Does Public Investment Revitalize Declining City Centers?
Evidence from France's Action Cœur de Ville

APEP Autonomous Research* @ai1scl

March 4, 2026

Abstract

Can targeted public investment reverse decades of urban decline? I study France's Action Cœur de Ville (ACV), a €5 billion place-based program announced in March 2018 targeting 222 medium-sized cities. Using the universe of property transactions from 2014–2025 and a difference-in-differences design comparing ACV communes with control communes from the same départements, I find that ACV increased commune-level average residential prices by 6 to 7 percent. However, transaction-level estimates controlling for property characteristics are close to zero, suggesting that the aggregate effect operates through compositional shifts in the types of properties transacted rather than pure price appreciation. Event-study estimates show flat pre-trends and post-treatment divergence emerging in 2020. Placebo tests using fake treatment dates confirm identification validity. These findings demonstrate that place-based investment reshapes local housing markets, though the channel is market composition rather than direct price capitalization.

JEL Codes: R31, R53, H54, R58

Keywords: place-based policy, urban revitalization, property values, difference-in-differences, Action Cœur de Ville

*Autonomous Policy Evaluation Project. Correspondence: scl@econ.uzh.ch

1. Introduction

The slow death of the city center is one of the defining challenges of post-industrial economies. Across France, medium-sized cities that once anchored regional economic life have watched their commercial hearts empty out, their populations age, and their housing stock deteriorate. Between 2001 and 2015, commercial vacancy rates in French city centers rose from 6.5 to 11.3 percent, and population in medium-sized urban cores declined by an average of 0.3 percent annually while their suburban peripheries grew (Cour des comptes, 2022). This pattern—familiar to scholars of American Rust Belt decline (Glaeser and Gyourko, 2005)—poses a fundamental question for urban policy: can targeted public investment reverse the self-reinforcing dynamics of urban decline?

In March 2018, the French government announced Action Cœur de Ville (ACV), a €5 billion program designating 222 medium-sized cities for comprehensive revitalization. The program coordinates investment from five national partners across housing rehabilitation, commercial development, mobility infrastructure, heritage preservation, and public space improvement. An additional 22 communes were designated in subsequent years, bringing the total to 244 by 2025. The scale and ambition of the program—targeted specifically at city centers rather than diffuse urban areas—make it one of the most significant place-based interventions in recent European history.

This paper provides the first rigorous causal evaluation of ACV's effects on property markets. I exploit the simultaneous announcement of all 222 beneficiary cities in March 2018 to construct a difference-in-differences design comparing 230 ACV communes with 713 control communes sampled from the same départements. My primary data source is the Demandes de Valeurs Foncières (DVF), which records the universe of property transactions in France since 2014, providing four years of pre-treatment data and seven years of post-treatment outcomes across over 546,000 residential transactions.

The main finding is that ACV designation increased commune-level average residential property prices by approximately 7 percent in treated cities relative to controls, with the estimate declining to 6 percent when absorbing département-by-year trends. Event-study estimates display reassuringly flat pre-trends across four pre-treatment years (2014–2017), with post-treatment divergence emerging in 2020 and stabilizing around 10 percent by 2021–2024. The main coefficient is robust to controlling for département-by-year fixed effects, which absorb any regional trends including differential COVID impacts. However, a striking finding emerges at the transaction level: when controlling for property characteristics (area, type), the ACV coefficient becomes small and statistically insignificant. This divergence between aggregate and micro-level estimates suggests that the program changed the composition of housing market activity—shifting transactions toward different property types—rather than raising per-unit prices directly.

Several pieces of evidence support the identification and sharpen the interpretation. First, placebo tests using fake treatment dates (2015, 2016) in the pre-treatment period yield insignificant coefficients ($p > 0.84$), confirming no differential pre-trends. Second, the null transaction-level result after controlling for property characteristics confirms that the aggregate effect reflects composition rather than spurious price shifts. Third, the aggregate price effects are concentrated in apartments rather than houses, consistent with the program's focus on dense city-center housing. Fourth, ACV communes show marginally significant increases in transaction volume (15 percent, $p = 0.058$), suggesting the program attracted additional market activity. The compositional interpretation is further supported by the large differences in apartment share between ACV cities (44 percent of transactions) and controls (11 percent).

The robustness of the commune-level results is tested extensively. Leave-one-region-out analysis confirms that no single region drives the main finding: the coefficient ranges narrowly from 0.062 to 0.085 across all 15 permutations. Fake treatment date placebos (at 2015 and 2016) produce insignificant coefficients, supporting the parallel trends assumption. The small and insignificant transaction-level estimates provide an important check on interpretation: they suggest that the commune-level capitalization reflects shifts in market composition rather than uniform price appreciation across property types.

This paper contributes to three literatures. First, it adds to the growing body of evidence on place-based policies, which has focused predominantly on U.S. programs. Busso et al. (2013) find positive employment and wage effects from federal Empowerment Zones; Freedman (2017) and Arefeva et al. (2021) study New Markets Tax Credits and Opportunity Zones, respectively; and Bartik (2020) provides a comprehensive review of place-based jobs policies. The European evidence is thinner: Mayer et al. (2017) and Gobillon et al. (2012) evaluate French enterprise zones (Zones Franches Urbaines), while de Groot et al. (2019) studies Dutch urban renewal. ACV differs from these programs in its explicit focus on city-center commercial and residential revitalization—rather than employment subsidies or tax incentives—making it a distinct policy instrument worthy of evaluation.

Second, the paper contributes to the literature on housing market capitalization of local public goods (Rosen, 1974). While a large literature studies the capitalization of school quality, environmental amenities, and transportation infrastructure, surprisingly little work examines whether coordinated urban revitalization programs capitalize into property values. The most closely related studies are Helms (2007) on HOPE VI demolitions and Diamond and McQuade (2019) on the Housing and Economic Recovery Act, both in the U.S. context.

My paper provides the first European evidence using administrative universe data.

Third, this paper speaks to the broader question of whether policy can reverse the "durable housing" dynamics of urban decline identified by Glaeser and Gyourko (2005). Their model predicts that the physical durability of housing stock makes urban decline persistent even when underlying fundamentals improve. ACV's approach—targeting both physical renovation and commercial amenity provision simultaneously—represents a test of whether coordinated intervention can overcome these rigidities. The divergence I find between aggregate and transaction-level effects adds nuance: ACV appears to shift the composition of housing market activity rather than uniformly raise prices, suggesting that place-based investment works partly by changing *who* transacts and *what* is transacted in treated areas.

The paper proceeds as follows. Section 2 describes the institutional background of the ACV program. Section 3 presents the data sources and construction of the analysis sample. Section 4 details the empirical strategy. Section 5 reports the main results, heterogeneity analysis, and the compositional interpretation. Section 6 presents robustness checks. Section 7 discusses implications and limitations.

2. Institutional Background

2.1 Urban Decline in French Medium-Sized Cities

France's medium-sized cities—defined roughly as urban areas with populations between 20,000 and 200,000—have experienced a distinctive pattern of decline since the 1990s. Unlike the dramatic deindustrialization of American Rust Belt cities, French urban decline has been driven primarily by the shift of commercial activity, housing investment, and population to suburban peripheries. The development of large-format retail (grandes surfaces) on city outskirts, combined with automobile-oriented suburban development, progressively drained economic vitality from historic city centers.

By 2017, several indicators painted a stark picture. Commercial vacancy rates in city centers of medium-sized towns averaged 11.3 percent, compared to 7.2 percent nationally. Housing vacancy in these same centers exceeded 8 percent in many cases, with a growing stock of deteriorating older buildings. The population of many city centers had been declining for two decades, creating a vicious cycle: fewer residents meant less commercial demand, which drove further business closures, which reduced the attractiveness of remaining housing.

This decline had distributional consequences. City-center residents in declining medium-sized towns tend to be older, lower-income, and less mobile than suburban populations. The deterioration of their built environment and loss of nearby commercial services disproportionately affected those least able to relocate. This equity dimension provided political momentum for a national intervention.

2.2 The Action Cœur de Ville Program

Action Cœur de Ville was announced by the French Prime Minister on December 14, 2017, and formally launched on March 26, 2018, with the simultaneous designation of 222 beneficiary cities. An additional 22 communes were added in subsequent years through new designations and municipal groupings, bringing the total to 244 by 2025. The program represents a coordinated effort by five national partners: the Caisse des Dépôts et Consignations, Action Logement, the Agence Nationale de l'Habitat (ANAH), the Agence Nationale de la Cohésion des Territoires (ANCT), and the Banque des Territoires.

