Austerity and Populism in Europe
Final Draft

Master Thesis in Policy Economics

Abstract

This paper seeks to empirically show the importance of austerity policies in driving populist voting patterns in Europe. Extending the empirical research of Guiso et al. (2017) into the economic insecurity mechanism underlying populist voting, and making use of recent data on austerity from Alesina et al. (2019), I show that economic factors are more strongly associated with populist voting in fiscal consolidation years than otherwise. I also find that more left-wing individuals vote for populist parties during expenditure-based consolidations and more right-wing individuals vote populist during revenue-based consolidations. This analysis provides tentative evidence for an "austerity channel" of the economic insecurity mechanism, where austerity policies increase individuals' disappointment with traditional "status quo" parties by increasing economic insecurity, leading to an increase in populist votes.

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1 Introduction

The surge in support for populist parties in Europe is well documented and shows little sign of abating. One in four European voters voted for a populist party in their most recent national elections, up from around one in ten in the mid-90s (TIMBRO, 2019). These parties seek to reclaim politics from an established elite and return power to "the people", a uniform body politic whose democratic rights have been trampled by the status quo. Populists are thus best described by what they stand in opposition to: globalisation, immigration, metropolitan elites, and - particularly in Europe - supranational government.

What might explain this rise? Globalisation, rising inequality, the erosion of organised labour, immigration, trade integration, changing cultural attitudes, technological change, the Great Recession and subsequent Eurozone Crisis have all been investigated as potential causes. Following the Great Recession, European economies contracted sharply, and many national governments instigated austerity as a policy response, diverging from the traditional Keynesian policy prescription of a stimulus package (House et al., 2019). This paper seeks to answer the question: "Did austerity policies implemented in Europe over the period 2002-2014 cause an increase in populist voting?" as well as the related questions "do austerity policies which are composed primarily of spending cuts lead to an increase in left-wing populist votes?" and "do austerity policies which are composed primarily of tax-increases lead to an increase in right-wing populist votes?"

Ideally, to answer my research question, I would have data on the *incidence* of austerity policies at the individual level - as in Fetzer (2019) - but unfortunately my main data set - The European Social Survey (ESS) - does not contain this information. I instead test this question empirically by building on the work of Guiso et al. (2017). They find a significant effect of "economic insecurity" on populist voting, using data from the ESS on 31 European countries over years 2002-2014. This effect, they argue, works through an "economic insecurity mechanism", whereby economic insecurity causes individuals to be disappointed with the status quo, leading them to vote for populist parties. They used principal component analysis (PCA) to construct an "economic insecurity" variable from the ESS, as well as a measure of populist voting, and tested the effect of the former on the latter. Their "economic insecurity" variable is constructed from information on unemployment, income difficulties, and blue collar manufacturing occupations. They account for endogeneity with the use of a Heckprobit model, using rainfall on election days as an instrument for selection into electoral participation. They also use country and survey-wave fixed effects.

I hypothesise that austerity policies cause greater economic insecurity amongst individuals whom are reliant on social benefits and therefore that austerity policies cause populist voting through an "austerity channel" of the economic insecurity mechanism.

In order to test this hypothesis, my analysis differs from Guiso et al. (2017) in two

important ways. Firstly, I construct a different independent variable, which is a proxy for "exposure to austerity". This is constructed using PCA from information on unemployment, income difficulties, and a reliance on social benefits. This change means that I am broadly testing the same "economic insecurity" mechanism, with the same underlying model of voting, but also testing a different *channel* through which it might operate. Namely, where individuals who are more exposed to cuts in social spending are affected by these cuts in times of austerity, causing disappointment with the status quo, thus inducing them to vote for populist parties. Secondly, incorporating data on austerity periods from Alesina et al. (2019), I look at how an individual's exposure to austerity affects the likelihood that they will vote for a populist party across years when governments are enacting austerity policies and those in which they aren't. This serves to further focus the analysis on austerity policies as a cause of populism. Unfortunately, this change also means reducing the number of countries in the analysis from 31 to 10. In addition to these changes, I use disability as an instrument for selection into election participation. This instrument is unfortunately weaker than an election-day-rainfall instrument, leaving room for sample selection bias in the model.

To be clear, this paper investigates the relationship between austerity policies and populist voting by answering the more nuanced question: "Is there an *austerity channel* through which the economic insecurity mechanism as a driver of populist voting operates?" Of course, there could well be other mechanisms, both economic and otherwise, through which austerity could affect populist voting, though they lie beyond the scope of this analysis.

I find evidence for the existence of an austerity channel through which the economic insecurity mechanism operates - my "exposure to austerity" variable is significantly associated with populist voting, and this association is strongest in years of austerity. However, evidence of a causal link between austerity and populist voting would require better data on the incidence of austerity policies at the individual or district level. For now, it is difficult to distinguish between the effect of austerity policies and those of the Great Recession, as the former is in part an endogenous policy response to the latter.

I also go on to check whether the austerity channel of the economic insecurity mechanism operates differently in austerity periods which are expenditure-based (i.e. mainly consist of cuts to government spending) or revenue-based (i.e. mainly consist of tax increases). One might expect to find that "exposure to austerity" (which consists in part of a reliance on social benefits) drives populist voting to a greater extent during expenditure-based austerity than in revenue-based austerity, as social benefits are often cut when government spending is reduced. I indeed find support for this.

Finally, I take advantage of the expenditure-revenue distinction to examine the ideological component of the relationship between austerity and populist voting. How does the ideology of populist voters differ across expenditure-based and revenue-based periods of austerity? We may expect more left-wing populist voters during expenditure-based austerity, and more right-wing populist voters during revenue-based austerity (given that left-wing voters tend to favour increased government spending, and right-wing voters tend to favour reduced taxes). I also find support for this.

The remainder of the paper is structured as follows. In section 2 I outline related literature on the economic causes of populism, and issues with measuring austerity. In section 3 I describe data sources and how the variables used in the analysis are made, and section 4 explains the empirical strategy. Section 5 provides the results from the analysis, and section 6 concludes based on the findings.

2 Literature Review

2.1 Economic causes of populism

Recent years have seen a plethora of papers released on the subject of the populist surge and its correlates. For the subject to be addressed meaningfully, a proper definition of populism seems a helpful starting point. The term is applied to a range of political movements spanning the traditional left-right spectrum. Rodrik (2018) describes certain traits which can be attributed to this diverse group: "What all these share is an anti-establishment orientation, a claim to speak for the people against the elites, opposition to liberal economics and globalization, and often (but not always) a penchant for authoritarian governance." This is very useful description, though it inevitably leaves a little room around the edges for borderline cases, especially with regards to authoritarian tendencies. For example, Jeremy Corbyn - former leader of the UK Labour Party - arguably fits all but the last of these criteria; the Labour leader is fiercely democratic to the point of advocating democratisation of institutions beyond the traditional political sphere. Moreover, when classifying political parties as either "traditional/status quo" or "insurgent/populist" the UK Labour Party fits more readily into the former category.

This nuance at such an early juncture leads immediately to issues with empirical investigation into populism and its causes. If the dependent variable in such research is "number of votes for a populist party", we must classify political parties as populist or otherwise. The analysis in this paper builds on the empirical work of Guiso et al. (2017), who combine the definitions of van Kessel (2015) and *Encylopaedia Brittanica* to identify three components: "(1) the claim to be on the side of the people against the elite - which we label "supply rhetoric;" (2) the "fears or enthusiasms" of people - the demand conditions to which the populists pander; and (3) the disregard for longer-term consequences." (Guiso et al., 2017). This allows for the creation of a binary variable which indicates populist party voting. I follow this definition in this analysis to identify populist parties. A full list of these parties can be found in appendix A.

Guiso et al. (2017) focus on identifying empirically an "economic insecurity" mechanism, which leads from said insecurity to increased disappointment with traditional parties. This simultaneously increases support for populists and reduces electoral participation, negatively affecting the vote share of traditional parties. They leave the exact contributors to this insecurity unspecified, leaving room for a range of economic factors, such as import competition, automation, wage pressure from increasing low-skilled immigration, or recessions - though a certain emphasis is placed on the forces of globalisation in the construction of their "economic insecurity" variable. Papers which take a closer look at of the role of globalisation, and specifically trade integration include Colantone and Stanig (2018a), Colantone and Stanig (2018b), Che et al. (2016), and Dippel et al. (2015).

Intimately related to the subject of globalisation is European Integration. The free

flow of people, capital and goods between participating nations underpins the project, and many populist parties cite the European Union as the particular establishment against which they stand.[1] Foster and Frieden (2017) argue that the Euro crisis led to a loss of trust in traditional pro-EU parties. While they concede that cultural attitudes explain much of the baseline distrust, they find it to be primarily economic factors which explain the recent decline. Guiso et al. (2019) find evidence to support the hypothesis that the constraints on monetary and fiscal policy imposed by Eurozone institutions frustrate the efforts of Eurozone countries to respond to economic shocks, and that this feeds frustration with national governments, contributing to the rise in support for populist parties.

Immigration as an economic and cultural threat is another often cited reason for the populist surge. Mayda et al. (2018) and Barone and D'Ignazio (2014) exploit variation in levels of immigration and voting data to examine changes in political sentiments caused by immigration. Both find evidence of a casual relationship between increased low-skilled immigration and right-wing sentiment, with the effect driven by rural areas. Steinmayr (2016) by contrast finds that increased exposure to refugees in Austria led to reduced support for far-right parties.

Automation as a threat to blue-collar work is less commonly cited as a concern by populist parties and politicians, despite empirical evidence from authors such as Devaraj and Hicks (2015 & 2017) and Acemoglu and Restrepo (2017) which show automation to be a significant cause of the decline in manufacturing jobs and wages in the US. Whilst the concern is rarely raised explicitly on populist platforms, Anelli et al. (2019) study automation and voting behaviour in Western Europe. They find that exposure to automation increased votes for nationalist and far-right parties, and attribute this to an economic insecurity mechanism.

