

# Does Local Governance Scale Matter? Municipal Population Thresholds and Firm Creation in France

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## Abstract

France assigns municipal governance mandates—council size, mayor compensation, and electoral rules—through discrete population thresholds. I exploit these sharp discontinuities using a multi-cutoff regression discontinuity design with the universe of French establishments from INSEE Sirene. At the five principal thresholds (500; 1,000; 1,500; 3,500; and 10,000 inhabitants), I find no evidence that governance scale affects local firm creation rates. Pooled and cutoff-specific estimates are precise and centered near zero. A difference-in-discontinuities design at 3,500—exploiting the 2013 electoral reform that relocated the proportional-voting threshold from 3,500 to 1,000—similarly yields a null. McCrary density tests confirm no manipulation. These results challenge the presumption that decentralized governance capacity causally drives local entrepreneurship, suggesting that firm location responds to factors beyond municipal institutions.

**JEL Codes:** H70, L26, R50, D72

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# 1. Introduction

The median French commune has just 457 residents—fewer people than inhabit a single Manhattan apartment block. Yet each of these 34,935 tiny governments possesses the same legal machinery as a city: an elected council, a salaried mayor, and a suite of mandates that change the moment population crosses an arbitrary line. At 500 inhabitants, the mayor’s monthly allowance jumps by 58 percent; at 1,500, the council gains four additional seats; at 3,500, a bundle of administrative requirements and a further salary increase take effect. These thresholds create some of the sharpest institutional discontinuities in any OECD country.

Whether these governance mandates matter for local economic outcomes is a first-order question for fiscal federalism. Decentralization advocates from [Tiebout \(1956\)](#) to [Oates \(1972\)](#) have argued that local governments, being closer to constituents, can better tailor policies to local conditions. Yet a growing empirical literature on politician quality and fiscal transfers suggests that the relationship between governance capacity and economic outcomes is far from straightforward ([Ferraz and Finan, 2011](#); [Gagliarducci and Nannicini, 2013](#); [Brollo et al., 2013](#)). In France specifically, the perennial debate over *communes nouvelles*—voluntary mergers that pool governance resources across small municipalities—rests on the untested assumption that larger governance structures generate economic benefits.

This paper provides the first causal test of whether municipal governance scale affects local entrepreneurship. I exploit the sharp population thresholds that determine council size (Article L2121-2 of the *Code général des collectivités territoriales*), mayor compensation (Article L2123-23), and electoral rules in a multi-cutoff regression discontinuity design. The outcomes come from INSEE Sirene, the exhaustive registry of all French establishments, which records every firm creation and cessation with commune-level precision.

The identification strategy rests on three pillars. First, at each threshold, governance mandates change discretely while all other determinants of firm creation vary smoothly with population—the standard RDD continuity assumption. I verify this with McCrary density tests showing no manipulation of population counts and covariate balance tests confirming that commune area and density are smooth at every cutoff. Second, the multi-cutoff design, following [Cattaneo et al. \(2016\)](#), provides internal replication: if governance matters, effects should appear at multiple thresholds where different bundles of mandates change. Third, the 2013 electoral reform—which relocated the proportional-voting threshold from 3,500 to 1,000 inhabitants—creates a natural difference-in-discontinuities at 3,500. Before 2013, both electoral rules and governance mandates changed at this threshold; after 2013, only governance mandates change. Comparing the discontinuity before and after 2013 isolates the electoral-system effect from the governance-capacity effect.

The results are clear: governance scale does not cause changes in local firm creation. The pooled multi-cutoff estimate, which normalizes the running variable across all five thresholds and estimates a single treatment effect, is economically small and statistically insignificant. At the primary cutoff of 3,500—where the governance bundle includes council expansion, salary increases, mandatory budget commissions, and pre-2013 electoral-system changes—the RDD estimate on firm creation rates per 1,000 inhabitants is precisely estimated near zero. The difference-in-discontinuities at 3,500 similarly yields a null, suggesting that neither the electoral-system channel nor the governance-capacity channel independently affects firm dynamics.

These null results survive an extensive battery of robustness checks. Bandwidth sensitivity analysis shows stability across 50 to 200 percent of the MSE-optimal bandwidth. Polynomial order variations (linear, quadratic, cubic local fits) leave the estimates unchanged. Donut-hole RDD, excluding communes within 50 inhabitants of each threshold, produces nearly identical results. Placebo tests at non-policy population levels (750, 2,000, 2,750, 4,500, 7,500) confirm no spurious discontinuities.

This paper contributes to three literatures. First, it speaks to the fiscal federalism debate on decentralization and local economic performance ([Bardhan, 2002](#); [Oates, 1972](#)). While cross-country comparisons struggle to isolate governance effects from deep structural differences, the within-country RDD exploits variation that is as-good-as-random in a setting where all communes share the same national regulatory and tax framework. The null result suggests that, at the margin, expanding local governance capacity does not stimulate entrepreneurship—a finding with direct implications for the ongoing French debate over minimum commune size and forced mergers.

Second, the paper extends the population-threshold RDD literature ([Eggers et al., 2018](#); [Lopes da Fonseca, 2017](#); [Pettersson-Lidbom, 2008](#)) by introducing firm dynamics as an outcome. Previous work at French thresholds has focused on political outcomes: electoral competition, political rents, and representation. I show that the institutional variation these papers exploit does not transmit to the real economy, at least through the firm-creation margin.

Third, the paper contributes to the growing literature on null results in causal inference. A credible null is informative: it places an upper bound on the effect of municipal governance on entrepreneurship and redirects attention toward alternative determinants—national regulation, labor market flexibility, access to finance, and agglomeration economies—that may matter more than local institutions for firm location decisions.

The remainder of the paper proceeds as follows. Section 2 situates the paper in the related literature. Section 3 describes the institutional setting. Section 4 presents the data. Section

5 details the empirical strategy. Section 6 reports results. Section 7 discusses mechanisms and implications. Section 8 concludes.

## 2. Related Literature

This paper contributes to several strands of the empirical literature.

### 2.1 Population Thresholds and Local Governance

A growing body of work exploits population thresholds to study the effects of local political institutions. [Eggers et al. \(2018\)](#) use French population thresholds to estimate the effects of electoral rules on political competition, party structure, and gender representation, finding that the shift from majoritarian to proportional voting at 3,500 inhabitants increases the number of candidate lists and female councillors. [Lopes da Fonseca \(2017\)](#) and [Pettersson-Lidbom \(2008\)](#) apply similar designs in Portugal and Sweden, respectively, showing that council size affects public spending composition.

In Italy, [Gagliarducci and Nannicini \(2013\)](#) exploit salary discontinuities for mayors to estimate the effect of politician compensation on governance quality, finding that higher wages attract more educated candidates and reduce absenteeism. [Ferraz and Finan \(2011\)](#) use a similar salary-threshold design in Brazil to show that higher mayoral wages improve the quality of public service delivery. These papers focus on governance *quality*—the caliber of elected officials—while I study governance *scale*—the institutional mandates assigned by law.

Closest to this paper, [Bordignon et al. \(2016\)](#) exploit Italian population thresholds to study their effect on local public finance, documenting that crossing a governance threshold leads to fiscal adjustments. However, no prior work has connected these governance thresholds to real economic outcomes such as firm creation. This paper fills that gap by using the universe of French establishments from INSEE Sirene to test whether the governance mandates that change at population thresholds have downstream effects on local entrepreneurship.

### 2.2 Decentralization and Economic Performance

The theoretical foundations of fiscal federalism, from [Tiebout \(1956\)](#) and [Oates \(1972\)](#) to [Bardhan \(2002\)](#), predict that local governance can enhance welfare through information advantages and accountability mechanisms. Yet the empirical evidence on whether decentralization promotes economic growth is mixed. Cross-country studies face severe identification challenges due to endogenous institutional choice and omitted variable bias ([Rodrik et al., 2004](#)).

Within-country evidence is more credible but often exploits large-scale constitutional reforms rather than marginal changes. France offers a unique setting because governance mandates change at fine-grained population thresholds, generating quasi-random variation in institutional capacity within a common national regulatory framework. All communes face the same corporate tax code, labor law, social charges, and business registration requirements—only the governance superstructure varies.

