Therefore any effect it may have on manufacturing employment X can affect the unemployment rate M only through its effect on X, and is generally no source of endogeneity concerns for the path $X \rightarrow M$. Since this rules out any unobserved shifters of manufacturing employment, any source of endogeneity must stem from the other mechanical co-determinant of the unemployment rate, namely changes in non-manufacturing employment. For this to cause endogeneity in $X \rightarrow M$, it would additionally require a direct effect of employment changes in e.g. the service sector on changes in manufacturing employment. Put differently, this mechanical relationship between changes in (manufacturing and non-manufacturing) employment implies that endogeneity concerns in the causal path $X \rightarrow M$ primarily stem from between sector employment spillover effects.[16]

While the endogeneity concerns in $X \rightarrow M$ primarily stem from the mechanical relationship between employment changes and unemployment changes, the path $M \rightarrow Y$ is primarily affected by the same endogeneity concerns that were already articulated in section 4.2, that is by political incentives to settle a firm in region r, regional agglomeration economies or regional demand shocks that might (i) affect the conditions of the local labor market and thereby also unemployment rates and (ii) may be influenced by the regional political climate.

[16]This explanation abstracts away from changes in labor market participation. Changes in labor market participation however could themselves be regarded as a mediator variable, as they may be an outcome of changes in manufacturing employment, but could be considered a cause of far-right voting increases in a similar fashion as changes in unemployment rates. Changes in labor market participation are thus best characterized as an unobserved mediator in this setting. It is shown in Dippel et al. (2020) that this does not affect the identification of the mediation relationship between changes in manufacturing employment, unemployment rates and far-right vote shares. Corresponding robustness checks in Table A3 in the Appendix, where the change in labor market participation rates is controlled for, confirm this notion.

Notice, however, that any unobserved shock that affects both the manufacturing and the non-manufacturing sectors poses an additional threat to the partial confoundedness assumption. Even in the absence of between sector spillovers an unobserved demand shock that affects both sectors induces a correlation between employment changes in the two sectors. In this scenario unobserved demand shocks are an endogeneity concern in both causal pathways $X \rightarrow M$ and $M \rightarrow Y$, which would violate the partial confoundedness assumption. Corresponding robustness checks in Table A3 in the Appendix however indicate that all results from the mediation analysis are robust to additionally controlling for changes in non-manufacturing employment. This indicates that the partial confoundedness assumption appears to be reasonable in this setting.

If the partial confoundedness assumption holds, the causal pathway $M \rightarrow Y$ can be identified by using the Bartik instrument as an IV for the mediator variable, while conditioning on the treatment X.[17] The details and main equations of this mediation model are summarized in Panels C, D and E of Table 3.

Panel A of Table 3 presents the results of the mediation analysis. Notice that since the change in the unemployment rate of natives serves as mediator variable M, their start-of-period unemployment rate (which were included in the set of regional controls in Tables 1 and 2) has to be removed from the vector of control variables. The reason for this modification of the vector of controls is that the start-of-period native unemployment rate is obviously itself a component of the mediator variable. Omitting this variable from the set of controls leads to a drop in the size of the estimated total effect ($\hat{\gamma}^{IV}$ in Table 3). Here the point estimate drops from -1.181 (Table 1, column 6) to -0.917 (Table 3, column 1) when using the percentage-change in far-right vote shares as dependent variable. A similar drop arises when using the percentage-point change in far-right vote-shares as dependent variable. Here the point estimate drops slightly from -0.272 (Table 2, column 5) to -0.248. These differences in point estimates are however not statistically significant.

[17] As is outlined in Dippel et al. (2020), conditioning on X is a necessary condition to be able to identify $M \rightarrow Y$ in the single instrument setting.

Table 3: Mediation Analysis: The Role of Unemployment Increases

	%Δ Vote-Share	pptΔ Vote-Share	
	(1)	(2)	Computation
Panel A: Mediation Analysis			
Total Effect:	-0.917	-0.248	$\hat{\gamma}^{IV} = \hat{\gamma}_Y^X + \hat{\gamma}_M^X \times \hat{\gamma}_Y^M$
	(0.326)***	(0.044)***	
	[0.237]***	[0.017]***	
Direct Effect:	-0.099	0.055	$\hat{\gamma}_Y^X$
	(0.173)	(0.034)	
	[0.034]***	[0.003]***	
Indirect Effect:	-0.819	-0.303	$\hat{\gamma}_M^X \times \hat{\gamma}_Y^M$
	(0.369)**	(0.056)***	
	[0.239]***	[0.018]***	
Panel B: Model Parameters			
$\hat{\gamma}^{IV}$	-0.917	-0.248	Estimation of equation 1 via 2SLS
	(0.326)***	(0.044)***	(Estimates deviate from Tables 1 and 2 because
	[0.237]***	[0.017]***	unemployment rate is excluded from controls)
$\hat{\gamma}_M^X$	-0.042	-0.042	Estimation of equations M1.1 and M1.2
	(0.021)**	(0.021)**	
	[0.017]**	[0.017]**	
$\hat{\gamma}_Y^X$	-0.099	0.055	Estimation of equations M2.1 and M2.2
	(0.173)	(0.034)	
	[0.034]***	[0.003]***	
$\hat{\gamma}_Y^M$	19.591	7.242	Estimation of equations M2.1 and M2.2
	(17.349)	(4.161)*	
	[15.538]	[3.559]**	

Summary Mediation Model (see also Figure A3):

Panel C: Model Notation

Outcome Y:	Δ Vote-Share Farright (meassured in %-changes or %-point-changes)
Treatment X:	Δ Manufacturing Employment
Mediator M:	Δ Unemployment Rate (Natives only)
Instrument IV:	Bartik-Instrument
Vector of Controls C:	Full set of controls, plus period and commuting zone fixed effects
u, v:	Unobservable Confounders

Panel D: Model Equations:

Effect of Treatment X on Mediator M

M1.1 (First Stage): $X = \gamma_X^{IV} IV + C\beta_X^C + \epsilon_X$

M1.2 (Second Stage): $M = \gamma_M^X X + C\beta_M^C + \epsilon_M$

Effect of Mediator M on Outcome Y

M2.1 (First Stage): $M = \gamma_M^{IV} IV + \beta_M^X X + C\beta_M^C + \tilde{\epsilon}_M$

M2.2 (Second Stage): $Y = \gamma_Y^M M + \gamma_Y^X X + C\beta_Y^C + \epsilon_Y$

Panel E: Assumptions:

Instrument Exogeneity: $IV \perp\!\!\!\perp (\epsilon_X, \epsilon_M, \epsilon_Y)$

Partial Confoundendness: For $\epsilon_X = f(u)$, $\epsilon_M = f(u, v)$ and $\epsilon_Y = f(v)$ we need $\epsilon_X \perp\!\!\!\perp \epsilon_Y$ (or equivalently $u \perp\!\!\!\perp v$)

Notes: * < 0.10, ** < 0.05, *** < 0.01. Heteroskedasticity robust standard errors are reported in round brackets. Industry-structure clustered standard errors from Adao, Kolesár, and Morales (2019) are reported in square brackets. The standard errors for the indirect effect are calculated as the standard error of the estimate difference $\hat{\gamma}^{IV} - \hat{\gamma}_M^X$ (i.e. from the difference between the estimated total and direct effects). All estimations are weighted by the start-of-period native voting-age population, and include all controls and fixed effects from Tables 1 and 2, with the exception of the start-of-period unemployment rate (which is itself part of the mediator variable Δ Unemployment Rate).

Applying the mediation analysis to decompose the total effect into the direct and indirect effects in Panel A of Table 3 shows that the effect of changes in manufacturing employment on far-right voting is almost completely mediated through changes in natives' unemployment rates. When using percentage changes in far-right vote shares in column 1, changes in natives' unemployment explain around 90% of the total effect. Using percentage-point changes in far-right voting in column 2 even suggests that the entire effect is mediated through natives' unemployment. This absence of a direct effect of manufacturing employment changes on changes in far-right voting is consistent with the estimated effect of the unemployment rate on far-right voting (i.e. the estimated parameter $\hat{\gamma}_X^M$ in panel B of Table 3). This estimate, while being large and positive in both cases, is very noisily estimated in column 1 and gains more precision when using percentage-point changes as dependent variable in column 2. This estimate for $\hat{\gamma}_X^M$ in column 2, which implies that a one percentage-point increase in natives' unemployment rates leads to a roughly seven percentage-points increase in far-right vote shares, is also very similar (albeit slightly larger) in magnitude to comparable estimations in Algan et al. (2017), who show that a one percentage point increase in unemployment rates has led to an increase of around 4.4 percentage points in vote shares of anti-establishment parties in the European Union during the aftermath of the Great Recession.

In sum the results of the mediation analysis in Table 3 indicate that increases in natives' unemployment are the main driver behind the electoral effects of the manufacturing decline. As manufacturing employment declines, natives' employment prospects worsen, which in turn increases electoral support for far-right populist parties.

5 The Role of Trade & Technology

It is a well established result in the literature on local labor demand shocks that employment in the manufacturing industries primarily declined because of increases in trade exposure from China and the former Eastern Block (see Autor, Dorn, and Hanson, 2013 or Dauth, Findeisen, and Suedekum, 2014) as well as advancements in automation technologies (see Autor and Dorn, 2013; Acemoglu and Restrepo, 2020 or Dauth et al., 2021). In this Section I assess the relative importance of those two forces for the overall effect of the manufacturing decline on far-right voting. For this I estimate equations of the following form:

$$\%\Delta Voteshare_{rt} = \gamma \Delta Shock_{rt} + X_{rt}\beta + \rho_r + \tau_t + \epsilon_{rt} \tag{5}$$

where $\Delta Shock_{rt}$ corresponds to a regional measure of either trade- or robot-exposure:

$$\Delta Shock_{rt} \in \left\{ \Delta \textit{Net-Imports}_{rt}, \Delta Robots_{rt} \right\} \tag{6}$$

Since detailed data on regional exposure to international trade and robotization are not available for the Austrian case, I follow the available literature and predict regional trade-/robot-exposure as shift-share-variables. These shift-share-variables use industry level information on trade- and robotization changes to predict regional shock exposure.

$$\Delta \textit{Net-Imports}_{rt} = \sum_i \frac{Emp_{irt}}{Emp_{rt}} \times \frac{\Delta \textit{Net-Imports}_{it}}{Emp_{it}} \tag{7}$$

$$\Delta Robots_{rt} = \sum_i \frac{Emp_{irt}}{Emp_{rt}} \times \frac{\Delta Robots_{it}}{Emp_{it}} \tag{8}$$

The intuition behind equations 7 and 8, which broadly follows the intuition of the Bartik instrument in Section 4, is that any commuting zone r is exposed to industry wide trends in trade and technology depending on their regional industry structure (measured by regional industry employment shares). Hence equations 7 and 8 use regional industry-employment shares to project industry level changes in trade-exposure and robot-adoption onto the regional level.

Predicting regional exposure to trade shocks as outlined in equation 7 was pioneered in the seminal contribution of Autor, Dorn, and Hanson (2013), who have shown that the rise of Chinese import competition has had major adverse effects on manufacturing employment in US local labor markets, and has contributed strongly to the increased political polarization in the US (Autor et al., 2020). While the results for the US find exclusively negative labor market effects of trade exposure, a related study by Dauth, Findeisen, and Suedekum (2014) finds more mixed results for Germany, where the negative effects of rising import exposure were fully offset by similar sized positive effects of increased export possibilities. This study also showed that for the German case, trade exposure from the former Eastern Block has had more pronounced effects as opposed to Chinese trade exposure. Since Austria is much more similar to the German than to the US economy, expression 7 thus follows the approach of Dauth, Findeisen, and Suedekum (2014) and uses changes in net-import exposure from both China and the former Eastern Block. While computing the trade shock as net-import exposure already takes into account possible increases in exports to China and the East, I also regard import- and export-exposure separately in additional estimations. Trade exposure is measured as the trade value in thousand-Euros per worker (in 2017 values).

The predicted robotization measure in equation 8, which follows the exact same intuition as the trade measure in equation 7, was first introduced by Acemoglu and Restrepo (2020) who found that robotization has had strong negative effects on manufacturing employment in US local labor markets. Using the same methodology Dauth et al. (2021) found for the German case that, while robotization also has had negative effects on manufacturing employment, these adverse effects were fully offset by job growth in the service sector. As in Acemoglu and Restrepo (2020) and Dauth et al. (2021) the industry level change in robotization in equation 8 is measured as the change in the number of installed robots per 1000 workers. Data on these industry level robot stocks comes from the International Federation of Robotics (IFR).

Because the robotization data is only available for Austrian industries from 2003 onward, equation 5 is estimated separately for the trade- and robotization-shock. This way the trade estimation can be performed on the same time window as the employment regressions in Table 1 (1995-2017), while the robotization regression is restricted to the period 2002-2017.[18] Consequently the trade-regressions can only control for the robotization shock when the time window is restricted to the period 2002-2017, while the robotization regressions can always control for the net-import shock. Additionally changes in Information- and Communication Technology (ICT) capital is included as control variable in the trade- and robotization regressions, to account for other types of technology shocks.

