

ARTICLE

Austerity, economic vulnerability, and populism

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Abstract

Governments have repeatedly adjusted fiscal policy in recent decades. We examine the political effects of these adjustments in Europe since the 1990s using both district-level election outcomes and individual-level voting data. We expect austerity to increase populist votes, but only among economically vulnerable voters, who are hit the hardest by austerity. We identify economically vulnerable regions as those with a high share of low-skilled workers, workers in manufacturing and in jobs with a high routine-task intensity. The analysis of district-level elections demonstrates that austerity increases support for populist parties in economically vulnerable regions, but has little effect in less vulnerable regions. The individual-level analysis confirms these findings. Our results suggest that the success of populist parties hinges on the government's failure to protect the losers of structural economic change. The economic origins of populism are thus not purely external; the populist backlash is triggered by internal factors, notably public policies.

Governments have regularly implemented fiscal adjustment measures in recent decades. Adjustment policies have strong distributional consequences, especially in contemporary, industrialized democracies. These countries have experienced major economic transformations, such as globalization or automation, that increase economic insecurity among voters. Public safety nets are crucial in this context because they provide insurance against enhanced economic risk and stabilize societies both socially and politically. By contrast, government decisions to cut fiscal spending magnify rather than mitigate the adverse effects of economic transformations. Therefore, government decisions are crucial during periods of structural economic change.

Economic explanations of populism in recent years have paid surprisingly little attention to governments and their policy choices. Prior studies have significantly improved our understanding of political

backlash by highlighting how economic outcomes—trade shocks, financial crises, and technological innovations—affect voters. This literature, however, has largely overlooked the role of governments. Yet, governments have traditionally been at the center of analyses of globalization politics (e.g., Mosley, 2003) and should play an important role in how we think about the economic origins of populism and political backlash. We therefore examine *how government policy, particularly fiscal austerity, affects voters' political behavior during periods of enhanced economic risk*.

Our analysis concentrates on the impact of fiscal austerity on economically vulnerable voters. Although fiscal cutbacks are generally national-level decisions that apply to the entire country, exposure to them varies across regions and societal groups. Cutbacks primarily affect economically vulnerable voters, who rely on government support to cope with increased economic risk. By contrast, voters who have sufficient resources to ride out economic downturns are less affected by public spending cuts. Austerity policies therefore cause disenchantment primarily among voters who face social decline because they are hit the

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hardest by fiscal cutbacks and infer from austerity that their well-being is not a priority of their government. As a result, vulnerable voters are increasingly swayed by populist pledges to rectify their economic situation either by reversing spending cuts or by curtailing globalization as the original source of economic risk.

To identify economically vulnerable voters, we draw on the international political economy literature, which has investigated the winners and losers from economic transformations for decades (e.g., Frieden, 2000; Milner, 1988). This research highlights the extent to which factor endowment (Scheve & Slaughter, 2001), sectoral competitiveness (Jensen et al., 2017), and occupational characteristics (Gingrich, 2019; Owen, 2020) affect income and job security. Following these different theoretical logics, low-skilled workers, workers in manufacturing, and those in routine jobs are particularly vulnerable and suffer the most from austerity. We therefore expect these workers to be more likely to support populist parties when the government adjusts fiscal policy.

Empirically, we examine how austerity has affected voting patterns in Western countries since the early 1990s using both district-level election outcomes and individual-level voting data. The results of our two-way fixed effects (TWFE) analysis illustrate that austerity increases support for populist parties in economically vulnerable regions, but has little effect on voting in less vulnerable regions. Moreover, we find that especially radical right parties gain votes in economically vulnerable regions where austerity measures have been implemented. Our individual-level analyses confirm these results. There are different possible mechanisms that lead to these results, for example, direct material effects versus government responsiveness concerns, and our paper does not adjudicate between these different mechanisms. It is plausible that these different aspects of austerity work together in ways that expand the appeal of populists.

We implement several additional tests to strengthen our identification strategy. First, we include lead variables of austerity, which capture anticipatory effects, and region-specific trends, which leave our main results unchanged. Second, we show that our results hold if we include our measures of economic vulnerability in interaction with election-year fixed effects. Third, exploiting the fact that European countries implement austerity measures even in good times, we show that our results are not driven by the occurrence of economic crises. Put differently, even when macroeconomic conditions are normal, economically vulnerable areas and individuals support populist parties where austerity measures have been implemented. While this additional evidence lends credibility to our findings, we acknowledge that the assumptions supporting our identification strategy are more demand-

ing than they would be in a case study with a single episode of austerity, which varies subnationally. We trade off stronger identification assumptions for a stronger external validity.

Our study makes three main contributions. First, it advances the literature on the backlash against globalization by moving public policy and governments to the center of the analysis. There is now a large body of evidence that economically vulnerable voters increasingly turn toward populist parties (Baccini & Weymouth, 2021; Ballard-Rosa et al., 2021; Colantone & Stanig, 2018; Gidron & Hall, 2017; Gingrich, 2019; Jensen et al., 2017; Milner, 2021; Owen & Johnston, 2017). In line with prior single-country studies (Fetzer, 2019; Wiedemann, 2024; Cremaschi et al., 2023), our results show that the success of populist parties across Europe critically hinges on governments' failure to protect and help the losers of structural economic change. The economic origins of populism are thus not purely external and unavoidable; the populist backlash is triggered by internal factors, notably public policies.

Second, we contribute to the literature on the political effects of fiscal policy by isolating the impact of fiscal cutbacks on different groups of voters. Although the political risks of austerity were previously pointed out (Blyth, 2013), empirical tests thus far highlighted the average response of the electorate to fiscal adjustments (Alesina et al., 2019; Arias & Stasavage, 2019; Bansak et al., 2021; Giger & Nelson, 2011; Grittersová et al., 2016; Talving, 2017) and how mainstream party economic convergence influences the choices of voters (Hübscher et al., 2023). To the extent that voter heterogeneity is examined, material explanations are dismissed in favor of ideological ones (Barnes & Hicks, 2018; Hübscher et al., 2021). To the best of our knowledge, our paper is the first to demonstrate that the economic vulnerability of voters strongly affects the intensity of their response to fiscal austerity, both regionally and individually. The political disruptions of austerity can therefore be significant even if the median voter or the majority of voters support an austerity package.

Finally, our analysis sheds new light on government accountability in open economies. It suggests that economic policy continues to influence popular evaluations even if voters hold governments less accountable for economic outcomes in open economies (Hellwig & Samuels, 2007; Kayser & Peress, 2012). While outcomes convey less information about policymaker competence in such economies, the policy response to these outcomes still signals the government's economic priorities to voters. Vulnerable voters infer from fiscal cutbacks that the government's policy position is incompatible with their needs and interests, and hold it accountable accordingly.

