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

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Abstract

We empirically study the effects of austerity on electoral support for radical-right parties. 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 our main difference-in-discontinuities analysis, we show that austerity policies contribute to radical-right voting. These effects are stronger in municipalities where local fiscal rules are binding — that is, where fiscal adjustments are required to comply with the new institutional framework. We also explore the mechanisms underlying our main result, and find suggestive evidence for an anti-immigration channel, whereby austerity increases the appeal of the anti-immigration rhetoric supported by those on the radical-right. Our findings contribute to a broader literature investigating the economic roots of radical-right voting.

Keywords: Austerity, Fiscal Rules, Elections, Radical-Right, Municipalities
JEL codes: D72, H30, H60, H70

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1 Introduction

Rising support for radical-right parties in the Western world is considered one of the most significant changes in the recent political landscape (Colantone and Stanig, 2019). According to Bó et al. (2022), the last two decades have been marked by a strengthening of radical-right parties across countries and electoral systems — particularly in Europe (Dehdari, 2022) — leading to researchers across academic fields taking on the "urgent task" of understanding the proliferation of the radical-right.[1] Moreover, the ascent of radical-right parties in the aftermath of the 2008 Great Recession (Guriev and Papaioannou, 2022) prompted inquiries into the mechanisms through which the crisis may have favored their success.

Here, whilst a host of potential explanations behind the radical-right's success have been investigated,[2] certain factors have so far been explored much less in-depth. More concretely, as pointed out by Baccini and Sattler (2023), recent economic explanations for this phenomenon have "paid surprisingly little attention to governments and their policy choices" and, in particular, little work exists on the role played by *austerity* implemented after the crisis (Guriev and Papaioannou, 2022), despite austerity being widely carried out in the aftermath of the 2008 recession (Alesina et al., 2019).[3]

From a statistical perspective, the slow progress in this field is unsurprising, seeing that empirically assessing whether austerity *causally* triggers an increase in support for those on the radical-right is a challenging task, plagued by a host of endogeneity issues. For example, since austerity policies are often implemented in economic downturns — such as the period following the 2008 crisis — it is difficult to ascertain whether any subsequent proliferation of the radical-right is a result of said austerity, rather than a consequence of economic distress more broadly.[4] Such difficulties may partially explain why the extant scholarship "has largely overlooked the role of governments" (Baccini and Sattler, 2023) in the proliferation of the radical-right.

[1] 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/`.

[2] Ranging 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)

[3] 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; 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. (2023), who use regional level variation to show that fiscal consolidation led to increases in extreme parties' vote shares.

[4] In Section 2, we discuss these statistical challenges more in-depth.

In this paper, addressing these challenges, we add to the empirical literature on austerity and radical-right support by focusing on one type of austerity policy, namely *numerical fiscal rules*,[5] which despite their widespread use (Larch et al., 2021), have received little attention in terms of their electoral consequences.

We make two contributions to this field. Most importantly, we investigate whether austerity causally triggers an increase in support for radical-right parties. From a theoretical perspective, the existing literature proposes two mechanisms by which those on the radical-right may benefit from the imposition of austerity. On the one hand, there is "anti-establishment" channel, whereby radical-right forces capitalize on austerity by blaming mainstream parties for its societal costs. Then, as resentment builds, those on the radical-right become an increasingly appealing outlet for those dissatisfied with mainstream parties, that are seen as unable to protect the losers of structural economic change (see e.g., Baccini and Sattler, 2023). On the other hand, there is an "anti-immigration" channel, whereby the imposition of policy restrictions reduces the scope of public programs governments can provide and, therefore, potential immigrant competition for what are now (perceived to be) scarcer governmental resources becomes more threatening in the eyes of those affected (see e.g., Facchini and Mayda, 2009, Hainmueller and Hiscox, 2010, or early work by Borjas, 1999). As a result, the anti-immigration agenda of those on the radical-right becomes more appealing to those affected by austerity.

In line with these theoretical predictions, we begin by empirically investigating the link between austerity and radical-right support by asking whether a set of local fiscal rules implemented in 2013 led to an increase in the vote share obtained by Italy's major radical-right parties.[6] 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, because these parties adopted hard eurosceptic and anti-immigration stances at the time (Huysseune, 2010; Zappettini and Maccaferri, 2021), making them appealing political formations for those impacted by austerity.

Following 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

[5] Defined as laws meant to constrain fiscal policy (Grembi et al., 2016) — such as limits on government debt or deficits (Schaechter et al., 2012).

[6] As detailed below, this set of fiscal rules is particularly appealing for our investigation because it allows us to circumvent a host of endogeneity issues that generally plague studies in this field. This is our main justification for homing in on this policy.

target, resulting in sanctions if the target was not met. This policy change allows us to circumvent endogeneity concerns, 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),[7] 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, adding credible causal evidence to what has been explicitly signaled as a shortcoming of the existing scholarship (Baccini and Sattler, 2023).

Building on this main result, in the second part of our analysis, we explore the political mechanisms underlying our findings, by asking *why* those on the radical-right benefited from the imposition of austerity (in our setting). Exploiting several appealing contextual features — chiefly the presence in Italian politics at the time of another non-mainstream party, the Five Star Movement [M5S], which promoted a similar anti-establishment messaging to the LN and FdI, but which differed in terms of their discourse on immigration — we find little evidence for the aforementioned anti-establishment channel explaining our results. Unlike the two radical-right parties studied in our main analysis, the M5S actually *lost* support in municipalities affected by austerity, a result inconsistent with the hypothesis whereby revenge against the mainstream elites is the primary driver of austerity's effects.[8]

Conversely, we find some evidence in support of the *anti-immigration mechanism*, by looking at local public spending information. Using data on the balance sheets of municipalities, 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 (Drazen and Eslava, 2010), leading to notable changes even in the short-run. We posit that the now scarcer resources increase the perceived competition for public

[7] 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.

[8] Further corroborating this point, we also show that austerity did not lead to an increase in the "anti-elite" index developed by Norris and Inglehart (2019).

services between natives and immigrants, justifying the electorate's willingness to participate in elections and vote for anti-immigrant parties on the radical-right. While acknowledging that further work is required on precisely breaking down the mechanism linking austerity and radical-right support, we posit that our results add a piece of evidence to a literature where establishing the underlying mechanism has proven a challenging task.[9]

Lastly, 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 — 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 is larger in magnitude in the subsample of municipalities where the fiscal rules bind.

In summary, our results provide novel evidence for European austerity causally leading to increased support for radical-right parties, and suggest that an anti-immigration, rather than an anti-establishment channel, might be a key driver. In our paper's conclusion, we discuss the most relevant policy implications, as well as our article's limitations.

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. On the other hand, the Colombian policy instead created

[9]For instance, Baccini and Sattler (2023) mention, when discussing their results showing that the radical right benefits from austerity in economically vulnerable regions, that their investigation does not adjudicate between different possible underlying mechanisms. Hence, we argue that, whilst indeed tentative in nature, our evidence suggesting that resource competition and an anti-immigration mechanism, rather than an anti-establishment channel, may be a driver underlying austerity's effects does help us better disentangle the nuances underpinning this so-far understudied link between austerity and the proliferation of the radical-right.

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 political effects of restrictions by asking how support for the *local* incumbent party changes after the rules are imposed. In contrast, 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 two other closely related studies, Gabriel et al. (2023) and Baccini and Sattler (2023) 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 non-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, and investigate two possible underlying mechanisms that could explain our results. Furthermore, the identification strategy employed differs.

Finally, our work contributes to an expanding literature on the political effects of austerity more broadly, with studies such as Hübscher et al. (2021) employing survey experiments to show that the implementation of austerity hurts incumbent parties, and Bojar et al. (2022) finding evidence that this effect is contingent on the health of the economy, with a stronger penalty imposed in periods of rising unemployment and high protest intensity. Additionally, Hübscher et al. (2023), in a macro-analysis, recently documented a link between austerity and political polarization, with small radical parties benefiting the most.

2 Theory and Hypotheses: Austerity and the Proliferation of the Radical-Right

We begin by asking whether austerity increases 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 radical-right support should largely be seen as a cultural backlash against multiculturalism and loss of national iden-

tity (e.g., Norris and Inglehart, 2019). According to this view, recent socio-political and economic shifts such as the 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 groups, which are often over-represented among radical-right supporters (Bó et al., 2022).

On the other hand, more in line with our study, an emerging literature focuses on the economic roots of radical-right support (e.g., Dehdari, 2022; Fetzer, 2019; Altomonte et al., 2019). Here, scholars posit that voting for those on the radical-right (Bó et al., 2022) may be interpreted as a signal of protest (Aron and Superti, 2022) caused by existing economic grievances. A theoretical connection between austerity and support for radical-right parties then emerges, and the literature proposes two key mechanisms linking austerity to the proliferation of the radical-right.