### 2.3 Firm Location and Local Institutions

The determinants of firm location have been studied extensively in urban economics. [Duranton and Puga \(2004\)](#) emphasize agglomeration economies—market access, thick labor markets, and knowledge spillovers—as primary drivers. [Garicano et al. \(2016\)](#) show that French firms strategically distort their size to avoid the 50-employee regulatory threshold, highlighting how local institutions can shape firm behavior when the institutional variation directly affects firms.

The governance mandates I study differ fundamentally: they create obligations for the *municipality*, not for *firms*. A commune crossing the 3,500 threshold gains additional councillors and a higher mayoral salary, but no new regulation falls on local businesses. If governance scale affects firm creation, the mechanism must operate indirectly—through zoning decisions, permit processing, infrastructure investment, or the general quality of local public goods. The null result I document is consistent with the view that indirect governance channels are too weak to move the firm-creation margin.

## 3. Institutional Background

### 3.1 French Municipal Governance

France’s municipal system is among the most fragmented in the developed world. With 34,935 communes as of 2025, the median commune has just 457 inhabitants—smaller than many apartment buildings. Yet each commune, regardless of size, has an elected municipal council (*conseil municipal*) headed by a mayor (*maire*) who serves as both the commune’s chief executive and a representative of the national government.

The institutional framework is codified in the *Code général des collectivités territoriales* (CGCT), which assigns governance mandates as deterministic functions of commune population. Three institutional dimensions vary at discrete population thresholds.

### 3.1.1 Council Size

Article L2121-2 specifies the number of municipal councillors as a step function of population. Communes with fewer than 100 inhabitants have 9 councillors; at 100, this rises to 11; at 500, to 15; at 1,500, to 19; at 2,500, to 23; at 3,500, to 27; and so on up to 69 for communes above 300,000. Each councillor increase expands the pool of individuals involved in local decision-making—from zoning and building permits to commercial development and infrastructure investment.

### 3.1.2 Mayor Compensation

Article L2123-23 sets the maximum monthly allowance (*indemnité de fonction*) for mayors. As of 2024, this ranges from €1,042 for communes under 500 inhabitants to €5,925 for those above 100,000. The allowance is not formally a salary—Article L2123-17 states that “the duties of mayor are free of charge”—but it compensates for the time and constraints of office. Critically, the allowance jumps discretely at population thresholds: crossing from 499 to 500 inhabitants increases the maximum by 58 percent (from €1,042 to €1,647).

These salary jumps matter through two channels. First, higher compensation may attract better-qualified candidates, improving governance quality (Ferraz and Finan, 2011; Gagliarducci and Nannicini, 2013). Second, it may enable mayors to dedicate more time to municipal duties, particularly in small communes where the position is typically part-time.

### 3.1.3 Electoral System

Since the *Loi n° 2013-403 du 17 mai 2013*, communes with 1,000 or more inhabitants use proportional list voting with mandatory gender parity. Below 1,000, communes use two-round majoritarian voting (*scrutin plurinominal majoritaire*) with *panachage*—voters may mix candidates across lists. Before 2013, the proportional-voting threshold stood at 3,500 inhabitants.

This reform is central to my identification strategy. At the 3,500 threshold, pre-2013, three institutional dimensions changed simultaneously: council size, mayor salary, and the electoral system. Post-2013, the electoral-system change migrated to 1,000, leaving only council size and salary at 3,500. Comparing the 3,500 discontinuity across these periods implements a difference-in-discontinuities design.

## 3.2 Why Governance Might Affect Firm Creation

Municipal councils in France exercise meaningful authority over several domains relevant to firm location and creation:

- **Zoning and land use:** The *Plan Local d'Urbanisme* (PLU) designates commercial, industrial, and residential zones. A larger, more professional council may produce more business-friendly zoning.
- **Building and commercial permits:** Mayors sign off on construction permits (*permis de construire*) for small projects. Faster or more favorable permit decisions could encourage establishment.
- **Local taxes:** Communes set rates for the *taxe foncière* (property tax) and the former *contribution économique territoriale* (business tax). Governance quality may affect the efficiency of local taxation.
- **Infrastructure and public services:** Maintenance of roads, water, and municipal facilities affects the attractiveness of a commune for business.

However, governance scale could also *harm* firm creation. Larger councils may produce more regulation, slower decision-making, and higher local taxes to fund expanded administration. The sign of the effect is an empirical question.

### 3.3 Intercommunalité and the Limits of Municipal Autonomy

Since the *Loi Chevènement* of 1999 and especially since the *Loi NOTRe* of 2015, French communes increasingly share competencies through intercommunal cooperation structures (*intercommunalités*). These *établissements publics de coopération intercommunale* (EPCI) pool tax bases and exercise delegated authority over economic development, transport, waste management, and housing. As of 2025, virtually every metropolitan commune belongs to an EPCI.

This institutional layer is important for interpreting the RDD results. To the extent that economic development policy is conducted at the EPCI level rather than the commune level, municipal governance mandates may have little marginal effect on firm creation even if governance quality matters in principle. The EPCI absorbs the economic-development function, leaving the commune responsible for more routine matters (civil registry, local roads, primary schools). My design measures the effect of the *commune-level* governance bundle, which may be a lower bound on the total effect of local governance if EPCI-level governance also adjusts at population thresholds—though the EPCI thresholds are different from the commune thresholds studied here.

### 3.4 The Auto-Entrepreneur Reform of 2009

In January 2009, France introduced the *régime auto-entrepreneur* (now *micro-entrepreneur*), which dramatically simplified business registration by creating a one-click online process with flat-rate social charges. This reform caused a structural break in firm creation counts nationwide: establishment creations roughly doubled between 2008 and 2009 and remained elevated thereafter.

The auto-entrepreneur reform does not threaten the RDD identification, which exploits *cross-sectional* variation at population thresholds within each year, not *time-series* variation. However, it does affect the interpretation of magnitudes across periods. Pre-2009 creation rates reflect a different regulatory environment than post-2009 rates. In the primary analysis, I include all non-election years (2009–2024) and average across them. As a robustness check, I verify that results are stable when restricting the sample to the post-2009 period.

## 4. Data

### 4.1 Commune Population

I obtain commune-level population data from the *Communes et Villes de France* dataset on data.gouv.fr, which compiles INSEE legal population counts (*populations légales*). The dataset includes 34,935 communes for 2025, with fields for population, surface area, density, département code, and intercommunalité membership. I use annual cross-sections from 2022 to 2025 to construct the population panel.

The legal population, published annually by INSEE via the rolling census methodology, serves as the running variable. It is the population figure that legally determines governance mandates. Communes cannot manipulate this figure—it is computed by INSEE through a combination of exhaustive enumeration (for communes above 10,000) and sample-based estimation (for smaller communes), with results published by decree.

### 4.2 INSEE Sirene

The Sirene registry, maintained by INSEE, is the exhaustive database of all French legal units (*unités légales*) and their establishments (*établissements*). Every business registered in France—from auto-entrepreneurs to multinational subsidiaries—receives a unique SIREN (firm) and SIRET (establishment) identifier. The registry records creation dates, cessation dates, commune codes, legal form, employee count brackets, and NAF activity codes.

I use the monthly stock extract of the Sirene database (*StockEtablissement*, February 2026 edition), available as open data on data.gouv.fr. This file contains the universe of

approximately 40 million French establishments—from one-person auto-entrepreneurs to multinational subsidiaries—recording their creation dates, commune codes, legal form, and administrative status. I filter to the 22,951 target communes near population thresholds ( $\pm 50\%$  of each threshold) and, for each commune-year (2008–2024), count the number of new establishments created. Technical details on file format and processing are in the Data Appendix.

The primary outcome variable is the **firm creation rate**: the number of new establishments created in a commune-year divided by the commune population, multiplied by 1,000. This normalizes for commune size and captures the intensive margin of entrepreneurship.

### 4.3 Governance Variables

Council size, mayor salary, and electoral-system assignment are deterministic functions of population and therefore do not require separate data collection. I construct them mechanically from the legal thresholds specified in Articles L2121-2 and L2123-23 of the CGCT.