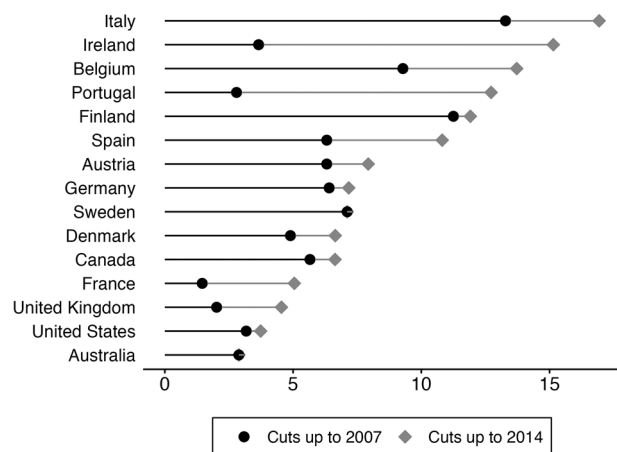


FIGURE 1 Spending cuts in industrialized countries, 1978–2014. *Note:* Total spending cuts announced by governments, as percent of GDP. The figure shows the cumulative amount, that is, the sum of all announced cuts, from 1978 until 2007 (black line) and until 2014 (black and gray lines). *Source:* Alesina et al. (2019) and Devries et al. (2011).

AUSTERITY AND THE ECONOMIC ORIGINS OF POPULISM

Fiscal adjustments in times of enhanced economic risk

We define fiscal austerity as a government decision to adjust fiscal policy to reduce the public deficit, that is, the difference between public expenditures and revenues. These decisions generally center on reducing government spending, for example, by cutting social security entitlements or public investment, but they can also entail tax increases, such as VAT or income taxes, to increase public revenues.¹ A prominent example is the wave of fiscal adjustments in the wake of the European debt crisis (Copelovitch et al., 2016). These recent cutbacks, however, are not unique and represent the peak of a longer lasting movement toward “permanent austerity” that has been noted for a long time (Pierson, 2001, ch. 13). As Figure 1 illustrates, most industrialized countries had implemented significant cutbacks long before the start of the global financial crisis in 2007.² The figure also shows that adjustments have been quite common throughout Europe, including Germany, Austria, and the Scandinavian countries.

Fiscal adjustments can affect a wide range of budgetary areas. We focus on the consistent and strong impact of austerity on policies that are important to

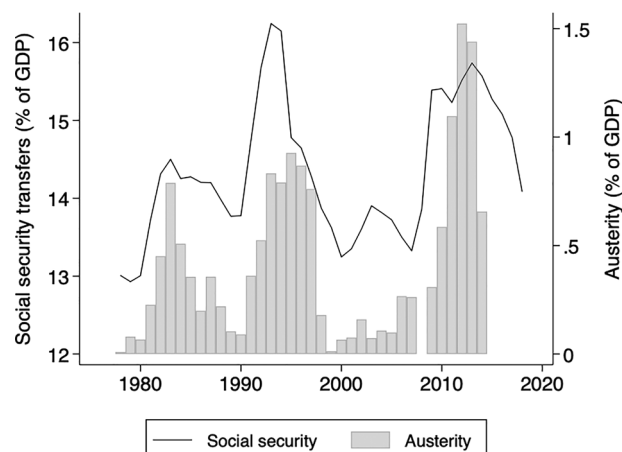


FIGURE 2 Austerity and social security transfers over time. *Note:* Bars show the average amount of deficit-reducing measures (as percent of GDP) in the countries listed in Figure 1. Black line shows average social security transfers (social assistance grants and welfare benefits) by general government (as percent of GDP) in the same countries. *Source:* Alesina et al. (2019), Armingeon et al. (2019), and Devries et al. (2011).

vulnerable voters, such as social transfers and other welfare state policies. Figure 2 shows that government transfers are central to austerity packages. Average social security transfers in industrialized countries vary considerably over time and declined particularly strongly during the 1990s and again from 2013 onwards. These declines in transfers coincide with the large waves of austerity that governments have implemented in recent decades (see also Armingeon et al., 2016). As the Online Appendix A shows on p.2, government transfers are cut more than any other budgetary category. We find a similar pattern for public spending on unemployment benefits, education, and pensions in Figure C1 in Online Appendix C on p. 7.³ Austerity, therefore, has been associated with cutbacks in public safety nets and other public schemes that are important for the welfare of economically vulnerable citizens in open economies.⁴

Given how fiscal adjustments affect core policies, these decisions are generally intensively debated as part of budgetary debates and in the public discourse. Opposition parties that expect to benefit politically by exploiting discontent with fiscal austerity tend to instigate such debates. This creates a public discourse that informs voters of the government's fiscal plans and how they will affect voters. Political saliency also increases with the size of the fiscal adjustment because

³ In this figure, we also present the same indicators adjusted for the percentage of entitled citizens, to rule out that this pattern is simply catching business cycle effects.

⁴ While total social expenditures have gradually increased over time, this is not the case for the spending items that are central to our argument. The increase in total social expenditures is mostly due to pensions and health care. It is politically very difficult to make cuts in these areas related to so-called life-cycle risks (Pierson, 2001). This increases the pressure to concentrate cuts in other social spending categories.

¹ We concentrate on policy choices rather than actual changes in public expenditures and revenues. The former can be directly attributed to the government, while the latter can also vary for other reasons, such as macroeconomic shocks, which are beyond the government's direct control.

² We discuss the measurement of austerity used in this figure in detail in Section “District-level elections”.

the impact on voters is more serious and therefore more contentious. This opposition to large austerity packages often manifests itself in the form of protests, which raise public awareness of the issue (Bremer et al., 2020). Our qualitative analysis of newspaper reports during major austerity episodes in Europe in the 1990s and 2000s, described in Online Appendix B on pp. 3–6, confirms this. These findings demonstrate that austerity measures were often at the center of the political debate, played an important role in the national media and in electoral campaigns, and that voters were aware of these issues, such as in France in 1997 or 2011/2012, in Italy in 1994/1995, and in Germany in 2004.

Fiscal adjustments are also politically salient because they take place in a context of increased social risk. Industrialized economies have experienced major transformations in recent decades, such as a massive increase in trade, offshoring, and the automation of jobs (Autor et al., 2013). In this context, public safety nets are important to stabilize countries socially and politically because they help voters cope with economic risk in open economies (Gingrich & Ansell, 2012; Kurer & Gallego, 2019; Walter, 2010). For instance, these policies increase support for openness (Hays, 2009; Rickard, 2015) and decrease the risk of political backlash (Halikiopoulou & Vlandas, 2016; Margalit, 2011; Richtie & You, 2020; Rudra, 2005; Vlandas & Halikiopoulou, 2022). Yet, austerity has distributional effects that operate in the same direction as these economic transformations. It magnifies rather than mitigates the negative economic effects of globalization and technological change and exacerbates the social decline of vulnerable individuals and communities (Sambanis et al., 2018).

