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Floods, Public Budgets and Fiscal Resilience: Evidence from Italian Municipalities

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Abstract
Hydrogeological disasters are an increasingly important source of fiscal stress for local governments, yet evidence on how municipal budgets adjust to these events remains limited. This paper estimates the fiscal effects of hydrogeological State of Emergency declarations on Italian municipal budgets over 2016–2022, exploiting their staggered timing across municipalities. We find that treated municipalities increase both total revenues and total expenditures after first exposure. The response is primarily investment-led, with capital expenditures accounting for the largest estimated component of the adjustment while current expenditures remain broadly unchanged. The largest changes in expenditure composition involve civil protection activities and sustainable development. On the revenue side, the adjustment is driven mainly by capital revenues and intergovernmental transfers. We further examine municipal heterogeneity using an *ex-ante* measure of fiscal resilience based on debt-service burden and tax autonomy. Municipalities with lower debt-service burden display sharper immediate fiscal adjustments, whereas tax autonomy matters more for sustaining the fiscal response over time. The weakest responses are observed among municipalities combining high debt-service burden with weak tax autonomy. Overall, the evidence suggests that hydrogeological emergencies trigger a structured, investment-led fiscal response, but that pre-existing fiscal conditions shape local governments' capacity to absorb and manage these disaster-related fiscal pressures.

Keywords Hydrogeological disasters · Municipal budgets · Fiscal resilience

JEL H71 · H72 · H74 · Q54

1 Introduction

Climate change poses a growing threat to both environmental and socio-economic systems worldwide (Nordhaus 2019). According to the European Centre for Medium-Range Weather Forecasts (ECMWF, part of the Copernicus Programme), 2024 was the first year in which the average global temperature exceeded the pre-industrial level by 1.5° C, highlighting

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the accelerating impact of anthropogenic activities on the climate system (ECMWF 2025). One major consequence has been the increasing frequency and severity of climate-related natural disasters, particularly extreme precipitation events that trigger floods and landslides (AghaKouchak et al. 2020; World Meteorological Organization 2021).

Hydrogeological disasters are increasingly affecting urban areas, although their impacts remain uneven across space. Mediterranean regions, often described as a climate-change hotspot, are especially exposed because of their climatic and geographical characteristics (Llasat et al. 2010; Marchi et al. 2010; Amponsah et al. 2018). Within this area, Italy represents a particularly relevant case study, given its complex orography, patterns of urban development, and exposure to extreme weather events, which make it highly vulnerable to hydrogeological hazards (Tropeano and Turconi 2004; Guida et al. 2016; Esposito et al. 2023). According to the Italian Landslide Inventory (IFFI) and the EM-DAT database, more than 1,000 landslides and 11 major floods affected Italy between 2016 and 2024. Hydrogeological disasters accounted for 41% of all extreme natural events in recent years (Guha-Sapir et al. 2013).

The economic consequences of these events are substantial. Hydrogeological disasters destroy physical capital, disrupt production and supply chains, and generate losses in income and wealth (Hallegatte et al. 2007; Hallegatte and Przyluski 2010; Cavallo and Noy 2011; Collalti 2024). More specifically, infrastructure damage reduces labour income by interrupting business activity and employment (Tovar Reaños 2021; Jia et al. 2022; Clò et al. 2024), while declining property values erode household wealth and long-term investment returns (Leiter et al. 2009; Roy 2023; Ballocci et al. 2024). These private-sector shocks quickly spill over into public finances: municipal tax revenues weaken precisely when expenditure should increase to finance emergency services, infrastructure reconstruction, and social assistance (Ouattara and Strobl 2013; Mohan et al. 2018; Ahmadu and Nukpezah 2022; Deryugina 2022).

Municipal governments stand at the frontline of disaster management. They are responsible for immediate emergency response, the continuity of essential public services, and the planning of longer-term recovery (Noy and Nualsri 2011). Meeting these responsibilities requires the capacity to mobilise scarce resources and translate them into effective policy actions, including preparedness, emergency intervention, reconstruction, and adaptation investments (Melecky and Raddatz 2015; Richert et al. 2019; Fan et al. 2022; Motlagh et al. 2024). Yet local governments differ markedly in their ability to respond to these shocks. Hydrogeological risk interacts with geographical, fiscal, and institutional heterogeneity. Geomorphology, land use, urbanisation, governance quality, and socio-economic characteristics shape both exposure to disasters and the local capacity to manage their fiscal and operational consequences (Marchi et al. 2010; Lazzaroni and Van Bergeijk 2014; Bosseler et al. 2021; Feng et al. 2021; Marin et al. 2021).

As a result of this heterogeneity, similar hydrogeological shocks may translate into different fiscal pressures across municipalities. Some municipalities enter a disaster episode with greater *room for manoeuvre*, while others face the shock under tighter financial constraints. This dimension is captured by the concept of *fiscal resilience*, defined as a municipality's ability to absorb disaster-related fiscal stress, mobilise resources, and preserve long-term financial sustainability. Recent work increasingly links local resilience to concrete fiscal buffers, such as limited debt-service pressure and greater revenue autonomy, rather than to formal autonomy alone (Lis and Nickel 2010; Barbera et al. 2013; Haksar et al. 2018;

OECD 2022; Jerch et al. 2023). Beyond these budgetary margins, the disaster response also depends on the administrative capacity to activate and implement extraordinary resources once they become available (Crow and Albright 2021). Where fiscal buffers are weak or administrative capacity is limited, local governments may face difficult trade-offs, including cuts to essential public services, postponed infrastructure investment, or increasing debt burdens (Deria et al. 2020; Deryugina 2022; Ishida et al. 2023).

In Italy, municipal fiscal adjustment takes place within a regulated institutional framework. On the one hand, municipal borrowing can finance capital expenditures only and is subject to debt-service sustainability and balanced-budget requirements (Monacelli et al. 2016). On the other hand, post-disaster intervention is activated through formal emergency declarations, hereafter *State of Emergency*, and financed largely through intergovernmental transfers (IFRC 2022). These features make Italy a compelling setting for studying how fiscal capacity shapes local responses to hydrogeological disasters, since the municipal fiscal response depends on the interaction between fiscal rules and external support.

Against this background, this paper studies how Italian municipal governments adjust their budgets in response to hydrogeological disasters between 2016 and 2022. We address two main questions. First, *how do these disasters affect municipal budget composition on the revenue and expenditure sides?* Second, *to what extent does pre-existing fiscal resilience shape the capacity of municipal governments to absorb and manage disaster-related fiscal pressures?* To answer these questions, we exploit the staggered timing with which municipalities enter the official emergency-fiscal framework through declared hydrogeological States of Emergency, and estimate dynamic treatment effects using the staggered difference-in-differences estimator proposed by Callaway and Sant'anna (2021). The baseline empirical design focuses on the immediate municipal fiscal response to first-time exposure to a formally declared State of Emergency.[1] This framing allows us to isolate the first fiscal adjustment associated with a disaster episode, while preserving a transparent identification strategy. We then construct a measure of *ex-ante* fiscal resilience based on debt-service burden and tax autonomy in order to evaluate how pre-disaster municipal fiscal conditions shape the magnitude and persistence of budgetary responses.

The paper contributes to the literature in three ways. First, it moves beyond existing evidence on the average fiscal effects of natural disasters and their aggregate implications for local budgets (Lis and Nickel 2010; Noy and Nualsri 2011; Miao et al. 2020; Jerch et al. 2023; Lodi et al. 2023) by tracing the composition of municipal budget adjustment across revenue titles and expenditure missions. This allows us to identify which fiscal margins are activated, distinguishing between adjustment through current resources and capital transfers, and between routine spending and investment-oriented expenditure. Second, while previous work has shown that fiscal rules and institutional constraints shape local government behaviour (Grembi et al. 2016), the paper provides evidence on the role of *ex-ante* fiscal resilience in post-disaster adjustment. By combining debt-service burden and tax autonomy into a predetermined measure of fiscal resilience, we document how initial fiscal conditions shape both the intensity and the persistence of municipal budgetary responses. This highlights a resilience gap that remains hidden when local governments are treated as having homogeneous response capacity (Barbera et al. 2021; Jerch et al. 2023; Lodi et al.

[1] Municipalities experiencing repeated exposure during the study window are excluded from the baseline specification to avoid conflating first-exposure effects with fiscal adjustments induced by subsequent disaster episodes.

2023). Third, by covering the universe of Italian municipalities over the harmonised-budget period, the analysis provides national-scale evidence within a unified institutional and accounting framework. This broader coverage improves external validity relative to studies based on selected events or narrower territorial samples (Trezzi and Porcelli 2016; Masiero and Santarossa 2020; Lodi et al. 2023), while preserving internal consistency in treatment assignment and budget measurement.

Our results show that municipalities exposed to a hydrogeological State of Emergency experience substantial increases in total expenditure, driven mainly by capital expenditure, while current expenditure remains broadly unchanged. The largest changes in expenditure composition involve civil protection activities and sustainable development. On the revenue side, the expenditure increase is primarily matched by higher capital revenues, largely reflecting intergovernmental transfers. We also find that the strength and persistence of fiscal responses depend on municipal fiscal resilience. Lower debt-service burdens are associated with stronger and earlier expenditure responses, while tax autonomy matters mainly for the persistence of the adjustment over time. Notably, the weakest responses are observed among municipalities that combine high debt burdens with weak tax autonomy.

From a policy perspective, these findings underscore the importance of strengthening the institutional and financial architecture that supports local fiscal resilience. Timely, predictable, and equitable access to external resources is especially important for municipalities with limited fiscal resilience, as it can improve their capacity to respond to hydrogeological disasters without delaying essential interventions. At the same time, the results point to the value of building fiscal buffers before shocks occur. Accumulating reserves during periods of fiscal stability can increase the ability of local governments to absorb climate-related shocks without resorting to disruptive expenditure cuts or unsustainable borrowing, thereby safeguarding the continuity of public services and long-term financial sustainability.

The remainder of the paper is organised as follows. Section 2 outlines the theoretical framework, covering the municipal budget constraint and the concept of fiscal resilience. Section 3 describes the institutional setting for disaster management in Italy, with attention to the State of Emergency declaration process and the financing and management of post-disaster intervention. Section 4 presents the empirical analysis, focusing on data, key variables, and identification strategy. Section 5 discusses the main results, including dynamic treatment effects, budget composition, and heterogeneity by fiscal resilience. Section 6 concludes.

2 Theoretical Framework

2.1 The Municipal Budget Constraint

The municipal budget constraint provides an accounting framework for analysing how local governments finance expenditure needs, absorb fiscal shocks, and preserve financial sustainability over time. For municipality i in year t, it can be written as:

$$T_{i,t} + \mathrm{Tr}_{i,t} + \Delta B_{i,t} = G_{i,t} + rB_{i,t-1}. \tag{1}$$

The left-hand side of (1) denotes the total fiscal resources available to the municipality. These include own-source tax revenues ($T_{i,t}$), intergovernmental transfers ($\mathrm{Tr}_{i,t}$) received from higher levels of government (regional or national), and net new borrowing ($\Delta B_{i,t}$), that is, the change in outstanding debt. The right-hand side captures total fiscal obligations. These comprise primary expenditure ($G_{i,t}$), which includes both current and capital outlays, and interest payments on existing debt ($rB_{i,t-1}$), where r denotes the nominal interest rate and $B_{i,t-1}$ is outstanding debt at the beginning of the period t.

We are interested in whether and how hydrogeological disasters affect the dynamics of the municipal budget constraint. Let $X_{i,t}$ denote disaster exposure in municipality i at time t. Since the inherited stock of debt, $B_{i,t-1}$, is predetermined at the time of the shock, the contemporaneous fiscal response can be studied by differentiating (1) with respect to $X_{i,t}$, conditional on the pre-existing debt burden $B_{i,t-1}$:[2]

$$\left.\frac{\partial G_{i,t}}{\partial X_{i,t}}\right|_{B_{i,t-1}} = \underbrace{\frac{\partial T_{i,t}}{\partial X_{i,t}}}_{\text{Tax revenues}} + \underbrace{\frac{\partial \mathrm{Tr}_{i,t}}{\partial X_{i,t}}}_{\text{Transfers}} + \underbrace{\frac{\partial \Delta B_{i,t}}{\partial X_{i,t}}}_{\text{New borrowing}} . \tag{2}$$

Equation (2) identifies three *adjustment margins* through which disaster exposure affects municipal expenditure: (i) tax revenues, (ii) intergovernmental transfers, and (iii) new borrowing. The inherited debt burden does not enter this derivative as a contemporaneous outcome of the shock. Rather, it enters the budget constraint as a pre-existing fiscal obligation that reduces the municipality's fiscal space[3] and conditions the extent to which expenditure needs can be accommodated through additional borrowing.

The effect of $X_{i,t}$ on municipal expenditure, $\partial G_{i,t}/\partial X_{i,t}$, therefore depends on three contemporaneous fiscal channels and on one pre-existing constraint:

- *Tax revenues* may decline in the aftermath of hydrogeological disasters because of disruptions to local economic activity, damage to taxable assets, and population displacement. Assuming constant tax rates, these effects erode the local tax base and reduce own-source revenues. The observed response, however, may be ambiguous $\left(\partial T_{i,t}/\partial X_{i,t} \lesseqgtr 0\right)$, since municipalities may adjust tax rates or collection efforts in response to fiscal stress (Ahmadu and Nukpezah 2022). This trade-off reflects the tension between short-term revenue needs and long-term economic sustainability.

- *Intergovernmental transfers* typically increase after a disaster, especially when an official State of Emergency activates targeted support from regional and central authorities (Lodi et al. 2023). A positive marginal effect ($\partial \mathrm{Tr}_{i,t}/\partial X_{i,t} > 0$) is therefore expected when the declaration leads to additional funding for affected municipalities. The fiscal effect, however, also depends on the timing, adequacy, and composition of these finan-

[2] We assume that the interest rate is exogenous and unaffected by the shock, i.e., $\partial r/\partial X_{i,t} = 0$. While Jerch et al. (2023) shows that disaster exposure can raise local government borrowing costs in the United States, where municipal governments rely on market-based debt issuance, this channel is less relevant in the Italian context. Indeed, municipal borrowing in Italy is regulated and mediated by centralised institutions, such as *Cassa Depositi e Prestiti (CDP)*, which limits the scope for municipality-specific interest-rate adjustments following idiosyncratic shocks.

[3] The IMF defines fiscal space as "room in a government's budget that allows it to provide resources for a desired purpose without jeopardising the sustainability of its financial position or the stability of the economy" (Heller 2005).

cial transfers (Deryugina 2017; Miao et al. 2020).

- *New borrowing* may increase when municipalities rely on debt financing to support reconstruction and resilience-related investment $(\partial \Delta B_{i,t}/\partial X_{i,t} \geq 0)$. The magnitude of this response depends on pre-existing fiscal conditions, since legal borrowing limits, balanced-budget requirements, and debt-service constraints may restrict the ability to take on additional debt even when reconstruction needs are urgent (Monacelli et al. 2016; Jerch et al. 2023; Miao et al. 2023).
- *Debt-service obligations* constrain fiscal policy when municipalities enter the disaster period with an already elevated debt burden. A larger share of the budget is then pre-committed to interest payments $(rB_{i,t-1})$, reducing the fiscal space available for essential services and post-disaster interventions. In this sense, inherited debt does not represent a contemporaneous fiscal response to the shock, but a pre-existing constraint that shapes the municipality's ability to absorb it. Moreover, any disaster-induced increase in borrowing can increase future debt-service costs, further tightening budgetary constraints (Melecky and Raddatz 2015; Chen 2020; Jerch et al. 2023).

These channels show that natural disasters may generate a *fiscal squeeze*. Expenditure needs increase, own-source revenues may weaken, and the ability to expand borrowing depends on the municipality's pre-existing fiscal position. In favourable cases, timely and sufficient intergovernmental transfers can offset revenue losses and support post-disaster spending. However, when transfers are delayed or insufficient, municipalities with limited fiscal space may be forced to postpone investment, compress other expenditure, or accumulate additional debt. The same logic can be read directly off the budget constraint by focusing on municipal expenditure in (1):

$$G_{i,t} = T_{i,t} + \mathrm{Tr}_{i,t} + \Delta B_{i,t} - rB_{i,t-1}. \tag{3}$$

Equation (3) shows that the expenditure a municipality can sustain is supported by own tax revenues $T_{i,t}$, intergovernmental transfers $\mathrm{Tr}_{i,t}$, and new borrowing $\Delta B_{i,t}$, while it is reduced by the burden of inherited debt $rB_{i,t-1}$. After a disaster, expenditure needs typically rise, especially for reconstruction and capital investment. Whether these needs can be met or not depends not only on the size of the shock, but also on the fiscal position from which the municipality enters the emergency.

In this framework, two pre-disaster conditions are central. On the one hand, the strength of the *autonomous revenue base* determines how much of the adjustment can be supported through own resources. A weak tax base narrows the municipality's ability to mobilise resources internally and increases dependence on external support. On the other hand, the inherited debt-service burden mechanically subtracts fiscal space and, together with legal borrowing limits, constrains the scope for additional borrowing. High debt service therefore reduces both the resources immediately available for post-disaster spending and the room for using debt as an adjustment margin.

When both conditions are unfavourable, post-disaster spending is critically dependent on intergovernmental transfers. If transfers are delayed or insufficient, expenditure needs cannot be fully accommodated, and reconstruction may be postponed or essential current spending compressed. By contrast, municipalities entering the shock with stronger own revenues and lower debt-service obligations can rely on broader margins of adjustment and are less dependent on the timing and adequacy of external support.

The accounting framework therefore identifies the fiscal channels through which disasters operate on municipal budgets. It also makes clear that the same hydrogeological shock can leave different fiscal traces depending on the fiscal position from which a municipality enters the emergency.

2.2 Fiscal Resilience

Fiscal resilience builds on the notion of fiscal space, but adapts it to the specific case of an exogenous disaster shock. It captures observable pre-disaster budgetary conditions that shape the municipal response when the shock occurs. The concept also relates to the broader literature on governmental financial resilience, which studies how governments anticipate, absorb, and recover from external shocks affecting public finances and the continuity of public services (Barbera 2017; Barbera et al. 2017, 2020, 2021; Steccolini et al. 2017; Ahrens and Ferry 2020; Saliterer et al. 2021).

In the post-disaster context studied here, fiscal resilience denotes the ability of a municipality to mobilise resources for relief and reconstruction, sustain essential public services, and preserve long-term financial sustainability without resorting to disruptive expenditure cuts or unsustainable borrowing (Melecky and Raddatz 2015; Noy and Edmonds 2019; OECD and The World Bank 2019; Padovani et al. 2021). We anchor this concept directly to the channels of the budget constraint introduced in Sect. 2.1.

Two conditions are particularly relevant. The first is the *debt-service burden*, captured by the share of current revenues pre-committed to interest payments on inherited debt. A larger burden mechanically narrows the borrowing margin and crowds out current and capital outlays precisely when they are most needed (Melecky and Raddatz 2015; Chen 2020; Berset et al. 2023; Jerch et al. 2023). The second is the *strength of the autonomous revenue base*, captured by the share of own-source taxes in current revenues. A stronger autonomous revenue base provides a flexible margin of adjustment. Municipalities with a broader own tax base can lean on local revenues to bridge financing gaps, sustain essential spending, and access credit on more favourable terms while waiting for external support (Barbera 2017; Shi and Varuzzo 2020; Jimenez and Afonso 2022). The composition of that base also matters. A more diversified local economy spreads exposure across firms and sectors, whereas a base concentrated on a few large taxpayers leaves the municipality more vulnerable when those activities are directly hit by the shock (Carroll 2009). Where own revenues are thin or narrowly concentrated, post-disaster adjustment depends more heavily on intergovernmental transfers, with the timing, conditionality, and adequacy issues these typically entail (Garrett and Sobel 2003; Naess et al. 2005; Kondoh and Miyazaki 2022; Arachi et al. 2023).

Beyond these budgetary margins, the post-disaster response also depends on the administrative ability to mobilise available resources. Emergency procedures must be activated, eligible projects prepared, procurement carried out, and reconstruction projects executed within the relevant accounting cycle (Albright and Anderson 2021; Crow and Albright 2021; Bafundi et al. 2023; Baltrunaite et al. 2024). Administrative capacity therefore operates as a complementary enabling channel alongside fiscal resilience, rather than as part of its construction. We do not incorporate it into the typology developed below, which is built on predetermined budgetary margins, but it enters the empirical analysis as a municipal-level control to improve comparability across municipalities with different administrative environments.

Combining debt-service burden and the strength of the autonomous revenue base yields a compact typology of fiscal resilience. Figure 1 arranges municipalities along these two dimensions and identifies four configurations.

Municipalities with low debt-service burden and strong tax autonomy (Quadrant 2) display the highest fiscal resilience, combining a less constrained borrowing margin with a more flexible own-revenue base. The opposite configuration (Quadrant 3) displays the lowest resilience, since inherited interest obligations reduce fiscal room while a thin tax base limits the capacity to mobilise internal resources after the shock. The off-diagonal cases (Quadrants 1 and 4) are intermediate, as one budgetary margin partly offsets weakness in the other.

This typology links the conceptual framework to the empirical analysis. The channels identified by the budget constraint define the fiscal outcomes examined below: total revenues and expenditures, their composition across revenue titles and expenditure missions, and the distinction between current and capital spending. Fiscal resilience defines the predetermined dimension along which we assess heterogeneous responses. We test whether municipalities entering the emergency-fiscal framework with lower debt-service burden and stronger tax autonomy experience stronger and more persistent post-treatment fiscal

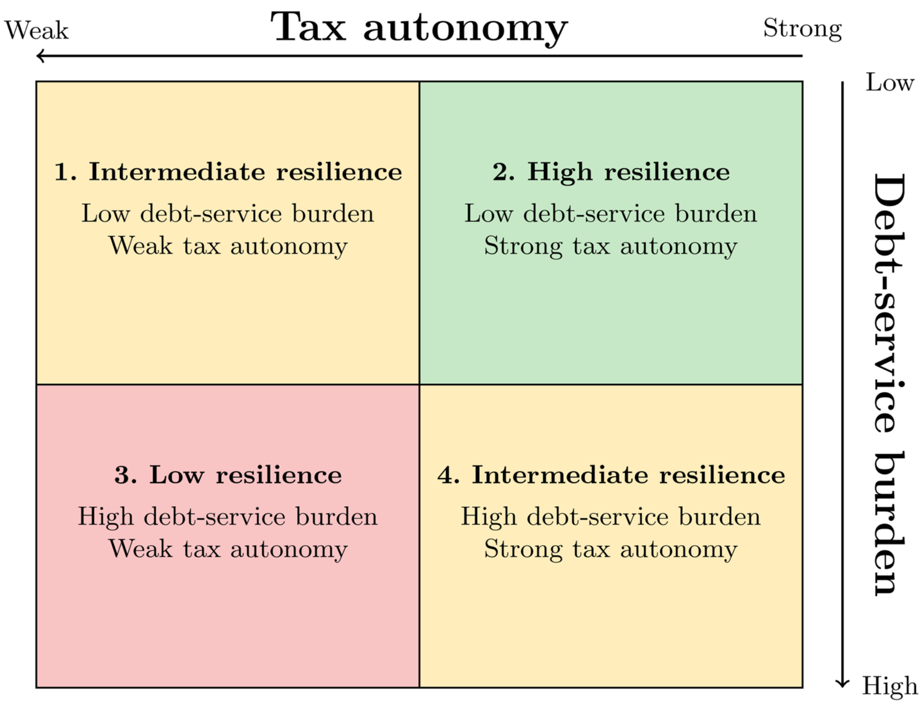

Fig. 1 Fiscal resilience typology. Note. Municipalities are classified into four categories based on their 2016 pre-treatment values of debt-service burden and tax autonomy. Debt-service burden is measured as the ratio of interest payments to current revenues; tax autonomy is measured as the ratio of tax revenues to current revenues. Fiscal resilience is stronger when debt-service burden is lower and tax autonomy is greater

adjustment than municipalities entering with thinner own resources and tighter borrowing margins.

3 Disaster Management in Italy

Italy's civil protection system, a core component of the country's disaster risk management and multi-level resilience governance, constitutes the institutional backbone of our empirical strategy. It defines both the legal framework for public intervention in the wake of hydrogeological disasters and the fiscal instruments through which reconstruction funds are channelled to municipal governments. This section outlines the key institutional features relevant to our identification approach.[4]

The modern National Civil Protection Service (NCPS) was established by L. 225/1992, which for the first time conceptualised disaster management as a continuous cycle encompassing forecasting, prevention, emergency response, and recovery. However, major disasters – including the Irpinia earthquake (1980), the Piemonte floods (1994), the L'Aquila earthquake (2009), and the Emilia earthquake (2012) – exposed persistent coordination failures. These challenges prompted significant institutional reforms, notably D.L. 59/2012, converted into L. 100/2012, followed by the Civil Protection Code (D.Lgs. 1/2018). The Code consolidated a previously fragmented legal framework and formalised key principles of disaster governance: solidarity, subsidiarity, and unified command. Most relevant for our study, it (i) standardises the classification of emergency events, (ii) imposes a uniform limit on national States of Emergency, and (iii) centralises the entire response chain, from emergency relief to long-term reconstruction, within the Civil Protection Service until the restoration of ordinary administrative authority (Articles 24–27).

