Credit Shocks and Populism*

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Abstract

This paper shows that credit shocks are an important determinant of the recent rise of populism. Exploiting spatial variation in exposure to an exogenous lending cut by a large German bank in 2007–08, we find that exposure to the credit shock leads to a persistent increase in populist political preferences. To explore the shift in demand for populism activated by the shock, we measure the degree of populist rhetoric and the salience of bank-related topics to each party over time using a machine learning technique on the corpus of parliamentary speeches in Germany. A county-level analysis suggests that the underlying mechanism lies in the perceived decay of the local economy across voters that stems from the credit shock. A machine learning decomposition of the individual causal effects indicates that labour market history is the most important factor shaping the response in populist preferences.

JEL Classification: P16; G21; D72; E51.

Keywords: Populism; Credit; Banking Crisis; Electoral Behaviour.

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1 Introduction

Following the Great Financial Crisis, populist parties scored major electoral successes in a number of countries.[1] As a result, many observers regarded the financial crisis as being responsible for the emergence of populism.[2] However, while the recession originated as a banking crisis characterised by a steep decline in lending (Ivashina and Scharfstein, 2010), empirical studies of the economic causes of populism focused on other aspects of the crisis.[3] The evidence of the direct effect of the recent crisis in bank lending on the intention to vote for populists is hence largely unexplored.

This paper addresses this gap in the literature empirically by investigating whether sharp reductions in bank credit increase electoral support for populist parties. To identify a causal link, we exploit a significant and unexpected lending cut by Commerzbank, the second-largest private German bank, during the onset of the global financial crisis of 2007–2008. The bank suffered losses on its international trading books, which were unrelated to domestic economic conditions. This shock propagated heterogeneously across Germany, hitting counties that were more exposed to the bank harder, while leaving others less affected or unaffected. Using data on the bank relationships of each firm in the country, we are able to estimate the exposure of each German county to the bank, and therefore to the credit shock. We combine these data with individual-level survey

[1] In their paper, Funke, Schularick and Trebesch (2020) highlight a striking descriptive trend in the surge in populist governments in the last two decades. In 2018, according to their data, 16 of 60 countries in charge of roughly 95% of global GDP are led by populist governments. These key numbers go hand in hand with the interest of the general audience. In 2017, the Cambridge Dictionary proclaimed *populism* 'Word of the Year' as channels of communication dramatically intensified their reporting, and occurrences of 'populist' and similar words in the New York Times nearly quadrupled from 2015 to 2017 (Rooduijn, 2019). Research on populism has shown the same trends, although economics has lagged behind until experiencing a sharp increase in interest after the 2016 US presidential election, which clearly showed that economists had a motivation for studying the concept of populism and not only from a political perspective (Hunger and Paxton, 2022; Rovira Kaltwasser et al., 2017).

[2] Examples of contrasting views in newspaper articles include the following: 'Populism is the true legacy of the global financial crisis', *Financial Times*, 30 August 2018. 'From Trump to trade, the financial crisis still resonates 10 years later' *New York Times*, 10 September 2018. 'Populism was not sparked by the financial crisis', *Financial Times*, 29 August 2018; 'No, the Financial Crisis Didn't Spawn Populism', *Wall Street Journal*, 18 September 2018.

[3] Studies of the economic drivers of populism exploring the role of recessions revolve around economic insecurity (Guiso et al., 2019, 2020; Garro, 2021), labour market distress (Algan et al., 2017; Hobolt and de Vries, 2016; Barros and Santos Silva, 2019; Dehdari, 2022) and austerity (Fetzer, 2019; Fetzer, Sen and Souza, 2020; Galofré-Vilà et al., 2021; Dal Bó et al., 2022). A number of papers have also looked at historical crises (Funke, Schularick and Trebesch, 2016; de Bromhead, Eichengreen and O'Rourke, 2012; Doerr et al., 2022), accountability of public finance (Sartre, Daniele and Vertier, 2020), mortgage value in foreign currencies (Ahlquist, Copelovitch and Walter, 2020; Gyöngyösi and Verner, 2022), and a marginal part on credit swings (Herrera, Ordoñez and Trebesch, 2020; Braggion, Manconi and Zhu, 2020; Antoniades and Calomiris, 2020) with no consensus on the effect and no clear reference to populism in modern democracies. Further research focuses on the role of secular trends regardless of the presence of a recession, where globalisation and trade (Autor et al., 2020; Dippel et al., 2022) or technological progress (Frey, Berger and Chen, 2018; Anelli, Colantone and Stanig, 2019) generate natural winners and losers in the economy. The latter are dissatisfied with the status quo because of the impact on labour markets and therefore express their discontent using nationalistic and far-right platforms. Alternative explanations of populism are found in the interaction between identity (Mukand and Rodrik, 2018) and culture (Ferrara, 2022; Autor et al., 2020; Norris and Inglehart, 2019) and economics, moral values (Enke, 2020), immigration (Steinmayr, 2021; Dinas et al., 2019; Alabrese et al., 2019), or the media (Campante, Durante and Sobbrio (2018), Zhuravskaya, Petrova and Enikolopov (2020) for a review). See Guriev and Papaioannou (2022) for an extensive review of these topics.

data to study the effect of this shock on the political preferences of German voters. This approach allows us to compare changes in the preferences of voters who were hit by the shock with different intensity.

We find that an increase of one standard deviation in exposure to the credit shock increased the likelihood of populist preferences by 0.5 percentage points, which roughly corresponds to an increase of 16% in our survey baseline of populist preferences. This result therefore supports the view that the credit crisis contributed to the electoral growth of populist parties in the recent German federal elections. In a back-of-the-envelope calculation, assuming a linear relationship and that the whole country would endure the same average treatment as in our reckoning, the credit shock would create a one percentage point increase in demand for populist parties, which could then explain around 8% of the increase in the share of the vote of populist parties in the German federal elections from 2009 to 2017.

We then explore the reasons underlying this shift in political preferences towards populism, exploring which type of parties are most rewarded as a result. In particular, it could be that populist parties focused more on the issue of the banking crisis than other parties. If this were the case, our results would just describe voters being more attracted to parties' commitment to a salient issue rather than to their populist rhetoric. We test this hypothesis by analysing separately the focus on banking issues and the supply of populism. To this end, we apply text analysis to the parliamentary speeches of representatives in the Bundestag, the German federal parliament. Employing a semi-supervised machine learning technique, for each party in each year, we estimate its focus on the topic of banking as well as its populist rhetoric. The text analysis also gives us a continuous classification of populism that moves us from a dichotomous and more scholastic classification towards a more flexible and data-driven identification of populism, in addition to measuring the salience of banks in the political discourse. Our results indicate that voters hit by the shock were more likely to support both parties that talked more often about banking and that, in particular, adopted a more populist rhetoric. Interestingly, this probability also holds when we average over these two measures. These results indicate that the voters most hit by the credit shock became more likely to favour parties that talked more often about the banking crisis and adopted a populist rhetoric, compared to their peers that either talked about the crisis but with a moderate rhetoric or that adopted a populist rhetoric but did not focus as much on banking issues.

The findings in this paper provide a nuanced interpretation of the recent rise of populism in advanced economies. While we identify that the credit shock rewards parties that adopt a populist rhetoric, our evidence indicates that this link does not necessarily stem only from voters' irrational attraction to an anti-establishment rhetoric. On the contrary, we show that parties that simply adopt a populist rhetoric are less rewarded than their peers that adopt this rhetoric and focus on the issue of the banking crisis.[4]

[4]The salience of topics that populists leverage to secure approval among voters is pivotal in Ahlquist, Copelovitch

What is the mechanism behind the activation of populism demand by the credit contraction? We validate an economic channel through which the credit shock conveys voters' support away from the establishment. While there is little evidence that individuals were affected by the credit shock on their own balance sheets, through a county-level analysis, we find that the credit shock has a negative and persistent impact on the local GDP of those counties that were more exposed. This is coherent with the findings in Huber (2018). We also observe that, in the more exposed areas, the local economy fails to converge back to pre-crisis levels even nine years after the Great Recession. This suggests that, in those areas that lag behind in economic performance due to the long-lasting effect of the credit supply shock and the consequent recession, voters respond to the perceived insecurity in their local economy by retaliating against the traditional parties and expressing their discontent in the political market.

Next, we turn to the causal links between credit shocks and individual political preferences. To this end, we identify the individual response to the credit shock by comparing the average outcome of each individual with the average outcome of all individuals receiving a different exposure to the shock, before and after its occurrence. From these estimates, we deploy a supervised machine learning algorithm that predicts them based on a broad set of individual characteristics. We find that the labour market history of individuals plays the primary role in determining their response in terms of populist preferences. In particular, aggregate unemployment is more relevant than individuals' current labour market condition and education, which indicates that those who have been systematically marginalised in the labour market find it more appealing to retaliate against the current status quo in the political market.

The main contribution of this paper is to provide evidence of the effect of banking crises on the rise of populism. The most similar studies to ours are Antoniades and Calomiris (2020) and Doerr et al. (2022). Antoniades and Calomiris (2020) examine the impact of a household mortgage credit contraction in the US on voters' preferences in the presidential election of 2008. They find that voters responded to the drop in credit by shifting their support away from the incumbent party. However, their analysis focuses on the dynamic between incumbent and opponent, and does not examine the impact on populism. Doerr et al. (2022) analyse the political effects of the banking crisis of 1931 in Germany. They show that votes for the Nazi Party surged in areas more exposed to *Danatbank*, the bank at the heart of the collapse, which was led by a Jewish manager. The evidence in their paper and in our work complements each other by unravelling the political effects of lending cuts which, at least in the case of Germany, favour emerging anti-establishment parties such as populist or fascist ones.

Our paper is linked to the emerging literature on the political effect of bank lending and, more

and Walter (2020) on the bailout of homeowners with mortgages in Swiss francs, Ochsner and Roesel (2019) on manipulating historical narratives, Doerr et al. (2022) on the Jewish origin of the defaulting bank's chairman in nourishing NSDAP's propaganda. In other cases, populists might instead offer a platform to activate a latent cultural backlash (Cantoni, Hagemeister and Westcott, 2020; Ferrara, 2022).

broadly, of financial crises. Based on an exogenous credit contraction experienced by China in 1933, Braggion, Manconi and Zhu (2020) show that firms with greater exposure to the lending shock experienced higher labour unrest and Communist Party penetration among their workers. On the other hand, Herrera, Ordoñez and Trebesch (2020) show that (excessive) credit expansions only favour the incumbent in emerging markets. Mian, Sufi and Trebbi (2014) show for a large sample of countries that, following a financial crisis, voters become more polarised and ideologically extreme. Gyöngyösi and Verner (2022) study the impact of debtor distress during a financial crisis on support for a populist far-right party, exploiting variation in exposure to foreign currency household loans during a currency crisis in Hungary in 2008. They postulate that foreign currency debt exposure leads to a large and persistent increase in the populist far-right vote share. Ahlquist, Copelovitch and Walter (2020) document the effect of the 2015 surprise revaluation of the Swiss franc on the political preferences of Polish citizens holding mortgages in Swiss francs. Households exposed to the financial shock were more likely to demand government support and desert the government in favour of populist parties, which proposed a more generous bailout scheme at the expense of largely foreign-owned banks. Funke, Schularick and Trebesch (2016) complement this quasi-experimental evidence with a comparative study of financial crises and elections. They find that political uncertainty rises strongly after a financial crisis, leading to an increase in political fractionalisation and preferences for far-right parties.

Finally, our findings add to the broader literature on the economic causes of populism. As mentioned at the beginning of this section, existing papers focus on a number of other economic factors than the decline in bank lending to explain the rise of populism.[5] In this paper, we look at the long-standing economic distress caused by the credit contraction during and after the Great Recession, which led to general economic insecurity that voters can transfer to the political market. There is limited causal evidence of the rise of populism in Germany where, unlike other advanced economies, traditional mainstream parties have not consistently changed their position in response to the rise of authoritarian populism, and a new wave of populism has emerged. We complement the work of Dippel et al. (2022) who find that trade integration favoured support for extreme-right parties in Germany from 1987 to 2009. However, our contribution to this literature is not limited to the study of an unexplored economic cause of populism, but is also methodological. In particular, our text-based index of populism goes in the direction suggested by Guriev and Papaioannou (2022), who suggest moving from binary classifiers of populism to finer measures that enhance our understanding of the differences between populist parties.[6] By doing so,

[5]See Guriev and Papaioannou (2022) for a comprehensive review, Grasso and Giugni (2016) and Burgoon et al. (2019) for the consequences of positional deprivation for populist preferences.

[6]A few researchers have already explored text-based methods for political concept detection, but using different methodologies and outcomes, such as dictionaries in Enke, Rodríguez-Padilla and Zimmermann (2020), Hassan et al. (2019) and Breyer (2022), or word embeddings in Gennaro, Lecce and Morelli (2020), where they measure populism at the level of the political speech in campaign messages during the 2016 presidential election and the 2018 and 2020 congressional elections in the US. They use the same starting point of seeds, but combine it with a Term-Frequency

we not only distinguish between different intensities of populist rhetoric, but also between populist parties that focus with different intensity on bank-related issues. Moreover, our method addresses another gap identified by Guriev and Papaioannou (2022), that is the need to blend the demand and supply of populism using textual analysis of political speeches. We do so by combining changes in individual-level responses, which capture the demand for populism, with variations in the populist rhetoric used in parliamentary speeches, which capture the supply.

Explaining the recent rise of populism has important implications for policy and our understanding of modern political institutions. The influence of populist parties on the decision-making process is socio-economically *expensive*. While such movements favour debt-financed, short-term expansions at the expense of longer-term economic development, they also lead to deterioration in political institutions such as the constraints on the executive, checks and balances, the rule of law, as well as reshaping beliefs, norms and values.[7] Therefore, it is important to shed more light on their origin and mechanisms.

The remainder of this paper is organised as follows. The next section describes the origin of the credit shock, the political spectrum and the origin of populism in Germany. Section 3 discusses our identification strategy. In Section 4, we outline the data we use to estimate our model. Sections 5 and 6 describe the methods we adopt to measure the credit shock and populism. Section 7 presents the main results and a number of robustness checks. Sections 8 and 9 discuss a mechanism and the decomposition of the main contribution. The final section concludes.

2 Background Information

2.1 The Origin of the Credit Shock

Our aim is to investigate the effect of negative credit shocks on support for populism. The main challenge is to overcome the potential omitted variable bias that could affect this relationship. Omitted variables may simultaneously affect changes in credit and populism, leading to a spurious correlation between lending and populism, even if the true causal effect of the credit shock was null. To this end, we need to identify a shock that was generated exogenously and had potential repercussions on the preferences of German voters. We focus on the imported lending cut suffered by Commerzbank, the second-largest private German bank, in 2008-2009. The lending

Inverse-Document-Frequency (td-idf) approach.

[7] In economic terms, Funke, Schularick and Trebesch (2020) estimate a decrease of 10% in GDP per capita and consumption in the longer term when populist governments are in power, mostly attributable to right-wing populists in recent decades and to left-wing populists before the 1990s. Moreover, Bellodi, Morelli and Vannoni (2021) estimate the effect of electing a populist mayor on about 8,000 Italian municipalities, finding that populist mayors lead to more debt, a larger share of procurement contracts with cost overruns, higher turnover among top bureaucrats, and a sharp decrease in the share of graduate bureaucrats. Recent papers on beliefs and norms and populism have shown the social cost of populism in terms of limiting the social stigma associated with discrimination and hate crime against immigrants, minorities and religious groups (Bursztyn, Egorov and Fiorin, 2020; Romarri, 2020; Müller and Schwarz, 2020, 2021).

cut is particularly fit for our research purpose, as it stemmed from losses on the bank's international trading books, and was therefore driven by exogenous causes. This peculiarity allows us to compare German households that were more exposed to the Commerzbank lending cut with those that were less exposed.

Commerzbank is the second-largest private bank in Germany by the total value of its balance sheet, and at the time of the shock it was in charge of around nine per cent of the total bank lending to German non-financial customers.[8] As a universal bank, income earnings come from investing in international markets, interest income from lending and non-interest income from trading.

[FIGURE 1 ABOUT HERE.]

In Figure 1 we show the natural logarithm of the lending stock of German Banks to non-financial customers. The figure shows that, in 2008 and 2009, lending by Commerzbank declined sharply relative to all other banks, whereas it shows a parallel trend in the period preceding (*i.e.* until 2007) and following (from 2011 onwards) the shock. This difference from the rest of the German banking sector is related to the significant exposure of Commerzbank's trading portfolio to international finance, especially related to investments in asset-backed securities linked to the United States subprime mortgage market, as well as the bank's exposure to the insolvency of Lehman Brothers and the bailout of the Icelandic banks. Given this exposure to foreign securities markets, Commerzbank incurred significant losses on its trading portfolio – decreasing the equity capital by 68 per cent during this period – and reacted by cutting its loan supply to the internal economy, to fulfil Basel II regulations and to reduce risk exposure to be able to access funding markets again.[9] Therefore, the lending cut was completely unexpected and unrelated to changes in the local economy.

2.2 Germany's Party System

Germany is a federal parliamentary republic where legislative power is vested in the *Bundestag* (the German parliament) and the *Bundesrat* (the representative body of the Länder, Germany's regional states). After the reunification, Germany has been a multi-party system with Christian-liberal dominance characterised by ruling coalitions, which is particularly interesting for us because it expresses the heterogeneity of the political system and of populism in Germany compared to the US two-party system. Based on the classification we describe in detail in Section 6.1,

[8]Taken from the rankings in '*Top 100 der deutschen Kreditwirtschaft*' of die.bank, in absolute terms at the time of the shock and among private banks at the current state.

[9]The German government fund Soffin supported Commerzbank twice, on 3rd November 2008 and on 8th January 2009, but it was unable to entirely prevent the lending cut. Commerzbank managed to fully recover swiftly by mid-2011 by repaying around 80% of the Soffin equity. The shock was not triggered by the acquisition of Dresdner Bank at the beginning of 2009, as it had been planned way ahead, since 2006. Huber (2018) provides a comprehensive overview of the exogenous lending cut implemented by Commerzbank in the selected years.

we identify three different populist parties on the German political spectrum: (i) Alternative for Germany, (ii) The Left, and (iii) the National Democratic Party of Germany.

First, Alternative for Germany (in German, *Alternative für Deutschland*, AfD) is a far-right party that won 12.6 per cent of the vote in the 2017 German election and became the first new party to enter the Bundestag since the 1990s. The history of the party can be divided into two phases. During the first phase, the party was a conservative right-wing party embracing economic liberalism. The second phase started in 2015 and saw the transformation of the AfD into a far-right, anti-immigration party. It was in this second phase that the AfD reached its electoral peak in the 2017 election. When it was founded, AfD was not a traditional far-right party. It was a soft Eurosceptic party with an economic ideology that combined economic liberalism with socially conservative policies (Arzheimer, 2015). At the time, the party leadership encompassed conservative members of the German elite, including economists, lawyers and former centre-right politicians, but it also attracted far-right supporters from the very beginning. Already in this first phase, the AfD's stances were more anti-migration and market liberal than those of the centre-right CDU (Jankowski, Schneider and Tepe, 2017; Ceyhan, 2016). According to Schmitt-Beck (2017), in 2013 the AfD attracted two types of voters: those who opposed the euro bailout packages and those who opposed immigration. The AfD's ideological U-turn took place in 2015, when its leader and many supporters left the party. In that year, the party shifted its focus from the euro crisis to migration, and it took strong anti-migrant and anti-Muslim stances. These topics became prominent in the party's political communication as well (Müller and Schwarz, 2021; Doerr, 2021). Based on a text analysis of the party's Facebook posts between March 11th, 2013 (the day they started using Facebook) and September 24th, 2017 (the day of the federal election), Arzheimer and Berning (2019) show that the euro and Greece were among the most prominent topics in the AfD's online communication until 2015. In the second half of 2015, asylum and immigration policies gained traction and have remained prominent since then. This change of focus reflected the AfD's transformation from a single-issue party to a populist radical-right party (Arzheimer and Berning, 2019).

Second, The Left (in German, *Die Linke*) is a traditional radical left party. It emerged in 2007 from the merger of two leftist parties: the Party of Democratic Socialism (the successor to the German communist party) and the WASG (which broke away from the left wing of the Social Democrats). The main electoral basis of the Party of Democratic Socialism was in East Germany, whereas the WASG drew its support from the West. The parties decided to unite into a single party, The Left, following the disappointing result in the 2005 North-Rhine Westphalia election, when neither of the two managed to poll more than the five percent electoral threshold. Coffé and Plassa (2010) used quantitative text analysis to compare the Left's manifesto with that of its two predecessors on economic policies. They found the agenda of the Left to be closer to that of the Party of Democratic Socialism on economic issues. Lastly, the National Democratic Party of

Germany was founded in 1964 as the natural successor to the German Reich Party, a nationalist far-right organisation operating in West Germany. It has been regarded as basically a neo-Nazi organisation.

3 Identification

We use a difference-in-differences identification strategy, with heterogeneity across groups and continuous treatment, using two-way fixed effects for inference. In particular, we compare long-term outcomes across counties with different exposure to the credit shock by estimating the following reduced form relationship:

$$y_{ikt} = \delta_k + \lambda_t + \beta \cdot (Exposure_k \times Post) + \boldsymbol{\gamma} \boldsymbol{X}_{ikt} + \varepsilon_{ikt} \tag{1}$$

where y_{ikt} denotes the outcome of interest for individual i resident in county (*kreise*) k at time t. Depending on the model, y_{ikt} captures individual preferences towards populist parties or preferences in respect of a specific party. $\boldsymbol{X}_{ikt}$ are observed unit-specific controls at individual and household level (and specific regional controls in stricter specifications). δ_k and λ_t are county and year fixed effects, respectively. The central variable of interest, $Exposure_k$, is the heterogeneous pre-shock exposure to the credit shock measured as Commerzbank dependence, which we describe in more detail in Equation (4) in Section 5. $Post$ is an indicator variable that equals one for all the years following 2008, and zero for all other periods. To account for the fact that we measure our variable of interest at county level, whereas the outcomes are at individual level, we cluster standard errors at county level (Bertrand, Duflo and Mullainathan, 2004). Throughout the analysis, we use individual-year-specific design weights in our repeated cross-sectional data to overcome survey stratification, sampling and non-response rate.[10]

The coefficient of interest, β, measures the average treatment effect on the treated (ATT) after the realisation of the credit shock on individuals located in a county having had a higher pre-shock exposure to Commerzbank compared to individuals resident in a county with a lower exposure beforehand. While it is easier to identify the coefficient of interest through this *textbook* definition of the treatment effect, Callaway, Goodman-Bacon and Sant'Anna (2021) show that β will capture two different types of causal effects arising from the non-binary difference-in-difference setting: (i) the *level* effect, the treatment effect of $Exposure_k = d$, which equals the difference between a unit's potential outcome under exposure d (dose) and its untreated potential outcome, and (ii) the *slope* effect, the causal response to an incremental change in the exposure at d (dose), as in Angrist and Imbens (1995). Relying on four specific assumptions, β is equal to a positively weighted av-

[10] In a robustness specification presented in Table A5 in the Appendix, we apply longitudinal weights to a balanced panel that substitutes for our main rolling panel sample, where the weights are calculated by multiplying initial sampling weights by the inverse staying probability at the individual level throughout the period of the panel.

erage of the average causal response (ACR) parameters across doses over a baseline level effect given by the weighted average treatment effect on the lowest dose, where the weights are given by the variance-weighted average of all the comparisons among units that experience a different dose.[11] The first three assumptions consist of random sampling, monotonic continuous support of the treatment and no anticipation, and they are achieved by design.[12] The fourth assumption is a stronger common trends assumption than the standard conditional common trends assumption we would make in a difference-in-differences design with homogeneity across groups, involving different potential outcomes under different doses, rather than only the untreated potential outcomes. Given that each county is assigned to a different dose d, and that, for all doses, the trajectories of populist preferences over time of all individuals if they were located in a county with dose d are the same for all the individuals actually located in a county with that dose d.[13] Under the standard conditional common trends assumption, for which individuals' political preferences in a county treated with dose d would have followed the same trend in the absence of the lending cut (for all d doses in the exposure distribution), the identification of β would include a selection bias for each marginal dose that comes from the comparisons of the effect of a different dose among individuals experiencing that dose and the same effect relative to individuals that experienced the different dose.

It is unfeasible to formally test the validity of the stronger common trends assumption. Nevertheless, in Section 7, we offer some evidence concerning the linearity of the slope effect and the functional form of the treatment. On the other hand, the exogeneity of the credit shock and the regional validation of Commerzbank exposure, together with the absence of unobservable shocks within counties correlated with the measure of Commerzbank dependence validates the standard common trends assumption, and evidence presented in Huber (2018) supports this assumption.

We test more formally for differences in the likelihood that individuals would prefer more populist parties in a county more exposed to Commerzbank after the credit shock, by splitting our heterogeneous treatment into different cut-offs, that I can present in a different table. Specifically,

[11] Callaway, Goodman-Bacon and Sant'Anna (2021) add that β might not equal the natural overall average causal response since the weights of each marginal effect are generally not equal to the density of the treatment conditional on positive treatment. However, when the dose distribution is symmetric and closer to the normal, these weights are closer or identical to weighting the average causal responses to the population distribution of the treatment, leading to the true effect. Figure 2 shows a slightly positive skewness in the distribution of the treatment, which implies that the weights might be closer to the mean dose than their population weights. This is an additional rationale for testing the effect on binary treatments assigned at different cut-offs in Table 3, where we see a consistent effect while considering only the interquartile range of the treatment distribution.

[12] The original assumption supporting continuous treatment calls for the presence of never-treated units, such that the probability of being untreated is higher than zero. However, the decomposition of β that establishes the indicated treatment effect continues to apply by extending it to a null probability of being untreated, or no stayers. This is our case under the preferred proxy measure of the exposure to the credit shock.

[13] In practice, it might be easier to interpret it as assuming that the path of potential outcomes an individual would have experienced if assigned to a county with dose d after the occurrence of the shock would be equal to the path of outcomes for the individuals who were actually assigned to d. The strong common trends assumption is weaker than the latter, because it allows for some selection into a particular dose, but requires that there is no selection into a particular dose on average across all doses.

we estimate a more flexible difference-in-differences model:

$$y_{ikt} = \delta_k + \lambda_t + \sum_{r \neq 2008} \beta_r \cdot \left[\mathbf{1} \left(Exposure_k > s \right) \times \mathbf{1} \left(t = r \right) \right] + \boldsymbol{\gamma} \boldsymbol{X}_{ikt} + \varepsilon_{ikt}. \tag{2}$$

In addition to the more flexible year-by-year model, we estimate a more aggregated difference-in-differences model with three time periods. This enables us to clean some noise and gain a better idea of the trends: first, all the lags from 2000 to 2007, next the omitted first lag in 2008, and finally all the leads when the lending cut takes place, from 2009 to 2017, that is:

$$\begin{gathered} y_{ikt} = \delta_k + \lambda_t + \sum_{\tau=1}^{3} \beta_\tau \cdot \left[\mathbf{1} \left(Exposure_k > s \right) \times \mathbf{1} \left(t \in \tau \right) \right] + \boldsymbol{\gamma} \boldsymbol{X}_{ikt} + \varepsilon_{ikt}, \\ \tau \ \in \ \left\{ \left[2000, 2007 \right], 2008, \left[2009, 2017 \right] \right\} \end{gathered} \tag{3}$$

This specification enables us to remove part of the noise when estimating the year-by-year treatment interactions due to sample constraints, since we only have three time periods.

4 Data

We combine multiple databases in order to estimate the effect of the credit shock on populism. This section outlines the features of the data we use and provides some descriptive statistics before introducing the empirical results. We use firm-level data to compute the exposure of each German county (*landkreis*) to Commerzbank's business cycle as a proxy for exposure to the credit shock. More precisely, based on information on the bank relationships of each firm, we detect the degree of exposure of each firm to Commerzbank. This allows us to capture variation in exposure to the shock across regions and time. We then use individual-level survey data to capture political preferences, in addition to providing a rich set of characteristics. We match this information with our indicator of exposure to the shock based on the county where firms and survey respondents are located. This allows us to identify changes in political preferences depending on the degree of exposure to the credit shock. Since our measure of exposure is at county level, we include a number of economic indicators at county level as controls, which we reapply when looking at the economic performance of those counties more affected by the shock. Finally, we use expert surveys and textual data to identify a party as populist. Given the complexity of the exposure measure and the indicators we construct, we analyse these two last data sources separately in the following sections.

4.1 Firm-Level Data and Bank Relationships

We collect firm-level data for German companies from the database AMADEUS. The database is one of the products in Bureau van Dijk's OSIRIS and provides comprehensive information for approximately 19 million companies in both Western and Eastern Europe. Banks and financial companies are generally excluded from the database, such that the remainder are only commercial companies. Companies are defined in different categories by their operating revenue, total assets and number of employees, which are handled in four separate tables in the database (small, medium, large and very large). The majority of firms are in the first two categories.[14] In addition to information about the firms, the database AMADEUS BANKERS provides cross-sectional information about the bankers the company has relationships with. When this information is available, we are able to match each firm with its bank relationships through a unique identifier.

The company profile data in AMADEUS provide us with useful information enabling us to match them with the county of their location. For the matching procedure, we first use the postcode information (that is available for most of the firms) to allocate firms to the county where they are situated.[15] For the remaining unmatched firms, we use information on their region or city to identify the county where they are located.[16] To harmonise county-level data with individual level data, we consider 2017 administrative district nomenclature, since some counties were merged into existing districts during our considered time window as a result of state reforms.[17] Figure A3 in the Appendix shows the distribution of the number of bank relationships for each firm in the final sample.[18]

The AMADEUS database has some *ex ante* limitations. First, it is not a historical database, as it strives to contain recent information, deleting companies if they have not reported anything in the last 5 years.[19] This would lead to a survivorship bias in our underlying firm-level data (Kalemli-Ozcan et al., 2015). Second, financial data from companies in Amadeus are retained for a rolling

[14] Small and medium-sized firms constitute around 85% of our companies sample. Throughout the analysis, we use a firm sample from July 2020 without firm size category available due to data loss. We retrieved a different vintage of the firm data (March 2022) with firm size indicators, and the two samples are comparable.

[15] We implement the ZIP Code – Official Municipality Key (AGS) matching table using the list provided by suche-postleitzahl.org.

[16] Stepwise, we use strings for the region or the city where the firms are located to run the Jaro-Winkler distance algorithm (Winkler, 1990) implemented in the `stringdist` R package (van der Loo, 2014) to fuzzy match the string contained in the company profile data to the names included in the matching table we have previously created. We manually adjust the remainder of firms that are not perfectly matched and exclude a few firms with unreasonable locations.

[17] We retrieve the correct county identifiers for the 401 German counties at 31st December 2017 from the SOEPRemote server at DIW Berlin and the Bundesamt für Kartographie und Geodäsie (BKG).

[18] In the Appendix, we also present in Figure A4 the distribution of the number of firm-bank relationships including the financial and public sector firms that we exclude in the calculation of our main proxy of the credit shock in Section 5, the distribution of the number of firms per county (Figure A5 and A7, excluding or including finance and public sector firms), and their spatial distribution across the country's area (Figure A6 and A8).

[19] Unlike Amadeus, the database ORBIS keeps a company as long as it is active in the business register. For future comparison, we can achieve a more representative sample combining both AMADEUS and ORBIS, even though the survivorship bias within the AMADEUS BANKERS data would also extend to these data.

period of 8 years: when a new year of data is added, the oldest set of data is dropped, which means that only the most recent data for each company are available. Third, there is a reporting lag of two years on average, and there are differences in the coverage of particular variables depending on when the product was released. Whereas the latter issues are of minor concern to the goal of our analysis, except for limitations on addressing within-county heterogeneity of firms in terms of observable financials and characteristics,[20] the former affects the data coverage of the universe of German firms in the calculation of our key proxy of the credit shock described in Section 5, where we describe how we address these issues.

4.2 County-Level Macro Data

We record several macroeconomic variables at the regional level from Statistisches Bundesamt (DeStatis), Statistische Ämter des Bundes und der Länder (RegionalStatistik) and Bundesamt für Kartographie und Geodäsie (BKG). We retrieve population, size, percentage of foreign citizens, regional GDP, employment, average household income, an indicator variable of whether the country is a rural or an urban area, whether it is a county of the former German Democratic Republic (GDR) or whether it is exposed to a similar simultaneous crisis as the lending cut performed by Commerzbank (see Puri, Rocholl and Steffen, 2011). The indicator variables are absorbed by the county-level fixed effects in Equation (1).

4.3 Individual Political Preferences

We exploit individual data from the German Socio-Economic Panel (SOEP, Goebel et al., 2019, v36) – a nationally representative longitudinal household survey that, every year, interviews around $30,000$ individuals in different samples. We consider waves from 2000 to 2017. The main advantage of this survey is that it provides detailed information about individual and household characteristics and, for our purpose, it annually records political support and intention to vote, which are our main individual-level outcomes. Political support is registered as an indicator variable that is equal to one for an affirmative answer to the question (translated from German) '*Many people in Germany lean towards one party in the long term, even if they occasionally vote for another party. Do you lean towards a particular party?*'. The question is repeated for each considered wave, and its framing gives us a long-term perspective on political preferences. Our key outcome measures the intention to vote for a populist party conditional on political support. The data provide individual preferences for political parties.

We consider individuals who are at least sixteen years old, which corresponds to the eligibility to vote in administrative elections in several counties and allows respondents of the SOEP to an-

[20]In unreported results, we use the more recent vintage of Amadeus firms and include firm-size categories and different financial variables to address within-county heterogeneity in the exposure to the credit shock.

swer political questions.[21] Administrative district keys of residence are available at the individual level in the data: this allows us to match each individual with the pre-shock county-level exposure to the credit shock.

[TABLE 1 ABOUT HERE.]

Table 1 describes the data in our full sample, with the variables relevant to our analysis. Monetary values are adjusted for inflation at 2016 current prices. We derive the annual household disposable income as household market income (defined as post-government income in Becker and Hauser, 2000) plus public pensions and state monetary transfers minus direct taxes and social security contributions, but including the rental value of owner-occupied homes (Grabka and Goebel, 2018). We include an indicator variable for former residence of the individual in the GDR before reunification to control for political preferences towards extremism (*e.g.* see Avdeenko, 2018; Lichter, Löffler and Siegloch, 2020).