The papers in this field find much support for the notion that economic factors of various sorts underpin the recent populist surge across the Western world. However, a range of what might be termed cultural phenomena, such as the rise of "identity politics" and the emergence of political fault lines between liberal internationalists and nationalists cannot be ignored[2], while it seems likely that both economic and cultural factors are important. Colantone and Stanig (2019) caution against a false economic vs. cultural causes dichotomy: "it's not status threat or economic hardship, it's and, or even because". The authors note also the dangers of including both economic factors and social attitudes as separate variables explaining populist voting outcomes in a regression. This speaks to the endogenous relationship between economic and cultural causes. An increase in economic insecurity could plausibly be expected to represent a threat to status in one's community. Likewise, a cultural/psychological predisposition to want to stay in one's local environment despite increasing regional inequalities and economic opportunities elsewhere, may negatively affect economic outcomes.

Margalit (2019) echoes some of these arguments and gives a more robust critique of the literature on the economic causes of populism to date, cautioning against overstating the role of economic factors. The author identifies a frequent conflation in the literature

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[1]Prominent examples include the UK Independence party, The Five Star Movement in Italy, Alternative for Germany, Front National in France, and Forum for Democracy in the Netherlands.

[2]This notion is exemplified in a speech following the Brexit referendum from then Prime Minister Theresa May, which was layered with populist rhetoric: "*Today, too many people in positions of power behave as though they have more in common with international elites than with the people down the road, the people they employ, the people they pass on the street ... but if you believe you are a citizen of the world, you are a citizen of nowhere. You don't understand what citizenship means.*"

between explanatory significance and outcome significance. It may be that economic factors can explain as much as 10% of votes for a populist parties (Fetzer, 2019), which can be enough to change the outcome of an election, even though the majority of the vote is explained by other factors. Additionally, the methodological techniques that remove regional or national differences (used also in the analysis of this paper), whilst removing a portion of endogeneity from models of the relationship in question, also serve to remove a lot of the variation which likely explains regional and national differences. As such, we should not overstate the importance of economic factors by treating them as the root cause of political discontent.

Nevertheless, the literature supports the notion that economic factors of various kinds have played a role in increases in support for populist parties. Austerity policies have far reaching economic implications for citizens, which naturally makes them an aspect of the economic insecurity mechanism which warrants investigation.

2.2   Measuring austerity

The analysis contained in this paper depends in large part upon the successful identification of plausibly exogenous periods of fiscal consolidation. When and where are austerity policies enacted? This question is in fact fiendishly difficult, and must be answered before investigating the relationship between austerity and populism - or indeed any other outcome variable - empirically. As such, I will focus mostly on the measurement of austerity in this part of the literature review, rather than findings on fiscal multipliers or other outcomes. Developments in this area can be thought of as a journey from a very simple look at changes in a government's primary balance, towards the narrative method of identification which informs this paper. A more detailed summary of the contrast between these two approaches can be found in Guajardo et al. (2011).

Traditional attempts to identify periods of fiscal consolidation focused on the cyclically adjusted primary balance (CAPB), whereby changes to the government's primary balance are adjusted using a measure of the current stage in the business cycle, usually unemployment. Introduced by Blanchard (1990), this indicator aims to remove some of the bias which may appear in regressions of changes in output on changes in the primary balance. This method is used to examine the link between fiscal consolidations and electoral outcomes in Alesina et al. (2012), and in fact no significant relationship is found. Guajardo et al. (2011) however, provide nice examples of how bias could emerge when examining the link between fiscal policy and output using the CAPB method; changes in the CAPB may reflect non-policy factors, such as a stock market boom, which will increase the CAPB through increased capital gains receipts, and will likely increase output through increased investment and private consumption. These developments bias estimates so that it appears austerity policies (proxied by an increase in CAPB) lead to increased output. Here, I will illustrate how the same issue emerges when looking at the relationship between large fiscal changes and populist voting outcomes.

If we regress populist votes on the change in the CAPB (putting to one side the problem of different frequencies, country-specific effects, and lagged variable effects), we could expect correlation between the CAPB measure and the error term. Imagine a technological development which rewards capital owners such that government receipts increase, but that also causes widespread panic about the effects of automation on employment (without any actual change in unemployment). This could lead to an increase in the

CAPB, as well as perhaps cause those afraid of losing their jobs to turn to a populist party promising extra taxes on the technology which is causing distress. In a regression, this could bias estimates of the effect of fiscal consolidation on populist votes upwards.

The problem then is in identifying periods of fiscal consolidation which are plausibly exogenous - an increase in taxes, or decrease in spending, which is not caused by other economic factors such as those discussed above, but which is instigated with the specific aim of reducing the budget deficit. One solution, put forth by Romer and Romer (2010) and commonly referred to as the narrative method, was to discern the intentions of governments' fiscal consolidations by examining their stated aims directly. In practice, this means examining (in the case of the United States) economic reports of the president, speeches, tax bills, and congressional budget office reports amongst others. This Herculean task was performed in search of " [...] tax changes that are not systematically correlated with other developments affecting output"(Romer & Romer, 2010). This method is taken up by Guajardo et al. (2011) and Devries et al. (2011), and extended to an OECD multi-country setup. They find, contrary to the literature based on the traditional CAPB measure, contractionary short term effects of austerity policies.

Finally, Alesina et al. (2015), as part of a series of papers on the effects of austerity, extend the Devries et al. (2011) data set up until 2014. Additional changes are made in terms of identification. In particular, the authors focus on the construction of multi-year fiscal plans, accounting for the manner in which fiscal announcements are frequently modified in subsequent years. As different components of fiscal consolidation are variously permanent or transitory, this new approach mainly aids in distinguishing between different *categories* of consolidation, such as expenditure-based vs. revenue-based consolidations. The authors find that spending-based consolidations are less contractionary (in terms of output) than revenue-based consolidations. Given that the focus of this paper turns towards individuals who rely on social benefits, it will be interesting to see if my proposed austerity channel of the economic insecurity mechanism is more effective in spending-based consolidations.

A critique of the Alesina et al. (2019) data set attributable to Beetsma et al. (2018) is that the narrative method is based upon government spending plans, with the emphasis on plan. Often, actual fiscal policy diverges from these plans, such that there is a divergence between what is announced and what comes to be. These authors find that at least part of the finding that revenue-based consolidations are more harmful to GDP growth than expenditure-based consolidations is attributable to a discrepancy in "follow-through" of the two types of fiscal announcement.

These methods were developed with a view to estimating the effect of austerity policies on a range of macroeconomic variables using panel data for either one country or many. However, austerity policies enacted at a national level of course also vary in the severity with which they affect regions and individuals. This variation can be exploited in order to measure the effect of austerity policies on populist voting outcomes. Fetzer (2019) uses rich panel data containing estimates of an individual's exposure to welfare-reforms to look at within-individual variation in exposure to such reforms and support for the UK Independence Party in the run-up to the 2016 EU referendum. The author finds the association to be strong and arguably causal, concluding that the EU referendum "could have resulted in a victory for Remain, had it not been for austerity" (Fetzer, 2019). In related work, Galofré-Vilà et al. (2017) find a positive association between austerity measures and the rise of the Nazi Party in 1930s Germany, while Ponticelli and Voth (2020) look in particular at expenditure cuts and incidents of social unrest in the period

1919-2009.

2.3 Control and explanatory variables in related literature

Table 2.1 shows control and explanatory variables typically used in related literature on voting outcomes. There is relatively little literature on populist voting as an outcome variable *per se*, as in Guiso et al. (2017), though there is plenty concerning voting outcomes which we may expect to correlate highly with populist votes, owing to their ideological similarities. This crude generalisation serves to help form a hypothesis about how variables will behave in the analysis of this paper. Such outcomes include Leave votes in the Brexit referendum (Becker et al., 2017; Colantone & Stanig, 2018b; Fetzer, 2019), votes for right-wing parties (Anelli et al., 2019; Galofré-Vilà et al., 2017), confidence in political institutions (Foster & Frieden, 2017), and expression of illiberal values (Colantone & Stanig, 2018a).

All of the control variables in table 2.1, with the exception of austerity incidence are available and used at some stage in this analysis. They are the same as those used in Guiso et al. (2017), with the exception of reliance on social benefits, which is added to better aim at the austerity channel of the economic security mechanism. I expect this to be positively related to populist votes, in line with my hypothesis concerning the aforementioned austerity channel. This is supported by findings from Becker et al. (2017) and Fetzer (2019), who find a significant effect of fiscal cuts on Leave votes in the 2016 Brexit referendum. These authors' measures of fiscal cuts are informed by the share of individuals who receive benefits at the district level, as well as data on those receiving benefits at the individual level. I expect similar signs on all control variables as those found in Guiso et al. (2017), though some may change owing to the smaller sample of countries[3].

Table 2.2 shows control variables typically used in related literature on electoral participation. It is important for this analysis also to look at electoral participation, as the same factors which affect voting behaviour may also affect participation. An equation which describes selection into voting (i.e participation) is thus necessary to deal with potential unobserved outcomes. Failure to include participation as a "selection" equation can lead to biased estimates of factors affecting voting behaviour (more of which in section 4). Guiso et al. (2017) show an important component of the economic insecurity mechanism driving populist votes to be an "abstention effect"; alongside the direct mechanism involving an increase in disappointment with traditional political parties leading to an increase in populist votes, economic insecurity leads to an increase in abstention amongst those whose economic insecurity increases disappointment in traditional parties. This abstention disproportionately affects traditional parties, such that the overall vote shares of populist parties are increased.

