Austerity, Turnout and Support for Radical-Right Parties: The Case of Local Fiscal Rules*

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Abstract

We empirically study the effects of austerity, in the form of local fiscal rules, on party preferences and political participation. For causal inference, we leverage exogenous variation created by a 2013 Italian reform which imposed deficit targets in municipalities with fewer than 5,000 residents. In a difference-in-discontinuities analysis, we first show that rules contribute to right-wing populist voting. Complementing this finding, we also show that local fiscal rules led to greater turnout. These results are stronger in municipalities where local fiscal rules are binding — that is, where fiscal adjustments are required to comply with the new institutional framework. Our findings add to a broader literature investigating the economic roots of populism.

Keywords: Austerity, Fiscal Rules, Elections, Turnout, Populism
JEL codes: D72, H30, H60, H70

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1 Introduction

In the aftermath of the 2008 global financial crisis, a renewed sense of urgency concerning the sustainability of public finances led to the adoption of large structural and fiscal austerity measures throughout Europe (Alesina et al., 2019). Part of this effort has been the imposition of *numerical fiscal rules*, defined as laws meant to constrain fiscal policy (Grembi et al., 2016) — such as limits on government debt or deficits (Schaechter et al., 2012). The theoretical benefits of fiscal rules are intuitive: rules ameliorate time-inconsistency problems that electorally-motivated policy-makers face (Kydland and Prescott, 1977), reduce any welfare losses resulting from political budget cycles (Nordhaus, 1975), and limit short-term policy myopia (Aidt and Dutta, 2007).[1]

That said, despite their widespread use (Larch et al., 2021), little attention has been paid to understanding the *electoral* consequences of fiscal rules, particularly in terms of how they affect party preferences and political participation. Such an effort is, however, worthwhile since turnout levels and the distribution of political choices in the electorate directly affect who wields power and, consequently, what policy platforms will be implemented both in the short and long-run (Cascio and Washington, 2014; Cantoni and Pons, 2022). Moreover, much alike the policies they aim to constrain, fiscal rules are not carried out in a vacuum. Rather, policy-makers account for possible political consequences when deciding whether to implement restrictions (Talving, 2017; Arias and Stasavage, 2019), and when choosing whether to maintain rather than revert the rules once implemented. Therefore, studying the electoral effects of fiscal rules ultimately allows us to better understand their socio-economic consequences, as well as their credibility as a policy instrument.

We make two contributions to this field. We begin by asking whether fiscal rules lead to an increase in support for *right-wing populist parties* (Mudde, 2004) in the affected electorate. Rising support for radical-right parties in the Western world (Colantone and Stanig, 2019) is considered one of the most significant recent political changes, especially in Europe (Dehdari, 2022), and has therefore attracted much scholarly attention. According to Dal Bó et al. (2022), the last two decades have been marked by a strengthening of radical-right parties across countries and electoral systems, leading to researchers in different academic fields taking on the

[1] In order to assess whether these predictions hold in practice, an active literature has emerged aiming to study the effectiveness of fiscal rules (see Grembi et al., 2016, Daniele and Giommoni, 2021, Caselli and Reynaud, 2020, and Heinemann et al., 2018, for a broader overview), as well as their possible undesired effects (e.g., Pavese and Rubolino, 2021).

"urgent task" of understanding the proliferation of the radical-right.[2] In particular, scholars noted that the success of radical-right parties (Guriev and Papaioannou, 2022) surged precisely following the recession, leading many to investigate the mechanism by which the crisis may have benefited populists. Here, whilst a host of potential explanations behind their success have been investigated,[3] certain factors have so far been explored less in-depth, among which the role played by austerity implemented after the crisis (Guriev and Papaioannou, 2022).[4]

In particular, little attention has been paid to the effects of fiscal rules specifically in a causal setting. We therefore contribute to this scholarship by asking whether fiscal rules causally increase the vote share obtained by radical-right parties, thus bridging the gap between these two strands of the literature. The argument here is that voting for radical-right populists can be seen as a form of protest against traditional parties, deemed responsible for existing economic grievances (Altomonte et al., 2019). And, indeed, despite the fact that no universally-agreed upon definition of right-wing populism exists, one common feature shared by many radical-right parties is their anti-system stance (Colantone and Stanig, 2018), promising "revenge" against a hypothetical out-group usually including "mainstream" politicians (Müller, 2017).

The theoretical link between fiscal rules and radical-right voting is then intuitive to grasp: restrictions on policy-making limit the possible actions politicians can take when faced with unfavorable economic conditions, triggering frustration among voters who start believing that their grievances can no longer be addressed within the newly adopted institutional framework. As a result, the expected utility from voting for radical-right platforms increases, as this may lead to the reversal or softening of restrictions.

Building on this narrative, we investigate empirically whether a set of local fiscal rules implemented in 2013 led to an increase in the vote share obtained by Italy's major right-wing populist parties. We hypothesize that the radical-right parties Northern League [*Lega Nord, LN*] and Brothers of Italy [*Fratelli d'Italia, FdI*] stand to benefit from the imposition of restrictions for several reasons. First, these parties, and especially the LN (Giordano, 2000), campaigned

[2] We see that Kay Arzheimer's bibliography on the Radical Right in Western Europe alone stood at 1196 articles as of June 2023 — `https://www.kai-arzheimer.com/extreme-right-western-europe-bibliography/`.

[3] These range from cultural backlashes against "progressive" and "cosmopolitan" values (Norris and Inglehart, 2019), to immigration (e.g., Vertier et al., 2022), automation (Frey et al., 2018), as well as globalization and trade integration (e.g., Autor et al., 2020).

[4] Three recent studies should be pointed out: Fetzer (2019), who links support for UKIP in the United Kingdom, as well as the Leave option in the 2016 Brexit referendum to welfare-retrenchment policies carried out earlier that decade; Dal Bó et al. (2022), who provides evidence for the far-right Swedish Democrats benefiting electorally from cuts in spending on sick leaves, disability insurance and unemployment benefits; and Gabriel et al. (2022), who use regional level variation to show that fiscal consolidation led to increases in extreme parties' vote shares.

on a regionalist agenda, emphasizing the importance of sub-national areas having political power and influence. Second, both parties adopted hard eurosceptic and anti-immigration stances and values (Huysseune, 2010; Zappettini and Maccaferri, 2021), making them appealing political formations for those dissatisfied with EU or EU-backed policies, such as the 2013 set of fiscal rules that were specifically designed to limit local spending, and thus increase the (perceived) scarcity of local public resources.

In line with existing work (Grembi et al., 2016; Marattin et al., 2022; Aaskoven, 2021), we exploit a policy change in the application of fiscal rules, which were extended in 2013 to municipalities below 5,000 residents. Briefly, municipalities were required to achieve a specific deficit target, resulting in sanctions if the target was not met. This policy change allows us to circumvent endogeneity concerns that generally plague observational studies, exploiting the discontinuity given by population size. Leveraging quasi-experimental variation created by this policy in a difference-in-discontinuities framework (Eggers et al., 2018),[5] we document a positive effect of local fiscal rules on radical-right voting, when comparing municipality-level shares in the (post-policy) 2014 European elections with the shares observed in the (pre-policy) 2013 general elections. We find that local fiscal rules led to a statistically-significant and highly-robust increase in the vote share secured by Italy's radical-right parties of just under one percentage point in our preferred specification — a politically non-negligible impact relative to the average share of roughly 10 percent. This result constitutes our paper's first and main contribution.[6]

As a second contribution, we assess how fiscal rules imposed on *local* governments affect turnout. According to existing work, policy restrictions can reduce turnout by diminishing the incentive of citizens to acquire information about and take part in the political process (Hortala-Vallve and Larcinese, 2017). As explained in Marshall and Fisher (2014), the utility citizens derive from casting a vote partially depends on their perception of government *efficacy* — that is, a government's strength and influence on policy-making. If this perceived

[5] A more conventional regression discontinuity model (Imbens and Lemieux, 2008) is inappropriate in this setting because other policies changed discontinuously at the cut-off prior to our investigation period, which might also influence turnout — institutional details are discussed below. The use of difference-in-discontinuities allows us to eliminate the confounding influence of these other policies on our estimates.

[6] Of course, an immediate natural question relating to the mechanism underlying this effect is whether the 2013 fiscal rules had any notable effects on local public finance to begin with, and therefore whether they were a justifiable cause for concern and frustration among those affected. We address this question in Appendix C where, using data from the Italian Ministry of Interior, we show that the imposition of fiscal rules led to decreases in local expenditures across the board, and in particular to current expenditures relating to highly "visible" spending categories such as road maintenance — leading to notable changes even in the short-run, which we posit justifies the electorate's willingness to participate in elections and vote for those on the radical-right.

efficacy falls, then voting becomes less meaningful, as who is in power matters less for future socio-economic conditions (Downs, 1957). Hence, fiscal rules can depress political participation by lowering the perceived importance of election results for future policies, a prediction corroborated by existing empirical studies which document either null or negative effects of restrictions on turnout (see Häusermann et al., 2018; Aaskoven, 2021).

We expand on these insights. Concretely, we relax an implicit assumption that underpins the above argument. Namely, that the relevant fiscal rules are applied to the same governmental tier where participation is measured. For instance, fiscal rules imposed on *local* governments are predicted to lower turnout in *local* elections, as it is the discretion of *local* politicians that is curtailed by restrictions.[7] Conversely, we ask whether restrictions imposed on one governmental tier increase the perceived efficacy of politicians elected to other tiers of government *in relative terms*. Building on the results from our radical-right parties analysis, we posit that *local* fiscal rules lead to the outcomes of *supra-local* (that is, national or international) elections becoming more consequential for future policies, thus increasing the utility citizens derive from participating in supra-local ballots.[8] By studying the effects of local fiscal rules on supra-local turnout, we provide novel evidence for policy restrictions causally affecting participation incentives.

Employing the same empirical methodology to identify causal effects, we find evidence suggesting that supra-local turnout increased following the imposition of local fiscal rules, with our preferred specification suggesting an increase of roughly 3.7 percentage points in targeted municipalities — relative to the average participation rate of 73 percent.

With all this said, one potential issue regarding our empirical strategy arises from the fact that we are comparing the results of two different election types — general and European — to estimate our treatment effects. One might then argue that any significant differences identified at the 5,000 population threshold are not the causal consequence of implementing fiscal rules, but rather the delayed effects of previous policies implemented at the same threshold having heterogeneous impacts in different election types. Addressing this point of concern, we ask whether the effects of fiscal rules are more notable in municipalities where they are binding

[7] In line with this, in Aaskoven (2021), the effect of local fiscal rules on local turnout is appraised when studying Italian municipalities.

[8] For intuition, consider an extreme restriction that limits the role of mayors to simply implementing policies dictated from the center. In this scenario, who the mayor is becomes irrelevant, thus drastically decreasing their perceived efficacy. In contrast, future socio-economic outcomes — even local ones — will now fully depend on who controls the central government, or on other (inter)national institutions and the policies they implement. Hence, the perceived efficacy of politicians elected in national or international elections has increased.

— that is, where local fiscal adjustments are required to comply with the new institutional framework. In a heterogeneity evaluation, we find corroborative evidence. The impact of local fiscal rules on radical-right support and turnout is larger in magnitude in the subsample of municipalities where the fiscal rules bind. Interestingly enough, in this subsample, we also find that the at-the-time incumbent parties in control of the central government lost support.

In summary, our results provide novel evidence for European austerity in the form of local fiscal rules causally leading to increased support for right-wing, eurosceptic populists, as well as a boost in supra-local turnout.

Related work In a paper closely related to our own, Carreri and Martinez (2022), analyzing a set of local fiscal rules instituted in Colombia, find that the probability of current deficits fell and that support for the local incumbent's party increased following the imposition of restrictions. We complement Carreri and Martinez (2022) in several ways. First, although both papers deal with local fiscal rules broadly-defined, the specific rules implemented in Italy and Colombia differ substantially. As we explain below, the Italian reform imposed clear deficit targets for municipalities, and the local governments had to implement spending cuts in order to comply with the new framework (see Appendix C). On the other hand, the Colombian policy instead created a cap for operating expenditures, chiefly related to payroll and administrative procurement, whilst leaving the remuneration of front-line service providers as well as the spending on main local public goods, such as education, roads or health, unaffected.

More importantly, in line with previous work on local fiscal rules, Carreri and Martinez (2022) study the local political effects of restrictions, by asking how support for the *local* incumbent party changes after the rules are imposed. In contrast, the main objective of our investigation is to study how local fiscal rules affect participation and preferences expressed in *supra-local*, (inter)national elections. Finally, the bulk of our analysis focuses on the link between fiscal restraints and support for the radical-right, thus complementing the restrictions-incumbent support relationship highlighted in Carreri and Martinez (2022).

In another closely related study, Gabriel et al. (2022) exploit regional variation in fiscal consolidation across Europe to show that reductions in public spending are associated with increased support for extreme parties. While our papers share a similar theoretical foundation — austerity aiding populists by exacerbating uncertainty and sowing distrust in mainstream parties — we focus on a particular type of austerity policy, namely fiscal rules — to the best of

our knowledge, ours is the first investigation to zoom in on this specific link. We also conduct our analysis at the local level. Furthermore, the identification strategy employed differs.

Finally, the turnout analysis we perform relates most closely to Aaskoven (2021), who exploits a similar difference-in-discontinuities strategy in the Italian context to show that the 2001 relaxation of local fiscal rules did not lead to strong turnout effects. Our analysis differs in several key dimensions. First, the time period considered, since our focus is placed on austerity carried out after the financial crisis, when right-wing populists surged in popularity. Second, Aaskoven (2021) exploits the effect of *relaxing* rather than imposing fiscal rules on municipalities. A priori, it is unclear whether the effects need be symmetric. Third, similar to Carreri and Martinez (2022), Aaskoven (2021) focuses on the effects of local fiscal rules on local participation rates, while our objective is to assess how local fiscal rules affect supra-local electoral outcomes. Finally, of course, the analysis of Aaskoven (2021) is solely focused on *local* turnout effects, while our main goal is to asses whether fiscal rules causally affect radical-right parties and *supra-local* turnout.

2 Institutional Background: The Domestic Stability Pact

After the European Stability and Growth Pact was adopted in 1997, the Italian government decided to implement its own set of rules for the fiscal policy of local authorities. These rules were first introduced in 1999 under the name of Domestic Stability Pact (DSP). The DSP established a financial target for each municipality, which consisted of reaching a specific deficit, expressed as the difference between final revenues and final expenditures, excluding debt service and current transfers.