5 Measuring Exposure to the Credit Shock

Having assessed the exogeneity of the shock in Section 3, we now need to find a measure to distinguish between subjects who were hit by the shock (treatment group) and subjects who were not (control group). As the accurate degree of exposure to the credit shock at county level is not directly observable, we use the link between firms and bank relationships described in Section 4.1 to compute a proxy based on a weighted average of the firm-level dependence on Commerzbank. For each set of firms F_k in a county k in 2006, we apply the following equation as proposed in Huber (2018):

$$Exposure_k = \sum_{f \in F_k} \omega_{fk} \underbrace{\frac{\text{\#Commerzbank Relationships}_{fk}}{\text{\#Total Bank Relationships}_{fk}}}_{\text{firm-level Commerzbank dependence}} \in (0,1) \tag{4}$$

where #Total Bank Relationships$_{fk}$ is the number of bank relationships of firm $f \in F_k$ in county k that are with Commerzbank branches, #Total Bank Relationships$_{fk}$ is the total number of bank relationships of firm f in county k, and ω_{fk} is a firm-county weighting factor. We identify bank relationships with Commerzbank by using string matching. Following Huber (2018), we primarily set it to $\omega_{fk} = 1/F_k$ equally weighting within a county.[22] We plot the distribution of the meas-

[21]For the analysis, we include all the SOEP sub-samples collected during the time span of interest, from 2000 to 2017, which might affect the outcome variable due to oversampling of immigrants, refugees and low-income families. Even though design weights will take these differences into account, in unreported results we test results excluding these particular sub-samples with only negligible differences in the main results

[22]In an unreported extension of the analysis, we weight firms within a county based on the number or cost of employees (payrolls) in 2007, or we match the average number or cost of employees with each firm size category described

ure of county-level Commerzbank exposure in Figure 2, whereas Figure 3 plots the geographical distribution of the exposure to the shock.[23]

[FIGURE 2 ABOUT HERE.]

[FIGURE 3 ABOUT HERE.]

To ensure that the firms in our sample were active in 2006, we select only firms established before 2007. Moreover, we exclude firms without industry sector and firms that are engaged in the finance and public sector other than those already excluded by default in the database (*i.e.* banks and insurance companies).[24] Figure 4 shows the distribution of the firm-specific Commerzbank dependence in the firms sample, which is specifically the ratio within the summation in Equation (4), with details on the frequency of the firms' sample.

[FIGURE 4 ABOUT HERE.]

For the measure calculated in Equation (4) to be a good proxy of the county-level exposure to the credit shock, we rely on two assumptions: (i) banks prefer to form relationships with geographically close customers, and (ii) bank relationships are consistently stable over time. While the former establishes a correct relationship between the county-level exposure to the credit shock and the proxy computed in Equation (4), and there is robust support for it in the literature (see Guiso, Sapienza and Zingales, 2004; Degryse and Ongena, 2005), we require the latter to compensate for data limitation, as we only have cross-sectional data for bank relationships based on the last updated information.[25] In support of our assumption, Giannetti and Ongena (2012) and Chodorow-Reich (2014) document the enduring stickiness of bank-borrower relationships. In particular, Chodorow-Reich (2014) shows that, establishing the link between credit market frictions

in section 4.1 Unfortunately, due to the survivorship bias in AMADEUS, we lose a significant portion of the firm data. Therefore, we decided to run the main analysis using equal weighting, regarding firm heterogeneity to the response to the credit shock as a subject for future study.

[23]From the picture, it is interesting to highlight four major areas of exposure in between which the exposure fades, excluding a major part of Bavaria (except for Munich): (i) East Germany, part of Upper Franconia and Upper Palatinate, (ii) Ruhr Area, (iii) Hessen, and (iv) Hamburg Area. The exposure in East Germany can be explained by the strategy chosen by Commerzbank compared to the other major banks upon Reunification in the 1990s (Klein, 1993). In fact, Commerzbank decided not to take over existing bank branches from the former Kreditbank and mix them with new own branches, but instead exclusively pursued internal growth by creating its own new branches. Its expansion was much slower than for the other two institutions, but it was completed before the 2000s. The other three areas are a by-product of the 'mandated banking zones' imposed by the Allies after World War II to decentralise the former Third Reich financial sector. These areas were chosen for reasons unrelated to economic activity but based on arbitrary decisions by the Allies (Huber, 2018).

[24]Following Berg, Reisinger and Streitz (2021), we exclude financial services and related industries, including holding companies, industries that in Germany are mostly in the public sector – such as administrative services, education, healthcare, arts, culture and gambling – and activities of organisations and private households. We repeat the exercise in Klapper, Laeven and Rajan (2006) but translated to NACE Rev. 2 codes as in Berg, Reisinger and Streitz (2021). In unreported results, we perform the analysis including those firms, with no significant difference in the main results.

[25]Unfortunately, bank relationships data for 2006 are not publicly available, nor on request. However, the data provider for the bank relationship data for 2006 exploited in Huber (2018) is the same as in AMADEUS BANKERS.

and employment during the financial crisis, pre-crisis clients of banks that restricted lending during the crisis could at no cost switch to borrowing from less constrained banks, but they do not do so due to stickiness in the borrower-lender relationship deriving from asymmetric information. Furthermore, Kalemli-Özcan, Laeven and Moreno (2022) compare different vintages of AMADEUS BANKERS with negligible differences, and Berg, Reisinger and Streitz (2021) use the same data to validate Huber (2018) results at firm level.

6 Measuring Populism: Expert Surveys and Text Analysis

Our aim is to identify an indicator that captures support for populist parties, *i.e.* the dependent variable in Equation (1). Defining a party as populist is not easy since populism may rely on different aspects, such as a certain set of policy preferences, ideology, or rhetoric (Norris, 2020; Guriev and Papaioannou, 2022). To account for these nuances, we employ three different, but complementary approaches to obtain comparable indicators of populism at party level. The first method is based on a binary classification of parties as populist based on expert surveys. The second and third are based on semi-supervised and supervised text analysis techniques, respectively.

6.1 Dichotomous Classification using Expert Surveys

We create an indicator variable that is equal to one when the individual leans towards a populist party. To identify populist parties in Germany, we rely on the *PopuList* proposed by Rooduijn et al. (2019) as in Guiso et al. (2020). The PopuList is a list of populist European parties that won no less than two percent of the vote in at least one national parliamentary election since 1998. The list was peer-reviewed by more than thirty academics. On the basis of these data, we identify as populist the *Die Linke* and *Alternative Für Deutschland* (AfD) parties. Since our individual survey data are not constrained by any threshold on the share of votes, we are also able to include on the list of populist parties the *Nationaldemokratische Partei Deutschlands* (National Democratic Party of Germany, NPD), a party that has never won a seat in federal elections, but that features in the classification of Norris and Inglehart (2019) based on the 2014's Chapel Hill Expert Survey (CHES). Figure A12 illustrates the geographical distribution of the difference in the share of populist preferences at county level from the individual party preferences assigned using the expert surveys.

[FIGURE 5 ABOUT HERE.]

This measure has two main limitations. First, its binary structure only allows comparisons of populist and non-populist parties, but not different degrees of populism. Therefore, this measure is not able to describe whether some parties have more populist stances than others. Second, this indicator is time invariant. As a result, we cannot identify whether the degree of a party's populism changes over time and whether voters' preferences change accordingly.

6.2 Continuous Measurement using Text Analysis

To overcome these limitations, we employ a second measure of populism based on text analysis. We rely on two sources of political textual data. First, we use the text from parliamentary speeches by party representatives in debates in the Bundestag. We use the *ParlSpeech (v2)* database from Rauh and Schwalbach (2020), which contains the full text corpora of 6.3 million parliamentary speeches in nine representative democracies, including Germany. From this source we select the sub-sample of speeches from the German Bundestag in the years from 1991 to 2018. Our sub-sample includes 379,545 speeches, with an average of 13,555 speeches per year from 1990 to 2018. By construction, this measure necessarily rules out the NDP from the sample, as it never held a seat in the Bundestag. This also applies to other populist parties that held no seats in specific years.

We compute the degree of populism using seeded Latent Dirichlet Allocation (seeded LDA). Seeded LDA is a semi-supervised, machine learning method used to extract the intensity of a topic from a given set of textual documents (Lu et al., 2011; Watanabe and Zhou, 2020). Seeded LDA works similarly to a classical LDA, which is an unsupervised method used to uncover the latent topics in a text. LDA (Blei, Ng and Jordan, 2003) is a generative probabilistic model based on the assumption that each document contains a mixture of topics and that the words observed in the document in a corpus are generated by latent topics. The main difference between the two approaches is that seeded LDA extracts these topics based on a prior 'seed' of selected terms that capture the object of interest (*i.e.* populist rhetoric in our case). The seeds train the model to extract the latent topics for each document based on the words provided as priors. Watanabe and Zhou (2020) and Ferner et al. (2020) show that this method fixes the inconsistency of topics that is generally produced by LDA. Another advantage of seeded LDA, unlike LDA, is that it does not require the selection of a pre-determined number of topics K. Overall, LDA can be helpful if we want to identify the topics that a text comprises, and we have no priors on them. On the other hand, seeded LDA is preferable in our case since we already know which topics we intend to identify, *i.e.* the topic of banking and the financial crisis and populist rhetoric. We summarise the seeded LDA generative process in the plate diagram in Figure A2 together with the full text analysis workflow we adopt for the computation of our measures in Figure A1 in the Appendix.

We first select a seed of words that, together, capture the topics of banking, finance and the financial crisis (we provide the full list of words in Section B in the Appendix). This enables us to obtain an indicator of how much each party discusses the topic of the crisis and banking. We intentionally do not focus on keywords related solely to crisis. In this way, we can capture how much each party focused on the topic of banking before the financial crisis and the credit crunch happened. For the sake of brevity, we will refer to this first macro-topic as 'banking and financial crisis' in the rest of the paper. Second, we select a seed of terms that captures populist rhetoric. We take these terms from the populist lexicon composed by Rooduijn and Pauwels (2011) to capture the degree of populist rhetoric of German-speaking parties. This lexicon is particularly interesting as it builds on the definition of populism provided by Mudde (2004, p. 543) as *'a "thin" ideology (Freeden, 1998) that considers society to be ultimately separated into two homogeneous and antagonistic groups, "the pure people" versus "the corrupt elite", and which argues that politics should be an expression of the* volonté générale *(general will) of the people'*. Against this backdrop, populist ideology rests on the belief that the people should govern over the elites due to the people's moral superiority. Guriev and Papaioannou (2022) apply this definition to identify the five most common traits that define modern populist parties: (i) no clear common ideology (in line with the 'thin-centred ideology'), (ii) anti-elite and anti-expert sentiment, (iii) anti-globalisation and anti-EU angle, (iv) anti-pluralism and authoritarian angle, and (v) communication style. Given our focus on political speeches, we are particularly interested in the latter of these traits. Guriev and Papaioannou (2022) further unpack the communication style of populist parties and identify three common patterns in their rhetoric: (i) simplicity of message, (ii) aggressive style, and (iii) social media.[26] The other four elements can help to establish a link between the occurrence of banking crises and the rise of populist parties. Opposition to elites and globalisation can easily fit into a rhetoric that opposes the banking sector and banking more broadly, which is the focus of our lexicon. The list is composed of twenty stemmed terms, such as *elit** and *korrupt**.

We select the top twenty tokens based on the topic-specific posterior probability distribution of the topic model. Figure 6 shows the top twenty terms for the topic of banking and financial crisis (divided into four subcategories: bank, crisis, ECB and finance) and for populist rhetoric based on the populist dictionary.

[FIGURE 6 ABOUT HERE.]

Based on the top twenty terms associated with each topic through seeded LDA, we compute an indicator that, for each party, captures (i) its focus on the topic of banking, finance and the crisis, and (ii) its populist rhetoric. Formally, we calculate the year-party measure L_{pt} for each party p in year t as:

[26] Many populist platforms owe their success to the use of social media and the use of new communication technology to circumvent the gatekeeping role of mainstream media. Populist parties have been active on social media also in the German context: Müller and Schwarz (2021) show how activities on the AfD's Facebook page fuelled hate crimes against migrants. However, the diffusion of nationwide mainstream media over local newspapers has also contributed to wider ideological polarisation in Germany from the 1980s up to 2009 (Ellger et al., 2021).

$$L_{pt} = \sum_{d \in D_{pt}} \left[\frac{\sum_{n \in N_d} \mathbf{1} \left(\omega_{dn} \in B_L \right)}{N_d} \right] \forall \, L = \{BF, POP\} \tag{5}$$

ω_{dn} is the observed word $n \in N_d$ in document d, while N_d is the per-document d number of words. B_L with $L = \{BF, POP\}$ is a bag of words of $\nu = 20$ tokens with the highest per-topic probability $\hat{\varphi}_k$ for the 'banking and financial crisis' topic and for populist rhetoric defined as the set (A.1) described in section A of the Appendix. $D_{pt} \subset \mathcal{C}$ is the collection of speeches for party p in year t of the corpus $\mathcal{C}$. The sum of matched terms, $\sum_{n \in N_d} \mathbf{1} \left(\omega_{dn} \in B_L \right)$ is weighted by the total number of terms in each document, N_d. This allows us to control for variations in the length of speeches in line with previous works (*e.g.*, (Fraccaroli, Giovannini and Jamet, 2020; Cantarella, Fraccaroli and Volpe, 2020).

[FIGURE 7 ABOUT HERE.]

Figures 7 and 8 show the evolution of the scores by party from 1991 to 2018. Figure 7 shows the focus of each party on the topic of banking and financial crisis over time. We note that all parties increase their attention on banking issues at the beginning of the crisis. The attention most parties devote to the topic peaks in 2010, which marks the beginning of the Eurozone crisis. The low score of the AfD party for banking may appear surprising at first, considering that the party was established by a group of economists with strong stances on the euro crisis and the Greek bailout. However, the party only entered parliament (and hence our sample) in 2017. By that year, AfD had been taken over by its most extremist faction, which focused on topics such as immigration, nationalism and Islamophobia, whereas the economist faction left the party.[27]

[FIGURE 8 ABOUT HERE.]

Figure 8 shows the scores for populist rhetoric. We note that the supply of populist rhetoric increases substantially for all parties from 2009 to 2010. However, this change has different intensity depending on the party. The centre-right CDU/CSU and the liberals (FDP) have the lowest scores, followed by the centre-left socialists (SPD) and the Green party (GRÜNE). Populist rhetoric increases sharply for the left-wing party Die Linke, and reaches its peak in 2011 and later on in 2018. From 2006, Die Linke has the highest score in the whole sample until 2017, when the far-right AfD enters the sample. In the last year of our database, Die Linke and AfD are the two parties with the highest degree of populist rhetoric, reflecting the general categorisation of these parties as populist.

[27] For a comprehensive description of this transition, as well as of the transition of the AfD's populist rhetoric and issue salience in their speeches, see Cantoni, Hagemeister and Westcott (2020).

Since, for each party, we are interested in comparing (i) its focus on banking and financial crisis, (ii) its populist rhetoric and (iii) the combination of the two, we need a third indicator that provides us with an estimate of the latter. To this end, we create an indicator that we call *Combined* which, for each party and year, equals the average of a party's score for the topic of banking and financial crisis and the party's score for populist rhetoric for each year.

For robustness, we compute the focus on the topic of banking and financial crisis and the degree of populist rhetoric using an alternative text analysis, known as the dictionary approach, or *bag-of-words* approach (Baker, Bloom and Davis, 2016; Shapiro, Sudhof and Wilson, 2020; Ash and Hansen, 2022). We apply the same lexicons that we applied as seeds in the previous approach, in which we define S_V, $V = \{BF^s, POP^s\}$. We then compute the dictionary-based scores as follows:

$$V_{pt} = \sum_{d \in D_{pt}} \left[\frac{\sum_{n \in N_d} \mathbf{1}\left(\omega_{dn} \in S_V\right)}{N_d} \right] \forall\, V = \{BF^s, POP^s\} \tag{6}$$

where the numerator computes the frequency of terms in dictionary S_V that occur in document d, and the denominator weights the frequency by the length in terms of words of the document, N_d. In other words, we compute a similar score to the one in Equation 5. The main difference is that, in this case, we use the raw dictionaries rather than the terms with the highest posterior probability drawn using the seeded LDA. Similarly to the seeded LDA, we also in this case compute a third index based on the average of the dictionary-based score for banking and financial crisis and for populism.

7 The Effect of the Credit Shock on Political Preferences

In this section, we analyse the effect of the Commerzbank's lending cut on the likelihood of supporting a populist party. We present the results from estimating Equation (1) for various specifications in Table 2. In Column 1, we report the estimates of the simplest specification with county and year fixed effects and no additional controls. We find a substantial increase in the likelihood of populist preferences in counties that had higher levels of exposure to the credit shock *ex ante*. That is, the coefficient of $Exposure_k \times Post$ denotes that an increase in county-level Commerzbank exposure by one standard deviation makes individuals 0.42 percentage points more likely to prefer a populist party. This counts as a roughly 12.5% increase in populist preferences in the sample, a sizeable effect and statistically significant at 1%. In Columns 2 to 4, we introduce stepwise controls at the individual level (*i.e.* gender, second-order age polynomial, having lived in the GDR, marital status, migration background, categories of occupation, years of education) and household level (*i.e.* size, number of children, home ownership, presence of outstanding loans) and county-specific controls (*e.g.* ln(share of foreigners), ln(population density), etc.). The coefficient of interest remains fairly stable and significant throughout the specification, varying between a 0.52 and 0.55 percentage point increase in the likelihood of populist preferences for one standard deviation increase in pre-shock county-level exposure, up to a 16.3% increase in populist preferences in the sample. In Column 5, we introduce linear county-specific time trends, partialling out the unobserved variables that move simultaneously with the timing of the credit shock at county level, and obtain consistent results. Table A1 presents estimates of the same above-mentioned specifications in Table 2, but including individual fixed effects throughout, since the SOEP works as a rolling panel and individuals might be present in more than one single wave of the survey. Although we do not prefer results with individual fixed effects since we lose all individuals who are only present in a single wave, this allows us to control for individual-specific unobserved factors as robustness, providing significant estimates that are stable with an increase of between 0.511 in the most basic specification and 0.536 percentage points with full controls.

[TABLE 2 ABOUT HERE.]

Figure 9 presents all our estimates from Equation (2) and (3) graphically, qualifying the cutoff for the treatment indicator variable at the median of the treatment distribution. We plot dots indicating year-by-year 'treatment above median' interaction estimates (and bars indicating 95% confidence intervals). These estimates show the difference between individuals located in counties where pre-shock exposure to a credit shock is higher than the median of the county-level distribution and individuals resident in counties up-to-the-median exposed in terms of the likelihood of supporting a populist party in a particular year, relative to the difference in 2008 (first lag before the shock). We can see that, prior to 2008, the difference in populist preferences was not significant and without a clear trend. Then, from 2009 there is a sharp jump in populist preferences among individuals in counties more affected by the shock. Each interaction is positive from 2009, and especially significant in 2010 and 2011. Figure 9 also shows the estimated coefficients of the interactions between the more aggregated time period indicator variables and the same treatment indicator variable from estimating the pooled specification in Equation (3). We draw point estimates from the pooled year interactions as horizontal lines with 95% confidence intervals denoted as boxes. Prior to the credit shock, the figure shows close to zero and insignificant differences in populist preferences between individuals resident in more exposed counties and individuals located in up-to-the-median exposed counties beforehand. Conversely, after the occurrence of the credit shock, there is a statistically significant increase in the likelihood of individuals in counties more exposed to the credit shock supporting a populist party.

[FIGURE 9 ABOUT HERE.]

Figure 10 shows all the estimates in Figure 9, conditioning the results on the covariates in Column 4 of Table 2 in panel 10a and adding individual-level fixed effects on top in panel 10.

We can see from the figures that the common trends assumption holds both with and without conditioning for covariates. Moreover, in Table 3 we estimate Equation (1) substituting continuous treatment with an indicator variable equal to one when the exposure to the credit shock exceeds a certain cut-off of the treatment distribution, each different in every specification in addition to sample exclusion and the inclusion of individual-level fixed effects.

[FIGURE 10 ABOUT HERE.]

In Tables A2 and A3 in the Appendix, we present results using an indicator variable of political support as the outcome in Equation (1). Even though, in some specifications, we see statistically significant point estimates, we cannot reject the null hypothesis for which there are differences in political support for individuals located in counties with a higher exposure to the credit shock compared to individuals located in counties with a lower exposure after the shock happens. This is particularly justified by the visualisation of the point estimates from Equation (2) and (3), using political support as the outcome. In Figure A16a, we cannot exclude the presence of pre-trends, and estimates of the pooled pre- and post-periods are not significantly different from each other.

[FIGURE 11 ABOUT HERE.]

To identify the amplitude of the slope effect on our results, we draw the functional form of populist preferences on our continuous treatment. Figure 11 describes the functional form of the outcome variable on the exposure to the credit shock in levels. The dots define the difference in levels of the residualised mean of populist preferences at the individual level before and after the lending cut for each level of the distribution of county-level exposure. It is possible to observe that the difference in populist preferences linearly increases with the intensity of exposure to the credit shock in the linear space, giving that the effect on populist preferences follows a linear relationship with the intensity of exposure, as the fitted line shows (the shaded area represents a 95% confidence interval). To more formally test the linearity of the slope effect, in Figure A18 in the Appendix, we highlight estimates of Equation (2) in separate regressions, where we characterise different treatment and control groups based on the position of the exposure to the credit shock in the county where an individual lives within the treatment. Despite the noisy estimates, it is possible to see that point estimates are higher on average for higher treatment.

[TABLE 3 ABOUT HERE.]

In addition to testing for the presence of pre-trends in Figure 9, we run a battery of robustness checks based on the specification of Column 4 in Table 2 by restricting the sample for some conditions. Table 4 shows the estimates for each of these robustness checks. In Column 2, we test for conditioning the sample to individuals who are at least 18 years old, finding negligible differences from the baseline estimate in Column 1. In all our results, the binary classification of support for a populist party is unconditional on the indication of a political preference in the underlying question described in section 4.3 to preserve sample size, which is based on the assumption that individuals who state no political support are more likely to prefer a populist party when it comes to non-revealed preferences. We test this assumption in Column 3 by only conditioning preferences for populist parties on individuals with revealed political support, finding robust estimates of a higher magnitude, with an effect of 0.994 percentage points. In addition to this evidence, our results are intensity-to-treat estimates and thus provide a conservative lower bound of the true effect since we are estimating a reduced form. Moreover, it is possible that we underestimate the effect of the credit shock on populist preferences due to the tendency of individuals to not reveal extreme political preferences when asked, but rather indicate a preference for the establishment, which is consistent with our findings in Column 3 of Table 4 and the low non-response rate to the political support question indicated in section 4.3. Finally, in Columns 4 and 5, we carry out placebo tests using different timings of the shock, accounting for the large drop in lending stock in 2008Q3-Q4 at the time of Lehman Brothers' default and the anticipation effect of the subprime crisis. We find consistent estimates of smaller magnitude, suggesting a correct selection of the credit shock's timing. As we are exploiting a rolling panel for the analysis, there might be reason for concern about the validity of the results in a more formal sense, where we can actually observe preferences at the individual level, where individuals switch from a more traditional party towards populist parties upon the occurrence of the credit shock. In Table A5, we address this potential issue. For a restricted sample, we are able to create a balanced panel of individuals that can be followed year by year from 2006 to 2012, 2004 to 2013, and 2000 to 2015, respectively. Of course, this procedure significantly downgrades the statistical power of our analysis, but it allows us to check whether our results still hold when comparing exactly the same individuals with different treatment before and after the occurrence of the shock, applying longitudinal weights. For the first two panels, we do find significant results with a higher magnitude compared to the repeated cross-sectional sample, whereas, for the longest balanced panel, we do not find significant estimates, which is probably related to the dispersion in the number of individuals. Overall, these results strengthen the robustness of our main analysis.

[TABLE 4 ABOUT HERE.]

These estimates are based on populism defined as a binary classification of the political parties supported by each individual as being populist or not. While our estimates indicate that being exposed to the credit shock increases the probability of voting for a populist party, it is not clear yet whether individuals do so because of a party's populist rhetoric, because of its focus on banking issues or because of a combination of the two. To this end, we now replace the dependent variable with the continuous text-based indicator of populism we described in Section 6.2. The first indicator we study is the score based on the seeded LDA estimates and computed on the basis of the parliamentary speeches of the representatives of each party. The estimates of the model for this dependent variable are shown in Table 5. It presents the results for the seeded LDA using the populist lexicon of Rooduijn and Pauwels (2011). As in the previous table, in this case Column 1 also includes country-level and wave fixed effects, as well as basic controls, while in Columns 2 and 3 we progressively add controls at the household and regional level. Columns 1 to 3 show the estimates for the topic of 'Banking and Financial Crisis'. The coefficients capture the probability of an individual hit by the credit shock voting for a party based on the party's focus on the topic of banking and financial crisis. As we saw from the descriptive analyses, many of the terms related to this topic are related to credit and the crisis. The coefficient is positive and significant at the one per cent level, indicating that individuals exposed to the credit crunch were more likely to vote for parties that spoke more frequently about banking and credit issues, regardless of their degree of populist rhetoric.

[TABLE 5 ABOUT HERE.]

In Columns 4 to 6, we replace the dependent variable with the text-based indicator of populist rhetoric. The interpretation of these estimates is the same as for the previous columns, but for populist rhetoric. The positive and significant coefficients in Columns 4-6 indicate that individuals exposed to the lending cut are more likely to vote for parties that use populist rhetoric, regardless of their focus on banking issues. This result supports the overall finding presented in Table 2, where the credit shock causes an increase in intentions to vote for populists. The difference is that, here, populism is defined as a continuous - and not a binary - variable, meaning that we are not just comparing populist and non-populist parties, but parties with different degrees of populism. Moreover, here we focus specifically on the use of populist rhetoric in the context of parliamentary debates, whereas binary classifiers are based on a number of factors, such as a party's policy stances. These results are particularly interesting when compared with the estimates presented in Columns 7-9. In this set of columns, the dependent variable captures the intention to vote for parties that use populist rhetoric *and* talk frequently about the topic of banking and financial crisis. Also in this case, the coefficient of the credit shock is positive and significant at the one per cent level. This result indicates that the credit shock had a positive effect on individual intentions to vote for populist parties that focused on banking and financial crisis. However, it should be noted that the coefficient of the shock is largest when the dependent variable is only populism (Columns 4-6), also when compared to the combined dependent variable (Columns 7-9). This means that the credit shock mostly rewarded parties that adopted a populist rhetoric, holding constant their focus on the topic of banking and financial crisis. Table 6 provides the results based on the dictionary approach using parliamentary speeches. The estimates are not significantly different from the baseline results.

[TABLE 6 ABOUT HERE.]

8 Political Preferences as a Reaction to the Local Economic Distress

In the previous section we identified the overall effect of the credit shock on populist preferences. However, a key challenge when disentangling the effect is to understand what the main drivers are that link the credit supply shock to the revealed political preferences. One limitation of this paper is related to the firm-level data and the missed opportunity to link individuals with their employees if they are employed. In an ideal framework, we would observe the direct effect of the credit shock on individuals employed by firms that had relationships with Commerzbank before the shock, and thus of the potential experience of a firm-level credit contraction on employees. Nevertheless, a direct link would not enable us to explain the channel for the joint reaction of both sides on labour market demand. Instead, we focus on capturing the perceived economic insecurity of individuals located in areas where the credit shock hit hardest. Our view is that individuals in more exposed counties perceive a widespread economic insecurity after the shock takes place, independently of their direct link to the shock *per sé*.[28] If so, individuals would be more likely to radicalise and express dissatisfaction with the political system and punish the mainstream parties – the elites supporting institutions that fostered the local economic distress – explaining the shift in demand for populism. We test this hypothesis by analysing, at county level, whether areas that are more affected by the credit shock experience a contraction in economic outcomes compared to counties that are less affected beforehand. In practice, we estimate the following specification:

$$y_{kt} = \delta_k + \lambda_t + \sum_{r \neq 2008} \beta_r \left[\mathbf{1}\left(Exposure_k > s\right) \times \mathbf{1}\left(t = r\right) \right] + \gamma \boldsymbol{X}_k + \eta_{kt} \tag{7}$$

where y_{kt} denotes the local economic indicator of interest for county k, δ_k and λ_t are county and year fixed effects, respectively, $Exposure_k$ is the exposure to the credit shock measured as dependence on Commerzbank of county k as calculated in Equation (4), s indicates a preferred cut-off for the exposure distribution and $\boldsymbol{X}_k$ are time-invariant covariates at county level. The set of coefficients β_r capture the year-by-year effect of the credit shock in county k on the specific economic indicator y_{kt} relative to the first lag.

Figure 12 plots the estimates from equation (7) for the natural logarithm of the county GDP for each given year using the doubly robust difference-in-differences estimator in Sant'Anna and Zhao (2020), and adopting the median as the cut-off for the treatment variable in analogy to Figure 9. Each dot indicates the estimate of the interaction between the year indicator and the treat-

[28] Huber (2018) provides evidence that the credit shock has no effect on household credit, given features of the German financial system that facilitate switching banks. At most, aggregate lending to commercial banks rose from 2007 to 2010, showing that other commercial banks were able to re-balance the consequences of Commerzbank's lending cut on households.

ment above the median indicator variable, with bars indicating 95% confidence intervals. These estimates show semi-elasticity between counties where the pre-shock exposure to the credit shock is higher than the median of the treatment distribution and counties up to the threshold on the logarithm of the local GDP, re-weighting the outcome evolution for the covariates. We observe that, prior to 2008, the difference in GDP was not significant and constant over time, whereas from 2009 onwards there is a rapid decline in the difference in GDP for counties that are more affected by the shock, except for the first lead which is not significant. The ATT measured from the leads of the estimation in Figure 12 indicates a decrease in regional GDP of around 2.5% for counties that are exposed to the credit shock above the median after the shock takes place. In a similar specification with two-way fixed effects, where the continuous treatment interacts with an indicator variable equal to one for the years after the shock, we find an ATT of -2.1% in local GDP for a one standard deviation increase in exposure to the credit shock. These estimates are comparable to the evidence in Huber (2018), which finds an average GDP loss of 2.2 percent from a standard deviation increase in exposure to the credit shock. Huber (2018) documents extensively that the temporary lending cut propagated from firms to the real economy and may have contributed to the sluggish recovery from the Great Recession even though the banking sector had stabilised by 2010, without any sign of convergence to the level of less affected counties in the subsequent two years.[29] Figure 12 not only confirms this evidence, but also suggests that the convergence might not yet have happened.

[FIGURE 12 ABOUT HERE.]

In Figure A20, we show estimates from equation (7) for the natural logarithm of county-level employment per $1,000$ units using the same method as in Figure 12. From the pre-shock point estimates and the few significant coefficients in the leads after the shock, we cannot exclude the existence of a pre-trend on employment, even though the ATT measured from the leads of the estimation indicates a significant decrease in employment of around 0.9% for counties that are exposed to the credit shock above the median after the shock takes place. Similarly, using the con-

[29] Huber (2018) contributes to an emerging literature that studies the effect of lending cuts on the economy. Using a selected sample of firms, he finds a negative direct real effect on firms dependent on Commerzbank following the credit shock, which reduced their bank debt, capital stock and employment, and which propagates into the real economy with spillover or indirect effects that impact producers of non-tradables and firms with high innovation activities independently of their direct connections to Commerzbank when the aggregate economic environment of a county responds to the lending cut. Amiti and Weinstein (2018) decompose aggregate loan movements in Japan for the period 1990-2010 into bank, firm and industry common shocks, finding that supply-side financial shocks have a large impact on firms' investments. Similarly to Huber (2018), Chodorow-Reich (2014) studies the effect of bank lending frictions on unemployment outcomes, finding that firms that had pre-crisis relationships with less healthy lenders had a lower likelihood of obtaining a loan following the Lehman Brothers bankruptcy in the United States and that they reduced employment by more than pre-crisis clients of healthier lenders. Bentolila, Jansen and Jiménez (2018) show that, during the Great Recession in Spain, a credit shock was responsible for 24% of job losses in client firms of weak banks compared to comparable firms with no significant pre-crisis relationship with weak banks. Finally, Gutierrez, Jaume and Tobal (2021) study the effect of positive bank credit supply shocks available to small and medium firms on fostering formal employment in Mexico, finding a positive and sizeable effect compared to studies in more developed countries.

tinuous treatment interacted with the post dummy in a similar specification with two-way fixed effects, we find an ATT of -0.75% in local employment for a one standard deviation increase in exposure to the credit shock. As suggested in Burda (2011), a combination of factors that provided disincentives for employers to lay off workers during the economic downturn may explain the meagre effect on employment.[30]

To sum up, the evidence suggests that the credit shock generated general economic distress at the local level that perseveres long after the credit shock and, in those counties that experienced more severe economic insecurity, individuals are more likely to shift their political preferences towards more populist parties. However, it does not seem to be the case that the effect is only driven by a specific category of voters who are more directly affected by the credit shock, what we would call an ego-tropic prediction of electoral preferences. In this case, citizens preoccupied with their wallets would support candidates and parties that advance their own economic interests and oppose candidates who appear to threaten them (Kinder and Kiewiet, 1981). Rather, the political preference response is most influenced by the overall local economic condition and the sense of general increased insecurity. This is mostly explained as a socio-tropic reaction compared to a more pocketbook reaction. The socio-tropic citizens are going to vote more according to the country's pocketbook, not their own, which is reflected in the perceived economic situation in their local territory. Those voters will move towards parties that promise to be a contrast to the mainstream that placed people's economic well-being in jeopardy. There is a recurrent argument in political science (Kinder and Kiewiet, 1981; Lewis-Beck and Stegmaier, 2000; Lewis-Beck, Nadeau and Elias, 2008; Colantone and Stanig, 2018*b*) that asserts that voters punish mainstream parties when the economy is weak in favour of parties that promise to restore economic well-being and fight against the elites that caused distress – *i.e.* the banks and the more traditional establishment that supported them, as we discussed in the previous section. We provide further evidence in the analysis in Figure A21. It plots estimates of the coefficient of the interaction between exposure to the credit shock and various moderators in a regression with our binary indicator of individual populist preferences as the outcome variable and using a balanced panel of individuals from 2006 to 2012, including time, county and individual fixed effects. The point estimates express the differential effect of the indicated pre-shock sub-population over the baseline effect of the credit shock on individual populist preferences after the occurrence of the shock. Despite finding negative point estimates for retired, house-owning and higher-educated individuals and positive estimates for the other considered mediating factors, only the additive effect on unemployed individuals is slightly significant with 10%, supporting our thesis.

[30] Burda (2011) only identifies overall cuts in hours per worker during the Great Recession, due to the reticence of employers to hire in the preceding expansion, wage moderation and the adoption of working time accounts. In a separate analysis, we test for the effect of the credit shock on furloughing (*kurzarbeitergeld*), accounting for the total months of this short-term measure in 2009 and 2010 at the individual level, but find no significant effect when comparing individuals who experienced this outcome with those who did not. The results for unemployment insurance are similar.

Are there some residual pre-shock differences between counties? In Table 7, we try to address this question. Using our preferred specification of equation (1) with county and year fixed effects, and individual, household and regional controls, we estimate the effect of the credit shock on political preferences, selecting individuals in county sub-samples based on pre-shock and time-invariant characteristics. In Columns 2 and 3, we restrict estimates to solely rural or urban areas, respectively, using the German denomination in the name of the county as an indicator. We see that the credit shock has a higher magnitude and significant effect on populist preferences of individuals based in rural areas compared with individuals residing in cities.[31] We then analyse pre-shock differences in GDP growth and employment, measured as the average annual growth rate between 2001 and 2006. Splitting the individual sample into counties above and below the 25th percentile for each measure, we find significant differences in the effect of different average pre-shock GDP growth rates on the response to the credit shock in terms of populist preferences, we find a slightly higher effect for counties where employment growth was slow during the last economic expansion. However, we should be careful when interpreting these results as they might highlight unobserved differences between those counties that we are not able to capture with this specification. Instead, we would need to fully saturate the regression, which would yield noisy results.

