The Local Political Economy of Austerity: Lessons from Hospital Closures in Romania

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Reference Details

The Local Political Economy of Austerity: Lessons from Hospital Closures in Romania [*]

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Abstract

I study whether austerity measures implemented by a central government incentivize electorally-motivated policy adjustments at the local level. To do so, I first quantify the effects of a controversial 2011 health-sector reform carried out in Romania, whereby a significant proportion of the country's public hospitals was discontinued. Exploiting geographic constituency-level variation in austerity exposure created by this measure, I document a significant increase in local "voter-friendly" government spending targeted towards infrastructure investments in the policy's catchment areas. Consistent with an electoral mechanism explaining this response, the evidence suggests that the effect is driven by the actions of local politicians affiliated with those responsible for the reform. To rule out alternative explanations, I take advantage of a second natural experiment wherein a party previously in opposition to those responsible for the closure of hospitals allied itself with the measure's principal orchestrator. Following this re-alignment, I find heterogeneous increases in local voter-friendly spending which further corroborate the electoral mechanism. Overall, my results indicate that the electorally-driven responses of sub-national governments may partially mitigate the political costs of carrying out austerity.

Keywords: Austerity, Political Costs, Central and Local Governments, Electoral Spending

1 Introduction

Motivation A widely held view in political economy is that a country's development can be hampered by its national government's reluctance to adopt "unpopular" austerity policies - defined as measures whose costs are incurred upfront for (possible) later materializing benefits (see e.g. Weaver, 1986; Roubini and Sachs, 1989). According to this argument, politicians often decide against implementing austerity policies - such as a retrenchment of public spending or welfare programs - due to anticipated electoral repercussions (Pierson, 1996).

This electoral reticence to carrying out austerity is believed to have many pertinent consequences. Early scholarship, for instance, argued that a government's electorally-driven resistance to reform may prevent necessary efficiency-enhancing economic changes from occurring (Fernandez and Rodrik, 1991). Additionally, the strong role electoral considerations might play in shaping national policies provides a justification for the existence of *supra*national organizations such as the International Monetary Fund [IMF], whose programs can change the incentives of policy-makers in ways that allow them to commit to electorally costly measures.[1]

However, despite such arguments highlighting the importance of understanding how and why electorally-constrained governments might resist reforms, whether those who implement austerity do incur an electoral cost for doing so remains an open question in the literature, with recent studies providing mixed results. Notably, Alesina, Carloni and Leece (2013) and Arias and Stasavage (2019), focusing on a large sample of cross-country fiscal adjustments, broadly conclude that austerity policies have limited political costs, while other investigations, including Nyman (2014), Talving (2017), Hubscher, Sattler and Wagner (2019), and Ardanaz, Hallerberg and Scartascini (2020) contest the view whereby austerity is not electorally costly. Employing different methodologies, they find that implementing austerity led to lower levels of support for those responsible.

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[1]For example, by conditioning financial support on the implementation of unpopular policies, supranational agreements allow governments to shield themselves from potential electoral repercussions (Przeworski and Vreeland, 2000).

These conflicting findings suggest that the relationship between austerity and incumbent electability is not as straightforward as public and scholarly imagination often suggests (Arias and Stasavage, 2019), and might instead depend on the socio-economic and institutional circumstances in which such measures are carried out, leading to a natural question: *What factors mitigate or exacerbate the electoral costs of carrying out austerity?*

Addressing this query, a number of potential moderating variables have been investigated in recent work, ranging from the ideology of the electorate (here, the existing scholarship argues that a conservative electorate who worries about large deficits will be less likely to sanction budget-tightening reforms - see Brender and Drazen, 2008) to the health of the overall economy at the time of austerity (with the general view being that austerity is more likely to be sanctioned if implemented during economic downturns when voters place those in power under greater scrutiny - see e.g. Talving, 2017, as well as Arias and Stasavge, 2019), to the type of consolidation policies being carried out (with recent work suggesting that tax increases may be more heavily sanctioned than expenditure cuts - e.g. Ardanaz, Hallerberg and Scartascini, 2020).

Objective In this paper, I add to the existing debate by arguing that the electoral repercussions of austerity implemented by *national* governments may be partially mitigated by the electorally-driven reactions of *sub-national* politicians.

Specifically, in line with a political business cycle view, I propose the following core argument: when a central government adopts austerity, the electability of local politicians[2] affiliated with the governing parties falls. As this fall in popularity lowers, all else equal, their party's chances of winning the next election, said local politicians are incentivized to engage in so-called "voter-friendly policy adjustments" (termed as such in line with Drazen and Eslava, 2010) in order to recuperate from the shock, ultimately diminishing the negative effects of austerity on incumbent support.

In light of this argument, my goal is to empirically evaluate the following hypothesis: centrally implemented austerity causally incentivizes local politicians to increase voter-

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[2]In the investigated context, mayors representing local constituencies.

friendly government spending.[3] If confirmed, I argue that any non-negative aggregate relationship between austerity and incumbent support documented in the literature may not necessarily indicate that implementing such reforms is not costly electorally. At least contextually, local policy changes might serve as a "buffer" against the damaging political consequences of austerity, diminishing its electoral costs. Thus, accounting for local responses may partially explain the existing scholarship's conflicting findings, and paint a more complete picture on the underlying economic link between austerity and governmental stability. Figure 1 provides a schematic representation of this argument.

Nevertheless, assessing how local politicians respond to austerity is an inherently difficult statistical task primarily for two reasons. First, methodologically, retrieving the causal effect of austerity on local fiscal choices is often unfeasible because austerity measures are generally implemented nation-wide. Hence, in the absence of suitable "control units", simple before-and-after comparisons of local policies are likely to be endogenous, especially if austerity is carried out in already volatile times when historical trends do not constitute reasonable counterfactuals.

Second, in terms of the underlying mechanism, austerity policies usually include the imposition of so-called "fiscal discipline" prerequisites - such as requirements to increase taxes or to cut certain expenses.[4] These impositions can - by design - mechanically alter local spending over and above any electorally-driven responses. If this happens, any estimates retrieved by analyzing austerity exposure, *even if causal*, will be informative only of a bundled effect, capturing both direct mechanical changes as well as electorally-motivated adjustments. My investigation's objective is to isolate the latter electoral channel.

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[3]In this paper, in line with a number of studies examining electoral spending, I focus on *fiscal* policy responses for which data are systematically available, and operationalize voter-friendly adjustments as changes in the amount of public funds targeted towards local infrastructure investments - which lead to "visible" local effects such as improved roads, illumination services, or renovated public spaces. In Section 4.2, I introduce and justify my variable choices in depth, but also discuss the limitations associated with using fiscal decisions to operationalize local responses.
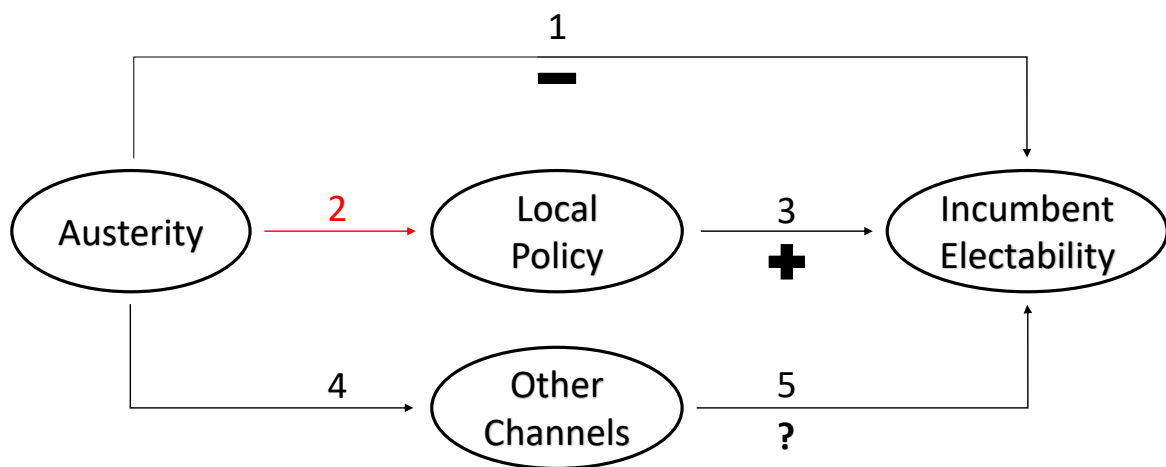
[4]See e.g. Aaskoven (forthcoming) for a recent discussion on fiscal rules and their consequences.

Figure 1: The Electoral Consequences of Implementing Austerity: Mechanism Decomposition

(a) Aggregate Effect
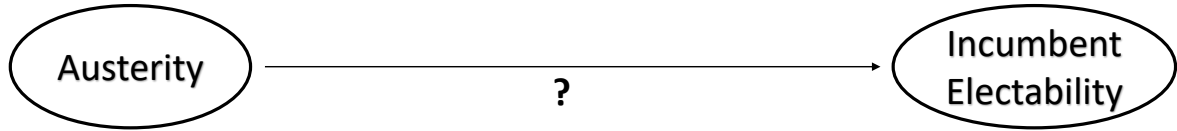
(b) The Role of Local Policy Responses

Note: Existing empirical studies on the aggregate electoral effects of implementing austerity, illustrated in panel A, provide mixed results. I argue that we can better understand these conflicting findings by breaking down the economic mechanism underlying the relationship. As shown in panel B, even if carrying out austerity entails direct electoral costs (arrow 1), sub-national voter-friendly policy adjustments driven by *local* electoral considerations (arrow 2, the focus of this paper) might partially mitigate the electoral repercussions of implementing such measures (arrow 3). Although not the object of my analysis, I also note that other factors may also moderate this relationship (arrows 4 and 5).

Contribution I address these difficulties and provide evidence suggesting that centrally-implemented austerity may incentivize local electorally-driven fiscal adjustments in two steps.

I begin by exploiting a novel quasi-experimental source of spatial variation in local austerity exposure created by a controversial health-sector measure implemented in Ro-

mania. In 2011, aiming to reduce public spending and maintain an agreement with the IMF, the central government announced the closure of 67 public hospitals deemed to be performing at suboptimal standards [henceforth, "the shut-down"]. The shut-down was unprecedented in its scale, with roughly fifteen percent of the total number of operating public hospitals being discontinued, as shown in Figure 2. The measure also became one of the most publicized and politically polarized policies at the time (Scintee et al., 2018), making this context ideal to study the effects of highly-impactful austerity reforms.

I argue that investigating the local consequences of the shut-down is appealing for my purposes, as it allows me to circumvent the two difficulties mentioned above. First, pertaining to the endogeneity issue, given that the shut-down heterogeneously impacted different geographic areas, I can contrast fiscal adjustments in comparable affected and unaffected constituencies to retrieve arguably causal effects. Second, mechanism-wise, given that there were no fiscal discipline requirements discriminatorily imposed in constituencies affected by the closure of hospitals, I can isolate the indirect response of local fiscal choices to austerity - which I argue is driven by local electoral considerations.

To do so, I first use a geographic software to identify constituencies affected by the shut-down. I classify a constituency as impacted (i.e. "treated") if the public hospital closest to its centroid was discontinued in 2011 (constituencies are assigned to the "control" group otherwise). Intuitively, the electoral costs of austerity should be highest precisely in areas where citizens are (or at least perceive themselves to be) more heavily impacted by its effects. I argue that this geographical definition constitutes a reasonable proxy for (perceived) consequence intensity given several at the time testimonies and media reports asserting that the shut-down's consequences will be more dire in the so-called "catchment areas" of targeted hospitals.[5]

Next, in the baseline evaluation, I employ a difference-in-differences method to contrast the evolution of fiscal adjustments in treated and control constituencies following the shut-down.

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[5]Concrete examples of such reports are presented in Section 3.4.

Figure 2: Number of Operating Public Hospitals in Romania
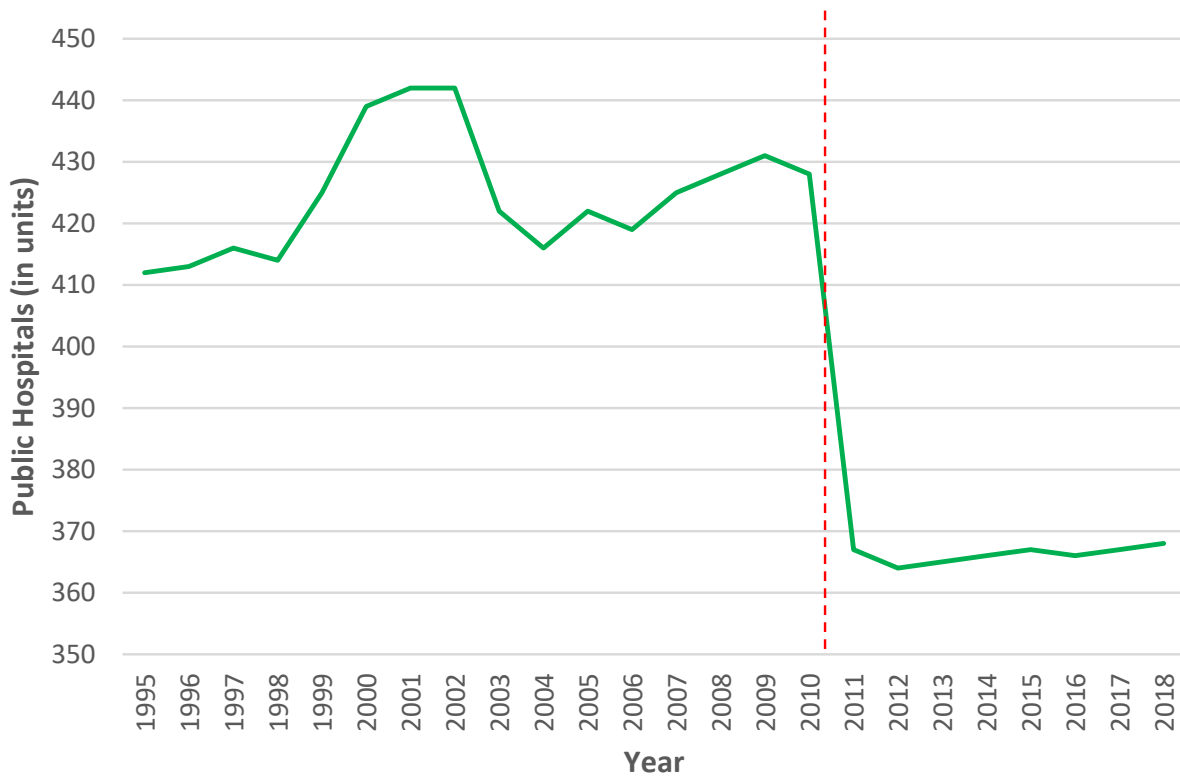
Note: Data reported by the National Institute of Statistics at the end of each year. According to the institute, a hospital is defined as a "public health unit meant to provide preventive, diagnostic and curative medical services". The dotted red line marks April 2011, when the shut-down was carried out.

The baseline results can be summarized as follows. First, using administrative data provided by Romania's Ministries of Finance and Regional Development, I find evidence suggesting that austerity led to local fiscal adjustments. Relative to control constituencies, treated units experienced a statistically significant increase, estimated at roughly 7.6 percent in my preferred specification[6], in the amounts of funds used for local infrastructure purposes following the shut-down - expense categories which the literature on

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[6]Of course, whether these estimates can be interpreted as capturing the *causal* effect of austerity exposure crucially hinges on the so-called "common trends" assumption holding: in the absence of the shut-down, fiscal choices in treatment and control constituencies would have evolved in a statistically indistinguishable manner. Under this postulate, unaffected constituencies constitute a reasonable counterfactual proxy, and any additional impact retrieved by the difference-in-differences technique gives a plausibly casual estimate of the measure's impacts. I address these endogeneity concerns extensively throughout my investigation - in Section 5.2, I do so formally by employing a dynamic leads-and-lags specification, and find evidence corroborating the existence of pre-intervention "parallel trends" in local visible spending.

political budget cycles has repeatedly documented to be influential electoral instruments (see e.g. Drazen and Eslava, 2010, or Aidt, Veiga and Veiga, 2011).

Despite being suggestive, I argue that observing an increase in visible spending in treated constituencies does not corroborate the electoral mechanism driving local responses to austerity in and of itself. More specifically, the problem is that the baseline evaluation does not account for a crucial source of heterogeneity as - in line with the political hypothesis investigated here - any electorally-driven responses to the shut-down should be more pronounced in constituencies where the mayor is politically-affiliated with the parties comprising the central government responsible for austerity [henceforth, "governmental constituencies"]. Intuitively, it is these politicians that are expected to react since they anticipate being "punished" by local voters for measures implemented by their own parties' government.

Empirically, I use information disseminated by Romania's Central Electoral Bureau [CEB] for the 2008 mayoral elections to assess this postulate, and find that the shut-down's impact on voter-friendly fiscal outcomes is driven almost entirely by adjustments occurring in governmental constituencies. Here, visible spending increased by more than 21 percent - a strongly statistically significant and economically meaningful effect. In contrast, I find negligible, statistically null estimates in constituencies represented by at the time opposition forces.

I assert that the results of this heterogeneity analysis provide new evidence indicating that electorally-motivated local politicians engage in voter-friendly fiscal adjustments in response to central austerity reforms. This represents the central takeaway of my investigation.

In order to rule out alternative explanations consistent with this core insight, I proceed in a novel manner by exploiting a second natural experiment in the latter part of my analysis - consisting of a re-alignment of the country's political parties which took place in 2014. Briefly[7], roughly three years after the shut-down, the opposition coalition at

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[7]I describe the relevant political actors and timeline in-depth in Section 6.

the time of austerity - the "Social Liberal Union" [SLU] - dissolved and split into two major independent political parties: the "Social Democrats" [SDs] and the "National Liberals" [NLs]. Following the union's dissolution, the NLs, facing strong electoral pressures concerning the presidential ballot taking place later that year, ended up forming an alliance and eventually merging with the "Democratic Liberal" party [DLs], the shut-down's principal orchestrator. I term this series of events "*the marriage*" henceforth.

My key argument is that the marriage constitutes a second electability shock which, this time, *heterogeneously diminishes support for the NLs in constituencies previously affected by the shut-down.* Intuitively, in these areas, the electorate will be inclined to punish the NLs precisely for allying themselves with the DLs, the party responsible for past austerity - a response consistent with memory-based models of bounded rationality (Mullainathan, 2002), wherein new events can influence behavior if they bring back (negative) reminders of past experiences (Fouka and Voth, 2016).

My hypothesis now is that, anticipating this response, NL - but *not* SD - local politicians will find it optimal to engage in voter-friendly fiscal adjustments, echoing and corroborating the electorally-driven baseline responses estimated when appraising the shut-down's impact on local policy.

Extending the baseline difference-in-differences specification, I find strong evidence corroborating this proposition. Following the marriage, voter-friendly infrastructure investments increased in an economically meaningful and strongly statistically significant manner in NL-aligned constituencies exclusively. In contrast, I find virtually no changes in constituencies represented by the SDs, a convenient "placebo" group that allows me to rule out explanations unrelated to the electoral incentives of local politicians.

I assert that the heterogeneous responses documented in response to the marriage complement the central findings primarily in three ways. First, they bring further corroborating evidence in support of a causal interpretation of the shut-down's estimated effects. Unlike the 2011 shut-down investigation, where one could argue that politically-strategic hospital selection by the central government may represent a concern for identification,

the marriage was a national-level shock driven primarily by electoral considerations pertaining to the upcoming presidential race and, as such, likely unrelated to any local-level discrepancies between areas previously affected and unaffected by austerity.

Second, and more importantly, the retrieved heterogeneous effects further substantiate the paper's core narrative whereby electorally-motivated local politicians respond to austerity by carrying out voter-friendly adjustments. In particular, were I to not account for local electoral incentives, it would be difficult to explain why visible infrastructure investments rose in treated constituencies three years following the implementation of austerity, heterogeneously in areas represented by the party that merged with those responsible for the closure of hospitals.

Finally, more tentatively, my findings suggest that the hypothesized political costs of austerity policies can extend even further than expected. Specifically, it seems that parties which implement austerity essentially "stain their hands" by doing so. Once they carry out unpopular measures, it appears that even more benign actions subsequently undertaken - such as an alliance with another political party - can trigger unanticipated (and potentially undesirable) reactions.

Overall, the empirical findings derived via the two natural experiments exploited in this paper - acknowledging their individual weaknesses and questionable external validity - suggest that accounting for local electorally-driven policy responses may help us better reconcile some disagreements on the political costs of austerity reforms present in the broader literature.[8]

Organization I proceed as follows. In Section 2, I briefly review the related literature and highlight my contributions. In Section 3, I describe the institutional context, with a focus on the shut-down's appealing features that allow me to overcome the empirical challenges associated with assessing the local effects of austerity. In Section 4, I detail

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[8]I provide two further complementary pieces of evidence suggesting that local responses are electorally-motivated. First, I show that the fiscal adjustments observed in response to the shut-down are markedly stronger in politically contested constituencies. Second, I show that the observed increase in visible spending is unlikely to have been financed by raising local taxes. These supplementary analyses are discussed in-depth in Appendix A.

the paper's hypotheses linking austerity exposure to local fiscal adjustments, and present the data used to test these propositions. In Section 5, I conduct the main empirical investigation. I describe the techniques used, the central results, and discuss their interpretation and limitations. Next, in Section 6, I rule out alternative explanations which do not account for local electoral incentives by exploiting the marriage as a second natural experiment. Section 7 concludes and discusses a number of avenues for future research. Finally, in the Appendix, I present the results of several complementary analyses and robustness checks.

2 Literature Overview

Appraising whether austerity incentivizes electorally-driven local policy adjustments contributes to two main literatures. In this section, I provide a brief overview.

I primarily add to the scholarship investigating the consequences of austerity (see Monastiriotis, 2014, and Alesina, Favero and Giavazzi, 2019 for excellent overviews) and, in particular, to an emerging strand concerning their political effects. Here, the pertinent research agenda has employed two complementary approaches.

First, as already discussed, several studies inquire whether austerity directly impacts incumbent vote shares, with Alesina, Carloni and Leece (2013), and Arias and Stasavage (2019) finding limited electoral costs, and the results of Nyman (2014), Talving (2017), Hubscher, Sattler and Wagner (2019) and Ardanaz, Hallerberg and Scartascini (2020) suggesting that carrying out austerity is detrimental.

Relatedly, several investigations go beyond conventional measures of incumbent electability, and evaluate the consequences of austerity for broader socio-political outcomes. Here, two recent articles by Galofre-Vila et al. (2019) and Fetzer (2019) document how austerity exposure can lead to increased support for extremist forces - such as the Nazi party historically or, more recently, the United Kingdom Independence Party (which Fetzer, 2019 argues ultimately contributed to a "Leave" victory in the 2016 EU Referendum).