First, as explained by Guriev and Papaioannou (2022), those on the radical-right 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 (as in the present context) — radical-right parties have an easier time blaming the international elites and those mainstream parties collaborating with them. We term this channel the "*anti-establishment*" mechanism.

The second mechanism draws upon the literature on the link between immigration and radical-right support.[10] Here, a leading hypothesis is that native-born workers, particularly those low-skilled, are likely to turn their support towards the radical-right 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;

[10]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.)

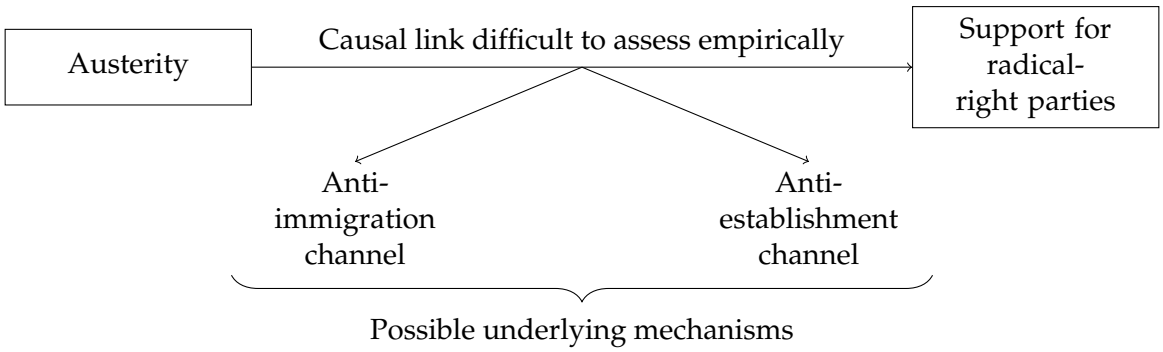


Figure 1: The theoretical link between austerity and radical-right support

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[11] — 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. We call this the "*anti-immigration*" mechanism.

Summarizing the discussion here, the diagram in Figure 1 provides a graphical representation of the theoretical link between the imposition of austerity and the proliferation of radical-right support.

That said, despite this appealing theoretical prediction, empirically assessing whether those on the radical-right do indeed benefit from austerity is a challenging task, in light of severe endogeneity issues plaguing most statistical analyses. After all, austerity was generally implemented in times of underlying economic distress. Therefore, even if one were to find a positive relationship between austerity episodes in a country and support for the radical-right, one would be unable to tell whether it was the austerity itself having the effect, rather than the preexisting economic distress.

For reasons such as this, many studies acknowledge that internal validity is a significant challenge in the literature, and therefore researchers often acknowledge that most identification strategies require strong assumptions to produce consistent estimates of the effects of austerity. For example, Baccini and Sattler (2023) write that the "*assumptions supporting [their] identification strategy are more demanding than they would be in a case study with a single episode of*

[11] An effect which we document contextually in Section 5.2, where we show that 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.

austerity, which varies sub-nationally. We trade-off stronger identification assumptions for a stronger external validity."

In our paper, it is here where we make our main contribution, as the Italian context we investigate provides an appealing setting for assessing the *causal* empirical link between austerity and support for the radical-right. First, in terms of operationalizing our outcome variable, we argue that the radical-right parties in Italy, LN and FdI, were in a particularly favorable position to benefit from the introduction of austerity for the aforementioned reasons — 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, and both parties, chiefly the LN, were in support of tight border controls, putting forth a clear anti-immigration stance in the investigated time-period.

Second, we operationalize our treatment of interest — austerity — as the set of fiscal rules imposed in Italy at the time. As further detailed below, this policy change allows us to circumvent the aforementioned endogeneity concerns, because these rules heterogeneously impacted Italian municipalities depending on their population size — with a clear cutoff in implementation occurring at a population size of 5,000 residents. This leads to our paper's core hypothesis:

H1: *Austerity policies increase support for the radical-right in the affected electorate. Contextually, Italy's radical-right parties are expected to benefit from the 2013 imposition of municipal fiscal rules.*

3 Institutional Context and Data

3.1 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,[12] 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, in line with the literature (Grembi et al., 2016), we use a difference-in-discontinuities strategy to measure the reform effects.

3.2 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.[13] 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 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

[12] 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.

[13] See Appendix A.

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 the vote shares of Italy's radical-right parties, LN and FdI. Additionally, for one of our mechanism 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.[14] Finally, we also measure the effects on 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.

3.3 Anti-elite score

To investigate potential mechanisms, we also measure parties' ideologies. 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 judgments of 337 political scientists. The Italian parties covered by the study are 13 — mostly, those running in the 2014 European elections. The answers of experts are converted into a 0-100 scale for each party, and we focus on the anti-elite (Antielite_p) or anti-establishment stance, defined as believing in a contrast in society between the people and the elite — generally believed to be corrupt (Albanese et al., 2022).

Figure D.1 reports the scores for each party. The Five Star Movement has a high score, followed by the minor party *Rivoluzione civile* and the Northern League.[15] 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.[16] We then use the municipality-level score

[14] 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.

[15] 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 D.1.

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

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

as outcome variable in our analyses.

3.4 Balance Sheet Data

To further elucidate on the mechanisms behind our results, we use data on balance sheets of Italian municipalities, collected 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.[17] We divide monetary amounts by population in 2011, so to focus on per capita quantities.

3.5 Sample Restrictions and Descriptive Statistics

We restrict our sample to municipalities with population between 1,000 and 10,000 (population size is taken from the 2011 census, available through the Italian National Statistical Institute). We also exclude municipalities in special statute regions,[18] 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 municipalities observed over the two rounds of elections in 2013 and 2014.

For this subsample, Figure 2 shows the average vote shares for each party group or for anti-

where Share_{itp} is the vote share accruing to party p over total valid votes in municipality i and election year t.

[17] 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.

[18] 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.

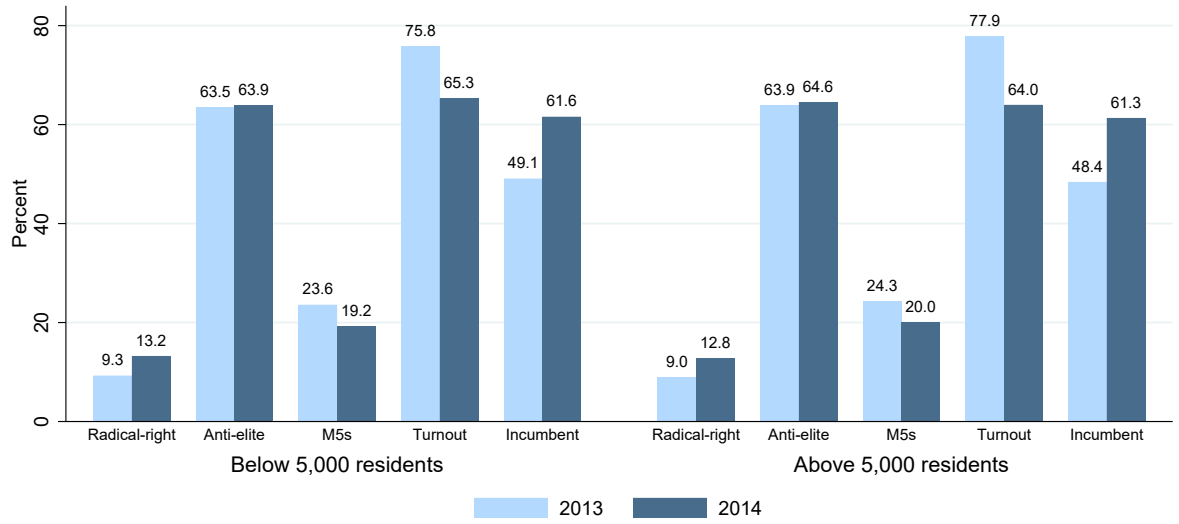


Figure 2: Electoral outcomes in 2013 and 2014

Notes. The figure reports party vote shares and turnout in the Parliamentary elections held in 2013 and in the European elections held in 2014 in municipalities below and above 5,000 residents.

elite parties and the electoral turnout in both election rounds in treated (below 5,000 residents) and control (above 5,000 residents) municipalities. Focusing on parties, the incumbents gained the most votes between the general and European elections, followed by radical-right parties, in both groups of cities. In contrast, the M5S lost consent. In addition, 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.

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 austerity and radical-right support.

4 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, conventional regression discontinuity designs are unable to credibly recover causal estimates of the policy of interest's effects. In our case, two competing policies also change discontinuously at the 5,000 cut-off: (i) the wage of the mayor increases (Gagliarducci and Nannicini, 2013) and (ii) in 2011 transfer cuts from the central government were

reduced to municipalities above 5,000 residents.[19] 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 time-invariant 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.