[TABLE 7 ABOUT HERE.]

9 Drivers of the Populist Preferences Response

In the previous section, we provided evidence that the credit shock has real consequences for the local economy that have translated into individuals changing their political preferences towards populist parties focusing on their county's finances. However, it is possible that there are multiple margins along which heterogeneity in populist preferences may arise even considering a generalised shock. It might be that the general average movement towards populist parties generated by the credit shock identified by the ATT estimated from equation (1), includes considerable differences in the response that are driven by particular features of those individuals. In this section, we address this question by taking a machine learning approach to revealing the key dimensions of the heterogeneity. For this purpose, we first need to determine the individualised treatment effects deriving from our general difference-in-differences settings. In this exercise, for each individual receiving a certain exposure to the credit shock (dose) d determined by the county of residence, we calculate her individualised treatment effect comparing her with a counterfactual individual constructed as the average individual receiving any other dose, after averaging the individual out-

[31] This is in line with Ziblatt, Hilbig and Bischof (2020), who explore the reason why those who vote for far-right parties in Germany tend to cluster in rural communities.

comes for the periods before and after the shock, respectively.[32] In practice, we map individuals i into a group g given their assigned dose $d \in Exposure(k)$, and we match each individual to a synthetic individual assigned to a dose $d' = \mathbb{E}\left[Exposure_k | \, d' \neq d\right]$, which is the leave-out mean of all the doses other than the dose d received by i. Then, we define a two-period specification $\tau \in \{0,1\}$ averaging over the outcome variable $\mathbf{1}\left(Populist = 1\right)_{it}$ for the periods before and after the credit shock for both each individual i and their assigned synthetic individual, who will be assigned to the average outcome of all individuals treated with a different dose than individual i for the two periods. Hence, for each group g, we estimate the following equation:

$$y_{i\tau} = \alpha_g + \delta \cdot D_i + \lambda \cdot \mathbf{1}\left(\tau = 1\right) + \beta_i \cdot D_i \times \mathbf{1}\left(\tau = 1\right) + \gamma \boldsymbol{X}_i + \zeta_{i\tau} \tag{8}$$

where $y_{i\tau}$ measures the average outcome of interest for individual i in the period τ, D_i identifies the treatment received by individual i – either the group dose or the average leave-out dose, $\boldsymbol{X}_i$ is a vector of characteristics that we may want to take into account and $\zeta_{i\tau}$ captures other determinants of the populist preferences. $\hat{\beta}_i$ are the estimates of causal effects at the individual level given by the difference-in-differences specification in Equation (8).[33] Next, I consider the total response in populist preferences as a function of the individualised treatment effects of a matrix of individual characteristics that are shaping the individual response in populist preferences, $\beta \rightarrow \beta_i\left(\boldsymbol{X}_{it}\right)$, using an extreme gradient-boosted tree (XGB) algorithm (Chen and Guestrin, 2016) to predict the response in populist preferences using $\boldsymbol{X}_{it}$ defined as a sparse set of individual characteristics (features vector). XGB is a highly effective and efficient supervised learning algorithm that has been shown to provide state-of-the-art results in many classification tasks, independently of the dichotomous or continuous nature of the assigned target variable. It is particularly well known for being robust to outliers and for automatically handling feature selection. Building on the general gradient tree boosting method (Friedman, 2001), this is preferable to the classic random forest algorithm (Breiman, 2001) because of its training process, leaving the output interpretation unchanged but building predictions on the previously trained decision trees.[34]

[32]This means that, for each individual, we must have at least one period before and after the shock, which is not guaranteed by the rolling panel applied in Table 2, and this reduces the total sample size. When averaging over time for the periods before and after the shock, we calculate a weighted average using the sampling weights for those variables that are affected by sampling at the individual level.

[33]The weighted average of the individualised treatment effects that we obtain through this exercise should be equal to the ATT identified by Equation (1) with the correct weights.

[34]Other methods exploit the random forest algorithm in Breiman (2001) to identify heterogeneous treatment effects using a target variable at the level of observational unit averaging numerous causal trees, where trees differ from one another due to subsampling (Athey and Imbens, 2016, 2018). The estimation of conditional average treatment effects (CATE) with *causal forest* is not preferable for two reasons. First, the methodology in Athey and Imbens (2016) and Athey and Imbens (2018) implies random assignment, which is naturally achieved by randomisation. In our case, we develop a difference-in-differences design as in Equation (1), which relies on the (strong) common trends assumption to achieve orthogonality. Therefore, using causal forests, the unconfoundedness assumption would decay if we did not extract the individualised treatment effects *ex ante* as in Equation (8) as the target variable of supervised learning. Second, the ATT in Equation (1) arises from a county-level treatment, which would not allow us to decompose until we

In Figure 13, we look at the relative informativeness of each feature in predicting the response in populist preferences to the credit shock at the individual level, measuring the prediction gain achieved by the gradient-boosted tree splitting along the dimension of a given feature, *i.e.* its importance relative to all the other features. We estimate four different models including different and/or progressively added features, in addition to a model including all the available features to gain an overview of what features contribute most to explaining the causal effects.[35]

[FIGURE 13 ABOUT HERE.]

In the first bar of Figure 13, a model based only on individual characteristics highlights three main important features that explain the response in populist preferences: East Germany heritage, year of birth and education. In the second bar, we add proxies for health and emotions to the individual characteristics, which seem to play a minor role compared to the three main features. In the third and fourth bars, we show a model based on employment and workplace characteristics and an additive model including wealth and household characteristics, respectively. A key finding arises from these two models: cumulative employment status and occupation prestige play a major role in explaining the response in populist preferences. This also applies in the last bar, where we show a model including all the previously used features. The main takeaway from this evidence is that, even though the response to the credit supply is widespread through the perception of the general economic difficulties in the local economy, the response is driven by those features that characterise individuals as more or less 'vulnerable'. This is similar to the 'outsider status' in Dal Bó et al. (2022): individuals who are already at the margin of the socio-economic status might recognise themselves more in the rhetoric of populist parties when they propose an alternative to the establishment, characterising those parties as the alternative to the 'elite' that endangered their local economy.[36]

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allocate the individual contribution to the county's populist preference response of each individual due to the credit shock. For an overview of supervised machine learning methods in economics, see Athey and Imbens (2019).

[35] Here, we provide a summary of the characteristics included in the different models. Individual characteristics include birth year, age, residence in East Germany before Reunification, marital status, gender, migrant status, years of education, indicators for high-school, college and apprenticeship, retirement status, and living in urban areas. Health and emotions include the number of GP visits, worries about own finances, job security if employed and general economic development, being sad, worried, happy and angry in the last four weeks. Employment and workplace characteristics include being in the labour force, registered as unemployed, total years of unemployment, total working years, months of unemployment and unemployment insurance in the previous year, gross labour income, job prestige scale (EGP), being furloughed, weeks on furlough in the previous year, number of contractual working hours, industry sector, company size, seniority within the firm, commuting distance, and working in the job for which one was trained for. Wealth and household characteristics include individual net wealth, household size, number of children in the household, household disposable income, hand-to-mouth indicator (Kaplan, Violante and Weidner, 2014), household financial assets, household net wealth and presence of outstanding loans. Total combined features are all the previous features taken together.

[36] In Figure A23, we plot the summary plots of the Shapley values from the combined model with features decreasing in the value of their mean SHAP score (Lundberg and Lee, 2017; Lundberg et al., 2020). In terms of robustness, the ranking provided by the average Shapley values is similar to the relative importance plot using the simple gains from the gradient-boosted trees.

10 Conclusion

The electoral rise of populist parties after the Great Financial Crisis triggered a debate on the influence of banking crises on electoral behaviour. So far, however, existing research has identified the economic drivers of populist sentiments outside the banking sector. In this paper we fill this gap and study the causal effect of a drop in credit on electoral preferences in Germany.

Based on an exogenous shock that decreased bank lending in some German counties in 2007–08, we are able to identify the causal effect of the crisis on individual political preferences. We find that voters in counties more exposed to the credit shock were 0.55 percentage points more likely to vote for a populist party than their peers. We study in more depth the link between the shock and individual intentions to vote for populists, taking into consideration the supply side of populism. We find that individuals who were more exposed to the shock were more likely to vote for parties that adopted populist rhetoric, but also for parties that focused on the topic of banking and financial crisis more than others. This suggests that, while populist rhetoric matters to gain the support of individuals exposed to the shock, voters also care about parties that speak closely to their topic of interest, *i.e.* the crisis and bank-related issues for voters hit by the shock. Nevertheless, we identify the effect of the shock to be larger for populist rhetoric than for banking-related issues. This means that, while voters care about these topics being discussed, the credit shock increases their probability of supporting a party that adopts populist rhetoric, regardless of its focus on banking and financial issues.

While we find a robust effect of shock exposure on populist voting, more research is needed to understand the nuances of the mechanism linking the two phenomena. The evidence in Huber (2018) on the economic effect of the Commerzbank shock provides interesting insights that can be used to further explore this matter. While his study shows that the lending cut had a negative impact on the performance of firms exposed to the shock, it also finds that household debt was not directly affected. It partially rules out the hypothesis of a direct mechanism through which individuals more exposed to the shock tend to vote for populist parties *because* they suffer a direct reduction in their personal portfolio. As we also find little evidence of a direct effect of the credit shock on individuals' balance sheets, our plausible interpretation is that voters follow a socio-tropic reaction (Colantone and Stanig, 2018*a*,*b*; Duch and Stevenson, 2008), for which we provide evidence by analysing the impact of the credit shock on counties' economic performance. In line with Huber (2018), counties more affected by the credit shock experience a negative and persistent effect on local GDP. Adding this to our main results, this means that voters base their political preferences on changes in local economic conditions triggered by the shock, rather than on changes in their individual or household-level conditions. In other words, the effect of the lending cut extended broadly across many segments of the population in exposed counties and was not restricted to a specific category of voters.

However, vulnerable individuals are still a key element through which populist parties can gain in popularity. Through the decomposition of the causal effect at the individual level and its prediction, we understand that the features that matter the most to shaping the response in populist preferences are related to the lifetime labour market and the current prestige of the occupation of those individuals. Our evidence is in line with the recent findings on how labour market outsiders align more with populist parties that 'look alike' and adopt a clear position against the establishment, which is seen as responsible for the economic distress and to not care about these borderline socio-economic categories (*e.g.* Dal Bó et al. (2022) and many others).

While we make a clear contribution to explaining the effect of credit supply shocks on populist preferences and the drivers of populism in Germany, our study opens for other lines of research on both the political outcomes affected by credit supply shocks and the alternative drivers of populism. First, from our evidence, it is clear that finance and the economy are not the only drivers of the rise of populism in Germany, as they can only explain a marginal part of the general event. Interestingly, these drivers can interact with latent non-economic causes such as moral values and deeper beliefs, which are an interesting focus for future research in this and a more general context. Second, our analysis leaves general room for a complementary story about which voters could be more attracted to the parties whose speeches stood out, discussing the same issues as other parties but expressing the same ideas in a more emotional way. It could be that voters are not only attracted by the topics that parties discuss, but also care about the relevance of the information that they provide. Linking firms with employers and employees' political preferences would allow us to explore the interaction between the credit shock, the employers and the employees' implications for the political market. Lastly, we limited the scope of our analysis of populist preferences, leaving many other political aspects that might be affected by the credit supply shock as to be studied later. All these interesting extensions that we left unexplained will be material for future research.

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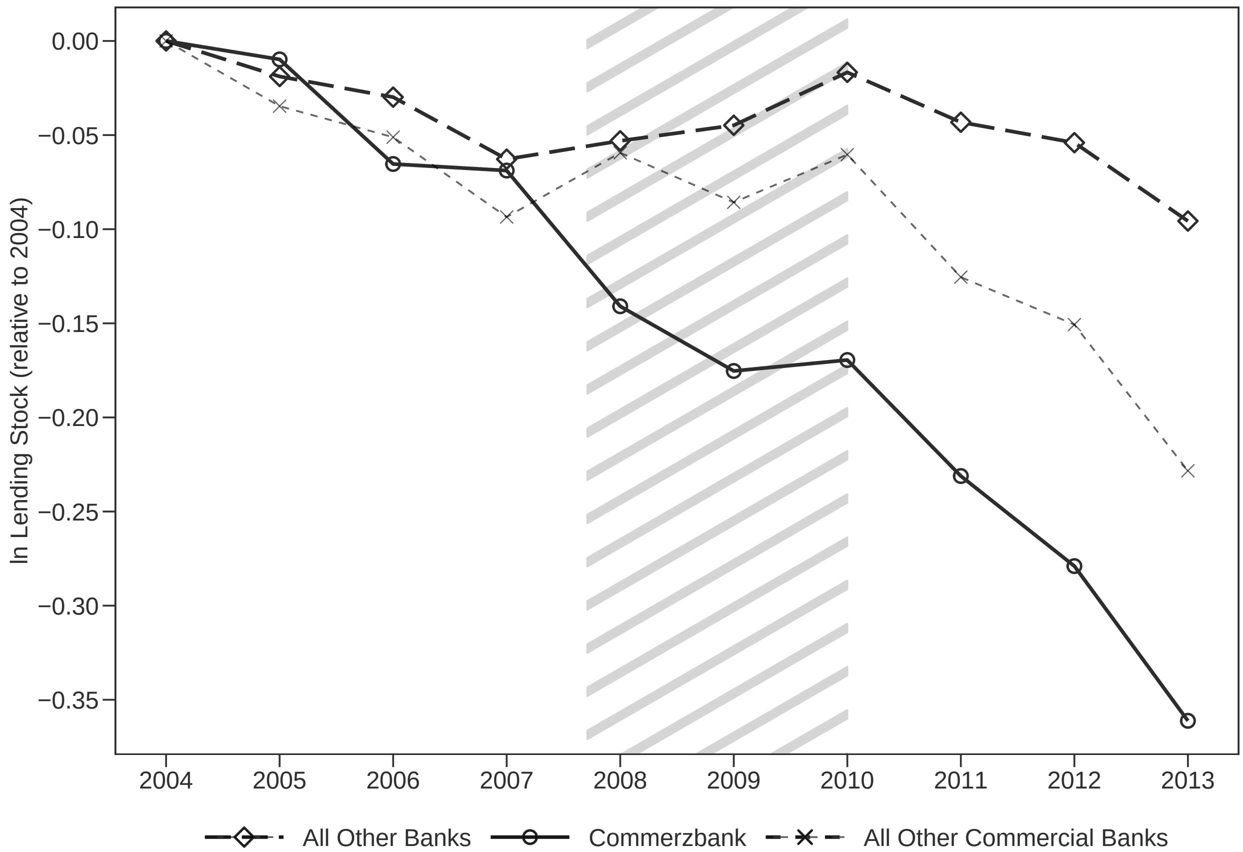
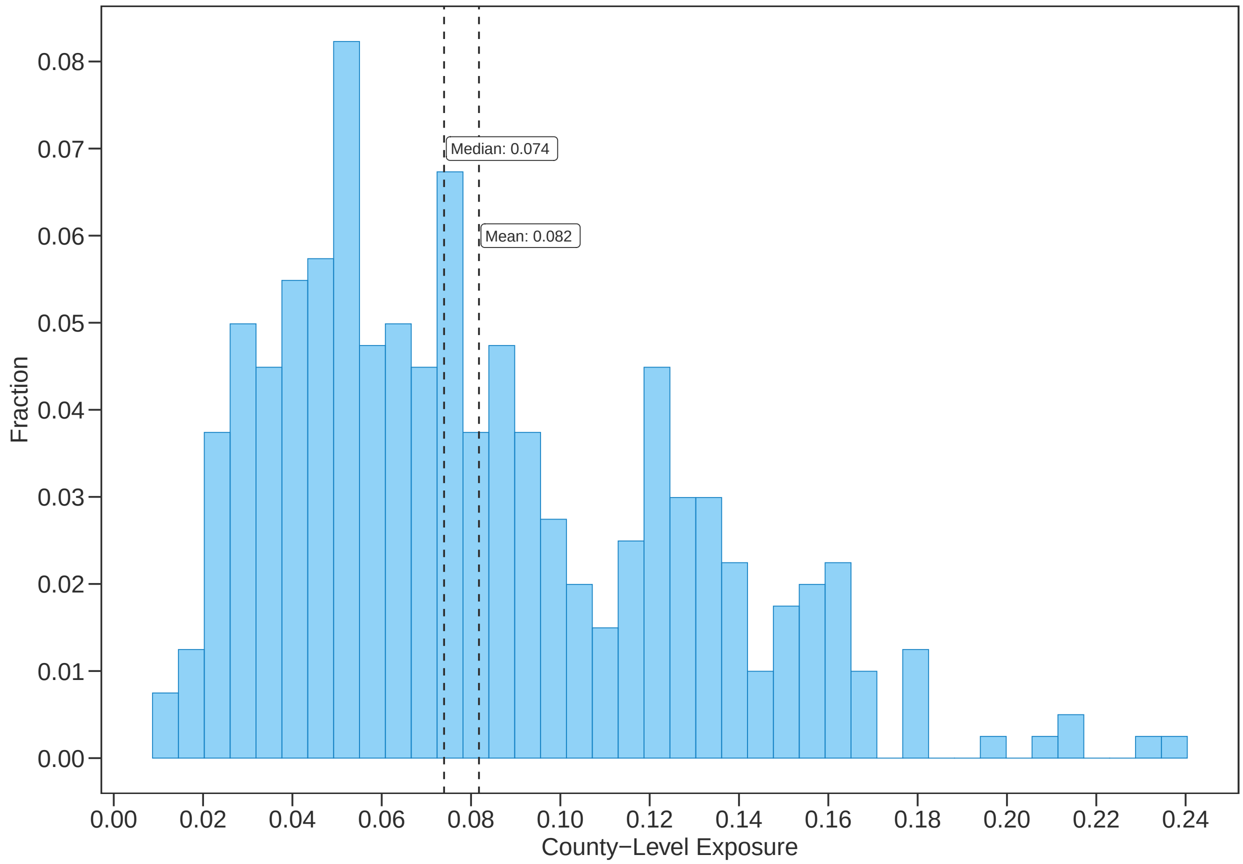
Figures

Figure 1: The Lending Stock of German Banks: Commerzbank vs. All



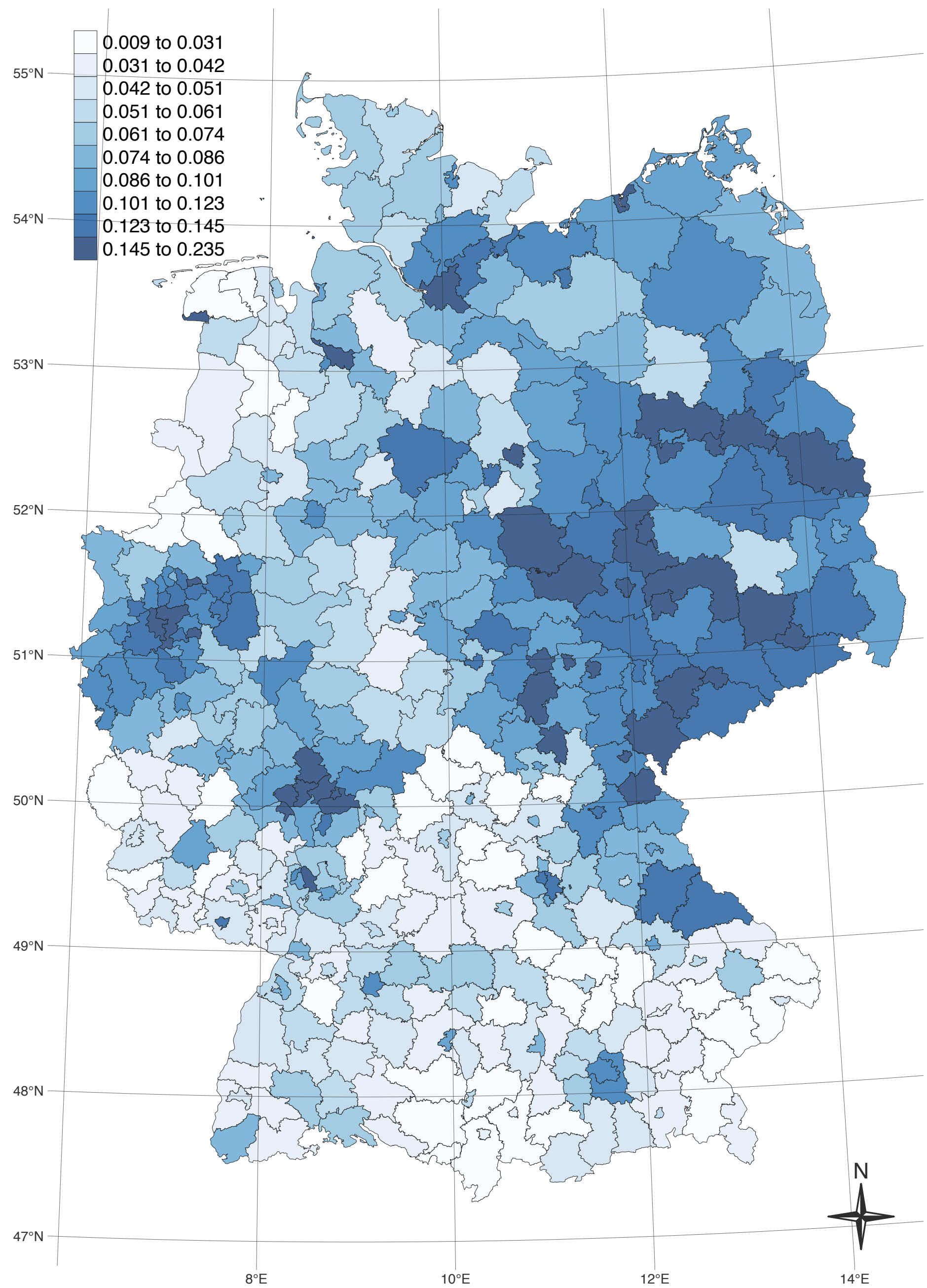
Notes: The graph shows the ln lending stock to German non-financial customers, relative to the year 2004, in 2010 billion euros. Data for Commerzbank include lending by branches of Commerzbank and Dresdner bank, summing their lending stock for the years before the 2009 Dresdner Bank take-over, using information from the annual reports. For all other banks, data come from Deutsche Bundesbank on German banks and subtract lending by Commerzbank. For all other commercial banks, lending stock of Commerzbank, the savings banks, the Landesbanken, and the cooperative banks is removed. The striped area highlights the period during which the Commerzbank lending cut takes place, anticipating the general downturn in the domestic credit. The area starts before 2008 as the lending stocks are at closing date. Replicated from data and calculation in Huber (2018).

Figure 2: Distribution of County-Level Commerzbank Dependence



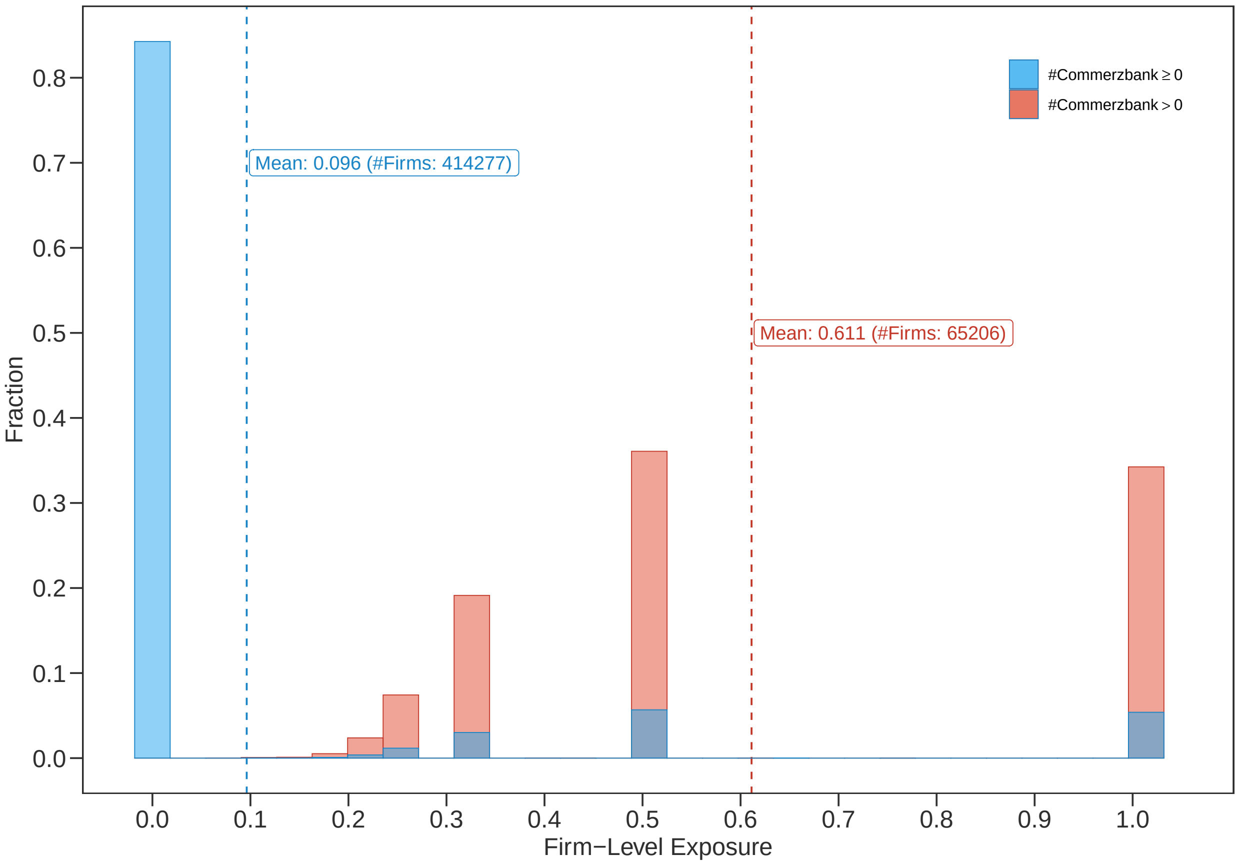
Notes: This figure plots the distribution of the measure of Commerzbank dependence calculated as in Equation (4) at county level in 40 bins. We assign equal weights to firms within each county. We highlight the mean and the median of the distribution with the dashed lines labelled with the exact number. The description of the underlying firms sample is included in Section 4.1 and Figure 4. Source(s): Amadeus, Amadeus Bankers and authors' calculation.

Figure 3: Spatial Variation of the Proxy for the Credit Shock



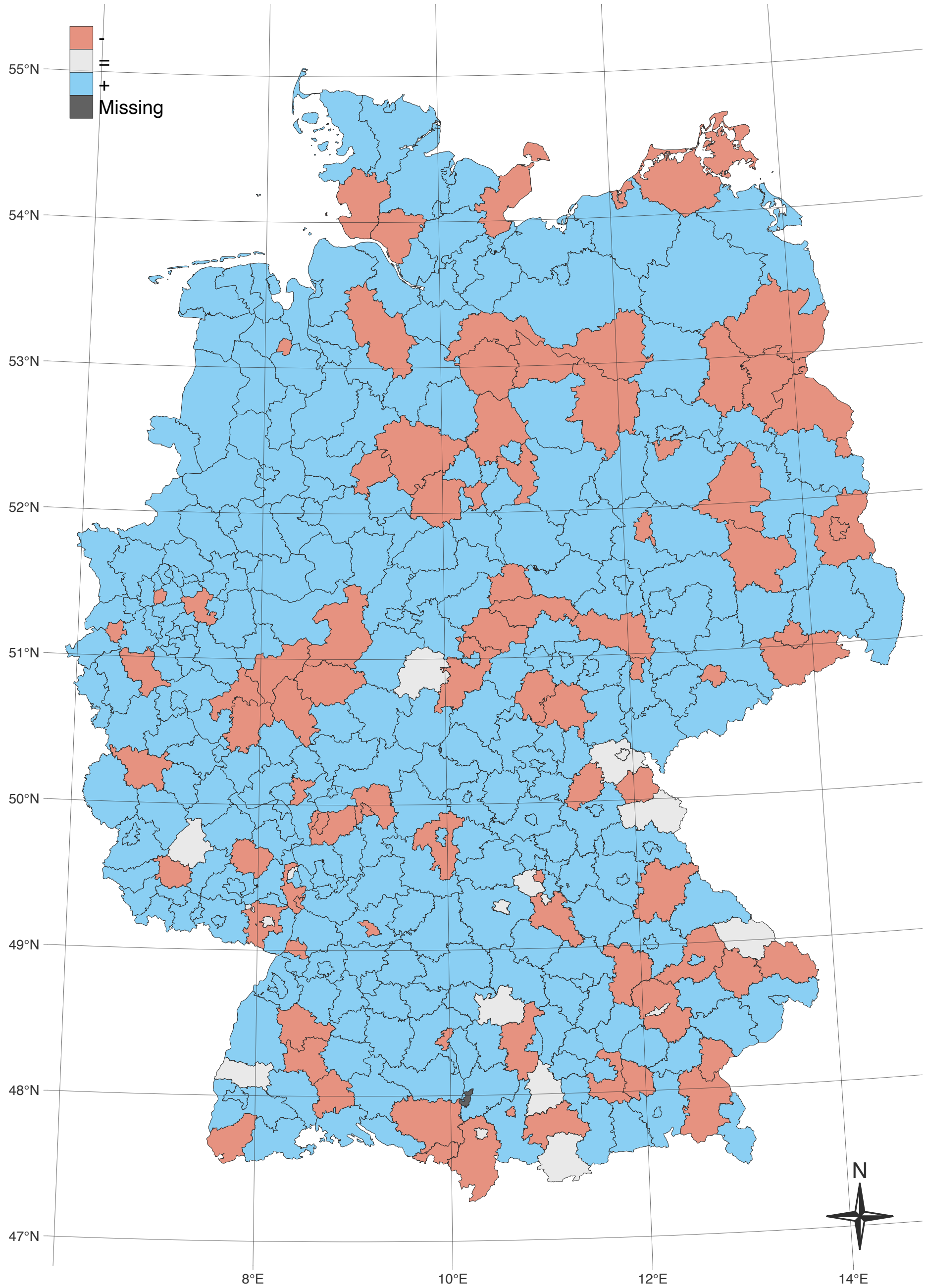
Notes: This picture maps the distribution of the measure of Commerzbank dependence in 2 calculated as in Equation (4) over the German counties. We assign equal weights to firms within each county. Different values of exposure are binned by deciles. The description of the underlying firms sample is included in Section 4.1 and Figure 4. Source(s): Amadeus, Amadeus Bankers, BKG, and authors' calculation.

Figure 4: Distribution of Firm-Level Commerzbank Dependence



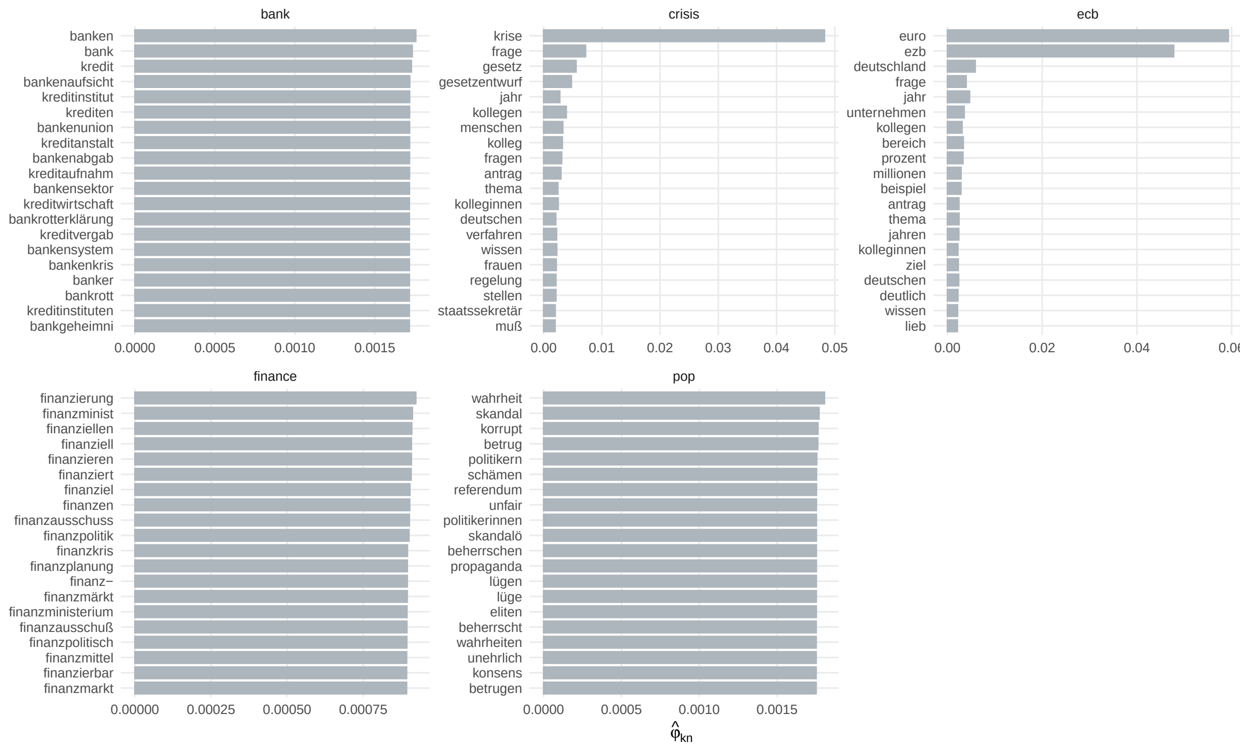
Notes: This picture shows the distribution of the firm-specific Commerzbank dependence in the firms sample. We provide overlapping histograms of a) the unconditional distribution of the firm-level Commerzbank dependence for all firms, and b) the conditional distribution of the firm-level Commerzbank dependence for firms with at least one bank relationship with Commerzbank. The dashed lines indicate the average firm exposure of the unconditional and conditional distribution, respectively. The labels next to the lines indicate the exact mean value and the number of firms involved in the computation. After extracting the firm profiles and matching them with the county code of each firm's location and with the bankers' data as described in Section 4.1, we filter for firms with an incorporation date safely prior to the credit shock in 2007, with a total of 624, 258 firms. We exclude firms in the finance and public sector as in Huber (2018) and Berg, Reisinger and Streitz (2021), following Klapper, Laeven and Rajan (2006) but translated for NACE Rev. 2 two-digit codes, for a total of 414, 277 firms. Before filtering out firms in the finance and public sector from the sample, we are able to match firms with bank relationships for a total of 946, 184 bank relationships, of which 98, 990 are with Commerzbank. After filtering out those firms, the total number of bank relationships in the data amounts to 644, 265, 65, 232 (10.1%) of which are with Commerzbank. Source(s): Amadeus, Amadeus Bankers, and authors' calculation.