Similarly, Ponticelli and Voth (2020), analyzing a panel of countries from 1919 to 2008, find that consolidation policies lead to social unrest, an insight echoing the results of Vegh and Vuletin (2015) in Latin America.

My paper contributes to better understanding the political costs of austerity in three ways. First, by finding evidence for local electorally-driven responses, my results suggest that a country's sub-national institutional factors may partially mitigate the costs of carrying out austerity. Second, by analyzing the effects of the shut-down (a health-sector reform), I move away from evaluating the consequences of *fiscal* adjustments on which the existing literature has largely focused. As further described in 3.4, examining a non-fiscal shock allows me to isolate the indirect (electorally-driven) effects of austerity exposure from other mechanical consequences brought upon by fiscal discipline - a separation that has thus far not been rigorously documented. Finally, analyzing two complementary natural experiments to appraise the local effects of austerity is a novel approach, which permits me to corroborate the electoral channel driving the documented local responses.

I also contribute to a growing body of empirical studies investigating the strategic employment of government spending for electoral purposes - defined as the use of fiscal or economic instruments with the purpose of generating popular support. Concretely, by evaluating fiscal responses to austerity exposure, I am able to assess the extent to which (local) economic outcomes are a function of incumbent electability.

The literature on so-called political budget cycles (starting with Nordhaus, 1975) is vast and primarily relies on elections as exogenous shifts in politician incentives. Reviewed in detail in de Haan and Klomp (2013) and Dubois (2016), several studies empirically document the impacts of elections on the real economy. At the local level specifically, Kneebone and McKenzie (2001), Khemani (2004), Veiga and Veiga (2007), and Labonne (2016) find evidence for the existence of strategic pre-electoral fiscal choices in Canada, India, Portugal and the Philippines, respectively. Complementarily, other investigations rely on direct measures of incumbent popularity to test the electoral spending proposition. To this point, Aidt, Veiga and Veiga (2011) develop a model where fiscal choices

are a function of constituency competitiveness (and vice versa), and corroborate their hypotheses using information on Portuguese municipalities.

My paper adds to this field by exploiting an alternative source of variation in local incumbent electability: exposure to a centrally-implemented austerity policy. In addition to corroborating the external validity of previous findings to the Romanian context, this approach allows me to both address several endogeneity issues present in this field, and to further extend the view wherein local electoral considerations may ultimately impact sub-national development by influencing the allocation of (often scarce) fiscal funds.

Finally, my findings add to an emerging sub-strand of the political budget cycles literature documenting the use of infrastructure investments specifically for opportunistic purposes, with Khemani (2004), Veiga and Veiga (2007), Vergne (2009), Drazen and Eslava (2010), and Klein and Sakurai (2015) bringing supportive empirical evidence linking electoral pressures to increased infrastructure spending. In a complementary fashion, the results of Gonzalez (2002), Brender (2003) and, more recently, Huet-Vaughn (2019) suggest that voters indeed reward incumbents for their infrastructure-related policies.

3   Background

The Romanian setting provides an opportunity to better understand how local politicians respond to austerity. In this section, I provide a contextual overview in four steps. First, in Section 3.1, I discuss the country's local administrative organization - I touch upon how mayors are elected, on their responsibilities and powers, and explain why analyzing electorally-driven fiscal adjustments in this context is useful for my purposes. Next, in Section 3.2, I highlight that the well-functioning of the health-sector is crucial for the well-being of local voters and, therefore, I argue that austerity policies implemented here should have significant repercussions for the electability of their representatives. Subsequently, I introduce the relevant political parties - those responsible for austerity

and those opposing it - in Section 3.3. Finally, and most importantly, I describe the shock I use to assess how local politicians respond to austerity - the shut-down - in Section 3.4.

3.1 Romania's Territorial Administrative Organization

Romania is divided into 41 counties, and then over 3,000 territorial-administrative units [UATs], or "*constituencies*" - the main unit of investigation used in my analysis.[9] Constituencies, which are further classified as municipalities, cities or communes, depending on their population size and degree of urbanization[10], are ruled by a mayor and a local council, elected every four years via ballots that are simultaneously organized throughout the country typically in June.

The mayor is the head of a constituency's public administration, and represents its executive authority. Mayors are primarily responsible for managing the local budget, both in terms of accruing revenues and allocating funds to specific fiscal projects (of particular relevance here - local infrastructure investments[11]). In the vast majority of cases, local fiscal initiatives are proposed by the mayor. Once brought forward, projects are voted on by the local council and, subject to approval, implemented once again by the mayor.[12]

For my purposes, this setting has two appealing features. First, given that I rely on the mayor's partisan affiliation to assess how electoral incentives influence local fiscal responses to austerity, the large amount of policy discretion mayors enjoy in this domain is useful.

Second, I argue that rural Romanian voters are likely responsive to expansionary fiscal policies. This is in line with an argument put forth by Jones, Meloni and Tommasi (2012), whereby the electorate is more likely to electorally reward deficit spending if the

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[9]As shown in Appendix Figure B1.

[10]See Law No. 351/2001 (in Romanian) for further details.

[11]I detail what such investments entail in Section 4.2, when explaining the operationalization of my variables.

[12]The organization "Ne Reprezinta" [translation: They represent us] provides a brief, yet insightful discussion on mayoral responsibilities - see https://bit.ly/2lpfItm (in Romanian).

financing of public investments is predominantly external rather than internal - in which case voters can take advantage of immediate concentrated benefits, while associated costs are diffused nation-wide. The rural Romanian context is representative of this scenario, given that the degree to which most local administrations rely on central government financing is high because of limited self-funding capacities (Borcan, forthcoming).

3.2 The Health System

Vladescu et al. (2016) provide an in-depth overview of Romania's health-system, focusing on its governance, reforms and performance. While detailing the findings of this evaluation goes beyond the scope of my investigation, I point out five key features here, and discuss their relevance for my study. First, the system is highly centralized, with most major decisions being carried out by the Ministry of Health. Second, while the private health sector is growing, the vast majority of Romanians still rely on the public system, particularly so in rural areas. Third, large rural-urban inequalities exist in health care accessibility, with elements such as an underdeveloped ambulatory infrastructure making the physical access of rural residents to hospitals difficult. Fourth, when representative surveys are conducted asking Romanians to indicate their political priorities, the well-functioning of the health system is indicated as a key objective in most cases. Fifth, Romanian patients extensively rely on inpatient care, as opposed to other means of receiving medical services (such as outpatient or community care).

The review's findings suggest that public health-sector reforms are highly influential, both in terms of citizen well-being, as well as politically. Therefore, austerity implemented in this domain is likely to have significant electoral repercussions. Furthermore, because the Romanian health-system is highly centralized and local politicians exert little control over health-sector policies, such measures can be seen as largely exogenous disturbances from a local official's perspective. Thus, the context is appropriate for investigating local responses to central policies. Next, seeing that physical access is a constraint in an environment where patients rely extensively on inpatient care, the reasonableness of using

a distance-based measurement to proxy for austerity exposure[13] is corroborated. Lastly, any negative effects of health-sector austerity are expected to be particularly strong in rural communes, on which I focus.

3.3   Relevant Political Actors and Austerity in Romania

Two political parties formed the austerity-implementing central government: the Democratic Liberal Party [DLs], a right-wing conservative party to which the prime-minister belonged, and their minority partner, the Democratic Alliance of Hungarians in Romania [UDMR], a smaller party.[14] At the time, the opposition chiefly consisted of three parties: the Social Democratic Party [SDs], a major left-wing entity, the National Liberal Party [NLs], a right-wing national-liber party, and the Conservative Party [PC], a small political unit. It is important to note that the opposition parties were at the time part of a joint alliance - the so-called Social Liberal Union [SLU].

Economically, Romania was first hit by the financial crisis in the second half of 2008. In March 2009, confronted with these difficulties, the central government signed a "standby agreement" with the IMF that conditioned a relief amount of roughly 13 billion euros on the consolidation of Romania's public finances in different domains, which importantly included the health-sector. The hope was that austerity would reduce unnecessary public spending, ultimately leading to healthy consolidation and long-term growth.[15]

Exact measures were formally announced starting May 2010 - Stoiciu (2012) provides a detailed overview. The so-labelled "anti-crisis" policies involved fiscal adjustments - a 25 percent public wages reduction, and a 5 percent VAT increase, as well as structural modifications - labour market reforms facilitating the termination of employment con-

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[13]See Section 4.2 for details on how I code exposure to the shut-down.

[14]To be exact, a third political party - *The National Union for the Progress of Romania* - was also part of the government. However, this entity was founded after the 2008 local elections and is, therefore, not pertinent for my analysis. For expositional conciseness, I omit mentioning it further.

[15]To this point, a declaration by former president Traian Basescu captures the ideological justification of austerity. Basescu described the public sector as a "very fat man hanging on the back of a very thin one, which is the Romanian [private] economy" (May 2010). He argued that austerity would allow private enterprise in Romania to "breathe and grow".

tracts and, the focus of the present study, the closure of 67 public hospitals announced in 2011.

Stoiciu (2012) further provides some qualitative evidence on the short-run consequences of Romanian austerity. Economically, she argues that the evidence is mixed, but suggests that most stated objectives have not been achieved, particularly relating to societal well-being. Politically, the DLs - main party responsible for austerity - saw its popularity decline by over 20 percentage points. In addition, the percentage of people indicating that their life quality had deteriorated rose from 31 percent in 2009 to 68 percent in 2011.

In a suggestive manner (of course, no causal inference should be made here), these numbers indicate that Romanian austerity constituted a strong shock to the electability of the responsible parties. Thus, if the core political hypothesis holds, I should observe correspondingly strong electorally-driven local fiscal adjustments in constituencies hard hit by austerity - and, in particular, in those controlled by politicians aligned with the responsible central government. I now describe the natural experiment used to test this proposition.

3.4   The Shut-Down

I describe the shut-down, the health-sector reform used to appraise how local politicians respond to central austerity.[16]

Announced in 2011 by the national government, the shut-down entailed the proposed closure of 67 public hospitals deemed to be functioning at suboptimal standards. Starting from April 1st the same year, targeted institutions were no longer allowed by law[17] to sign contracts with Romania's National Health Insurance House, effectively discontinuing their inpatient care facilities.

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[16]Note that my objective here is to highlight the measure's most appealing features for the purposes of the subsequent empirical exercise, not to provide an exhaustive overview. For a detailed presentation of the measure, the reader should consult Scintee et al. (2018).

[17]Specifically, Law No. 345/2011 alongside its annex - https://bit.ly/2NvMzYg - which lists the 67 targeted hospitals.

Part of the larger "National Strategy for Hospital Rationalization"[18], the policy's core objective was to improve sustainable public financing in the health sector by reducing wasteful spending and enhancing the efficient use of scarce financial resources - an initiative supported by the IMF as part of the above-mentioned relief deal.

Figure 3 graphically illustrates the shut-down's geographical distribution. With red crosses, I mark the location of hospitals targeted by the measure (the UATs in which they are situated). With blue pluses, I mark the location of non-affected hospitals. Two comments are necessary here. First, no constituencies are marked in both blue and red, because no hospitals were shut-down in UATs where more than one health unit operated. Second, I exclude for the purposes of my analysis those hospitals that specialized in the treatment of a particular ailment (e.g. pneumonia, TBC, psychiatric illnesses and so forth). I do so because these specialized units cannot be seen as appropriate substitutes for closed general-purpose hospitals. Among the 67 hospitals targeted by the measure, two were classified as specialized.[19] This is why only 65 red crosses (rather than 67) appear in Figure 3.

Overall, I argue that investigating how the shut-down affected local policies is particularly well-suited for my purposes for three reasons. First, the shut-down was a highly publicized, controversial, and - importantly - strongly polarized measure, with the governmental coalition being widely perceived as responsible for the shock. In the national media, for instance, it was not uncommon for news stations to run pieces decrying the policy whilst blaming those in power[20]. Empirically, this polarization creates a useful a priori theoretical division in local electoral incentives which I use to separate politically-driven fiscal responses from other changes induced by the reform's structural effects.[21]

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[18]Law No. 303/2011 provides the full, extensive justification behind the shut-down as well as a summary of other reforms implemented alongside it.

[19]Namely, the Pneumonia Senatorium in Guranda (Botosani county), and the TBC prevention unit in Poiana Tapului (Prahova county).

[20]See an example here: https://bit.ly/30rqOxR (in Romanian) - the article, titled "Chaos in the country's abolished hospitals. Hundreds of people, without employment", talks about "chaos" and "pain" resulting from the Ministry's decision, as well as the "desperation" of patients who are forced to re-locate to other hospitals.

[21]Consistent with Nyman (2014), who finds that voters are more likely to punish incumbents for implementing austerity when transparency is high and responsibility can be clearly assigned.

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Figure 3: The Shut-Down: Geographic Illustration
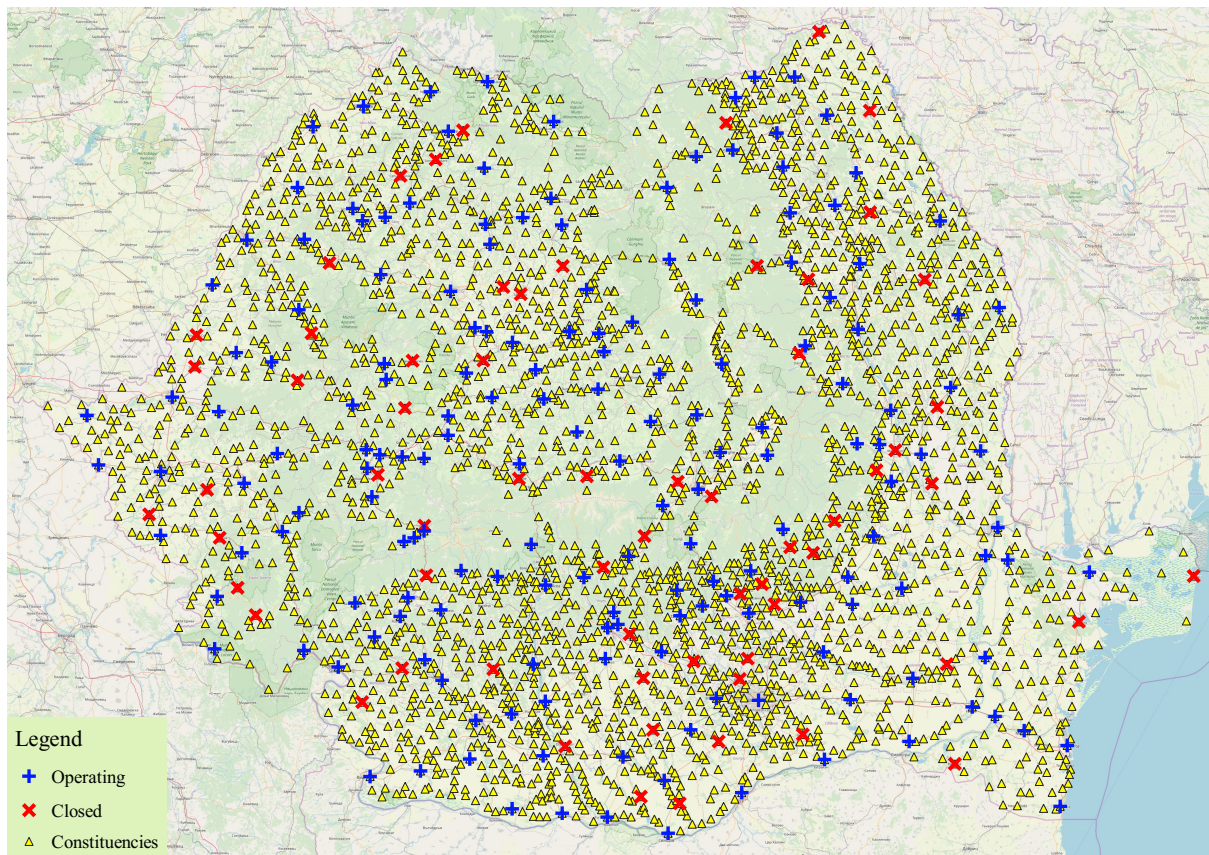
Note: I map the locations of public hospitals at the time of the 2011 shut-down. With red crosses, I mark the location of units discontinued by the policy. With blue pluses, I mark the location of unaffected units. Hospital coordinates were obtained from the Ministry of Health.

Second, the policy was (or at the very least was perceived to be at the time) more impactful in constituencies situated in the "catchment areas" of targeted hospitals. That is, in areas which would lose access to their old hospital, and would therefore be forced to re-locate their patients. Qualitative evidence on such geographic differences in exposure comes both from media stories, which often presented the situation in highly-alarming terms[22], as well as from local political authorities, often themselves decrying the measure

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[22]The news story here: https://bit.ly/2TzPUJt (in Romanian), for instance, is explicitly titled "The complete list of abolished hospitals. *Is the one next to you still there?*", while the one here: https://bit.ly/372YFjl (in Romanian), titled "67 hospitals closed starting next month. Doctors and patients, on the roads", warns about how, following the shut-down, "thousands of patients will be evacuated, because they will not have anyone to look after them anymore. In less than a week, patients from Sacele [i.e. an affected locality], for example, will be removed *by force* from the hospital (...)".

and how it was carried out (Scintee et al., 2018). In my empirical analysis, it is precisely (and solely) this geographic variation in exposure that I exploit to investigate the shut-down's local effects.

Finally, because the measure was not accompanied by so-called "fiscal discipline" impositions (such as requirements to cut certain expenses or raise local taxes) discriminatorily targeted towards constituencies situated in the catchment area of discontinued hospitals, I am able to isolate the "indirect" effects of austerity exposure on local fiscal adjustments from possible mechanical changes that discipline requirements would by design induce. This allows me to make a contribution to the literature that has so far largely focused on evaluating the consequences of fiscal austerity, which, even if causally identified, might not adequately separate politically-driven responses from mechanical effects.[23]

4 Hypotheses and Data

The objective of this section is threefold. First, I develop the paper's hypotheses whereby austerity exposure incentivizes local electorally-driven policy adjustments. Second, I describe the operationalization of the variables used to test these these propositions - namely, the measurement for local voter-friendly spending, the assignment of constituencies as affected or unaffected by the shut-down, and the classification of local politicians by their partisan affiliation. Finally, I outline how the final sample is constructed.

4.1 Austerity and Voter-Friendly Local Spending: Hypotheses

This paper's core narrative closely follows the theory of political budget cycles[24], whereby incumbent parties are expected to manipulate economic and fiscal conditions prior to elections in order to increase their likelihood of being re-elected (Rogoff, 1990).

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[23]Although I investigate a non-fiscal policy precisely in order to isolate this electoral channel, I note that the proposed mechanism does not crucially hinge on this feature, and should be generalizable to fiscal measures as well, as long as the electability of local incumbents is negatively impacted by the shock.

[24]As summarized in Section 2.

Empirically, politicians have been documented to systematically augment investments in items that are most visible to voters in election years - often operationalized as infrastructure projects (see e.g. Veiga and Veiga, 2007) - in order to generate short-term electoral goodwill. Drazen and Eslava (2010), who document a positive effect of election years on local roads, water and energy investments in Colombia, term such investments "voter-friendly". I adopt this terminology.

My investigation builds upon these studies by exploiting an alternative source of variation in the electability of local politicians: austerity measures implemented by the national government. Concretely, I argue that austerity will influence local fiscal decisions in ways akin to changes brought upon by electoral cycles. That is, following austerity, voters in affected areas will choose to punish the political parties (perceived to be) responsible by threatening to vote against their local political candidates. This immediately leads to the first core hypothesis:

H1: *Anticipating the unfavorable response of the electorate, local politicians increase voter-friendly investments in constituencies exposed to austerity.*

Nevertheless, despite being suggestive, one must acknowledge that simply documenting a positive effect of austerity exposure on voter-friendly local investments is not, in and of itself, necessarily indicative of an electoral mechanism driving fiscal choices. To this point, similar effects may be observed if austerity led to systematic changes in the composition of services *demanded* by voters. For example, one might assert that the closure of a nearby public hospital may trigger an increase in demand for better infrastructure in order to allow people to more easily commute to a still operating unit.

Addressing this concern, I argue the following: if changes are partially driven by electoral considerations, then I expect the observed fiscal adjustments to be larger in magnitude in constituencies whose mayor is politically affiliated with the parties ruling at the national level at the time of austerity - perceived by voters as responsible for the shut-down. In line with the electoral mechanism, it is for these politicians that austerity

increases the expected benefits of voter-friendly adjustments, leading to the second core hypothesis:

H2: *The positive relationship between austerity and local voter-friendly investments is driven by fiscal choices made in governmental constituencies.*

Although corroborating this hypothesis does bring evidence supporting a local political motif driving responses to austerity, I acknowledge that other narratives exist that might still explain such a heterogeneous pattern. I return to this issue explicitly in Section 5.3, where I detail a number of these alternative explanations, and discuss what I do to address them.

4.2 Data and Variables

I combine three data sources in order to empirically assess the extent to which the shutdown incentivized local politicians to increase voter-friendly governmental spending.

First, to measure local fiscal outcomes, I use administrative information provided by the Romanian Ministries of Finance and Regional Development. The baseline dataset tracks public revenues and expenses for all Romanian constituencies between 2009 and 2012[25], broken down by financing sources and operational targets, respectively. I use these data to construct a panel by matching all Romanian constituencies over time.

In order to operationalize voter-friendly visible expenses, I define my main dependent variable *VISIBLE SPENDING$_{it}$* as the (log per capita[26]) amount of public funds used for local infrastructure purposes. These include expenses related to the expansion, repair and modernization of local roads (i.e. "pothole-filling"), as well as to the provision of illumination, electricity, housing and water at the local level. In addition, to more

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[25]The complete dataset - employed in various dynamic analyses below - instead extends from 2007 to 2017. I do not go further back in time, since data quality and compatibility drops significantly in years prior to 2007 as the legislation regarding the collection and classification of local fiscal information changed that year - see Law no. 273/2006.

[26]More concretely, I first add one to the computed per capita amounts before taking the logarithm of the resulting figures to account for the fact that, in some years, a number of constituencies reported zero visible investments. Working with log amounts allows me to diminish the influence of outliers.

explicitly measure the relative burden of visible expenses for local finances, I define a second outcome variable, the *VISIBLE RATIO$_{it}$*, as the (log) ratio of local development expenses to internally-generate revenues (i.e. income generated via local taxes)[27].

Proxying for voter-friendly policy adjustments using local infrastructure investments is appealing for two reasons. First, given that local leaders enjoy a high degree of administrative discretion over these expenses, such categories are a priori theoretically subjectable to electorally-driven adjustments. Second, as seen in Section 2, an extensive literature exists documenting a positive association between infrastructure provisions and improved electoral prospects. As Drazen and Eslava (2010, p. 45) argue, projects of this type are electorally appealing because they "*are highly visible and benefit specific (yet potentially large) groups of voters*". Hence, it is precisely by influencing these easily adjustable voter-friendly investments that sub-national incumbents are most likely to recuperate from the popularity shock induced by austerity.

