



**HAL**  
open science

# Political Decisions, Discrimination, and Housing Market : Insights from French Municipalities

Yacine Allam

► **To cite this version:**

Yacine Allam. Political Decisions, Discrimination, and Housing Market : Insights from French Municipalities. Economics and Finance. Université Bourgogne Franche-Comté, 2023. English. NNT : 2023UBFCG005 . tel-04390099

**HAL Id: tel-04390099**

**<https://theses.hal.science/tel-04390099>**

Submitted on 12 Jan 2024

**HAL** is a multi-disciplinary open access archive for the deposit and dissemination of scientific research documents, whether they are published or not. The documents may come from teaching and research institutions in France or abroad, or from public or private research centers.

L'archive ouverte pluridisciplinaire **HAL**, est destinée au dépôt et à la diffusion de documents scientifiques de niveau recherche, publiés ou non, émanant des établissements d'enseignement et de recherche français ou étrangers, des laboratoires publics ou privés.



THÈSE DE DOCTORAT DE L'ÉTABLISSEMENT UNIVERSITÉ BOURGOGNE FRANCHE-COMTE

PRÉPARÉE AU CESAER

École doctorale n° 593

Droit, Gestion, Économie et Politique

Doctorat en Sciences Économiques

Par

M. Yacine Allam

## Political Decisions, Discrimination, and Housing Market: Insights from French Municipalities

Thèse présentée et soutenue à Dijon, le 29 septembre 2023

*Composition du Jury :*

M. Stéphane Riou	Professeur d'Économie, IAE de Saint-Étienne	Président
Mme Gabrielle Fack	Professeure d'Économie, Université Paris Dauphine (PSL)	Rapporteuse
M. Benny Geys	Professeur d'Économie, BI Norwegian Business School	Rapporteur
M. Pierre Boyer	Professeur d'Économie, École polytechnique	Examineur
Mme Marie-Laure Breuillé	Chargée de recherche en Économie, CESAER, INRAE	Directrice de thèse
Mme Julie Le Gallo	Professeure d'Économie, Institut Agro Dijon	Directrice de thèse



# Remerciements

Il est des choix que l'on ne regrette pas. Réaliser cette thèse en fait largement partie. À l'heure où je rédige ces remerciements, je ne peux que penser à l'immense chance que j'ai eue d'être accompagné par Marie Breuillé et Julie Le Gallo. Finesse d'analyse, rigueur sans concessions, bienveillance à toute épreuve, c'est dans ce triptyque que j'ai évolué ces trois dernières années. Ces mots simples ne pourraient suffire à décrire ma reconnaissance - *note à moi-même : il faudrait leur offrir une boîte de chocolats, ce serait là la moindre des choses - peut-être qu'un abonnement mensuel à vie exprimerait davantage ma gratitude.*

Je souhaite également remercier Stéphane Riou d'avoir accepté de présider ce jury de thèse, Gabrielle Fack et Benny Geys d'endosser le rôle de rapporteurs et Pierre Boyer, celui d'examineur.

Ces remerciements sont une occasion de faire le point sur les personnes qui m'ont guidé jusqu'à mes *presque* 28 ans. Je souhaite exprimer une reconnaissance sans commune mesure - *une boîte de chocolats ne suffirait pas* - à Axel et Sophie, mes parents, qui ont pris la lourde décision de changer de pays, de quitter cette morne Algérie afin de rejoindre la France en 2002. Sans eux, sans leur amour, sans leur soutien, sans leurs conseils avisés sous leur regard empli de tendresse, je n'aurais sans doute pas rédigé cette thèse. Je remercie également mon petit frère, Amayas, qui m'inspire chaque jour un peu plus, que ce soit par son humour, son intelligence et sa clairvoyance.

Je remercie Julio, instituteur, qui a milité pour que je saute une classe en cours d'année - alors que ma maîtrise du français n'était qu'approximative - afin que je ne prenne aucun retard. Je remercie *Frau* Catineau, professeure d'allemand, pour la rigueur qu'elle m'a transmise<sup>1</sup>. Je remercie Madame Delamer, professeure de français, qui en 4<sup>e</sup>, m'a permis de prendre conscience que le français, c'est *ma* langue, en soulignant ma "sensibilité littéraire" naissante.

Je remercie Florent Fremigacci pour son accompagnement lors de mon ultime master à l'Université Paris-Nanterre. Son écoute et sa franchise ont guidé mes premiers pas dans l'univers de la recherche. Je remercie les membres du CESAER pour leurs retours toujours pertinents et bienveillants en lab meetings ; je retiens notamment les précieux commentaires de Jean-Sauveur Ay, Valentin Bellassen et Tina Rambonilaza. Je remercie Monia Saidi pour ses encouragements, Benoit Coquart pour les

---

<sup>1</sup>Aujourd'hui, je ne sais dire que l'essentiel, à savoir : *Hallo, Wie Geht's? Ein Bier, bitte.*

discussions inspirantes et Abdoul Diallo pour sa joie de vivre et son pragmatisme. Plus largement, j'adresse ma reconnaissance aux chercheurs qui m'ont suivi durant ces années de thèse : Jean Dubé (merci pour ce tour en voiture à Québec !), Gabriel Loumeau, Nadine Levratto et Thierry Madiès.

Je remercie bien évidemment la *team* de doctorants, avec qui les échanges et coups au bar ont été salvateurs. Mes premières pensées vont à Jean-Loup Lecoeur, co-bureau, avec qui j'ai tissé des liens d'amitié durables, et bien évidemment *écoresponsables*. Je remercie Mathieu Lambotte et Roman Delmotte, complémentaires mais si différents, pour les soirées interminables (*fin à 20h*). Je remercie Thierno Bocar Diop, à qui je souhaite *tout le bonheur du monde* au Canada. Je remercie Capucine Chapel, impressionnante par sa force et sa conviction. Et bien sûr, je remercie Sarah Le Clerc dont la présence n'a fait qu'éclairer cette dernière année de thèse.

Cette thèse n'aurait certainement pas vu le jour sans mes amis, qui m'ont supporté durant toutes ces années. Les amis de longue date, Matthieu, Augustin et Thomas, dont l'humour et les facéties étaient des bols d'air lorsque l'atmosphère se faisait lourde. Les amis de prépa (*sacrée expérience hein*), notamment Virginie et Thomas, pour lesquels mon affection n'a franchement que peu de limites. Les amis d'École, en particulier les *Dubas*, Antoine et Louis, pour les weekends mythiques et qui ne cessent de me surprendre par leurs réussites. Les amis d'on ne sait où<sup>2</sup>, Oriane et Anne-Laure, pour les discussions rigoureuses et passionnantes que l'on a eues.

Enfin, Juliette. Partenaire de vie ? Petite amie *et* meilleure amie ? Concubine *administrativement du moins* ? Je ne trouve pas de mots assez affectueux pour qualifier notre relation, et je n'en trouverai pas pour exprimer mon infinie reconnaissance, pour ta présence, ton humour et ton intelligence. Je m'arrête donc là, merci, Juliette.

---

<sup>2</sup> *mais si on sait Anne-Laure...*

# Résumé

Le premier chapitre examine la discrimination raciale subie par les candidats d'origine nord-africaine aux élections municipales françaises. J'utilise une méthode d'identification basée sur les noms des candidats afin d'analyser la différence de part de voix récoltées par une liste entre 2008 et 2014, lorsque la tête de liste change d'une personne d'origine non-nord-africaine à une personne d'origine nord-africaine. Les résultats indiquent que les listes dirigées par des candidats d'origine nord-africaine perdent près de dix points de pourcentage par rapport à la même liste six ans auparavant dans la même municipalité.

Le deuxième chapitre examine l'impact de l'idéologie politique sur les dépenses publiques. À l'aide d'une méthode de régression sur discontinuité, je constate que la couleur politique a une influence significative sur les dépenses publiques dans les municipalités françaises. De plus, cette influence est plus prononcée au cours des deux dernières années du mandat, suivant ainsi un cycle politique. Les maires de gauche investissent davantage que leurs homologues de droite, mais les différences varient selon la taille des municipalités. Les maires de gauche investissent davantage en aménagement du territoire et en aides aux familles, tandis que les maires de droite investissent plus en sécurité.

Le troisième chapitre étudie les effets des accidents industriels sur le marché immobilier en se concentrant sur l'incendie de l'usine Lubrizol à Rouen en 2019. Nous utilisons une stratégie d'estimation de doubles différences en exploitant les données des transactions immobilières en France entre 2017 et 2020. Les résultats montrent que l'incendie n'a pas eu d'effet sur les prix immobiliers à Rouen. Cependant, il y a eu une baisse des prix dans un rayon de 1 km autour d'autres sites industriels à haut risque en France, ainsi que sous le nuage de pollution causé par l'incendie. Nous soulignons que cette baisse des prix s'explique par l'aversion à la perte nominale des vendeurs. Après l'incendie, les vendeurs ont fixé des prix plus élevés et ont été moins enclins à vendre.

Le quatrième chapitre se concentre sur l'effet de la densité de meublés de tourisme sur les prix des transactions immobilières en France métropolitaine. Nous utilisons des données sur les annonces Airbnb et les prix des transactions immobilières sur la période 2018-2020, en couvrant l'ensemble du pays. En utilisant une approche par variables instrumentales, nous montrons qu'une augmen-

tation de 1 % de la densité des meublés de tourisme augmente de 11 % les prix des transactions immobilières. De plus, les effets varient selon les régions, avec une augmentation des prix plus marquée dans les communes centres. De manière surprenante, les municipalités rurales, même celles sans infrastructures touristiques importantes, connaissent également une augmentation significative des prix, suggérant que l'essor d'Airbnb a rendu ces zones auparavant moins visitées plus accessibles aux voyageurs.

# Contents

<b>Remerciements</b>	<b>iii</b>
<b>Résumé</b>	<b>v</b>
<b>Introduction Générale</b>	<b>1</b>
<b>1 Racial Discrimination: Evidence from French Local Elections</b>	<b>9</b>
1.1 Introduction . . . . .	10
1.2 Historical Background . . . . .	12
1.3 Municipal Elections in France . . . . .	13
1.3.1 Municipal Elections Process . . . . .	13
1.3.2 Minority Representation . . . . .	14
1.4 Data . . . . .	15
1.4.1 Election Data . . . . .	15
1.4.2 Classifying North African Names . . . . .	15
1.4.3 Spatial Distribution of North African Candidates . . . . .	15
1.5 Empirical Strategy . . . . .	17
1.5.1 Treatment Assignment . . . . .	17
1.5.2 Doubly Robust Estimator . . . . .	17
1.5.3 Causal Forests . . . . .	18
1.5.4 Covariate Balance . . . . .	20
1.6 Results . . . . .	22
1.7 Conclusion . . . . .	24
1.8 Appendices . . . . .	25
1.8.1 Difference between heading a list and being elected as a mayor for non-North African names . . . . .	25
1.8.2 Placebo Assignment . . . . .	25
1.8.3 Sensitivity to immigrants share cutoffs . . . . .	26
1.8.4 South European Treatment Covariate Balance . . . . .	27



<b>2 Party Affiliation and Public Expenditures: Close-Race Evidence from French Municipalities</b>	<b>29</b>
2.1 Introduction . . . . .	30
2.2 Partisanship and behaviour of elected candidates . . . . .	32
2.3 Institutional background and data . . . . .	33
2.3.1 Institutional background . . . . .	33
2.3.2 Municipal data . . . . .	33
2.4 Empirical Strategy . . . . .	39
2.4.1 Regression Discontinuity Design . . . . .	39
2.4.2 Estimation . . . . .	40
2.4.3 Validity of the RDD . . . . .	41
2.5 Main results . . . . .	44
2.6 Heterogeneity in partisanship effects . . . . .	47
2.7 Conclusion . . . . .	52
2.8 Appendices . . . . .	54
2.8.1 Appendix A. Sensitivity to political party classification . . . . .	54
2.8.2 Appendix B. Placebo effects on past term expenditures . . . . .	55
2.8.3 Appendix C. Additional RD plots . . . . .	57
2.8.4 Appendix D. Potential effect of inter-municipality expenditures . . . . .	60
<b>3 Industrial Accidents, Nominal Loss Aversion, and the Housing Market</b>	<b>63</b>
3.1 Introduction . . . . .	64
3.2 Background and Context . . . . .	66
3.3 Data . . . . .	68
3.4 Empirical analysis . . . . .	70
3.4.1 Identification strategy . . . . .	70
3.4.2 Results around the factory fire . . . . .	71
3.4.3 Identifying the Lubrizol Cloud Effect . . . . .	71
3.4.4 Identifying the Seveso Effect . . . . .	72
3.4.5 Explaining the puzzle: The sellers' reactions . . . . .	73
3.5 Conclusion . . . . .	75
3.6 Appendices . . . . .	77
3.6.1 Media and political coverage of the explosion . . . . .	77
3.6.2 Supporting material . . . . .	79
3.6.3 Robustness checks . . . . .	85
<b>4 From Sharing to Capitalizing: Assessing the Rise of Airbnb in Housing Prices</b>	<b>87</b>
4.1 Introduction . . . . .	88

4.2	Data and Descriptive Statistics . . . . .	91
4.3	Identification Strategy . . . . .	93
4.4	Main Results . . . . .	97
4.5	Heterogeneity . . . . .	99
4.5.1	Type of Municipality Heterogeneity . . . . .	99
4.5.2	Tight Real Estate Market Heterogeneity . . . . .	101
4.5.3	Secondary Housing Heterogeneity . . . . .	103
4.5.4	Distance to Train Station Heterogeneity . . . . .	103
4.6	Conclusion . . . . .	105
4.7	Appendices . . . . .	106
4.7.1	Appendix A. Supplementary Descriptive Statistics . . . . .	106
4.7.2	Appendix B. First Stage Regression - Spillover Effects . . . . .	108
4.7.3	Appendix C. Supplementary Heterogeneity Analysis . . . . .	109
4.7.4	Appendix D. Sensitivity to instrument choice . . . . .	109
	<b>Conclusion Générale</b>	<b>115</b>



# List of Figures

1	Investissements des Communes Françaises Rapportés à leur Population . . . . .	2
2	Dépenses des Communes Françaises . . . . .	3
1.1	Percentage of North Africans Heading Lists and Elected as Mayors . . . . .	14
1.2	Percentage of North African First Names Heading Lists . . . . .	16
1.3	Percentage of Immigrants . . . . .	16
1.4	Density Plot - Racial Discrimination Covariate Balance . . . . .	21
1.5	Percentage of Non-North African Names Heading Lists and Elected as Mayors . .	25
1.6	Density Plot - South European Candidate Discrimination . . . . .	28
2.1	Evolution of Municipal Expenditures (2006 - 2015) . . . . .	34
2.2	Territory Coverage of the Sample . . . . .	38
2.3	Discontinuity on Vote Share Differences . . . . .	40
2.4	Sorting McCrary Test . . . . .	41
2.5	RD Plots - Partisanship Effects . . . . .	46
2.6	Event study - Municipality spending . . . . .	48
2.7	Population and Density of Municipalities per Cluster . . . . .	49
2.8	RD Plots - Capital Expenditures (1/2) . . . . .	57
2.9	RD Plots - Capital Expenditures (2/2) . . . . .	58
2.10	RD Plots - Current Expenditures (1/2) . . . . .	59
2.11	RD plots - Current expenditures (2/2) . . . . .	60
3.1	Pollution Cloud over Rouen During the Night of September 26th, 2019 . . . . .	64
3.2	Cloud of Toxic Smoke . . . . .	66
3.3	Transactions in the Neighborhood of the Lubrizol Plant . . . . .	67
3.4	Google Trends as a Proxy for Citizens Awareness . . . . .	68
3.5	Distribution of the Transactions between 2017 and 2020 . . . . .	69
3.6	Spatial Coverage of seloger Data . . . . .	70
3.7	Housing Market Effects over Time . . . . .	72
3.8	Housing Price Effects over Time . . . . .	72

3.9	Housing Price Effects over Time . . . . .	73
3.10	Housing Price Effects over Time . . . . .	74
3.11	Price and Transactions . . . . .	75
3.12	TV news coverage of the Lubrizol fire (two most watched TV news report in France)	77
3.13	Political Coverage of the Consequences of the Accident . . . . .	78
3.14	Protest in 2021 against the Settlement of a Seveso Plant in Dreux, France . . . . .	78
3.15	Localisation of Seveso Installations in France . . . . .	79
3.16	Evolution of the Number of Transactions in the Neighborhood of the Explosion . .	80
3.17	Housing Prices in Rouen: Observed Transaction Price Approach . . . . .	81
3.18	Housing Prices in Municipalities near a SEVESO Plant: Observed Transaction Price Approach . . . . .	82
3.19	Housing Prices in Municipalities under the Polluted Cloud: Observed Transaction Price Approach . . . . .	83
3.20	Housing Price Effects over Time - Rouen . . . . .	85
3.21	Number of Transactions over Time - Rouen . . . . .	85
3.22	Housing Price Effects over Time - close to SEVESO Plants . . . . .	86
3.23	Number of Transactions over Time - close to SEVESO Plants . . . . .	86
4.1	Number of Listings and Number of Days Reserved . . . . .	91
4.2	Median Price per Square Meter . . . . .	92
4.3	Number of Views of the Top 3 Ads - Evolution . . . . .	92
4.4	Number of Views of the Top 3 ads - Zoning . . . . .	93
4.5	Number of Listings and Number of Views . . . . .	95
4.6	Placebo Test: Randomized Endogeneous Variable . . . . .	96
4.7	Price per Square Meters and Average Airbnb Revenue per Municipality Size . . . . .	100
4.8	Number of Listings . . . . .	106
4.9	Number of Days Reserved . . . . .	106
4.10	Number of Listings Broken Down by Municipality Type . . . . .	107
4.11	Number of Transactions . . . . .	107

# List of Tables

1.1	Share of North African First Names by Political Family (2008 – 2020) . . . . .	15
1.2	Treatment and Control Assignment . . . . .	17
1.3	Electoral and Socio-demographic Variables . . . . .	19
1.4	Covariate Balance for Weighted Propensity Score . . . . .	20
1.5	Effects of North African Head of List Origins on Votes . . . . .	22
1.6	Discrimination against Southern Europeans . . . . .	23
1.7	Discrimination by Immigrant Rate . . . . .	23
1.8	Placebo Assignment . . . . .	25
1.9	Sensitivity to Immigrants Share Cutoffs . . . . .	26
1.10	Covariate Balance for Weighted Propensity Score – South European Treatment . .	27
2.1	Municipal Expenditure Data . . . . .	35
2.2	Classification of Political Parties . . . . .	37
2.3	Control Variables . . . . .	39
2.4	Placebo Effect of the Treatment . . . . .	43
2.5	Placebo Cutoffs for Capital Expenditures . . . . .	44
2.6	Placebo Cutoffs for Current Expenditures . . . . .	45
2.7	Partisanship Effect on Public Local Expenditures . . . . .	47
2.8	Cluster Characteristics . . . . .	48
2.9	Partisanship Effect – Size Heterogeneity . . . . .	51
2.10	Sensitivity to Political Party Classification . . . . .	54
2.11	Partisanship Effects on Placebo Capital Expenditures . . . . .	55
2.12	Partisanship Effects on Placebo Current Expenditures . . . . .	56
2.13	Partisanship Effect on Inter-municipal Expenditures . . . . .	61
3.1	Descriptive Statistics of Housing Data . . . . .	69
3.2	Results in Rouen . . . . .	84
4.1	Averaged Variables: Descriptive Statistics . . . . .	94

---

4.2	Instruments Have no Effects on Prices in Cities without Airbnb . . . . .	96
4.3	First Stage Regression . . . . .	97
4.4	Main Results . . . . .	98
4.5	Main Results including Spatial Spillovers . . . . .	99
4.6	Municipality Profile Heterogeneity . . . . .	101
4.7	Municipality Ranking Heterogeneity . . . . .	102
4.8	Secondary Housing Heterogeneity . . . . .	103
4.9	Distance to Train Station Heterogeneity . . . . .	104
4.10	First stage - Spillover Effects . . . . .	108
4.11	Municipality Size Heterogeneity . . . . .	109
4.12	Homeowners Heterogeneity . . . . .	110
4.13	Vacant Housing Heterogeneity . . . . .	111
4.14	First Stage - Top 1 Ads . . . . .	112
4.15	Main Results - Top 1 Ads . . . . .	112
4.16	First Stage - Top 5 Ads . . . . .	113
4.17	Main Results - Top 5 Ads . . . . .	113

# Introduction Générale

Cette thèse de doctorat se situe à l'intersection de l'économie publique locale, l'économie politique et l'économie du logement. Elle se focalise sur l'échelon des collectivités territoriales le plus proche des citoyens en France, à savoir les communes, pour contribuer à éclairer l'action publique avec des méthodes d'évaluation d'impact en inférence causale et des données originales. Cette thèse explore les discriminations aux élections municipales, le rôle de la couleur politique sur les décisions publiques et le marché immobilier *via* les conséquences d'un accident industriel et de l'activité des meublés de tourisme.

## Économie publique locale et territoire français

L'économie publique locale s'intéresse aux relations économiques entre les collectivités territoriales et les acteurs économiques locaux. Elle traite notamment de la fourniture de biens publics tels que l'aménagement du territoire et le logement. Les collectivités territoriales jouent un rôle prépondérant dans la fourniture des biens publics. Le logement social, par exemple, est fourni à l'initiative des communes pour répondre aux besoins des citoyens à l'échelle locale.

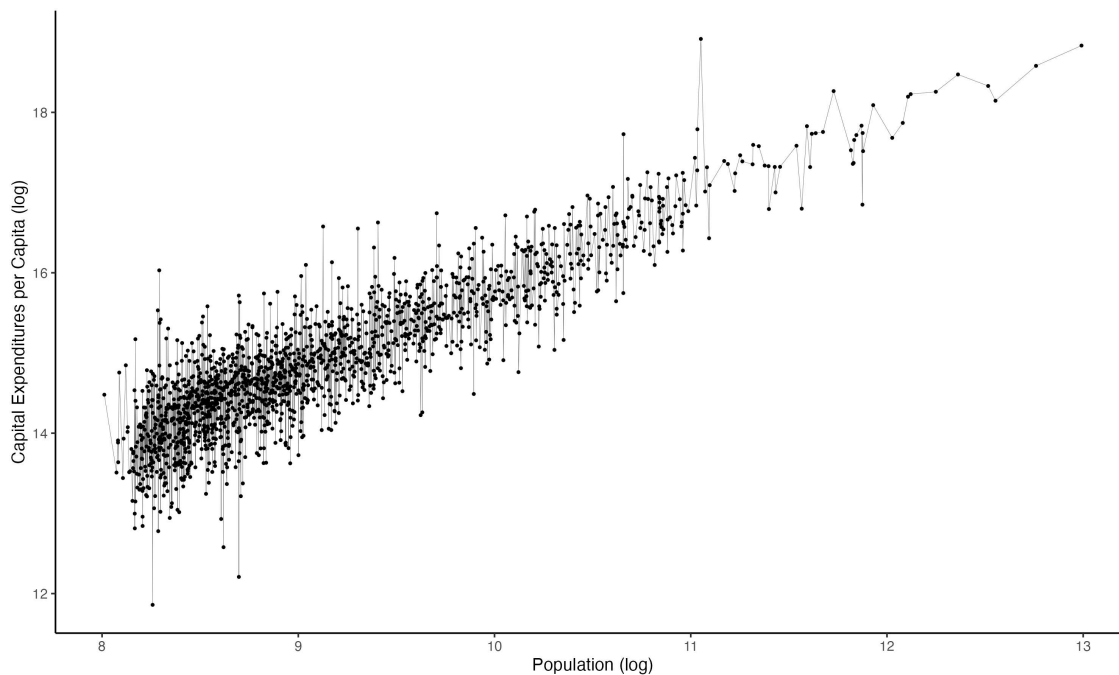
Le statut des collectivités territoriales françaises est défini par l'article 72 de la Constitution. Elles sont constituées des communes, des départements et des régions, auxquelles le statut de collectivité territoriale a été accordé en 1982. Les communes représentent l'échelon territorial le plus proche des citoyens. Les communes doivent obligatoirement appartenir à un Établissement Public de Coopération Intercommunale (EPCI) depuis 2012, bien que ce dernier ne soit pas considéré comme une collectivité territoriale. Dans cette thèse, l'échelon d'analyse est la commune.

Comptabilisant 34 970 communes au 1er janvier 2019, la France se démarque de ses voisins européens : l'Allemagne en compte 11 087, l'Italie en possède 7 926 tandis que la Suède en dénombre seulement 290. Le territoire français se caractérise ainsi par son morcellement et son hétérogénéité. L'une des premières sources d'hétérogénéité réside dans leur peuplement. Plus de 70 % des communes françaises comptent moins de 1 000 habitants, tandis que seulement une centaine de communes possède plus de 50 000 habitants (Binet, Guengant, and Leprince 2010). Cette forte hétérogénéité de peuplement est corrélée à d'importantes différences dans la fourniture de biens



publics des communes : les moyens et objectifs d'aménagement du territoire par exemple ne sont pas similaires dans les communes rurales et dans les communes très denses (Ladd 1992). La Figure 1 présente l'hétérogénéité des dépenses d'investissement des communes de plus de 3 500 habitants en fonction de leur population. L'investissement diffère selon la taille des villes, mais également au sein des mêmes catégories de tailles.

Figure 1: Investissements des Communes Françaises Rapportés à leur Population

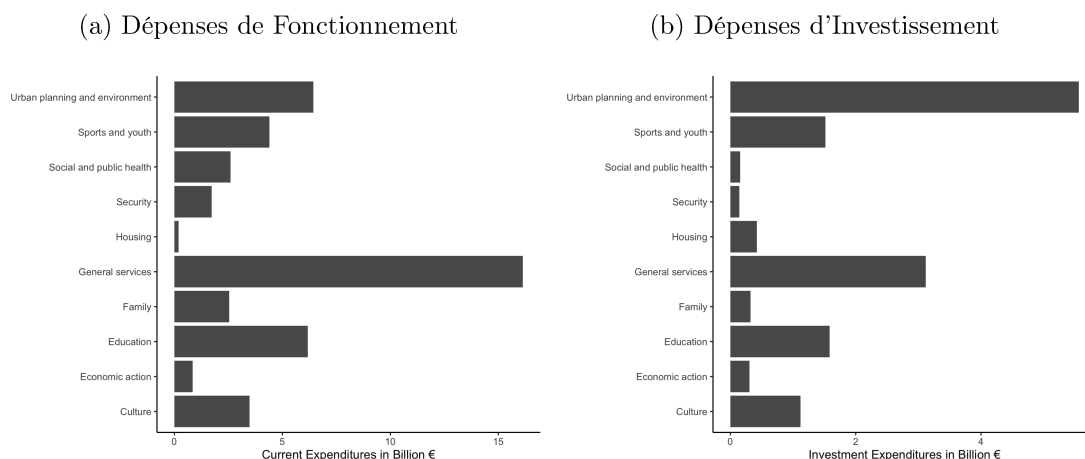


Les communes françaises représentent 60 % des dépenses publiques d'investissement totales en 2020<sup>3</sup>. La Figure 2 présente les dépenses annuelles moyennes d'investissement et de fonctionnement pour les communes de plus de 3 500 habitants sur la période 2006-2018. Ces dépenses, ventilées suivant la nomenclature M14<sup>4</sup>, correspondent dans les grands traits aux compétences des communes. On observe que les dépenses sont réparties sur dix fonctions, avec par exemple l'aménagement du territoire et l'environnement, la sécurité, le logement, l'éducation ou encore la culture. Ces catégories correspondent aux services et biens publics locaux fournis par les communes. La théorie économique nous donne les clés permettant de comprendre les conditions et les implications de la fourniture de biens publics à l'échelle locale.