Figure 5: Difference in Average Populist Preferences after the Credit Shock



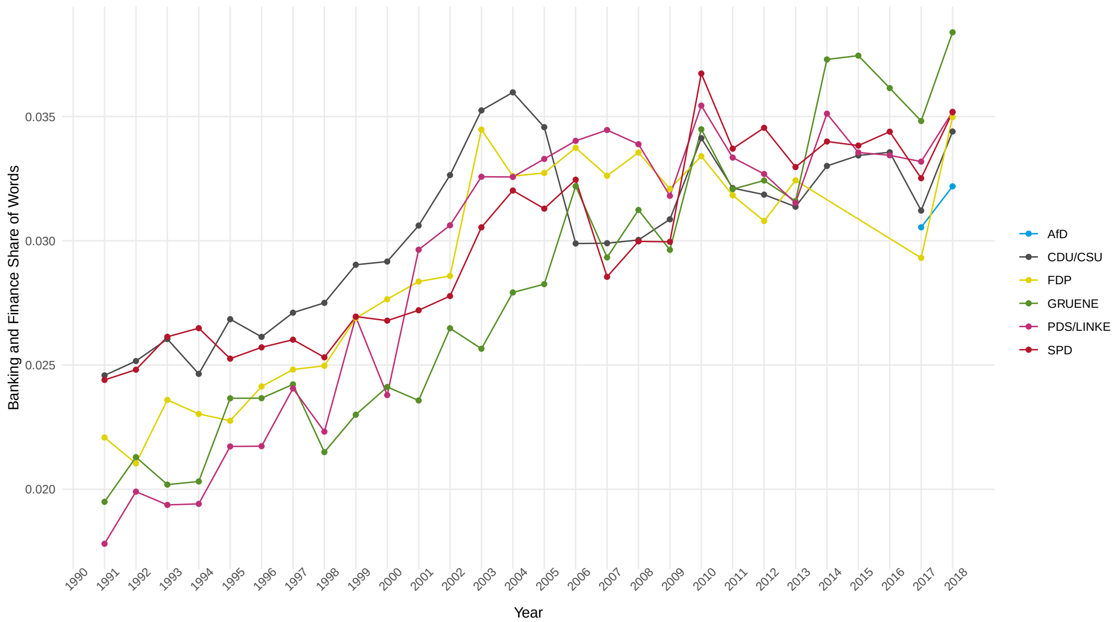
Notes: This map shows the German counties with a higher difference in the share of populist preferences after the credit shock based on the relevant question in the German Socio-Economic Panel (SOEP) described in Section 4.3. First, we calculate the sample-weighted mean at county level of the binary question on political support, pooling all the respondents for all years before the credit shock and all years after, and we note the difference in the two shares of populist preferences. Second, we construct an indicator variable equal to one if the difference in the share is above the median of the distribution. We distinguish between counties that present a difference in the share above the median of the distribution and counties below or at the median of the distribution. In Figure A12, we provide the difference in percentage points before and after 2009 in bins. The sample includes individuals ages at least 16, considers self-reported politically inactive individuals as non-populist, and excludes non-respondents or invalid answers. Non-respondents or invalid answers on political party preferences are only around 2% of the entire sample among those individuals that answer the first question in the affirmative, whereas individuals that refuse to answer the question are around 0.5% in the sample. Source(s): German Socio-Economic Panel (SOEP, Goebel et al., 2019, v36), BKG and authors' calculation.

Figure 6: Top Terms by Posterior Probability using seeded LDA



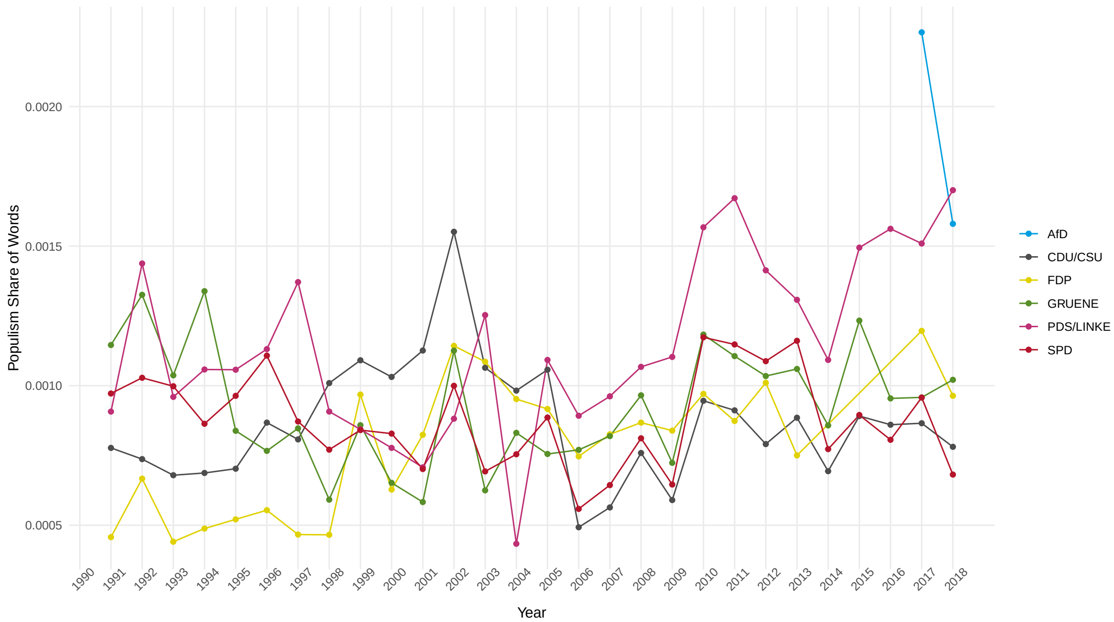
Notes: We use these bar plots to describe the top twenty terms selected using the posterior probability of the seeded LDA models in indicating populist rhetoric and salience of banking and finance-related topics. From left to right, the first four bar plots indicate the top terms for different seeded LDA models on the sub-topics of banking, crises, European Central Bank and finance, from which we select the first five topics to create a score for the salience of banking-related topics. The fifth bar plot shows the top terms for the seeded LDA model on the topic of populism rhetoric, where we select all terms. Seeds for the first four models are uninformative, whereas for the latter we use the Rooduijn and Pauwels (2011) lexicon. The corpus comprises all parliamentary speeches in the German federal parliament from 1991 to 2018 included in the ParlSpeech database (Rauh and Schwalbach, 2020).

Figure 7: Salience of Banking and Finance to Political Discourse by Political Party



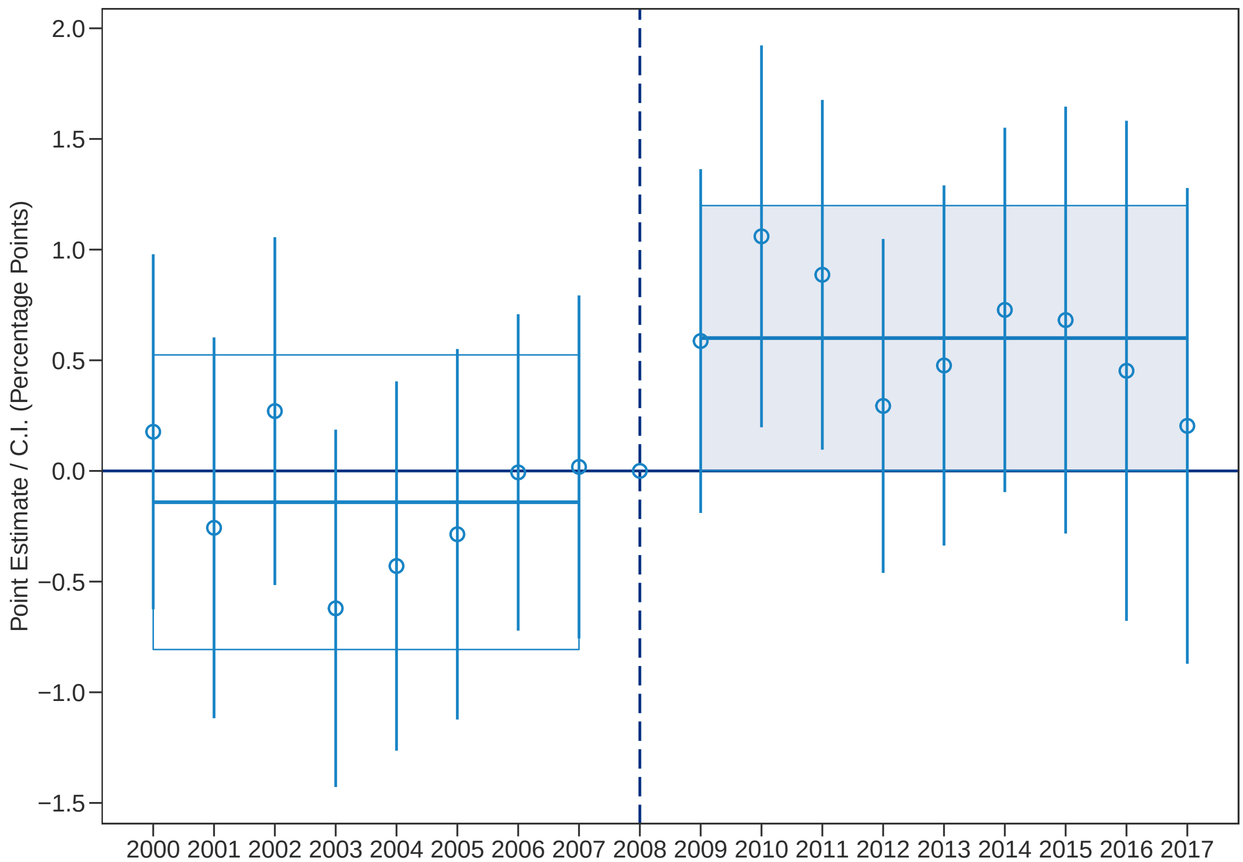
Notes: The picture plots the evolution over time of the score for salience of banking and finance in the political discourse for the major political parties in Germany, including those parties that we identify as populist following Rooduijn et al. (2019) in Section 6.1. For each party p, we calculate a measure over time as described by Equation (5) based on a bag of words of 20 tokens, created by each of the top five terms in each subgroup for banking and finance topics as shown in Figure 6. The corpus of parliamentary speeches comprises all parliamentary speeches in the German federal parliament from 1991 to 2018 included in the ParlSpeech database (Rauh and Schwalbach, 2020), the same corpus is used in the topic modelling exercise and the time window goes from 1991 to 2018. FDP held no seats in the German federal parliament from 2014 to 2016, and we therefore have no values for this party in those years. AfD gained seats in the German federal parliament only from the 2017 elections, which explains the missing values for most of the series. Over the indicated period, German federal elections took place in 1994, 1998, 2002, 2005, 2009, 2013 and 2017.

Figure 8: Populist Rhetoric in Political Discourse by Political Party



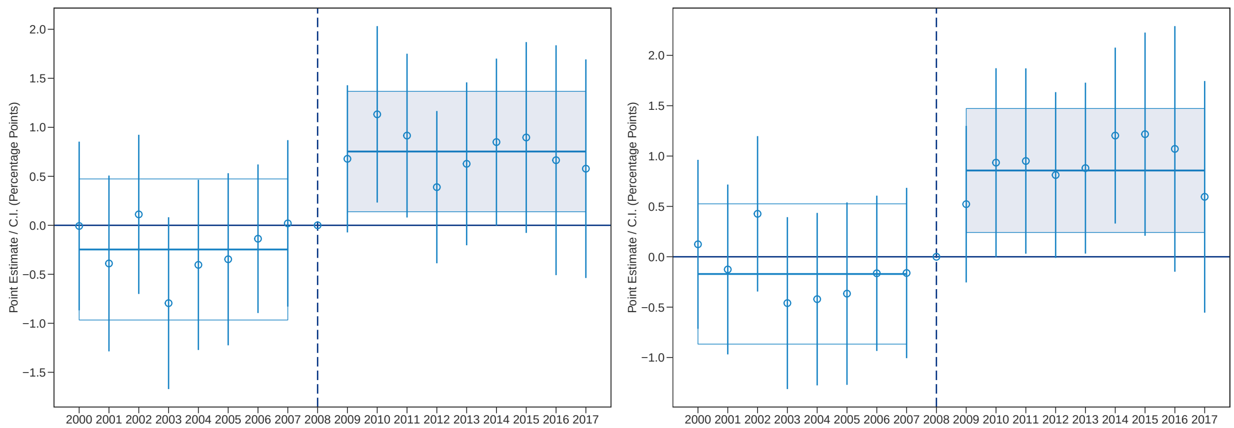
Notes: The picture plots the evolution over time of the score for populist rhetoric in the political discourse for the major political parties in Germany, including those parties that we identify as populist following Rooduijn et al. (2019) in Section 6.1. For each party p, we calculate a measure over time as described by Equation (5) based on a bag of words of 20 tokens, created by each of the top five terms of each subgroup for banking and finance topics as shown in Figure 6. The corpus of parliamentary speeches comprises all parliamentary speeches in the German federal parliament from 1991 to 2018 included in the ParlSpeech database (Rauh and Schwalbach, 2020), the same corpus is used in the topic modelling exercise and the time window goes from 1991 to 2018. FDP held no seats in the German federal parliament from 2014 to 2016, we therefore have no values for this party in those years. AfD gained seats in the German federal parliament only from the 2017 elections, which explains the missing values for most of the series. Over the indicated period, German federal elections took place in 1994, 1998, 2002, 2005, 2009, 2013 and 2017.

Figure 9: The Effect of the Credit Shock on Populist Preferences: Difference-in-Differences Estimates



Notes: In this graph, we formally test for differences in the likelihood that individuals would have stronger political preferences towards populist parties after the occurrence of the credit shock, comparing individuals resident in a treated county at time t with individuals resident in an untreated county at time t. A county k is considered as treated after the occurrence of the credit shock when its exposure to the credit shock – calculated as in Equation (4) with firms being assigned equal weights within the county and excluding finance and public sector firms from the calculation – lies above a cut-off s of the treatment distribution. Here, we qualify the cut-off as $s = med\,(Exposure_k)$, the median of the treatment distribution. Regressions are estimated at the individual level on the full sample of 385, 248 individual-year observations in 401 counties with no controls, weighted using sampling weights and they include county and year fixed effects. Year regression coefficients of interest from the flexible difference-in-differences design in Equation (2) are interactions between an indicator variable equal to one for treated counties and year fixed effects and are estimated relative to the omitted interaction with the first lag before the occurrence of the credit shock. In the more aggregated differences-in-differences design with three time periods in Equation (3), coefficients of interest are interactions between an indicator variable equal to one for treated counties and between a 2000–2007 dummy ($\beta = -0.141,\ p = 0.677$) and a 2009–2017 dummy ($\beta = 0.600,\ p = 0.049$), estimated relative to the omitted interaction with the first lag before the occurrence of the credit shock. Coefficient estimates of the year interactions are plotted as dots with their 95% confidence intervals indicated by vertical lines. Coefficient estimates of the aggregate interactions are shown by horizontal lines, and their 95% confidence intervals are indicated as boxes, unshaded or shaded for the pre- and post-period, respectively. All the point estimates and 95% confidence bands are re-scaled by 100 to be interpreted as percentage points difference from the baseline, and standard errors are clustered at the county level.

Figure 10: The Effect of the Credit Shock on Populist Preferences: Difference-in-Differences Estimates (with controls)

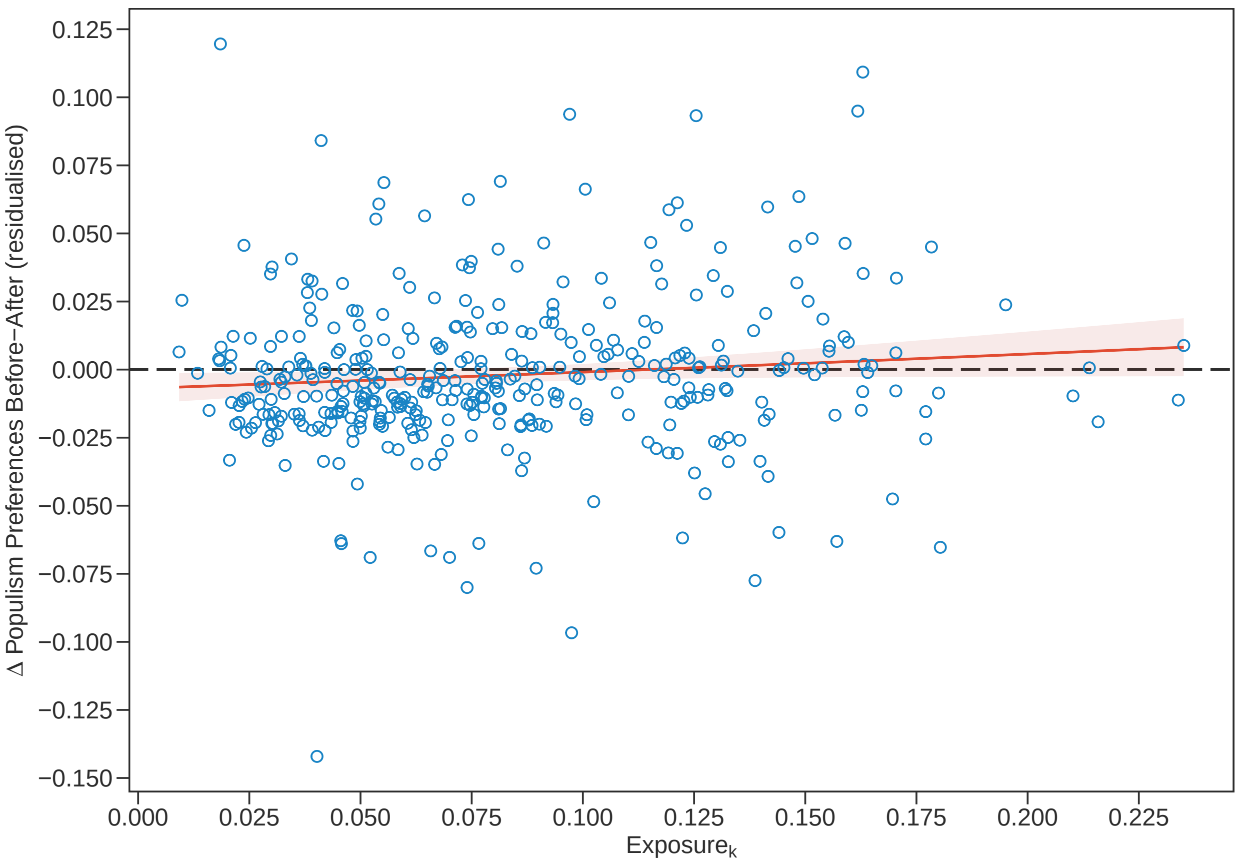


(a) without individual fixed effects

(b) with individual fixed effects

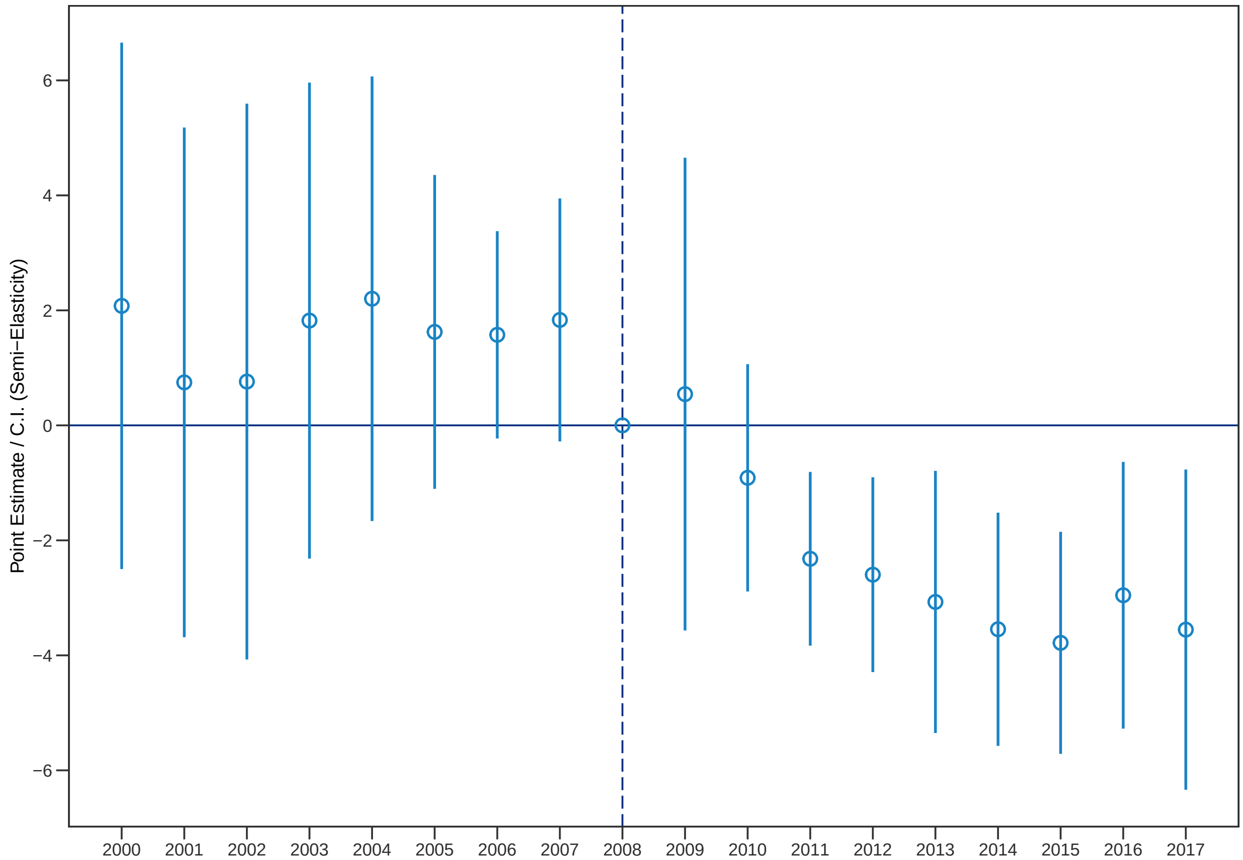
Notes: These graphs are equivalent to the plot in Figure 9 with the sole difference being that regressions are estimated with the addition of the same controls as in Column 4 in Table 2. In Panel 10a regressions are estimated at the individual level for the sample of 362, 122 individuals-year observations within 401 counties with individual-, household- and county-specific controls. See Table 2 for details on the applied covariates. With this specification, the coefficient of interest to the interactions between the indicator variable for treated counties and a 2000–2007 dummy and a 2009–2017 dummy are $\beta = -0.247$ ($p = 0.501$) and $\beta = 0.753$ ($p = 0.016$), respectively. In Panel 10b, regressions are estimated at the individual level for the sample of 351, 304 individuals-year observations within 401 counties with individual-, household- and county-specific controls, introducing individual fixed effects, and omitting time-invariant individual-level covariates. With this specification, the coefficient of interest on the interactions between the indicator variable for treated counties and a 2000–2007 dummy and a 2009–2017 dummy are respectively $\beta = -0.171$ ($p = 0.630$) and $\beta = 0.857$ ($p = 0.006$). Both aggregated coefficients are estimated relative to the omitted interaction, with the first lag before the occurrence of the credit shock. All the point estimates and 95% confidence bands are re-scaled by 100 to be interpreted as percentage points difference from the baseline, and standard errors are clustered at the county level. See Equation (2), Equation (3) and Figure 9 for further details on the difference-in-differences design and the regression estimates.

Figure 11: Functional Form of the Populist Preferences on the Exposure to the Credit Shock: Accounting for Treatment Heterogeneity



Notes: This figure describes the linear relationship between the likelihood that individuals would have stronger political preferences towards populist parties after the occurrence of the credit shock and the distribution of the exposure to the credit shock, calculated as in Equation (4) with firms given equal weight within the county and excluding finance and public sector firms from the calculation. We plot the difference in levels of the (residualised) mean of populist preferences at individual level before and after the lending cut against the distribution of the county-level exposure in levels. Residuals are predicted from a regression of the individual-level outcome variables on individual-, household- and county-specific controls with a sample of 362,122 individual-year observations in 401 counties, using sampling weights, including county and year fixed effects and clustering standard errors at the county level, with the same covariates as in Column 4 in Table 2. After residualising, we compute the mean of the residuals for each level of the continuous treatment before and after the credit shock, and we use the difference in the mean of those values, denoted by the dots in the picture. We filter for three outliers to better depict the linear relationship. The red line illustrates the linear fit, with the shaded area as the 95% confidence intervals. Both variables are kept as levels.

Figure 12: The Effect of the Credit Shock on Local Economic Performance: Difference-in-Differences Estimates

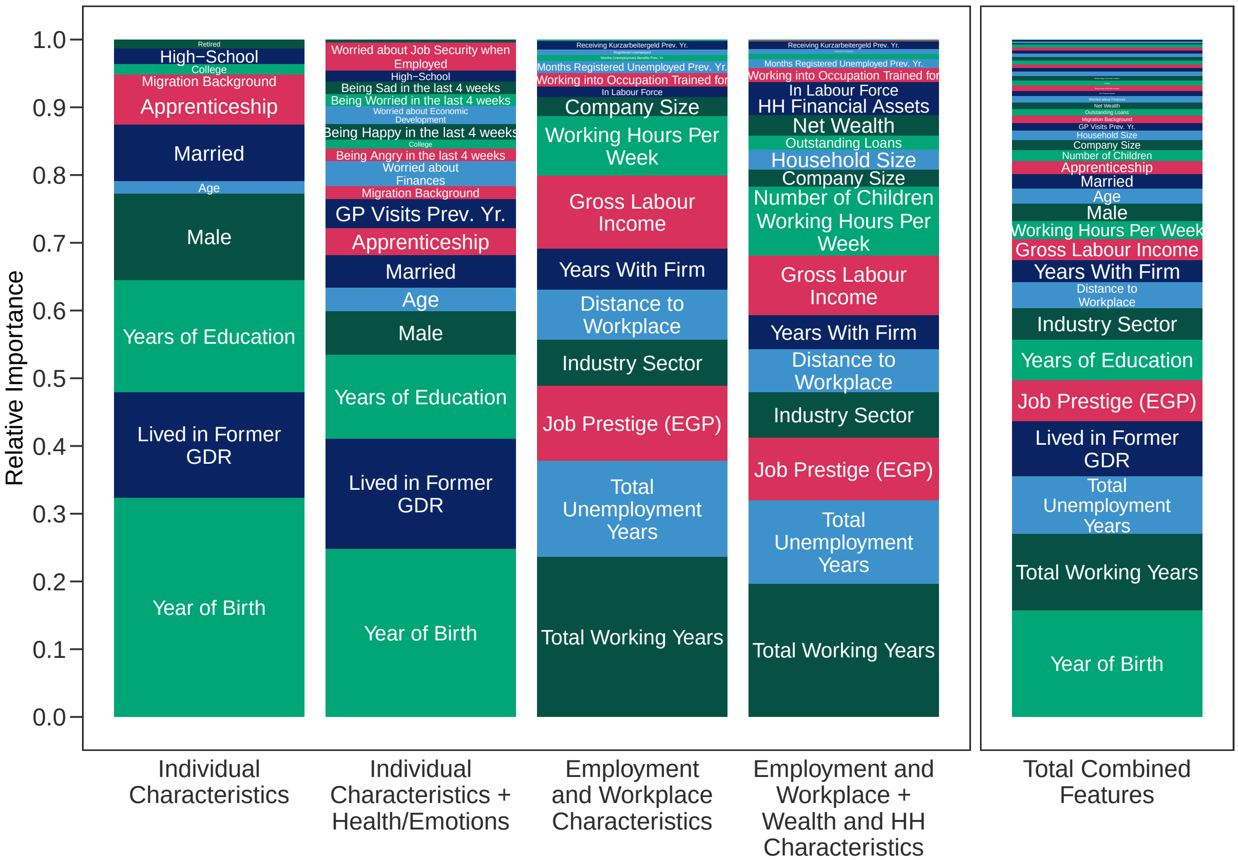


Notes: In this graph, we describe the evolution of the semi-elasticity of the local GDP between treated and untreated counties after the occurrence of the credit shock. A county k is considered as treated when its exposure to the credit shock – calculated as in equation (4) with firms given equal weight in the county and excluding finance and public sector firms from the calculation – lies above a cutoff- s of the treatment distribution. In this figure, we qualify the cut-off as $s = med\,(Exposure_k)$, which is the median of the treatment distribution. Regressions are estimated at county level on a balanced panel of 401 counties from 2000 to 2017 using the doubly-robust difference-in-differences estimator of Sant'Anna and Zhao (2020), which uses weighted least squares to estimate the outcome regressions and inverse probability tilting to estimate the propensity score. The included covariates that serve as re-weighting are an indicator variable for East Germany and an indicator variable for counties exposed to a similar simultaneous crisis of the *Landesbanken* (Puri, Rocholl and Steffen, 2011). Year regression coefficients of interest from equation (7) are interactions between an indicator variable equal to one for treated counties and year fixed effects and are estimated relative to the omitted interaction with the first lag before the occurrence of the credit shock. Coefficient estimates of the year interactions are plotted as dots with their 95% confidence intervals indicated by vertical lines. All the point estimates and confidence bands are re-scaled by 100 to be interpreted as semi-elasticities from the baseline, and standard errors are clustered and bootstrapped at county level. The weighted average of all time average treatment effects on the treated is -2.47% (std. err. 0.0082). Estimating the coefficient of interest β for the continuous treatment in the following specification with two-way fixed effects:

$$y_{kt} = \delta_k + \lambda_t + \beta Exposure_k \times Post + \boldsymbol{X}_{kt} \times Post + \iota_{kt}$$

where $\boldsymbol{X}_{kt}$ includes a dummy for East Germany, a dummy for Landesbanken in Crisis (Puri, Rocholl and Steffen, 2011), the logarithm of population, population density and GDP per capita at county level, we obtain a 2.1% decrease in GDP for each standard deviation increase in the treatment dose ($std.err. = 0.0059$, $t = -3.551$).

Figure 13: Feature Importance in Explaining Populist Preferences Response



Notes: This figure describes the relative informativeness of each feature in predicting the response in populist preferences triggered by the credit supply shock. We measure this by first estimating the causal effects at individual level in a two-period difference-in-differences setting averaging outcomes before and after the shock, where we compare each individual with an assigned dose d with the average individual receiving a different dose d' that we rationalise to the leave-out average dose excluding d. Contrary to what we specify in equation (8), we include no controls in the estimation of the individualised treatment effects. In Figure A22 we show negligible differences in gains when including controls for the estimation of individualised treatment effects. We use the estimates of the causal effects at the individual level as the target variable in an extreme gradient-boosted tree algorithm (Chen and Guestrin, 2016) where we predict the response in populist preferences using features at individual-year level matched with the individual level estimates. The importance of each feature is given by the gain in prediction achieved over all trees by splitting along the dimension of a given feature. We normalise the total gain to one, giving the relative importance of each feature in each model. In the prediction, we first proceed through cross-validation, using the entire sample, to identify the best iteration of trees (settings: rounds = 25000, sample folds = 10, early stopping rounds = 25, metric = RMSE, objective function = squared error regression). Then, we predict the target variable using the best iteration achieved through the cross-validation. Details of the features included in the different models can be found in section 9.

Tables

Table 1: Summary Statistics of Individuals, Households and Counties

	Mean	SD	Median	Min	Max	Non-Missing Obs.
Panel A: Demographic Variables						
Male	0.484	0.500	0.000	0.000	1.000	416,493
Birth Year	1,958.064	19.006	1,959.000	1,902.000	2,000.000	416,490
Age	50.371	18.540	50.000	16.000	105.000	416,490
Residence in GDR in 1989	0.208	0.406	0.000	0.000	1.000	410,576
Married	0.540	0.498	1.000	0.000	1.000	414,609
Direct/Indirect Migrant	0.174	0.379	0.000	0.000	1.000	416,493
Panel B: Education						
Vocational Degree or Higher	0.862	0.345	1.000	0.000	1.000	407,397
University Degree	0.187	0.390	0.000	0.000	1.000	407,397
Years of Education	11.952	2.560	11.500	7.000	18.000	398,224
Panel C: Occupational Status						
Currently Unemployed	0.065	0.246	0.000	0.000	1.000	409,059
In Working Age	0.740	0.439	1.000	0.000	1.000	416,493
In Labour Force	0.807	0.395	1.000	0.000	1.000	332,361
Self-Employed	0.031	0.174	0.000	0.000	1.000	416,493
In Education	0.040	0.197	0.000	0.000	1.000	416,493
Retired	0.046	0.209	0.000	0.000	1.000	416,493
EGP Score (Job Prestige Scale)	4.710	3.025	4.000	1.000	11.000	301,724
Contractual Working Hours per Week	34.249	9.601	38.500	0.300	80.000	191,318
Officially Unemployed Prev. Yr. No. Months	0.916	2.889	0.000	0.000	12.000	332,354
Monthly Gross Earnings (in 2016 EUR)	1,954.152	2,278.426	1,609.442	0.000	1.63e+05	332,361
Panel D: Household Variables						
Household Size	1.934	0.847	2.000	1.000	10.000	416,493
Number of Children in HH	0.411	0.814	0.000	0.000	11.000	416,493
Home-Ownership	0.488	0.500	0.000	0.000	1.000	409,794
Presence of Outstanding Loans	0.357	0.479	0.000	0.000	1.000	409,556
Annual Household Disposable Income (in 2016 EUR)	22045.461	19852.624	20247.000	-8.63e+04	8.38e+05	416,493
Panel E: County-Level Variables						
County GDP (in 2016 mln EUR)	7,128.984	10959.988	4,374.532	1,009.482	1.40e+05	7,155
County GDP per capita (in 2016 EUR)	32418.014	14219.515	28785.982	13772.455	1.81e+05	7,155
Population (1000 units)	205.681	231.220	151.546	33.944	3,613.495	7,155
Population Density (units/km2)	523.811	678.680	199.596	36.129	4,712.758	7,155
Unemployment Rate	7.970	4.256	7.000	1.200	25.400	7,155
Average Household Income (in 2016 EUR)	1,734.457	229.053	1,720.812	1,246.867	3,498.927	7,155
Share of Foreigners	7.607	4.734	6.800	0.600	35.000	7,155
County of Former GDR	0.190	0.392	0.000	0.000	1.000	7,155
Panel G: Variable of Interest						
County-Level Commerzbank Exposure	0.082	0.044	0.074	0.009	0.235	7,155
Panel F: Outcome Variables						
Intention to Vote for Populist Party	0.033	0.180	0.000	0.000	1.000	385,248
Political Supporter	0.453	0.498	0.000	0.000	1.000	385,248
Banking and Financial Crisis Index (sLDA)	3.181	0.270	3.208	2.357	3.745	161,680
Populism Index (sLDA)	0.090	0.024	0.089	0.043	0.227	161,680

Notes: This table shows the mean, standard deviation, median, minimum and maximum value, and non-missing observations of several individual-, household- and county-level characteristics in our repeated cross-sections sample over the period 2000–2017, as well as our outcomes and variable of interest. Individual- and household-level features are analytically weighted for non-response rate, survey stratification and sampling characteristics. Monetary values are adjusted for inflation at 2016 current prices. The first three sections contain summary statistics for demographics, education and occupational status at individual level. The fourth section provides summary statistics for household-level characteristics. The annual household disposable income is partially imputed in five different steps and calculated following Becker and Hauser (2000). The fifth section displays the county-level continuous measure of exposure to the credit shock proxied by the measure of Commerzbank dependence as calculated in Equation (4). Finally, the sixth section lists our outcome variables at the individual level, where the intention to vote for a populist party is described in **??**, and the banking and financial crisis and the populism indexes are the result of the text analysis workflow illustrated in **??** after matching with individual political preferences. Source(s): German Socio-Economic Panel (SOEP, Goebel et al., 2019, v36) for the individual- and household-level variables, Statistisches Bundesamt (DeStatis), Statistische Ämter des Bundes und der Länder (RegionalStatistik) and Bundesamt für Kartographie und Geodäsie (BKG) for county-level variables, AMADEUS for data on firms and bankers, ParlSpeech (Rauh and Schwalbach, 2020, v2) for the parliamentary debates used in the text analysis workflow and authors' calculation.