As before, estimating equation 7 for the trade- and robotization-shocks via OLS poses some potential endogeneity concerns. These concerns primarily stem from the possible presence of unobserved demand shocks, which might simultaneously influence trade-activity or robotization decisions of firms and voting outcomes. While such shocks are to some extent controlled for by additionally including changes in ICT adoption into the set of controls, or by controlling for the inflow of migrants during that period, a more rigorous strategy is required to plausibly obtain causal estimates. For this I follow the approaches layed out in Autor, Dorn, and Hanson (2013) and Acemoglu and Restrepo (2020) and instrument the shock measures with corresponding shift-share instruments:

$$\Delta \textit{Net-Imports}_{rt}^{IV} = \sum_i \frac{Emp_{irt-15}}{Emp_{rt-15}} \times \frac{\Delta \textit{Net-Imports}_{it}^{OtherCountries}}{Emp_{it-15}} \tag{9}$$

$$\Delta Robots_{rt}^{IV} = \sum_i \frac{Emp_{irt-15}}{Emp_{rt-15}} \times \frac{\Delta Robots_{it}^{OtherCountries}}{Emp_{it-15}} \tag{10}$$

Like the Bartik instrument from equation 4 these instrumental variables use variation in trade- or robot-exposure from other high income countries and project them to region r via the 15-year lagged industry structure. As is discussed in detail in Borusyak, Hull, and Jaravel (2022), the validity of the instrumental variables in expressions 9 and 10 hinges on the exogeneity of the shocks $\Delta \textit{Net-Imports}^{OtherCountries}$ and $\Delta Robots^{OtherCountries}$ used to construct the instruments. Hence the

[18] Since the 2002 data points for Austria are not available in the IFR data, datapoints for 2003 are used instead.

identifying assumption that is required to be fulfilled in order for these instruments to be able to isolate causal effects, requires that trade- and robotization-trends in the countries used to construct the instruments are exogenous to unobserved regional demand shocks in Austria.[19] The plausibility of this exogenous shocks assumption is examined alongside the Bartik instrument in Section 7.

Since the changes in the exposure to net-imports and industrial robotization are largely confined to the manufacturing industries (with some minor exceptions), the exposure shares used to construct the shift-share measures (in equations 7 and 8) and the instrumental variables (in equations 9 and 10) generally do not sum to one, such that $\sum_i \frac{Emp_{ir}}{Emp_r} < 1$. As is emphasized in Borusyak, Hull, and Jaravel (2022), conventional period fixed effects fail to fully absorb between period variation in shift-share settings with incomplete exposure shares. To achieve this they recommend to interact the period fixed effects with the sum of incomplete exposure shares. Hence the period fixed effects τ_t in equation 5 refer to the interaction of conventional period dummies with the sum of incomplete exposure shares. For OLS estimations the shares used in the construction of the shift-share measures are used, while in 2SLS estimations the shares used to construct the instrumental variables are used.

[19]To avoid that the instruments pick up on common macroeconomic shocks in Austria and the countries used to construct the instruments, the shocks used in the computation of equations 9 and 10 are calculated strictly from countries outside of the European Monetary Union. Since the Comtrade-database covers all developed countries Δ*Net-Imports*^{OtherCountries} is constructed from the same country group as in Dauth, Findeisen, and Suedekum (2014) (Australia, Canada, Japan, New Zealand, Norway, Singapore, Sweden and the United Kingdom). Because the IFR-data covers much less countries, the country selction for the robotization shock is somewhat limited by data availability. Hence the robotization-shock $\Delta Robots^{OtherCountries}$ is constructed from changes in robotization in all countries outside the European Monetary Union with sufficient data (Canada, Denmark, Mexico, Norway, the Republic of Korea, Sweden, the United Kingdom and the United States). Japan is excluded from the computation because it underwent major re-classifications in the IFR-data (see Acemoglu and Restrepo, 2020, footnote 8). The industry level trade- and robotization shocks were computed at the most detailed level available. This corresponds to the 4-digit level for the trade shocks and (roughly) the 2-digit level for the robotization shocks, according to the NACE Rev. 2 classification.

5.1 Results

Before turning to the results for the estimated effects of trade and robotization on far-right voting, Figure A4 in the Appendix presents the evolution of trade and robot-exposure in Austria. Overall both factors have risen drastically in importance since 1995. In the case of trade exposure, panel A of Figure A4 shows that the volume of both imports from and exports to China and the former Eastern Block have roughly tripled (in per worker terms) since 1995. While the export volume was higher than the import volume for most of the observational period, imports have risen much stronger since the late 2000s and have even overtaken exports in more recent years, leading to a sharp increase in net-import exposure since 2008 (panel B). While the Austrian economy thus experienced strong net benefits from trade with China and the East (especially from 1995 to 2008), it has lost some ground during and after the Great Recession and even shows a negative trade balance with these countries in more recent years.

Looking at the evolution of robot density in panel C of Figure A4 shows that the number of industrial robots (per 1000 workers) has also drastically increased since 1995.[20] Here the number of industrial robots installed at the end of the observational period is almost five times as large as opposed to the year 1995. In contrast to the development of trade exposure in panels A and B, this trend was rather unaffected by the Great Recession, as it continues rather smoothly throughout the entire period 1995-2017.

To examine the labor market impact of the drastic increase in trade- and robot-exposure, Table A4 in the Appendix presents estimations for the corresponding employment effects (separately for all workers and native workers only). Here both increases in exposure to net-imports (Table A4, panel A) and robotization (panel C) lead to declines in manufacturing employment. These negative effects are entirely concentrated on the manufacturing sector, as corresponding estimations for non-manufacturing employment result in small and insignificant estimates. Also considering only

[20] While the IFR data includes a breakdown by industry for Austria only from 2003 onwards, country level information is available starting in 1993.

native workers in columns 5 to 8 of Table A4 has only a negligible impact on the size of the estimated employment effects. Consistent with international evidence, these estimates thus show that both trade and industrial robotization have contributed to declines in manufacturing employment. Since Table 1 has shown that declines in manufacturing employment lead to increases in the electoral success of far-right parties, it thus stands to reason that both trade and technology have contributed to the rise of the far-right to some extent.

To further investigate this question, Table 4 presents the results for the trade effect on far-right vote-shares. Here columns (1) to (6) present estimations for the period 1995-2017, while columns (7) and (8) present results for the restricted period 2002-2017.

The results in Table 4 show positive estimates in both the OLS (panel A) and the 2SLS estimates (panel B). The 2SLS estimates in panel B are also relatively stable to the inclusion of additional control variables. While the inclusion of controls for the regional economic and political conditions in column (2) and for the demographic composition of the local electorate in column (3) lead to some (albeit rather limited) movement in the point estimate, the size of the estimated effect stabilizes thereafter. The specification including a full set of controls in column (6) indicates a precisely estimated positive effect of 9.175. This estimate implies that an increase in net-import exposure of 1000 Euros per Worker leads to an increase in far-right voting of around 9.2%.

To check whether the estimated effect of net-imports is confounded by the simultaneously occurring (and possibly correlated) robotization shock, column (7) repeats the estimation from column (6) on the shortened time window 2002-2017. Restricting the time window has almost no discernible impact on the point estimate, which remains practically unchanged. Including the change in robots per 1000 workers as additional control in column (8) however leads to a minor reduction in the magnitude of the point estimate. While the difference in point estimates between columns (7) and (8) is not statistically significant, the full estimation controlling for robotization trends indicates a somewhat smaller estimated effect of 7.366, indicating that an increase in net-import exposure of 1000 Euros per worker increases the vote-share of the far-right by about 7.4%.

Table 4: Trade exposure and far-right voting (1995-2017)

	Dependent Variable: %Δ Far-right vote share							
	1995-2017						2002-2017	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: OLS Estimations:								
Δ Net-Imports	0.402	1.744	3.79	3.864	3.3	2.974	3.266	2.799
	(2.627)	(2.629)	(1.907)**	(1.919)**	(1.938)*	(1.866)	(1.542)**	(1.55)*
	[1.79]	[1.568]	[0.798]***	[0.922]***	[0.94]***	[0.886]***	[1.17]***	[1.236]**
Panel B: 2SLS Estimations:								
Δ Net-Imports	6.922	12.031	9.727	8.961	8.794	9.175	9.444	7.366
	(6.873)	(6.277)*	(3.515)***	(3.561)**	(3.643)**	(3.431)***	(3.255)***	(3.597)**
	[3.481]**	[3.638]***	[2.014]***	[1.927]***	[1.986]***	[2.106]***	[1.432]***	[1.235]***
Panel C: First Stage Estimations:								
Δ Net-Imports^{IV}:	0.015	0.014	0.014	0.014	0.014	0.014	0.014	0.014
	(0.004)***	(0.004)***	(0.004)***	(0.004)***	(0.004)***	(0.004)***	(0.003)***	(0.002)***
	[0.002]***	[0.002]***	[0.002]***	[0.002]***	[0.002]***	[0.002]***	[0.002]***	[0.002]***
First-Stage F-Statistic:	13.16	11.19	11.95	14.95	15.29	15.36	27.13	29.99
Period Fixed Effects	x	x	x	x	x	x	x	x
Commuting Zone Fixed Effects	x	x	x	x	x	x	x	x
Industry Structure	x	x	x	x	x	x	x	x
Regional Characteristics		x	x	x	x	x	x	x
Demographic Characteristics			x	x	x	x	x	x
Tech. Shock: Δ ICT				x	x	x	x	x
Migrant shares (by skill)					x	x	x	x
Δ Migrant shares						x	x	x
Tech. Shock: Δ Robots								x
Commuting Zones	100	100	100	100	100	100	100	100
Periods	4	4	4	4	4	4	3	3
Observations	400	400	400	400	400	400	300	300

Notes: * < 0.10, ** < 0.05, *** < 0.01. Heteroskedasticity robust standard errors are reported in round brackets, while industry structure clustered standard errors from Adao, Kolesár, and Morales (2019) are reported in square brackets. Units of observation are 100 clustered commuting zones. All specifications include a set of region and period fixed effects, as well as start-of-period employment shares of several sub-industries of manufacturing (production of food products, consumer goods, industrial goods and capital goods), as well as industries outside of manufacturing (utilities, construction, personal services and business services). Since the exposure shares used to construct the trade-exposure measure and the instrument are incomplete (i.e. do not sum to one), the period fixed effects are interacted with the sum of incomplete exposure shares (as is recommended in Borusyak, Hull, and Jaravel, 2022). Regional characteristics control for the start-of-period unemployment rate (decomposed by the respective contribution of natives and immigrants), the logarithm of the gross regional product, as well as start-of-period party affiliation of the local governor and vice governor. Demographic controls include the start-of-period structure of the native voting-age population, as well as the start-of-period degree of urbanization. Migrant shares (in start-of-period levels and percentage-changes) are included separately for three skill groups (high-, medium- and low-skilled migrants). Additionally two types of technology shock controls are included to capture changes in ICT- and robot-exposure. All estimations are weighted by the start-of-period native voting-age population.

Table 5: Robotization and far-right voting (2002-2017)

	Dependent Variable: %Δ Far-right vote share						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: OLS Estimations:							
Δ Robots	0.669	-0.293	2.457	3.058	2.53	3.078	3.071
	(2.902)	(2.242)	(1.698)	(1.711)*	(1.753)	(1.653)*	(1.484)**
	[39.212]	[7.043]	[3.279]	[1.205]**	[1.144]**	[1.067]***	[1.057]***
Panel B: 2SLS Estimations:							
Δ Robots	10.51	7.739	11.653	10.169	8.841	8.814	6.421
	(6.246)*	(4.063)*	(3.824)***	(3.56)***	(3.441)**	(3.27)***	(3.09)**
	[44.781]	[5.026]	[5.574]**	[2.218]***	[2.307]***	[2.219]***	[2.033]***
Panel C: First-Stage Estimations:							
Δ $\text{Robots}^{\text{Other Countries}}$	0.008	0.008	0.005	0.005	0.006	0.006	0.006
	(0.002)***	(0.002)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***
	[0.001]***	[0.002]***	[0.001]***	[0.001]***	[0.001]***	[0.001]***	[0.001]***
First-Stage F-Statistic:	14.05	23.52	20.03	23.33	27.06	27.45	27.94
Period Fixed Effects	x	x	x	x	x	x	x
Commuting Zone Fixed Effects	x	x	x	x	x	x	x
Industry Structure	x	x	x	x	x	x	x
Regional Characteristics		x	x	x	x	x	x
Demographic Characteristics			x	x	x	x	x
Tech. Shock: Δ ICT				x	x	x	x
Trade Shock: Δ Net-Imports					x	x	x
Migrant share (by skill)						x	x
Δ Migrant shares							x
Commuting Zones	100	100	100	100	100	100	100
Periods	3	3	3	3	3	3	3
Observations	300	300	300	300	300	300	300

Notes: * < 0.10, ** < 0.05, *** < 0.01. Heteroskedasticity robust standard errors are reported in round brackets, while industry structure clustered standard errors from Adao, Kolesár, and Morales (2019) are reported in square brackets. Units of observation are 100 clustered commuting zones. All specifications include a set of region and period fixed effects, as well as start-of-period employment shares of several sub-industries of manufacturing (production of food products, consumer goods, industrial goods and capital goods), as well as industries outside of manufacturing (utilities, construction, personal services and business services). Since the exposure shares used to construct the robot-exposure measure and the instrument are incomplete (i.e. do not sum to one), the period fixed effects are interacted with the sum of incomplete exposure shares (as is recommended in Borusyak, Hull, and Jaravel, 2022). Regional characteristics control for the start-of-period unemployment rate (decomposed by the respective contribution of natives and immigrants), the logarithm of the gross regional product, as well as start-of-period party affiliation of the local governor and vice governor. Demographic controls include the start-of-period structure of the native voting-age population, as well as the start-of-period degree of urbanization. Migrant shares (in start-of-period levels and percentage-changes) are included separately for three skill groups (high-, medium- and low-skilled migrants). Additionally two types of labor market shock controls are included to capture changes in ICT- and trade-exposure. All estimations are weighted by the start-of-period native voting-age population.