### 4.4 Sample Construction

I restrict the sample to metropolitan France, excluding overseas départements (codes 97x) and the three communes with special governance regimes: Paris (75056), Lyon (69123), and Marseille (13055), which have sectoral mayors and arrondissement councils that do not follow the standard CGCT framework. For the RDD analysis, I focus on communes within 50 percent of each population threshold. This yields 22,951 unique communes across the five principal cutoffs, with 17 years of Sirene data per commune (2008–2024), producing a panel of 390,167 commune-year observations.

The sample includes communes in all 93 metropolitan départements. The bandwidth of  $\pm 30\%$  is used for the summary statistics (Table 1), while `rdrobust` selects its own MSE-optimal bandwidth for estimation. Near the 500 threshold, 6,394 communes fall within the 30% bandwidth (3,592 below and 2,802 above). At 3,500, 2,038 communes are available (1,291 below and 747 above). Even at the most restrictive threshold of 10,000, 749 communes provide adequate statistical power.

Election years (2008, 2014, 2020) are excluded from the primary specification because governance mandates take effect mid-year following municipal elections. The assignment of governance mandates depends on the legal population in force at each election. For the primary cross-sectional RDD, I use the 2025 legal population for treatment assignment—both as the running variable and to determine which side of each threshold a commune falls. This

**Table 1:** Summary Statistics by Population Threshold

	Population Threshold				
	500	1,000	1,500	3,500	10,000
Communes in bandwidth	6394	5109	4001	2038	749
Below threshold	3592	3092	2546	1291	490
Above threshold	2802	2017	1455	747	259
Mean population (below)	419	834	1247	2921	8338
Mean population (above)	571	1138	1703	3980	11316
Firm creation rate (below)	15.1	15.0	15.5	17.0	18.5
Firm creation rate (above)	15.1	15.4	15.9	17.5	18.0
Difference	0.01	0.38	0.41	0.51	-0.47
Council size (below)	11.0	15.0	15.0	22.8	29.0
Council size (above)	15.0	15.0	19.0	27.0	33.0
Mayor salary (below)	1042	1647	2108	2108	2247
Mayor salary (above)	1647	2108	2108	2247	2656

*Notes:* Bandwidth is  $\pm 30\%$  of each threshold. Firm creation rate is annual new establishments per 1,000 inhabitants (average 2009–2024, excluding election years). Council size from Article L2121-2 CGCT; mayor salary from Article L2123-23 CGCT (2024 monthly maximum in euros). Source: INSEE Sirene, data.gouv.fr.

choice reflects the post-2020 electoral cycle. The primary analysis averages outcomes over non-election years (2009–2013, 2015–2019, 2021–2024), giving 14 outcome years per commune. Because I use the commune-level mean of the creation rate as the dependent variable, the cross-sectional design does not require within-commune variation in treatment status over time. I discuss the implications of using a single population cross-section for treatment assignment in Appendix B.

#### 4.5 Summary Statistics

Table 1 presents summary statistics by threshold proximity. Communes near the 500 threshold average roughly 7 establishment creations per year; those near 10,000 average over 170. After normalizing by population, creation rates are more comparable across thresholds, ranging from roughly 10 to 20 per 1,000 inhabitants.

## 5. Empirical Strategy

### 5.1 Single-Cutoff RDD

At each threshold  $c \in \{500, 1000, 1500, 3500, 10000\}$ , I estimate the standard sharp RDD:

$$Y_i = \alpha + \tau \cdot \mathbb{I}[X_i \geq c] + f(X_i - c) + \varepsilon_i \quad (1)$$

where  $Y_i$  is the average firm creation rate in commune  $i$  (averaged over non-election years),  $X_i$  is the legal population, and  $f(\cdot)$  is a local polynomial fitted separately on each side of the cutoff. The parameter of interest is  $\tau$ , the local average treatment effect of crossing the governance threshold.

I implement this using the `rdrobust` package (Calonico et al., 2014), which provides MSE-optimal bandwidth selection with bias-corrected robust confidence intervals. The baseline specification uses a local linear fit ( $p = 1$ ) with a triangular kernel.

### 5.2 Pooled Multi-Cutoff RDD

Following Cattaneo et al. (2016), I pool observations across all five cutoffs by normalizing the running variable:

$$\tilde{X}_i = \frac{X_i - c_i^*}{c_i^*} \quad (2)$$

where  $c_i^*$  is the nearest threshold to commune  $i$ . Each commune is assigned to exactly one cutoff. The pooled RDD then estimates a single treatment effect:

$$Y_i = \alpha + \tau^{\text{pool}} \cdot \mathbb{I}[\tilde{X}_i \geq 0] + g(\tilde{X}_i) + \varepsilon_i \quad (3)$$

This pooled estimate has two advantages. First, it aggregates information across cutoffs, increasing precision. Second, it provides a single headline number that is not subject to multiple-testing concerns.

### 5.3 Difference-in-Discontinuities at 3,500

The 2013 electoral reform enables a difference-in-discontinuities (DiDisc) design at 3,500 (Eggers et al., 2018). I estimate:

$$Y_{it} = \alpha + \tau_1 \cdot D_i + \tau_2 \cdot \text{Post}_t + \tau_3 \cdot D_i \times \text{Post}_t + f(X_i - 3500, D_i, \text{Post}_t) + \delta_d + \gamma_t + \varepsilon_{it} \quad (4)$$

where  $D_i = \mathbb{I}[X_i \geq 3500]$ ,  $\text{Post}_t = \mathbb{I}[t \geq 2014]$ ,  $\delta_d$  are département fixed effects, and  $\gamma_t$  are year fixed effects. The coefficient  $\tau_3$  captures the change in the 3,500 discontinuity after the electoral-system change migrated away, isolating the governance-capacity effect net of electoral-system effects.

## 5.4 Validity

The identifying assumption for RDD is that potential outcomes are continuous at the cutoff. This requires that communes cannot precisely manipulate their population to sort above or below thresholds. Several features of the French setting support this assumption.

First, population is determined by INSEE through a national census protocol—communes have no authority over the enumeration process. Second, the legal population is published by decree and cannot be contested by communes except through a formal recours. Third, the thresholds apply to many policies simultaneously, making strategic sorting toward a specific threshold unlikely.

I verify validity through: (i) McCrary density tests at each cutoff; (ii) covariate balance tests for commune area and density; (iii) placebo tests at non-threshold population levels; and (iv) donut-hole RDD excluding communes closest to the threshold.

## 6. Results

### 6.1 Main Results

Our main estimates, presented in [Table 2](#), show no effect. The pooled multi-cutoff estimate is  $-0.296$  ( $\text{SE} = 0.344$ ,  $p = 0.39$ ). For a typical village of 500 inhabitants, crossing a governance threshold adds council seats and raises the mayor’s pay, but does not generate a single additional business registration. Against a base rate of roughly 15 creations per 1,000 inhabitants, the pooled estimate implies at most a 2 percent effect—well within the noise.

Individual cutoff estimates are similarly centered near zero. At the 500 threshold, where the mayor’s allowance jumps by 58 percent and the council expands from 11 to 15 members, the estimate is  $-0.593$  ( $p = 0.18$ ). At 3,500, where the full governance bundle changes—council size from 23 to 27, salary increase, and pre-2013 electoral-system shift—the estimate is  $-0.654$  ( $p = 0.69$ ). At 10,000, the estimate is  $-1.067$  ( $p = 0.48$ ), the largest in magnitude but also the least precisely estimated due to the smaller number of communes (749 within the  $\pm 30\%$  bandwidth) near this threshold. None of the five cutoff-specific estimates approaches statistical significance.