That said, I note here that local visible expenses represent an imperfect and, most likely, incomplete measure for local electorally-driven actions. For instance, this variable does not capture any direct vote-buying efforts, which have been documented to be widespread in related settings (see e.g. Mitra, Mitra and Mukherji, 2017), and which may also moderate the political costs of austerity if carried out. Hence, providing a fully comprehensive picture encompassing the entire set of electorally-motivated responses goes beyond the possibilities of my analysis. Rather, by focusing on fiscal adjustments, I outline a tentative electoral narrative on which future work can build. With this caveat in mind, I now proceed to describe the investigated austerity shock.

In determining to what extent a given constituency is exposed to austerity, I employ registry information from the Ministry of Health. Using corresponding coordinates, I match each general-purpose hospital operating prior to the shut-down to its constituency,

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[27]Echoing the notion of public spending to GDP ratios investigated by Roubini and Sachs (1989), or Fatas and Summers (2018), who emphasize the importance of the relative burden of governmental spending for available finances. I prefer working with internally-generated revenues as this allows me to partially investigate how austerity impacts local self-sufficiency. That said, result remain qualitatively analogous if total income is used as a denominator instead, as shown in Appendix Table A6.

distinguishing units affected and unaffected by the reform. In total, 65 general public hospitals were closed, as illustrated in Figure 3.

I classify constituency i as treated with an indicator variable *HOSPITAL CLOSED$_i$* equal to one if the hospital operating closest its centroid in absolute distance was discontinued. Figure 4 gives a geographical mapping of affected and unaffected constituencies. I mark with red dots treated constituencies, whose centroid is closest to a shut-down unit. Marked with blue triangles are control constituencies.

I argue that this binary classification is intuitively appealing, as long as the effects of the shock are *relatively* more pronounced in treated constituencies[28] - a reasonable assumption in the case of physical entities such as hospitals being closed in a context where healthcare accessibility is already a cause for concern (Vladescu et al., 2016), corroborated by extensive qualitative evidence and testimonies mentioned above.In essence, the treated constituencies can be seen as belonging to the above-mentioned "catchment areas" of the closed hospitals.[29]

Nonetheless, I acknowledge here the possibility of geographical spill-overs, in that voters residing in constituencies classified as unaffected might also be negatively impacted by the shock. For instance, the "control" electorate might be unhappy if they expect patients from "treated" constituencies to be re-located and thus end up agglomerating the still operating nearby hospitals - potentially increasing wait times and decreasing service quality across the board.[30] Then, in anticipation, local politicians in unaffected constituencies might also expect a popularity shock and, consequently, engage in voter-friendly manipulations. In light of this, the estimates retrieved in the following analysis should be regarded as lower, conservative bounds of the shut-down's effects.

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[28]In fact, the required assumption is even weaker - namely, it should be that constituents in the catchment area of targeted hospitals *perceive* themselves to be relatively more affected by the measure.

[29]A similar geographic classification of treated and control constituencies is employed by Huet-Vaughn (2019, p. 301), who studies the electoral effects of highway expenditures in the US.

[30]Although a preliminary investigation into this issue suggests that such re-locations might not have been particularly pertinent ex-post (Ciutan et al., 2012), spill-over effects could still be relevant depending on the *ex-ante expectations* of the control electorate which may drive administrative responses.

Figure 4: Exposure to the Shut-Down: Assignment of Constituencies to the Treatment and Control Groups
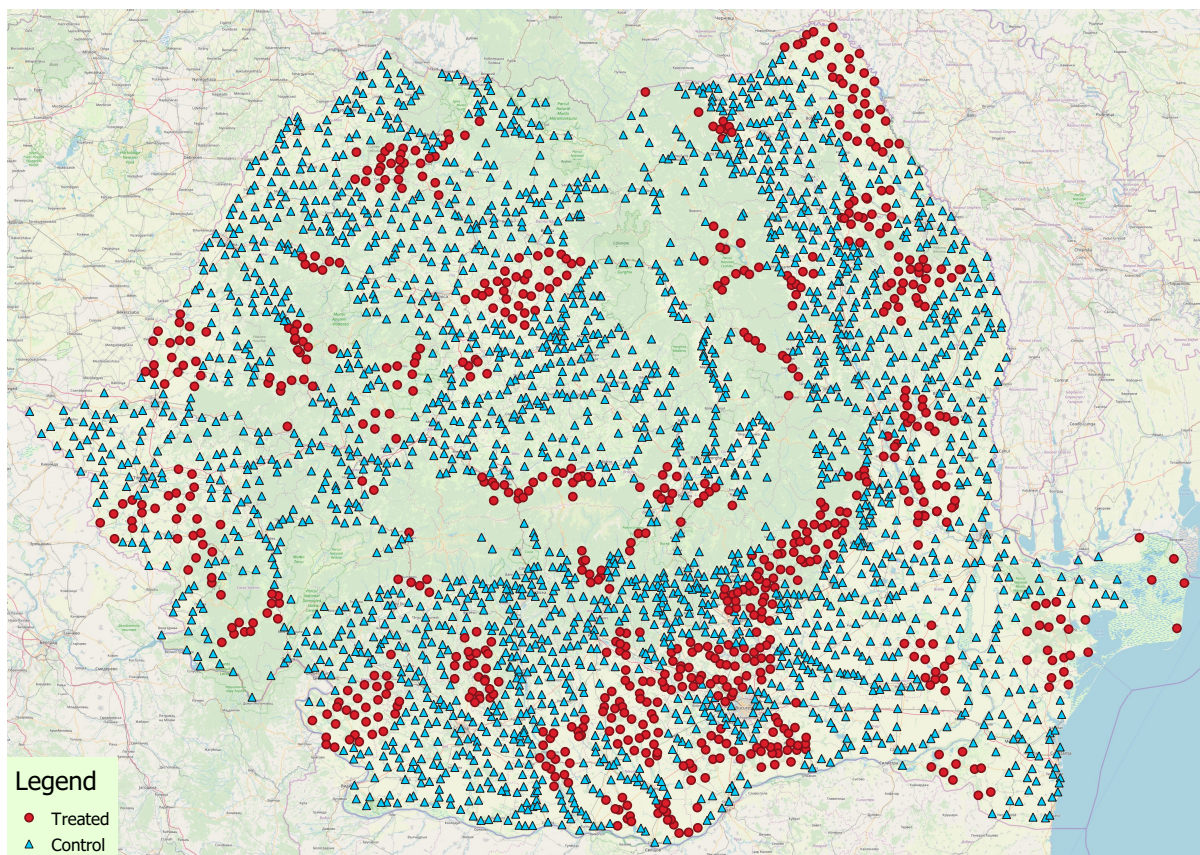
Note: For each constituency, I compute two variables: the distance from its centroid to the nearest general-purpose hospital discontinued in 2011, as well as the distance to the nearest hospital unaffected by the reform. Treated (control) constituencies are those for which the former distance is smaller (larger) than the latter.

Finally, to appraise the moderating role of political affiliations on local responses to austerity (i.e. to test **H2**), I use information on the 2008 mayoral elections provided by the CEB, Romania's electoral authority. For each UAT i, the dataset reveals the partisanship of the resulting incumbents. I categorise a constituency as governmental with a dummy variable $GOVT_i$ equal to unity if the winner of the 2008 mayoral election belongs to either the DLs or the UDMR, the two governmental parties responsible for austerity, and zero otherwise. I note here that, for the purposes of the main analysis, I do not distinguish between the individual parties which form the SLU (i.e. chiefly the

NLs and SDs) and instead treat them together as the main force opposing governmental austerity. In Section 6, however, when further assessing the electoral mechanism driving local responses, I explicitly account for the dissolution of the SLU into individual parties and the subsequent alignment of the NLs with the DLs.

4.3   Sample Construction

I restrict the dataset in five ways. Here, I justify each restriction and briefly describe the resulting final sample.

First, I exclude all constituencies in which public hospitals were actually situated prior to the 2011 shut-down, be them targeted by the reform or not (that is, constituencies marked with either a cross or a plus symbol in Figure 3), and therefore restrict my analysis to "indirectly impacted" units. By doing so, I reduce the possibility of endogenous strategic selection by the central government, which would arise either if substandard hospital performance was partially caused by the evolution of certain local public expenditures, or if the government chose which units to discontinue based on anticipated political responses. In such cases, effects retrieved by comparing affected and unaffected constituencies would be biased as the underlying common trends assumption would be violated. By excluding directly affected constituencies, I reduce selection concerns to the extent that such trends are not perfectly correlated geographically. This approach is similar to Alder (2015).

Second, I remove all constituencies classified as towns and municipalities, and hence focus on communes. I do so to increase comparability, but also because it is in these small, mostly rural constituencies where I expect austerity-induced fiscal adjustments to be most effective electorally. This is in line with the argument detailed in Section 3.1, whereby rural Romanian voters can be classified as "fiscal liberals" (Jones, Meloni and Tommasi, 2012).

Third, I exclude constituencies situated in Ilfov, the small county surrounding Bucharest, as a large portion of its electorate is likely to rely on the capital's health services to begin

Table 1: Cross-Tabulation of Austerity Exposure and Local Official Partisanship

		HOSPITAL CLOSED		
		0	1	Total
GOVT	0	1,288 (75.1)	427 (24.9)	1,715 (64.7)
	1	738 (79.0)	196 (21.0)	934 (35.3)
	Total	2,026 (76.5)	623 (23.5)	2,649 (100)

Note: HOSPITAL CLOSED is a dummy variable equal to one in constituencies belonging to the shut-down's catchment area as defined in text (zero otherwise). GOVT is a dummy variable equal to one in constituencies whose mayor is politically affiliated with the parties comprising the central government responsible for austerity (zero if affiliated with the opposition). The information presented here pertains to the final sample - see Section 4.3 for the restrictions applied.

with and would therefore not be severely impacted by the shut-down even if a nearby local hospital would be discontinued.

Fourth, I remove the UATs whose mayors at the time of the shut-down were not affiliated with either the governing parties or the SLU opposition group. This restriction allows for a cleaner heterogeneity investigation because the extent to which the local leaders affiliated with "other" political entities resisted or supported the central administration is unclear and highly variable. In light of this, I argue that classifying these units as representing the "opposition" would be inappropriate - in some instances, voters might choose to punish those local leaders who expressed prior support for the central government.

Finally, I remove three constituencies which were established after the 2008 local elections.[31] Prior to the subsequent 2012 ballots, these units were assigned apolitical representatives to manage local finances. Hence, I do not expect the investigated electoral mechanism to hold here.

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[31]Namely, Otelec (Timis county), Racsa (Satu Mare county), and Suhurlui (Galati county) - see Laws no. 108/2008, 110/2008, and 86/2010, respectively, for details concerning the foundation of these constituencies.

Following these restrictions, 2,649 constituencies remain in the final sample[32], of which roughly 23.5 percent are classified as exposed to the shut-down and 35.3 percent are represented by a sub-national incumbent affiliated with the central government responsible for austerity. In Table 1, I present a cross-tabulation of these numbers.

Next, in Table 2 panel A, I present the summary statistics for my dependent variables, separately for affected and unaffected constituencies before and after the shut-down. I note that visible expenses increased across the board in the observed timeframe, but more so in treated relative to control constituencies. In anticipation of the main analysis, I further present analogous descriptive statistics separately for governmental and opposition constituencies in panels B and C, respectively. In line with the investigated electoral mechanism, the observed increase in voter-friendly investments appears to be driven by changes noticeable only in governmental areas. I now proceed to formally assess these insights in the main analysis.

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[32]Out of a total of 3,181.

Table 2: Disaggregated Descriptive Statistics of Key Fiscal Outcomes

| (1) | (2) | Before | | | After | | | (9) |
Variable	Status	(3) No. Obs.	(4) Mean	(5) Std. Dev.	(6) No. Obs.	(7) Mean	(8) Std. Dev.	Diff. in Diff.
		Panel A - All Constituencies						
VISIBLE SPENDING	Treated	1,246	317	425	1,246	465	690	+28.0
	Control	4,052	332	383	4,052	452	685	-
VISIBLE RATIO	Treated	1,246	0.867	1.13	1,246	1.25	1.93	+0.125
	Control	4,052	0.882	1.02	4,052	1.14	1.75	-
		Panel B - Governmental Constituencies						
VISIBLE SPENDING	Treated	392	428	489	392	673	891	+129
	Control	1,476	383	371	1,476	499	703	-
VISIBLE RATIO	Treated	392	1.15	1.53	392	1.67	2.24	+0.288
	Control	1,476	0.988	0.974	1,476	1.22	1.80	-
		Panel C - Opposition Constituencies						
VISIBLE SPENDING	Treated	854	266	382	854	370	550	+23.0
	Control	2,576	303	387	2,576	424	674	-
VISIBLE RATIO	Treated	854	0.737	0.858	854	1.06	1.74	+0.054
	Control	2,576	0.821	1.05	2,576	1.09	1.72	-

Note: The two dependent variables considered are visible spending per capita and the visible ratio, as defined in text. Treated (control) constituencies are those whose nearest hospital was affected (unaffected) by the 2011 reform. Statistics pertaining to observations recorded in 2009-2010 and 2011-2012 are presented in columns (3) - (5) and (6) - (8), respectively. For each outcome in each panel, the number in column (9) is computed by subtracting the after-before difference observed in control localities from the after-before difference recorded in treated constituencies.

5 Main Analysis

I conduct the main analysis in three subsections. First, in Section 5.1, I introduce the difference-in-differences specification used to appraise the shut-down's local fiscal effects, and present the results derived therein. In line with the core local electoral mechanism investigated here, I find that the shock led to a meaningful increase in infrastructure investments, driven entirely by the actions of mayors affiliated with those responsible for the closure of hospitals. Next, in Section 5.2, I exploit long-run data availability to verify the existence of pre-austerity "parallel trends" in voter-friendly spending between treated and control constituencies, in order to corroborate the causal interpretation of the shut-down's estimated impact. Lastly, in Section 5.3, I provide a brief interpretation of the results as corroborating a local electoral mechanism driving austerity responses, and highlight several possible alternative narratives which I address in the latter part of the investigation.

5.1 Austerity and Voter-Friendly Spending: Appraising the Shut-Down's Local Effects

I begin by contrasting fiscal adjustments following the shut-down between treatment and control constituencies in order to quantify the local effects of austerity. To this end, I fit the following specification:

$$Y_{it} = \alpha_i + \lambda_t + \omega_{ct} + \beta \text{SHUTDOWN}_{it} + \epsilon_{it} \tag{1}$$

In equation 1, Y_{it} is a measure for local infrastructure spending in constituency i at time t - either *VISIBLE SPENDING*$_{it}$ or the *VISIBLE RATIO*$_{it}$, as defined in Section 4.2. *SHUTDOWN*$_{it}$ captures the product of two binary indicators - *HOSPITAL CLOSED*$_i$[33]

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[33]Equal to one for a given constituency if the nearest hospital was shut-down in 2011 (zero otherwise), as seen above.

and $POST_t$, equal to one for all observations recorded after the measure was implemented (i.e. in 2011 and 2012), and zero otherwise (i.e. in 2009 and 2010).

The coefficient of interest is β, whose estimate provides a measure for the shut-down's local fiscal impact. Given the geographical classification employed, the resulting figure can intuitively be interpreted as the shock's "catchment area" effect. Assuming that infrastructure spending would have evolved similarly in treated and control constituencies absent the shut-down[34], a non-zero β estimate would signify that constituencies affected by austerity experienced noticeable causal changes in visible investments, relative to unaffected units. In line with hypothesis **H1**, I expect this coefficient estimate to be positive.

Importantly, the regression also includes three sets of fixed effects. First, I add a vector of constituency-specific dummies α_i, which absorb the influence of any time-invariant differences in local demographics or political preferences that may play a role in determining public expenditures. Including these variables ensures that the impact of austerity is quantified by solely exploiting within-constituency variation in investments and, therefore, removes a possible source of endogeneity resulting from strategic treatment selection based on time-invariant factors. Second, I include year fixed effects λ_t, which correct for the influence of aggregate macro shocks affecting all units simultaneously at a given point in time. In this setting, these time dummies correct for the influence of other potentially confounding austerity measures carried out concomitantly with the shut-down at the national level, and allow me to retrieve estimates by exploiting local-level heterogeneous geographic exposure to the shut-down as my sole source of variation in austerity intensity. Third, in my preferred specification, I also add a set of county-year dummy variables ω_{ct} in order to filter out possible confounding influences resulting from time-varying county-specific disturbances, arising because local and regional administrations are not fully independent.[35]

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[34]A core assumption assessed in Section 5.2.

[35]For robustness, I also consider specifications which account for linear county-specific trends instead.

Table 3: Austerity and Local Fiscal Adjustments: Baseline Analysis

	Panel A - Dependent Variable: VISIBLE SPENDING			Panel B - Dependent Variable: VISIBLE RATIO		
	(1)	(2)	(3)	(4)	(5)	(6)
SHUTDOWN	0.098**	0.076*	0.080*	0.044**	0.036*	0.038*
	(0.042)	(0.043)	(0.042)	(0.020)	(0.021)	(0.021)
	[0.018]	[0.076]	[0.058]	[0.029]	[0.087]	[0.067]
Observations	10,590	10,590	10,590	10,596	10,596	10,596
Constituencies	2,649	2,649	2,649	2,649	2,649	2,649
Within R-squared	0.022	0.059	0.046	0.017	0.057	0.044
Year FE	YES	YES	YES	YES	YES	YES
Constituency FE	YES	YES	YES	YES	YES	YES
County x Year Dummies	NO	YES	NO	NO	YES	NO
Linear County Time Trends	NO	NO	YES	NO	NO	YES

Note: I report the difference-in-differences coefficient estimates of the shut-down's effect on local voter-friendly governmental spending, retrieved by fitting specification 1 on the final sample. The dependent variables are (log) per capita visible spending, and the (log) visible ratio (as described in text) in panels A and B, respectively. SHUTDOWN captures the interaction of two dummy variables - HOSPITAL CLOSED, which equals one in constituencies whose nearest hospital was targeted by the 2011 reform, and POST, equal to one for observations recorded after the measure was carried out (zero otherwise). All specifications include year and constituency fixed effects. The models in columns (2) and (5) include county-year dummies, while the regressions in columns (3) and (6) include county-specific linear time trends. Standard errors, given in (round brackets), are clustered at the constituency-level; p-values are given in [square brackets]; Significance levels: ***p<0.01, **p<0.05, *p<0.1

Finally, ϵ_{it} is an error term. I cluster standard errors at the constituency-level to account for the fact that the same units are observed at different points in time (Bertrand, Duflo and Mullainathan, 2004).

I present the estimated impact of the shut-down on local *VISIBLE SPENDING$_{it}$* in Table 3, panel A. Across the different model alterations employed, the coefficient estimate on *SHUTDOWN$_{it}$* suggests a positive, statistically significant association between austerity exposure and local voter-friendly investments. Concretely, the FE point estimate present in column (1) implies that the shock led to an approximate 10 percent increase in local infrastructure expenses in treated constituencies, relative to control units - a non-negligible economic effect. Although adding the vector of county-year dummies in column (2) somewhat reduces the retrieved impact, it is reassuring that the qualitative implications of my findings remain unchanged even in this more statistically-demanding model. Looking at column (3), I note that the documented catchment area effect fur-

ther remains statistically and economically unaltered when instead accounting for linear county-specific time trends.

Similarly, the analogous analyses present in panel B show that the results remain qualitatively similar when visible expenses are recorded relative to a constituency's internally-generated revenues. According to the estimate in column (5), the shut-down led to an increase in the *VISIBLE RATIO$_{it}$* of just above 3.6 percent, statistically significant at the ten percent level. Intuitively, this suggests that the "local burden" of voter-friendly spending is augmented by austerity.

Overall, I argue that these baseline results corroborate hypothesis **H1** in that I find some evidence suggesting that austerity exposure triggered an increase in local voter-friendly investments. That said, even assuming (currently uncorroborated) causality, these results do not speak of an underlying electoral mechanism in and of themselves. For instance, austerity exposure might increase local visible spending by increasing the local demand for infrastructure renovations, resulting from the fact that patients must be re-located after their hospital was discontinued. In such scenarios, the electoral considerations of local officials might not play a sizeable role in shaping the observed response.

In light of this, I proceed to empirically explore the electoral mechanism underlying the local responses observed here in the remainder of this paper.

I begin making some progress by assessing whether the documented relationship between austerity exposure and visible spending is driven by the actions of governmental-affiliated local leaders, in line with hypothesis **H2**. Empirically, I do so in two complementary manners. First, I estimate the following interaction specification:

$$\text{Y}_{it} = \alpha_i + \lambda_t + \lambda_{tG} + \omega_{ct} + \beta_0 \text{SHUTDOWN}_{it} + \beta_1 \text{SHUTDOWN}_{it} * \text{GOVT}_i + \epsilon_{it} \quad (2)$$

Equation 2 is a simple extension of equation 1 that allows me to test whether the estimated catchment area effect is substantially stronger in governmental constituencies. Technically, I do so by augmenting the baseline model with a term capturing the interac-

tion between the shock exposure measure $SHUTDOWN_{it}$ and the government-affiliation dummy $GOVT_i$.[36] In addition, I also include a vector of variables for the interaction between $GOVT_i$ and the set of year dummies - λ_{tG} above - to account for the fact that fiscal outcomes in governmental and opposition constituencies would have likely evolved differently regardless of austerity exposure.[37] In light of **H2**, I expect β_1's estimate to be positive.

Alternatively, I test the moderating role of mayoral affiliations by running equation 1 separately for the subsamples of governmental and opposition constituencies, respectively. Although less efficient due to the reduced number of observations included in this analysis, this approach does not require the coefficient on all other explanatory variables[38] to be the same between the two groups (see Shi and Svensson, 2006 for a related discussion). Here, I expect a positive association between austerity exposure and voter-friendly investments in governmental constituencies. At the same time, I am agnostic concerning the sign and size of this relationship in opposition constituencies.[39]

My results, presented in Table 4, provide some prima facie evidence in support of the underlying electoral channel driving local policy responses to austerity. In panel A, the coefficient on the interaction term enters the regression with the expected positive sign in a highly statistically significant manner, regardless of the fitted model or the measurement for visible spending considered.

Quantitatively, looking at panels B and C - where the baseline specification is separately fitted for governmental and opposition constituencies, respectively - the point estimate suggests that austerity exposure led to a noticeable, economically meaningful

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[36]Note that I do not include GOVT as a separate regressor here because it is time-invariant - its direct effect on local spending is already captured by the constituency-specific vector of dummies.