Disaster response in Italy follows a multi-level governance structure. At the local level, mayors serve as the primary civil protection authorities. Presidents of regional (NUTS-2 level) governments coordinate responses to medium-scale events. In cases of major disasters, the Civil Protection Department, under the direct authority of the Prime Minister, takes command via the Permanent Operational Committee (POC) (Natoli 2022). When local capacity is overwhelmed, the Council of Ministers, upon formal request from the affected region, issues a State of Emergency decree. This decree appoints a Commissioner with extraordinary powers to issue urgent orders, including derogations from standard administrative procedures. Crucially for municipal finance, the decree also authorises the creation of a special accounting line into which state funds are deposited and from which all emergency-related expenditures are disbursed. This institutional step is central to our empirical design. The hydrogeological event is the underlying shock, but the treatment used in the analysis is the municipality's inclusion in an officially declared State of Emergency, since this is the moment at which the fiscal response mechanism becomes operative through the emergency governance framework and the associated special accounting line. For this reason, when the underlying physical event and the emergency declaration do not coincide exactly in calendar time, treatment timing is anchored to the declaration year, in line with the annual structure of budget data.

[4] For a discussion of the legal and institutional evolution of Italy's civil protection system, see Spuntarelli (2021) and Del Pinto et al. (2024), who examine both the normative foundations and operational implementation.

Initial relief is financed through the National Emergency Fund (Art. 44 of D.Lgs. No. 1/2018) and may be supplemented by ad hoc appropriations, intra-ministerial reallocations, or, for large-scale disasters, grants from the European Union Solidarity Fund (EUSF) (Hochrainer-Stigler et al. 2023).

As reported in Table 1, between 2016 and 2022, the Council of Ministers declared 92 hydrogeological States of Emergency, allocating approximately €991.75 million across 104 provinces and 4,045 municipalities.[5]

Because States of Emergency are event-specific, their geographic coverage varies across time and space. Some municipalities are affected multiple times, while others are never exposed. This staggered and heterogeneous exposure structure, illustrated in Fig. 2, forms the empirical foundation of our identification strategy. Two institutional features make treatment assignment plausibly exogenous to pre-existing municipal fiscal conditions. First,

Table 1 Hydrogeological States of emergency declared in Italy, 2016–2022

Region	Number of emergency declarations	Provinces involved (NUTS-3)	Unique municipalities involved (LAU)	Allocated funds (million €)
Abruzzo	1	3	19	10.94
Basilicata	5	2	31	22.37
Calabria	6	5	254	64.22
Campania	5	5	82	31.82
Emilia-Romagna	12	9	327	183.73
Friuli-Venezia Giulia	4	4	215	30.42
Lazio	3	5	194	8.38
Liguria	8	4	222	87.52
Lombardia	7	12	590	61.08
Marche	2	5	34	24.33
Molise	1	2	117	5.40
Piemonte	7	8	837	136.62
Puglia	4	6	167	41.38
Sardegna	2	3	46	7.26
Sicilia	8	9	258	52.07
Toscana	5	11	246	79.38
Trentino-Alto Adige	2	2	82	6.65
Umbria	1	1	12	0.60
Valle d'Aosta	2	1	27	5.72
Veneto	7	7	285	131.87
Italy	**92**	**104**	**4,045**	**991.75**

Note. Figures refer to hydrogeological States of Emergency formally declared by the Italian Council of Ministers between 2016 and 2022. The number of affected provinces and municipalities reflects unique jurisdictions involved across all declarations; areas affected by multiple events are counted only once. Allocated funds represent the total financial resources disbursed under each State of Emergency decree, divided equally among the regions affected by that event and then aggregated at the regional level. Source. Authors' calculationsbased on data from the Italian Civil Protection Department

[5] Some municipalities were affected repeatedly. Forlì and Cesena experienced 12 events; Bologna, Ferrara, Modena, Parma, and Reggio Emilia each faced 11; Piacenza and Ravenna were hit 10 times. Several municipalities, including Argenta, Cesenatico, Comacchio, Compiano, Farini, Modena, and Neviano degli Arduini, were affected by up to 9 events. However, most affected municipalities (2,309) experienced only one event.

eligibility is rule-based: municipalities are automatically included if they fall within the declared emergency perimeter delimited under Articles 24–27 of the Civil Protection Code, limiting the scope for post-declaration lobbying. Second, post-disaster expenditures are financed almost entirely through central government transfers (OECD 2018; Spuntarelli 2021). These features mitigate concerns about selection bias and simultaneity, supporting a quasi-experimental interpretation of the observed fiscal responses, in line with the broader literature exploiting disaster declarations as exogenous sources of variation in local fiscal conditions (Garrett and Sobel 2003; Reeves 2011; Deryugina 2017; Miao et al. 2020, 2023; Roth Tran and Wilson 2025).

Taken together, these institutional arrangements provide a credible basis for the empirical design. The State of Emergency mechanism, governed by standardised legal procedures and objective eligibility rules, generates plausibly exogenous variation in the timing and geographic scope of treatment. Moreover, the centralisation of emergency funding, via intergovernmental capital transfers, reduces the influence of local fiscal discretion on treatment intensity.

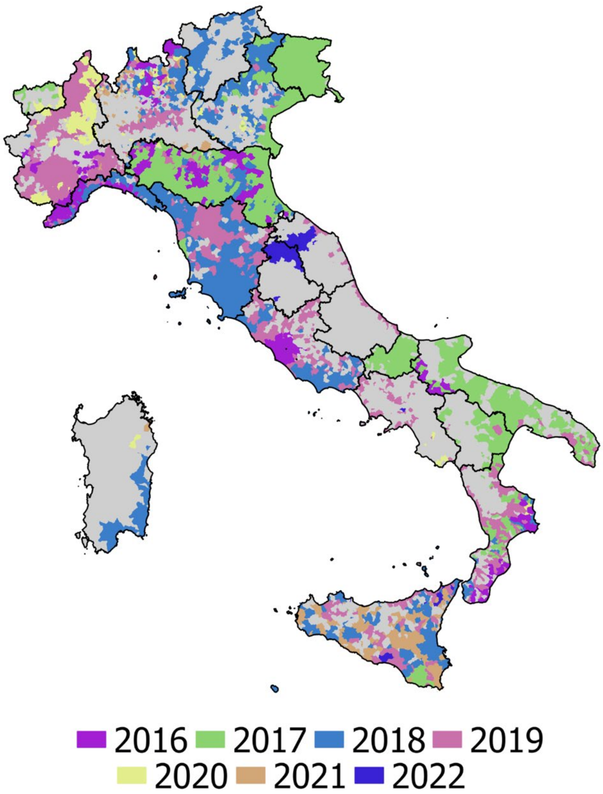

Fig. 2 First year of treatment across municipalities. Note. The map displays the year in which each municipality was first included in a hydrogeological State of emergency declared between 2016 and 2022. Colours represent the first year of exposure, while grey indicates municipalities that were not included during the period. Authors' calculations based on data from the Italian Civil protection department

4 Empirical Analysis

4.1 Data Description

To estimate the fiscal effects of official hydrogeological emergency exposure on municipal budgets, and to assess the role of local fiscal resilience, we construct a comprehensive longitudinal dataset covering the universe of Italian municipalities over the period 2016–2022. The dataset combines administrative, fiscal, and institutional sources. This allows us to identify the timing of treatment and to trace its budgetary consequences with detailed spatial and temporal resolution.

The sample period is institutionally determined by the availability of harmonised municipal budget data. The panel spans 2016–2022 because municipal budget data become harmonised and fully comparable from 2016 under the current accounting framework introduced by D.Lgs. No. 118/2011, while 2022 is the last available and validated year in the dataset (Gatto et al. 2023). We retain 2016 in the estimation rather than treating it as a holdout year. It serves both as the common pre-treatment baseline and as the benchmark used to construct the ex-ante fiscal resilience typology, since the lack of harmonised data for 2015 makes anchoring the typology to an earlier year infeasible.

Municipalities are classified as treated when two conditions are satisfied. First, they are included in a formal State of Emergency declaration issued by the national government in response to a hydrogeological event. Second, they receive post-disaster reconstruction funds through a dedicated accounting line. These criteria ensure that treatment captures both the institutional recognition of the emergency and the activation of the fiscal mechanism relevant to our analysis. The resulting treatment therefore measures exposure to the official emergency-fiscal framework, rather than physical damage alone.

Two implications follow. First, municipalities included in the same declaration may experience different degrees of direct damage, while smaller hydrogeological events may occur outside the national State of Emergency framework. Second, when the physical event and the declaration do not coincide in calendar time, we date treatment to the declaration year, since this is when disaster-related fiscal resources are transmitted in annual budget data.[6]

Between 2016 and 2022, a total of 4,045 municipalities were included in at least one hydrogeological State of Emergency, across 16 major events (see Tables A.1–A.2). Our source for treatment assignment is the georeferenced database by Gatto et al. (2023), which documents floods and landslides associated with formal emergency declarations and harmonises definitions and administrative classifications. This data structure is important because it connects harmonised municipal budget data to event-level treatment information and pre-disaster fiscal conditions, allowing us to study both the composition of budgetary adjustment and its heterogeneity across municipalities with different initial levels of fiscal resilience.

To strengthen identification, we exclude municipalities that were already exposed to a hydrogeological State of Emergency between 2013 (first year covered by Gatto et al. 2023) and 2016 and therefore appear as always-treated units in the 2016–2022 panel. This reduces the risk that the comparison group is contaminated by lingering fiscal adjustment from earlier emergencies and improves the credibility of the counterfactual.

[6] This choice is consistent with the annual frequency of the budget data and with the institutional focus of the paper, which is on the activation of the post-disaster fiscal response rather than on the physical occurrence of the hydrogeological event itself.

Table 2 summarises the cumulative number of municipalities treated for the first time in each year from 2017 to 2022, together with their shares of national population, public expenditure, and revenue. In the baseline design, we focus on first-time treatment episodes and exclude municipalities that experience multiple declarations during the study period. This restriction is introduced to preserve a clear treatment structure. In a staggered-adoption framework with absorbing treatment, repeated declarations generate overlapping event times and make the interpretation of dynamic coefficients substantially less transparent.[7] The resulting baseline estimand should therefore be interpreted as the fiscal effect of first exposure among municipalities experiencing an initial hydrogeological State of Emergency.

We analyse treatment effects on a broad set of budgetary outcomes using data from OpenBDAP, the official accounting database maintained by the Italian Treasury (*Ragioneria Generale dello Stato*). On the expenditure side, our main outcome is total expenditure, which captures all authorised budgetary outlays in a given fiscal year. We further distinguish between current expenditure, which includes wages, utilities, materials, and other costs associated with the routine provision of public services, and capital expenditure, which captures investment in infrastructure and other durable assets. This distinction is particularly relevant in the context of climate-related disasters, since post-treatment adjustment may operate through different budgetary margins. Current expenditure captures pressures on short-run service provision, while capital expenditure captures reconstruction and longer-run investment needs. We also examine expenditure composition by functional “missions” (*Missioni*), which group spending by policy area. Analysing these missions allows us to assess whether exposure to the hydrogeological emergency-fiscal framework induces targeted reallocations across municipal functions rather than only changes in aggregate expenditure.

On the revenue side, we consider total revenues and their decomposition by institutional “titles” (*Titoli*). Titles classify revenues by source, including local taxes, intergovernmental transfers, and asset-related items, and provide a structured view of municipal income composition. Since this classification is standardised across Italian municipalities, it allows for consistent comparisons across jurisdictions and over time. Full details are reported in Table A.3.

Table 2 Cumulative exposure to first-time hydrogeological States of emergency: treated municipalities, population and budget shares (2017–2022)

	Treated municipalities	Population share	Expenditure share	Revenue share
2017	263	0.064	0.053	0.053
2018	470	0.123	0.107	0.108
2019	1,077	0.308	0.342	0.346
2020	1,296	0.363	0.435	0.443
2021	1,444	0.401	0.457	0.463
2022	1,476	0.408	0.439	0.440

Note. The table reports cumulative counts of municipalities first treated between 2017 and 2022. Municipalities exposed to a hydrogeological emergency declaration before 2017 or experiencing multiple declarations during the study period are excluded. Shares are calculated relative to the full universe of municipalities observed in each year

[7] Appendix C.5 returns to repeated exposure in a complementary analysis of first versus subsequent declarations.

In line with the theoretical framework, we next examine whether treatment effects vary systematically with municipalities' pre-existing fiscal resilience. We measure fiscal resilience using predetermined 2016 budget values. The proxy combines two indicators.

- *Debt-service burden*. Debt-service burden is measured as the ratio of interest payments to current revenues. Municipalities above the 2016 sample median are classified as having a high debt-service burden.
- *Tax autonomy*. Tax autonomy is measured as the ratio of tax revenues to current revenues. Municipalities above the 2016 sample median are classified as having strong tax autonomy.

These binary indicators operationalise the four fiscal resilience profiles introduced in Sect. 2.2: *resilient* (low debt service, high autonomy), *vulnerable* (high debt service, low autonomy), and two intermediate categories. Table 3 reports the distribution of treated municipalities across these profiles.

Only a small share of treated municipalities, about 15%, are classified as fiscally resilient, whereas roughly one-third fall into the most vulnerable category. This distribution already suggests that exposure to hydrogeological emergencies is not confined to municipalities with strong fiscal fundamentals and reinforces the importance of examining how initial fiscal capacity conditions post-treatment adjustment. Figure 3 illustrates the geographical distribution of fiscal resilience across Italian municipalities.

Panel (a) presents debt-service burden, Panel (b) shows tax autonomy, and Panel (c) combines both dimensions into the four-category fiscal resilience typology. The maps reveal substantial territorial heterogeneity in fiscal conditions. Municipalities with high debt-service burden or limited tax autonomy are not concentrated in a single part of the country, but are dispersed across the North, Centre, and South. At the same time, the fiscal resilience proxy in Panel (c) highlights clearer clusters of vulnerability in traditionally disadvantaged southern areas, as well as in structurally weaker inland and mountain zones of the Centre-North. These patterns underscore the importance of accounting for both fiscal and territorial disparities when assessing local governments' capacity to respond to hydrogeological emergencies.

This fiscal resilience measure defines the ex-ante dimension along which we analyse heterogeneous treatment effects. Consistent with the theoretical framework, we expect more resilient municipalities to sustain or expand reconstruction expenditure with greater budgetary flexibility, while vulnerable municipalities are expected to rely more heavily on external transfers and adjust more slowly. Identifying these differential responses is important for two reasons. It helps explain the aggregate fiscal effects of official hydrogeological emer-

Table 3 Distribution of treated municipalities by 2016 fiscal resilience profile

Debt-service burden	Tax autonomy		
	Weak	Strong	Total
Low	230	220	450
High	455	571	1,026
Total	685	791	1,476

Note. The table reports the number of municipalities in the baseline estimation sample that were first treated between 2017 and 2022, classified by fiscal resilience profile using 2016 values of debt-service burden and tax autonomy

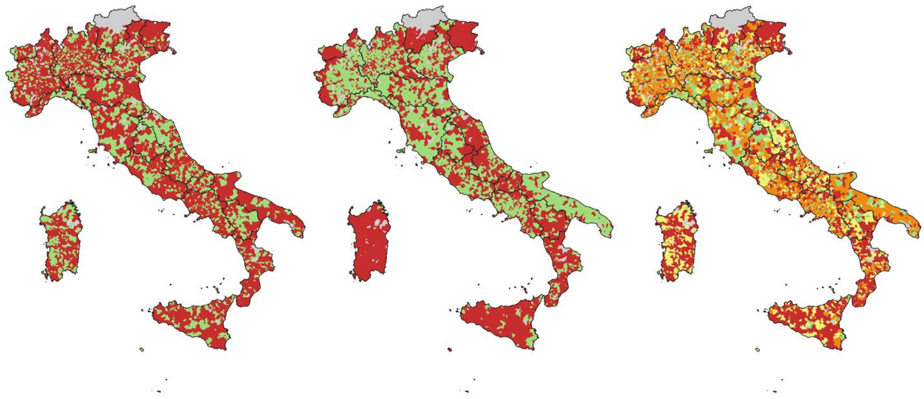

Fig. 3 Spatial distribution of municipal fiscal indicators. Note. This figure displays the spatial distribution of fiscal characteristics across Italian municipalities using 2016 pre-treatment values. Panel (**a**) maps debt-service burden, measured as the ratio of interest payments to current revenues, distinguishing above- and below-median values. Panel (**b**) shows tax autonomy, defined as the share of tax revenues in current revenues. Panel (**c**) combines these two indicators into a four-category typology of fiscal resilience: resilient municipalities combine low debt-service burden and strong tax autonomy; vulnerable municipalities combine high debt-service burden and weak tax autonomy; the remaining two groups are intermediate profiles

gency exposure, and it clarifies whether post-treatment funding mechanisms mitigate or reinforce pre-existing territorial and fiscal disparities.

To account for systematic differences in municipal characteristics that may shape both official emergency exposure and fiscal adjustment capacity, we complement treatment and budget data with a rich set of covariates. These capture demographic, socio-economic, geographic, and institutional characteristics, including demographic and territorial indicators, household income, hydrogeological vulnerability, disaster risk and resilience indices, and local institutional quality. Institutional quality is measured by the Municipal Administrative Quality Index (MAQI) developed by Cerqua et al. (2025), a composite indicator of local administrative performance and institutional effectiveness. Including MAQI is important because the effective use of fiscal space may also depend on local administrative capacity. Table 4 summarises the key variables used in the analysis and their respective data sources.

4.2 Descriptive Statistics

Before turning to the empirical model, we describe the main fiscal outcomes and the baseline characteristics of treated and never-treated municipalities. This provides a first view of municipal budget size, budget composition, and pre-treatment comparability.

Table 5 reports summary statistics for municipal revenues and expenditures over the period 2016–2022. The figures show substantial variation in both budget size and budget composition across municipalities, reflecting differences in population, economic structure, and local fiscal choices.

On the revenue side, the largest and most stable component is tax revenue. This includes locally raised taxes such as the municipal property tax, waste collection and disposal fees, and local income taxes. Tax revenue averages € 3.62 million per municipality per year and

Table 4 Overview of key variables and data sources

Variable	Years	Source
Municipal budget data	2016–2022	Ragioneria Generale dello Stato
Demographics	2016–2022	ISTAT
Geography	-	ISTAT
Household income	2016–2022	MEF
Disaster resilience/vulnerability	-	Marin et al. (2021)
Hydrogeological emergency exposure	2013–2022	Gatto et al. (2023)
Institutional quality	2001–2022	Cerqua et al. (2025)

Note. This table lists the principal variables used in the empirical analysis, along with their temporal coverage and data sources. Variables cover financial, demographic, geographic, institutional, and hydrogeological emergency-related dimensions at the municipal level

accounts for nearly 38% of total revenue, reflecting the autonomous fiscal capacity of local governments. The large standard deviation, equal to € 20.1 million, points to substantial differences in population size, economic structure, and local tax bases. Capital revenues are the second-largest component, accounting for 18.1% of total revenue. They include asset sales, investment-related transfers, and other non-recurrent flows connected to capital formation. Current transfers account for 13.9% and come from regional, national, or EU sources, reflecting the role of upper levels of government in redistribution, compensation, and targeted support. Revenues on behalf of third parties account for 13.4% of total revenue and are highly volatile, with a standard deviation of € 31.6 million. Although they do not represent discretionary resources, they capture the administrative scale of municipalities, especially larger ones. Extra-tax revenues, including fines, service fees, and rental income, account for 11.7% and provide an additional margin of fiscal flexibility. Borrowing and sales of financial assets contribute only marginally, each accounting for less than 1% of total revenue, while cash advances are irregular and largely non-discretionary. Given their limited comparability or irregular use, these items are excluded from the empirical analysis.

On the expenditure side, the largest category is institutional and general public services, which account for 26% of total spending and capture the core administrative and governance functions common to all municipalities. Expenditures on behalf of third parties follow at 14.2%, mirroring Title 9 on the revenue side and reflecting the role of municipalities as intermediaries in delegated or pass-through operations. The sustainable-development mission accounts for 14% of total expenditure and includes spending on environmental protection, urban planning, and territorial conservation, areas that are especially relevant in disaster-prone municipalities. Transport and mobility account for 9.2% of spending, while social and family policies account for 8.0%. Education represents 7.4% of total expenditure, largely reflecting municipal responsibilities for school buildings and related services. Smaller but consistently reported missions include public order, cultural heritage, youth, sport and leisure, and spatial planning. Other missions are largely inactive in municipal budgets, with several categories displaying more than 80% zero observations. To reduce the influence of sparsely reported categories, the empirical analysis focuses on missions with non-missing data in at least 60% of municipality-year observations.

Beyond aggregate descriptives, we also examine the comparability of treated and never-treated municipalities before treatment. Table 6 reports 2016 baseline values for the two

Table 5 Descriptive statistics of municipal revenue and expenditure categories (2016–2022)

Item	Mean	Std. Dev.	Min.	Max.	Avg. Share	% Zero
Revenue categories (Titles)						
T1. Tax revenue	3.62	20.10	0.00	934.00	37.75%	2%
T2. Current transfers	1.26	7.68	0.00	644.00	13.89%	2%
T3. Extra-tax revenue	1.12	6.70	0.00	349.00	11.73%	2%
T4. Capital revenue	1.35	6.61	0.00	662.00	18.10%	2%
T5. Sale of financial assets	0.05	0.79	0.00	57.00	0.26%	93%
T6. Borrowing	0.17	4.25	0.00	523.00	0.98%	80%
T7. Cash advances	0.78	14.40	0.00	1 310.00	3.84%	81%
T9. Revenue on behalf of third parties	2.03	31.60	0.00	2 820.00	13.44%	2%
Total revenue	10.40	77.50	0.00	5 470.00	100%	2%
Expenditure categories (Missions)						
M1. Institutional and general services	1.77	7.97	0.00	483.00	25.98%	2%
M2. Justice	0.00	0.15	0.00	26.00	0.02%	92%
M3. Public order and security	0.27	1.97	0.00	102.00	2.42%	10%
M4. Education	0.59	2.61	0.00	156.00	7.36%	3%
M5. Protection of cultural heritage	0.17	0.94	0.00	59.10	1.78%	12%
M6. Youth, sport and leisure	0.15	0.69	0.00	33.80	1.89%	11%
M7. Tourism	0.06	0.27	0.00	18.70	0.75%	47%
M8. Spatial planning and housing	0.27	1.07	0.00	47.30	3.74%	15%
M9. Sustainable development	1.33	6.14	0.00	322.00	14.05%	2%
M10. Transport and mobility	0.69	5.77	0.00	367.00	9.22%	7%
M11. Civil protection	0.07	0.61	0.00	30.10	0.94%	33%
M12. Social policies and family	0.86	4.44	0.00	239.00	8.01%	3%
M13. Health protection	0.01	0.04	0.00	1.57	0.09%	77%
M14. Economic development	0.07	0.60	0.00	38.30	0.66%	39%
M15. Employment and training	0.01	0.10	0.00	10.90	0.08%	82%
M16. Agriculture and fisheries	0.01	0.07	0.00	6.40	0.13%	79%
M17. Energy	0.05	0.44	0.00	25.90	0.74%	76%
M18. Inter-municipal relations	0.01	0.09	0.00	2.80	0.20%	94%
M19. International relations	0.00	0.04	0.00	3.99	0.00%	99%
M20. Funds and provisions	0.00	0.02	0.00	1.88	0.02%	98%
M50. Public debt	0.36	4.35	0.00	337.00	3.66%	6%
M60. Financial advances	0.79	14.40	0.00	1 310.00	4.05%	81%
M99. Third-party services	2.03	31.60	0.00	2 820.00	14.20%	2%
Total expenditure	9.56	69.90	0.00	4 520.00	100%	2%

Note. All monetary values are expressed in millions of euros. "Avg. Share" indicatesthe average percentage contribution of each item to total revenue orexpenditure. "% Zero" refers to the proportion of municipal-year observationswith zero reported values for a given category. Budget items with more than 40% zero entries are excluded from the empirical estimates because of limitedcoverage and lack of comparability

groups, providing a preliminary assessment of differences that may be related to both emergency exposure and subsequent fiscal outcomes.

Several systematic differences emerge. Treated municipalities are larger on average, with about 7,160 inhabitants compared with 5,630 among never-treated municipalities, but they are less densely populated, with 272 inhabitants per km^2 against 418. This pattern suggests that treated municipalities are not simply the most urbanised areas, but also include larger jurisdictions with more dispersed settlement structures. They are also more frequently

Table 6 Pre-treatment characteristics of treated and never-treated municipalities

Variable	Never treated (a)	Treated (b)	Diff. (a–b)	S.E.	*p*-value
Population	5,633	7,161	−1,528.26	743.24	0.040
Mean taxable income (€/capita)	17,402	17,666	−264.07	128.21	0.040
Municipal administrative quality (index)	103.77	103.55	0.22	0.12	0.057
Population density (inh./km^2)	418.28	272.03	146.25	26.16	< 0.001
Near the coast (0/1)	0.053	0.094	−0.041	0.008	< 0.001
Coastal municipality (0/1)	0.125	0.164	−0.039	0.011	0.001
Urban classification (1–3)	2.586	2.603	−0.018	0.018	0.337
Resilience index	0.535	0.548	−0.012	0.005	0.007
Vulnerability index	0.484	0.464	0.020	0.004	< 0.001
Flood hazard index	0.043	0.056	−0.013	0.003	< 0.001
Landslide hazard index	0.101	0.076	0.025	0.005	< 0.001
Debt-service burden	0.018	0.021	−0.003	0.001	0.001
Tax autonomy	0.606	0.630	−0.024	0.006	< 0.001

Note. The table reports 2016 mean values for a range of socio-economic, geographical, environmental, and fiscal variables, comparing municipalities that experienced at least one hydrogeological State of Emergency between 2017 and 2022 (treated) with municipalities that were never included during the same period (never treated). The final three columns report the difference in means (a-b), the corresponding standard error, and the *p*-value from a two-sample t-test. Sample size varies across variables because of missing values. These covariates are included in the baseline covariate-adjusted specification

located along the coast, with significant differences for both coastal proximity indicators. In socio-economic terms, treated municipalities display a slightly higher average taxable income, equal to € 17,666 compared with € 17,402 among never-treated municipalities. They also show slightly lower administrative quality, although this difference is not statistically significant.

