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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 around one-third of the observed increase in far-right vote-shares between 1995 and 2019. 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.

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 et al., 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. Fig. 1 depicts this graphically for the case of Austria, while Fig. A.1 in the Appendix shows that the same pattern holds 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 Fig. 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 A.1 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, possess lower skill levels 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 typical case in terms of the observed simultaneity in the rise of the far-right and the decline of the manufacturing sector (Fig. A.1) as well as the demographic and labor market characteristics of the far-right voters base (Table A.1).

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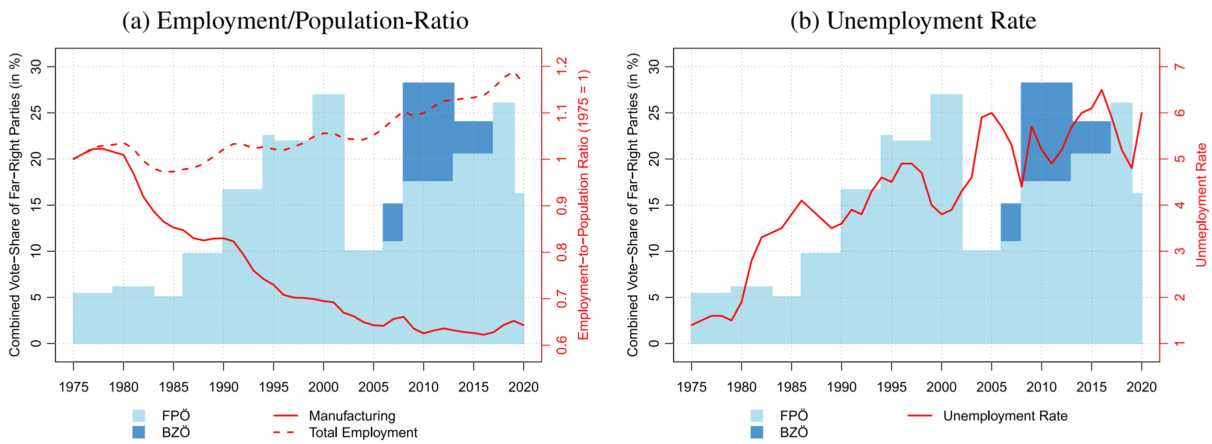

Fig. 1. Far-Right voting and the decline of manufacturing employment (1975–2020). *Notes:* The employment-to-population ratios in Panel (a) are calculated using employment data from EU-Klems (March 2007 and February 2023 releases) and OECD population data (as in Fig. A.1). The seasonally adjusted unemployment 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 Fig. A.1 (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).

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. 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.

This paper investigates the effect of the manufacturing decline on the rise of the far-right 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 32% of the observed increase in far-right voting during 1995–2019. 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 is the stronger factor, the manufacturing decline has made a strong contribution to the rise of the far-right in Austria. Looking at the impacts of trade and technology – two forces that strongly shaped the manufacturing decline – shows that both factors lead to declines in manufacturing employment and increase far-right voting. 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.[2] With regard to the economic determinants of rising political polarization, a growing literature has recently provided evidence that employment shocks (Dehdari, 2021), job loss and unemployment (Algan et al., 2017; Margalit, 2013), austerity measures (Fetzer, 2019), financial crises (Funke et al., 2016), rising trade exposure (Dippel et al., 2022; Autor et al., 2020; Rodrik, 2018; Colantone and Stanig, 2018a, 2018b, Margalit, 2011) and automation technologies (Anelli et al., 2019, 2021, Frey et al., 2018, or Kurer and Palier, 2019) benefit the far-right at the ballot box.

This paper contributes to this literature by showing that the large and lasting structural decline of the manufacturing sector, which characterizes most industrialized economies, has played an important role in recent increases in far-right voting. This contribution of the manufacturing decline is only slightly smaller than the contribution of migration, highlighting that economic conditions, especially with regard to the labor market, play an important role in preserving political stability.

The rest of this paper is structured as follows: Section 2 provides a brief overview of 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 analyzes 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 recent rise of the far-right until the late 1990s/early 2000s, the Austrian Freedom Party (FPÖ) was among the first modern far-right parties in Europe

[2] See for example, Halla et al. (2017) and Steinmayr (2021) for the case of Austria. Similar results have been documented for Denmark (Dustmann et al., 2019), France (Edo et al., 2019), Italy (Barone et al., 2016) or Switzerland (Brunner and Kuhn, 2018), among others.

that found wide spread electoral success already during the 1980s (see Fig. A.1 in the Appendix). Building on a radical anti-immigration stance, the FPÖ 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 (Fig. 1). After this remarkable success, 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.[3] Consequently, it was met with widespread opposition and even caused the European Union to impose economic sanctions on Austria.

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 Fig. 1, this led to a dramatic drop in the FPÖ's vote-share and a secession of parts of the party into the newly formed (and similarly positioned) Alliance For The Future Of Austria (BZÖ). 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%.

In 2016, the FPÖ candidate for the Austrian presidency finished the first round of the presidential elections in second place, and thus was able to enter the runoff election. While the FPÖ candidate lost this runoff election, he gathered a - for a far-right candidate unprecedented - vote-share of 46%. With this remarkable performance in the presidential elections under their belt, the FPÖ again entered a government coalition with the conservative ÖVP after the national elections of 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 had 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 successfully positioned itself as a Covid-skeptic party. Consequently the party swiftly recovered from the losses caused by the Ibiza-scandal. In the national elections of September 2024, the FPÖ (with a vote-share of 28.8%) not only gathered their strongest electoral support yet, but has – for the first time – become the strongest political party 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 used at the industry level.

While most regional data sources are available for all Austrian municipalities, the analysis is performed at the level of 158 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), and perform much better in capturing spatial spillover effects than municipalities or political districts.[4] A detailed discussion of the estimation procedure, as well as an evaluation of their performance is available in Online Appendix C.

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). 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 Statistics.[5]

To isolate the causal effect of changes in manufacturing employment on far-right voting, I rely on a Bartik-type instrumental variable. 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 3-digit industries using the `concordance`-package in R (Liao et al., 2020). The trade values have been inflated to 2019 US-Dollars and converted to Euros using the average USD–Euro exchange rate for 2019.

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

[3] 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.

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

[5] The Austrian census is only available for the years 1991, 2001 and 2011. Since 2008 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, they are internally consistent and directly comparable. Both data sources are available online at the Austrian statistical office Statistik Austria. Since this data is only available for 1991, 2001 and from 2008 onward, missing years have been imputed using linear interpolation techniques. Online Appendix D presents a deeper discussion of these imputations.

equation at the level of Austrian commuting zones:

$$\%\Delta Voteshare_{rt} = \gamma \times \%\Delta Manufacturing_{rt} + C'_{rt}\beta + \rho_r + \tau_t + \epsilon_{rt} \quad (1)$$

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.

The analysis is performed on the time-frame 1995–2019. During this time eight national elections took place. This allows to partition the data into seven periods, measuring election-to-election changes (1995–1999, 1999–2002, 2002–2006, 2006–2008, 2008–2013, 2013–2017 and 2017–2019). 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).

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, mining and quarrying) serves as the baseline category and is thus excluded.[6]

Next, I include controls for start-of-period differences in regional economic and political conditions. For this, I include the start-of-period logarithm of the gross regional product (total and per-capita) and the regional unemployment rate (separately for natives and migrants). 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 the nine Austrian federal states. These controls are constructed as the share of a commuting zone's population living in a federal state governed by either a governor or vice-governor of the far-right (FPÖ or BZÖ), the Austrian People's Party (ÖVP), the Austrian Social Democrat Party (SPÖ), or the Green Party. For commuting zones that are entirely contained within one of Austria's 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.

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, with 65+ used as omitted baseline category) and three educational groups (medium- and low-education, highly educated serve as baseline) in a commuting zone's native voting age population. Additionally, this set of controls includes the share of a commuting zone's native voting age population living in central rural areas and remote rural areas to control for the degree of urbanization.[7]

If employment trends are highly persistent over time, it is conceivable that part of the estimated effect of the contemporaneous change in manufacturing employment reflects persisting effects of past employment changes (see Jaeger et al., 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 immigrants in a commuting zone's overall population (differentiated by three educational groups), as well as the change of these skill 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 changes in those migrant shares occurs simultaneously with the change in employment. Because of this simultaneity between the explanatory variable of interest (the percentage-change in manufacturing employment) and the 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 et al., 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 Eq. (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 Eq. (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.