In 2011, the DSP rules became more stringent. Specifically, current expenditures sustained on average over a three year period, instead of the previous fiscal year, became the new reference of the financial target. Furthermore, following the change, municipalities that failed to respect the DSP incurred in sanctions, such as a cut in transfers from upper-level governments, limitations on spending capacity in the current account, on debt to finance investments and on personnel hiring, and a cut of compensations to local administrators by 30 percent. These pecuniary penalties have proven effective in enforcing the DSP (Grembi et al., 2016).

Crucially for our empirical analysis, in 2013, the DSP rules were extended to municipalities

with fewer than 5,000 residents,[9] with the stated goal of ensuring the economic stability of the Republic. This policy change — conditional on population size — allows us to exploit the discontinuity at 5,000 to analyze the short-run effects of fiscal rules. As we further discuss below, other policies also change at the 5,000 cut-off. In particular, the wage of the mayor increases (Gagliarducci and Nannicini, 2013), and in 2011 transfer cuts from the central government were reduced to municipalities above 5,000 residents.[10] Following Grembi et al. (2016), we thus combine the discontinuous change in DSP at the 5,000 threshold with a before/after comparison of outcomes in election results between 2013 and 2014 to estimate the policy's causal effects on electoral results.

3 Theory and Hypotheses

3.1 Fiscal Rules and Support for Radical-Right Parties

We begin by asking whether fiscal rules increase support for radical-right parties. The literature has recently attempted to provide explanations for the rise in Europe of parties on the radical-right following the 2008 crisis (Colantone and Stanig, 2019; Dehdari, 2022). Two broad explanations exist (Algan et al., 2017). On the one hand, some argue that populism should largely be seen as a cultural backlash against multiculturalism and loss of national identity (e.g., Norris and Inglehart, 2019). According to this view, recent socio-political and economic shifts such as a movement away from manufacturing towards service employment, increased racial and gender equality, and more expansive higher education, led to a reduction in the relative well-being of certain demographic groups (largely lower-educated, white-collar or unemployed men — see e.g., Minkenberg, 2000, Kriesi et al., 2006). These changes then produced a sense of nostalgia among these demographic groups, which are often over-represented among radical-right supporters (Dal Bó et al., 2022).

On the other hand, more in line with our study, an emerging literature focuses on the economic roots of populist support (e.g., Dehdari, 2022; Fetzer, 2019; Altomonte et al., 2019). Here, scholars posit that voting for those on the radical-right (Dal Bó et al., 2022) may be interpreted as a signal of protest (Aron and Superti, 2022) against traditional parties, which are

[9] It was not the first time that municipalities below 5,000 had to comply with the DSP. They had to between 1999 and 2000 and, between 2005 and 2008, the DSP constrained municipalities between 3,000 and 5,000 residents. From 2009 to 2012, instead, the DSP applied only to municipalities above 5,000.

[10] The transfer cuts impacted local public finance decisions, as shown in Marattin et al. (2022): mayors in affected municipalities raised local taxes to compensate for lower transfers from the central government.

blamed for existing economic grievances. A theoretical link between austerity and support for radical-right parties then emerges. As explained by Guriev and Papaioannou (2022), populists may blame the mainstream incumbents for the social costs of austerity. As resentment builds — especially when fiscal cuts target social safety nets (Kaplanoglou et al., 2015), and when austerity is implemented as part of programs imposed by international institutions such as the EU (such as in the present context) — radical-right parties have an easier time blaming the international elites and those mainstream parties collaborating with them.

We argue that the radical-right parties in Italy, LN and FdI, were in a particularly favorable position to benefit from the introduction of fiscal rules for three reasons. First, both parties adopted tough anti-EU stances at the time (Huysseune, 2010; Zappettini and Maccaferri, 2021), making them appealing outlets for those decrying EU-backed austerity, in line with the above argument. Relatedly, these parties, and especially the LN (Giordano, 2000), promoted a regionalist message, emphasizing the importance of sub-national control.

Finally, one potential explanation draws upon the literature on the link between immigration and radical-right support.[11] Here, a leading hypothesis is that native-born workers, particularly those low-skilled, are likely to turn their support towards right-wing populists in response to immigrants with similar skill-sets, perceived to compete for the same jobs (Dehdari, 2022). More pertinent for our investigation, these workers are believed to oppose immigration in order to reduce potential competition for the country's welfare services (Facchini and Mayda, 2009; Hainmueller and Hiscox, 2010). The link to fiscal rules is then intuitive to grasp: if the imposition of policy restrictions reduces the scope of welfare programs local governments can provide — since local officials may need to cut down spending to comply with the new institutional framework[12] — then potential immigrant competition for the now (perceived to be) scarcer welfare resources becomes increasingly threatening (Borjas, 1999), leading local voters to switch their support towards the political parties promising tighter border controls.

[11] The literature on immigration and the proliferation of the radical-right is vast, with anti-immigrant attitudes being, by far, the most common self-reported justification for supporting the radical-right (Oesch, 2008). Since the work of Allport (1954) on the "contact hypothesis", scholars have attempted to establish whether increased immigration fosters or harms tolerance and understanding, with many arguing that one should account whether immigrants are competing for the same scarce resources in the form of jobs or public funds — in which case, hostility between groups may arise (on this point, see Scheve and Slaughter, 2001, and Mayda, 2006, among others). And indeed, when it comes to the relation to economic insecurity, existing work (e.g., Alesina et al., 2023) has shown that those in vulnerable economic positions are more likely to exaggerate immigrant numbers, fueling broader anti-immigrant stances, and consequently support for the radical-right (Guiso et al., 2017; Billiet et al., 2014.)

[12] An effect which we document contextually in Appendix C, where we show that while the imposition of fiscal rules led to a reduction in expenditures across the board, with the effect being most notable for easily visible current expenditures.

Contextually, this captures another channel by which Italy's right-wing, anti-immigration populists may benefit from the implementation of rules.

In line with the above arguments, we posit that local fiscal rules lead to an amplification of perceived economic insecurity, as those in the affected communities know that their local policy-makers will be less able to respond to the effects of any subsequent negative economic shock. Consequently, we predict that fiscal rules will increase the appeal of Italy's right-wing populist, the LN and FdI, parties. This constitutes our first hypothesis.

H1: *Local fiscal rules increase support for the radical-right in the affected electorate.*

3.2 Fiscal Rules and Turnout

We also investigate the causal effect of local fiscal rules on supra-local electoral participation. The mechanism studied here draws from a broader literature studying how policy restrictions affect turnout. According to Franklin (2004), citizens are more likely to participate in elections when their outcomes matter, meaning that significant policy differences emerge based on who wields power. Complementing this *instrumental* view of turnout, the *expressive* voting model instead suggests that voters choose not to participate in elections to influence their outcome, but rather to express their beliefs, values and ideology (Hamlin and Jennings, 2011). Therefore, more salient elections are argued to be characterized by higher turnout rates not solely because they are more consequential for future socio-economic outcomes, but also because voters derive greater utility from expressing their views in salient ballots (Gerber et al., 2008). It follows that voters' incentives to acquire information on and participate in the political process decrease as governmental efficacy falls, since citizens increasingly conclude that their time invested in the electoral process is not worthwhile. As explained by Hortala-Vallve and Larcinese (2017), restrictions should decrease turnout, because they "lower the private returns of political information" (p. 411), and therefore diminish the salience of elections.

Such a mechanism is formalized by Marshall and Fisher (2014), who present a reduced-form model where individual i's utility from turning out (U_i) depends on their perceived probability that their vote can influence the election's outcome (Q_i), the expected utility derived from doing so (B_i), as well as from their personal costs (C_i) and a generic sense of duty (D_i) derived from participating. The authors argue that an individual will decide to vote if and

only if their utility from doing so exceeds the outside option's utility, normalized to zero:

$$U_i = Q_i B_i - C_i + D_i > 0$$

Here, governmental restrictions lead to a fall in the expected utility derived from an election's outcome B_i, resulting in a decrease in the desire to participate. As explained by Häusermann et al. (2018), imposing fiscal rules means that the ensuing government will have limited discretion. Hence, the electorate increasingly comes to understand that their representatives' "hands are tied," leading to lower participation incentives.[13]

In this paper, we assess the predictions of this simple framework by studying a so-far untested implication: namely, that restrictions imposed on one governmental tier should increase the perceived efficacy of governments or institutions operating at different tiers *in relative terms*. Then, if this argument holds in our setting, *local* fiscal rules — which reduce the discretion of *local* officials — should lead to policy outcomes becoming more dependent on the decisions carried out by the central government, or on international institutions such as the EU.

Going back to the Marshall and Fisher (2014) model, when considering a citizen's decision to participate in supra-local elections, local fiscal rules should then *increase* the expected utility B_i derived from the citizen's vote influencing the race's outcome or from their generic sense of duty D_i, thus leading to a ceteris paribus increase in their desire to participate. This constitutes our paper's second hypothesis.

H2: *Local fiscal rules increase the perceived efficacy of upper-tier institutions, and therefore incentivize turnout in supra-local elections.*

3.3 Fiscal Rules and Other Parties

Lastly, we address a complementary question: if the radical-right parties benefit from the imposition of fiscal rules, which parties lost out? Building on the literature investigating the electoral effects of austerity (e.g. Talving, 2017, Jacques and Haffert, 2021), we ask whether fiscal rules negatively affect the party controlling the central government. Theoretically, if governmental restrictions lead to frustration among those affected (Colantone and Stanig, 2019)

[13] This insight builds on a related scholarship investigating the effects on participation of increasing globalization (e.g., Steiner and Martin, 2012).

and to dissatisfaction with mainstream parties (Guiso et al., 2019), we expect support for the incumbent parties to fall in targeted municipalities. This constitutes our third hypothesis:

H3: *Local fiscal rules decrease support for the incumbent parties in the affected electorate.*

Despite the appealing simplicity of this prediction, we note that it rests on the assumption that the affected electorate consists of *fiscal liberals* (Jones et al., 2012), generally opposed to conservative proposals that curtail the discretion of their local representatives. This assumption is, however, contested as several studies (e.g. Alesina et al., 2012; Arias and Stasavage, 2019) find no empirical evidence supporting the theoretical view wherein fiscal austerity harms the popularity of those responsible. These results suggest that a non-negligible part of the electorate may be fiscally conservative instead, thus supporting the adoption of adjustment policies (Brender and Drazen, 2008). It is therefore an empirical question whether fiscal rules benefit or not the ruling party.

4 Data and Descriptive Statistics

4.1 Election Data

We use the municipality-level results for two elections: the general elections of 2013 and the European elections of 2014, available through the Italian Ministry of Interior. As to the general elections, we only use the results concerning the Parliament (the lower house) because of its better representation of the Italian electorate (voting rights are given to each citizen older than 18 years old, contrarily to the Senate — the higher house — where the age limit is at 25 years old). We only consider the 2013 and 2014 elections. In the main analysis, we do not include elections that took place before this period (e.g., general or European elections in 2008 and 2009) because of the aforementioned policy change in fiscal rules in 2009 and transfer cuts implemented in 2011 at the 5,000 threshold. However, we do use those elections as a robustness check when investigating parallel trends in electoral turnout.[14] Moreover, we do not include elections after 2014 (e.g., Parliamentary elections in 2018 or European elections in 2019) because the DSP was abolished in 2016, and fiscal rules were considerably revised, with the new rules being applied to all municipalities irrespective of population size. Specifically, the overcoming of the DSP has significantly reduced the public finance objective for the local authorities and

[14] See Appendix A and Figure A.3 in particular.

has allowed them to use the surplus from previous years.

Another important caveat is that we use 2013 as pre-policy period, although fiscal rules were changed starting from 2013. However, since the 2013 election takes place in February and the budget balance is not approved until April, it is unlikely that between January and February 2013 the consequences of the new rules were already evident to voters. Moreover, if there are anticipation effects — e.g., as a consequence of municipalities coping in advance with the DSP by raising taxes and lowering deficits — they are likely to bias our results downwards. Therefore, our estimates could be interpreted as lower bounds to the effect of fiscal rules.

Our main dependent variables are electoral turnout, computed as the ratio between the number of total votes in each election and the number of voters from electoral registries at the municipal level, and the vote shares of Italy's two major right-wing populist parties, LN and FdI, which we aggregate in a group termed the "populist right". Additionally, for one of our complementary analyses, we have data for vote shares secured by The Five Star Movement (*Movimento 5 Stelle* or M5S), and Italy's at the time *incumbent parties*, defined as the ones supporting the central government in 2013 and 2014: the Democratic Party and the People of Freedom.[15]

4.2 Sample Restrictions and Descriptive Statistics

We restrict our sample to municipalities whose population size lies between 1,000 and 10,000 residents (population size is taken from the 2011 census, available through the Italian National Statistical Institute). We also exclude municipalities in special statute regions,[16] because, since 2002, they have been subject to different fiscal rules and have had a stronger degree of autonomy when it comes to the local budget (Grembi et al., 2016). Moreover, we exclude municipalities affected by the 2009 and 2012 earthquakes, because they were allowed additional fiscal flexibility to cope with the damages produced by the earthquakes. Finally, since some municipalities were suppressed between 2013 and 2014, either by being incorporated in bigger municipalities or because of fusions between municipalities, we exclude them from the sample. After these restrictions, the sample consists of 7,890 observations, stemming from 3,945

[15]The People of Freedom is the main center-right party, which — for most of 2013 and 2014 — supported the Democratic government, though not expressing the Prime Minister. In a robustness exercise, we are going to define the incumbent vote share as that of the Democratic Party only.

[16]The 15 "ordinary" regions are: Piemonte, Lombardia, Liguria, Veneto, Emilia-Romagna, Toscana, Umbria, Marche, Lazio, Abruzzo, Molise, Campania, Puglia, Basilicata, Calabria. The 5 "special" regions are Sicilia, Sardegna, Valle d'Aosta, Friuli-Venezia Giulia and Trentino Alto Adige.