Age and sex appear frequently as pre-treatment control variables. The positive relationship between age and right-wing voting is by now well documented, and older voters are more likely to participate (Blais, 2000; Wolfinger & Rosenstone, 1980a). Consist-

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[3]These countries are Austria, Belgium, Denmark, Finland, France, Germany, Ireland, Italy, Sweden and the United Kingdom. Guiso et al. (2017) also looked at Bulgaria, Croatia, Cyprus, Czech Republic, Estonia, Greece, Hungary, Iceland, Latvia, Lithuania, Luxembourg, Malta, the Netherlands, Norway, Poland, Portugal, Romania, Slovakia, Slovenia, Spain and Switzerland.

ently, women vote less than men (Blais, 2000; Wolfinger & Rosenstone, 1980a) and are less likely to vote for nationalist or right-wing parties (Anelli et al., 2019), or express illiberal values (Colantone & Stanig, 2018a). As such, these are important explanatory variables which warrant inclusion in this analysis.

The relationship between political ideology and populist voting appears to depend to some degree on the population studied, with a much greater proportion of left-wing populists in Latin America (Edwards, 2019) and an association between right-wing sentiment and populist voting in Europe (Guiso et al., 2017). Owing to a change of sample in this analysis (I study fewer countries, which tend to have larger economies and populations), it's unclear how ideology will be related to populism or participation in this analysis. Its inclusion is nonetheless warranted to mitigate omitted variables bias.

Education levels are negatively associated with Leave votes in the 2016 Brexit referendum (Becker et al., 2017; Fetzer, 2019) and illiberal values (Colantone & Stanig, 2018a). We may thus expect a negative correlation with populist votes across Europe, though Guiso et al. (2017) find no significant association. They do however echo the results of Blais (2000) and Wolfinger and Rosenstone (1980b) in finding a positive association with participation.

Time spent watching TV and time spent watching political TV are control variables included by Guiso et al. (2017) to proxy for interest in politics. I include them in this analysis for the same reason. The authors find a positive effect of watching TV on populist voting, and no effect for watching political TV. As could be expected, the variables have opposing relationships with participation, increasing in political TV watching, and decreasing in general TV watching.

Anti-immigration rhetoric is a key feature of populist parties, especially in Europe, and a distrust of traditional political parties and governmental institutions are arguably the raison d'être for populist parties (Rodrik, 2018). As such, both attitudes are important inclusions as control variables. As Margalit (2019) points out, the inclusion of economic determinants as well as cultural/political attitudes in the same model of voting is potentially problematic; the relationship between these variables is likely complex and endogenous. Nonetheless, to the extent that we can argue they are distinct, they may help to more accurately measure the effect of economic determinants in general and the role of austerity policies in particular when included as control variables.

Unemployment, income difficulties and "exposure to globalisation" - captured through a dummy for working in blue collar manufacturing - form the constituent elements of the "economic insecurity" variable which is the main explanatory variable in Guiso et al. (2017), attained through PCA. Each are economic variables, increases in which could be expected to increase disappointment in the status quo, and induce voters to turn to parties offering short-term, simple policies to improve their economic situation. All are positively associated with populist voting or its correlates (Foster & Frieden, 2017; Galofré-Vilà et al., 2017), and often negatively associated with participation (Beramendi & Anderson, 2008; Wolfinger & Rosenstone, 1980b).

2.4   How does this research differ?

In this analysis I will also conduct principal component analysis, though I will modify the main explanatory variable, replacing "exposure to globalisation" with reliance on social benefits. This is motivated by Becker et al. (2017) and Fetzer (2019), who use reliance

on social benefits to proxy for exposure to austerity policies enacted following the 2008 financial crisis. This helps to better aim my analysis at austerity. See section 3 for the results of the PCA.

Another key difference with what has come before: I will incorporate data from Alesina et al. (2019), to create a variable which captures whether fiscal consolidation is occurring in the current year, previous year and country where the survey participant is responding. This way I can interact this with my "exposure to austerity" main explanatory variable to see how it affects populist voting decisions between years where austerity is occurring and when it is not. I will explain this variable in greater detail in section 3.

As such, whilst I am very much examining the economic insecurity mechanism hypothesised and first tested by Guiso et al. (2017), I am looking in greater detail at the role of austerity in this mechanism.

Finally, in this analysis I will use a different instrument for selection. Guiso et al. (2017) used regional rainfall data on election days. As these authors used a Heckprobit method, they required an instrument for selection into electoral participation. They argue that rainfall on election days is an exogenous factor that raises the "cost" of voting (by making it slightly more difficult to get to the polls), whilst not changing political preferences (the decision to vote populist or otherwise). The Heckprobit approach requires such a variable to be present in the selection equation, and not present in the outcome equation. I would have liked to use the same instrument in this analysis, but the relevant data was not accessible in an appropriate timescale.

I will use disability - whether a respondent is physically or mentally hampered in their daily activities, such that the "cost" of voting is increased, whilst having little effect on the decision of which party to vote for. This is a poor substitute for rainfall data, as disability is likely to be correlated with my main "exposure to austerity" explanatory variable, which partly contains reliance on social benefits. More on this in section 4.

The insights derived from the literature on other economic causes of populism, innovations in measurement of austerity to better examine its effects, and the study of control variables in the related literature lead to the formation of my main hypothesis: austerity policies enacted following the 2008 financial crash had the effect of increasing the economic insecurity of those who were most vulnerable to them, such that their disappointment with traditional parties increased, and they became both more likely to vote populist, and less likely to participate.

In sections 3 and 4 I will explain the variables in my analysis in more detail, and crucially how I structure my analysis so as to better test my hypothesis.

Table 2.1: Explanatory and control variables in related literature on voting outcomes

Dependent variables	Populist votes, Leave votes in Brexit referendum, right-wing parties, illiberal values, distrust towards political institutions.	
Control Variable	Sign	Related literature
Age	+	Anelli et al. (2019), Becker et al. (2017), Colantone and Stanig (2018a, 2018b)
	unreported	Fetzer (2018)
	insig.	Guiso et al. (2017)
Female	-	Anelli et al. (2019), Colantone and Stanig (2018a, 2018b), Guiso et al. (2017)
Right-wing	+	Guiso et al. (2017)
Risk aversion	insig.	Guiso et al. (2017)
Education	-	Becker et al. (2017), Colantone and Stanig (2018a, 2018b)
	unreported	Fetzer (2018)
	insig.	Guiso et al. (2017),
Hours of TV watched	+	Guiso et al. (2017)
Hours of political TV watched	insig.	Guiso et al. (2017)
Against non-EU migration	+	Guiso et al. (2017)
Trust in political parties	-	Guiso et al. (2017)
Austerity incidence	+	Becker et al. (2017), Fetzer (2018), Galofre et al. (2017)
Unemployment	unreported	Fetzer (2018)
	+	Becker et al. (2017), Foster and Frieden (2017), Galofre et al. (2017), Guiso et al. (2017),
Income difficulties	+	Becker et al. (2017), Guiso et al. (2017)
Blue collar manufacturing	+	Becker et al. (2017), Guiso et al. (2017)
Reliance on social benefits	+	Fetzer (2018)

Table 2.2: Explanatory and control variables in related literature on electoral participation

Dependent variable	Electoral participation	
Control Variable	Sign	Related literature
Age	+	Blais (2000), Guiso et al. (2017), Wolfinger and Rosenstone (1980)
Female	-	Blais (2000), Guiso et al. (2017)
Right-wing	+	Guiso et al. (2017)
Risk aversion	+	Guiso et al. (2017)
Education	+	Blais (2000), Guiso et al. (2017), Wolfinger and Rosenstone (1980)
Hours of TV watched	-	Guiso et al. (2017)
Hours of political TV watched	+	Guiso et al. (2017)
Against non-EU migration	-	Guiso et al. (2017)
Trust in political parties	+	Guiso et al. (2017)
Income difficulties	-	Beramendi and Anderson (2008), Blais (2000), Guiso et al. (2017), Wolfinger and Rosenstone (1980)
Unemployment	insig.	Blais (2000)
	-	Guiso et al. (2017)
Blue collar manufacturing	-	Guiso et al. (2017)

3 Data

3.1 European Social Survey

The main data source for this analysis is the European Social Survey (ESS)[1]. Conducted every two years, the ESS consists of interviews in the respondents' native language on a wide range of topics, including politics and values, socioeconomic indicators and subjective well-being. All explanatory and control variables - with the exception of my "austerity year" dummy variable (see subsection 3.2) - are taken from this data set. Data is available for the years 2002-2016, for a total of eight rounds. I use rounds one to seven, as data on fiscal consolidations was not available beyond 2014[2].

3.1.1 PCA results

From the socioeconomic indicators in the ESS it is possible to construct a variable which reflects "economic insecurity" by taking the first principal component of a selection of said indicators (Guiso et al., 2017). Following this method, I use slightly different indicators in order to construct my main explanatory variable, which might be better termed "exposure to austerity". This change turns out to be significant when investigating economic factors behind populist voting.

I considered using factor analysis (FA) to construct my "exposure to austerity" variable, but eigen values consistently below unity indicated that it was not suitable. By contrast, principal component analysis (PCA) - with its weaker assumptions about an underlying functional form - returned an eigen value above unity for the first principal component, and so it is the variable constructed using this method that informs my main result. More of which below.

I take the principal component of three variables in order to construct my "exposure to austerity" variable:

Unemployment status in the previous five years. Respondents were asked in an earlier question whether they had been unemployed for a period of three months or longer. They were then asked: *"Have any of these [unemployment] periods been within the past 5 years?"* This variable is designed to capture an element of economic insecurity and thus exposure to austerity, as unemployment typically of course leads to a lower income. The five year period excludes older periods of unemployment, which we may expect to influence voting decisions to a lesser extent. This variable was coded as a one in the affirmative and a zero otherwise. If the question was not applicable to the respondent, this was recorded as missing.