Vulnerable voters and their political reactions

We draw on theories of comparative and international political economy to identify the economically vulnerable voters that are most affected by austerity. In short, economic vulnerability varies according to (1) skill level, (2) economic sector, and (3) routine job intensity. First, from a factor logic, low-skilled workers tend to be worse off in open, industrialized economies, while high-skilled laborers tend to benefit in such countries (Scheve & Slaughter, 2001). Second, from a sectoral logic, the manufacturing sector faces the greatest competition from firms in developing and emerging markets, while high-skilled service industries thrive in open economies (Jensen et al., 2017). Also, small firms find it harder to succeed in open economies, while large, productive firms are best positioned to exploit the gains from trade (Baccini et al., 2017). Finally, from an occupational logic, workers in

routine jobs are most likely to lose their jobs due to offshoring or automation (Gallego et al., 2022; Gingrich, 2019; Owen, 2020).

Austerity creates political disenchantment among these vulnerable voters because they are the most exposed to its material impacts (Wiedemann, 2024). They rely more heavily on social safety nets and public transfers than more privileged voters, and are directly affected by austerity measures. This particularly applies to large fiscal adjustments that do not allow governments to spare vulnerable voters or compensate them with parallel measures. Fiscal adjustments then raise doubts among vulnerable voters that governments are committed to make globalization a success for everybody, including the economically vulnerable. They infer that government parties are more responsive to economically more privileged voters who are less affected or potentially benefit from austerity than to vulnerable voters when fiscal trade-offs sharpen the divide between them (Bartels, 2008).⁵

Although we focus on voter characteristics (i.e., the demand side), political parties (i.e., the supply side) play an important role in our argument in two ways. First, as we discuss in greater detail in Online Appendix B on pp. 3–6, populist parties have increasingly positioned themselves against fiscal austerity. In sum, left-leaning populist parties have opposed austerity on average, although there was a tendency towards acceptance in the early 2000s. Right-leaning populist parties were more accepting of austerity in the past, but have become increasingly critical over the past two decades. This characterization is consistent with our qualitative analysis of major austerity periods in Europe, and is in line with recent findings that radical right parties oppose austerity measures proposed by government parties (Enggist & Pinggera, 2022). This, in turn, is consistent with new findings that populist parties quickly adapt to shifting voter opinions and emphasize new issues that allow them to challenge established parties (De Vries & Hobolt, 2020).⁶ Populist parties, therefore, allow vulnerable voters to express antigovernment sentiment.

Second, our argument requires that many vulnerable voters see a lack of alternatives among traditional, nonpopulist parties (Hübscher et al., 2023). This is plausible because a broad consensus among traditional (nonpopulist) political parties has supported the recent austerity waves (Blyth, 2013; Hopkin, 2020).⁷ Again, our qualitative analysis of traditional parties' positions on major austerity episodes in

⁵ This implies that vulnerable voters respond in similar ways in different types of welfare states. Our empirical analysis examines whether this is indeed the case.

⁶ Dissatisfied voters can also abstain to express their discontent, and may vote for populist parties at a later point in time. We empirically examine this possibility to the extent that our data allow.

⁷ There are multiple possible reasons for this tendency, including financial pressure (Barta & Johnston, 2018; Hallerberg & Wolff, 2008; Sattler, 2013),

European countries confirms this (see Online Appendix B on pp. 3–6). These results demonstrate that traditional political parties regularly supported austerity measures. If they opposed them, they did so while in opposition, but supported them as government parties in earlier or later austerity episodes. This gives voters fewer opportunities to sanction governments, for example, by voting for the nonpopulist opposition, especially when the proausterity consensus cuts across political camps. But despite the importance of supply-side politics, our point is that vulnerable voters are more likely than their better-off counterparts to support populist parties when they lack nonpopulist, antiausterity alternatives.

More generally, voters' responses can vary across contexts, countries, and time periods, depending on the existence of populist parties, nonpopulist parties' past involvement in implementing austerity measures, the type of austerity, the nature of welfare states, and economic conditions. We explore these various moderating factors in the empirical analysis, but our key goals are to identify which types of voters are affected most strongly and to examine how their reactions differ from those of nonvulnerable voters. We expect that economically vulnerable individuals, on average, are more likely than economically safe individuals to vote for populist parties after an austerity package is implemented. Similarly, populist vote share, on average, should increase more in economically vulnerable than in economically prosperous electoral districts after an austerity package is introduced.

DISTRICT-LEVEL ELECTIONS

The first part of our analysis examines district-level election results in 12 Western European countries and (up to) 195 NUTS-2 (Nomenclature of Territorial Units for Statistics, level 2) regions. Our time span covers (up to) 99 elections between 1986 and 2018. We focus on elections for the lower house of the legislature. Each country appears only in years in which elections are held. The data on party vote shares on the district level are from the Constituency-Level Elections Archive (CLEA) database (Kollman et al., 2019).

Data

Measuring populism

Our main outcome variable measures support for populist parties in an electoral district in a given election.

To compute this variable, we first match the CLEA data with the Global Party Survey's (Norris, 2019) classifications of political parties on an 11-point populism scale.⁸ This allows us to calculate a populism score for each district-election. This score is the weighted average of the populism scores of all parties in the district-election where parties are weighted by their vote shares. This variable theoretically ranges from 0 (pluralist parties receive all the votes) to 10 (populist parties receive all the votes). This measure varies across electoral districts and over time.⁹ We label this variable *Populism Score*. We also examine support for radical parties, many of which take a populist position. Using data from PopuList (Rooduijn et al., 2019), we evaluate the share of votes for radical left and right parties, which allows us to explore how austerity influences ideology.

Figures C2 and C3 in Online Appendix C on p. 8 display the distribution of our outcome variable across NUTS-2 regions and over time. The figures illustrate that half of the countries in the sample had already experienced a surge of votes for populist parties in the 1990s and not only during the past decade.

Measuring austerity

We measure austerity as the amount of deficit-reducing policies implemented by the government in a given time period (Alesina et al., 2019; Devries et al., 2011). This indicator is based on government policy documents (e.g., budgetary reports) and reports from international organizations, such as International Monetary Fund country reports, to identify the timing and magnitude of a fiscal adjustment package. It captures policy decisions to reduce public spending or increase taxes as announced by the government and recorded in these documents, and indicates by how many percentage points these policies are expected

⁸ In this data set, parties are classified according to a range of dimensions based on expert surveys. The conceptualization and operationalization of populism relies on Norris and Inglehart (2019), which treats populist rhetoric as antithetical to pluralist rhetoric. Populist language "typically challenges the legitimacy of established political institutions and emphasizes that the will of the people should prevail," while pluralist language "rejects these ideas, believing that elected leaders should govern constrained by minority rights, bargaining and compromise, as well as checks and balances on executive power" (Global Party Survey codebook, p. 10). Populist rhetoric is measured from 0 (less populist) to 10 (more populist).