[19] 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.

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,[20] alongside 95 percent confidence intervals. We report in Appendix A validity tests for the difference-in-discontinuities design.

5 Results on the Electoral Effects of Fiscal Rules

In this section, we empirically test the main hypothesis discussed in section 2, linking exposure to austerity with the proliferation of support for those on the radical-right, illustrating our main causal results from the difference-in-discontinuities estimates. In Appendix B, we discuss their robustness to a number of sensitivity checks.

5.1 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 3. 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. The estimate remains largely unaffected by using alternative bandwidths in columns 2 and 3, and in Figure B.1.

All in all, our findings add to the literature on the economic roots of populism by providing rigorous evidence showing that austerity policies lead to a causal increase in support for those on the radical right in the affected electorate — thus corroborating **H1**. This novel finding —

[20] 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).

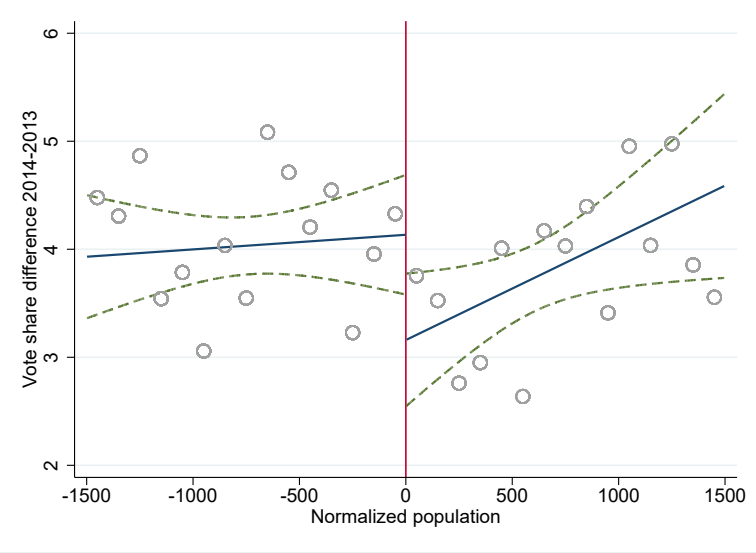


Figure 3: The effect of local fiscal rules on the vote share of radical-right parties

Notes. The figure plots binned averages of radical-right 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.

our paper's main contribution — adds credible causal evidence to a theoretically-rich literature in political science on the link between austerity and support for non-mainstream parties which struggles empirically on account of widespread issues of endogeneity (see e.g., Baccini and Sattler, 2023; Guriev and Papaioannou, 2022).

5.2 Assessing the Underlying Mechanism: Anti-establishment or Anti-immigration?

Moving on, while empirically assessing the overall causal relationship between austerity and radical-right support represents our paper's main contribution to the literature, the setting at hand is also appealing because it permits us to tentatively make some progress on disentangling the mechanisms underlying this link. As shown in Figure 1 above, one explanation proposed by the literature is that of austerity exacerbating anti-establishment sentiments among those affected, thus triggering an increase in support for the forces positioning themselves as opposing mainstream parties. We argue that this is pertinent in the setting at hand, as the 2013 set of fiscal rules were widely perceived as 'imposed' from the center at the EU's request.

Conveniently enough, the Italian setting allows us to make some progress towards testing this "anti-establishment" mechanism because of the presence in at-the-time Italian politics of another non-mainstream party, the M5S. Similarly to the LN and the FdI, the M5S adopted a eurosceptic anti-establishment stance, positioning themselves in opposition to the mainstream

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

	(1)	(2)	(3)
	Dependent variable: Radical 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

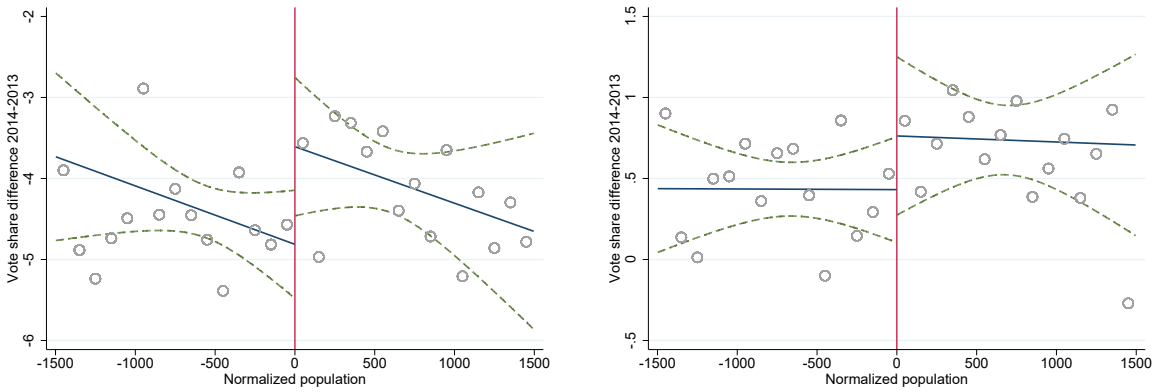
Notes. The table shows difference-in-discontinuities estimates for the effect of fiscal rules on the vote share of radical-right parties. Columns 1-3 show estimates from local linear regressions, obtained after restricting the sample to municipalities within the Calonico et al. (2014) optimal bandwidth, 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

governing parties, but with a significantly less staunch stance on immigration.[21] In addition, importantly, the M5S was the largest non-mainstream Italian party at the time — as seen in Figure 2 above, the M5S enjoyed roughly twice as much support (in terms of vote shares) than the two radical-right parties combined. Moreover, as shown in Figure D.1 in the Appendix, the M5S scored the highest among Italy's major parties in the "anti-elite" score developed by Norris and Inglehart (2019).

Therefore, all in all, if indeed the effects of austerity are chiefly driven by an anti-establishment channel, then we would expect the M5S to also benefit from the imposition of fiscal rules, given their size, their eurosceptic agenda, and their strong "anti-elite" characterization. Moreover, as further detailed below, we can also look at Norris and Inglehart's anti-elite measure directly as an outcome variable. Once more, if the impacts of austerity chiefly operate via an anti-establishment mechanism, we would expect "anti-elite" sentiments to rise in the municipalities affected by the imposition of restrictions.

Empirically, we run our difference-in-discontinuities regression to assess the causal effect of imposing fiscal rules. The results are illustrated in panel A of Figure 4 and Table 2. Interestingly enough, we find that, unlike the LN or the FdI, the M5S's vote share actually fell in municipalities where fiscal rules were implemented relative to their control counterparts. To this point, the coefficient estimate in our preferred specification suggests that the imposition

[21] A quote by Conti (2015) is informative here. He writes "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*".



(A) M5S vote share

(B) Anti-elite score

Figure 4: The effect of local fiscal rules on the M5S vote share and the anti-elite score

Notes. The figure plots binned averages of the M5S (panel A), and anti-elite (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 2.

of fiscal rules led to a 1.2 percentage points loss in support for the M5S on average, or just over 5 percent relative to the control mean.

Since the M5S was actually the largest non-mainstream party at the time, we argue that this result suggests that an anti-establishment mechanism may not be the chief driver of our results. After all, if affected voters had indeed searched for a political outlet to channel their anti-elite frustration, then one would expect the M5S to benefit just as much, if not more than those on the radical-right. Instead, we observe the opposite.

To further bolster this interpretation, we estimate the difference-in-discontinuities using the anti-elite score share as outcome. The results are reported in panel B of Figure 4 and Table 2. We find evidence suggesting that the restrictions had a negative effect on the vote share of parties with a large "anti-elite" score, though the estimate is not statistically significant.

Overall, our results do not lend themselves to the interpretation that austerity increases radical-right support chiefly by exacerbating anti-establishment sentiments among those affected.[22]

We next consider the second hypothesis illustrated in Figure 1, whereby it is the radical-right parties' anti-immigration policy agenda which allows them to benefit from the imposition of austerity — a policy which increases the (perceived) scarcity of and competitiveness for local welfare services among those affected. As mentioned above, an important difference between

[22]In Appendix C, we discuss this particular result more in-depth, and explain how it fits with the broader literature.