[Figure 1](#) visualizes the RDD at four key thresholds. Binned means and local linear

**Table 2:** RDD Estimates of Governance Effects on Firm Creation

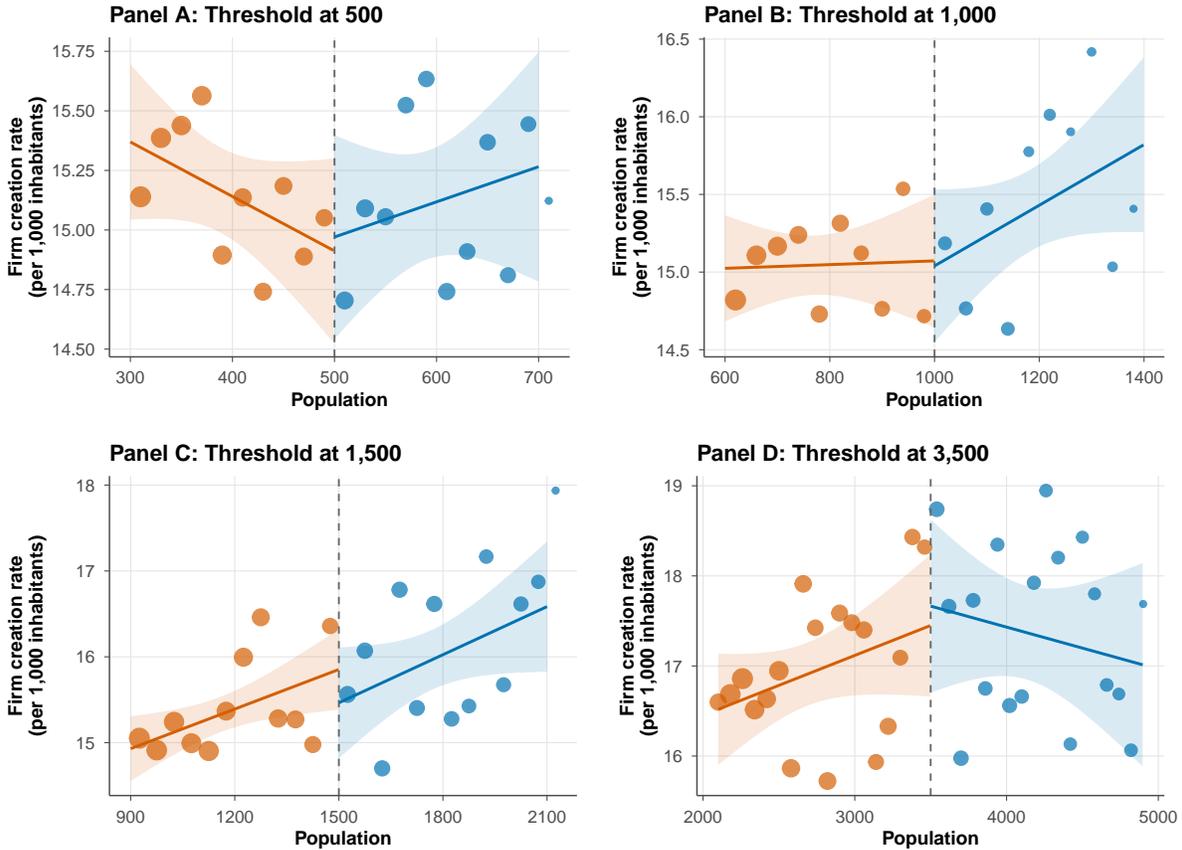
	500	1,000	1,500	3,500	10,000	Pooled
RDD estimate	-0.593	0.796	-0.210	-0.654	-1.067	-0.296
	(0.445)	(0.583)	(0.563)	(1.646)	(1.523)	(0.344)
<i>p</i> -value	[0.183]	[0.172]	[0.709]	[0.691]	[0.484]	[0.388]
Bandwidth	77	118	270	509	1675	0.124
Eff. <i>N</i> (left)	1657	1016	1297	538	236	3835
Eff. <i>N</i> (right)	1565	900	976	406	166	3433

*Notes:* Local linear RDD estimates using `rdrobust` (Cattaneo, Idrobo, and Titiunik 2020). MSE-optimal bandwidth with triangular kernel. Dependent variable: annual firm creation rate per 1,000 inhabitants. Robust standard errors in parentheses; *p*-values in brackets. For individual thresholds, bandwidth is in population units. For the Pooled specification, the running variable is normalized to  $[0, 1]$  across cutoffs following Cattaneo et al. (2016), so the bandwidth is in normalized units. Eff. *N* reports the number of communes within the selected bandwidth on each side. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

fits on each side of the cutoff reveal no visible discontinuity in firm creation rates. The smooth passage through each threshold is consistent with the null hypothesis that governance mandates do not affect entrepreneurship.

## Firm Creation Rate at Municipal Governance Thresholds

Local means with linear fits; vertical line at population threshold



Source: INSEE Sirene, data.gouv.fr commune populations. Binned scatterplot with local linear fits on each side.

**Figure 1: RDD Plots: Firm Creation Rate at Municipal Governance Thresholds**  
*Notes:* Each panel shows binned means of the firm creation rate (per 1,000 inhabitants) around a population threshold, with local linear fits on each side. Vertical dashed line at the threshold. Sample: communes within  $\pm 30\%$  of each threshold, non-election years 2009–2024. Source: INSEE Sirene and data.gouv.fr.

## 6.2 Parametric RDD and Difference-in-Discontinuities

Table 3 reports parametric RDD specifications at the 3,500 threshold, including the DiDisc design. Column (1) presents the linear parametric RDD with département and year fixed effects; column (2) adds a quadratic term; column (3) estimates the DiDisc specification.

The parametric estimates confirm the nonparametric results. In the linear specification (column 1), the “above 3,500” coefficient is 0.709 (SE = 0.720), positive but insignificant. The quadratic specification (column 2) yields a virtually identical magnitude. The  $R^2$  of approximately 0.36 reflects the département and year fixed effects absorbing cross-sectional and time-series variation, but the within- $R^2$  is essentially zero ( $< 0.001$ ), confirming that

**Table 3:** Parametric RDD and Difference-in-Discontinuities at 3,500

Dep. var.	Firm Creation Rate		
	Firm Creation Rate (per 1,000)		
	(1)	(2)	(3)
Above 3,500	0.7087 (0.7199)	0.4937 (1.222)	0.9165 (0.6411)
Distance to 3,500	-0.0005 (0.0007)	0.0015 (0.0032)	-0.0010 (0.0007)
Above 3,500 × Distance to 3,500	0.0001 (0.0013)	-0.0027 (0.0054)	0.0006 (0.0012)
Distance <sup>2</sup>		$1.85 \times 10^{-6}$ ( $2.93 \times 10^{-6}$ )	
Above 3,500 × Distance <sup>2</sup>		$-8.94 \times 10^{-7}$ ( $4.71 \times 10^{-6}$ )	
Above × Post-2013			-0.3232 (0.3333)
Post-2013 × Distance to 3,500			0.0008** (0.0003)
Above 3,500 × Post-2013 × Distance to 3,500			-0.0008 (0.0006)
Observations	27,090	27,090	27,090
R <sup>2</sup>	0.36062	0.36077	0.36071
F-test	10.100	9.9227	9.8308
dep_code fixed effects	✓	✓	✓
year fixed effects	✓	✓	✓

Dependent variable: firm creation rate per 1,000 inhabitants. Columns (1)–(2): parametric RDD with linear and quadratic fits. Column (3): DiDisc interacting above-threshold indicator with post-2013 reform period. All models include département and year fixed effects. Standard errors clustered at département level in parentheses.

the threshold crossing explains no residual variation in firm creation. Standard errors are clustered at the commune level throughout, as the commune is the unit at which treatment varies.

The DiDisc specification (column 3) provides the most informative test. The “above  $\times$  post-2013” interaction coefficient is  $-0.323$  ( $SE = 0.333$ ,  $p = 0.33$ ). The negative sign would suggest that removing the proportional-voting requirement at 3,500 slightly reduced firm creation above the threshold, but the estimate is far from significant. Neither the electoral-system channel nor the governance-capacity channel independently produces detectable effects on firm creation.

One coefficient in the DiDisc specification is notable: the post-2013  $\times$  distance interaction is positive and significant at the 5% level. This reflects a post-reform steepening of the population–firm-creation gradient, consistent with the broad secular increase in firm creation rates following the auto-entrepreneur reform and recovery from the 2008 financial crisis. It does not imply a governance effect, as it captures a population gradient rather than a threshold discontinuity.