⁹ The Global Party Survey's classification of parties is fixed since it is difficult to judge the degree of populist rhetoric in the more distant past using current expert surveys. Nonetheless, populism scores vary over time and across districts when party vote shares in a district change. Our measure thus captures the demand-side effects that arise when voters switch to a different political party, and rules out supply-side effects that are created when mainstream parties become more populist. This generates more conservative estimates. We also examine the vote share of strongly populist parties, which arguably includes those that have been populist for the entire period. We also rely on different (but related) time-varying measures, such as Colantone and Stanig's (2018) nationalism score, and find similar effects.

international integration (Konstantinidis et al., 2019; Mosley, 2003), the diffusion of proausterity ideas (Blyth, 2013), and institutional constraints (Bodea & Higashijima, 2017).

to reduce the deficit (as percent of GDP). The indicator captures the year when the policy change takes effect and has the advantage of directly capturing the government's policy decision.¹⁰

We use the cumulative amount of austerity implemented between the two elections, that is, the previous election in our data set and the election for which we examine votes.¹¹ Note that this measure is continuous; 0s indicate governments that do not implement austerity measures, that is, our control group. The fact that austerity measures are continuous implies that treated units receive the treatment with different degrees of intensity: Some austerity policies are mild, whereas others are quite severe. We label this variable *Austerity*.

In additional analyses, we also use a disaggregated version of this indicator that distinguishes between the number of deficit-reducing measures that are due to spending cuts versus tax increases. Moreover, we build a variable that captures the share of austerity measures that entail spending cuts over total austerity measures. Similarly, we build a dummy that takes a value of 1 if austerity measures involving spending cuts are larger than those concerning tax increases. The data come from Alesina et al. (2019). These last two variables allow us to explore whether cutting welfare expenditure triggers a stronger demand for populism than increasing taxes.

Figure C4 in Online Appendix C on p. 9 displays the temporal evolution of our austerity variable by country. There is evidence that the intensity of this measure varies quite dramatically among countries and over time. Overall, it illustrates that European governments have implemented austerity measures very frequently over the past three decades.

Measuring economic vulnerability

To measure economic vulnerability, we follow the international political literature on the distributional effects of globalization and automation. We use the share of unskilled workers and the share of workers

in manufacturing. Low-skilled workers have been negatively affected by both competition with cheap labor from emerging markets and technological shocks, whereas the manufacturing sector has been particularly hard hit by trade liberalization over the past 30 years. The data come from Colantone and Stanig (2018) and vary by NUTS-2 regions.¹² We map each district to its NUTS-2 region to merge the outcome variable with variables capturing economic vulnerability.

We use the share of workers exposed to automation as a further proxy for economic vulnerability. To build this variable, we rely on the EU Labour Force Survey, which is a large household sample survey providing quarterly results on (1) the labor participation of people aged 15 and over and (2) people outside the labor force. For each labor participant, we have information on her occupation, which we match to the routine task intensity (RTI) score developed by Goos et al. (2014). We take the top 33% most routine task-intense occupations and count their employment as routine jobs. We then calculate the share of workers in the most RTI occupations in each NUTS-2 region.

Note that we use economic vulnerability variables at their baseline value at the beginning of our period of analysis; this value does not change over time.¹³ We label these variables *Share of Low-Skilled Workers*, *Share of Manufacturing Workers*, and *Share of Workers Exposed to Automation*. Figure C5 in Online Appendix C on p. 9 displays the geographic distribution of the share of low-skilled workers across NUTS-2 regions.¹⁴

Empirical strategy

Our analysis at the district level is a standard TWFE with a continuous treatment. We estimate the following baseline model:

$$y_{cd,t} = \alpha + X_{cd(r),baseline} \times Austerity'_{c,t} \beta + \gamma_{ct} + \delta_r + \varepsilon_{cd,t}, \quad (1)$$

where $y_{cd,t}$ is our outcome variable capturing the populism score in each district in each election year. $X_{cd(r),baseline}$ is a matrix including our measures of economic vulnerability at baseline. The function $r(d)$

¹⁰ The data distinguish between the year of announcement and the year of implementation. In case of multiyear adjustment plans, a large part of the plan is usually implemented in the year in which it is announced. Some of the announced policy changes, however, only take effect in later years. Where this is the case, we use the year in which the policy is effectively implemented. Since we measure austerity across electoral periods rather than years, the announcements and implementation mostly coincide in our data set.

¹¹ Originally, the austerity variable was an annual time series for each country. It captures the amount of deficit-reducing measures that a government implements in a particular year. We sum these annual values for each electoral period, which gives us the total amount of austerity (as % of GDP) implemented during an electoral period. It is straightforward to attribute the annual consolidations to an election period in years without elections. It is trickier for election years where we had to make some judgment calls. We manually attributed fiscal consolidations in election years to one of the two election periods as accurately as possible.

¹² The share of low-skilled workers is the share of employees with a lower secondary education and below in a region, according to Eurostat Regional Statistics. The share of manufacturing workers is the share of employees in a region who work in the manufacturing sector, according to Eurostat and national statistical offices. The manufacturing sector is identified using NACE two-character alphabetical codes (DA to DN).

¹³ We use the baseline value to avoid that a time-varying vulnerability measure picks up the effect of austerity. Due to fast-moving economic transformations, such as deindustrialization and automation, vulnerability at the baseline value may deviate from vulnerability in the later periods that we examine. This is more relevant for the share of manufacturing and RTI workers than for share of low-skilled workers. To overcome this challenge, we use several measures of vulnerability, which capture different parts of the society.

¹⁴ The figures for the other two variables are available upon request.

maps district d to its NUTS-2 region r . $Austerity_{c,t}$ is a continuous variable scoring strictly positive values if austerity measures were implemented during the electoral period preceding the election. The key coefficient of interest is β , which estimates the interaction term between the two main independent variables. It reflects how the impact of national-level austerity measures varies across districts with different degrees of economic vulnerability.

We are unable to estimate the coefficient of $X_{cr(d),baseline}$ alone, because it is absorbed by (NUTS-2) region fixed effects, that is, δ_r . Similarly, we are unable to estimate the coefficient of $Austerity_{c,t}$ alone, because it is absorbed by country-year fixed effects, that is, $\gamma_{c,t}$. These fixed effects net out time-invariant differences across districts as well as time-variant differences across countries. The term $\epsilon_{cd,t}$ captures any unaccounted-for variation.