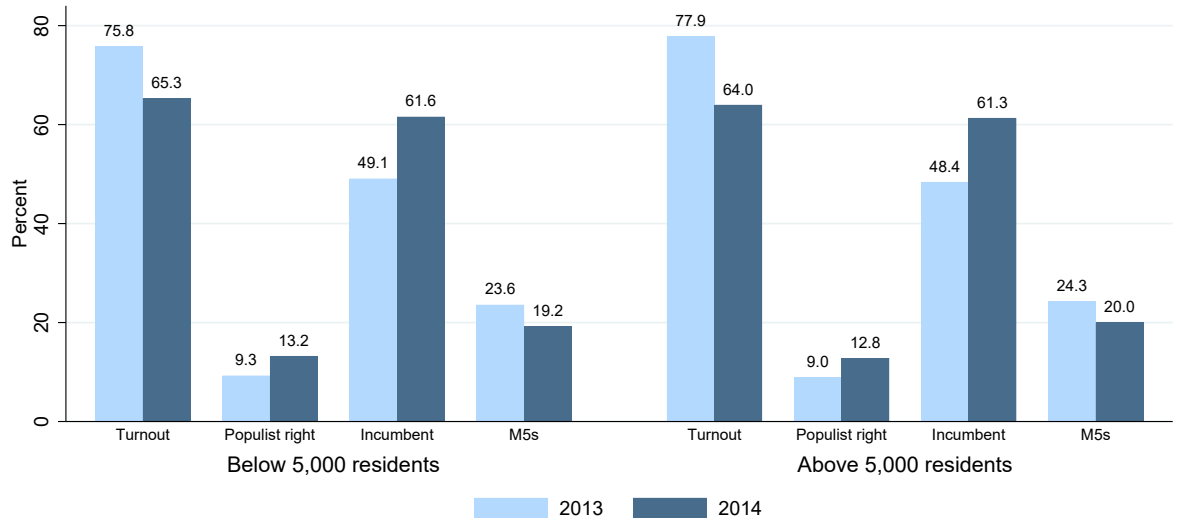


Figure 1: Turnout and party vote shares in 2013 and 2014

Notes. The figure reports average turnout and party vote shares in the Parliamentary elections held in 2013 and in the European elections held in 2014.

municipalities observed over the two rounds of elections in 2013 and 2014.

For this subsample, Figure 1 shows the average electoral turnout and vote shares for each party in both election rounds in treated (below 5,000 residents) and control (above 5,000 residents) municipalities. Overall, we observe a substantial change in political participation between the two rounds of elections: European elections have historically lower levels of turnout with respect to general elections. That said, we observe that the decrease in political participation is lower in magnitude in treated relative to control municipalities — providing some descriptive evidence in support of our hypothesis whereby local fiscal rules encourage supra-local turnout.

Focusing on parties, the incumbents gained the most votes between the general and European elections, followed by populist right-wing parties, in both groups of cities. In contrast, the M5S lost consent. Populist parties as a whole performed only slightly worse in European elections relative to general elections.

How much of these dynamics are due to the onset of fiscal rules? This is our main research question. Now, we turn to the discussion of the empirical strategy to clarify how to identify the causal link between fiscal rules and electoral outcomes.

5 Empirical Strategy

As explained in Grembi et al. (2016) and Eggers et al. (2018), when more than one policy change happens at the cut-off — in our case, the increase in mayor's wage and the transfer cuts from the central government — conventional regression discontinuity designs are unable to credibly recover causal estimates of the policy of interest. To address this issue, we use a difference-in-discontinuities design to estimate the causal effect of fiscal rules on electoral results.

Intuitively, taking the difference between the discontinuities at the cut-off (5,000 residents) in the pre- and post-treatment periods allows us to control for other policies changing discontinuously at the threshold. Formally, let P_i be the population size of municipality i, so that $T_i = 1\{P_i < P^*\}$ is a treatment assignment rule equal to 1 if municipality i has less than $P^* = 5,000$ residents. Furthermore, let Y_{1it} and Y_{0it} be the potential outcomes when $T_i = 1$ and $T_i = 0$, respectively, so that the observed outcome is $Y_{it} = (1 - T_i)Y_{0it} + T_i Y_{1it}$. The local average treatment effect of imposing fiscal rules is identified at the cut-off $P_i = P^*$ by taking the differences in the limit of the difference in outcomes in the post- and pre-treatment periods:

$$\tau^{DD} = (Y^- - Y^+) - (\bar{Y}^- - \bar{Y}^+),$$

where $Y^- - Y^+ = \lim_{\Delta \to 0} E(Y_{it}|P_i - P^* < -\Delta, t > t_0) - E(Y_{it}|P_i - P^* > \Delta, t > t_0)$ and $\bar{Y}^- - -\bar{Y}^+ = \lim_{\Delta \to 0} E(Y_{it}|P_i - P^* < -\Delta, t \leqslant t_0) - E(Y_{it}|P_i - P^* > \Delta, t \leqslant t_0)$ for $t_0 = 2013$. Empirically, τ^{DD} can be identified within an OLS regression. Following Gelman and Imbens (2019), we restrict the sample to municipalities with population size P_i in the interval $(P^* - h, P^* + h)$, where h is the bandwidth that defines the sample. We then estimate a local linear regression, running the following model:

$$Y_{it} = \alpha + \beta_0 \widetilde{P}_i + T_i \left(\gamma_0 + \gamma_1 \widetilde{P}_i\right) + A_t \left[\delta_0 + \delta_1 \widetilde{P}_i + T_i \left(\eta_0 + \eta_1 \widetilde{P}_i\right)\right] + \varepsilon_{it}, \tag{1}$$

where $\widetilde{P}_i = P_i - P^*$ is the normalized population, centered around $P^* = 5,000$, T_i identifies treated municipalities, A_t is a dummy equal to 1 for 2014. The local average treatment effect in the post period τ^{DD} is identified by η_0. The optimal bandwidth h is chosen according to the algorithm proposed by Calonico et al. (2014). We report estimates with three different bandwidths: one estimated before the introduction of fiscal rules ($h = h_0$); one estimated after the introduction of fiscal rules ($h = h_1$); and one that equals the average of the pre-period

and post-period optimal bandwidths ($h = \frac{1}{2}h_0 + \frac{1}{2}h_1 = \overline{h}$). The latter will be our preferred specification.

We also show graphical analyses of the relationships of interest. Specifically, we plot local sample means of the difference in outcomes between 2014 and 2013 in population bins over the normalized population $\widetilde{P}_i$, where the size of each bin is 100 residents, together with a 1^{st} order polynomial fit on both sides of the threshold,[17] alongside 95 percent confidence intervals. We report in Appendix A validity tests for the difference-in-discontinuities design.

6 Results on the Electoral Effects of Fiscal Rules

In this section we empirically test the three hypotheses laid out in section 3, illustrating our main causal results from the difference-in-discontinuities estimates. We discuss in Appendix B their robustness to a number of sensitivity checks. Further, we show in Appendix C the fiscal adjustment taken by municipalities to adapt to the new institutional framework.

6.1 Do Fiscal Rules Increase Support for Radical-Right Parties?

First, we quantify the causal effect of local fiscal rules on support for those on the radical-right. We assess hypothesis **H1** by evaluating how the radical-right vote share responded to the 2013 imposition of fiscal rules in Italy. We illustrate the results from our preferred specification in Figure 2. Consistent with hypothesis **H1**, we observe a sharp, discontinuous jump taking place at the threshold. Relative to the control group, treated municipalities experienced an increase in radical-right support between the 2013 and 2014 elections. Visual inspection suggests a large jump, a result that would be difficult to explain by alternative models (even highly non-linear ones) which do not account for the direct effect of imposing fiscal rules on party preferences.

Quantitatively, the effect is meaningful both statistically and politically as shown in Table 1, which reports estimates of equation (1) in columns 1-3. The results from our preferred specification are given in column 1. Here, we see that fiscal rules led to a statistically significant increase in the radical-right vote share of just under 1 percentage point, or roughly 9.3 percent relative to the average level in the control group of 10.5 percent. The estimate remains largely unaffected by using alternative bandwidths.

[17] The linear fit is estimated within the optimal bandwidth h and extrapolated to observations with population size between 3,500 and 6,500 (i.e., $\pm 1,500$ residents from the cut-off).

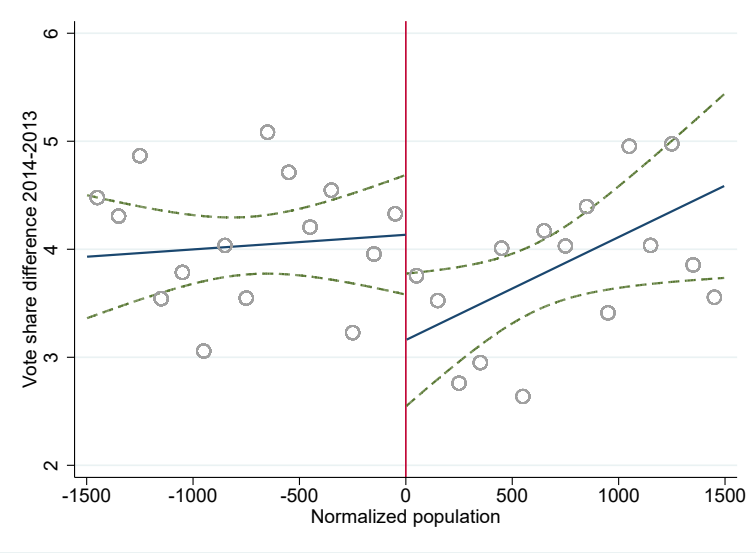


Figure 2: The effect of local fiscal rules on the vote share of right-wing populist parties

Notes. The figure plots binned averages of right-wing populist parties' (Northern League and Brothers of Italy) vote shares against normalized population size. The size of each bin is 100 residents. The solid lines are local linear regressions fit on both sides of the threshold. Dashed lines are 95 percent confidence intervals. The numerical coefficient estimates for the treatment effects illustrated here is given in Table 1.

All in all, our findings add to the literature on the economic roots of populism by providing rigorous evidence showing that austerity policies in the form of local fiscal rules may lead to a causal increase in support for right-wing, eurosceptic populists in the affected electorate — thus corroborating **H1**.

6.2 Do Fiscal Rules Increase Supra-Local Turnout?

Next, we run our difference-in-discontinuities regression to assess the causal effect of imposing fiscal rules on supra-local turnout and test hypothesis **H2**. The results from our preferred specification are graphically illustrated in Figure 3, with corresponding numerical estimates shown in Table 2. Overall, our findings suggest that the imposition of fiscal rules led to a statistically and politically-significant increase in the participation rate of roughly 3.7 percentage points, or about 5 percent of the mean in the control group.

These results corroborate our second hypothesis, and suggest that a more comprehensive understanding of the link between fiscal rules and turnout can result from acknowledging that the participation trade-offs faced by citizens vary asymmetrically between governmental tiers.

We further investigate our hypothesis that fiscal rules incentivize turnout in supra-local elections only — in line with the above theory — by analyzing whether the imposition of the 2013 local fiscal rules affected turnout in local (municipality-level) elections. As shown

Table 1: Difference-in-discontinuities estimates of the impact of fiscal rules on the vote share of right-wing populist parties

	(1)	(2)	(3)
	Dependent variable: Populist right vote share		
Treatment × Post	0.975**	0.866**	0.955**
	(0.422)	(0.411)	(0.429)
Control mean pre	10.48	10.49	10.55
Bandwidth	1379	1422	1336
Observations	1898	1958	1818

Notes. The table shows difference-in-discontinuities estimates for the effect of fiscal rules on the vote share of right-wing populist parties. Columns 1-3 show estimates from local linear regressions, obtained after restricting the sample to municipalities within the Calonico et al. (2014) optimal bandwidth, where column 1 uses the average bandwidth between the one computed in the pre-period (column 2) and the one computed in the post-period (column 3). Robust standard errors, clustered at the municipal level, in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Difference-in-discontinuities estimates of the impact of fiscal rules on electoral turnout

	(1)	(2)	(3)
	Dependent variable: Turnout		
Treatment × Post	3.717**	3.886**	3.493**
	(1.552)	(1.638)	(1.472)
Control mean pre	73.17	72.99	73.24
Bandwidth	1144	1038	1250
Observations	1566	1400	1720

Notes. The table shows difference-in-discontinuities estimates for the effect of fiscal rules on electoral turnout. Columns 1-3 show estimates from local linear regressions, obtained after restricting the sample to municipalities within the Calonico et al. (2014) optimal bandwidth, where column 1 uses the average bandwidth between the one computed in the pre-period (column 2) and the one computed in the post-period (column 3). Robust standard errors, clustered at the municipal level, in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

in Table E.1 and Figure E.1 in the Appendix, we do not find sufficient statistical evidence to reject the null hypothesis whereby fiscal rules have a negligible effect on local-level turnout, corroborating our hypothesis and the broader literature (e.g., Aaskoven, 2021, Häusermann et al., 2018). Our results suggest that how turnout is measured matters: the utility citizens derive from participation need not be symmetric or respond symmetrically to policy changes across different tiers of elections.