Environmental indicators confirm that treated municipalities face greater exposure to flood hazards and lower exposure to landslide risks, in line with the geography of the events included in the treatment definition. They also display slightly higher resilience scores and lower vulnerability levels, suggesting that treatment is not concentrated among municipalities with weaker baseline conditions. Fiscal differences are smaller in magnitude but statistically meaningful. Treated municipalities have a higher debt-service burden, equal to 2.1% of current revenues compared with 1.8% among never-treated municipalities, and rely more heavily on tax revenues, with tax autonomy equal to 63.0% compared with 60.6%. These two dimensions are central to the analysis of heterogeneous responses by ex-ante fiscal resilience.

Overall, these pre-treatment differences are systematic enough to motivate covariate adjustment in the empirical strategy. The covariate set is therefore included across all specifications. Full descriptive statistics and pre-treatment budget averages are reported in Appendix Tables A.4 and A.5.

4.3 Empirical Model

While the timing of hydrogeological events is largely exogenous, their spatial incidence is not random. A growing literature shows that exposure to disaster risk is systematically related to structural characteristics such as geography, urban development, past hazard exposure, and local institutional conditions (Kahn 2005; Deryugina 2017; Kocornik-Mina et al.

2020). Municipalities located near rivers or coasts, characterised by mountainous terrain, or burdened by ageing infrastructure are more exposed to hydrogeological risk. Because empirical treatment is anchored to the official State of Emergency declaration, as discussed in Sect. 4.1, access to emergency resources may further reflect institutional and political factors, including administrative quality (Reeves 2011).

These features create two main identification challenges. First, treatment is staggered across municipalities and over time, as municipalities enter the treatment group in different years depending on the timing of their first emergency declaration. Second, municipalities differ in size, fiscal capacity, geography, and governance quality, characteristics that may jointly affect exposure to the emergency-fiscal framework and subsequent fiscal outcomes. In this setting, standard two-way fixed effects estimators can produce biased estimates under staggered adoption and treatment-effect heterogeneity (De Chaisemartin and D'Haultfœuille 2020; Goodman-Bacon 2021; Sun and Abraham 2021). We therefore rely on the estimator proposed by Callaway and Sant'anna (2021), which is designed for settings with staggered treatment timing and heterogeneous treatment effects.

The estimator identifies group-time average treatment effects, $ATT_{g,t}$, for each cohort of municipalities first treated in year g and each year $t \geq g$. Using the last pre-treatment year, $g-1$, as the reference period, these effects can be written as

$$ATT_{g,t} = [Y(g)_t - Y(g)_{g-1}] - [Y(U_t)_t - Y(U_t)_{g-1}], \tag{4}$$

where $Y(g)_t$ and $Y(g)_{g-1}$ are the average outcomes for municipalities first treated in year g at time t and in the last pre-treatment year, respectively. Similarly, $Y(U_t)_t$ and $Y(U_t)_{g-1}$ are the corresponding averages for municipalities untreated at time t, including both never-treated municipalities and municipalities not yet treated by that date. The first bracket captures the change in outcome for the treated cohort between $g-1$ and t, while the second captures the change over the same window for the comparison group, providing the counterfactual trend under the parallel-trends assumption. The corresponding dynamic effects can be summarised in event time as

$$Y_{it} = \alpha_i + \lambda_t + \sum_{g \in \mathcal{G}} \sum_{l \neq -1} \beta_{g,l} \mathbf{1}\{G_i = g\} \mathbf{1}\{t - g = l\} + \varepsilon_{it}, \tag{5}$$

where Y_{it} is the fiscal outcome of municipality i in year t, α_i and λ_t denote municipality and year fixed effects, G_i is the year of first treatment, and $l = t - g$ is event time. We use this notation to display the underlying estimand, namely cohort-specific dynamic treatment effects, with $\beta_{g,l} = ATT_{g,g+l}$ measured relative to the last pre-treatment year. These parameters are not estimated by direct two-way fixed-effects regression, but within the Callaway-Sant'Anna framework using a doubly robust procedure that combines inverse probability weighting with outcome-regression adjustment.[8]

The control group consists of municipalities untreated at the relevant comparison date, including both never-treated and not-yet-treated municipalities. This choice is natural in a staggered-adoption setting because these municipalities operate in the same institutional and macro-fiscal environment as the treated units. The not-yet-treated units are especially useful because many are exposed later in the sample period, making them a relevant com-

[8] The Stata implementation relies on the csdid (v. 1.82) and drdid (v. 1.91) packages.

parison group for municipalities treated earlier. Comparability is further strengthened through covariate adjustment in both the propensity-score and outcome models. The set of controls includes the log of population and taxable income, population density and surface area, the MAQI index (Cerqua et al. 2025), indicators for urban, coastal, and near-coastal status, the resilience and vulnerability indices from Marin et al. (2021), and binary indicators for flood and landslide hazard exposure. The hazard indicators improve balance in underlying disaster risk regardless of whether a comparison municipality is never treated or not yet treated.[9] Heterogeneity by debt-service burden, tax autonomy, and fiscal resilience class is implemented through separate subgroup estimations of this same baseline specification, which keeps the interpretation of subgroup-specific dynamic responses transparent. As robustness checks, we also estimate alternative specifications based on outcome regression, inverse probability weighting, and a control group restricted to never-treated municipalities.

Identification rests on a conditional parallel trends assumption:

$$\mathbb{E}[Y_t(0) - Y_{g-1}(0) \mid X, G = g] = \mathbb{E}[Y_t(0) - Y_{g-1}(0) \mid X, i \in U_t], \tag{6}$$

for all $t \geq g$, where G identifies the treatment cohort, X denotes observed covariates, and U_t is the set of municipalities untreated at time t. Conditional on X, this control group should provide valid counterfactual outcome paths for treated municipalities in the absence of treatment. Although this assumption is not directly testable, its plausibility is supported by covariate adjustment in the doubly robust estimator, the balancing diagnostics reported in Appendix B, and pre-treatment dynamics that, for the main fiscal outcomes, namely total revenue, total expenditure, and capital expenditure, are consistent with the parallel-trends assumption at conventional significance levels (Table B.2). Placebo exercises in Appendix C produce no comparable effects under artificial treatment timing.

To summarise treatment effects across cohorts and post-treatment periods, we aggregate the estimated group-time effects into a single parameter:

$$AGTT = \sum_{g} \sum_{t \geq g} \omega_{g,t} \, ATT_{g,t}, \tag{7}$$

with $\omega_{g,t} \geq 0$ and $\sum_g \sum_{t \geq g} \omega_{g,t} = 1$. Following Callaway and Sant'anna (2021), these weights reflect the relative contribution of each cohort to the population of post-treatment observations. Cohorts with more treated units, or observed over longer post-treatment horizons, may thus receive greater weight. To keep the interpretation of the results consistent, we use $ATT_{g,t}$ to denote group-time average treatment effects on the treated, *event-time effects* to denote dynamic effects plotted against relative time, and *aggregated post-treatment effect* ($AGTT$) to denote the weighted post-treatment summary of the $ATT_{g,t}$ estimates.

Two final qualifications concern empirical support and inference. First, because the harmonised-budget panel covers only 2016–2022, longer-horizon coefficients are mechanically identified from a smaller subset of earlier-treated cohorts, and the farthest post-treatment horizons rely more heavily on observations from 2020 to 2022, an exceptional period for

[9] Table A.1 reports balancing evidence based on propensity score matching between treated and never-treated municipalities. The results show good balance across the main covariates after matching. This evidence supports the availability of comparable untreated municipalities after conditioning on observed characteristics and motivates the use of covariate adjustment across the empirical specifications.

municipal finances due to the Covid-19 pandemic and the Recovery Fund. The farthest event-time coefficients are therefore interpreted as informative about the persistence of fiscal responses, with the caveat that they reflect a specific subset of cohorts observed in an exceptional period. Appendix Table B.3 reports the empirical support available at each event-time horizon, and Appendix Table B.4 complements the event-study profile with averages over short-, medium-, and longer-run windows. Second, statistical inference follows the default procedure delivered by the Callaway-Sant'Anna implementation for the estimator and aggregation used in each specification. This is standard in the staggered-adoption literature, but it does not explicitly model spatial covariance across neighbouring municipalities, commuting zones, or broader region-level common shocks. Because hydrogeological events and municipal fiscal outcomes may be spatially correlated within both institutional and functional economic areas, Appendix Table C.9 reports additional inference checks for the baseline aggregated post-treatment effects. These checks hold the estimand, sample, treatment timing, controls, and estimator fixed, and only recompute inference at two spatial levels, namely NUTS2 regions, capturing broad regional institutional and fiscal dependence, and commuting zones, capturing local economic linkages in tax bases and demand for municipal works.

5 Results

We first estimate the overall effect of hydrogeological disasters on municipal revenues and expenditures (Sect. 5.1). We then examine whether these shocks reshape budget composition across major revenue titles and expenditure missions (Sect. 5.2). We next study heterogeneity in fiscal responses across municipalities with different levels of pre-disaster fiscal resilience (Sect. 5.3). Finally, we assess the robustness of the results through sensitivity checks, including alternative estimators, placebo tests, event- and region-based exclusion exercises, spatial inference checks, and complementary analyses of first and subsequent declarations (Sect. 5.4). Unless otherwise noted, the estimates refer to the baseline specification.

5.1 Revenue and Expenditure

Figure 4 reports the dynamic effects on municipal revenues and expenditures. All variables are expressed in natural logarithms, so the estimated coefficients can be interpreted approximately as percentage changes. These outcomes map directly into the fiscal channels discussed in Sect. 2. Total revenues and total expenditures capture the overall budgetary adjustment, while the distinction between current and capital expenditure indicates whether the response operates through routine spending or through investment-oriented intervention.

Municipal revenues increase after first exposure to a hydrogeological State of Emergency. The aggregated post-treatment effect is 0.039 ($p < 0.01$), implying an increase of about 4.0% relative to the counterfactual path. The dynamic profile points to a gradual build-up over the observed post-treatment horizons, from about 1.5% in the treatment year ($t + 0$) to 8.5% by $t + 5$. Intermediate effects are 3.4% in $t + 1$, 4.0% in $t + 2$, and 5.7% in both $t + 3$ and $t + 4$. This pattern suggests the progressive arrival of transfers and reconstruction-related inflows, rather than an immediate one-off fiscal adjustment (Deryugina 2017; Jerch et al. 2023; Miao et al. 2023). The strongest support comes from the average post-treatment

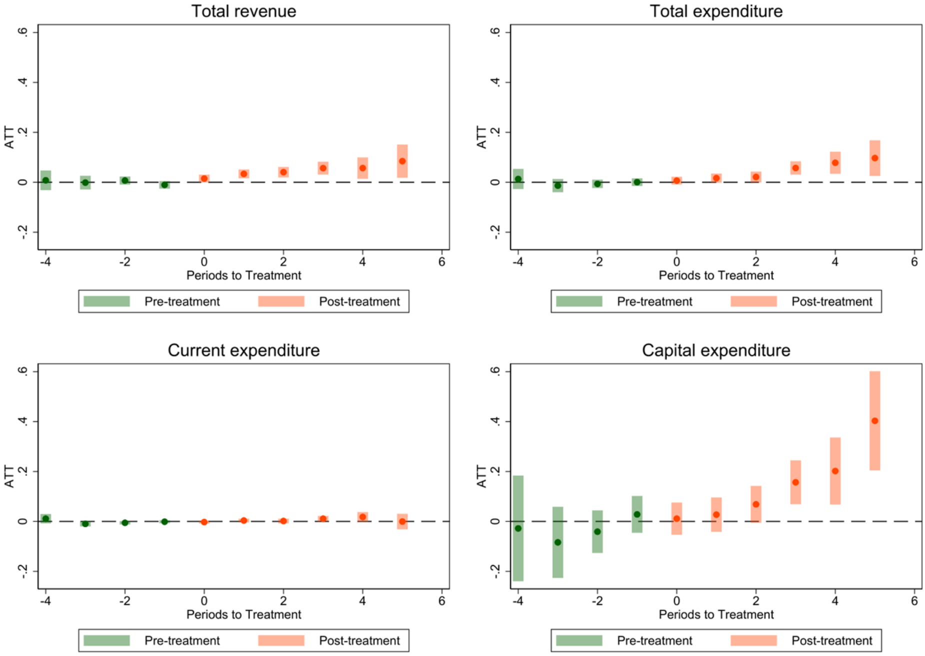

Fig. 4 Dynamic effects on revenues and expenditures. Note. The dependent variables are expressed in natural logarithms. The plotted coefficients are event-time averages of group-time treatment effects estimated within the Callaway-Sant'Anna framework, using the improved doubly robust estimator. The control group consists of municipalities untreated at the relevant comparison date, including both never-treated and not-yet-treated units. Ninety-five percent confidence intervals are displayed

effect and the short- and medium-run post-treatment periods, while the broader aggregation windows reported in Appendix Table B.4 point in the same direction.

Municipal expenditures also rise after exposure, although more gradually than revenues. The aggregated post-treatment effect is 0.030 ($p < 0.01$), corresponding to an increase of about 3.0% relative to the counterfactual path. The event-study estimates show little immediate adjustment in the treatment year, followed by a clearer increase over the subsequent post-treatment years. This pattern suggests that reconstruction-related expenditure takes time to materialise in realised budgets (Masiero and Santarossa 2020; Chrysanthakopoulos and Tagkalakis 2026).

The distinction between current and capital expenditure shows where this adjustment takes place. Current expenditure remains broadly unchanged. The aggregated post-treatment effect is only 0.004 and is not statistically significant, while the dynamic coefficients remain small throughout the post-treatment window. The evidence therefore does not point to a generalised expansion of day-to-day spending. This is consistent with the interpretation that personnel costs and ordinary administrative operations are not the main margins through which municipalities accommodate disaster shocks (Cavallo et al. 2013).

Capital expenditure, by contrast, shows the largest average response. The aggregated post-treatment effect is 0.085 ($p < 0.01$), corresponding to an increase of about 8.5% relative to the counterfactual path. Coefficients are small and not statistically significant in the treatment year and in the first post-treatment year. They become larger by $t + 2$ and are highest at the later observed horizons, reaching about 15.7% in $t+3$, 20.2% in $t+4$, and 40.3% in $t+5$.

This delayed adjustment is consistent with the institutional timing of reconstruction expenditure. Unlike routine current spending, capital outlays require several administrative and financial steps, including formal emergency recognition, fund allocation, project design, procurement, and implementation. In the Italian setting, these procedures can be especially time-consuming, making a gradual investment response more plausible than an immediate jump in realised capital spending (Baltrunaite et al. 2024; Puri et al. 2024). Although the most distant coefficients should be interpreted cautiously because they rely on thinner empirical support, the evidence indicates that the increase in total expenditure is most closely associated with the capital component.

The pre-treatment diagnostics are generally reassuring. Joint tests on the pre-treatment coefficients do not reject the null of no differential pre-treatment dynamics for the main fiscal outcomes, especially total revenue, total expenditure, and capital expenditure.

Overall, the estimates indicate a clear fiscal response in the baseline sample. Revenues rise after treatment, total expenditure also increases, and the composition of spending shows that this response is driven much more by capital than by current expenditure. These results point to a post-disaster adjustment centred on reconstruction-related and investment-oriented spending rather than on a generalised expansion of routine outlays.

5.2 Breakdown by Revenue Titles and Expenditure Missions

We now examine which revenue titles and expenditure missions account for the budgetary adjustment documented above. The figures in this section summarise the average effects, while the full dynamic estimates are reported in Appendix Tables A.6–A.11. Unlike Sect. 5.1, where outcomes were measured in logarithmic levels, the dependent variables here are expressed as shares of total revenue or total expenditure. Since the outcomes are budget shares, even statistically significant coefficients may correspond to economically modest reallocations.

Figure 5 shows that the expenditure response is concentrated in a limited set of missions, while most other missions display small and statistically weak effects. This pattern is consistent with targeted post-treatment adjustment rather than a broad reallocation of municipal spending across functions.

Three positive reallocations stand out. The share of expenditure allocated to *sustainable development* rises by about 0.46 percentage points. This is the clearest investment-oriented change in expenditure composition and accords with the role of reconstruction, environmental protection, and resilience-related interventions after first exposure to a hydrogeological State of Emergency. *Civil protection* increases by about 0.24 percentage points. The effect is modest in absolute terms, but economically meaningful given the very small pre-treatment weight of this mission in municipal budgets. *Youth, sport and leisure* records a smaller increase of about 0.18 percentage points, suggesting localised adjustments in community facilities and related services.

Two categories lose relative importance. The most visible contraction concerns *general services*, whose average budget share declines by about 0.57 percentage points. This points to a relative decline in broad administrative and general-purpose spending within the municipal budget. *Public debt* also declines, by about 0.18 percentage points, indicating that

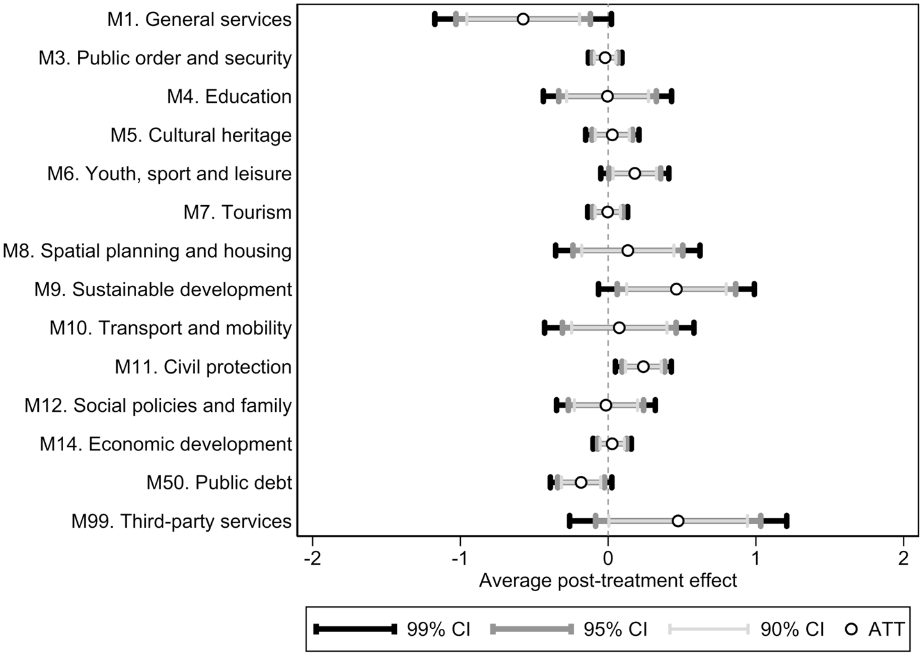

Fig. 5 Average post-treatment effects on expenditure composition. Note. The figure reports average post-treatment effects for the main expenditure missions. Dependent variables are expressed as shares of total expenditure, and effects are reported in percentage points. Estimates are obtained within the Callaway-Sant'Anna framework using the improved doubly robust estimator. The control group consists of municipalities untreated at the relevant comparison date, including both never-treated and not-yet-treated units. Circles denote average ATT estimates; horizontal bars report 99, 95, and 90% confidence intervals. Full dynamic estimates, together with the alternative specification using only never-treated municipalities as controls, are reported in Appendix Tables A.8–A.11

debt-related expenditure becomes less prominent within total expenditure after treatment. A further positive coefficient, about 0.47 percentage points, emerges for *third-party services*, but it is only marginally significant and should be interpreted cautiously. For most other missions, including education, cultural heritage, tourism, transport and mobility, and social policies, the estimated effects are small and statistically indistinguishable from zero.

Figure 6 reports the corresponding effects for revenue titles.

A similar pattern of concentration characterises the revenue side. The clearest result is the expansion of *capital revenue*, whose average share rises by about 1.86 percentage points. This is the largest and statistically strongest effect among the revenue titles and points to reconstruction funding and earmarked investment resources from higher levels of government as the main driver of revenue adjustment. The counterpart is a decline in the relative importance of current sources. *Tax revenue* falls by about 0.99 percentage points on average, while *current transfers* decline by about 0.39 percentage points. Other revenue titles display smaller and statistically insignificant changes.

Recalling that total revenues rise after treatment (Sect. 5.1), what changes here is the *internal structure* of municipal financing, with capital resources gaining weight relative to standard current sources.

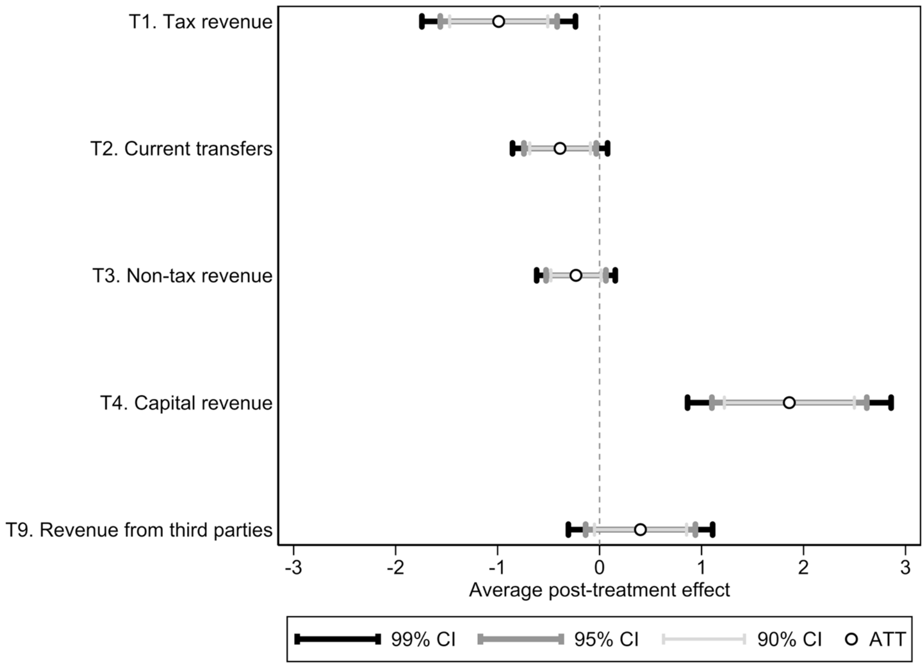

Fig. 6 Average post-treatment effects on revenue composition. Note. The figure reports average post-treatment effects for the main revenue titles. Dependent variables are expressed as shares of total revenue, and effects are reported in percentage points. Estimates are obtained within the Callaway-Sant'Anna framework using the improved doubly robust estimator. The control group consists of municipalities untreated at the relevant comparison date, including both never-treated and not-yet-treated units. Circles denote average ATT estimates; horizontal bars report 99, 95, and 90% confidence intervals. Full dynamic estimates, together with the alternative specification using only never-treated municipalities as controls, are reported in Appendix Tables A.6–A.7

Taken together, the evidence from titles and missions points to a coherent pattern of selective fiscal adjustment. On the revenue side, the compositional shift is concentrated in capital revenue. On the expenditure side, it is concentrated in civil protection, sustainable development, and a few related functions, alongside a relative decline in the budget shares of general services.

5.3 The Role of Fiscal Resilience

Having documented clear average fiscal effects and changes in budget composition, we now examine heterogeneity in budget adjustment. Average effects may mask substantial differences across municipalities with different pre-treatment fiscal conditions. The central question is whether municipalities are equally able to translate external support into sustained expenditure increases. To address this question, we re-estimate the baseline specification separately within pre-treatment subgroups.

As discussed above, we proxy fiscal resilience using two predetermined indicators measured in 2016, before the treatment window begins: (i) debt-service burden, defined as the ratio of interest payments to current revenues, and (ii) tax autonomy, defined as the share of tax revenues in current revenues.[10] We first consider each dimension separately (Fig. 7; Tables A.12–A.13), and then combine them into the four-group typology used to capture differences in ex-ante fiscal resilience (Fig. 7; Tables A.14–A.15).

Figure 7 shows that debt-service burden is an important margin of heterogeneity.

Municipalities with low debt-service burden display a stronger and more sustained expenditure response after first exposure. Their aggregated post-treatment effect is about 0.055 and is statistically significant. The event-study profile becomes clearly positive in the first post-treatment years and reaches roughly 0.125 by $t{+}4$. By contrast, municipalities with high debt-service burden exhibit a much weaker response. Their aggregated post-treatment effect is only about 0.017 and is not statistically distinguishable from zero. The event-study coefficients remain small for much of the post-treatment window and become clearly positive only at later horizons. This pattern is consistent with the idea that debt-service obligations restrict the effective fiscal space available to absorb and deploy post-disaster resources.