Table 2: Difference-in-discontinuities estimates of the impact of fiscal rules on the M5S vote share and the anti-elite score share

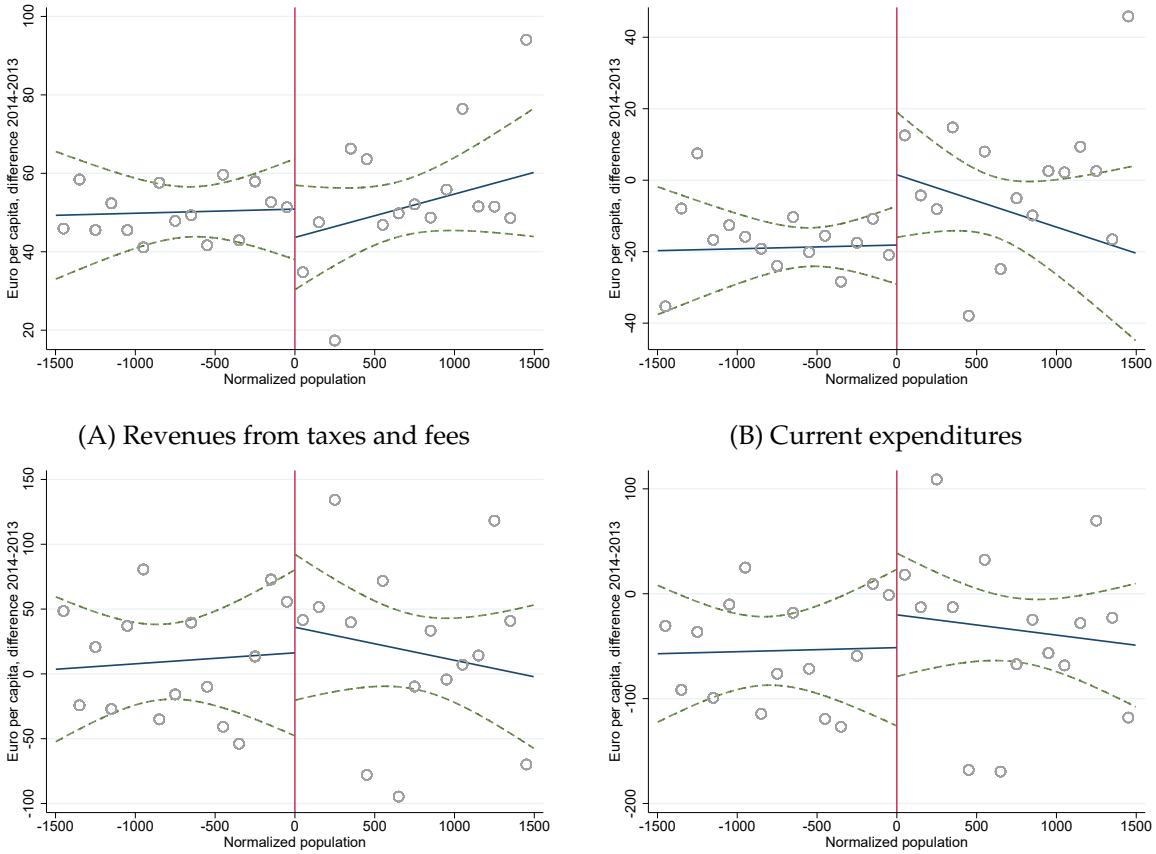
	(1)	(2)	(3)
	[A] Dependent variable: M5S vote share		
Treatment × Post	-1.206**	-1.199**	-1.208**
	(0.553)	(0.548)	(0.552)
Control mean pre	22.65	22.65	22.64
Bandwidth	1094	1102	1086
Observations	1470	1486	1462
	[B] Dependent variable: Anti-elite score share		
Treatment × Post	-0.331	-0.328	-0.337
	(0.300)	(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 for the M5S vote share (panel A) and the anti-elite score share derived from Norris and Inglehart (2019) (panel B). 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

the parties on the radical-right in Italy and the M5S is the former group's significantly stronger anti-immigration stance, with the LN in particular positioning itself as a strong critique of looser border controls at the time. Therefore, the fact that we find those on the radical-right (the M5S) benefiting (being hurt by) the imposition of fiscal rules constitutes an early piece of evidence supporting the anti-immigration mechanism.

To further corroborate this interpretation, ideally, one would directly investigate a municipality-level anti-immigration index, checking whether a jump at the policy discontinuity is observed. As this information is not available for the years we analyze,[23] we tackle

[23]The 2019 Global Party Survey contains a score on anti-immigration views for only 4 Italian parties or party coalitions: the Centre-right (which comprises the Radical-right, but also more moderate right-wing parties), the Centre-left (which includes the Democratic Party), the M5S, and Free and Equals (*Liberi e Uguali*, a party that locates between the Democratic Party and the old Communist Party). Although imprecise, we attempt at using this variable as an outcome. We convert the score into a municipality-level measure using the same approach described above for the anti-elite scores. In doing so, we adjust the anti-immigration score of M5S, which was governing together with the Radical-right in 2019 and has, therefore, a score of 77 out of 100 (where larger values imply more restrictive views on immigration), much closer to the value for the Centre-right (95) than to that of the Centre-left (28). As in 2013-14 M5S had much more liberal views on immigration, we impute the Centre-left score to the M5S. We also use the M5S score divided by 2 as a robustness. The difference-in-discontinuities estimates are reported in Table D.3 in the Appendix for both anti-immigration scores in Panels A and B. The table documents that fiscal rules induce more restrictive views on immigration in the affected municipalities. The effect is statistically significant at 10 percent level in all specifications, and corresponds to approximately 1.3-1.8 percent of the pre-reform average score in the control group.



(A) Revenues from taxes and fees

(B) Current expenditures

(C) Capital expenditures

(D) Budget deficit

Figure 5: The effect of local fiscal rules on local public finance

Notes. The figure plots binned averages of differences in local public finance in euros per capita — revenues from taxes and fees (panel A), expenditures in the current (panel B) and capital (panel C) account, and the budget deficit, defined as the difference between revenues and total expenditures (panel D) — 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 are in Table 3.

the anti-immigration mechanism from a different angle: by investigating the impact of austerity on municipal-level public finance. To this end, we use data on balance sheets of Italian municipalities. Figure 5 and Table 3 report 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.

Table 3: 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 D.2 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 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.[24]

All in all, the main takeaway of our mechanism analyses is the following: while the data do not provide support for austerity exacerbating anti-establishment sentiments among those affected, we are able to retrieve some suggestive evidence in favor of an anti-immigration channel. We see that, of Italy's major non-mainstream parties, it is the anti-immigrant radical-right formations that ultimately benefited from the imposition of restrictions. Fiscal rules led to a statistically significant reduction in local expenditures, thus providing support for the hypothesis wherein an increase in the perceived competition for public resources is an important determinant of austerity's political effects. While, of course, future work should ideally seek to corroborate these links in settings where richer data would allow for a more direct assessment,[25] we posit that they nevertheless provide an important contribution to a literature where investigating the underlying political mechanism has been explicitly highlighted as a prominent shortcoming (Baccini and Sattler, 2023).

[24]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 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.

[25]E.g., in a context where an "immigration sentiment" is more readily available and measurable.

5.3 Supplementary Analyses: Turnout and Incumbent Support

Before concluding this section, we briefly present the results of two complementary analyses we performed. First, in order to assess whether our main results are partially driven by a voter mobilization mechanism, we ask whether the imposition of fiscal rules had an effect on voter participation, building on the literature investigating the relationship between fiscal restrictions and voter turnout (Aaskoven, 2021; Hortala-Vallve and Larcinese, 2017).

Empirically, we run our difference-in-discontinuities regression to assess the causal effect of imposing fiscal rules on turnout. The results from our preferred specification are graphically illustrated in Figure 6 Panel A, with corresponding numerical estimates shown in Table 4 Panel A. 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, suggesting that, indeed, a mobilization effect is at play in the investigated setting.[26]

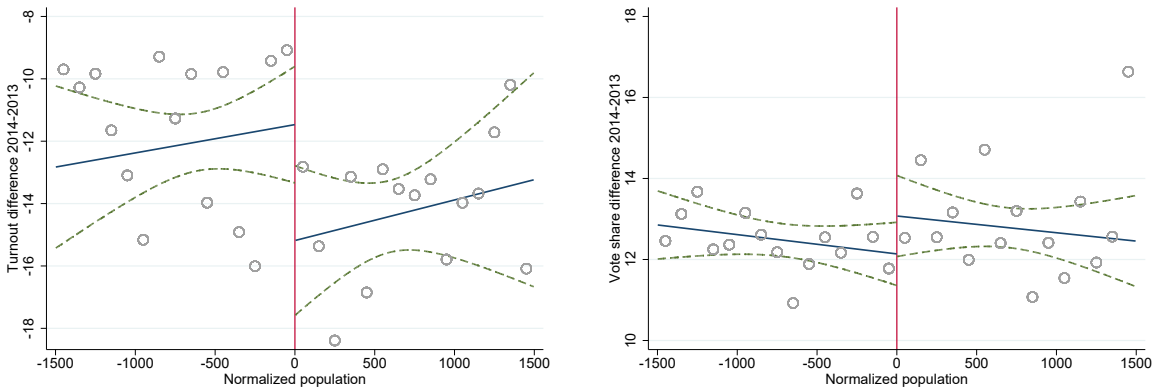
Lastly, building on the literature investigating the electoral effects of austerity (e.g., Talving, 2017, Jacques and Haffert, 2021), we ask whether fiscal rules negatively affect the party controlling the central government. Theoretically, if governmental restrictions lead to frustration among those affected (Colantone and Stanig, 2019), we expect support for the incumbent parties to fall in targeted municipalities.[27]

The difference-in-discontinuities results are illustrated in Figure 6 Panel B. Overall, when considering the entire 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.[28] This null aggregate effect masks an important source of het-

[26] According to existing work, policy restrictions are actually expected to reduce turnout by diminishing the incentive of citizens to acquire information about and take part in the political process (Hortala-Vallve and Larcinese, 2017). However, 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. Conversely, we investigate here whether restrictions imposed on one governmental tier increase the perceived efficacy of politicians elected to other tiers of government *in relative terms*. Our results therefore suggest 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.