6.3 Do Fiscal Rules Affect the Electoral Shares of Other Parties?

Finally, we examine the effects on the incumbent party. The difference-in-discontinuities results are illustrated in panel A of Figure 4 and Table 3. Overall, when considering the entire

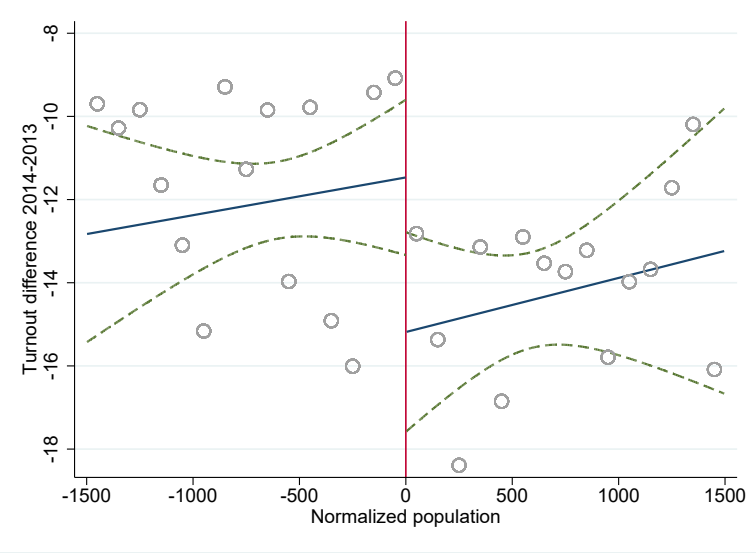


Figure 3: The effect of local fiscal rules on electoral turnout

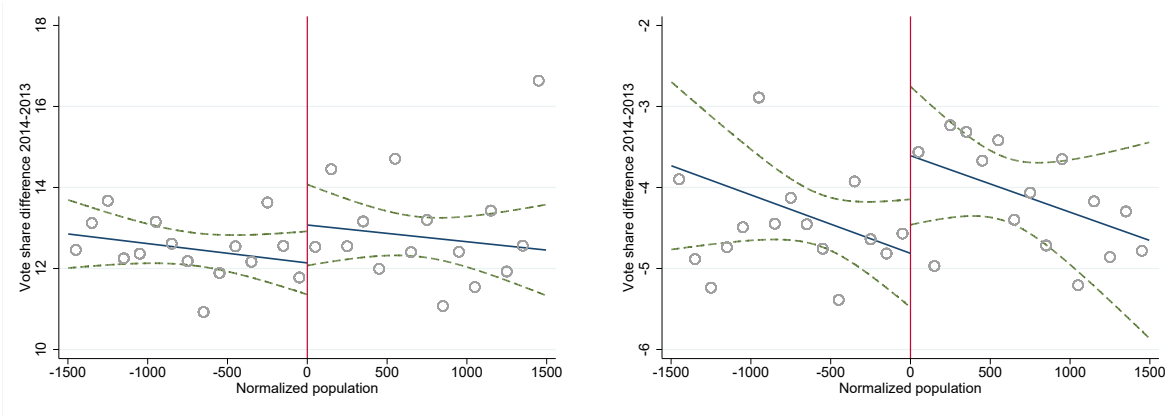
Notes. The figure plots binned averages of differences in electoral turnout between the 2013 and 2014 elections against the normalized population size. The size of each bin is 100 residents. The solid lines are local linear regressions fit on both sides of the cut-off. Dashed lines are 95 percent confidence intervals. The numerical coefficient estimate for the treatment effect illustrated here is given in Table 2.

sample of municipalities, our findings are noisy: while the treatment effect is negative across all the different model alterations, we cannot reject the null hypothesis whereby the estimate differs significantly from zero in our data, except for a marginally significant effect in column 2.[18] This null aggregate effect masks an important source of heterogeneity, however, which we investigate more thoroughly in section 7: when zooming in on constituencies where fiscal rules bind we find that the incumbents suffered an electoral penalty of roughly 3.4 percentage points. This estimate is reassuring for our empirical setup, as it is precisely in municipalities where restrictions have real policy-making effects that one would expect the political impact of fiscal rules to be most apparent, providing at least partial support of hypothesis **H3**.

For completeness, we also investigate the effects on the other populist party at the time: the M5S. Empirically, we run our difference-in-discontinuities regression to assess the causal effect of imposing fiscal rules. The results are illustrated in panel B of Figure 4 and Table 3. We find that the M5S's vote share fell in municipalities where fiscal rules were implemented relative to their control counterparts.

In Appendix D, we discuss this particular result more in-depth, and explain how it fits with the broader literature on populism heterogeneity. The key takeaway of Appendix D dis-

[18] We here define the incumbent party vote share as the sum of the Democratic Party and People of Freedom's vote shares. In Table E.2 in the Appendix we define the incumbent as the Democratic Party alone, as it expressed the prime minister at the time. The table confirms the null result.



(A) Incumbent (B) M5s

Figure 4: The effect of local fiscal rules on the vote share of the incumbent party and M5s: disaggregate analysis

Notes. The figure plots binned averages of the incumbent (panel A), and 5 Star Movement (panel B) vote shares against normalized population size. The size of each bin is 100 residents. The solid lines are local linear regressions fit on both sides of the threshold. Dashed lines are 95 percent confidence intervals. The numerical coefficient estimates for the treatment effects illustrated here in panels A and B are in Table 3.

cussion is the following: we posit that this result contributes to our understanding of which type of populism proliferates under austerity. In particular, we rationalize this finding as exemplifying the heterogeneity in the populist movement. Indeed, the populist ideology is not homogeneous in its values, and individuals that broadly align themselves with the populist umbrella term (much like the supporters of any political party) may exhibit important differences in their policy preferences.[19] Our results suggest that austerity in the form of fiscal restrictions may influence which parts of this discursive framework will garner more electoral support — in this case, documenting a movement towards the more right-wing, eurosceptic, regionalist and anti-immigrant of the Italian parties.

7 Heterogeneity by Fiscal Capacity

So far, we showed that significant differences in participation and party preferences arise at the 5,000 population threshold. Given the difference-in-discontinuities design employed, we discussed how these findings constitute evidence for the causal consequences of implementing local fiscal rules. That said, one concern remains: since we are comparing general and European elections in our analysis, one might argue that the estimated effects are not the result of

[19] As detailed in Colantone and Stanig (2019), despite some similarities, such as a shared anti-establishment stance, populist parties are fundamentally heterogeneous. For instance, populist agendas may range from extreme left proposals involving extensive redistribution to extreme right isolationist, anti-immigration policy proposals (see, e.g., Funke et al., 2020). As argued by Bonikowski (2016), populism should not even be perceived as a cohesive ideology, but rather a discursive frame which may be used to propagate very different platforms.

Table 3: Difference-in-discontinuities estimates of the impact of fiscal rules on the incumbent parties and Five Star Movement vote shares

	(1)	(2)	(3)
	[A] Dependent variable: Incumbent vote share		
Treatment × Post	-0.936	-1.219*	-0.916
	(0.648)	(0.697)	(0.626)
Control mean pre	52.08	52.22	52.04
Bandwidth	1469	1309	1628
Observations	2016	1790	2238
	[B] Dependent variable: M5S vote share		
Treatment × Post	-1.206**	-1.199**	-1.208**
	(0.553)	(0.548)	(0.552)
Control mean pre	22.65	22.65	22.64
Bandwidth	1094	1102	1086
Observations	1470	1486	1462

Notes. The table shows difference-in-discontinuities estimates for the incumbent party and M5S vote shares in Panel A and B, respectively. Columns 1-3 show estimates from local linear regressions, obtained after restricting the sample to municipalities within the Calonico et al. (2014) optimal bandwidth, where column 1 uses the average bandwidth between the one computed in the pre-period (column 2) and the one computed in the post-period (column 3). Robust standard errors, clustered at the municipal level, in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

local fiscal rules, but rather that of previous policies — e.g., the 2011 transfer cuts discussed above — affecting different types of elections differently.

To address this concern, we investigate here whether the political effects of fiscal rules are larger in magnitude in municipalities where the rules bind, as it is here where one would expect the treatment effects to be most notable — *if* the estimates above indeed capture the consequences of implementing fiscal rules. As explained above, the Domestic Stability Pact establishes an objective financial target for each municipality, equal to a given fraction, which varies over time, of average expenditures over a three year period. The objective target is then compared to the so called DSP target, which equals the difference between total revenues and total expenditures, i.e., the municipal deficit.[20] We call the difference between the DSP target and the objective target the *fiscal gap*. If the fiscal gap is negative, the municipality has to either increase revenues or reduce expenditures to meet the objective target. Therefore, we define a dummy variable equal to one if the municipality has a negative fiscal gap based on balance

[20]Total revenues (expenditures) are computed as the sum of current revenue (expenditure) accruals and capital revenue (expenditure) cash flows.

sheet quantities in 2012.[21] Treated municipalities in this group have to undertake austerity measures once fiscal rules are introduced in 2013 in order to meet the objective target. Among the 3,945 municipalities in our data, 2,431 have a negative fiscal gap in 2012 and 1,514 have a positive fiscal gap. We then estimate difference-in-discontinuities regressions, interacting all regressors in equation (1) with dummies for negative and positive fiscal gaps, restricting the sample to the optimal bandwidth according to Calonico et al. (2014).[22]

Table 4 reports the difference-in-discontinuities coefficients for the two groups of municipalities. The table shows that the increase in support for populist right-wing parties is positive and statistically significant only in municipalities with a negative fiscal gap (1.6 percentage points) — that is, in municipalities needing to adopt austerity measures after the enforcement of fiscal rules, while the incumbent party reduces its vote share by 3.4 percentage points in municipalities with negative fiscal gap. In the latter case, the difference between coefficients for municipalities with positive and negative fiscal gaps is also statistically significant at 5 percent level. The table also shows that the increase in turnout is statistically significant only in municipalities with a negative fiscal gap (6.6 percentage points). We note, however, that the estimates are notably noisier. Finally, the estimated treatment effects for the M5S are negative in both groups of municipalities, but not statistically significant.

Overall, this evidence provides ground for the interpretation that the increase in support for right-wing eurosceptic populist parties and the increase in turnout are driven by voters' negative valuation of the effects of fiscal rules on municipalities' budget balance, as opposed to the interpretation where these effects are to be seen as lagged consequences of previous policies.[23]

[21] We therefore define the DSP and objective target for each municipality in 2012. According to the rules at the time, the objective target was set equal to 17 percent of average expenditures in 2006-2008 minus the cuts in transfers from the central government happening between 2009 and 2011.

[22] Thus, we estimate:

$$Y_{it} = \kappa + \{\beta_0^- \widetilde{P}_i + T_i\left(\gamma_0^- + \gamma_1^- \widetilde{P}_i\right) + A_t\left[\delta_0^- + \delta_1^- \widetilde{P}_i + T_i\left(\eta_0^- + \eta_1^- \widetilde{P}_i\right)\right]\} \times NegativeFiscalGap$$
$$+ \{\beta_0^+ \widetilde{P}_i + T_i\left(\gamma_0^+ + \gamma_1^+ \widetilde{P}_i\right) + A_t\left[\delta_0^+ + \delta_1^+ \widetilde{P}_i + T_i\left(\eta_0^+ + \eta_1^+ \widetilde{P}_i\right)\right]\} \times PositiveFiscalGap + \nu_{it},$$

where κ is a constant, ν_{it} is an error term, and all other variables are defined as in equation (1). $NegativeFiscalGap$ and $PositiveFiscalGap$ are dummy variables. The parameters of interest are η_0^- and η_0^+, which measure the difference-in-discontinuities estimates for municipalities with negative and positive fiscal gaps, respectively.

[23] We provide further suggestive evidence in favor of the causal interpretation of our findings in the validity tests in Appendix A, where we show that no significant turnout differences can be identified at the 5,000 population threshold when comparing the results of the 2008 and 2009 elections (occurring before the possibly confounding policy changes) with those of the 2013 Parliamentary ballot.

Table 4: Difference-in-discontinuities estimates of the impact of fiscal rules on electoral results, heterogeneous effects by whether municipalities have a negative or positive fiscal gap

	(1)	(2)	(3)	(4)
Dependent variable:	Populist right vote share	Turnout	Incumbent vote share	M5S vote share
Treat × Post × Pos. fiscal gap	0.259	3.421	0.465	-1.060
	(0.622)	(2.462)	(1.113)	(0.848)
Treat × Post × Neg. fiscal gap	1.598**	6.553***	-3.359***	-0.488
	(0.781)	(2.467)	(1.181)	(0.918)
p-value diff. coeff.	0.180	0.369	0.019	0.648
Bandwidth	1379	1144	1469	1094
Observations	1898	1566	2016	1470

Notes. The table reports local linear difference-in-discontinuities coefficients interacted with dummies for municipalities with positive and negative fiscal gap in columns 1-4 for the main outcomes. The fiscal gap is defined as the difference between the DSP target and the objective target: the former equals the difference between total revenues and expenditures; the latter equals 17 percent of average current expenditures in the period 2006-08 net of cuts in transfers from the central government. The bottom of the table reports the p-value of the equality of coefficients for positive vs. negative fiscal gap. All regressions are run on the sample within the Calonico et al. (2014) optimal bandwidth. Robust standard errors, clustered at the municipal level, in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

8 Summary and Conclusions

Fiscal rules aim to curtail the undesirable political behavior of those who govern, yet little work exists on how they affect the political behavior of those *governed*. In this paper, we make a contribution by evaluating the causal effects of local fiscal rules on two key political dimensions: voter preferences and electoral participation. First, we ask whether local restrictions may exacerbate support for right-wing populists among those affected. After all, if local socio-economic outcomes are now perceived to be more dependent on policy-making enacted by (inter)national institutions, one might expect that those wishing to restore the previous status-quo are now incentivized to support those anti-system, contextually regionalist and eurosceptic entities, which promise to do so. Exploiting exogenous variation created by a 2013 reform enacted in Italy wherein deficit targets were imposed in municipalities with fewer than 5,000 residents, we find causal empirical evidence supporting this prediction.

Our findings contribute new rigorous causal evidence to an emerging literature on the economic origins of right-wing populism proliferation in Europe. In particular, we show that, alongside macroeconomic shocks (Dal Bó et al., 2022), layoffs and unemployment (Dehdari,

2022), immigration (Dustmann et al., 2018), import competition (Colantone and Stanig, 2018) and welfare retrenchments (Fetzer, 2019), policy restrictions in the form of fiscal rules may partially explain the recent electoral success of right-wing populists.

Next, we refine a theoretical argument suggesting that fiscal rules may discourage turnout by limiting the perceived efficacy of policy-makers (Hortala-Vallve and Larcinese, 2017; Häusermann et al., 2018). We do so by showing that an asymmetry exists. While fiscal rules may constrain the powers of certain politicians, in relative terms, policy-making becomes more dependent on the influence of others. Contextually, we argue that local fiscal rules increase the perceived efficacy of national and international institutions, thus increasing participation in supra-local races. Employing the same empirical method, we bring supporting evidence.

While acknowledging that the external validity of our conclusions can be justifiably scrutinized, as our analysis focuses on one particular policy change in a specific institutional context, our results may guide future work on the political consequences of fiscal rules more broadly. For instance, one important question for future research would be: how do the populist political parties, rather than the electorate, respond to fiscal rules — in terms of their ideological discourse and policy proposals? In the Italian context, for example, reports in the popular media[24] discuss how the Five Star Movement has become increasingly anti-globalist, anti-immigration and eurosceptic, suggesting an ideological shift towards the Northern League's framing of these issues. Future work may try to disentangle how much of this re-alignment is the result of policies such as fiscal rules.

These questions also have broader applicability. For instance, following 2016, the US popular media began concerning itself with understanding whether the Republican Party began adopting increasingly populist stances,[25] perhaps reflecting a change in its electorate's preferences. Building on our insights, future research may look into the extent to which economic policies such as fiscal rules may play a role in explaining such trajectories towards one extreme or the other of the populist discourse.

[24] See, for example `https://bit.ly/3psMNmW` or `https://bit.ly/3jr4mQk` (in Italian).
[25] See a relevant article from The Economist here: `https://econ.st/3jmTdzZ`.

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Supplementary Material for

Austerity, Turnout and Support for Radical-Right Parties: The Case of Local Fiscal Rules

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Summary

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A Validity tests

We test whether difference-in-discontinuities is a suitable identification strategy in the context of Italian municipalities over the period under analysis.