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, which is plausibly exogenous to region specific unobservables (see Goldsmith-Pinkham et al., 2020). 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 constructed by interacting regional industry-employment-shares with the corresponding industry level growth rates in other geographical regions.

$$Bartik^{IV}_{rt} = \sum_i \frac{Employment_{irt-10}}{Employment_{rt-10}} \times \%\Delta Employment^{OtherCountries}_{it} \quad (2)$$

where $Employment_{irt}$ is the number of employees in industry i in commuting zone r in period t, and $Employment_{rt}$ is the total number of employees in all industries in commuting zone r at period t. The emplyoment growth rates in industry i at period t in other regions is denoted by $\%\Delta Employment^{OtherCountries}_{it}$. 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 rely on industry level variation in other high income countries for identification, and has the straightforward 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.[8] To avoid mechanical correlations between the instrument and the explanatory variable, the

[6] These employment shares are defined analogously to Dauth et al. (2021).

[7] Urban and (central and remote) rural areas are defined according to the official Urban–Rural classification published by the Austrian Statistical Agency. The share of the electorate living in urban areas serves as omitted baseline category.

[8] Data on employment changes by 3-digit manufacturing industry for the computation of $\%\Delta Employment^{OtherCountries}_{it}$ comes from the Structural Business Statistics (SBS) from EuroStat, and is available from 1995 onward. The industry-employment growth rates $\%\Delta Employment^{OtherCountries}_{it}$ 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. While

employment shares used to project these industry growth rates onto the regional level are lagged 10 years into the past.

To ensure that the instrument in Eq. (2) 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 $\sum_i \frac{Employment_{irt-10}}{Employment_{rt-10}} = 1$. Since the exposure shares used to construct the instrument thus sum up to one, this further ensures that conventional period fixed effects are able to isolate within-period variation.[9]

Recently, several papers have thoroughly discussed under which conditions Bartik-type instrumental variables are able to plausibly isolate causal effects. This work has shown that the primary source of endogeneity concerns relates to the idiosyncratic regional component of employment growth, which may be correlated with unobserved regional confounders (see Goldsmith-Pinkham et al., 2020). The instrument in Eq. (2) thus must be orthogonal to this region specific component of employment growth. Recent papers by Adao et al. (2019) and Borusyak et al. (2022) argue that this condition is satisfied when the industry level employment growth rates used to compute the instruments $\%\Delta Employment_{it}^{OtherCountries}$ 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 et al., 2022). The exogenous shocks condition thus requires that the growth rates of manufacturing employment in other European countries are only related to regional manufacturing growth via common industry level trends and are uncorrelated with idiosyncratic regional determinants of employment growth.[10]

As 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 $\%\Delta Employment_{it}^{OtherCountries}$ 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. While the *exogenous shocks* condition cannot be tested directly (as it essentially mirrors a standard exclusion restriction), Borusyak et al. (2022, 2024) propose several plausibility checks to assess its plausibility. 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, clustered at the level of the nine Austrian federal states. To correct for possible bias due to an insufficient number of clusters, I apply the bias correction procedures for few clusters described in Cameron and Miller (2015). Secondly, I use alternative 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 et al. (2019). Adao et al. (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.

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. In sum, all estimations show a clear and robust negative relationship between manufacturing employment and far-right voting. Here the 2SLS estimations in Panel B indicate an elasticity of the far-right vote share of around 1. This estimated effect is rather stable over all specifications, and indicates that a 1% decrease in manufacturing employment leads to an increase in far-right voting of between 0.741% (column 1) to 1.163% (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. changes in the share of high-, medium- and low-skilled immigrants) occur simultaneously with the change of manufacturing employment. Notably, the inclusion of these simultaneous controls has only a very small impact on the overall magnitude of the point estimates. 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. Here, the first-stage F-statistic is large and clearly exceeds the critical value proposed by Stock and Yogo (2005), indicating that any possible weak-instrument bias is well below 10% of the estimated effect size. Furthermore, the instrument is highly relevant, as the first-stage coefficients are very precisely estimated. The first-stage coefficient has the expected sign and is robust across all specifications. Fig. B.1 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 Fig. B.1 show that the 2SLS estimation is not driven by the presence of outliers, while panels B and D show that the 2SLS relationship is also not determined by the capital city Vienna.

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 was removed because Austria shares very strong trade-linkages with the German economy. The final sample of countries in the SBS used to construct $\%\Delta Employment_{it}^{OtherCountries}$ consists of Belgium, Czechia, Finland, France, Hungary, Italy, the Netherlands, Norway, Portugal, Spain and Sweden. Since the SBS-data changes the used industry classification in 2008, the periods 1995–1999, 1999–2002, 2002–2006 and 2006–2008 use the NACE Rev. 1.1 industry classification, while the periods 2008–2013, 2013–2017 and 2017–2019 use the NACE Rev. 2 classification.

[9] As is shown in Borusyak et al. (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.

[10] In related work Goldsmith-Pinkham et al. (2020) have shown that the exogeneity of exposure shares (i.e., the lagged employment shares in Eq. (2)) 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. See also Borusyak et al. (2024) for a detailed comparison of the exogenous shocks and exogenous shares conditions in shift-share settings.

Table 1
Manufacturing employment and Far-Right voting (1995–2019).

	Dependent variable: %Δ Vote-Share Far-Right parties					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: OLS estimations:						
%Δ Manufacturing Emp.	−0.117	−0.111	−0.080	−0.085	−0.070	−0.036
	(0.078)	(0.061)*	(0.058)	(0.049)*	(0.043)	(0.051)
Panel B: 2SLS estimations:						
%Δ Manufacturing Emp.	−0.741	−0.782	−0.706	−0.890	−0.868	−1.163
	(0.240)***	(0.332)**	(0.322)**	(0.334)***	(0.341)**	(0.404)***
	[0.129]***	[0.056]***	[0.054]***	[0.061]***	[0.065]***	[0.063]***
Panel C: First-Stage estimations:						
$Bartik^{IV}$	0.112	0.123	0.109	0.096	0.094	0.109
	(0.016)***	(0.024)***	(0.017)***	(0.015)***	(0.016)***	(0.011)***
	[0.005]***	[0.004]***	[0.004]***	[0.004]***	[0.004]***	[0.005]***
Kleibergen–Paap rk Wald F-Statistic	51.83	26.98	39.02	43.91	35.66	92.22
Stock–Yogo critical value (10% max. Bias)	16.38	16.38	16.38	16.38	16.38	16.38
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	158	158	158	158	158	158
Periods	7	7	7	7	7	7
Observations	1106	1106	1106	1106	1106	1106

Notes: * <0.10, ** <0.05, *** <0.01. Conventional robust standard errors clustered at the level of the nine Austrian federal states are reported in round brackets. Industry structure clustered standard errors from Adao et al. (2019) are reported in square brackets. Since the OLS estimations in Table 1 do not include a shift-share variable (i.e., the Bartik-instrument), only conventional cluster robust standard errors are reported here. Units of observation are 158 clustered commuting zones. All specifications include a set of commuting zone 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 logarithm of the gross regional product (total and per-capita), the regional unemployment rates of natives and immigrants, as well as start-of-period party affiliation of the regional government. 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 changes) are included separately for three skill groups (high-, medium- and low-skilled migrants). Heteroskedasticity robust first-stage F-statistics from Kleibergen and Paap (2006) are reported alongside the critical value for a maximum weak-instrument bias of 10% from Stock and Yogo (2005). All estimations are weighted by the start-of-period native voting-age population.