While Table 4 measures trade exposure as net-imports (i.e. imports minus exports), Tables A4 and A5 in the Appendix also regard the separate effects of import- and export-exposure on manufacturing employment and far-right vote-shares. As is shown in Dauth, Findeisen, and Suedekum (2014), increases in exports have the potential to offset negative effects of rising import exposure. This is also confirmed by the estimated employment effects in panel B of Table A4, which show that rising import- and export-exposure have roughly equal sized (but opposite) effects on manufacturing employment. Job growth caused by increased exports thus has the potential to fully offset job loss which is caused by increased import exposure. This symmetry in the estimated employment effects of import and export exposure however does not carry over to the effects on voting outcomes. As is shown in Table A5, the estimated increase in far-right voting caused by rising imports from China and the East is drastically larger than the offsetting effect of rising exports. In the full specification for the period 1995-2017 in column (1) of Table A5 the effect of imports is around 50% larger than the offsetting effect of exports. Again the pattern of the point estimates stays unchanged when restricting the sample period to 2002-2017 in column (2). Additionally controlling for the change in robot adoption in column (3) however leaves the increasing effect of imports around three times as large as the offsetting effect of exports. While this difference in point estimates in column 3 of Table A5 is very noisy and statistically insignificant in the conventional heteroskedasticity robust standard errors, it is much more precisely estimated in the AKM-standard errors. Here the difference between the two effects is statistically significant at the 5%-level (t=2.01).

In sum this asymmetry in the electoral effects of import- and export-exposure indicates that, even if the employment gains of increased export possibilities fully offset employment losses due to increased import penetration, the electoral effects may be very different, with the far-right benefiting from increased trade exposure even if the net-employment effects are zero.

To assess the impact of industrial robotization on the vote share of the far-right, Table 5 presents estimates for the semi-elasticity of the far-right vote-share with respect to increases in robots per 1000 workers. As with the trade regressions, the 2SLS estimates indicate a relatively stable positive effect of industrial robots on far-right voting, which is consistent with the negative effect on manufacturing employment presented in Table A4 in the Appendix. Throughout all estimations in Table 5 the estimated effect of industrial robotization of far-right voting is remarkably stable, and only drops slightly once the (simultaneously occurring) change in migrant shares is included in the set of controls in column (7). Here the full specification indicates that one additional robot per 1000 workers increases far-right voting by around 6.4%.

5.2 Mechanism

Tables 4 and 5 show that increases in trade- and robot- exposure lead to increases in far-right voting. Since both factors also have a negative impact on manufacturing employment (Table A4), it is interesting whether the effects on far-right voting in Tables 4 and 5 entirely operate through the adverse effect on manufacturing employment and thus represent a reduced form relationship, or whether part of the estimated effects operate independent of the employment channel.

To investigate this question Table A6 in the Appendix presents the results of a parametric mediation analysis, where Trade/Robotization serve as treatment X, the change in manufacturing employment serves as mediator M and outcome Y is the change in far-right vote shares.

Since the Bartik instrument is available as additional instrumental variable for the mediator M, this mediation model could in principle be identified using two instrumental variables (as in Frölich and Huber, 2017). In practice this is however complicated by the fact that the estimations in equations 1 and 5 use different sets of control variables. In particular the structure of the period fixed effects is very different in these two equations. Since the exposure shares used to construct the Bartik instrument sum to one (such that $\frac{Emp_{ir}}{Emp_r} = 1$) conventional period fixed effects are sufficient in the estimation of equation 1 to properly isolate within period variation. In contrast to this the shift-share instruments used in the estimation of equation 5 are constructed from incomplete exposure shares (such that $\frac{Emp_{ir}}{Emp_r} < 1$). As is discussed in Borusyak, Hull, and Jaravel (2022), conventional period fixed effects do not properly isolate within period variation in shift-share settings with incomplete exposure shares. To achieve this, the period fixed effects in equation 5 are interacted with the sum of incomplete exposure shares.

Because this difference in the period fixed effects makes a parametric identification of the mediation model very challenging in a multiple instrument setting, I again rely on the identification of the mediation model in the single instrument setting from Dippel et al. (2020, 2022).The structure of this mediation model is depicted graphically in Figure A3 in the Appendix. As before in Section 4.6 identification in this setting requires that the *partial confoundedness* assumption holds, i.e. that the unobservables that cause endogeneity in the causal path $X \rightarrow M$ (the effect of trade/robots on manufacturing employment) are orthogonal to the unobservables causing endogeneity in the causal path $M \rightarrow Y$ (the effect of changes in manufacturing employment on far-right voting). Provided this assumption holds the shift-share instrument for trade (robots) from equation 9 (equation 10) can be used to identify the causal pathway $M \rightarrow Y$.[21] Since the parametric mediation model in single instrument settings from Dippel et al. (2020, 2022) has been proposed in a very similar setting it appears as an appropriate model to disentangle the direct and indirect electoral effects of trade- and robot-exposure. In particular this model has been proposed to estimate the direct and indirect effects of a trade-shock on far-right populist voting in Germany, whereby the indirect effect is mediated through labor market adjustments.[22]

While the partial confoundedness assumption, which is crucial for proper identification of the mediation model, was not testable in the setting discussed in Section 4.6, the availability of the Bartik instrument essentially makes this assumption testable. In particular the estimated effect $\hat{\gamma}_Y^M$ in panel B of Table A6 should correspond to the estimated effect in column 6 of Table 1 if the causal pathway $M \rightarrow Y$ is properly identified by the mediation model. This $\hat{\gamma}_Y^M$ corresponds to the elasticity of changes in far-right vote-shares with respect to changes in manufacturing employment as estimated by the mediation model, and thus its interpretation directly corresponds to the estimated

[21] As is discussed in Section 4.6, the two other causal pathways of interest $X \rightarrow Y$ and $X \rightarrow M$ are identified in a straightforward way by using the shift-share instruments in equations 9 and 10.

[22] See also Dippel et al. (2022) for a discussion of the partial confoundedness assumption in this setting.

effects in Table 1. Since identification in the mediation model is achieved using the trade and robot instruments respectively (equations 9 and 10), while identification in Table 1 is achieved using the Bartik instrument (equation 4), $\hat{\gamma}_Y^M$ and the estimated effects in Table 1 leverage different sources of exogenous variation. Therefore a comparison of these estimates can be used to assess the plausibility of the partial confoundedness assumption in the mediation model.

Both in the cases of the mediation model for net-import exposure in column 1 of Table A6 (where $\hat{\gamma}_Y^M = -1.615$), as well as in the mediation model for robot-exposure in column 2 (where $\hat{\gamma}_Y^M = -1.147$) this estimated effect is very similar in magnitude to the estimated effect in Table 1, column 6 (with a point estimate of -1.181). More formally testing for the equality of the $\hat{\gamma}_Y^M$ coefficients vis-a-vis the estimated effect of changes in manufacturing employment on far-right vote-shares in Table 1, indicates that the $\hat{\gamma}_Y^M$ estimates are statistically indistinguishable from the estimated effect in Table 1 in both standard error definitions. This further suggests that the partial confoundedness assumption holds in this setting, and the parametric mediation model in single instrument settings from Dippel et al. (2020, 2022) can be used to disentangle the direct and indirect (i.e. mediated through employment changes) effects of the trade- and robotization-shocks on the change in far-right vote-shares.

Panel A of Table A6 in the Appendix presents the results of this mediation analysis. Both in the case of the trade-shock (column 1) and the robotization-shock (column 2) the majority of the estimated total effect is mediated through changes in manufacturing employment. Overall the adjustment of manufacturing employment explains around 77% of the trade effect, and around 58% of the robotization effect on far-right voting. While the adverse impact on manufacturing employment thus is the major channel that mediates the electoral effects of trade- and robotization-shocks, it does not explain the entirety of each effect. There thus appear to be other channels through which these two specific labor demand shocks may affect far-right voting. While the precise nature of these other channels lies beyond the scope of this paper, possible candidates relate to possible negative employment effects in certain sub-industries of the non-manufacturing sector (as the corresponding estimates in Table A4 for the entirety of non-manufacturing employment are negative, but very imprecisely estimated), or adverse wage effects of the trade- and robotization shocks (as discussed in Dippel et al., 2022). Another possible explanation could be related to trade and robotization shocks causing regional decline along other dimensions than just the employment impact. For example, in Bekhtiar (2022) I show that the robotization shock has contributed to pronounced population declines in many rural areas in Austria which could have (i) compositional effects on election outcomes and (ii) influence far-right voting through prolonged regional decline.

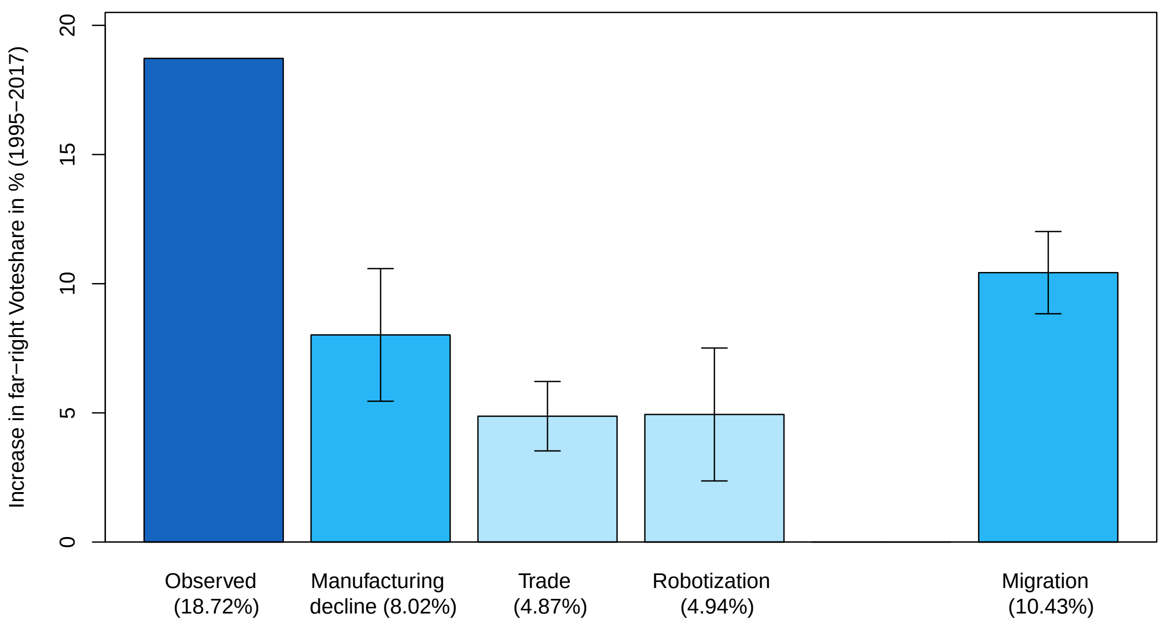
6 Benchmarking Effect Sizes

While the results discussed so far show that the decline in manufacturing employment and its underlying forces (trade and robotization) have led to increases in far-right voting, the relative magnitudes of the estimated effects remain somewhat elusive. This follows from the different definitions of the explanatory variables in Tables 1 to 5 which produce estimates that are not directly comparable. To get a concrete picture of the relative importance of the effect of the manufacturing decline for the rise of the far-right, as well as the relative importance of trade and robotization, this section quantifies the magnitude of these effects against (i) the observed increase in far-right voting and (ii) the contribution of immigration.

For this purpose Figure 2 presents the results of a benchmarking exercise, which compares the contributions of the decline in manufacturing employment and the increases in trade and robotization to the overall increase in far-right voting during 1995-2017. For this the estimated effects from Tables 1 to 5 are multiplied with the observed changes in manufacturing employment, net-import exposure and robot density.[23]

[23]For the effect of manufacturing employment the 2SLS- estimate from Table 1 (Panel B, column 6) is multiplied with the observed ‰-change in the manufacturing share from Figure 1. For net-imports, the 2SLS-estimate in column 8 of Table 4 is multiplied with the observed Euro-per-worker change in net-import exposure during 1995-2017, while for robotization the 2SLS-estimate in column 7 is multiplied with the observed change in robots-per-1000 workers at the country level (country level robotization data is available for Austria starting in 1993, while industry level data starts

Figure 2: Benchmarking Effect-Size (1995-2017):



Notes: The contribution of the decline in manufacturing employment is calculated using the estimated effect of manufacturing employment on the far-right vote-share from Table 1 (panel B, column 6) and multiplying it with the observed percentage-change in manufacturing employment. Similarly, the contributions of trade-exposure (robot-exposure) is calculated by multiplying the estimated coefficients from Table 4, column 8 (table 5, column 7) and multiplying it with the observed change in net-imports per worker (robots per 1000 workers). Since the robotization effect can only be estimated on the timeframe 2002-2017, it is assumed that the same effect size also applies to the period 1995-2002. The contribution of migration to the increase in the far-right vote-share is calculated using the estimated effect of the migrant-share on far-right vote-shares for Austrian municipalities from Halla, Wagner, and Zweimüller (2017) (Table 8, column 2) and multiplying it with observed increases in the migrant share from the Austrian census data (1991-2011) and the Austrian Labor Market Statistics (2012 onwards).