A related but different pattern emerges when municipalities are split by tax autonomy. Municipalities with weak tax autonomy show a positive aggregated post-treatment effect of

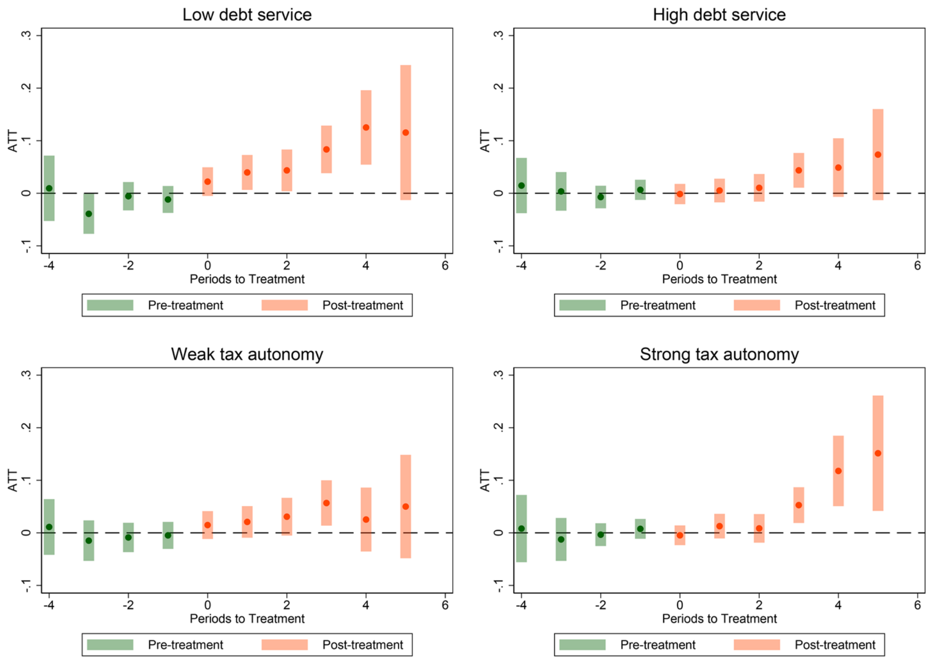

Fig. 7 Dynamic effects on total expenditure by debt-service burden and tax autonomy. Note. The dependent variable is expressed in natural logarithms. The plotted coefficients are event-time averages of group-time treatment effects estimated within the Callaway-Sant'Anna framework using the improved doubly robust estimator. The control group consists of municipalities untreated at the relevant comparison date, including both never-treated and not-yet-treated units. Panels are obtained by re-estimating the baseline specification separately by pre-treatment subgroup, defined using 2016 values of debt-service burden and tax autonomy. Ninety-five percent confidence intervals are displayed

[10] Both indicators are measured before any treatment occurs. Using the 2016 cross-section avoids mechanical feedback from post-treatment revenues and expenditures to the fiscal-resilience classification.

about 0.030, while those with strong tax autonomy show an effect of about 0.026; both are statistically significant at conventional levels. The dynamic profiles, however, differ. In the weak-autonomy group, the response is positive but uneven, with the clearest increases concentrated in the middle post-treatment years. In the strong-autonomy group, the response builds more gradually but becomes more persistent at longer horizons, reaching about 0.118 at $t{+}4$ and 0.151 at $t{+}5$. These results suggest that tax autonomy matters less for the initial activation of spending than for the capacity to sustain and consolidate the response over time.

These comparisons provide a first indication that the two resilience dimensions capture different margins of adjustment. Debt-service burden appears more closely associated with the strength of the expenditure response, whereas tax autonomy appears more closely associated with its persistence. Considered in isolation, however, these subgroup comparisons do not show how the two dimensions interact within the same municipality.

Figure 8 addresses this point by cross-classifying municipalities along both dimensions.

This joint typology shows that fiscal resilience is not a one-dimensional concept. Municipalities with *low debt-service burden and weak tax autonomy* display the strongest average response, with an aggregated post-treatment effect of about 0.079 and a clearly positive dynamic profile. This suggests that, when inherited debt-service obligations are limited, even municipalities with weaker own-revenue capacity can expand expenditure substan-

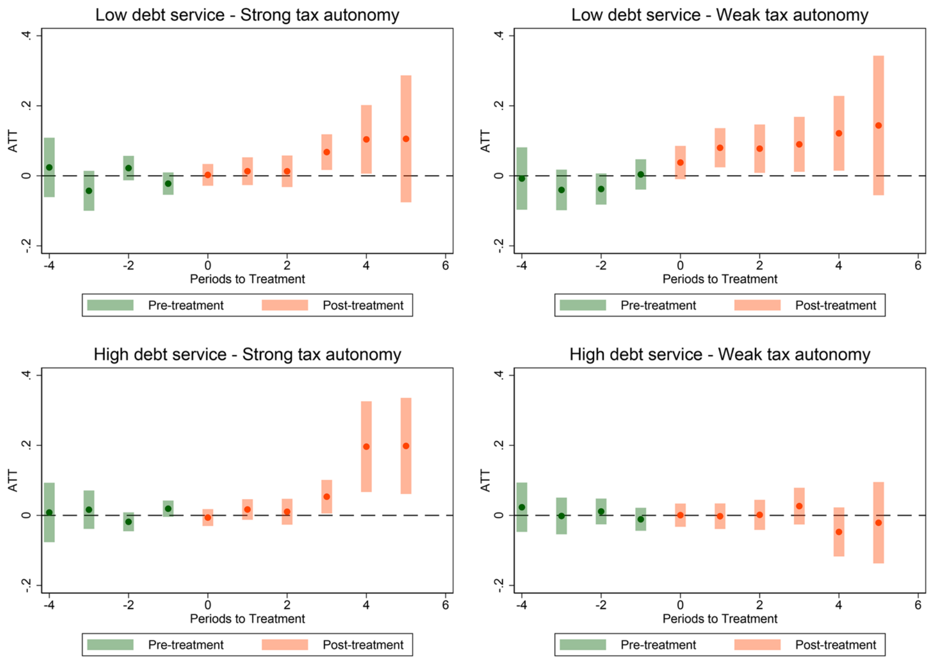

Fig. 8 Dynamic effects on total expenditure by fiscal-resilience typology. Note. The dependent variable is expressed in natural logarithms. The plotted coefficients are event-time averages of group-time treatment effects estimated within the Callaway-Sant'Anna framework using the improved doubly robust estimator. The control group consists of municipalities untreated at the relevant comparison date, including both never-treated and not-yet-treated units. The four panels correspond to the fiscal-resilience typology based on the 2016 cross-classification of debt-service burden and tax autonomy. Ninety-five percent confidence intervals are displayed

tially, consistent with a response supported by external transfers and reconstruction-related resources. Municipalities with *high debt-service burden and strong tax autonomy* also show a positive response, but the effect is smaller on average, at about 0.029, and becomes clearly visible only at later event times. This is consistent with the view that tax autonomy can partly offset debt-related rigidity, but only gradually.

Municipalities with *low debt-service burden and strong tax autonomy* also show a positive, although only marginally significant, aggregated post-treatment effect of about 0.030, with a dynamic profile that becomes more favourable in the medium run. By contrast, municipalities with *high debt-service burden and weak tax autonomy* show essentially no evidence of systematic expenditure expansion. The aggregated post-treatment effect is negative and far from statistically significant, while the event-study coefficients fluctuate around zero throughout the post-treatment window. This group appears to be the most fiscally constrained, as it combines limited budgetary room with weak autonomous revenue capacity.

The joint classification adds two insights. First, debt-service burden appears to dominate the immediate capacity to expand expenditure, with municipalities facing high debt-service obligations reacting more weakly and more slowly, even when they have relatively stronger own-revenue capacity. Second, tax autonomy seems less important for the initial expenditure jump than for the continuation of the adjustment. It may support a more persistent expenditure path where fiscal space is available, and it may partly compensate for debt-related rigidity over time. Fiscal resilience is therefore best understood as the joint effect of available fiscal space and discretionary budgetary capacity, rather than as either dimension in isolation.

5.4 Robustness and Sensitivity Analyses

To assess the credibility of the baseline estimates, Appendix C reports a broad set of robustness and sensitivity exercises. These checks cover alternative estimators, alternative comparison groups, leave-one-out procedures, formal spatial inference at the NUTS2-region and commuting zone levels, placebo interventions, a pre-pandemic sample restriction, and complementary evidence on repeated exposure. Together, they serve two purposes. First, they evaluate whether the main findings depend on specific modelling choices, on the particular composition of the estimation sample, or on how dependence within regions and local labour market areas is handled. Second, they provide indirect evidence on the plausibility of the identifying assumptions by asking whether similar patterns emerge under artificial timing, different comparison groups, shorter post-treatment windows, or broader exposure definitions.

A recurring theme throughout these exercises is the role of the control group. Because the baseline design uses municipalities untreated at the relevant comparison date, including both never-treated and not-yet-treated units, several robustness checks re-estimate the main specifications using only never-treated municipalities as an alternative comparison group. The placebo exercises assess whether the observed dynamics could reflect spurious timing patterns or residual trends unrelated to disaster exposure. The pre-pandemic restriction helps evaluate whether the farthest post-treatment coefficients are driven by overlap with the pandemic years. Finally, the repeated-exposure analysis reintroduces municipalities with multiple declarations and compares first versus subsequent declarations, clarifying how far the main conclusions extend beyond the selected first-treatment sample.

5.4.1 Alternative Estimators

Appendix C.1 re-estimates the main specifications using a set of widely used difference-in-differences estimators, including doubly robust procedures, outcome-regression-based estimation, and inverse-probability-weighted approaches. The purpose is to verify that the conclusions do not depend on one particular implementation.

The results are reassuring. Across estimators and control-group definitions, the average post-treatment effects remain similar for the main outcomes. For total expenditure, the estimated effects range approximately between 0.029 and 0.034 (Tables C.1–C.2); for total revenue, between 0.037 and 0.043 (Tables C.3–C.4); for current expenditure, the estimates remain close to zero throughout (Tables C.5–C.6); and for capital expenditure, the estimates remain positive and sizeable, ranging between about 0.053 and 0.092 (Tables C.7–C.8). The corresponding event-study profiles are also similar across specifications, especially for total revenue, total expenditure, and capital expenditure.

The pre-treatment diagnostics are likewise reassuring. Some individual pre-treatment coefficients are noisy, but the overall pattern does not suggest systematic anticipation effects or pervasive violations of the identifying assumptions. In particular, the most policy-relevant results, namely the positive responses in total revenue, total expenditure, and especially capital expenditure, remain stable across estimator choice. This reinforces the view that the core findings are not driven by one specific estimation strategy.

5.4.2 Leave-One-Out, Regional Exclusion, and Spatial Inference

Appendix C.2 assesses whether the baseline results are influenced by individual disaster cohorts, by the inclusion of specific regions, or by how dependence within regions and local labour markets is handled. We focus on the two main aggregate outcomes, total expenditure and total revenue, and implement two complementary exclusion exercises before turning to formal spatial inference.

First, we exclude one treated cohort at a time and re-estimate the average treatment effect. As shown in Figs. C.1–C.2, the resulting estimates remain close to the baseline values, with deviations that are quantitatively small and within the confidence interval of the full-sample specification. This indicates that the main results are not driven by a single disaster year or by one influential treatment cohort.

Second, we iteratively omit one region at a time, including the *regions with special status*, whose fiscal institutions differ from those of ordinary-statute regions. Figures C.3–C.4 show that the estimated treatment effects remain stable. The qualitative conclusions are unchanged, and the ATT estimates are statistically indistinguishable from the baseline in nearly all cases. This suggests that the main findings do not depend on the fiscal structure or disaster experience of any one region. The leave-one-region-out exercise shows that the point estimates are not driven by any single territorial unit, but it does not address spatial correlation in the error structure.

Tables C.9 and C.10 in Appendix report robustness checks related to inference issues. In the two tables, we cluster standar errors by, respectively, NUTS2 region and commuting zone (*SLL - Sistemi Locali del Lavoro)*. The former is meant to allow for correlated errors within the same NUTS2 region as regional governments in Italy are entitled of a number of functions related to disaster preparedness and management. The latter instead allows correlation of errors across municipalities within the same commuting zone, which could arise from both shocks affecting the tax base for local taxation and the local demand for construction works and, more generally, for public procurement in response to the event.

The revenue response is robust, remaining positive and statistically significant at both clustering levels and across all time windows. Expenditure inference is instead more sensitive to the level of spatial aggregation. Total expenditure is positive throughout and statistically significant under SLL inference, including over the 0–3 window, but the more conservative NUTS2-region intervals widen and cross zero. Current expenditure is small and statistically insignificant in all specifications. For capital expenditure, SLL inference confirms the baseline aggregated effect, whereas significance is lost under NUTS2-region clustering and in the shorter 0–2 and 0–3 windows.

Taken together, these checks support the main interpretation while clarifying where the evidence is strongest. SLL inference supports the expenditure response, whereas NUTS2-region inference provides a deliberately conservative benchmark for broader regional dependence.

5.4.3 Placebo Interventions

Appendix C.3 reports two placebo exercises designed to test whether the estimated post-treatment dynamics could be reproduced mechanically in the absence of genuine treatment.

The first placebo is a post-treatment-window exercise. It extends the event-time window beyond the actual treatment horizon and re-estimates the dynamic effects over this artificial period. As shown in Figs. C.5–C.6, the coefficients for both total expenditure and total revenue lose statistical significance and become statistically indistinguishable from zero once the actual treatment window has passed. This is what one would expect if the baseline dynamics are tied to the disaster-related fiscal response rather than to a generic tendency of treated municipalities to display permanently different trends.

The second placebo assigns municipalities randomly selected pseudo-treatment years between 2017 and 2021, conditional on no actual disaster occurring in those years, and then re-estimates the model. Figures C.7–C.8 show that the resulting coefficients fluctuate around zero and display no systematic post-treatment pattern. In other words, the data do not mechanically produce the type of revenue and expenditure responses documented in the main analysis when treatment timing is artificially reassigned.

These placebo exercises suggest that the main findings are unlikely to be driven by spurious timing, broad calendar-year shocks, or residual trend differences unrelated to actual disaster exposure.

5.4.4 First Versus Subsequent Declarations

Because the baseline design excludes repeatedly treated municipalities in order to preserve a transparent first-treatment estimand, Appendix C.5 reintroduces them in a complementary exercise comparing first and subsequent State of Emergency declarations. The aim is to assess whether the core qualitative conclusions continue to hold once the analysis moves beyond the selected first-treatment sample.

The sample is stratified by exposure sequence. Dynamic treatment effects are estimated using municipalities not yet exposed to the relevant treatment sequence as controls. In a complementary specification, first exposures are compared with never-treated municipalities, while second exposures are compared with once-treated municipalities (Figs. C.9–C.12).

The results show a clear contrast. First declarations reproduce the main pattern documented in Sect. 5.1, namely a pronounced increase in capital expenditure over the observed post-treatment period, accompanied by a comparable rise in revenue. Subsequent declarations, by contrast, generate substantially smaller coefficients that are generally not statistically significant, with little evidence of an additional large fiscal expansion.

This attenuation is consistent with the institutional structure of Italy's disaster-financing framework. The initial declaration typically activates a multi-year reconstruction plan and absorbs a substantial share of the available extraordinary funding, often up to the relevant per-capita ceiling. Subsequent emergencies are therefore more likely to be managed within an already activated financing framework rather than through a fresh and equally large budgetary shift. In this sense, the fiscal response appears to be front-loaded, with the first declaration triggering the major adjustment and later declarations tending to have lower marginal fiscal effects (IFRC 2022).

This complementary evidence clarifies the interpretation of the baseline estimand. The main results should be read primarily as the fiscal response to *first* exposure within the selected sample, not as the cumulative effect of repeated disasters. At the same time, the broader exercise confirms that the main mechanism identified in the paper, namely a one-time, investment-oriented budgetary expansion linked to formal emergency recognition, remains visible beyond the strict baseline design.

Across the full range of robustness checks, the overall picture is broadly consistent with the baseline interpretation, although the strength of the evidence varies across outcomes and exercises. The revenue response is the most robust result across estimator choices, comparison groups, sample restrictions, placebo exercises, and spatial inference checks. The expenditure response remains positive and investment-oriented, with capital expenditure accounting for the largest estimated component of the adjustment, but some checks weaken the statistical support, especially under conservative NUTS2-region inference and in the shorter pre-pandemic panel. Current expenditure shows little evidence of an economically meaningful response, although the joint pre-trend tests are rejected in the truncated pre-pandemic sample. Taken together, these exercises support the central interpretation of the empirical results. First exposure to a hydrogeological State of Emergency induces a structured fiscal adjustment, centred on revenues and investment-oriented spending, rather than a diffuse expansion of routine municipal spending.

6 Conclusions

This paper examines how hydrogeological disasters reshape municipal public finances in Italy. Using harmonised municipal budget data for 2016–2022 and exploiting variation in the timing of official State of Emergency declarations, we show that floods and landslides trigger a clear fiscal response. Municipal revenues and total expenditures increase after treatment, but the adjustment is concentrated rather than diffuse. On the revenue side, the response is driven mainly by capital revenues and intergovernmental transfers. On the expenditure side, it is led by investment-oriented spending, while current expenditure remains broadly unchanged. The strongest evidence, coming from the average post-treatment effects and the short- to medium-run estimates, points to a targeted fiscal adjustment centred on revenue mobilisation and investment, rather than to a broad expansion of routine municipal activity. The most visible functional adjustments concern civil protection activities, environmental intervention, and local economic support, confirming that municipalities adjust the budget items most directly connected to emergency management, reconstruction, and local recovery.

Complementary evidence on repeated declarations strengthens this interpretation. The first formal emergency recognition appears to be the moment at which the municipal fiscal response is most clearly activated, as revenues, transfers, and investment expenditure adjust within the reconstruction framework. Later declarations generate weaker marginal responses, suggesting that subsequent shocks are more often managed through institutional and financial channels already opened by the initial declaration. The post-disaster fiscal response is therefore not simply a mechanical reaction to physical damage. It also reflects the activation of a formal emergency framework that shapes how resources enter municipal budgets and how they are converted into public expenditure.

The paper also shows that this fiscal response is systematically heterogeneous. A central contribution is the construction of an operational measure of fiscal resilience based on two ex-ante structural features of municipal budgets, namely debt-service burden and tax autonomy. Debt-service burden appears to be the most binding short-run constraint, while tax autonomy matters more for the persistence and consolidation of the response. Municipalities with a low debt-service burden react more strongly on average, even when tax autonomy is weak. By contrast, the weakest response is observed among municipalities that combine high debt-service burdens with weak tax autonomy. Similar climate-related shocks can therefore generate markedly different post-disaster fiscal trajectories depending on the interaction between fiscal space and own-revenue capacity.

The results have three policy implications. First, fiscal resilience should be treated as a core component of climate adaptation, alongside infrastructure protection, land-use planning, and risk prevention. Municipalities can respond more effectively to disasters when they have both fiscal *room for manoeuvre* and sufficient discretion over their revenue base. Second, intergovernmental transfers remain crucial, especially for municipalities with weaker pre-existing fiscal conditions, but their effectiveness depends on timely and transparent allocation procedures. Third, financial resources alone are not sufficient. The conversion of extraordinary reconstruction funds into effective public action also requires administrative capacity in project design, procurement, implementation, and monitoring. This capacity may be more limited in smaller local governments, suggesting that financial support should be accompanied by technical and institutional support.

Several extensions can build on these findings. Higher-frequency fiscal data can help distinguish immediate liquidity support from slower-moving reconstruction expenditure. Linking municipal budgets to project-level data can make it possible to assess whether post-disaster investment mainly restores damaged infrastructure or also contributes to longer-run adaptation. Integrating fiscal trajectories with local outcomes such as employment, firm dynamics, or service provision can provide a fuller picture of how public finance mediates the socio-economic consequences of climate shocks.

Overall, the evidence shows that effective climate adaptation is not only a matter of physical infrastructure. It is also a matter of fiscal institutions. In a context of increasingly frequent and severe hydrogeological hazards, local fiscal capacity should be understood as an essential component of resilience. Post-disaster resilience depends not only on whether resources arrive, but also on whether local governments have the fiscal space, revenue autonomy, and administrative capability needed to transform those resources into timely and sustained public action.

Online Appendices

These online appendices provide supplementary material that supports the empirical analysis in the main text. Appendix A collects additional descriptive evidence, institutional detail, and the full set of supplementary figures and tables. In particular, it reports background information on the hydrogeological events covered in the dataset, the regional distribution of State of Emergency declarations, the structure of Italian municipal budgets, summary statistics, pre-disaster budget averages, the full disaggregated results for revenue titles and expenditure missions under alternative control-group definitions, and the subgroup estimates by debt-service burden, tax autonomy, and the four-way fiscal resilience typology.

Appendix B presents additional evidence on the credibility of the identifying assumptions. It reports balancing diagnostics for the matching exercise, joint tests on pre-treatment coefficients for the main fiscal outcomes, information on empirical support across event-time horizons, and aggregated short-, medium-, and longer-horizon post-treatment effects.

Appendix C gathers the robustness and sensitivity analyses. It includes alternative estimators, event- and region-based jackknife exercises, formal spatial inference at the NUTS2-region and SLL levels, covering both the aggregated effects and the short- and medium-run post-treatment windows, placebo tests, a pre-pandemic sample restriction that excludes the 2020–2022 period, and complementary evidence comparing first and subsequent emergency declarations.

A Additional Figures and Tables

Table A.1 Main hydrogeological events documented in the CNR-IRPI polaris database

Event	Date	Reference
Flooding in Liguria and Piedmont	Nov. 2016	https://polaris.irpi.cnr.it/event/alluvione-liguria-e-piemonte
Flooding in Livorno	Sep. 2017	https://polaris.irpi.cnr.it/event/alluvione-livorno
Flooding in Emilia-Romagna	Dec. 2017	https://polaris.irpi.cnr.it/event/alluvione-a-lentigione-brescello-pr
Raganello flash flood (Calabria)	Aug. 2018	https://polaris.irpi.cnr.it/event/piena-improvvisa-del-torrente-raganello-civita-cs-2
Flooding in Calabria	Oct. 2018	https://polaris.irpi.cnr.it/event/alluvione-in-calabria
Severe weather in Italy	Oct. 2018	https://polaris.irpi.cnr.it/event/maltempo-in-italia
Flooding in Sicily	Nov. 2018	https://polaris.irpi.cnr.it/event/alluvione-in-sicilia
Flooding in Emilia-Romagna	Jan. 2019	https://polaris.irpi.cnr.it/event/esondazioni-in-emilia-romagna
Hydrogeological instability in Lecco	Jun. 2019	https://polaris.irpi.cnr.it/event/dissesti-in-provincia-di-lecco
Flooding in northern Italy	Oct. 2019	https://polaris.irpi.cnr.it/event/evento-alluvionale-nel-nord-italia
Flooding in north-central Italy	Nov. 2019	https://polaris.irpi.cnr.it/event/evento-alluvionale-in-italia-centro-settentrionale
Flooding in northwestern Italy	Nov. 2019	https://polaris.irpi.cnr.it/event/evento-alluvionale-in-italia-nord-occidentale
Flooding in Liguria and Piedmont	Oct. 2020	https://polaris.irpi.cnr.it/event/evento-alluvionale-in-liguria-e-piemonte
Bitti flood (Nuoro)	Nov. 2020	https://polaris.irpi.cnr.it/event/alluvione-di-bitti-nu
Flooding in Sicily and Calabria	Oct. 2021	https://polaris.irpi.cnr.it/event/evento-alluvionale-in-sicilia-e-calabria
Flooding in the Marche region	Sep. 2022	https://polaris.irpi.cnr.it/event/alluvione-nelle-marche

Note. This table lists major hydrogeological events documented in the CNR-IRPI Polaris database over the period covered by the analysis. The table is descriptive and is used to provide background information on the main eventsaffecting Italian municipalities between 2016 and 2022. The empirical treatment used in the analysis is based on official State of Emergency declarations and the associated special accounting lines, drawn from the database compiled by Gatto et al. (2023)

Table A.2 Municipalities covered by hydrogeological States of emergency, by region and year

Region	2016	2017	2018	2019	2020	2021	2022	Total
Abruzzo	0	0	0	19	0	0	0	19
Basilicata	0	28	0	2	0	0	1	31
Calabria	116	0	16	119	3	0	0	254
Campania	0	0	0	70	10	0	2	82
Emilia–Romagna	57	187	44	34	3	0	2	327
Friuli-Venezia Giulia	0	214	1	0	0	0	0	215
Lazio	9	0	65	120	0	0	0	194
Liguria	108	0	83	30	1	1	0	223
Lombardia	80	0	158	194	48	117	0	597
Marche	0	0	0	15	0	0	19	34
Molise	0	117	0	0	0	0	0	117
Piemonte	41	0	3	585	203	5	0	837
Puglia	16	129	0	22	0	0	0	167
Sardegna	0	0	45	0	1	0	0	46
Sicilia	0	5	83	58	0	98	14	258
Toscana	0	3	143	100	0	0	0	246
Trentino–Alto Adige	0	0	81	0	1	0	0	82
Umbria	0	0	0	0	0	0	12	12
Valle d'Aosta	0	13	0	0	14	0	0	27
Veneto	0	57	180	16	32	0	0	285
Total	427	753	902	1,384	316	221	50	4,053

Note. Entries report municipality-year counts of municipalities included in hydrogeological State of Emergency declarations, by region and year. Because a municipality may be covered by State of Emergency declarations in more than one year, the same municipality can contribute to multiple year-specific counts

Table A.3 Structure of Italian municipal budgets: revenue titles and expenditure missions