Table 2: The Effect of the Credit Shock on Populist Preferences: Difference-in-Differences Results

	(1)	(2)	(3)	(4)	(5)
$Exposure_k \times Post$	0.418**	0.517***	0.546***	0.549***	0.542***
	(0.173)	(0.187)	(0.187)	(0.190)	(0.145)
Number of Observations	385,248	362,295	362,122	362,122	362,122
Number of Counties	401	401	401	401	401
Outcome Mean (%)	3.347	3.371	3.373	3.373	3.373
$\sigma\left(Exposure_k\right)$ (%)	4.863	4.861	4.861	4.861	4.861
Adjusted R^2	0.049	0.058	0.059	0.059	0.064
County-Level FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Individual FE	No	No	No	No	No
Individual Controls	No	Yes	Yes	Yes	Yes
Household Controls	No	No	Yes	Yes	Yes
Regional Controls	No	No	No	Yes	No
County Time Trends	No	No	No	No	Yes

Notes: This table reports the results of the estimation of our difference-in-differences specification described in Equation (1), where the dependent variable is our indicator variable of intention to vote for a populist party of an individual i resident in county k at time t, constructed as depicted in Section 6.1, and the variable of interest is the (standardised) exposure to the credit shock of county k, as calculated in Equation (4) using equal weights for all firms within each county, interacted with an indicator variable equal to one for all years after the occurrence of the credit shock. The treatment unit is given by the individuals within each cell (k, t). Sampling weights and county and wave fixed effects apply throughout the results. Column 1 shows the results of two-way fixed effects and no additional controls. Columns 2 to 4 introduce step-wise controls at the individual level, household level, and county-specific controls. Finally, in Column 5 we introduce county-specific linear time trends. The table includes the number of observations, the number of counties, the outcome mean and the standard deviation unit of the treatment for all the results. The coefficients of interest are scaled by 100 to be interpreted as the percentage point increase in the outcome mean of one-σ units increase of treatment after the occurrence of the shock. Standard errors are clustered by county for all specifications, and *, **, and *** denote statistical significance at 10%, 5%, and 1% levels, respectively. In Table A1 we show estimates using the same specifications but including individual fixed effects.

Table 3: The Effect of the Credit Shock on Populist Preferences: Difference-in-Differences Estimates with Binary Treatment

	Median		75th		90th		25th – 75th		10th – 90th	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$\mathbf{1}\left(Exposure_k > s\right) \times Post$	0.972^{***}	0.998^{***}	0.889^{*}	1.158^{**}	1.195^{*}	1.175	1.201^{**}	1.410^{***}	1.301	1.290
	(0.352)	(0.329)	(0.497)	(0.490)	(0.701)	(0.755)	(0.551)	(0.523)	(0.821)	(0.782)
Number of Observations	362,122	351,304	362,122	351,304	362,122	351,304	179,100	173,196	71,851	69,441
Number of Counties	401	401	401	401	401	401	208	208	82	82
Outcome Mean (%)	3.373	3.363	3.373	3.363	3.373	3.363	3.669	3.673	3.773	3.73
s (%)	8.949	8.949	13.093	13.093	16.495	16.495	5.834	5.834	3.886	3.886
Adjusted R^2	0.059	0.526	0.059	0.526	0.059	0.526	0.060	0.531	0.069	0.538
County-Level FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Time Trends	No	No	No	No	No	No	No	No	No	No

Notes: This table shows the estimates of our difference-in-differences specification described in Equation (1) with variation in the treatment variable of interest. The dependent variable is our indicator variable of populist preferences of an individual i resident in county k at time t constructed as depicted in Section 6.1, while the variable of interest is an indicator variable equal to one if the exposure to the credit shock in county k is above a defined cut-off in the treatment distribution, interacted with an indicator variable equal to one for all years after the occurrence of the credit shock. The treatment unit is given by the individuals within each cell (k, t). Sampling weights and county and wave fixed effects apply throughout the results. Each pair of columns defines the cut-off at which the treatment is assigned, where, in the first column, we use individual- and household-level and county-specific controls, and, in the second column, we absorb for individual fixed effects excluding time-invariant individual controls applied in the specification of the first column. Treatment (control) groups in each specification are those individuals that are located in a county with exposure to the credit shock: (i) above (up to) median of the treatment distribution; (ii) above (up to) the 75$^{\text{th}}$ percentile of the treatment distribution; (iii) above (up to) the 90$^{\text{th}}$ percentile of the treatment distribution; (iv) above the 75$^{\text{th}}$ percentile of the treatment distribution (up to the 25$^{\text{th}}$ percentile of the treatment distribution); (v) above the 90$^{\text{th}}$ percentile of the treatment distribution (up to the 10$^{\text{th}}$ percentile of the treatment distribution). The coefficients of interest are scaled by 100 to be interpreted as the percentage point increase in the outcome mean for individuals located in counties with exposure to the credit shock above the indicated threshold relative to individuals located in counties that have a lower exposure beforehand. Standard errors are clustered by county for all specifications, and *, **, and *** denote statistical significance at 10%, 5%, and 1% levels, respectively.

Table 4: The Effect of the Credit Shock on Populist Preferences: Sample Restrictions and Robustness

	Full Sample	Age ≥ 18	Conditional	$\mathbf{1}(t \geq 2007)$	$\mathbf{1}(t \geq 2008)$
$Exposure_k \times Post$	0.549^{***}	0.547^{***}	0.994^{***}	0.461^{**}	0.524^{**}
	(0.190)	(0.189)	(0.384)	(0.196)	(0.204)
Number of Observations	362,122	361,619	162,845	362,122	362,122
Number of Counties	401	401	400	401	401
Outcome Mean (%)	3.373	3.375	7.342	3.373	3.373
$\sigma(Exposure_k)$ (%)	4.861	4.861	4.881	4.861	4.861
Adjusted R^2	0.059	0.059	0.174	0.059	0.059
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Individual FE	No	No	No	No	No
Individual Controls	Yes	Yes	Yes	Yes	Yes
Household Controls	Yes	Yes	Yes	Yes	Yes
Regional Controls	Yes	Yes	Yes	Yes	Yes
County Time Trends	No	No	No	No	No

Notes: In this table, we show the results of the estimation using our preferred specification from Column 4 in Table 2 of the difference-in-differences specification described in Equation (1), where the dependent variable is our indicator variable for intention to vote for a populist party of an individual i resident in county k at time t, constructed as depicted in Section 6.1, and the variable of interest is the (standardised) exposure to the credit shock of county k, as calculated in Equation (4) using equal weights for all firms within each county, interacted with an indicator variable equal to one for all years after the occurrence of the credit shock. In Column 2, we restrict the sample to individuals who have reached the age of majority. In Column 3, we vary our outcome variable conditioning populist preferences for answering affirmatively the question described in Section 4.3, relaxing the assumption that people not actively supporting any party are transferring their preferences to more traditional parties when actually voting. In Columns 4 and 5, we perform placebo tests using different timing for the dummy variable indicating the starting period of the shock. The coefficients of interest are scaled by 100 to be interpreted as the percentage point increase in the outcome mean of one-σ units increase of treatment after the occurrence of the shock. Standard errors are clustered by county for all specifications, and *, **, and *** denote statistical significance at 10%, 5%, and 1% levels, respectively. A further description of the results is provided in Table 2.

Table 5: The Effect of the Credit Shock on Political Preferences: Outcome as Topic Model Scores

	Banking and Financial Crisis			**Populism**			**Combined**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Exposure_k \times Post$	0.060***	0.059***	0.055***	0.137***	0.136***	0.144***	0.070***	0.068***	0.065***
	(0.016)	(0.016)	(0.016)	(0.022)	(0.023)	(0.023)	(0.016)	(0.017)	(0.017)
Number of Observations	161,680	154,562	154,562	161,680	154,562	154,562	161,680	154,562	154,562
Number of Counties	400	400	400	400	400	400	400	400	400
$\bar{y}$	3.181	3.184	3.184	.09	.091	.091	1.636	1.637	1.637
σ_y	.27	.268	.268	.024	.024	.024	.14	.139	.139
$\sigma\,(Exposure_k)$ (%)	4.875	4.873	4.873	4.875	4.873	4.873	4.875	4.873	4.873
Adjusted R^2	0.602	0.600	0.601	0.479	0.482	0.483	0.582	0.580	0.580
County-Level FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Household Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Regional Controls	No	No	Yes	No	No	Yes	No	No	Yes

Notes: The table reports the results of the estimation of our difference-in-differences specification described in Equation (1) where the outcome variables are given by the (standardised) scores of each party at time t calculated as in Equation (5) using the bag of words for populist rhetoric and salience of bank-related topics obtained using the seededLDA algorithm on the representatives' parliamentary speeches for each party, and matched to the individual political preferences from the survey data. The variable of interest is the (standardised) exposure to the credit shock of county k, as calculated in Equation (4), interacted with an indicator variable equal to one for all years after the occurrence of the credit shock. Both outcome and treatment are expressed in standard deviations. Standard errors are clustered by county for all specifications, and *, **, and *** denote statistical significance at 10%, 5%, and 1% levels, respectively.

Table 6: The Effect of the Credit Shock on Political Preferences: Outcome as Dictionary Scores

	Banking and Financial Crisis			**Populism**			**Combined**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Exposure_k \times Post$	0.037***	0.039***	0.043***	0.136***	0.135***	0.143***	0.052***	0.054***	0.058***
	(0.006)	(0.006)	(0.006)	(0.022)	(0.024)	(0.024)	(0.008)	(0.008)	(0.008)
Number of Observations	161,680	154,562	154,562	161,680	154,562	154,562	161,680	154,562	154,562
Number of Counties	400	400	400	400	400	400	400	400	400
$\bar{y}$	.84	.842	.842	.121	.121	.121	.48	.482	.482
σ_y	.247	.245	.245	.029	.029	.029	.126	.126	.126
$\sigma\,(Exposure_k)$ (%)	4.875	4.873	4.873	4.875	4.873	4.873	4.875	4.873	4.873
Adjusted R^2	0.900	0.902	0.902	0.435	0.439	0.440	0.872	0.874	0.875
County-Level FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Household Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Regional Controls	No	No	Yes	No	No	Yes	No	No	Yes

Notes: The table shows the results of the estimation of our difference-in-differences specification described in Equation (1) where the outcome variables are given by the (standardised) scores of each party at time t calculated as in Equation (5) using the bag of words for populist rhetoric and salience of bank-related topics that we use as seeds included as priors in the seededLDA algorithm on the representatives' parliamentary speeches for each party, and matched to the individual political preferences from the survey data. The variable of interest is the (standardised) exposure to the credit shock of county k, as calculated in Equation (4), interacted with an indicator variable equal to one for all years after the occurrence of the credit shock. Both outcome and treatment are expressed in standard deviations. Standard errors are clustered by county for all specifications, and *, **, and *** denote statistical significance at 10%, 5%, and 1% levels, respectively.

Table 7: The Differential Effect of the Credit Shock on Political Preferences: County Characteristics

	Full Sample	Urban Areas	Rural Areas	$\overline{\Delta GDP}_k^{2001-2006}$ $\geq 25^{th}$	$\overline{\Delta GDP}_k^{2001-2006}$ $< 25^{th}$	$\overline{\Delta Employment}_k^{2001-2006}$ $\geq 25^{th}$	$\overline{\Delta Employment}_k^{2001-2006}$ $< 25^{th}$
$Exposure_k \times Post$	0.549***	0.287	0.424**	0.395**	0.901***	0.470**	1.060***
	(0.190)	(0.351)	(0.196)	(0.177)	(0.287)	(0.238)	(0.386)
Number of Observations	362,122	108,637	253,485	275,308	86,814	274,095	88,027
Number of Counties	401	107	294	302	99	302	99
Outcome Mean (%)	3.373	3.944	3.109	3.053	4.374	2.481	6.684
$\sigma\left(Exposure_k\right)$ (%)	4.861	4.405	3.965	4.742	5.061	5.102	3.613
Adjusted R^2	0.059	0.059	0.059	0.053	0.076	0.052	0.055
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	No	No	No	No	No	No	No
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table explores the difference in the effect of the credit shock on populist preferences among different county-level, pre-shock or time-invariant characteristics. It shows the results of the estimation of our difference-in-differences preferred specification, analogue to Column 4 of Table 2, excluding controls for GDP and GDP per capita. Column 1 shows the estimate of the effect of the credit shock on the full sample. Columns 2 and 3 report results of the coefficient of splitting urban and rural areas, respectively. In Columns 4 and 5 we split the sample, considering the average annual growth rate between 2001 and 2006 for counties above and below the 25^{th} percentile, respectively. Finally, in column 6 and 7 we split the sample considering the average annual growth in employment between 2001 and 2006 for counties above and below the 25^{th} percentile, respectively. For all sub-samples, we include county and year fixed effects, individual, household and regional controls for variables like population density and share of foreigners. The table includes the number of observations, the number of counties, the outcome mean and the standard deviation unit of the treatment for all the results. The coefficients of interest are scaled by 100 to be interpreted as the percentage point increase in the outcome mean of one-σ units increase of the treatment after the occurrence of the shock. Standard errors are clustered by county for all specifications, and *, **, and *** denote statistical significance at 10%, 5%, and 1% levels, respectively. In Table A6 we provide additional evidence of different splits in the distribution of average annual growth and employment growth, and looking at the two growth rates in 2006.

Credit Shocks and Populism

Online Appendix

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Contents

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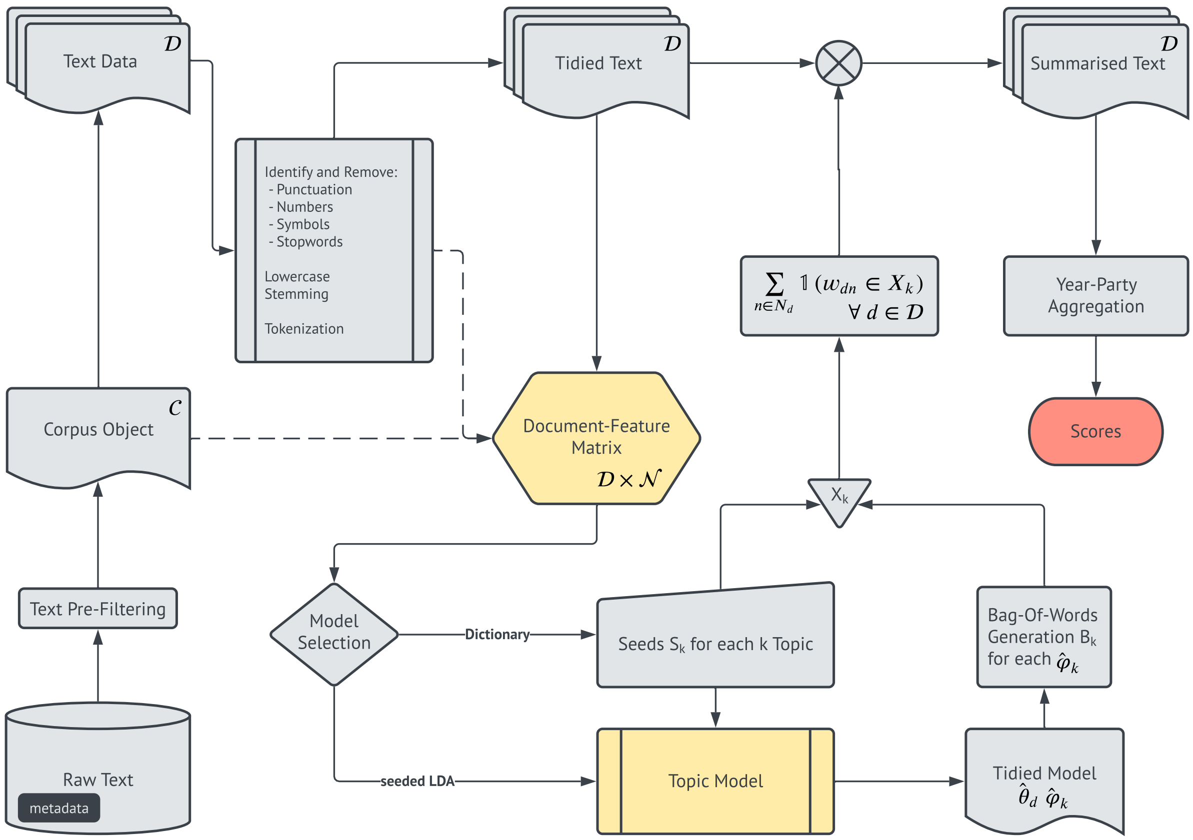
A Text Analysis Workflow with Topic Modelling

Figure A1: Flowchart of the Text Analysis Workflow including topic modelling

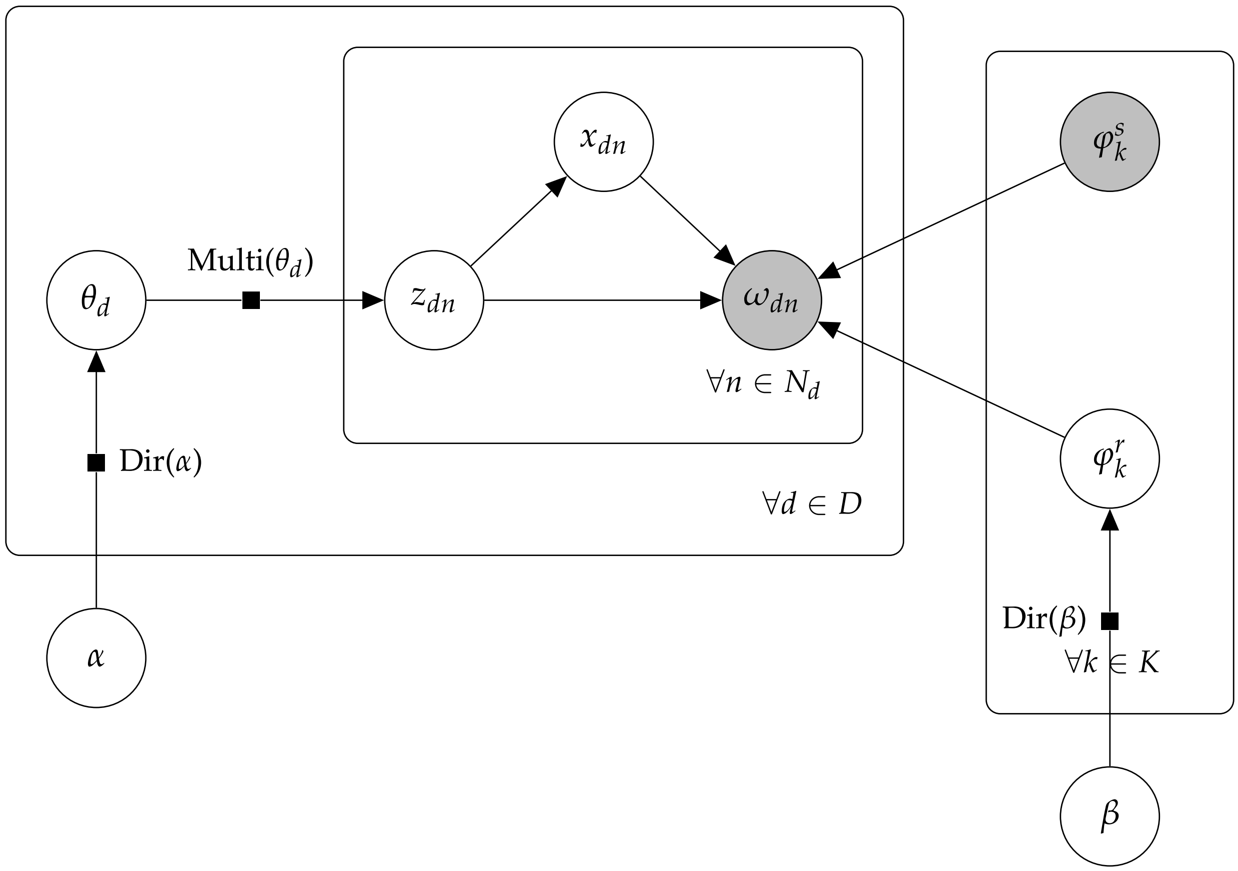


In Figure A1, we describe in detail the process of our text analysis workflow. The entry point is always the raw textual data, either from ParlSpeech (Rauh and Schwalbach, 2020, v2) or the *Comparative Manifesto Database* (Burst et al., 2020) with some related metadata describing in particular the party, the year and the contributor. The raw text is pre-filtered using simple adjustments on metadata and common mistakes, and shaped as a Corpus object. Once the text is shaped as a data-frame, we pre-process it. In particular, we identify and remove punctuation, numbers, symbols and stopwords[1]. For simplicity, we transform the text data as lowercase to perform the tokenisation in uni-grams. From the token data, we create the document-feature matrix at which we either apply the topic model or not based on the model selection decision, and we calculate the sum of matched terms for each topic using either the bag-of-words obtained as in (A.1) or the seeds lexicon. After that, we apply the aggregation decision at year-party level as described by Equation (5) or (6) in Section 6. Theoretical guidance for the right level of aggregation is often

[1]For the identification of stopwords, we both use the standard dictionary of German stopwords in the `quanteda` R package and an extended dictionary from the Github repository of `solariz`.

limited, which makes it an important dimension along which to check the sensitivity of results. This is an additional reason to why we also include textual data from political manifestos, where aggregation is irrelevant as we have one single manifesto for each election year and each party.

Figure A2: Plate Notation Diagram of the seeded Latent Dirichlet Allocation (seededLDA)



We illustrate the Bayesian network of topic model applied to the workflow using the plate notation in Figure A2. We define $\mathcal{D}$ and $\mathcal{N}$ as respectively the row and column dimensions of the document-feature matrix $\mathcal{D} \times \mathcal{N}$ obtained from the corpus $\mathcal{C}$. $\theta_d \sim \text{Dir}\,(\alpha)$ and $\varphi_k^r \sim \text{Dir}\,(\beta)$ are respectively independent draws for each document $d \in \mathcal{D}$ and for each topic $k \in K$ to generate the document-specific topic distribution and the per-topic general words distribution. In our exercise, the hyper-parameters α and β are sparsely selected ($\alpha = 0.5, \beta = 0.1$). Each (observed) word ω_{dn} in document d is generated from a *two-step* process:

(i) draw the topic assignment $z_{dn} \sim \text{Multinomial}\,(\theta_d)$ which gives a Markov blanket with α as parent and $z_{dn}\ \forall\ n \in N_d \subset \mathcal{N}$ as children;

(ii) draw $\omega_{dn} \sim \text{Multinomial}\left(\varphi_k^f \mid x_{dn}\right)$ with $f = \{r, s\}$, where x_{dn} is a switch variable drawn from a Beta distribution for each topic and on the basis of the value of x_{dn} either the draw from the general per-topic words distribution φ_k^r or the draw from the prioritised named entity words distribution from the (observed) seeds φ_k^s is selected.

In our application, we perform Bayesian inference using Gibbs sampling as Markov Chain Monte Carlo algorithm. In this case, as in the simpler formats of LDA, the Dirichlet distribution is particularly useful because when blended with a Multinomial distribution returns again a Dirichlet posterior. From the Bayesian network we obtain two main important predictions for our purpose:

(a) $\hat{\theta}_d$ the document-specific posterior probability distribution of topics, which we use to identify the most salient documents for each topic k as in the examples of Section C;

(b) $\hat{\varphi}_k$ the per-topic posterior probability distribution of (unique) words, which we use to create the bag-of-words for the creation of the time-party index for each topic.

We can think of $\hat{\varphi}$ simply as a $\mathcal{B} \times \mathcal{K}$ matrix of posterior probability scores, with $\mathcal{B} = \{b_1, b_2, \ldots, b_B\} \subset \mathcal{N}$ the set of unique words in the corpus $\mathcal{C}$ and $\hat{\varphi}_k = (\hat{\varphi}_{kb_1}, \hat{\varphi}_{kb_2}, \ldots, \hat{\varphi}_{kb_B})$ the set of posterior probabilities for each unique word in the topic k. On the basis of each $\hat{\varphi}_k$, we can retrieve the subset of $\nu < B$ features with the highest posterior probability within a topic $k \in K = \{BF, POP\}$ as the following set:

$$B_k := \left\{ b_j : \hat{\varphi}_{kb_j} \geq \hat{\varphi}_{kb_r} \; \forall \, \mathcal{B} \setminus \{b_1, b_2, \ldots, b_\nu\} \right\} \tag{A.1}$$

where $j = \{1, 2, \ldots, \nu\}$ is an index to identify any j word in the ν set of words fulfilling the requirements in the set rule. The obtained set from (A.1) defines the bag-of-word for each topic k used in the year-party aggregation at (5) in Section 6, where $\nu = 20$.

B Text Analysis Seeds and Lexicons

We input two main sets of keywords in order to perform both text analysis approaches, i.e. seeded LDA and dictionary technique. While the terms are the same we use them differently depending on the approach. For seeded LDA, we use them as initial 'seeds' to guide the topic model (see Section A for more details). For the dictionary approach, we use them as lexicons, meaning that we compute the frequency of these terms in each document (weighted by the number of terms in each document).

In order to capture the discussions on banking, finance and the crisis, we create four different subgroups based on a parsimonious selection of terms. The lists of stemmed terms for each subtopic are the following:

- Banking: 'bank*', 'kredit*';
- Finance: 'finanz*';
- Central banking: 'ezb', 'europaeische zentralbank', 'euro';

- Crisis: 'krise', 'finanzkrise', 'bankenkrise'.

We use the list of terms provided by Rooduijn and Pauwels (2011) to capture populist rhetoric. This list is made of the following twenty stemmed terms: 'elit*', 'konsens*', 'undemokratisch*', 'referend*', 'korrupt*', 'propagand*', 'politiker*', 'taüsch*', 'betrüg*', 'betrug*', '*verrat*', 'scham*', 'schäm*', 'skandal*', 'wahrheit*', 'unfair*', 'unehrlich*', 'establishm*', '*herrsch*', 'lüge*'.

C Examples of Speeches

In this section we provide some examples of speeches that feature a high score as captured by the seeded LDA relative to other speeches. For each example we report the original text and the translation using Google Translate and DeepL[2].

Populist Rhetoric. The following speeches score high in the seeded LDA trained on populist rhetoric:

Frau Präsidentin! Meine Damen und Herren! Wir lehnen diesen Antrag ab, und zwar allein deshalb, weil die peinliche Einbringungsrede des Bundesfinanzministers eine sofortige Antwort erfordert.
Translation: *Madam President! Ladies and Gentlemen! We reject this motion, for the sole reason that the embarrassing contribution speech of the Federal Minister of Finance requires an immediate response.*

Matthäus-Maier [SPD]: Dummes Zeug! Theo Waigel [CDU/CSU]: Das ist kein dummes Zeug, Frau Kollegin Matthäus-Maier.
Translation: *Matthäus-Maier [SPD]: Stupid stuff! Theo Waigel [CDU/CSU]: That's not stupid stuff, Ms Kollegin Matthäus-Maier.*

Hans-Dietrich Genscher (FDP, 1991): Herr Kollege, so ist es. Wenn Sie Unterlegenheitsgefühle haben, schlage ich Ihnen vor: Wirken Sie mit bei der Entwicklung des europäischen Pfeilers, dann werden Sie auch dieses letzte Gefühl der Unterlegenheit verlieren . Briefs [PDS/Linke Liste]: Sie glauben gar nicht, mit welch dumpfen Gefühlen Men - schen in Westeuropa die Politik dieser Bun - desregierung betrachten!
Translation: *Hans-Dietrich Genscher (FDP, 1991): Sir, that's how it is. If you feel inferior, I suggest that you help develop the European pillar, then you will lose that last feeling of inferiority. Briefs [PDS/Linke Liste] : You do not believe the dull feelings with which people in Western Europe view the policy of this federal government!*

Banking and Financial Crisis topic. The following speeches score high in the seeded LDA

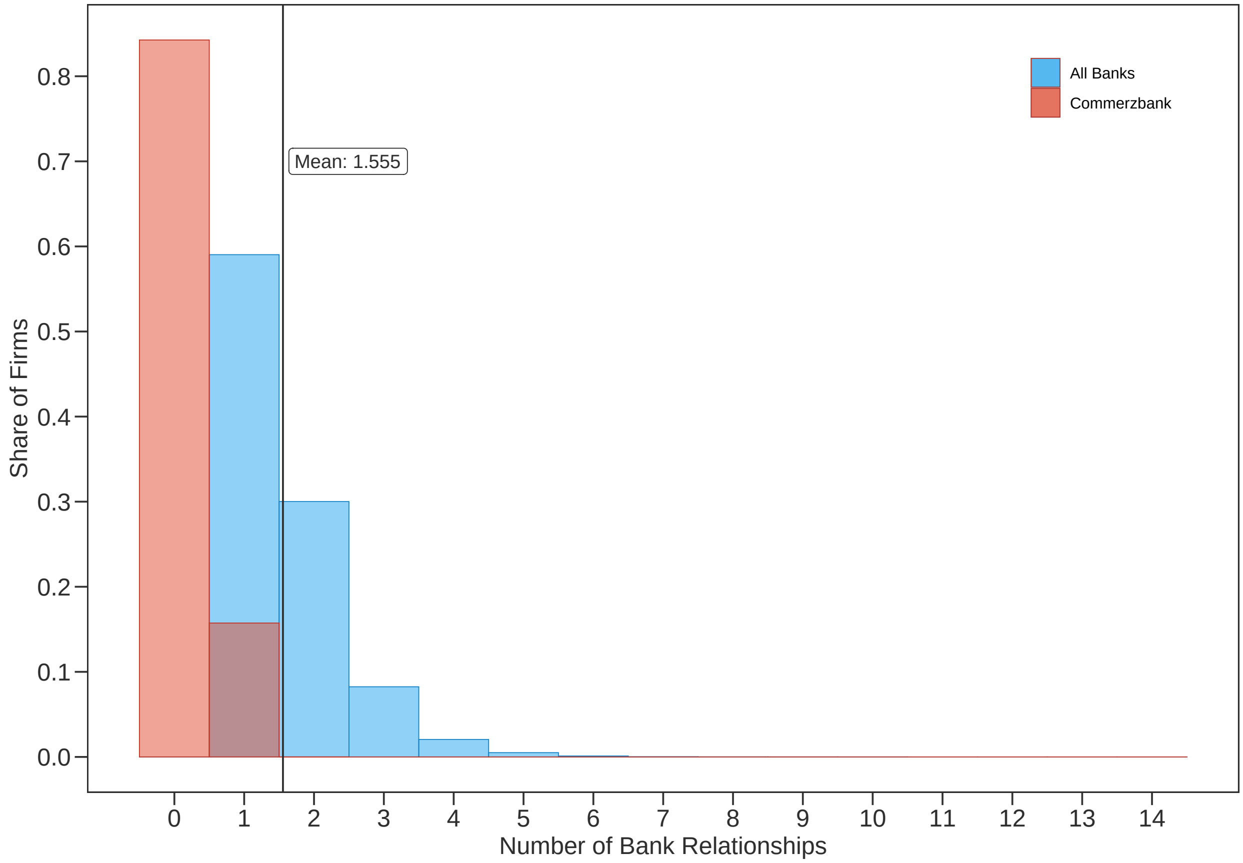
[2] A deep-learning powered translator freely available at https://www.deepl.com/translator.

trained to capture the topic of banking, finance and the crisis:

(TODO) ADD TOPIC EXAMPLES HERE.