[37]Results are further robust to the inclusion of interaction terms between the $GOVT$ dummy and the county-by-year fixed effects ω_{ct}. Findings from this more demanding specification are available upon request.

[38]Here, the influence of county-by-year dummies.

[39]Even if the core electoral-mechanism investigated here is not pertinent / is substantially weaker in opposition-represented constituencies, it is still possible that the shut-down lead to structural changes which ultimately impacted local fiscal outcomes in a priori indeterminate ways irrespective of political affiliations. Therefore, I cannot confidently predict (and it goes beyond the objective of my paper to do so) that the catchment area coefficient estimate will be indistinguishable from zero in the opposition subsample.

Table 4: Austerity and Local Fiscal Adjustments: Heterogeneity by Mayoral Political Affiliation

	Dependent Variable: VISIBLE SPENDING			Dependent Variable: VISIBLE RATIO		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A - Interaction Analysis						
SHUTDOWN	0.012	0.005	0.005	0.009	0.007	0.008
	(0.049)	(0.049)	(0.049)	(0.023)	(0.023)	(0.023)
	[0.811]	[0.920]	[0.923]	[0.695]	[0.746]	[0.739]
SHUTDOWN * GOVT	0.249***	0.219**	0.230**	0.104**	0.089*	0.092**
	(0.092)	(0.090)	(0.090)	(0.046)	(0.045)	(0.045)
	[0.007]	[0.016]	[0.011]	[0.025]	[0.055]	[0.041]
Observations	10,590	10,590	10,590	10,596	10,596	10,596
Constituencies	2,649	2,649	2,649	2,649	2,649	2,649
Within R-squared	0.025	0.062	0.049	0.019	0.058	0.045
Panel B - Sample Restricted to Governmental Constituencies						
SHUTDOWN	0.261***	0.213**	0.221***	0.113***	0.089**	0.096**
	(0.078)	(0.084)	(0.082)	(0.040)	(0.043)	(0.042)
	[0.001]	[0.012]	[0.007]	[0.001]	[0.037]	[0.023]
Observations	3,735	3,735	3,735	3,736	3,736	3,736
Constituencies	934	934	934	934	934	934
Within R-squared	0.014	0.080	0.052	0.014	0.074	0.050
Panel B - Sample Restricted to Opposition Constituencies						
SHUTDOWN	0.012	0.020	0.018	0.009	0.012	0.012
	(0.049)	(0.050)	(0.049)	(0.023)	(0.023)	(0.023)
	[0.811]	[0.685]	[0.712]	[0.695]	[0.603]	[0.609]
Observations	6,855	6,855	6,855	6,860	6,860	6,860
Constituencies	1,715	1,715	1,715	1,715	1,715	1,715
Within R-squared	0.032	0.074	0.054	0.022	0.070	0.050
Year FE	YES	YES	YES	YES	YES	YES
Constituency FE	YES	YES	YES	YES	YES	YES
County x Year Dummies	NO	YES	NO	NO	YES	NO
Linear County Time Trends	NO	NO	YES	NO	NO	YES

Note: I quantify the moderating influence of mayoral political affiliations for the shut-down's effects on local voter-friendly governmental spending. The dependent variables are (log) per capita visible spending and the (log) visible ratio, in columns (1) - (3) and (4) - (6), respectively. SHUTDOWN captures the interaction of two dummy variables - HOSPITAL CLOSED, which equals one in constituencies whose nearest hospital was targeted by the 2011 reform, and POST, equal to one for observations recorded after the measure was carried out (zero otherwise). GOVT is a dummy variable equal to one in constituencies whose mayor is politically affiliated with the parties comprising the central government which carried out austerity (zero if affiliated with the opposition). Coefficient estimates in panel A are retrieved by fitting specification 2 on the entire final sample. Coefficient estimates in panels B and C are obtained by running regression 1 separately for the subsample of constituencies where GOVT equals one and zero, respectively. All specifications include year and constituency fixed effects. The models in columns (2) and (5) include county-year dummies, while the regressions in columns (3) and (6) include county-specific linear time trends. The specification employed in panel A also includes interaction terms between GOVT and the year dummies. Standard errors, given in (round brackets), are clustered at the constituency-level; p-values are given in [square brackets]; Significance levels: ***p<0.01, **p<0.05, *p<0.1

increase in voter-friendly expenses (estimated at roughly 21 percent in per capita terms, and 9 percent in relative terms in my preferred specifications, respectively) exclusively in governmental constituencies. In contrast, I am unable to reject the null zero-effect hypothesis in the opposition subsample, regardless of the model employed.

Overall, I argue that these findings provide empirical support for hypothesis **H2**.

5.2 Dynamic Model: Corroborating the Identification Assumption

Despite being suggestive of a domestic electoral motif partially explaining how local politicians respond to central austerity, interpreting the above coefficient estimates as causal crucially hinges on the following assumption holding: *Had the shut-down not been carried out, voter-friendly investments would have evolved similarly in treated and control constituencies.* Before further discussing the interpretation and limitations of the main results, in this section, I exploit long-run data availability to corroborate this postulate.[40]

Concretely, I compare pre-intervention trends in voter-friendly investments between treatment and control constituencies, exploiting the fact that the dataset provides suitable information from 2007 onwards. If the shut-down does create appropriate quasi-random spatial variation, I should observe few trend divergences between affected and unaffected constituencies prior to 2011, when the measure was implemented.

Empirically, I employ a dynamic leads-and-lags specification to assess the existence of pre-intervention parallel trends. Specifically, I run:

$$Y_{it} = \alpha_i + \lambda_t + \omega_{ct} + \sum_{j=2007}^{2009} (\beta_j \mathrm{ShD}_{it}^j) + \sum_{j=2011}^{2013} (\beta_j \mathrm{ShD}_{it}^j) + \epsilon_{it} \tag{3}$$

---

[40]Note, however, that the identification assumption can never be fully "confirmed" as this would require observing a counterfactual scenario wherein the treatment did not take place. While showing that pre-intervention trends in visible investments are "parallel" (which I do in this section) partially reduces endogeneity concerns, it does not rule them out completely. A bias might still arise, for instance, if other disturbances heterogeneously impact treated constituencies *simultaneously* with austerity. I return to this issue in Section 6 where I argue that the results retrieved when analyzing the 2014 marriage further retroactively corroborate the internal consistency of the main findings.

In equation 3, Y_{it}, α_i, λ_t, ω_{ct} and ϵ_{it} have the same interpretation as their counterparts given in equation 1.[41] The ShD$_{it}^j$ terms (short for "Shut-Down") are a set of dummy variables equal to one in period j solely for constituencies in the shut-down's catchment areas.[42] Note that I exclude an indicator corresponding to 2010 (the last pre-closures year) to avoid the dummy variable trap. Hence, all retrieved estimates should be interpreted in relation to 2010 values.[43]. Moreover, although I also have observations for later years (up until 2017), I do not include this information here in light of the 2014 marriage, which I argue had further systematic effects in constituencies affected by the shut-down. An analysis of these data warrants its own considerations and caveats, and I return to it in Section 6

Finally, since I posit that the political affiliation of local incumbents moderates the effects of austerity provided that the electoral mechanism holds, I run specification 3 both on the overall sample, as well as separately for subsamples where $GOVT_i$ is one and zero, respectively. On the one hand, this allows me to corroborate the existence of parallel trends conditional on mayoral affiliations. In addition, I can appraise the heterogeneity of the shut-down's dynamic effects.

Returning to equation 3, the β_j coefficients capture any systematic divergences in fiscal adjustments between treated and control units in year j. Intuitively, the coefficient estimates for the lead regressors β_{2007} - β_{2009} allow me to test whether significant trend differences exist between the treatment and control constituencies in periods where the shut-down had not yet been implemented. Under the core common trends assumption, I do not expect these estimates to be systematically statistically different from zero. The lag coefficients capture differences in the years following the closure of hospitals. In governmental constituencies, I expect their estimates to be positive, and I am once more agnostic with regards to their sign and size in the opposition subsample.

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[41]Namely voter-friendly government spending, locality fixed effects, year fixed effects, county-year dummies or county-specific linear time trends and the error term.

[42]Technically, ShD$_{it}^j$ is the product of *HOSPITAL CLOSED$_i$* and a dummy variable equal to one in year j, zero otherwise.

[43]Excluding the final pre-intervention year is a conventional approach when estimating dynamic difference-in-differences models - see e.g. Fetzer (2019).

Results are illustrated in Figure 5, where I plot the estimated β_j coefficients against the year corresponding to their regressors, alongside corresponding 90 percent confidence intervals. The dependent variable is *VISIBLE SPENDING$_{it}$*.[44] In panel A, I show the estimated catchment area effect retrieved from fitting the model on the entire final sample, while in panel B, I present results derived separately in the subsamples of governmental and opposition constituencies, respectively. For illustration purposes, I combine the two in panel C.[45]

Corresponding numerical estimates are given in Table 5, once again obtained by analyzing the entire sample as well as the governmental and opposition subsamples separately, in panels A, B and C, respectively. My preferred specifications, which correct for potentially confounding county-specific disturbances, are shown in columns (2), (5) and (8).

All in all, I argue that the pre-austerity lead estimates corroborate the underlying identification assumption, with treatment and control constituencies following similar, albeit not perfectly parallel trends prior to the shut-down. Importantly, this also holds when conditioning on the political affiliation of mayors - suggesting that the baseline heterogeneous findings cannot be explained by any pre-treatment affiliation-specific differential visible spending trends. Technically, in my preferred specifications, I am unable to reject the zero-effect null hypothesis for any lead coefficient estimate regardless of the sample employed.

Furthermore, in addition to validating comparability, these findings once more echo this paper's local electoral narrative. As in the baseline, I find a noticeable increase in voter-friendly investments in treated constituencies, driven by the actions of governmental politicians.

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[44]For conciseness purposes, I do not present here results from regressions where the *VISIBLE RATIO* is used as the dependent variable. They are qualitatively consistent with the findings discussed here and are available in the Appendix.

[45]Here, instead of showing the confidence intervals which would clutter the diagram, I mark statistically significant estimates with different amounts of stars depending on their significance levels - see the corresponding note for details.

Figure 5: Austerity and Local Fiscal Adjustments: Dynamic Analysis

(a) Coefficient Estimates Retrieved by Analysing the Full Final Sample

VISIBLE SPENDING
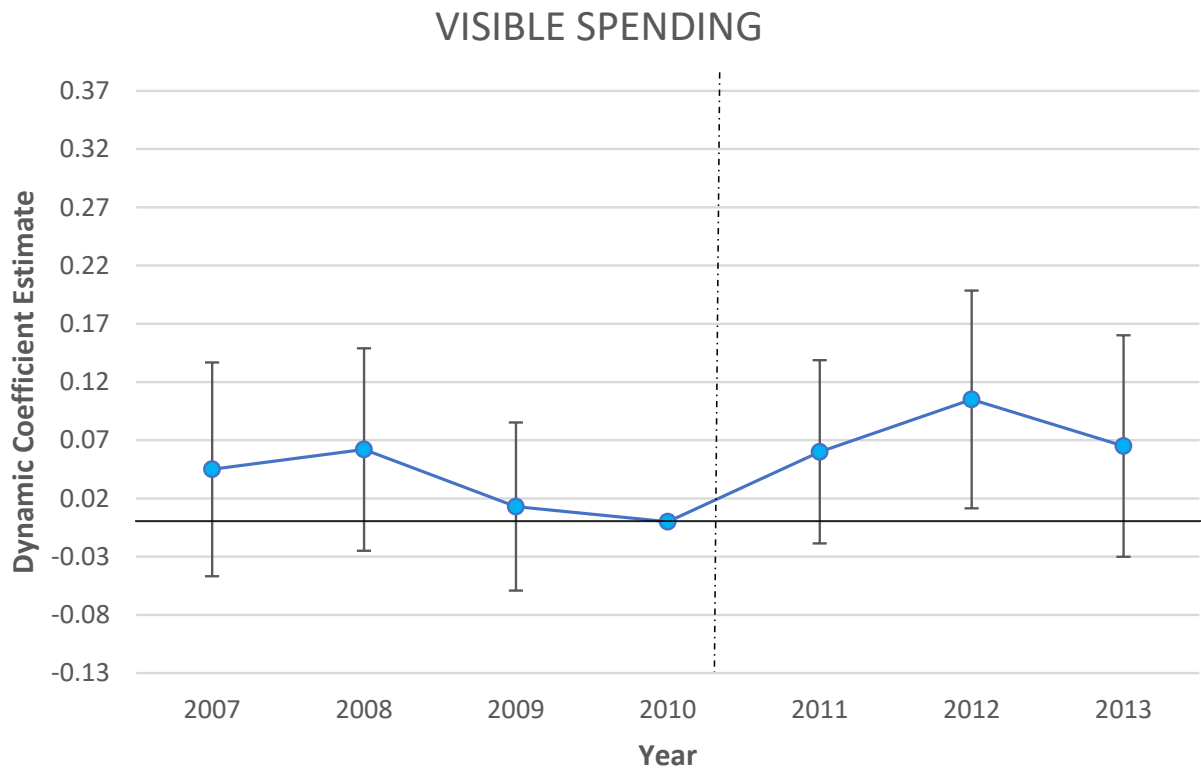
Note: I illustrate the shut-down's effect on local voter-friendly spending. To do so, I plot the coefficient estimates, alongside their 90 percent confidence intervals, retrieved from fitting specification 3 (including county-year fixed effects) on the full sample, against the year corresponding to each regressor. The dotted black line marks April 2011, when the shut-down was carried out. The dependent variable is (log) visible spending per capita.

(b) Coefficient Estimates Retrieved by Analysing Governmental and Opposition Constituencies Separately

VISIBLE SPENDING (GOVT = 1)

VISIBLE SPENDING (GOVT = 0)
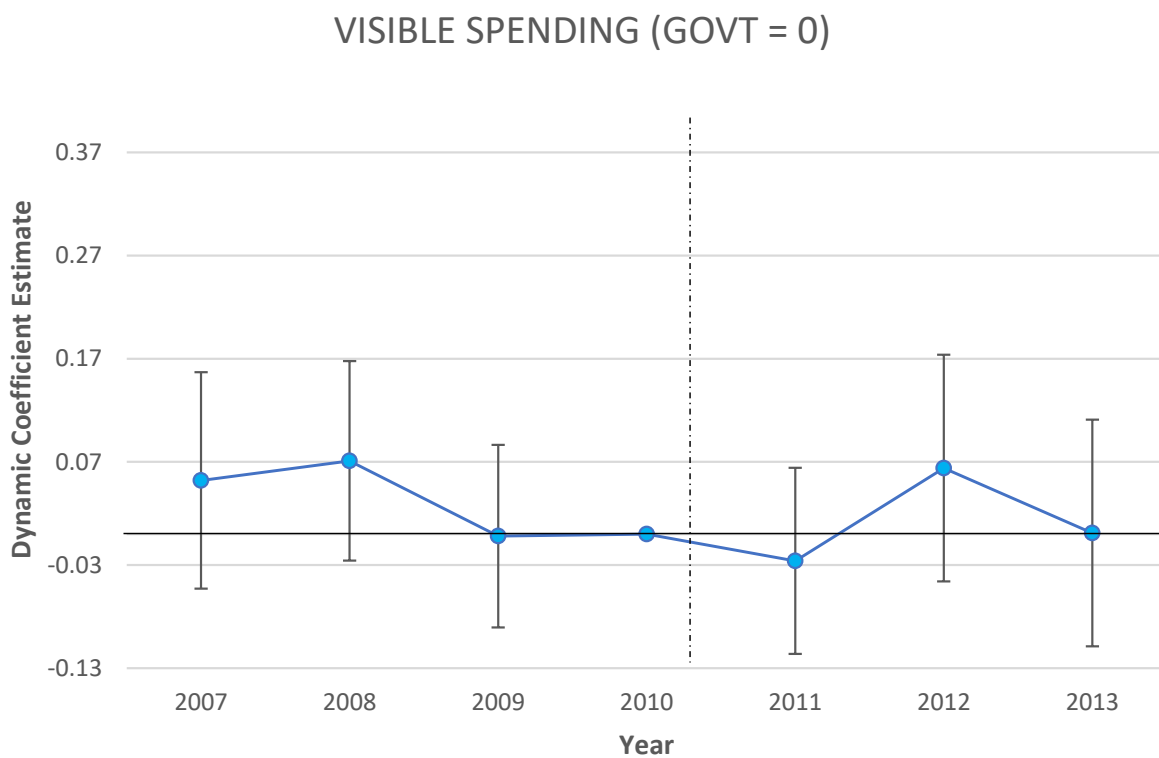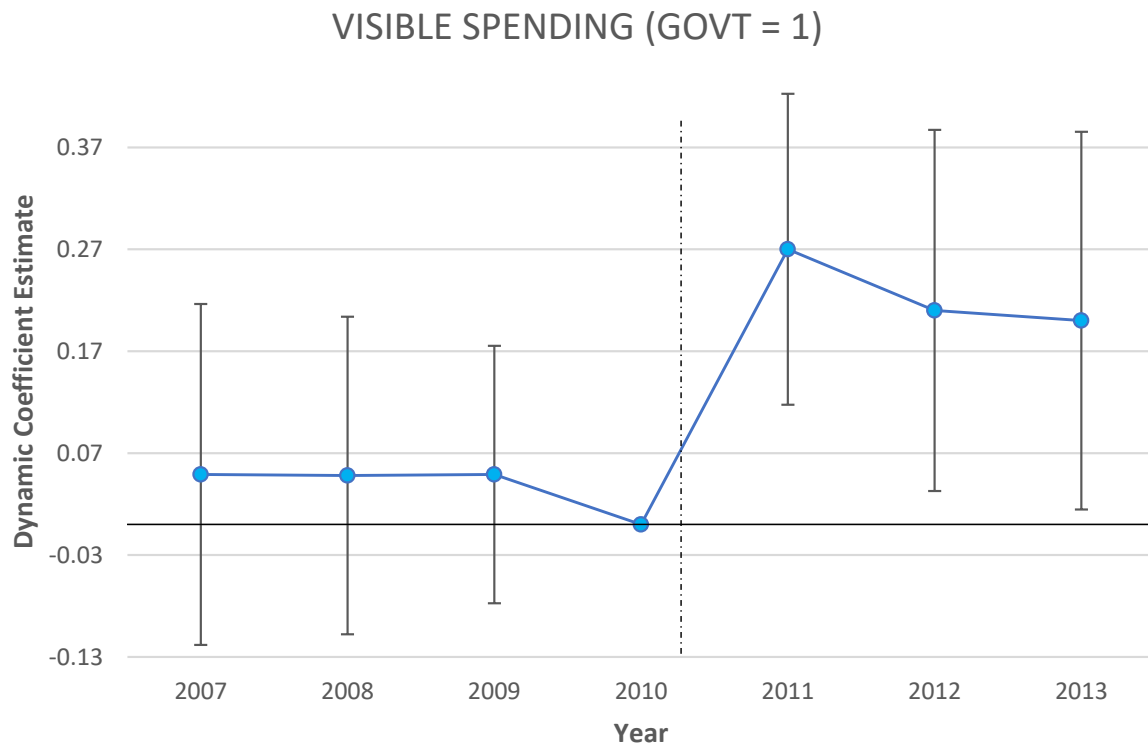
Note: The coefficient estimates are retrieved by fitting specification 3 separately on the subsample of governmental and opposition constituencies in the first and second graph, respectively.

(c) Coefficient Estimates Retrieved by Analysing Governmental and Opposition Constituencies Jointly Illustrated

VISIBLE SPENDING
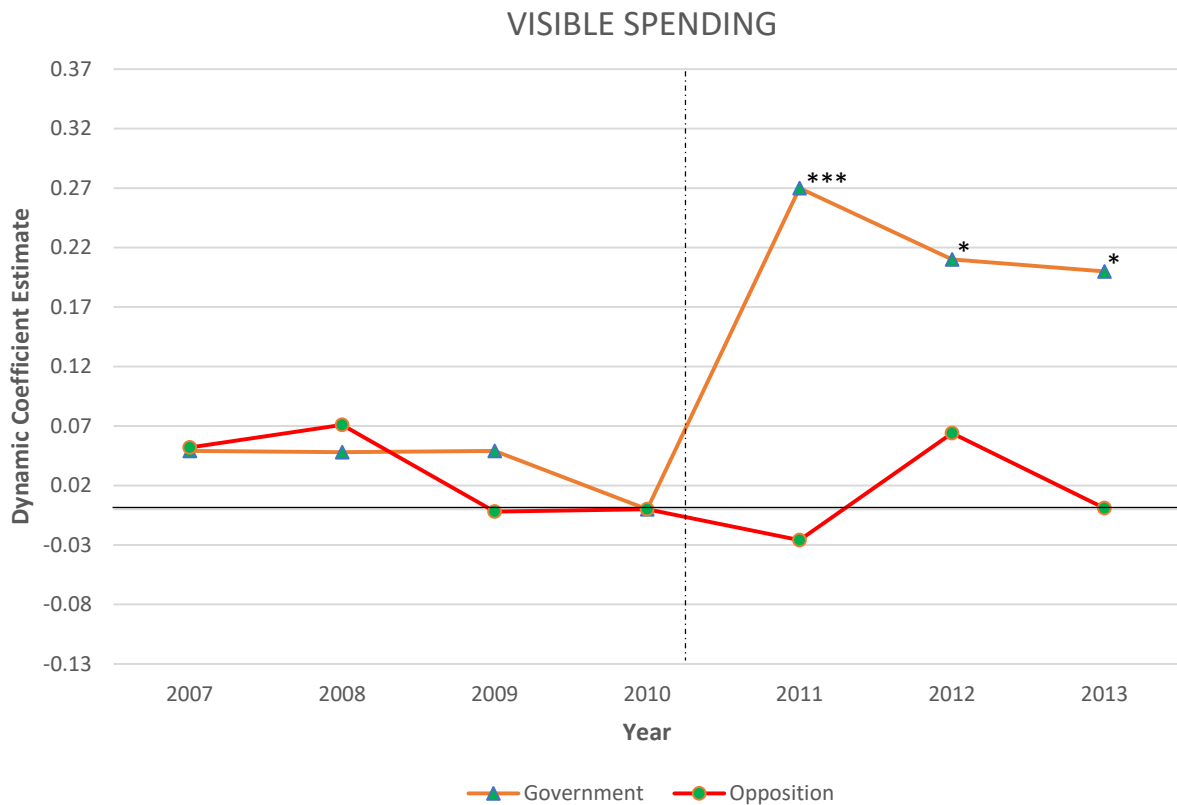
Note: To better visualize the observed heterogeneous political responses to the shut-down, I combine the first (corresponding to the orange line) and second (corresponding to the red line) graphs from panel B above. For expositional clarity, confidence intervals are not presented - instead, significant coefficient estimates are marked with stars, as follows: ***p<0.01; **p<0.05; *p<0.1; starless coefficient estimates are statistically indistinguishable from zero in my preferred specification.