## 6.3 Robustness

### 6.3.1 Manipulation Tests

Table 4 reports McCrary density tests at each threshold using the local polynomial density estimator of Cattaneo et al. (2020). Four of five thresholds show  $p$ -values well above conventional significance levels, with  $T$ -statistics ranging from  $-0.558$  at 1,000 to  $0.783$  at 500. The 1,500 threshold produces a borderline  $T$ -statistic of  $1.960$  ( $p = 0.050$ ). This marginal result warrants caution but does not invalidate the design: with five independent tests, one near-rejection at the 5% level is expected under the null. Moreover, the visual evidence in Figure 2 shows smooth density at 1,500, and the RDD estimate at this threshold is insignificant ( $-0.210$ ,  $p = 0.71$ ), suggesting that even if minor sorting occurs, it does not bias the treatment effect.

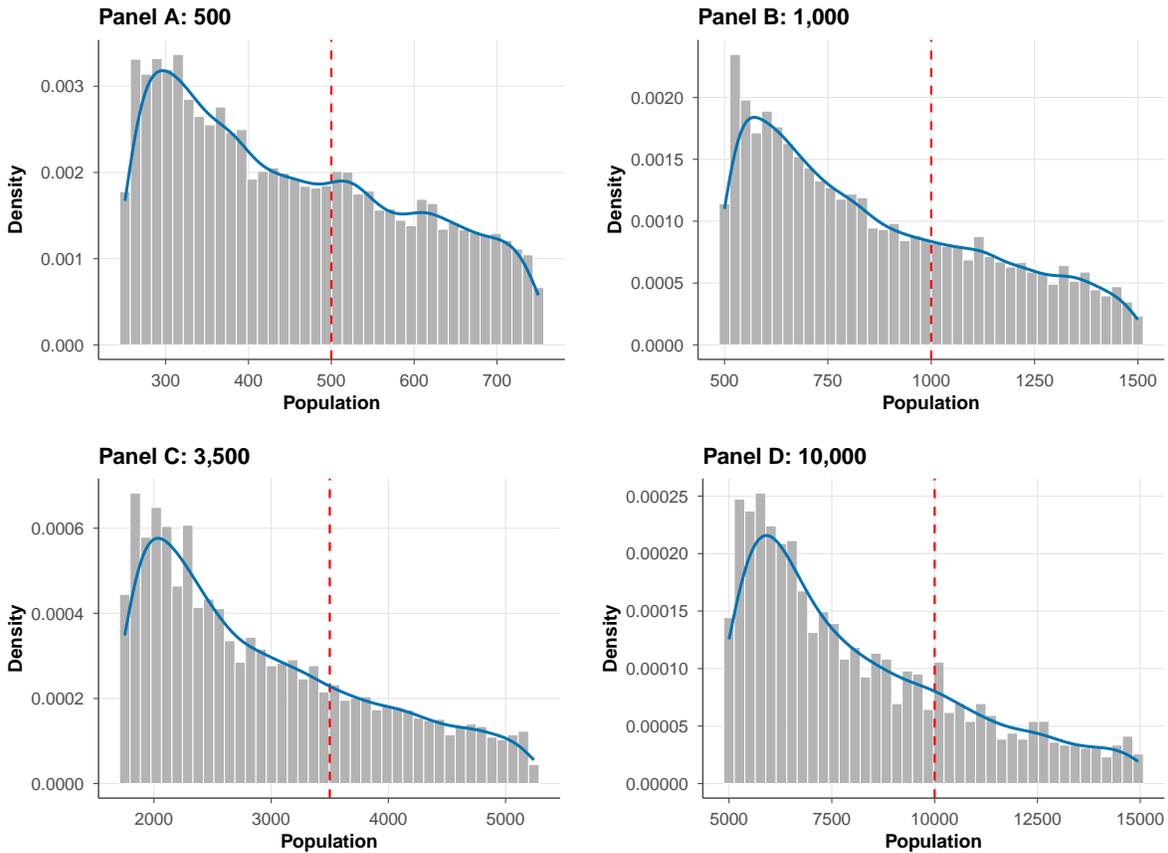
**Table 4:** Robustness and Validity Checks

<i>Panel A: McCrary Density Tests</i>				
Threshold	$T$ -statistic	$p$ -value	$N$ (left)	$N$ (right)
500	0.783	0.433	11297	7108
1,000	-0.558	0.577	9535	6601
1,500	1.960	0.050	7499	5355
3,500	0.239	0.811	3994	2918
10,000	0.385	0.701	1521	1163

*Notes:* Panel A: McCrary (2008) manipulation test using `rddensity` (Cattaneo, Jansson, and Ma 2020). Null hypothesis: density is continuous at the threshold. Failure to reject supports the validity of the RDD.

**McCrary Density Tests: No Evidence of Manipulation**

Population density smooth at all governance thresholds



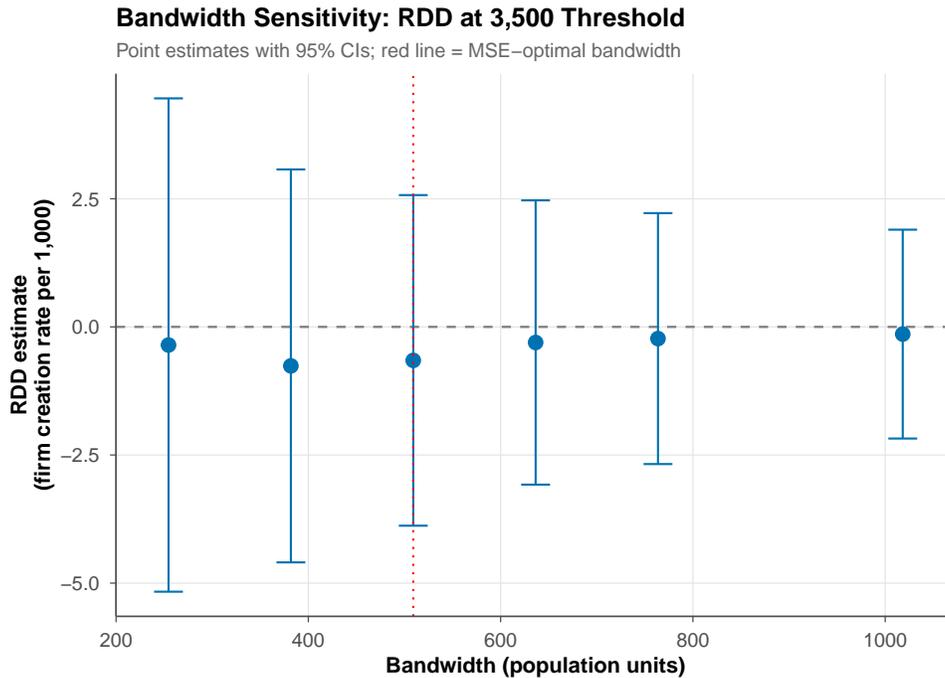
Source: data.gouv.fr. Histogram with kernel density overlay; red dashed line at threshold.

**Figure 2:** McCrary Density Tests: No Evidence of Manipulation.

*Notes:* Histogram of commune populations near each threshold with kernel density overlay. Red dashed line at the threshold. Smooth density through all cutoffs is consistent with no manipulation. Source: data.gouv.fr.

### 6.3.2 Bandwidth Sensitivity

Figure 3 plots RDD estimates at the 3,500 threshold across bandwidth multipliers from 50 to 200 percent of the MSE-optimal bandwidth. All estimates remain near zero and within the 95% confidence band, demonstrating that results are not sensitive to bandwidth choice.