Revenue		
Title	Description	Typology
T1	Tax revenue	101–104, 301
T2	Current transfers	101–105
T3	Extra-tax revenue	100, 200, 300, 400, 500
T4	Capital revenue	100, 200, 300, 400, 500
T5	Revenues from the sale of financial assets	100, 200, 300, 400
T6	Borrowing	100, 200, 300, 400
T7	Cash advances	100
T9	Revenue on behalf of third parties	100–200
Expenditure		
Mission	**Description**	**Programmes**
M1	Institutional and general services	01–12
M2	Justice	01–03
M3	Public order and security	01–03
M4	Education	01–08
M5	Protection of cultural heritage	01–03
M6	Youth, sport and leisure	01–03
M7	Tourism	01–02
M8	Spatial planning and housing	01–03
M9	Sustainable development	01–09
M10	Transport and mobility	01–06
M11	Civil protection	01–03
M12	Social policies and family	01–11
M13	Health protection	01–08
M14	Economic development	01–05
M15	Employment and training	01–04
M16	Agriculture and fisheries	01–03
M17	Energy	01–02
M18	Inter-municipal relations	01–02
M19	International relations	01–02
M20	Funds and provisions	01–03
M50	Public debt	01–02
M60	Financial advances	01
M99	Third-party services	01–02

Note. Budget classifications follow the harmonised accounting framework for Italianlocal governments established by Legislative Decrees 118/2011 and 126/2014,consistent with EU accounting standards (Benfante et al. 2024). Municipalbudgets are organised by revenue titles and expenditure missions/ programmes andare reported under both cash and accrual accounting definitions; see Rossi (2015)for more details

Table A.4 Summary statistics of key variables

Variable	Obs.	Mean	Std. Dev.	Min.	Max.
Population	4,136	6,165.81	22,790.71	0.00	961,011.00
Mean taxable income (€/capita)	4,014	17,495.60	3,887.35	8,020.91	45,711.05
Municipal administrative quality (MAQI)	4,092	103.69	3.51	87.43	114.94
Population density (inh./km^2)	3,992	366.14	794.57	1.06	12,099.95
Near the coast (0/1)	4,139	0.07	0.25	0.00	1.00
Coastal municipality (0/1)	4,139	0.14	0.35	0.00	1.00
Urban classification (1–3)	4,139	2.59	0.56	1.00	3.00
Resilience index	3,992	0.54	0.14	0.04	1.00
Vulnerability index	3,992	0.48	0.13	0.15	1.00
Flood hazard index	3,992	0.05	0.10	0.00	1.00
Landslide hazard index	3,992	0.09	0.17	0.00	1.00
Debt service / current revenue	2,837	0.02	0.02	0.00	0.15
Tax revenue / current revenue	3,925	0.61	0.19	0.02	0.96

Note. Summary statistics refer to the municipalities included in the estimation sample in 2016. Variables are measured at the municipal level. Sample size varies across variables because of missing values

Table A.5 Pre-disaster averages of municipal budgets: totals and composition shares

Variable	Obs.	Mean	Std. Dev.	Min–Max
Budget totals (millions of euros)				
Total expenditure	8,284	11.50	101.00	0.00–4,520.00
Total revenue	8,285	12.20	106.00	0.00–4,580.00
Total capital expenditure	8,285	1.66	7.74	0.04–347.00
Total current expenditure	8,285	5.74	38.20	0.00–1,270.00
Revenue composition (shares)				
T1. Tax revenue	7,933	0.389	0.171	0.011–0.845
T2. Current transfers	7,933	0.118	0.118	0.000–0.840
T3. Extra-tax revenue	7,933	0.125	0.086	0.003–0.713
T4. Capital revenue	7,933	0.183	0.156	0.000–0.944
T9. Revenue on behalf of third parties	7,933	0.132	0.094	0.000–0.819
Expenditure composition (shares)				
M1. Institutional and general services	7,933	0.264	0.109	0.020–0.890
M3. Public order and security	7,932	0.024	0.022	0.000–0.316
M4. Education	7,933	0.079	0.067	0.000–0.553
M5. Protection of cultural heritage	7,932	0.018	0.032	0.000–0.654
M6. Youth, sport and leisure	7,932	0.020	0.033	0.000–0.686
M7. Tourism	7,932	0.008	0.025	0.000–0.626
M8. Spatial planning and housing	7,931	0.036	0.067	0.000–0.714
M9. Sustainable development	7,932	0.140	0.080	0.000–0.740
M10. Transport and mobility	7,932	0.093	0.076	0.000–0.685
M11. Civil protection	7,932	0.006	0.032	0.000–0.892
M12. Social policies and family	7,932	0.075	0.069	0.000–0.696
M14. Economic development	7,932	0.006	0.023	0.000–0.623
M50. Public debt	7,932	0.037	0.032	0.000–0.356
M99. Third-party services	7,932	0.138	0.097	0.000–0.825

Note. This table reports descriptive statistics for municipal budgets observed before first State of Emergency exposure. Budget totals are expressed in millions of euros and include total, current, and capital expenditure as well as total revenue. Composition shares indicate the proportion of each revenue title or expenditure mission relative to the annual total

Table A.6 Breakdown of revenue by title (municipalities untreated at the comparison date as controls)

	Tax revenue	Current transfers	Extra-tax revenue	Capital revenue	Revenue on behalf of third parties
ATT (*t*-4)	−0.0009	−0.0012	0.0003	0.0021	−0.0060
	(0.0067)	(0.0045)	(0.0043)	(0.0099)	(0.0078)
ATT (*t*-3)	0.0040	−0.0007	−0.0035	−0.0048	0.0007
	(0.0057)	(0.0027)	(0.0029)	(0.0076)	(0.0039)
ATT (*t*-2)	−0.0032	−0.0006	−0.0005	0.0028	−0.0018
	(0.0031)	(0.0017)	(0.0018)	(0.0044)	(0.0026)
ATT (*t*-1)	0.0040	−0.0001	−0.0003	−0.0015	−0.0002
	(0.0028)	(0.0016)	(0.0015)	(0.0041)	(0.0025)
ATT (t)	−0.0037	−0.0033**	−0.0019	0.0090**	0.0023
	(0.0026)	(0.0015)	(0.0014)	(0.0039)	(0.0022)
ATT (*t*+1)	−0.0096***	−0.0011	−0.0005	0.0098**	0.0043
	(0.0029)	(0.0019)	(0.0017)	(0.0043)	(0.0029)
ATT (*t*+2)	−0.0114***	−0.0045**	−0.0001	0.0237***	0.0031
	(0.0037)	(0.0022)	(0.0019)	(0.0051)	(0.0032)
ATT (*t*+3)	−0.0135***	−0.0005	−0.0032	0.0234***	0.0046
	(0.0044)	(0.0027)	(0.0021)	(0.0060)	(0.0042)
ATT (*t*+4)	−0.0153**	−0.0119***	−0.0070**	0.0294***	0.0119*
	(0.0070)	(0.0044)	(0.0033)	(0.0094)	(0.0069)
ATT (*t*+5)	−0.0149	−0.0201***	−0.0145***	0.0582***	0.0006
	(0.0100)	(0.0070)	(0.0046)	(0.0143)	(0.0102)
ATT (average)	−0.0099***	−0.0039**	−0.0023	0.0186***	0.0040
	(0.0029)	(0.0018)	(0.0015)	(0.0039)	(0.0027)
Test on pre-trend (Chi sq.)	25.7232	8.2334	8.0168	15.7657	11.1959
p-value	0.0041	0.6061	0.6272	0.1065	0.3425
Unweighted average	0.3775	0.1389	0.1173	0.1810	0.1344
Weighted average	0.3496	0.1211	0.1075	0.1298	0.1954
No. of observations	27,164	27,164	27,164	27,163	27,164

Note. Dependent variables are expressed as shares of total revenue. Standard errors are reported in parentheses. Significance levels are denoted as follows: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Estimates are obtained within the Callaway-Sant'Anna framework using the improved doubly robust estimator. The control group consists of municipalities untreated at the relevant comparison date, including both never-treated and not-yet-treated units

Table A.7 Breakdown of revenue by title (never-treated municipalities as controls)

	Tax revenue	Current transfers	Extra-tax revenue	Capital revenue	Revenue on behalf of third parties
ATT (t-4)	−0.0009	−0.0012	0.0003	0.0021	−0.0060
	(0.0067)	(0.0045)	(0.0043)	(0.0099)	(0.0078)
ATT (t-3)	0.0044	−0.0005	−0.0033	−0.0052	0.0004
	(0.0056)	(0.0027)	(0.0029)	(0.0075)	(0.0039)
ATT (t-2)	−0.0028	−0.0010	−0.0007	0.0025	−0.0020
	(0.0031)	(0.0017)	(0.0018)	(0.0044)	(0.0027)
ATT (t-1)	0.0037	−0.0001	−0.0005	−0.0011	−0.0006
	(0.0028)	(0.0016)	(0.0015)	(0.0040)	(0.0024)
ATT (t)	−0.0039	−0.0030**	−0.0020	0.0092**	0.0023
	(0.0026)	(0.0015)	(0.0014)	(0.0039)	(0.0022)
ATT ($t+1$)	−0.0100***	−0.0009	−0.0007	0.0105**	0.0044
	(0.0029)	(0.0019)	(0.0016)	(0.0042)	(0.0028)
ATT ($t+2$)	−0.0115***	−0.0045**	−0.0002	0.0240***	0.0031
	(0.0037)	(0.0022)	(0.0019)	(0.0051)	(0.0032)
ATT ($t+3$)	−0.0135***	−0.0005	−0.0032	0.0234***	0.0046
	(0.0044)	(0.0027)	(0.0021)	(0.0060)	(0.0042)
ATT ($t+4$)	−0.0153**	−0.0119***	−0.0070**	0.0294***	0.0119*
	(0.0070)	(0.0044)	(0.0033)	(0.0094)	(0.0069)
ATT ($t+5$)	−0.0149	−0.0201***	−0.0145***	0.0582***	0.0006
	(0.0100)	(0.0070)	(0.0046)	(0.0143)	(0.0102)
ATT (average)	−0.0101***	−0.0038**	−0.0024	0.0189***	0.0040
	(0.0029)	(0.0018)	(0.0015)	(0.0039)	(0.0028)
Test on pre-trend (Chi sq.)	24.9011	10.2742	8.2127	15.5919	12.0338
p-value	0.0055	0.4168	0.6081	0.1119	0.2828
Unweighted average	0.3775	0.1389	0.1173	0.1810	0.1344
Weighted average	0.3496	0.1211	0.1075	0.1298	0.1954
No. of observations	27,154	27,154	27,154	27,153	27,154

Note. Dependent variables are expressed as shares of total revenue. Standard errors are reported in parentheses. Significance levels are denoted as follows: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Estimates are obtained within the Callaway-Sant'Anna framework using the improved doubly robust estimator. The control group is restricted to never-treated municipalities

Table A.8 Breakdown of expenditure by mission (municipalities untreated at the comparison date as controls) - part 1

	Institutional and general services	Public order and security	Education	Protection of cultural heritage	Youth, sport and leisure	Tourism	Spatial planning and housing
ATT (*t*-4)	−0.0008	0.0009	0.0055*	0.0004	−0.0008	−0.0019	0.0029
	(0.0054)	(0.0009)	(0.0030)	(0.0014)	(0.0038)	(0.0016)	(0.0032)
ATT (*t*-3)	−0.0028	0.0016**	−0.0022	−0.0036*	−0.0018	0.0029	−0.0011
	(0.0042)	(0.0007)	(0.0026)	(0.0022)	(0.0021)	(0.0022)	(0.0024)
ATT (*t*-2)	−0.0020	−0.0001	0.0032	−0.0002	−0.0017	−0.0019	0.0003
	(0.0025)	(0.0006)	(0.0021)	(0.0011)	(0.0011)	(0.0012)	(0.0018)
ATT (*t*-1)	0.0020	−0.0007	−0.0034*	−0.0002	0.0006	0.0009	0.0006
	(0.0023)	(0.0004)	(0.0018)	(0.0008)	(0.0009)	(0.0006)	(0.0016)
ATT (t)	−0.0019	−0.0001	0.0004	−0.0002	0.0012	−0.0001	−0.0029*
	(0.0022)	(0.0004)	(0.0018)	(0.0007)	(0.0009)	(0.0005)	(0.0017)
ATT (*t*+1)	−0.0033	0.0002	0.0001	0.0001	0.0010	−0.0001	−0.0013
	(0.0026)	(0.0005)	(0.0020)	(0.0008)	(0.0011)	(0.0007)	(0.0021)
ATT (*t*+2)	−0.0043	0.0003	0.0002	0.0009	0.0024**	−0.0003	−0.0002
	(0.0030)	(0.0006)	(0.0023)	(0.0010)	(0.0012)	(0.0009)	(0.0025)
ATT (*t*+3)	−0.0088**	−0.0004	−0.0019	0.0001	0.0028*	−0.0008	0.0066**
	(0.0035)	(0.0006)	(0.0025)	(0.0011)	(0.0015)	(0.0007)	(0.0031)
ATT (*t*+4)	−0.0169***	−0.0014	−0.0009	0.0003	0.0023	0.0031**	0.0092*
	(0.0054)	(0.0011)	(0.0040)	(0.0016)	(0.0022)	(0.0014)	(0.0051)
ATT (*t*+5)	−0.0166**	−0.0023	0.0044	0.0017	0.0019	−0.0003	0.0122
	(0.0079)	(0.0014)	(0.0054)	(0.0021)	(0.0023)	(0.0017)	(0.0078)
ATT (average)	−0.0057**	−0.0002	−0.0000	0.0003	0.0018**	−0.0000	0.0013
	(0.0023)	(0.0004)	(0.0017)	(0.0007)	(0.0009)	(0.0005)	(0.0019)
Test on pre-trend (Chi sq.)	6.2241	12.3771	13.0720	6.8823	17.0128	9.6765	11.0261
p-value	0.7961	0.2606	0.2197	0.7365	0.0741*	0.4693	0.3555
Unweighted average	0.2598	0.0242	0.0736	0.0178	0.0189	0.0075	0.0374
Weighted average	0.1847	0.0284	0.0613	0.0177	0.0161	0.0059	0.0279
No. of observations	27,164	27,157	27,164	27,157	27,157	27,157	27,156

Note. Dependent variables are expressed as shares of total expenditure. Standard errors are reported in parentheses. Significance levels are denoted as follows: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Estimates are obtained within the Callaway-Sant'Anna framework using the improved doubly robust estimator. The control group consists of municipalities untreated at the relevant comparison date, including both never-treated and not-yet-treated units

Table A.9 Breakdown of expenditure by mission (municipalities untreated at the comparison date as controls) - part 2

	Sustainable development	Trans-port and mobility	Civil protection	Social policies and family	Economic development	Public debt	Third-party services
ATT (*t*-4)	0.0048	−0.0007	−0.0004	−0.0023	−0.0018**	−0.0006	−0.0079
	(0.0047)	(0.0038)	(0.0006)	(0.0030)	(0.0009)	(0.0022)	(0.0079)
ATT (*t*-3)	−0.0022	−0.0010	−0.0006	−0.0010	−0.0003	0.0006	0.0019
	(0.0040)	(0.0041)	(0.0005)	(0.0022)	(0.0009)	(0.0014)	(0.0040)
ATT (*t*-2)	0.0019	0.0022	−0.0028***	0.0010	−0.0007	0.0016*	−0.0000
	(0.0020)	(0.0021)	(0.0008)	(0.0013)	(0.0011)	(0.0009)	(0.0026)
ATT (*t*-1)	0.0022	0.0014	0.0007	−0.0019	0.0001	−0.0002	−0.0015
	(0.0019)	(0.0021)	(0.0007)	(0.0012)	(0.0006)	(0.0009)	(0.0025)
ATT (t)	−0.0007	0.0036*	0.0010	−0.0014	0.0005	−0.0008	0.0033
	(0.0019)	(0.0021)	(0.0006)	(0.0010)	(0.0005)	(0.0007)	(0.0022)
ATT (*t*+1)	0.0041*	−0.0014	0.0029***	−0.0005	−0.0003	−0.0009	0.0065**
	(0.0023)	(0.0023)	(0.0010)	(0.0014)	(0.0005)	(0.0008)	(0.0030)
ATT (*t*+2)	0.0071***	0.0012	0.0026***	0.0010	−0.0000	−0.0020**	0.0046
	(0.0027)	(0.0028)	(0.0009)	(0.0015)	(0.0006)	(0.0010)	(0.0034)
ATT (*t*+3)	0.0074**	0.0011	0.0033***	−0.0012	0.0000	−0.0027**	0.0043
	(0.0032)	(0.0029)	(0.0012)	(0.0018)	(0.0007)	(0.0012)	(0.0044)
ATT (*t*+4)	0.0082*	−0.0022	0.0023	0.0017	−0.0001	−0.0051***	0.0086
	(0.0049)	(0.0044)	(0.0017)	(0.0035)	(0.0013)	(0.0019)	(0.0071)
ATT (*t*+5)	0.0069	−0.0017	0.0025	0.0043	0.0057***	−0.0026	−0.0018
	(0.0075)	(0.0065)	(0.0032)	(0.0049)	(0.0016)	(0.0024)	(0.0105)
ATT (average)	0.0046**	0.0008	0.0024***	−0.0001	0.0003	−0.0018**	0.0047*
	(0.0020)	(0.0020)	(0.0007)	(0.0013)	(0.0005)	(0.0008)	(0.0029)
Test on pre-trend (Chi sq.)	7.2509	9.8036	25.9302	13.7609	11.8801	8.8559	15.7039
p-value	0.7016	0.4579	0.0038	0.1842	0.2932	0.5458	0.1084
Unweighted average	0.1405	0.0922	0.0094	0.0801	0.0066	0.0366	0.1420
Weighted average	0.1389	0.0727	0.0075	0.0904	0.0077	0.0373	0.2118
No. of observations	27,157	27,157	27,157	27,157	27,157	27,157	27,157

Note. Dependent variables are expressed as shares of total expenditure. Standard errors are reported in parentheses. Significance levels are denoted as follows: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Estimates are obtained within the Callaway-Sant'Anna framework using the improved doubly robust estimator. The control group consists of municipalities untreated at the relevant comparison date, including both never-treated and not-yet-treated units

Table A.10 Breakdown of expenditure by mission (never-treated municipalities as controls) - part 1

	Institutional and general services	Public order and security	Education	Protection of cultural heritage	Youth, sport and leisure	Tourism	Spatial planning and housing
ATT (*t*-4)	−0.0008	0.0009	0.0055*	0.0004	−0.0008	−0.0019	0.0029
	(0.0054)	(0.0009)	(0.0030)	(0.0014)	(0.0038)	(0.0016)	(0.0032)
ATT (*t*-3)	−0.0032	0.0017**	−0.0023	−0.0036	−0.0021	0.0025	−0.0009
	(0.0042)	(0.0007)	(0.0026)	(0.0022)	(0.0022)	(0.0021)	(0.0025)
ATT (*t*-2)	−0.0027	0.0001	0.0030	−0.0004	−0.0019*	−0.0016	0.0005
	(0.0025)	(0.0006)	(0.0021)	(0.0011)	(0.0010)	(0.0010)	(0.0018)
ATT (*t*-1)	0.0019	−0.0006	−0.0032*	−0.0003	0.0004	0.0006	0.0006
	(0.0023)	(0.0004)	(0.0018)	(0.0007)	(0.0009)	(0.0006)	(0.0016)
ATT (t)	−0.0019	−0.0002	0.0003	−0.0004	0.0011	−0.0001	−0.0029*
	(0.0022)	(0.0004)	(0.0018)	(0.0006)	(0.0009)	(0.0005)	(0.0016)
ATT (t+1)	−0.0034	−0.0000	0.0001	−0.0000	0.0009	−0.0002	−0.0012
	(0.0026)	(0.0005)	(0.0020)	(0.0008)	(0.0011)	(0.0007)	(0.0020)
ATT (t+2)	−0.0043	0.0003	0.0002	0.0009	0.0023*	−0.0002	−0.0002
	(0.0030)	(0.0006)	(0.0023)	(0.0010)	(0.0012)	(0.0009)	(0.0025)
ATT (t+3)	−0.0088**	−0.0004	−0.0019	0.0001	0.0028*	−0.0008	0.0066**
	(0.0035)	(0.0006)	(0.0025)	(0.0011)	(0.0015)	(0.0007)	(0.0031)
ATT (t+4)	−0.0169***	−0.0014	−0.0009	0.0003	0.0023	0.0031**	0.0092*
	(0.0054)	(0.0011)	(0.0040)	(0.0016)	(0.0022)	(0.0014)	(0.0051)
ATT (t+5)	−0.0166**	−0.0023	0.0044	0.0017	0.0019	−0.0003	0.0122
	(0.0079)	(0.0014)	(0.0054)	(0.0021)	(0.0023)	(0.0017)	(0.0078)
ATT (average)	−0.0058**	−0.0003	−0.0001	0.0002	0.0017*	−0.0000	0.0014
	(0.0023)	(0.0004)	(0.0017)	(0.0007)	(0.0009)	(0.0005)	(0.0019)
Test on pre-trend (Chi sq.)	5.7859	13.0946	12.7190	6.7794	16.7821	9.7110	9.6876
p-value	0.8329	0.2184	0.2398	0.7461	0.0793*	0.4662	0.4683
Unweighted average	0.2598	0.0242	0.0736	0.0178	0.0189	0.0075	0.0374
Weighted average	0.1847	0.0284	0.0613	0.0177	0.0161	0.0059	0.0279
No. of observations	27,154	27,147	27,154	27,147	27,147	27,147	27,146

Note. Dependent variables are expressed as shares of total expenditure. Standard errors are reported in parentheses. Significance levels are denoted as follows: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Estimates are obtained within the Callaway-Sant'Anna framework using the improved doubly robust estimator. The control group is restricted to never-treated municipalities

Table A.11 Breakdown of expenditure by mission (never-treated municipalities as controls) - part 2

	Sustainable development	Transport and mobility	Civil protection	Social policies and family	Economic development	Public debt	Third-party services
ATT (*t*-4)	0.0048	−0.0007	−0.0004	−0.0023	−0.0018**	−0.0006	−0.0079
	(0.0047)	(0.0038)	(0.0006)	(0.0030)	(0.0009)	(0.0022)	(0.0079)
ATT (*t*-3)	−0.0009	−0.0011	−0.0006	−0.0012	−0.0003	0.0007	0.0014
	(0.0034)	(0.0041)	(0.0005)	(0.0022)	(0.0008)	(0.0015)	(0.0040)
ATT (*t*-2)	0.0019	0.0025	−0.0029***	0.0008	−0.0009	0.0016*	−0.0003
	(0.0020)	(0.0020)	(0.0008)	(0.0012)	(0.0011)	(0.0009)	(0.0026)
ATT (*t*-1)	0.0023	0.0014	0.0008	−0.0023*	0.0001	−0.0001	−0.0016
	(0.0019)	(0.0020)	(0.0006)	(0.0012)	(0.0006)	(0.0008)	(0.0025)
ATT (t)	−0.0005	0.0040**	0.0009	−0.0014	0.0006	−0.0008	0.0032
	(0.0018)	(0.0020)	(0.0006)	(0.0010)	(0.0005)	(0.0007)	(0.0023)
ATT (*t*+1)	0.0043*	−0.0009	0.0030***	−0.0004	−0.0002	−0.0007	0.0067**
	(0.0023)	(0.0022)	(0.0010)	(0.0013)	(0.0005)	(0.0008)	(0.0030)
ATT (*t*+2)	0.0070***	0.0013	0.0026***	0.0009	0.0000	−0.0020**	0.0046
	(0.0027)	(0.0028)	(0.0009)	(0.0015)	(0.0006)	(0.0010)	(0.0034)
ATT (*t*+3)	0.0074**	0.0011	0.0033***	−0.0012	0.0000	−0.0027**	0.0043
	(0.0032)	(0.0029)	(0.0012)	(0.0018)	(0.0007)	(0.0012)	(0.0044)
ATT (*t*+4)	0.0082*	−0.0022	0.0023	0.0017	−0.0001	−0.0051***	0.0086
	(0.0049)	(0.0044)	(0.0017)	(0.0035)	(0.0013)	(0.0019)	(0.0071)
ATT (*t*+5)	0.0069	−0.0017	0.0025	0.0043	0.0057***	−0.0026	−0.0018
	(0.0075)	(0.0065)	(0.0032)	(0.0049)	(0.0016)	(0.0024)	(0.0105)
ATT (average)	0.0047**	0.0010	0.0024***	−0.0001	0.0003	−0.0018**	0.0048*
	(0.0020)	(0.0019)	(0.0007)	(0.0013)	(0.0005)	(0.0008)	(0.0029)
Test on pre-trend (Chi sq.)	7.6427	9.9016	25.7909	14.1532	10.6482	8.4310	15.9760
p-value	0.6637	0.4492	0.0040	0.1661	0.3856	0.5868	0.1003
Unweighted average	0.1405	0.0922	0.0094	0.0801	0.0066	0.0366	0.1420
Weighted average	0.1389	0.0727	0.0075	0.0904	0.0077	0.0373	0.2118
No. of observations	27,147	27,147	27,147	27,147	27,147	27,147	27,147

Note. Dependent variables are expressed as shares of total expenditure. Standard errors are reported in parentheses. Significance levels are denoted as follows: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Estimates are obtained within the Callaway-Sant'Anna framework using the improved doubly robust estimator. The control group is restricted to never-treated municipalities