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[1]Guiso et al. (2017) also use this data set, though for a larger number of countries.

[2]This means I have cross-sectional survey data for the years 2002, 2004, 2006 and so on through to 2014.

Income difficulties. Respondents were asked how they felt about their current income level, with answers ranging from *"Living comfortably on present income"* (coded as a zero) to *"[Matters are] very difficult on present income"* (coded as a three). This subjective measure of income difficulties is important. Whilst it is clearly a variable which reflects economic conditions, there is likely heterogeneity in response amongst participants for a given income level, owing to its subjectivity. As such, it is also attitudinal.

Reliance on social benefits. This is a dummy variable which takes the value one when the main source of income in the respondent's household is social benefits. This can be unemployment benefits, or any other type of benefit except pensions. I exclude pensions as this variable is intended to capture individuals who are exposed to government spending cuts in times of austerity, and there is no distinction between public and private pensions in the data. Considering recent evidence on the link between cuts to benefits and other social spending and political discontent (see subsection 2.3), I expect that the component which captures variation in this variable, together with unemployment and income difficulties, will reflect individuals who stand to be most affected by austerity policies; in particular, those austerity policies which are driven by cuts to public spending on social benefits.

Guiso et al. (2017) also use the first two of the above variables in constructing their "economic insecurity" variable. Instead of reliance on social benefits, however, they focus more on globalisation with the variable:

Exposure to globalisation. This variable is constructed from several others, encompassing type of occupation and skill level. Blue collar workers in manufacturing are considered to be more exposed to globalisation, so they take a value of one, whilst others take a value of zero.

Table 3.1 shows the results of the PCA. The first component returns an eigenvalue of approximately 1.6, whilst the second and third components have eigenvalues below unity, with a large difference (~ 0.8) between the first and the second. This first component also explains a fairly large proportion of the data (~ 0.53). Following the Kaiser rule, I take the first component forwards for the rest of the analysis.

Table 3.2 shows the eigenvector for the first component. Each of the component loadings is fairly high for all of the original variables, indicating that PCA for these variables was suitable.

Table 3.1: Principal component correlation

Component	Eigenvalue	Difference	Proportion	Cumulative
Comp1	1.59582	0.838489	0.5319	0.5319
Comp2	0.757329	0.110477	0.2524	0.7844
Comp3	0.646852	-	0.2156	1

Number of obs.	96,310	Rotation: (unrotated = principal)		
Number of comp.	1			
Trace	3			
Rho	0.5319			

Can we label this component "exposure to austerity"? This question raises two others. First, does this variable differ substantively from the "economic insecurity" variable constructed by Guiso et al. (2017)? I have constructed this "economic insecurity" vari-

Table 3.2: Principal component eigenvector

Variable	Comp1	Unexplained
Unemployed	0.5547	0.5089
Reliance on social benefits	0.6097	0.4068
Income difficulties	0.5662	0.4885

able as well, and the results of the PCA can be found in appendix A for comparison. I will also perform my main analysis using both variables and present them together as a robustness check, to see whether this variable really takes a better aim at austerity as a cause of populist voting.

Second, what else could this component represent? The component loads positively on the three input variables. Is there some unobserved cultural attitude or predisposition which correlates highly with all of them? We cannot know for sure. However, I will control for several cultural/attitudinal variables in my main analysis to allay these fears somewhat.

3.1.2 Dependent variables

The main dependent variables in this analysis are "voted populist in the most recent national election" and "voted in the most recent national election". Whilst the key variable of interest is of course populist voting, the participation variable is necessary to deal with potential selection bias due to unobserved outcomes. More of which in section 4. For now, I will briefly explain how each of these variables are made.

Electoral participation. Survey participants were asked "Some people don't vote nowadays for one reason or another. Did you vote in the last [country] national election in [month/year]?" Individuals took a value of one if they answered in the affirmative and zero otherwise.

Populist voting. In the event that participants responded to the participation question in the affirmative, the ESS contains country-specific questions which ask which party they voted for in the most recent election. As mentioned in section 2.1, I follow the definition of Guiso et al. (2017) in classifying parties as populist or otherwise. Parties are thus classed as populist if they satisfy three conditions:

1. They claim to be on the side of the public against the elite.

2. They speak to specific fears/ennthusiasms of people, such as increased immigration, European integration or cultural erosion.

3. They express disregard the long-term consequences of their policies.

The list of parties can be found in appendix A. The variable was thus coded with a one if the respondent said they voted for one of these parties, and zero if they did not. Responses were coded as missing if they had not answered the electoral participation question in the affirmative.

3.1.3 Control variables

As discussed in section 2.3, control variables are added for the purpose of controlling for pre-treatment characteristics, as well as attitudinal, ideological, and economic variables which have an established relationship with my populist voting and electoral participation outcome variables. These variables, if absent, could bias estimates of the main explanatory variable "exposure to austerity". The variables are as follows:

Age. Participants' age in years at the time the survey was recorded, ranging from 18 to 100.

Education. Education levels were recorded as the number of years of formal, full-time education the participant had completed, ranging from zero to 25 years.

Female. This variable is coded as a one if the respondent is female and zero otherwise.

Risk aversion. Risk aversion is proxied using a survey question which asked how important it is to seek adventures and have an exciting life. This was ascertained by showing respondents a character card describing an adventurous person. The respondents were then asked how similar they thought they were to the character. Coded from one to six, answers were decreasing in similarity with the character; thus, increasing in risk aversion.

Total TV and Political TV. These variables proxy for interest in politics, with a high amount of political TV watching proxying for high interest in politics, and a high amount of regular TV watching proxying for low interest in politics. Each are coded from zero to seven, zero meaning zero hours a week, and seven meaning more than three hours a week.

Right wing. This variable is a left-right scale, ranging from zero to 10, and increasing in right-wing sentiment. Respondents had to choose where they would place themselves on this scale.

Trust in political parties. Respondents were asked the extent to which they have trust in political parties, on a scale from zero to 10. Considering the anti-establishment nature of populist parties, this seems an important control variable.

Against non-EU migration. This control captures attitudes to non-EU migration. Respondents were asked how many immigrants from poorer countries outside Europe should be allowed to come and live and work in the respondent's home country, ranging from 1 - "allow many to come and live here" to 4 - "allow none".

3.1.4 Instrument

The Heckprobit method requires an instrument for sample selection, or in the context of this paper, an instrument for electoral participation. To this end, I use disability:

Hampered in daily activities. Respondents were asked "Are you hampered in your daily activities in any way by any longstanding illness, or disability, infirmity or mental health problem? If yes, is that a lot or to some extent?" Answers were coded as follows: 1 - "No" 2 - "Yes, to some extent" 3 - "Yes". The idea behind this is that disability raises the "cost" of going to vote. Almost all countries require voters to vote in person or to send a postal vote ahead of the election; both of these options require either physical movement, which is more difficult with a physical disability, or more forward-planning.

3.2 Austerity data

The innovation in this paper is introducing data on fiscal consolidation periods from Alesina et al. (2019) in order to model the role of austerity on populist voting. Fiscal consolidation is defined as a government policy or group of policies designed to reduce government deficits or debt accumulation. As explained in the literature section of this paper, measuring austerity and its effect on other variables is fiendishly difficult, owing to its endogeneity as a policy response to prevailing economic conditions. Building upon the work of Devries et al. (2011)[3] and following the narrative approach pioneered by Romer and Romer (2010)[4], Alesina et al. (2019) present a new data set[5] which has been compiled using text analysis - identifying as exogenous those fiscal consolidations which were stated to have been instigated in the name of long-term deficit reduction.

In this data set, there is no mathematical decision rule as such which leads to the labelling of a particular country in a particular year as having undergone fiscal consolidation, except of course that the country indeed introduces spending cuts or tax hikes (or both) which in the short run would be expected to have a positive budgetary effect. Consolidations in the name of short-term cyclical adjustment are considered endogenous. For all exogenous fiscal consolidation years which are included in the data set, Devries et al. (2011) and later Alesina et al. (2019) provide motivation as to why they can plausibly be argued to be so. For example, Germany in 2003 introduced tax hikes which totalled 0.74% of GDP. This is labelled as exogenous based on contemporaneous government documents, such as the annual report 2002/3 from the German Council of Economic Advisors. This states (p. 30): "Public finance is veering out of control - budgetary consolidation must be resolutely continued. The general government budget deficit of 3.7 percent in 2002 breaches the criteria laid down in the Stability and Growth Pact" and later (p. 32): "The rules of the European Stability and Growth Pact are not 'stupid.' It was right to adopt the Stability Pact and it is right to continue to uphold it".

The data set allows for interesting comparisons, such as distinguishing between fiscal consolidations which were driven by spending cuts or by tax increases[6]. One disadvantage, however, is that it covers 16 countries, 10 of which are common to both data sets. As a result, the analysis of this paper is restricted to these 10 countries. This means many fewer observations, but as the ESS data is at the individual level we are left with enough statistical power for the main analysis.

Table 3.3 shows the countries involved in the austerity analysis and the years which were labelled as fiscal consolidation years in Alesina et al. (2019). These data are used to create a dummy variable:

Austerity year. This variable equals one when, in a given country, the current year or year preceding it was a fiscal consolidation year according to the conditions of exogeneity above described, and zero otherwise. This can then be used to split the analysis to see how my "exposure to austerity" variable is associated with populist voting and electoral participation in austerity years vs. otherwise.

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[3]This data set covered OECD countries.

[4]These authors' data set covered the United States

[5]Countries in the Alesina data set: Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Ireland, Italy, Japan, Portugal, Spain, Sweden, United Kingdom and United States.

[6]The above example from Germany was labelled as a tax-based consolidation. Fiscal consolidation periods are rarely composed of tax-hikes and spending-cuts in similar measure; one method is almost always decidedly dominant.