Additionally Figure 2 presents a benchmarking for the effect of immigration on the far-right vote share. The contribution of immigration is calculated using the estimated elasticity of the far-right vote-share with respect to the immigrant-share in Austria from Halla, Wagner, and Zweimüller (2017).[24] Since the estimated effect of changes in manufacturing employment in Table 1 controls for changes in the migrant-share, while the estimate from Halla, Wagner, and Zweimüller (2017) controls for regional employment, these two estimates reflect ceteris-paribus effects and should thus not be confounded by simultaneous changes in the other factor.

The benchmarking in Figure 2 shows that the vote-share of the far-right has increased by 18.72% between 1995 and 2017. Roughly 8 percentage points of this increase are explained by the decline in manufacturing employment, while the simultaneous rise in immigration explains roughly 10.4 percentage points. The contribution of the increase in immigration is thus only slightly larger as the contribution of the manufacturing decline. Together the manufacturing decline and the increase in immigration appear to fully explain the observed increase in far-right voting.

While Figure 2 shows that increased immigration is the most important driver of increases in far-right voting, the decline in manufacturing has made a substantial contribution to this development. Regarding the contributions of increases in trade- and robot-exposure, Figure 2 shows that both factors have made comparable contributions. The sum of their respective contributions (9.78 percentage points) also slightly exceeds the contribution of the manufacturing decline (8.02 percentage points). While this again points towards the potential importance of other channels un-

in 2003). Since the estimate for manufacturing employment corresponds to the elasticity of the far-right vote share with respect to changes in manufacturing employment, while the estimates for trade and robotization correspond to semi-elasticities, this benchmarking procedure results in the contribution of each factor in percentages. The observed changes in net-import exposure and robot-density are depicted in Figure A4 in the Appendix.

[24]The estimate for this elasticity is 0.097 (see Table 8, column 2 in Halla, Wagner, and Zweimüller, 2017). As before this elasticity is multiplied with the observed change in the migrant-share in Austria between 1995 and 2017. Data on the migrant-share comes from the Austrian census (available for 1991, 2001 and 2011) and the register based Austrian labor market statistics (available from 2012 onward). Migrant-shares for the missing years between census-years are linearly interpolated.

related to changes in manufacturing employment, the practical importance of these other channels appears rather limited, as this deviation is well within the estimated uncertainty of the effect of manufacturing employment.

7 Robustness Checks

This Section presents plausibility checks for the used instrumental variables as well as other robustness tests. For the plausibility checks I follow the recommendations in Borusyak, Hull, and Jaravel (2022) to assess the validity of the exogenous shocks assumption underlying the used instruments (see Section 4).

7.1 Plausibility of exogenous shocks assumption

As is outlined in detail in section 4.2, the shift-share instrumental variables used in this paper crucially rely on the exogeneity of the industry level shocks (i.e. the industry level changes in either employment, net-import penetration or robot exposure from other high income countries). While this exogenous shocks condition essentially mirrors a standard exclusion restriction, and is thus not directly testable, recent work of Borusyak, Hull, and Jaravel (2022) proposes a host of checks to assess the plausibility of shock exogeneity.

Pre-Trend tests

The first test to assess the plausibility of shock exogeneity, is to check whether the results presented in Tables 1 to 5 are driven by pre-existing trends. For this test pre-period changes in the vote-share of far-right parties are regressed on the instruments that are used during the analysis. The pre-period changes in far-right voting are measured over the period 1986-1995, during which a large fraction of the rise of the Austrian far-right took place (see Figure 2). Table 6 presents the results of separate pre-trend tests for each of the used instrumental variables.

To follow the recommendations in Borusyak, Hull, and Jaravel (2022) column 1 of Table 6 presents

Table 6: Pre-trend tests for instrumental variables:

	1986 – 1995		N
	(1)	(2)	(3)
$Bartik^{IV}$	0.985	0.058	
	(0.09)***	(0.093)	
	[4.069]	[0.098]	400
Δ*Net-Imports*^{IV}	-0.014	-0.078	
	(0.21)	(0.057)	
	[1.289]	[0.191]	400
$\Delta Robots^{IV}$	-0.089	0.019	
	(0.034)***	(0.025)	
	[2.114]	[0.049]	300
Period Fixed Effects	x	x	
Industry Structure		x	
Regional Characteristics		x	
Demographic Characteristics		x	
Shift-Share Controls		x	
Migrant share (by skill groups)		x	
Δ Migrant shares		x	

Notes: * < 0.10, ** < 0.05, *** < 0.01. Heteroskedasticity robust standard errors are reported in round brackets, while industry structure clustered standard errors from Adao, Kolesár, and Morales (2019) are reported in square brackets. Units of observation are 100 clustered commuting zones. All specifications include a full set of controls corresponding to the controls used in the respective estimations in Tables 1, 4 and 5. Since the pre-period changes in far-right voting do not vary between panel periods, fixed effects for the commuting zone cannot be included. All estimations are weighted by the start-of-period native voting-age population.

pre-trend tests that only control for period fixed effects to isolate within-period variation in the instruments. These pre-trend tests indicate the presence of significant pre-trends in the Bartik instrument and the robotization instrument. While those pre-trends are statistically significant, the estimates point into the wrong direction, as they indicate a positive pre-trend in the Bartik instrument, and a negative pre-trend in the robotization instrument. Since the estimated effects in Tables 1 and 5 point into the opposite direction, they are unlikely to be caused by the significant pre-trends in column 1 of Table 6.

To assess whether these unconditional pre-trends persist when conditioning on all available controls, column 2 of Table 6 repeats the pre-trend test, including all available control variables in the estimation.[25] Doing this leads to sharp drop in the size of the point estimates for the Bartik instrument as well as the robotization instrument and renders the coefficients statistically insignificant in

[25] Since the pre-period changes in far-right voting do not vary between panel periods, fixed effects for the commuting zone cannot be included.

both standard error definitions.

In sum Table 6 shows that, at least conditional on observed control variables, the used instrumental variables do not pick up on pre-existing trends in far-right voting. The result presented in Tables 1 to 5 are thus not driven by pre-existing trends, and the estimated coefficients reflect contemporaneous treatment effects of the change in manufacturing employment, trade- and robots.

Industry balance tests

The next test to assess the plausibility of shock exogeneity checks whether the industry level shocks from other high income countries used to construct the instruments are balanced with respect to observed start-of-period characteristics in Austrian industries. For this the industry level shocks are regressed on a set of industry level balance variables. These balance variables measure start-of-period industry characteristics related to the age-distribution of the workforce, the share of blue collar workers, the share of migrant workers (computed from the ASSD data) as well as indicators for the capital-to-labor ratio, labor productivity, the ICT capital stock and the logarithm of the average hourly wage rate (computed from EU-KLEMS data). Since the balance variables are fixed at the start of each panel period they are pre-determined with respect to the ensuing shocks during the period. The result of these industry level balance tests is presented in Table 7. As is recommended in Borusyak, Hull, and Jaravel (2022), these industry level balance tests only control for period fixed effects, to isolate the within period variation in the shocks. Since the industry level employment changes used for the construction of the Bartik instrument have a break in their industry classification after 2008, the balance test for the shocks underlying the Bartik instrument are conducted separately for each of the two NACE revisions.[26] Since the balance regressions in Table 7 are conducted on the industry level, only heteroskedasticity robust standard errors are reported (as shift-share clustered AKM-standard errors cannot be estimated in industry level regressions).

[26]Notice that, while the SBS switched from NACE Rev. 1.1 to NACE Rev. 2 in 2008, the information for the year 2008 is thankfully available in both classification schemes.

Overall the employment, trade and robotization shocks used to construct the instrumental variables appear to be reasonably balanced, as almost all of the tested balance variables are statistically insignificant. The sole exception to this is the start-of-period ratio of old to middle aged workers used to measure the age structure of each industries workforce. This balance variable shows a significantly negative relationship with all three of the tested shocks. This highlights the importance of thoroughly controlling for the demographic characteristics of the local population, as the shocks used to construct the instruments appear to not be fully orthogonal to the industry level age structure.

Importantly the shocks used to construct the instruments are uncorrelated with the start-of-period share of migrant workers. Since the confounding effect of changes in immigration is of particular concern in this setting, it is reassuring that the shocks appear to be orthogonal to the relative size of migrants among the workforce.

Regional balance tests

Another way to assess the plausibility of instrument exogeneity, which is proposed both by Borusyak, Hull, and Jaravel (2022) and Goldsmith-Pinkham, Paul, and Swift (2020), is to regress the instrumental variable on a set of regional balance variables to check if they are predictive for the instrument. The intuition underlying this test is that if the instrument is not orthogonal to observable regional characteristics, it is likely that it is correlated with unobservables as well.

The results of these balance tests are presented in Table 8. In particular, the balance tests in Table 8 check for balance with respect to the structure of the local workforce by including the age and skill level, the share of immigrants, the manufacturing share, the share of blue collar workers, as well as other indicators for the economic conditions in a commuting zone like the regional unemployment rate, labor market participation rate and gross regional product. Lastly the set of balance variables also includes the vote share of the far-right. All balance variables are measured at the beginning of each panel period, and are thus pre-determined with respect to the shocks occurring during the period.

Table 7: Industry level balance tests:

	$\%\Delta Employmen^{Other}$		$\Delta Net - Imports^{Other}$	$\Delta Robots^{Other}$
	1995-2008	2008-2017	1995-2017	2002-2017
	(1)	(2)	(3)	(4)
Start-of-period ratio of old to middle aged workers	-0.012**	0.003	-0.012*	-0.033**
	(0.005)	(0.003)	(0.006)	(0.014)
Start-of-period share of blue collar workers	0.004	0	0.003	0.004
	(0.003)	(0)	(0.01)	(0.003)
Start-of-period share of migrant workers	0.008	0	-0.019	-0.019
	(0.01)	(0)	(0.018)	(0.014)
Start-of-period capital/labor ratio	-0.003	0.001	-0.016*	0.003
	(0.002)	(0.001)	(0.009)	(0.002)
Start-of-period labor productivity	0	-0.009	0.225	0.038
	(0)	(0.009)	(0.172)	(0.047)
Start-of-period ICT-capital/capital stock	0	-0.006	-0.014	-0.169
	(0.005)	(0.008)	(0.067)	(0.156)
Start-of-period log(avg. hourly real wage)	0.702	0.058	-0.586	0.063
	(0.425)	(0.063)	(0.749)	(0.038)
Classification	NACE Rev. 1.1 3-digit	NACE Rev. 2 3-digit	NACE Rev. 2 4-digit	NACE Rev. 2 2-digit
Industries	101	94	244	26
Periods	2	2	4	3
Industry-Period Shocks	202	188	976	78

Notes: * < 0.10, ** < 0.05, *** < 0.01. This Table shows industry level regressions of the respective industry level shocks used to construct the instrumental variables in equations 4, 9 and 10 on a set of industry characteristics. The industry level shocks are summed up over all countries and are then normalized to have zero-mean and unit variance. In the case of the Bartik-Instrument from equation 4 the available sample is split into two sub-periods, to be able to perform industry balance tests on both available industry classifications (NACE Rev. 1.1 and Rev. 2). This is necessary because the Structural Business Statistics data from EuroStat changes the used industry classification after 2008. The ratio of old workers to middle aged workers is constructed by dividing industry level employment of workers aged 50 or older, by employment of workers age 35 to 49. Industry level data on employment by age, worker type (blue collar) and nationality (migrant workers) is taken from the ASSD data, while all remaining industry level balance variables are taken from EU-KLEMS. Here the November 2009 Release (March 2011 Update) is used in column (1), while the September 2017 Release (July 2018 Update) is used in columns (2) to (4). Since the EU-KLEMS data is only available at the 2-digit industry level, the EU-KLEMS values for the aggregated 2-digit industry are assigned to it's sub-industries whenever the shocks used to construct the instruments use either 3- or 4-digit classifications. All industry level shocks are computed at the most detailed level available. All regressions control for period fixed effects and are weighted by industry size. Heteroskedasticity robust standard errors are reported in brackets.