In augmented model specifications, we enrich our baseline model with potential confounders. We include a China shock variable as in Colantone and Stanig (2018). Moreover, we include foreign direct investment (FDI) inflow, FDI outflow, and export growth to account for regional economic conditions. We also include the share of foreign-born people as a proxy for migration. This set of controls is at the baseline, that is, they vary only at the NUTS-2 level. Thus, we interact each of these controls with our austerity variable to estimate their effects. We run ordinary least squares (OLS) regressions with robust standard errors clustered at the country-election year level.

Identification

Identifying the effect of austerity presents at least three challenges. First, in an effort to rule out anticipatory effects, we include lead variables that fake austerity measures *before* they are implemented. If we find that these are significant, this would be a clear indication of the presence of anticipatory effects because it would indicate that areas with large shares of manufacturing and low-skilled workers support populism *regardless of the presence of austerity measures*, which we do not observe. We also include NUTS-2-specific trends and find no evidence of pretrend effects, except for *Share of Workers Exposed to Automation*. Furthermore, we include our economic vulnerability variables in interaction with election-year to fixed effects to test whether economic vulnerability alone drives our results. We find no evidence that this is the case. In addition, we show that the results are similar if we include constituency fixed effects. Online Appendix D on pp. 14–17 reports the results of all of these tests.

In a second challenge, austerity is a potential outcome of negative economic conditions. Thus, economic crises may trigger support for populism among

vulnerable voters. To address this point, we leverage the fact that austerity measures do not perfectly correlate with negative economic conditions in Western European countries. In other words, while austerity correlates negatively with economic growth and fiscal balance, our data indicate that such measures have also been implemented during periods of economic stability and growth. Thus, to determine whether periods of economic crisis are driving our estimates, we run our main models on two subsamples: (1) observations experiencing sluggish economic growth and negative fiscal balance and (2) observations experiencing average or fast economic growth and average or positive fiscal balance.¹⁵

Third, it seems likely that governments strategically implement austerity measures. In particular, they probably anticipate the negative electoral consequences of austerity measures and time their implementation to mitigate voters' responses. For instance, there is a clear tendency for governments to implement austerity policies early and avoid them later in the electoral cycle, especially if their legislative majority is at risk (Hübscher & Sattler, 2017). We note that governments' strategic behavior leads us to *underestimate* the effect of austerity on vulnerable voters.

Our goal is to test the general relationship between austerity and populism in a broad range of countries and periods. Cleaner identification strategies are possible for specific, well-selected austerity episodes in specific, well-selected countries (see, for instance, Fetzer, 2019). For one, our continuous treatment (i.e., austerity) switches on and off and it does not vary subnationally, though its interaction with economic vulnerability does. The question arises to what extent these results are unique to a particular case, or whether they apply to a broader range of countries and time periods. This leads to a well-documented trade-off between internal and external validity. For a broad, comparative analysis over time, identification is more difficult and requires stronger assumptions. Yet in exchange, we are able to explore the extent to which austerity contributes to populism in general, or only in particular and unique circumstances.

Results

Populism

Table 1 reports the results of our main analysis. The coefficient of the interaction between variables capturing economic vulnerability and *Austerity* is positive and significant, as expected, in both the baseline models (Models 1–3) and in models that include

¹⁵ We use the value of the lower quartile to split the sample.

TABLE 1 Austerity and populism: Main results.

	Populism Score						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Share of low-skilled workers * Austerity	1.041** (.356)			1.037** (.302)			.604† (.345)
Share of manufacturing workers * Austerity		1.248** (.334)			1.459** (.456)		1.107** (.341)
Share of workers exposed to automation * Austerity			1.581** (.513)			1.525* (.620)	
Constant	4.352** (.105)	4.439** (.058)	4.198** (.154)	4.360** (.121)	4.502** (.059)	4.205** (.199)	4.290** (.110)
Observations	14,110	14,158	14,435	11,607	11,607	11,583	14,110
R-squared	.867	.868	.870	.847	.849	.848	.868
Controls	No	No	No	Yes	Yes	Yes	Yes
NUTS-2 fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-election year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Ordinary least square (OLS) results with robust standard errors clustered by county-election year in parentheses. The unit of observation is NUTS-2-election year. The outcome variable is populism score. The key independent variable is the share of low-skilled workers, the share of manufacturing workers, or the share of workers exposed to automation interacted with austerity measures. District-level controls are interacted with austerity. The coefficient of the austerity variable alone is absorbed by country-year fixed effects, the coefficient of the moderating variable alone is absorbed by region fixed effects.

† $p < .1$; * $p < .05$; ** $p < .01$.

controls (Models 4–6). The share of low-skilled workers, share of manufacturing workers, and share of workers exposed to automation give similar results; their coefficients remain positive and significant, even when we include both at the same time on the right-hand side of the models (Model 7).¹⁶

To ease the interpretation of the interaction term, we plot the estimates of the share of low-skilled workers. Figure 3 displays the linear predictions of *Populism Score* for different low-skilled workers in the case of austerity measures at their minimum and in the case of an average value of austerity measures. This figure reveals three key points. First, support for populism is always higher with austerity than without, but the difference is small in areas that are not economically vulnerable. Second, support for populism does not increase in regions with high shares of low-skilled workers with minimum levels of austerity, that is, the linear prediction is a flat line. Third, *Populism Score* increases dramatically in regions with high shares of low-skilled workers with (average) austerity measures.¹⁷

The magnitude of these effects is substantial. In countries implementing (average) austerity measures, *Populism Score* increases by 4.6%, moving *Share of*

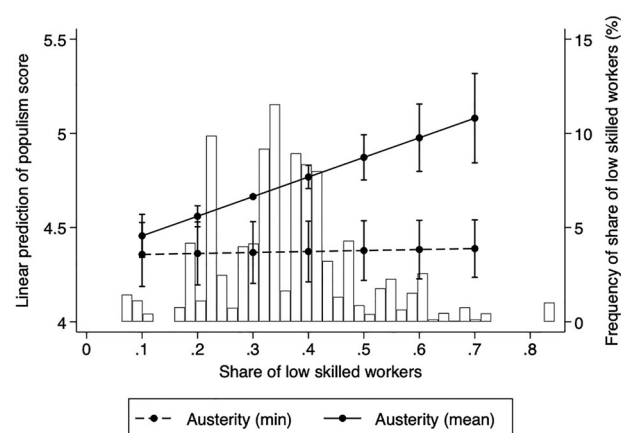


FIGURE 3 Austerity and populism: Share of low-skilled workers. Note: Linear predictions refer to Model 1 in Table 1. The dashed line reports the linear predictions of minimum level of austerity for different shares of low-skilled workers in NUTS-2 regions. The solid line reports the linear predictions of average level of austerity for different shares of low-skilled workers in NUTS-2 region; point estimates with 90% confidence intervals.