As is shown in Fig. 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 confirmed by corresponding estimations in Table B.1 in the Appendix, which show that declines in manufacturing employment directly lead to increases in natives' unemployment rates. Additionally, declines in manufacturing employment also decreased native labor force participation. Hence, the manufacturing decline has contributed to an overall decline in labor market prospects of natives. Algan et al. (2017) have shown that declining labor market prospects of natives lead to increased political polarization. Hence, this appears to be a plausible mechanism through which the manufacturing decline has bolstered the far-right. It is important to note, however, that studies relying on individual micro-data to investigate individual political preferences, have shown that it is not necessarily the individual experience of job loss and unemployment that moves voters to the far-right, but rather the perception of overall labor market decline and increased labor market risk (Kurer, 2020; Abou-Chadi and Kurer, 2021) and disappointed labor market expectations (Kurer and Van Staalduinen, 2022). In related work, Cotofan et al. (2024) have shown that the experience of economic decline shapes natives' attitudes towards immigrants potentially for life, specifically fostering anti-immigrant political views. This suggests that the effect of the manufacturing decline on far-right voting, documented in Table 1, not only operates through the measurable decline in labor market conditions, but also through complex psychological effects on natives perception of their own labor market risk and immigration.

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 their position in the political spectrum (according to their average Right–Left Score from the Manifesto Project, see Merz et al., 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 1995–2019).[11] Lastly, column (7) shows the change in the share of non-voters.

[11] 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 24 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 'others' category who were part of the Austrian parliament at some point during 1995–2019 (and thus held some degree of political relevancy) are the liberal NEOS party (from 2013 onward), and the populist Team Stronach of billionaire Frank Stronach (part of the parliament between 2013 and 2017). The left-wing populist PILZ party (who was formed as a secession from the Green party and entered the Austrian

Table 2
Inter-Party dynamics (1995–2019).

	(1) Communists	(2) Social Democrats	(3) Greens	(4) Conservatives	(5) Far-right	(6) Other	(7) Non-voters
Avg. Manifesto Right–Left Score	−21.83	−18.31	−12.96	2.97	6.86		
%Δ Manufacturing Emp.:	0.014	0.308	−0.042	0.085	−0.166	−0.082	−0.119
	(0.006)**	(0.069)***	(0.062)	(0.047)*	(0.051)***	(0.027)***	(0.047)**
	[0.002]***	[0.020]***	[0.006]***	[0.010]***	[0.010]***	[0.013]***	[0.006]***
Kleibergen–Paap rk Wald F-Statistic	35.66	35.66	35.66	35.66	35.66	35.66	35.66
Stock–Yogo critical value (10% max. Bias)	16.38	16.38	16.38	16.38	16.38	16.38	16.38
Average share of electorate	0.516	24.289	6.462	22.572	16.336	3.949	25.736
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
Commuting zones	158	158	158	158	158	158	158
Periods	7	7	7	7	7	7	7
Observations	1106	1106	1106	1106	1106	1106	1106

Notes: * <0.10, ** <0.05, *** <0.01. Conventional robust standard errors clustered at the level of the nine Austrian federal states are reported in round brackets. Industry structure clustered standard errors from Adao et al. (2019) are reported in square brackets. Units of observation are 158 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 (6) summarizes all parties that did not consistently participate in the Austrian parliamentary elections during the sample period. The parties are sorted according to the average Right–Left score from the Manifesto Project (Merz et al., 2016) for the period 1995–2019, starting with the most left party (the Communist Party in column 1) to the far-right (in column 5). To be able to account for changes in 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 commuting zone 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 logarithm of the gross regional product (total and per-capita), the regional unemployment rates of natives and immigrants, as well as start-of-period party affiliation of the regional government. 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) are included separately for three skill groups (high-, medium- and low-skilled migrants). Since the controls for the change in the migrant-share could be regarded as an outcome of the manufacturing decline themselves, and are therefore to be regarded as 'bad controls', they are removed from the estimation. Heteroskedasticity robust first-stage F-statistics from Kleibergen and Paap (2006) are reported alongside the critical value for a maximum weak-instrument bias of 10% from Stock and Yogo (2005). All estimations are weighted by the start-of-period native voting-age population.

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 changes in vote participation (via the inclusion of the percentage point change in non-voting). This ensures that the estimates in Table 2 add up to zero, and allows an interpretation of the results as shifts between parties, while also considering possible effects on vote participation. It is, however, important to stress that the results in Table 2 cannot be interpreted as voter-flows between parties, because the changes in aggregate vote-shares do not carry information on individual decisions to move from party A to party B, or about selection into vote participation (see Cohen et al., 2024). For example, it is conceivable that the manufacturing shock has prompted voters to move to the party directly to the right of the party they were supporting in the previous election. In such a scenario, one would observe a decrease in vote-shares at the far-left of the political spectrum, coinciding with an increase for the far-right. This shift would happen, without any individual voter having directly moved from the far-left to the far-right. These results, therefore, can only be interpreted as the effect of the manufacturing decline on the party-spectrum as a whole, but not on any kind of directed flows.

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 and, to a lesser extent, also the conservative Austrian People's Party (ÖVP). The combined losses of those two parties, which for the majority of the sample period formed a government coalition, are roughly twice as large as the gains of the far-right. The remainder of their loss is explained by an increase in voting for small fringe parties (column 6) and also an increase in non-voting (column 7). These small fringe parties in column 6 (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. They thus appeal to similar sentiments as the far-right, and are likely to benefit from dissatisfaction in the wake of labor market decline. The same is plausible for the estimated increase in non-voting. Hence, the estimated pattern in Table 2 hints at an increasing effect of the manufacturing decline on dissatisfaction among voters (a phenomenon that Algan et al., 2017 call a 'trust crisis'), which consequently pushes many voters towards non-mainstream political parties, or out of participation.

While it cannot be inferred with certainty that lost voters of the Social Democrats and the Conservatives directly moved to the far-right, it is clear (from regarding only those parties that were consistently part of the Austrian parliament in columns 2 to 5 of Table 2) that the manufacturing decline has contributed to a pronounced rightward shift in the Austrian parliament. This shift has swung the composition of the Austrian parliament away from the established Social Democrat and Conservative parties and towards far-right populism.

Of all the parties in Table 2 that consistently took part in every election since 1995, the Communist Party (column 1) is the only one who was never 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

parliament briefly in the 2017 election but dropped out in 2019) is aggregated with the Green party. Since the Manifesto Project does not report Right–Left scores for the majority of small fringe parties, no average R/L-score can be computed for the residual category. The parties summarized in this category are however very heterogeneous and are positioned all across the political spectrum.

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. The estimation result for the Communist Party in column (1) of Table 2, however, do not support vote gains at both ends of the political spectrum. Rather the Communist Party appears to have slightly suffered from the manufacturing decline.

5. The role of trade & technology

It is a well established finding 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 Bloc (see Autor et al., 2013 or Dauth et al., 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 the following equation:

$$\%\Delta Voteshare_{rt} = \gamma_1 \Delta\textit{Net-Imports}_{rt} + \gamma_2 \Delta Robots_{rt} + C'_{rt}\beta + \rho_r + \tau_t + \epsilon_{rt} \quad (3)$$

Here, the change in far-right vote-shares in commuting zone r is regressed on regional shift-share measures of net-import- and robot-exposure:

$$\Delta\textit{Net-Imports}_{rt} = \sum_i \frac{Emp_{irt}}{Emp_{rt}} \times \frac{\Delta\textit{Net-Imports}_{it}}{Emp_{it}} \quad (4)$$

$$\Delta Robots_{rt} = \sum_i \frac{Emp_{irt}}{Emp_{rt}} \times \frac{\Delta Robots_{it}}{Emp_{it}} \quad (5)$$

The intuition behind Eqs. (4) and (5) is that any commuting zone r is exposed to industry wide trends in trade and technology depending on its regional industry structure (measured by regional industry employment shares). Hence, Eqs. (4) and (5) use regional industry-employment shares to project industry level changes in trade- and robot-exposure onto the regional level. The corresponding shift-share variables then measure a commuting zone r's regional exposure to these industry wide shocks.

Measuring regional exposure to trade shocks as outlined in Eq. (4) was pioneered in the seminal contribution of Autor et al. (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 et al. (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 Bloc 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 (4), thus, follows the approach of Dauth et al. (2014) and uses changes in net-import exposure from both China and the former Eastern Bloc. Trade exposure is measured as the trade value in thousand-Euros per worker (in 2019 values).