Table 5: Austerity and Local Fiscal Adjustments: Dynamic Analysis

	Dependent Variable: VISIBLE SPENDING								
	Panel A			Panel B			Panel C		
	Full Sample			GOVT = 1			GOVT = 0		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ShD_{2007}	0.029	0.045	0.043	-0.098	0.049	-0.013	0.066	0.052	0.054
	(0.053)	(0.056)	(0.054)	(0.099)	(0.102)	(0.099)	(0.062)	(0.064)	(0.062)
	[0.582]	[0.419]	[0.426]	[0.321]	[0.633]	[0.896]	[0.285]	[0.416]	[0.389]
ShD_{2008}	0.046	0.062	0.055	-0.137	0.048	-0.080	0.091	0.071	0.083
	(0.052)	(0.053)	(0.052)	(0.096)	(0.095)	(0.094)	(0.058)	(0.059)	(0.058)
	[0.382]	[0.248]	[0.296]	[0.151]	[0.611]	[0.395]	[0.117]	[0.232]	[0.157]
ShD_{2009}	0.006	0.013	0.010	0.022	0.049	0.051	-0.003	-0.002	-0.007
	(0.042)	(0.044)	(0.042)	(0.077)	(0.077)	(0.077)	(0.051)	(0.054)	(0.051)
	[0.896]	[0.777]	[0.817]	[0.771]	[0.525]	[0.508]	[0.954]	[0.970]	[0.884]
ShD_{2011}	0.082*	0.060	0.077*	0.306***	0.269***	0.278***	-0.031	-0.026	-0.027
	(0.046)	(0.048)	(0.046)	(0.089)	(0.093)	(0.089)	(0.053)	(0.055)	(0.053)
	[0.076]	[0.207]	[0.093]	[0.001]	[0.004]	[0.002]	[0.564]	[0.642]	[0.619]
ShD_{2012}	0.119**	0.105*	0.110**	0.239**	0.207*	0.182*	0.051	0.064	0.059
	(0.054)	(0.057)	(0.054)	(0.099)	(0.108)	(0.101)	(0.065)	(0.067)	(0.064)
	[0.028]	[0.066]	[0.042]	[0.016]	[0.055]	[0.072]	[0.430]	[0.340]	[0.356]
ShD_{2013}	0.061	0.065	0.048	0.153	0.202*	0.068	-0.001	0.001	0.011
	(0.055)	(0.058)	(0.056)	(0.107)	(0.113)	(0.110)	(0.064)	(0.067)	(0.064)
	[0.270]	[0.263]	[0.396]	[0.153]	[0.075]	[0.540]	[0.987]	[0.992]	[0.859]
Observations	18,533	18,533	18,533	6,536	6,536	6,536	11,997	11,997	11,997
Constituencies	2,649	2,649	2,649	934	934	934	1,715	1,715	1,715
Within R-squared	0.063	0.111	0.086	0.082	0.173	0.125	0.079	0.131	0.102
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Constituency FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
County x Year Dummies	NO	YES	NO	NO	YES	NO	NO	YES	NO
Linear County Time Trends	NO	NO	YES	NO	NO	YES	NO	NO	YES

Note: I quantify the shut-down's effects on local voter-friendly governmental spending in a dynamic framework, by fitting specification 3 on the entire final sample, on the subsample of governmental constituencies, and on the subsample of opposition constituencies, in columns (1) - (3), (4) - (6) and (7) - (9), respectively. ShD_j captures the interaction of two dummy variables - HOSPITAL CLOSED, which equals one in constituencies whose nearest hospital was targeted by the 2011 reform, and a binary variable equal to one in year j (zero otherwise). The dependent variable is (log) per capita visible spending. Standard errors, given in (round brackets), are clustered at the constituency-level; p-values are given in [square brackets]; Significance levels: ***p<0.01, **p<0.05, *p<0.1

5.3 Discussion and Alternative Interpretations

Overall, I posit that my results provide some evidence indicating that austerity exposure can lead to noticeable increases in voter-friendly spending at the local level. In addition, the underlying channel appears to be electoral in nature given the documented heterogeneous pattern, whereby the effect is almost entirely driven by the actions of local politicians affiliated with those who orchestrated the shut-down.

That said, while suggestive, this interpretation should not be taken at face value since a number of alternative mechanisms which do not account for local electoral incentives may potentially explain these findings instead. I outline four such narratives here.

First, although the results of the dynamic leads-and-lags analysis conducted above corroborate the existence of pre-intervention parallel visible investment trends, these findings do not completely rule out the possibility of strategic treatment targeting based on *anticipated* local policies. For instance, the central government might have systematically implemented austerity in regions where it expected (or even mandated) affiliated mayors to increase voter-friendly spending in preparation for the 2012 elections. If so, the same heterogeneous pattern would have emerged even absent the shut-down, and the estimated coefficients would instead be capturing these anticipated changes.

Second, given that the partisanship of local incumbents is not randomly assigned to constituencies, the baseline heterogeneous pattern might be driven by other demographic or economic characteristics which correlate with mayoral affiliations. To exemplify, if governmental constituencies were systematically more populated by older or less healthy voters, then a similar empirical pattern would plausibly emerge in response to the shock, even absent any electorally-driven adjustments. Rather, underlying demographics might simply increase the sensitivity of the local population to health-sector measures, thus compromising the validity of the interaction analysis. Given that the affiliation of local politicians can be correlated with numerous underlying demographics (many of which unobservable to researchers), ruling out these alternative channels is challenging.

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Third, it is possible that the shut-down indeed led to structural changes in local-level public service demands (homogenous across the board), but only governmental leaders, taking advantage of their alignment with the central government, were able adjust to these changes in a timely manner - as a result of preferential access to centrally-controlled financial resources. Then, the same pattern could be explained by a local constraints story rather than by electoral considerations.

Finally, I note that several potentially relevant personal characteristics of local leaders might vary non-randomly across political parties and it might be these underlying discrepancies that explain the observed pattern. For instance, if governmental mayors were systematically more competent or experienced, I would expect to observe a more timely response in their constituencies, once more explaining the results. Other personal factors such as personal ideology or education might also play a role.

Unfortunately, ruling out these alternative mechanisms is an extremely challenging and data-intensive empirical task which, I argue, cannot be satisfactorily carried out by just focusing on the shut-down as the sole source of quasi-experimental variation. Simply put, the data requirements are too demanding, and even if additional information is retrieved, several of the above explanations hinge on unobservable confounding influences, and cannot be decisively discarded.

In the remainder of this paper, I make some progress towards ruling out these alternative narratives by exploiting the effects of the 2014 marriage as a second natural experiment.

6   The Marriage: Ruling Out Alternative Explanations

I exploit a second natural experiment, consisting of a re-alignment of the country's national political parties, in order to further corroborate the electoral mechanism linking austerity exposure to increased local voter-friendly spending. I first provide the back-

ground and describe the shock in Section 6.1. I explain why this setting entails a convenient opportunity to rule out several alternative narratives that might otherwise be consistent with the main findings. The key hypothesis is also introduced here. Then, in Section 6.2, I empirically assess the postulate and present the results. I interpret these findings and discuss how they corroborate the main evaluation.

6.1 Political Context and Key Hypothesis

I present a condensed timeline of this second natural experiment - that said, I once more note that the objective here is to highlight the features most relevant for the subsequent empirical evaluation, not to give an exhaustive overview.

The timeline is as follows:

- As previously discussed, both local and parliamentary elections took place in 2012, organized on June 10 and December 9, respectively. The SLU, the opposition union at the time of austerity[46], won both ballots decisively.[47]

- Following these victories, the SLU continued to govern with Victor Ponta (the leader of the SDs) as prime-minister, and the DLs in opposition.

- At the time, the union maintained that it would be supporting a joint candidate in the upcoming November 2014 presidential ballot. Nonetheless, throughout 2013, cross-party disagreements within the SLU became more and more commonplace.[48]

- Eventually, after a year of mounting tensions, the SLU officially dissolved on February 25 2014 when the NLs broke off from the governing coalition and entered the opposition. At this point, we can thus speak of three major, independent political

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[46]In power throughout these elections, however, after the DL/UDMR government was ousted by a Parliamentary vote of no confidence on April 27 2012.

[47]Securing over half of the available mayoral seats, and obtaining more than 60 percent of the total votes cast in the parliamentary race.

[48]For instance, the SDs and NLs had diverging views on a number of economic aspects pertaining to taxation policies, as well as on several political dimensions such as a proposal to amend the constitution. In fact, in May 2013, several coalition members were already suggesting that the union be disbanded, to be replaced instead by an alliance of independent political parties.

entities: the governing SDs[49], the NLs, and the DLs, the latter two now jointly opposing the Ponta regime.

- Importantly, following the disbandment of the SLU, the SDs announced that they would now instead be nominating one of their members for the upcoming presidential race.[50]

- Crucially for my investigation, facing strong electoral pressures as a result (as polling largely indicated that the SD nominee was likely to win the presidency), the NLs and DLs began negotiations following the dissolution, and ultimately announced that they would be supporting a joint presidential ticket with Klaus Iohannis as their nominee.[51]

- To this end, they formed an electoral coalition[52] in July and, following Iohannis' successful presidential bid, officially merged together under the "National Liberal Party" name. Effectively, the DLs, as an independent political party, ceased to be.

I label the above series of events, chiefly consisting of the SLU's dissolution and the subsequent allying of the NLs and DLs, "the marriage", and posit that this re-arrangement of the country's major political parties constitutes a convenient second natural shock which allows me to further corroborate the local electoral mechanism driving responses to austerity.

For this, recall that in Section 5, I have investigated the hypothesis whereby exposure to the shut-down incentivized an increase in local voter-friendly spending by virtue of lowering the electability of local officials affiliated with those responsible. The assumption behind this argument, therefore, was that austerity had been negatively perceived by (a significant fraction of) the local electorate and thus, anticipating being sanctioned at the ballot box, governmental incumbents engaged in voter-friendly fiscal adjustments.

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[49]Together with their minor allies, the Conservative Party, which for expositional convenience I will henceforth include under the "SD" denomination.

[50]Which, in July that year, was officially revealed to be Ponta.

[51]Who, in the meantime, became the leader of the NLs.

[52]The Christian Liberal Alliance.

Here, the reasoning is similar and once more hinges on a political motif: concretely, I argue that *if* the electoral channel and its underlying assumption are valid, voters previously exposed to shut-down will remember the shock, as well as the now ex-governmental parties (chiefly the DLs) as those who orchestrated it. Therefore, these voters will be unhappy with and choose to sanction any political entities which contemporaneously ally themselves with those responsible (i.e. the NLs following their "marriage" with the DLs). This leads to this paper's third hypothesis:

H3: *Anticipating the unfavorable response of the electorate to the marriage, local politicians increase voter-friendly investments in constituencies previously exposed to austerity. This effect will materialize, however, exclusively in NL constituencies.*

Statistically-speaking, this setting is appealing as it provides a unique natural separation between two political groups which are, a priori, expected to react heterogeneously to the marriage. Under the paper's electoral mechanism, the marginal benefits associated with increasing voter-friendly spending go up in NL constituencies previously exposed to the shut-down, because it is in these constituencies that mayors expect electoral repercussions in response to the marriage. Implicitly, I assume that a number of voters in constituencies exposed to the shut-down remember the "hurt" inflicted upon them (or, at the very least, the perceived intention thereof) by the previous administration which the NL opposed and, following the marriage, will choose to sanction the NLs.

At the same time, SD constituencies[53] constitute a convenient "placebo" group which allows me to separate any electorally-motivated impacts from other possibly confounding effects.[54] Importantly, these predictions contrast with those made in the main analysis, where, following the shut-down, both NL and SD politicians were expected to have a null or weaker response relative to those affiliated with the responsible central government.

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[53]And to some extent, constituencies aligned with the previous DL and UDMR governmental entities.

[54]Although, once more, it goes beyond the purposes of this investigation to claim that no lagged structural effects occurred. Therefore, more conservatively, I acknowledge that changes might occur in SD areas as well, but, under the electoral-channel, these should be weaker relative to those observed in NL constituencies.

To this point, if the documented responses to the shut-down are either spurious or pri-marily driven by an underlying channel wherein electoral considerations play a negligible role, I do not expect **H3** to hold.

6.2   Empirical Analysis

I assess **H3** by investigating how local politicians responded to the marriage, conditional on their partisan affiliation. To do so, I first replace the $GOVT_i$ dummy previously employed with an indicator variable POL_i, which takes on three distinct values: one in constituencies affiliated with the ex-governmental parties responsible for austerity[55], two in NL constituencies, and three in SD territories. Before proceeding, it is important to note that POL_i is defined in terms of affiliations at the time of austerity, which does not perfectly match contemporaneous partisanship given that mayoral elections took place mid-2012. Therefore, my results will give imperfect estimates of the marriage's local effects[56]. With this caveat in mind, I proceed by fitting the following model, separately for subsamples defined by the three values of POL_i:

$$\text{Y}_{it} = \alpha_i + \lambda_t + \omega_{ct} + \sum_{j=2007}^{2009} \left(\delta_j \text{ShD}_{it}^j \right) + \sum_{j=2011}^{2013} \left(\beta_j \text{ShD}_{it}^j \right) + \sum_{j=2014}^{2017} \left(\sigma_j \text{ShD}_{it}^j \right) + \epsilon_{it} \quad (4)$$

The above model resembles equation 3, with most terms having an analogous interpre-tation. There are, however, two noteworthy differences: first, the time window used has been extended to include the post-marriage period[57], and second, the specification is now

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[55]That is, POL_i equals one when $GOVT_i$ equals one.

[56]Unfortunately, post-2012 local affiliations may be themselves a function of austerity exposure and, as detailed in Montgomery, Nyhan and Torres (2018, pp. 770-771), using post-treatment moderators will lead to a bias "if the moderator could be affected by the experimental manipulation". Hence, I cannot properly condition my regression on contemporaneous partisanship. Even so, I argue that any effects retrieved by running the specifications here will underestimate the true impact when looking at NL constituencies - the main object of interest - seeing that most switches occurred from the NLs towards the SDs following the 2012 elections, where manipulation incentives are a priori reduced.

[57]Note that, although the marriage occurred in 2014, running equation 4 provides an estimate for its impacts relative to 2010 values. Alternatively, I can substitute 2013 for 2010 as the base year - an approach which produces similar insights. I nevertheless prefer to show estimates relative to 2010, as this allows for a more intuitive comparison between the results of the marriage analysis and those pertaining to the shut-down evaluation discussed in Section 5.

fitted separately for constituencies represented by the individual political parties (the SDs and NLs) which previously formed the SLU.

I argue that this latter distinction is appropriate for my investigation for three main reasons. First, estimating the δ_j terms above separately for the NLs and SDs allows me to further corroborate the existence of pre-austerity parallel trends in visible investments, now conditional on individual party affiliations. Under the paper's core identification assumption, I expect the estimates of these coefficients to be once more systematically indistinguishable from zero.

Second, this model permits me to assess whether the two former SLU parties responded heterogeneously to the shut-down by estimating the β_j coefficients separately for the NLs and the SDs. In this way, I can check that the aggregate null impacts retrieved in opposition areas in Section 5.1 were not driven by potential "within-SLU" cross-party heterogeneous responses. In line with the main results, my expectation here is that these coefficient estimates will be significantly weaker in constituencies where POL_i equals two or three, relative to constituencies where it equals one.

Finally, and most importantly, I can assess the marriage's heterogeneous local effects by estimating the σ_j coefficients separately by political affiliation. Under the **H3** hypothesis - that is, assuming that voters remember austerity and that any lingering unfavorable sentiments are relatively more pronounced in areas previously impacted by the shut-down - I expect these estimates to be positive and statistically significant in constituencies where POL_i equals two and, just as importantly, significantly weaker (or null) when POL_i equals three.[58]

I graph my findings in Figure 6 by plotting the estimated coefficients by year, separately for subsamples defined by the values of POL_i in panel A. In Panel B, I combine the three graphs for illustration purposes. Corresponding numerical estimates are given in Table 6, obtained for ex-governmental, NL and SD constituencies in panels A, B and

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[58]For completeness, I also estimate these coefficients in ex-governmental constituencies (i.e. units where POL_i equals one) and, once more, do not expect to find any notable divergences given that these leaders have already "paid their due" at the time when austerity was originally carried out.

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C, respectively. Columns (2), (5) and (8) are my preferred specifications, correcting for potentially confounding county-year disturbances.

Overall, the results are indicative of the following three insights. First, the core identification assumption is further corroborated as I am once more unable to reject the zero effect null-hypothesis for the coefficient estimates corresponding to pre-austerity years, regardless of the subsample considered. Therefore, I argue that the shut-down effects previously documented in opposition areas, as well as the marriage impacts estimated here, are unlikely to be driven by any party-specific pre-intervention trend divergences that had been potentially concealed by Section 5.2's aggregate investigation.

Second, a similar observation applies to the timeframe immediately following the shut-down: the main aggregate opposition findings do not mask any underlying heterogeneous party-specific responses. As seen in Figure 6, the results in fact suggest that visible investments evolved in a roughly parallel manner in constituencies represented by the SDs and NLs, with no statistically-noticeable divergences to speak of.

Most importantly, however, I document a significant and economically meaningful response following the marriage, noticeable - in line with **H3** above - exclusively in NL constituencies previously exposed to austerity - estimated at roughly 17 percent in the period the shock occurred and observable for another two years thereafter. At the same time, further in line with the local electoral channel, there are no pertinent effects retrieved for SD constituencies.

More broadly, I argue that these findings complement the paper's main narrative in a way that I summarize as follows: absent an electoral motif causally driving the observed local policy responses to austerity, the heterogeneous pattern documented here would be difficult to explain. Specifically, these results allow me to tentatively rule out a number of alternative explanations consistent with the main findings that I have previously outlined.

Figure 6: The Marriage: Dynamic Analysis

(a) Coefficient Estimates Retrieved by Analysing Constituencies Represented by Different Political Forces Separately
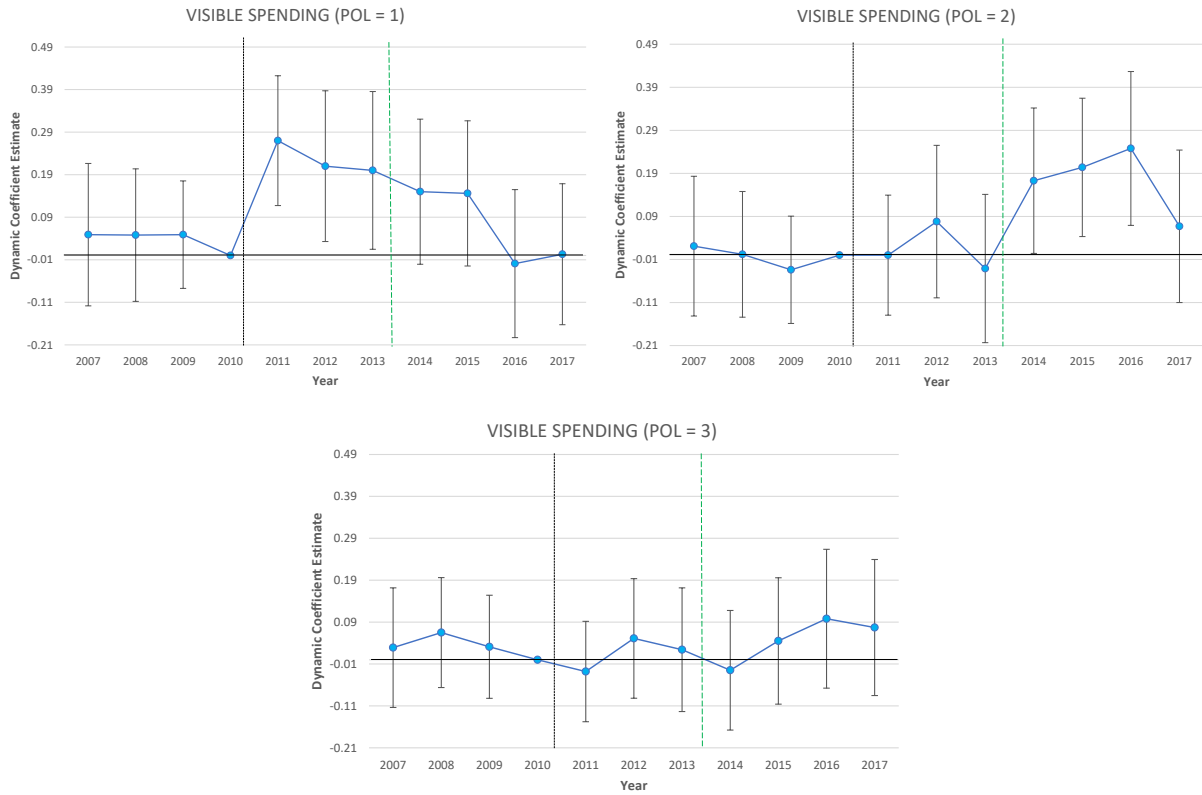
Note: I illustrate the marriage's dynamic effects, conditional on the political affiliation of local officials. To do so, I plot the coefficient estimates, alongside their 90 percent confidence intervals, retrieved from fitting specification 4 (including county-year fixed effects) separately on the subsamples defined by POL = 1, POL = 2 and POL = 3 (see text for details), in the first, second and third graph, respectively, against the year corresponding to their regressor. The dotted black line marks April 2011, when the shut-down was carried out. The dashed green line marks the first half of 2014, when the national re-alignment of political forces described in Section 6.1 took place. The dependent variable is (log) visible spending per capita.