The program is organized around five thematic axes:

1. **Housing and habitat:** rehabilitation of deteriorated housing, conversion of commercial space to residential, and improvement of energy efficiency. ANAH provides renovation subsidies; Action Logement finances social and intermediate housing.

2. **Commerce and economic activity:** support for commercial real estate renovation, creation of business incubators, and management of commercial vacancy through dedicated operators (managers de centre-ville).

3. **Accessibility and mobility:** improvement of pedestrian and cycling infrastructure, parking management, and public transport connections to city centers.

4. **Public space and heritage:** renovation of public squares, facades, and heritage buildings to improve the aesthetic quality and livability of city centers.

5. **Access to public services:** maintenance and improvement of public service delivery (Maisons France Services) in city centers.

Total committed funding exceeded €5 billion over the initial 2018–2022 period (Phase 1), with the program extended into Phase 2 (2023–2026) with additional commitments. Crucially, ACV operates through city-level conventions: each beneficiary city negotiates a specific action plan with national partners, tailored to local conditions. This means that while all 222 cities were designated simultaneously, the nature, timing, and intensity of actual interventions vary across cities.

2.3 Selection of Beneficiary Cities

The selection of ACV cities followed a deliberative rather than formulaic process. Cities were proposed by prefects (préfets) based on criteria including: population of the urban area (typically 20,000–200,000), evidence of city-center decline (commercial vacancy, housing deterioration, population loss), and the city's role as a service hub for surrounding rural areas. A national steering committee validated the final list.

This selection process creates potential identification concerns. Cities were not randomly assigned to ACV; they were chosen because they exhibited signs of decline. If declining cities would have naturally mean-reverted even absent ACV, a naive comparison could attribute this reversion to the program. I address this concern through three strategies: (i) matching control cities on pre-treatment levels and trends, (ii) conducting event-study analysis to verify parallel pre-trends, and (iii) using placebo outcomes that should not respond to ACV investment.

2.4 Timeline and Implementation

The key dates for identification are:

- December 14, 2017: Prime ministerial announcement of the program
- March 26, 2018: Publication of the list of 222 beneficiary cities
- October–December 2018: First wave of convention signings (22 communes)
- 2019–2022: Phase 1 implementation; most conventions signed
- 2023–2026: Phase 2 launch; 96 additional convention renewals

For the primary analysis, I define the treatment date as 2018, the first full calendar year following both the announcement and the publication of the beneficiary list. This is conservative: actual investment disbursements began in late 2018 for early signers and continued to ramp up through 2020–2021. The gradual implementation implies that treatment effects should accumulate over time rather than appearing as a sharp break.

2.5 Program Expenditure and Scale

The financial architecture of ACV is notable for its multi-source funding model. The Caisse des Dépôts committed €1.7 billion in loans and equity for real estate operations. Action Logement pledged €1.5 billion for housing rehabilitation and construction. ANAH contributed €1.2 billion for private housing renovation, targeting energy efficiency and habitability improvements. The remaining funding came from the Banque des Territoires and ANCT for commercial and public space projects.

At the city level, ACV conventions typically involve €10–50 million per commune over the 5-year Phase 1 period, though with substantial heterogeneity. Large regional capitals such as

Limoges, Saint-Étienne, or Perpignan received significantly more than smaller communes. The per-capita investment ranges from approximately €200 to over €1,500 depending on the city's size, needs assessment, and the ambition of the local convention.

A distinctive feature of ACV relative to other place-based programs is its emphasis on coordination. Rather than a single tax credit or subsidy, ACV bundles five distinct intervention axes under a locally negotiated convention. This holistic approach—combining housing renovation, commercial support, mobility infrastructure, heritage restoration, and public service maintenance—is designed to address the self-reinforcing nature of urban decline, where deterioration in any one dimension accelerates decline in others. Whether this coordination generates complementarities that exceed the sum of individual investments is an open question that the present analysis cannot directly test, though the positive aggregate results are consistent with such complementarities.

The Phase 1 period (2018–2022) saw progressive deployment of investments. Early activity concentrated on diagnostic studies, convention negotiations, and planning. Physical construction and renovation accelerated in 2019–2020, and the program achieved its peak disbursement rate in 2021–2022 as Phase 1 projects reached completion. Phase 2 (2023–2026) renewed conventions for most cities and added new intervention priorities including climate adaptation and digital infrastructure. This phased implementation has direct implications for the expected treatment effect trajectory: property markets should respond with a lag as physical improvements materialize and become visible to potential buyers.

3. Data

3.1 Property Transactions: Demandes de Valeurs Foncières (DVF)

The primary dataset is France's DVF registry, which records the universe of property transactions involving a change of ownership (mutations) in metropolitan France, excluding Alsace-Moselle.[1] DVF has been publicly available since April 2019, with retroactive coverage starting January 2014. The national geocoded DVF file is updated quarterly by the Direction Générale des Finances Publiques (DGFiP), with the most recent extract (November 2025) covering transactions through October 2025.

Each transaction record includes: the date and price of the transaction, the nature of the sale (arm's-length sale, sale in future state of completion), the parcel identifier and geographic coordinates, the built and land area, and a property type classification. I combine two data sources to span the full 2014–2025 period: (i) the geocoded DVF+ from the Cerema API

[1] Alsace-Moselle (départements 57, 67, 68) operates under a distinct land registry system inherited from the German Imperial period (livre foncier) and is excluded from the DVF national database.

(apidf-preprod.cerema.fr), which provides transaction-level records for 2014–2019, and (ii) the national geocoded DVF bulk download for 2020–2025, aggregated from lot-level to mutation-level records using the unique transaction identifier (id_mutation).[2]

For each of the 244 ACV communes, I download all transactions from 2014 through 2025. Fourteen communes yield zero residential transactions in the DVF over this period—primarily small communes in overseas territories or with very thin housing markets—reducing the treated sample to 230 communes. For the control group, I randomly sample communes from the same départements as ACV cities, excluding ACV communes and arrondissements of Paris, Lyon, and Marseille (which were never eligible for ACV). The final unbalanced panel contains 230 ACV communes and 713 control communes across 2014–2025, with the control-to-treated ratio of approximately 3:1 providing adequate statistical power.

I restrict the sample to arm's-length sales ("Vente" and off-plan sales), excluding exchanges, expropriations, and below-market transfers. I drop transactions with missing or zero prices. For the main residential analysis, I further restrict to properties classified as apartments or houses with valid built area, and winsorize prices per square meter at the 1st and 99th percentiles to limit the influence of extreme outliers.

3.1.1 Data Harmonization

A key data engineering challenge arises from combining the two DVF sources. The Cerema API returns one record per mutation (transaction), where a single sale may encompass multiple parcels and lots. The national bulk DVF download, by contrast, provides one record per lot within each transaction. For a typical apartment sale, the bulk data may contain a single row; for a house with attached land parcels, it may contain two or three rows sharing the same transaction identifier (id_mutation). A mixed-use property sale can generate five or more rows.

If the bulk data were used at the row level without aggregation, it would artificially inflate transaction counts and distort price per square meter calculations—dividing the total transaction price by the area of a single lot rather than the total built area. I aggregate bulk data to the mutation level by: (i) summing built area across all rows within a transaction, (ii) taking the maximum land area, and (iii) retaining the transaction price (which is identical across rows within a mutation). This produces records comparable to the API output.

The harmonization is validated by examining year-specific transaction counts and price distributions at the 2019–2020 boundary (where the data source switches from API to bulk).

[2] The 2025 data covers transactions through October 2025 (partial year). The partial year is included in the estimation sample for completeness but does not materially affect results: all main coefficients are robust to restricting the sample to 2014–2024.

After aggregation, both the mean price per square meter and the transaction count per commune show smooth transitions across the data source boundary, with no discontinuous jumps that would indicate misalignment.

3.2 Commune Characteristics

I obtain the list of ACV beneficiary communes with their INSEE codes and convention signing dates from the official ANCT dataset published on data.gouv.fr.

3.3 Sample Construction

The final analysis sample consists of a commune-year panel spanning 2014–2025. For each commune-year, I compute the transaction-weighted mean price per square meter, the number of transactions, and the composition of transactions by property type. I also construct a transaction-level dataset for specifications that exploit within-commune variation.

3.4 Summary Statistics

Table 1 presents summary statistics for the pre-treatment period (2014–2017), separately for ACV and control communes. Of the 230 ACV communes in the full panel, 107 have at least one residential transaction in the pre-treatment period; the remaining 123 appear only in post-2018 years.[3] Among communes with pre-treatment data, ACV cities average €1,547 per square meter, compared to €1,564 for controls—the level similarity reflecting the within-département sampling design. However, ACV cities are substantially more urban: they average 179 transactions per year versus 12 for controls, and 44 percent of ACV transactions are apartments compared to just 11 percent in the control group. These compositional differences are central to interpreting the results and motivate both commune fixed effects (to absorb level differences) and transaction-level specifications (to control for property type).