Absence of discontinuities in covariates We verify that there are no discontinuities in the distribution of changes in baseline demographic characteristics in treated and control municipalities. Given the short time horizon, we cannot use data from different censuses (which are available every ten years and the most recent one at the time of writing dates back to 2011). We use, instead, available variables collected by the Italian National Statistical Institute on a time-varying basis for 2013 and 2014: the female population share, the number of immigrants per 1,000 residents, the number of new citizenships to foreigners in a given year, the number of cohabiting couples, and the average family dimension. The results of the graphical analysis are presented in Figure A.1. There is a wide overlap in confidence intervals and municipal characteristics vary almost continuously with population size. The results of the local linear regressions are presented in Table A.1. There is a significant and negative discontinuity in the

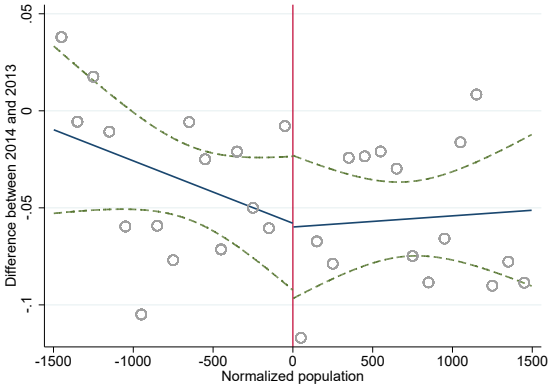
number of immigrants per 1,000 residents, which is however very small in economic terms (0.15 less immigrants per 1,000 residents). The discontinuity in the number of immigrants may be source of concern in light of our result on the increased consent expressed in treated municipalities for right-wing parties, which base their political platform on anti-immigration policies. However, in this case the sign of the discontinuity is negative and, therefore, if anything, the lower presence of immigrants in treated municipalities should represent a downward bias on our estimates for right-wing populist parties.

Absence of discontinuities in the density of the running variable We test for the presence of sorting, i.e., the ability of mayors to strategically manipulate population size to avoid falling on one side of the cut-off. Figure A.2, panel A, shows the population density in 2013 and 2014, highlighting no sign of discontinuity at the 5,000 cut-off. Panel B shows the result of the McCrary test (McCrary, 2008) on the difference between the density in 2014 and the density in 2013. We do not find evidence of strategic manipulation of the running variable.

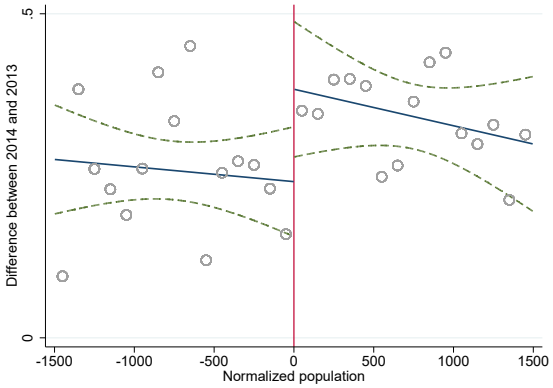
Parallel trends We test for the presence of parallel trends between treated and control municipalities before the introduction of fiscal rules. Specifically, we use Parliamentary elections in 2008 and European elections in 2009 to test for parallel trends, by estimating a dynamic version of equation (1), replacing the single post-treatment dummy A_t with year dummies and reporting the coefficients of their interactions with the treatment dummy T_i. Testing for parallel trends is a challenging task in our setting, as the Italian political landscape went through profound changes between 2008-2009 and 2013-2014, making it hard to compare parties' vote shares across different elections.[i] We will therefore focus the investigation of parallel trends on electoral turnout. Even in this case, the analysis of parallel trends warrants a note of caution. As highlighted when discussing the empirical strategy, there are policy changes over time at the 5,000 cut-off. In particular, the central government operated a sizable cut in funds transferred to municipalities with more than 5,000 residents in 2011 (see, e.g., Marattin et al., 2022, for an encompassing discussion). If the cut in transfers is correlated with electoral outcomes, then dynamic difference-in-discontinuities estimates capture both the effects of the in-

[i]For example, the Five Star Movement was born in 2008 and did not compete in national elections until 2013. The Northern League went from being a local party in Northern Italy to being a national party with a broader political platform and, in 2008, did not run in Parliamentary elections in approximately one-third of Italian municipalities (mainly in the South). The Democratic Party changed five leaders between 2008 and 2013, while the People of Freedom failed in its goal of unifying center-right parties in a common platform and endured many divisions (one of which gave birth to Brothers of Italy) between 2008-9 and 2013-4. It is, therefore, difficult to test for parallel trends in the presence of varying definitions of parties over time.

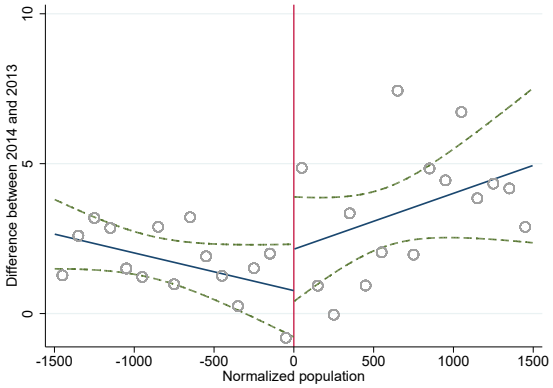
troduction of fiscal rules and that of transfer cuts. Moreover, until 2008 fiscal rules applied to municipalities between 3,000 and 5,000 residents, raising a further source of concern on the interpretation of dynamic effects in this context. With these caveats in mind, we report in Figure A.3 the dynamic difference-in-discontinuities estimates for electoral turnout. Estimates are not statistically significant in election years 2008 and 2009, while we observe a statistically and politically significant jump in 2014, when fiscal rules are in place, highlighting the absence of pre-trends in one of our main outcome variables. This finding is reassuring on the validity of our empirical strategy — although the aforementioned caveats should be kept in mind.



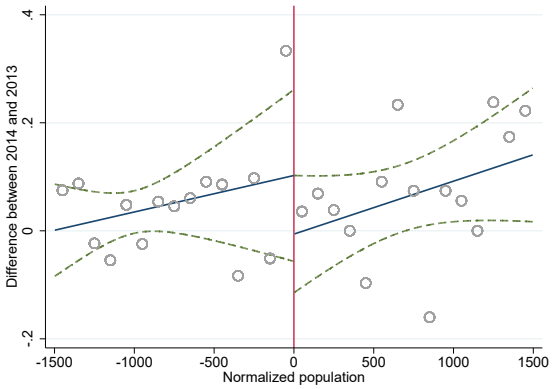
(A) Female share



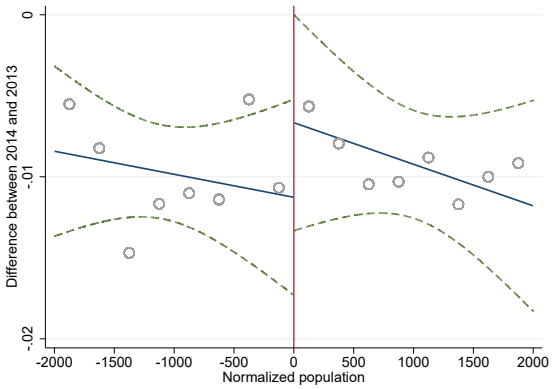
(B) Immigrant share



(C) New citizenships to foreigners



(D) No. cohabiting couples



(E) Average no. of family components

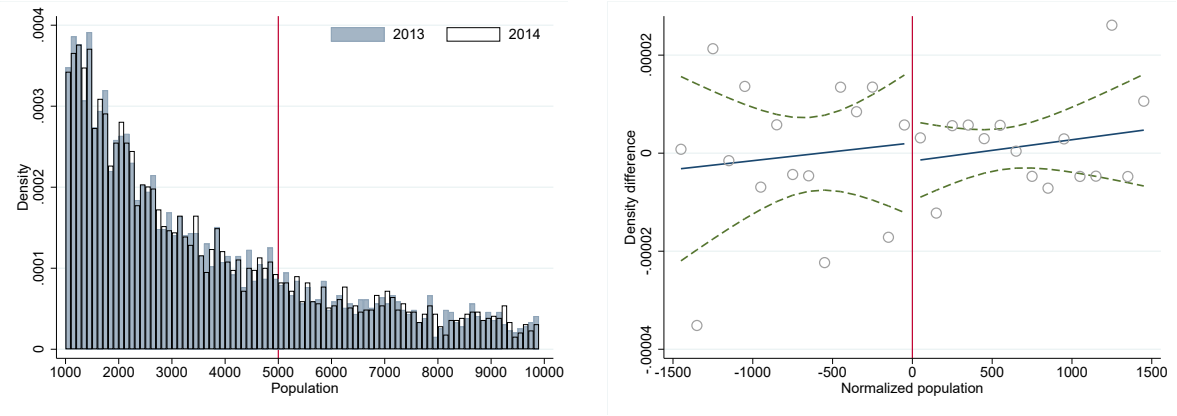
Figure A.1: Difference-in-discontinuities for demographic variables

Notes. The figure plots binned averages of demographic characteristics against normalized population size: the female population share (panel A), the number of immigrants per 1,000 inhabitants (panel B), the number of new citizenships to foreigners (panel C), the number of cohabiting couples (panel D), the average number of family components (panel E). The size of each bin is 100 residents. The solid lines are local linear regressions fit on both sides of the cut-off. Dashed lines are 95 percent confidence intervals. The numerical coefficient estimates for the treatment effects illustrated here are given in Table A.1.

Table A.1: Difference-in-discontinuities estimates of the impact of fiscal rules on demographic variables

	(1)	(2)	(3)
	[A] Dependent variable: Female share		
Treatment × Post	-0.003	0.001	0.008
	(0.025)	(0.024)	(0.026)
Control mean pre	50.83	50.83	50.83
Bandwidth	1581	1689	1472
Observations	2160	2342	2018
	[B] Dependent variable: Immigrant share		
Treatment × Post	-0.145**	-0.167**	-0.133*
	(0.073)	(0.073)	(0.074)
Control mean pre	7.43	7.44	7.44
Bandwidth	1315	1335	1294
Observations	1790	1818	1766
	[C] Dependent variable: New citizenships to foreigners		
Treatment × Post	-1.249	-1.269	-0.955
	(1.357)	(1.308)	(1.441)
Control mean pre	10.42	10.55	10.22
Bandwidth	1150	1279	1021
Observations	1572	1750	1370
	[D] Dependent variable: No. cohabiting couples		
Treatment × Post	0.059	0.094	0.044
	(0.094)	(0.098)	(0.090)
Control mean pre	1.93	1.94	1.92
Bandwidth	1609	1526	1691
Observations	2214	2088	2344
	[E] Dependent variable: Avg no. of family components		
Treatment × Post	-0.002	-0.002	-0.002
	(0.005)	(0.005)	(0.005)
Control mean pre	2.45	2.45	2.45
Bandwidth	1286	1289	1284
Observations	1754	1758	1752

Notes. The table shows difference-in-discontinuities results for demographic characteristics of municipalities: the female population share (panel A), the number of immigrants per 1,000 residents (panel B), the number of new citizenships to foreigners (panel C), the number of cohabiting couples (panel D), and the average number of family components (panel E). Columns 1-3 show estimates from local linear regressions, obtained after restricting the sample to municipalities within the Calonico et al. (2014) optimal bandwidth, where column 1 uses the average bandwidth between the one computed in the pre-period (column 2) and the one computed in the post-period (column 3). Robust standard errors, clustered at the municipal level, in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.



(A) Population density, 2013 and 2014

(B) Density difference, post-pre treatment

Figure A.2: McCrary test

Notes. Panel A shows the density of population size in 2013 and 2014. Panel B plots the density difference between 2014 and 2013. The solid lines are local linear regressions fit on both sides of the threshold. Dashed lines are 95 percent confidence intervals.

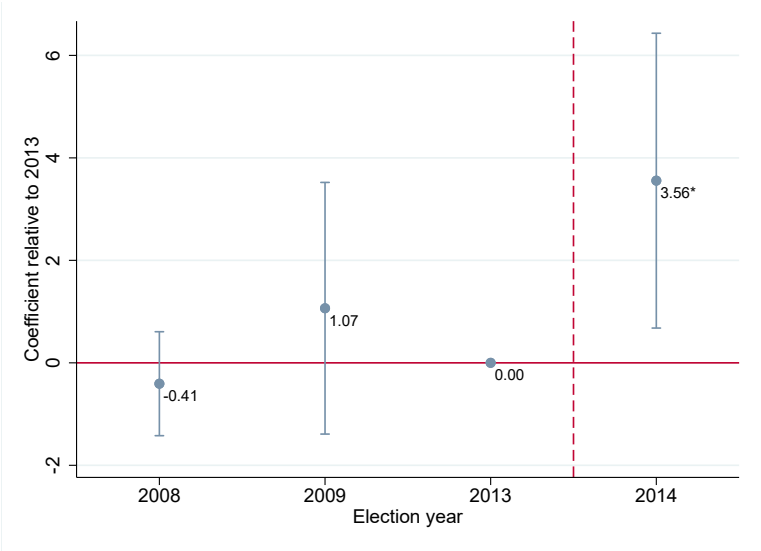


Figure A.3: Dynamic difference-in-discontinuities estimates

Notes. The figure reports dynamic difference-in-discontinuities estimates, obtained by regressing electoral turnout on a treatment dummy, normalized population size, year dummies and their full set of interactions. The figure shows the coefficients on the interaction between treatment and year dummies. The sample includes municipalities within the Calonico et al. (2014) optimal bandwidth, computed by averaging year-specific bandwidths. The horizontal axis reports election years, which refer to 2008 and 2013 Parliamentary elections, and 2009 and 2014 European election. Vertical lines are 95 percent confidence intervals, obtained from cluster-robust standard errors at the municipal-level.

B Robustness checks

We report here robustness exercises on the definition of the outcomes and other standard checks in the context of difference-in-discontinuities designs.