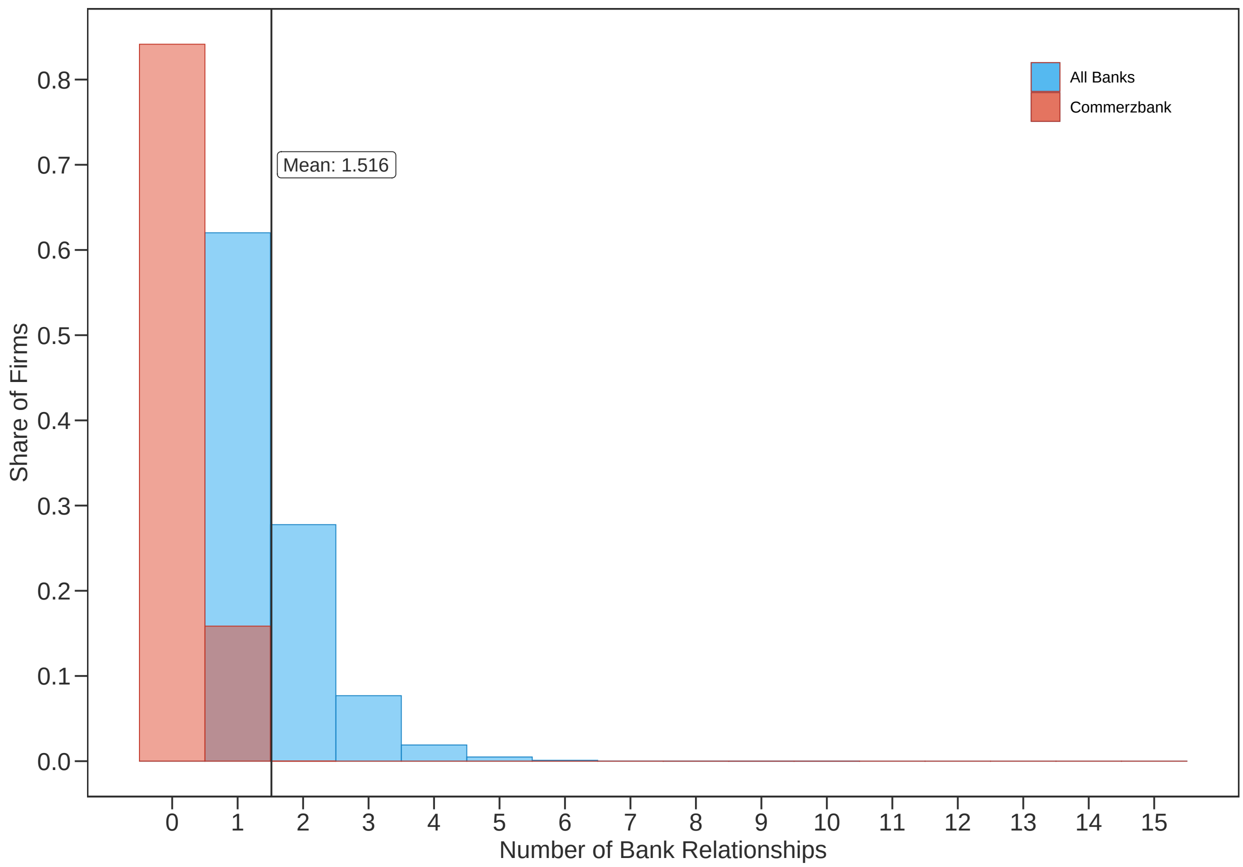
D Additional Figures

Figure A3: Distribution of the Number of Firm-Bank Relationships per firm



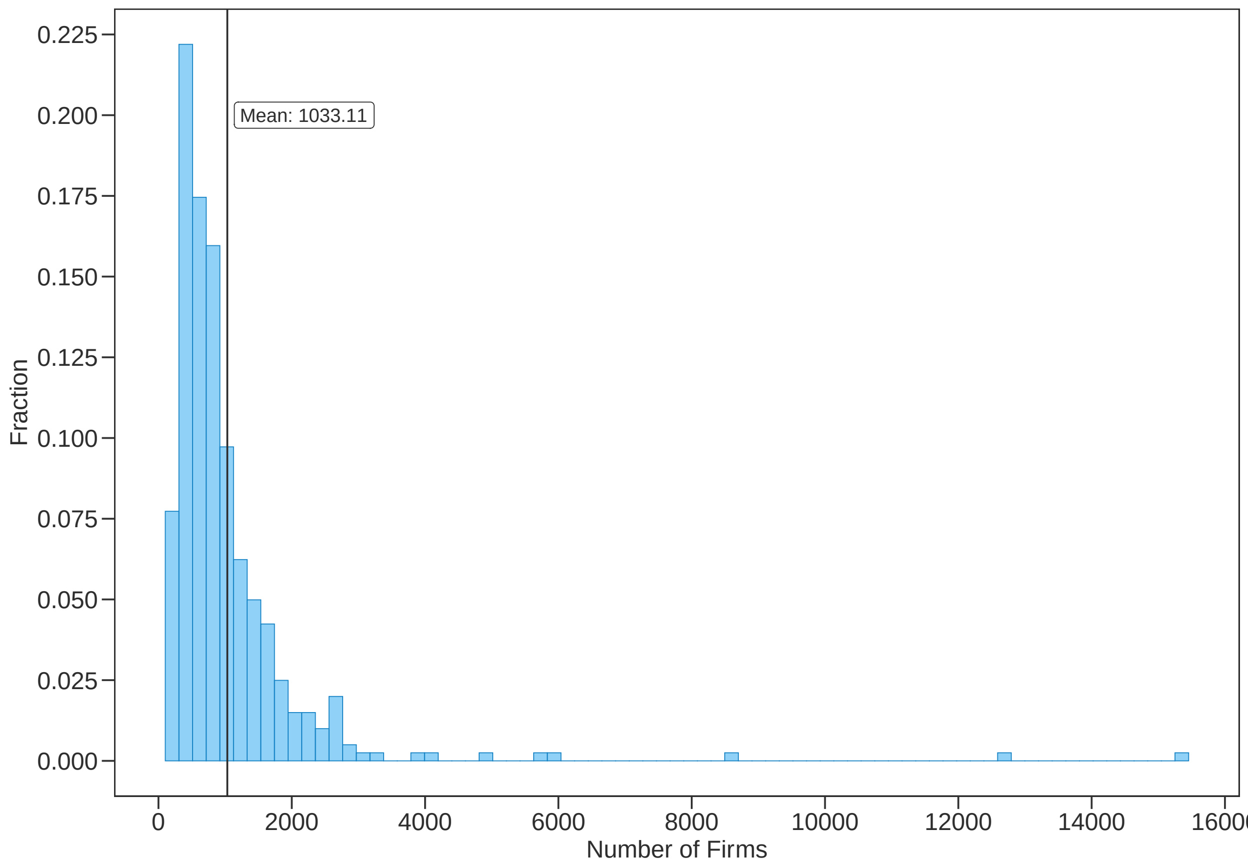
Notes: This figure displays the distribution of the number of total bank relationships for each firm in the sample, and the distribution of the number of those bank relationships that are with Commerzbank. On average, each firm has 1.5 bank relationships, as indicated by the labelled vertical line over the histogram. Frequencies of bank relationships and firms' sample are explained in Figure 4. Source(s): Amadeus, Amadeus Bankers and authors' calculation.

Figure A4: Distribution of the Number of Firm-Bank Relationships per firm



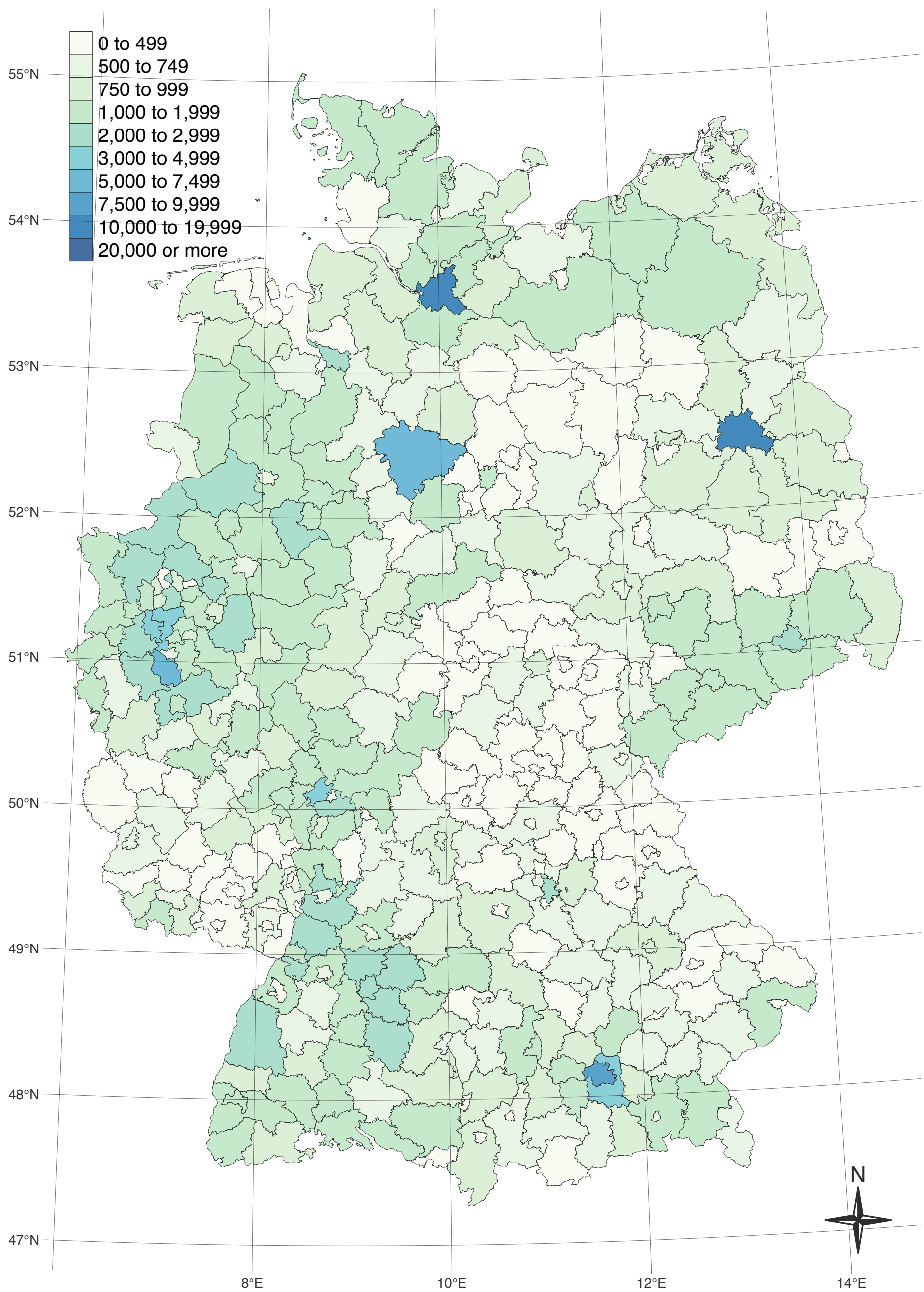
Notes: This figure displays the distribution of the number of total bank relationships for each firm in the sample, and the distribution of the number of those bank relationships that are with Commerzbank. On average, each firm has 1.5 bank relationships, as indicated by the labelled vertical line over the histogram. We include financial and public sector firms in the sample, having a similar distribution when excluding those firms. Frequencies of bank relationships and firms' sample are explained in Figure 4. Source(s): Amadeus, Amadeus Bankers and authors' calculation.

Figure A5: Distribution of the Number Firms in the sample per County



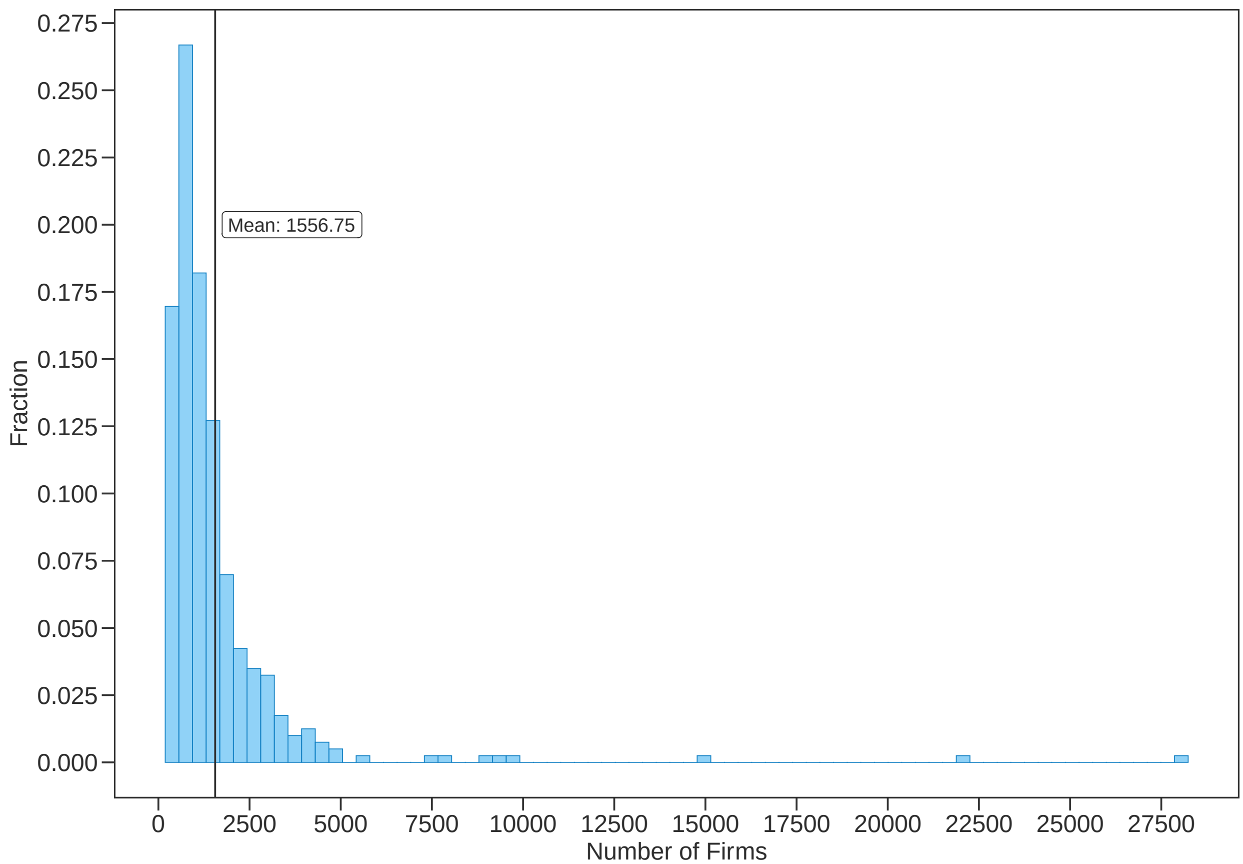
Notes: This figure plots the distribution of the number of firms per county in the sample, using 75 bins. On average, for each county there are 1003 firms, from a total of 414277 firms excluding those in the financial and public sector industry codes. Figure 4 provides a more detailed description of the firms' sample. Source: Amadeus and authors' calculation.

Figure A6: Spatial Distribution of the Firms Sample in Germany



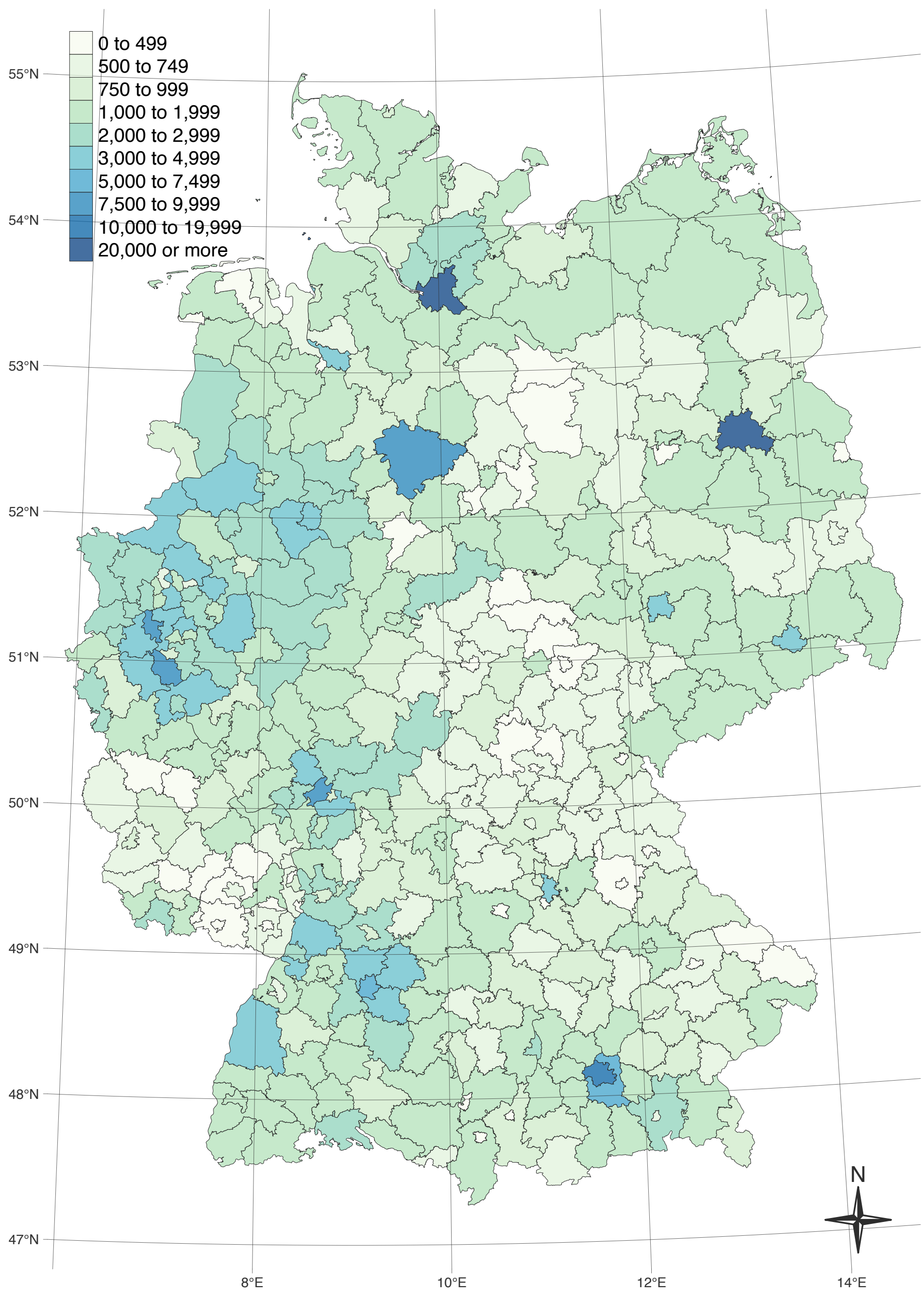
Notes: This figure maps the distribution in Figure A5 over the German counties, binning the frequencies by fixed breaks. Frequencies of bank relationships and firms' sample are explained in Figure 4. Source(s): Amadeus, BKG, and authors' calculation.

Figure A7: Distribution of the Number Firms in the sample per County



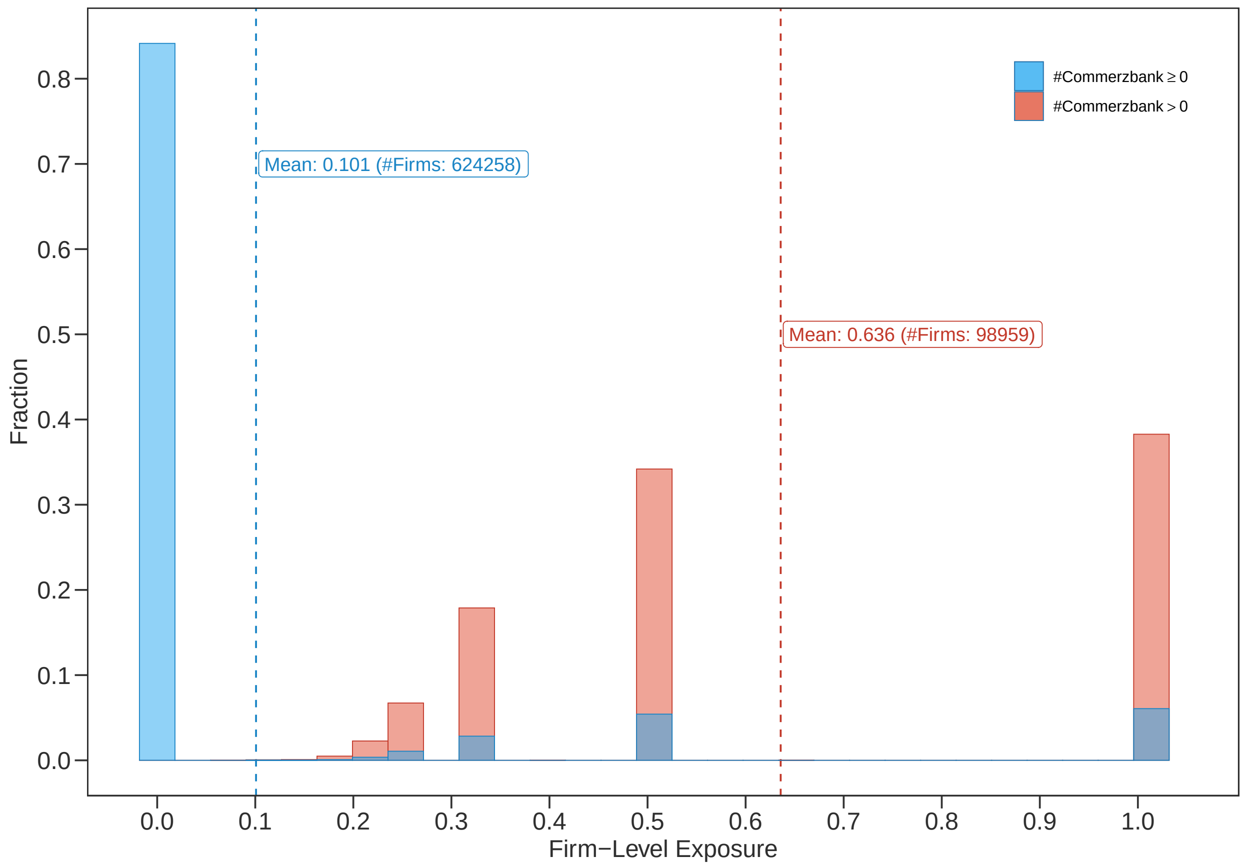
Notes: This figure plots the distribution of the number of firms per county in the sample, using 75 bins. On average, for each county there are 1557 firms, from a total of 624258 firms. Figure 4 provides a more detailed description of the firms' sample. Source: Amadeus and authors' calculation.

Figure A8: Spatial Distribution of the Firms Sample in Germany



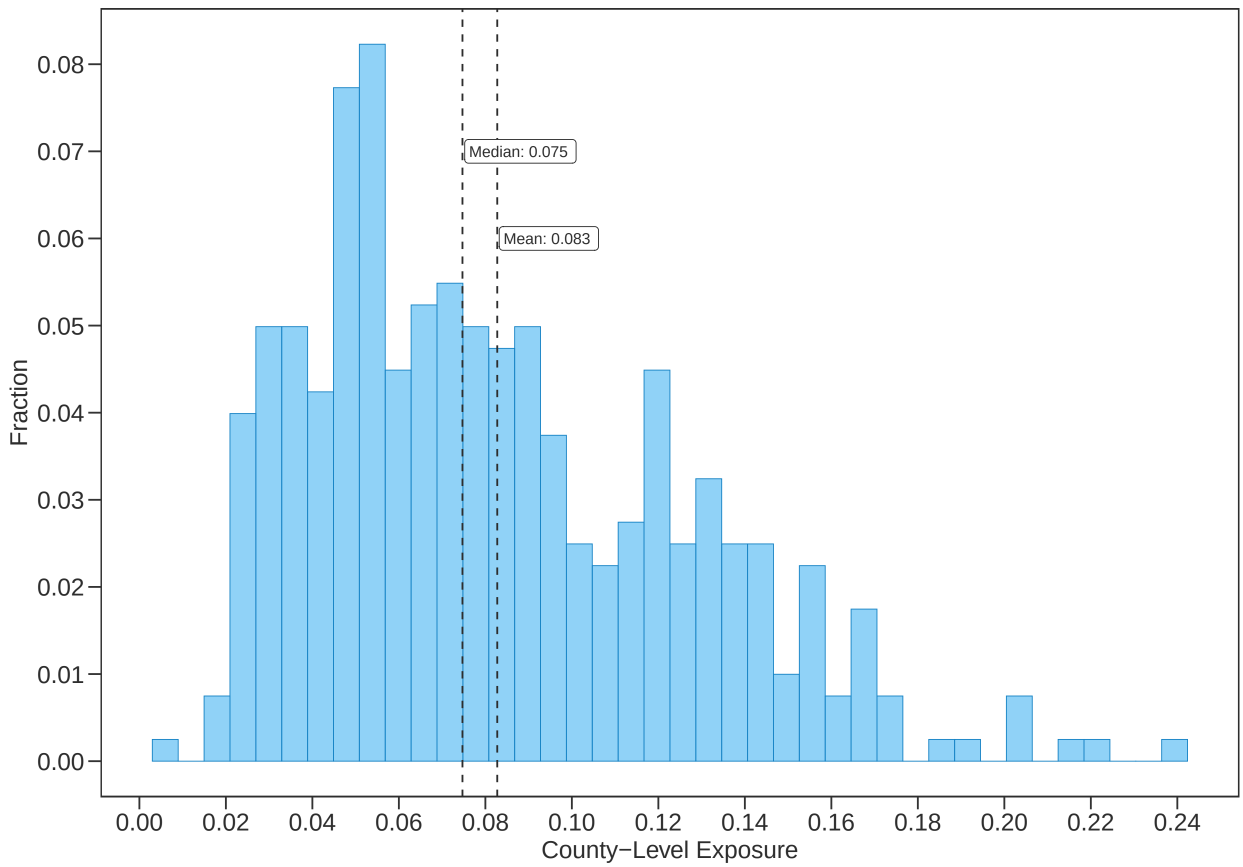
Notes: This figure maps the distribution in Figure A5 over the German counties, binning the frequencies by fixed breaks. Frequencies of bank relationships and firms' sample are explained in Figure A9. Source(s): Amadeus, BKG, and authors' calculation.

Figure A9: Distribution of Firm-Level Commerzbank Dependence



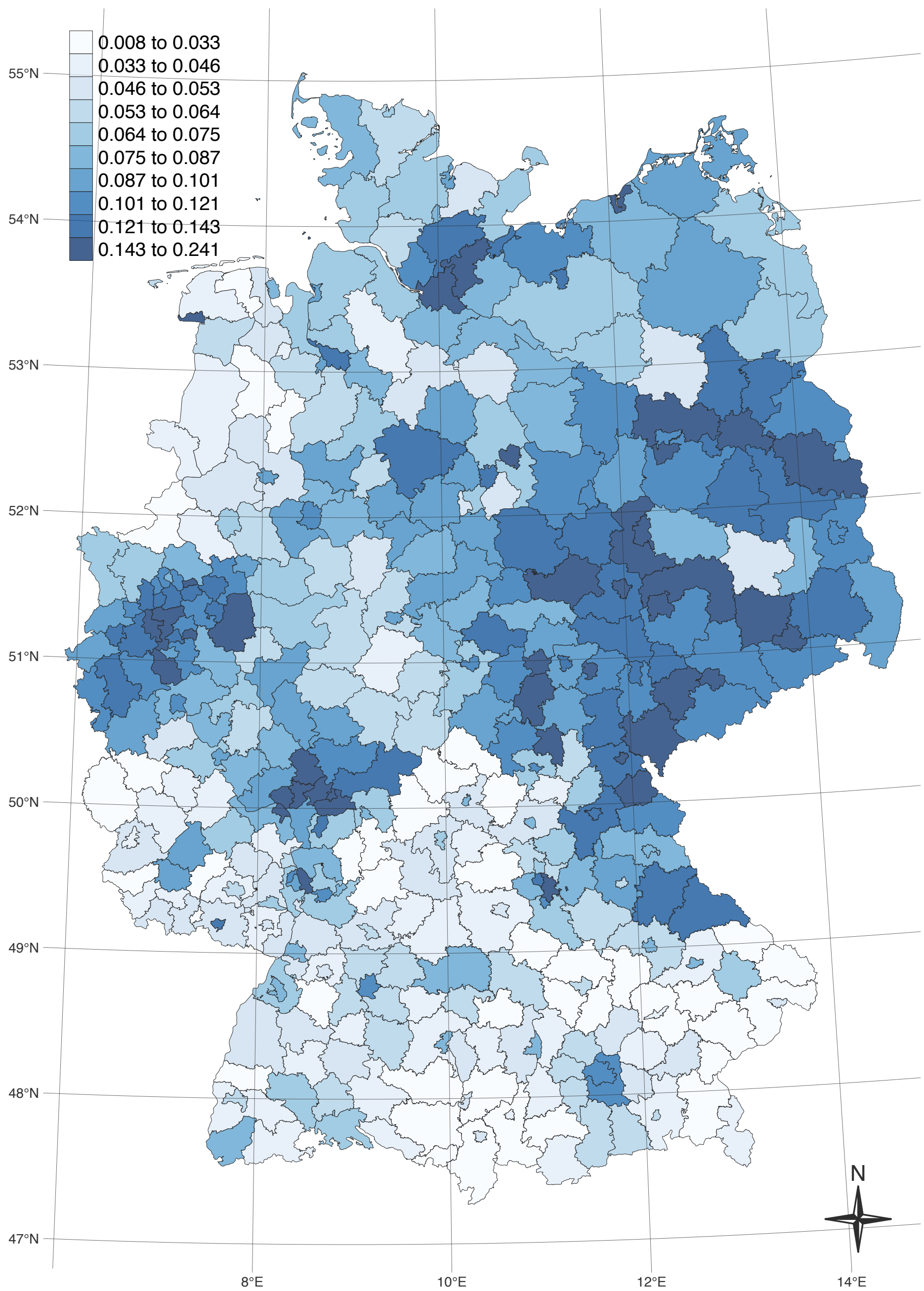
Notes: This picture displays the distribution of the firm-specific Commerzbank dependence at the firms' sample. We provide overlapping histograms of a) the unconditional distribution of the firm-level Commerzbank dependence for all firm, and b) the conditional distribution of the firm-level Commerzbank dependence for those firms having at least one bank relationship with Commerzbank. The dashed lines indicate the average firm exposure of respectively the unconditional and conditional distribution. The labels next to the lines indicate the exact mean value and the number of firms involved in the computation. We include firms in the financial and public sectors. See Figure 4 for the description of the sample and the firm-bank relationships. Source(s): Amadeus, Amadeus Bankers, and authors' calculation.

Figure A10: Distribution of County-Level Commerzbank Dependence



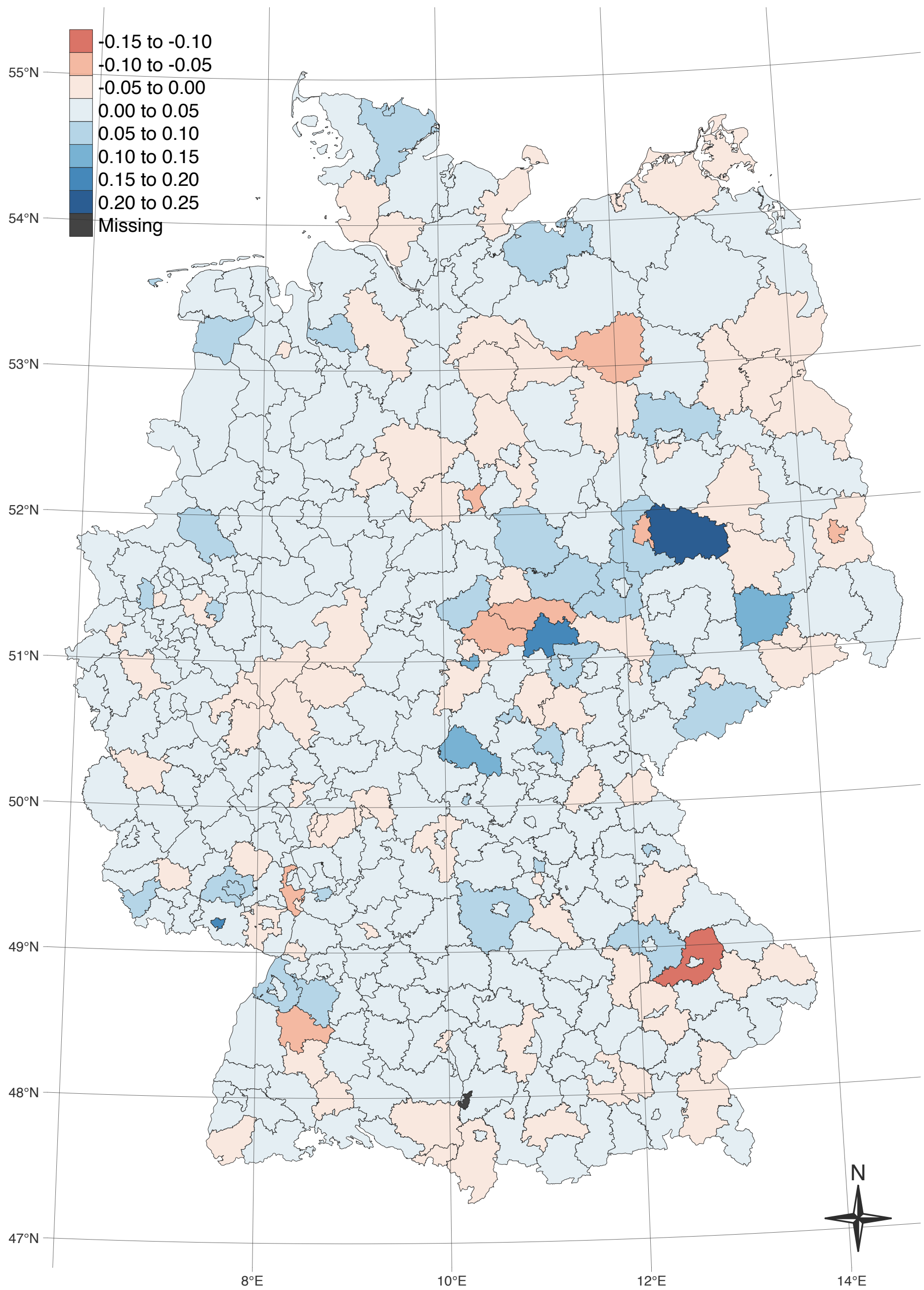
Notes: This figure plots the distribution of the measure of Commerzbank dependence calculated as in (4) at county level in 40 bins. We use equal weights to firms within each county. We highlight the mean and the median of the distribution with the dashed lines labelled by the exact number. We include financial and public sector firms in the calculation. The description of the underlying firms' sample is included in Figure A9. Source(s): Amadeus, Amadeus Bankers and authors' calculation.

Figure A11: Spatial Variation of the proxy for Exposure



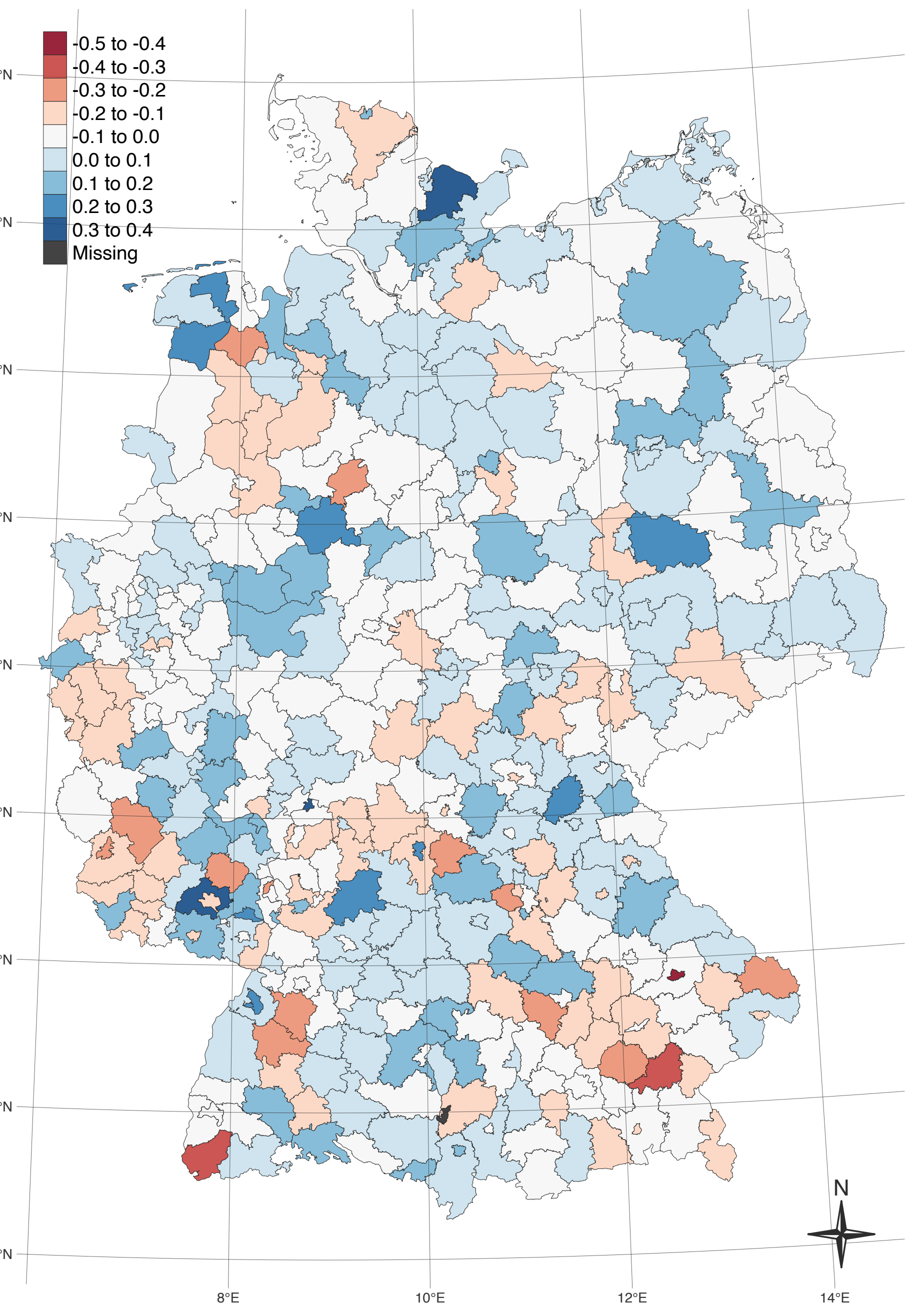
Notes: This picture maps the distribution of the measure of Commerzbank dependence in A10 calculated as in (4) over the German counties. We use equal weights to firms within each county. Different values of exposure are binned by deciles. We include financial and public sector firms in the calculation. The description of the underlying firms' sample is included in Figure A9. Source(s): Amadeus, Amadeus Bankers, BKG, and authors' calculation.