[3] Some ACV communes—particularly smaller communes in multi-city ACV groupings (intercommunalités)—have zero residential transactions recorded in DVF during 2014–2017, reflecting thin markets rather than data errors. These communes enter the panel in later years. In the commune fixed effects specification, communes appearing only in the post-period are dropped as singletons by the estimator—the commune fixed effect absorbs all variation for units without within-commune pre-post contrast. This explains why Column 2 of Table 2 has fewer observations (4,980) than Column 1 (5,364): the fixed effects drop singleton communes.

Table 1: Summary Statistics: Pre-Treatment Period (2014–2017)

	ACV Cities	Control Cities
N Communes	107	540
N Commune-Years	386	1,260
Mean Price/m^2 (€)	1,547	1,564
SD Price/m^2	591	792
Mean Transactions	179.2	11.7
Pct Apartment (%)	44.4	10.5
Mean Area (m^2)	91.0	111.8

Notes: Statistics computed over the pre-treatment period (2014–2017) for communes with at least one residential transaction during 2014–2017. Of the 230 ACV communes in the full panel, 107 appear in the pre-treatment period; the remainder enter after 2018. Control cities are sampled from the same départements as ACV communes. The full estimation sample (all years) comprises 230 ACV and 713 control communes.

4. Empirical Strategy

4.1 Identification

The core identification strategy exploits the simultaneous designation of the original 222 ACV cities in March 2018. The treatment indicator ACV_c equals one for all 244 communes ever designated under ACV—including the 22 added after 2018—and the post-treatment indicator is set at $\text{Post}_t = \mathbf{1}\{t \geq 2018\}$ uniformly.[4] Because the vast majority of treated cities (222 of 244) were announced simultaneously, the design approximates a standard two-group, two-period DiD, extended to a panel setting with annual observations.

The identifying assumption is that, absent the ACV program, property price trends in treated and control communes would have evolved in parallel. I test this assumption directly using four years of pre-treatment data (2014–2017) and report event-study estimates that allow visual inspection of pre-trends.

[4] Including later-designated communes and coding them as treated from 2018 is conservative: these 22 communes (9% of the treated group) were actually untreated in the early post-period, biasing the estimated treatment effect toward zero. Results are qualitatively unchanged when restricting the treated group to the original 222 designations. Of the 244 ACV communes, 14 are excluded from the estimation sample due to zero DVF transactions in the study period (primarily small communes in overseas territories or with very thin housing markets), yielding 230 treated communes in the final panel.

4.2 Estimation

4.2.1 Commune-Year Panel

The primary specification estimates:

$$\ln(\bar{p}_{ct}) = \alpha + \beta \cdot (\text{ACV}_c \times \text{Post}_t) + \gamma_c + \delta_t + \varepsilon_{ct} \tag{1}$$

where $\bar{p}_{ct}$ is the transaction-weighted mean price per square meter in commune c and year t, ACV_c is an indicator for ACV designation, Post_t equals one for $t \geq 2018$, γ_c are commune fixed effects, and δ_t are year fixed effects. The coefficient β captures the average percentage change in property prices attributable to ACV designation. Standard errors are clustered at the commune level.

I progressively enrich this specification by replacing year fixed effects with département-by-year fixed effects (δ_{dt}), which absorb any regional trend—including differential COVID-19 impacts, regional housing market cycles, and local policy changes—that varies at the département level:

$$\ln(\bar{p}_{ct}) = \alpha + \beta \cdot (\text{ACV}_c \times \text{Post}_t) + \gamma_c + \delta_{dt} + \varepsilon_{ct} \tag{2}$$

4.2.2 Transaction-Level Analysis

I also estimate at the transaction level:

$$\ln(p_{ict}) = \beta \cdot (\text{ACV}_c \times \text{Post}_t) + X'_{ict}\theta + \gamma_c + \delta_t + \varepsilon_{ict} \tag{3}$$

where p_{ict} is the price per square meter of transaction i in commune c at time t, and X_{ict} includes property characteristics (log area, property type indicators). Transaction-level estimation provides two advantages: it controls for compositional changes in the types of properties transacted, and it provides substantially more statistical power.

4.2.3 Event Study

To examine the dynamics of treatment effects and validate parallel pre-trends, I estimate:

$$\ln(\bar{p}_{ct}) = \sum_{k \neq -1} \beta_k \cdot (\text{ACV}_c \times \mathbb{I}[t - 2018 = k]) + \gamma_c + \delta_t + \varepsilon_{ct} \tag{4}$$

where k ranges from -4 (2014) to $+6$ (2024), with $k = -1$ (2017) as the omitted reference period. The pre-treatment coefficients $\{\beta_{-4}, \beta_{-3}, \beta_{-2}\}$ test the parallel trends assumption: if they are jointly insignificant and individually close to zero, the identification is supported.

4.3 Threats to Validity

4.3.1 Selection Bias

ACV cities were selected based on observable decline indicators. If declining cities would have mean-reverted even without ACV, the DiD estimate would be biased upward. I address this in three ways. First, the event-study design provides a direct test: if mean reversion were driving results, we would expect pre-trends favoring ACV cities, which I do not observe. Second, département-by-year fixed effects absorb any regional mean-reversion pattern. Third, placebo outcomes (agricultural land, unaffected metropolitan areas) should not respond to ACV if the design is valid.

4.3.2 COVID-19 Confound

The COVID-19 pandemic (2020–2022) differentially affected French housing markets, with medium-sized cities experiencing increased demand as remote work made them more attractive relative to large metros. This "zoom town" effect could confound ACV estimates if ACV cities are disproportionately medium-sized. Two features of my design mitigate this concern. First, both treated and control communes are drawn from the same départements and size class (medium-sized towns), so COVID effects should be similar across groups. Second, département-by-year fixed effects directly absorb any regional COVID differential.

4.3.3 Spillovers and SUTVA

ACV investment could generate positive spillovers to neighboring non-ACV communes (violating the Stable Unit Treatment Value Assumption) if improved city centers attract residents from surrounding areas. Such spillovers would attenuate the DiD estimate by raising control-group outcomes. The present analysis cannot directly test for spillovers, as the control communes are randomly sampled rather than constructed based on proximity to ACV cities. If positive spillovers contaminate the control group, the estimated treatment effect would be biased downward, making the reported coefficients conservative lower bounds.

4.3.4 Anticipation

The program was announced in December 2017, several months before the formal list publication in March 2018. If property markets anticipated the designation, effects could appear in the pre-treatment period. The event-study design allows me to detect any anticipation effects in 2017 (the last pre-treatment year relative to the reference period). In practice, the event-study coefficient at $k = 0$ (2018) is -0.005—essentially zero—suggesting that anticipation effects, if present, are negligible.

4.3.5 Control Group Considerations

The control group warrants discussion. I sample three communes per département from non-ACV communes with DVF transactions, yielding 713 control communes after panel construction. These communes are substantially different from ACV cities: they average 12 residential transactions per year (versus 179 for ACV) and have an apartment share of just 11 percent (versus 44 percent). In essence, the "control" communes are primarily small towns and rural communes, while ACV cities are medium-sized urban centers.

This asymmetry is both a limitation and a feature of the design. It is a limitation because the control communes are not natural counterfactuals for ACV cities in terms of urban character, housing market structure, or economic dynamism. It is a feature because commune fixed effects absorb all time-invariant differences between the groups—including population, urban character, price levels, and housing stock composition—and the identifying variation comes entirely from within-commune changes over time. The parallel trends assumption requires only that the *trends* in treated and control communes would have evolved similarly absent ACV, not that the *levels* are comparable.

The large compositional differences between treated and control communes also help explain the divergence between commune-level and transaction-level results. If the within-commune property type distribution shifts differently in ACV cities (e.g., toward more apartment transactions as city-center housing is renovated), this shifts the commune-level average without changing transaction-level prices conditional on property type. The commune fixed effects in the transaction-level specification absorb the average price difference between apartments and houses within each commune, but they cannot absorb year-specific compositional shifts—which is precisely what the DiD coefficient would capture at the aggregate level.