Alternative Definition of Populism Our definition of right-wing populist parties arbitrarily comprises the Northern League and Brothers of Italy, and we report separately results for the Five Star Movement, the other populist political formation. These are the three biggest populist parties in Italy, but we verify in this section that our choice of parties does not influence our results. Thus, we report an alternative classification of populist parties based on a synthetic index, which measures the ideology of all parties running in 2014 European elections. To this end, we use the scores reported in Norris and Inglehart (2019) coming from the 2014 Chapel Hill Expert Survey, which ranks the position of 268 parties according to the judgements of 337 political scientists. The Italian parties covered by the study are 13. The answers of experts are converted into a 0-100 scale for each party on two main aspects of populism: (1) anti-elite (Antielite_p) ideology, defined as one that believes in a contrast in society between the people and the elite—generally believed to be corrupt; (2) authoritarian or nationalist (Authoritarian_p) ideology, defined as one that believes in a hierarchical society where those who break the law should be punished (Albanese et al., 2022).

Figure B.1 reports the scores. The three parties with the largest populist scores are, in fact, the Northern League, Brothers of Italy and the Five Star Movement, with large differences between the two components of the populist score. For example, while the Five Star Movement has a strong anti-establishment component, the Northern League and Brothers of Italy have much higher authoritarian scores.[ii] We map the party scores into municipalities multiplying them by the share of votes of party p in each election round and summing them up for each municipality.[iii]

We therefore estimate the difference-in-discontinuities (equation 1) using these scores as

[ii] We impute the scores to other parties, too. The imputation is done on a case-by-case basis and involves in most cases parties that, in at least one of the two elections, run in the same coalition or were forming the same party (which ended up splitting in subsequent elections) with those reported in Norris and Inglehart (2019). For a comprehensive list of party scores, see Table B.1.

[iii] In other terms, we construct the following quantities:

$$\text{Score}_{it} = \sum_p \text{Share}_{itp} \times \text{Score}_p,$$

where $\text{Score}_p = \{\text{Antielite}_p, \text{Authoritarian}_p\}$ and Share_{itp} is the vote share accruing to party p over total valid votes in municipality i and election year t.

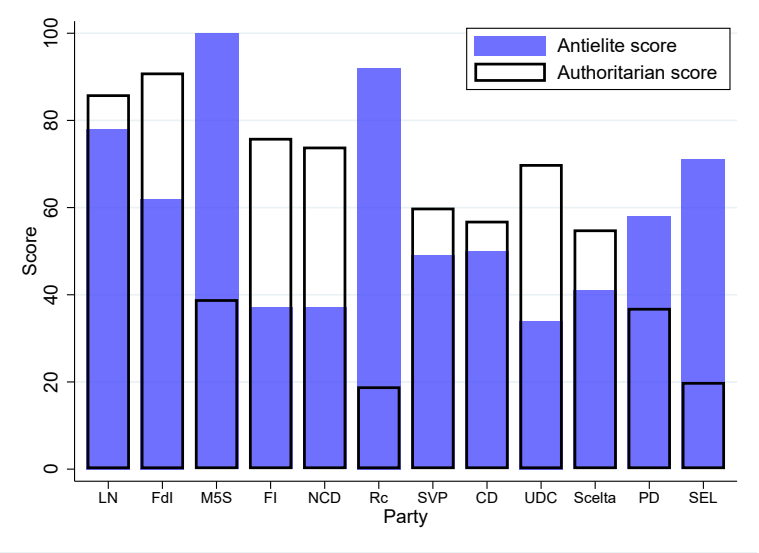


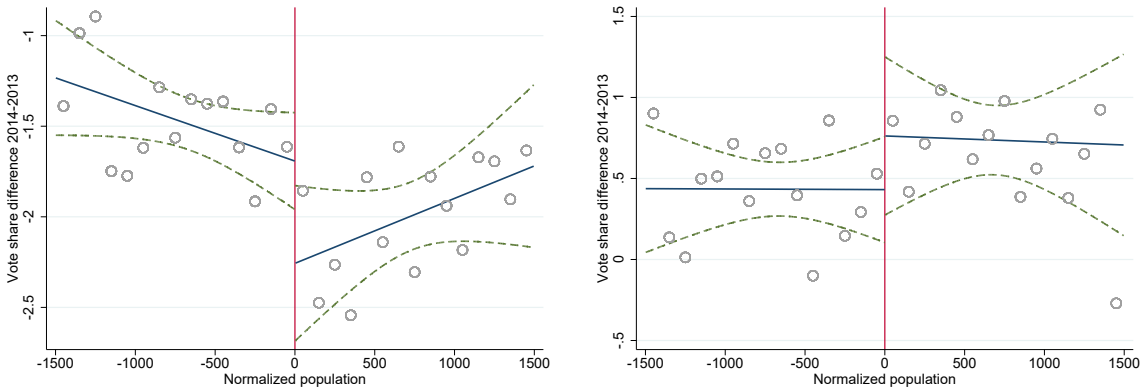
Figure B.1: Populist, anti-elite and authoritarian scores from Norris and Inglehart (2019)

Notes. The figure reports scores for *anti-elite* and *authoritarian* ideologies for Italian parties running at European elections in 2014. Parties: LN, *Lega Nord*; FdI, *Fratelli d'Italia*; M5S, *Movimento 5 Stelle*; FI, *Forza Italia*; NCD, *Nuovo Centro-Destra*; Rc, *Rivoluzione civile*; SVP, *Südtiroler Volkspartei*; CD, *Centro Democratico*; UDC, *Unione di Centro*; Scelta, *Scelta civica*; PD, *Partito Democratico*; SEL, *Sinistra Ecologia e Libertà*.

outcome variables. The results are reported in Table B.2 and Figure B.2. Panel A shows that fiscal rules heterogeneously affected the two populist scores. Concretely, we find evidence suggesting that the restrictions increased the vote share of parties ranking high on the "authoritarian" metric, while having a negative effect on the vote share of parties with a large "anti-elite" score, though the estimate for the latter effect is not statistically significant.

Estimates including covariates We investigate whether the inclusion of covariates in equation (1) affects our main results. Difference-in-discontinuities estimates, conditional on the inclusion of covariates (the female population share, the number of immigrants per 1,000 residents, the number of new citizenships to foreigners, the number of cohabiting couples, and the average family dimension) and province fixed effects, are reported in Table B.3, and broadly confirm our results, reassuring on their robustness and, indirectly, on the validity of our research design.

Non-parametric estimates We replicate our results using a non-parametric approach. To this end, we take the first difference of each outcome and estimate non-parametric regression discontinuities, by computing the difference in intercepts of two local linear estimators, fitted on first-differenced outcomes on both sides of the threshold (Calonico et al., 2014; Hahn et al., 2001). Results are shown in Table B.4, which reports conventional estimates with conventional



(A) Authoritarian score

(B) Antielite score

Figure B.2: Difference-in-discontinuities for Norris and Inglehart (2019) ideology scores

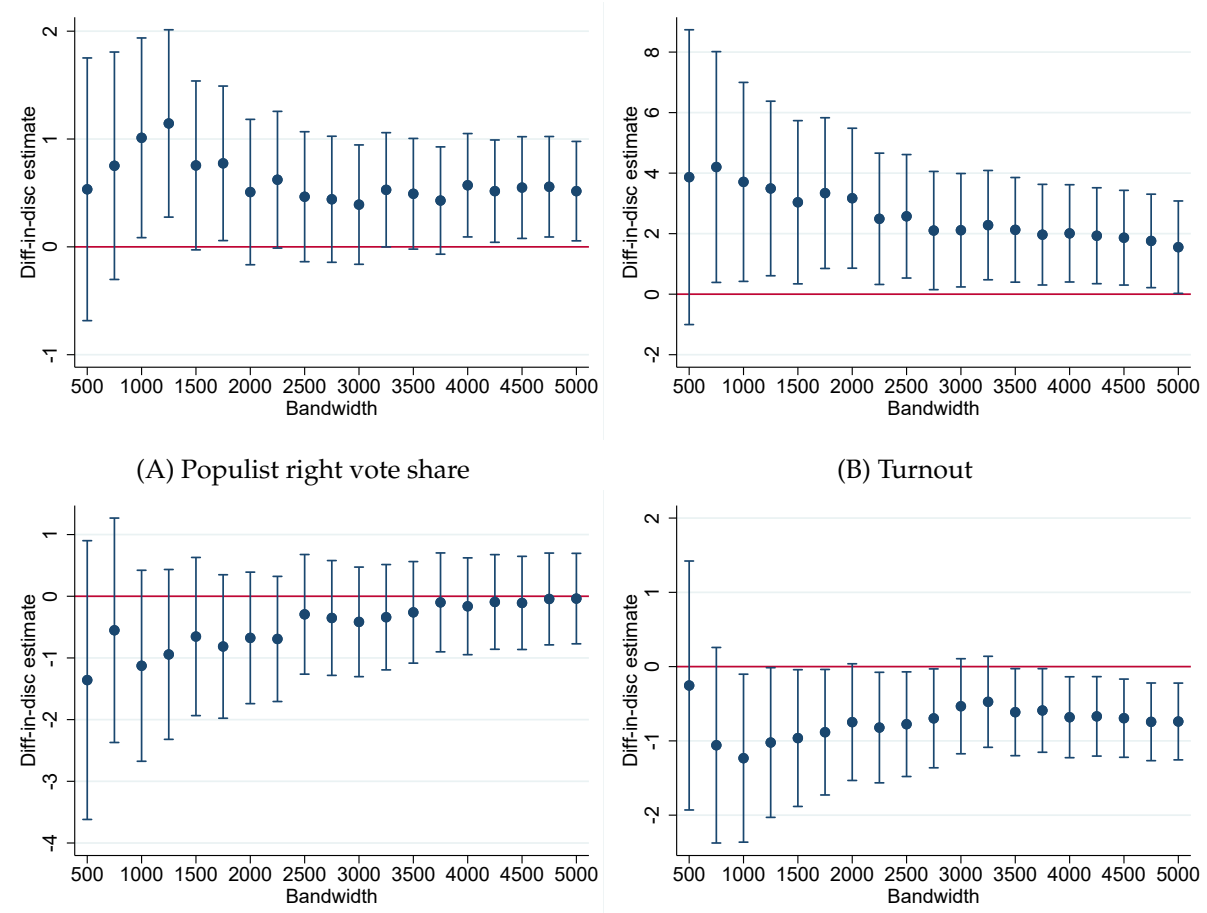
Notes. The figure plots binned averages of differences in electoral results for Norris and Inglehart (2019) scores against normalized population size. Panels A and B report results for the authoritarian and anti-elite scores, respectively. The size of each bin is 100 residents. The central lines are local linear regressions fit on both sides of the threshold. Dashed lines are 95 percent confidence intervals. The numerical coefficient estimates for the treatment effects illustrated here are given in Table B.2.

standard errors in column 1, bias-corrected estimates with conventional standard errors in column 2, and bias-corrected estimates with robust standard errors in column 3.[iv] The table shows that the magnitude, significance and sign of non-parametric estimates are very similar to those of parametric estimates reported in the main text.

Robustness to alternative bandwidths We verify that our estimates are not affected by the chosen bandwidth. Figure B.3 reports difference-in-discontinuities estimates for each outcome from local linear regressions estimated at various bandwidths from 500 to 5,000, with each point increasing the bandwidth by 100 residents. The estimates for electoral turnout in panel B are statistically significant at all bandwidths, but slightly larger in magnitude at smaller bandwidths. Albeit noisier at smaller bandwidths, we reach broadly similar conclusions when inspecting panels A and D, which report results for the populist right-wing parties and the M5S. Estimates for the incumbent party (panel C) are not statistically significant at any bandwidth value.

Placebo estimates As a final robustness check, we compare our main estimates with a distribution of 1,000 placebos. Each placebo estimate is obtained by permuting the thresholds

[iv] The bias correction, introduced by Calonico et al. (2014), takes into account the fact that standard non-parametric estimators (e.g., cross-validation or asymptotic MSE minimization) usually lead to "large" bandwidths when performing local distributional approximations. The bias correction recenters the t-statistic with an estimate of the leading bias. "Robust" standard errors take into account the additional variability induced by the bias correction.



(A) Populist right vote share

(B) Turnout

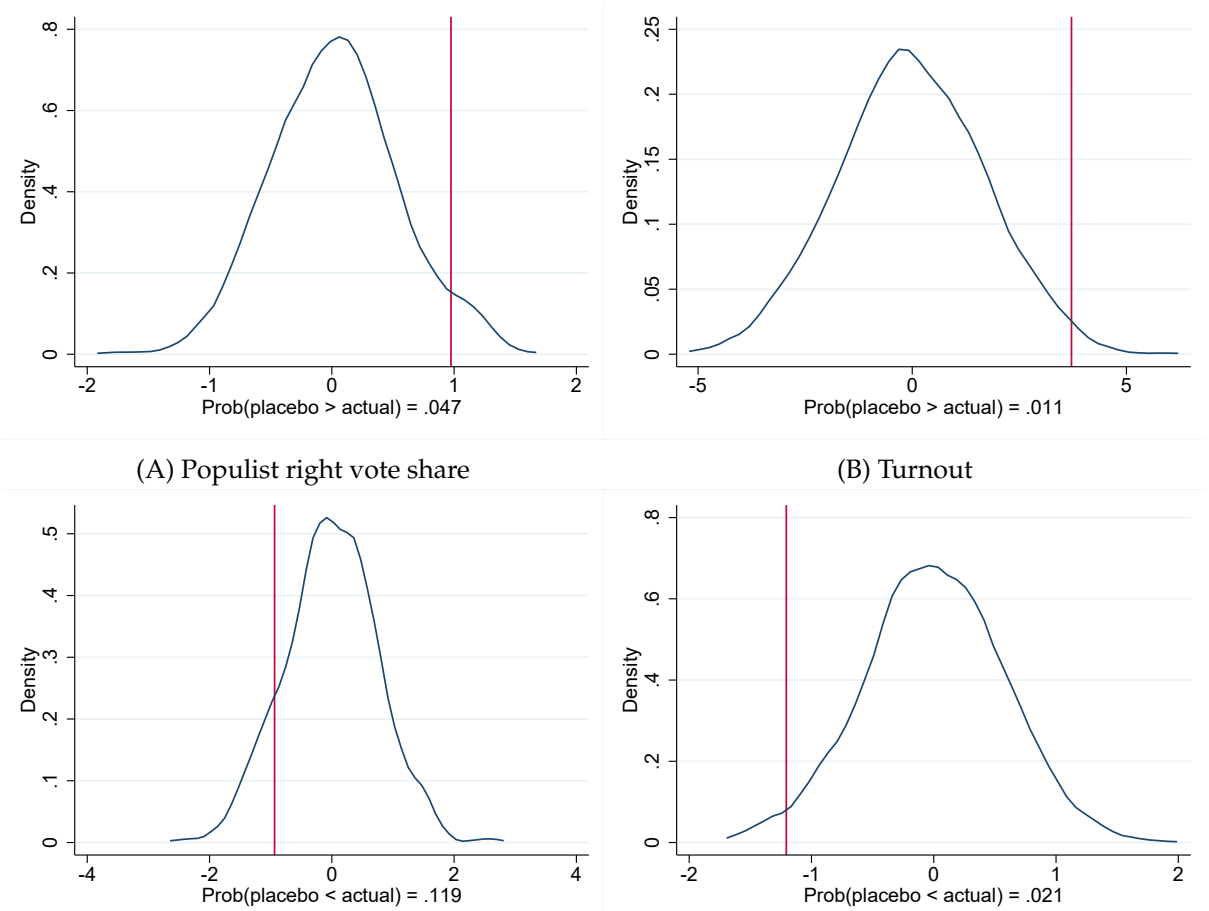
(C) Incumbent vote share

(D) M5S vote share

Figure B.3: Difference-in-discontinuities at different bandwidths

Notes. The figure plots coefficients of local linear regressions at different bandwidths around the 5,000 cut-off. Each dot reports the difference-in-discontinuities estimate and the horizontal axis reports the bandwidth. Vertical lines are 95 percent confidence intervals.

randomly across municipalities, computing a "fake" forcing variable $\hat{P}$, which equals the difference between population size of each municipality and the placebo threshold, and estimating the regression discontinuity in each electoral outcome at $\hat{P} = 0$, via local linear regression within the optimal bandwidth $\overline{h}$. The distributions of the placebo estimates are reported in Figure B.4, which also reports the true estimates from the main text. The density of placebos is centered at zero, and the probability of obtaining values that are larger in magnitude than the estimates at the true threshold is below 0.05 for right-wing populists (panel A), turnout (panel B), and the M5S (panel D), confirming our results. We can interpret these p-values as the probability that, under the null hypothesis of no effect of fiscal rules, the estimation bias is large enough to account for the magnitude of the estimated coefficient. The comparison of the placebos with the true estimates appears to exclude such possibility.