[27] 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.

[28] We here define the incumbent party vote share as the sum of the Democratic Party and People of Freedom's



(A) Turnout

(B) Incumbent

Figure 6: The effect of local fiscal rules on turnout and the vote share of the incumbent party

Notes. The figure plots binned averages of turnout (panel A), and the incumbent party vote share (panel B) 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 4.

erogeneity, however, which we investigate more thoroughly in Section 6: 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.

6 Heterogeneity by Fiscal Capacity

So far, we showed that significant differences between 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 effects of austerity on radical-right support. That said, one internal validity 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 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 causal consequences of implementing fiscal rules. As explained above, the Domestic Stability Pact es-

vote shares. In Table D.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.

Table 4: Difference-in-discontinuities estimates of the impact of fiscal rules on electoral turnout and the incumbent vote share

	(1)	(2)	(3)
	[A] 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
	[B] 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

Notes. The table shows difference-in-discontinuities estimates for turnout and the incumbent party (Democratic Party and People of Freedom) vote shares in Panels 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

tablishes 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.[29] 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 sheet quantities in 2012.[30] 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

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

[30]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.

sample to the optimal bandwidth according to Calonico et al. (2014).[31]

Table 5 reports the difference-in-discontinuities coefficients for the two groups of municipalities. The table shows that the increase in support for radical-right 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. We do not detect statistically significant effects for the M5S vote share or the anti-elite score share in either group of municipalities, 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.

Also, we notice that the effects on local public finance outcomes are always larger in magnitude in municipalities constrained by a negative fiscal gap, as shown in Table D.4 in the Appendix. The estimates are 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.

Overall, this evidence provides ground for the interpretation that the increase in support for radical-right parties 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.[32]

[31] 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.

[32] 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 5: 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)	(5)
Dependent variable:	Radical right vote share	M5S vote share	Anti-elite	Turnout	Incumbent vote share
Treat × Post × Pos. fiscal gap	0.259	-1.060	-0.506	3.421	0.465
	(0.622)	(0.848)	(0.459)	(2.462)	(1.113)
Treat × Post × Neg. fiscal gap	1.598**	-0.488	0.773	6.553***	-3.359***
	(0.781)	(0.918)	(0.613)	(2.467)	(1.181)
p-value diff. coeff.	0.180	0.648	0.095	0.369	0.019
Bandwidth	1379	1094	1343	1144	1469
Observations	1898	1470	1830	1566	2016

Notes. The table reports local linear difference-in-discontinuities coefficients interacted with dummies for municipalities with positive and negative fiscal gap in columns 1-5 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

7 Summary and Conclusions

In this paper, we empirically study the causal electoral effects of fiscal austerity. Exploiting the 2013 imposition of fiscal rules in Italy as a natural experiment, we find that support for parties on the radical-right increases in affected areas following the implementation of governmental austerity. Concretely, we show in a difference-in-discontinuities analysis that the imposition of fiscal rules benefited the Northern League and Brothers of Italy, Italy's at the time major radical-right parties. Reassuringly, our analysis also shows that this effect is larger in magnitude in municipalities that were required to implement public expenditure cuts in order to comply with the new institutional framework.

Our findings contribute new rigorous causal evidence to a highly active scholarship on the economic origins of radical-right support proliferation in Europe. In particular, we show that, alongside macroeconomic shocks (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), governmental austerity policies in the form of fiscal restrictions may partially explain the recent electoral success of those on the radical-right. And

indeed, while a host of studies now exist both on the consequences of austerity and on the rise of the radical-right, the literature (Guriev and Papaioannou, 2022, Baccini and Sattler, 2023) has noted that properly understanding the link between these two phenomena remains a challenge, particularly in light of widespread endogeneity issues plaguing most empirical analyses. Our results provide valuable insights here, contributing precisely to what has been signaled as a shortcoming of the existing scholarship.

In line with Baccini and Sattler (2023), our findings are highly-topical in a post Covid-19 crisis era, where governments around the world must figure out how to manage the large levels of public debt that have been generated throughout the pandemic and its aftermath. In this context, austerity is a tool which governments may choose to employ. Therefore, the possibility of further fueling the radical-right rhetoric if implemented is an aspect that governments may wish to keep in mind before deploying any disruptive changes.

As a secondary contribution, we take our analysis one step further by exploring the political mechanism underlying our results. That is, we ask *why* might those on the radical-right benefit from governmental austerity? — once more, a shortcoming of the existing literature that remains a challenge for research in this field. Here, in line with the extant theoretical literature, we investigate two possible mechanisms: an anti-establishment one, wherein radical-right parties benefit from austerity following an increase in voters' frustration with mainstream political forces, and an anti-immigration one, wherein fiscal austerity and the rise in (perceived) competitiveness for now scarcer governmental resources increase the appeal of an anti-immigration rhetoric — often a central point in the policy agenda of those on the radical-right.

Taking advantage of several appealing contextual features, our paper provides some evidence in support of the latter anti-immigration, rather than of the former anti-establishment mechanism. Taken at face value, these results further bolster the above policy point regarding the use of austerity to address public debt. If indeed the anti-immigration rhetoric plays a role, then governments need to be aware of such effects when designing their policy response, particularly in an environment where the evidence suggests that ethnic tensions can have important, often undesirable, consequences (see, e.g., the discussion in Dehdari, 2022).

Finally, we conclude by highlighting some limitations of our analysis that future work might tackle. In terms of the empirics, our study has three main limitations. First and most straightforwardly, since we focus on one particular austerity episode, we cannot make any

strong claims concerning the external validity of our results. Whether other forms of austerity (that is, other than fiscal rules) would have similar political effects, and whether the institutional setting of 2013 Italy plays a role in moderating the link we document are pertinent questions that we cannot tackle in this study. That said, reassuringly, our results do align with the cross-country investigation of Baccini and Sattler (2023), who similarly find, in a cross-country analysis, that the radical-right is the primary electoral beneficiary of austerity in their analysis. Therefore, while they "*trade-off stronger identification assumptions for a stronger external validity*," we do the opposite, thus providing a complementary piece of evidence to the broader debate. Second, we once more point out that, because of contextual limitations, we had to compare two different election types in our difference-in-discontinuity analysis. While this does not represent a problem for the consistency of our estimates, since the effect we are interested in retrieving occurs at the running variable's threshold, it does raise the question of whether we would have observed weaker or stronger effects had a second set of national elections been used instead of the European ones investigated here. The evidence showing that results are stronger in municipalities required to undertake fiscal consolidation is nevertheless reassuring in this respect. Finally, our analysis is solely focused on the short-term electoral effects of austerity. In the long-run, however, one might posit that austerity could ultimately benefit a constituency's finances, following a short-run period of unrest. In this scenario, would the effects we observe here be reversed? If we are to fully understand the link between austerity and radical-right support, this question requires a thorough investigation of its own.

Lastly, from a theoretical perspective, future work should build upon our findings by more thoroughly exploring the political mechanisms underlying the effect we document. While our analysis, as discussed above, points towards an anti-immigration channel as a driver, the evidence we provide in support of this claim is suggestive in nature. We believe that a controlled study, perhaps in the form of a survey experiment, would be best suited to provide some insights here.

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Supplementary Material for

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

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Summary

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). 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

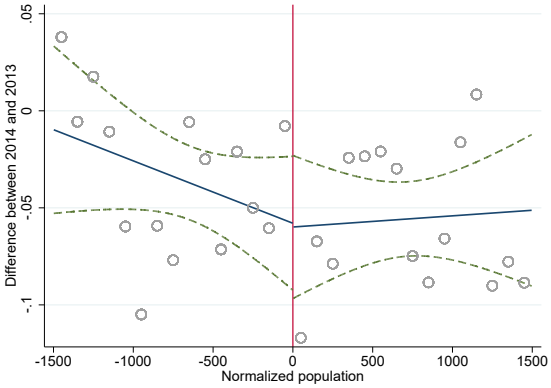
number of immigrants may be source of concern in light of our result on the increased consent expressed in treated municipalities for radical-right parties, which base their political platform on anti-immigration policies. However, in this case the sign of the discontinuity is negative and, therefore, if anything, the lower presence of immigrants in treated municipalities should represent a downward bias on our estimates for radical-right 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.