Table 8: Regional Balance Tests:

	$Bartik^{IV}$	Δ*Net-Imports*^{IV}	$\Delta Robots^{IV}$
	(1)	(2)	(3)
Start-of-Period ratio of old to middle aged workers	-0.289	-0.101	-0.039
	(0.208)	(0.122)	(0.647)
	[0.347]	[0.188]	[9.734]
Start-of-Period % of medium education population	-3.475	0.47	-1.155
	(2.503)	(0.716)	(4.286)
	[1.34]***	[0.784]	[31.775]
Start-of-Period % of low education population	-1.259	0.51	-7.79
	(1.849)	(0.38)	(4.053)*
	[1.178]	[0.744]	[31.641]
Start-of-Period % of foreign born population	0.877	0.305	-1.234
	(0.661)	(0.49)	(2.513)
	[0.992]	[0.49]	[9.83]
Start-of-Period manufacturing share	0.092	0.376	3.148
	(0.158)	(0.287)	(1.205)***
	[0.22]	[0.199]*	[19.195]
Start-of-Period share of blue collar workers	0.113	-0.281	-1.245
	(0.219)	(0.181)	(0.912)
	[0.206]	[0.239]	[11.821]
Start-of-Period unemployment rate	4.736	0.955	12.815
	(2.548)*	(0.717)	(5.179)**
	[2.203]**	[1.123]	[27.073]
Start-of-Period labor market patricipation rate	-2.844	-0.387	1.771
	(2.109)	(0.745)	(5.719)
	[1.393]**	[1.207]	[33.327]
Start-of-Period log(Gross Regional Product)	-0.01	-0.012	0.218
	(0.039)	(0.022)	(0.132)
	[0.04]	[0.027]	[0.397]
Start of period vote share farright	-0.293	0.044	3.732
	(0.321)	(0.28)	(2.327)
	[0.429]	[0.292]	[4.073]
Commuting Zones	100	100	100
Periods	4	4	3
Observations	400	400	300

Notes: * < 0.10, ** < 0.05, *** < 0.01. Heteroskedasticity robust standard errors are reported in round brackets, while industry structure clustered standard errors from Adao, Kolesár, and Morales (2019) are reported in square brackets. Units of observation are 100 clustered commuting zones. All specifications include a full set period effects. In the case of the instruments for net-import exposure and robot exposure these period fixed effects are interacted with the incomplete sum of exposure shares used to construct the instruments. All estimations are weighted by the start-of-period native voting-age population.

Column 1 shows the results of the balance test for the Bartik instrument. Overall the instrument appears to be reasonably balanced. The only clear imbalance is found for the start-of-period unemployment rate, which is significantly correlated with the Bartik Instrument in both standard error definitions. Also there appear to be some additional imbalances with respect to the share of medium skilled individuals and the labor market participation rate. These imbalances are however not robustly significant across both standard error definitions.

Regarding the balance tests for the net-imports instrument (column 2) and the robotization instrument (column 3), the tests again suggest that the instruments are rather well balanced. Both of these instruments show a slight (and not robust) imbalance with respect to the start-of-period manufacturing share. This possible imbalance is hardly surprising, as both trade and technology shocks are primarily confined to the manufacturing sector, which is also the motivation behind controlling for the regional industry structures during the main part of the analysis. Lastly the robotization instrument shows slight imbalance with respect to the share of low educated individuals (which is only marginally significant at the 10% level for the conventional robust standard errors) and the unemployment rate.

Importantly all three instruments are balanced with respect to (i) the start-of-period vote-share of far-right parties and (ii) the start-of-period share of immigrants in a commuting zone. Hence all used instruments are orthogonal to the initial strength of the far-right as well as to the size of the immigrant population which is a prime driver of far-right voting. The instruments being balanced with respect to those two particular start-of-period characteristics is very reassuring in that the estimated effects are not driven by differential trends in the instruments in commuting zones with preexisting low or high support for far-right parties.

While all instruments thus appear to be reasonably balanced, the sole serious source of possible imbalance stems from the correlations with the unemployment rate and possibly also the skill distribution of the local population. This highlights the importance of thoroughly controlling for economic conditions and demographic characteristics.

7.2 Further robustness checks

Fixed exposure shares

To maximize the strength of the first stage the instrumental variables in equations 4, 9 and 10 are calculated with updated exposure shares. This means that the industry structures used to project the industry level shocks onto the regional level are lagged by 15 years for each panel period. Following the arguments laid out in Borusyak, Hull, and Jaravel (2022), updating the exposure shares is valid in this application, since the estimations in equations 1 and 5 are specified in stacked differences. An alternative approach that is often applied in the literature is to fix the exposure shares at a common base year for all panel periods. To investigate the sensitivity of the results with respect to the lag structure of the exposure shares, column 2 in Table A7 in the Appendix shows estimation results when the exposure shares are fixed at 15 years before the start of the first panel period. Overall these results confirm the results from the baseline specification with updated exposure shares presented during the main part of this paper.

Changes in voter turnout

If employment shocks in the manufacturing sector have a separate impact on voter turnout, it may be that the effects on far-right vote shares reflect declines in voter turnout (i.e. a decrease in the denominator) rather than increases in far-right voting. To investigate whether this is the case, column 3 of Table A7 presents estimation results where the percentage change in the absolute number of cast votes for the far-right is used as dependent variable (instead of the change in vote shares). Since the change in the absolute number of votes cast for the far-right is not expressed relative to voter turnout these estimations do not mechanically pick up on declines in turnout. As can be seen in column 3 of Table A7 all results are robust to this alternative specification of the dependent variable, and thus do not reflect effects on voter turnout.

Population reactions

Another possible source for concern relates to possible internal migration responses to the manufacturing decline. As a vast literature has shown local labor demand shocks lead to the out-migration of predominantly young and highly educated individuals.[27] Since these individuals are generally less likely to support far-right political parties, the estimated effects could thus potentially reflect compositional changes of the local electorate, rather than an actual increasing effect on far-right voting. This possibility is investigated in columns 4 to 6 of Table A7. Column 4 includes the percentage-change in the native voting age population as additional control variable to net out any possible correlations between declines in manufacturing employment and declines in the size of the local population due to internal migration responses. Column 5 includes the end-of-period demographic composition of the local electorate as additional set of controls to check whether the results are driven by compositional changes and lastly column 6 combines the approaches from columns 4 and 5. As before all estimations are robust to controlling for these possible population reactions, and the estimates are thus not driven by declining population trends or compositional changes in the electorate.

Definition of regional units

During the main part of the analysis I rely on commuting zones as units of observations. These commuting zones are constructed analogously to commuting zones for the US (see Tolbert and Sizer, 1996). Their computation is based on data on municipality-to-municipality commuting flows, which are input into an horizontal clustering algorithm. This algorithm clusters municipalities until the average between cluster distance reaches a pre-defined threshold h. Table A8 in the Appendix assess the robustness of the main results of this paper with respect to different configurations of the

[27] See for example Blanchard and Katz (1992), Bound and Holzer (2000), Foote, Grosz, and Stevens (2019), Huttunen, Møen, and Salvanes (2018), Greenland, Lopresti, and McHenry (2019) or Faber, Sarto, and Tabellini (2021) for international evidence, and Bekhtiar (2022) for results for the Austrian case.

clustering algorithm (i.e. different values of h).[28] Additionally the results using commuting zones are compared to estimates using the Austrian administrative districts.

Looking at the estimations results for the effect of manufacturing employment changes in the first panel of Table A8 shows that all configurations of the algorithm result in a robust negative effect of manufacturing employment growth on far-right voting. However, the configuration of the clustering algorithm used during the main part of the paper ($h = 0.99$ in column 6 of Table A8) is the only one where a Moran's I test rejects the present of spatial autocorrelation. Comparing the magnitude of the point estimate for the main configuration of the algorithm ($h = 0.99$) to the estimates for lower configurations ($h < 0.99$) shows that the estimates are remarkably stable. Thus, even though the Moran's I test indicates the presence of spatial autocorrelation for $h < 0.99$, the practical importance of any bias arising from this problem appears to be very limited. The sole exception to this is the point estimate in column 1 where political districts are used as units of observation. Here the point estimate is much smaller and thus appears to be biased. This highlights that clustered commuting zones, which are specifically designed to contain a larger fraction of commuters within their borders, are much more appropriate to account for spatial spillovers than pre-defined administrative areas.

While spatial spillovers do not appear to be a problem for the estimation of the effect of manufacturing employment on far-right voting, the Morans-I test for the corresponding trade- and robotization estimations does indicates the presence of spatial autocorrelation. This is a potential concern, as this might lead to a bias in the estimates. Comparing the estimated effects of net-import and robot-exposure for the $h = 0.99$ configuration of the clustering algorithm to a lower configuration for which the Morans-I test indicates no spatial spillover effects ($h = 0.985$) however suggests that this bias is likely limited in practice, as the point-estimates are very similar in magnitude. As this pattern also emerged in the case of the effect of changes in manufacturing employment, the practical

[28]A more thorough discussion of the used commuting zones can be found in the Online Appendix of Bekhtiar (2022).

importance of spatial autocorrelation for the results regarding the electoral effects of trade- and robot-exposure appear to be rather limited.

8 Conclusion

Recent decades have seen a drastic shift in the political landscape in many western democracies, with far-right populist movements gaining growing support at the ballot box. While this trend for a long time has been relatively concentrated in European countries, the victory of Donald Trump in the 2016 US presidential elections has established far-right populist ideas also in the US. While this rise of the far-right coincides with a sharp increase in immigration, recent literature also emphasizes an important role for economic conditions.

In this paper I analyze the connection between the manufacturing decline and the rise of the far-right in Austria. As in several other countries, the increase in far-right vote-shares in Austria coincides not only with an increase in immigration, but also with a steady decline in manufacturing employment. This decline of manufacturing employment is arguably one of the most important structural changes which affected labor markets in recent decades. The results of the analysis show that this decline in manufacturing employment has made a strong contribution to the increase in far-right vote-shares in Austria between 1995 and 2017. During this time period, the manufacturing decline explains roughly 43% of the observed increase in far-right voting. Applying a causal mediation analysis reveals that the entirety of this effect is mediated through an increase in natives' unemployment rates. Hence the deterioration of employment prospects of native workers in the manufacturing industries has translated to increased support for far-right populist parties in Austria. Separately regarding the contributions of international trade and industrial robotization suggests that trade and technology have contributed in roughly equal parts to the overall effect of the manufacturing decline.

Overall the results of this paper highlight the importance of labor market conditions for the political sphere. As employment prospects for broad parts of the electorate erode, support for more radical political forces increases. This political backlash appears to happen both if labor market conditions deteriorate temporarily (e.g. in the wake of financial crises as shown in Algan et al., 2017 and Funke, Schularick, and Trebesch, 2016), as well as when structural changes have a lasting adverse impact on certain segments of the labor market. In both cases, weakening the blow of economic shocks on those most affected is likely to also contribute to more stability in the political system.

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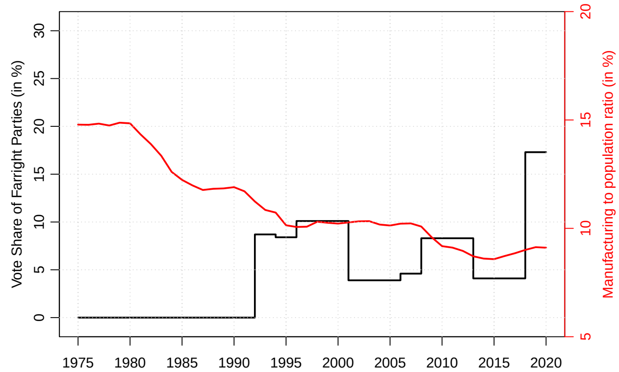
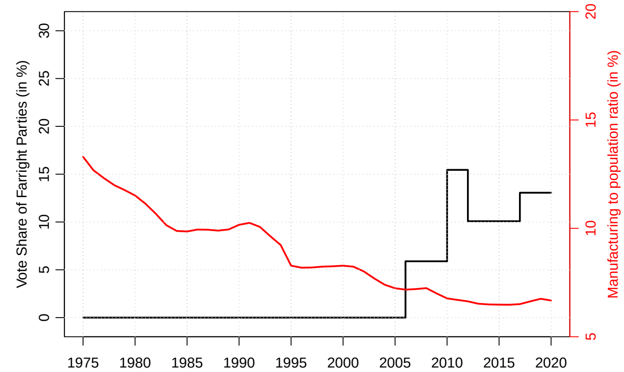
Figure A1: Farright-Populist Voting and the Decline of Manufacturing Employment across different european countries

Figure A1: Farright-Populist Voting and the Decline of Manufacturing Employment across different european countries (continued)

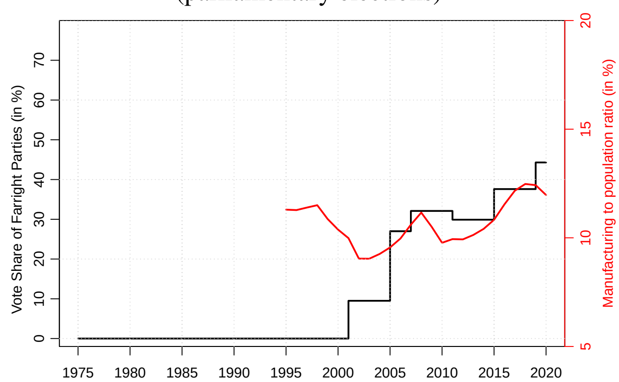
(g) Italy - Lega Nord
(parliamentary elections)