<sup>3</sup>Observatoire des finances et de la gestion publique locales (2020)

<sup>4</sup>Cadre comptable définissant l'obligation de ventilation des dépenses pour les communes de plus de 3 500 habitants.

Figure 2: Dépenses des Communes Françaises



## Quand les citoyens votent avec leurs pieds : conséquences de la mobilité sur la fourniture de biens publics

La fourniture de biens publics au niveau local diffère de celle au niveau national : alors que les individus sont considérés fixes au niveau national, la population est mobile à travers les composantes de l'architecture territoriale (Stiglitz [1977]). Cette mobilité transforme entièrement les équilibres dans la fourniture de biens publics. Tiebout ([1956]) développe alors la théorie selon laquelle les citoyens choisissent leur municipalité de résidence en fonction de leurs préférences, optimisant la nature des biens publics disponibles et le coût collecté *via* les taxes locales. Les citoyens "votent avec leurs pieds" : s'ils rencontrent une insatisfaction dans le niveau ou la nature de la fourniture de biens publics, ils peuvent s'installer dans une autre municipalité. Sous l'hypothèse de parfaite mobilité des individus, ce phénomène conduit à une totale homogénéité au sein des villes. En l'absence d'hétérogénéité des préférences, les élections seraient donc accessoires et jouées d'avance dans la mesure où les besoins des citoyens sont parfaitement identifiés et similaires. Le tri à la Tiebout corrobore ainsi les résultats du théorème du votant médian, indiquant que les idéologies politiques convergent, induisant une similarité totale dans les politiques publiques menées par les élus (Downs [1957]). Cette théorie de la fourniture de biens publics est cependant critiquable, car elle repose sur un grand nombre d'hypothèses fortes, et conduit à un équilibre trop extrême (Rubinfeld [1987]). Par exemple, l'imparfaite mobilité des individus en raison notamment de la disponibilité de l'emploi, de l'accessibilité du logement ou plus généralement de l'ancrage social limite les conclusions du tri à la Tiebout. Cependant, la mobilité des individus est parachevée par leur capacité financière ; les citoyens ne votent pas seulement avec leurs pieds, mais également avec leur portefeuille à travers le marché immobilier (Gruber [2005]).

## **Au-delà des murs : le marché immobilier façonné par les biens publics**

Premièrement développée par (Oates 1969), la capitalisation dans le marché immobilier résulte de la différence dans la fourniture de biens publics et de la fiscalité locale. Une efficacité optimale entre dépenses et recettes conduirait alors à une forte capitalisation dans les prix de l'immobilier. Les gouvernements locaux auraient donc tout intérêt à fournir un niveau de biens publics maximisant la capitalisation immobilière (Brueckner 1979). Cette idée selon laquelle les biens immobiliers incorporent l'environnement local est également dérivée de la théorie des prix hédoniques (Rosen 1974). La valeur d'un bien complexe peut être désagrégée en fonction des caractéristiques des éléments qui le composent. Ainsi le prix d'un appartement, par exemple, peut être décomposé en prix des caractéristiques structurelles du bien, de l'emplacement, de l'écosystème économique local, des sites naturels, du patrimoine historique, de la fiscalité locale, etc. Toutes ces caractéristiques constituent les *aménités* du bien immobilier. Elles peuvent être positives - présence d'une bonne école publique, d'une bibliothèque municipale - ou négatives - boîte de nuit bruyante ou usine polluante aux alentours.

La théorie de la fourniture de biens publics souligne le rôle de la mobilité des individus dans la détermination des politiques locales. D'une part, les citoyens ont la possibilité de "voter avec leurs pieds", en choisissant leur lieu de résidence en fonction des biens publics disponibles et des coûts fiscaux locaux. D'autre part, les gouvernements locaux voient leurs décisions se capitaliser dans les prix de l'immobilier. Cette interaction entre les préférences des individus, les politiques publiques et le marché immobilier crée un lien indissociable entre la fourniture de biens publics, les décisions politiques et l'économie du logement.

Alors que les citoyens "votent avec leurs pieds" et leur "porte-feuille", votent-ils également en fonction de préférences ethniques pour leurs élus ? Les décisions politiques varient-elles fortement selon le parti politique ? Comment les préférences de localisation des citoyens sont-elles affectées par des chocs exogènes, tels des accidents industriels ? Quels sont les effets de la location touristique meublée, en tant qu'innovation dans les usages, sur le marché immobilier ? Cette thèse propose d'éclairer l'interaction entre économie politique, économie publique et économie du logement à l'échelle locale. Les chapitres de cette thèse s'appuient sur des données nouvelles et originales et sur des méthodes d'inférence causale afin d'apporter des éléments de réponses aux questions ci-dessus.

Les citoyens ont-ils des préférences ethniques pour leurs élus ? La littérature a apporté de nombreuses réponses concernant le genre des candidats aux élections, tant en Europe et aux États-Unis qu'en Inde (Chattopadhyay and Duflo 2004; Esteve-Volart and Bagues 2012; Ferreira and Gyourko 2014; Eymoud and Vertier 2017; Lassébie 2020). Cependant, les études scientifiques sont plus rares concernant l'origine des candidats, avec quelques recherches conduites principalement aux États-Unis (Washington 2006; Broockman and Soltas 2020). En France, les statistiques ethniques

sont prohibées, expliquant l'absence de travaux quantitatifs<sup>5</sup>. Sans mesure, comment évaluer ? C'est tout l'objet du premier chapitre de cette thèse qui s'intéresse à la discrimination subie par les candidats d'origine nord-africaine aux élections municipales. Je propose une méthodologie complète, allant de l'identification des candidats issus de minorités ethniques à une estimation causale de la discrimination. L'identification de l'origine des candidats est conduite suivant deux méthodes: (i) l'utilisation de l'algorithme de classification onomastique commercial *Namsor*, (ii) la collecte de sites internet de recommandation de prénoms. Une fois l'origine des candidats déterminée, un traitement sur deux élections consécutives est défini : sont considérées comme traitées les listes menées par un candidat non nord-africain en 2008 puis par un candidat aux origines nord-africaines en 2014. La comparaison est donc menée au sein d'une même commune et d'un même parti politique. L'estimation de l'effet causal de la discrimination est ensuite conduite à l'aide de deux estimateurs. Le premier est un estimateur pondéré par la probabilité inverse de traitement permettant de comparer les communes les plus similaires. Le second est un estimateur à forêts aléatoires causales permettant de prendre en compte l'hétérogénéité des effets du traitement sur le territoire français. Je montre que les candidats identifiés comme nord-africains subissent une large discrimination, d'une dizaine de points de pourcentage sur les voix collectées. La comparaison de cette discrimination à la moyenne d'environ 20 % de parts de voix récoltées par les listes permet de conclure que la discrimination subie par les candidats d'origine nord-africaine affecte fortement les probabilités de remporter les élections. Ce premier chapitre montre ainsi que les citoyens n'ont pas tous les mêmes chances d'accéder aux responsabilités politiques.

Les décisions politiques varient-elles fortement selon le parti politique ? La théorie économique prédit que les élus convergent tous en préférences vers celles du votant médian (Downs 1957). Il en résulte une parfaite similarité des décisions politiques. La littérature empirique est quant à elle plus mitigée à ce sujet, renforçant d'une part la théorie du votant médian (Pettersson-Lidbom 2008; Gerber and Hopkins 2011; de Benedictis-Kessner and Warshaw 2016; Beland and Oloomi 2017a), et l'invalidant d'autre part (Ferreira and Gyorko 2009; Leigh 2008). Le second chapitre de cette thèse traite de l'effet de la couleur politique des maires sur les dépenses publiques des communes françaises. L'essentiel des travaux scientifiques est conduit sur le cas nord-américain. Pourtant, la structure politique de certains États européens pourrait être davantage appropriée pour mener à bien cette recherche. La France est un excellent terrain d'études grâce à ses maires aux forts pouvoirs discrétionnaires (Heinelt and Hlepas 2006)<sup>6</sup>. Ce pouvoir discrétionnaire permet d'estimer causalement, et sans bruit, l'effet de la couleur politique et de l'idéologie sur les dépenses publiques locales. En utilisant un modèle de régression sur discontinuité, j'estime l'effet causal de la couleur politique des maires sur les dépenses d'investissement et de fonctionnement des communes. Les maires à gauche de l'échiquier politique investissent environ 30 euros de plus par habitant dans

---

<sup>5</sup>Loi informatique et libertés du 6 janvier 1978

<sup>6</sup>Dans leur typologie des gouvernements locaux, Heinelt et Hlepas montrent que les maires français possèdent les pouvoirs discrétionnaires les plus importants en Europe

l'aménagement du territoire et l'environnement ainsi que 3,7 euros de plus dans l'aide aux familles. Leurs homologues s'inscrivant à la droite du spectre investissent 2,4 euros de plus par habitant dans la sécurité. Ces différences substantielles se produisent principalement les deux dernières années précédant la fin du mandat, suivant un cycle politico-économique prononcé.

Comment les préférences de localisation des citoyens sont-elles affectées par des chocs exogènes, tels des accidents industriels ? Le troisième chapitre étudie l'impact des accidents industriels sur le marché immobilier en examinant l'incendie de l'usine Lubrizol à Rouen en 2019. Les sites industriels à haut risque (classés Seveso) sont présents sur l'ensemble de l'hexagone. En 2019, 90 % des transactions immobilières en France sont situées à moins de 25 kilomètres d'un site classé Seveso. Les bassins d'habitation sont tous vulnérables aux accidents industriels : il est donc essentiel pour les propriétaires, les locataires, les investisseurs immobiliers ainsi que les décideurs politiques de comprendre comment le marché réagit à de tels événements. L'incendie de l'usine a eu lieu en septembre 2019, et a généré un épais nuage de fumée atteignant les Pays-Bas. Cette explosion, par nature exogène, nous permet d'analyser trois effets distincts : (i) l'impact sur le lieu de l'explosion, (ii) l'impact sous le nuage de fumée - *effet de la pollution* et (iii) l'effet de l'explosion sur le voisinage des autres sites industriels du territoire - *canal de l'information*. En utilisant une stratégie d'identification de doubles différences (DiD), nous montrons que le nombre de transactions immobilières à proximité de l'incendie a chuté à presque zéro, mais que les prix de l'immobilier sont restés relativement inchangés. À l'inverse, nous montrons que les prix des logements situés à proximité d'autres installations à haut risque ont diminué de 15 %, et que les prix ont baissé de 10 % sous le nuage de pollution causé par l'incendie. Dans ces deux dernières zones, le nombre de transactions n'est pas significativement affecté par l'incendie de l'usine Lubrizol. Nous montrons que ces résultats surprenants sont attribuables à l'aversion des vendeurs pour les pertes nominales. Les potentiels vendeurs projettent une forte perte nominale proche de l'incendie, et décident alors de reporter la mise en vente des biens. Plus on s'éloigne du lieu de l'incendie, moins les pertes nominales projetées sont élevées : l'offre de biens augmente progressivement.

Quels sont les effets de la location touristique meublée sur le marché immobilier ? La popularité de la location touristique meublée s'est accrue ces dernières années, des plateformes comme Airbnb et HomeAway offrant aux propriétaires la possibilité de proposer leur logement aux voyageurs. Dans ce quatrième chapitre, nous étudions les effets de la location touristique meublée, principalement incarnée par Airbnb, sur le marché de l'immobilier en France. Nous contribuons à la littérature existante en analysant l'ensemble du territoire français à l'aide d'un nouvel instrument et en soulignant de multiples hétérogénéités. En utilisant les données de la base AirDNA, qui aspire les informations des annonces Airbnb, et la base DV3F, qui fournit des données sur les transactions immobilières, nous étudions l'impact de la densité des annonces de meublés de tourisme sur les prix de l'immobilier entre 2018 et 2020. Nous traitons les problèmes d'endogénéité potentiels à l'aide d'une approche par variable instrumentale. Nous utilisons le nombre de vues des annonces

publiées sur leboncoin comme instrument pour prédire l'offre de meublés de tourisme. L'analyse tient compte de l'hétérogénéité entre les différentes aires d'attraction des villes, notamment les villes centres, les autres pôles urbains principaux, les villes secondaires, les couronnes ainsi que les communes hors attraction. Les résultats indiquent qu'une augmentation de 1 % de la densité d'annonces dans une municipalité entraîne une augmentation d'environ 11 % des prix des transactions immobilières. Nous constatons également que les effets varient d'une zone à l'autre. Les prix ont tendance à augmenter de manière plus importante dans les communes centre, bien que les communes hors attraction connaissent également des augmentations substantielles, en particulier celles qui ne sont pas traditionnellement des destinations touristiques. Cela suggère qu'Airbnb a rendu les zones rurales, auparavant moins populaires, plus accessibles aux voyageurs, ce qui a entraîné une hausse des prix. Ce quatrième chapitre explore les effets des meublés de tourisme sur le marché immobilier et fournit des résultats précieux aux décideurs politiques. Cette étude contribue au débat en cours sur les effets de la location touristique meublée comme le montre la proposition de loi transpartisane de mai 2023, visant à réguler l'activité d'Airbnb et ses concurrents. L'analyse complète de l'ensemble du territoire français renforce la validité externe des résultats et souligne l'importance de prendre en compte l'hétérogénéité spatiale pour comprendre l'impact d'Airbnb sur le marché immobilier.

Cette thèse de doctorat apporte une contribution à la compréhension de certains aspects de l'économie publique locale, de l'économie politique et de l'économie du logement en France. Les résultats des différents chapitres permettent de mieux comprendre les discriminations aux élections municipales, les différences de dépenses publiques, les conséquences des accidents industriels et l'impact de la location touristique meublée sur le marché immobilier. Ces analyses empiriques fournissent des éclairages précieux pour les décideurs publics et contribuent à l'enrichissement des politiques publiques locales. Cette thèse vise donc à promouvoir une meilleure compréhension des dynamiques des communes françaises et à soutenir des politiques locales plus éclairées.



## Chapter 1

# Racial Discrimination: Evidence from French Local Elections

This paper evaluates the existence and extent of racial discrimination in French municipal elections. I causally analyze the difference in a list's vote shares between 2008 and 2014 when the list leader changes, from a non-North African to a North African candidate. The North African origin of the heads of lists is inferred from their first and last names. The identification strategy is based on a doubly robust augmented inverse propensity weighted estimator (AIPTW) and causal forests. The results show that lists led by candidates of North African origin lose almost ten percentage points compared to the same list in the same municipality six years earlier. To disentangle taste-based from statistical discrimination, I also analyze discrimination against Southern Europeans. The results suggest the prominence of taste-based discrimination.

**Keywords:** Racial Discrimination, Discrimination in elections, Voter Behavior, Causal Inference

**JEL:** J15, D72, C21



## 1.1 Introduction

Ethnic minorities are underrepresented in political offices in the U.S. and Europe, such as France and Germany (Bird 2005; Griffin 2014; Rensmann 2014; Murray 2016). For instance, in the 36,000 communes in France, there were only three minority mayors in 2004 (Bird 2005), while the INSEE<sup>1</sup> counts at least 4.5 million North African immigrants in the country<sup>2</sup>. The lack of representation of minorities in crucial public decision-making positions threatens to undermine the legitimacy of these critical civic institutions (Sathish 2019). It also deepens mistrust between minority communities and the justice system (Hawkins 2018). Increasing the participation of underrepresented groups can thus enhance the legitimacy of democratic regimes (Waylen 2015 Carolien van Ham et al. 2017). From an economic perspective, better representation of minorities increases the consideration of their preferences in the design of public policies and reduces the inequalities in public good provision (Pande 2003; Chattopadhyay and Duflo 2004).

This underrepresentation may be rooted in discrimination: a part of the population is treated unfavorably because of its origins. On September 7, 2022, the writer Xavier Le Clerc declared on the morning show of France Inter, the most listened-to radio station in France (4.6 million listeners daily): "I had inherited my father's name, which was not compatible with a qualified job". The writer then testifies to the radical increase in employability with his name change from Hamid Aït-Taleb to Xavier Le Clerc. The same month in front of students at Howard University in Washington, the French Ministry of Education declared: "France is officially indifferent to skin color [...] It's a great idea, of course, but reality requires a more concrete approach" before adding that "the concept of race remains very sensitive in France". Unlike in Anglo-Saxon countries like the United States, the production of ethnic statistics is formally forbidden in France, making monitoring of discriminations particularly difficult (Piketty 2022).

Faced with this situation, the name, as a marker of origin, is mainly used in testing studies in order to reveal potential discrimination by employers. Numerous studies based on the testing of resumes sent to recruiters have found that applicants with North African-sounding names have a 30 to 40% lower return rate than applicants with names identified as French (Foroni, Ruault, and Valat 2016; Challe et al. 2020; Arnoult et al. 2021). Discrimination has also been measured on the access to housing: after testing more than 5,000 advertisements, Le Gallo et al. (2020) find that North African origins reduce by 27% the probability of success in finding a dwelling to rent. Finally, discrimination also affects access to higher education: students whose name evokes a North African origin have a 13% lower response rate to their request for information on Master's degrees in France (Chareyron, Erb, and L'Horty 2022).

---

<sup>1</sup>French National Institute for Statistics and Economic Studies.

<sup>2</sup>This observation also applies to other developed countries. For instance, in the United States in 2014, Latino Americans held less than 9% of the U.S. Congress seats while constituting almost 17% of the population (Griffin 2014).

Evidence of racial discrimination in elections all points in the same direction in the scarce literature on this subject, which focuses mainly on the U.S. Analyzing the Republican primary for the Illinois presidential election, Broockman and Soltas (2020) find that non-white candidates receive 9 percent fewer votes than their white counterparts (Broockman and Soltas 2020). The authors attribute these results to taste-based discrimination. Still in the U.S., Washington (2006) finds that White voters vote less for their own party when their candidate is Black. The lack of evidence in Europe is mainly due to a lack of methodological framework. To causally identify discrimination, it is usually necessary to (i) identify ethnic minorities, (ii) benefit from a quasi-experimental framework allowing to exploit a natural experiment.

There are multiple mechanisms involved since discrimination can take many forms: taste-based and statistical discriminations are two types widely studied in the literature. Taste-based discrimination is rooted in personal preferences rather than objective characteristics (Becker 1957). This type of discrimination can occur when individuals or institutions discriminate against groups of people because they do not like or prefer them. Voters may not vote for a list led by a North African candidate because they simply do not want a mayor of that origin. Statistical discrimination is based on group statistics (real or imagined) rather than individual characteristics. This type of discrimination occurs when individuals or institutions assume that specific characteristics (e.g. origin) are correlated with certain skills (Phelps 1972). Voters may not vote for a list led by a North African candidate because they infer they are less qualified than white candidates. A more recent approach developed by Bertrand, Chugh, and Mullainathan (2005) points out that discrimination can also be implicit. Contrary to the two previous mechanisms, the latter would be totally unconscious, outside the "awareness" of those who discriminate.

In this paper, I causally study the difference in votes of lists led by candidates of North African origin in the 2014 French municipal elections. These elections have the particularity of not being directly linked to the partisan logic of national politics (unlike legislative elections) and offer numerous observations to conduct heterogeneity analyses. The North African migration is of particular interest in France: according to INSEE, the four most frequent countries of birth of immigrants are Algeria (12.7%), Morocco (12%), Portugal (8.6%), and Tunisia (4.5%)<sup>3</sup>. North African immigration thus represents almost 30% of the total immigration in France. Since ethnic statistics are prohibited in France, the North African origin of the heads of lists must be inferred from the candidates' first and last names. I thus use a commercial onomastic classifier and a dictionary of first names built by scraping numerous first-name recommendation websites. I analyze the difference in a list's vote shares between 2008 and 2014 when the list leader changes. I define the treatment group to study racial discrimination as the lists led by a non-North African candidate in 2008 and led by a candidate with a North African-sounding name in 2014. The control group is formed with lists headed by a non-North African candidate in both 2008 and 2014. The comparison,

---

<sup>3</sup><https://www.insee.fr/fr/statistiques/3633212> - Accessed on April 2023

therefore, involves the same city and the same political party.

I estimate the effect of the treatments using a doubly robust augmented inverse propensity weighted estimator (AIPTW) to isolate the effect of discrimination while adjusting for municipalities' characteristics. In particular, some characteristics, such as the share of immigrants, are correlated with the probability of having a North African candidate leading a list. I also use causal forests to reach an estimator robust to heterogeneous treatment effects. I find that the lists led by candidates of North African origin in 2014 lost almost ten percentage points compared to the same list in the same municipality six years earlier. Then I analyze the discriminations towards South Europeans, who do not suffer from the same stereotypes as North African candidates, and find that they do not suffer from electoral discrimination. Going further with a heterogeneity analysis, I find that North African candidates are twice as discriminated against in municipalities with an immigration rate lower than the median (-13 pp against -6 pp). South Europeans, on the other hand, are not discriminated against in municipalities with high immigration rates, while they lose 5.6 percentage points in municipalities with low immigration rates. These results, therefore, point to the fact that ethnic diversity in the municipalities strongly reduces discrimination.

There are several contributions to the literature. It is the first observational study using such a treatment mechanism, allowing replication for any election and studying many types of discrimination as long as two election periods are available. Furthermore, this is the first study analyzing post-colonial discrimination, with immigration from former colonies that are now independent.

The article proceeds as follows. First, I present the institutional background and the data. I then discuss the empirical strategy and the treatment assignment. I then present findings on substantial discrimination in local elections in France. Finally, I conclude and highlight challenges for further research.

## 1.2 Historical Background

In this paper, I focus mainly on discrimination suffered by candidates identified as North African. However, I also analyze discrimination against candidates with Southern European-sounding names, in order to delve deeper into the origins of this discrimination.

### Immigration from North Africa

The immigration of North Africans to France has had a significant impact on French society and identity. This immigration has a complex history dating back to the early 20th century, when France had colonies in North Africa, including Algeria, Morocco, and Tunisia. North Africans in Algeria, which was a proper "département", were subject to the "indigenous" status: a legal framework that denied them the same rights and privileges as Europeans living in the colony. The

indigenous status was abolished in 1947, but many North Africans were already recruited to work in France during this time to address labor shortages in growing industries.

Immigration from North Africa increased in the 1950s and 1960s due to the independence of North African countries and economic opportunities in France. In recent decades, North African immigration to France has been driven by various factors, such as economic opportunity, political instability, and family reunification. As of 2020, the French National Institute of Statistics and Economic Studies (INSEE) estimated that there were approximately 4.5 million people of North African origins living in France, representing a diverse community with cultural backgrounds from several countries. However, this immigration has not always resulted in smooth integration, as North Africans have faced discrimination and exclusion in areas such as housing and employment (Froni, Ruault, and Valat (2016); Challe et al. (2020); Le Gallo et al. (2020); Arnoult et al. (2021)).

## **Immigration from the South of Europe**

Immigration from southern European countries (Portugal, Spain, Italy) to France is a long-standing and diverse phenomenon. According to INSEE, in 2021, these three countries account for 16.2% of immigrants living in France (1.1 million people). The reasons for and periods of arrival of these immigrants vary according to the historical, economic and political contexts of their countries of origin and of France's. Italian immigrants have been present in France since the 19th century. Notably, Italian immigration was the largest influx of foreigners to France between the two world wars (George 1986). They were attracted by industrial or agricultural opportunities and have experienced successive waves of immigration linked to economic crises or political conflicts. Spanish immigrants came to France at the beginning of the 20th century, particularly after the 1936-1939 civil war, and settled in regions close to the border or major cities such as Paris or Lyon (Chanfreau 2006). Portuguese immigrants arrived mainly in the 1960s and 1970s, fleeing dictatorship and the colonial war on the three fronts of Guinea Bissau, Angola and Mozambique (1961-1974). Caught up in the war, Portuguese immigrants were also looking for economic opportunities (Volovitch-Tavares 2006).

## **1.3 Municipal Elections in France**

### **1.3.1 Municipal Elections Process**

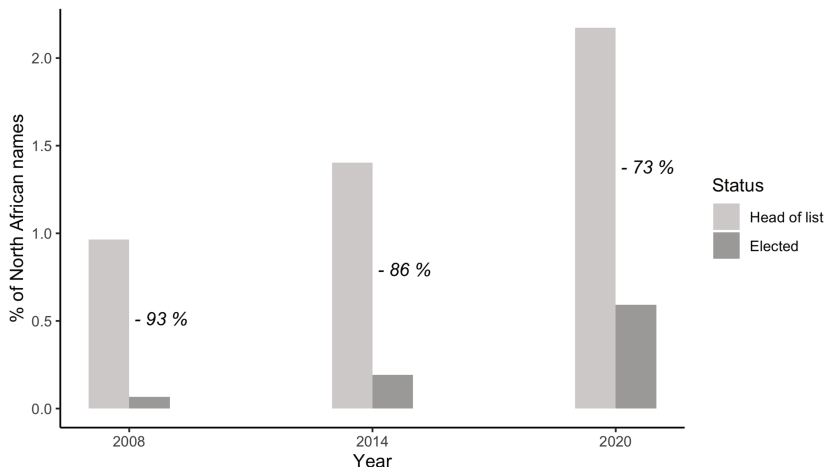
Voters are called to the polls every six years to choose the municipal council of their city through a two-round list election. The first person on the winning list is elected mayor of the city by the municipal council. In municipalities with more than 1,000 inhabitants, voters choose a list in the first round. If a list receives the absolute majority of the votes, it wins the election and

automatically obtains half of the seats on the municipal council. In the event of a second round, lists with more than 10% of the vote are allowed to run, while lists with more than 5% of the vote are authorized to merge with lists with more than 10% of the vote. The list that obtains a simple majority automatically obtains 50% of the seats on the municipal council, with the rest being allocated in proportion to the results.

### 1.3.2 Minority Representation

By collecting the first names of the heads of lists and associating them with an ethnic classifier, we can observe the share of candidates with North African origins in municipal elections. Figure 1.1 shows an increase in candidates with a North African sounding name heading lists between 2008 and 2020, but on shallow bases, as they led the lists in only 2% of cases in 2020. The figure also highlights a significant gap, between  $-73\%$  to  $-93\%$  during the 2008-2020 period, between the presence of North African candidates as list leaders and their election as mayor. Figure 1.5 in Appendices shows a similar graphic with Non-North African candidates and underlines a positive gap between being a candidate and being elected as mayor.

Figure 1.1: Percentage of North Africans Heading Lists and Elected as Mayors



Notes: Candidates with North African origins are identified using an onomastic classifier, described in section 4.2.