The robotization measure in Eq. (5) 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 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 Eq. (5) is measured as the change in the number of installed robots per 1000 workers.

Since the net-import measure in Eq. (4) and the robotization measure in Eq. (5) are measured in different units, estimates using these expressions directly as explanatory variables are very difficult to compare in terms of the magnitudes of the estimated effects. To facilitate comparison, both measures are, therefore, standardized to have zero mean and unit standard deviation. The estimates can thus be interpreted as the effects of a one-standard deviation increase in each respective measure.

As before, estimating Eq. (3) 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 laid out in Autor et al. (2013) and Acemoglu and Restrepo (2020) and instrument the shock measures with corresponding shift-share instruments:

$$\Delta\textit{Net-Imports}^{IV}_{rt} = \sum_i \frac{Emp_{irt-10}}{Emp_{rt-10}} \times \frac{\Delta\textit{Net-Imports}^{OtherCountries}_{it}}{Emp_{it-10}} \quad (6)$$

$$\Delta Robots^{IV}_{rt} = \sum_i \frac{Emp_{irt-10}}{Emp_{rt-10}} \times \frac{\Delta Robots^{OtherCountries}_{it}}{Emp_{it-10}} \quad (7)$$

Like the Bartik-instrument from Eq. (2) these instrumental variables use variation in trade- or robot-exposure from other high income countries and project them to commuting zone r via the 10-year lagged industry structure. As is discussed in detail in Borusyak et al. (2022), the validity of the instrumental variables in expressions (6) and (7) hinges on the exogeneity of the shocks $\Delta\textit{Net-Imports}^{OtherCountries}_{it}$ and $\Delta Robots^{OtherCountries}_{it}$ used to construct the instruments. Hence the 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.[12] 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 Eqs. (4) and (5)) and the instrumental variables (in Eqs. (6) and (7)) generally do not sum to one, such that $\sum_i \frac{Emp_{ir}}{Emp_r} < 1$. As is emphasized in Borusyak et al. (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 Eq. (3) refer to the interaction of conventional period dummies with the sum of incomplete exposure shares.

[12] 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 Eqs. (6) and (7) are calculated strictly from countries outside of the European Monetary Union. For the Net-Imports instrument these countries are Australia, Canada, Japan, New Zealand, Norway, Sweden and the United Kingdom. Because the IFR-data covers much less countries, the country selection for the robotization shock is somewhat limited by data availability. Hence the robotization-shock $\Delta Robots^{OtherCountries}_{it}$ 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 3-digit level for the trade shocks and (roughly) the 2-digit level for the robotization shocks, according to the NACE Rev. 2 classification.

Table 3
Trade, Technology and Far-Right voting (1995–2019).

	1995–2019	2002–2019			
	(1)	(2)	(3)	(4)	(5)
Panel A: Manufacturing employment:					
Δ Net-Imports	−1.397	−2.913		−2.599	−2.129
	(1.354)	(1.654)*		(1.630)	(1.525)
	[0.639]**	[0.785]***		[1.011]**	[1.022]**
Δ Robots			−2.980	−2.246	−2.265
			(1.496)**	(1.712)	(1.731)
			[0.135]***	[0.221]***	[0.221]***
Panel B: Far-Right voting:					
Δ Net-Imports	1.505	2.235		2.269	2.993
	(2.339)	(1.510)		(1.892)	(1.809)*
	[0.579]***	[0.820]***		[0.893]**	[1.104]***
Δ Robots			1.240	1.144	1.177
			(1.351)	(1.975)	(2.036)
			[0.130]***	[0.181]***	[0.214]***
Kleibergen–Paap rk Wald F-Statistic: Δ Net-Imports	88.94	119.12		84.90	105.77
Kleibergen–Paap rk Wald F-Statistic: Δ Robots			118.02	175.55	155.60
Stock–Yogo Critical Value (10% max. Bias)	16.38	16.38	16.38	7.03	7.03
Period fixed effects	x	x	x	x	x
Commuting zone fixed effects	x	x	x	x	x
Industry structure	x	x	x	x	x
Regional characteristics	x	x	x	x	x
Demographic characteristics	x	x	x	x	x
Lagged employment changes	x	x	x	x	x
Migrant shares (by skill groups)	x	x	x	x	x
Δ Migrant shares					x
Δ ICT					x
Commuting zones	158	158	158	158	158
Periods	7	5	5	5	5
Observations	1106	790	790	790	790

Notes: * <0.10, ** <0.05, *** <0.01. Conventional robust standard errors clustered at the level of the nine Austrian federal states are reported in round brackets. Industry structure clustered standard errors from Adao et al. (2019) are reported in square brackets. Units of observation are 158 clustered commuting zones. The set of control variables includes a full set of commuting zone and period fixed effects. Since the exposure shares used to construct the trade- and robot-exposure instruments 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 et al., 2022). Industry structure controls include the 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 logarithm of the gross regional product (total and per-capita), the regional unemployment rates of natives and immigrants, as well as start-of-period party affiliation of the regional government. 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 changes) are included separately for three skill groups (high-, medium- and low-skilled migrants). Additionally the change in ICT-capital-exposure is included to control for other types of technological advances. Heteroskedasticity robust first-stage F-statistics from Kleibergen and Paap (2006) are reported alongside the critical values for a maximum weak-instrument bias of 10% from Stock and Yogo (2005). In columns 1–3 these critical values refer to a just-identified model with one endogenous variable, while in columns 4 and 5 they refer to a just-identified model with two endogenous variables. All estimations are weighted by the start-of-period native voting-age population.

5.1. Results

Before turning to the results for the estimated effects of trade and robotization on far-right voting, Fig. A.2 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 Fig. A.2 shows that the volume of both imports from and exports to China and the former Eastern Bloc 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. The evolution of robot density in panel (c) of Fig. A.2 shows that the number of industrial robots (per 1000 workers) has also drastically increased since 1995.[13] Here, the number of industrial robots at the end of the observational period is almost five times as large as in 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 smoothly throughout 1995–2019.

To examine the labor market impact of the drastic increase in trade- and robot-exposure, Panel A of Table 3 presents estimations for the corresponding employment effects. Here, both increases in exposure to net-imports and robotization lead to declines in manufacturing employment. 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, Panel B of Table 3 presents the results for the trade- and technology effects on far-right vote-shares. Here column (1) presents the results for the period 1995–2019. Since industry level robotization data is not available for the years 1995–2002, this estimation can only regard the effect of net-import exposure. This estimation in column (1) indicates a positive effect, which is robust to restricting the time window to 2002–2019 in column (2) and also for simultaneously including the change in robotization into the estimation (column 4) and for controlling for the simultaneously occuring changes

[13] While the IFR data includes country level data since 1993, a breakdown by industry for Austria is only available from 2003 onward.

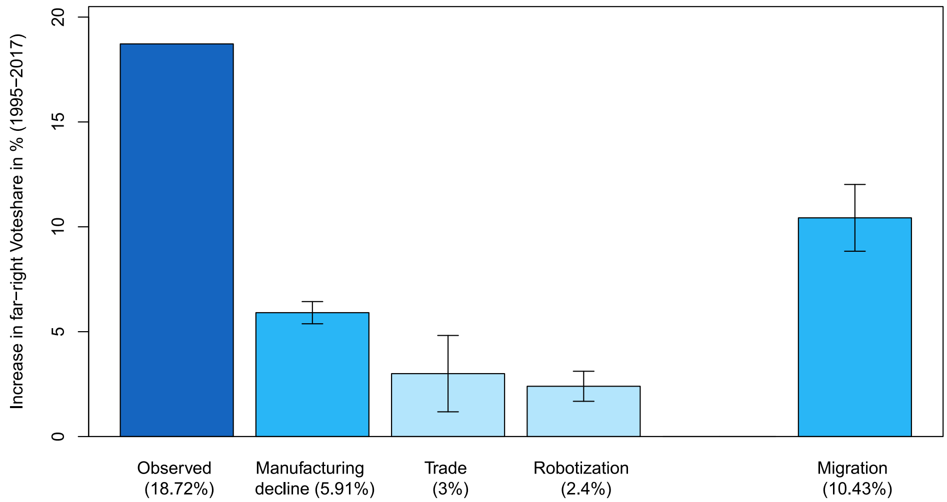

Fig. 2. Benchmarking effect size (1995–2017): *Notes:* The contribution of the decline in manufacturing employment is calculated using the estimated effect of manufacturing employment on the far-right vote-share from Table 1 (panel B, column (6) and multiplying it by the observed percentage-change in manufacturing employment. Similarly, the contributions of trade- and robot-exposure are calculated by multiplying the estimated coefficients from a non-standardized version of the estimations in Table 3, column (5) and multiplying them by the observed change in net-imports per worker and robots per 1000 workers respectively (Fig. A.2). The contribution of migration to the increase in the far-right vote-share is calculated using the estimated elasticity of the far-right vote-share with respect to the migrant-share for Austrian municipalities from Halla et al. (2017) (Table 8, column (2) and multiplying it by observed increases in the migrant share from the Austrian census data (1991–2011) and the Austrian Labor Market Statistics (2008 onward).

in skill specific migrant shares and exposure to information- and communication technologies (column 5). Regarding the effect of changes in robotization on voting behavior a similar picture arises. Overall, the full specification in column (5) indicates that both trade and technology had a robust increasing effect on far-right-voting.