Figure A12: Difference in Average Populist Preferences after the Credit Shock



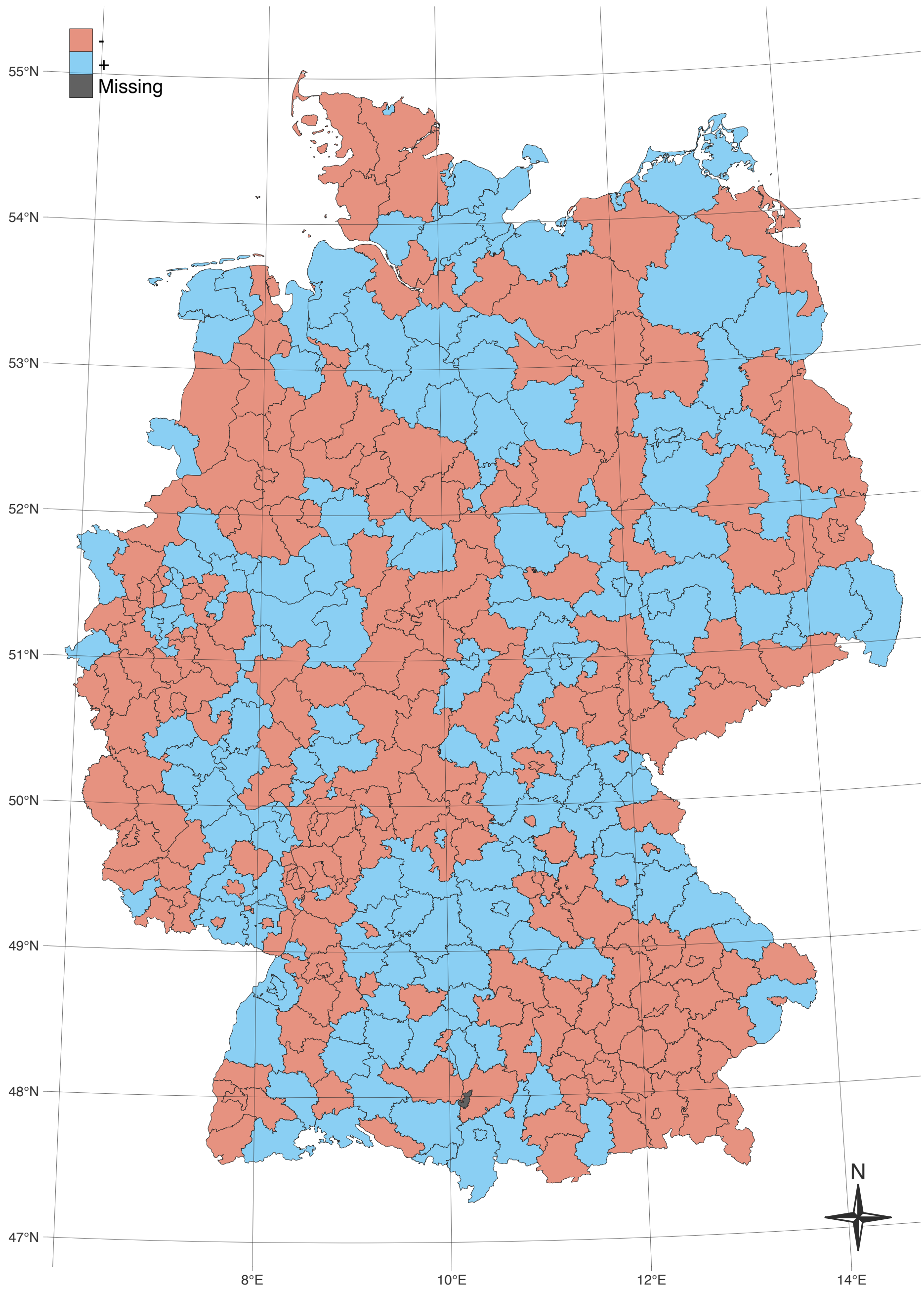
Notes: This figure depicts the difference in the share of populist preferences at county level from the relative question in the German Socio-Economic Panel (SOEP) described in Section 4.3 as percentage points before and after 2009, extending Figure 5 with a more granular representation of the variation. We calculate the sample-weighted mean at county level of the binary question on political support pooling all the respondents for all years before the credit shock and all years after, and we take the difference of the two shares of populist preferences. The sample includes individuals at least at the age of 16, considers self-stated politically inactive individuals as non-populist, and excludes non-respondents or invalid answers. Non-respondents or invalid answers on political party preferences are only around 2% in the entire sample among those individuals that affirmatively answer to the first question, whereas individuals that refuse to answer the question are around 0.5% in the sample. Source(s): German Socio-Economic Panel (SOEP, Goebel et al., 2019, v36), BKG and authors' calculation.

Figure A13: Difference in Average Political Support after the Credit Shock



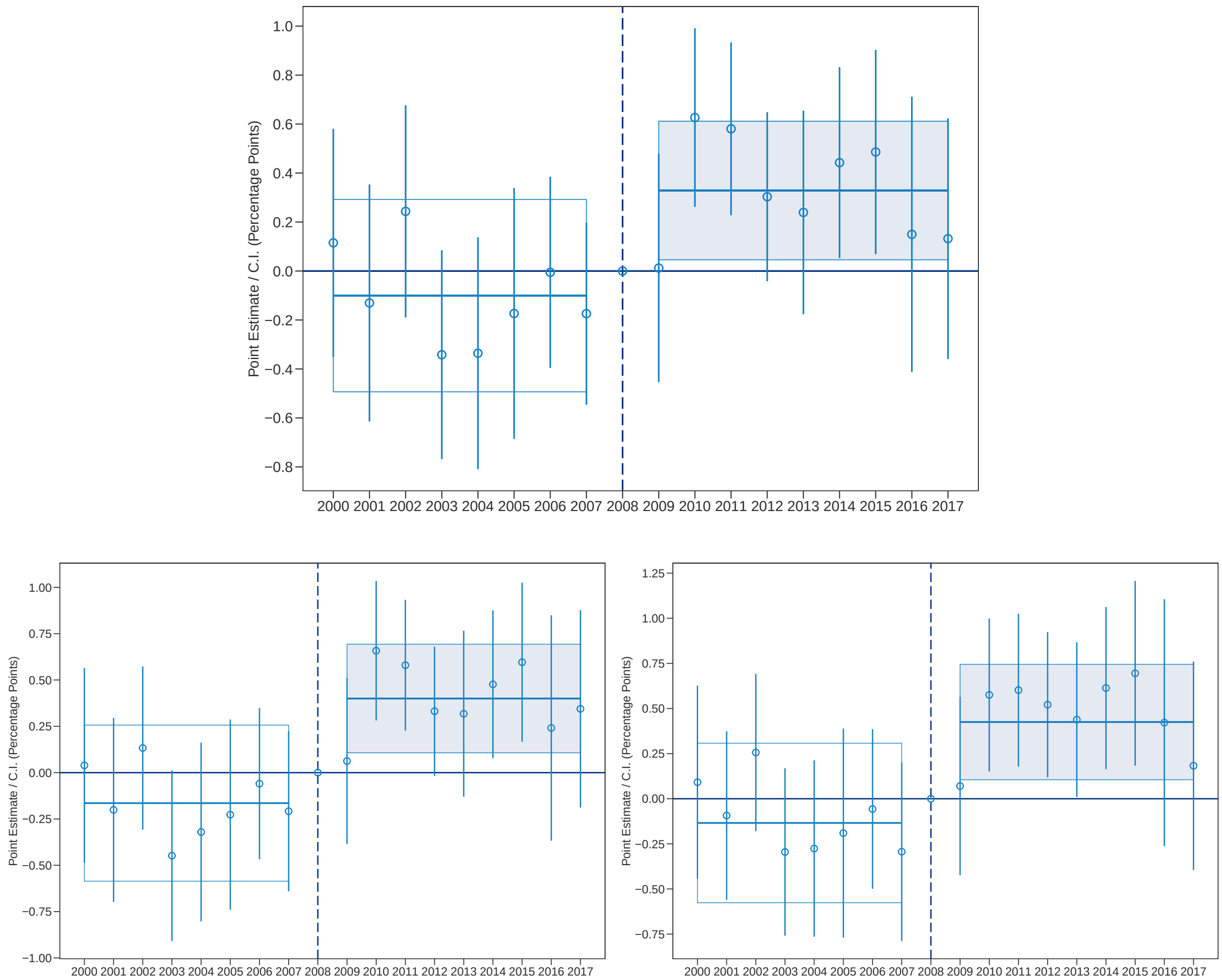
Notes: This figure describes the difference in the share of political support at county level from the relative question in the German Socio-Economic Panel (SOEP) depicted in **??** as percentage points before and after (including) 2009. We calculate the sample-weighted mean at county level of the binary question on political support pooling all the respondents for all years before the credit shock and all years after, and we take the difference of the two shares of political support. The sample includes individuals at least at the age of 16, and excludes non-respondents (<0.5% of the total sample) or invalid answers. Source(s): German Socio-Economic Panel (SOEP, Goebel et al., 2019, v36), BKG and authors' calculation.

Figure A14: Difference in Average Political Support after the Credit Shock



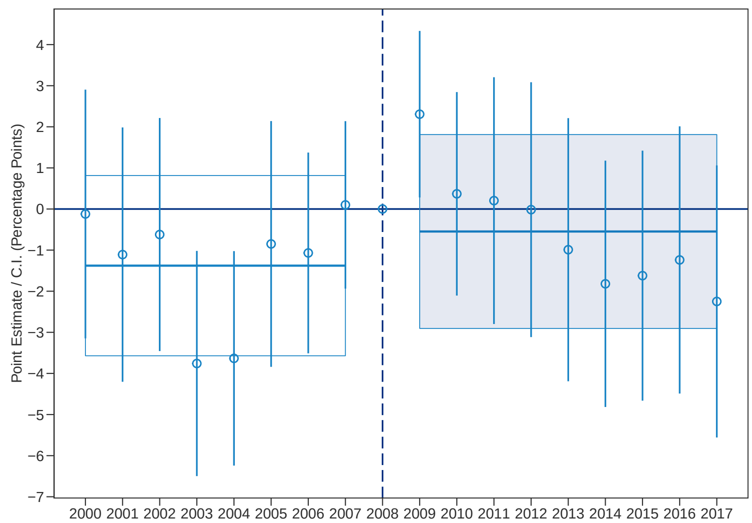
Notes: This figure depicts the German counties with a higher difference in the share of political support after the credit shock as calculated in Figure A13. We construct an indicator variable equal to one if the difference in the share is above the median of the distribution. We distinguish between counties that present a difference in the share above the median of the distribution and counties below or at the median of the distribution. Other details are provided in Figure A13. Source(s): German Socio-Economic Panel (SOEP, Goebel et al., 2019, v36), BKG and authors' calculation.

Figure A15: The Effect of the Credit Shock on Populist Preferences: Difference-in-Differences Estimates (with Continuous Treatment)

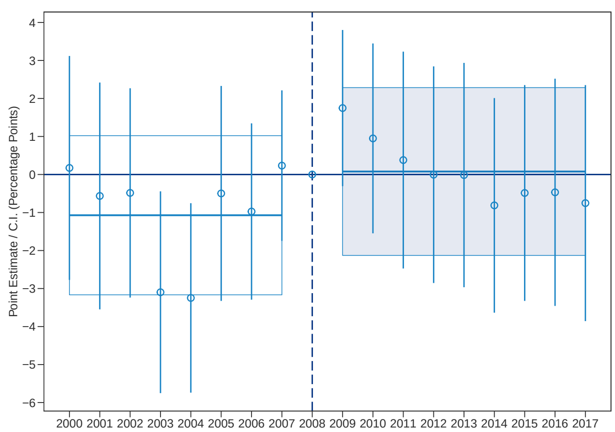


Notes: The plots are symmetric to Figure 9 and 10, estimating Equation (2) and Equation (3) on the same samples respectively. In these estimates, instead of qualifying a cut-off s, we interact the (standardized) continuous treatment respectively with the year fixed effects of Equation (2) and with a 2000–2007 dummy and a 2009–2017 dummy for the estimates of Equation (3). Therefore, the interpretation of the point estimates will be given by a one standard deviation increase in exposure on populist preferences for each interacted time window. In the more aggregated difference-in-differences design with three time periods, estimates of the interactions between the 2000–2007 dummy and the 2009–2017 dummy respectively for each panel are the following: (**??**) $\beta = -0.101$ ($p = 0.615$); $\beta = 0.329$ ($p = 0.023$); (**??**) $\beta = -0.165$ ($p = 0.463$); $\beta = 0.400$ ($p = 0.008$); (**??**) $\beta = -0.134$ ($p = 0.551$); $\beta = 0.425$ ($p = 0.009$); All regressions include sampling weights as well as county and year fixed effects. Coefficient estimates on the year interactions are plotted as dots with their 95% confidence intervals indicated with vertical lines. Coefficient estimates on the aggregate interactions are shown with horizontal lines, and their 95% confidence intervals are indicated as boxes, unshaded or shaded for the pre- and post-period, respectively. All the point estimates and 95% confidence bands are re-scaled by 100 to be interpreted as percentage points difference from the baseline, and standard errors are clustered at the county level.

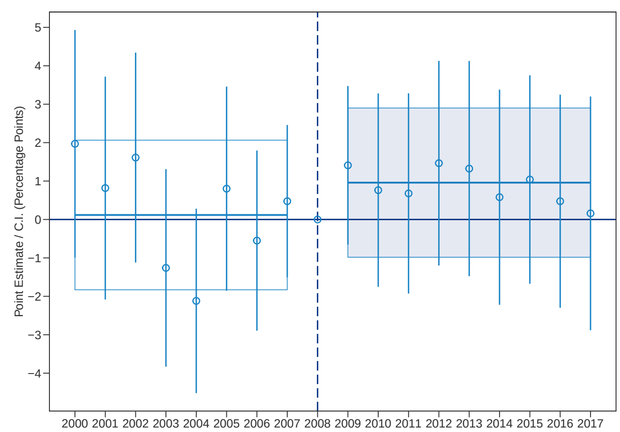
Figure A16: The Effect of the Credit Shock on Political Support: Difference-in-Differences Estimates



(a) without controls



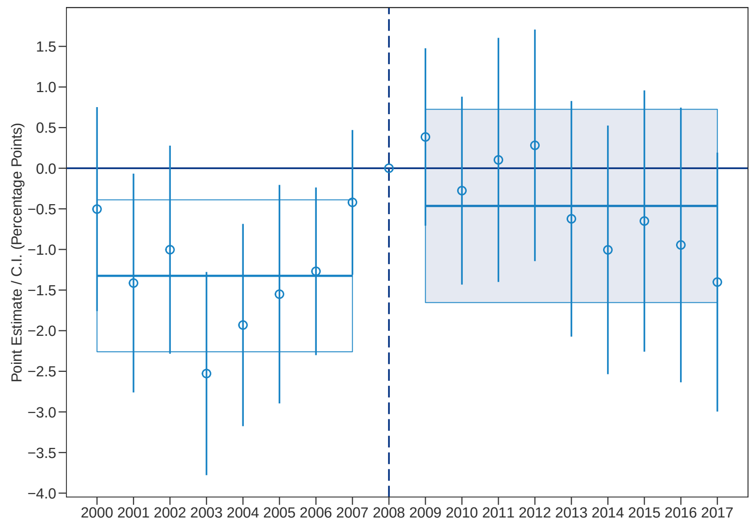
(b) with controls without individual fixed effects



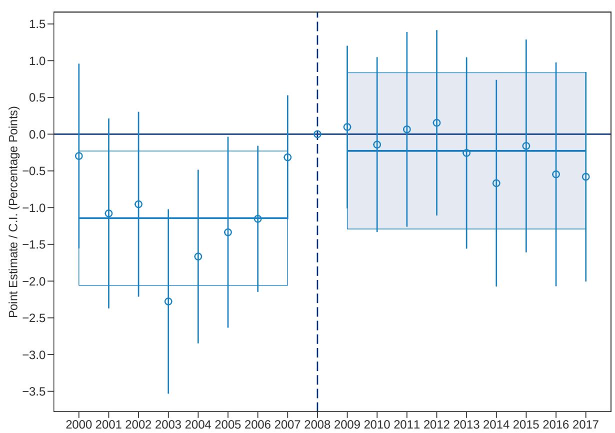
(c) with controls with individual fixed effects

Notes: In this graph we formally test for differences in the likelihood that individuals would be more actively supporting political parties in general after the occurrence of the credit shock, comparing individuals resident in a treated county at time t with and individuals resident in an untreated county at time t. A county k is considered as treated after the occurrence of the credit shock when its exposure to the credit shock – calculated as in Equation (4) with firms' equal weights within the county and excluding finance and public sector firms in the calculation – lies above a cut-off s of the treatment distribution. Here, we qualify the cut-off as $s = med\,(Exposure_k)$, the median of the treatment distribution. Year regression coefficients of interest from the flexible difference-in-differences design in Equation (2) are interactions between an indicator variable equal to one for treated counties and year fixed effects and are estimated relative to the omitted interaction with the first lag before the occurrence of the credit shock. In the more aggregated differences-in-differences design with three time periods in Equation (3), coefficients of interest are interactions between an indicator variable equal to one for treated counties and respectively between a 2000–2007 dummy and a 2009–2017 dummy, estimated relative to the omitted interaction with the first lag before the occurrence of the credit shock. In Panel A16a, regressions are estimated at individual level on the full sample of 385, 248 individual-year observations within 401 counties with no controls. In Panel A16b, regressions are estimated at the individual level the sample of 362, 122 individuals-year observations within 401 counties with individual-, household- and county-specific controls. In Panel A16c regressions are estimated at the individual level the sample of 351, 304 individuals-year observations within 401 counties adding individual fixed effects to the controls and omitting time-invariant individual-level covariates. For the three panels, the coefficient of interest on the interactions between the indicator variable for the treated counties and the pooled dummies are respectively: A16a: $\beta = -1.378$ ($p = 0.217$) and $\beta = -0.547$ ($p = 0.648$); A16b: $\beta = -1.072$ ($p = 0.315$) and $\beta = 0.078$ ($p = 0.944$); A16c: $\beta = 0.117$ ($p = 0.906$) and $\beta = 0.959$ ($p = 0.332$). All regressions include sampling weights as well as county and year fixed effects. Coefficient estimates on the year interactions are plotted as dots with their 95% confidence intervals indicated with vertical lines. Coefficient estimates on the aggregate interactions are shown with horizontal lines, and their 95% confidence intervals are indicated as boxes, unshaded or shaded for the pre- and post-period, respectively. All the point estimates and 95% confidence bands are re-scaled by 100 to be interpreted as percentage points difference from the baseline, and standard errors are clustered at the county level.

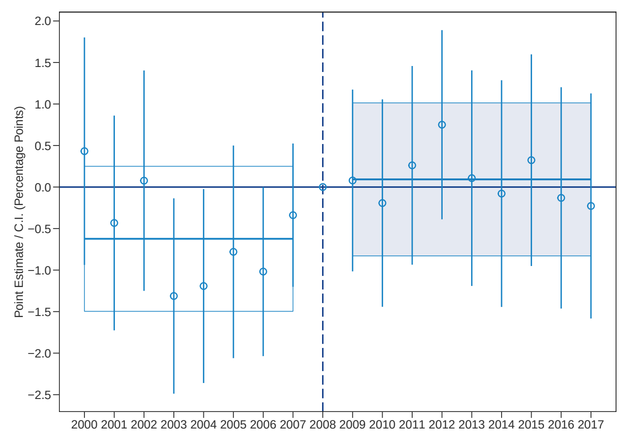
Figure A17: The Effect of the Credit Shock on Political Support: Difference-in-Differences Estimates (with Continuous Treatment)



(a) without controls



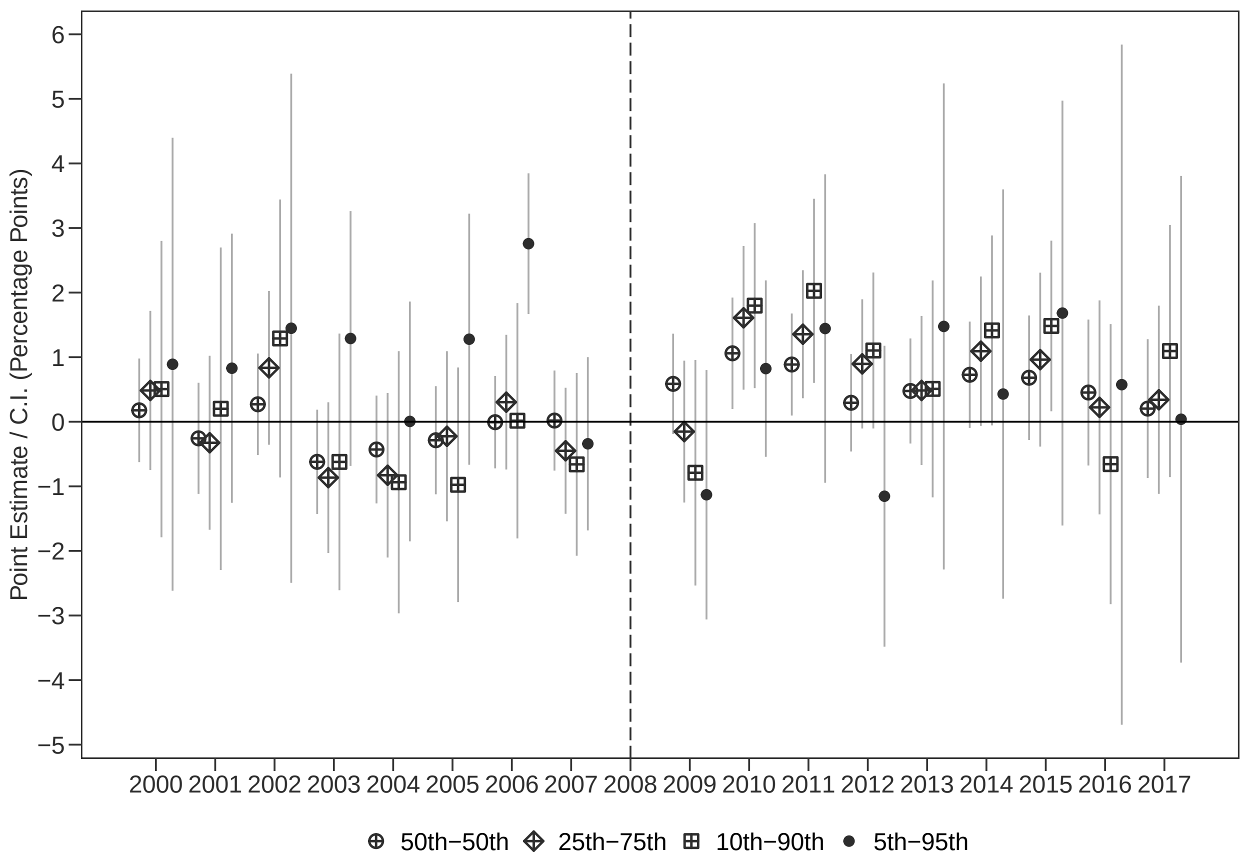
(b) with controls without individual FE



(c) with controls with individual FE

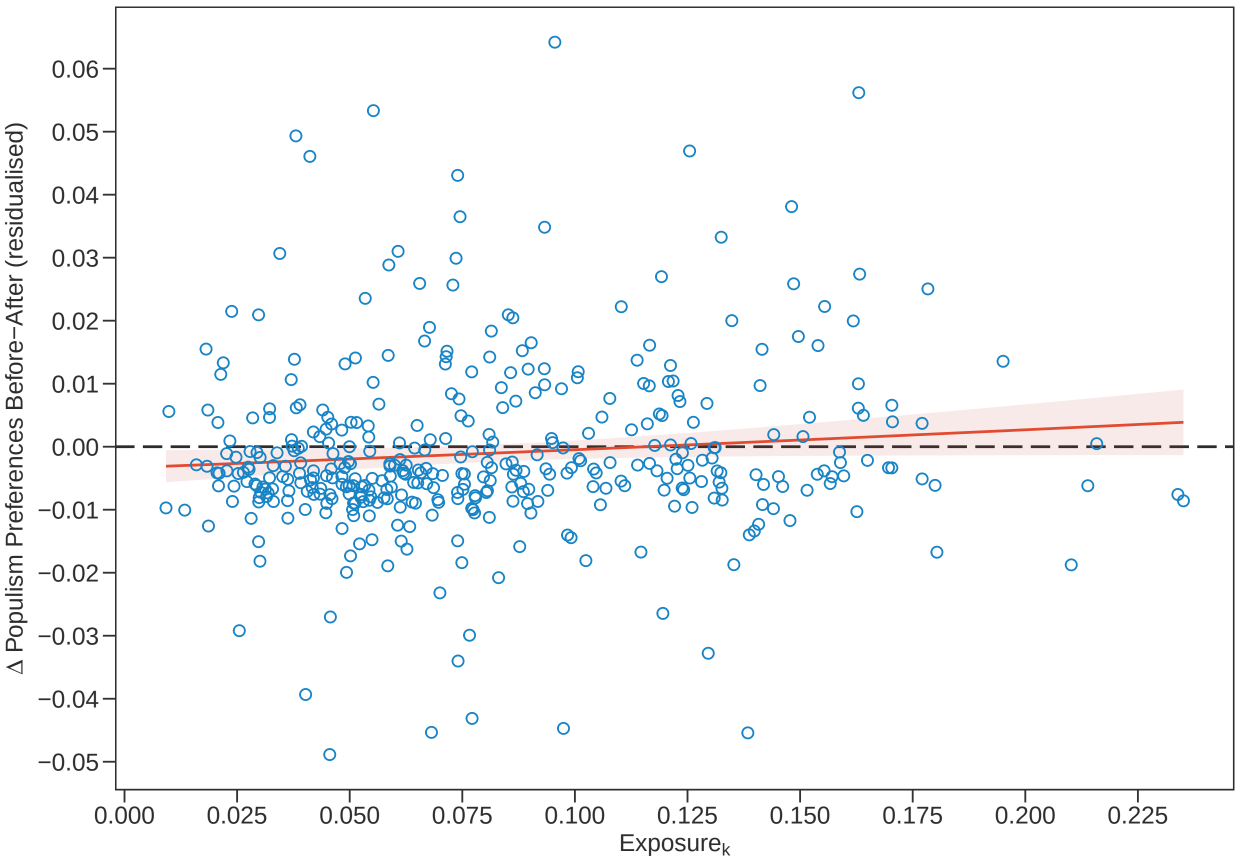
Notes: The plots are symmetric to Figure A16, estimating Equation (2) and Equation (3) on the same samples respectively. In these estimates, instead of qualifying a cut-off s, we interact the (standardized) continuous treatment respectively with the year fixed effects of Equation (2) and with a 2000–2007 dummy and a 2009–2017 dummy for the estimates of Equation (3). Therefore, the interpretation of the point estimates will be given by a one standard deviation increase in exposure on populist preferences for each interacted time window. In the more aggregated difference-in-differences design with three time periods, estimates of the interactions between the 2000–2007 dummy and the 2009–2017 dummy respectively for each panel are the following: (A17a) $\beta = -1.324$ ($p = 0.006$); $\beta = 0.464$ ($p = 0.443$); (A17b) $\beta = -1.143$ ($p = 0.014$); $\beta = 0.228$ ($p = 0.674$); (A17c) $\beta = -0.623$ ($p = 0.161$); $\beta = 0.093$ ($p = 0.843$); All regressions include sampling weights as well as county and year fixed effects. Coefficient estimates on the year interactions are plotted as dots with their 95% confidence intervals indicated with vertical lines. Coefficient estimates on the aggregate interactions are shown with horizontal lines, and their 95% confidence intervals are indicated as boxes, unshaded or shaded for the pre- and post-period, respectively. All the point estimates and 95% confidence bands are re-scaled by 100 to be interpreted as percentage points difference from the baseline, and standard errors are clustered at the county level.

Figure A18: The Effect of the Credit Shock on Populist Preferences: Difference-in-Differences Estimates on different Treatment Indicators



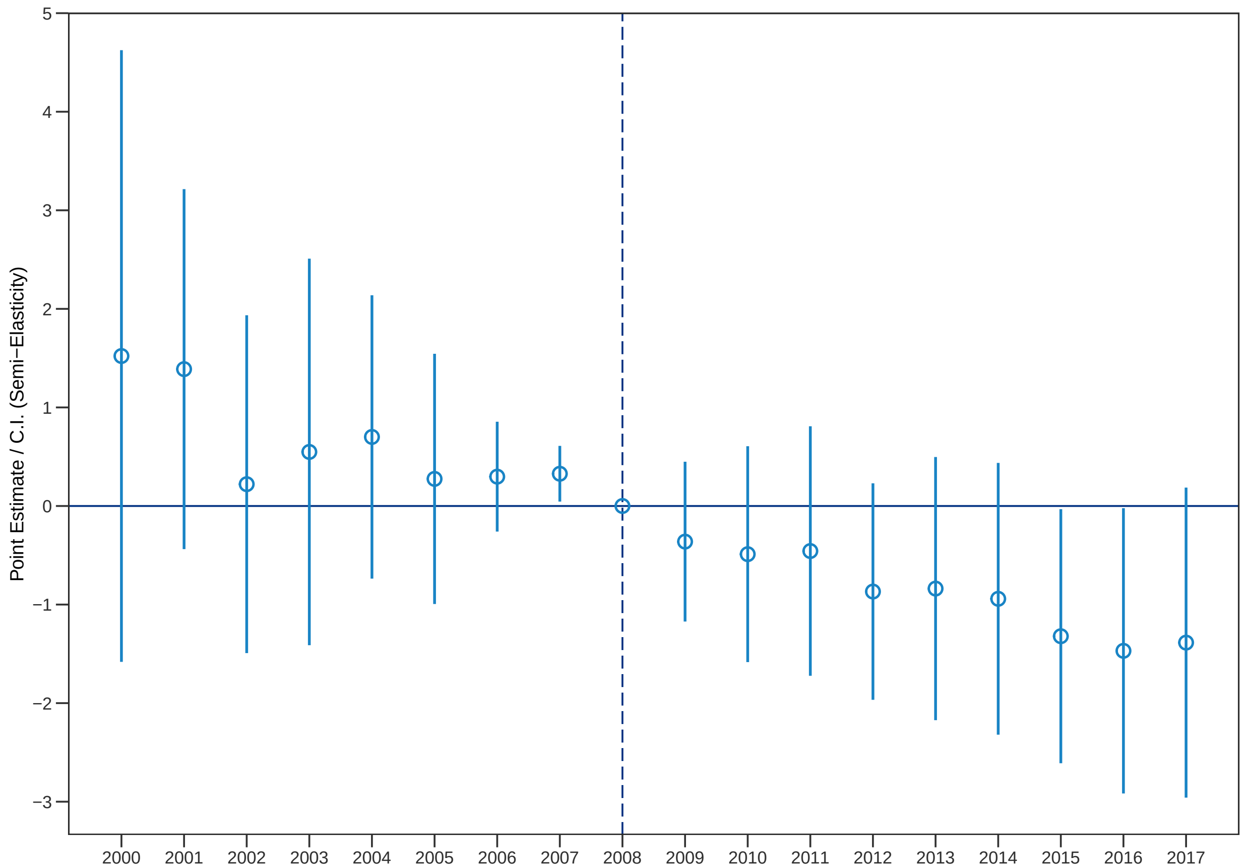
Notes: In these graphs, test for the linearity of the treatment effect of the credit shock on populist preferences. We compare treated individuals with untreated individuals at time *t*, where the treatment is assigned by the intensity of the exposure to the credit shock of their county of residence, calculated as in Equation (4). Year regression coefficients of interest from the flexible difference-in-differences design in Equation (2) are interactions between an indicator variable equal to one for treated counties and zero for the counties considered as untreated, and year fixed effects and are estimated relative to the omitted interaction with the first lag before the occurrence of the credit shock. We estimate Equation (2) in separate regressions where treatment indicators are differently defined based on the position of a county's exposure in the treatment distribution. In particular, the indicator variable assumes value one when the individual lives in a county with treatment above a certain threshold of the treatment distribution, and zero when the individual lives in a county below a certain threshold of the same distribution, defined as it follows: a) above the median and below the median; b) above the 75^{th} percentile and below the 25^{th} percentile; c) above the 90^{th} percentile and below the 10^{th} percentile; d) above the 95^{th} percentile and below the 5^{th} percentile. Regressions are run without additional controls. Coefficient estimates on the year interactions are plotted as different dots for each treatment-control selection with their 95% confidence intervals indicated with vertical lines. All the point estimates and 95% confidence bands are re-scaled by 100 to be interpreted as percentage points difference from the baseline, and standard errors are clustered at the county level.