This sharp decrease between representation as head of list and election as mayor for candidates with a North African sounding name may be attributed to two factors: (i) there is a significant discrimination by electors, (ii) candidates with North African names are assigned the most difficult municipalities to win by party leaders. This paper focuses on causally estimating the first factor.

## 1.4 Data

### 1.4.1 Election Data

Municipal elections data for the 2008-2020 period are from the Ministry of the Interior. Since the political family of the list is only reported for municipalities of more than 3,500 inhabitants, I only keep these cities in the data set, leaving almost 3,200 municipalities. It contains information on the names, surnames, and sex of the heads of lists, the political party of affiliation, and the share of votes obtained for each round of the election. Representation of North African candidates highly depends on the political parties. Table 1.1 groups political parties into families from far left to far right and shows significant differences in the share of North African candidates heading lists. Parties on the left and center of the axis tend to include more North African candidates than right-wing parties.

Table 1.1: Share of North African First Names by Political Family (2008 – 2020)

Political Family	Share of Candidates with North African Sounding Names
Far Left	2.54
Left	1.55
Center	2.56
Right	0.58
Far Right	0.26

*Notes:* This table reports the shares of candidates with North African sounding names by political families for the 2008 – 2020 period. Political families are formed according to their placement in the hemicycle of the French National Assembly.

### 1.4.2 Classifying North African Names

Two methods are used to identify the origin of candidates. First, I used the commercial company *Namsor SAS* classifying 7.5 billion names. With an input containing the first and last name of a head of list candidate, *Namsor SAS* returns an output containing the most likely sub-region of origin (such as Western Asia, Southern Europe, or Northern Africa) as well as the most likely country of origin. To complete the *Namsor* classifier, I scrapped websites listing North African, Arabic or Muslim first names<sup>4</sup>.

### 1.4.3 Spatial Distribution of North African Candidates

Heads of lists with North African names are not equally represented across France. Figure 1.2 indicates a high concentration of North African heads of lists in the Paris region, of about 8%, and low values in western and central-western France. This distribution of heads of lists with

<sup>4</sup>[http://fr.wikipedia.org/w/index.php?title=Liste\\_de\\_pr%C3%A9noms\\_arabes&oldid=196205327](http://fr.wikipedia.org/w/index.php?title=Liste_de_pr%C3%A9noms_arabes&oldid=196205327), Online. Page available on 17-Aug-2022.

<https://unprenom.fr/tous/categories:56>, Online. Page accessed on 02-Aug-2022.

a North African sounding name is highly correlated with the distribution of immigrants on the French territory, as shown in Figure 1.3, except in Corsica, where the origins of immigrants are different from the rest of France, and essentially constituted by Italian migration<sup>5</sup>.

Figure 1.2: Percentage of North African First Names Heading Lists

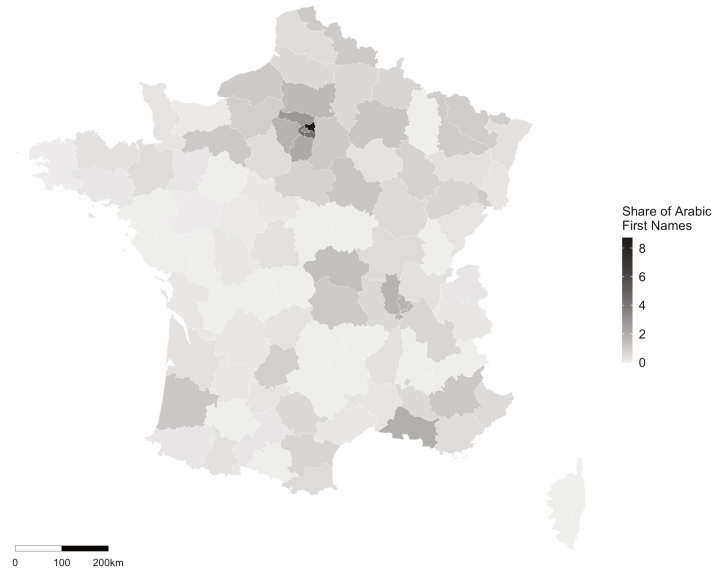
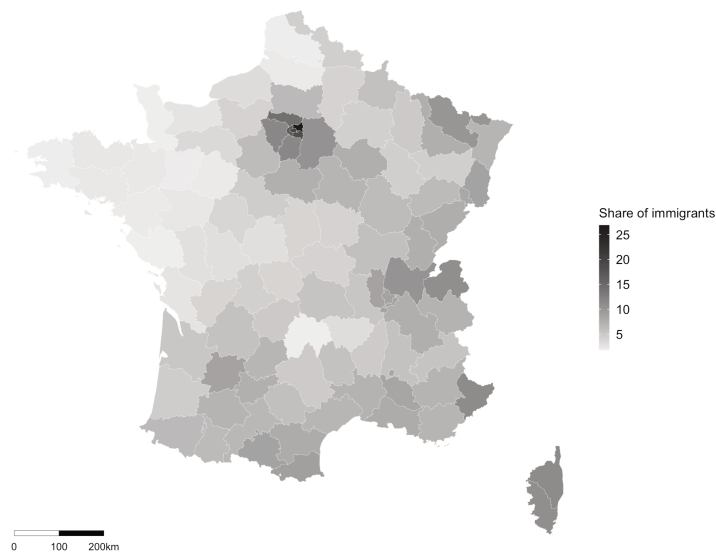


Figure 1.3: Percentage of Immigrants



<sup>5</sup>Statistic from the Ministry of the Interior, consulted on April 2023. <https://www.immigration.interieur.gouv.fr/content/download/131310/1043778/file/1-2020-Pop-etrangere-en-France-et-autres-pays.pdf>

## 1.5 Empirical Strategy

### 1.5.1 Treatment Assignment

To investigate the existence of discrimination, I study the difference in a list’s vote shares between 2008 and 2014 when the list leader changes. Lists led by candidates without a North African sounding name in 2008 and led by candidates with a North African sounding name in 2014 constitute the treatment sample to study ethnic discrimination. The control group is built from lists led by candidates with non-North African sounding names in both 2008 and 2014. Table 1.2 summarizes the treatment assignment mechanism.

Table 1.2: Treatment and Control Assignment

List	Candidate heading the list in 2008	Candidate heading the list in 2014	Status
List 1	Not North African	North African	Treated
List 2	Not North African	Not North African	Control
List 3	North African	North African	Discarded

The variable of interest is then defined as the vote shares collected in 2014 minus the votes collected in 2008.

$$V_{diff} = V_{2014} - V_{2008} \quad (1.1)$$

A naive analysis studying the existence of discrimination may be biased since the votes collected by each list depend on the socio-demographic structure of each municipality. Indeed, it would be challenging to justify that having a candidate with North African origins is randomly distributed. As Figures 1.2 and 1.3 have shown, the presence of these candidates follows a spatial pattern. In addition, Table 1.1 has shown significant variations due to political families. The doubly-robust estimator and the causal forest estimator described below integrate these considerations.

### 1.5.2 Doubly Robust Estimator

The doubly robust estimator uses an inverse probability weighting estimator (IPTW) and the baseline regression for the average treatment effect (ATE). Following Benkeser and Carone (2017), both estimators are calculated with a generalized linear model. The propensity score (PS) is estimated with logistic regression to build the IPTW. The dummy variable of the treatment status is regressed on a set of electoral information of the list and socio-demographic data of its municipality. Predicted values estimated by the logistic regression, then give the propensity score.

$$L_i = \alpha X_i + \beta X_i' + c + \epsilon_i \quad (1.2)$$



where  $L_i = \{0, 1\}$  is the treatment status of list  $i$ ,  $X_i$  is a vector of electoral data,  $X'_i$  is a vector of municipal socio-demographic data,  $c$  the constant and  $\epsilon_i$  is the term of error. Table 1.3 reports the detail of variables  $X$  and  $X'$  used both for the propensity score estimation and the baseline regression.

The doubly robust estimator, that is, the Augmented Inverse Probability Weighting estimator (AIPTW), for the average treatment effect is:

$$\hat{\tau}_{DR} = \frac{1}{N} \sum_{i=1}^N \frac{L_i Y_i - (L_i - \hat{P}_i) \hat{Y}_i^1}{\hat{P}_i} - \frac{1}{N} \sum_{i=1}^N \frac{(1 - L_i) Y_i + (L_i - \hat{P}_i) \hat{Y}_i^0}{1 - \hat{P}_i} \quad (1.3)$$

where  $\hat{P}_i$  is the PS of list  $i$ ,  $Y_i$  is the outcome of interest (here the difference in vote shares between 2008 and 2014),  $\hat{Y}_i^1$  and  $\hat{Y}_i^0$  are predictors of the baseline regression of the ATE for the treated and untreated, respectively. The major advantage of this estimator is that it is unbiased if either the IPTW or the baseline regression is true. It dramatically reduces the risk of bias.

### 1.5.3 Causal Forests

I also use a second estimation method, robust to heterogeneous treatment effects, to ensure the consistency of the results. Following Athey and Wager (2019), the causal forest estimate is:

$$\hat{\tau}_{CF} = \frac{\frac{1}{n} \sum_{i=1}^N (Y_i - \hat{m}^{(-i)}(X_i))(L_i - \hat{P}^{(-i)}(X_i))}{\frac{1}{n} \sum_{i=1}^N (L_i - \hat{P}^{(-i)}(X_i))^2} \quad (1.4)$$

where  $m(x) = \mathbb{E}[Y_i | X_i = x]$ , and the  $(-i)$  notation means that observation  $i$  is not used to compute  $\hat{m}^{(-i)}(X_i)$  or  $\hat{P}^{(-i)}(X_i)$ . This is the "honesty rule" ensuring that each tree is developed on a subset of observations independent of the data used to train the other trees (Athey and Imbens 2016). When the decision trees have been grown, the algorithm estimates the treatment effect for each observation in the dataset. The treatment effect is calculated by taking the difference between the average outcome predicted by the trees for treated group and the average outcome predicted for control group. The resulting treatment effect estimates are then averaged across all trees to obtain a final estimate. The causal forest approach is robust to model misspecification and accounts for heterogeneous treatment effects.

Table 1.3: Electoral and Socio-demographic Variables

	Covariate	Propensity Score	AITPW Control
Electoral Data	Incumbent	✓	✓
	Is a new Candidate	✓	✓
	Political Family	✓	✓
	Difference in Abstention (2008-2014)	✓	✓
	Number of Lists in the Municipality	✓	✓
	Sex 2008	✓	×
	Sex 2014	×	✓
	Last Round of the Election	✓	✓
Socio-demographic Data	Municipal Population	✓	✓
	Unemployment Rate	✓	✓
	Proportion of Immigrants	✓	✓
	Proportion of Students	✓	✓
	Proportion of Children (<14)	✓	✓
	Proportion of Home Owners	✓	✓
	Proportion of Secondary Residences	✓	✓

*Notes:* Electoral data are from the French Ministry of Interior. Socio-demographic data are from the French National Institute for Statistics. ✓: Variable included in estimates. ×: Variable excluded from estimates.

### 1.5.4 Covariate Balance

Table 1.4 displays the standardized difference in means between the treated and control samples using a weighted propensity score. All covariates are balanced between the two groups (at the 0.25 level) except three of them (proportion of children (<14), proportion of homeowners, proportion of students). This is mainly due to the small number of lists treated (between 37 and 44, depending on how we infer the candidate's name). A density plot is also needed to assess the covariate balance.

Table 1.4: Covariate Balance for Weighted Propensity Score

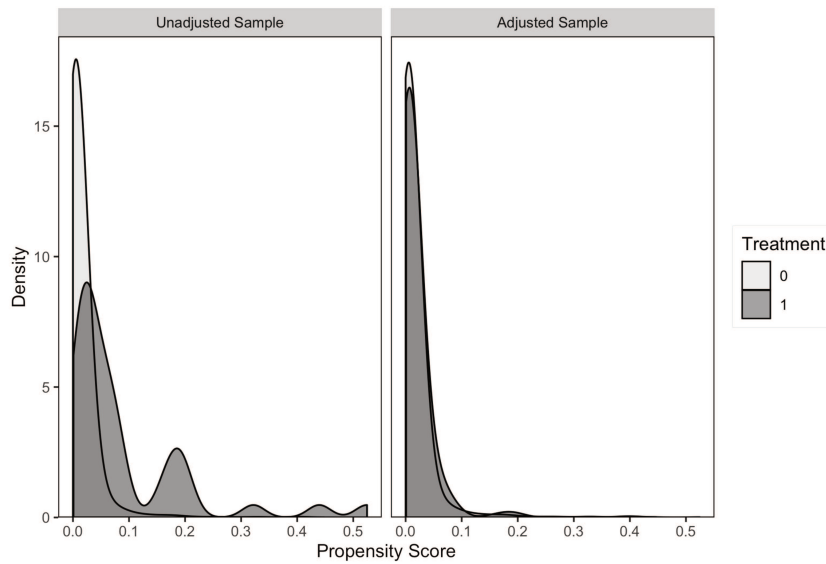
(1) Covariates	(2) Standardized Difference in Means
Propensity Score	0.029
Incumbent Party	0.053
Difference in Abstention	-0.013
Number of Lists in the Municipality	0.097
French Communist Party	-0.010
Rights (various)	0.165
Left (various)	0.002
Far Right	-0.001
Far Left	0.022
National Front (Far Right)	-0.010
Socialist Party	-0.001
Union of the Left	-0.089
Union for a Popular Movement (Right)	-0.066
Ecologists	-0.013
Sex 2008	0.095
Sex 2014	-0.075
Last Round of the Election	-0.178
Proportion of Immigrants	-0.030
Proportion of Foreigners	0.022
Unemployment Rate	0.216
Proportion of Students	-0.303
Proportion of Home Owners	-0.213
Proportion of Secondary Residences	-0.103
Proportion of Children (<14)	0.555
Municipal Population	0.088

*Notes:* The standardized difference in means is calculated between the treatment and control group after propensity score weighting.

Figure 1.4 shows the density of observations given their propensity score for each treatment status used to assess racial discrimination. It shows a nearly perfect overlapping after adjusting the samples with the weights calculated from the propensity score.

Other validation tests are available in Appendices, including Table 1.8 detailing a placebo treatment assignment test.

Figure 1.4: Density Plot - Racial Discrimination Covariate Balance



## 1.6 Results

Table 1.5 presents the main results on the difference of vote shares collected when the head of list is identified as North African, in the same city and political party. Estimates were conducted for four different combinations of identification of the North African origin, using combinations of the Namsor classifier and the web scrapping on North African names. Column (1) and column (2) show detailed results for the AIPTW and causal forest estimators, while column (3) enumerates the number of treated lists. Using the AITPW estimator, I find a causal negative effect of being identified as North African when leading a list of 8 to 9.5 vote share points, significant at the 1% level. It means that a treated list loses 8 to 9.5 vote share points between 2008 and 2014 compared to a list headed by a non-North African candidate both in 2008 and 2014. The causal forest estimator shows similar results but in a greater magnitude, where lists headed by a candidate with North African origins lose 8.6 to 9.8 vote share points, also significant at the 1% level. These estimates are substantial given the average vote share of the sample, which is 20%.

Table 1.5: Effects of North African Head of List Origins on Votes

	(1) AIPTW	(2) Causal Forest	(3) Treated
Arabic First Names and Arabic Namsor	-9.107*** (2.505)	-9.146*** (2.161)	37
Arabic First Names or Arabic Namsor	-8.216*** (2.336)	-9.404*** (1.937)	44
Arabic First Names	-9.487*** (2.441)	-9.782*** (2.077)	40
Arabic First Namsor	-8.003*** (2.369)	-8.584*** (1.978)	41

*Notes:* \*Significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

Estimates are calculated using an AIPTW Benkeser and Carone (2017) and causal forests Athey and Wager (2019). Standard errors are in parenthesis.

The main finding of this paper is that lists led by a North African candidate receive significantly fewer votes (between -8 to -9.8 percentage points) than lists led by a non-North African candidate in the same city and political party. The results are consistent with Brookman and Soltas (2020), where Black candidates receive 9 vote share points less than White candidates. The finding suggests that voters have negative preferences against North African candidates (taste-based discrimination) or infer skills from ethnicity (statistical discrimination). In the first case, agents may be irrational and are willing to pay a cost (not voting for their preferred party) to avoid minorities to be elected. In the second, voters are rational and infer unobserved characteristics (diplomas, political experience) from candidates' ethnicity.

To distinguish between these two types of discrimination, I explore the discrimination against lists headed by candidates with Southern Europe origins (Italy, Spain and Portugal). According to

INSEE, these three countries are among the seven countries of origin of immigrants<sup>6</sup>. Using the same onomastic classifier (Namsor), I show that these candidates are discriminated against but in a lower magnitude than candidates with North African origins. Table 1.6 details the AIPTW and causal forest estimates: candidates with Southern Europe origins face less significant discrimination, of about -5 vote shares compared to non-Southern Europe head of lists<sup>7</sup>. Immigration from Southern Europe is different because it is not structured on colonial stereotypes, as North African immigration can be. Thus, this difference in discrimination highlights a greater part of taste-based discrimination, depending on the subjective perceptions of voters.

Table 1.6: Discrimination against Southern Europeans

	(1) AIPTW	(2) Causal Forest	(3) Treated
South Europe Candidate	-4.293 (2.73)	-5.178** (2.307)	66

*Notes:* \*Significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.  
Estimates are calculated using an AIPTW Benkeser and Carone (2017) and causal forests Athey and Wager (2019). Standard errors are in parenthesis.

Table 1.7 shows that municipalities with the highest immigrant population (rate higher than the median of 13.9 in the North African treated group) exhibit lower levels of discrimination than municipalities with a lower rate of immigrants than the median for both Southern Europe and North African head of lists. Heads of lists with a North African-sounding name are twice as less discriminated against in municipalities with higher immigration rates (-4.9 against -9.6 vote share points). Southern Europe heads of lists are not discriminated against in municipalities with a high rate of immigrants while they are in municipalities with a lower immigrants rate (-5.6 vote share points).

Table 1.7: Discrimination by Immigrant Rate

	(1) AIPTW	(2) Causal Forest	(3) Treated
North African (< immigrants median)	-9.59*** (2.935)	-13.106*** (3.146)	22
North African (> immigrants median)	-4.917*** (0.538)	-6.121*** (2.202)	22
South European (immigrants share < 12)	-3.666 (2.992)	-5.637** (2.924)	48
South European (immigrants share > 12)	-4.548 (4.724)	-4.238 (3.808)	18

*Notes:* \*Significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.  
Estimates are calculated using an AIPTW Benkeser and Carone (2017) and causal forests Athey and Wager (2019). Standard errors are in parenthesis.

<sup>6</sup><https://www.insee.fr/fr/statistiques/3633212> - Accessed on April 2023

<sup>7</sup>Validation tests for these estimates are in appendices.

Candidates of North African or South European origin are thus less discriminated against in municipalities with higher immigration rates. This result can be analyzed in the light of the tipping point literature, which explains that the link between discrimination and the ethnic composition of neighborhoods can decrease when reaching a specific point. For example, Chareyron et al. (2023) shows that ethnic discrimination in access to housing is lower in neighborhoods with a minority share of over 40% in New Caledonia, France. Two cumulative interpretations emerge: (i) discrimination decreases in municipalities with more diversity, (ii) immigrants vote more for their minority candidates, which reduces the negative effect of discrimination. However, given the structure of immigration (first-generation immigrants, mainly from North Africa and sub-Saharan Africa), the fact that Southern European candidates are not discriminated against in municipalities with more immigration accentuates the role played by the first point.

## 1.7 Conclusion

This paper has provided causal evidence of racial discrimination against candidates of North African origin in the 2014 French municipal elections. By using an inverse probability of treatment weighting and causal forests approaches and various methods to infer the ethnic origin of the candidates from their names, I have shown that lists led by a candidate with a North African-sounding name receive significantly fewer votes than lists led by a non-North African candidate in the same city and political party compared to 2008. This effect ranges from 8 to 10 vote share points, which is substantial given that the average vote share in the sample is around 20%. This finding reveals that racial prejudice and stereotypes play an important role in shaping voters' preferences and behavior in local elections. It also has implications for the representation and integration of ethnic minorities in France and other countries with similar contexts.

By analyzing the discrimination suffered by candidates of Southern European origin (Italy, Spain, Portugal), I show that the discrimination is mainly taste-based. Moreover, this double analysis allows us to realize that candidates are less discriminated in municipalities with more diversity. It would therefore be interesting to work to increase diversity in the municipalities with the least diversity, for example by tightening the rules governing the rate of social housing.

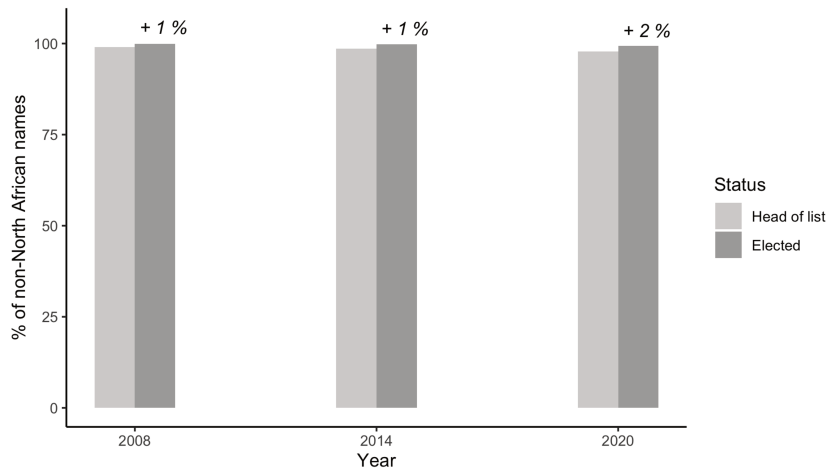
The contribution of this paper is twofold. First, it adds to the literature on electoral discrimination by providing a causal estimate of its extent and impact in a large-scale setting. Second, it demonstrates the feasibility and validity of using name-based methods to identify ethnic origin in contexts where direct measures are unavailable or unreliable.

## 1.8 Appendices

### 1.8.1 Difference between heading a list and being elected as a mayor for non-North African names

Figure 1.5 highlights a gap, between +1% to +2% during the 2008-2020 period, between the presence of non-North African candidates as list leaders and their election as mayor. It contrasts with the negative gap outlined by Figure 1.1

Figure 1.5: Percentage of Non-North African Names Heading Lists and Elected as Mayors



### 1.8.2 Placebo Assignment

Table 1.8 shows that both Doubly Robust AIPTW and Causal Forests are not sensitive to a placebo treatment assignment.

Table 1.8: Placebo Assignment

	(1) Doubly Robust	(2) Causal Forest	(3) Treated
North African Head of List	-0.331 (-2.161)	-0.142 (2.443)	50

Notes: \*Significant at 10%; \*\* significant at 5%; \*\*\*significant at 1%.



### 1.8.3 Sensitivity to immigrants share cutoffs

Table 1.9 details the AIPW and Causal Forests estimates for different cutoffs of immigrants share in municipalities. The estimates are not sensitive for the different cutoffs.

Table 1.9: Sensitivity to Immigrants Share Cutoffs

	(1) AIPW	(2) Causal Forest	(3) Treated
South European (immigrants share < 11)	-3.905 (3.036)	-5.754** (2.926)	48
South European (immigrants share > 11)	-3.369 (3.849)	-3.988 (3.708)	18
South European (immigrants share < 13)	-3.407 (3.012)	-5.965** (2.84)	50
South European (immigrants share > 13)	0.332 (3.898)	-3.777 (4.013)	16

*Notes:* \*Significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.  
Estimates are calculated using an AIPW Benkeser and Carone (2017) and causal forests Athey and Wager (2019). Standard errors are in parenthesis.

### 1.8.4 South European Treatment Covariate Balance

#### Covariate Balance

Figure 1.10 displays the standardized difference in means between the treated and control samples for the South European treatment using a weighted propensity score. All covariates are balanced between the two groups (at the 0.2 level).

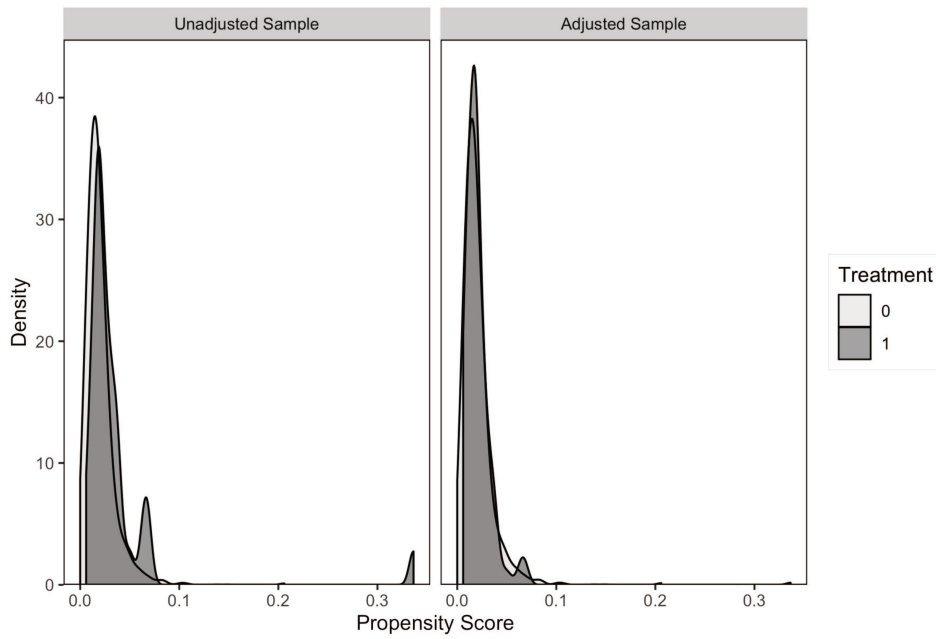
Table 1.10: Covariate Balance for Weighted Propensity Score – South European Treatment

(1)	(2)
Covariates	Standardized Difference in Means
Propensity Score	0.022
Incumbent Party	-0.051
Difference in Abstention	-0.030
Number of Lists in the Municipality	-0.026
French Communist Party	-0.007
Rights (various)	-0.011
Left (various)	0.032
Far Right	-0.001
Far Left	0.008
National Front (Far Right)	-0.010
Socialist Party	0.017
Union of the Left	-0.035
Union for a Popular Movement (Right)	0.011
Ecologists	-0.005
Sex 2008	-0.038
Sex 2014	0.011
Last Round of the Election	-0.022
Proportion of Immigrants	-0.030
Proportion of Foreigners	-0.056
Unemployment Rate	-0.173
Proportion of Students	-0.033
Proportion of Home Owners	0.114
Proportion of Secondary Residences	-0.014
Proportion of Children (<14)	0.024
Municipal Population	-0.098

### Density Plot

Figure 1.6 shows the density of observations given their propensity score for each treatment status used to assess racial discrimination. It shows density overlapping after adjusting the samples with the weights calculated from the propensity score.