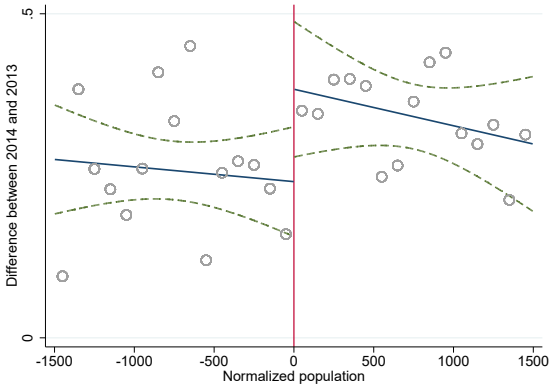
Parallel trends We test for the presence of parallel trends between treated and control municipalities before the introduction of fiscal rules. Specifically, we use Parliamentary elections in 2008 and European elections in 2009 to test for parallel trends, by estimating a dynamic version of equation (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 introduction 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

[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.

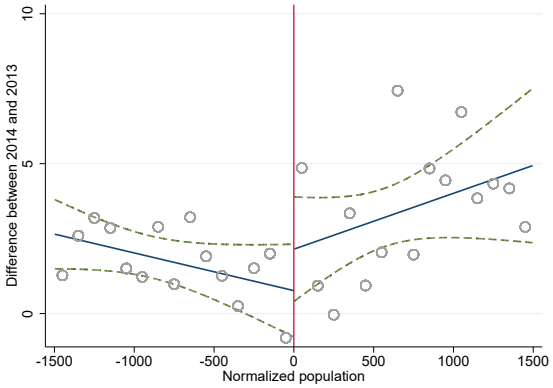
interpretation of dynamic effects in this context. With these caveats in mind, we report in Figure A.3 the dynamic difference-in-discontinuities estimates for electoral turnout. Estimates are not statistically significant in election years 2008 and 2009, while we observe a statistically and politically significant jump in 2014, when fiscal rules are in place, highlighting the absence of pre-trends in one of our outcome variables. This finding is reassuring on the validity of our empirical strategy — although the aforementioned caveats should be kept in mind.



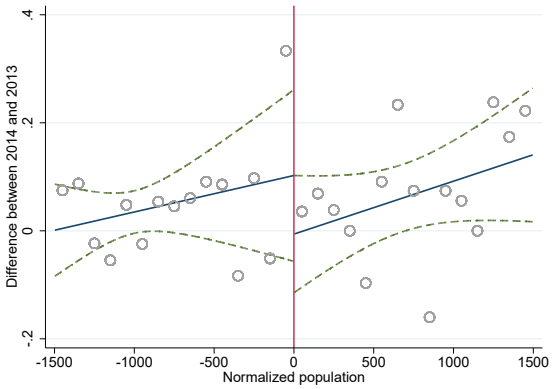
(A) Female share



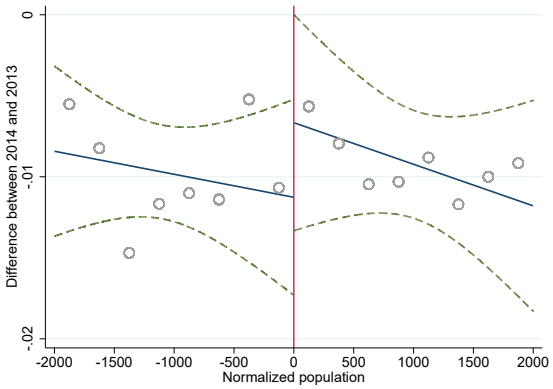
(B) Immigrant share



(C) New citizenships to foreigners



(D) No. cohabiting couples



(E) Average no. of family components

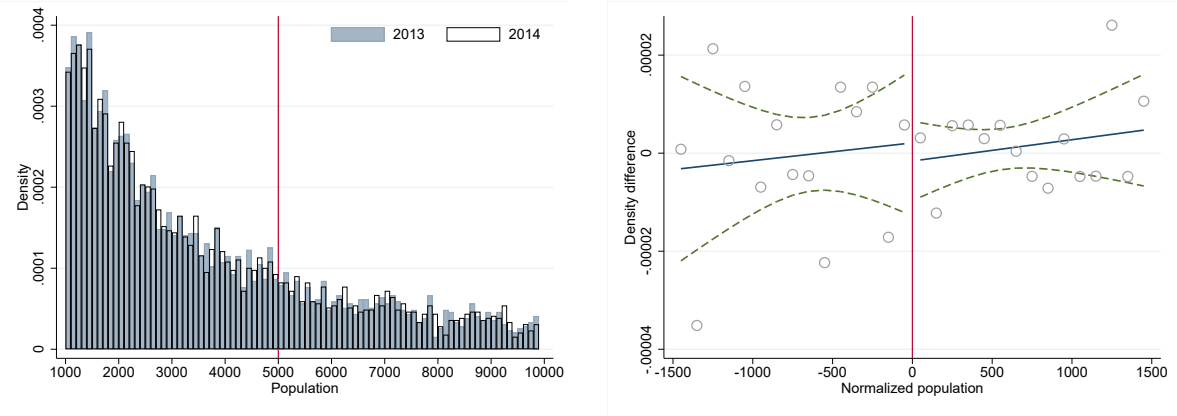
Figure A.1: Difference-in-discontinuities for demographic variables

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.

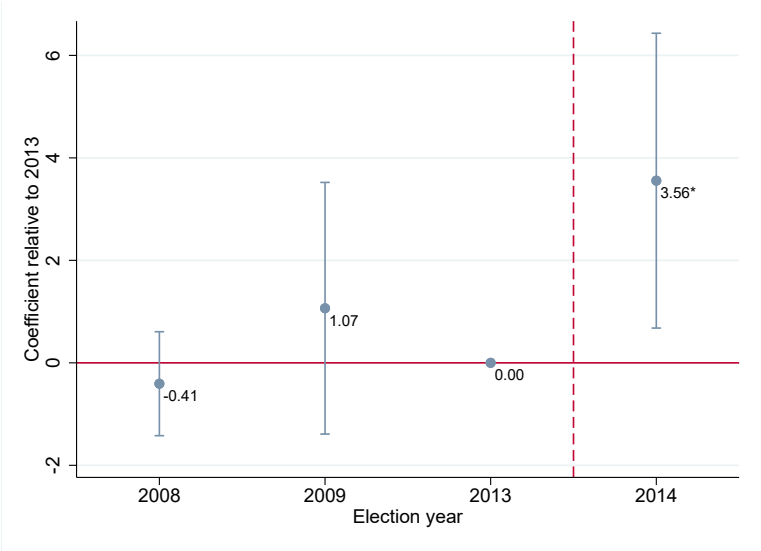


Figure A.3: Dynamic difference-in-discontinuities estimates

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.

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.1, 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.2, which reports conventional estimates with conventional 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.[ii] 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.1 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 radical-right parties, although noisier at smaller bandwidths, are remarkably stable across different sample selections (panel A). Similarly, the negative effects on the M5S vote share and anti-elite score share are not statistically significant at very small bandwidths, but always negative and largely unaffected

[ii] 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.

by the size of the bandwidth (panels B and C). Finally, the effects for turnout are almost always significant irrespective of the bandwidth (panel D), while those on the incumbent party tend to become closer to 0 when increasing the sample size (panel E).

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 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.2, 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 the radical-right (panel A), the M5S (panel B), and turnout (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.