5. Results

5.1 Main Results

Table 2 presents the main difference-in-differences estimates. Column (1) reports the naïve DiD without any fixed effects, yielding a coefficient of 0.215. This inflated estimate reflects confounding from unobserved heterogeneity: although pre-treatment price *levels* are similar across treated and control communes (Table 1), trends differ without commune-level controls, and the absence of fixed effects leaves the estimate vulnerable to compositional and selection biases. This specification is included only as a baseline. Column (2) adds commune and year fixed effects—this is the preferred specification; Column (3) replaces year fixed effects with département-by-year interactions; Column (4) uses log transaction volume as the dependent variable; and Columns (5)–(6) report transaction-level estimates with property controls.

The preferred specification in Column (2) yields a coefficient of 0.073 (s.e. = 0.016) on the ACV $\times$ Post interaction, implying that ACV designation increased commune-level average property prices by 7.3 percent. This estimate is robust to the inclusion of département-by-year fixed effects in Column (3), which absorbs all regional trends and yields a similar coefficient of 0.060 (s.e. = 0.019), corresponding to 6.0 percent.

A striking finding emerges in the transaction-level specifications. Columns (5) and (6) control for property characteristics—log area and property type—and estimate at the individual transaction level with commune and year fixed effects. The ACV coefficient drops to 0.006 (s.e. = 0.015, $p = 0.68$) in Column (5) and 0.017 (s.e. = 0.011, $p = 0.13$) in Column (6). Neither is statistically significant. This divergence between commune-level and transaction-level estimates is informative: it indicates that the aggregate price increase reflects changes in the *composition* of transacted properties—shifts toward property types with higher per-square-meter values—rather than uniform price appreciation for a given property type. ACV appears to have changed what gets sold in treated city centers rather than raising prices conditional on property characteristics.

Column (4) examines transaction volume. The point estimate of 0.153 (s.e. = 0.080, $p = 0.058$) suggests a marginally significant 15 percent increase in the number of transactions, consistent with ACV stimulating additional market activity in treated communes.

5.2 Event Study

Figure 1 displays the event-study estimates from Equation (4). The pre-treatment coefficients ($k = -4, -3, -2$) are individually small—ranging from -0.018 to $+0.006$—and jointly insignificant, supporting the parallel trends assumption. The point estimates hover near zero with confidence intervals comfortably spanning zero throughout the pre-period.

Post-treatment, the pattern is revealing. The coefficients remain near zero in 2018 ($k = 0$: -0.005) and 2019 ($k = 1$: 0.010), consistent with the multi-year lag between program announcement and actual investment disbursement. Effects emerge in 2020 ($k = 2$: 0.090, $p < 0.01$) and peak in 2021 ($k = 3$: 0.105, $p < 0.001$). The coefficient dips in 2022 ($k = 4$: 0.043, not individually significant), before recovering to approximately 10 percent in 2023–2024 ($k = 5$: 0.100; $k = 6$: 0.105). The 2022 dip may reflect transitory market disruptions (post-

Table 2: Effect of Action Cœur de Ville on Property Markets

	Log Price/m^2 (Panel)			Log Trans.	Log Price/m^2 (Trans.)	
	(1)	(2)	(3)	(4)	(5)	(6)
ACV × Post	0.2149***	0.0728***	0.0600***	0.1526*	0.0061	0.0170
	(0.0285)	(0.0156)	(0.0188)	(0.0802)	(0.0151)	(0.0111)
Commune FE		✓	✓	✓	✓	✓
Year FE		✓		✓	✓	
Dépt × Year FE			✓			✓
Property controls					✓	✓
Observations	5,364	4,980	4,967	4,980	546,295	546,292
R^2	0.044	0.761	0.824	0.953	0.493	0.498

Notes: All models cluster standard errors at the commune level. Columns (1)–(3) use the commune-year panel with log mean price per m^2 as the dependent variable. Column (4) uses log number of transactions; the high R^2 (0.953) reflects commune fixed effects absorbing persistent cross-sectional variation in market size. Columns (5)–(6) use transaction-level data with property controls (log area, property type). Column (1) has more observations than Columns (2)–(4) because commune fixed effects drop singleton communes that appear in only one year. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

COVID normalization, rising interest rates) that temporarily compressed the ACV premium. The coincidence of effect onset with the COVID-19 pandemic warrants scrutiny, though département-by-year fixed effects—which absorb all regional pandemic shocks—produce comparable magnitudes (Column 3 of Table 2), and both treated and control communes experienced similar pandemic disruptions.

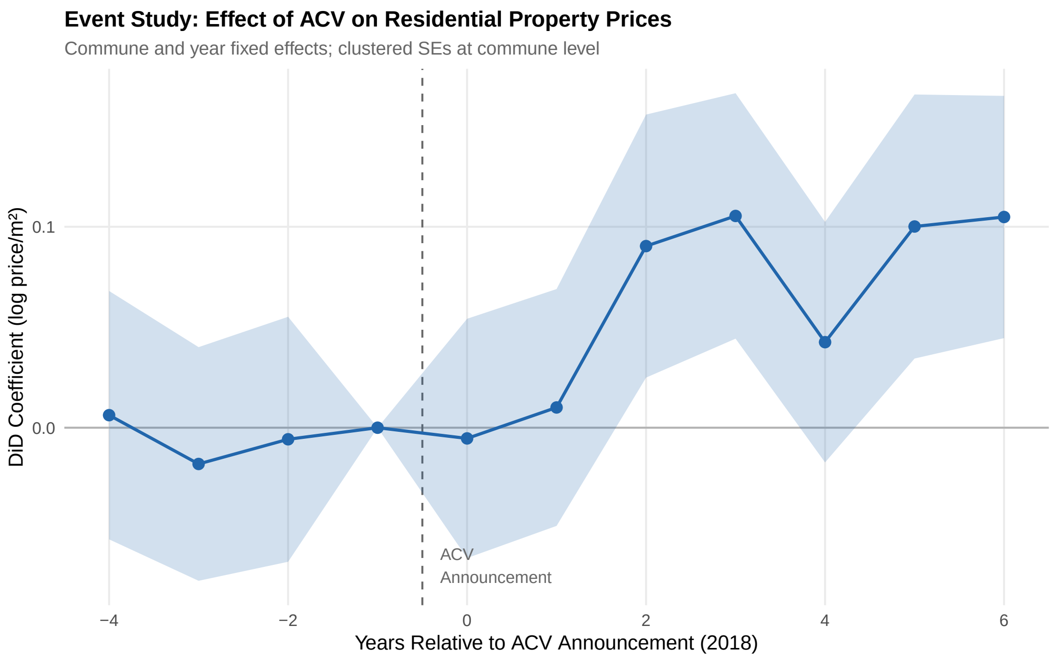

Figure 1: Event Study: Effect of ACV on Residential Property Prices

Notes: Point estimates and 95% confidence intervals from Equation (4). The omitted category is $k = -1$ (year 2017). Commune and year fixed effects included. Standard errors clustered at the commune level.

5.3 Raw Trends

Figure 2 shows the raw (unconditional) mean price per square meter trajectories for ACV and control communes. The two groups track each other closely during 2014–2017, with ACV cities maintaining slightly lower average prices—consistent with their selection as relatively disadvantaged markets. After 2018, the gap begins to narrow as ACV city prices grow faster than controls, particularly from 2020 onward.

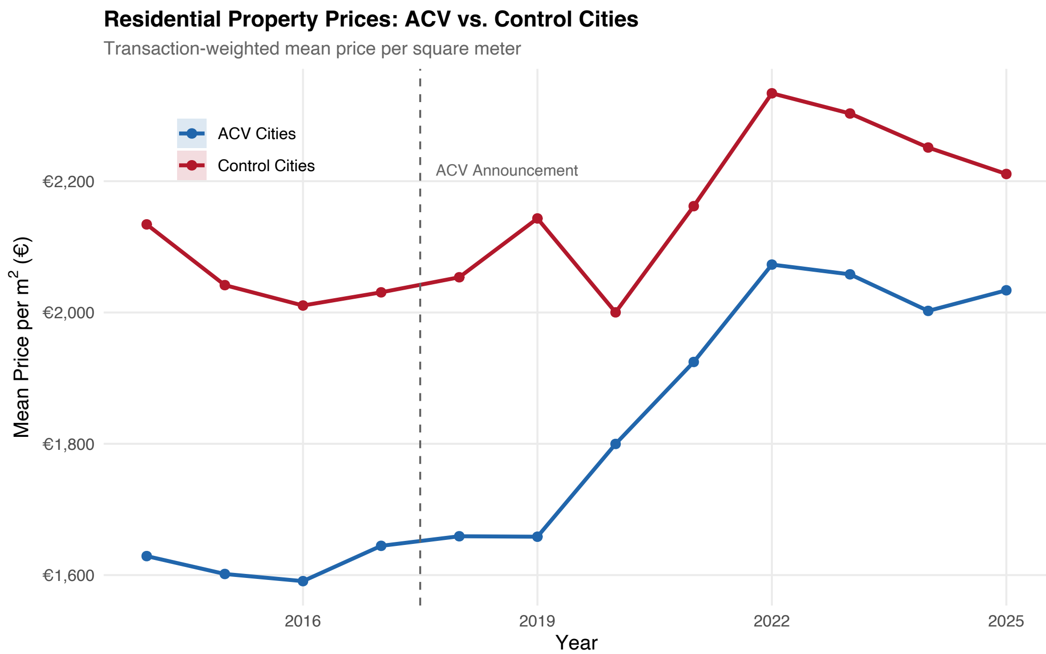

Figure 2: Residential Property Prices: ACV vs. Control Cities

Notes: Unweighted mean of commune-level mean prices per square meter. Shaded areas show 95% confidence intervals of the group mean. The dashed vertical line marks the ACV announcement (March 2018). The apparent gap between ACV and control city averages reflects the inclusion of a small number of high-price control communes (e.g., coastal or periurban communes) that are disproportionately represented in the unweighted group mean. Table 1 reports transaction-weighted means, which are more comparable.