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(b) Coefficient Estimates Retrieved by Analysing Constituencies Represented by Different Political Forces Jointly Illustrated

VISIBLE SPENDING
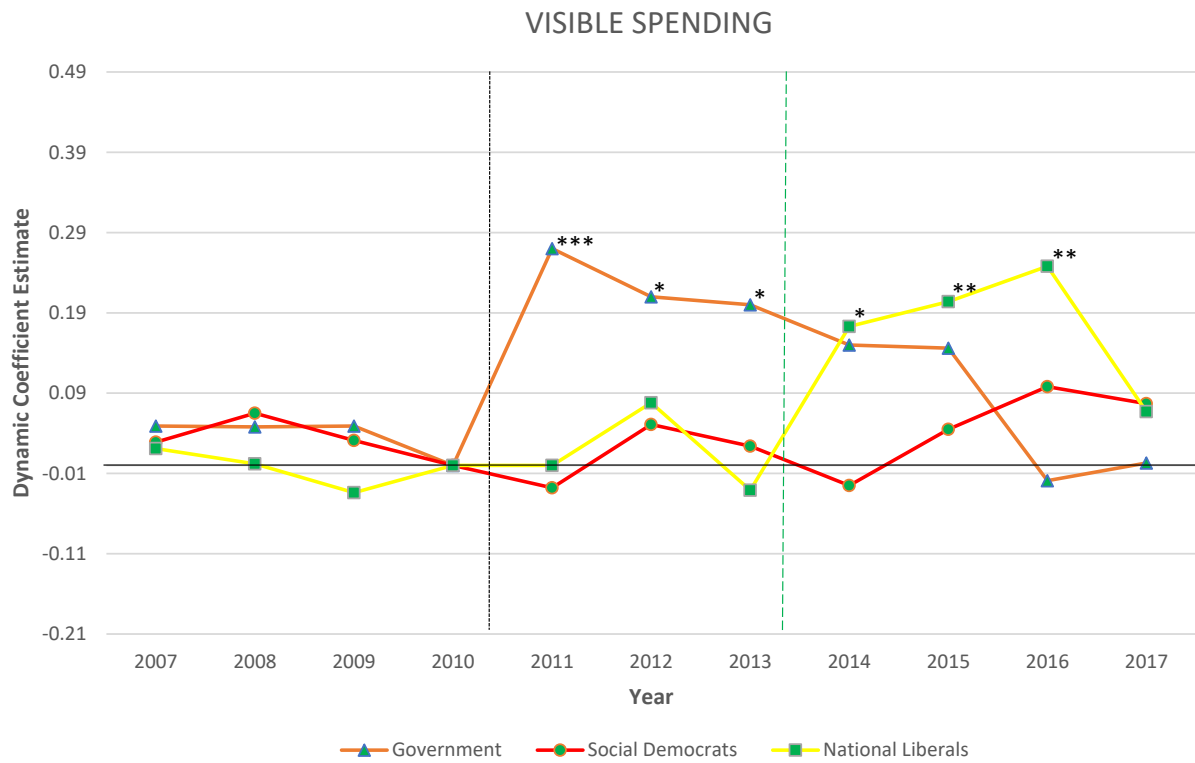
Note: To better visualize the observed heterogeneous political response to the marriage, I combine the three graphs from panel A above. For expositional clarity, confidence intervals are not presented - instead, significant coefficient estimates are marked with stars, as follows: ***p<0.01; **p<0.05; *p<0.1; starless coefficient estimates are statistically indistinguishable from zero in my preferred specification.

Table 6: The Marriage: Heterogeneous Fiscal Responses by the Mayor's Political Affiliation

	Dependent Variable: VISIBLE SPENDING								
	Panel A POL = 1			Panel B POL = 2			Panel C POL = 3		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ShD_{2007}	-0.098	0.049	-0.070	0.060	0.021	0.058	0.063	0.029	0.077
	(0.099)	(0.102)	(0.099)	(0.095)	(0.099)	(0.095)	(0.082)	(0.087)	(0.083)
ShD_{2008}	-0.137	0.048	-0.119	0.062	0.002	0.061	0.092	0.065	0.102
	(0.096)	(0.095)	(0.095)	(0.087)	(0.089)	(0.087)	(0.078)	(0.080)	(0.079)
ShD_{2009}	0.022	0.049	0.032	-0.024	-0.034	-0.027	0.039	0.031	0.044
	(0.077)	(0.077)	(0.077)	(0.069)	(0.076)	(0.070)	(0.070)	(0.075)	(0.070)
ShD_{2011}	0.306***	0.269***	0.297***	0.000	-0.000	0.001	-0.048	-0.028	-0.053
	(0.089)	(0.093)	(0.089)	(0.083)	(0.085)	(0.083)	(0.070)	(0.073)	(0.070)
ShD_{2012}	0.239**	0.207*	0.221**	0.070	0.078	0.071	0.040	0.051	0.031
	(0.099)	(0.108)	(0.100)	(0.101)	(0.108)	(0.101)	(0.085)	(0.087)	(0.084)
ShD_{2013}	0.153	0.202*	0.125	-0.075	-0.031	-0.073	0.054	0.024	0.040
	(0.107)	(0.113)	(0.107)	(0.094)	(0.105)	(0.094)	(0.088)	(0.090)	(0.086)
ShD_{2014}	0.114	0.150	0.077	0.221**	0.173*	0.224**	0.033	-0.025	0.014
	(0.101)	(0.104)	(0.101)	(0.097)	(0.103)	(0.096)	(0.088)	(0.087)	(0.085)
ShD_{2015}	0.099	0.146	0.053	0.249**	0.204**	0.251***	0.094	0.045	0.071
	(0.101)	(0.104)	(0.101)	(0.097)	(0.098)	(0.094)	(0.092)	(0.092)	(0.089)
ShD_{2016}	-0.004	-0.019	-0.060	0.250**	0.248**	0.253**	0.164	0.098	0.135
	(0.103)	(0.106)	(0.102)	(0.106)	(0.109)	(0.104)	(0.103)	(0.101)	(0.098)
ShD_{2017}	-0.055	0.003	-0.120	0.108	0.067	0.112	0.127	0.076	0.093
	(0.098)	(0.101)	(0.099)	(0.101)	(0.108)	(0.103)	(0.098)	(0.099)	(0.096)
Observations	10,271	10,271	10,271	7,037	7,037	7,037	11,819	11,819	11,819
Constituencies	934	934	934	640	640	640	1,075	1,075	1,075
Within R-squared	0.115	0.197	0.139	0.190	0.278	0.213	0.189	0.260	0.217
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Constituency FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
County x Year Dummies	NO	YES	NO	NO	YES	NO	NO	YES	NO
Linear County Time Trends	NO	NO	YES	NO	NO	YES	NO	NO	YES

Note: I quantify the marriage's effects on local voter-friendly spending in a dynamic framework, by fitting specification 4 on the subsample of constituencies for which POL equals one, two and three (as defined in text), in columns (1) - (3), (4) - (6) and (7) - (9), respectively. The dependent variable is (log) per capita visible spending. Standard errors, given in (round brackets), are clustered at the constituency-level; Significance levels: ***p<0.01, **p<0.05, *p<0.1

First, given the marriage evaluation, the main investigation's internal credibility is further "retroactively" corroborated. I argue that, had the retrieved shut-down effects been entirely spurious (for instance, driven by strategic closures based on anticipated fiscal changes rather than by the shut-down's negative electoral effects), then I would not expect past austerity exposure to have any long-lasting effects in line with those documented here - materializing three years after the shut-down conditional on a different political affiliation.[59] It is also noteworthy that, unlike the closure of hospitals which might have been implemented geographically in a politically strategic manner, the marriage was a nationwide shock chiefly motivated by considerations pertaining to the upcoming presidential ballot and, thus, unlikely to have been driven by any politically-heterogeneous constituency-level trends specific to areas previously impacted by the shut-down.

Second, it is difficult to reconcile the observed pattern with alternative narratives that do not account for local electoral incentives. Foremost, unlike the initial response which might have been driven by looser resource constraints resulting from central government favoritism, any fiscal adjustments observed following the marriage are unlikely to be explained by this channel, as they were implemented in constituencies affiliated with the NLs - an opposition party. Furthermore, the probability of popular demand changes driving the main impact - heterogeneously in ex-governmental regions possibly due to underlying demographics - is also diminished: visible spending increased in NL-constituencies only three years following the original implementation of austerity, after having displayed a parallel evolution with investments observed in SD territories in the interim (which remained roughly unaltered thereafter).

Finally, more tentatively, the marriage results suggest that any political effects associated with carrying out austerity policies might extend beyond the short-run. In line with memory-based models of bounded rationality (Fouka and Voth, 2016), it seems that austerity can influence electoral responses to future, even more benign measures imple-

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[59]Of course, endogeneity issues can never be decisively ruled out in observational studies such as this. Nevertheless, my argument is that the probability of the main results being spurious conditional on the marriage results is lower relative to the unconditional probability of a generally spurious relationship.

mented by those perceived to be responsible, such as an alliance with another political party. More informally, my findings indicate that political forces can "stain their hands" by carrying out electorally unappealing reforms, providing a further possible justification for the documented widespread reluctance to do so.

6.3 Complementary Analyses: The Role of Local Competition, and the Effect of Austerity on Internal Local Revenues

To further corroborate the electoral mechanism driving local responses to the shut-down, I conduct two analyses complementing the marriage investigation. First, in line with the political budget cycles literature (see e.g. Aidt, Veiga and Veiga, 2011), I evaluate whether the documented fiscal adjustments are more pronounced in politically competitive governmental constituencies, where the austerity-induced fall in electability is presumably more pertinent for incumbents. I find suggestive evidence in this sense. Second, I show that local politicians did not finance the observed voter-friendly investments by raising local taxes, consistent with an electoral motif. These complementary analyses are discussed in-depth in Appendix A.

7 Summary and Conclusions

In this paper, I investigate whether centrally-implemented austerity policies incentivize local electorally-motivated fiscal adjustments. To do so, I exploit a novel source of variation in austerity exposure created by a highly impactful and politically controversial measure whereby a significant proportion of Romania's public hospitals was discontinued. In response to the shut-down, I document a marked increase in voter-friendly local infrastructure expenses in affected constituencies. In line with an underlying electoral mechanism, the data suggest that the observed catchment area effect is significantly larger, both statistically and economically, in areas represented by mayors affiliated with the responsible central government.

In order to rule out alternative explanations consistent with the main findings, I exploit a second related natural experiment consisting of a re-alignment of the country's political forces - whereby a party previously opposing the orchestrators of austerity became their ally. I find that austerity exposure can have unexpected politically-driven effects in the medium-run, and argue that the documented heterogeneous responses are difficult to reconcile without accounting for the local electoral mechanism investigated here.

Therefore, this paper's core contribution has been to isolate and provide a rigorous novel empirical assessment of a local electoral mechanism potentially shaping the political costs of carrying out austerity. Methodologically, using two interrelated quasi-experimental sources of variation in order to corroborate the overarching electoral narrative is an approach that has not been previously employed for the purpose of appraising austerity effects. Nevertheless, I argue that it is valuable, as it allows one to partially address widespread internal validity concerns generally associated with such analyses, and therefore diminish the possibility of spurious relationships. In light of this, although both analysis have clear limitations individually, I posit that the joint investigation conducted here offers novel insights which allow us to better understand austerity effects. It seems that, in addition to triggering changes in the political preferences of exposed voters (such as an increase in demand for extremist forces as seen in Fetzer, 2019 or Galofre-Villa et al., 2019), austerity can also influence the policy choices of "traditional" local political officials.

That said, my findings ought to be interpret cautiously. While I argue that investigating the shut-down as a natural experiment allowed me to partially alleviate a number of widespread endogeneity concerns associated with estimating the local impacts of austerity, and permitted me to isolate a political mechanism driving such responses, I acknowledge that more work is needed to appraise whether the patterns retrieved from analyzing what is ultimately a specific shock are externally representative. For instance, one potential avenue would be to assess the moderating role of sub-national responses by appraising whether the electoral costs of austerity are larger in magnitude in contexts where more

stringent restrictions are placed on the discretion of local politicians. Alternatively, an exercise for future research would be to check whether media responses - which in this case were highly critical of the measure - can partially shape political responses to austerity. Had the media been more neutral in its coverage, would we have observed similar effects? Such analyses may provide further evidence in support of the mechanism described here and corroborate its external validity. Finally, future research might take the evaluation one step further by asking whether local electoral responses may, in fact, work against the budget-tightening objective of austerity policies, thus undermining their effectiveness and providing a possible explanation for why the results of austerity in terms of budgetary consolidation have often been underwhelming (Jorda and Taylor, 2016; Fatas and Summers, 2018).[60]

To conclude, my findings suggest that a better informed public discourse on the link between austerity and governmental instability can result from acknowledging the potential role sub-national electoral incentives play in shaping the political costs of carrying out such policies.

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[60]Although the investigated setting does not permit me to make too much progress in this sense, I elaborate upon this point and conduct a tentative preliminary evaluation in Appendix A

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Appendix

Appendix A - Supplementary Analyses

I conduct several tests and robustness checks complementing the main analysis. Here, I briefly explain their purposes, and discuss their results.

The Moderating Role of Political Competition

Recall that, in Section 5, I found that austerity exposure led to a noticeable increase in local visible spending, driven by the actions of sub-national incumbents affiliated with those responsible for the shut-down - consistent with the core electoral mechanism studied in this paper. In order to rule out alternative explanations, I investigated the marriage as a second natural experiment in Section 6, and observed politically-heterogeneous responses further corroborating the local electoral narrative.

In line with the political budget cycle literature (see e.g. Aidt, Veiga and Veiga, 2011), an alternative way to corroborate the core mechanism would be to assess whether austerity-driven fiscal adjustments are larger in magnitude in so-called "*contested*" constituencies - defined as constituencies where mayors (or their party's candidate if the incumbent herself does not intend to run) do not have a comfortable electoral lead going into upcoming elections. Intuitively, it is precisely in these contested areas that the austerity-caused fall in electability is most pressing for local officials, given their party's already uncertain re-election prospects. Then, if electability concerns indeed play a role in shaping austerity responses, it is in these contested constituencies where I would expect the shut-down's impacts to be most notable.

Unfortunately, measuring constituency competitiveness is difficult in the investigated setting. Chiefly due to the formation of the SLU in anticipation of the 2012 ballots, I cannot simply rely on the 2008 difference in vote shares between winners and runner-ups to proxy for contest intensity - as one would conventionally. To see why, consider a

constituency with the following hypothetical results in the 2008 mayoral ballot: *DL - 55 percent; SD - 15 percent; NL - 15 percent; PC - 10 percent; Other - 5 percent.* Absent the SLU being formed, this constituency would likely be perceived as (relatively) safe by the incumbent, given that the runner-up's score was 40 percentage points below her own. However, once the SLU is introduced (assuming that no ideological spill-overs occur), the combined share of the alliance (SD, NL and PC) becomes 40 percent, thus more than halving the current incumbent's perceived historic lead. Such differences may arguably alter the mayor's impression concerning the level of political competition and, thus, may affect ensuing electorally-motivated responses to austerity.

With this difficulty in mind, in this section, I make some progress towards investigating the moderating role of electoral competitiveness as follows. First, for each *governmental* constituency i, I look at the 2008 first-round mayoral scores obtained by each party, and define a variable $MARGIN_i$ as the difference between the vote share obtained by the eventual winner (either a DL or UDMR representative) and the combined vote share of the three political parties comprising the SLU.[61] While imperfect, I argue that $MARGIN_i$ constitutes a reasonable proxy for the level of political competition in a given constituency, under the assumption that the incumbent's *perception of competitiveness* is positively associated with the SLU's combined strength - a reasonable contextual postulate given that the SLU was the main force opposing those responsible for austerity at the time.

Then, to test the electoral mechanism, I assess whether the observed local responses to austerity are significantly larger in magnitude in constituencies where $MARGIN_i$ is small - that is, areas where electoral pressures are strong, in light of the SLU's combined historic vote share being high relative to that of the governmental incumbent. Empirically, I appraise this hypothesis by fitting my preferred specification 1 separately on the

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[61]I need to look at first-round as opposed to second-round voting in order to obtain a number for each individual SLU party.

four subsamples of governmental constituencies defined by the quartiles of $MARGIN_i$'s distribution.[62]

Table A1 reports the results. Corroborating the local electoral mechanism, the evidence suggests that political competitiveness does moderate local responses to austerity. To this point, I make three observations looking at panel A. First, the retrieved coefficient point estimates become progressively smaller in magnitude as we move from more to less contested constituencies. Second, despite some amount of noise introduced by low subsample sizes, I am able to reject the zero effect null-hypothesis in column (1), where I appraise the shut-down's effects in constituencies situated in the bottom quartile of $MARGIN_i$'s distribution (i.e. the "most contested" ones). Conversely, when zooming in on the "safest" areas in column (4), the coefficient estimate drops noticeably in both magnitude and statistical significance, suggesting that austerity exposure did not lead to meaningful fiscal adjustments in these constituencies. The same insights are echoed in panel B. Finally, I prefer not to draw any conclusions based on the effects retrieved in columns (2) and (3) - while the magnitude of the point estimates does suggest that some degree of austerity-driven fiscal manipulations took place in constituencies with "moderate" levels of electoral competition, the evidence is weak.

Overall, I argue that the moderating role of political competitiveness documented here further corroborates the core local electoral mechanism driving austerity responses - complementing and strengthening the conclusions derived from exploring the marriage's heterogeneous effects in the main text.

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[62]This approach allows for a better qualitative interpretation of the results. Alternative specifications, such as fitting the dynamic model 3 instead, produce similar insights. Results are also insensitive to dropping the set of county-year dummies, or to including linear county-specific time trends instead.

Table A1: Austerity and Local Fiscal Adjustments: Appraising the Moderating Role of Political Competition

	(1) First Quartile	(2) Second Quartile	(3) Third Quartile	(4) Fourth Quartile	(5) Difference p-value
Panel A - Dependent Variable: VISIBLE SPENDING					
SHUTDOWN	0.426** (0.182) [0.020]	0.162 (0.142) [0.253]	0.130 (0.169) [0.443]	-0.060 (0.204) [0.770]	0.038
Observations	931	936	932	936	-
Constituencies	233	234	233	234	-
Within R-squared	0.22	0.15	0.21	0.16	-
Panel B - Dependent Variable: VISIBLE RATIO					
SHUTDOWN	0.155* (0.088) [0.079]	0.057 (0.072) [0.432]	0.077 (0.090) [0.394]	-0.070 (0.116) [0.548]	0.062
Observations	932	936	932	936	-
Constituencies	233	234	233	234	-
Within R-squared	0.20	0.14	0.19	0.18	-
Year FE	YES	YES	YES	YES	-
Constituency FE	YES	YES	YES	YES	-
County x Year Dummies	YES	YES	YES	YES	-

Note: I quantify the moderating influence of local political competition on representative responses to the shut-down. To do so, I fit specification 1 separately on the four subsamples of governmental constituencies defined by the quartiles of MARGIN's distribution (as defined in text) in columns (1) through (4). The dependent variables are (log) per capita visible spending and the (log) visible ratio in panels A and B, respectively. SHUTDOWN captures the interaction of two dummy variables - HOSPITAL CLOSED, which equals one in constituencies whose nearest hospital was targeted by the 2011 reform, and POST, equal to one for observations recorded after the measure was carried out (zero otherwise). The p-value reported in column (5) is associated with the one-sided null hypothesis that the SHUT-DOWN coefficient estimate is larger in column (1) relative to column (4). All specifications include year and constituency fixed effects, as well as county-year dummies. Standard errors, given in (round brackets), are clustered at the constituency-level; p-values are given in [square brackets]; Significance levels: ***$p<0.01$, **$p<0.05$, *$p<0.1$

Austerity Exposure and Local Taxes

In the main text, I found evidence suggesting that austerity exposure incentivized increases in visible investments at the local level. In this section, I further corroborate the hypothesized electoral mechanism driving these responses by assessing whether the shutdown had a noticeable impact on internally-generated revenues. In addition, I investigate whether any such effects are moderated by the partisan affiliation of local officials. Intuitively, if electoral concerns drive local responses, I do not expect politicians to finance their voter-friendly investments by increasing taxes, a conventionally "unfriendly" policy.

To test this postulate empirically, I estimate the effect of the shut-down on a variable measuring constituency-level revenues generated via local taxes - denoted $INTERNAL$ $REVENUES_{it}$ - constructed based on information retrieved from the Ministry of Finance. I present the results in Table A2, where coefficient estimates given in columns (1) to (3) and columns (4) to (6) are retrieved by fitting specifications 1 and 2 on the full final sample, respectively.

All in all, the effects retrieved here are small in magnitude and statistically indistinguishable from zero regardless of the model employed. In addition, the estimated interaction terms are not indicative of politically-heterogeneous responses[63]. Hence, I assert that there is insufficient evidence to conclude that visible investment increases were financed by generating additional internal revenues, in line with the local electoral hypothesis.

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[63]Running the baseline specification separately for different subsamples or looking at dynamic estimates instead does not change anything substantially.

Table A2: Austerity and Internal Local Revenues

	Dependent Variable: INTERNAL REVENUES					
	(1)	(2)	(3)	(4)	(5)	(6)
SHUTDOWN	0.001	0.001	0.002	0.002	-0.004	-0.003
	(0.008)	(0.008)	(0.008)	(0.010)	(0.010)	(0.010)
	[0.949]	[0.897]	[0.837]	[0.859]	[0.705]	[0.779]
SHUTDOWN * GOVT	-	-	-	-0.004	0.013	0.012
				(0.018)	(0.017)	(0.017)
				[0.834]	[0.423]	[0.466]
Observations	10,596	10,596	10,596	10,596	10,596	10,596
Constituencies	2,649	2,649	2,649	2,649	2,649	2,649
Within R-squared	0.071	0.13	0.11	0.071	0.13	0.11
Year FE	YES	YES	YES	YES	YES	YES
Constituency FE	YES	YES	YES	YES	YES	YES
County x Year Dummies	NO	YES	NO	NO	YES	NO
Linear County Time Trends	NO	NO	YES	NO	NO	YES

Note: I report the difference-in-differences coefficient estimates of the shut-down's effect on revenues generated via local taxes, retrieved by fitting specification 1 on the final sample in columns (1) - (3). In columns (4) - (6), I quantify the moderating influence of mayoral partisan affiliations by running regression 2. SHUTDOWN captures the interaction of two dummy variables - HOSPITAL CLOSED, which equals one in constituencies whose nearest hospital was targeted by the 2011 reform, and POST, equal to one for observations recorded after the measure was carried out (zero otherwise). GOVT is a dummy variable equal to one in constituencies whose mayor is politically affiliated with the parties comprising the central government which carried out austerity (zero if affiliated with the opposition). All specifications include year and constituency fixed effects. The models in columns (2) and (5) include county-year dummies, while the regressions in columns (3) and (6) include county-specific linear time trends. Standard errors, given in (round brackets), are clustered at the constituency-level; p-values are given in [square brackets]; Significance levels: ***p<0.01, **p<0.05, *p<0.1

Do Local Responses Undermine the Budget-Tightening Effectiveness of Austerity? Deficits and the Composition of Spending

Thus far, I retrieved empirical evidence suggesting that exposure to austerity led to marked increases in local visible investments in governmental constituencies, not matched by a rise in local taxes. A related question naturally follows: in addition to partially mitigating the political costs of carrying out austerity, do local fiscal electorally-motivated fiscal responses work against the budget-tightening objectives of austerity policies, and thus undermine their effectiveness?

Although the investigated context does not allow me to draw any definitive conclusiosn, in this section, I make some progress by analyzing how the documented fiscal adjustments were financed.