(A) Populist right vote share

(B) Turnout

(C) Incumbent vote share

(D) M5S vote share

Figure B.4: Difference-in-discontinuities placebo estimates

Notes. The figure reports the distribution of 1,000 placebo estimates for each electoral outcome, obtained by permuting the thresholds randomly across municipalities, computing a "fake" forcing variable $\hat{P}$, which equals the difference between population size of each municipality and the placebo threshold, and estimating the difference-in-discontinuities in each electoral outcome at $\hat{P} = 0$, via local linear regression within the optimal bandwidth. Vertical lines are the true estimates. The bottom part of each graph reports the probability that placebo estimates are larger in magnitude than true estimates.

Table B.1: Authoritarian and anti-elite scores

Party	Authoritarian	Antielite	Year	Description
Lega Nord	86	78	2013, 2014	Norris and Inglehart (2019)
Fratelli d'Italia (FdI)	91	62	2013, 2014	Norris and Inglehart (2019)
Movimento 5 Stelle (M5S)	39	100	2013, 2014	Norris and Inglehart (2019)
Forza Italia (FI)	76	37	2014	Norris and Inglehart (2019)
Il Popolo della Libertà (PdL)	76	37	2013	Norris and Inglehart (2019)
Rivoluzione Civile	19	92	2013	Norris and Inglehart (2019)
Svp	60	49	2013, 2014	Norris and Inglehart (2019)
Nuovo Centro Destra	74	34	2014	Norris and Inglehart (2019)
Centro Democratico	57	50	2013	Norris and Inglehart (2019)
Unione Di Centro (UDC)	70	34	2013	Norris and Inglehart (2019)
Scelta Civica	55	41	2013	Norris and Inglehart (2019)
Partito Democratico (PD)	37	58	2013, 2014	Norris and Inglehart (2019)
Sinistra Ecologia Libertà (SEL)	20	71	2013	Norris and Inglehart (2019)
Casapound Italia	100	100	2013	Arbitrary
Forza Nuova	100	100	2013	Arbitrary
La Destra	91	62	2013	FdI
Fiamma Tricolore	91	76.5	2013	Fratelli d'Italia
Die Freiheitlichen	86	78	2014	Lega Nord
Indipendenza Veneta	86	78	2013	Lega Nord
Io Amo l'Italia	86	78	2013	Lega Nord
Lg.Veneta Repubblica	86	78	2013	Lega Nord
Veneto Stato	86	78	2013	Lega Nord
Io Cambio - Maie	63.5	42	2014	Mean(Centro Democratico, UDC)
Fare Per Fermare Il Declino	56.5	52	2013	Mean(PD, PdL)
Futuro e Libertà	83.5	49.5	2013	Mean(PdL, FdI)
Riformisti Italiani	28	75	2013	Mean(Rivoluzione Civile, PD)
L'Altra Europa con Tsipras	19.5	81.5	2014	Mean(Rivoluzione Civile, SEL)
Lista Amnistia Giustizia Libertà	37	58	2013	PD
Grande Sud - Mpa	76	37	2013	PdL
Liberali Per L'Italia - Pli	76	37	2013	PdL
Mir - Moderati In Rivoluzione	76	37	2013	PdL
Movimento P.P.A.	76	37	2013	PdL
P.Liberale Italiano	76	37	2008	PdL
Partito Comunista Dei Lavoratori	19	92	2013	Rivoluzone Civile
Partito Di Alternativa Comunista	19	92	2013	Rivoluzone Civile
Scelta Europea	55	41	2014	Scelta Civica
Italia Dei Valori	20	71	2014	SEL
Verdi Europei-Green Italia	20	45.5	2014	SEL
Intesa Popolare	70	34	2013	UDC

Notes. The table shows all parties (except those in special statute regions) that run for Parliamentary election in 2013 and European elections in 2014. Columns labelled *Authoritarian* and *Antielite* report the scores for each party. *Year* indicate the election year in which the party run for election. *Description* reports the source used for the scores: for most parties the score is taken from other parties with similar values or ideologies.

Table B.2: Difference-in-discontinuities estimates of the impact of fiscal rules on Norris and Inglehart (2019) scores

	(1)	(2)	(3)
	[A] Dependent variable: Authoritarian score		
Treatment × Post	0.564**	0.567**	0.669**
	(0.258)	(0.248)	(0.267)
Control mean pre	53.56	53.51	53.61
Bandwidth	1402	1516	1289
Observations	1932	2070	1762
	[B] Dependent variable: Anti-elite score		
Treatment × Post	-0.331	-0.328	-0.337
	(0.300)	(0.299)	(0.300)
Control mean pre	63.97	63.96	63.96
Bandwidth	1343	1348	1338
Observations	1830	1838	1826

Notes. The table shows difference-in-discontinuities estimates. Panels A and B report results for authoritarian and anti-elite scores derived from Norris and Inglehart (2019). Columns 1-3 show estimates from local linear regressions, obtained after restricting the sample to municipalities within the Calonico et al. (2014) optimal bandwidth, where column 1 uses the average bandwidth between the one computed in the pre-period (column 2) and the one computed in the post-period (column 3). Robust standard errors, clustered at the municipal level, in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.3: Difference-in-discontinuities estimates of the impact of fiscal rules on electoral results, covariates included

	(1)	(2)	(3)
	[A] Dependent variable: Populist right vote share		
Treatment × Post	1.009**	0.892**	0.996**
	(0.434)	(0.422)	(0.441)
Control mean pre	10.48	10.49	10.55
Bandwidth	1379	1422	1336
Observations	1898	1958	1818
	[B] Dependent variable: Turnout		
Treatment × Post	3.748**	3.916**	3.523**
	(1.599)	(1.693)	(1.512)
Control mean pre	73.17	72.99	73.24
Bandwidth	1144	1038	1250
Observations	1566	1400	1720
	[C] Dependent variable: Incumbent vote share		
Treatment × Post	-0.996	-1.292*	-0.941
	(0.669)	(0.720)	(0.643)
Control mean pre	52.08	52.22	52.04
Bandwidth	1469	1309	1628
Observations	2016	1790	2238
	[D] Dependent variable: M5S vote share		
Treatment × Post	-1.156**	-1.143**	-1.161**
	(0.570)	(0.565)	(0.569)
Control mean pre	22.65	22.65	22.64
Bandwidth	1094	1102	1086
Observations	1470	1486	1462

Notes. The table shows difference-in-discontinuities estimates for the main outcomes, controlling for the following set of covariates: the female population share, the number of immigrants per 1,000 residents, the number of new citizenships to foreigners, the number of cohabiting couples, the average number of family components, and province dummies. Columns 1-3 show estimates from local linear regressions, obtained after restricting the sample to municipalities within the Calonico et al. (2014) optimal bandwidth, where column 1 uses the average bandwidth between the one computed in the pre-period (column 2) and the one computed in the post-period (column 3). Robust standard errors, clustered at the municipal level, in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.4: Non-parametric difference-in-discontinuities estimates of the impact of fiscal rules on electoral results

	(1)	(2)	(3)
	Conventional	Bias-corrected	Robust
	[A] Dependent variable: Populist right vote share, first difference		
Treatment	0.918**	1.010**	1.010**
	(0.439)	(0.439)	(0.514)
Bandwidth	1329	2080	2080
Observations	906	906	906
	[B] Dependent variable: Turnout, first difference		
Treatment	3.533**	3.953***	3.953**
	(1.429)	(1.429)	(1.673)
Bandwidth	1713	2827	2827
Observations	1187	1187	1187
	[C] Dependent variable: Incumbent vote share, first difference		
Treatment	-0.914	-0.956	-0.956
	(0.796)	(0.796)	(0.969)
Bandwidth	1259	1897	1897
Observations	864	864	864
	[D] Dependent variable: M5S vote share, first difference		
Treatment	-0.915*	-0.987**	-0.987*
	(0.484)	(0.484)	(0.579)
Bandwidth	1741	2690	2690
Observations	1210	1210	1210

Notes. The table shows difference-in-discontinuities non-parametric estimates for the main outcomes. Difference-in-discontinuities estimates are obtained by estimating non-parametric regression discontinuities (RD) on first-differenced outcomes. Column 1 reports conventional RD estimates with conventional variance estimator. Column 2 reports bias-corrected RD estimates with conventional variance estimator. Column 3 reports bias-corrected RD estimates with robust variance estimator. The sample include municipalities within the Calonico et al. (2014) optimal bandwidth. Standard errors in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C Effects of Fiscal Rules on Local Public Finance

In this section, we investigate the effects of local fiscal rules on local public finance. To this end, we use data on balance sheets of Italian municipalities, provided by the Italian Ministry of Interior, which provides information on local public expenditures and revenues. We focus on spending commitments in current and capital account and on revenue accruals. Spending categories are organized in "functions", which we aggregate in six categories: administration, justice and police; education and culture; sport and tourism; roads and environment; social services; economic development and productive services. Revenues are organized in "titles". We consider revenue titles related to tax collection and fees. We do not consider revenues from alienations and credit collection, as they do not have a direct effect on citizens' electoral preferences; revenues from third party services and contributions, that are collected by the municipality but allocated to other levels of government (the central government, the region or the province), and are therefore out of mayors' control; current transfers from supra-local administrations, that are the direct target of fiscal adjustment programs initiated in the aftermath of the Great Recession, and are out of mayors' control, as well. We divide monetary amounts by population in 2011, so to focus on per capita quantities.

Table C.1 reports the difference-in-discontinuities estimates. Panel A reports the effects on revenues from taxes and fees. Panels B and C report the effects for expenditures in the current and capital accounts, respectively. Panel D shows the estimates for the budget deficit, computed as the difference between revenues and the sum of current and capital expenditures. The coefficients have the expected signs: the introduction of fiscal rules increases revenues and decreases expenditures with a net negative effect on the budget deficit. The estimates are noisy, but we do find a significant effect on expenditures in the current account, which decrease by approximately 19.7 euros per capita in our preferred specification in column (1), i.e. by 2.8 percent relative to the control mean. The effects are always larger in magnitude in municipalities constrained by a negative fiscal gap, i.e., those required to undertake austerity measures in order to meet the criteria of the fiscal rules, as shown in Table C.2 (see Section 7 for details on the construction of the fiscal gap indicator). The estimates are again noisy, but we do find a marginally significant difference in the budget deficit coefficient between municipalities with positive and negative fiscal gap, where the deficit increases by 130 euros and decreases by 118 euros, respectively.

Table C.1: Difference-in-discontinuities estimates of the impact of fiscal rules on local public finance

	(1)	(2)	(3)
	[A] Dependent variable: Revenues from taxes and fees		
Treatment × Post	7.22	13.84	8.76
	(9.45)	(10.05)	(8.97)
Control mean pre	455.81	454.32	454.99
Bandwidth	1347	1233	1461
Observations	2481	2289	2685
	[B] Dependent variable: Current expenditures		
Treatment × Post	-19.67*	-22.52**	-17.74*
	(10.53)	(10.91)	(10.44)
Control mean pre	698.65	692.59	696.29
Bandwidth	1035	957	1114
Observations	1875	1740	2013
	[C] Dependent variable: Capital expenditures		
Treatment × Post	-19.75	-19.39	-9.54
	(43.48)	(46.51)	(40.07)
Control mean pre	180.11	178.11	178.69
Bandwidth	1498	1314	1682
Observations	2748	2424	3114
	[D] Dependent variable: Budget deficit		
Treatment × Post	-31.27	-21.89	-35.34
	(48.45)	(44.23)	(52.08)
Control mean pre	422.06	422.57	421.01
Bandwidth	1466	1672	1260
Observations	2697	3087	2340

Notes. The table shows difference-in-discontinuities estimates for local public finance outcomes: revenues from taxes and fees in panel A, expenditures in the current and capital account in panels B and C, and the budget deficit, defined as the difference between revenues and total expenditures, in panel D. Columns 1-3 show estimates from local linear regressions, obtained after restricting the sample to municipalities within the Calonico et al. (2014) optimal bandwidth, where column 1 uses the average bandwidth between the one computed in the pre-period (column 2) and the one computed in the post-period (column 3). Robust standard errors, clustered at the municipal level, in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

We then investigate in Figure C.1 which spending categories were more affected by cuts to cope with the fiscal rules. The figure reports two sets of coefficients, representing the difference-in-discontinuities estimate within the optimal bandwidth, for both the level effect (in terms of euros per capita) and the percent effect (computed by taking the inverse hyperbolic sine of the dependent variable). The figure shows that the effects are negative across all

Table C.2: Difference-in-discontinuities estimates of the impact of fiscal rules on local public finance, heterogeneity by fiscal capacity

	(1)	(2)	(3)	(4)
Dependent variable:	Revenues from taxes and fees	Current expenditures	Capital expenditures	Budget deficit
Treat × Post × Pos. fiscal gap	-0.44	-14.04	101.92	129.84
	(13.99)	(12.80)	(111.40)	(128.88)
Treat × Post × Neg. fiscal gap	8.00	-39.69	-65.77	-118.17
	(15.46)	(27.20)	(77.34)	(77.58)
p-value diff. coeff.	0.685	0.394	0.217	0.100
Bandwidth	1347	1035	1498	1466
Observations	2481	1875	2748	2697

Notes. The table reports local linear difference-in-discontinuities coefficients interacted with dummies for municipalities with positive and negative fiscal gap in columns 1-4 for the main public finance outcomes. The fiscal gap is defined as the difference between the DSP target and the objective target: the former equals the difference between total revenues and expenditures; the latter equals 17 percent of average current expenditures in the period 2006-08 net of cuts in transfers from the central government. The bottom of the table reports the p-value of the equality of coefficients for positive vs. negative fiscal gap. All regressions are run on the sample within the Calonico et al. (2014) optimal bandwidth. Robust standard errors, clustered at the municipal level, in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

spending groups, except for those related to economic development and productive services. They are statistically significant especially for spending in road maintenance and environment (including waste collection), amounting to a negative effect of 9 euros per capita or 5 percent. They are also marginally significant at 90 percent level for sports and tourism and social services (focusing on the percent effect only). We argue that these spending categories bring about visible changes to citizens in terms of a deterioration of public services, even in the very short run, which would therefore explain their increased willingness to participate in elections and vote for radical right parties.