6. Benchmarking effect sizes

While the results discussed so far show that the decline in manufacturing employment and its underlying forces (trade and automation technologies) have led to increases in far-right voting, the relative magnitudes of the estimated effects remain somewhat elusive. To get a concrete picture of the relative importance of the effect of the manufacturing decline on 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, Fig. 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.[14] For this, the estimated effects from Tables 1 and 3 are multiplied by the observed changes in manufacturing employment, net-import exposure and robot density.[15]

Additionally, Fig. 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 et al. (2017).[16] Since the estimated effect of changes in manufacturing employment in Table 1 controls for changes in the migrant-share, while the estimate from Halla et al. (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 Fig. 2 shows that the vote-share of the far-right has increased by 18.72% between 1995 and 2017. Roughly 5.9 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 than the contribution of the manufacturing decline. Together the manufacturing decline and the increase in immigration appear to almost fully explain the observed increase in far-right voting.

While Fig. 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, Fig. 2 shows that both factors have made comparable contributions.

7. Robustness checks

This Section presents plausibility checks for the used instrumental variables as well as other robustness tests. For the plausibility checks

[14] While the primary analysis is performed on the time-frame 1995–2019, I restrict the benchmarking to the period 1995–2017. This is motivated by the drastic decline of the far-right's vote-share in the 2019 election, which had no systemic reasons, but was rather caused by a large scale corruption scandal in the far-right's leadership known as the 'Ibiza-scandal'.

[15] For the effect of manufacturing employment, the 2SLS-estimate from Table 1 (Panel B, column 6) is multiplied by the observed %-change in the manufacturing share from Fig. 1. For net-import exposure the 2SLS-estimate from an estimation analogous to column (5) of Table 3 (using a non-standardized net-import measure) is multiplied by the observed Euro-per-worker change in net-import exposure during 1995–2017. The same is done for robotization, where a non-standardized version of the 2SLS-estimate in column (5) of Table 3 is multiplied by 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 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 Fig. A.2 in Appendix A.

[16] The estimate for this elasticity is 0.097 (see Table 8, column 2 in Halla et al., 2017). As before this elasticity is multiplied by 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 2008 onward). Migrant-shares for the missing years between census-years are linearly interpolated.

I follow the recommendations in Borusyak et al. (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- 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 et al. (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 checks whether the results presented in Tables 1 to 3 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 Fig. 2). Table 4 presents the results of separate pre-trend tests for each of the used instrumental variables.

To follow the recommendations in Borusyak et al. (2022) column (1) of Table 4 presents 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 all three instruments. 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 net-import and robotization instruments. Since the estimated effects in Tables 1 to 3 point into the opposite direction, they are unlikely to be caused by the significant pre-trends in column (1) of Table 4. If anything, these pre-trends would cause an attenuation of the estimated effects.

To assess, whether these unconditional pre-trends persist when conditioning on all available controls, column (2) of Table 4 repeats the pre-trend test, including all available control variables in the estimation.[17] Doing this leads to a sharp drop in the size of the point estimates and renders the coefficients statistically insignificant in both standard error definitions.

In sum, Table 4 shows that, at least conditional on the observed control variables, the instrumental variables do not pick up on pre-existing trends in far-right voting. The results presented in Tables 1 to 3 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 several industry level balance variables are regressed directly on the industry level shocks. 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 labor share, 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 B.2 in the Appendix. As is recommended in Borusyak et al. (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.[18] 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.

Table 4
Pre-trend tests for instrumental variables:

	1986 – 1995		N
	(1)	(2)	(3)
$Bartik^{IV}$	0.585	0.029	
	(0.103)***	(0.044)	
	[1.347]	[0.047]	1106
$\Delta Net\text{-}Imports^{IV}$	−0.287	−0.002	
	(0.096)***	(0.051)	
	[0.616]	[0.034]	1106
$\Delta Robots^{IV}$	−0.103	0.019	
	(0.054)*	(0.014)	
	[0.422]	[0.012]	790
Period fixed effects	x	x	
Full controls		x	

Notes: * <0.10, ** <0.05, *** <0.01. Conventional robust standard errors clustered at the level of the nine Austrian federal states are reported in round brackets. Industry structure clustered standard errors from Adao et al. (2019) are reported in square brackets. Units of observation are 158 clustered commuting zones. Estimations in column (1) include only period fixed effects. Estimations in column (2) additionally control for the remaining set of available controls used in the full specifications of Tables 1 and 3. Since the exposure shares used to construct the trade- and robot-exposure instruments are incomplete (i.e. do not sum to one), the period fixed effects in these estimations are interacted with the sum of incomplete exposure shares (as is recommended in Borusyak et al., 2022). As the pre-period changes in far-right voting do not vary between panel periods, fixed effects for the commuting zone cannot be included. Instead regional fixed effects are included at the more aggregated level of the nine Austrian federal states. All estimations are weighted by the start-of-period native voting-age population.

Regional balance tests

Another way to assess the plausibility of instrument exogeneity, which is proposed both by Borusyak et al. (2022) and Goldsmith-Pinkham et al. (2020), is to regress several regional characteristics directly on the instrumental variable to check if they are correlated with 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 B.3 in the Appendix. These tests check for balance with respect to the structure of the local workforce by including the age and skill level, the share of immigrants, the share living in urban areas, the share of blue collar workers, as well as other indicators for the economic conditions in a commuting zone like the regional unemployment rate and the gross regional product. 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.

Column (1) shows the results of the balance test for the Bartik-instrument. Overall, the instrument shows imbalance with respect to

[17] Since the pre-period changes in far-right voting do not vary between panel periods, fixed effects for the commuting zone cannot be included. Instead these fixed effects are included at the more aggregated level of the nine Austrian federal states.

[18] 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.

the age and skill composition and importantly also the migrant share and the unemployment rate of migrants. These imbalances are potentially concerning as the instrument does not appear to be fully orthogonal to those variables. It is, however, noted in Borusyak et al. (2022), that even precisely measured imbalance does not imply serious bias. To assess this they recommend to gradually include these variables as controls into the primary estimations to check how sensitive the estimations are to omitting them. Looking at the main estimation results in Table 1 shows that neither the inclusion of the unemployment rate of immigrants (column 2) nor the native age and skill distribution (column 3) or the migrant share (column 5) cause pronounced movements in the estimated effect size. This suggests that the imbalances in the Bartik-instrument found in Table B.3 are rather minor and do not cause serious bias in the estimations.

7.2. Further robustness checks

Fixed exposure shares

The instrumental variables in Eqs. (2), (6) and (7) 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 10 years for each panel period. Following the arguments laid out in Borusyak et al. (2022), updating the exposure shares is valid in this application, since the estimations in Eqs. (1) and (3) 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 B.4 in the Appendix shows estimation results when the exposure shares are fixed at 10 years before the start of the first panel period. Overall, these estimations confirm the results from the baseline specification with updated exposure shares presented during the main part of this paper.