Expenditure-based (EB) and revenue-based (TB) fiscal consolidation years.
For the analysis concerning expenditure-based (composed primarily of public spending-cuts) and revenue-based (composed mainly of tax increases) fiscal consolidation, I take advantage of the distinctions in the Alesina et al. (2019) data set to create dummy variables for each type of fiscal consolidation year. The EB variable takes a value of one when, in a given country, the current year or previous year was an expenditure-based fiscal consolidation year, i.e. when said fiscal consolidation was based primarily on spending-cuts, and zero otherwise. The same goes for my TB variable with tax-hikes. Where an expenditure-based year was immediately followed by a revenue-based year (and vice versa), these years were excluded.

Table 3.3: Fiscal consolidation years

Country	Year
Austria	$2001_s, 2011_t, 2012_s, 2014_s$
Belgium	$2010_s, 2011_t, 2012_s, 2013_s$
Denmark	$2009_t, 2010_s, 2012_s$
Finland	$2010_t, 2012_t, 2014_s$
France	$2010_s, 2011_s, 2012_t, 2013_s, 2014_s$
Germany	$2003_t, 2004_s, 2006_s, 2011_t, 2012_s$
Ireland	$2008_s, 2009_t, 2010_s, 2011_s, 2012_s, 2013_s, 2014_s$
Italy	$2004_t, 2005_s, 2006_s, 2007_t, 2010_s, 2011_t, 2012_t, 2013_s, 2014_t$
United Kingdom	$2010_s, 2011_s, 2012_s, 2013_s, 2014_s$

$_t$ denotes fiscal consolidation years which were driven in large part by tax increases.

$_s$ denotes those driven in large part by spending cuts.

3.3 Descriptive statistics

The variables of interest and control variables involved in the analysis are shown in Table 3.4. All figures are for the ten European countries used in the analysis[7] and the first seven rounds of the ESS. The number of observations varies, owing to missing values for different questions on the survey. For example, the number of observations for "Voted populist" is necessarily smaller than those for "Voted", as the former is only observed when the latter is answered in the affirmative. Answers "don't know", "refused to answer" or "not applicable" are recorded as missing values, further contributing to this variation in observations.

All of the economic variables behave in the way we might expect, when we consider that austerity policies in the short run decrease private wealth, either through spending-cuts or tax-hikes, and that 37 of the 45 austerity years (82%) in the data take place during or after the financial crash of 2008. Unemployment is higher in austerity years, as are income difficulties and reliance on social benefits. One surprise is that "exposure to globalisation" - proxied by blue collar employment in manufacturing - has a mean value of 0.18 in non-austerity years and 0.1 in austerity years. Although we may expect a decline in manufacturing employment for the above mentioned reasons as well as general

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[7] These countries are Austria, Belgium, Denmark, Finland, France, Germany, Ireland, Italy, Sweden and the United Kingdom.

industrial decline in Europe, coupled with import competition from developing countries, the magnitude of this difference is extreme.

Populist voting is higher in austerity years than otherwise, while electoral participation was slightly lower, motivating the analysis which follows in section 4. Interestingly, risk aversion was slightly higher in austerity years, indicating that individuals might be less inclined to take risks when times are economically tougher. The changes in the other attitudinal variables are fairly small, though individuals seem to have lower trust in political parties, and to be more against non-EU migration in austerity years.

Table 3.4: Descriptive statistics

Variable	Obs.		Mean		St. Dev.		Min.	Max.
	Non-Aust.	Aust.	Non-Aust.	Aust.	Non-Aust.	Aust.		
Unemployment	59,971	59,294	0.12	0.14	(0.32)	(0.35)	0	1
Income difficulties	57,002	59,020	0.72	0.91	(0.75)	(0.79)	0	3
Exposed to globalisation	60,559	59,343	0.18	0.1	(0.39)	(0.30)	0	1
Reliance on social benefits	44,663	53,910	0.06	0.09	(0.24)	(0.28)	0	1
Economic insecurity	56,442	58,973	0.18	0.21	(0.20)	(0.21)	0	1
Exposure to austerity	42,631	53,679	0.13	0.17	(0.19)	(0.22)	0	1
Voted	57,172	55,885	0.84	0.81	(0.36)	(0.40)	0	1
Voted populist	48,125	45,024	0.04	0.07	(0.20)	(0.26)	0	1
Age	60,559	59,343	48.36	49.68	(17.83)	(17.82)	18	100
Education	60,559	59,343	12.61	13.25	(3.77)	(3.68)	0	25
Female	60,525	59,334	0.52	0.52	(0.50)	(0.50)	0	1
Risk aversion	55,062	58,465	3.92	4.01	(1.40)	(1.42)	1	6
Total TV	60,467	59,304	4.22	4.31	(2.00)	(2.04)	0	7
Political TV	58,642	57,021	2.06	1.98	(1.29)	(1.30)	0	7
Right-wing	55,913	54,609	5.16	4.92	(2.11)	(2.01)	0	10
Trust in political parties	46,336	53,602	4.28	3.59	(2.24)	(2.23)	0	10
Against non-EU migration	59,109	58,062	2.41	2.56	(0.86)	(0.87)	1	4
Hampered in daily activities	60,343	59,267	1.31	1.33	(0.58)	(0.59)	1	3
Austerity year	60,559	59,343	0.49		(0.50)		0	1

The table shows summary statistics for all variables used in the main analysis, in non-austerity years and austerity years. Data sources: European Social Survey, Alesina et al. (2019).

4 Empirical Strategy

4.1 Specifications

This analysis aims to investigate a proposed "austerity channel" of the economic insecurity mechanism underlying populist voting specifically by examining how my "exposure to austerity" variable acts differently in exogenously defined fiscal consolidation years and otherwise. A simple probit estimation of the probability of voting for a populist party given the observables used to construct my "exposure to austerity" variable could return biased estimates. This is in part because we only observe the binary outcome "voted populist" in the instance that an individual has voted at all. Factors which affect the participation decision are correlated with those which affect the party-choice decision, often with opposite signs. For example, a high level of trust in traditional political parties will likely encourage an individual to participate in an election, while discouraging them from voting for a populist party. By contrast, I hypothesize that an individual's level of "exposure to austerity" will increase their likelihood of voting for a populist party, whilst decreasing the likelihood that they participate at all, and that the size of these effects will differ according to whether a country is enacting fiscal consolidation policies.

To test this, I use a two stage Heckprobit approach. Selection equation 4.1 below functions as a first stage, and outcome equation 4.2 as a second stage. It could be the case that an omitted variable causes correlation between the error terms of the first (4.1) and second (4.2) stages, leading to biased coefficient estimates of my "exposure to austerity" variable. The Heckman approach is used in this event to deal with sample selection bias, because we only observe individuals' political party preferences in the instance that they choose to vote.

Take, for example, trust in political parties[1]. If trust is positively associated with electoral participation, but is omitted from the selection equation, it will be positively correlated with the error term of that equation (4.1). If trust is negatively associated with populist voting, and omitted from the outcome equation, it will be negatively correlated with the error term of that equation (4.2). This will cause the error terms of the two equations to be negatively correlated. If the true relationship between exposure to austerity and trust in political parties is positive, then estimates of this relationship will be biased upwards.

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[1]This is included as a control variable in this analysis, but illustrates well where sample selection bias could arise.

With this in mind, I estimate the two-step Heckprobit model:

$$z_{it} = \gamma_1 Hamp_{it} + \gamma_2 Exp_{it} + \gamma_3 Aust_{jt} + \gamma_4 Exp_{it} Aust_{jt} + \gamma_5 X_{it} + \alpha_j + \delta_t + u_{ijt} \quad (4.1)$$

$$y_{it} = \beta_1 Exp_{it} + \beta_2 Aust_{jt} + \beta_3 Exp_{it} Aust_{jt} + \beta_4 X_{it} + \alpha_j + \delta_t + \varepsilon_{ijt} \quad (4.2)$$

Where equation 4.1 above is the first stage, and equation 4.2 is the second stage. z_{it} is a dummy for electoral participation for individual i at survey-round t, and y_{it} is a dummy for populist voting. Note that the Heckprobit model takes into account the selection effect into voting, such that y_{it} is only observed when the latent $z_{it} > 0$, that is, when an individual casts a vote.

Exp_{it} is my measure of "exposure to austerity" for individual i in survey-round t. Note that as have pooled cross-sectional data, I do not observe the same individual i across different survey-rounds t. $Aust_{jt}$ is a dummy which which equals one when country j - of which individual i is a resident - is enacting austerity measures according to the Alesina et al. (2019) definition. The variable $Hamp_{it}$ is my instrument for selection into electoral participation. α_j and δ_t are sets of country and ESS-round fixed effects respectively, and u_{ijt} and ε_{ijt} are error terms. In examining how an individual's exposure to austerity as a determinant of populist voting differs across different fiscal policy environments, the coefficient (β_3) of interaction term $Exp_{it} Aust_{jt}$ is of interest. X_{it} is a vector of control variables, which includes age, sex, risk aversion, education level, gender, proxies for interest in politics, political ideology, trust in political parties, and attitudes to non-EU immigration. An important aspect of this data set is that I have information on these characteristics both for those who voted and those who didn't. This is a significant advantage over most voting data, as it removes a great deal of uncertainty with regards to selection. The only uncertainty that we're left with is the party-voting preference of individuals who didn't vote - if they had voted, would they have voted for a populist party?

In the presence of sample selection bias, an instrument for selection into electoral participation is warranted. $Hamp_{it}$ is a measure of the extent to which an individual reports being hampered in daily activities for reasons of illness, infirmity, or disability. I tried this instrument because this measure could plausibly be argued to raise the "cost" of voting whilst being unrelated to the political preferences of the individual.