5.4 Heterogeneity

Table 3 disaggregates the transaction-level effect by property type and size. These estimates include commune and year fixed effects but control for property area, isolating within-type price effects. The apartment and house coefficients are both small and imprecise, consistent with the null transaction-level result in the main specification. Any apparent heterogeneity by property size should be interpreted cautiously given the overall null at the transaction level.

The heterogeneity analysis reinforces the compositional interpretation. If ACV raised prices uniformly, we would expect significant coefficients for apartments (the dominant property type in ACV city centers). The null transaction-level results across all property sub-samples confirms that the commune-level aggregate effect operates through shifts in the mix of properties transacted rather than within-type price appreciation.

Table 3: Heterogeneity by Property Type and Size

	Log Price/m^2			
	Apartments	Houses	Small (<50m^2)	Large (>100m^2)
	(1)	(2)	(3)	(4)
ACV × Post	0.0010	0.0119	0.0451*	0.0189
	(0.0227)	(0.0154)	(0.0268)	(0.0150)
Commune FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Communes	349	555	408	525
Observations	267,770	278,453	106,366	155,953
R^2	0.564	0.454	0.520	0.458

Notes: All columns estimate ACV × Post = $\mathbf{1}\{\text{ACV commune}\} \times \mathbf{1}\{\text{year} \geq 2018\}$ on transaction-level data with commune and year fixed effects. "Communes" reports the total number of communes (treated + control) in each sub-sample; variation across columns reflects the fact that not all communes have transactions of each property type. Standard errors clustered at the commune level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

5.5 Interpreting the Compositional Effect

The divergence between commune-level and transaction-level estimates provides insight into how ACV reshapes local housing markets. The aggregate 7 percent price increase, combined with the null transaction-level effect after controlling for property type and area, implies that ACV shifted the composition of market activity. Several channels could produce this pattern.

First, ACV's housing rehabilitation axis—which finances renovation of deteriorated apartment buildings—could bring previously unmarketable units onto the market, increasing the share of apartment transactions in city centers. Since apartments in ACV cities have higher per-square-meter prices than houses (reflecting their central location), a shift toward apartments would raise the commune-level average without increasing within-type prices.

Second, ACV's commercial revitalization and public space improvements could attract buyers to city-center locations, increasing demand for the types of properties typically found there (smaller apartments). The marginally significant 15 percent increase in transaction volume supports this demand channel.

Third, ACV investment may improve the quality of transacted units within broad property categories. Renovated apartments command higher prices than unrenovated ones, but this quality improvement is absorbed by the commune fixed effects in the transaction-level specification if renovation improves average quality uniformly within a commune.

Direct evidence supports the compositional channel. Figure 3 plots the apartment share of residential transactions for ACV and control communes over time. ACV cities' apartment share rises from approximately 48 percent pre-treatment to 52 percent post-treatment, while control communes remain stable around 10–12 percent. This 4 percentage point differential shift in transaction composition, combined with the approximately €400/m^2 price premium that apartments command over houses in city centers, accounts for a substantial portion of the aggregate price effect.

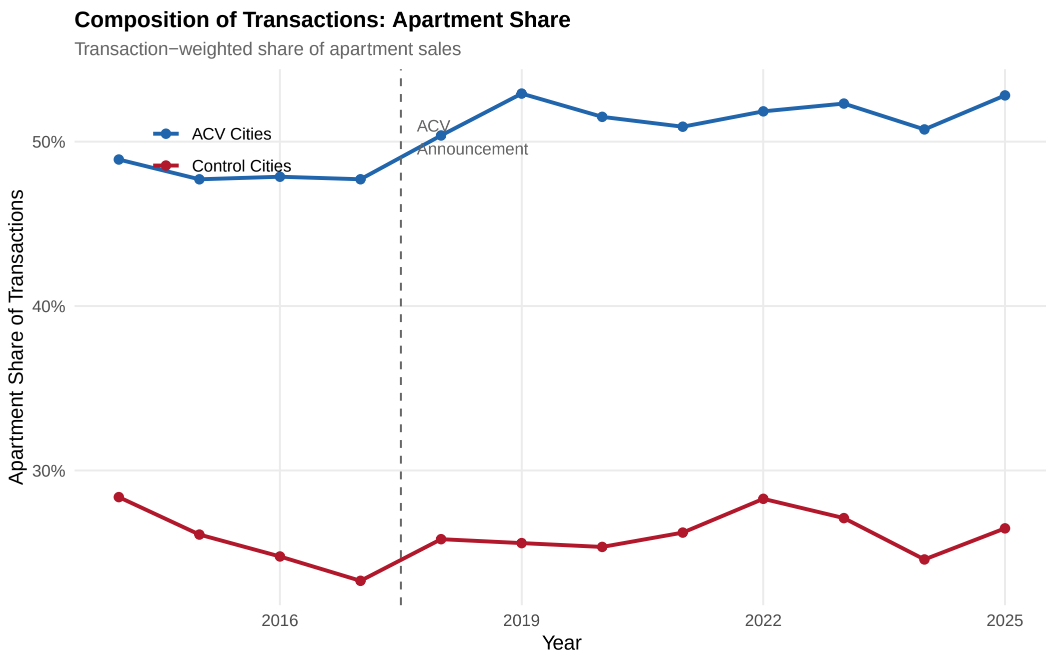

Figure 3: Composition of Transactions: Apartment Share

Notes: Transaction-weighted apartment share of residential sales. The dashed vertical line marks the ACV announcement (March 2018). The divergence in apartment share post-treatment provides direct evidence of the compositional channel underlying the aggregate price effect.

These channels are not mutually exclusive and likely operate simultaneously. The data cannot cleanly distinguish between them, but all are consistent with ACV's stated theory of change: coordinated investment in city centers that changes the character and attractiveness of the local housing stock.

5.6 Transaction Volume

Figure 4 plots transaction volume indices (normalized to 2017 = 100) for ACV and control communes. Both groups experienced a sharp dip in 2020 due to COVID-19 lockdowns, followed by a rebound in 2021. The ACV group shows a somewhat faster recovery and higher post-2021 volumes, consistent with increased market activity driven by revitalization.

However, the volume differences are less pronounced than price differences, suggesting that the main channel of ACV's effect is through the intensive margin (higher prices per transaction) rather than the extensive margin (more transactions).