Figure 1.6: Density Plot - South European Candidate Discrimination



## Chapter 2

# Party Affiliation and Public Expenditures: Close-Race Evidence from French Municipalities

Do elected politicians use public money differently depending on their ideology? When is partisanship most apparent during a mandate? The existing literature is mixed. Based on a regression discontinuity design, I show that political color significantly impacts public spending in French municipalities. Moreover, partisanship mainly influences public expenditures in the last two years of the mandate, thus following a political cycle. Focusing on city size heterogeneity, I show that left-wing mayors invest substantially more than their right-wing counterparts in both small and large municipalities but in different spending areas. In contrast, differences in current expenditures concern only small municipalities. These findings highlight that city size and density may explain the conflicting results in the literature.

**Keywords:** close race elections, regression discontinuity design, heterogeneous treatment effects, local public economics, public expenditures.

**JEL:** C21, D72, D78, H71, H72

## 2.1 Introduction

How does partisanship affect the use of public money? From a theoretical perspective, partisanship should not affect public expenditures since governments should converge in preferences to the median voter ones (Downs 1957). However, the existing empirical literature has shown mixed evidence of the effects of partisanship on local budgets. Some authors emphasize the role played by political parties in economic outcomes (Pettersson-Lidbom 2008; Le Maux, Rocaboy, and Goodspeed 2011; Gerber and Hopkins 2011; de Benedictis-Kessner and Warshaw 2016; Beland and Oloomi 2017b; Hill and Jones 2017), while others highlight the lack of partisanship effects on public expenditures (Ferreira and Gyourko 2009; Leigh 2008).

Particularly, while Ferreira and Gyourko (2009) and de Benedictis-Kessner and Warshaw (2016) both analyse partisanship effects in U.S. municipal elections, they reach contradictory results. Although de Benedictis-Kessner and Warshaw (2016) use a larger database, the main difference between these studies is the lower bound on city size. They include all municipalities with more than 75,000 population and find various partisanship effects. On the contrary, Ferreira and Gyourko (2009) decrease the population size lower bound to 25,000 inhabitants and found no effects. Thus, population size heterogeneity may explain the conflicting results in the literature. The underlining explanation may be that smaller municipalities focus on managerial performances rather than political preferences (Oliver, Ha, and Callen 2012) and thus lead to only slight differences due to partisanship. Moreover, large municipalities are more likely to offer a wider range of public goods and services, such as zoos or large parks (Oates 1988). This "zoo effect" may lead to partisanship effects because of broader public expenditure choices that permit differences (Hajnal and Trounstein 2010).

The majority of the literature focuses on local governments in the United States. Studying other countries and political systems reinforces the generality of the findings to inform the discussion on the existence and magnitude of partisan effects. In this sense, France is an excellent choice, given the strength of the political power of the mayor. In their typology of local governments, Heinelt and Hlepas show that French mayors possess the largest discretionary powers in Europe (Heinelt and Hlepas 2006)<sup>1</sup>. This discretionary power reduces the noise in the estimates of the causal effect of partisanship on public expenditures, as the decision is mainly in the hands of the executive power, i.e., the mayors.

In this paper, I first analyze the impact of political parties on public expenditures during a full term in French municipalities after the 2008 elections. Using a regression discontinuity design (RDD) on close race elections, I estimate the causal effect of partisanship between comparable municipalities. I add covariates selected with machine learning to increase the efficiency of the estimations. I find

---

<sup>1</sup>French mayors are properly called 'political mayors' and take discretionary decisions based on their political preferences unlike mayors in Scandinavian countries who are, for example, 'executive mayors' and focus on managerial decisions.

strong effects of partisanship; left-wing mayors invest 30 euros more per capita in urban planning and environment and 3.7 euros more in family aid, while right-wing ones invest 2.4 euros more per capita in security. Second, I investigate the political cycle of right-wing and left-wing local governments. Both political colors increase investments the two years before the end of the term: partisanship crystallizes during this period. Finally, I analyze the heterogeneous influence of city size and density to clarify its effects on partisanship. With 1,739 municipalities of more than 3,500 inhabitants<sup>2</sup>, France is an ideal field of investigation to study this heterogeneity. I build two groups of municipalities based on their size and density using a k-means algorithm. Partisanship effects on each group of municipalities are estimated using an RDD with covariates. I also control for the false discovery rate to obtain robust estimates. I find that partisanship affects budget allocation and total expenditure in both small and large municipalities. While capital expenditures are affected in a range of areas (security, education, family, urban planning and environment), current expenditures are affected only in security in small municipalities.

This paper contributes to the literature by testing partisanship effects in a political environment where mayors have a free hand to impose their preferences. Partisanship effects are analyzed through ten spending areas legally defined to identify political preferences. Furthermore, it sheds light on one of the points of divergence in the literature through the study of city size heterogeneity: the roles played by density and population in precipitating these partisan effects. Finally, estimates of causal effects are data-driven to strengthen the internal validity of the results. The article is organised as follows. First, I discuss the previous literature on partisan effects on local governments. Next, I present the institutional background and the data. I also discuss the empirical strategy, followed by findings on the impact of political families on local public expenditure. Then, I analyze heterogeneous treatment effects using machine learning to find optimal clusters of French municipalities. Finally, I conclude and highlight challenges for further research.

---

<sup>2</sup>Detailed data on public expenditures is only available for municipalities of more than 3,500 inhabitants.

## 2.2 Partisanship and behaviour of elected candidates

The empirical evidence of partisanship effects is mixed. On the one hand, some authors point out the existence of partisanship effects on different outcomes. In a seminal paper, Lee, Moretti, and Butler (2004) analyzed the behavior of representatives in the U.S. House. They demonstrate that candidates have fixed policy preferences that do not change once elected. Petterson-Lidbom emphasizes the influence of party control in Swedish local governments on several aggregated economic outcomes, such as total public expenditures and employment (Petterson-Lidbom 2008). Specifically, left-wing governments raised taxes and public spending about 2 to 3 % more than right-wing ones between 1974 and 1994. Similarly, in France, social *départements* expenditures are highly determined by the political party in power, especially if the majority is not fragmented (Le Maux, Rocaboy, and Goodspeed 2011). In addition, Duboz, Le Gallo, and Houser (2020) find that left-wing *départements* spend more on current expenditures and right-wing *départements* on capital expenditures. Analogous results were found by Gerber and Hopkins while analyzing 134 mayoral elections in large U.S. cities with more than 170,000 inhabitants (Gerber and Hopkins 2011). Likewise, de Benedictis-Kessner and Warshaw highlight differences of more than \$100 per capita due to partisanship in large U.S. cities (de Benedictis-Kessner and Warshaw 2016).

More recently, analysis of capital spending at the U.S. state level between 1960 and 2012 found that Democratic governors allocate more of the budget to education and health, but with similar total spending to Republicans (Beland and Oloomi 2017b). Focusing on budget allocation rather than total spending may highlight differences between policy families. To test this hypothesis, Hill and Jones (2017) analyze the willingness of politicians to promote specific policy areas through budget allocation, given fixed total expenditure. They find that school districts with a high proportion of minority students get larger state transfers when the Democrats are in power. On a national level, Herwartz demonstrates that the size of the public sector is highly influenced by ideology (following the left-right typology) for a panel of OECD countries (Herwartz and Theilen 2017). These differences in policies are strengthened in times of economic contraction: left-wing governments are associated with an increase in public expenditures, while right-wing ones are linked to austerity.

On the other hand, several studies outline the lack of partisanship effect. Ferreira and Gyourko (2009) observed no difference in local public expenses between political parties in U.S. cities with more than 25,000 inhabitants. These results are attributed to Tiebout sorting, which states that individuals migrate to municipalities where their preferences are best represented (Tiebout 1956). This phenomenon leads to homogeneity within cities that allows no political divergence. A comparable conclusion is drawn by Leigh, revealing only slight differences in outcomes between Democrats and Republicans in the U.S. States from 1941 to 2002 (Leigh 2008).

While most studies at the national or regional level find that partisanship has a significant effect on public expenditures, those on local governments are much more mitigated. Heterogeneous city

sizes may be one of the main reasons explaining the contradictory results. As stated by Oliver, Ha, and Callen (2012), small cities in the U.S. are less subject to ideology because mayors run towns as managers. Moreover, a "zoo effect" can affect large municipalities (Oates 1988), strengthened by the fact that mayors are more exposed to national's party's ideology. Thus, partisanship does not have a homogeneous impact according to city size. It may explain the diversity of results across three studies of U.S. mayoral elections using different lower population limits (Ferreira and Gyourko 2009; Gerber and Hopkins 2011; de Benedictis-Kessner and Warshaw 2016).

## 2.3 Institutional background and data

### 2.3.1 Institutional background

There were almost 36,000 *communes* in France in 2008. These are the first level of democracy, followed by inter-municipalities, *départements* and regions. Elections are organized every six years to elect the municipal council. In municipalities of more than 3,500 inhabitants, the list that receives more than 50% of the votes gets half of the available seats in the municipal council. The remaining seats are allocated in proportion to the score of each list (including the first). A second round is held if there is no absolute majority in the first round. Winning the election, therefore, guarantees a majority on the municipal council. A mayor is then elected within the municipal council to manage the city.

Municipalities have a wide range of general competencies. For example, they manage urban planning and building permits, social housing, municipal police, municipal libraries, primary schools, childcare centers, and sports facilities. To carry out these missions, French municipalities collect local taxes (such as residential and property taxes) and benefit from equalization. They then spend their budget according to the decisions of the municipal council. The rules for accounting reports depend on the size of the city. Specifically, municipalities with more than 3,500 inhabitants must report their expenditures by expenditure area. These are the dependent variables described below that I will use in this paper.

### 2.3.2 Municipal data

Public expenditure data for cities with more than 3,500 inhabitants over the 2008-2014 period are from the French General Directorate of Public Finances (DGFIP). For each city, capital and current expenditures for the 2008-2014 municipal term are broken down following the M14 nomenclature<sup>3</sup>. Table 2.1 shows averaged municipal expenditures and highlights strong variability in each spending item. Municipalities have, on average, higher current expenditures than capital outlays. Column (5) presents the proportion of total capital expenditures and indicates that the largest area of

---

<sup>3</sup>The M14 nomenclature is the legal framework that defines the accounting rules for French municipalities. Municipalities of more than 3,500 inhabitants break out public expenditures into ten categories, listed in Table 2.1



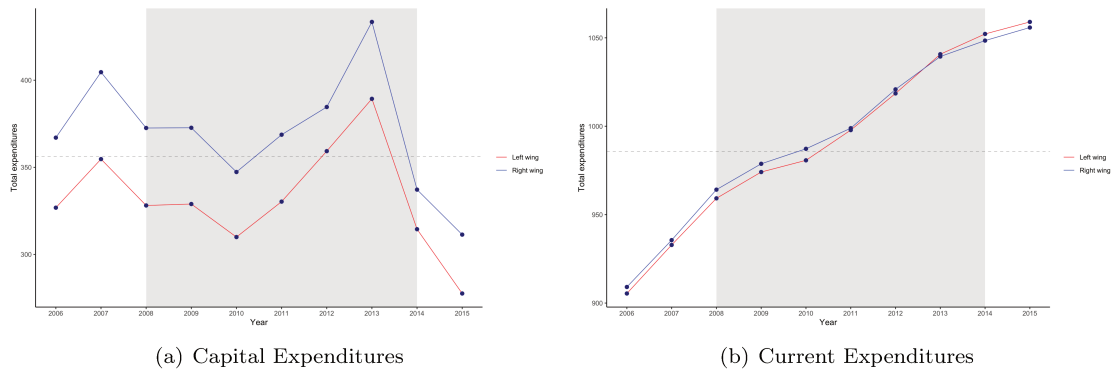


Figure 2.1: Evolution of Municipal Expenditures (2006 - 2015)

expenses is urban planning and environment (39.5%) followed by general services (22.6%) and education (10.6%). Column (9) shows that municipalities allocate current expenditures across spending items in the same order of magnitude as capital expenses. <sup>4</sup>

Finally, figure 2.1(a) shows the evolution of average capital expenditures for both left-wing and right-wing governments throughout the mandate (shaded part). There is a high degree of variability and an electoral cycle in which spending rises sharply in the last two years of the mandate for both left-wing and right-wing parties. On the contrary, figure 2.1(b) shows that current expenditures increase throughout the political term, following a linear trend.

<sup>4</sup>The negative values in columns (2) and (6) of Table 2.1 represent budgetary operations to balance the accounts. The accounts can be credited (debited) with the amount of capital gains (losses) on the disposal of assets by the municipalities.

Table 2.1: Municipal Expenditure Data

(1) Spending items	Capital expenditures					Current expendi		
	(2) Min (euros per capita)	(3) Mean (euros per capita)	(4) Max	(5) Proportion of total expenditures	(6) Min (euros per capita)	(7) Mean (euros per capita)	(8) Max	
General services	-48.41	79.99 (79.89)	1619.56	22.56%	33.66	387.99 (167.85)	1993.10	
Security	-0.02	2.88 (7.28)	158.01	0.81%	0.00	37.14 (32.45)	280.23	
Education	0.00	37.74 (40.38)	343.47	10.64%	0.00	152.43 (58.97)	446.03	
Culture	-0.46	28.02 (36.48)	356.36	7.9%	0.00	69.06 (52.11)	381.71	
Sports and youth	0.00	42.56 (44.30)	622.42	12%	0.00	102.33 (66.63)	787.37	
Social and public health	0.00	4.68 (14.06)	303.89	1.32%	-35.77	37.50 (36.58)	297.06	
Family	0.00	7.53 (16.64)	322.73	2.12%	0.00	48.32 (52.03)	363.71	
Housing	0.00	5.13 (16.35)	389.17	1.45%	-0.22	3.70 (9.12)	152.78	
Urban planning and environment	0.00	140.03 (100.30)	1106.33	39.49%	0.00	151.72 (93.20)	1319.94	
Economic stimulus	-6.14	6.06 (19.51)	313.56	1.71%	-6.04	12.50 (32.71)	805.98	
Total expenditures	33.48 (177.38)	354.63	2602.26	100%	336.57 (350.45)	1002.70	5366.70	

33  
34

Notes: The variables are from DGFP for the 2008-2014 period. Standard deviations are in parentheses.

Data on the 2008 municipal election for cities with more than 3,500 inhabitants are from the Ministry of the Interior. It includes vote shares and the number of municipal seats won by each electoral list. The broad French political spectrum is composed of many parties, among which the Union for a Popular Movement (UMP) and the Socialist Party (PS) were dominant on the right-wing and left-wing spectrum, respectively, in 2008. I create a left-wing and right-wing aggregate from a list of parties reported by the Cevipof<sup>5</sup> by excluding centrist parties which are ambiguous. Table 2.2 shows that parties of this typology have won the majority of elections compared to removed parties in the grey area. Specifically, left-wing and right-wing parties respectively won 48.8 % and 47.7 % of total races. The cities in the data are shown on the map in Figure 2.2, along with the election results. I keep only municipalities where the grouped families of our typology are directly opposed. This brings the number of observations to 1,739 municipalities. Finally, as mayors in Paris, Marseille and Lyon are elected indirectly by the district mayors, I exclude them from this analysis.

---

<sup>5</sup>Centre de recherches politiques de Sciences Po or "Centre for Political Research at Sciences Po", Paris.

Table 2.2: Classification of Political Parties

Code	Signification (French)	Signification (English)	Number of elections won
LEXG	Liste d'extrême gauche	Far Left List	0
LCOM	Liste du Parti Communiste	List of the Communist Party	55
LUG	Liste d'union de la gauche	List of the Union of the Left	552
LSOC	Liste du Parti Socialiste	List of the Socialist Party	337
LVEC	Liste des Verts	Green List	8
LDVG	Liste divers gauche	List of various leftists	370
IGC	Liste gauche-centristes	Left-Centrist List	50
LAUT	Autre liste	Other list	14
LREG	Liste régionaliste	Regionalist list	3
LCMD	Liste centre-MoDem	Centre-Democratic Movement (MoDem) list	30
LMC	Liste majorité-centristes	Majority-Centre List	61
LMAJ	Liste de la majorité (UMP)	List of the majority (UMP)	584
LDVD	Liste divers droite	List of various right-wingers	647
LPN	Liste du Front National	National Front List	0
LEXD	Liste d'extrême droite	Far Right List	0

Source: Cevipof modified by the author

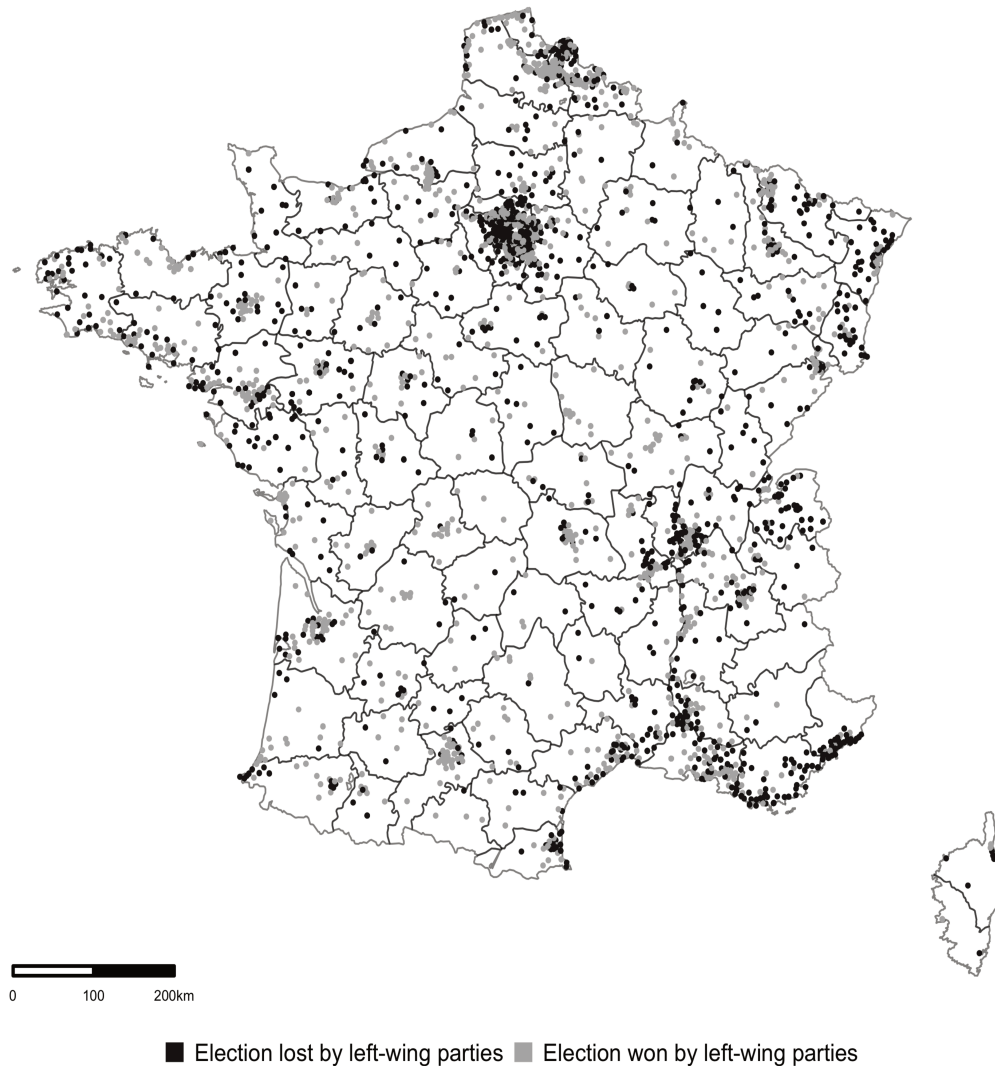


Figure 2.2: Territory Coverage of the Sample

The 2002-2007 socioeconomic characteristics of municipalities with more than 3,500 inhabitants are from the French National Institute of Statistics and Economical Studies (INSEE) and DGFIP <sup>6</sup>. Table 2.3 reports these covariates and highlights the heterogeneity of the 1,739 municipalities in the sample.

With mayors elected in March and quickly taking office, I exclude 2014 data from the sample. Symmetrically, the new city council votes on the budget after it takes office; hence I keep the 2008 expenditures in the sample. Since local governments increase public spending in the years before elections (Foucault, Madies, and Paty 2008), I average expenditure data over the 2008-2013 period to remove the effects of the electoral cycle. The use of annual data may yield noisy results if there is partisan heterogeneity in the magnitude of election cycles.

<sup>6</sup>Predetermined socioeconomic features are independent from treatment assignment, see Table 2.4

Table 2.3: Control Variables

(1)	(2) Min	(3) Mean	(4) Max
Population	3,015.50	15,441.58 (25,958.64)	438,584.00
Average annual rate of change in employment	-5.83	1.69 (1.76)	11.99
Employment per capita	0.09	0.43 (0.23)	3.44
Proportion of individuals without high school diploma	13.97	38.71 (8.60)	64.39
Proportion of higher education graduates	5.26	21.28 (8.69)	60.58
Unemployment rate	3.78	11.27 (4.18)	30.40
Proportion of unoccupied housing	0.32	5.82 (2.90)	19.92
Proportion of secondary residences	0.00	4.62 (10.50)	87.99
Proportion of social housing	0.00	16.63 (11.69)	71.09
Proportion of residences built before 1946	0.47	22.63 (12.12)	63.39
Proportion of foreigners in the population	0.00	4.93 (4.62)	37.10
Proportion of 65+ year olds in the population	2.38	17.27 (5.38)	40.09
Proportion of 18-24 year olds in the population	3.67	8.63 (2.45)	31.04
Median income	8,940.50	17,679.59 (3,376.72)	34,311.00
Tourist accommodation capacity (number of beds)	0.36	2,596.70 (8,845.67)	153,972.97
Surface area of the municipality (ha)	70.00	2,420.47 (2,974.76)	75,780.00
Number of municipalities in the intermunicipal cooperation	0.00	16.84 (15.85)	128.00
General Operating Grant per capita	73.18	238.35 (100.93)	1,142.75
Municipal debt per capita	0.00	916.32 (553.37)	5,245.13

Notes: The variables are from INSEE and DGFIP for the 2002-2007 period. Standard deviations are in parentheses.

## 2.4 Empirical Strategy

### 2.4.1 Regression Discontinuity Design

I use a regression discontinuity design to estimate the effect of local partisanship on public expenditures. The RDD uses a discontinuity along a reference variable to exhibit a jump in dependent outcomes interpreted as a causal effect. It has been widely used in the literature testing the impact of partisanship since the seminal paper by Lee, Moretti, and Butler (2004). In this paper, I exploit the majority bonus: the fact that lists winning the most votes automatically obtain half of the available seats in a municipal council. Thus, a large discontinuity in the number of seats appears between candidates with  $(V - \epsilon)\%$  and  $(V + \epsilon)\%$  vote share, as Figure 2.3 illustrates. The difference in vote shares  $X = V_{left} - V_{right}$  fully determines the elected list. A left-wing party

wins the election if  $X > 0$ . Symmetrically, a right-wing party wins if  $X < 0$ . Assuming that public expenditures are continuous over  $X$  (*i.e.* an  $\epsilon$  increase in a list's vote share should not create a jump in public expenditures, other than through the majority bonus), the difference in political party spending can be interpreted as a causal effect. Unlike the existing literature, I do not rely on the random assignment assumption, which is extensively challenged because of the use of arbitrary bandwidths to create quasi-experiments. It also requires more hypotheses than the continuity assumption (see de la Cuesta and Imai (2016) for a clear overview of the discussion).

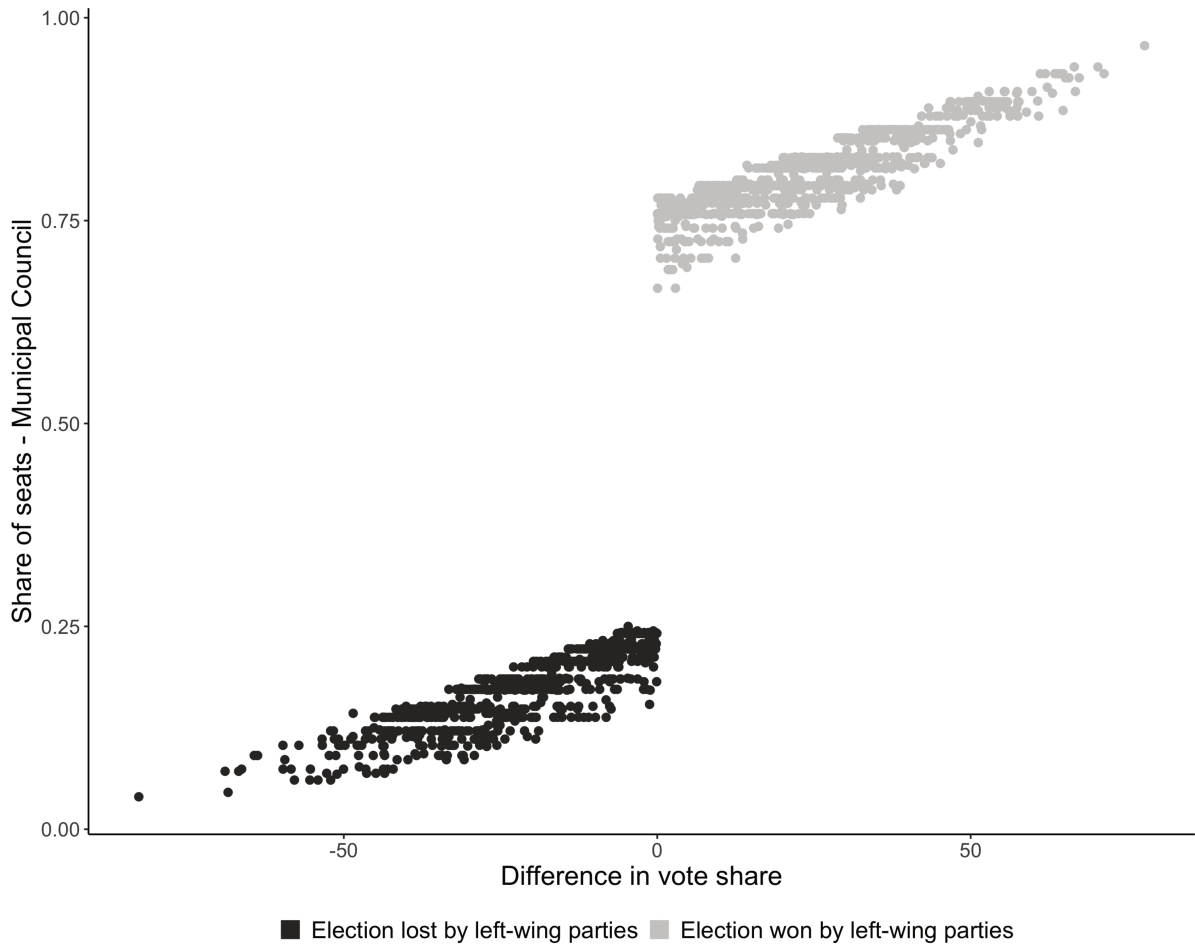


Figure 2.3: Discontinuity on Vote Share Differences

### 2.4.2 Estimation

I estimate the causal effect of partisanship on public expenditures using robust non-parametric local polynomial regressions of order 1 as recommended by (Calonico, Cattaneo, and Titiunik 2014). Regressions of order 1 are more stable and are less likely to overfit the data than regressions of order 2. Intending to increase the precision of the estimates (Calonico et al. 2019), I include the socioeconomic features of municipalities before the 2008-2014 political mandate. These covariates are selected using post-lasso to avoid multicollinearity and optimize the inference efficiency. Post-

lasso is a two-step regularisation less biased than the widely used lasso (Belloni and Chernozhukov [2013](#)). A triangular kernel is chosen to decrease the weight of observations far from the cutoff. Finally, the bandwidth is determined to minimize the mean squared error.