I test the strength of this instrument by calculating the inverse mills ratio (IMR) following the instrument's inclusion in the the selection equation. A low correlation between this IMR and my main explanatory variable "exposure to austerity" indicates a strong instrument for selection. Unfortunately, this is not case. I return a correlation of 0.6, which is higher than would be desirable.

This is likely because individuals who are hampered in daily activities are more likely to claim social benefits, to experience income difficulties, and to have experienced unemployment.

A common test for the presence of sample selection bias in the model is to examine the correlation between the residuals (u_{ijt} and ε_{ijt}) in the selection and outcome equations. Following my above example of how sample selection bias can arise, a finding of zero correlation between these residuals can be an indication that no sample selection is present. I indeed find no correlation between these residuals. However, when using a

weak instrument (signified by a high correlation between Exp_{it} and the IMR), the likelihood of finding a significant correlation between these residuals is reduced, even in the presence of sample selection bias. Therefore, I cannot conclude that there is no sample selection bias in the model.

This is a significant setback for the analysis. To be clear, this development does not mean that sample selection bias is certainly present, but rather that we cannot rule out its presence, calling the credibility of my main results into question. At this stage it is also worth noting that the Heckman approach is only employed to deal with potential sample selection bias. There are other potential sources of endogeneity which could arise in estimation.

4.2   Other endogeneity problems

Controls and fixed effects are also included to deal with other sources of endogeneity[2]. My control variables are included to deal with omitted variables bias. If a variable is omitted from my outcome equation, and is correlated with both my dependent variable (populist voting) and my main explanatory variable (exposure to austerity), it will appear in the error term, leading to a violation of one of the key assumptions of the model, where regressors should be uncorrelated with the error term. The same is true for my selection equation.

I include country and survey-round fixed effects[3]. The country fixed effects remove the effect of time-invariant factors which differ across countries and could affect populist votes. The survey-round fixed effects remove country-invariant factors which differ across survey rounds. Together, these fixed effects mean the analysis is still vulnerable to unobserved country and survey-round invariant factors which are important in driving populist votes and electoral participation.

There are advantages and disadvantages to the use of fixed effects here. On the one hand, they appear to help remove signs of selection bias from the model[4]. However, by removing country and time-variant factors, they also remove a great deal of explanatory power. Their use in this model is warranted, as the purpose of the analysis is to investigate an austerity channel through which the economic insecurity mechanism operates, thereby suggesting that austerity policies matter for populist voting, whilst accounting for endogeneity problems as much as possible.

The use of control variables and fixed effects unfortunately leave room for omitted variables bias. For example, factors which are time and country-invariant could be correlated both with individuals' exposure to austerity policies and their decision to vote for a populist party. Without plausibly exogenous variation in either measures of exposure to austerity or austerity policies themselves, this omitted-variables problem persists.

Additional endogeneity problems are present when interpreting the coefficient of the interaction terms in my outcome and selection equations. The purpose of the narrative method used to identify periods of austerity which inform the Alesina et al. (2019) data set - and thereby the "austerity year" dummy variable in my analysis - is to identify plausibly exogenous austerity measures. However, these decisions are not made in isolation, and

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[2]Though controls and fixed effects can also mitigate sample selection bias.

[3]These are not the same as year fixed effects, as I have repeated cross-sectional data, with surveys conducted every two years on different cross-sectional samples.

[4]With the caveat that we cannot be sure about this given the weak instrument.

are frequently a response to prevailing economic conditions, despite their stated aim of reducing government deficits. Something approaching a consensus in the necessity of austerity policies throughout Europe emerged following the 2008 financial crash and subsequent Great Recession and Euro debt crisis. As such, examining heterogeneous effects of economic drivers of populist voting across these years risks examining how these effects differed as a result of the Great Recession, instead of as a result of said austerity policies.

5 Results

5.1 Austerity analysis

Table 5.1 shows simultaneous Heckprobit estimations of specification equations 4.2 and 4.1. All specifications contain ESS-round and country fixed effects and are estimated using robust standard errors[1]. Additionally, population weights are included to account for differences in sample sizes between countries, and post-stratification weights are included to reduce sampling error and possible non-response bias. In model (1) we see the main specification with some controls and no interaction between exposure to austerity and austerity year. Model (2) adds the interaction. Model (3) shows the main specification with all controls and no interaction, which is added in model (4).

In all four models, "exposure to austerity" has a positive and statistically significant association with populist voting. Recall that this variable has been amended to include reliance on social benefits, and remove exposure to globalisation. Additionally, this same variable is negatively associated with participation in all regressions, indicating the presence of two effects. A person's exposure to austerity raises the likelihood that they will vote for a populist party, while decreasing the likelihood that they will participate in elections at all. Taking model (3) and quantifying: at sample means, a one standard deviation increase in "exposure to austerity" increases an individual's probability of voting populist by 29%, a fairly substantial increase.

Recall that "austerity year" is a dummy variable which equals one when the country in which a survey respondent resides is undergoing fiscal consolidation according to the Alesina et al. (2019) definition. Interestingly, the coefficient estimates for this variable in models with controls and fixed effects indicate populist voting was lower in austerity years than otherwise; at sample means, the effect of being in a country undergoing austerity is to decrease the probability of voting populist by 33%. In all models there is no evidence of a change in participation across year types, signified by coefficients which do not differ significantly from zero in the participation equations. The interaction of "exposure to austerity" and "austerity year" in models (2) and (4) is positive and statistically significant in the outcome equation. This provides evidence of heterogeneous economic effects on populist voting and abstention across different year types. That is to say, whilst populist voting appears lower in countries undergoing austerity, individuals who are more greatly exposed to austerity are more likely to vote for populist parties in those same countries enacting austerity. This effect heterogeneity is quite large: in non-austerity years, the effect of a one standard deviation increase in "exposure to austerty" is to increase the chance of voting populist by 6%. In austerity years, this rises to 47%. The interaction in the participation equation is statistically significant and negative, indicating that whilst participation does not on average differ across different fiscal-year types, the

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[1]Estimations with fewer controls and no fixed effects can be found in appendix A.

abstention effect as driven by economic factors is stronger in fiscal consolidation years.

All of the control variables behave in the way that might be expected given the precedents set in related literature[2]. Risk aversion seems unrelated to populist voting, though it has a significant positive effect on participation. This is also true for age, reflecting the established result that older individuals are more likely to participate in elections. TV watching as a proxy for interest in politics is indeed negatively associated with participation, and interestingly, positively related to populist voting. The variable female is negatively associated with participation, again matching the established literature, and also negatively related to populist voting. The reason for this is unclear, and requires investigation beyond the scope of this analysis to be understood better.

One surprise is with the coefficient for "left-right scale". This shows that in this sample populist voters tend to be more left-wing, where previous research found more right-wing populist sentiment in Europe. It is notable also that the addition of "trust in political parties" and "against non-EU migration" reduces the coefficients for "exposure to austerity" in both the populist voting and participation regressions. I suspect this is because in models (1) and (2) "exposure to austerity" was picking up some of this variation. These are exactly the type of attitudinal variables which if omitted can lead to omitted variables bias, and speaks to the inherent difficulties in trying to separate economic and cultural causes, given the endogeneity of each to the other.

5.1.1 Different austerity types

In order to further examine how economic determinants of populism differ according to prevailing fiscal policy, I take advantage of the distinction in the Alesina et al. (2019) data set between expenditure-based and revenue-based fiscal consolidation. Table 5.2 shows results from estimation of the Heckprobit model with controls and country fixed effects in two types of fiscal consolidation year. The first model (EB) reports estimation of the main specification (with no interaction) when the survey respondent is in a country undergoing expenditure-based consolidation, while the second model (TB) reports results under revenue-based consolidation. Unfortunately, there are not enough observations when examining these conditions to be confident about results, especially in the case of revenue-based consolidation. Many coefficients lose significance in these smaller samples. Nonetheless, we see a positive and statistically significant coefficient for the "exposure to austerity" variable in the outcome equation when under expenditure-based consolidation, where the corresponding coefficient under revenue-based consolidation is insignificant. This result at least warrants further investigation. Given the inclusion of reliance on social benefits in the construction of the "exposure to austerity" variable, we may expect stronger effects in expenditure-based fiscal consolidation years, though it is hard to have confidence at this stage.

Also of note in table 5.2 are the coefficients for the two specifications for "left-right scale" in the outcome equation. These coefficients are statistically significant and change signs from one model to the next. Under expenditure-based consolidations, populist voters tend to be more left-wing, whilst in revenue-based consolidations they tend to be more right-wing.

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[2]Especially Guiso et al. (2017), who use the same control variables on a larger sample of countries; see section 2.3.