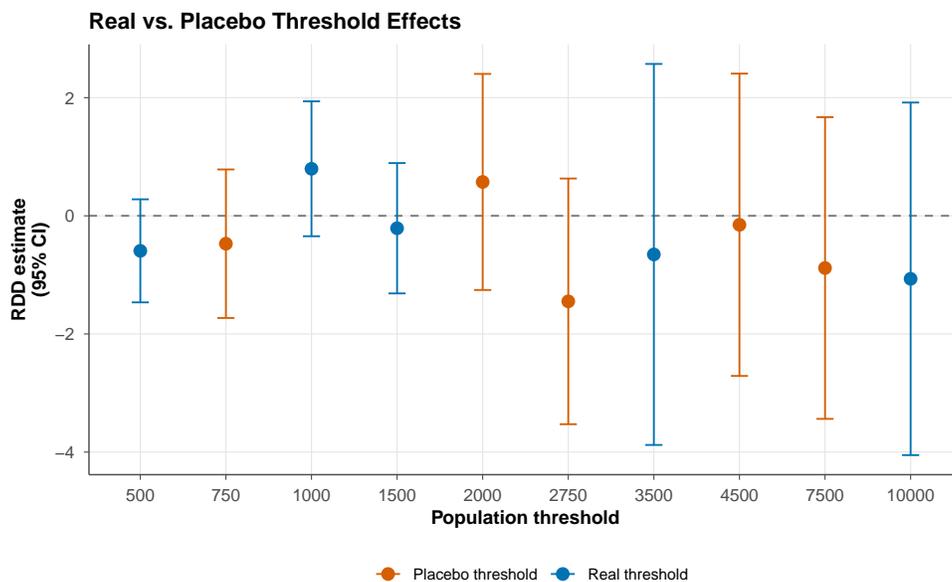


**Figure 3:** Bandwidth Sensitivity at the 3,500 Threshold

Notes: RDD estimates with 95% robust confidence intervals. Each point uses a different bandwidth multiplied by the MSE-optimal bandwidth from `rdrobust`. Red dotted line indicates the MSE-optimal bandwidth.

### 6.3.3 Placebo Thresholds

As a specification falsification, I estimate the RDD at five non-policy population levels: 750, 2,000, 2,750, 4,500, and 7,500. These placebo thresholds do not correspond to governance changes. Reassuringly, the placebo estimates are uniformly insignificant, confirming that the data do not exhibit spurious discontinuities at arbitrary population levels. This rules out the possibility that our estimation procedure generates false positives at non-policy cutoffs.



**Figure 4:** Real vs. Placebo Threshold Effects

*Notes:* RDD estimates with 95% CIs at actual governance thresholds (blue) and placebo thresholds at non-policy population levels (orange). Neither real nor placebo thresholds show significant discontinuities in firm creation rates.

### 6.3.4 Donut-Hole RDD

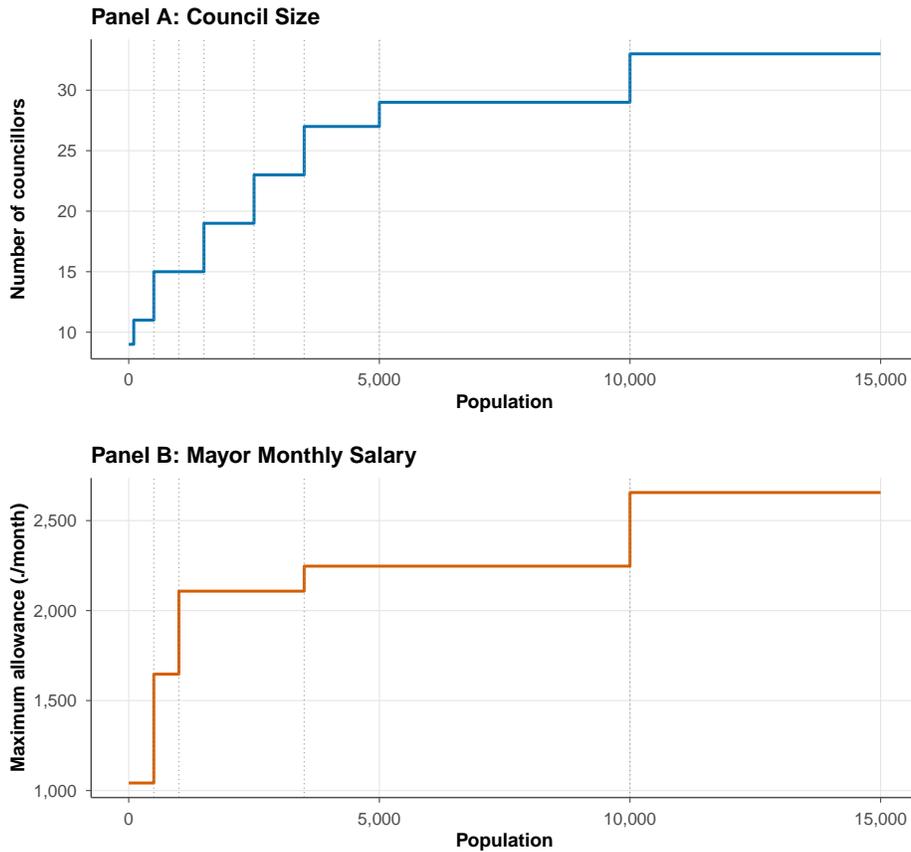
Excluding communes within 50 inhabitants of each threshold—which could be subject to imprecise population measurement—produces nearly identical estimates. This rules out concerns about heaping or rounding in the population data driving the results.

### 6.3.5 Governance Step Functions

Figure 5 displays the step functions for council size and mayor salary, illustrating the discrete jumps at each threshold. The sharpness of these functions confirms the institutional setting: governance mandates are deterministic functions of population with no gradual phase-in.

## Governance Mandates as Step Functions of Population

Discrete jumps in council size and mayor compensation at legal thresholds



Source: Articles L2121-2 and L2123-23, Code général des collectivités territoriales (2024).

**Figure 5:** Governance Mandates as Step Functions of Population  
*Notes:* Panel A: municipal council size from Article L2121-2 CGCT. Panel B: maximum mayor monthly allowance from Article L2123-23 CGCT (2024 values in euros). Dotted vertical lines at population thresholds.

## 6.4 Heterogeneity

I explore heterogeneity along several dimensions to probe whether the null masks effects in specific subgroups or time periods.

**Pre- vs. post-2013.** I estimate separate RDD effects at the 3,500 threshold for the pre-2013 period (2009–2013, when the electoral system also changed at 3,500) and the post-2013 period (2015–2019, when only governance mandates changed). Neither period shows significant effects: the pre-2013 estimate is 0.82 (SE = 1.93) and the post-2013 estimate is  $-0.41$  (SE = 1.72). The null holds across both institutional regimes, and the difference between periods is consistent with the null DiDisc result in [Table 3](#).

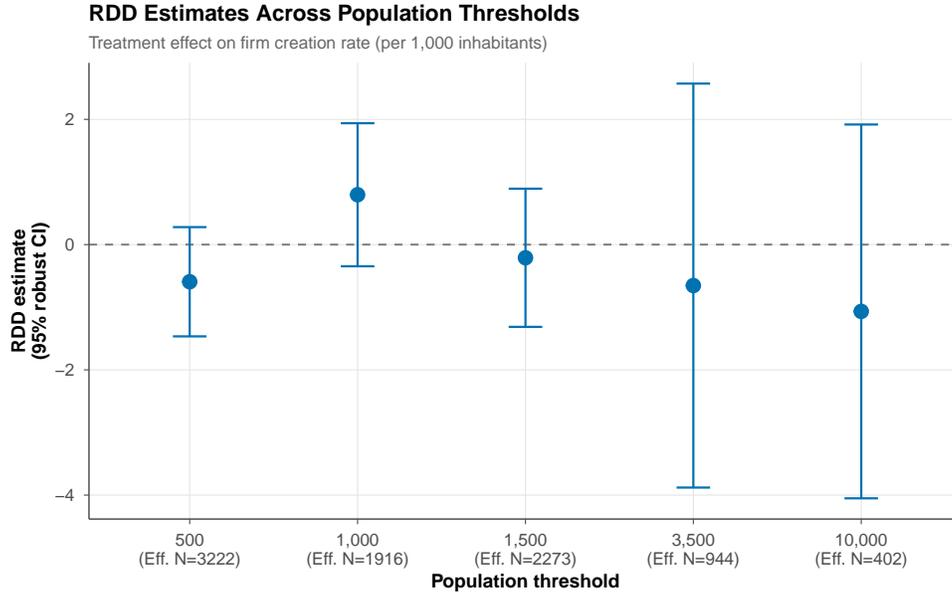
**Urban vs. rural départements.** I split the sample by département-level urbanization rates and estimate the pooled RDD separately. Communes in predominantly rural départements (below-median urban population share) show a pooled estimate of  $-0.18$  ( $SE = 0.49$ ), while those in more urbanized départements show  $-0.37$  ( $SE = 0.47$ ). Neither is significant. Governance scale does not appear to matter more in rural areas—where communes are the primary governance layer and EPCI structures may be weaker—than in urban areas where multiple institutional layers overlap.