Table A.12 Debt-service burden and tax autonomy (municipalities untreated at the comparison date as controls)

	Low debt-service burden	High debt-service burden	Weak tax autonomy	Strong tax autonomy
ATT (*t*-4)	0.0094	0.0147	0.0112	0.0081
	(0.0317)	(0.0268)	(0.0270)	(0.0327)
ATT (*t*-3)	−0.0391**	0.0036	−0.0148	−0.0125
	(0.0194)	(0.0187)	(0.0196)	(0.0208)
ATT (*t*-2)	−0.0056	−0.0072	−0.0088	−0.0034
	(0.0138)	(0.0109)	(0.0142)	(0.0110)
ATT (*t*-1)	−0.0118	0.0066	−0.0048	0.0076
	(0.0130)	(0.0098)	(0.0131)	(0.0096)
ATT (t)	0.0221	−0.0015	0.0148	−0.0047
	(0.0139)	(0.0098)	(0.0135)	(0.0096)
ATT (*t*+1)	0.0396**	0.0052	0.0208	0.0127
	(0.0170)	(0.0115)	(0.0153)	(0.0119)
ATT (*t*+2)	0.0435**	0.0103	0.0307*	0.0085
	(0.0203)	(0.0134)	(0.0183)	(0.0138)
ATT (*t*+3)	0.0835***	0.0436***	0.0568***	0.0527***
	(0.0231)	(0.0168)	(0.0220)	(0.0174)
ATT (*t*+4)	0.1252***	0.0489*	0.0253	0.1178***
	(0.0361)	(0.0284)	(0.0310)	(0.0342)
ATT (*t*+5)	0.1155*	0.0735*	0.0500	0.1514***
	(0.0655)	(0.0442)	(0.0502)	(0.0560)
ATT (average)	0.0548***	0.0174	0.0298**	0.0256**
	(0.0156)	(0.0112)	(0.0150)	(0.0113)
Test on pre-trend (Chi sq.)	13.2113	7.5211	11.3807	12.9029
p-value	0.2121	0.6755	0.3286	0.2292
No. of observations	9,312	17,845	12,573	14,584

Note. Standard errors are reported in parentheses. Significance levels are denoted as follows: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Estimates are obtained within the Callaway-Sant'Anna framework using the improved doubly robust estimator. The control group consists of municipalities untreated at the relevant comparison date, including both never-treated and not-yet-treated units. Subgroups are defined using pre-disaster values of debt-service burden and tax autonomy

Table A.13 Debt-service burden and tax autonomy (never-treated municipalities as controls)

	Low debt-service burden	High debt-service burden	Weak tax autonomy	Strong tax autonomy
ATT (*t*-4)	0.0094	0.0147	0.0112	0.0081
	(0.0317)	(0.0268)	(0.0270)	(0.0327)
ATT (*t*-3)	−0.0392**	0.0033	−0.0117	−0.0158
	(0.0194)	(0.0175)	(0.0189)	(0.0216)
ATT (*t*-2)	−0.0073	−0.0047	−0.0086	−0.0015
	(0.0138)	(0.0107)	(0.0142)	(0.0107)
ATT (*t*-1)	−0.0124	0.0043	−0.0051	0.0038
	(0.0132)	(0.0096)	(0.0130)	(0.0094)
ATT (t)	0.0211	0.0022	0.0130	−0.0024
	(0.0138)	(0.0098)	(0.0136)	(0.0093)
ATT (t+1)	0.0391**	0.0075	0.0221	0.0141
	(0.0164)	(0.0114)	(0.0152)	(0.0118)
ATT (t+2)	0.0452**	0.0098	0.0301*	0.0085
	(0.0201)	(0.0134)	(0.0183)	(0.0138)
ATT (t+3)	0.0835***	0.0436***	0.0568***	0.0527***
	(0.0231)	(0.0168)	(0.0220)	(0.0174)
ATT (t+4)	0.1252***	0.0489*	0.0253	0.1178***
	(0.0361)	(0.0284)	(0.0310)	(0.0342)
ATT (t+5)	0.1155*	0.0735*	0.0500	0.1514***
	(0.0655)	(0.0442)	(0.0502)	(0.0560)
ATT (average)	0.0548***	0.0188*	0.0295*	0.0266**
	(0.0154)	(0.0112)	(0.0151)	(0.0113)
Test on pre-trend (Chi sq.)	13.4394	6.7615	11.0179	13.8728
p-value	0.2001	0.7478	0.3561	0.1789
No. of observations	9,307	17,840	12,567	14,580

Note. Standard errors are reported in parentheses. Significance levels are denoted asfollows: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Estimates are obtainedwithin the Callaway-Sant'Anna framework using the improved doubly robustestimator. The control group is restricted to never-treated municipalities. Subgroups are defined using pre-disaster values of debt-service burden and taxautonomy

Table A.14 Fiscal resilience (municipalities untreated at the comparison date as controls)

	Low debt-service burden, strong autonomy	Low debt-service burden, weak autonomy	High debt-service burden, strong autonomy	High debt-service burden, weak autonomy
ATT (*t*-4)	0.0240	−0.0078	0.0083	0.0233
	(0.0432)	(0.0454)	(0.0433)	(0.0359)
ATT (*t*-3)	−0.0428	−0.0403	0.0163	−0.0017
	(0.0291)	(0.0295)	(0.0279)	(0.0267)
ATT (*t*-2)	0.0221	−0.0377*	−0.0183	0.0113
	(0.0178)	(0.0227)	(0.0138)	(0.0187)
ATT (*t*-1)	−0.0223	0.0040	0.0191	−0.0112
	(0.0163)	(0.0220)	(0.0118)	(0.0167)
ATT (t)	0.0027	0.0380	−0.0062	0.0007
	(0.0158)	(0.0242)	(0.0122)	(0.0169)
ATT (t+1)	0.0132	0.0801***	0.0169	−0.0023
	(0.0202)	(0.0285)	(0.0150)	(0.0186)
ATT (t+2)	0.0130	0.0776**	0.0104	0.0015
	(0.0229)	(0.0352)	(0.0188)	(0.0219)
ATT (t+3)	0.0678***	0.0899**	0.0534**	0.0265
	(0.0260)	(0.0400)	(0.0243)	(0.0267)
ATT (t+4)	0.1041**	0.1215**	0.1963***	−0.0473
	(0.0499)	(0.0544)	(0.0661)	(0.0357)
ATT (t+5)	0.1056	0.1438	0.1983***	−0.0209
	(0.0924)	(0.1016)	(0.0700)	(0.0592)
ATT (average)	0.0298*	0.0792***	0.0285*	−0.0005
	(0.0179)	(0.0269)	(0.0150)	(0.0181)
Test on pre-trend (Chi sq.)	12.1272	14.0369	10.9160	8.8930
p-value	0.2766	0.1713	0.3641	0.5423
No. of observations	4,933	4,379	9,507	8,194

Note. Standard errors are reported in parentheses. Significance levels are denoted as follows: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Estimates are obtained within the Callaway-Sant'Anna framework using the improved doubly robust estimator. The control group consists of municipalities untreated at the relevant comparison date, including both never-treated and not-yet-treated units. Fiscal resilience groups are defined using pre-disaster values of debt-service burden and tax autonomy

Table A.15 Fiscal resilience (never-treated municipalities as controls)

	Low debt-service burden, strong autonomy	Low debt-service burden, weak autonomy	High debt-service burden, strong autonomy	High debt-service burden, weak autonomy
ATT (*t*-4)	0.0240	−0.0078	0.0083	0.0233
	(0.0432)	(0.0454)	(0.0433)	(0.0359)
ATT (*t*-3)	−0.0423	−0.0416	0.0012	0.0050
	(0.0295)	(0.0296)	(0.0271)	(0.0248)
ATT (*t*-2)	0.0199	−0.0367	−0.0127	0.0104
	(0.0178)	(0.0226)	(0.0132)	(0.0187)
ATT (*t*-1)	−0.0222	−0.0018	0.0133	−0.0097
	(0.0165)	(0.0221)	(0.0117)	(0.0165)
ATT (t)	0.0033	0.0334	−0.0022	0.0000
	(0.0155)	(0.0247)	(0.0120)	(0.0170)
ATT (*t*+1)	0.0166	0.0754***	0.0194	−0.0019
	(0.0194)	(0.0280)	(0.0159)	(0.0185)
ATT (*t*+2)	0.0137	0.0766**	0.0097	0.0012
	(0.0229)	(0.0354)	(0.0188)	(0.0219)
ATT (*t*+3)	0.0678***	0.0899**	0.0534**	0.0265
	(0.0260)	(0.0400)	(0.0243)	(0.0267)
ATT (*t*+4)	0.1041**	0.1215**	0.1963***	−0.0473
	(0.0499)	(0.0544)	(0.0661)	(0.0357)
ATT (*t*+5)	0.1056	0.1438	0.1983***	−0.0209
	(0.0924)	(0.1016)	(0.0700)	(0.0592)
ATT (average)	0.0310*	0.0768***	0.0303**	−0.0006
	(0.0177)	(0.0270)	(0.0153)	(0.0182)
Test on pre-trend (Chi sq.)	11.9271	14.0668	10.9709	8.0938
p-value	0.2900	0.1700	0.3598	0.6197
No. of observations	4,932	4,375	9,445	8,192

Note. Standard errors are reported in parentheses. Significance levels are denoted as follows: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Estimates are obtained within the Callaway-Sant'Anna framework using the improved doubly robust estimator. The control group is restricted to never-treated municipalities. Fiscal resilience groups are defined using pre-disaster values of debt-service burden and tax autonomy

B Balancing, Pre-Treatment, and Event-Time Support

This appendix brings together three complementary pieces of diagnostic evidence for the baseline design. We proceed in sequence. First, we examine observable comparability between treated and control municipalities through a matching-based balancing exercise. Second, we report joint tests on pre-treatment coefficients for the main fiscal outcomes. Third, we document how empirical support varies across event time and summarise the post-treatment dynamics over broader windows. Taken together, these diagnostics help clarify both the comparability of treated and control units and the amount of information underlying the dynamic estimates discussed in the main text.

We begin with observable balance. To do so, we estimate the propensity score on the subsample of municipalities that were treated only once during 2017–2022 or were never treated throughout 2013–2022. The purpose of this exercise is not to redefine the baseline estimator, which in the main analysis also exploits not-yet-treated municipalities as controls, but rather to assess whether the pool of never-treated municipalities provides a plausible benchmark in terms of pre-treatment characteristics. This is especially useful as a diagnostic because never-treated units provide a stable comparison group throughout the sample period. The propensity score model includes the main pre-treatment characteristics that may jointly shape disaster exposure and fiscal outcomes: socio-economic variables (log population, taxable income, and urban classification), institutional quality, geographic exposure, and the territorial resilience and vulnerability indicators used in the paper.

Table B.1 reports the average marginal effects from the probit specification together with balance statistics before and after matching. Matching is implemented using nearest-neighbour matching, with a caliper equal to one-quarter of the standard deviation of the estimated propensity score and imposing common support. Several covariates are significantly associated with treatment status, which suggests that adjustment is warranted in this setting. After matching, however, the differences between treated and control municipalities become markedly smaller, and the remaining discrepancies are generally limited. The most persistent residual differences concern the territorial resilience and vulnerability indices, which likely capture broader structural features that are not easily eliminated through a simple nearest-neighbour procedure. Overall, the balancing exercise indicates that the matched control group is substantially more comparable to treated municipalities along the main observable dimensions considered in the analysis.

Observable balance is informative, but it is not sufficient on its own for the identifying assumption used in the staggered difference-in-differences design.

For that reason, we next turn to pre-treatment dynamics. Table B.3 reports joint tests of the pre-treatment coefficients for the four main fiscal outcomes under both control-group definitions: municipalities untreated at the relevant comparison date and never-treated municipalities only. This evidence should be interpreted as complementary to the event-study. Although the conditional parallel trends assumption is not directly testable, pre-treatment diagnostics remain useful for assessing whether treated and control municipalities display systematically different patterns before treatment.

Table B.1 Probit estimates and covariate balance before and after matching

Dep. var.: Pr(Treated)	Average marginal effects	Before matching: t-stat [*p*-value]	After matching: t-stat [*p*-value]
Population (log)	0.0708***	0.54 [0.588]	−0.16 [0.870]
	(0.0101)		
Mean taxable income p.c. (log)	−0.1542**	2.48 [0.013]	0.38 [0.705]
	(0.0738)		
Municipal administrative quality	−0.0027	−1.88 [0.061]	1.27 [0.206]
	(0.0022)		
Population density (log)	−0.0577***	−1.49 [0.135]	−0.04 [0.964]
	(0.0099)		
Near the coast	0.1561***	5.04 [<0.001]	−1.03 [0.301]
	(0.0399)		
Coastal municipality	0.1086***	3.33 [0.001]	−0.71 [0.476]
	(0.0309)		
Urban classification	0.0353*	1.32 [0.188]	0.28 [0.781]
	(0.0211)		
Territorial resilience index	−0.8187***	2.66 [0.008]	0.22 [0.829]
	(0.1380)		
Territorial vulnerability index	−0.5524***	−4.50 [<0.001]	−0.38 [0.705]
	(0.0959)		
Flood hazard index	0.0805	3.94 [<0.001]	0.46 [0.646]
	(0.0742)		
Landslide hazard index	−0.3420***	−4.68 [<0.001]	−0.78 [0.436]
	(0.0500)		
N.	3,975		
Pseudo R-sq.	0.0483		

Note. The table reports average marginal effects from a probit model for treatment status and covariate balance statistics before and after matching. Matching is implemented using nearest-neighbour matching, with a caliper equal to one-quarter of the standard deviation of the estimated propensity score and imposing common support. Covariates refer to 2016. Treated municipalities are compared with never-treated municipalities. Standard errors are reported in parentheses

The results are broadly supportive of the identifying design. The null of jointly zero pre-treatment coefficients is not rejected for total revenue, total expenditure, or capital expenditure under either control-group definition. The only exception is current expenditure, for which the joint test rejects at conventional levels. We interpret this result cautiously, since current expenditure is a broad aggregate of heterogeneous routine items and the corresponding event-study profile does not display a clear anticipatory pattern. Taken together, Table B.2, the graphical evidence in the main text, and the additional robustness exercises in Appendix C support the credibility of the baseline design.

The final part of this appendix clarifies how much information underlies the dynamic coefficients. Because the panel is finite, the event-study profile is not estimated with uniform support at all horizons. Coefficients close to treatment are informed by a broader set of cohorts, while coefficients farther from treatment are identified only from earlier-treated municipalities that remain observable at those horizons.

Table B.2 Evidence on pre-treatment dynamics: joint tests for the main fiscal outcomes

Outcome	Pre-treatment window	Test statistic	p-value	No. of observations
Panel A. Municipalities untreated at the comparison date as controls				
Total revenue	$t-4$ to $t-1$	14.8377	0.1381	27,164
Total expenditure	$t-4$ to $t-1$	12.2015	0.2718	27,157
Current expenditure	$t-4$ to $t-1$	21.1325	0.0202	27,164
Capital expenditure	$t-4$ to $t-1$	11.5712	0.3148	27,065
Panel B. Never-treated municipalities as controls				
Total revenue	$t-4$ to $t-1$	14.9061	0.1355	27,154
Total expenditure	$t-4$ to $t-1$	11.9445	0.2888	27,147
Current expenditure	$t-4$ to $t-1$	24.1478	0.0072	27,154
Capital expenditure	$t-4$ to $t-1$	11.4202	0.3257	27,051

Note. The table reports joint tests of the null hypothesis that all pre-treatment coefficients are equal to zero in the event-study specification for the four main fiscal outcomes. Panel A uses the baseline control group, consisting of municipalities untreated at the relevant comparison date, including both never-treated and not-yet-treated units. Panel B restricts the control group to never-treated municipalities. The tests complement the graphical event-study evidence discussed in the main text and provide indirect evidence on the plausibility of the conditional parallel trends assumption

Table B.3 Empirical support by event-time horizon

Event time	Contributing treated cohorts	Group-time cells	Treated municipalities	Treated municipality-year observations
$t-4$	2	2	367	367
$t-3$	3	3	974	974
$t-2$	4	4	1,181	1,181
$t-1$	5	5	1,444	1,444
t	5	5	1,444	1,444
$t+1$	5	5	1,444	1,444
$t+2$	4	4	1,296	1,296
$t+3$	3	3	1,077	1,077
$t+4$	2	2	470	470
$t+5$	1	1	263	263

Note. The table documents the empirical support underlying each event-time coefficient in the baseline event-study specification. "Contributing treated cohorts" reports the number of treatment cohorts entering each horizon, while "group-time cells" reports the number of cohort-year cells used to identify the corresponding coefficient. "Treated municipalities" gives the number of distinct treated municipalities contributing at each event time, and "treated municipality-year observations" reports the associated number of municipality-year observations. As is standard in staggered-adoption settings with a finite panel, support declines at longer horizons because only earlier-treated cohorts remain available for identification

Table B.3 makes this composition explicit by reporting, for each event time, the number of contributing cohorts, group-time cells, treated municipalities, and treated municipality-year observations. The pattern is the expected one in a staggered-adoption design with a limited time window: support is richest around treatment and in the early post-treatment periods, then declines progressively at longer horizons.

This variation in support is important for interpretation. In particular, it cautions against placing too much weight on any single far-horizon coefficient.

For that reason, Table B.4 complements the horizon-by-horizon event-study profile with averages over broader post-treatment windows. Aggregating the coefficients into short-, medium-, and longer-horizon windows provides a compact summary of the dynamic response and makes it easier to assess whether the main features of the adjustment are stable across control-group definitions. The pattern is similar in the two panels: revenue effects are already visible in the short run and strengthen thereafter; total expenditure rises more gradually; current expenditure remains close to zero throughout; and capital expenditure becomes economically and statistically more pronounced from the medium horizon onward. This window-based summary therefore reinforces the interpretation emphasised in the main text, namely that the post-disaster fiscal response is driven primarily by capital spending rather than by a generalised expansion of current expenditure.

Table B.4 Aggregated post-treatment effects by event-time window

Outcome	Short run	Medium horizon	Longer horizon
	($t$tot $+ 1$)	**($t + 2$tot $+ 3$)**	**($t + 4$tot $+ 5$)**
Panel A. Municipalities untreated at the comparison date as controls			
Total revenue	0.0242***	0.0485***	0.0707***
	(0.0074)	(0.0108)	(0.0251)
Total expenditure	0.0115	0.0392***	0.0875***
	(0.0076)	(0.0112)	(0.0269)
Current expenditure	0.0005	0.0064	0.0088
	(0.0031)	(0.0047)	(0.0120)
Capital expenditure	0.0192	0.1127***	0.3027***
	(0.0300)	(0.0378)	(0.0784)
Panel B. Never-treated municipalities as controls			
Total revenue	0.0253***	0.0487***	0.0707***
	(0.0073)	(0.0108)	(0.0251)
Total expenditure	0.0132*	0.0393***	0.0875***
	(0.0075)	(0.0112)	(0.0269)
Current expenditure	0.0004	0.0062	0.0088
	(0.0031)	(0.0047)	(0.0120)
Capital expenditure	0.0227	0.1144***	0.3027***
	(0.0296)	(0.0379)	(0.0784)

Note. The table reports averages of the event-study coefficients over three broader post-treatment windows: short run t and $t+1$), medium horizon $t+2$ and $t+3$), and longer horizon $t+4$ and $t+5$). Panel A uses the baseline control group, consisting of municipalities untreated at the relevant comparison date, including both never-treated and not-yet-treated units. Panel B restricts the control group to never-treated municipalities. Estimates are obtained within the Callaway-Sant'Anna framework using the improved doubly robust estimator. Standard errors in parentheses are computed using lincom after estat event. Statistical significance is indicated as follows: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.The table should be read jointly with Table B.3, which reports the underlying event-time support

C Robustness Tests

C.1 Alternative Estimators

Table C.1 Total expenditure: alternative estimators (municipalities untreated at the comparison date as controls)

	Improved doubly robust DiD estimator based on inv. prob. of tilting and weighted least squares (no covariates)	Improved doubly robust DiD estimator based on inv. prob. of tilting and weighted least squares (with covariates)	Doubly robust DiD based on inverse probability weighting and OLS	Outcome regression DiD estimator based on OLS	Inverse probability weighting DiD estimator with stabilised weights
ATT (*t*-4)	0.0080	0.0131	0.0128	0.0063	0.0132
	(0.0190)	(0.0206)	(0.0374)	(0.0196)	(0.0375)
ATT (*t*-3)	−0.0001	−0.0134	−0.0128	−0.0131	−0.0126
	(0.0116)	(0.0135)	(0.0180)	(0.0126)	(0.0180)
ATT (*t*-2)	−0.0063	−0.0064	−0.0062	−0.0050	−0.0053
	(0.0080)	(0.0086)	(0.0102)	(0.0084)	(0.0103)
ATT (*t*-1)	0.0114	0.0004	0.0005	0.0010	0.0003
	(0.0073)	(0.0078)	(0.0094)	(0.0076)	(0.0094)
ATT (t)	0.0036	0.0064	0.0062	0.0069	0.0058
	(0.0073)	(0.0079)	(0.0112)	(0.0078)	(0.0112)
ATT (*t*+1)	0.0238***	0.0165*	0.0168	0.0180*	0.0159
	(0.0087)	(0.0094)	(0.0128)	(0.0093)	(0.0129)
ATT (*t*+2)	0.0375***	0.0211*	0.0215	0.0258**	0.0212
	(0.0105)	(0.0111)	(0.0136)	(0.0109)	(0.0136)
ATT (*t*+3)	0.0507***	0.0572***	0.0568***	0.0596***	0.0576***
	(0.0129)	(0.0136)	(0.0196)	(0.0134)	(0.0196)

Table C1 (continued)

	Improved doubly robust DiD estimator based on inv. prob. of tilting and weighted least squares (no covariates)	Improved doubly robust DiD estimator based on inv. prob. of tilting and weighted least squares (with covariates)	Doubly robust DiD based on inverse probability weighting and OLS	Outcome regression DiD estimator based on OLS	Inverse probability weighting DiD estimator with stabilised weights
ATT (t+4)	0.0426**	0.0781***	0.0777**	0.0956***	0.0782**
	(0.0200)	(0.0226)	(0.0391)	(0.0218)	(0.0392)
ATT (t+5)	0.0412	0.0968***	0.0984	0.0962***	0.0958
	(0.0313)	(0.0364)	(0.0690)	(0.0335)	(0.0694)
ATT (average)	0.0289***	0.0304***	0.0304**	0.0336***	0.0301**
	(0.0084)	(0.0091)	(0.0138)	(0.0089)	(0.0139)
Test on pre-trend (Chi sq.)	16.0808	12.2015	9.1285	14.5930	8.0094
p-value	0.0973	0.2718	0.5200	0.1476	0.6279
No. of observations	28,282	27,157	27,157	27,157	27,157

Note. The dependent variable is log total expenditure. Standard errors are reported in parentheses. Significance levels are denoted as follows: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. The table compares alternative estimators within the Callaway-Sant'Anna framework. Column (1) reports the improved doubly robust estimator without covariates. Column (2) reports the improved doubly robust estimator with the baseline covariate set and corresponds to the main specification. Column (3) reports the doubly robust estimator based on inverse probability weighting and OLS. Column (4) reports the outcome-regression estimator. Column (5) reports the inverse probability weighting estimator with stabilised weights. The control group consists of municipalities untreated at the relevant comparison date, including both never-treated and not-yet-treated units

Table C.2 Total expenditure: alternative estimators (never-treated municipalities as controls)

	Improved doubly robust DiD estimator based on inv. prob. of tilting and weighted least squares (no covariates)	Improved doubly robust DiD estimator based on inv. prob. of tilting and weighted least squares (with covariates)	Doubly robust DiD based on inverse probability weighting and OLS	Outcome regression DiD estimator based on OLS	Inverse probability weighting DiD estimator with stabilised weights
ATT (*t*-4)	0.0080	0.0131	0.0128	0.0063	0.0132
	(0.0190)	(0.0206)	(0.0374)	(0.0196)	(0.0375)
ATT (*t*-3)	0.0001	−0.0129	−0.0121	−0.0124	−0.0120
	(0.0115)	(0.0132)	(0.0178)	(0.0124)	(0.0178)
ATT (*t*-2)	−0.0058	−0.0061	−0.0057	−0.0052	−0.0045
	(0.0080)	(0.0085)	(0.0101)	(0.0083)	(0.0102)
ATT (*t*-1)	0.0109	−0.0011	−0.0012	−0.0014	−0.0014
	(0.0072)	(0.0077)	(0.0093)	(0.0075)	(0.0093)
ATT (t)	0.0053	0.0082	0.0080	0.0083	0.0078
	(0.0072)	(0.0079)	(0.0107)	(0.0078)	(0.0107)
ATT (*t*+1)	0.0243***	0.0182*	0.0184	0.0193**	0.0178
	(0.0086)	(0.0093)	(0.0121)	(0.0092)	(0.0122)
ATT (*t*+2)	0.0385***	0.0214*	0.0218*	0.0263**	0.0216
	(0.0105)	(0.0111)	(0.0132)	(0.0109)	(0.0133)
ATT (*t*+3)	0.0507***	0.0572***	0.0568***	0.0596***	0.0576***
	(0.0130)	(0.0136)	(0.0190)	(0.0134)	(0.0190)
ATT (*t*+4)	0.0426**	0.0781***	0.0777**	0.0956***	0.0782**
	(0.0200)	(0.0226)	(0.0391)	(0.0218)	(0.0392)
ATT (*t*+5)	0.0412	0.0968***	0.0984	0.0962***	0.0958
	(0.0313)	(0.0364)	(0.0690)	(0.0335)	(0.0694)
ATT (average)	0.0296***	0.0312***	0.0313**	0.0343***	0.0311**
	(0.0084)	(0.0091)	(0.0134)	(0.0089)	(0.0135)
Test on pre-trend (Chi sq.)	16.0813	11.9445	8.7118	14.8874	7.7128
p-value	0.0973	0.2888	0.5597	0.1362	0.6569
No. of observations	28,272	27,147	27,147	27,147	27,147