Table 5.1: Heckprobit estimation results

	(1)		(2)		(3)		(4)	
	Pop. vote	Particip.	Pop. vote	Particip.	Pop. vote	Particip.	Pop. vote	Particip.
Hampered in daily activities		-0.0805***		-0.0810***		-0.0669***		-0.0674***
		(0.0155)		(0.0155)		(0.0155)		(0.0156)
Exposure to austerity	0.633***	-0.923***	0.359***	-0.693***	0.546***	-0.865***	0.296***	-0.659***
	(0.100)	(0.0426)	(0.103)	(0.0698)	(0.0907)	(0.0435)	(0.0986)	(0.0714)
Austerity year	-0.0962**	-0.0362	-0.154***	0.0185	-0.174***	0.00637	-0.225***	0.0552
	(0.0443)	(0.0349)	(0.0480)	(0.0380)	(0.0454)	(0.0356)	(0.0494)	(0.0387)
Austerity interaction			0.360***	-0.315***			0.319***	-0.282***
			(0.121)	(0.0836)			(0.123)	(0.0855)
Risk aversion	-0.00375	0.0340***	-0.00450	0.0343***	-0.00820	0.0353***	-0.00868	0.0355***
	(0.0113)	(0.00673)	(0.0113)	(0.00673)	(0.0115)	(0.00679)	(0.0115)	(0.00679)
Age	-0.00138	0.0192***	-0.00156	0.0192***	-0.00195	0.0197***	-0.00202	0.0197***
	(0.00145)	(0.000655)	(0.00148)	(0.000655)	(0.00141)	(0.000672)	(0.00142)	(0.000672)
Education	-0.0166***	0.0570***	-0.0171***	0.0570***	-0.00926*	0.0541***	-0.00943*	0.0542***
	(0.00533)	(0.00289)	(0.00541)	(0.00289)	(0.00522)	(0.00299)	(0.00524)	(0.00299)
Total TV	0.0553***	-0.0529***	0.0560***	-0.0530***	0.0477***	-0.0495***	0.0481***	-0.0496***
	(0.00934)	(0.00535)	(0.00935)	(0.00535)	(0.00934)	(0.00546)	(0.00934)	(0.00546)
Political TV	-0.000616	0.0711***	-0.00132	0.0710***	0.0136	0.0605***	0.0133	0.0605***
	(0.0131)	(0.00804)	(0.0131)	(0.00804)	(0.0130)	(0.00816)	(0.0130)	(0.00815)
Female	-0.161***	-0.0307*	-0.160***	-0.0310*	-0.153***	-0.0366**	-0.153***	-0.0368**
	(0.0275)	(0.0175)	(0.0275)	(0.0175)	(0.0282)	(0.0178)	(0.0282)	(0.0178)
Left-right scale	-0.0279***	0.00698	-0.0282***	0.00707	-0.0319***	0.00893**	-0.0321***	0.00896**
	(0.00733)	(0.00436)	(0.00733)	(0.00436)	(0.00787)	(0.00453)	(0.00787)	(0.00453)
Trust in political parties					-0.0833***	0.0553***	-0.0834***	0.0553***
					(0.00798)	(0.00445)	(0.00798)	(0.00445)
Against non-EU migration					0.124***	-0.0447***	0.123***	-0.0439***
					(0.0184)	(0.0111)	(0.0184)	(0.0111)
Observations	78877		78877		77248		77248	
Fixed effects	Yes		Yes		Yes		Yes	
Non-selected observations	12501		12501		12151		12151	
SE	Robust		Robust		Robust		Robust	
Rho	-0.0323		-0.0579		-0.0575		-0.0674	
Log likelihood	-53649.4		-53626.4		-51892.2		-51874.5	

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5.2: Expenditure-based (EB) vs. revenue-based (TB) fiscal consolidations

	EB		TB	
	Pop. Vote	Particip.	Pop. Vote	Particip.
Hampered in daily activities		-0.0272		-0.187**
		(0.0271)		(0.0733)
Exposure to austerity	0.347***	-0.756***	0.267	-0.826***
	(0.123)	(0.0761)	(0.272)	(0.204)
Risk aversion	-0.00439	0.0372***	-0.0205	0.0134
	(0.0199)	(0.0119)	(0.0327)	(0.0320)
Age	0.00315*	0.0220***	-0.00608*	0.0111***
	(0.00173)	(0.00119)	(0.00333)	(0.00287)
Education	-0.00818	0.0592***	-0.00515	0.0378***
	(0.00736)	(0.00505)	(0.0116)	(0.0106)
Total TV	0.0287*	-0.0420***	0.0508*	-0.0414
	(0.0161)	(0.00970)	(0.0300)	(0.0254)
Political TV	0.0440**	0.0571***	0.0414	0.00837
	(0.0203)	(0.0140)	(0.0400)	(0.0383)
Female	-0.0584	-0.0588*	-0.159*	0.0565
	(0.0504)	(0.0320)	(0.0898)	(0.0821)
Left-right scale	-0.0391***	0.00531	0.151***	0.00983
	(0.0125)	(0.00788)	(0.0210)	(0.0186)
Trust in political parties	-0.0931***	0.0413***	-0.0611**	0.0318*
	(0.0124)	(0.00783)	(0.0245)	(0.0178)
Against non-EU migration	0.171***	-0.0383**	0.0756	-0.0406
	(0.0318)	(0.0193)	(0.0585)	(0.0489)
Observations	22481		10829	
Fixed effects	Yes		Yes	
Non-selected observations	4131		1279	
SE	Robust		Robust	
Rho	0.429		-0.142	
Log likelihood	-15618.9		-4643.6	

The table shows Heckprobit estimations of the mains specification in two types of fiscal consolidation year, expenditure-based and revenue-based.
Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

5.2   Robustness

One concern about this analysis might be that the variable "exposure to austerity" may not differ substantively from the "economic insecurity" variable created by Guiso et al. (2017). These principal component variables have both unemployment and income difficulties in common, with the difference between them amounting to the replacement of "exposure to globalisation" with "reliance on social benefits" (see section 3.1.1).

This concern is addressed in table 5.3, which shows estimates of the key coefficients for the main specification with the "economic insecurity" variable of Guiso et al. (2017) in model (1), and with the "exposure to austerity" variable constructed for this paper in model (3). Interactions with "austerity year" are added in models (2) and (4) respectively. As expected, coefficients for the two variables do not significantly differ, with the notable exception of their interaction terms. The coefficient of the interaction of the "economic insecurity" variable with "austerity year" in the populist voting outcome equation is not statistically significant. Whereas, the corresponding coefficient for the "exposure to austerity" interaction with "austerity year" is statistically significant and positive. This result indicates that the "exposure to austerity" variable is well specified, and provides evidence that the economic insecurity mechanism as a determinant of populist voting itself operates through various channels, of which vulnerability to changing fiscal policies is one, and exposure to forces of globalisation is another.

Another concern is with the disability instrument used to mitigate sample selection bias in the model. In all regressions, a Wald test of independent equations suggests that we cannot reject the null hypothesis that the outcome and selection equations are independent. Table 5.4 shows estimations of the main specification equations, estimated as separate probit models. There is no substantive change in any of the coefficients as compared with the Heckprobit results in the main analysis. Together with the aforementioned Wald tests, this can mean two things. Firstly, conditional on the instrument, controls and fixed effects, there is no sign of sample selection bias. Secondly, the weak instrument might not be mitigating sample selection bias, and this bias is still present in the model. Considering the weakness of the instrument (see section 4.1), we can therefore not rule out the second of these possibilities.

Finally, I drop each of the countries from the analysis one by one, to see if this changes the results. I was particularly concerned about Italy, as the increase in probability of voting populist associated with being observed in Italy was quite high compared to other countries. However, dropping Italy from the analysis does not change the results. The removal of any country did not change results for the coefficient on "exposure to austerity", "austerity year" or any of the other control variables. However, the interaction between "exposure to austerity" and "austerity year" did lose significance if either Germany or Belgium were removed, suggesting these countries may be driving the heterogeneous effects result.

Table 5.3: Economic insecurity comparison

	(1)		(2)		(3)		(4)	
	Pop. vote	Particip.	Pop. vote	Particip.	Pop. vote	Particip.	Pop. vote	Particip.
Exposure to austerity	0.546***	-0.865***	0.296***	-0.659***				
	(0.0907)	(0.0435)	(0.0986)	(0.0714)				
Economic insecurity					0.571***	-0.825***	0.549***	-0.555***
					(0.0783)	(0.0440)	(0.0953)	(0.0707)
Austerity year	-0.174***	0.00637	-0.225***	0.0552	-0.177***	0.0112	-0.183***	0.0992**
	(0.0454)	(0.0356)	(0.0494)	(0.0387)	(0.0450)	(0.0352)	(0.0521)	(0.0406)
Exp. aus. interaction			0.319***	-0.282***				
			(0.123)	(0.0855)				
Econ. insecurity interaction							0.0265	-0.377***
							(0.125)	(0.0859)
Observations	77248		77248		79164		79164	
Fixed effects	Yes		Yes		Yes		Yes	
Controls	Yes		Yes		Yes		Yes	
Non-selected observations	12151		12151		12387		12387	
SE	Robust		Robust		Robust		Robust	
Rho	-0.0575		-0.0674		0.0415		0.0441	
Log likelihood	-51892.2		-51874.5		-52522.6		-52496.6	

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5.4: Separate probit estimations

	(1) Pop. vote	(2) Particip.	(3) Pop. vote	(4) Particip.
Hampered in daily activities		-0.0673***		-0.0679***
		(0.0153)		(0.0153)
Exposure to austerity	0.520***	-0.864***	0.275***	-0.658***
	(0.0706)	(0.0435)	(0.0916)	(0.0714)
Austerity year	-0.173***	0.00624	-0.223***	0.0551
	(0.0454)	(0.0356)	(0.0493)	(0.0387)
Austerity interaction			0.307**	-0.282***
			(0.119)	(0.0855)
Risk aversion	-0.00697	0.0352***	-0.00729	0.0355***
	(0.0113)	(0.00679)	(0.0113)	(0.00679)
Age	-0.00156	0.0197***	-0.00156	0.0197***
	(0.000990)	(0.000672)	(0.000990)	(0.000672)
Education	-0.00845*	0.0542***	-0.00844*	0.0542***
	(0.00454)	(0.00299)	(0.00454)	(0.00299)
Total TV	0.0465***	-0.0494***	0.0467***	-0.0495***
	(0.00885)	(0.00545)	(0.00885)	(0.00545)
Political TV	0.0148	0.0605***	0.0147	0.0605***
	(0.0126)	(0.00816)	(0.0126)	(0.00815)
Female	-0.156***	-0.0367**	-0.155***	-0.0369**
	(0.0283)	(0.0178)	(0.0283)	(0.0178)
Left-right scale	-0.0323***	0.00897**	-0.0324***	0.00901**
	(0.00786)	(0.00452)	(0.00786)	(0.00452)
Trust in political parties	-0.0824***	0.0553***	-0.0824***	0.0552***
	(0.00731)	(0.00445)	(0.00732)	(0.00445)
Against non-EU migration	0.122***	-0.0446***	0.121***	-0.0438***
	(0.0183)	(0.0111)	(0.0183)	(0.0111)
Observations	65170	77248	65170	77248
Fixed effects	Yes	Yes	Yes	Yes
SE	Robust	Robust	Robust	Robust
Log likelihood	-11491.6	-40411.5	-11487.9	-40397.7

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

5.3 Discussion and causality

What might explain these mixed results? If the model is well specified, we have evidence of an austerity channel through which the economic insecurity mechanism as a determinant of populist voting might work. This is supported by the increased effect of "exposure to austerity" on the probability of voting populist in years and countries when austerity is taking place vs. otherwise.