In line with the electoral spending literature, two notable financing alternatives exist. First, local incumbents can finance visible investments by increasing their constituency's deficit spending (see e.g. Shi and Svensson, 2006). In the Romanian case specifically, any expenses exceeding a constituency's local revenue capacity are chiefly serviced via intergovernmental transfers from the central government, rather than by directly incurring debt as in other contexts. Therefore, any resulting local deficits ultimately end up indirectly harming the national budget balance, which the austerity-implementing administration presumably finds unappealing.

To measure deficit spending empirically, I retrieve information from the Ministry of Finance and construct a variable *DEFICIT FINANCING$_{it}$* equal to the (log) per capita amounts of funds syphoned from the center with the purpose of financing decentralized investments and equilibrating the local budgets. Given that an explicitly stated purpose of these grants is to correct local budgetary imbalances, I argue that this measure constitutes a reasonable proxy for deficit spending at the local level. That said, before proceeding, I acknowledge a caveat in that I am not able to distinguish to what degree these grants were received after being *demanded* by local officials versus initially *offered*

from the center. Hence, I cannot make any conclusive claims as to the exact underlying mechanism driving the effect of austerity exposure on this variable.

Second, as argued chiefly by Drazen and Eslava (2010), mayors can also finance visible investments by altering the composition of expenses. Here, I expect austerity exposure to induce cuts in spending categories whose effects only become noticeable to local voters with a lag rather than immediately. Intuitively, once hit by the shut-down, I argue that local officials become more willing to incur future costs which such cuts might bring in return for contemporaneous gains in electability triggered by syphoning funds towards voter-friendly investments.

Empirically, to test for possible expense substitutions, I construct a variable *LONG-RUN SPENDING$_{it}$*, measuring constituency-level (log) per capita investments used for educational and social-assistance purposes.[64] Intuitively, a significant proportion of these expenditures constitutes targeted monetary transfers to specific groups of individuals (such as teachers or the unemployed), which I posit makes this dependent variable appropriate for my purposes for two reasons. First, any changes made by local officials to these funds in one year only become effective with a delay since contemporaneous payments are based on previously budgeted amounts. Second, if a local administration becomes unable to honor these obligations, the central government once again steps in in order to ensure their financing. Hence, I argue that reductions in educational or social-assistance investments at the local level, while beneficial for austerity-hit sub-national incumbents, are once more reasonably a priori undesirable for the central administration carrying out austerity.

In Figure A1, I graph the coefficient estimates retrieved by fitting the preferred dynamic equation 3 on the subsample of governmental constituencies.[65]

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[64]Formally, the first category includes expenditures related to the well-functioning of the local education system, including the management of public schools and employee remunerations. The second category encompasses a broad array of expenses related to social aid, including spending for people with severe disabilities, welfare transfers and unemployment benefits. Note that I also investigate two other expense categories - concerning defence and general spending - and find negligible effects.

[65]In this case, I prefer the dynamic relative to the baseline model as responses to the shut-down in terms of the two outcomes investigated here actually vary by year as seen in the graph. Employing

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Figure A1: Financing Local Voter-Friendly Fiscal Responses to Austerity
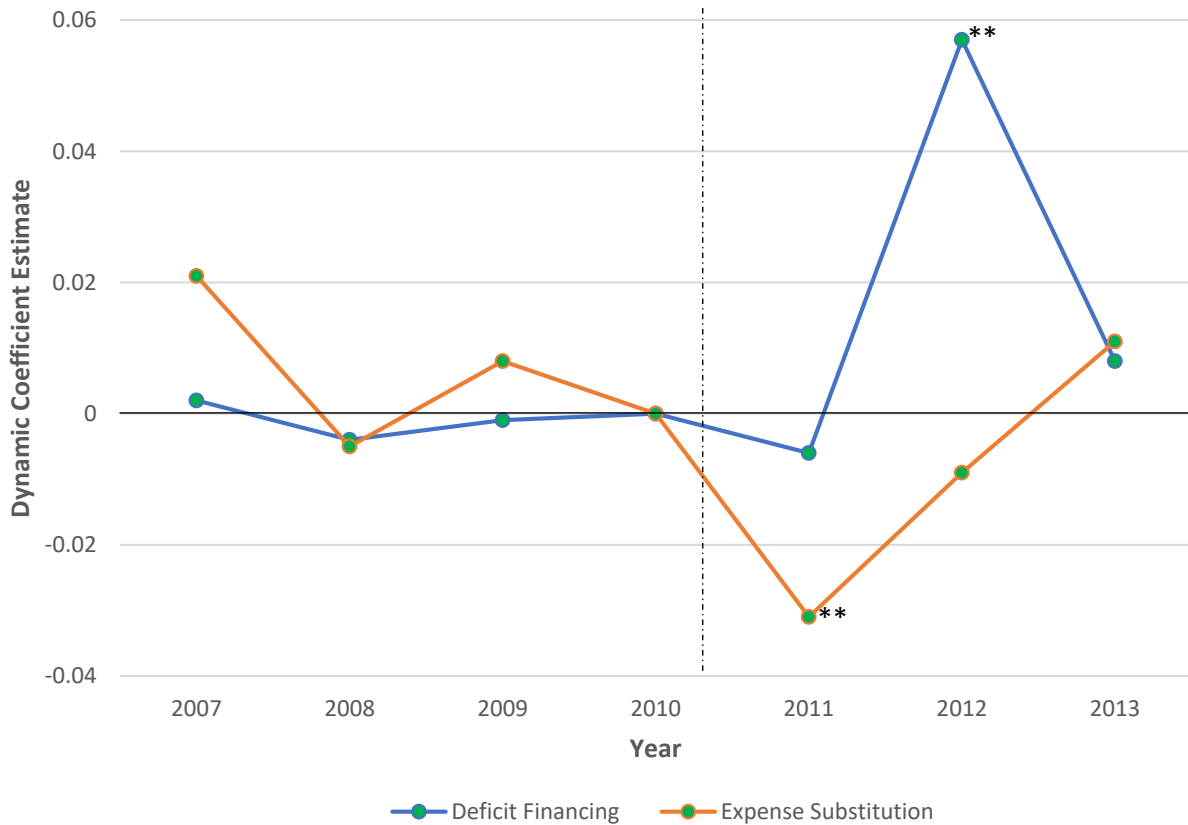
Note: I plot the coefficient estimates retrieved from fitting specification 3 (including county-year fixed effects) on the subsample of governmental constituencies, against the year corresponding to each regressor. The dotted black line marks April 2011, when the shut-down was carried out. The dependent variables are (log) central deficit per capita (captured by the blue line) and (log) long-run spending per capita (captured by the orange line), as defined in text. For expositional clarity, confidence intervals are not presented - instead, significant coefficient estimates are graphically marked with stars, as follows: ***p<0.01; **p<0.05; *p<0.1; starless coefficient estimates are statistically indistinguishable from zero in my preferred specification.

Overall, the evidence suggests that visible expenses were at least partially financed via both an increase in local deficits, as well as by a re-allocation of funds across spending categories. Interestingly enough, however, I find that this process was not fully symmetric. It appears that local officials initially proceeded by altering the composition of expenses

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the baseline method instead leads to similar, but noisier estimates. These results, alongside the table containing the information illustrated in Figure A1, are available upon request.

before increasing deficitary local spending. Concretely, I document that the shut-down led to an approximate 3 percent fall in long-run investments when initially carried out, followed by a rough 6 percent rise in deficit spending the year after.

Although tentative, this supplementary analysis brings some evidence suggesting that local politicians may undermine the consolidation of public spending - a core stated objective of austerity - by increasing local deficits and re-allocating funds in possibly undesirable ways. These findings complement recent work which conclude that austerity policies have had suboptimal results in terms of improving solvency. For instance, Jorda and Taylor (2016) document that, because such measures were implemented in a slump, austerity led to significant decreases in real GDP in the medium-run. Similarly, DeLong and Summers (2012) and Fatas and Summers (2018) argue that austerity was "self-defeating" because fiscal consolidation efforts resulted in higher debt-to-GDP ratios in the long-run.[66]

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[66]The view by which austerity has been ineffective is also documented in the media. A recent article in the Financial Times (2019) highlights how, after a decade of austerity, UK deficits returned to pre-crisis levels. See https://on.ft.com/2OdUGJh.

Additional Robustness Checks

I conduct several conventional robustness checks.

First, as indicated in text, I replicate both the dynamic analysis in Section 5.2, as well as the marriage evaluation in Section 6.2 using the *VISIBLE RATIO$_{it}$* rather than *VISIBLE SPENDING$_{it}$* as the dependent variable. Retrieved coefficient estimates are presented in Tables A3 and A4, respectively, with corresponding graphical illustrations in Figures A2 and A3, respectively. Overall, the main qualitative implications remain unchanged - these analysis show that austerity exposure increases the average "burden" of voter-friendly spending in a constituency's internal budget.

Second, in Table A5, I re-run the main baseline and interaction analyses after clustering standard errors at the county rather than the constituency-level. Once more, results remain fundamentally unaltered.

Third, in Table A6, I replace local constituency revenues with total revenues as *VISIBLE RATIO$_{it}$*'s denominator. Results here show that the burden of visible investments increases relative to overall revenues as well.

Fourth, in Table A7, I test the robustness of my results to including in the sample UATs where public hospitals were situated at the time of the shut-down, as well as constituencies officially classified as towns or municipalities. Although, as expected given the discussion in Section 4.3, retrieved coefficients fall slightly in magnitude, the same patterns continue to emerge.

Fifth, in Table A8, I re-assess the effects of austerity exposure after including in the sample constituencies whose mayors were not affiliated with either the governmental parties responsible for austerity, or the core SLU opposition group. As outlined in Section 4.3, I had initially excluded these "other" constituencies because it is a priori unclear to what extent their mayors were seen as supporting or opposing the then central government. For the purposes of this analysis, I classify them as opposition forces. Although the baseline effects once again become slightly weaker (as expected since I am essentially

including additional constituencies for which the electoral mechanism is unlikely to hold) the documented qualitative patterns remain robust.

Sixth, in Table A9, I fit the baseline equation 1 on the subsample of localities represented by DL officials exclusively, rather than by DL or UDMR incumbents as in Table 4, panel B. I do so in order to check whether retrieved effects are driven by the actions of the minority partner. Evidence suggests that this is not the case.

Finally, for completeness, I replicate the cross-tabulation from Table 1 to account for the SLU breaking down. Concretely, I repeat the analysis after replacing the $GOVT_i$ dummy used in the main analysis with the POL_i employed in the marriage evaluation. The numbers are presented in Table A10.
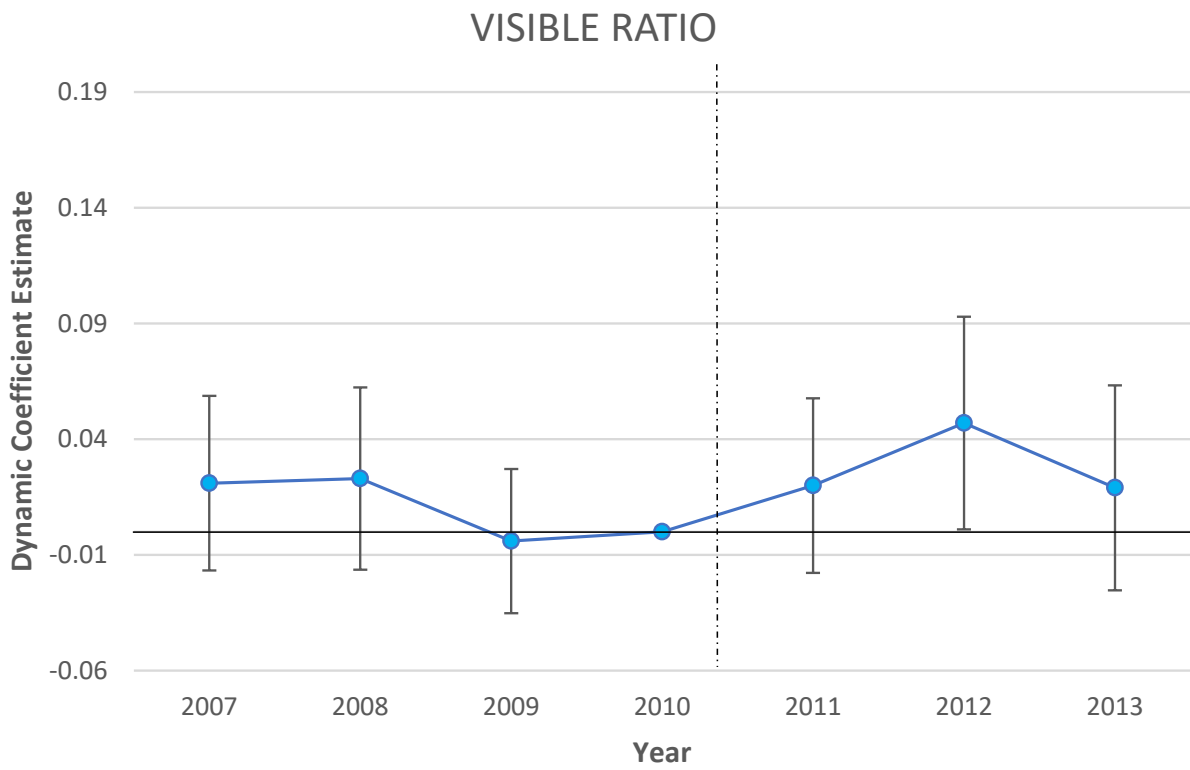
Table A3: Austerity and Local Fiscal Adjustments: Dynamic Analysis (Alternative Outcome)

	Dependent Variable: VISIBLE RATIO								
	Panel A Full Sample			Panel B GOVT = 1			Panel C GOVT = 0		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ShD_{2007}	0.011	0.021	0.015	-0.036	0.017	-0.007	0.024	0.025	0.020
	(0.022)	(0.023)	(0.022)	(0.041)	(0.041)	(0.041)	(0.026)	(0.027)	(0.026)
	[0.622]	[0.353]	[0.495]	[0.384]	[0.674]	[0.856]	[0.355]	[0.351]	[0.450]
ShD_{2008}	0.017	0.023	0.020	-0.059	0.012	-0.040	0.036	0.030	0.034
	(0.024)	(0.024)	(0.024)	(0.041)	(0.040)	(0.041)	(0.028)	(0.028)	(0.027)
	[0.472]	[0.324]	[0.396]	[0.151]	[0.758]	[0.324]	[0.186]	[0.288]	[0.219]
ShD_{2009}	-0.007	-0.004	-0.006	-0.006	0.002	0.003	-0.007	-0.004	-0.008
	(0.019)	(0.019)	(0.019)	(0.037)	(0.037)	(0.037)	(0.021)	(0.022)	(0.021)
	[0.701]	[0.847]	[0.762]	[0.866]	[0.956]	[0.932]	[0.749]	[0.845]	[0.703]
ShD_{2011}	0.030	0.020	0.029	0.125***	0.100**	0.115**	-0.016	-0.016	-0.015
	(0.022)	(0.023)	(0.022)	(0.046)	(0.047)	(0.046)	(0.024)	(0.025)	(0.024)
	[0.171]	[0.376]	[0.194]	[0.007]	[0.033]	[0.012]	[0.505]	[0.522]	[0.542]
ShD_{2012}	0.051*	0.047*	0.048*	0.094*	0.081	0.075	0.027	0.036	0.030
	(0.027)	(0.028)	(0.027)	(0.050)	(0.053)	(0.051)	(0.032)	(0.033)	(0.031)
	[0.059]	[0.089]	[0.074]	[0.060]	[0.127]	[0.138]	[0.390]	[0.276]	[0.341]
ShD_{2013}	0.012	0.019	0.007	0.041	0.073	0.013	-0.008	-0.003	-0.004
	(0.026)	(0.027)	(0.026)	(0.052)	(0.054)	(0.053)	(0.029)	(0.030)	(0.029)
	[0.655]	[0.475]	[0.787]	[0.435]	[0.175]	[0.815]	[0.785]	[0.917]	[0.895]
Observations	18,540	18,540	18,540	6,538	6,538	6,538	12,002	12,002	12,002
Constituencies	2,649	2,649	2,649	934	934	934	1,715	1,715	1,715
Within R-squared	0.027	0.080	0.054	0.031	0.118	0.069	0.049	0.108	0.07
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Constituency FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
County x Year Dummies	NO	YES	NO	NO	YES	NO	NO	YES	NO
Linear County Time Trends	NO	NO	YES	NO	NO	YES	NO	NO	YES

Note: I replicate the analysis from Table 5 after replacing (log) visible spending per capita with the (log) visible ratio as the dependent variable.

Figure A2: Austerity and Local Fiscal Adjustments: Dynamic Analysis (Alternative Outcome)

(a) Coefficient Estimates Retrieved by Analysing the Full Final Sample

VISIBLE RATIO

(b) Coefficient Estimates Retrieved by Analysing Governmental and Opposition Constituencies Separately

VISIBLE RATIO (GOVT = 1)

VISIBLE RATIO (GOVT = 0)

(c) Coefficient Estimates Retrieved by Analysing Governmental and Opposition Constituencies Jointly Illustrated
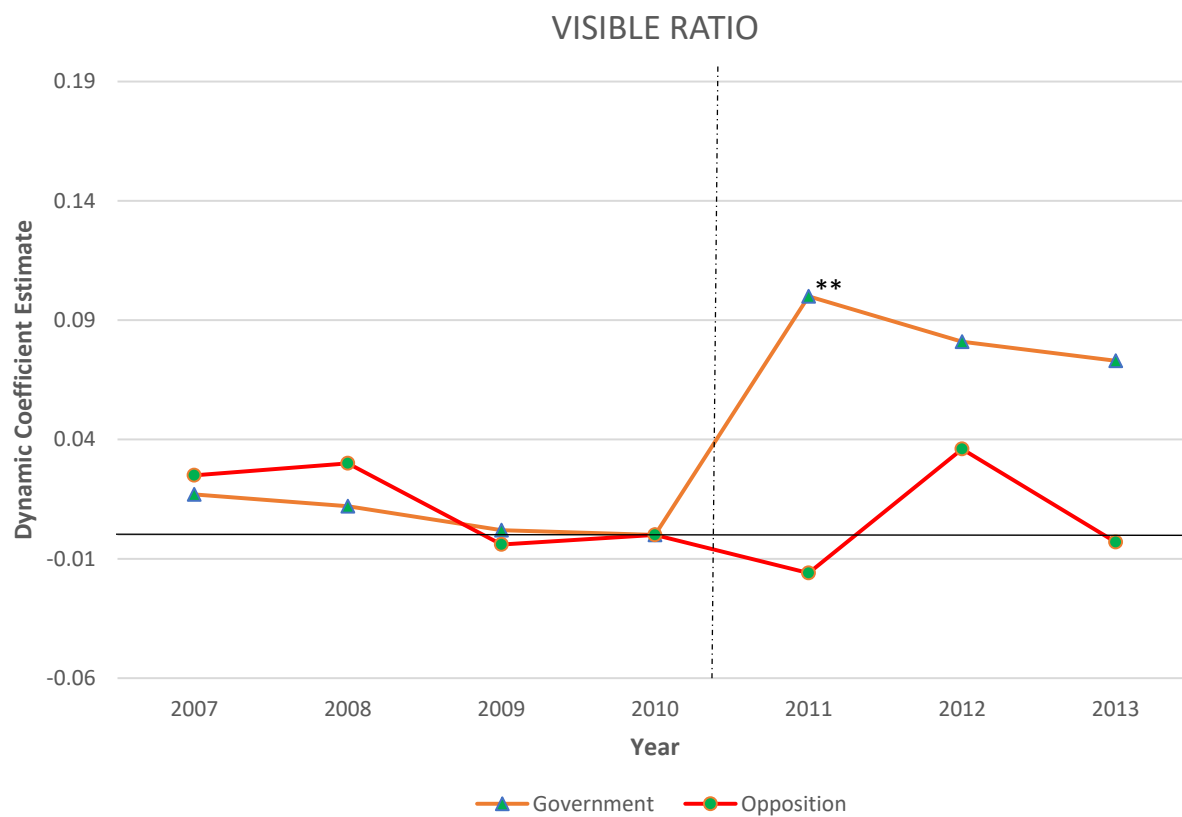
Note: I replicate the graphical analyses from Figure 5 using the (log) visible ratio, as defined in text, as the dependent variable.