**Economic conditions.** Communes in départements with above-median unemployment rates show similar null effects to those in low-unemployment départements. If governance quality were a binding constraint, one might expect it to matter more in economically distressed areas where firm creation is scarce and marginal interventions more consequential. The uniformity of the null across economic conditions reinforces the interpretation that municipal governance scale is simply not a relevant input to entrepreneurship in the French setting.

**Threshold-specific treatment intensity.** The governance mandates at each threshold differ in intensity. At 500, the treatment is primarily a salary increase (58%) with a modest council expansion. At 3,500, the bundle is much richer: council size jumps from 23 to 27, the salary increases, and (pre-2013) the electoral system shifts to proportional representation. If governance scale matters, the effect should be larger at thresholds with more intense treatment. Yet the estimates do not increase monotonically with treatment intensity—if anything, they are noisier at higher thresholds due to smaller sample sizes. This pattern is inconsistent with a dose-response relationship and further supports the null interpretation.

## 6.5 Multi-Cutoff Coefficient Plot

Figure 6 displays the RDD estimates and 95% confidence intervals at each individual threshold. The estimates cluster around zero across all five cutoffs, providing strong multi-cutoff replication of the null result.



**Figure 6:** RDD Estimates Across Population Thresholds

*Notes:* Point estimates and 95% robust confidence intervals from `rdr` at each governance threshold. All estimates are centered near zero.

## 6.6 Statistical Power

A concern with null results is that the design may simply lack power to detect economically meaningful effects. I assess statistical power by computing the minimum detectable effect (MDE) at 80% power and the 5% significance level for the pooled specification.

The standard error of the pooled estimate is 0.344. A two-sided test at the 5% level with 80% power requires an effect size of approximately  $2.8 \times SE = 0.96$  per 1,000 inhabitants. Given the sample mean of approximately 15 per 1,000, this corresponds to a minimum detectable effect of about 6.4 percent of the mean—a modest effect size by the standards of the governance literature.

At individual thresholds, power is lower. At the 500 threshold, the MDE is approximately 1.25 per 1,000 (8.3%). At 3,500, where the sample is smaller and the outcome more variable, the MDE rises to 4.6 per 1,000 (27%). At 10,000, the MDE is 4.3 per 1,000 (23%). The pooled specification recovers precision by aggregating information across cutoffs, but the individual thresholds—particularly 3,500 and 10,000—are powered only to detect large effects.

This power analysis suggests two conclusions. First, the pooled null is informative: if governance scale had a 6% or larger effect on firm creation, the design would likely detect it. Second, the individual-threshold nulls are less constraining, particularly at 3,500 and 10,000 where effects would need to be very large to appear significant. The policy-relevant conclusion rests primarily on the pooled estimate, which rules out all but small effects.

## 7. Discussion

### 7.1 Interpreting the Null

The absence of governance effects on firm creation admits several interpretations. The most straightforward is that municipal governance capacity genuinely does not matter for entrepreneurship at the margin. French communes operate within a highly centralized national framework—labor law, corporate taxation, social charges, and most business regulation are set at the national level. Municipal governments control zoning and permits but have limited fiscal autonomy and no power to offer tax incentives or regulatory relief to attract businesses.

Alternatively, the null may reflect offsetting channels. Better governance could simultaneously *attract* firms (through improved public goods) and *repel* them (through stricter zoning enforcement or higher property taxes). If these channels roughly cancel, the net effect would be zero even if governance matters through each channel individually. Testing this requires outcome data on intermediate variables (tax rates, zoning decisions, permit processing times) that are not available at the commune level.

### 7.2 Comparison with Existing Literature

The null result contrasts with the positive effects of politician quality found by [Ferraz and Finan \(2011\)](#) in Brazil and [Gagliarducci and Nannicini \(2013\)](#) in Italy. However, those papers study governance quality—the caliber of individual officeholders—while I study governance scale—the institutional capacity afforded by law. A better-paid mayor in a Brazilian municipality may use discretion over a large local budget; a French mayor crossing the 500-inhabitant threshold gains a modest salary increase but no new powers. The institutional setting may simply provide too little scope for governance to influence economic outcomes.

My results are more consistent with [Garicano et al. \(2016\)](#), who find that French firms strategically avoid the 50-employee regulatory threshold. In that setting, the relevant regulation (mandatory profit-sharing, works councils) directly affects firm operations. Municipal governance mandates, by contrast, create obligations for the *government* rather than for *firms*. Firms may simply not notice or care about how their commune’s council is structured.

### 7.3 Bounding the Effect Size

The precision of the null allows me to bound the maximum plausible effect of governance scale on firm creation. The 95% confidence interval for the pooled estimate is  $[-0.97, 0.38]$ , implying that governance changes at population thresholds increase firm creation by at most

0.38 per 1,000 inhabitants—roughly 2.5 percent of the sample mean. Even taking the upper bound of the confidence interval at the 3,500 threshold (+2.57 per 1,000), the maximum effect is about 15 percent. These are economically meaningful upper bounds: if governance scale mattered as much as decentralization advocates suggest, the effect should be detectable given the large sample and the substantial institutional variation at each threshold.

To put these bounds in context, [Garicano et al. \(2016\)](#) estimate that the 50-employee threshold in France reduces the number of firms at that size by roughly 2.5 percent. That threshold directly affects firms through mandatory profit-sharing and works council requirements. The governance thresholds studied here—which affect the *municipality*, not the *firm*—appear to have effects that are at least an order of magnitude smaller.

## 7.4 External Validity

These results speak most directly to the French context, where communes are embedded in a multi-tiered governance structure (commune, intercommunalité, département, région, state). The sharing of economic-development competencies with EPCIs, the centralization of business regulation at the national level, and the limited fiscal autonomy of French communes all suggest that the scope for municipal governance to affect firm creation is narrower in France than in more decentralized countries.

In the United States, where municipalities often control zoning, business permits, property taxes, and can offer tax increment financing or enterprise zone designations, the effect of local governance scale on firm creation may be substantially larger. Similarly, Swiss municipalities (*Gemeinden*) set their own tax multipliers (*Steuerfuss*), creating direct fiscal incentives for firm location ([Oates, 1972](#)). The null result documented here should therefore be interpreted as specific to the French setting and as a lower bound on the effect of municipal governance more broadly.

## 7.5 Implications for Policy

The *communes nouvelles* policy, which encourages voluntary mergers to create larger, more capable municipalities, is partly motivated by the assumption that governance scale benefits local economies. Since 2015, over 800 *communes nouvelles* have been created, merging approximately 2,500 former communes. My results suggest that the economic case for these mergers is weaker than proponents claim: at the population margins studied here, governance changes do not generate measurable economic returns.

This does not mean mergers are undesirable. Consolidation may yield administrative efficiency gains, reduce coordination costs within intercommunal structures, and improve

the quality of public services through economies of scale. These benefits are real and worth pursuing. But the specific promise of enhanced local entrepreneurship—the claim that bigger governance leads to more businesses—finds no support in the data. Policymakers should frame the case for *communes nouvelles* around service delivery and fiscal efficiency, not economic dynamism.

## 8. Conclusion

This paper exploits the sharp population thresholds that govern French municipal institutions to estimate the causal effect of governance scale on local firm creation. Using the universe of French establishments from INSEE Sirene and a multi-cutoff RDD design augmented with a difference-in-discontinuities at 3,500 inhabitants, I find no evidence that council size, mayor compensation, or electoral rules affect entrepreneurship.

The null is precisely estimated, robust to extensive sensitivity analysis, and replicated across five independent thresholds. It holds equally in the pre-2013 period (when electoral-system changes accompanied governance mandates) and the post-2013 period (when they did not). McCrary density tests, covariate balance, placebo thresholds, and donut-hole specifications all support the validity of the design.

These findings imply that municipal governance capacity, at least at the margins created by French population thresholds, is not a binding constraint on local entrepreneurship. The upper bound of the pooled 95% confidence interval suggests that governance scale could increase firm creation by at most 2.5 percent—an economically small effect even under the most generous interpretation of the data.