Table B.1: Difference-in-discontinuities estimates of the impact of fiscal rules on electoral results, covariates included

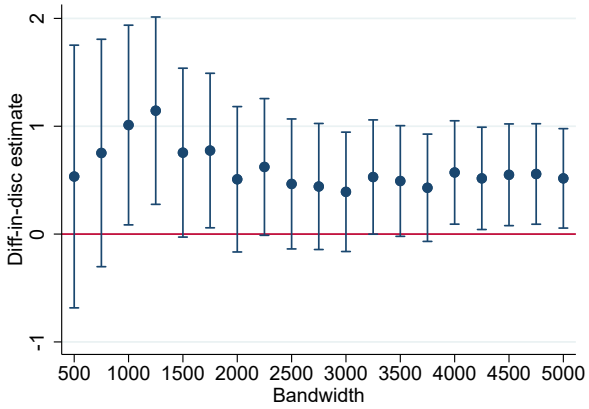
	(1)	(2)	(3)
	[A] Dependent variable: Radical-right vote share		
Treatment × Post	1.009**	0.892**	0.996**
	(0.434)	(0.422)	(0.441)
Control mean pre	10.48	10.49	10.55
Bandwidth	1379	1422	1336
Observations	1898	1958	1818
	[B] Dependent variable: M5S vote share		
Treatment × Post	-1.156**	-1.143**	-1.161**
	(0.570)	(0.565)	(0.569)
Control mean pre	22.65	22.65	22.64
Bandwidth	1094	1102	1086
Observations	1470	1486	1462
	[C] Dependent variable: Anti-elite score		
Treatment × Post	-0.293	-0.291	-0.299
	(0.308)	(0.307)	(0.308)
Control mean pre	63.97	63.96	63.96
Bandwidth	1343	1348	1338
Observations	1830	1838	1826
	[D] 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
	[E] 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

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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

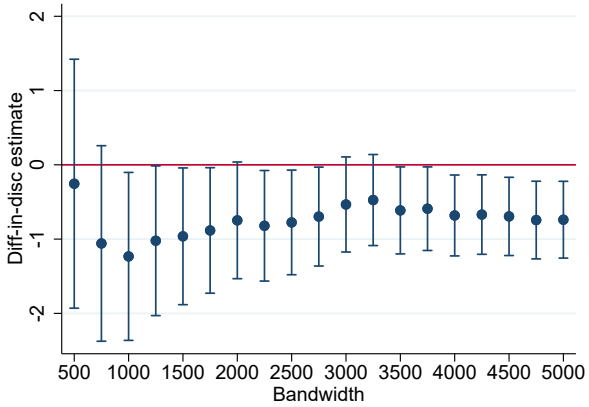
Table B.2: Non-parametric difference-in-discontinuities estimates of the impact of fiscal rules on electoral results

	(1)	(2)	(3)
	Conventional	Bias-corrected	Robust
	[A] Dependent variable: Radical-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: 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
	[C] Dependent variable: Anti-elite score, first difference		
Treatment	-0.378	-0.387	-0.387
	(0.290)	(0.290)	(0.354)
Bandwidth	1838	2755	2755
Observations	1296	1296	1296
	[D] 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
	[E] 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

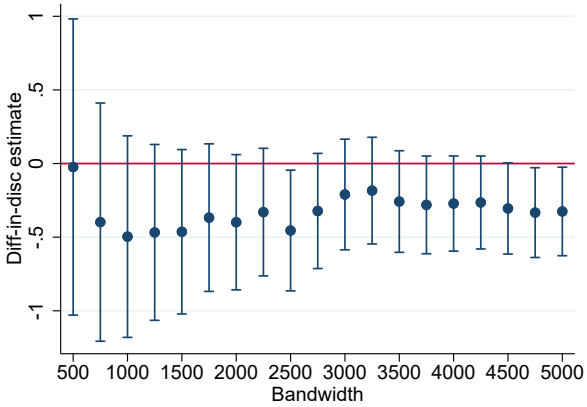
Notes. The table shows difference-in-discontinuities non-parametric estimates for the main outcomes. Difference-in-discontinuities estimates are obtained by estimating non-parametric regression discontinuities (RD) on first-differenced outcomes. Column 1 reports conventional RD estimates with conventional 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.



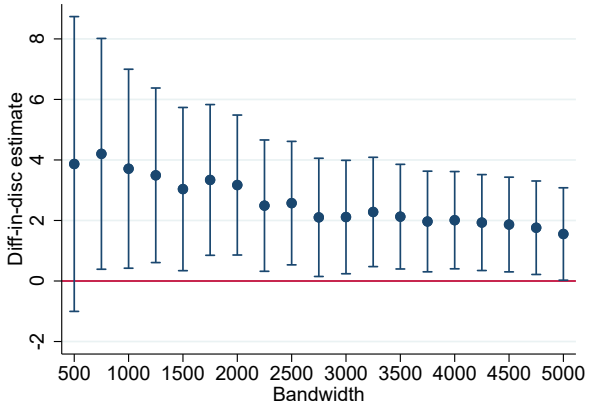
(A) Radical-right vote share



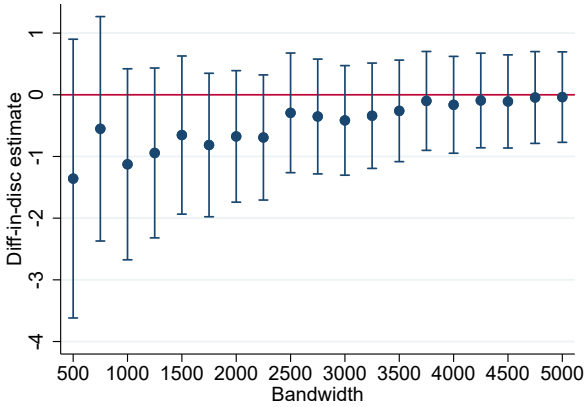
(B) M5S vote share



(C) Anti-elite



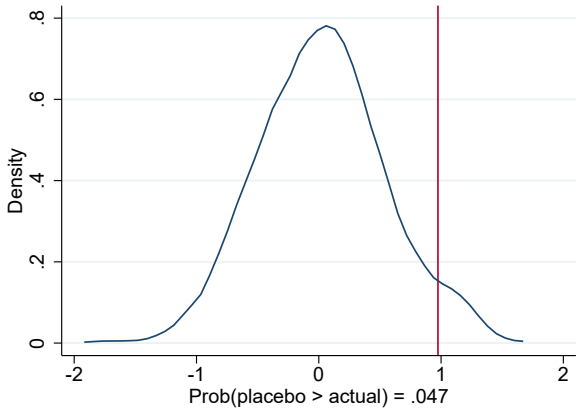
(D) Turnout



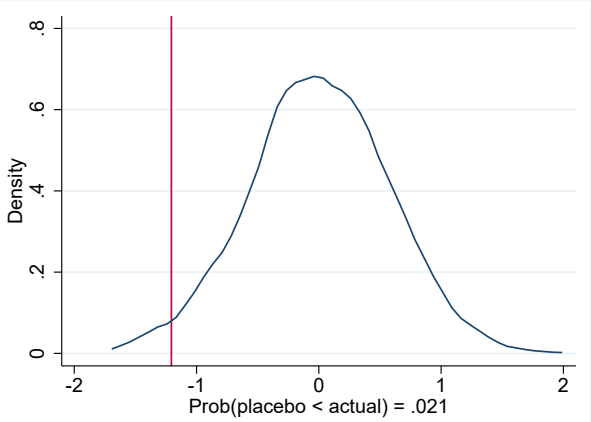
(E) Incumbent vote share

Figure B.1: Difference-in-discontinuities at different bandwidths

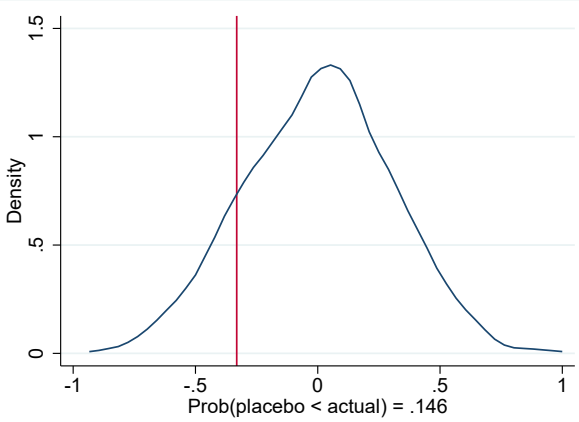
Notes. The figure plots coefficients of local linear regressions at different bandwidths around the 5,000 cut-off. Each dot reports the difference-in-discontinuities estimate and the horizontal axis reports the bandwidth. Vertical lines are 95 percent confidence intervals.



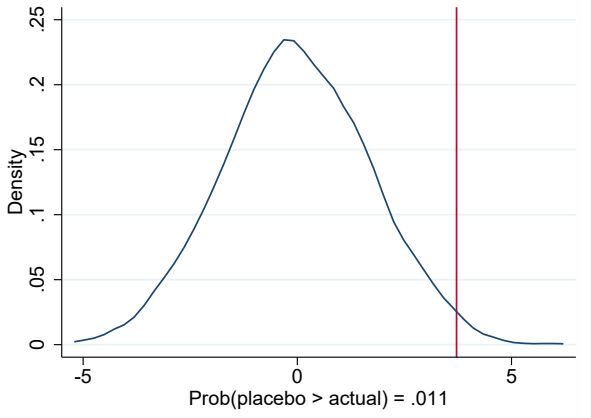
(A) Radical-right vote share



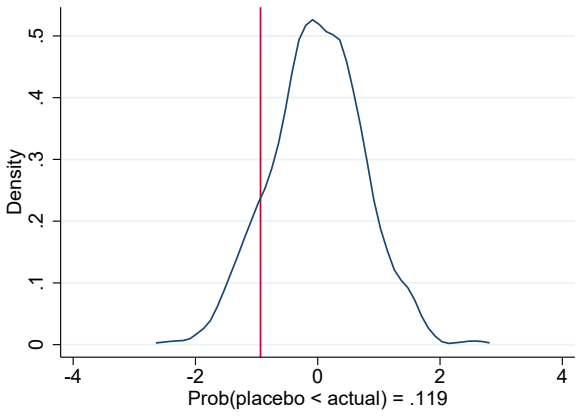
(B) M5S vote share



(C) Anti-elite



(D) Turnout



(E) Incumbent vote share

Figure B.2: 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.