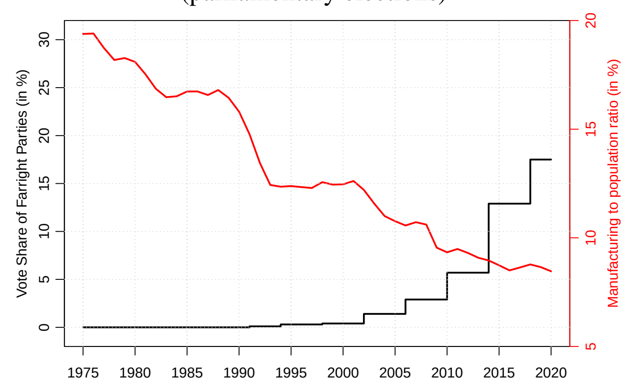
(h) Netherlands - Partij voor de Vrijheid
(parliamentary elections)



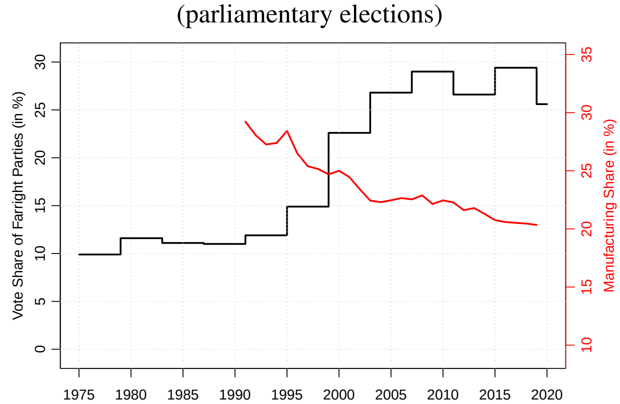
(i) Poland - PiS
(parliamentary elections)



(j) Sweden - Sweden Democrats
(parliamentary elections)

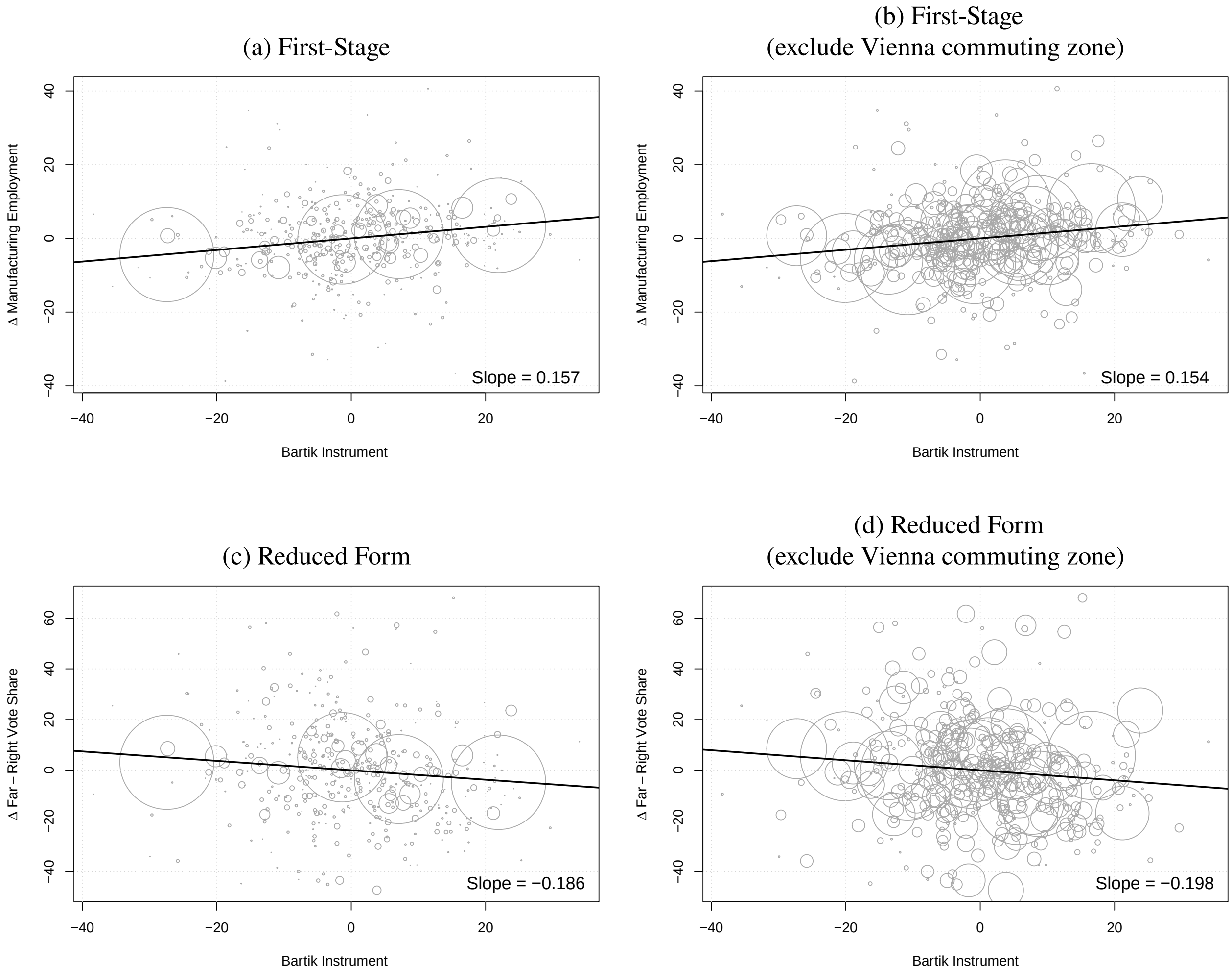


(k) Switzerland - Swiss People's Party (SVP)
(parliamentary elections)



Notes: Manufacturing shares are calculated from EU-KLEMS data (Releases: 2007 [March] & 2023). Since the EU-Klems data is not available for Switzerland, data on the manufacturing share from the World Bank's WDI data is used instead. Vote Shares of right-wing populist parties are collected from national sources. The y-axis for the vote shares has been extended for Hungary and Poland since those two countries are strong outliers in the electoral success of far-right populist parties.

Figure A2: First-stage and reduced form relationships (Bartik Instrument)



Notes: Plots show the first-stage and reduced form relationships for the Bartik instrument from the full specification in column 6 of Table 1. All included control variables as well as period and commuting zone fixed effects are partialled out. The plots in panels (b) and (d) show the respective relationships when the largest commuting zone (including the Austrian capital Vienna) is removed from the sample. All points are scaled by their respective weight in the regressions (i.e. the start-of-period size of the population eligible to vote). Since the Vienna commuting zone included in panels (a) and (c) is by far the largest commuting zone in the sample the scaling of the data points in these panels is different from the scaling in panels (b) and (c).

Figure A3: Mediation Analysis: Directed Acyclic Graph (DAG)

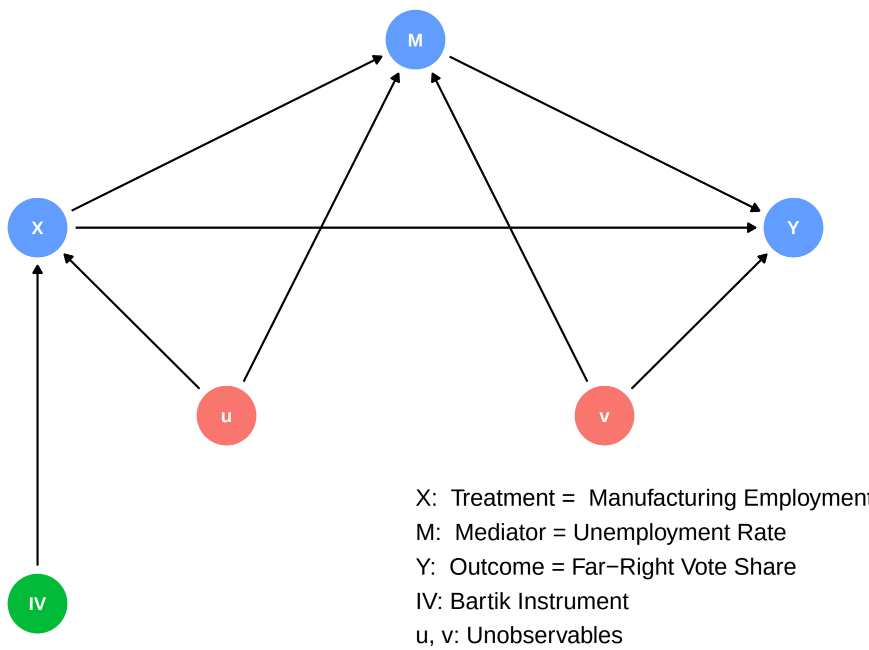
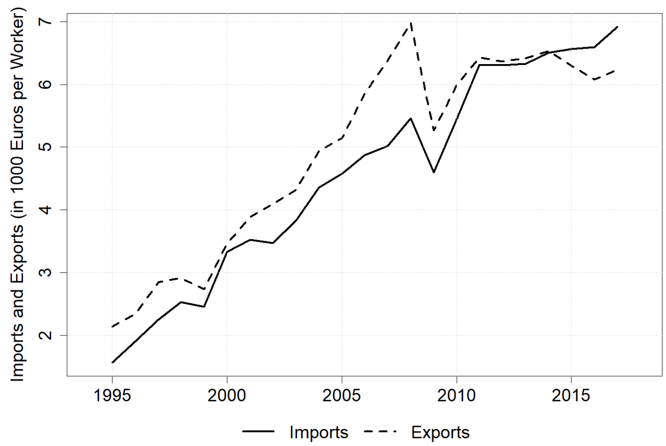
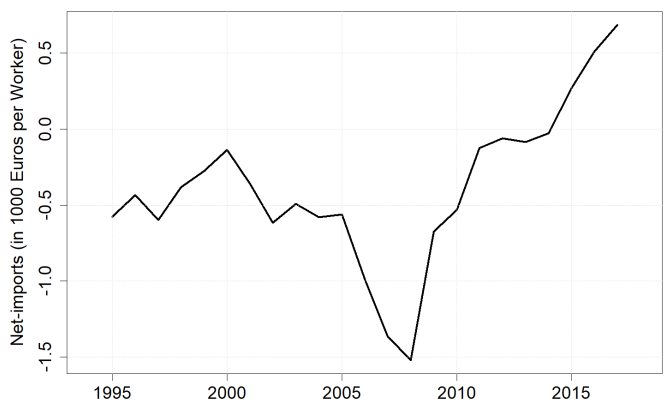


Figure A4: Changes in exposure to net-imports and industrial robots (1995-2017)

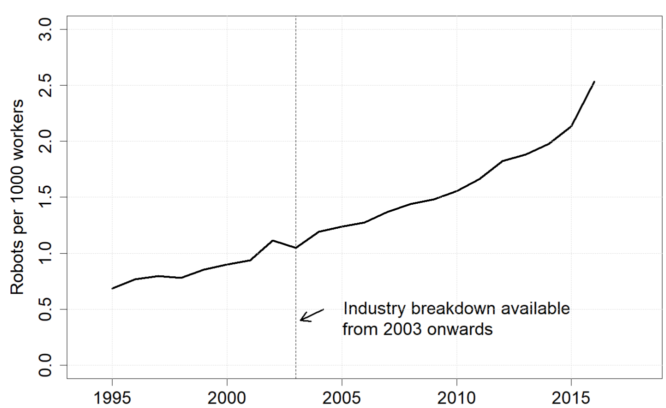
(a) Imports and Exports from China and the East (in 1000 Euros/worker)



(b) Net-imports from China and the East (in 1000 Euros/worker)



(c) Robots/1000 workers



Source: UN-Comtrade Database and International Federation of Robotics, own calculations

Table A1: Characteristics of voters of far-right parties from the European Social Survey (ESS; 2002-2018)