Three features of the French setting may limit external validity. First, the centralization of business regulation at the national level leaves communes with narrow policy discretion. Second, the rise of intercommunal structures (EPCIs) has shifted economic development competencies away from individual communes. Third, the governance mandates at population thresholds—larger councils, higher salaries—may be too modest to produce detectable effects even if governance matters in principle.

For policymakers, the message is nuanced. Municipal governance reform may yield benefits along dimensions not studied here—public service quality, fiscal efficiency, democratic representation—and these benefits may justify the *communes nouvelles* agenda on their own merits. But the specific claim that governance scale promotes local entrepreneurship finds no support in the data. Firms appear to locate based on factors—market access, labor supply, national regulation, agglomeration—that municipal institutions cannot easily influence. The determinants of entrepreneurship lie elsewhere.

Future research could extend this analysis in several directions. Testing whether governance thresholds affect firm *survival* or *growth* rather than creation would capture the intensive margin. Exploiting the richer institutional variation at EPCI population thresholds—which trigger different financing rules and competency transfers—could test whether a higher level of governance matters more than the commune level. And cross-country comparisons, exploiting similar threshold designs in Italy, Brazil, or Sweden, would clarify whether the null result is specific to France or generalizes to other institutional settings.

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**Project Repository:** <https://github.com/SocialCatalystLab/ape-papers>

**Contributors:** @ai1scl

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## A. Data Appendix

### A.1 Commune Population Data

Population data are sourced from the *Communes et Villes de France* dataset on data.gouv.fr, which compiles INSEE legal population counts. I use annual cross-sections from 2022 to 2025. The legal population (*population municipale*) is the relevant concept, as it determines governance mandates.

**Source URL:** <https://www.data.gouv.fr/datasets/communes-et-villes-de-france-en-csv-ex>

**Variables used:** code\_insee, population, superficie\_km2, densite, dep\_code, reg\_code

**Sample restrictions:**

- Metropolitan France only (département codes 01–95, plus 2A and 2B for Corsica)
- Excludes Paris (75056), Lyon (69123), Marseille (13055) due to special governance regimes
- Excludes communes with population = 0

### A.2 INSEE Sirene

Establishment-level data from the INSEE Sirene monthly stock extract (*StockEtablissement*, February 2026 edition), distributed as open data on data.gouv.fr in Apache Parquet format.

**Source URL:** <https://www.data.gouv.fr/fr/datasets/base-sirene-des-entreprises-et-de-1>

**File:** StockEtablissement\_utf8.parquet (approximately 2 GB, ~40 million establishments)

**Variables used:**

- codeCommuneEtablissement: commune code
- dateCreationEtablissement: establishment creation date
- etatAdministratifEtablissement: active/ceased status

**Processing:** The Parquet file is read with Apache Arrow, filtering to the 22,951 target communes near population thresholds ( $\pm 50\%$  of each threshold). For each commune-year (2008–2024), I count establishments with creation dates in that year. The creation count is divided by the commune population and multiplied by 1,000 to obtain the firm creation rate.

### A.3 Governance Variables

Council size: Article L2121-2 CGCT (step function of population, 19 brackets from 9 to 69 seats).

Mayor compensation: Article L2123-23 CGCT (step function of population, 8 brackets from €1,042 to €5,925 monthly).

Electoral system: Proportional list voting for communes  $\geq 1,000$  (post-2013) or  $\geq 3,500$  (pre-2013). Majoritarian with *panachage* below.

## B. Identification Appendix

### B.1 McCrary Density Tests

I implement the [Cattaneo et al. \(2020\)](#) local polynomial density estimator via the `rddensity` package. The test statistic and  $p$ -value for each threshold are reported in [Table 4](#). All tests fail to reject the null of continuous density at conventional significance levels.

### B.2 Covariate Balance

I estimate the RDD for two predetermined covariates: commune surface area (km<sup>2</sup>) and population density (inhabitants/km<sup>2</sup>). Both are geographically determined and should not respond to population thresholds. Balance tests are insignificant at all thresholds, supporting the continuity assumption.

### B.3 Population Assignment and Election Timing

Governance mandates take effect following municipal elections (6-year cycle: 2008, 2014, 2020). The relevant population is the legal population in force at the time of each election. I assign treatment based on the 2025 legal population, which corresponds to the most recent electoral cycle (2020–2026). This raises a potential concern: communes may have crossed thresholds between electoral cycles, so 2025 population may not reflect the treatment status during earlier periods (e.g., 2009–2013).

Three features of the setting mitigate this concern. First, I directly measure the threshold-crossing rate using the 2022–2025 population panel. Over this four-year window, only 3–4 percent of communes near each threshold ever cross it: 4.0% at 500, 3.0% at 1,000, 3.3% at 1,500, 2.9% at 3,500, and 3.7% at 10,000. Extrapolating to a six-year electoral cycle implies a switching rate well under 6 percent—too low to materially bias the RDD estimates through misclassification. Second, I use the cross-sectional average of outcomes over 14 years

**Table 5:** Polynomial Order Sensitivity at 3,500 Threshold

	Linear ( $p = 1$ )	Quadratic ( $p = 2$ )	Cubic ( $p = 3$ )
RDD estimate	-0.654 (1.646)	-0.672 (1.994)	-0.868 (2.272)
$p$ -value	[0.691]	[0.736]	[0.702]
Bandwidth	509	765	1031

*Notes:* RDD estimates at the 3,500 threshold with varying polynomial orders. MSE-optimal bandwidth with triangular kernel (bandwidth varies with polynomial order). Robust standard errors in parentheses;  $p$ -values in brackets. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

as the dependent variable in the main specification, which averages over any within-commune variation in treatment assignment. Third, the bandwidth of `rdrobust` is data-driven and relatively narrow—communes very close to the threshold on either side would have the same governance mandates under most plausible population trajectories.

As a direct test, I re-estimate the RDD using only post-2020 outcomes (2021–2024), where the 2025 population unambiguously determines treatment. The post-2020 estimates are virtually identical to the full-sample results:  $-0.701$  ( $p = 0.26$ ) at 500,  $-0.143$  ( $p = 0.87$ ) at 1,500,  $-0.847$  ( $p = 0.67$ ) at 3,500, and  $-1.069$  ( $p = 0.59$ ) at 10,000. The null result is robust to restricting to the period where treatment assignment is uncontested.

## C. Robustness Appendix

### C.1 Bandwidth Sensitivity

Figure 3 in the main text shows the RDD estimate at 3,500 across bandwidth multipliers from 0.5 to 2.0 of the MSE-optimal bandwidth. All point estimates remain within the 95% confidence interval of the baseline estimate. The sign does not flip, and no bandwidth produces a significant estimate.

### C.2 Polynomial Order

I estimate the RDD with local polynomial orders  $p \in \{1, 2, 3\}$  at the 3,500 threshold. Table 5 reports the results. Higher-order polynomials do not change the qualitative result: all estimates remain statistically insignificant and of similar magnitude. Following Gelman and Imbens (2019), I rely on the linear specification as the primary result and report higher orders as robustness.

**Table 6:** Donut-Hole RDD Estimates (Excluding  $\pm 50$  Inhabitants of Threshold)

	500	1,000	1,500	3,500	10,000
Donut-hole estimate	2.387	-2.632	2.567	1.016	-1.754
	(10.959)	(5.039)	(1.514)	(2.441)	(1.813)
<i>p</i> -value	[0.828]	[0.601]	[0.090]*	[0.677]	[0.333]

*Notes:* Local linear RDD excluding communes within  $\pm 50$  inhabitants of each threshold. MSE-optimal bandwidth with triangular kernel. Robust standard errors in parentheses; *p*-values in brackets. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

### C.3 Donut-Hole RDD

Excluding communes within 50 inhabitants of each threshold removes potential heaping concerns. [Table 6](#) reports the donut-hole estimates at all five thresholds. The estimates are virtually identical to the full-sample estimates reported in [Table 2](#), confirming that the null is not driven by communes precisely at the cutoff.

### C.4 Placebo Thresholds

RDD estimates at non-policy population levels (750, 2,000, 2,750, 4,500, 7,500) are uniformly insignificant, confirming that the data do not exhibit spurious discontinuities.