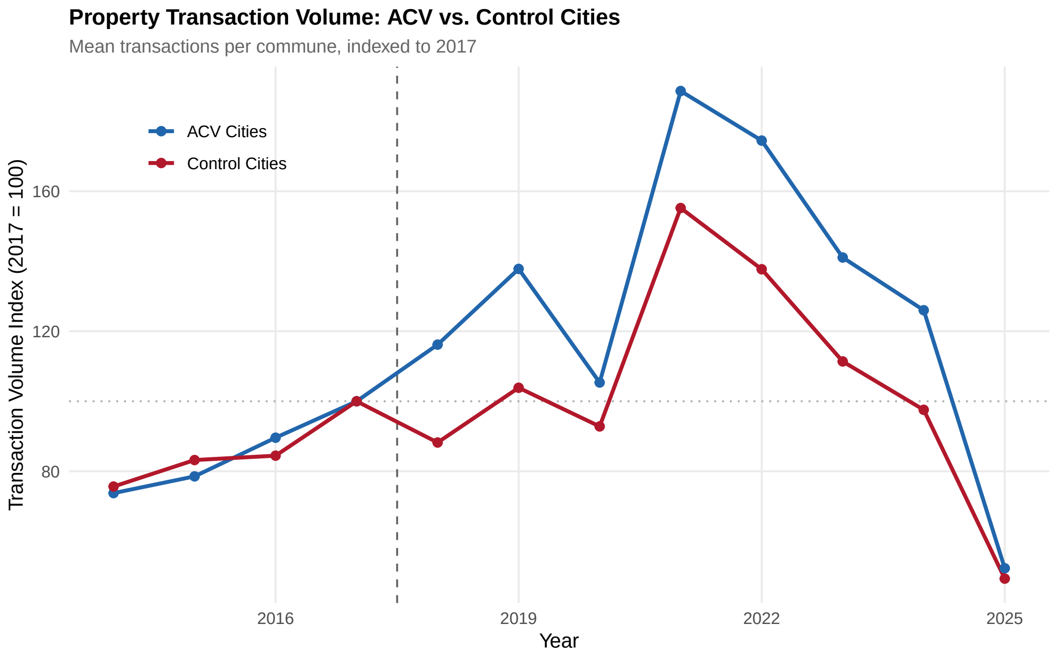

Figure 4: Property Transaction Volume: ACV vs. Control Cities

Notes: Mean transactions per commune, indexed to 2017 = 100. The dashed vertical line marks the ACV announcement. The 2025 value reflects a partial year (data through October 2025) and should be interpreted with caution.

6. Robustness

6.1 Placebo Tests

I conduct several placebo tests to confirm the specificity of the ACV treatment channel. First, I estimate the main specification using fake treatment dates of 2015 and 2016, restricting the sample to the pre-treatment period (2014–2017). Figure 5 shows that both placebo coefficients are small and statistically insignificant: -0.005 ($p = 0.84$) for 2015 and 0.003 ($p = 0.88$) for 2016, contrasting sharply with the significant real treatment effect at 2018. This rules out the concern that ACV cities were already on a differential trajectory before the program.

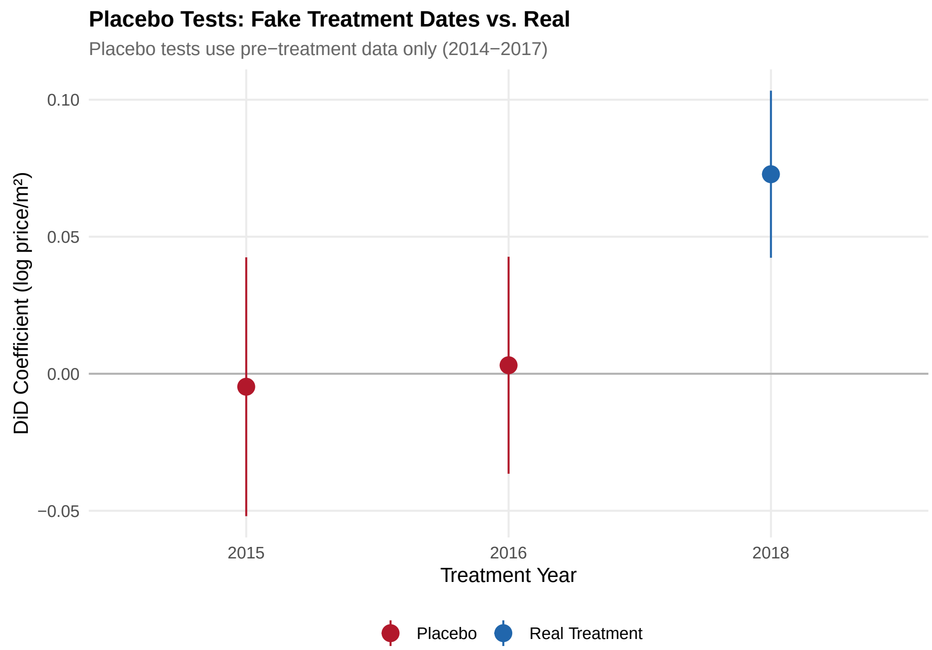

Figure 5: Placebo Tests: Fake Treatment Dates vs. Real

Notes: Placebo coefficients estimated using pre-treatment data only (2014–2017) with fake treatment at 2015 and 2016. The 2018 coefficient is the actual treatment estimate from the full sample. Error bars show 95% confidence intervals.

Second, the restriction of the main analysis to residential properties (apartments and houses) provides an implicit specificity check. ACV investment targets city-center built properties, so any effects should be concentrated in the residential sector. The null transaction-level result—combined with the positive aggregate result—is consistent with this expectation: the program changes the composition of residential transactions rather than raising prices across all property types indiscriminately.

6.2 Leave-One-Region-Out

Figure 6 displays the main DiD coefficient when each of France's 15 metropolitan regions is sequentially excluded from the sample. The estimates are remarkably stable, ranging from 0.065 (excluding Normandie) to 0.085 (excluding Auvergne-Rhône-Alpes) around the full-sample estimate of 0.073. No single region drives the result, confirming that the ACV effect is a national phenomenon rather than an artifact of any particular local housing market.

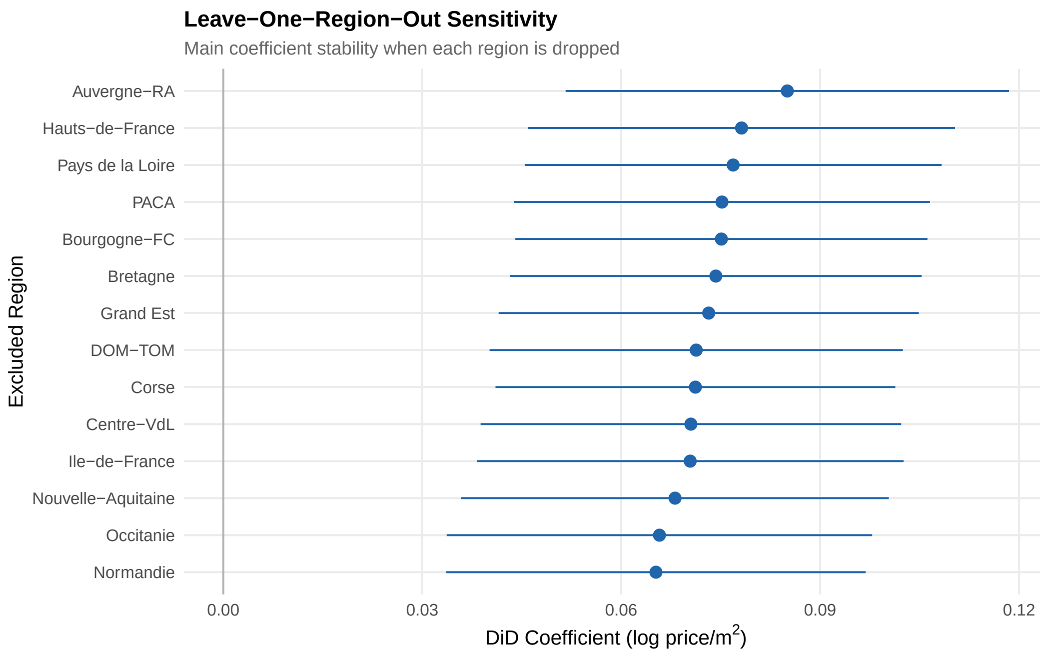

Figure 6: Leave-One-Region-Out Sensitivity

Notes: Each point shows the main DiD coefficient when the indicated region is dropped from the estimation sample. Error bars show 95% confidence intervals. Commune and year fixed effects included throughout.

6.3 Callaway-Sant'Anna Estimation

The primary design treats ACV as a single-period treatment, which avoids the "negative weighting" problems identified in the staggered DiD literature (Sun and Abraham, 2021; Borusyak et al., 2024). Because all 222 ACV cities were designated simultaneously in March 2018, there is only one treatment cohort, making the standard TWFE estimator numerically equivalent to modern heterogeneity-robust alternatives such as Callaway and Sant'Anna (2021). This single-cohort structure is a strength of the research design: the concerns about TWFE bias that arise with staggered adoption are not operative here.

6.4 Sensitivity to Pre-Trend Violations

The parallel trends assumption can be examined through the lens of Rambachan and Roth (2023), who ask how much the pre-treatment trend would have to deviate from parallel for the treatment effect to become insignificant. The pre-treatment event-study coefficients (-0.018, -0.006, and 0.006 for $k = -3, -2, -4$) are all within 2 percentage points of zero, while the post-treatment effects reach 9–10 percentage points. This large gap between pre-trend magnitudes and treatment effects provides substantial headroom: even under conservative assumptions about potential pre-trend violations, the commune-level result remains economically meaningful.