Our results differ from those reported in Grembi et al. (2016) who also find that fiscal rules have a significant effect on the budget deficit of Italian municipalities. They do find, however, that the adjustment mainly happens through the revenue margin rather than on the expenditure side. There are several reasons why our results and theirs may differ. First, we are considering different time periods and business cycle conditions. They focus on the early 2000s, while our analysis is conducted in the aftermath of the double dip recession. Moreover, they study the *relaxation* of fiscal rules in municipalities below 5,000 residents in 2001, while we investigate the effects of the *imposition* of fiscal rules in the same set of municipalities in

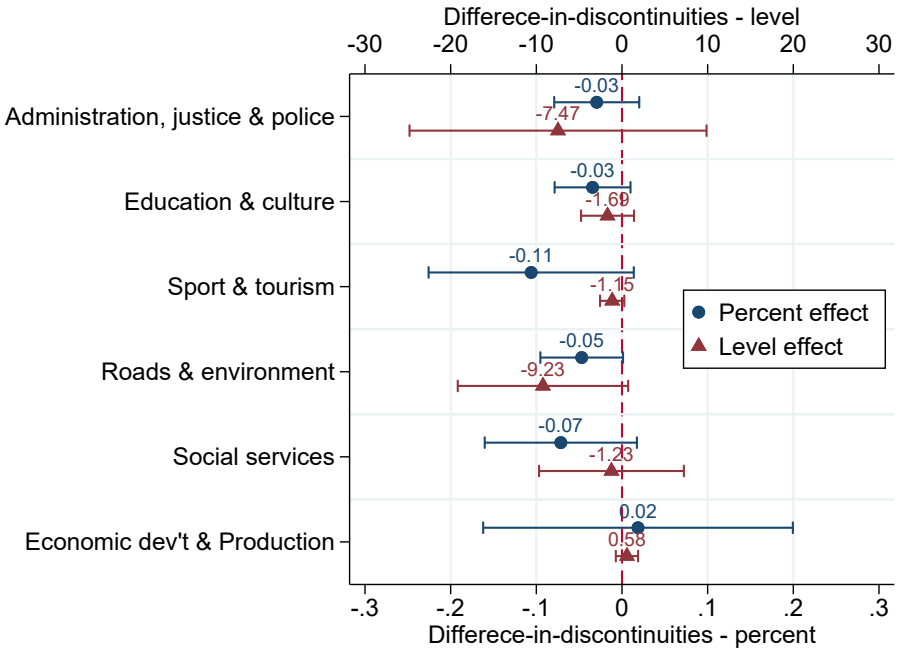


Figure C.1: The effect of local fiscal rules on expenditure categories

Notes. The figure shows difference-in-discontinuities estimates for expenditure categories, reporting both a level effect where the outcome is defined in euros per capita, and a percent effect where the inverse hyperbolic since of the outcome is used as dependent variable. The estimates are obtained from local linear regressions, after restricting the sample to municipalities within the Calonico et al. (2014) optimal bandwidth, computed as the average bandwidth between the one in the pre-period and the one in the post-period. Horizontal lines are 95 percent confidence intervals, from standard errors clustered at the municipal level.

2013. The effects of introducing or removing fiscal rules need not be symmetric in terms of the adjustment to the budget balance, especially in different business cycle conditions. Overall, however, our results are consistent with theirs in that we both find that fiscal rules have an effect on municipalities' budget deficit, which could therefore mediate the electoral effects documented in the main text.

D Effects of Fiscal Rules on Radical-Right versus Radical-Left Parties: Further Discussion

Recall that the core objective of our study has been to contribute to the expanding literature on the roots of radical-right party support by empirically documenting — in a novel causal setting — the effects of a thus far understudied determinant (Guriev and Papaioannou, 2022): austerity in the form of local fiscal rules. Consequently, we have shown above that the 2013 imposition of fiscal rules in Italian municipalities led to a meaningful increase in support for Italy's right-wing parties — an effect driven by voting in municipalities with a negative fiscal gap, where the new set of rules was binding. These findings constitute our main contribution.

Interestingly enough, however, when investigating the fiscal rules' effects on other parties in Section 6.3, we found that the Five Star Movement — Italy's other major populist formation — in fact lost support in the affected municipalities, leading to a natural follow-up question: why might economic policies such as fiscal rules have heterogeneous impacts on the success of radical-right versus other populist parties? While thoroughly addressing this question in a general framework falls outside the scope of our study,[v] we posit that this finding is consistent with the arguments we put forth in Section 3, when providing the theoretical reasons for why we expect Italy's radical-right parties to have benefited from the 2013 policy implementation.

In particular, we first mentioned the LN and FdI's eurosceptic stances, making them appealing outlets for those decrying EU-backed austerity. And while it is indeed true that the M5S also describes itself as eurosceptic (particularly so in recent years), the M5S' stance on key policy issues concerning the EU such as immigration (Salvati, 2019) has been significantly more ambivalent. According to Franzosi et al. (2015), the Movement often differs from its "hard" eurospectic allies and is often closer to pro-EU parties in terms of its voting behavior, suggesting that the M5S' "euroscepticism is more strategic than ideological" (p. 109). We argue that the M5S' "soft" eurosceptic stance weakens its perceived ability to commit to international EU opposition in the eyes of those affected by the restrictions, if not absolutely at least relative to its right-wing populist competitors.

Next, we mentioned that Italy's right-wing populists promoted a strong regionalist message, emphasizing the importance of sub-national control. And indeed — in strong contrast with the M5S, which originated as a national "catch-all" populist party with no core host ide-

[v] Moreover, it is a difficult query to tackle contextually, given our focus on one particular policy at one point in time.

ology (Berlucchi, 2022) — the LN and FdI's policy agendas from their inception have been nationalist — in the case of LN specifically, originally focusing on the economic interests of Italy's Northern region (or 'Pandania') before extending its platform to give a renewed impetus to the debates concerning local power in Italy, and across Europe more generally (see Giordano, 2000 for more details).

Finally, we provided the immigration argument whereby Italy's radical right parties could stand to benefit from the imposition of fiscal rules, because such restrictions — which by design reduce the scope of local governments' spending — could make immigration feel more threatening in the eyes of the affected electorate (e.g., fearing a cut in welfare programs in order to comply with the new framework), leading voters to switch support towards those promising tighter border controls (Facchini and Mayda, 2009; Hainmueller and Hiscox, 2010). This mechanism, once more, provides a possible explanation for why the M5S may not have befitted in a similar manner, as their immigration messaging was again characterized by ambiguity. As explained by Conti (2015), "the [M5S]'s stance on immigration is rather ambivalent and at times not easily decipherable for citizens; *moreover, other radical parties in the Italian party system represent anti-immigration feelings in a more linear way*".

Of course, while the discussion here helps shed some light on the documented effects, it is inconclusive, and further work exploring richer data sources is needed to better disentangle the exact mechanism underlying why radical-right parties may benefit from certain policies relative to their populist competitors. That said, before concluding this section, we do note that these results are not an isolated artifact of the setting at hand, but rather they echo recent findings from the literature — with several studies documenting a heterogeneous response of radical-right and radical-left support to certain policies and economic factors.

In particular, focusing on the effects of economic distress in Sweden, Dehdari (2022) shows that, while layoffs among low-skilled native-born workers increased, on average, the support for the Sweden Democrats (Sweden's radical-right party), they actually triggered a fall in support for the Left Party. While, like us, the author is unable to offer a fully compelling reason for why this heterogeneous effect might materialize, he proposes as a potential explanation the Left Party's favoring of multiculturalism and internationalism — aspects which might dissuade those affected by layoffs-triggered economic hardships. Similarly, investigating this time the effects of economic uncertainty in a panel of 24 EU countries, Gozgor (2022) finds that higher uncertainty increases populist support, an effect driven by a rise in right-wing, more

so than left-wing populist voting behavior. Once more, the precise underlying mechanism behind why this might be is not thoroughly explored. Finally and perhaps most relatedly, in a paper focusing on Italy specifically, Caselli et al. (2020) document two results. First, they show that exposure to globalization proxied by the intensity of import competition from China contributes to the success of far-right, but not far-left parties. Second, their findings suggest that, while immigration intensity does increase support for both types of radical parties, the effects are significantly stronger and more robust to alterations in the statistical model employed when focusing on voting for the far-right.[vi] Again, the driving mechanism behind these heterogeneous impacts is not explored in-depth, likely due to data limitations.

While a significant amount of work certainly remains to be done in the literature to disentangle the determinants of such heterogeneous effects, we posit that our results contribute to our understanding of which type of populism proliferates under austerity — thus adding to the broader literature showing that different policies and economic factors might impact the support for different populist platforms differently. In particular, we rationalize our finding as exemplifying precisely the heterogeneity existing within the populist movement. Indeed, as argued by Bonikowski (2016), populism may be best perceived not as a cohesive ideology, but rather as a discursive framework which may be used to propagate very different platforms. Our results suggest that austerity in the form of fiscal restrictions may influence which parts of this discursive framework garner more electoral support — in this case, documenting a movement towards the more right-wing, eurosceptic, regionalist and anti-immigrant of the Italian parties.

[vi] See, for instance, Table 6 in their paper. When employing an instrumental strategy to remove sources of endogeneity, the effects they document on the far-left become insignificantly different from zero, while the effects on the far-right for both of the variables they consider remain significant and large in magnitude.

E Additional Tables and Figures

Table E.1: Difference-in-discontinuities estimates of the impact of fiscal rules on turnout, municipal-level elections

	(1)	(2)	(3)
	Dependent variable: Turnout in municipal elections		
Treatment × Post	1.580	1.514	1.133
	(2.184)	(2.187)	(2.173)
Control mean pre	72.54	72.58	72.56
Bandwidth	1220	1205	1235
Observations	786	771	793

Notes. The table shows difference-in-discontinuities estimates for electoral turnout at the municipal level. Columns 1-3 show estimates from local linear regressions, obtained after restricting the sample to municipalities within the Calonico et al. (2014) optimal bandwidth, where column 1 uses the average bandwidth between the one computed in the pre-period (column 2) and the one computed in the post-period (column 3). Robust standard errors, clustered at the municipal level, in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E.2: Difference-in-discontinuities estimates of the impact of fiscal rules on the incumbent vote share, alternative definition

	(1)	(2)	(3)
	Dependent variable: Incumbent (PD only) vote share		
Treatment × Post	-0.938	-0.988	-0.888
	(0.643)	(0.679)	(0.612)
Control mean pre	27.95	28.10	28.03
Bandwidth	1400	1261	1539
Observations	1932	1734	2106

Notes. The table shows difference-in-discontinuities estimates for the incumbent vote share, defined as the vote share of the Democratic Party (PD). Columns 1-3 show estimates from local linear regressions, obtained after restricting the sample to municipalities within the Calonico et al. (2014) optimal bandwidth, where column 1 uses the average bandwidth between the one computed in the pre-period (column 2) and the one computed in the post-period (column 3). Robust standard errors, clustered at the municipal level, in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

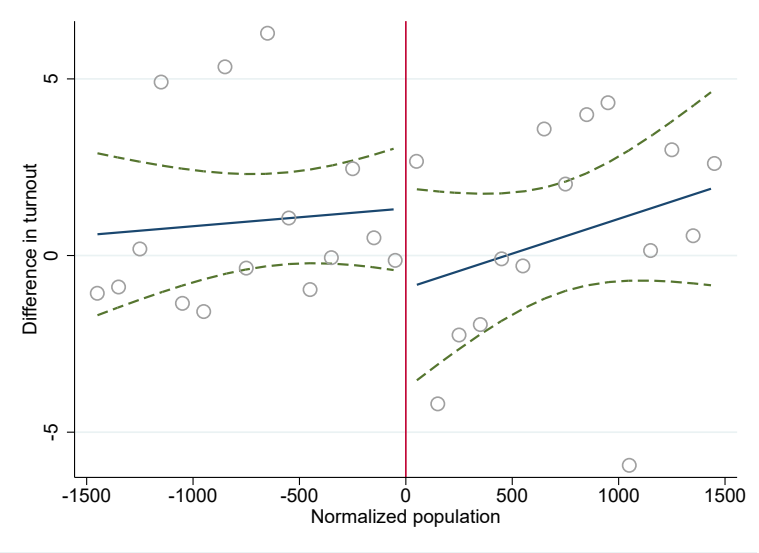


Figure E.1: Difference-in-discontinuities for electoral turnout, municipal-level elections

Notes. The figure plots binned averages of differences in municipal-level electoral turnout between the periods before and after the introduction of local fiscal rules against normalized population size. The sample includes municipalities between 1,000 and 10,000 residents holding local elections in the period 2011-2014. The size of each bin is 100 residents. The solid lines are local linear regressions fit on both sides of the cut-off. Dashed lines are 95 percent confidence intervals. The numerical coefficient estimate for the treatment effect illustrated here is given in Table E.1.