C Effects of Fiscal Rules on Radical-Right versus Other Populist 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 5.2, we found that the Five Star Movement — Italy's 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,[iii] we posit that this finding is consistent with the arguments we put forth in Section 2, when providing the theoretical reasons for why we expect Italy's radical-right parties to have benefited from the 2013 policy implementation.

More concretely, 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 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

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

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 other populist parties 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.[iv] 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 non-mainstream parties proliferate under austerity — with the evidence suggesting that those on the radical-right are well-positioned to capitalize on austerity policies.

[iv] 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.

D Additional Tables and Figures

Table D.1: Authoritarian and anti-elite scores

Party	Antielite	Year	Description
Lega Nord	78	2013, 2014	Norris and Inglehart (2019)
Fratelli d'Italia (FdI)	62	2013, 2014	Norris and Inglehart (2019)
Movimento 5 Stelle (M5S)	100	2013, 2014	Norris and Inglehart (2019)
Forza Italia (FI)	37	2014	Norris and Inglehart (2019)
Il Popolo della Libertà (PdL)	37	2013	Norris and Inglehart (2019)
Rivoluzione Civile	92	2013	Norris and Inglehart (2019)
Svp	49	2013, 2014	Norris and Inglehart (2019)
Nuovo Centro Destra	34	2014	Norris and Inglehart (2019)
Centro Democratico	50	2013	Norris and Inglehart (2019)
Unione Di Centro (UDC)	34	2013	Norris and Inglehart (2019)
Scelta Civica	41	2013	Norris and Inglehart (2019)
Partito Democratico (PD)	58	2013, 2014	Norris and Inglehart (2019)
Sinistra Ecologia Libertà (SEL)	71	2013	Norris and Inglehart (2019)
Casapound Italia	100	2013	Arbitrary
Forza Nuova	100	2013	Arbitrary
La Destra	62	2013	FdI
Fiamma Tricolore	76.5	2013	Fratelli d'Italia
Die Freiheitlichen	78	2014	Lega Nord
Indipendenza Veneta	78	2013	Lega Nord
Io Amo l'Italia	78	2013	Lega Nord
Lg.Veneta Repubblica	78	2013	Lega Nord
Veneto Stato	78	2013	Lega Nord
Io Cambio - Maie	42	2014	Mean(Centro Democratico, UDC)
Fare Per Fermare Il Declino	52	2013	Mean(PD, PdL)
Futuro e Libertà	49.5	2013	Mean(PdL, FdI)
Riformisti Italiani	75	2013	Mean(Rivoluzione Civile, PD)
L'Altra Europa con Tsipras	81.5	2014	Mean(Rivoluzione Civile, SEL)
Lista Amnistia Giustizia Libertà	58	2013	PD
Grande Sud - Mpa	37	2013	PdL
Liberali Per L'Italia - Pli	37	2013	PdL
Mir - Moderati In Rivoluzione	37	2013	PdL
Movimento P.P.A.	37	2013	PdL
P.Liberale Italiano	37	2008	PdL
Partito Comunista Dei Lavoratori	92	2013	Rivoluzone Civile
Partito Di Alternativa Comunista	92	2013	Rivoluzone Civile
Scelta Europea	41	2014	Scelta Civica
Italia Dei Valori	71	2014	SEL
Verdi Europei-Green Italia	45.5	2014	SEL
Intesa Popolare	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. The column labelled *Antielite* reports the scores for each party. *Year* indicate the election year in which the party runs 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 D.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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.3: Difference-in-discontinuities estimates of the impact of fiscal rules on the anti-immigration score

	(1)	(2)	(3)
	[A] Dependent variable: Anti-immigration score 1		
Treatment × Post	0.784*	0.785*	0.898*
	(0.436)	(0.415)	(0.461)
Control mean pre	50.75	50.69	50.91
Bandwidth	1453	1621	1284
Observations	1984	2230	1754
	[B] Dependent variable: Anti-immigration score 2		
Treatment × Post	0.732*	0.675*	0.760*
	(0.420)	(0.396)	(0.450)
Control mean pre	53.20	53.16	53.37
Bandwidth	1492	1693	1291
Observations	2040	2344	1762

Notes. The table shows difference-in-discontinuities estimates for the anti-immigration score derived from the Global Party Survey 2019. Panel A corrects the score for M5S by making it equal to the one of the Centre-left coalition. Panel B uses half of the score reported for M5S. 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 D.4: 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

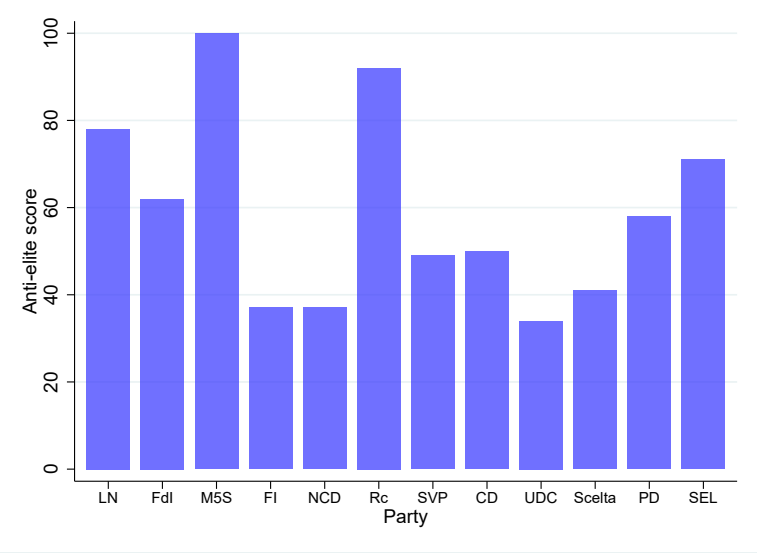


Figure D.1: Populist, anti-elite and authoritarian scores from Norris and Inglehart (2019)

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*.

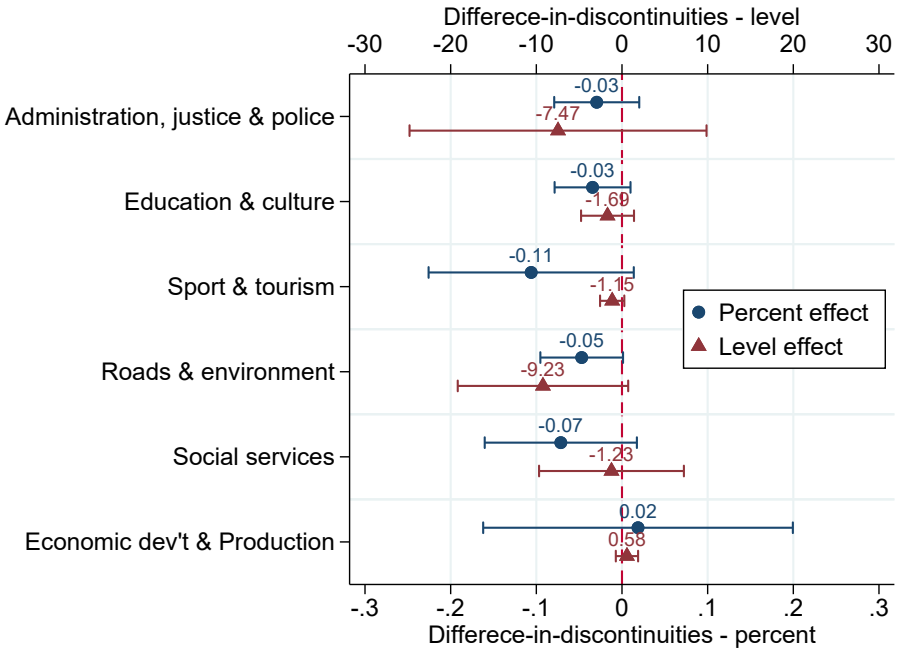


Figure D.2: The effect of local fiscal rules on expenditure categories

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.