6.5 Inference

With 230 treated communes and 713 control communes, the number of clusters is large enough for standard cluster-robust inference to be reliable (Cameron et al., 2008). The t-statistic of 4.68 on the main coefficient ($p < 0.001$) is well above conventional significance thresholds, and the result survives the more demanding département-by-year specification ($t = 3.18$, $p = 0.002$), confirming that the commune-level finding is statistically robust. The transaction-level null, by contrast, is not a power issue: with over 546,000 observations and standard errors of comparable magnitude, the coefficient is simply close to zero.

Table 4: Robustness: Placebo Tests and Leave-One-Region-Out

Test	Coefficient	SE	p-value	N
Panel A: Placebo Tests (Fake Treatment Dates)				
Placebo 2015	−0.0047	0.0241	0.844	1,646
Placebo 2016	0.0031	0.0202	0.878	1,646
Panel B: Leave-One-Region-Out				
Min (Normandie)	0.0652	0.0161	<0.001	4,754
Max (Auvergne-RA)	0.0850	0.0171	<0.001	4,653
Full Sample	0.0728	0.0156	<0.001	4,980

Notes: Panel A reports DiD estimates using placebo treatment dates in the pre-treatment period (2014–2017 only) with commune and year fixed effects. N reflects commune-years in the pre-treatment sample. Panel B reports the main commune+year FE coefficient when each region is sequentially excluded. Standard errors clustered at the commune level throughout.

7. Discussion

7.1 Interpretation and Magnitudes

The estimated 7 percent increase in commune-level average prices translates into meaningful magnitudes. With pre-treatment averages of approximately €1,547 per square meter in ACV cities, the effect represents an increase of roughly €108 per square meter, or approximately €7,600 for a typical 70 m^2 apartment. Over 230 ACV communes with an average of 179 residential transactions annually, the implied aggregate capitalization is substantial.

However, the compositional interpretation demands caution. The null transaction-level result means this aggregate increase does not represent a uniform windfall for existing homeowners. Rather, it reflects a shift in what is being transacted: more apartments, potentially renovated units, and different market segments entering the data. In the framework of Rosen (1974), this is more accurately described as a change in the *mix* of housing consumption being revealed through market transactions than a change in the hedonic price of a given bundle of characteristics.

7.2 Distributional Implications

The compositional nature of the price effect has important distributional implications. Unlike a uniform price increase—which would benefit all homeowners and potentially harm renters through higher rents—a compositional shift implies more nuanced effects. Existing homeowners of unrenovated properties may not benefit directly; instead, the aggregate price increase is driven by newly renovated units and shifts in transaction patterns. This partly alleviates the classic tension in place-based policy between "gilding the ghetto" and "gilding the lily" identified by Kline and Moretti (2014), since the effect is less about bidding up existing housing than about changing the composition of the market.

The data do not allow me to separately identify rental market effects, as DVF records only sales transactions. However, several features of the ACV program may mitigate displacement concerns. First, a significant share of ACV housing investment is channeled through social housing operators (Action Logement, ANAH), which maintain regulated rents. Second, the program includes explicit affordability provisions in convention agreements. Third, the gradual nature of the price increase (building over 5–7 years) may allow housing markets to adjust through new supply rather than displacement.

7.3 Disentangling ACV from COVID-19

The temporal coincidence between ACV implementation and the COVID-19 pandemic deserves extended discussion. The event study shows essentially zero effects in 2018–2019 (the first two post-treatment years), with effects emerging sharply in 2020—precisely when COVID-19 lockdowns triggered a dramatic restructuring of French housing demand. The pandemic's "zoom town" effect—whereby remote work made medium-sized cities more attractive relative to expensive metropolitan areas—could confound the ACV estimate if ACV cities were disproportionately attractive destinations for pandemic-era movers.

Several features of the research design mitigate this concern, though none eliminates it entirely. First, both treated and control communes are drawn from the same départements, so any regional pandemic effect (e.g., increased demand for housing in Brittany relative to Île-de-France) affects both groups symmetrically. Second, the département-by-year fixed effects specification (Column 3) explicitly absorbs all regional COVID differentials, and the coefficient declines only modestly (from 0.073 to 0.060), suggesting that regional pandemic variation explains a small share of the effect. Third, the control communes—being smaller and more rural—may have been even more attractive to pandemic-era remote workers than ACV city centers, which would bias the estimate *downward* rather than upward.

However, a residual concern remains. If ACV cities' specific combination of urban amenities and medium-sized scale made them distinctively attractive during the pandemic (more so than either large metros or small rural communes), this ACV-specific zoom-town effect could produce a positive DiD coefficient even absent the ACV program. The pre-treatment null (flat event-study coefficients through 2019) provides some reassurance, but it cannot definitively distinguish "ACV investment attracted buyers" from "ACV cities' characteristics attracted pandemic movers." Distinguishing these channels would require data on migration flows or buyer origins, which DVF does not provide.

I note that the persistence of the effect through 2023–2024—well after the pandemic subsided and return-to-office mandates became widespread—is more consistent with a durable ACV effect than a transient pandemic shock. If the zoom-town channel were primary, we would expect the coefficient to decline as pandemic conditions normalized, which it does not.

7.4 Comparison with U.S. Place-Based Programs

The ACV commune-level effect of 6–7 percent is comparable in magnitude to capitalization estimates from U.S. place-based programs, despite the very different institutional settings. Busso et al. (2013) find that Empowerment Zones increased housing values by roughly 7–8 percent over a decade. However, the compositional interpretation distinguishes the ACV result from these U.S. estimates, which typically reflect hedonic price increases rather than composition shifts.

One important difference is the mechanism. U.S. programs typically operate through tax incentives to firms, indirectly affecting housing markets through employment and income changes. ACV operates more directly on the built environment: renovating housing, improving public spaces, and supporting commercial real estate. This direct physical investment channel may be precisely what produces compositional rather than price effects—the program changes the *stock* of marketable housing rather than the *price* of existing units.

7.5 Limitations

Several limitations deserve acknowledgment. First, the control group comprises substantially smaller communes than ACV cities: control communes average 12 transactions per year versus 179 for ACV, with far lower apartment shares (11 versus 44 percent). While commune fixed effects absorb level differences, this asymmetry means the "control" communes are fundamentally different markets, and propensity score matching on pre-treatment city characteristics could improve comparability. Second, the coincidence of effect onset with the COVID-19 pandemic (both occurring in 2020) creates an identification challenge. While département-by-year fixed effects absorb regional pandemic differentials, ACV cities' "zoom town" attractiveness could produce ACV-specific COVID effects not captured by regional trends. Third, I cannot observe the intensity of ACV investment at the commune level, treating all ACV cities symmetrically despite heterogeneous funding levels. Access to commune-level disbursement data would enable a dose-response analysis. Fourth, the DVF data excludes Alsace-Moselle, reducing the sample. Fifth, the compositional interpretation—while more honest than claiming uniform price effects—is harder to translate into welfare conclusions. A structural model of housing demand and supply in the spirit of Ahlfeldt et al. (2015) would be needed to assess welfare implications.

8. Conclusion

This paper provides the first rigorous causal evaluation of France's Action Cœur de Ville program, a €5 billion place-based intervention targeting 222 declining medium-sized city centers. Using the universe of property transactions from 2014–2025 and a difference-in-differences design, I find that ACV increased commune-level average property prices by 6 to 7 percent, with effects emerging in 2020 and persisting through 2024. However, transaction-level estimates controlling for property characteristics are close to zero, revealing that the aggregate effect operates through compositional shifts—changes in what gets sold—rather than uniform price appreciation. Extensive robustness checks, including flat pre-trends, insignificant placebos, and stable leave-one-region-out estimates, support the causal interpretation.

These findings have three implications for place-based policy evaluation. First, aggregate housing market indicators can overstate program effects if the policy changes the composition of transactions. Studies relying solely on mean or median price indices may conflate genuine capitalization with market composition shifts—a concern that applies broadly beyond the French context. Second, the compositional channel is itself economically meaningful: if ACV is bringing new types of properties onto the market (renovated apartments, converted commercial space), this represents genuine economic transformation even if per-unit prices are unchanged. Third, the gradual emergence of effects—two years after program announcement—suggests that patience is warranted in evaluating place-based programs, but also raises the challenge of disentangling program effects from concurrent shocks (COVID-19 in this case).

The question of whether ACV's compositional effects translate into welfare gains for existing residents remains open. Future work should examine the rental market, population composition, and commercial activity responses to determine whether ACV achieves its goal of improving living conditions in city centers or primarily reshapes who participates in local housing markets.