Low-Skilled Workers from one standard deviation below the mean to one standard deviation above it. Conversely, in countries implementing minimum levels of austerity measures, *Populism Score* increases by a mere 0.2%, moving *Share of Low-Skilled Workers* from one standard deviation below the mean to one standard deviation above it.¹⁸

¹⁶ We are unable to include the share of workers exposed to automation with the other two measures of economic vulnerability due to their very high collinearity, that is, $\rho > .9$.

¹⁷ In Online Appendix D on pp. 12 and 13, we show the linear predictions of regions with high economic vulnerability (one standard deviation above the mean) and low economic vulnerability (one standard deviation below the mean) with minimum or average value of austerity. Results indicate that support for populism increases in both sets of regions, but the increase is significantly larger in regions with high economic vulnerability.

¹⁸ The result is similar for *Share of Manufacturing Workers* and *Share of Workers Exposed to Automation* (see Online Appendix D on p. 10). These effects are in line with the effects estimated by Colantone and Stanig (2018), who

TABLE 2 Austerity and populism: The role of fiscal balance.

	Populism score		
	Low fiscal balance only	High fiscal balance only	Full sample
	(1)	(2)	(3)
Share of low-skilled workers * Austerity	1.115 [†] (.564)	1.040 [†] (.525)	.950* (.371)
Share of low-skilled workers * Fiscal balance			-.057 (.068)
Constant	4.551** (.240)	4.277** (.133)	4.367** (.106)
Observations	3497	10,602	14,110
R-squared	.789	.884	.867
Controls	No	No	No
NUTS-2 fixed effects	Yes	Yes	Yes
Country-election year fixed effects	Yes	Yes	Yes

Notes: Ordinary least square (OLS) results with robust standard errors clustered by county-election year in parentheses. The unit of observation is NUTS-2 election year. The outcome variable is populism score. The key independent variable is the share of low-skilled workers interacted with austerity measures. Column headers indicate if a subsample or the full sample is used for the estimation.

[†] $p < .1$; * $p < .05$; ** $p < .01$.

The role of crises

To assess whether crises are a potential confounder, we rerun our main models, splitting the sample according to macroeconomic conditions, that is, low and high fiscal balance (Table 2). As expected, the effects are larger when fiscal balance is negative than when it is positive. However, our main findings remain unchanged even if there are no economic crises, that is, when macroeconomic conditions are sound. Our results are similar even when we interact fiscal balance with the share of low-skilled workers (Model 3). In short, our results are not a by-product of the correlation between austerity and economic crisis: Austerity independently sways voters toward populism.

In Online Appendix E on pp. 18–20, we show that our results are similar if we use (i) other proxies of economic vulnerability and (ii) economic growth rather than fiscal balance.

Types of austerity

In Table 3, we distinguish between different types of austerity. We show that our results are similar if we rely on spending cuts rather consolidation (Models 1 and 2). Moreover, Models 3 and 4 indicate that measures of austerity relying more heavily on spending cuts trigger more support for populism than measures emphasizing tax increases.¹⁹ Models 5 and 6

show that the effect is similar for countries with low and high levels of welfare spending.²⁰ This indicates that changes in welfare support matter more than levels: Current welfare state support is the reference point of a vulnerable voter against which she evaluates governments, and when the government cuts this baseline level of welfare, the voter reacts.²¹

In addition, we run our main model specification, replacing Austerity with two dummies to indicate high and low austerity measures. We use the average value of (strictly positive) Austerity to create these two dummies. Models 7 and 8 report the results, which show that severe austerity measures are driving our results. This finding has three implications. First, it seems to indicate that voters are more likely to be aware of and observe the economic effects of large consolidation measures, and therefore to electorally sanction the parties that implement them. Second, to the extent that large consolidation measures are typically implemented by cross-party agreement, there is evidence that the lack of alternatives to the dominant narrative in favor of austerity fuels populism because dissatisfied voters have no other means of expressing their discontent. Yet, we note that this is only the case for vulnerable voters, as our theory suggests. Third, severe austerity measures make it particularly difficult for governments to spare, with targeted policies, econom-

leverage a single event, that is, the China trade shock, over a relatively short period of time, whereas the occurrence of austerity is more frequent in our sample and our time span is longer.

¹⁹ Government transfers, which are relevant for job security, and government consumption, which disproportionately hits lower income voters, drive these results.

²⁰ We use data on baseline social expenditure to distinguish between countries with low (i.e. below average) and high (above average) levels of welfare spending.

²¹ Theoretically, the role of welfare state size is ambiguous. On the one hand, voters in large welfare states may react more strongly to austerity because they have higher expectations on state support. On the other hand, voters in small welfare states may react more strongly because their situation becomes even more precarious after an equivalent reduction in support.

TABLE 3 Austerity and populism: Types of austerity.

	Populism score							
	Full sample	Full sample	Full sample	Full sample	Low welfare only	High welfare only	Full sample	Full sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share of low-skilled workers * Austerity (cuts)	1.089* (.545)	1.470** (.412)						
Share of low-skilled workers * Share of spending cuts			1.779* (.690)					
Share of low-skilled workers * Predominantly spending cuts				1.011* (.493)				
Share of low-skilled workers * Austerity (consolidation)					.923 [†] (.509)	1.118* (.492)		
Share of low-skilled workers * Austerity (low)							-.373 (.546)	-.482 (.345)
Share of low-skilled workers * Austerity (high)							1.655** (.559)	1.492** (.468)
Constant	4.431** (.114)	4.362** (.119)	4.368** (.065)	4.451** (.041)	5.206** (.120)	3.824** (.162)	4.459** (.134)	4.523** (.102)
Observations	14,110	11,607	12,439	12,439	5334	8776	14,110	11,607
R-squared	.867	.847	.871	.871	.856	.809	.868	.848
Controls	No	Yes	Yes	Yes	No	No	No	Yes
NUTS-2 fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-election year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Ordinary least square (OLS) results with robust standard errors clustered by county-election year in parentheses. The unit of observation is NUTS-2-election year. The outcome variable is populism score. The key independent variable is the share of low-skilled workers interacted with types of austerity measures. District-level controls are interacted with austerity. Column headers indicate if a subsample or the full sample is used for the estimation.

[†] $p < .1$; * $p < .05$; ** $p < .01$.

ically vulnerable workers, which may explain why they turn their vote to populist parties. In Online Appendix E on p. 21, we show that our results are similar when we use other proxies for economic vulnerability.