Additional material is available in the Appendices to check the robustness of the results and assess the potential effect of inter-municipality expenditures.

### 2.4.3 Validity of the RDD

The RDD validity is conditioned by the absence of sorting mechanisms that invalidate the hypothesis of exogenous treatment. In this context, some candidates are more likely to be on one side of the cutoff than the other (see de la Cuesta and Imai [2016](#) for a discussion on sorting mechanisms in elections). Incumbency is one of the main sorting mechanisms encountered when studying close-race elections. Eggers et al. [\(2015\)](#) rejects the hypothesis of incumbency effect for French municipal elections in 2008. The McCrary test confirms this result: with a p-value of 0.76, we cannot reject the null hypothesis of continuity of the forcing variable (McCrary [2008](#)). [Figure 2.4](#) illustrates the continuity of the running variable.

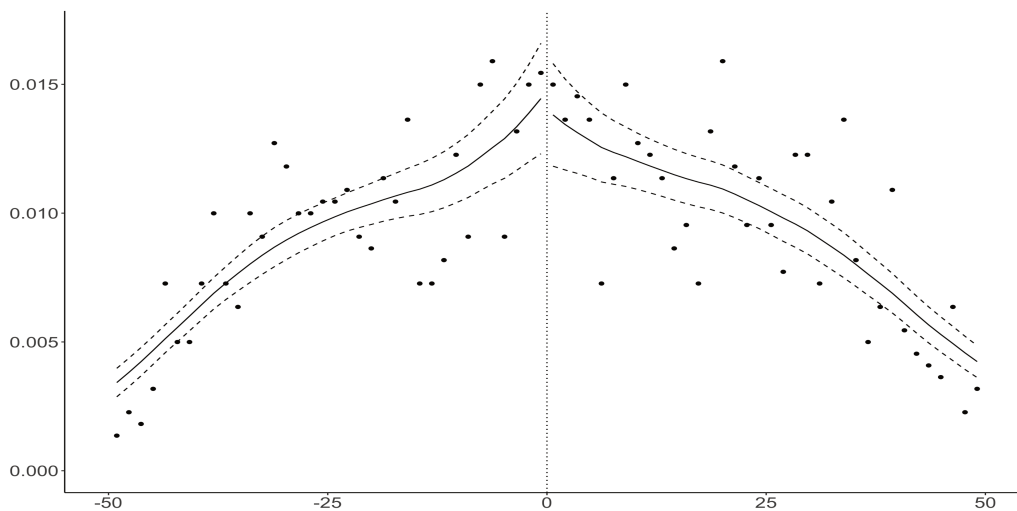


Figure 2.4: Sorting McCrary Test

Another essential identification assumption concerns the treatment effect on predetermined covariates. Predetermined covariates should not be affected by the running variable at the cutoff. Thus, I run several local non-parametric regressions to highlight any discontinuity of these variables at the cutoff point. As I run 19 tests, there will be an average of 2 false positive features at the 10% level. Therefore, I use a Benjamini-Hochberg correction to control the false discovery rate (Benjamini and Hochberg [1995](#)). [Table 2.4](#) shows no statistically significant variable at the 5% level. The placebo effect on the proportion of individuals without a high school diploma and the proportion of unoccupied housing are significant at 10% without controlling for the false discovery



rate. However, all 19 covariates are not affected by the treatment when applying the Benjamini-Hochberg correction displayed in column (5). Finally, we cannot reject the null hypothesis of continuity at the cutoff of these predetermined expenses along the forcing variable.

Table 2.4: Placebo Effect of the Treatment

(1)	(2)	(3)	(4)	(5)
	Estimate	SE	p value	BH correction
Population	0.110	0.160	0.500	0.840
Average annual rate of change in employment	-0.270	0.270	0.320	0.840
Employment per capita	-0.010	0.040	0.750	0.900
Proportion of individuals without high school diploma	2.580	1.490	0.080	0.790
Proportion of higher education graduates	-2.120	1.530	0.170	0.840
Unemployment rate	0.440	0.650	0.500	0.840
Proportion of unoccupied housing	0.900	0.510	0.070	0.790
Proportion of secondary residences	-2.190	1.780	0.220	0.840
Proportion of social housing	1.030	2.030	0.610	0.840
Proportion of residences built before 1946	1.660	2.100	0.430	0.840
Proportion of foreigners in the population	0.550	0.680	0.420	0.840
Proportion of 65+ year olds in the population	0.860	0.910	0.350	0.840
Proportion of 18-24 year olds in the population	-0.060	0.490	0.910	0.910
Median income	-471.480	573.870	0.410	0.840
Tourist accommodation capacity (number of beds)	848.110	1,507.860	0.570	0.840
Surface area of the municipality (ha)	-109.770	501.500	0.830	0.910
Number of municipalities in the inter-municipal cooperation	1.390	2.790	0.620	0.840
General operating grant per capita	-6.440	15.860	0.680	0.870
Municipal debt per capita	12.630	113.190	0.910	0.910

43

*Notes:* Estimates are calculated with a bandwidth minimising mean-squared errors and a triangular kernel (Calonico, Cattaneo, and Tchetgen, 2018). p-values are adjusted with the Benjamini-Hochberg correction.

Finally, the validity of the RDD is based on the assumption of continuity of public expenditures over the difference in vote shares. Even if this assumption is hardly testable, I provide evidence of the lack of discontinuity on placebo cutoffs. Table 2.5 displays estimates for capital expenditures with different cutoffs and highlights that significant results are concentrated on the 0 cutoffs. Similarly, Table 2.6 displaying the estimates for current expenditures shows the lack of patterns for placebo cutoffs.

Table 2.5: Placebo Cutoffs for Capital Expenditures

(1)	Cutoff				
	(2)	(3)	(4)	(5)	(6)
	-12	-6	0	6	12
General services	7.122 (14.11)	-12.038 (11.183)	0.753 (10.516)	-8.131 (9.035)	12.069 (13.98)
Security	-0.061 (0.91)	-0.84 (1.454)	-2.332*** (0.867)	1.246 (0.772)	-0.586 (0.693)
Education	6.354 (7.079)	4.842 (7.35)	7.506 (7.277)	4.158 (8.097)	-8.379 (7.653)
Culture	14.703 (10.239)	2.385 (5.72)	6.242 (5.697)	-6.656 (7.096)	0.257 (7.279)
Sports and youth	-4.207 (8.607)	5.423 (9.157)	-1.364 (8.372)	15.462* (9.064)	-1.202 (6.737)
Social and public health	2.313 (2.532)	-1.343 (1.955)	0.157 (2.245)	-2.206 (1.586)	-0.868 (1.564)
Family	0.59 (4.679)	0.684 (1.94)	3.151* (1.912)	-0.679 (2.818)	-2.334 (2.245)
Housing	2.073 (3.877)	-2.468 (2.915)	0.129 (1.781)	2.881 (1.974)	-2.88 (1.786)
Urban planning and environment	16.783 (25.174)	1.83 (14.107)	36.696*** (14.113)	-14.263 (18.002)	6.732 (16.743)
Economic stimulus	-4.338 (3.496)	1.833 (2.699)	-0.008 (2.323)	-2.266 (2.658)	4.707 (3.184)
Total expenditures	42.755 (40.797)	-1.313 (23.867)	65.422*** (24.453)	-10.851 (24.191)	9.115 (25.734)

*Notes:* Estimates are calculated with a bandwidth minimising mean-squared errors and a triangular kernel (Calonico, Cattaneo, and Titiunik 2014). Standard deviations are in parentheses. Errors are clustered by inter-municipality.

\*Significant at 10%; \*\*significant at 5%; \*\*\*significant at 1%.

## 2.5 Main results

The general results indicate substantial effects of partisanship on local public expenditures. Table 2.7 shows that differences in budget use mainly concern capital expenditures. Figure 2.5 illustrates these effects, which are the difference between the two curves at the cutoff for significant spending items.

Column (2) includes point estimates in euros per capita and standard errors for investments in each spending item. It indicates that left-wing municipalities invest 65 euros per capita more than right-wing ones. The effect is significant at the 1% level and is considerable given the average

Table 2.6: Placebo Cutoffs for Current Expenditures

(1)	Cutoff				
	(2)	(3)	(4)	(5)	(6)
	-12	-6	0	6	12
General services	12.708 (36.788)	17.213 (31.319)	-0.685 (25.308)	-32.667 (25.079)	33.336 (25.075)
Security	5.244 (6.321)	3.871 (6.763)	-8.009 (6.16)	-4.011 (5.135)	9.082 (6.087)
Education	1.997 (13.196)	-4.748 (9.563)	8.751 (10.365)	24.849** (12.598)	-14.42 (10.363)
Culture	17.396 (12.848)	-11.394 (9.523)	9.819 (9.906)	6.113 (12.078)	-3.101 (10.693)
Sports and youth	10.003 (12.654)	-1.865 (11.404)	7.542 (9.401)	9.163 (10.831)	6.867 (11.178)
Social and public health	13.785 (9.59)	-1.452 (7.24)	6.477 (6.251)	-9.837 (6.863)	4.051 (7.086)
Family	6.939 (11.274)	-7.958 (9.974)	9.245 (8.41)	-1.491 (10.15)	-16.96* (9.432)
Housing	-5.744 (4.13)	2.84 (1.975)	0.136 (1.936)	-0.515 (1.214)	0.782 (1.503)
Urban planning and environment	1.871 (18.07)	-8.727 (16.564)	11.043 (16.08)	-1.641 (16.801)	12.304 (16.225)
Economic stimulus	1.183 (5.353)	-0.441 (5.1)	-5.877 (5.128)	1.834 (5.022)	0.028 (3.234)
Total expenditures	85.501 (65.684)	-12.088 (63.924)	32.249 (57.202)	-13.2 (60.093)	45.387 (58.068)

*Notes:* Estimates are calculated with a bandwidth minimising mean-squared errors and a triangular kernel (Calonico, Cattaneo, and Titiunik 2014). Standard deviations are in parentheses. Errors are clustered by inter-municipality.

\*Significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

municipal capital expenditure of 355 euros per capita (see Table 2.1). Leftist governments also invest 30 euros per capita more in urban planning and environment and 3.7 euros per capita more in family. On the contrary, right-wing municipalities invest 2.4 euros per capita more in security. This effect is also substantial compared to the average of 2.88 euros per capita invested by cities. Column (3) shows that security is the only area that differentiates the two political families in terms of budget allocation; right-wing mayors allocate 0.74 percentage points more of their total investments in security. Columns (4) and (5) confirm that security is a divergence factor. Right-wing governments spend 9.4 euros more per capita and allocate 1.1 percentage points of their total current expenditures on security.

These general results can be deepened by observing the annual difference between left-wing and right-wing parties. An event study highlights the timing of partisanship effects and their concordance with the political-economic cycle. Figure 2.6 shows the evolution of the difference in spending between left and right-wing municipalities during the 2008-2014 term (shaded area). Highly significant effects presented in table 2.7, such as for security current expenditures and family capital spending, do not appear to occur in any particular year (Figure 2.6(e)). On the contrary, total

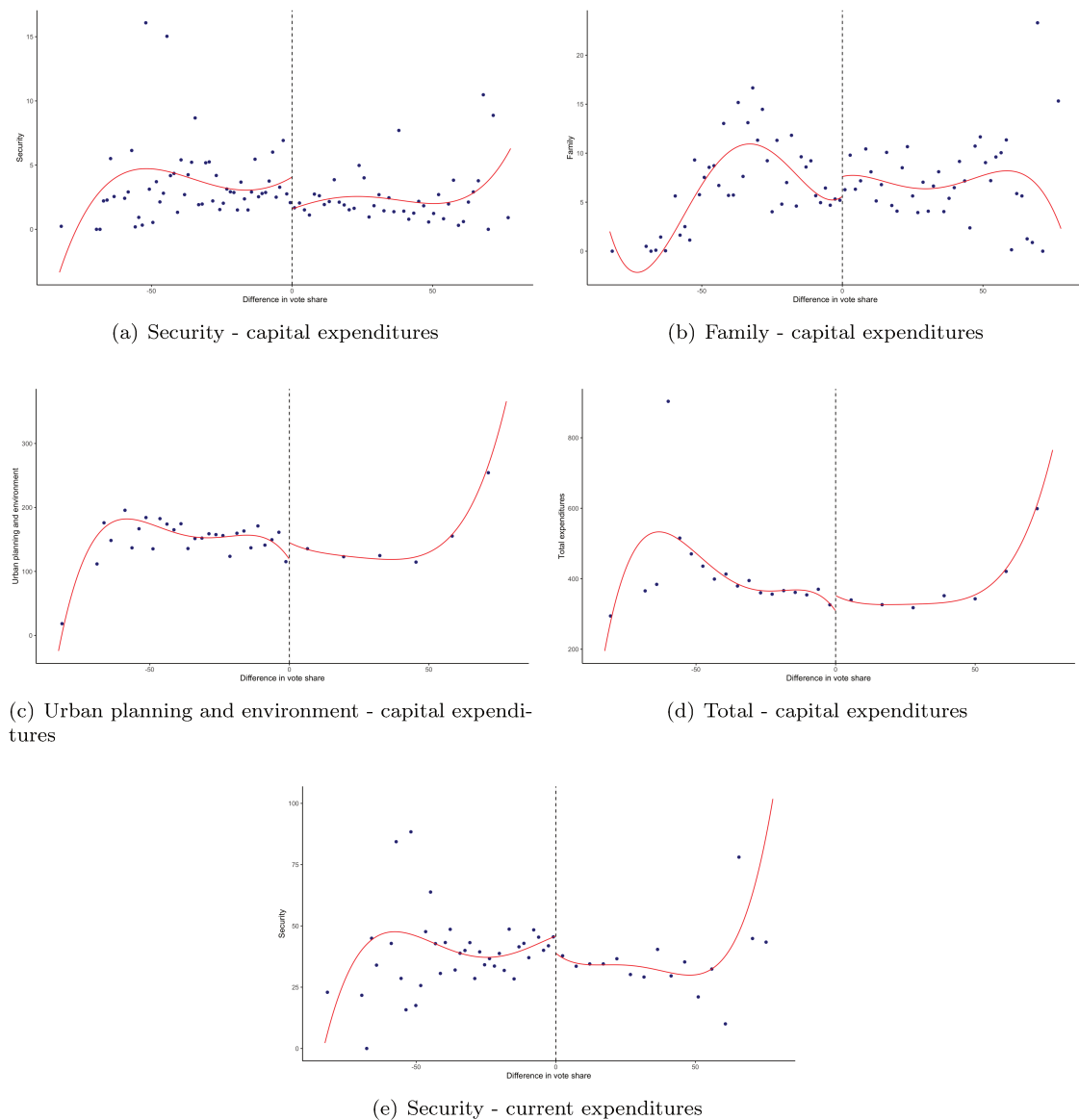


Figure 2.5: RD Plots - Partisanship Effects

capital expenditure, security, and urban planning and environment capital expenditure seem to follow a political-economic cycle, with differences occurring mainly at the end of the mandate.

The magnitude and sign of partisanship effects on total capital expenditures are consistent with previous work (Petterson-Lidbom (2008); de Benedictis-Kessner and Warshaw (2016)), as well as the results concerning security investments (Gerber and Hopkins (2011)). Compared to the existing literature, I find results with strong statistical significance. This statistical power is due to the large number of municipalities in France, which makes it an excellent field of study. It also allows to study heterogeneity in population size and density.

Table 2.7: Partisanship Effect on Public Local Expenditures

(1)	Capital expenditures		Current expenditures	
	(2) Euros per capita	(3) Percentage points	(4) Euros per capita	(5) Percentage points
General services	2.375 (8.9)	0.128 (2.193)	-5.406 (19.492)	0.125 (1.892)
Security	-2.44*** (0.854)	-0.744*** (0.207)	-9.374* (4.99)	-1.093** (0.462)
Education	8.976 (6.621)	-0.562 (1.828)	2.777 (8.844)	-0.406 (0.798)
Culture	6.745 (5.573)	-0.244 (1.735)	6.069 (7.941)	0.331 (0.716)
Sports and youth	-0.426 (8.32)	-1.968 (1.943)	2.693 (7.062)	0.122 (0.64)
Social and public health	0.288 (2.11)	-0.239 (0.543)	0.906 (4.716)	0.181 (0.459)
Family	3.69** (1.831)	0.766 (0.69)	7.68 (7.857)	0.602 (0.739)
Housing	0.129 (1.781)	-0.349 (0.481)	-0.093 (1.852)	-0.1 (0.201)
Urban planning and environment	30.219*** (11.71)	4.12 (2.76)	7.853 (12.589)	0.577 (1.204)
Economic stimulus	0.14 (2.144)	-0.227 (0.525)	-2.864 (3.814)	-0.205 (0.243)
Total expenditures	65.48*** (22.455)		3.744 (28.427)	

*Notes:* Estimates are calculated with a bandwidth minimising mean-squared errors and a triangular kernel (Calonico, Cattaneo, and Titiunik 2014). Covariates selected by post-lasso are added to increase the precision of estimates. Standard deviations are in parentheses. Errors are clustered by inter-municipality.

\*Significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

## 2.6 Heterogeneity in partisanship effects

I study heterogeneous partisanship effects concerning population by constructing clusters of municipalities. I also include the density along with city size to account for congestion issues. The clusters are formed with a data-driven method to avoid endogenous thresholds. I perform a k-means clustering on the logarithm of city size and density (Hartigan and Wong 1979). Following the Calinski-Harabasz criterion, two clusters are created (Caliński and Harabasz 1974). Table 2.8 shows that the first cluster regroups small cities with 6,878 inhabitants on average. The second cluster is made up of larger cities with an average population of 36,567. Row (3) indicates that the proportion of treated municipalities is the same in both clusters. Figure 2.7 represents the two clusters graphically.

Then, I use local polynomial regression with covariates selected using post-lasso to estimate the causal effect of partisanship for both small and large cities. I control the false discovery rate with the Benjamini-Hochberg correction (Benjamini and Hochberg 1995) since I test a causal effect twice for each expenditure item. Thus, the results may be underestimated because multiple

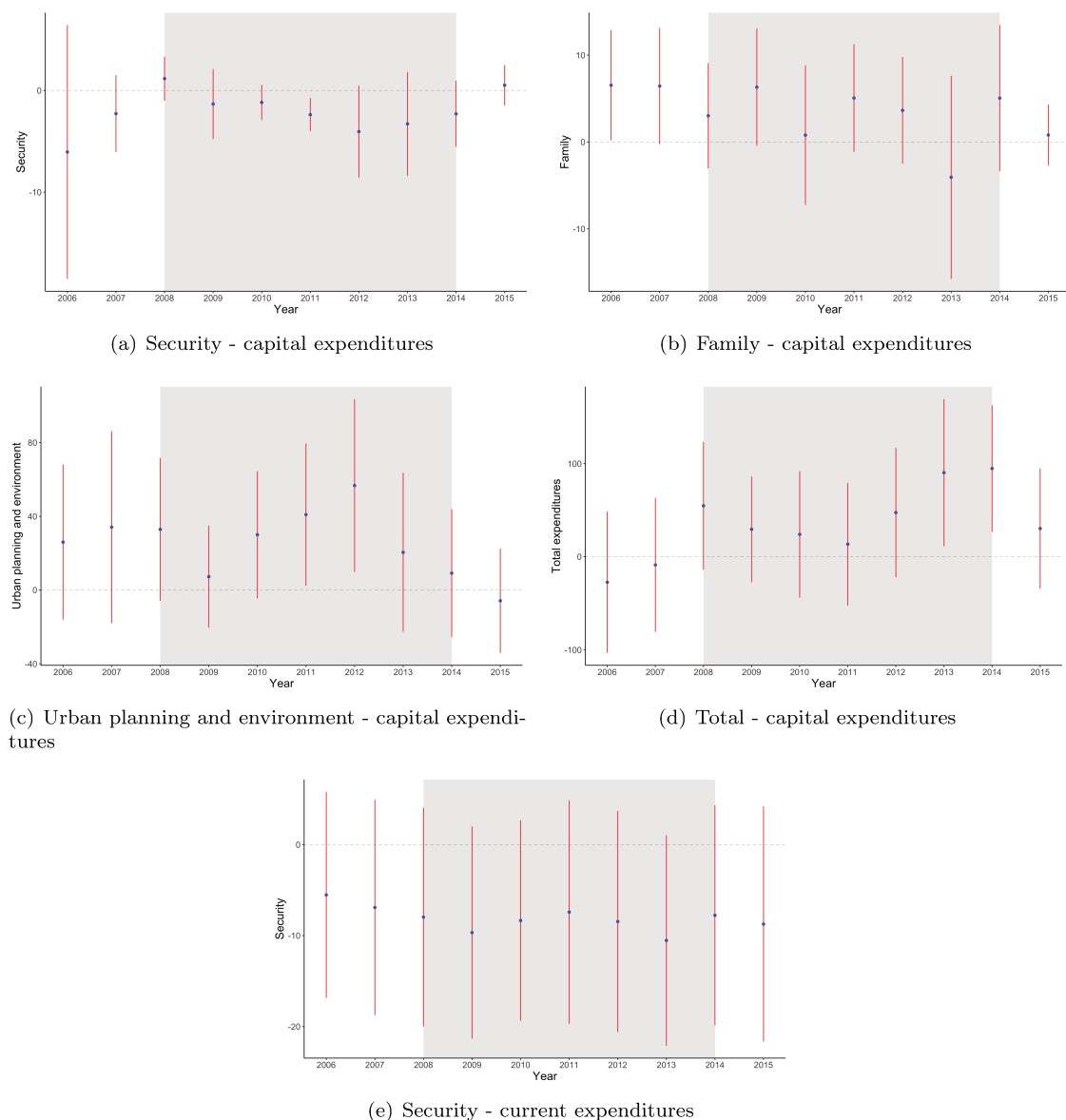


Figure 2.6: Event study - Municipality spending

Table 2.8: Cluster Characteristics

(1)	Cluster 1 ( $N = 1,231$ )			Cluster 2 ( $N = 499$ )		
	(2) Min	(3) Mean	(4) Max	(5) Min	(6) Mean	(7) Max
Population	3015.5	6878.01 (3211.72)	27658.5	7644.5	36567.36 (41055.92)	438584
Density	5.79	27.25 (12.54)	99.22	17.6	80.4 (58.66)	573.25
Proportion of left-wing municipalities		0.53 (0.5)			0.52 (0.5)	

Notes: Standard deviations are in parentheses. Columns (3) and (6) report each variable's mean and standard deviation.

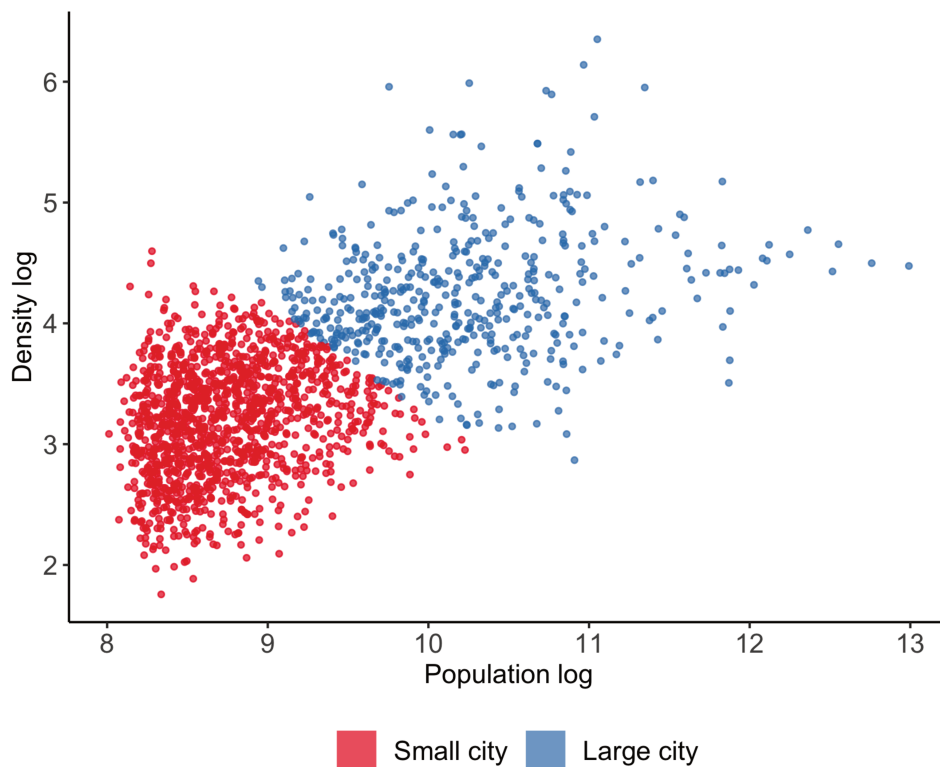


Figure 2.7: Population and Density of Municipalities per Cluster

testing corrections can produce over-rejected tests (Hsu and Shen [2019](#)).

The results for the heterogeneous effects of partisanship on local government spending show only slight differences between small and large cities. Table [2.9](#) reveals that the divergence between political families occurs primarily in capital expenditures, as is the case for the overall results without distinguishing between city size and density. The statistical significance of each spending item estimate is adjusted to account for the false discovery rate with the Benjamini-Hochberg correction. Row (2) shows that political families impact security spending in both small and large cities. Right-wing governments invest 2.2 euros per capita and 2.5 euros per capita more in each group of municipalities. Partisanship only affects current expenditures in small cities: left-wing mayors spend 12.7 euros less on security than their right-wing counterparts. Row (11) highlights that left-wing municipalities invest about 51 euros per capita and 67 euros per capita more in small and large cities, respectively. The magnitude of these results is consistent with the results shown in Table [2.7](#). In large cities, partisanship affects urban planning and environment, and education, where left-wing mayors invest 40 euros per capita and 28 euros per capita more than right-wing governments. Finally, there is a positive difference of 5.3 euros per capita in family capital expenditures in small cities. In summary, there is strong evidence of partisanship effects in small and large cities. While capital expenditures are affected in a range of areas (security, education, family, urban planning and environment), current expenditures are affected only in



security in small municipalities.