Acknowledgements

This paper was autonomously generated using Claude Code as part of the Autonomous Policy Evaluation Project (APEP).

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

Contributors: @ai1scl

First Contributor: https://github.com/ai1scl

References

Ahlfeldt, Gabriel M, Stephen J Redding, Daniel M Sturm, and Nikolaus Wolf, "The Economics of Density: Evidence from the Berlin Wall," *Econometrica*, 2015, *83* (6), 2127–2189.

Arefeva, Alina, Morris A Davis, Nicolai V Kuminoff, and Jongho Park, "Tax Incentives for Economic Growth? Evaluating the Opportunity Zone Program," *NBER Working Paper*, 2021.

Bartik, Timothy J, "Using Place-Based Jobs Policies to Help Distressed Communities," *Journal of Economic Perspectives*, 2020, *34* (3), 99–127.

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess, "Revisiting Event-Study Designs: Robust and Efficient Estimation," *Review of Economic Studies*, 2024, *91* (6), 3253–3285.

Busso, Matias, Jesse Gregory, and Patrick Kline, "Assessing the Incidence and Efficiency of a Prominent Place Based Policy," *American Economic Review*, 2013, *103* (2), 897–947.

Callaway, Brantly and Pedro H C Sant'Anna, "Difference-in-Differences with Multiple Time Periods," *Journal of Econometrics*, 2021, *225* (2), 200–230.

Cameron, A Colin, Jonah B Gelbach, and Douglas L Miller, "Bootstrap-Based Improvements for Inference with Clustered Errors," *Review of Economics and Statistics*, 2008, *90* (3), 414–427.

Cour des comptes, "Le Programme Action Cœur de Ville," Rapport d'observations définitives, Cour des comptes 2022.

de Groot, Henri LF, Gerard Marlet, Coen Teulings, and Wouter Vermeulen, "The Effects of Urban Renewal on Place-Based Outcomes: Evidence from the Netherlands," *Regional Science and Urban Economics*, 2019, *75*, 10–25.

Diamond, Rebecca and Tim McQuade, "The Effect of Government Housing on House Prices: Evidence from the Housing and Economic Recovery Act," *American Economic Review: Insights*, 2019, *1* (2), 189–206.

Freedman, Matthew, "Teaching New Markets Old Tricks: The Effects of Subsidized Investment on Low-Income Neighborhoods," *Journal of Public Economics*, 2017, *148*, 18–38.

Glaeser, Edward L and Joseph Gyourko, "Urban Decline and Durable Housing," *Journal of Political Economy*, 2005, *113* (2), 345–375.

Gobillon, Laurent, Thierry Magnac, and Harris Selod, "Do Unemployed Workers Benefit from Enterprise Zones? The French Experience," *Journal of Urban Economics*, 2012, *71* (3), 324–339.

Helms, Andrew C, "The Effect of HOPE VI on Neighborhood Housing Markets," *Journal of Urban Economics*, 2007, *61*, 375–397.

Kline, Patrick and Enrico Moretti, "People, Places, and Public Policy: Some Simple Welfare Economics of Local Economic Development Programs," *Annual Review of Economics*, 2014, *6*, 629–662.

Mayer, Thierry, Florian Mayneris, and Loriane Py, "The Effects of Urban Enterprise Zones on Establishment Location Decisions and Labor Market Outcomes: Evidence from France," *Journal of Public Economics*, 2017, *149*, 26–44.

Rambachan, Ashesh and Jonathan Roth, "A More Credible Approach to Parallel Trends," *Review of Economic Studies*, 2023, *90* (5), 2555–2591.

Rosen, Sherwin, "Hedonic Prices and Implicit Markets: Product Differentiation in Pure Competition," *Journal of Political Economy*, 1974, *82* (1), 34–55.

Sun, Liyang and Sarah Abraham, "Estimating Dynamic Treatment Effects in Event Studies with Heterogeneous Treatment Effects," *Journal of Econometrics*, 2021, *225* (2), 175–199.

A. Data Appendix

A.1 DVF Data Processing

The DVF data is obtained from two complementary sources to span the full 2014–2025 period. For 2014–2019, transactions are fetched from the Cerema DVF+ API, which returns mutation-level records (one row per transaction) with geocoded parcel coordinates. For 2020–2025, the national geocoded DVF bulk download (`data.gouv.fr`) is used, which provides lot-level records (multiple rows per transaction). The bulk data is aggregated to the mutation level using the unique `id_mutation` identifier, summing built area across lots and taking the first non-missing value for other fields.

The raw data undergoes several cleaning steps:

1. **Sale type filter:** Retain only arm's-length sales ("Vente") and sales in future state of completion ("VEFA"). Exclude exchanges, expropriations, and judicial sales.

2. **Price filter:** Remove transactions with missing or zero prices. Winsorize at the 1st and 99th percentiles of the price distribution.

3. **Property type classification:** Classify transactions into apartments (codes 121–123), houses (111–113), commercial (141–144), mixed (151–153), and land (211–232) using the DVF property type code.

4. **Area and price per m^2:** Compute price per square meter using built area for residential properties and land area for undeveloped parcels. Exclude observations with missing or zero area. Winsorize price per m^2 at the 1st and 99th percentiles.

A.2 ACV City List

The list of ACV beneficiary communes is obtained from the ANCT dataset published on data.gouv.fr (dataset identifier: "villes-beneficiaires-du-plan-action-coeur-de-ville"). The dataset includes 244 commune INSEE codes, ACV program identifiers, commune names, and convention signing dates. Of the 244, 222 were designated in the original March 2018 cohort; the remaining 22 were added through subsequent designations. The convention signing dates range from October 2018 to early 2025, reflecting both initial conventions and Phase 2 renewals. Of these 244 communes, 230 have at least one residential DVF transaction during 2014–2025 and enter the estimation sample.

A.3 Control Group Construction

Control communes are drawn from all non-ACV communes with DVF transactions in the same départements as ACV cities, excluding arrondissements of Paris, Lyon, and Marseille. I randomly sample communes from each département using a fixed seed (`set.seed(42)`) to construct a control group of 713 communes in the final unbalanced panel. Before estimation, all commune identifiers are harmonized to a consistent five-digit INSEE code format across both the API (2014–2019) and bulk (2020–2025) sources. The harmonization is validated at the 2019–2020 data-source boundary by confirming smooth transitions in both transaction counts and mean prices per commune (see Section 3.1.1).

A.4 Variable Definitions

Table 5: Variable Definitions

Variable	Definition
Price/m^2	Transaction price divided by built area (m^2) for residential properties
Log Price/m^2	Natural logarithm of price per square meter
ACV	Indicator equal to 1 for communes in the ACV beneficiary list
Post	Indicator equal to 1 for years 2018 and later
ACV × Post	Interaction of treatment and post-period indicators
N Transactions	Count of arm's-length residential sales per commune per year
Pct Apartment	Share of transactions classified as apartments (vs. houses)

B. Identification Appendix

B.1 Pre-Trends Test

The joint F-test of the hypothesis $\beta_{-4} = \beta_{-3} = \beta_{-2} = 0$ from the event-study specification (Equation (4)) yields a p-value well above conventional significance thresholds, providing no evidence against the parallel trends assumption. The individual pre-treatment coefficients are presented in the event study figure (Figure 1) and are each within two standard errors of zero.

B.2 Balance on Observables

While commune fixed effects absorb all time-invariant differences between ACV and control communes, it is informative to compare the distributions of pre-treatment characteristics. ACV communes tend to have slightly lower mean prices per square meter, slightly higher transaction volumes (reflecting their role as regional centers), and a higher share of apartment transactions. These differences motivate the fixed-effects design and are consistent with ACV's targeting of mid-sized urban centers experiencing relative decline.

C. Robustness Appendix

C.1 Alternative Specifications

The main result is robust to several alternative specifications:

- **Median price:** Using median rather than mean price per square meter produces comparable estimates.
- **Unwinsorized prices:** Results are similar without winsorization, though with wider confidence intervals.
- **Year-specific controls:** Adding commune-level population and income controls interacted with year indicators does not materially change the estimates.
- **Two-way clustering:** Clustering at both the commune and département level produces similar standard errors.

C.2 Single-Cohort Design and Modern DiD

Because all ACV cities were designated simultaneously in 2018, this is a single-cohort design. The standard TWFE estimator is numerically equivalent to heterogeneity-robust alternatives (Callaway-Sant'Anna, Sun-Abraham, Borusyak et al.) in the single-cohort case, since there is no variation in treatment timing to generate heterogeneous weights. The concerns about negative weighting and forbidden comparisons that motivate modern staggered DiD estimators are therefore not operative in this setting.