Changes in voter turnout

Since employment shocks in the manufacturing sector have a separate impact on voter turnout (see Table 2), 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 B.4 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. Column (3) of Table B.4 shows that all results are robust to this alternative specification of the dependent variable, and thus do not reflect effects on voter turnout.

Ecological inference

A well established literature in political science has shown that inference about voting behavior drawn from aggregated units (like the commuting zones used in this paper) can lead to erroneous inference (Russo, 2017). To address this issue, Russo and Beauguitte (2014) recommend performing the analysis at the most disaggregated level possible, which in the Austrian context would be the municipality. However, the type of labor market shocks under consideration here do not only affect municipalities, but rather a set of municipalities forming a local labor market (connected by commuting flows of workers). To nevertheless be able to address this problem, I estimate a modified version of Eq. (1), where I allow the dependent variable (i.e., the vote share of the far-right) to vary at the most disaggregated level possible (i.e., the municipality level), while the manufacturing shock varies at the level of the commuting zone. As shown in column (4) of Table B.4, all previous results are robust to this alternative specification.

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.[19] 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 (5) and (6) of Table B.4. Column (5) 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 (6) additionally controls for changes in the skill-composition of natives. 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.

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 in manufacturing employment is arguably the most important structural change, that has affected labor demand 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 2019. During this time period, the manufacturing decline explains roughly one third of the observed increase in far-right voting. 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 et al., 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 will also contribute to more stability in the political system.

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[19] See for example, Blanchard and Katz (1992), Bound and Holzer (2000), Foote et al. (2019), Huttunen et al. (2018), Greenland et al. (2019) or Faber et al. (2021) for international evidence, and Bekhtiar (2022) for results for the Austrian case.

Declaration of competing interest

The author declares that he does have no known competing financial interest or personal relationship that could have appeared to influence the work reported in this paper.

Appendix A. Additional descriptives

See Figs. A.1 and A.2 and Table A.1.

Appendix B. Additional results

See Fig. B.1 and Tables B.1–B.4.

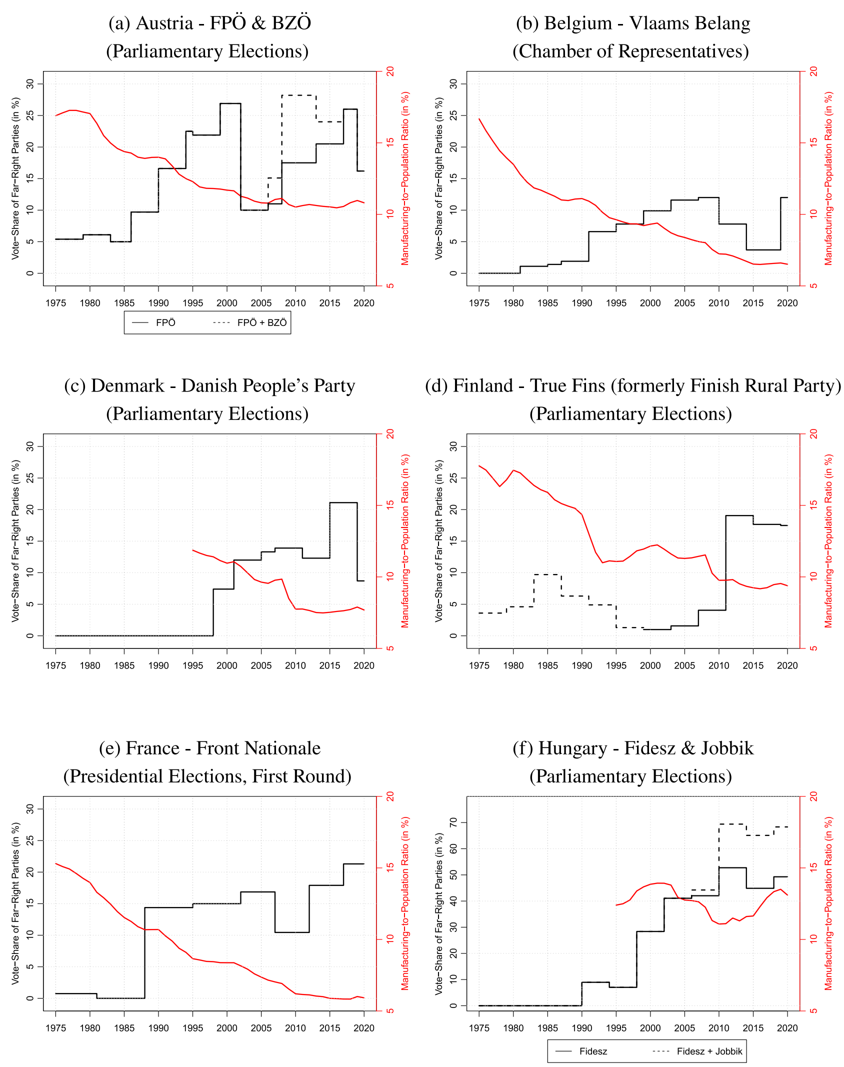
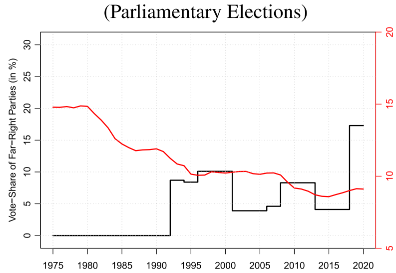
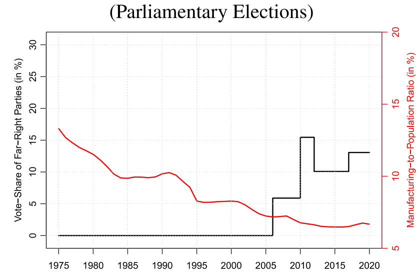


Fig. A.1. Far-Right voting and the decline of manufacturing employment across europe. *Notes:* Manufacturing employment-to-population ratios are calculated from EU-KLEMS data (Releases: March 2007 & February 2023) and OECD population data. Since the EU-Klems data is not available for Switzerland, data on the manufacturing share from the World Bank's World Development Indicators is used instead. Vote-shares of right-wing 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 parties.

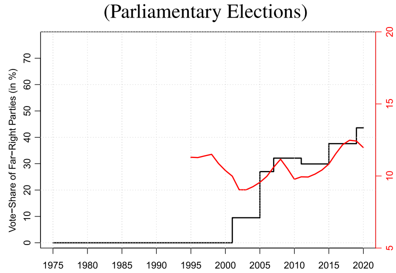
(g) Italy - Lega Nord
(Parliamentary Elections)



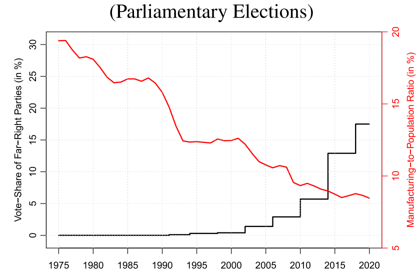
(h) Netherlands - Partij voor de Vrijheid
(Parliamentary Elections)



(i) Poland - PiS
(Parliamentary Elections)



(j) Sweden - Sweden Democrats
(Parliamentary Elections)



(k) Switzerland - Swiss People's Party (SVP)
(Parliamentary Elections)

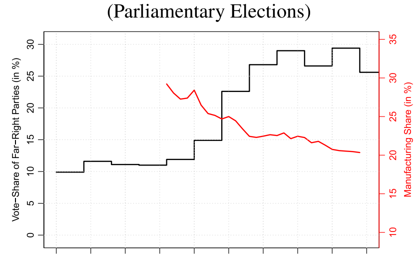


Fig. A.1. (*continued*).