Austerity and ideology

Table 4 reports the results of support for radical left and right parties. There are two take-away findings. First, only radical right parties gain from austerity; we find no effect for radical left parties (Models 1 and 4).²² Second, whether left or right governments implement austerity measures does not generally affect explanations of support for radical right/left parties (Models 2-3 and 5-6).²³ This finding is confirmed when we use the share of manufacturing workers to proxy for economic vulnerability. For the share of workers exposed

to automation, we find some evidence that radical right (left) parties gain electoral consensus when mainstream left (right) parties implement austerity (see Online Appendix F on p. 25).

Furthermore, we illustrate how austerity affects support for different socioeconomic policies (see Table F1 in Online Appendix F on p. 23). We replace our main outcome, *Populism Score*, with outcomes that capture support for parties that are against international trade, the EU, migration, and minorities, and that support conservative values. Our results indicate that where austerity measures are implemented, economically vulnerable areas experience a surge of support for parties advocating autarky or conservative values, as well as Eurosceptic parties and antimigration parties. Radical right parties typically hold such positions, which implies that austerity causes economically vulnerable voters to move rightwards on socioeconomic issues.

Robustness checks

We perform four additional robustness checks and report the results in Online Appendix G on pp. 27-34. First, our results are similar if we use other measures

²² These results are robust to the use of other proxies for economic vulnerability and different measures of support for radical left and right parties (see Online Appendix F on pp. 24 and 26).

²³ Left/right incumbency measures the ideology of the cabinet *before* the election using the average left-right position of all parties in government. The data come from the Comparative Manifestos Project.

TABLE 4 Austerity, economic vulnerability, and radical parties.

	Share of votes for radical left parties			Share of votes for radical right parties		
	Full sample	Left incumbent only	Right incumbent only	Full sample	Left incumbent only	Right incumbent only
	(1)	(2)	(3)	(4)	(5)	(6)
Share of low-skilled workers *	−0.006	−0.084**	0.013	0.152**	0.183*	0.136*
Austerity	(0.071)	(0.025)	(0.054)	(0.049)	(0.071)	(0.055)
Constant	0.042*	0.070**	0.029	0.017	0.025	0.012
	(0.021)	(0.006)	(0.018)	(0.014)	(0.016)	(0.019)
Observations	14,111	5624	8478	14,111	5624	8478
R-squared	.640	.739	.612	.833	.918	.767
Controls	No	No	No	No	No	No
NUTS-2 fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country-election year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Ordinary least square (OLS) results with robust standard errors clustered by county-election year in parentheses. The unit of observation is NUTS-2-election year. The outcome variables are the share of votes for radical left and right parties. The key independent variable is the share of low-skilled workers interacted with austerity measures. Column headers indicate if a subsample or the full sample is used for the estimation.

† $p < .1$; * $p < .05$; ** $p < .01$.

of support for populism. Second, our results are similar if we use the raw value of austerity rather than its logged value. Third, we find no evidence that austerity affects turnout. Fourth, we show that our results are *not* driven by the post-2010 period. Finally, we show that our results hold if we exclude one country at a time; thus they do not depend on the inclusion of any specific country in our sample.

INDIVIDUAL-LEVEL VOTING

Our individual-level analysis includes 12 Western European countries and (up to) 86,939 respondents for whom we have data. Our time span covers (up to) eight waves of the European Social Survey (ESS) administered between 2002 and 2016. Below, we describe the data and the empirical strategy and report our main results. In line with the district-level analysis, we exploit variation in individual exposure to national austerity policies depending on our measures of economic vulnerability. We expect economic vulnerability to moderate the extent to which national austerity measures affect individuals' tendency to vote populist.

Data

Our main outcome variable measures ESS respondents' support for populism. We use each party's populism score as described in the previous section and match it to the party for which the respondent voted in the most recent election before the ESS survey.²⁴

²⁴ For instance, ESS wave 6 from 2012 captures the vote of Irish respondents in the 2011 national election.

To measure austerity, we rely on the variable described in the previous section. We capture the austerity packages that were implemented during the electoral period that is leading up to the election recorded in a particular ESS wave.²⁵ In our main model specification, we use a dummy that takes a value of 1 if any austerity measure was introduced during the electoral period before the election that we analyze.²⁶ To capture economic vulnerability, we use the number of years of education of each respondent, which identifies low-skilled workers in line with the district-level analysis. Years of education is homogeneous across countries that have different education systems. We split this variable into three dummies: (1) *Lower Secondary* (less than 10 years of education), (2) *Upper Secondary* (10–15 years of education), and (3) *Tertiary* (more than 15 years of education). Tertiary is the baseline category in the analysis; that is, it is the excluded variable. Furthermore, we use a dummy coded as 1 if respondents work in manufacturing. This variable is built on the NACE (Statistical Classification of Economic Activities in the European Community) trade category reported in the ESS.

We also include a variable measuring exposure to automation at the individual level. Following Goos et al. (2014), we convert varying occupational measures into a two-digit ISCO-88 code and link it to an aggregated RTI index.²⁷ Then, following Gingrich

²⁵ In the example of the 2011 Irish election recorded in ESS wave 6, the austerity variable reflects the fiscal consolidation that the Irish government implemented between the preceding election in 2007 and the 2011 election.

²⁶ In additional analyses (available upon request), we show that our results are virtually the same for education if we use a continuous measure of austerity. They are weaker for manufacturing and RTI, though the sign of the main coefficient remains the same.

²⁷ The RTI index categorizes occupations based on the skills most affected by automation in the 1980s and 1990s. This measure is missing for three

TABLE 5 Austerity and populism: Individual-level analysis.

	Populism score					
	(1)	(2)	(3)	(4)	(5)	(6)
Lower secondary education	.054 (.055)	.047 (.059)				
Upper secondary education	.096* (.037)	.090* (.037)				
Manufacturing			−.011 (.021)	−.016 (.021)		
RTI					−.026 (.035)	.011 (.039)
Lower secondary education * Austerity (dummy)	.221** (.083)	.254** (.089)				
Upper secondary education * Austerity (dummy)	.164** (.062)	.173** (.062)				
Manufacturing * Austerity (dummy)			.093* (.038)	.085* (.039)		
RTI * Austerity (dummy)					.092† (.050)	.111* (.055)
Constant	4.621** (.040)	4.471** (.162)	4.843** (.005)	4.670** (.147)	4.779** (.017)	4.521** (.168)
Observations	86,939	86,717	82,317	82,128	72,613	72,449
R-squared	.301	.303	.299	.301	.299	.301
Controls	No	Yes	No	Yes	No	Yes
Country-wave fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Note: Ordinary least square (OLS) results with robust standard errors clustered at the country-wave level in parentheses. The unit of observation is individual-survey wave. The outcome variable is populism score. The key independent variables are economic vulnerability variables interacted with austerity measures. Individual-level controls are interacted with austerity. The coefficient of the austerity variable alone is absorbed by country-year fixed effects.