Table 2.9: Partisanship Effect – Size Heterogeneity

(1)	Small municipalities					Large municipalities	
	(2) Euros per capita	(3) Percentage points	(4) Euros per capita	(5) Percentage points	(6) Euros per capita	(7) Percentage points	(8) Euros per capita
General services	12.61 (11.819)	1.884 (3.083)	-20.531 (22.396)	-0.501 (2.391)	-9.588 (11.454)	-6.141** (2.654)	
Security	-2.24* (1.277)	-0.58*** (0.277)	-12.702** (5.667)	-1.526*** (0.507)	-2.479*** (0.893)	-1.151*** (0.33)	
Education	7.114 (7.858)	-0.923 (2.277)	-3.137 (9.417)	-0.818 (0.971)	28.107** (13.406)	4.071 (3.138)	
Culture	8.01 (7.497)	0.677 (2.29)	8.02 (7.795)	0.323 (0.779)	-1.38 (6.941)	-1.428 (2.08)	
Sports and youth	-2.988 (9.599)	-2.217 (2.491)	9.563 (8.326)	0.188 (0.725)	5.797 (7.013)	1.231 (1.524)	
Social and public health	-2.139 (1.807)	-0.631 (0.456)	3.632 (6.645)	0.421 (0.637)	2.727 (4.834)	0.454 (1.469)	
Family	5.32*** (2.42)	1.194 (0.874)	7.346 (8.03)	0.601 (0.812)	-0.04 (2.097)	-1.194 (0.84)	
Housing	-1.539 (1.757)	-0.62 (0.517)	0.076 (2.494)	-0.137 (0.242)	5.61 (3.685)	0.588 (0.855)	
Urban planning and environment	22.393 (15.25)	1.393 (3.438)	-0.031 (14.835)	0.433 (1.566)	40.314** (16.617)	5.574 (3.582)	
Economic stimulus	1.086 (2.597)	-0.026 (0.503)	-0.239 (4.043)	0.083 (0.252)	-0.539 (3.522)	-0.592 (1.061)	
Total expenditures	50.595** (25.16)		10.927 (34.281)		66.643* (39.911)		

51

*Notes:* Estimates are calculated with a bandwidth minimising mean-squared errors and a triangular kernel (Calonico, Cattaneo, and Titiunik) are added to increase the precision of estimates. Significance level is corrected for false discovery rate (Benjamini-Hochberg correction) constructed with k-means. Standard deviations are in parentheses. Errors are clustered by inter-municipality.

\* Significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

## 2.7 Conclusion

Local government spending represents a significant part of total government spending in many developed countries. Municipalities and inter-municipalities spend 60%<sup>7</sup> of total public capital expenditure in France. Understanding partisanship effects on public spending is therefore essential. The existing empirical literature on partisanship effects has produced contradictory results on the effect of partisanship on local public spending. Using a regression discontinuity design taking advantage of close-race elections, I show that partisanship effects on capital expenditures are substantial on average for all French cities of more than 3,500 inhabitants.

This result has four main implications. First, while the literature suggests an absence of partisanship in small municipalities, the results of this paper show that political ideology is prevalent in both small and large communes. The managerial view of small communities should therefore be tempered, at least in states where mayors have strong discretionary power (Oliver, Ha, and Callen 2012). Moreover, the presence of substantial partisan effects in small communes mitigates the role of the zoo-effect in the partisan effect (Hajnal and Trounstein 2010). Second, the large and significant differences between political parties run counter to the median voter theorem of Downs (1957), which states that candidates in an election compete for the voter with median preferences, and thus converge in ideology to win the political race. I find a divergence between political colors in the amount of spending and the budget allocation. The latter suggests that mayors have great differences in preferences under budget constraints and supports the findings of (Hill and Jones 2017). Third, these results mitigate the effects of Tiebout sorting (referred to by Ferreira and Gyourko (2009), who found no partisanship effects), which predicts that individuals migrate to municipalities where their preferences are best represented (Tiebout 1956). Observing strong partisanship effects means that divergence between politicians is possible thanks to population heterogeneity in a city: Tiebout sorting is incomplete. Finally, I show that security and urban planning and environment are the two key areas where political color drives differences in local spending. Expenditures at the local level are therefore highly correlated to the political debate at the national level. In the French Parliament, socialist party majorities vote 50% more laws related to environment, while right-wing party majorities vote for more laws linked to public safety (Persico, Froio, and Guinaudeau 2012).

Using a data-driven clustering approach, I construct a partition of French *communes* based on their population and density. Analyzing the impact of political ideology on public spending in each cluster, I highlight heterogeneous treatment effects in small and large municipalities for capital expenditures. Results on partisan effects heterogeneity show that left-wing governments invest more in small and large cities. However, the differences in spending are much more visible in the expenditures broken down by function for large municipalities. Therefore, the effect of political

---

<sup>7</sup>French Observatory of Local Public Finance and Management (2020)

color is more structured in the latter, crystallized in security, education, and urban planning and environment, with respectively -2.5, +28, and +40 euros per inhabitant for left-wing parties. In small municipalities, partisanship effects are less articulated around specific spending areas. While left-wing parties invest more than right-wing parties at 50.5 euros per inhabitant, the differences in expenditure by function are only in family and security, with respectively -2.2 and +5 euros per inhabitant for left-wing parties. Expenditures are thus closer to the national debate in large municipalities than in smaller ones. Further research should investigate the heterogeneous effects of partisanship by city size and density in various countries to increase external validity.

## 2.8 Appendices

### 2.8.1 Appendix A. Sensitivity to political party classification

Sensitivity to political party classification of Table 2.2. The parties at the extremes of the classification are removed to check the robustness of the results. I observe that removing a particular party only marginally affects the results.

Table 2.10: Sensitivity to Political Party Classification

	Political party removed from estimation					
	LCOM		LDVG		LMC	
	Euros per capita	Percentage points	Euros per capita	Percentage points	Euros per capita	Percentage points
General services	1.541 (10.581)	-1.212 (2.431)	-5.779 (11.803)	-3.143 (2.618)	-0.769 (10.708)	-1.467 (2.508)
Security	-2.317*** (0.869)	-0.696*** (0.213)	-2.172** (0.895)	-0.675*** (0.221)	-2.503*** (0.899)	-0.74*** (0.217)
Education	7.881 (7.342)	-0.481 (1.934)	0.047 (7.278)	-2.471 (2.034)	7.246 (7.384)	-0.352 (1.995)
Culture	6.486 (5.784)	-0.398 (1.823)	7.304 (5.356)	0.124 (1.716)	6.005 (5.723)	-0.618 (1.836)
Sports and youth	-0.923 (8.443)	-2.351 (1.986)	-5.893 (6.921)	-1.997 (1.962)	-2.279 (8.342)	-2.341 (2.008)
Social and public health	0.154 (2.277)	-0.182 (0.572)	-0.361 (2.019)	-0.281 (0.504)	0.01 (2.288)	-0.192 (0.572)
Family	2.623 (1.937)	0.452 (0.681)	5.329** (2.547)	1.278 (0.908)	3.142 (2.102)	0.695 (0.711)
Housing	0.128 (1.781)	-0.337 (0.478)	0.909 (1.88)	-0.143 (0.486)	0.365 (1.856)	-0.354 (0.497)
Urban planning and environment	36.882*** (14.162)	5.492* (3.174)	36.892*** (15.15)	6* (3.215)	25.292* (13.44)	5.938* (3.366)
Economic stimulus	0.055 (2.337)	-0.164 (0.558)	2.447 (2.65)	0.422 (0.599)	-0.214 (2.358)	-0.184 (0.553)
Total expenditures	66.34*** (24.535)		53.82** (26.004)		55.045** (24.598)	

*Notes:* Estimates are calculated with a bandwidth minimising mean-squared errors and a triangular kernel (Calónico, Cattaneo, and Titiunik 2014). Standard deviations are in parentheses. Errors are clustered by inter-municipality.

\*Significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

## 2.8.2 Appendix B. Placebo effects on past term expenditures

Tables 2.11 and 2.12 show estimates for partisanship effects on municipality spending before the 2008-2014 term respectively for capital and current expenditures.

Table 2.11: Partisanship Effects on Placebo Capital Expenditures

(1)	(2)	(3)	(4)	(5)	(6)
	Estimate	SE	p-value	BH correction	Bandwidth
General services	-12.513	13.150	0.341	0.559	19.884
Security	-4.129	4.144	0.319	0.559	21.030
Education	14.205	7.697	0.065	0.357	14.049
Culture	-16.720	18.112	0.356	0.559	15.224
Sports and youth	0.772	11.761	0.948	0.948	22.664
Social and public health	-1.155	2.056	0.574	0.632	20.640
Family	6.682	2.695	0.013	0.145	10.060
Housing	-3.106	4.201	0.460	0.632	16.313
Urban planning and environment	31.671	22.518	0.160	0.439	15.051
Economic action	-6.540	4.365	0.134	0.439	18.045
Total expenditures	-21.325	33.275	0.522	0.632	22.995

*Notes:* Estimates are calculated with a bandwidth minimising mean-squared errors and a triangular kernel (Calonico, Cattaneo, and Titiunik 2014). Errors are clustered by inter-municipality. p-values are adjusted with the Benjamini-Hoschberg correction.

Table 2.12: Partisanship Effects on Placebo Current Expenditures

(1)	(2)	(3)	(4)	(5)	(6)
	Estimate	SE	p-value	BH correction	Bandwidth
General services	0.736	26.808	0.978	0.978	25.265
Security	-6.278	6.023	0.297	0.682	20.293
Education	9.069	9.342	0.332	0.682	15.419
Culture	5.112	9.264	0.581	0.808	18.120
Sports and youth	8.509	9.003	0.345	0.682	18.097
Social and public health	1.151	6.545	0.860	0.946	17.174
Family	10.771	7.094	0.129	0.682	17.490
Housing	-0.832	1.534	0.588	0.808	18.699
Urban planning and environment	14.441	16.182	0.372	0.682	14.726
Economic action	-5.142	4.839	0.288	0.682	19.642
Total expenditures	24.788	56.906	0.663	0.810	16.919

*Notes:* Estimates are calculated with a bandwidth minimising mean-squared errors and a triangular kernel (Calonico, Cattaneo, and Titiunik 2014). Errors are clustered by inter-municipality. p-values are adjusted with the Benjamini-Hoschberg correction.

### 2.8.3 Appendix C. Additional RD plots

RD plots - Capital expenditures (1/2)

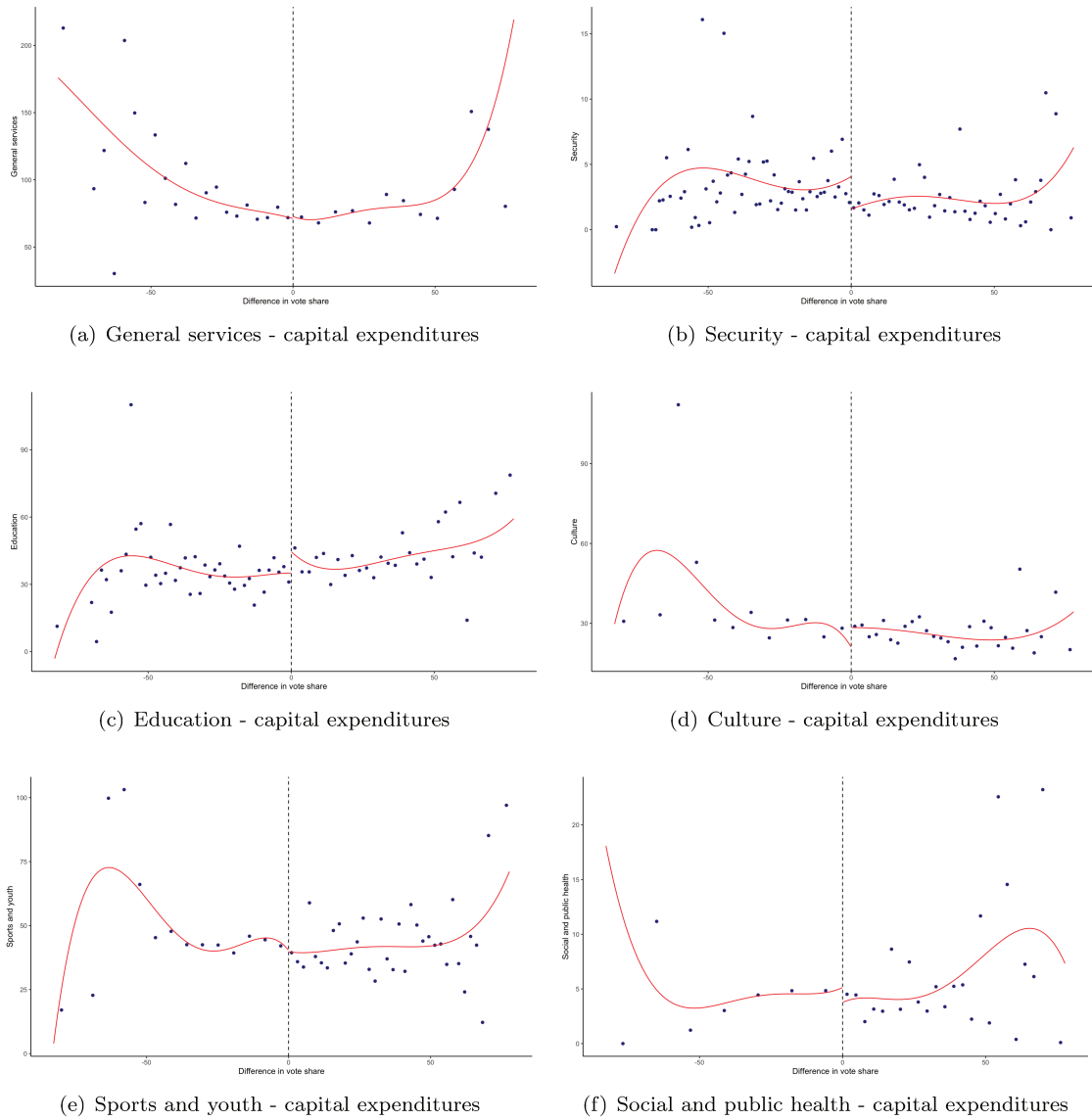


Figure 2.8: RD Plots - Capital Expenditures (1/2)



RD plots - Capital expenditures (2/2)

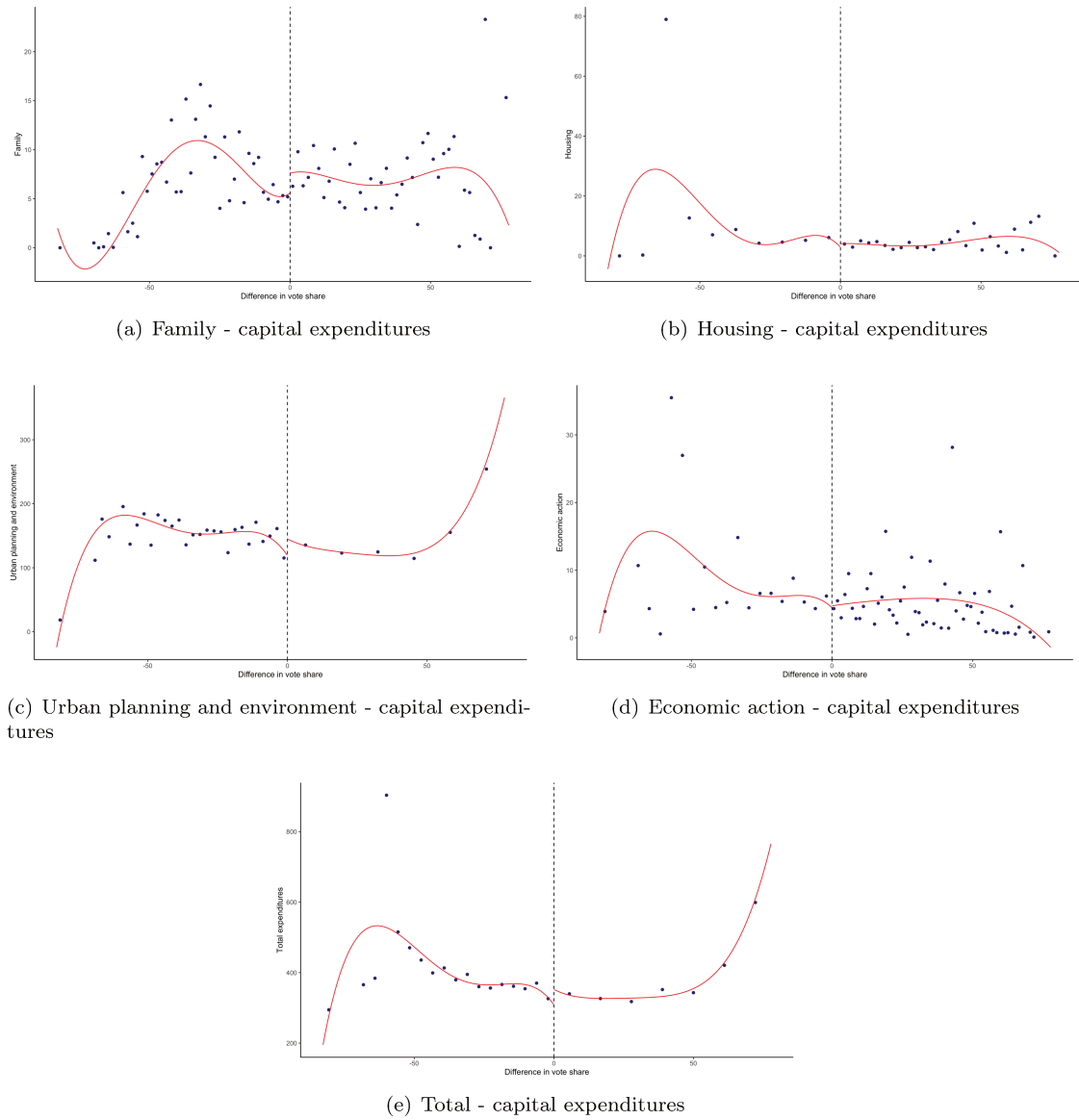


Figure 2.9: RD Plots - Capital Expenditures (2/2)

RD plots - Current expenditures (1/2)

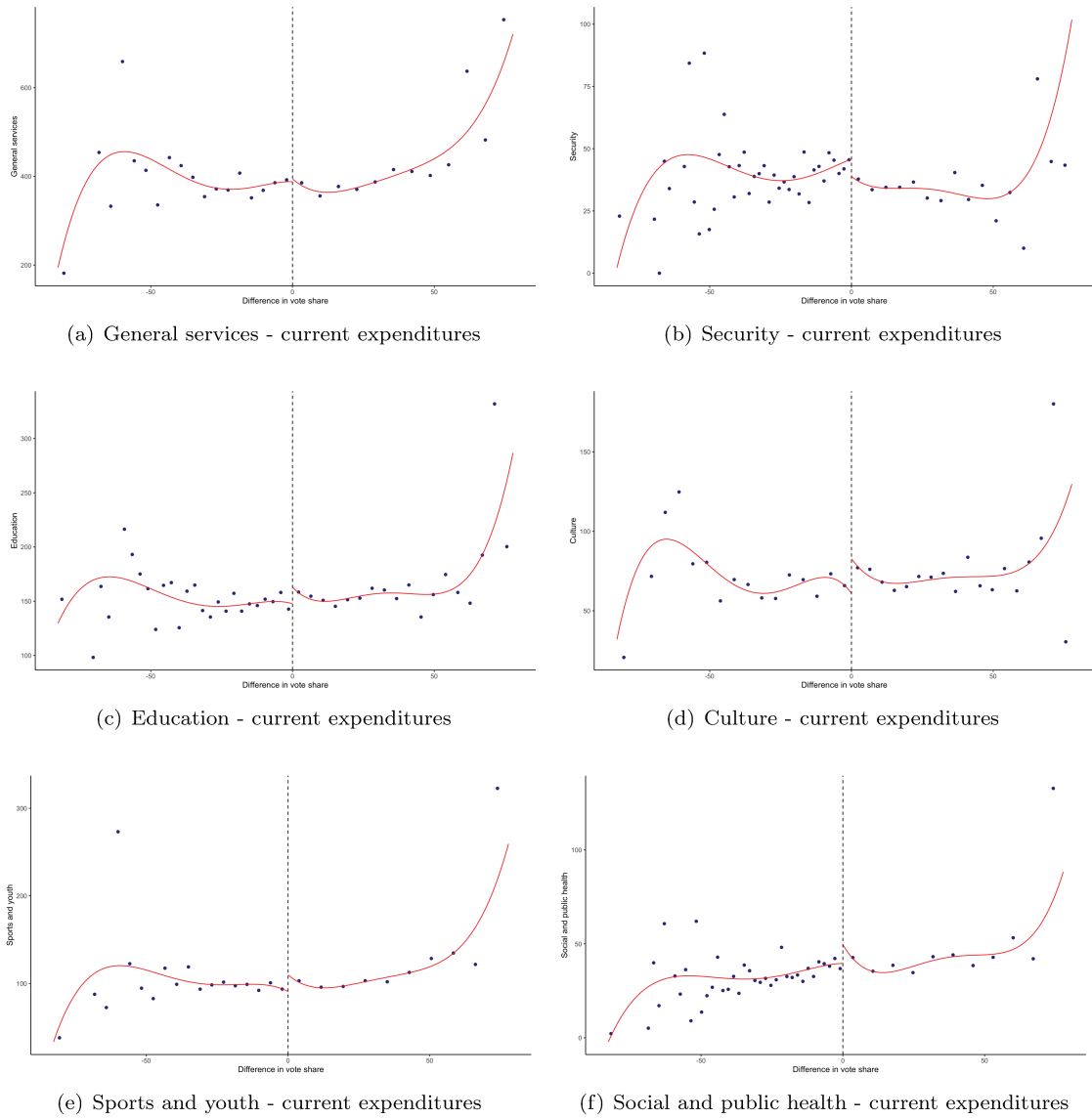


Figure 2.10: RD Plots - Current Expenditures (1/2)

RD plots - Current expenditures (2/2)

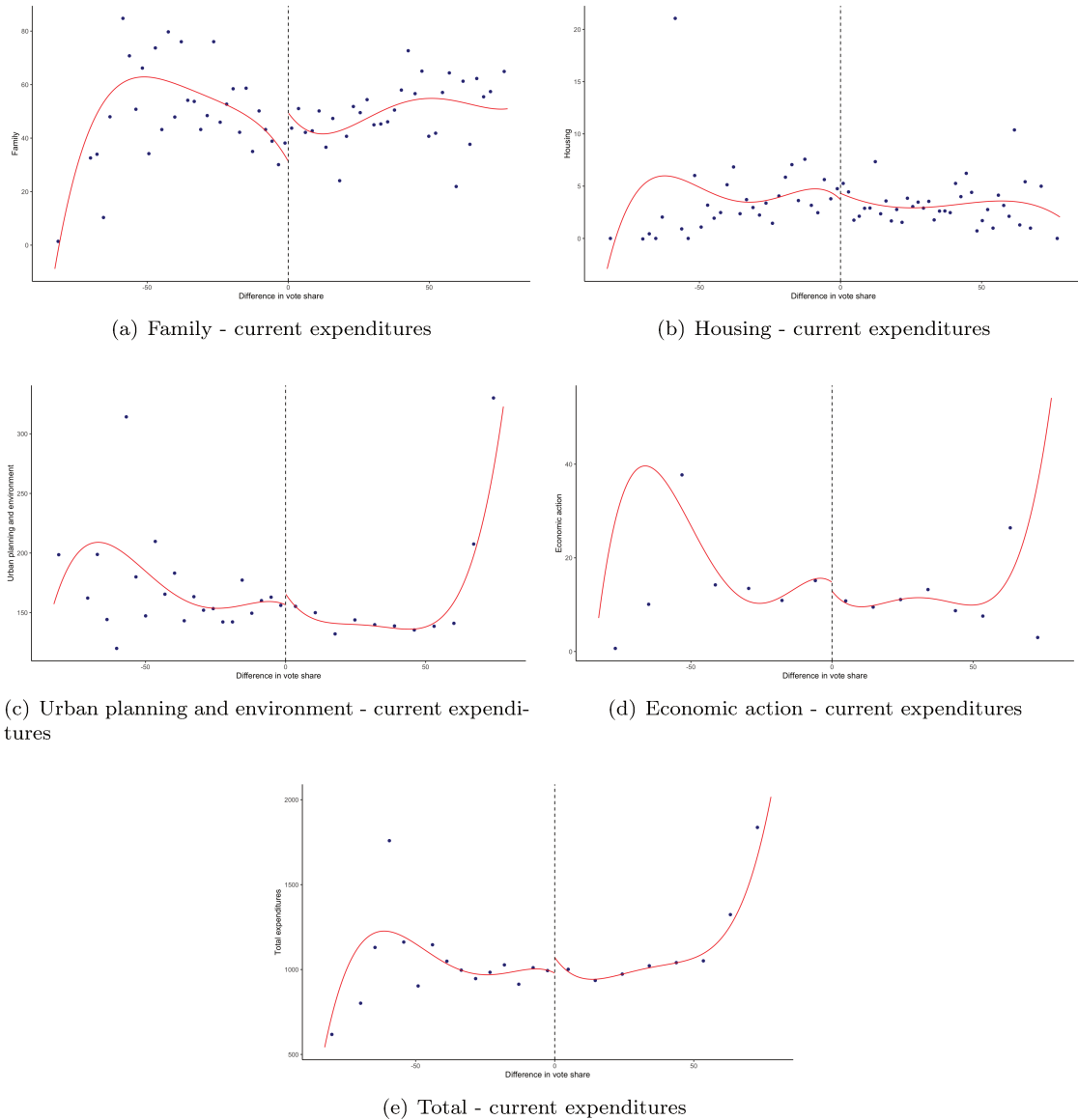


Figure 2.11: RD plots - Current expenditures (2/2)

### 2.8.4 Appendix D. Potential effect of inter-municipality expenditures

Inter-municipalities aim to strengthen cooperation between French municipalities. Their expenses may interact with those of cities. However, the 2010 territorial reform law requires all French "commune" to belong to an inter-municipality as of July 1, 2013. Thus, this inter-municipal cooperation only occurs at the end of the analysis period.

To verify a potential difference in regional integration according to communal political color, I

construct a variable of EPCI expenditures per capita for municipalities.

Table 2.13 shows the RDD on total current and capital expenditures. There is no difference between left-wing and right-wing municipalities in investment expenditures. However, the right-wing EPCIs have operating expenditures that are 80 euros more per capita on average. It means that right-wing communes could be more integrated: current expenditures of municipalities may therefore be carried over to the level of inter-municipalities. Since partisanship on current spending is slight, this paper's results may thus underestimate differences in current expenditures.

Table 2.13: Partisanship Effect on Inter-municipal Expenditures

	Capital Expenditures	Current Expenditures
Total Inter-municipal expenditures	-14.996 (16.125)	-80.392** (32.856)

*Notes:* Estimates are calculated with a bandwidth minimising mean-squared errors and a triangular kernel (Calonico, Cattaneo, and Titiunik 2014). Errors are clustered by inter-municipality.



## Chapter 3

# Industrial Accidents, Nominal Loss Aversion, and the Housing Market

*With Gabriel Loumeau*

This paper studies the housing market effects of industrial accidents by exploiting the 2019 Lubrizol factory fire in Rouen, France. Using a difference-in-differences estimation strategy and rich property-level data of real estate transactions in France between 2017 and 2020, we find that the fire has not impacted housing prices in Rouen, whereas – immediately after the fire – prices declined within a radius of 1km to other high-risk industrial sites in France as well as further away under the pollution cloud caused by the fire. Using neighborhood-level housing supply information, we show that the seller’s nominal loss aversion rationalizes the results observed well. Following the fire, sellers listed significantly higher prices (+35%) and exhibited a much lower selling hazard.