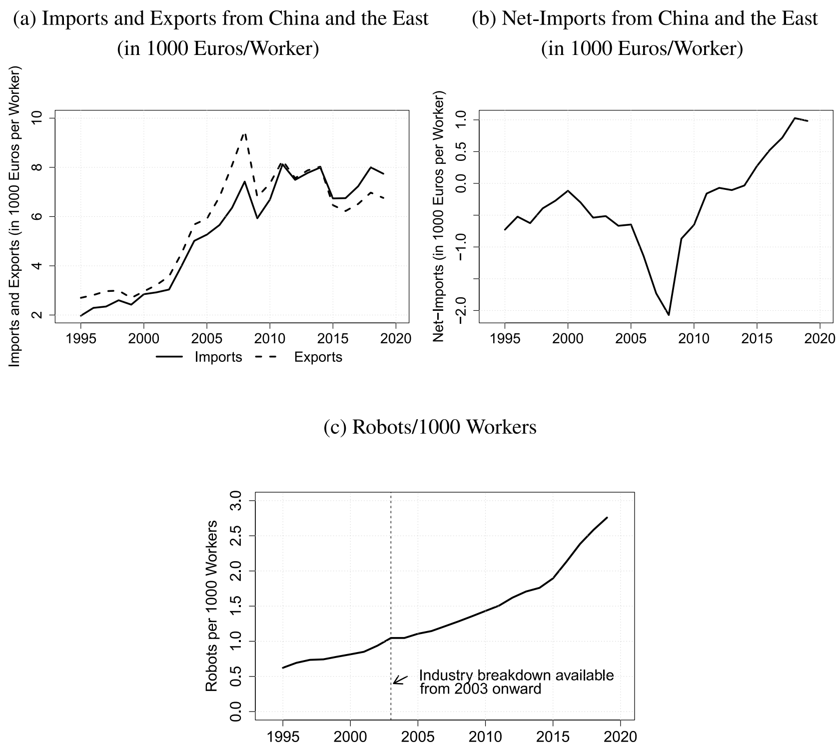


Fig. A.2. Changes in exposure to net-imports and industrial robots (1995–2019). *Source:* UN-Comtrade database and International Federation of Robotics (IFR), own calculations.

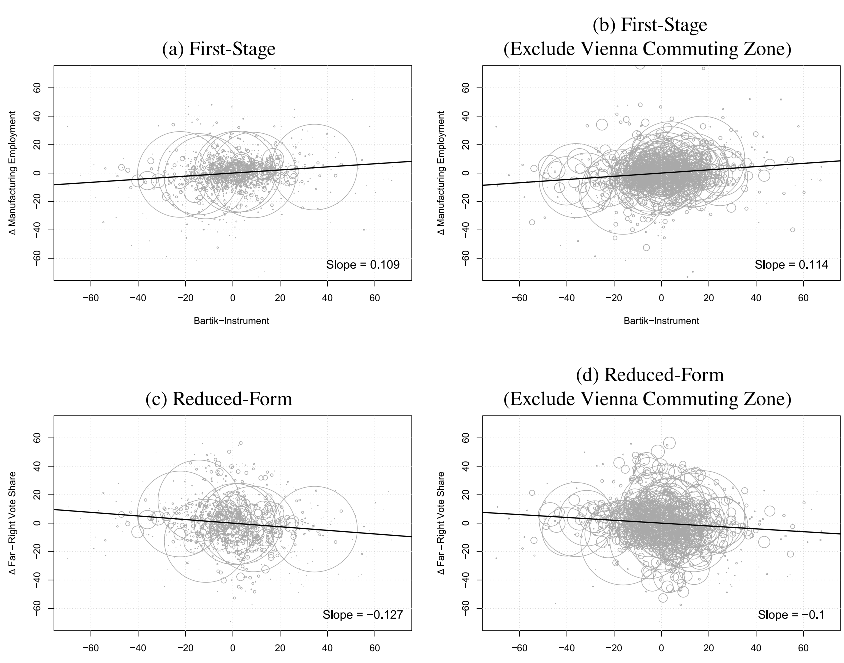


Fig. B.1. First-stage and reduced-form relationships (Bartik-Instrument). *Notes:* This Figure shows the first-stage and reduced-form relationships for the Bartik-instrument from the full specification in column (6) of Table 1. All available control variables as well as period and commuting zone fixed 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).

Table A.1
Characteristics of voters of Far-Right parties in the European Social Survey (ESS; 2002–2018).

Country	% Manuf.		% Blue Collar		% Unemp. (in last 5 years)		% Low skill		% Male		N		
	Right (1)	Other (2)	Right (3)	Other (4)	Right (5)	Other (6)	Right (7)	Other (8)	Right (9)	Other (10)	Right (11)	Other (12)	Parties (13)
Panel A: Period 2002–2006													
Austria	0.200	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	0.110	0.517	0.396	0.586	0.470	351	4089	Vlaams Block, FN
Denmark	0.209	0.168	0.512	0.313	0.170	0.111	0.370	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	0.440	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	17 801	
ESS Wave 2 (2004)	0.220	0.187	0.356	0.303	0.120	0.121	0.447	0.358	0.546	0.483	1353	16 458	
ESS Wave 3 (2006)	0.228	0.175	0.447	0.286	0.161	0.108	0.280	0.296	0.507	0.482	1517	15 860	
Pooled (2002–2006)	0.213	0.182	0.394	0.301	0.141	0.115	0.362	0.346	0.529	0.482	4196	50 119	
Panel B: Period 2014–2018													
Austria	0.170	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	0.220	0.188	0.681	0.454	515	3939	True Fins
France	0.177	0.156	0.425	0.239	0.132	0.100	0.285	0.257	0.530	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	0.490	0.454	1882	1547	Fidesz, Jobbik
Italy	0.187	0.141	0.346	0.280	0.133	0.168	0.470	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	0.130	0.125	0.438	0.303	0.499	0.474	1249	1882	PiS, Kukiz'15
Sweden	0.145	0.116	0.430	0.190	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	0.130	0.081	0.391	0.247	0.524	0.484	261	4360	UKIP
ESS Wave 7 (2014)	0.213	0.160	0.450	0.245	0.144	0.093	0.344	0.232	0.526	0.483	2034	14 426	
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	14 167	
ESS Wave 9 (2018)	0.187	0.140	0.394	0.227	0.106	0.092	0.338	0.218	0.542	0.478	2674	15 596	
Pooled (2016–2018)	0.200	0.150	0.406	0.235	0.122	0.098	0.342	0.229	0.534	0.480	7211	44 189	

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 9 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 B.1
Manufacturing employment and Labor market conditions (1995–2019).

	Δ Native's unemployment rate (1)	Δ Native's inactivity rate (2)		
Panel A: Full sample				
%Δ Manufacturing Emp.	−0.064	−0.210	Commuting zones	158
	(0.021)***	(0.030)***	Periods	7
	[0.004]***	[0.007]***	Observations	1106
Kleibergen–Paap rk Wald F-Statistic	35.66	35.66		
Stock–Yogo critical value (10% max. Bias)	16.38	16.38		
Panel B: Remove periods with high imputation error (2002–2006 and 2006–2006)				
%Δ Manufacturing Emp.	−0.087	−0.198	Commuting Zones	158
	(0.027)***	(0.029)***	Periods	5
	[0.006]***	[0.009]***	Observations	790
Kleibergen–Paap rk Wald F-Statistic	29.53	29.53		
Stock–Yogo critical value (10% max. Bias)	16.38	16.38		
Period fixed effects	x	x		
Commuting zone fixed effects	x	x		
Regional characteristics	x	x		
Demographic characteristics	x	x		
Industry structure	x	x		
Lagged employment changes	x	x		
Migrant share (by skill groups)	x	x		

Notes: * <0.10, ** <0.05, *** <0.01. Conventional robust standard errors clustered at the level of the nine Austrian federal states are reported in round brackets. Industry structure clustered standard errors from Adao et al. (2019) are reported in square brackets. Units of observation are 158 clustered commuting zones. All specifications include a set of commuting zone 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 logarithm of the gross regional product (total and per-capita), the regional unemployment rates of natives and immigrants, as well as start-of-period party affiliation of the regional government. 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) are included separately for three skill groups (high-, medium- and low-skilled migrants). Since the controls for the change in the migrant-share could be regarded as an outcome of the manufacturing decline themselves, and are therefore to be regarded as 'bad controls', they are removed from the estimation. Heteroskedasticity robust first-stage F-statistics from Kleibergen and Paap (2006) are reported alongside the critical value for a maximum weak-instrument bias of 10% from Stock and Yogo (2005). All estimations are weighted by the start-of-period native voting-age population. Since the linear interpolation of the unemployment variable leads to a pronounced imputation error in 2006, Panel B presents estimations where all periods using this year are excluded. For more details on the linear interpolation of the unemployment variable (and further census based controls) see Online Appendix D.