Country	% Manuf. Right (1)	% Manuf. Other (2)	% Blue Collar Right (3)	% Blue Collar Other (4)	% Unemp. (in last 5 years) Right (5)	% Unemp. (in last 5 years) Other (6)	% Low skill Right (7)	% Low skill Other (8)	% Male Right (9)	% Male Other (10)	N Right (11)	N Other (12)	Parties (13)
Panel A: Period 2002-2006													
Austria	0.2	0.134	0.318	0.223	0.116	0.092	0.285	0.246	0.631	0.472	253	4940	FPOE, BZOE
Belgium	0.244	0.191	0.484	0.296	0.151	0.11	0.517	0.396	0.586	0.47	351	4089	Vlaams Block, FN
Denmark	0.209	0.168	0.512	0.313	0.17	0.111	0.37	0.241	0.572	0.485	279	3637	DF
Finland	-	-	-	-	-	-	-	-	-	-	27	4304	True Fins
France	0.239	0.179	0.402	0.249	0.155	0.115	0.513	0.415	0.554	0.474	194	3342	FN
Germany	-	-	-	-	-	-	-	-	-	-	25	6432	NPD
Hungary	0.174	0.236	0.408	0.432	0.147	0.119	0.283	0.293	0.459	0.453	1309	2257	Fidesz, Jobbik
Italy	0.205	0.191	0.318	0.348	0.105	0.148	0.563	0.579	0.595	0.487	180	2085	Lega, AN
Netherlands	0.126	0.111	0.283	0.187	0.069	0.053	0.442	0.312	0.507	0.498	409	4492	LPF, LN
Poland	0.238	0.215	0.44	0.469	0.172	0.214	0.205	0.223	0.488	0.479	527	2841	PiS
Sweden	-	-	-	-	-	-	-	-	-	-	0	4792	SD
Switzerland	0.177	0.166	0.352	0.208	0.031	0.046	0.193	0.159	0.576	0.489	627	2649	SVP
United Kingdom	-	-	-	-	-	-	-	-	-	-	15	4259	BNP, UKIP
ESS Wave 1 (2002)	0.173	0.183	0.362	0.312	0.136	0.113	0.378	0.373	0.541	0.481	1326	17801	
ESS Wave 2 (2004)	0.22	0.187	0.356	0.303	0.12	0.121	0.447	0.358	0.546	0.483	1353	16458	
ESS Wave 3 (2006)	0.228	0.175	0.447	0.286	0.161	0.108	0.28	0.296	0.507	0.482	1517	15860	
Pooled (2002-2006)	0.213	0.182	0.394	0.301	0.141	0.115	0.362	0.346	0.529	0.482	4196	50119	
Panel B: Period 2014-2018													
Austria	0.17	0.128	0.338	0.268	0.111	0.082	0.227	0.177	0.567	0.484	727	4150	FPOE, BZOE
Belgium	0.251	0.184	0.531	0.265	0.135	0.099	0.392	0.282	0.482	0.481	96	4073	Vlaams Belaang, FN
Denmark	0.183	0.142	0.381	0.269	0.118	0.108	0.334	0.207	0.568	0.473	315	2272	DF
Finland	0.214	0.131	0.439	0.264	0.207	0.112	0.22	0.188	0.681	0.454	515	3939	True Fins
France	0.177	0.156	0.425	0.239	0.132	0.1	0.285	0.257	0.53	0.475	368	3163	FN, DLF
Germany	0.244	0.187	0.438	0.226	0.114	0.075	0.176	0.118	0.599	0.481	337	5981	AfD, NPD
Hungary	0.226	0.213	0.446	0.369	0.092	0.074	0.218	0.169	0.49	0.454	1882	1547	Fidesz, Jobbik
Italy	0.187	0.141	0.346	0.28	0.133	0.168	0.47	0.422	0.547	0.485	415	3244	Lega, FdI
Netherlands	0.169	0.103	0.454	0.173	0.119	0.102	0.428	0.235	0.574	0.491	311	3666	PVV, FVD
Poland	0.219	0.192	0.413	0.308	0.13	0.125	0.438	0.303	0.499	0.474	1249	1882	PiS, Kukiz'15
Sweden	0.145	0.116	0.43	0.19	0.125	0.085	0.297	0.161	0.729	0.473	306	3985	SD
Switzerland	0.174	0.117	0.273	0.145	0.027	0.069	0.176	0.118	0.555	0.495	429	1927	SVP
United Kingdom	0.114	0.099	0.337	0.194	0.13	0.081	0.391	0.247	0.524	0.484	261	4360	UKIP
ESS Wave 7 (2014)	0.213	0.16	0.45	0.245	0.144	0.093	0.344	0.232	0.526	0.483	2034	14426	
ESS Wave 8 (2016)	0.204	0.153	0.387	0.233	0.122	0.108	0.345	0.239	0.531	0.481	2503	14167	
ESS Wave 9 (2018)	0.187	0.14	0.394	0.227	0.106	0.092	0.338	0.218	0.542	0.478	2674	15596	
Pooled (2016-2018)	0.2	0.15	0.406	0.235	0.122	0.098	0.342	0.229	0.534	0.48	7211	44189	

Notes: This table compares voters of far-right parties with voters of all other parties in 13 European countries, with respect to the fraction employed in the manufacturing sector (columns 1 and 2), the fraction in blue collar occupations (columns 3 and 4), the fraction of individuals with some unemployment spell during the last 5 years (columns 5 and 6), the fraction of low educated individuals (columns 7 and 8) and the fraction of males (columns 9 and 10). Columns 11 and 12 show the number of available observations, while column 13 lists the parties defined as far-right. Data comes from the European Social Survey (ESS), where the survey items relating to the question "Party voted for in last national election" are used. To increase the number of available observations, panel A pools the survey waves 1 to 3 for the period 2002-2006, while panel B pools the survey waves 7 to 8 for the period 2014-2018. Countries for which the number of available observations is still too small are censored (only necessary in 2002-2006). These censored countries are Finland (27 observations who voted for the True Fins), Germany (25 observations who voted for the Neo-Nazi party NPD), and the United Kingdom (15 observations who voted for the BNP or UKIP). In the case of Sweden, the far-right Sweden democrats were not available as an option in the survey question in the period 2002-2006.

Table A2: Manufacturing Employment and unemployment rates (1995-2017)

	Dependent Variable: Percentage Point Change in Unemployment Rate of Natives × 100					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: OLS Estimations:						
%Δ Manufacturing Emp.:	0.005	-0.003	-0.01	-0.01	-0.009	-0.009
	(0.005)	(0.004)	(0.004)**	(0.004)**	(0.004)**	(0.004)**
Panel B: 2SLS Estimations:						
%Δ Manufacturing Emp.:	0.019	-0.006	-0.046	-0.049	-0.049	-0.042
	(0.017)	(0.026)	(0.021)**	(0.021)**	(0.023)**	(0.021)**
	[0.015]	[0.024]	[0.019]**	[0.019]**	[0.02]**	[0.017]**
Panel C: First Stage Estimations:						
Bartik^{IV}:	0.214	0.188	0.172	0.169	0.158	0.171
	(0.015)***	(0.019)***	(0.022)***	(0.021)***	(0.02)***	(0.02)***
	[0.01]***	[0.007]***	[0.009]***	[0.01]***	[0.011]***	[0.011]***
First-Stage F-Statistic:	207.67	96.04	62.82	67.16	61.14	76.16
Period Fixed Effects	x	x	x	x	x	x
Commuting Zone Fixed Effects	x	x	x	x	x	x
Industry Structure	x	x	x	x	x	x
Regional Characteristics		x	x	x	x	x
Demographic Characteristics			x	x	x	x
Lagged employment changes				x	x	x
Migrant shares (by skill groups)					x	x
%Δ Migrant shares						x
Commuting Zones	100	100	100	100	100	100
Periods	4	4	4	4	4	4
Observations	400	400	400	400	400	400

Notes: * < 0.10, ** < 0.05, *** < 0.01. Heteroskedasticity robust standard errors are reported in round brackets, while industry structure clustered standard errors from Adao, Kolesár, and Morales (2019) are reported in square brackets. Units of observation are 100 clustered commuting zones. All specifications include a set of region and period fixed effects, as well as start-of-period employment shares of several sub-industries of manufacturing (production of food products, consumer goods, industrial goods and capital goods), as well as industries outside of manufacturing (utilities, construction, personal services and business services). Regional characteristics control for the logarithm of the gross regional product, as well as start-of-period party affiliation of the local governor and vice governor. Demographic controls include the start-of-period structure of the native voting-age population, as well as the start-of-period degree of urbanization. Lagged employment controls include the first lag of the percentage-changes in manufacturing and non-manufacturing employment. Migrant shares (in start-of-period levels and percentage-changes) are included separately for three skill groups (high-, medium- and low-skilled migrants). The start-of-period unemployment rate (decomposed by the respective contribution of natives and immigrants) is removed from the set of controls. All estimations are weighted by the start-of-period native voting-age population.

Table A3: Mediation Analysis: The Role of Unemployment (Robustness)

	Baseline			
	(1)	(2)	(3)	(4)
Panel A: Y = percentage change in far-right vote-share				
Total Effect:	-0.917	-0.914	-1.225	-1.214
	(0.326)***	(0.361)**	(0.351)***	(0.378)***
	[0.237]***	[0.257]***	[0.206]***	[0.215]***
Direct Effect:	-0.099	-0.098	0.083	0.051
	(0.173)	(0.169)	(0.462)	(0.362)
	[0.034]***	[0.034]***	[0.042]**	[0.039]
Indirect Effect:	-0.819	-0.816	-1.308	-1.265
	(0.369)**	(0.399)**	(0.58)**	(0.524)**
	[0.239]***	[0.259]***	[0.21]***	[0.219]***
Panel B: Y = percentage-point change in far-right vote-share				
Total Effect:	-0.248	-0.289	-0.245	-0.282
	(0.044)***	(0.057)***	(0.052)***	(0.064)***
	[0.017]***	[0.018]***	[0.02]***	[0.022]***
Direct Effect:	0.055	0.055	0.096	0.091
	(0.034)	(0.035)	(0.107)	(0.09)
	[0.003]***	[0.003]***	[0.003]***	[0.003]***
Indirect Effect:	-0.303	-0.343	-0.341	-0.373
	(0.056)***	(0.067)***	(0.119)***	(0.11)***
	[0.018]***	[0.018]***	[0.02]***	[0.022]***
Include Δ Non-Manufacturing Employment:		x		x
Include Δ Labor Market Participation Rate:			x	x

Notes: * < 0.10, ** < 0.05, *** < 0.01. Heteroskedasticity robust standard errors are reported in round brackets, while industry structure clustered standard errors from Adao, Kolesár, and Morales (2019) are reported in square brackets. Units of observation are 100 clustered commuting zones. All specifications include a set of region and period fixed effects, as well as start-of-period employment shares of several sub-industries of manufacturing (production of food products, consumer goods, industrial goods and capital goods), as well as industries outside of manufacturing (utilities, construction, personal services and business services). Regional characteristics control for the logarithm of the gross regional product, as well as start-of-period party affiliation of the local governor and vice governor. Demographic controls include the start-of-period structure of the native voting-age population, as well as the start-of-period degree of urbanization. Lagged employment controls include the first lag of the percentage-changes in manufacturing and non-manufacturing employment. Migrant shares (in start-of-period levels and percentage-changes) are included separately for three skill groups (high-, medium- and low-skilled migrants). The start-of-period unemployment rate (decomposed by the respective contribution of natives and immigrants) is removed from the set of controls. All estimations are weighted by the start-of-period native voting-age population.

Table A4: Employment Effects of Trade and Robotization (2SLS estimates)

	Overall				Natives Only			
	1995-2017		2002-2017		1995-2017		2002-2017	
	Manuf. (1)	Non-Manuf. (2)	Manuf. (3)	Non-Manuf. (4)	Manuf. (5)	Non-Manuf. (6)	Manuf. (7)	Non-Manuf. (8)
Panel A: Net-Import Exposure								
Δ Net-Imports	-3.026	-0.528	-3.534	-0.758	-2.093	-0.362	-2.985	-0.432
	(2.624)	(0.995)	(1.873)*	(1.143)	(2.31)	(1.033)	(1.731)*	(1.236)
	[1.052]***	[0.498]	[0.779]***	[0.69]	[0.991]**	[0.445]	[0.831]***	[0.626]
First-Stage F-Statistic:	15.36	15.36	29.99	29.99	15.36	15.36	29.99	29.99
Panel B: Import- & Export-Exposure seperately								
Δ Imports	-3.096	-1.225	-3.55	-1.391	-1.924	-0.89	-2.871	-0.854
	(3.367)	(1.628)	(2.526)	(1.709)	(3.027)	(1.679)	(2.384)	(1.83)
	[1.115]***	[0.626]*	[1.397]**	[0.974]	[1.076]*	[0.559]	[1.382]**	[0.863]
First-Stage F-Statistic:	18.85	18.85	12.16	12.16	18.85	18.85	12.16	12.16
Δ Exports	2.973	-0.002	3.515	-0.027	2.221	-0.04	3.128	-0.091
	(2.18)	(0.911)	(1.503)**	(1.064)	(1.917)	(0.878)	(1.462)**	(1.055)
	[0.942]***	[0.441]	[0.797]***	[0.51]	[0.911]**	[0.395]	[0.819]***	[0.488]
First-Stage F-Statistic:	20.05	20.05	9.78	9.78	20.05	20.05	9.78	9.78
Panel C: Robot-Exposure								
Δ Robots			-3.244	-0.919			-3.575	-1.208
			(1.548)**	(1.273)			(1.745)**	(1.384)
			[1.116]***	[0.734]			[1.335]***	[0.773]
First-Stage F-Statistic:			27.94	27.94			27.94	27.94
Period Fixed Effects	x	x	x	x	x	x	x	x
Commuting Zone Fixed Effects	x	x	x	x	x	x	x	x
Industry Structure	x	x	x	x	x	x	x	x
Regional Characteristics	x	x	x	x	x	x	x	x
Demographic Characteristics	x	x	x	x	x	x	x	x
Tech. Shock: Δ ICT	x	x	x	x	x	x	x	x
Migrant shares (by skill)	x	x	x	x	x	x	x	x
Δ Migrant shares	x	x	x	x	x	x	x	x
Tech. Shock: Δ Robots			x	x			x	x
Commuting Zones	100	100	100	100	100	100	100	100
Periods	4	4	3	3	4	4	3	3
Observations	400	400	300	300	400	400	300	300

Notes: * < 0.10, ** < 0.05, *** < 0.01. Heteroskedasticity robust standard errors are reported in round brackets, while industry structure clustered standard errors from Adao, Kolesár, and Morales (2019) are reported in square brackets. Units of observation are 100 clustered commuting zones. All specifications include a set of region and period fixed effects, as well as start-of-period employment shares of several sub-industries of manufacturing (production of food products, consumer goods, industrial goods and capital goods), as well as industries outside of manufacturing (utilities, construction, personal services and business services). Since the exposure shares used to construct the trade- (robot-) exposure measure and the instrument are incomplete (i.e. do not sum to one), the period fixed effects are interacted with the sum of incomplete exposure shares (as is recommended in Borusyak, Hull, and Jaravel, 2022). Regional characteristics control for the start-of-period unemployment rate (decomposed by the respective contribution of natives and immigrants), the logarithm of the gross regional product, as well as start-of-period party affiliation of the local governor and vice governor. Demographic controls include the start-of-period structure of the native voting-age population, as well as the start-of-period degree of urbanization. Migrant shares (in start-of-period levels and percentage-changes) are included separately for three skill groups (high-, medium- and low-skilled migrants). Additionally two types of labor market shock controls are included to capture changes in ICT- and robot-exposure (for the trade regressions) and changes in ICT- and trade-exposure (for the robot regressions). All estimations are weighted by the start-of-period native voting-age population.