**Keywords:** Housing price, industrial accident, nominal loss aversion, difference-in-differences.

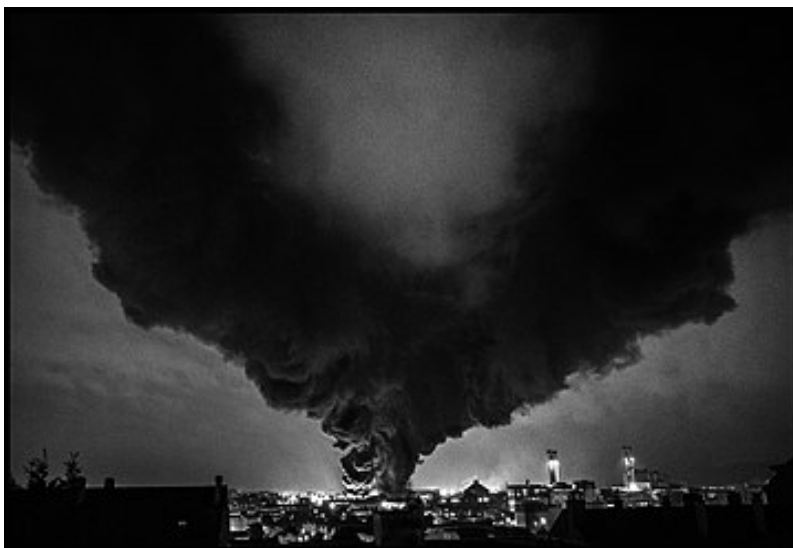
**JEL:** Q53, R31, D62, G14

### 3.1 Introduction

How does the housing market react to industrial accidents? From Bhopal, India, in 1984 to Fukushima, Japan, in 2011, industrial accidents are more common than anyone would hope. According to the European Major Accident Reporting System, over 30 industrial accidents occur each year in Europe on average<sup>1</sup> Only a few inhabited areas are distant enough from industrial sites to be free of disaster risk. Understanding how the housing market reacts to such events is of broad interest to homeowners, renters, real estate investors, and policymakers alike.

This paper exploits the 2019 accidental Lubrizol factory fire in Rouen (Northern France) to study the housing market effects of industrial accidents. Before the fire, the plant synthesized and stored chemical products. Due to its toxic nature, the site was covered by the EU’s Seveso–III Directive.<sup>2</sup> As illustrated in Figure 3.1, the factory fire generated a thick plume of black smoke. In the following days, due to the dominant North-Eastward wind, black residue where observed falling from the sky in Lille, Belgium, and up to The Netherlands (i.e., more than 400km away). This unique setting allows us to study both the effects of the explosion in the vicinity of the factory; but also how housing prices reacted to the pollution cloud even far from Rouen (*Lubrizol cloud effect*), and how housing prices evolved close to other high-risk facility effects (*Seveso effect*). For each estimate, we define a treated zone around the points of interest (respectively, the Lubrizol plant, the communes affected by the smoke cloud listed by the prefectures, and the Seveso-classified sites on the territory) as well as a control zone around these treated zones. We causally estimate these three distinct effects using a difference-in-differences (DiD) identification strategy.

Figure 3.1: Pollution Cloud over Rouen During the Night of September 26th, 2019



*Note: The smoke from the explosion formed a large black cloud reaching more than 20 km in width.*

<sup>1</sup>Source: <https://emars.jrc.ec.europa.eu/en/emars/content>. In total, about 12,000 establishments are considered high-risk industrial facilities in Europe.

<sup>2</sup>For more on the directive, consult: <https://ec.europa.eu/environment/seveso/>.

We use several microdata sources defined at a very fine temporal and spatial scale, such as ge-localized real estate transactions and seloger data which gives the volume of real estate ads and prices per square meter within an 80-kilometer radius of Rouen.

Surprisingly, we find that housing prices in Rouen are mostly unaffected by the fire, whereas the number of transactions drops to near zero. At the same time, we find that housing prices in the vicinity of other high-risk facilities declined by 15%, and that prices also declined by 10% away from Rouen but under the pollution cloud caused by the fire. This result contradicts research by Grislain-Letrémy and Katosky (2013) showing that property prices are unaffected by either local accidents or the AZF accident in Toulouse, France.

Nominal loss aversion by the seller (Genesove and Mayer 2001, Engelhardt 2003, Anenberg 2011) rationalizes well the results observed. Close to the factory fire, the housing supply drops as significant losses – below the historical purchased value – are expected. Hence, the number of transactions reaches near zero. Close to the fire, prices actually decline with distance as the supply shrinks relatively less. The negative effect on prices only reduces with the distance to the fire when sellers stop expecting nominal losses. We estimate that this tipping point is located 50km away from the fire. In line with this explanation, the number of transactions increases with the distance to the fire until reaching the tipping point. Beyond that, prices adjust and not transactions.

This paper contributes to several strands of the literature. First, the study sheds light on agents' reactions following a significant industrial incident in the immediate vicinity and the rest of the territory. This paper then feeds the literature on industrial accidents by underlining the counterintuitive result of the absence of a price decrease near the explosion. The surprising absence of price decline in the vicinity of the accident, coupled with the decline in transactions, can be explained by the nominal aversion loss. This last contribution for policymakers is salient, as industrialization and energy issues are central. Secondly, the analysis of the informational channel is naturally linked to information theories, underlining the imperfect character of the information available to the inhabitants, leading to an abrupt change in risk perception. Finally, we document the reaction of agents to pollution observed (smoke cloud) and recognized by the administrative authorities (ban on the sale of crops in several dozen communes). This paper further proves that pollution, as an adverse amenity, depresses real estate market prices.

The remainder of this paper proceeds as follows. Section 3.2 presents the context behind the 2019 Rouen factory fire. Before presenting our empirical strategy and results in Section 3.4, we describe the data used in Section 3.3. Finally, Section 3.5 concludes.



## 3.2 Background and Context

**Seveso sites** The Lubrizol plant in Rouen is one of 692 Seveso "high threshold" facilities in France, regulated by the European directive 2012/18/EU (known as Seveso 3) due to the significant environmental risk it represents. These installations, represented by figure 3.15, must be listed by the State and controlled by a competent authority to limit risks. However, these precautions are not enough to prevent disasters in Europe, such as the fireworks disaster in 2,000 that killed 23 people in Enschede, Netherlands, or the rupture of a tank in an alumina plant in 2010 in Ajka, Hungary.

**The 2019 Lubrizol factory fire** More recently, the explosion of the Lubrizol plant in Rouen, France, on September 26, 2019, released a very thick cloud of polluted smoke, which reached northern France, Belgium, and the Netherlands, as illustrated by the figure 3.2, produced by the French National Institute of Industrial Environment and Risks. To prevent any health risk related to the spread of these toxic fumes, three French departments (Seine-Maritime, Somme, and Aisne) have decided to ban the distribution of many agricultural products: products harvested after September 26, milk from grazing cows, eggs from free-range hens, honey and farmed fish.

Figure 3.2: Cloud of Toxic Smoke



Source: French National Institute for the Industrial Environment and Risks (INERIS)

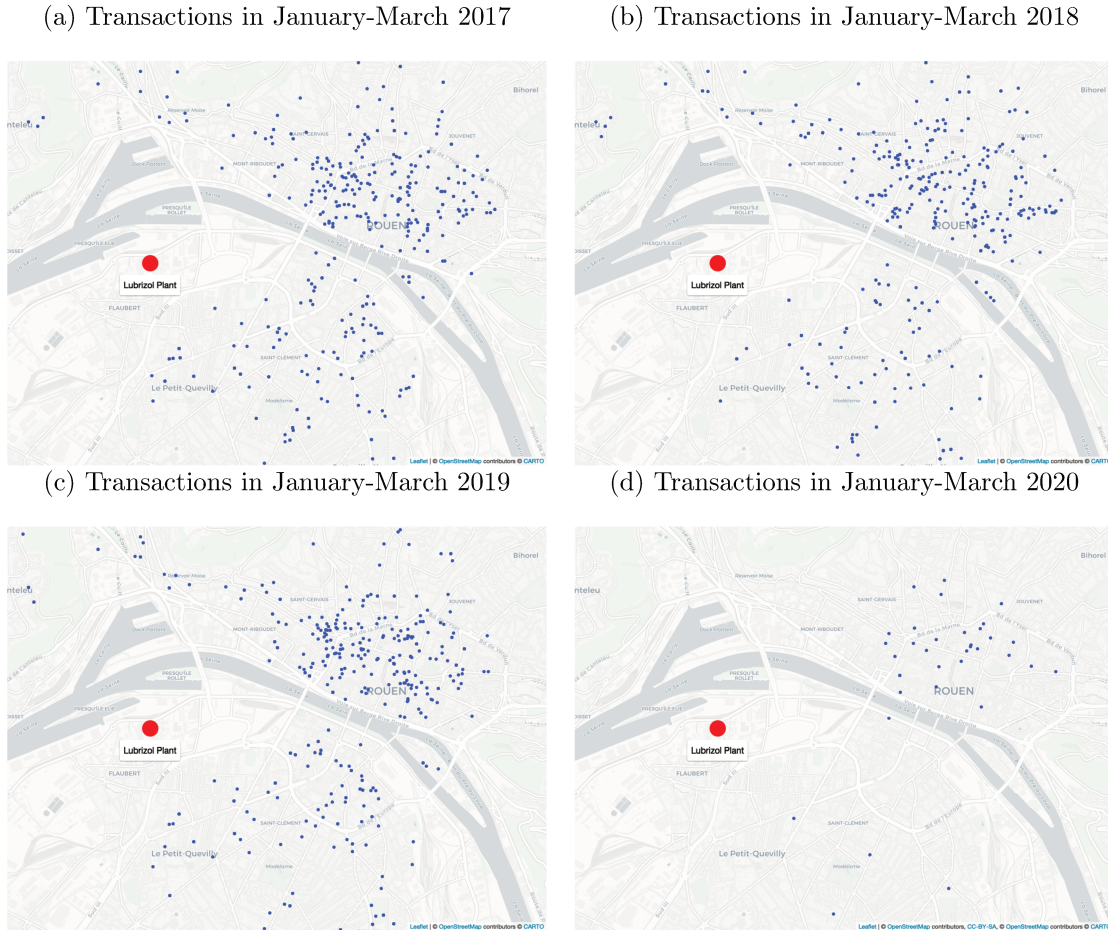
The Lubrizol industrial accident and its consequences, i.e., the propagation of the toxic smoke cloud depending on the wind direction, are exogenous.

**Shock to the housing market** Figure 3.3 illustrates each transaction's location in January-March 2017, 2018, 2019, and 2020, separately<sup>3</sup>. The January-March period is used so that the

<sup>3</sup>The data on real estate transactions are from the Cerema, a French public institution assisting the national government and local authorities in the development, deployment, and evaluation of urban planning and transport policies.

number of transactions is not impacted by restrictions linked to the Covid-19 pandemic. A drastic drop in the number of transactions is observed in the first quarter of 2020.

Figure 3.3: Transactions in the Neighborhood of the Lubrizol Plant



Notes on panels (a),(b), (c) and (d): Location of transactions in January-March in Rouen.

**Citizens awareness** Figure 3.12 shows that the two major TV news programs reported on the incident as it happened.

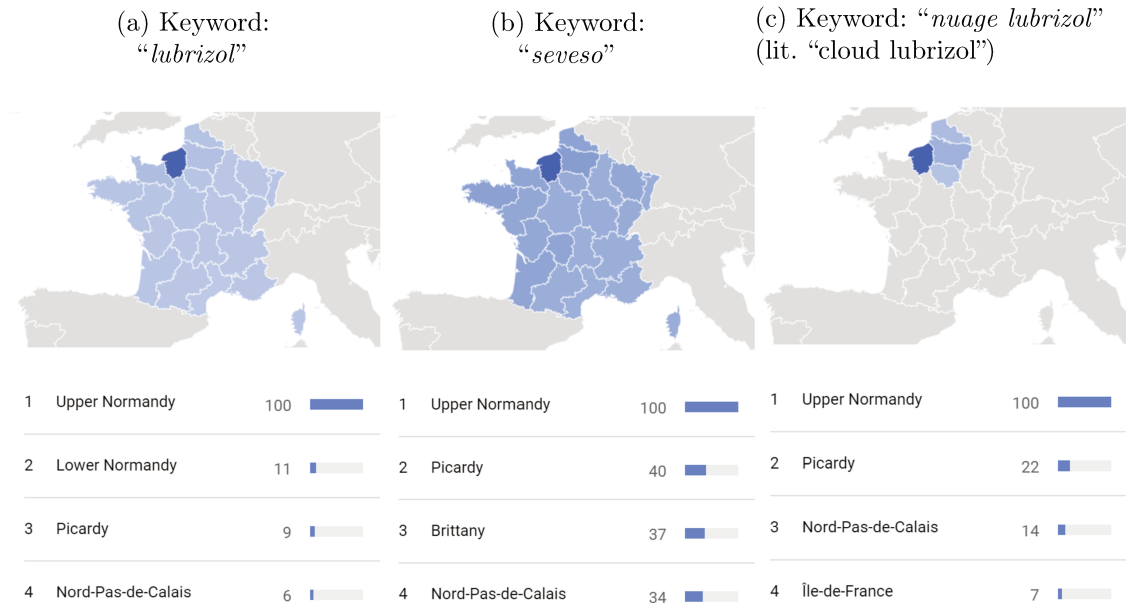
We exploit Google trends data – from 2019 at the regional level – to study the level of interest and awareness of French citizens on the Lubrizol incident. Figure 3.4 illustrates regional differences in the intensity of Google search for three keywords: “*lubrizol*” (panel a), “*seveso*” (panel b) and “*nuage lubrizol*” (panel c)<sup>4</sup>. We observe that searches for the keyword “*lubrizol*” are highly concentrated in Upper Normandy, the region where the incident occurred. The intensity of searches for that keyword is about ten times lower in lower Normandy, the second region in the intensity of “*lubrizol*” searches. The picture is very different when looking at searches for the keyword “*seveso*”. The highest intensity is still observed in Upper Normandy, but the relative intensity of searches is much higher in the country’s other regions. Finally, panel (c) reveals that it is primarily the

<sup>4</sup>*Nuage* means cloud in French.

regions under the cloud that searched for the keyword “nuage Lubrizol”. In the remaining regions, the number of searches is meager, so trends are not computed for these regions.

This exercise illustrates that French citizens were generally aware of the incident, that the connection between the incident and the possible Seveso industry next to them was known, and that residents of regions under the cloud were also concerned about the cloud pollution following the wind direction.

Figure 3.4: Google Trends as a Proxy for Citizens Awareness



Notes: Google trends presented at the regional level. Accessed on: 09/01/2023 at 12:29. Trends are relative to the maximum search (normalized to 100). Dark blue is used to represent the region with the highest number of searches for the keyword.

### 3.3 Data

We rely on two principal data sources to study the effects of the Lubrizol plant explosion on the real estate market.

**Data on real estate transactions in France.** The DV3F database published by Cerema<sup>5</sup> contains all real estate transactions in France from 2017 to the end of 2020. It is used to assess the impact of the Lubrizol explosion on the real estate market, in Rouen, under the cloud of toxic fumes, and near the other Seveso facilities in France. It contains geo-referenced location information, structural characteristics of the properties (type of property, surface area, number of rooms), and transaction value. Only apartments are kept in the data set to reduce heterogeneity when analyzing the effects of the Lubrizol explosion in the city of Rouen or for the information

<sup>5</sup>French public institution assisting the national government and local authorities in developing, deploying, and evaluating urban planning and transport policies.

channel when studying the neighborhood of other Seveso plants. Houses are included in the dataset to estimate the effect of the toxic cloud on property value since the territory is mainly rural. Moreover, the transactions identified by the Cerema as extraordinary properties are discarded<sup>6</sup>. Table 3.1 presents the main characteristics of the 1.1 million properties kept in the database between 2017 and 2020.

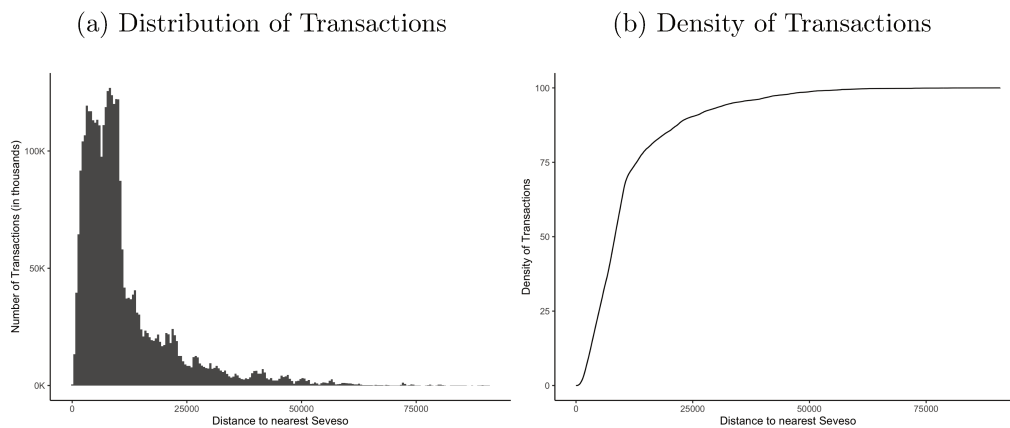
Table 3.1: Descriptive Statistics of Housing Data

Statistic	Mean	St. Dev.	Min	Max	N
Apartment Value	212,020.9	223,081.6	0	7,400,000	1,117,651
Area	57.4	26.5	0	284	1,117,651
Price per square meter	3,234.7	2,713.6	0.002	29,901.0	1,117,651
Distance to Lubrizol	378,365.1	264,828.0	454.7	1,101,610.0	1,117,651
Distance to nearest Seveso	11,318.6	10,509.8	37.9	90,863.9	1,117,651

**Data on classified Seveso facilities.** The database listing classified Seveso installations comes from the French Ministry of Ecological Transition and Territorial Cohesion. The observations are geolocated and broken down according to the risk threshold (high threshold, low threshold, not classified Seveso). Properties in the dataset are, on average, at 10,509 meters of a Seveso plant in France.

Figure 3.5 shows the transactions' distribution and cumulative distribution along the distance to the nearest Seveso. We can observe that 90% of the transactions in the dataset are within a 25km radius of a Seveso plant, and 75% are within a 13km radius of a Seveso plant.

Figure 3.5: Distribution of the Transactions between 2017 and 2020



<sup>6</sup>Exceptional properties are dwellings with more than eight rooms strictly speaking, houses with more than 300m<sup>2</sup> of built-up area, apartments with more than 200m<sup>2</sup> of built-up area

**Data on listings by sellers.** This measures the supply of housing before and after the factory fire. The data transmitted by the company seloger, one of the French leaders of real estate advertisement on the Internet (+7 Million unique visitors per month), contain the price per square meter as well as the number of monthly advertisements at the IRIS level<sup>7</sup> between 2017 and 2021, in a radius of 80km around the explosion. Figure 3.6 shows the spatial coverage of the data, pointing out that observations are within a radius of 80km around the explosion.

Figure 3.6: Spatial Coverage of seloger Data



## 3.4 Empirical analysis

### 3.4.1 Identification strategy

We adopt a difference-in-differences (DiD) identification strategy. The estimated model is as follows:

$$y_{it} = \alpha_1 + \alpha_2 T_i + \alpha_3 T_i \cdot P_t + \gamma_i + \delta_{dt} + \epsilon_{it} \quad (3.1)$$

where  $y_{it}$  represents the dependent variables of the analysis,  $T_i$  is a dummy variable indicating the localities in the treated area, i.e., close to the Lubrizol factory, and  $P_t$  is the dummy indicating the period after the Lubrizol fire. Moreover,  $\gamma_i$  and  $\delta_{dt}$  capture municipal and department-year fixed effects, respectively. Numerous robustness tests, available in the appendices, are carried out to determine the treatment and control zones. The DiD estimator is obtained by the interaction term  $T_i \cdot P_t$ , which captures the effect of the fire on the treatment group. Finally, the standard errors are clustered at the municipal level.

<sup>7</sup>The IRIS is a statistical unit used by the National Institute of Statistics and Economic Studies. The cuts are carried out to regroup blocks between 2,000 and 5,000 inhabitants, homogeneous in the habitat type and delimited by the principal ways of communication (roads, railroad network) or water courts.

To study the evolution of housing prices, we primarily use two metrics. First, we look at the *transaction price* in € per m<sup>2</sup>, which is the price at which the property was effectively sold. Second, we study the *hedonic price*, which we compute by regressing separately for each period the transaction price per m<sup>2</sup> on the characteristics of the property: type of housing (i.e., house or apartment), size of the housing unit, number of rooms, size of garden.<sup>8</sup> We then extract the residual – to which we add the regression constant – as an outcome. The hedonic price can be seen as the price of a reference dwelling when we purge it of all its characteristics.

When focusing on transactions, we sum the number of housing transactions within spatially defined areas – either equal-sized grid cells or municipalities.

### 3.4.2 Results around the factory fire

Table 3.2 reports the DiD estimator of the effects of the Lubrizol plant explosion on real estate prices and the volume of transactions. Columns (1), (2), and (3) respectively detail the results for the price per m<sup>2</sup>, the logarithm of the price per m<sup>2</sup>, and the hedonic price. Column (4) displays the difference in the number of transactions after the plant explosion. While the explosion has no impact on the prices per square meter in the plant’s immediate vicinity, there is a substantial decrease in the number of transactions. The volume of transactions drops drastically, from about 48 transactions per month, significant at the 1% level, indicating a complete market halt.

Figure 3.7 shows the event study associated with this DiD estimator, obtained when interacting semesters and the treatment. We can see that prices are not impacted over time but with a high variance. As for the volume of transactions, it falls drastically after the explosion and remains stable. Finally, Figure 3.3 illustrates this last phenomenon by showing the volume of transactions over the January-March period from 2017 to 2020. While the number of transactions was stable between 2017 and 2019, the market appeared deserted in January-March 2020. The lack of change in housing market prices is consistent with the work of Grislain-Letrémy and Katosky (2013), finding that local incidents do not negatively impact housing prices. However, Anstine (2003) highlights that prices in the real estate market decrease when pollution is observed. The smoke cloud emitted by the explosion did not impact housing prices in Rouen, which contradicts this statement.

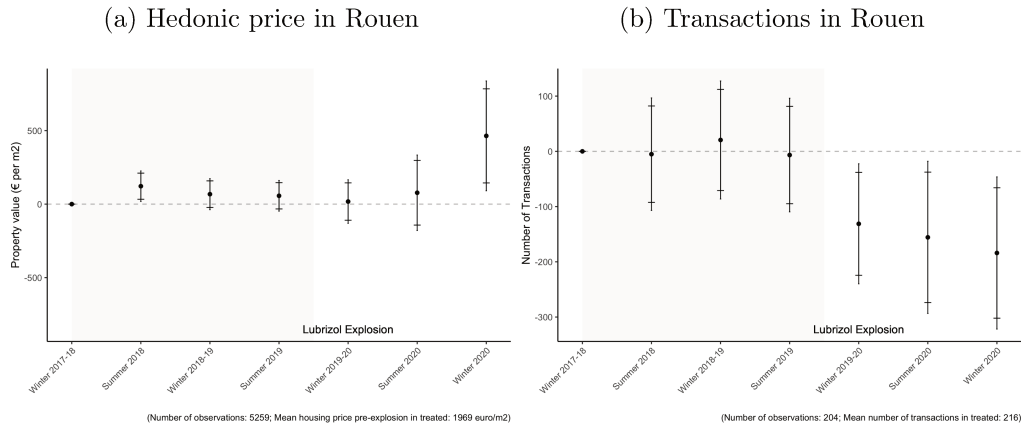
### 3.4.3 Identifying the Lubrizol Cloud Effect

Figure 3.8 shows the evolution of the effects of the Lubrizol explosion on prices and volume of transactions under the cloud. In contrast to the effects observed near the Lubrizol plant, panels (a) and (b) show a pronounced decrease of about 100 euros in the price per square meter of real estate transactions. The volume of transactions remains stable over time and does not seem

---

<sup>8</sup>On the hedonic price index approach using French housing data, see Gourieroux and Laferrere (2009), Musiedlak and Vignolles (2016), Combes, Duranton, and Gobillon (2019), and Tricaud (2021).

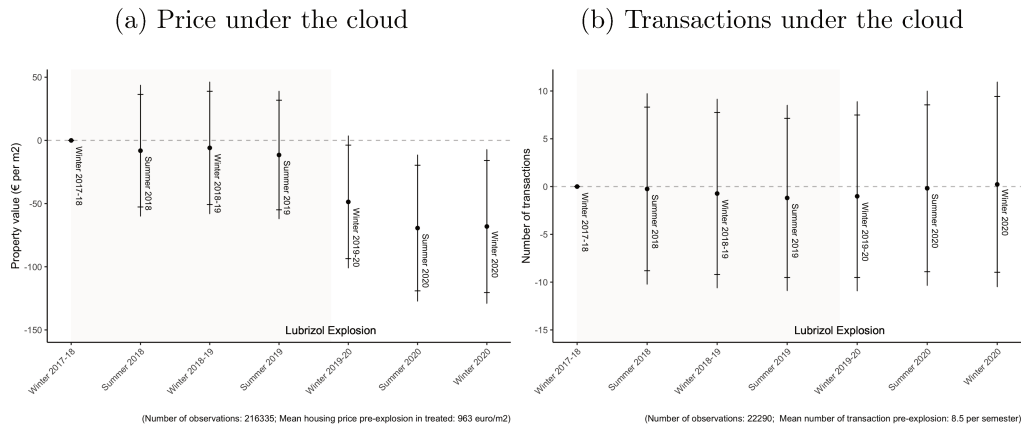
Figure 3.7: Housing Market Effects over Time



Notes on panels (a) and (b): An event study difference-in-difference strategy is adopted. 95% confidence intervals are presented. For (a) treated transactions are all transactions in the neighborhood of the explosion (3,000-meter radius). The control group comprises all transactions from 3,000 to 5,000 meters from the explosion. For (b) treated transactions are all transactions in the neighborhood of the explosion (3,000-meter radius). The control group comprises all transactions from 3,000 to 30,000 meters from the explosion.

affected by the Lubrizol explosion. The pollution has thus affected housing prices outside the city of Rouen but not near the explosion.