Table B.2
Industry balance tests.

	$\%\Delta Employment^{OtherCountries}$		$\Delta Net\text{-}Imports^{OtherCountries}$	$\Delta Robots^{OtherCountries}$
	1995–2008 (1)	2008–2019 (2)	1995–2019 (3)	2002–2019 (4)
Start-of-Period ratio of old to middle aged workers	−1.637	2.410***	−0.460	−0.832
	(1.456)	(0.778)	(0.480)	(0.523)
Start-of-Period share of blue collar workers	0.039	−3.999	−0.170	−2.307
	(0.837)	(3.043)	(0.665)	(2.091)
Start-of-Period share of migrant workers	−0.637	−0.489	−0.453	−1.120
	(2.000)	(1.358)	(0.816)	(0.729)
Start-of-Period labor share	0.597	−0.076	0.602***	2.194
	(0.657)	(2.647)	(0.160)	(9.906)
Start-of-Period log(Labor productivity)	2.724	6.058	−1.466	−10.861
	(3.061)	(4.098)	(1.285)	(50.807)
Start-of-Period ICT-Capital/Capital stock	0.149	−0.012	0.035	0.107
	(0.426)	(0.083)	(0.046)	(0.399)
Start-of-Period log(Avg. hourly real wage)	3.735	2.976	−0.358	−14.507
	(2.520)	(2.702)	(1.287)	(63.225)
Classification	NACE Rev. 1.1 3-Digit	NACE Rev. 2 3-Digit	NACE Rev. 2 3-Digit	NACE Rev. 2 2-Digit
Industries	101	94	109	26
Periods	4	3	7	5
Industry-Period shocks	404	282	760	127

Notes: * <0.10, ** <0.05, *** <0.01. This Table shows industry level regressions of several start-of-period industry characteristics on the respective industry level shocks used to construct the instrumental variables in Eqs. (2), (6) and (7). 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 Eq. (2) 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 number of industries used in columns (2) and (3) differ because column (2) only regards manufacturing industries, while some industries outside of manufacturing also have non-zero net-import flows in column (3). 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 February 2023 Release 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 industries are assigned to their sub-industries whenever the shocks used to construct the instruments use 3-digit classifications. All regressions control for period fixed effects and are weighted by average industry exposure shares. The estimations for the net-import and robotization shocks are performed on an unbalanced panel of observations, because not all industries experienced a trade- or robotization-shock in each period. Robust standard errors clustered by 2-digit industries are reported in brackets.

Table B.3
Regional balance tests.

	Bartik^{IV} (1)	*ΔNet-Imports*^{IV} (2)	*ΔRobots*^{IV} (3)
Start-of-Period ratio of old to middle aged workers	−0.029	0.016	0.013
	(0.017)*	(0.030)	(0.009)
	[0.137]	[0.055]	[0.027]
Start-of-Period % of highly education population	0.008	0.001	0.001
	(0.003)**	(0.002)	(0.001)
	[0.006]	[0.004]	[0.001]
Start-of-Period % of medium education population	−0.019	0.010	0.002
	(0.002)***	(0.006)	(0.001)
	[0.015]	[0.010]	[0.007]
Start-of-Period % of low education population	0.011	−0.002	−0.004
	(0.003)***	(0.006)	(0.002)**
	[0.010]	[0.007]	[0.005]
Start-of-Period % of foreign born population	0.024	−0.003	−0.001
	(0.006)***	(0.003)	(0.002)
	[0.045]	[0.013]	[0.002]
Start-of-Period % living in urban areas	0.006	0.023	0.008
	(0.019)	(0.011)**	(0.013)
	[0.128]	[0.057]	[0.009]
Start-of-Period share of blue collar workers	0.006	0.003	−0.001
	(0.004)	(0.014)	(0.005)
	[0.016]	[0.011]	[0.003]
Start-of-Period unemployment rate (Natives)	0.018	0.004	0.004
	(0.011)	(0.007)	(0.004)
	[0.054]	[0.017]	[0.006]
Start-of-Period unemployment rate (Immigrants)	0.010	0.002	0.004
	(0.005)**	(0.002)	(0.002)
	[0.027]	[0.007]	[0.002]
Start-of-Period log(Gross regional product)	0.190	0.003	0.040
	(0.116)	(0.108)	(0.060)
	[1.186]	[0.356]	[0.032]
Commuting zones	158	158	158
Periods	7	7	5
Observations	1106	1106	790

Notes: * <0.10, ** <0.05, *** <0.01. Conventional robust standard errors clustered at the level of the nine Austrian federal states are reported in round brackets. Industry structure clustered standard errors from Adao et al. (2019) are reported in square brackets. Units of observation are 158 clustered commuting zones. All specifications include a full set of period effects, as well as controls for the regional industry structure. Since the exposure shares used to construct the trade- and robot-exposure instruments are incomplete (i.e., do not sum to one), the period fixed effects in these estimations (in columns 2 and 3) are interacted with the sum of incomplete exposure shares (as is recommended in Borusyak et al., 2022). All estimations are weighted by the start-of-period native voting-age population.

Table B.4
Additional robustness checks.

	Baseline (1)	Fixed exposure shares (2)	Changes in turnout (3)	Ecological inference (4)	Δ Population size (5)	Δ Skill composition (6)
Panel A: Changes in manufacturing employment						
%Δ Manufacturing employment	−1.163	−1.267	−1.170	−1.109	−1.213	−0.820
	(0.404)***	(0.404)***	(0.355)***	(0.460)**	(0.398)***	(0.355)**
	[0.063]***	[0.069]***	[0.062]***	[0.062]***	[0.063]***	[0.060]***
Kleibergen–Paap rk Wald F-Statistic	92.22	56.45	92.22	22.33	75.35	93.19
Stock–Yogo critical value (10% max. Bias)	16.38	16.38	16.38	16.38	16.38	16.38
Regions	158	158	158	2090	158	158
Periods	7	7	7	7	7	7
Observations	1106	1106	1106	14 630	1106	1106
Panel B: Changes in trade & Technology exposure						
Δ Net-Imports	2.993	2.383	2.960	3.210	2.983	3.080
	(1.809)*	(4.397)	(2.219)	(1.834)*	(1.840)	(1.850)*
	[1.104]***	[1.365]*	[1.120]***	[1.085]***	[1.103]***	[0.938]***
Kleibergen–Paap rk Wald F-Statistic	105.77	169.62	105.77	34.02	113.84	86.76
Stock–Yogo critical value (10% max. Bias)	7.03	7.03	7.03	7.03	7.03	7.03
Δ Robots	1.177	3.007	0.635	1.216	1.067	0.812
	(2.036)	(2.033)	(2.132)	(1.657)	(1.821)	(2.252)
	[0.214]***	[0.283]***	[0.199]***	[0.214]***	[0.209]***	[0.302]***
Kleibergen–Paap rk Wald F-Statistic	155.60	454.05	155.60	73.29	157.25	69.29
Stock–Yogo critical value (10% max. Bias)	7.03	7.03	7.03	7.03	7.03	7.03
Regions	158	158	158	2090	158	158
Periods	5	5	5	5	5	5
Observations	790	790	790	10 450	790	790

Notes: * <0.10, ** <0.05, *** <0.01. Conventional robust standard errors clustered at the level of the nine Austrian federal states are reported in round brackets. Industry structure clustered standard errors from Adao et al. (2019) are reported in square brackets. Units of observation are 158 clustered commuting zones, except in column (4) where municipalities are used. All specifications include a full set of controls corresponding to the controls used in the respective estimations in Tables 1 and 3. Heteroskedasticity robust first-stage F-statistics from Kleibergen and Paap (2006) are reported alongside the critical values for a maximum weak-instrument bias of 10% from Stock and Yogo (2005). In panel A these critical values refer to a just-identified model with one endogenous variable, while in panel B they refer to a just-identified model with two endogenous variables. All estimations are weighted by the start-of-period native voting-age population.

Appendix C. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jpubeco.2025.105315.

Data availability

Data will be made available on request.

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