How might this "austerity channel" work? It could be that austerity policies (especially those which are expenditure-based) lead to reduced social security spending which otherwise could ameliorate the negative effects of economic downturns on those who are economically vulnerable. This leads to a dissatisfaction with established political parties which by turns increases populist support and encourages voter apathy. Additionally, cuts in government spending encourage those who are ideologically more left-wing to turn to populist parties, while increased taxes have a similar effect on more right-wing individuals, supported by the results of my analysis of different types of austerity.

Is there evidence in these results of a causal relationship between austerity policies and voting? The data currently does not allow us to draw this inference. Whilst the austerity periods identified by Alesina et al. (2019) could plausibly be argued to be an exogenous measure of austerity periods, they still only vary at the country level. There is no exogenous variation in my "exposure to austerity" explanatory variable, which in any case is only a proxy for this exposure, and in no way measures the *incidence* of austerity policies at the individual or regional level. With data on austerity incidence at either of these levels, together with repeated observations over time, it would be possible to exploit individual or regional variation in a difference-in-difference analysis to arguably discern the causal effect of austerity policies, as in Fetzer (2019). This was not possible with the data available.

The data instead allowed me to test the more nuanced question: "Is there an *austerity channel* through which the economic insecurity mechanism as a driver of populist voting operates?". This channel would provide tentative evidence of a causal relationship between austerity policies and populism, though quantifying this effect would still be beyond reach. Here, I took aim at unbiased estimates of the relationship between "exposure to austerity" and populist voting, and how this relationship changed when exogenous austerity policies were enacted, through the use of a Heckprobit model to mitigate sample selection bias. I also added controls based in related literature, and country and ESS round fixed effects to try and remove other potential sources of endogeneity from the model (see section 4).

However, there are reasons to doubt my estimates. Firstly, methodological constraints leave something to be desired. The lack of a convincing instrument for selection into participation gives cause for concern, as this means we cannot rule out sample selection bias. Whilst the addition of country and ESS round fixed effects is important for addressing this and other sources endogeneity, they still leave room for an unobserved confounding variable which is time and country invariant to bias my estimates. The construction of a dummy for austerity is also a blunt tool with which to analyse the effect of austerity policies. Future work which incorporates the magnitude of consolidations into a model of the relationship between austerity policies and populist voting would be welcome. Secondly, much rests on the identification of austerity periods as exogenous. The period under examination in this analysis - and many of the fiscal consolidation years - encompasses the financial crash of 2008, subsequent recession and Eurozone crisis. Following

these crises, something approaching an international consensus emerged in Europe that austerity was necessary, and could even be expansionary. This was intimately related to the idea that fiscal profligacy and excessive debt had been a key contributor to the financial crash of 2008-9. As such, an "exogenous" fiscal consolidation which is aimed at reducing the budget deficit can also be interpreted as an endogenous policy response to the Great Recession. Separating these effects will be of great help in further investigations into the relationship between austerity policies and populist voting.

6 Conclusion

Following much investigation in recent years into the economic causes of the populist surge, in this paper I took a closer look at the role of austerity policies as a particular economic determinant of this phenomenon. In so doing, I formed the hypothesis that there exists an "austerity channel" of the economic insecurity mechanism proposed in Guiso et al. (2017). This channel works through the enactment of austerity policies - cuts in public spending or rises in taxes - which negatively affect the economic security of individuals who have experienced unemployment, income difficulties, and whom rely on social benefits as their main source of income. The resulting insecurity causes disappointment with traditional "status quo" parties, which has two related effects. First, individuals are more likely to vote populist. Secondly, they are less likely to participate in elections, which can bias estimates of the first effect.

In order to test this hypothesis, I constructed a variable which measures "exposure to austerity" using principal component analysis on the ESS variables unemployment, income difficulties, and reliance on social benefits. I modelled the relationship between this variable and both populist voting and electoral participation using a Heckprobit model to deal with sample selection bias. Additionally, I incorporated data from Alesina et al. (2019) on exogenously identified austerity periods in order to see whether the relationship between "exposure to austerity" and my dependent variables was stronger in austerity periods than otherwise.

This analysis provides tentative support for my hypothesis. In particular, an increase in "exposure to austerity" increases the probability of voting populist and decreases the probability of electoral participation. This effect is much stronger in austerity years, though this particular result is not robust to the removal of Germany or Belgium from the analysis. The focus on social benefits as opposed to exposure to globalisation in constructing my "exposure to austerity" variable is important in driving the result. I also find some evidence to suggest that expenditure-based fiscal consolidation years are more important for this mechanism than revenue-based consolidations, and that left-wing sentiment is more strongly associated with populist voting during expenditure-based consolidations, while right-wing sentiment is more strongly associated with populist voting in revenue-based consolidation years.

However, a weak instrument for selection into observation, together with a lack of exogenous variation in exposure to austerity, and the endogeneity of austerity as a policy response to the Great Recession casts doubt on the credibility of the results, to the extent that we cannot infer causality at this stage. This research instead provides a useful starting point for future investigations into the relationship between austerity and populism in Europe. In such analyses, better data on the incidence of austerity policies[1] across Europe may provide more fruitful insights.

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[1]As in Fetzer (2019) for the UK case.

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A   Appendix

Table A.1: Populist parties
Parties identified as populist for this analysis (Following Guiso et al. (2017) precedent)

Country	Party
Austria	Freedom Party (FPÖ)
	Alliance for the Future of Austria (BZÖ)
	Team Stronach (TS)
Belgium	Flemish Interest (VB)
	National Front (VN)
	List Dedecker (LDD)
Denmark	Danish People's Party (DF)
Finland	True Finns (PS)
France	National Front (FN)
Germany	Party of Democratic Socialism/ The Left (PDS/Linke)
Ireland	Sinn Féin (SF)
Italy	Forza Italia (FI) / People for Freedom (PdL)
	Northern League (LN)
	5 Star Movement (M5S)
Sweden	Sweden Democrats (SD)
United Kingdom	British National Party
	UK Independence Party

Table A.2: Principal component correlation with "economic insecurity" variables (Guiso et al., 2017)

Component	Eigenvalue	Difference	Proportion	Cumulative
Comp1	1.24261	0.245894	0.4142	0.4142
Comp2	0.996718	0.236047	0.3322	0.7464
Comp3	0.760671	-	0.2536	1

Number of obs.	115,415	Rotation: (unrotated = principal)	
Number of comp.	1		
Trace	3		
Rho	0.4142		

Table A.3: Principal component eigenvector for "economic insecurity" variables (Guiso et al., 2017)

Variable	Comp1	Unexplained
Unemployed	0.7002	0.3907
Exposed to globalisation	0.1215	0.9816
Income difficulties	0.7035	0.385

Table A.4: Heckprobit models without fixed effects.

	Pop. vote	Particip.	Pop. vote	Particip.	Pop. vote	Particip.	Pop. vote	Particip.
Hampered in daily activities		0.0321**		0.0333***		-0.0725***		-0.0732***
		(0.0135)		(0.0129)		(0.0149)		(0.0149)
Exposure to austerity	1.310***	-1.393***	0.953***	-1.226***	0.684***	-0.900***	0.319***	-0.711***
	(0.0832)	(0.0356)	(0.138)	(0.0596)	(0.0978)	(0.0418)	(0.0998)	(0.0680)
Austerity year	0.215***	0.0161	0.167***	0.0533***	0.354***	-0.0147	0.288***	0.0280
	(0.0594)	(0.0160)	(0.0516)	(0.0202)	(0.0255)	(0.0179)	(0.0294)	(0.0224)
Austerity interaction			0.426***	-0.230***			0.446***	-0.261***
			(0.0967)	(0.0737)			(0.110)	(0.0817)
Risk aversion					-0.0154	0.0349***	-0.0156	0.0351***
					(0.0112)	(0.00658)	(0.0111)	(0.00658)
Age					-0.00303**	0.0191***	-0.00292**	0.0191***
					(0.00147)	(0.000648)	(0.00137)	(0.000648)
Education					-0.0325***	0.0517***	-0.0322***	0.0516***
					(0.00585)	(0.00284)	(0.00564)	(0.00284)
Total TV					0.00124	-0.0692***	0.00100	-0.0693***
					(0.00956)	(0.00521)	(0.00931)	(0.00521)
Political TV					0.0320***	0.0696***	0.0323***	0.0696***
					(0.0123)	(0.00813)	(0.0121)	(0.00812)
Female					-0.140***	-0.0335*	-0.140***	-0.0339*
					(0.0275)	(0.0174)	(0.0275)	(0.0174)
Left-right scale					-0.0196***	0.00556	-0.0198***	0.00567
					(0.00756)	(0.00428)	(0.00756)	(0.00428)
Trust in political parties								
Against non-EU migration								
Observations	90664		90664		78877		78877	
Fixed effects	No		No		No		No	
Non-selected observations	16138		16138		12501		12501	
SE	Robust		Robust		Robust		Robust	
Rho	-0.855		-0.814		-0.0823		-0.0679	
Log likelihood	-69402.2		-69380.4		-56651.4		-56629.6	

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$