Note. The dependent variable is log total expenditure. Standard errors are reported in parentheses. Significance levels are denoted as follows: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. The table compares alternative estimators within the Callaway-Sant'Anna framework. Column (1) reports the improved doubly robust estimator without covariates. Column (2) reports the improved doubly robust estimator with the baseline covariate set. Column (3) reports the doubly robust estimator based on inverse probability weighting and OLS. Column (4) reports the outcome-regression estimator. Column (5) reports the inverse probability weighting estimator with stabilised weights. The control group is restricted to never-treated municipalities

Table C.3 Total revenue: alternative estimators (municipalities untreated at the comparison date as controls)

	Improved doubly robust DiD estimator based on inv. prob. of tilting and weighted least squares (no covariates)	Improved doubly robust DiD estimator based on inv. prob. of tilting and weighted least squares (with covariates)	Doubly robust DiD based on inverse probability weighting and OLS	Outcome regression DiD estimator based on OLS	Inverse probability weighting DiD estimator with stabilised weights
ATT (*t*-4)	0.0012	0.0078	0.0076	−0.0085	0.0074
	(0.0188)	(0.0201)	(0.0369)	(0.0195)	(0.0369)
ATT (*t*-3)	0.0085	−0.0014	−0.0008	0.0039	−0.0000
	(0.0122)	(0.0140)	(0.0185)	(0.0130)	(0.0186)
ATT (*t*-2)	0.0050	0.0069	0.0073	0.0072	0.0084
	(0.0076)	(0.0083)	(0.0097)	(0.0080)	(0.0098)
ATT (*t*-1)	−0.0009	−0.0104	−0.0102	−0.0096	−0.0108
	(0.0072)	(0.0078)	(0.0092)	(0.0076)	(0.0093)
ATT (t)	0.0079	0.0149*	0.0144	0.0152*	0.0142
	(0.0074)	(0.0079)	(0.0111)	(0.0078)	(0.0112)
ATT (*t*+1)	0.0472***	0.0336***	0.0335***	0.0374***	0.0325***
	(0.0084)	(0.0090)	(0.0124)	(0.0089)	(0.0125)
ATT (*t*+2)	0.0529***	0.0404***	0.0404***	0.0433***	0.0400***
	(0.0104)	(0.0108)	(0.0134)	(0.0107)	(0.0135)
ATT (*t*+3)	0.0497***	0.0566***	0.0561***	0.0623***	0.0570***
	(0.0127)	(0.0132)	(0.0185)	(0.0130)	(0.0185)
ATT (*t*+4)	0.0220	0.0568***	0.0562	0.0760***	0.0568
	(0.0195)	(0.0219)	(0.0375)	(0.0212)	(0.0376)
ATT (*t*+5)	0.0316	0.0846**	0.0857	0.0823**	0.0838
	(0.0295)	(0.0340)	(0.0646)	(0.0320)	(0.0649)
ATT (average)	0.0368***	0.0386***	0.0383***	0.0425***	0.0381***
	(0.0082)	(0.0087)	(0.0131)	(0.0085)	(0.0131)
Test on pre-trend (Chi sq.)	17.4077	14.8377	7.2033	18.5653	6.6558
p-value	0.0658	0.1381	0.7061	0.0461	0.7575
No. of observations	28,289	27,164	27,164	27,164	27,164

Note. The dependent variable is log total revenue. Standard errors are reported in parentheses. Significance levels are denoted as follows: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. The table compares alternative estimators within the Callaway-Sant'Anna framework. Column (1) reports the improved doubly robust estimator without covariates. Column (2) reports the improved doubly robust estimator with the baseline covariate set and corresponds to the main specification. Column (3) reports the doubly robust estimator based on inverse probability weighting and OLS. Column (4) reports the outcome-regression estimator. Column (5) reports the inverse probability weighting estimator with stabilised weights. The control group consists of municipalities untreated at the relevant comparison date, including both never-treated and not-yet-treated units

Table C.4 Total revenue: alternative estimators (never-treated municipalities as controls)

	Improved doubly robust DiD estimator based on inv. prob. of tilting and weighted least squares (no covariates)	Improved doubly robust DiD estimator based on inv. prob. of tilting and weighted least squares (with covariates)	Doubly robust DiD based on inverse probability weighting and OLS	Outcome regression DiD estimator based on OLS	Inverse probability weighting DiD estimator with stabilised weights
ATT (*t*-4)	0.0012	0.0078	0.0076	−0.0085	0.0074
	(0.0188)	(0.0201)	(0.0369)	(0.0195)	(0.0369)
ATT (*t*-3)	0.0085	−0.0020	−0.0016	0.0042	−0.0009
	(0.0121)	(0.0136)	(0.0182)	(0.0128)	(0.0182)
ATT (*t*-2)	0.0061	0.0075	0.0081	0.0071	0.0094
	(0.0076)	(0.0083)	(0.0098)	(0.0080)	(0.0099)
ATT (*t*-1)	−0.0015	−0.0091	−0.0087	−0.0097	−0.0093
	(0.0072)	(0.0077)	(0.0092)	(0.0075)	(0.0092)
ATT (t)	0.0095	0.0150*	0.0144	0.0160**	0.0143
	(0.0073)	(0.0079)	(0.0106)	(0.0078)	(0.0106)
ATT (*t*+1)	0.0475***	0.0356***	0.0355***	0.0384***	0.0347***
	(0.0084)	(0.0089)	(0.0118)	(0.0088)	(0.0118)
ATT (*t*+2)	0.0536***	0.0407***	0.0408***	0.0438***	0.0404***
	(0.0105)	(0.0108)	(0.0131)	(0.0107)	(0.0131)
ATT (*t*+3)	0.0496***	0.0566***	0.0560***	0.0620***	0.0570***
	(0.0127)	(0.0132)	(0.0180)	(0.0130)	(0.0180)
ATT (*t*+4)	0.0220	0.0568**	0.0562	0.0760***	0.0568
	(0.0195)	(0.0219)	(0.0375)	(0.0212)	(0.0376)
ATT (*t*+5)	0.0316	0.0846**	0.0857	0.0823**	0.0838
	(0.0295)	(0.0340)	(0.0646)	(0.0320)	(0.0649)
ATT (average)	0.0374***	0.0392***	0.0389***	0.0430***	0.0387***
	(0.0082)	(0.0087)	(0.0127)	(0.0086)	(0.0127)
Test on pre-trend (Chi sq.)	17.4065	14.9061	7.3747	18.9188	6.9159
p-value	0.0658	0.1355	0.6897	0.0413	0.7334
No. of observations	28,279	27,154	27,154	27,154	27,154

Note. The dependent variable is log total revenue. Standard errors are reported in parentheses. Significance levels are denoted as follows: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. The table compares alternative estimators within the Callaway-Sant'Anna framework. Column (1) reports the improved doubly robust estimator without covariates. Column (2) reports the improved doubly robust estimator with the baseline covariate set. Column (3) reports the doubly robust estimator based on inverse probability weighting and OLS. Column (4) reports the outcome-regression estimator. Column (5) reports the inverse probability weighting estimator with stabilised weights. The control group is restricted to never-treated municipalities

Table C.5 Current expenditure: alternative estimators (municipalities untreated at the comparison date as controls)

	Improved doubly robust DiD estimator based on inv. prob. of tilting and weighted least squares (no covariates)	Improved doubly robust DiD estimator based on inv. prob. of tilting and weighted least squares (with covariates)	Doubly robust DiD based on inverse probability weighting and OLS	Outcome regression DiD estimator based on OLS	Inverse probability weighting DiD estimator with stabilised weights
ATT (t-4)	−0.0081	0.0110	0.0104	0.0029	0.0106
	(0.0069)	(0.0095)	(0.0147)	(0.0078)	(0.0148)
ATT (t-3)	−0.0160***	−0.0097	−0.0095	−0.0086	−0.0097
	(0.0051)	(0.0054)	(0.0077)	(0.0053)	(0.0076)
ATT (t-2)	−0.0057	−0.0055	−0.0052	−0.0065*	−0.0051
	(0.0031)	(0.0034)	(0.0040)	(0.0034)	(0.0040)
ATT (t-1)	−0.0012	−0.0011	−0.0013	−0.0007	−0.0012
	(0.0028)	(0.0030)	(0.0035)	(0.0029)	(0.0035)
ATT (t)	−0.0042	−0.0026	−0.0027	−0.0030	−0.0027
	(0.0029)	(0.0032)	(0.0045)	(0.0031)	(0.0045)
ATT (t+1)	0.0078**	0.0036	0.0038	0.0033	0.0036
	(0.0035)	(0.0038)	(0.0051)	(0.0037)	(0.0051)
ATT (t+2)	0.0074*	0.0017	0.0018	0.0035	0.0017
	(0.0045)	(0.0048)	(0.0063)	(0.0048)	(0.0063)
ATT (t+3)	0.0174***	0.0111**	0.0112	0.0136**	0.0107
	(0.0053)	(0.0056)	(0.0085)	(0.0056)	(0.0085)
ATT (t+4)	0.0299***	0.0180*	0.0187	0.0241**	0.0183
	(0.0091)	(0.0101)	(0.0177)	(0.0099)	(0.0178)
ATT (t+5)	0.0115	−0.0004	−0.0007	0.0071	−0.0016
	(0.0139)	(0.0159)	(0.0313)	(0.0154)	(0.0314)
ATT (average)	0.0084**	0.0039	0.0040	0.0054	0.0038
	(0.0037)	(0.0039)	(0.0061)	(0.0039)	(0.0062)
Test on pre-trend (Chi sq.)	31.0512	21.1325	13.2465	20.6585	12.8803
p-value	0.0006	0.0202	0.2102	0.0236	0.2304
No. of observations	28,289	27,164	27,164	27,164	27,164

Note. The dependent variable is log current expenditure. Standard errors are reported in parentheses. Significance levels are denoted as follows: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. The table compares alternative estimators within the Callaway-Sant'Anna framework. Column (1) reports the improved doubly robust estimator without covariates. Column (2) reports the improved doubly robust estimator with the baseline covariate set and corresponds to the main specification. Column (3) reports the doubly robust estimator based on inverse probability weighting and OLS. Column (4) reports the outcome-regression estimator. Column (5) reports the inverse probability weighting estimator with stabilised weights. The control group consists of municipalities untreated at the relevant comparison date, including both never-treated and not-yet-treated units

Table C.6 Current expenditure: alternative estimators (never-treated municipalities as controls)

	Improved doubly robust DiD estimator based on inv. prob. of tilting and weighted least squares (no covariates)	Improved doubly robust DiD estimator based on inv. prob. of tilting and weighted least squares (with covariates)	Doubly robust DiD based on inverse probability weighting and OLS	Outcome regression DiD estimator based on OLS	Inverse probability weighting DiD estimator with stabilised weights
ATT (*t*-4)	−0.0081	0.0110	0.0104	0.0029	0.0106
	(0.0069)	(0.0095)	(0.0147)	(0.0078)	(0.0148)
ATT (*t*-3)	−0.0162***	−0.0094*	−0.0091	−0.0087	−0.0093
	(0.0051)	(0.0054)	(0.0077)	(0.0054)	(0.0076)
ATT (*t*-2)	−0.0067**	−0.0058*	−0.0055	−0.0068**	−0.0053
	(0.0031)	(0.0034)	(0.0040)	(0.0034)	(0.0040)
ATT (*t*-1)	−0.0023	−0.0016	−0.0020	−0.0015	−0.0017
	(0.0028)	(0.0031)	(0.0035)	(0.0029)	(0.0035)
ATT (t)	−0.0048	−0.0029	−0.0030	−0.0031	−0.0030
	(0.0029)	(0.0032)	(0.0043)	(0.0031)	(0.0044)
ATT (*t*+1)	0.0068*	0.0037	0.0039	0.0034	0.0037
	(0.0035)	(0.0038)	(0.0049)	(0.0037)	(0.0049)
ATT (*t*+2)	0.0068	0.0014	0.0015	0.0032	0.0014
	(0.0045)	(0.0048)	(0.0061)	(0.0047)	(0.0061)
ATT (*t*+3)	0.0168***	0.0111**	0.0112	0.0135**	0.0107
	(0.0053)	(0.0056)	(0.0082)	(0.0056)	(0.0082)
ATT (*t*+4)	0.0299***	0.0180*	0.0187	0.0241**	0.0183
	(0.0091)	(0.0101)	(0.0177)	(0.0099)	(0.0178)
ATT (*t*+5)	0.0115	−0.0004	−0.0007	0.0071	−0.0016
	(0.0139)	(0.0159)	(0.0313)	(0.0154)	(0.0314)
ATT (average)	0.0078**	0.0038	0.0039	0.0053	0.0037
	(0.0037)	(0.0039)	(0.0060)	(0.0039)	(0.0060)
Test on pre-trend (Chi sq.)	31.0535	24.1478	15.6160	22.1229	14.6417
p-value	0.0006	0.0072	0.1112	0.0145	0.1457
No. of observations	28,279	27,154	27,154	27,154	27,154

Note. The dependent variable is log current expenditure. Standard errors are reported in parentheses. Significance levels are denoted as follows: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. The table compares alternative estimators within the Callaway-Sant'Anna framework. Column (1) reports the improved doubly robust estimator without covariates. Column (2) reports the improved doubly robust estimator with the baseline covariate set. Column (3) reports the doubly robust estimator based on inverse probability weighting and OLS. Column (4) reports the outcome-regression estimator. Column (5) reports the inverse probability weighting estimator with stabilised weights. The control group is restricted to never-treated municipalities

Table C.7 Capital expenditure: alternative estimators (municipalities untreated at the comparison date as controls)

	Improved doubly robust DiD estimator based on inv. prob. of tilting and weighted least squares (no covariates)	Improved doubly robust DiD estimator based on inv. prob. of tilting and weighted least squares (with covariates)	Doubly robust DiD based on inverse probability weighting and OLS	Outcome regression DiD estimator based on OLS	Inverse probability weighting DiD estimator with stabilised weights
ATT (*t*-4)	0.0042	−0.0278	−0.0285	−0.0221	−0.0290
	(0.0971)	(0.1080)	(0.1988)	(0.1042)	(0.1986)
ATT (*t*-3)	0.0549	−0.0838	−0.0839	−0.0836	−0.0857
	(0.0664)	(0.0727)	(0.1047)	(0.0710)	(0.1046)
ATT (*t*-2)	−0.0398	−0.0410	−0.0404	−0.0311	−0.0389
	(0.0412)	(0.0435)	(0.0499)	(0.0433)	(0.0501)
ATT (*t*-1)	0.0379	0.0282	0.0276	0.0226	0.0276
	(0.0353)	(0.0377)	(0.0433)	(0.0372)	(0.0434)
ATT (t)	0.0173	0.0112	0.0108	0.0115	0.0084
	(0.0307)	(0.0330)	(0.0463)	(0.0325)	(0.0464)
ATT (*t*+1)	0.0190	0.0273	0.0285	0.0309	0.0270
	(0.0324)	(0.0352)	(0.0478)	(0.0346)	(0.0478)
ATT (*t*+2)	0.0728**	0.0685*	0.0715	0.0756**	0.0717
	(0.0358)	(0.0377)	(0.0451)	(0.0374)	(0.0452)
ATT (*t*+3)	0.1095**	0.1568***	0.1577**	0.1511***	0.1575**
	(0.0428)	(0.0449)	(0.0631)	(0.0443)	(0.0632)
ATT (*t*+4)	−0.0224	0.2021***	0.2010*	0.2309***	0.2045*
	(0.0622)	(0.0685)	(0.1194)	(0.0667)	(0.1197)
ATT (*t*+5)	0.2598***	0.4032***	0.4055**	0.3880***	0.4045**
	(0.0906)	(0.1014)	(0.1939)	(0.0957)	(0.1943)
ATT (average)	0.0534*	0.0846***	0.0856*	0.0876***	0.0849*
	(0.0305)	(0.0324)	(0.0477)	(0.0318)	(0.0477)
Test on pre-trend (Chi sq.)	12.1409	11.5712	7.8878	18.3702	7.6850
p-value	0.2757	0.3148	0.6398	0.0490	0.6596
No. of observations	28,189	27,065	27,065	27,065	27,065

Note. The dependent variable is log capital expenditure. Standard errors are reported in parentheses. Significance levels are denoted as follows: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. The table compares alternative estimators within the Callaway-Sant'Anna framework. Column (1) reports the improved doubly robust estimator without covariates. Column (2) reports the improved doubly robust estimator with the baseline covariate set and corresponds to the main specification. Column (3) reports the doubly robust estimator based on inverse probability weighting and OLS. Column (4) reports the outcome-regression estimator. Column (5) reports the inverse probability weighting estimator with stabilised weights. The control group consists of municipalities untreated at the relevant comparison date, including both never-treated and not-yet-treated units

Table C.8 Capital expenditure: alternative estimators (never-treated municipalities as controls)

	Improved doubly robust DiD estimator based on inv. prob. of tilting and weighted least squares (no covariates)	Improved doubly robust DiD estimator based on inv. prob. of tilting and weighted least squares (with covariates)	Doubly robust DiD based on inverse probability weighting and OLS	Outcome regression DiD estimator based on OLS	Inverse probability weighting DiD estimator with stabilised weights
ATT (*t*-4)	0.0042	−0.0278	−0.0285	−0.0221	−0.0290
	(0.0971)	(0.1080)	(0.1988)	(0.1042)	(0.1986)
ATT (*t*-3)	0.0550	−0.0838	−0.0814	−0.0910	−0.0834
	(0.0657)	(0.0728)	(0.1050)	(0.0704)	(0.1050)
ATT (*t*-2)	−0.0362	−0.0493	−0.0479	−0.0432	−0.0457
	(0.0405)	(0.0427)	(0.0490)	(0.0424)	(0.0492)
ATT (*t*-1)	0.0368	0.0182	0.0166	0.0105	0.0167
	(0.0342)	(0.0369)	(0.0425)	(0.0364)	(0.0427)
ATT (t)	0.0228	0.0123	0.0121	0.0144	0.0100
	(0.0302)	(0.0329)	(0.0435)	(0.0324)	(0.0435)
ATT (*t*+1)	0.0240	0.0331	0.0340	0.0395	0.0332
	(0.0318)	(0.0346)	(0.0446)	(0.0339)	(0.0447)
ATT (*t*+2)	0.0794**	0.0720*	0.0750*	0.0806**	0.0756*
	(0.0358)	(0.0377)	(0.0441)	(0.0374)	(0.0442)
ATT (*t*+3)	0.1120***	0.1568***	0.1578**	0.1520***	0.1575**
	(0.0429)	(0.0449)	(0.0612)	(0.0444)	(0.0613)
ATT (*t*+4)	−0.0224	0.2021***	0.2010*	0.2309***	0.2045*
	(0.0622)	(0.0685)	(0.1194)	(0.0667)	(0.1197)
ATT (*t*+5)	0.2598***	0.4032***	0.4055**	0.3880***	0.4045**
	(0.0906)	(0.1014)	(0.1939)	(0.0957)	(0.1943)
ATT (average)	0.0578*	0.0871***	0.0881*	0.0917***	0.0877*
	(0.0303)	(0.0324)	(0.0460)	(0.0317)	(0.0460)
Test on pre-trend (Chi sq.)	12.1411	11.4202	7.8401	18.8898	7.6633
p-value	0.2757	0.3257	0.6445	0.0417	0.6617
No. of observations	28,175	27,051	27,051	27,051	27,051

Note. The dependent variable is log capital expenditure. Standard errors are reported in parentheses. Significance levels are denoted as follows: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. The table compares alternative estimators within the Callaway-Sant'Anna framework. Column (1) reports the improved doubly robust estimator without covariates. Column (2) reports the improved doubly robust estimator with the baseline covariate set. Column (3) reports the doubly robust estimator based on inverse probability weighting and OLS. Column (4) reports the outcome-regression estimator. Column (5) reports the inverse probability weighting estimator with stabilised weights. The control group is restricted to never-treated municipalities

C.2 Jackknife

C.2.1 Event-Based

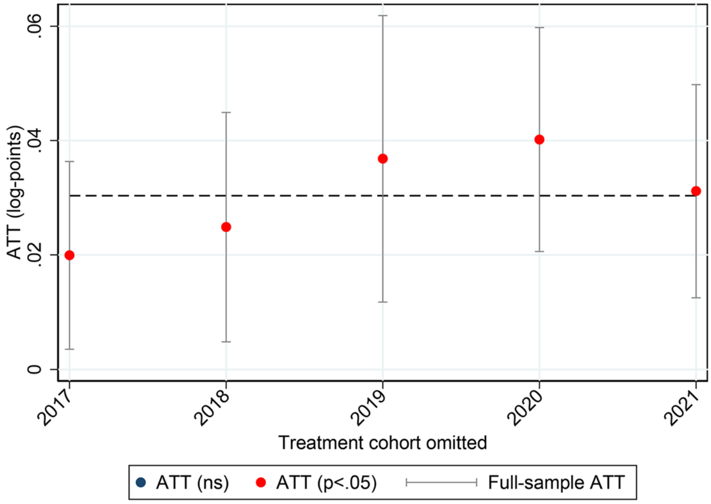

Fig. C.1 Event-based jackknife sensitivity: total expenditure. Note. Each dot reports the estimated ATT obtained after excluding one treatment-year cohort from the baseline sample and re-estimating the model. Vertical bars denote 95% confidence intervals. The dashed line marks the full-sample ATT estimate. Red dots indicate estimates statistically different from zero at the 5% level

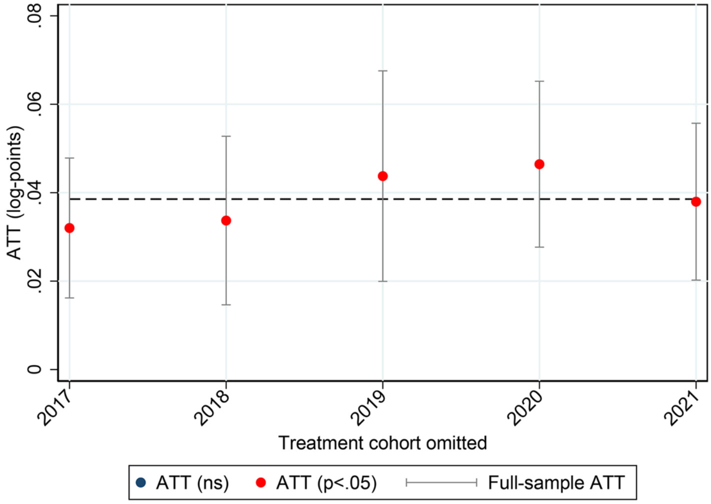

Fig. C.2 Event-based jackknife sensitivity: total revenue. Note. Each dot reports the estimated ATT obtained after excluding one treatment-year cohort from the baseline sample and re-estimating the model. Vertical bars denote 95% confidence intervals. The dashed line marks the full-sample ATT estimate. Red dots indicate estimates statistically different from zero at the 5% level

C.2.2 Region-Based

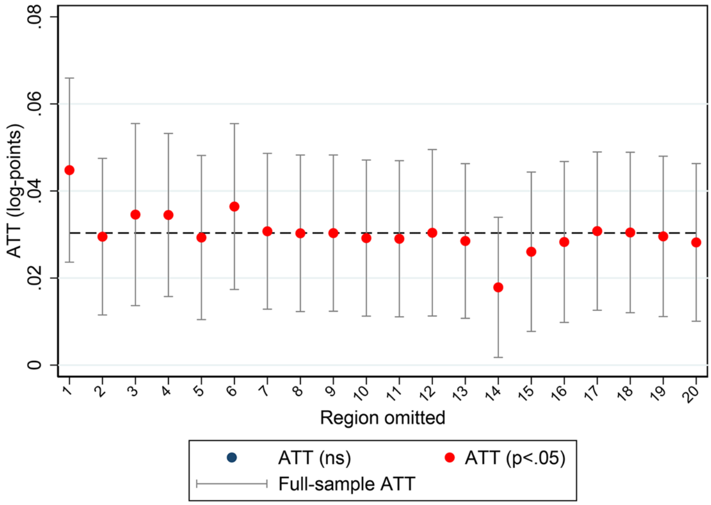

Fig. C.3 Region-based jackknife sensitivity: total expenditure. Note. Each dot reports the estimated average post-treatment effect obtained by re-estimating the baseline specification after excluding one region at a time from the sample. Vertical bars denote 95% confidence intervals. The dashed line marks the corresponding full-sample estimate. Red dots mark estimates that remain significant at the 5% level

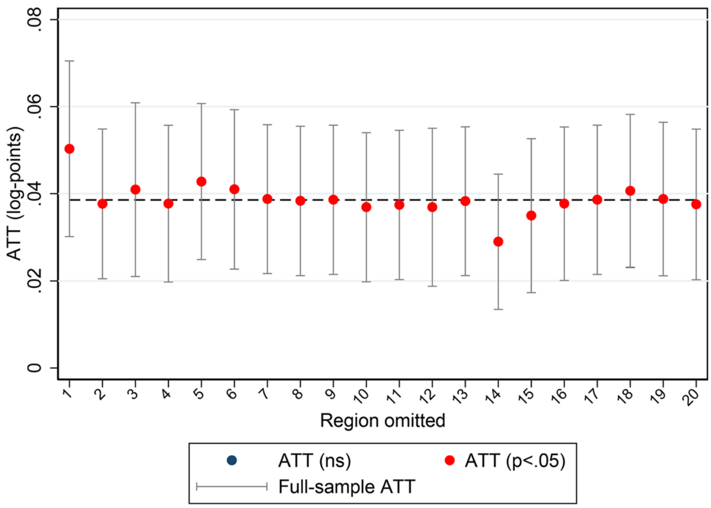

Fig. C.4 Region-based jackknife sensitivity: total revenue. Note. Each dot reports the estimated average post-treatment effect obtained by re-estimating the baseline specification after excluding one region at a time from the sample. Vertical bars denote 95% confidence intervals. The dashed line marks the corresponding full-sample estimate. Red dots mark estimates that remain significant at the 5% level

C.2.3 Spatial Inference at NUTS2-Region and SLL Levels

The leave-one-region-out exercise above evaluates whether the estimated point effects are driven by any single region. It does not, however, address correlation in the error structure within broader spatial units. We therefore complement it with formal spatial inference for the baseline aggregated post-treatment effects, reported in Table C.9. The estimand, sample, treatment timing, controls, and improved doubly robust estimator are unchanged relative to the baseline. Only the level and procedure used for inference vary. Inference is clustered at two spatial levels, namely NUTS2 regions, which capture broad regional institutional and fiscal dependence, and SLL local labour market areas, which reflect local economic linkages in tax bases and demand for municipal works. For the NUTS2 specification, we also add a delete-one-region block jackknife.