Table A5: Import- and export exposure and far-right voting (2SLS estimates)

	Dependent Variable: %Δ Far-right vote share		
	1995-2017	2002-2017	
	(1)	(2)	(3)
Δ Imports	11.41	12.046	10.28
	(5.371)**	(5.56)**	(5.88)*
	[3.192]***	[2.922]***	[2.97]***
Δ Exports	-7.472	-7.239	-3.755
	(3.407)**	(3.487)**	(3.91)
	[1.609]***	[1.468]***	[1.337]***
First-Stage F-Statistic: Δ Imports	18.85	13.96	12.16
First-Stage F-Statistic: Δ Exports	20.05	13.86	9.78
Period Fixed Effects	x	x	x
Commuting Zone Fixed Effects	x	x	x
Industry Structure	x	x	x
Regional Characteristics	x	x	x
Demographic Characteristics	x	x	x
Tech. Shock: Δ ICT	x	x	x
Migrant shares (by skill)	x	x	x
Δ Migrant shares	x	x	x
Tech. Shock: Δ Robots			x
Commuting Zones	100	100	100
Periods	4	3	3
Observations	400	300	300

Notes: * < 0.10, ** < 0.05, *** < 0.01. Heteroskedasticity robust standard errors are reported in round brackets, while industry structure clustered standard errors from Adao, Kolesár, and Morales (2019) are reported in square brackets. Units of observation are 100 clustered commuting zones. All specifications include a set of region and period fixed effects, as well as start-of-period employment shares of several sub-industries of manufacturing (production of food products, consumer goods, industrial goods and capital goods), as well as industries outside of manufacturing (utilities, construction, personal services and business services). Since the exposure shares used to construct the trade-exposure measure and the instrument are incomplete (i.e. do not sum to one), the period fixed effects are interacted with the sum of incomplete exposure shares (as is recommended in Borusyak, Hull, and Jaravel, 2022). Regional characteristics control for the start-of-period unemployment rate (decomposed by the respective contribution of natives and immigrants), the logarithm of the gross regional product, as well as start-of-period party affiliation of the local governor and vice governor. Demographic controls include the start-of-period structure of the native voting-age population, as well as the start-of-period degree of urbanization. Migrant shares (in start-of-period levels and percentage-changes) are included separately for three skill groups (high-, medium- and low-skilled migrants). Additionally two types of technology shock controls are included to capture changes in ICT- and robot-exposure. All estimations are weighted by the start-of-period native voting-age population.

Table A6: Mediation Analysis: Trade and Robot-exposure

	Δ Net-Imports	Δ Robots	
	(1)	(2)	Computation
Panel A: Mediation Analysis			
Total Effect:	7.366	6.421	$\hat{\gamma}^{IV} = \hat{\gamma}_Y^X + \hat{\gamma}_M^X \times \hat{\gamma}_Y^M$
	(3.597)**	(3.09)**	
	[1.765]***	[0.209]***	
Direct Effect:	1.659	2.701	$\hat{\gamma}_Y^X$
	(1.681)	(1.639)	
	[0.627]***	[0.135]***	
Indirect Effect:	5.707	3.72	$\hat{\gamma}_M^X \times \hat{\gamma}_Y^M$
	(3.971)	(3.498)	
	[1.873]***	[0.249]***	
Panel B: Model Parameters			
$\hat{\gamma}^{IV}$	7.366	6.421	Estimation of equation 1 via 2SLS
	(3.597)**	(3.09)**	(Estimates deviate from Tables 1 and 2 because
	[1.765]***	[0.209]***	unemployment rate is excluded from controls)
$\hat{\gamma}_M^X$	-3.534	-3.244	Estimation of equations M1.1 and M1.2
	(1.873)*	(1.548)**	
	[0.905]***	[0.103]***	
$\hat{\gamma}_Y^X$	1.659	2.701	Estimation of equations M2.1 and M2.2
	(1.681)	(1.639)	
	[0.627]***	[0.135]***	
$\hat{\gamma}_Y^M$	-1.615	-1.147	Estimation of equations M2.1 and M2.2
	(0.842)*	(0.978)	
	[0.455]***	[0.1]***	

Summary Mediation Model (see also Figure A3):

Panel C: Model Notation

Outcome Y:	%Δ Vote-Share Farright
Treatment X:	Δ Net-Imports (column 1) or Δ Robots (column 2)
Mediator M:	%Δ Manufacturing Employment
Instrument IV:	ΔNet-Imports^{IV} (column 1) or ΔRobots^{IV} (column 2)
Vector of Controls C:	Full set of controls, plus period and commuting zone fixed effects
u, v:	Unobservable Confounders

Panel D: Model Equations:

Effect of Treatment X on Mediator M

M1.1 (First Stage): $X = \gamma_X^{IV} IV + C\beta_X^C + \epsilon_X$

M1.2 (Second Stage): $M = \gamma_M^X X + C\beta_M^C + \epsilon_M$

Effect of Mediator M on Outcome Y

M2.1 (First Stage): $M = \gamma_M^{IV} IV + \beta_M^X X + C\beta_M^C + \tilde{\epsilon}_M$

M2.2 (Second Stage): $Y = \gamma_Y^M M + \gamma_Y^X X + C\beta_Y^C + \epsilon_Y$

Panel E: Assumptions:

Instrument Exogeneity: $IV \perp\!\!\!\perp (\epsilon_X, \epsilon_M, \epsilon_Y)$

Partial Confoundendness: For $\epsilon_X = f(u)$, $\epsilon_M = f(u, v)$ and $\epsilon_Y = f(v)$ we need $\epsilon_X \perp\!\!\!\perp \epsilon_Y$ (or equivalently $u \perp\!\!\!\perp v$)

Notes: * < 0.10, ** < 0.05, *** < 0.01. Heteroskedasticity robust standard errors are reported in round brackets. Industry-structure clustered standard errors from Adao, Kolesár, and Morales (2019) are reported in square brackets. The standard errors for the indirect effect are calculated as the standard error of the estimate difference $\hat{\gamma}^{IV} - \hat{\gamma}_M^X$ (i.e. from the difference between the estimated total and direct effects). All estimations are weighted by the start-of-period native voting-age population, and include all controls and fixed effects from Tables 4 and 5.

Table A7: Additional Robustness Checks:

	Baseline	Fixed Exposure Shares	Changes in Turnout	Internal Migration Responses		
				Δ Population Size	Dem. Composition in t=2	Both
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Changes in Manufacturing Employment						
%Δ Manufacturing Employment:	-1.181	-1.891	-1.413	-1.111	-1.123	-1.114
	(0.418)***	(0.684)***	(0.553)**	(0.41)***	(0.502)**	(0.517)**
	[0.23]***	[0.233]***	[0.385]***	[0.222]***	[0.246]***	[0.258]***
First Stage F:	51.3	15.77	51.3	47.18	26.72	23.75
Panel B: Changes in Trade Exposure						
Δ Net-Imports (1995-2017; excl. Robot controls):	9.175	9.633	9.225	9.508	10.487	10.676
	(3.431)***	(3.715)**	(3.697)**	(3.389)***	(3.571)***	(3.576)***
	[2.106]***	[2.272]***	[2.167]***	[2.087]***	[1.766]***	[1.828]***
First Stage F:	15.36	15.13	15.36	15.46	16.11	16.68
Δ Net-Imports (2002-2017; incl. Robot controls):	7.366	6.89	9.18	6.92	7.158	6.834
	(3.597)**	(3.876)*	(4.038)**	(3.557)*	(3.75)*	(3.636)*
	[1.235]***	[1.083]***	[1.237]***	[1.272]***	[1.696]***	[1.751]***
First Stage F:	29.99	26.39	29.99	29.51	28.53	29.88
Panel C: Changes in Robot Exposure						
Δ Robots:	6.421	6.876	5.734	6.349	8.035	8.118
	(3.09)**	(2.779)**	(3.093)*	(3.066)**	(3.187)**	(3.229)**
	[2.033]***	[1.892]***	[1.975]***	[1.991]***	[2.027]***	[2.051]***
First Stage F:	27.94	26.03	27.94	29.52	25.42	25.36

Notes: * < 0.10, ** < 0.05, *** < 0.01. Heteroskedasticity robust standard errors are reported in round brackets, while industry structure clustered standard errors from Adao, Kolesár, and Morales (2019) are reported in square brackets. Units of observation are 100 clustered commuting zones. All specifications include a full set of controls corresponding to the controls used in the respective estimations in Tables 1, 4 and 5. All estimations are weighted by the start-of-period native voting-age population.

Table A8: Local Labor Market Definition:

	(1)	(2)	(3)	(4)	(5)	(6)
						Baseline
LLM Definition:	Districts	h = 0.98	h = 0.9825	h = 0.985	h = 0.9875	h = 0.99
Units:	94	238	197	158	124	100
Commuters within LLM:	65.62 %	70.07 %	71.57 %	72.75 %	74.18 %	75.31 %
Δ Manufufacturing Employment:	-0.72	-1.126	-1.074	-1.184	-1.151	-1.181
	(0.173)***	(0.291)***	(0.285)***	(0.359)***	(0.378)***	(0.418)***
	[0.147]***	[0.071]***	[0.182]***	[0.225]***	[0.213]***	[0.23]***
First-Stage F:	0	63	73.97	56.81	54.96	51.3
Moran's I:	0.478	0.055	0.028	0.122	0.11	-0.027
(p-Value)	(0)***	(0)***	(0.088)*	(0)***	(0)***	(0.317)
Δ Net-Imports (1995-2017)	6.15	6.308	7.035	6.845	6.395	9.175
	(2.567)**	(3.173)**	(3.644)*	(3.869)*	(3.79)*	(3.431)***
	[1.467]***	[0.776]***	[1.026]***	[1.273]***	[1.663]***	[2.106]***
First-Stage F:	44.97	15.05	14.62	13.22	12.88	15.36
Moran's I:	0.432	0.085	0.06	0.156	0.138	0.066
(p-Value)	(0)***	(0)***	(0)***	(0)***	(0)***	(0.005)***
Δ Net-Imports (2002-2017)	5.865	5.351	6.566	6.385	6.803	7.366
	(2.553)**	(2.734)*	(3.077)**	(3.3)*	(3.604)*	(3.597)**
	[1.741]***	[0.527]***	[0.784]***	[0.932]***	[1.473]***	[1.235]***
First-Stage F:	8.55	1.98	2.15	0.05	10.35	2.92
Moran's I:	0.317	0.118	0.117	0.076	0.155	0.073
(p-Value)	(0)***	(0)***	(0)***	(0)***	(0)***	(0.006)***
Δ Robots	9.341	4.067	4.839	4.881	4.694	6.332
	(3.055)***	(2.024)**	(2.16)**	(2.457)**	(2.401)*	(3.103)**
	[3.331]***	[0.82]***	[1.039]***	[1.278]***	[1.323]***	[2.038]***
First-Stage F:	39.79	27.1	29.21	30.02	29.12	27.44
Moran's I:	0.27	0.049	0.057	0.015	0.079	0.06
(p-Value)	(0)***	(0.004)***	(0.003)***	(0.439)	(0.001)***	(0.025)**
Full Controls	x	x	x	x	x	x

Notes: * < 0.10, ** < 0.05, *** < 0.01. Heteroskedasticity robust standard errors are reported in round brackets, while industry structure clustered standard errors from Adao, Kolesár, and Morales (2019) are reported in square brackets. Commuting zones are computed from municipality-to-municipality commuting data from the Austrian Statistical Agency *Statistik Austria*. As is described in detail in Tolbert and Sizer (1996), this commuting data is inputed into a horizontal clustering algorithm. The h parameter, indicated at the top of the Table shows, depicts the tuning constant of the horizontal clustering algorithm used to compute the commuting zones. Higher values of h are more restrictive, in that they allow weaker between cluster commuting ties. The baseline configuration of the algorithm used during the main part of the analysis is $h = 0.99$. For a more detailed discussion of the computation and performance of the Austrian commuting zones, see Bekhtiar (2022). All specifications include a set of region and period fixed effects, as well as a full set of control variables. All estimations are weighted by the start-of-period native voting-age population.