† $p < .1$; * $p < .05$; ** $p < .01$.

(2019), we aggregate the RTI measure into five quintiles, rescaled to 0 (least affected) to 1 (most affected), which allows us to identify broad categories of exposure.²⁸

Empirical strategy

In line with the district-level analysis, our analysis at the individual level is a standard TWFE. We estimate the following baseline model:

$$y_{ic,w} = \alpha + X_{ic,w}\zeta' + X_{ic,w} \times Austerity'_{c(i),w}\eta + \gamma_{cw} + \epsilon_{ic,w}, \tag{2}$$

where $y_{ic,w}$ is our outcome variable, which captures respondent's i support for populism in wave w in country c . $X_{ic,w}$ is a matrix that includes our measures of economic vulnerability: education, manufacturing,

and exposure to automation.²⁹ $Austerity_{c,w}$ is a dummy scored as 1 if country c implements austerity measures in the electoral period preceding the election recorded in ESS wave w . The function $c(i)$ maps respondent i to its country c . In this analysis, the key coefficient of interest is η , which estimates the interaction term between the two main independent variables. We are unable to estimate the coefficient of $Austerity_{c,w}$ alone, because it is absorbed by country-year fixed effects, that is, γ_{cw} . The term $\epsilon_{ic,w}$ captures the residuals.

In the augmented model specifications, we enrich our baseline model with a host of individual-level characteristics including gender and age, which absorb an important variation of our outcome. We also add dummies for retired respondent, student, unemployed respondent, self-employed respondent, and respondent working in services. We interact each of these controls with $Austerity$ to estimate their effects.

major occupational groups (ISCO 23, 33, and 61), which are excluded from the analysis.

²⁸ The results are similar if we use the continuous version of the RTI developed by Goos et al. (2014).

²⁹ We are unable to use the baseline values of our measures of economic vulnerability, since the ESS is a repeated cross-section rather than a panel: Different respondents take part in each wave.

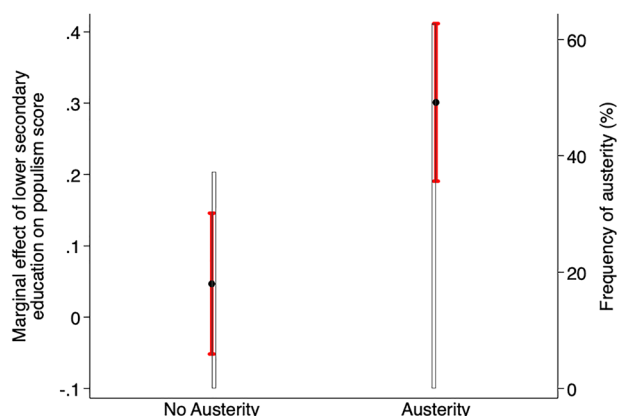


FIGURE 4 Austerity and populism: Lower secondary education. *Note:* Marginal effects refer to Model 2 in Table 5. The marginal effects of lower secondary education are reported for individuals in elections with no austerity and with austerity during the preceding electoral period. The histograms show the distribution of observations without and with austerity; point estimates with 90% confidence intervals.

We run OLS regressions with robust standard errors clustered at the country-wave level.³⁰

Results

We report the results in Table 5. In all models, the interaction terms between economically vulnerable individuals and austerity measures are positive and statistically significant, confirming the results of the district-level analysis. Importantly, the coefficient of the interaction term remains the same when we include individual-level controls interacted with austerity.

To ease the interpretation of the results, we plot the estimates of the interaction term between education and austerity in Figure 4. The figure displays the marginal effect of *Lower Secondary Education* on *Populism Score* with and without austerity. Less-educated individuals are significantly more likely than highly educated individuals to support populist parties if austerity measures have been implemented. Without austerity measures, less-educated individuals are not more likely to support populist parties than highly educated individuals.³¹ The effect is sizable: *Populism Score* is six times higher with austerity than without. We also find that individuals with an upper secondary education are more likely to support populist parties where austerity policies have been introduced, though the magnitude of the effect is significantly smaller

than that found for individuals with a lower secondary education.³²

Finally, we perform a large number of robustness checks in line with the district-level analysis (see Online Appendix H on pp. 35–41). All these tests leave our results unchanged. All in all, the individual-level analysis confirms our district-level findings: Austerity increases support for populism more among the losers than among the winners from globalization and automation.

CONCLUSION

This paper examines the political effects of fiscal austerity in open economies. It shows that economically vulnerable voters—that is, low-skilled workers, workers in the manufacturing industry, and workers in routine jobs—increasingly turn to populist parties when governments implement fiscal adjustment measures. We find this effect for both district-level election and individual-level voting data in Western European countries since the 1990s. Austerity has distributional effects that magnify, rather than mitigate, the negative economic effects of globalization and technological change for many workers. These voters therefore begin to question government promises to make globalization a success for everyone.

These results imply that economic policy and government decisions play a crucial role in the mechanism that led to the backlash against globalization. Governments have a variety of ways to moderate the adverse effects of globalization and technological change. But if they fail to use these means to compensate voters for the increased social risk they face in open economies, populist parties will be able to exploit the growing antiglobalization sentiment among dissatisfied voters. The economic origins of populism, therefore, are not purely external or unavoidable. Public policies—especially austerity policies—are crucial because they undermine the “embedded liberalism” compromise of the postwar period that protected vulnerable workers from the enhanced social risks inherent in open economies (Bisbee et al., 2020).

These findings also have important implications for government policy after the Covid-19 crisis. Governments have spent large amounts to reduce the pandemic’s economic impact. A crucial, long-term question is how to deal with the public debts this has generated. Our results demonstrate that a return to austerity policies after the crisis would be very politically contentious. While government spending has helped prevent large-scale economic and political

³⁰ All estimates include poststratification weights, including design weights.

³¹ Recall that we obtain these marginal effects controlling for student status, unemployed status, and age. Thus, years in school does not proxy for (youth) unemployment.

³² The result is similar for manufacturing workers and for workers exposed to automation (see Online Appendix H).

destabilization, the pandemic has had very unequal effects across societal groups (Bambra et al., 2021). If vulnerable groups are left to repay the bulk of the cost of these government interventions, this will likely fuel further support for populist rhetoric and populist parties.

Our study examines the overall effect of austerity on votes in different political and economic contexts. We evaluate how reactions to austerity vary across these contexts, but our research design cannot provide definitive answers to this question. We leverage a large amount of regional and individual variation for our key moderating factor, economic vulnerability, but there is less variation in our data on national contextual factors. Future research should further explore how the political-economic context matters, in addition to the voter characteristics that we study.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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