The point estimates are identical to the baseline, so the exercise concerns the uncertainty attached to the estimates rather than their stability. The total-revenue response remains statistically significant under both spatial clustering levels. Total expenditure is positive and statistically significant under SLL inference, but not under NUTS2-region inference. Current expenditure remains close to zero throughout. Capital expenditure is positive, with an SLL wild-bootstrap interval that excludes zero and a NUTS2-region interval that does not. The revenue response is therefore robust across spatial clustering levels, whereas the expenditure responses are more sensitive to the chosen clustering level.

Table C.9 Spatial inference for baseline aggregated effects

Outcome	Level	ATT	Muni. s.e.	Cluster s.e.	Wild CI	Jackknife CI
Total revenue	NUTS2	0.039	(0.009)	(0.012)	[−0.017, −0.060]	[−0.004, −0.073]
Total revenue	SLL	0.039	(0.009)	(0.009)	[−0.020, −0.058]	–
Total expenditure	NUTS2	0.030	(0.009)	(0.017)	[−0.001, −0.062]	[−0.014, −0.075]
Total expenditure	SLL	0.030	(0.009)	(0.011)	[−0.008, −0.053]	–
Current expenditure	NUTS2	0.004	(0.004)	(0.008)	[−0.010, −0.018]	[−0.018, −0.026]
Current expenditure	SLL	0.004	(0.004)	(0.005)	[−0.007, −0.015]	–
Capital expenditure	NUTS2	0.085	(0.032)	(0.063)	[−0.040, −0.209]	[−0.055, −0.225]
Capital expenditure	SLL	0.085	(0.032)	(0.041)	[−0.005, −0.165]	–

Note. The table reports the baseline aggregated post-treatment effects for the four main fiscal outcomes. Estimates come from the same Callaway-Sant'Anna improved doubly robust specification used in the baseline analysis. Column "Muni. s.e." reports the baseline municipality-level standard error. Column "Cluster s.e" reports the standard error clustered at the spatial level shown in the table. Column "Wild CI" reports the 95% confidence interval from the corresponding wild cluster bootstrap with Rademacher weights (Cameron et al. 2008). Column "Jackknife CI" reports the 95% confidence interval from a delete-one-region block jackknife and is reported only for the NUTS2 specification. NUTS2 inference uses 20 regions. SLL 2011 inference clusters observations by ISTAT's 2011 local labour market areas *Sistemi Locali del Lavoro*), with 446 clusters in the estimating sample

C.2.4 Short- and Medium-Run Spatial Inference

To assess whether the interpretation of the dynamic effects depends on the farthest event-time horizons, Table C.10 reports censored event-time averages over the post-treatment windows 0–2 and 0–3. The estimates rely on the same Callaway-Sant'Anna improved doubly robust specification as the baseline and on the two spatial clustering levels introduced above. Reporting both windows avoids selecting a single favourable horizon and keeps the focus on the short- to medium-run adjustment.

The revenue response remains statistically significant over both windows and at both spatial levels. For total expenditure, the 0–3 average is positive and statistically significant under SLL inference, while the corresponding NUTS2-region confidence interval includes zero. Current expenditure remains close to zero. Capital expenditure is positive, especially over the 0–3 window, but is not statistically significant under either spatial clustering level, consistent with the more heterogeneous nature of capital outlays.

Table C.10 Short- and medium-run spatial inference

Outcome	Window	Level	ATT	Cluster s.e.	Wild CI
Total revenue	0–2	NUTS2	0.029	(0.010)	[−0.012, −0.047]
Total revenue	0–3	NUTS2	0.035	(0.011)	[−0.016, −0.053]
Total revenue	0–2	SLL	0.029	(0.008)	[−0.014, −0.044]
Total revenue	0–3	SLL	0.035	(0.008)	[−0.018, −0.052]
Total expenditure	0–2	NUTS2	0.014	(0.014)	[−0.011, −0.040]
Total expenditure	0–3	NUTS2	0.023	(0.015)	[−0.004, −0.051]
Total expenditure	0–2	SLL	0.014	(0.009)	[−0.003, −0.032]
Total expenditure	0–3	SLL	0.023	(0.010)	[−0.004, −0.042]
Current expenditure	0–2	NUTS2	0.001	(0.005)	[−0.009, −0.011]
Current expenditure	0–3	NUTS2	0.003	(0.006)	[−0.008, −0.014]
Current expenditure	0–2	SLL	0.001	(0.004)	[−0.007, −0.009]
Current expenditure	0–3	SLL	0.003	(0.004)	[−0.005, −0.011]
Capital expenditure	0–2	NUTS2	0.035	(0.053)	[−0.071, −0.140]
Capital expenditure	0–3	NUTS2	0.059	(0.058)	[−0.056, −0.174]
Capital expenditure	0–2	SLL	0.035	(0.035)	[−0.035, −0.104]
Capital expenditure	0–3	SLL	0.059	(0.038)	[−0.015, −0.133]

Note. The table reports censored event-time averages over the indicated post-treatment windows, event years 0–2 and 0–3. Estimates come from the same Callaway-Sant'Anna improved doubly robust specification used in the baseline analysis. Column "Cluster s.e." reports the standard error clustered at the spatial level shown in the table. Column "Wild CI" reports the 95% confidence interval from the corresponding wild cluster bootstrap with Rademacher weights (Cameron et al. 2008). NUTS2 inference uses 20 regions. SLL 2011 inference clusters observations by ISTAT's 2011 local labour market areas (Sistemi Locali del Lavoro), with 446 clusters in the estimating sample

C.3 Placebo

C.3.1 Post-Treatment Window

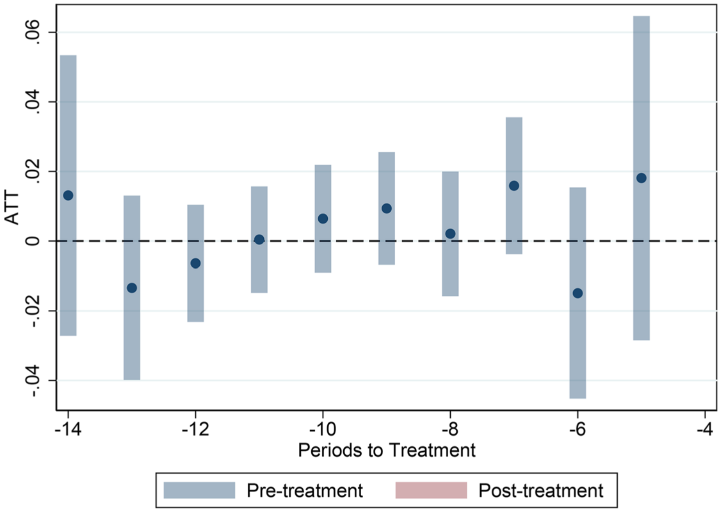

Fig. C.5 Post-window placebo: total expenditure. Note. This figure reports event-study coefficients from a placebo exercise in which observed treatment cohorts are shifted ten years forward, so that placebo treatment occurs outside the sample period. The specification is otherwise identical to the baseline one. Point estimates and 95% confidence intervals are displayed. The absence of a systematic pattern supports the interpretation that the baseline results are not driven by mechanical timing or spurious event-time dynamics

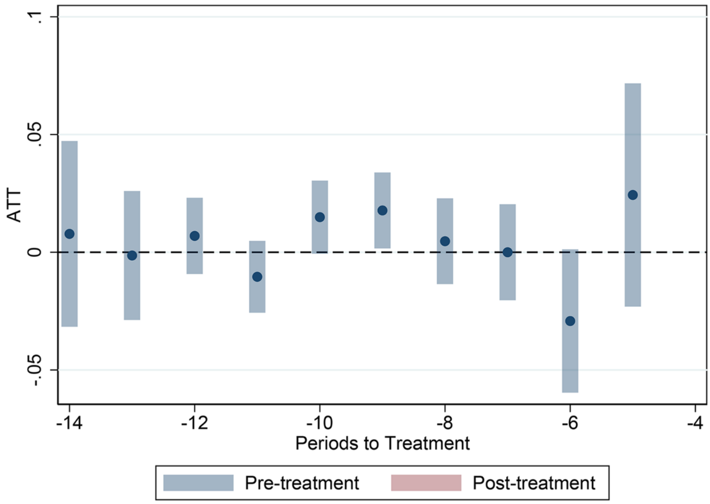

Fig. C.6 Post-window placebo: total revenue. Note. This figure reports event-study coefficients from a placebo exercise in which observed treatment cohorts are shifted ten years forward, so that placebo treatment occurs outside the sample period. The specification is otherwise identical to the baseline one. Point estimates and 95% confidence intervals are displayed. The absence of a systematic pattern supports the interpretation that the baseline results are not driven by mechanical timing or spurious event-time dynamics

C.3.2 Random-Year Treatment

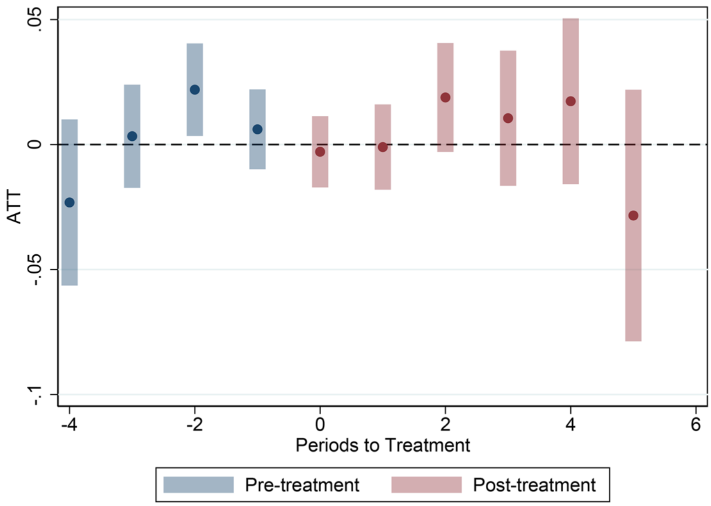

Fig. C.7 Random-year placebo: total expenditure. Note. This figure reports event-study coefficients for total municipal expenditure from a placebo exercise in which treated municipalities are randomly assigned a fake treatment year between 2017 and 2021, with the restriction that the assigned year differs from the municipality's actual treatment year. The specification is otherwise identical to the baseline one, replacing the true treatment timing with the random assignment. Point estimates and 95% confidence intervals are displayed

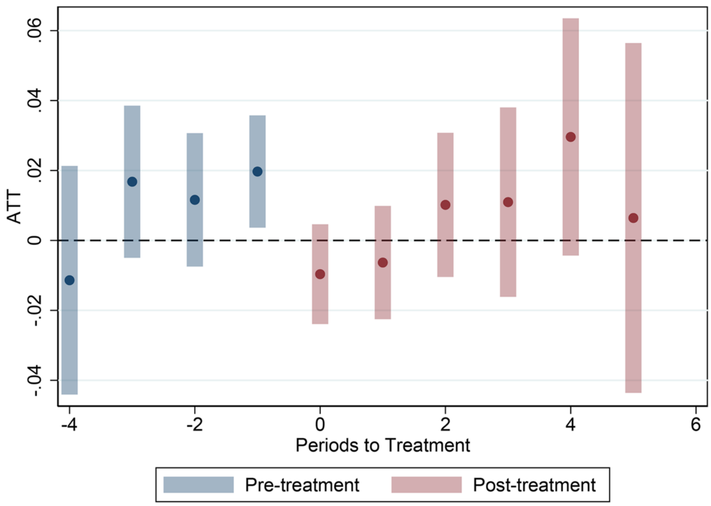

Fig. C.8 Random-year placebo: total revenue. Note. This figure reports event-study coefficients for total municipal revenue from a placebo exercise in which treated municipalities are randomly assigned a fake treatment year between 2017 and 2021, with the restriction that the assigned year differs from the municipality's actual treatment year. The specification is otherwise identical to the baseline one, replacing the true treatment timing with the random assignment. Point estimates and 95% confidence intervals are displayed

C.4 Pre-Pandemic Sample Restriction

As discussed in the main text, the farthest post-treatment horizons in the baseline event-study rely more heavily on observations from 2020 to 2022, a period in which municipal budgets were also shaped by the pandemic and exceptional fiscal conditions. To assess whether the main results are sensitive to this overlap, we re-estimate the baseline specification on a truncated pre-pandemic panel covering 2016–2019.

The exercise preserves the selected baseline sample used throughout the paper. We retain municipalities that are not always treated and are exposed at most once in the 2017–2022 window, but then restrict the estimation sample to years up to 2019. Within this truncated panel, municipalities first treated in 2020–2022 are treated as untreated, since their treatment occurs outside the estimation window. We then re-estimate the four main fiscal outcomes using the same baseline covariate set. Estimates are obtained within the Callaway-Sant'Anna framework using the improved doubly robust estimator. Panel A uses the baseline control group, consisting of municipalities untreated at the relevant comparison date, while Panel B restricts the control group to never-treated municipalities.

Table C.11 is broadly supportive of the baseline interpretation. The revenue response remains positive and statistically significant under both control-group definitions, and capital expenditure is still the largest estimated component of the adjustment. These two findings, central to interpreting the fiscal response as revenue-supported and investment-oriented, continue to hold in the truncated pre-pandemic panel. At the same time, the evidence is not equally strong across all outcomes, as expected given the shorter observation window. Total expenditure stays positive but loses statistical significance in the truncated sample. Current expenditure is small in magnitude and statistically insignificant, although the joint pre-trend test is rejected under both control-group definitions. Overall, the exercise indicates that the main fiscal pattern is not mechanically driven by the inclusion of the pandemic years, while also clarifying that the strongest support concerns the positive revenue response and the investment-oriented composition of the fiscal adjustment.

Table C.11 Pre-pandemic sample restriction (2016–2019)

Outcome	ATT	S.E.	p-value	Pre-trend p-value	No. of observations
Panel A. Municipalities untreated at the comparison date as controls					
Total revenue	0.0280**	(0.0110)	0.0105	0.4506	15,425
Total expenditure	0.0177	(0.0110)	0.1054	0.4308	15,421
Current expenditure	−0.0039	(0.0047)	0.4070	0.0249	15,425
Capital expenditure	0.0832*	(0.0432)	0.0543	0.3499	15,341
Panel B. Never-treated municipalities as controls					
Total revenue	0.0283**	(0.0110)	0.0101	0.4340	15,422
Total expenditure	0.0176	(0.0110)	0.1106	0.4244	15,418
Current expenditure	−0.0035	(0.0047)	0.4551	0.0082	15,422
Capital expenditure	0.0777*	(0.0434)	0.0736	0.3826	15,337

Note. The table re-estimates the baseline specification after truncating the panel at 2019. Municipalities first treated in 2020–2022 are treated as untreated within the truncated sample. Panel A uses the baseline control group, consisting of municipalities untreated at the relevant comparison date, including both never-treated and not-yet-treated units. Panel B restricts the control group to never-treated municipalities. Estimates are obtained within the Callaway-Sant'Anna framework using the improved doubly robust estimator and the same baseline covariate set used in the main analysis. The reported ATT is the average post-treatment effect from estat simple. "Pre-trend p-value" refers to the joint test of the pre-treatment coefficients. Significance levels: * $p<0.1$, ** $p<0.05$, *** $p<0.01$

C.5 Complementary Evidence on First Versus Subsequent Declarations

This appendix provides complementary evidence on repeated exposure by comparing the dynamic fiscal effects of first and second hydrogeological State of Emergency declarations. The objective is to assess whether the main budgetary adjustment is concentrated at the first declaration, when the emergency-response and reconstruction framework is initially activated, or whether subsequent declarations generate comparable additional effects. The figures suggest that the fiscal response is stronger after the first declaration than after the second. In both the expenditure and revenue specifications, the first-declaration profile becomes more clearly positive over post-treatment horizons, whereas the second-declaration profile is flatter and remains closer to zero for most event times. This pattern is consistent with the idea that the largest budgetary adjustment is associated with the initial activation of the emergency-response and reconstruction framework, while subsequent declarations tend to generate smaller marginal effects.

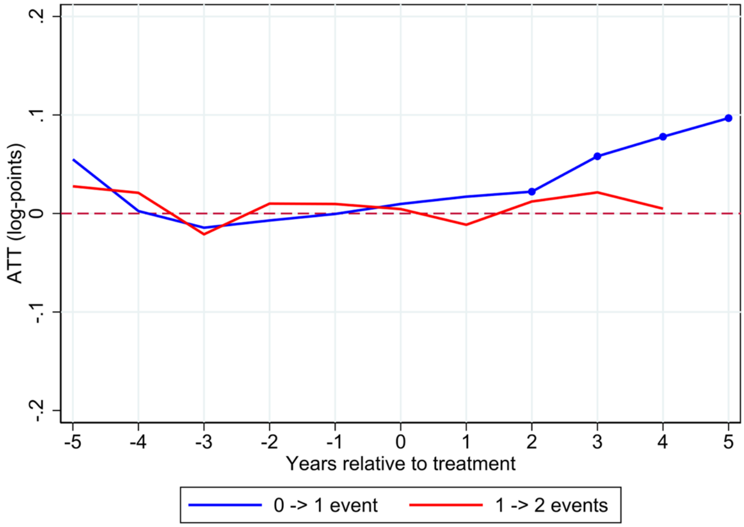

Fig. C.9 First vs. second declaration: total expenditure. Note. The figure compares the dynamic effects of first exposure to a hydrogeological State of emergency declaration (0→1 declaration, blue line) and second exposure (1→2 declarations, red line) on total municipal expenditure. Estimates are obtained within the Callaway-Sant'Anna framework using the improved doubly robust estimator. Comparison units are municipalities not yet exposed to the relevant declaration at the corresponding comparison date. Shaded areas denote 95% confidence intervals, and markers identify event-time coefficients whose confidence interval excludes zero

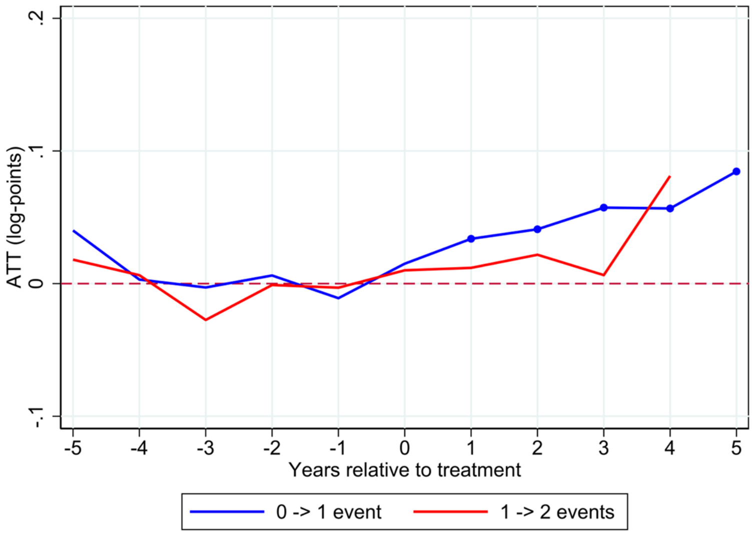

Fig. C.10 First vs. second declaration: total revenue. Note. The figure compares the dynamic effects of first exposure to a hydrogeological State of emergency declaration (0→1 declaration, blue line) and second exposure (1→2 declarations, red line) on total municipal revenue. Estimates are obtained within the Callaway-Sant'Anna framework using the improved doubly robust estimator. Comparison units are municipalities not yet exposed to the relevant declaration at the corresponding comparison date. Shaded areas denote 95% confidence intervals, and markers identify event-time coefficients whose confidence interval excludes zero

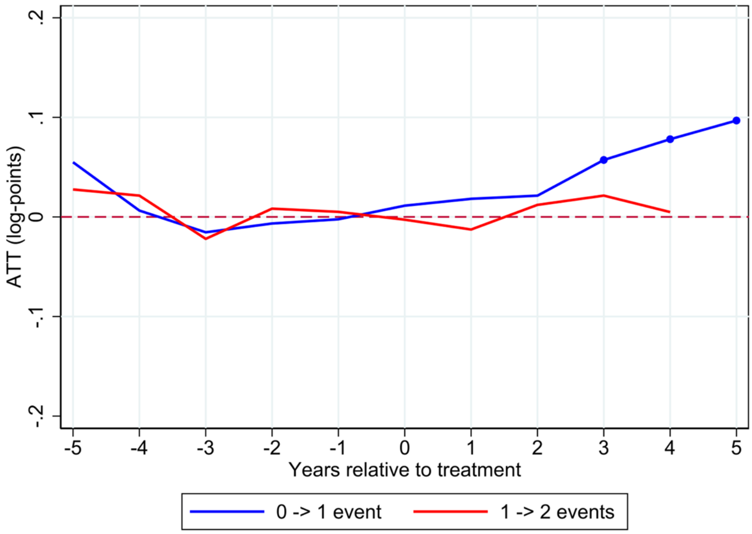

Fig. C.11 First vs. second declaration: total expenditure, restricted control groups. Note. The figure compares the dynamic effects of first exposure to a hydrogeological State of emergency declaration (0→1 declaration, blue line) and second exposure (1→2 declarations, red line) on total municipal expenditure. Estimates are obtained within the Callaway-Sant'Anna framework using the improved doubly robust estimator. For the first-declaration comparison, the control group is restricted to never-treated municipalities. For the second-declaration comparison, the control group is restricted to municipalities exposed only once over the sample period. Shaded areas denote 95% confidence intervals, and markers identify event-time coefficients whose confidence interval excludes zero

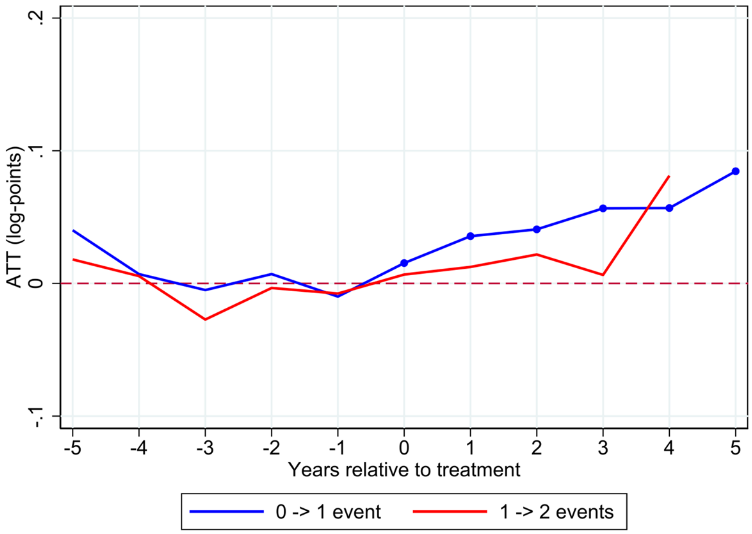

Fig. C.12 First vs. second declaration: total revenue, restricted control groups. Note. The figure compares the dynamic effects of first exposure to a hydrogeological State of emergency declaration (0→1 declaration, blue line) and second exposure (1→2 declarations, red line) on total municipal revenue. Estimates are obtained within the Callaway-Sant'Anna framework using the improved doubly robust estimator. For the first-declaration comparison, the control group is restricted to never-treated municipalities. For the second-declaration comparison, the control group is restricted to municipalities exposed only once over the sample period. Shaded areas denote 95% confidence intervals, and markers identify event-time coefficients whose confidence interval excludes zero

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Data Availability All data used in this study, together with the full set of scripts required to reproduce the analysis, are publicly available at https://doi.org/10.17632/ydmw37vtm3.1.

Declarations

Competing Interests The authors declare no competing interests.

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