Table A4: The Marriage: Heterogeneous Fiscal Responses by Political Affiliation (Alternative Outcome)

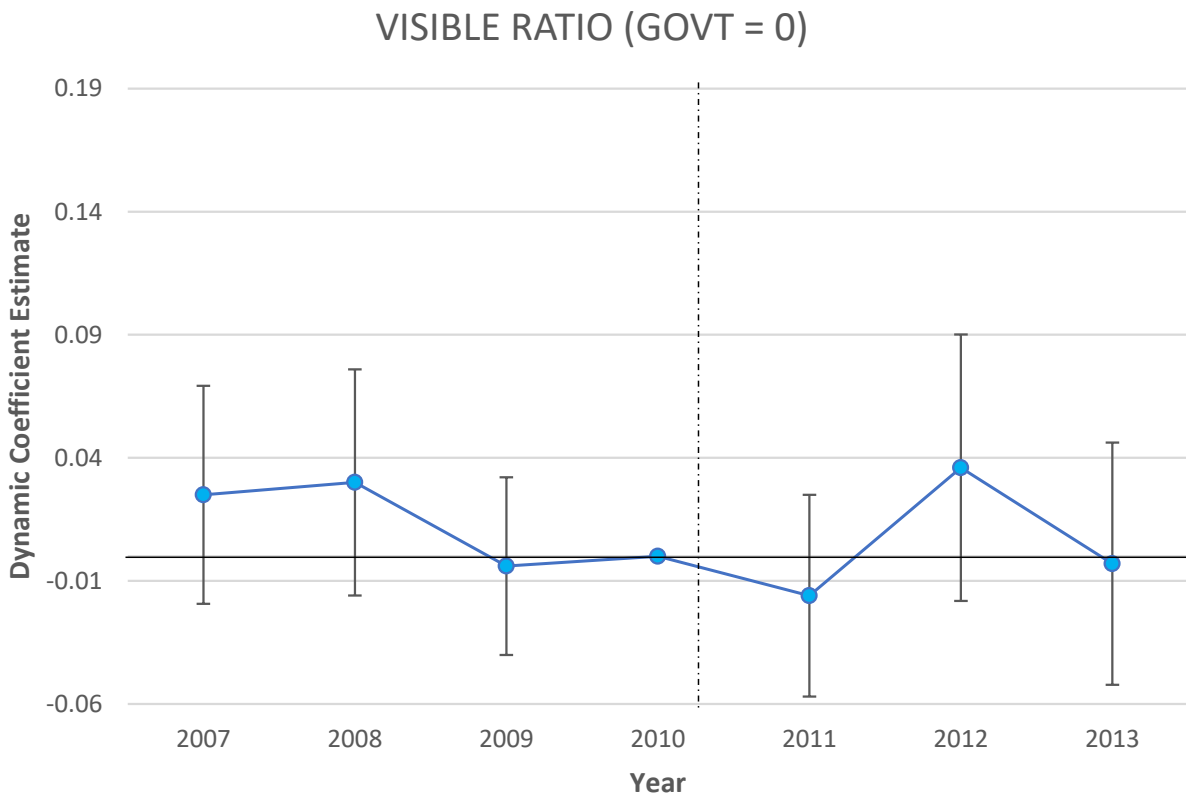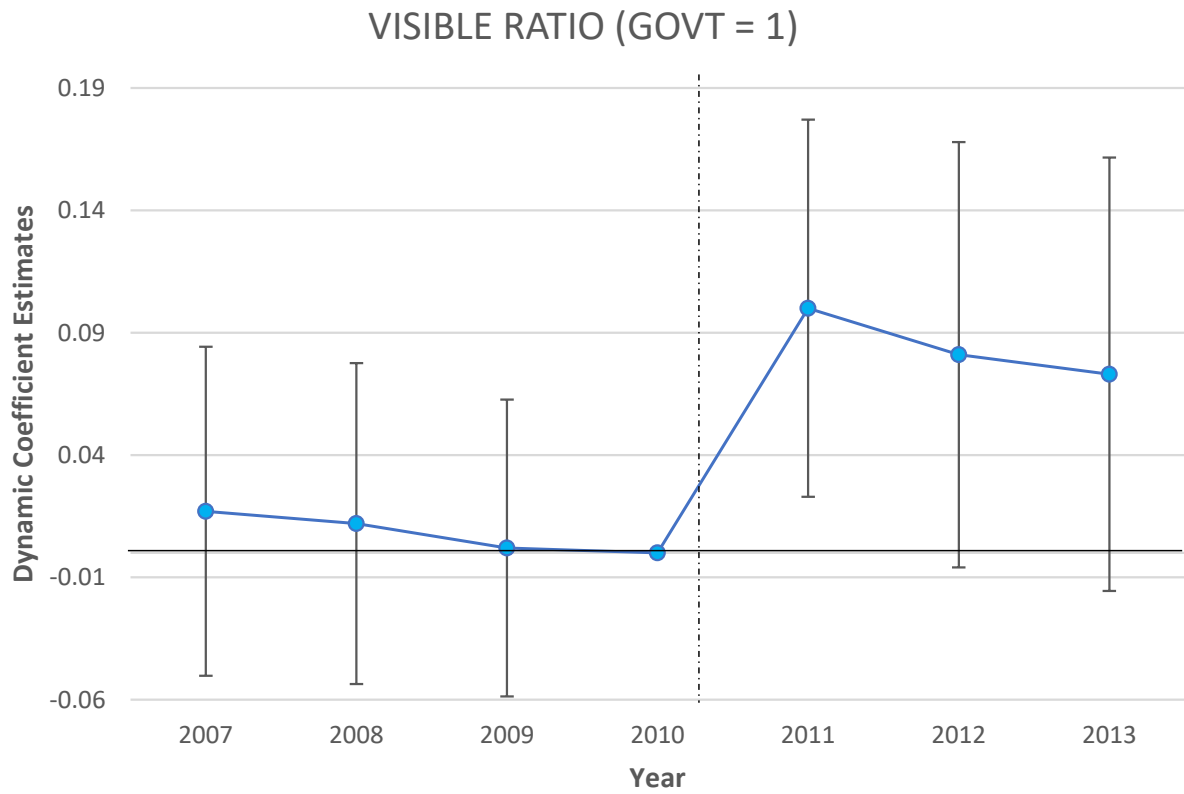|	Dependent Variable: VISIBLE RATIO								
	Panel A POL $=$ 1			Panel B POL $=$ 2			Panel C POL $=$ 3		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ShD_{2007}	-0.036	0.017	-0.025	0.058	0.053	0.055	-0.003	-0.010	0.004
	(0.041)	(0.041)	(0.041)	(0.042)	(0.045)	(0.042)	(0.032)	(0.034)	(0.033)
ShD_{2008}	-0.059	0.012	-0.052	0.073*	0.045	0.071*	0.001	-0.004	0.005
	(0.041)	(0.040)	(0.041)	(0.043)	(0.045)	(0.043)	(0.036)	(0.036)	(0.036)
ShD_{2009}	-0.006	0.002	-0.003	0.021	0.015	0.020	-0.014	-0.013	-0.012
	(0.037)	(0.037)	(0.037)	(0.028)	(0.031)	(0.028)	(0.030)	(0.032)	(0.030)
ShD_{2011}	0.125***	0.100**	0.121***	-0.004	-0.008	-0.003	-0.022	-0.013	-0.024
	(0.046)	(0.047)	(0.046)	(0.039)	(0.030)	(0.039)	(0.031)	(0.033)	(0.031)
ShD_{2012}	0.094*	0.081	0.087*	0.051	0.058	0.053	0.012	0.020	0.007
	(0.050)	(0.053)	(0.050)	(0.049)	(0.053)	(0.049)	(0.042)	(0.043)	(0.042)
ShD_{2013}	0.041	0.073	0.030	-0.025	-0.006	-0.022	0.006	-0.000	-0.001
	(0.052)	(0.054)	(0.052)	(0.042)	(0.048)	(0.042)	(0.040)	(0.041)	(0.039)
ShD_{2014}	0.043	0.067	0.029	0.119***	0.103**	0.123***	-0.023	-0.051	-0.033
	(0.049)	(0.050)	(0.049)	(0.045)	(0.048)	(0.044)	(0.041)	(0.040)	(0.040)
ShD_{2015}	0.049	0.065	0.032	0.150**	0.130***	0.154***	-0.014	-0.026	-0.025
	(0.050)	(0.049)	(0.049)	(0.049)	(0.049)	(0.048)	(0.043)	(0.043)	(0.042)
ShD_{2016}	0.000	-0.016	-0.021	0.145***	0.155**	0.151***	0.036	0.059	0.021
	(0.046)	(0.046)	(0.045)	(0.053)	(0.055)	(0.052)	(0.048)	(0.047)	(0.045)
ShD_{2017}	-0.033	-0.012	-0.057	0.076*	0.072	0.083*	0.043	0.025	0.026
	(0.041)	(0.042)	(0.042)	(0.045)	(0.050)	(0.047)	(0.040)	(0.040)	(0.039)
Observations	10,274	10,274	10,274	7,038	7,038	7,038	11,823	11,823	11,823
Constituencies	934	934	934	640	640	640	1,075	1,075	1,075
Within R-squared	0.039	0.127	0.062	0.084	0.191	0.111	0.072	0.157	0.101
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Constituency FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
County x Year Dummies	NO	YES	NO	NO	YES	NO	NO	YES	NO
Linear County Time Trends	NO	NO	YES	NO	NO	YES	NO	NO	YES

Note: I replicate the analysis from Table 6 after replacing (log) visible spending per capita with the (log) visible ratio as the dependent variable.

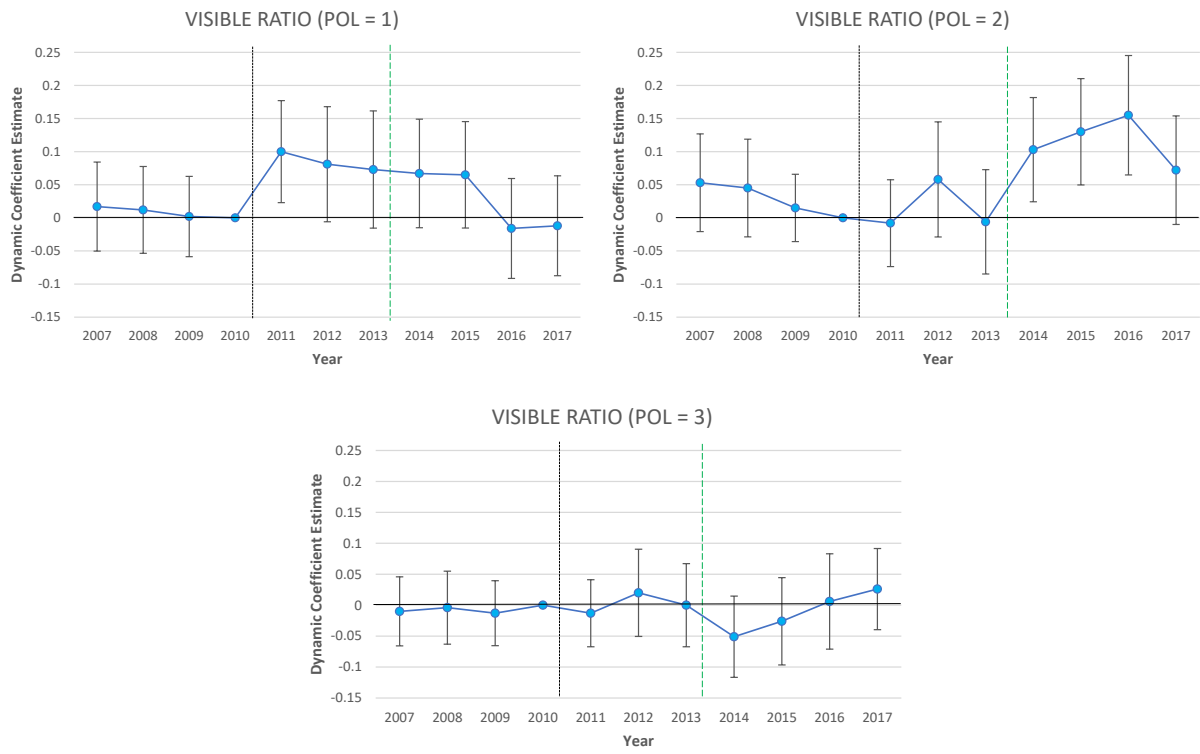**Figure A3:** The Marriage: Dynamic Analysis (Alternative Outcome)

(a) Coefficient Estimates Retrieved by Analysing Constituencies Represented by Different Political Forces Separately

(b) Coefficient Estimates Retrieved by Analysing Constituencies Represented by Different Political Forces Jointly Illustrated
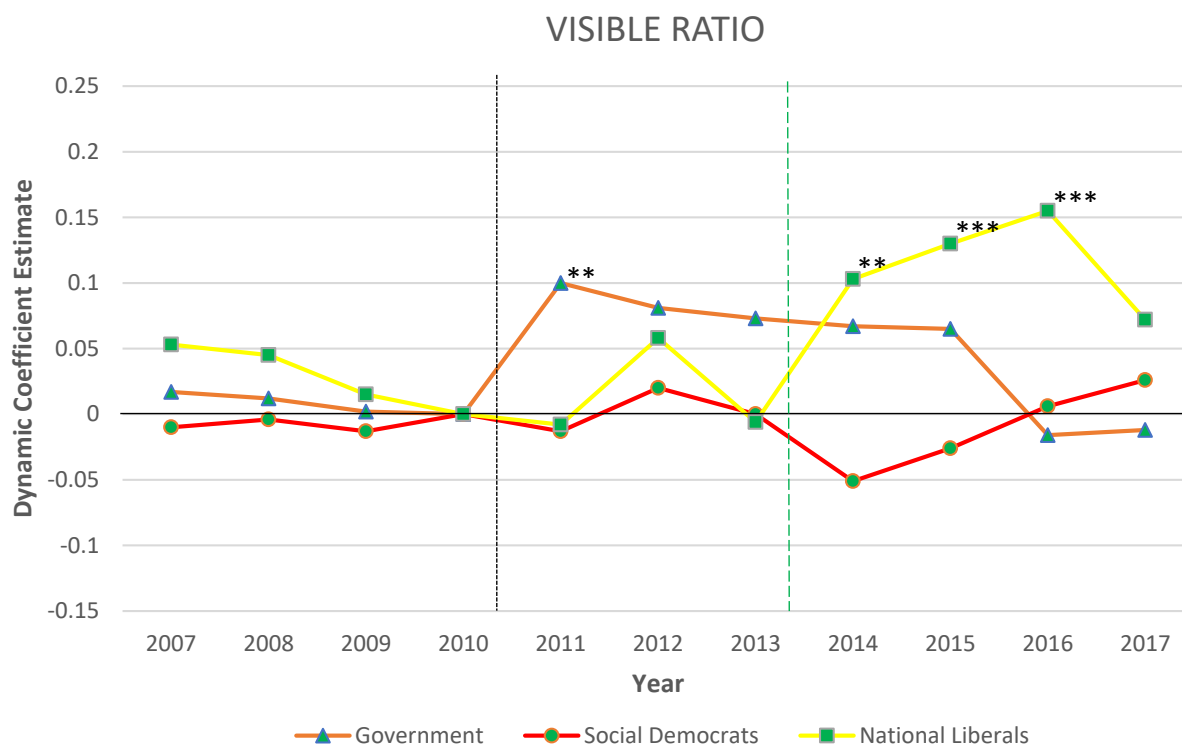
VISIBLE RATIO

Note: I replicate the graphical analyses from Figure 6 using the (log) visible ratio, as defined in text, as the dependent variable.

Table A5: Robustness of Baseline and Heterogeneity Results to Alternative Standard Error Clustering

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A - Dependent Variable: VISIBLE SPENDING						
SHUTDOWN	0.098**	0.076*	0.080*	0.012	0.005	0.005
	(0.042)	(0.042)	(0.042)	(0.049)	(0.053)	(0.053)
	[0.018]	[0.079]	[0.066]	[0.811]	[0.926]	[0.930]
SHUTDOWN * GOVT	-	-	-	0.249***	0.219**	0.230**
				(0.092)	(0.098)	(0.099)
				[0.007]	[0.032]	[0.025]
Observations	10,590	10,590	10,590	10,590	10,590	10,590
Constituencies	2,649	2,649	2,649	2,649	2,649	2,649
Within R-squared	0.022	0.059	0.046	0.025	0.062	0.049
Panel B - Dependent Variable: VISIBLE RATIO						
SHUTDOWN	0.044**	0.036*	0.038*	0.009	0.007	0.008
	(0.020)	(0.021)	(0.021)	(0.023)	(0.024)	(0.024)
	[0.029]	[0.094]	[0.076]	[0.695]	[0.759]	[0.752]
SHUTDOWN * GOVT	-	-	-	0.104**	0.087*	0.092*
				(0.046)	(0.048)	(0.048)
				[0.025]	[0.077]	[0.062]
Observations	10,596	10,596	10,596	10,596	10,596	10,596
Constituencies	2,649	2,649	2,649	2,649	2,649	2,649
Within R-squared	0.017	0.057	0.044	0.019	0.058	0.045
Year FE	YES	YES	YES	YES	YES	YES
Constituency FE	YES	YES	YES	YES	YES	YES
County x Year Dummies	NO	YES	NO	NO	YES	NO
Linear County Time Trends	NO	NO	YES	NO	NO	YES

Note: I assess the robustness of the baseline (Table 3) and interaction (Table 4 panel A) results in columns (1) - (3) and (4) - (6), respectively, to clustering standard errors at the county-level instead of the constituency-level as in the main text.

Table A6: Robustness: Alternative Visible Ratio Definition

Dependent Variable: (Log) VISIBLE SPENDING RELATIVE TO TOTAL INCOME						
	(1)	(2)	(3)	(4)	(5)	(6)
	0.014**	0.010*	0.011*	0.002	0.001	0.001
SHUTDOWN	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)
	[0.018]	[0.080]	[0.059]	[0.763]	[0.889]	[0.853]
SHUTDOWN *				0.034***	0.029**	0.030**
GOVT	-	-	-	(0.013)	(0.013)	(0.013)
				[0.010]	[0.024]	[0.020]
Observations	10,596	10,596	10,596	10,596	10,596	10,596
Constituencies	2,649	2,649	2,649	2,649	2,649	2,649
Within R-squared	0.033	0.068	0.055	0.036	0.070	0.057
Year FE	YES	YES	YES	YES	YES	YES
Constituency FE	YES	YES	YES	YES	YES	YES
County x Year Dummies	NO	YES	NO	NO	YES	NO
Linear County Time Trend	NO	NO	YES	NO	NO	YES

Note: I assess the robustness of the baseline (Table 3) and interaction (Table 4 panel A) results in columns (1) - (3) and (4) - (6), respectively, to changing the denominator in VISIBLE RATIO to overall income (instead of internal revenues as in the main text).

Table A7: Robustness of Baseline and Heterogeneity Results to the Inclusion of Directly Targeted Constituencies, as well as Towns and Municipalities

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A - Dependent Variable: VISIBLE SPENDING						
SHUTDOWN	0.074*	0.054	0.058	-0.003	-0.009	-0.009
	(0.038)	(0.040)	(0.039)	(0.045)	(0.045)	(0.045)
	[0.052]	[0.168]	[0.137]	[0.939]	[0.850]	[0.843]
SHUTDOWN * GOVT	-	-	-	0.216***	0.186**	0.195**
				(0.084)	(0.083)	(0.083)
				[0.010]	[0.025]	[0.018]
Observations	11,830	11,830	11,830	11,830	11,830	11,830
Constituencies	2,959	2,959	2,959	2,959	2,959	2,959
Within R-squared	0.021	0.055	0.043	0.023	0.057	0.045
Panel B - Dependent Variable: VISIBLE RATIO						
SHUTDOWN	0.035*	0.026	0.028	0.003	0.000	0.001
	(0.018)	(0.019)	(0.019)	(0.021)	(0.021)	(0.021)
	[0.057]	[0.165]	[0.133]	[0.899]	[0.982]	[0.971]
SHUTDOWN * GOVT	-	-	-	0.092**	0.077*	0.081**
				(0.042)	(0.041)	(0.041)
				[0.028]	[0.060]	[0.046]
Observations	11,836	11,836	11,836	11,836	11,836	11,836
Constituencies	2,959	2,959	2,959	2,959	2,959	2,959
Within R-squared	0.015	0.051	0.040	0.017	0.053	0.041
Year FE	YES	YES	YES	YES	YES	YES
Constituency FE	YES	YES	YES	YES	YES	YES
County x Year Dummies	NO	YES	NO	NO	YES	NO
Linear County Time Trends	NO	NO	YES	NO	NO	YES

Note: I assess the robustness of the baseline (Table 3) and interaction (Table 4 panel A) results in columns (1) - (3) and (4) - (6), respectively, to including in the sample constituencies where public hospitals were situated at the time of the 2011 reform, as well as localities classified as towns or municipalities.

Table A8: Robustness of Baseline and Heterogeneity Results to the Inclusion of Additional Political Affiliations

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A - Dependent Variable: VISIBLE SPENDING						
SHUTDOWN	0.095**	0.070*	0.073*	0.015	0.002	0.002
	(0.040)	(0.042)	(0.041)	(0.047)	(0.047)	(0.047)
	[0.018]	[0.096]	[0.076]	[0.753]	[0.753]	[0.972]
SHUTDOWN * GOVT	-	-	-	0.246***	0.220**	0.230***
				(0.091)	(0.089)	(0.089)
				[0.007]	[0.014]	[0.010]
Observations	11,210	11,210	11,210	11,210	11,210	11,210
Constituencies	2,804	2,804	2,804	2,804	2,804	2,804
Within R-squared	0.022	0.058	0.045	0.025	0.060	0.048
Panel B - Dependent Variable: VISIBLE RATIO						
SHUTDOWN	0.041**	0.030	0.032	0.008	0.003	0.003
	(0.019)	(0.020)	(0.020)	(0.022)	(0.022)	(0.022)
	[0.034]	[0.136]	[0.106]	[0.726]	[0.905]	[0.885]
SHUTDOWN * GOVT	-	-	-	0.105**	0.089**	0.094**
				(0.046)	(0.045)	(0.045)
				[0.022]	[0.046]	[0.034]
Observations	11,216	11,216	11,216	11,216	11,216	11,216
Constituencies	2,804	2,804	2,804	2,804	2,804	2,804
Within R-squared	0.017	0.056	0.043	0.019	0.057	0.045
Year FE	YES	YES	YES	YES	YES	YES
Constituency FE	YES	YES	YES	YES	YES	YES
County x Year Dummies	NO	YES	NO	NO	YES	NO
Linear County Time Trends	NO	NO	YES	NO	NO	YES

Note: I assess the robustness of the baseline (Table 3) and interaction (Table 4 panel A) results in columns (1) - (3) and (4) - (6), respectively, to including in the sample constituencies where candidates of "other" political parties won the 2008 mayoral election (see Section 4.3 for details). For the purposes of this analysis, I classify these constituencies as non-governmental - that is, GOVT is equal to zero in these areas.

Table A9: Robustness: Sample Restricted to Constituencies Represented by the Democratic Liberals

	Panel A - Dependent Variable: VISIBLE SPENDING			Panel B - Dependent Variable: VISIBLE RATIO		
	(1)	(2)	(3)	(4)	(5)	(6)
SHUTDOWN	0.208**	0.204**	0.206**	0.088**	0.086*	0.090**
	(0.082)	(0.088)	(0.086)	(0.043)	(0.045)	(0.044)
	[0.011]	[0.021]	[0.017]	[0.039]	[0.055]	[0.042]
Observations	3,083	3,083	3,083	3,084	3,084	3,084
Constituencies	771	771	771	771	771	771
Within R-squared	0.016	0.085	0.052	0.015	0.080	0.050
Year FE	YES	YES	YES	YES	YES	YES
Constituency FE	YES	YES	YES	YES	YES	YES
County x Year Dummies	NO	YES	NO	NO	YES	NO
Linear County Time Trends	NO	NO	YES	NO	NO	YES

Note: I replicated the analyses in Table 4 panel A after removing from the sample constituencies whose mayor was affiliated with the UDMR when the shut-down was carried out.

Table A10: Cross-Tabulation of Austerity Exposure and Local Official Partisanship (Alternative Definition)

		HOSPITAL CLOSED		
		0	1	Total
POL	1	738	196	934
		(79.0)	(21.0)	(35.3)
	2	462	178	640
		(72.2)	(27.8)	(24.1)
	3	826	249	1,075
		(76.8)	(23.2)	(40.6)
	Total	2,026	623	2,649
		(76.5)	(23.5)	(100.0)

Note: HOSPITAL CLOSED is a dummy variable equal to one in constituencies belonging to the shutdown's catchment area as defined in text (zero otherwise). POL is an categorical variable equal to one in constituencies whose mayor at the time of the shut-down is politically affiliated with the parties comprising the central government responsible for austerity, equal to two when the mayor is affiliated with the NLs, and equal to three when the mayor is affiliated with the SDs. The information presented here pertains to the final sample - see Section 4.3 for the restrictions applied.

Appendix B - Supplementary Figures

Figure B1: Romania's Administrative Divisions

Note: Black lines separate counties, while white lines capture constituency boundaries.