Figure A19: Functional Form of the Populist Preferences on the Exposure to the Credit Shock: Accounting for Treatment Heterogeneity (including individual fixed effects)



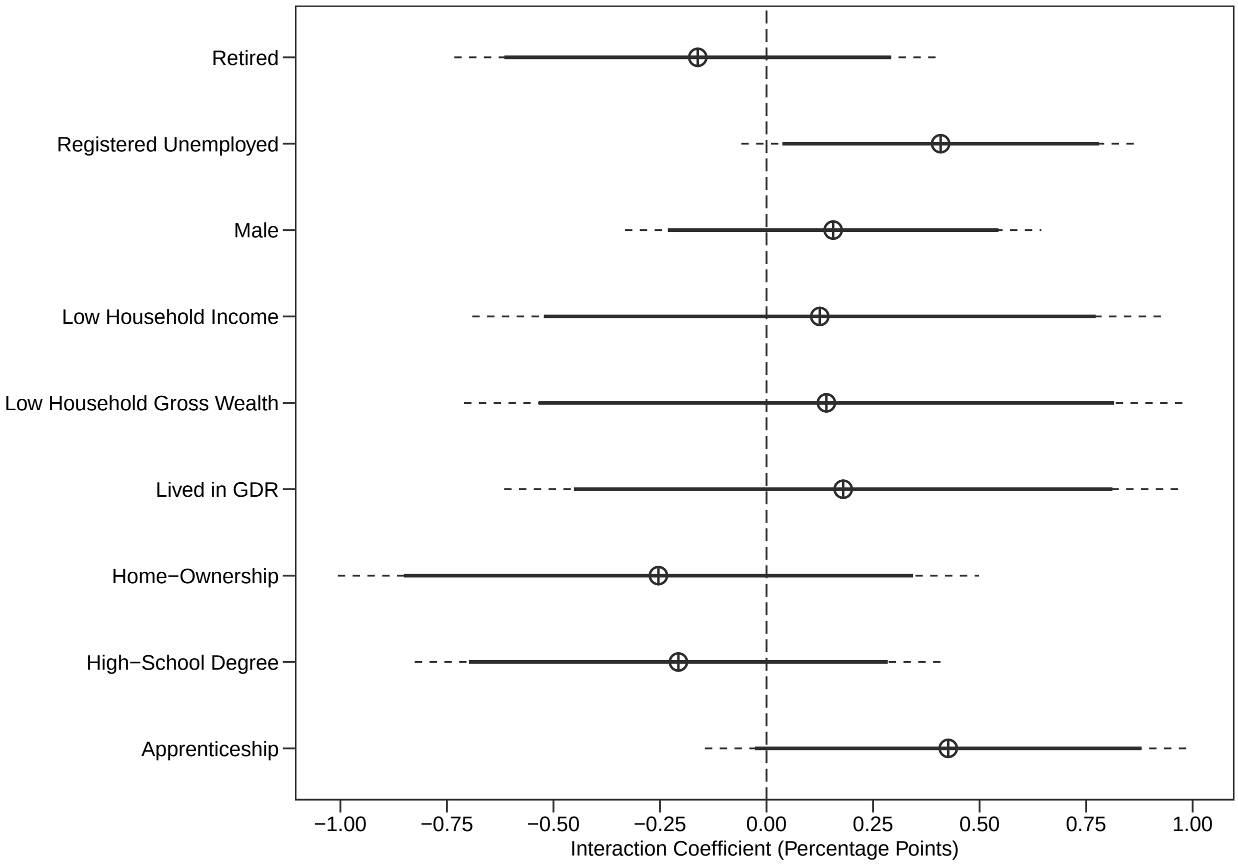
Notes: This picture repeats the exercise in Figure 11 while including individual-level fixed effects when residualising. We use a sample of 314,765 individuals-year observations within 400 counties with individual-, household- and county-specific controls, introducing individual fixed effects, and omitting time-invariant individual-level covariates. Further details on the specification are provided in Figure 11.

Figure A20: The Effect of the Credit Shock on Local Economic Performance: Difference-in-Differences Estimates



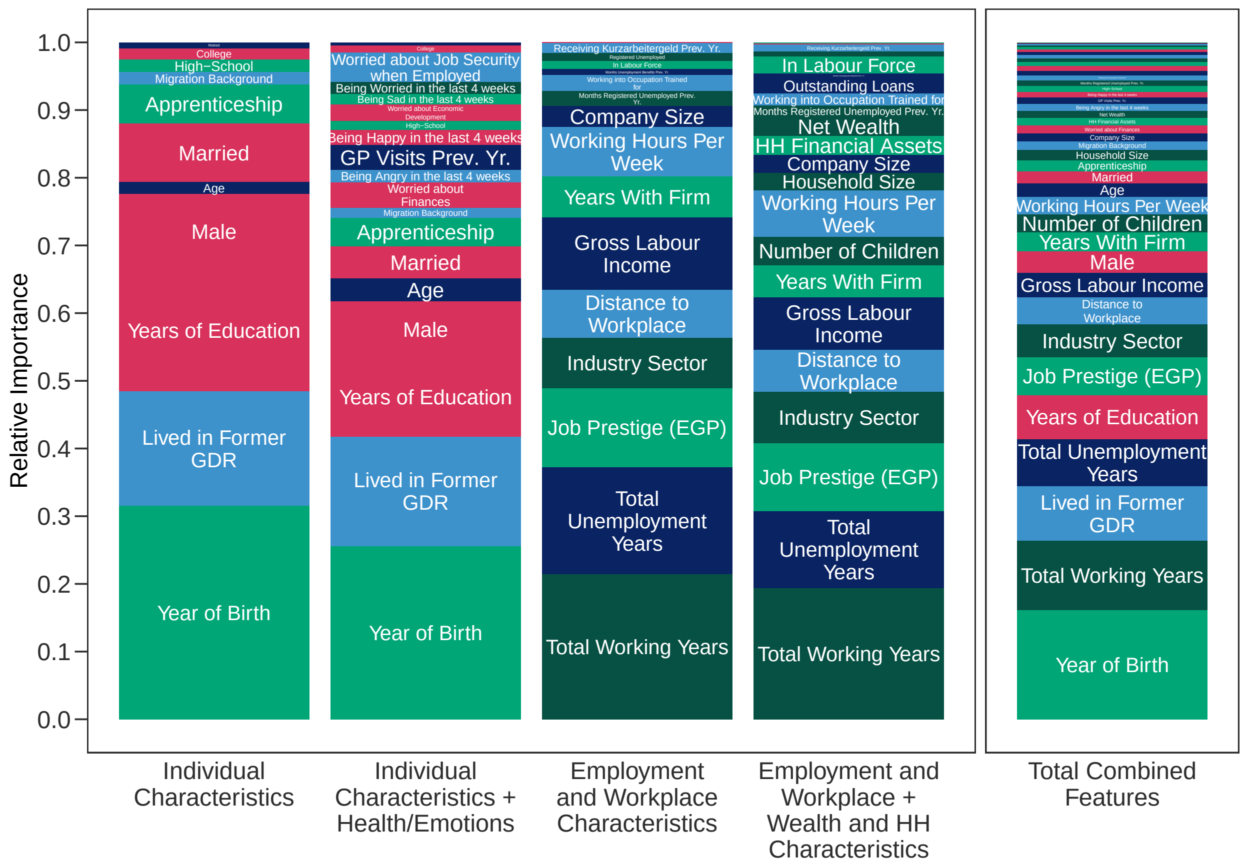
Notes: In this graph, we describe the evolution of the semi-elasticity of the local employment in $1,000$ units between treated and untreated counties after the occurrence of the credit shock. Estimates are obtained with the same settings of Figure 12. The weighted average of all time average treatment effects on the treated is -0.9% (*std.err.* 0.0039). Estimating the coefficient of interest β on the continuous treatment, we obtain a 0.75% decrease in GDP for each standard deviation increase in the treatment dose ($std.err. = 0.00368$, $t = -2.046$).

Figure A21: The Effect of the Credit Shock on Local Economic Performance: Difference-in-Differences Estimates



Notes: This figure explores the heterogeneity at individual level behind the effect of the credit shock on populist preferences. It presents estimates from separate regressions as in table A5 of the interaction of each of the indicated characteristics with the treatment variable and the indicator variable for the periods after the shock on a balanced panel of individuals from 2006 to 2012 with county, time and individual fixed effects. The estimate expresses the cumulative effect in percentage points of belonging to the pointed subpopulation. The outcome variable is our binary indicator for individual populist preferences. Specifically, we consider the following subpopulations of individuals: a) retired, b) officially registered as unemployed, c) male, d) low household income, e) low household wealth, f) lived in East Germany before Reunification, g) home-ownership, h) high-school degree, and j) having received an apprenticeship. All the subpopulations are specified as a dummy in case the individual belongs to that specific category and are fixed pre-shock at 2007. Low household income and wealth means income or wealth below the 25^{th} percentile. Solid bars indicate the 90% confidence bands, whereas thinner dashed lines indicate the 95% confidence intervals. All estimates are rescaled by 100 to be expressed as percentage points. Longitudinal sampling weights are applied on each regression and standard errors are clustered at county level.

Figure A22: Feature Importance in Explaining Populist Preferences' Response



Notes: add notes...

Figure A23: Feature Importance in Explaining Populist Preferences' Response: Shapley Summary Plot

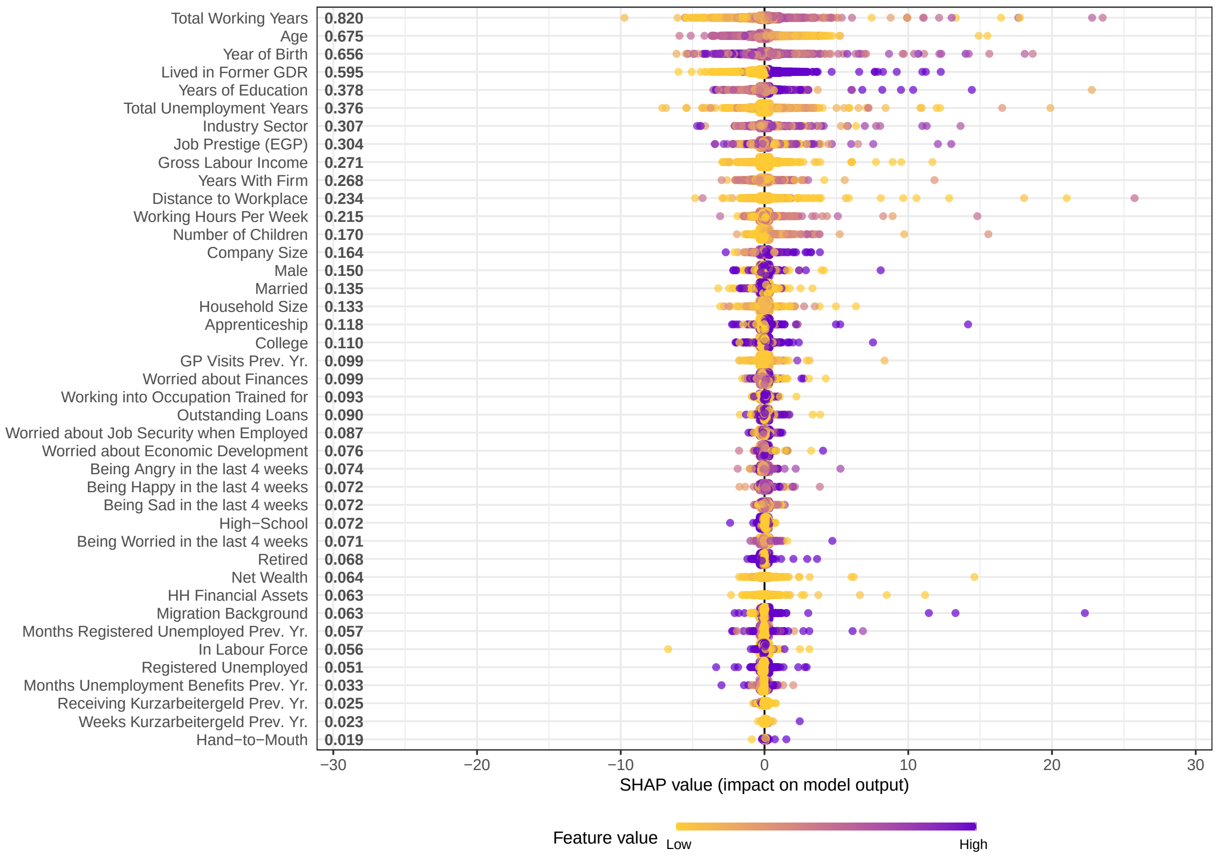


Figure A24: Top Twenty Terms by Posterior Probability using Seeded LDA for the electoral manifestos, Populism using the Rooduijn and Pauwels (2011) lexicon.

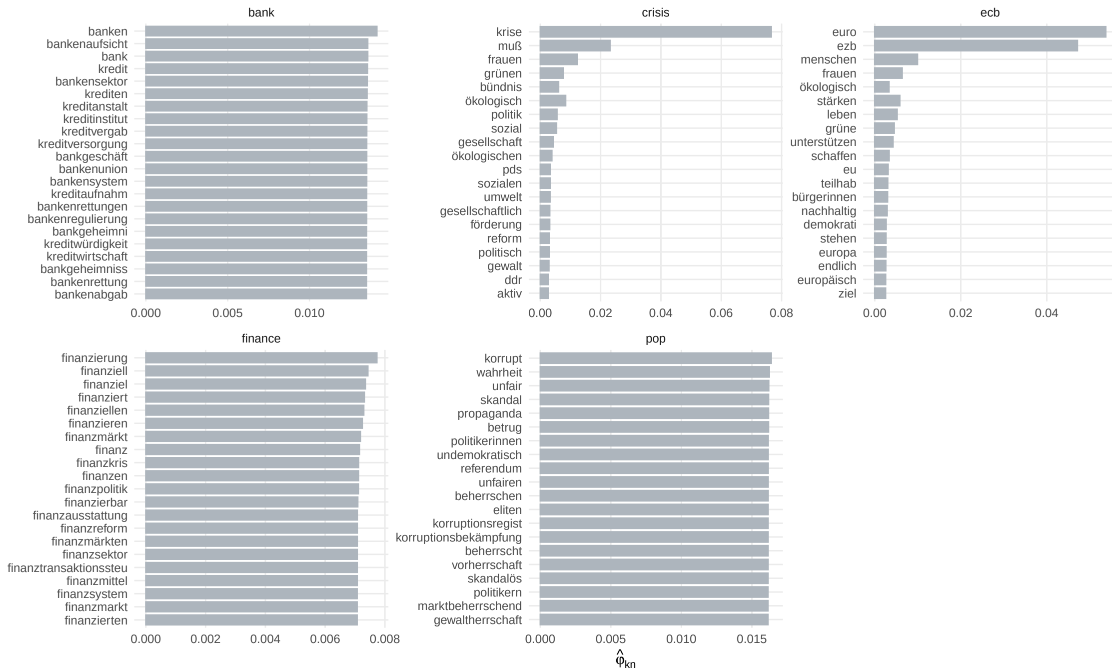


Figure A25: Focus on Banking & Finance in parliamentary speeches using dictionary approach, by political party (1991-2018)

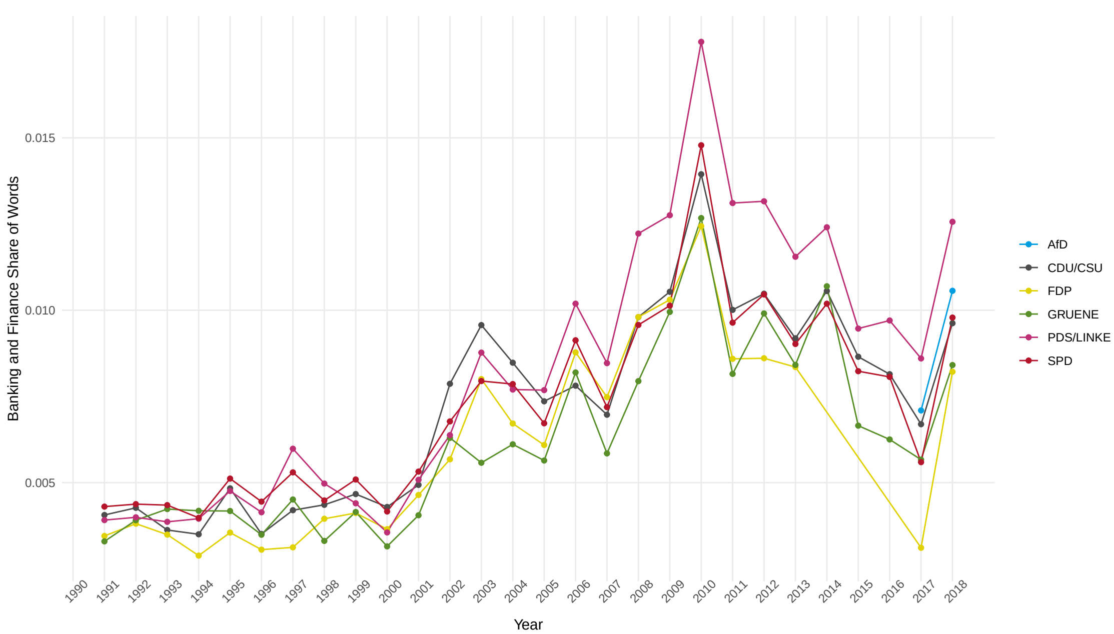


Figure A26: Populist Rhetoric using Rooduijn and Pauwels (2011) in parliamentary speeches using dictionary approach, by political party (1991-2018)

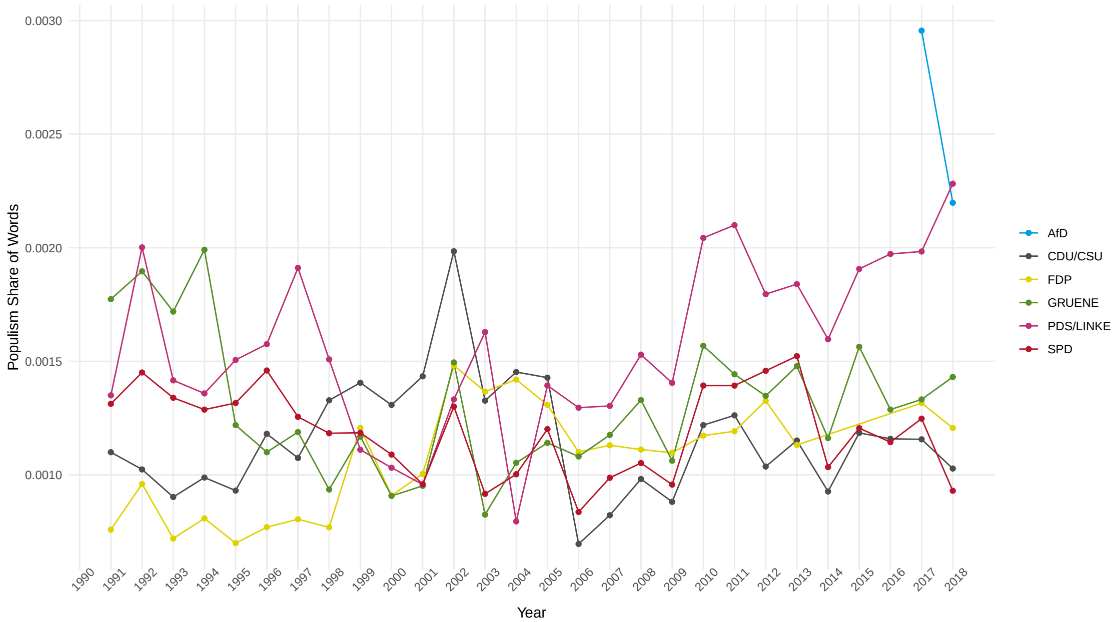


Figure A27: Focus on Banking & Finance in electoral manifestos, by political party (1991-2018)

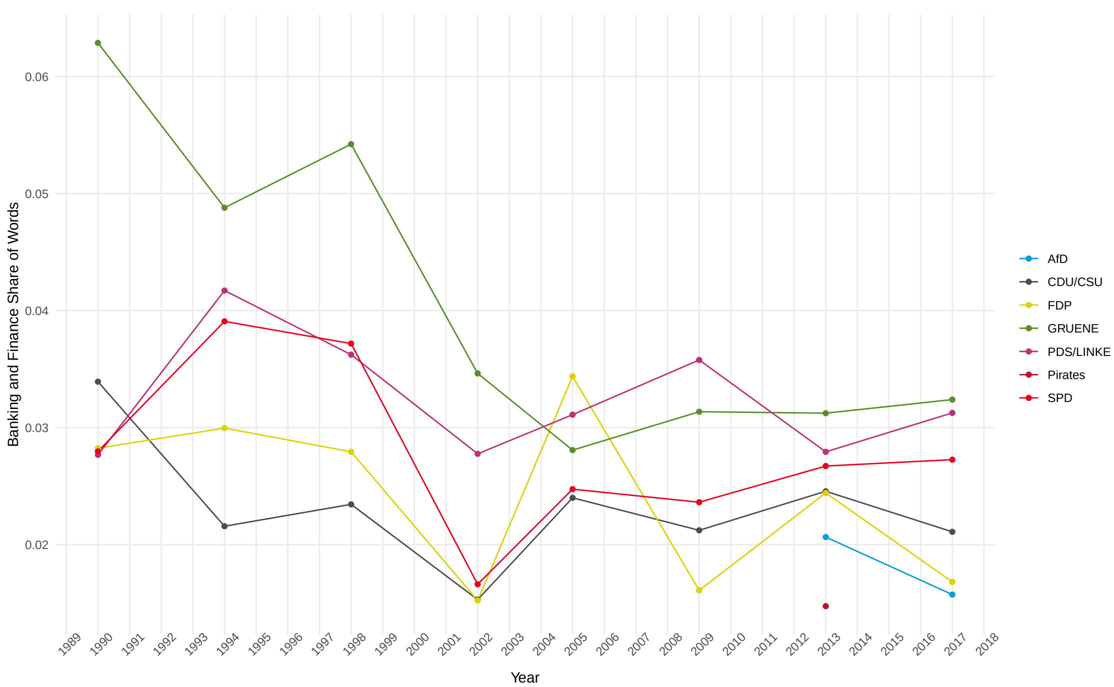
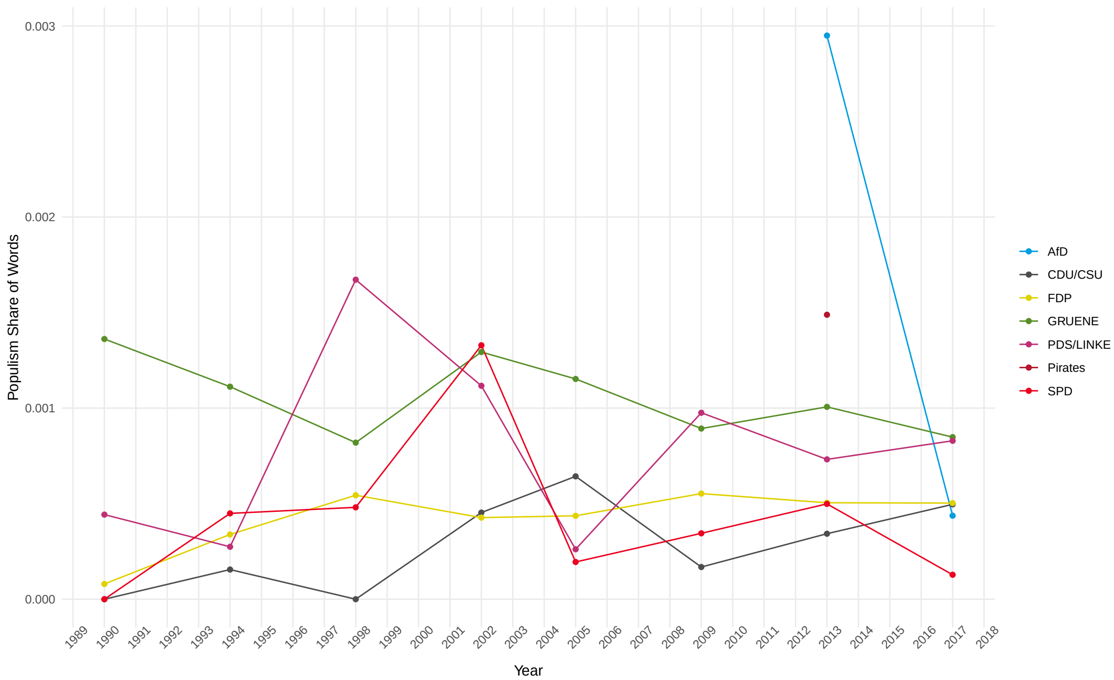


Figure A28: Populist Rhetoric using Rooduijn and Pauwels (2011) in electoral manifestos, by political party (1991-2018)



E Additional Tables

Table A1: The Effect of the Credit Shock on Populist Preferences: Difference-in-Differences Results

	(1)	(2)	(3)	(4)	(5)
$Exposure_k \times Post$	0.511^{***}	0.522^{***}	0.533^{***}	0.536^{***}	0.600^{***}
	(0.186)	(0.191)	(0.193)	(0.191)	(0.157)
Number of Observations	366,403	351,470	351,304	351,304	351,304
Number of Counties	401	401	401	401	401
Outcome Mean (%)	3.34	3.362	3.363	3.363	3.363
$\sigma\,(Exposure_k)$ (%)	4.86	4.862	4.862	4.862	4.862
Adjusted R^2	0.521	0.526	0.526	0.526	0.528
County-Level FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes
Individual Controls	No	Yes	Yes	Yes	Yes
Household Controls	No	No	Yes	Yes	Yes
Regional Controls	No	No	No	Yes	No
County Time Trends	No	No	No	No	Yes

Notes: This table is symmetric to Table 2, reporting the results of the estimation of Equation (1) where the dependent variable is the populist preferences indicator of an individual i resident in county k at time t, constructed as depicted in Section 6.1, and the variable of interest is the (standardized) exposure to the credit shock of county k, as calculated in Equation (4) using equal weights for all firms within each county, interacted with an indicator variable equal to one for all years after the occurrence of the credit shock. The difference with Table 2 stems from the introduction of individual fixed effects throughout all specifications. For all specifications that include individual-level controls, we remove time-invariant individual covariates. For additional details on the columns, we refer to Table 2. The coefficients of interest are scaled by 100 to be interpreted as the percentage points increase of the outcome mean of one-σ units increase of treatment after the occurrence of the shock. Standard errors are clustered by county for all specifications, and *, **, and *** denote statistical significance at 10%, 5%, and 1% levels, respectively.

Table A2: The Effect of the Credit Shock on Political Support: Difference-in-Differences Results

	(1)	(2)	(3)	(4)	(5)
$Exposure_k \times Post$	0.712	0.854**	0.819**	0.785*	0.881*
	(0.485)	(0.413)	(0.411)	(0.413)	(0.466)
Number of Observations	385,248	362,295	362,122	362,122	362,122
Number of Counties	401	401	401	401	401
Outcome Mean (%)	45.349	45.942	45.941	45.941	45.941
$\sigma\,(Exposure_k)$ (%)	4.863	4.861	4.861	4.861	4.861
Adjusted R^2	0.046	0.130	0.131	0.131	0.137
County-Level FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Individual FE	No	No	No	No	No
Individual Controls	No	Yes	Yes	Yes	Yes
Household Controls	No	No	Yes	Yes	Yes
Regional Controls	No	No	No	Yes	No
County Time Trends	No	No	No	No	Yes

Notes: This table is symmetric to Table 2, reporting the results of the estimation of Equation (1), but here the dependent variable is the political support indicator of an individual i resident in county k at time t, stemming from the affirmative answer to the question described in Section 4.3, and the variable of interest is the (standardized) exposure to the credit shock of county k, as calculated in Equation (4) using equal weights for all firms within each county, interacted with an indicator variable equal to one for all years after the occurrence of the credit shock. For additional details on the columns, we refer to Table 2. The coefficients of interest are scaled by 100 to be interpreted as the percentage points increase of the outcome mean of one-σ units increase of treatment after the occurrence of the shock. Standard errors are clustered by county for all specifications, and *, **, and *** denote statistical significance at 10%, 5%, and 1% levels, respectively.

Table A3: The Effect of the Credit Shock on Political Support: Difference-in-Differences Results

	(1)	(2)	(3)	(4)	(5)
$Exposure_k \times Post$	0.584*	0.623*	0.624*	0.609*	0.905**
	(0.340)	(0.338)	(0.337)	(0.338)	(0.451)
Number of Observations	366,403	351,470	351,304	351,304	351,304
Number of Counties	401	401	401	401	401
Outcome Mean (%)	45.552	46.116	46.116	46.116	46.116
$\sigma\,(Exposure_k)$ (%)	4.86	4.862	4.862	4.862	4.862
Adjusted R^2	0.548	0.550	0.551	0.551	0.553
County-Level FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes
Individual Controls	No	Yes	Yes	Yes	Yes
Household Controls	No	No	Yes	Yes	Yes
Regional Controls	No	No	No	Yes	No
County Time Trends	No	No	No	No	Yes

Notes: This table is symmetric to Table A2, reporting the results of the estimation of Equation (1) where the dependent variable is the political support indicator of an individual i resident in county k at time t, stemming from the affirmative answer to the question described in Section 4.3, and the variable of interest is the (standardized) exposure to the credit shock of county k, as calculated in Equation (4) using equal weights for all firms within each county, interacted with an indicator variable equal to one for all years after the occurrence of the credit shock. The difference with Table A2 stems from the introduction of individual fixed effects throughout all specifications. For all specifications that include individual-level controls, we remove time-invariant individual covariates. For additional details on the columns, we refer to Table 2. The coefficients of interest are scaled by 100 to be interpreted as the percentage points increase of the outcome mean of one-σ units increase of treatment after the occurrence of the shock. Standard errors are clustered by county for all specifications, and *, **, and *** denote statistical significance at 10%, 5%, and 1% levels, respectively.

Table A4: The Effect of the Credit Shock on Political Support: Difference-in-Differences Estimates with Binary Treatment

	Median		**75th**		**90th**		**25th – 75th**		**10th – 90th**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$\mathbf{1}\left(Exposure_k > s\right) \times Post$	1.029	0.861	1.499	0.954	3.127**	2.535***	2.271*	1.459	4.350**	3.493**
	(0.888)	(0.762)	(1.113)	(0.817)	(1.313)	(0.947)	(1.317)	(1.092)	(1.667)	(1.566)
Number of Observations	362,122	351,304	362,122	351,304	362,122	351,304	179,100	173,196	71,851	69,441
Number of Counties	401	401	401	401	401	401	208	208	82	82
Outcome Mean (%)	45.941	46.116	45.941	46.116	45.941	46.116	45.634	45.869	46.535	46.762
s (%)	8.949	8.949	13.093	13.093	16.495	16.495	5.834	5.834	3.886	3.886
Adjusted R^2	0.131	0.551	0.131	0.551	0.131	0.551	0.134	0.547	0.140	0.544
County-Level FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Time Trends	No	No	No	No	No	No	No	No	No	No

Notes: This table is similar to Table 3, with the only difference that here the dependent variable is the indicator variable for political support, which is equal to one in case of affirmative answer to the question described in Section 4.3, and zero otherwise. The coefficients of interest are scaled by 100 to be interpreted as the percentage points increase of the outcome mean for individuals located in counties with exposure to the credit shock above the indicated threshold against individuals located in counties that have a lower exposure beforehand. Standard errors are clustered by county for all specifications, and *, **, and *** denote statistical significance at 10%, 5%, and 1% levels, respectively.

Table A5: The Effect of the Credit Shock on Populist Preferences: Difference-in-Differences with Balanced Panels

	Full Sample	Balanced Panels		
		2006–2012	2004–2013	2000–2015
	(1)	(2)	(3)	(4)
$Exposure_k \times Post$	0.418**	0.611***	0.705**	0.195
	(0.173)	(0.209)	(0.281)	(0.428)
Number of Observations	385,248	71,049	78,647	82,663
Number of Counties	401	391	392	383
Outcome Mean (%)	3.347	3.938	3.732	3.666
$\sigma\left(Exposure_k\right)$ (%)	4.863	4.862	4.868	4.916
Adjusted R^2	0.049	0.623	0.570	0.506
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Individual FE	No	Yes	Yes	Yes

Notes: This table reports the results of the estimation of our difference-in-differences specification in Equation (1) applied to the balanced panels of individuals indicated in each column. More specifically, we estimate a difference-in-differences design where all the variables are the same of Equation (1), but we add α_i individual fixed effects, and no additional controls. In this way, we are able to compare the same individuals before and after the occurrence of the shock. The dependent variable is our indicator variable of populist preferences of a same individual i resident in county k at time t, and the variable of interest is the (standardized) exposure to the credit shock of county k, as calculated in Equation (4) using equal weights for all firms within each county, interacted with an indicator variable equal to one for all years after the occurrence of the credit shock. Longitudinal weights are applied in every sample, and they are constructed as the sampling weights for the first wave of the considered period in the relative panel multiplay for each inverse staying probability weight of all the subsequent waves that are considered. Column 2 specifies the results for a balanced panel considering waves from 2006 to 2012, with three lags and three leads relative to the timing of the shock. Column 3 shows results for a panel from 2004 to 2013. Finally, Column 4 considers a longer sample following individuals from 2000 until 2015. The coefficients of interest are scaled by 100 to be interpreted as the percentage points increase of the outcome mean of one-σ units increase of treatment after the occurrence of the shock. Standard errors are clustered by county for all panels, and *, **, and *** denote statistical significance at 10%, 5%, and 1% levels, respectively.

Table A6: The Differential Effect of the Credit Shock on Political Preferences: Split Samples on Pre-Shock Growth Rates

	$\overline{\Delta GDP}_k^{2001-2006}$		$\overline{\Delta Employment}_k^{2001-2006}$		ΔGDP_k^{2006}				$\Delta Employment_k^{2006}$			
					25$^{\text{th}}$ Percentile		50$^{\text{th}}$ Percentile		25$^{\text{th}}$ Percentile		50$^{\text{th}}$ Percentile	
	$\geq 50^{th}$	$< 50^{th}$	$\geq 50^{th}$	$< 50^{th}$	$\geq$	$<$	$\geq$	$<$	$\geq$	$<$	$\geq$	$<$
$Exposure_k \times Post$	0.390*	0.675***	0.149	0.756***	0.581**	0.421**	0.533**	0.672**	0.466**	0.848**	0.429	0.736***
	(0.231)	(0.255)	(0.193)	(0.240)	(0.235)	(0.180)	(0.255)	(0.270)	(0.227)	(0.335)	(0.311)	(0.189)
Number of Observations	183,842	178,280	181,563	180,559	271,131	90,991	179,550	182,572	274,933	87,189	187,165	174,957
Number of Counties	202	199	202	199	302	99	204	197	303	98	204	197
Outcome Mean (%)	3.065	3.682	1.948	4.907	3.487	3.042	3.091	3.65	3.299	3.61	3.419	3.324
σ ($Exposure_k$) (%)	4.851	4.804	4.712	4.698	5.048	4.255	4.173	5.256	5.156	3.752	5.557	3.922
Adjusted R^2	0.053	0.066	0.037	0.062	0.060	0.057	0.056	0.063	0.060	0.059	0.058	0.062
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	No	No	No	No	No	No	No	No	No	No	No	No
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: add notes...

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