Figure 3.8: Housing Price Effects over Time



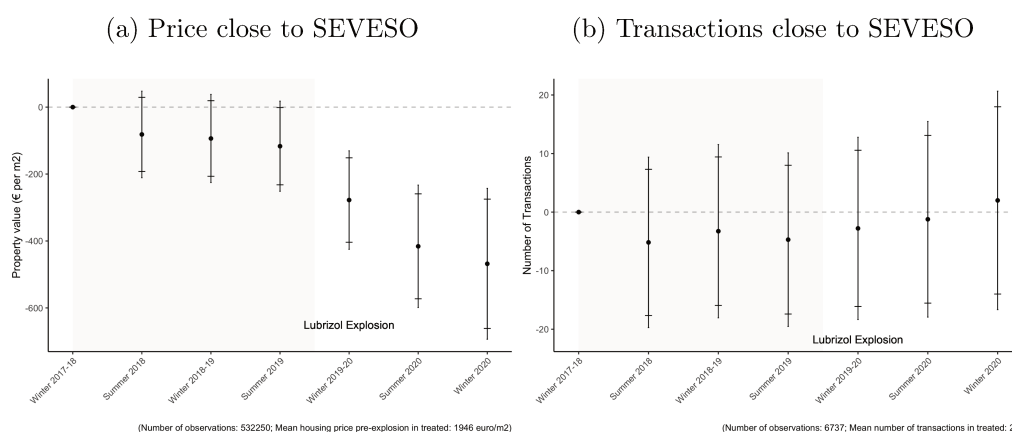
Notes on panels (a) and (b): Treated municipalities are defined as all municipalities facing restrictions due to possible pollution from the cloud. The control group is constituted by all the municipalities in the same departments, but outside of the restriction zone. Semesters are defined on a six-month basis, with winter spanning from October to March and summer from April to September.

### 3.4.4 Identifying the Seveso Effect

Figure 3.9 shows the evolution of the effects of the Lubrizol explosion on prices and volume of transactions near other Seveso plants. It illustrates the role of explosion risk information in areas where no objective incidents or pollution have been detected. In contrast to the effects observed near the Lubrizol plant, panels (a) and (b) show a pronounced decrease of 250 to 500 euros in the price per square meter of real estate transactions near other plants. This effect is substantial given the average price per square meter of 1,946 euros (-13% to -25%). These results align with

the interest generated by the explosion among French citizens, highlighted by Figure 3.4 showing Google trends on the "Seveso" keyword. The decrease in housing prices is also consistent with the results of Bauer, Braun, and Kvasnicka (2017), studying the informational channel of nuclear explosions between Japan and Germany (price decrease ranging from -5% to -10% in Germany). A better flow of information could explain the difference in magnitude, the explosion of the Lubrizol plant, and the zones studied being in the same country. Finally, the total volume of transactions remains stable over time and does not seem affected by the Lubrizol explosion.

Figure 3.9: Housing Price Effects over Time



Notes on panels (a) and (b): For (a) treated transactions are all transactions in the neighborhood of a high-risk infrastructure (1,000-meter radius). The control group comprises all transactions from 1,000 to 10,000 meters from a high-risk infrastructure. For (b) treated transactions are all transactions in the neighborhood of a high-risk infrastructure (2,000-meter radius). The control group comprises all transactions from 2,000 to 5,000 meters from a high-risk infrastructure.

### 3.4.5 Explaining the puzzle: The sellers' reactions

After the fire, sellers in Rouen expected large losses if they were to sell their property in the short run. This is confirmed by multiple interviews with real estate agents and housing unit sellers themselves.<sup>9</sup> Following findings from the 1990s study of the sellers' behavior when facing possible nominal loss in Genesove and Mayer (2001), we can expect sellers in the vicinity of the fire to react in at least two ways: first, they may set significantly higher prices; second, they may exhibit a much lower selling hazard.

**Listed prices.** Using neighborhood-level data on listings provided by the leading online advertiser in France, Figure 3.10 reports the median price and volume of listings between 2017 and 2020 in the neighborhood of the Lubrizol plant. Neighborhoods are defined using the French National Institute of Statistics and Economic Studies (INSEE) as homogeneous areas with a target population of 2,000 residents per area. Due to data availability, the total area studied in Figure

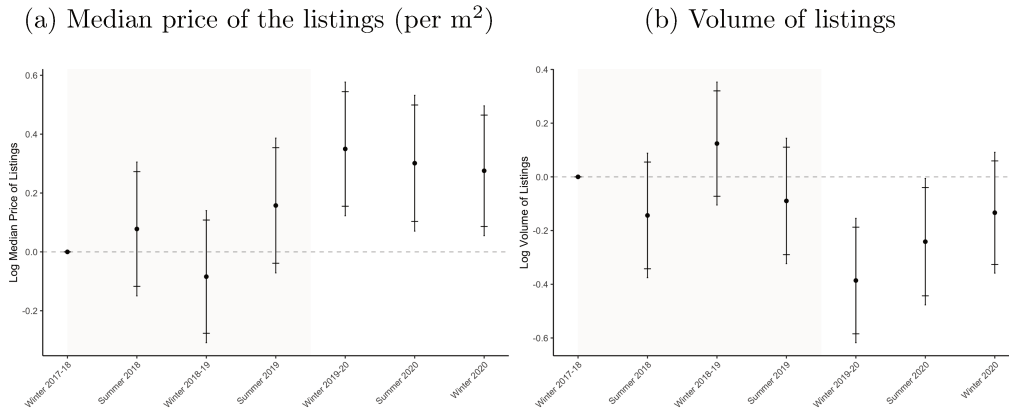
<sup>9</sup>Five days after the fire, prominent media outlets reported estimates from real estate specialists that the shock on the housing market would be "brutal", and would be driving the market for at least 3 to 6 months. See, among others, the coverage by [bfmtv.com](https://www.bfmtv.com) and [boursorama.com](https://www.boursorama.com).



3.10 includes all neighborhoods in Rouen and the neighboring municipalities<sup>10</sup> – but does not extend beyond. For this reason, Figure 3.10 reports an event study estimation where we report the estimated parameters of semester fixed effects.

Panel a of Figure 3.10 shows the seller’s clear and significant increase in the median listed price following the factory fire. The median listed square meter price after the fire is 35% larger on average than before the fire. The effect slowly attenuates over time but remains at about 25%

Figure 3.10: Housing Price Effects over Time



Notes: Figure reports the median price and volume of listings between 2017 and 2020 in the neighborhood of the Lubrizol plant. The neighborhood is defined based on data availability and includes all IRIS neighborhoods in Rouen and the neighboring municipalities. This corresponds to an approximately 5km radius around the fire. The data on the listings come from seloger (<https://www.seloger.com/>), the leading advertiser on the Internet in France.

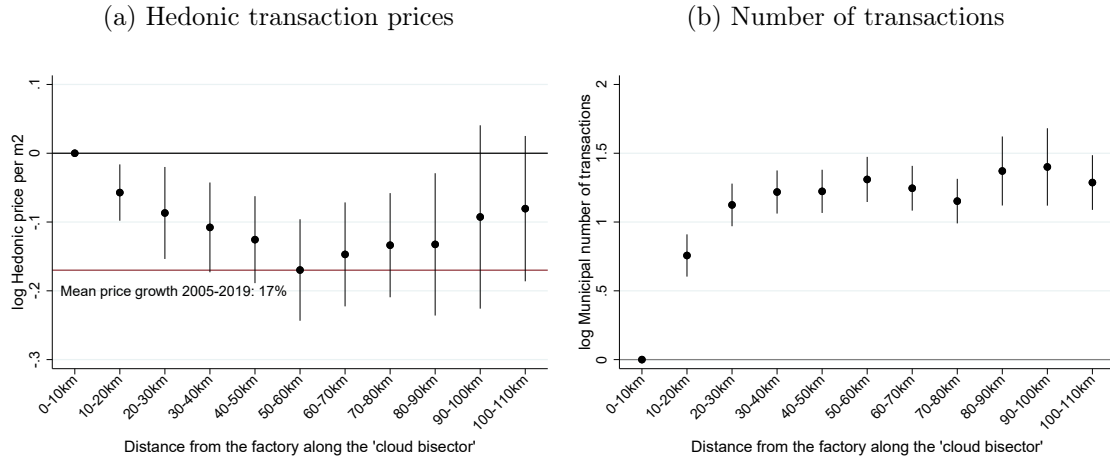
**Sellers’ selling hazard.** As opposed to the median price of the listings, the volume of listings – which does not imply a transaction– by neighborhood declined significantly after the fire by about 40%. This suggests “wait-and-see” behavior by the sellers, which may have kept prices at a higher level by restricting the supply.

The study of the evolution of the effect on transaction prices and the number of transactions as one moves away from the fire provides further support for sellers’ nominal loss aversion as a critical mechanism. Figure 3.11 estimates the effect of the fire on transaction prices and the number of transactions in Panels (a) and (b), respectively, for 10km bins from the fire and up to 100km from it along the pollution cloud bisector. The bin next to the fire [0km-10km] constitutes the omitted category. With a simple supply and demand model in mind, one would expect prices would decline more as one approaches the fire. This is indeed what is observed when moving from 110km to 50km from the fire. However, as one gets even closer to the fire (i.e., less than 50km), we observe an ever smaller decline in housing prices – which can be explained by the results in Figure 3.10. What is striking is that the tipping point at 50km corresponds to a decline in housing prices of about 17% – which is precisely the French national average growth in housing prices in the last

<sup>10</sup>These neighborhoods correspond to the IRIS defined by INSEE, which are areas of around 2,000 inhabitants on average. The sample includes all IRIS whose centroid is less than 80 kilometers from Rouen’s centroid.

14 years (i.e., the average holding time for a housing unit in France). Hence, the tipping point corresponds to the point below which sellers would face a nominal loss.

Figure 3.11: Price and Transactions



Notes: Figure reports the effect of the fire on transaction prices and the number of transactions in Panels (a) and (b), respectively, for 10km bins from the fire and up to 100km from it along the pollution cloud bisector. The omitted category is the bin next to the fire [0-10km]. An event study difference-in-difference strategy is adopted. 95% confidence intervals are presented.

### 3.5 Conclusion

The installation and operation of risky industrial sites are seized upon by all stakeholders in the civil debate, opposing several interests: attractiveness through employment, environmental protection, political interests, strategic considerations for industrial sovereignty, etc. Opponents of these projects point to the risk and often document it through previous accidents, such as the AZF accident in Toulouse, France, in 2003 or Fukushima in Japan in 2011. More recently, the Lubrizol incident was followed by significant mobilizations in Dreux, France, against a storage site classified as Seveso (*see Appendix A, Figure 3.14*). Despite European-wide regulations, incidents occur and modify both the environmental ecosystem and the local economic fabric.

In this paper, we analyze the effect of the explosion of the Lubrizol plant in Rouen through three different aspects. First, we show that real estate prices are not affected near the explosion, while the volume of transactions drops drastically. This counterintuitive result mitigates the teaching of the theory of real estate capitalization. Second, we analyze the effect of pollution due to the thick cloud of smoke resulting from the explosion, which extended from Rouen to the Netherlands. We then show that the price of real estate transactions decreases by about 100 euros per square meter with no significant effect on the volume of transactions. Finally, we identify the informational channel of the explosion near other Seveso sites in the rest of the territory. Prices then fall by 250 to 500 euros per inhabitant (-13% to -25%), which is very significant. These results are rationalized by the theory of nominal loss aversion, explaining that sellers, projecting a significant loss in the

value of their property, position themselves in "wait-and-see" behavior. We then confirm this phenomenon by analyzing ad data from the seloger website, one of the French leaders in online advertising.

In conclusion, our analysis sheds light on the complex interplay between economic and environmental concerns after an industrial accident. The Lubrizol explosion is a reminder that such incidents have immediate and visible impacts and long-term and far-reaching effects. Our findings challenge the predictions of the theory of real estate capitalization, highlighting the need for more nuanced and context-specific approaches to understanding the relationship between real estate markets and environmental risks. Furthermore, our study contributes to the growing literature on the economic costs of environmental pollution. By quantifying the effect of the Lubrizol explosion on real estate prices and transactions, we provide policymakers and regulators with valuable insights into the potential economic consequences of industrial accidents. Our results underscore the importance of regulations and risk management to protect the environment and the housing economy.

Looking ahead, our study opens up several avenues for future research. One crucial area of inquiry is the role of public perception and media coverage in shaping the economic and social consequences of industrial accidents. Another promising direction is to examine the effectiveness of different risk management and compensation mechanisms in mitigating the economic impact of such incidents. By addressing these questions, we can deepen our understanding of the multifaceted challenges of risky industrial sites and help develop more effective policies and practices for ensuring a sustainable future.

## 3.6 Appendices

### 3.6.1 Media and political coverage of the explosion

Figure 3.12: TV news coverage of the Lubrizol fire (two most watched TV news report in France)

(a) Screenshot of the Coverage by TF1



(b) Screenshot of the coverage by France 2



Figure 3.13: Political Coverage of the Consequences of the Accident



Figure 3.14: Protest in 2021 against the Settlement of a Seveso Plant in Dreux, France



Notes: Photography by *Agence DREUX*

### 3.6.2 Supporting material

#### Figures

Figure 3.15: Localisation of Seveso Installations in France

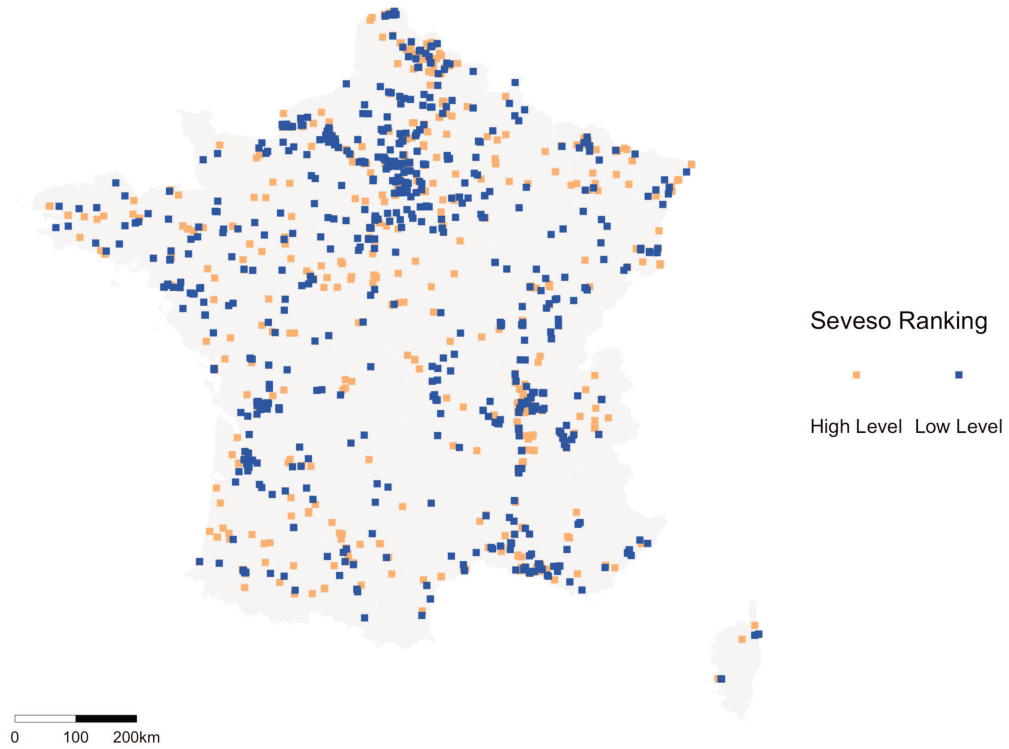
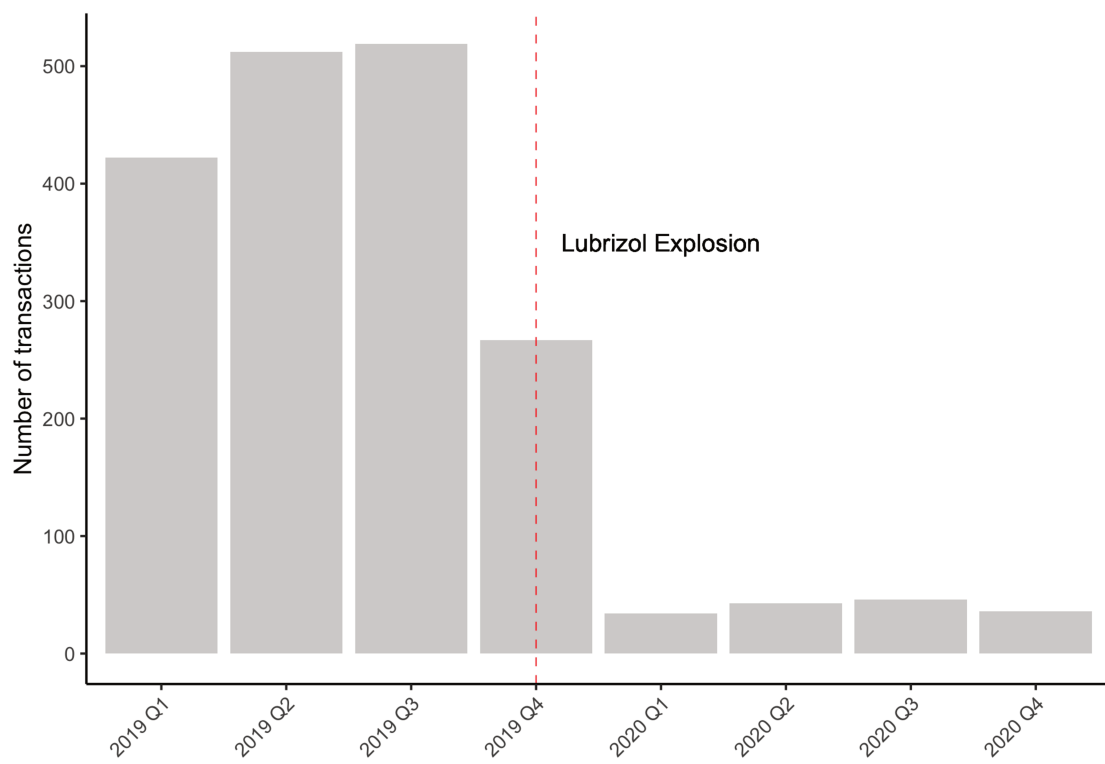
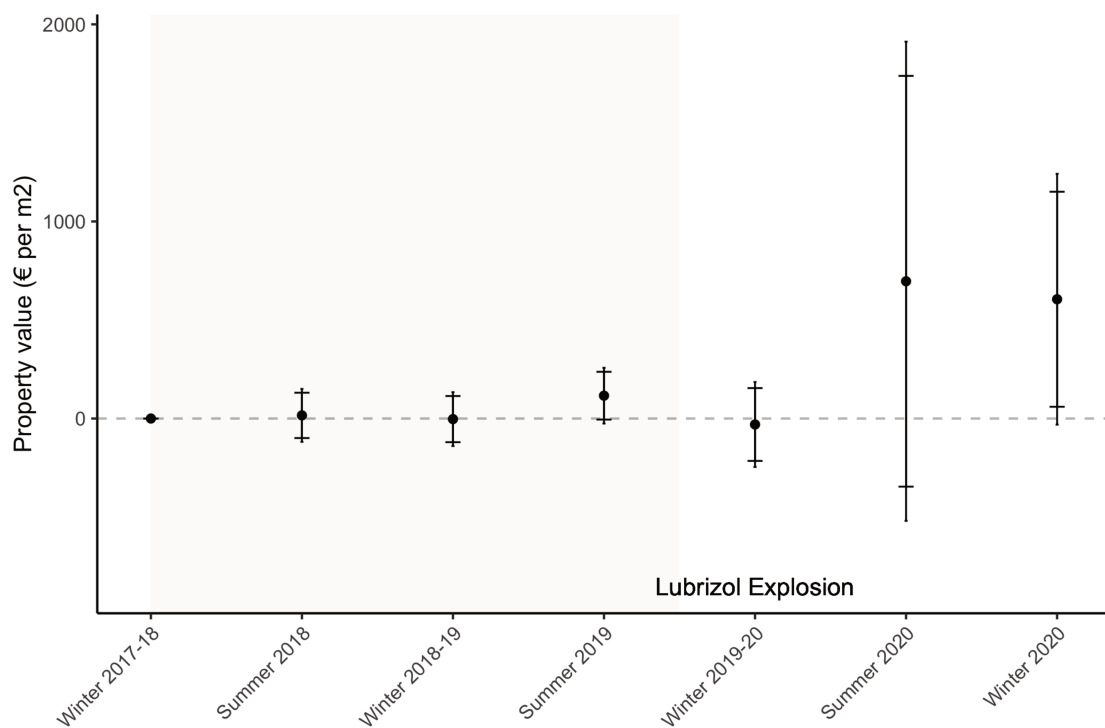


Figure 3.16: Evolution of the Number of Transactions in the Neighborhood of the Explosion



*Note: Number of transactions in the neighborhood of the explosion (3,500-meter radius).*

Figure 3.17: Housing Prices in Rouen: Observed Transaction Price Approach

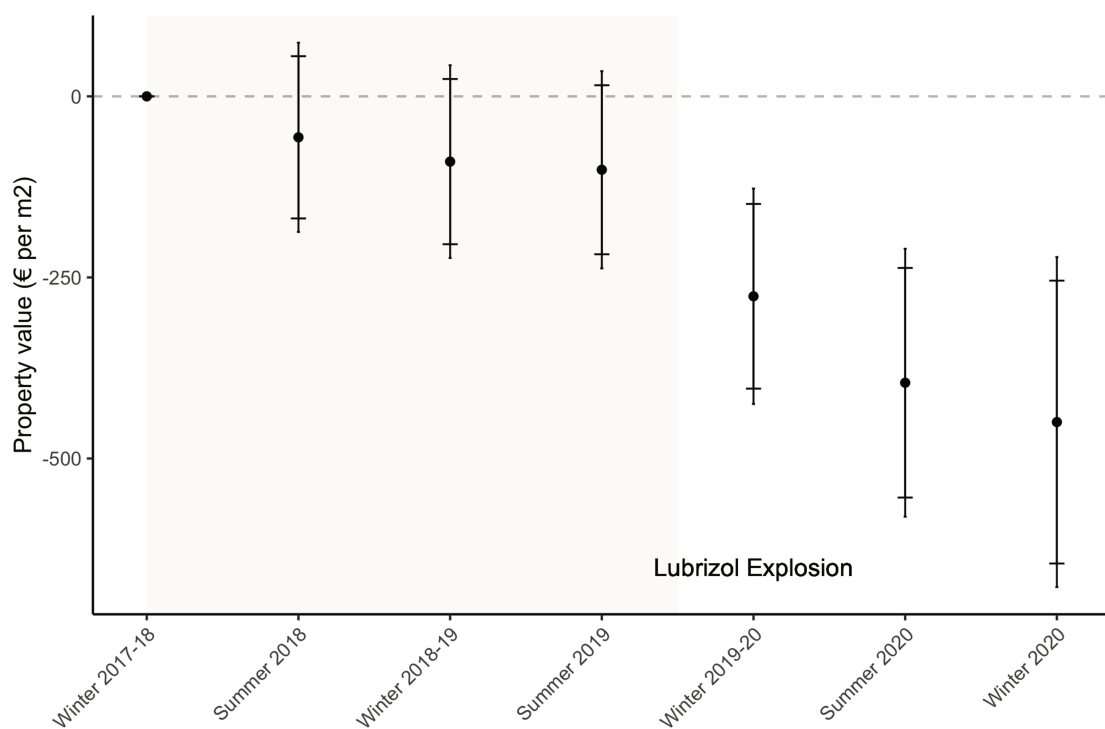


(Number of observations: 6030; Mean housing price pre-explosion in treated: 1940 euro/m2)

*Note: An event study difference-in-difference strategy is adopted. Observed transaction prices constitute the outcome variable. Treated municipalities are defined as all municipalities facing restrictions due to possible pollution from the cloud. The control group is composed of all municipalities in the same departments but outside of the restriction zone. Semesters are defined on a six-month basis, with winter spanning from October to March, and summer from April to September. 90% and 95% confidence intervals are presented.*



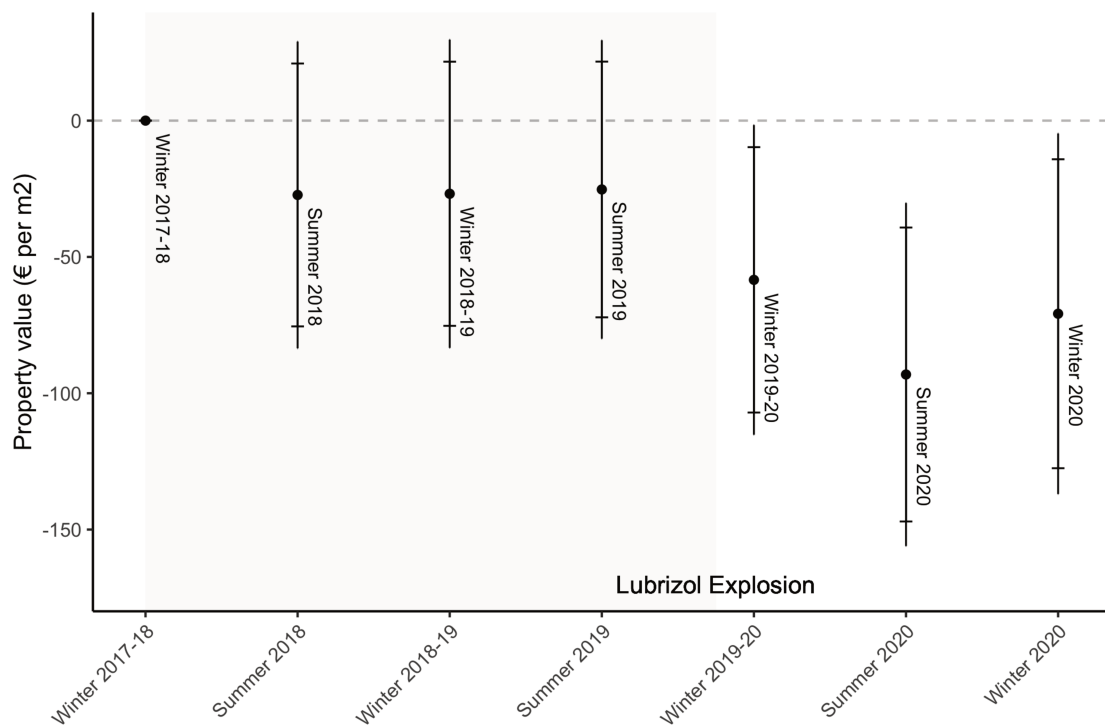
Figure 3.18: Housing Prices in Municipalities near a SEVESO Plant: Observed Transaction Price Approach



(Number of observations: 532250; Mean housing price pre-explosion in treated: 1946 euro/m<sup>2</sup>)

*Note: An event study difference-in-difference strategy is adopted. Observed transaction prices constitute the outcome variable. Treated municipalities are defined as all municipalities facing restrictions due to possible pollution from the cloud. The control group is composed of all municipalities in the same departments but outside of the restriction zone. Semesters are defined on a six-month basis, with winter spanning from October to March, and summer from April to September. 90% and 95% confidence intervals are presented.*

Figure 3.19: Housing Prices in Municipalities under the Polluted Cloud: Observed Transaction Price Approach



(Number of observations: 219245; Mean housing price pre-explosion in treated: 963 euro/m2)

*Note: An event study difference-in-difference strategy is adopted. Observed transaction prices constitute the outcome variable. Treated municipalities are defined as all municipalities facing restrictions due to possible pollution from the cloud. The control group is composed of all municipalities in the same departments but outside of the restriction zone. Semesters are defined on a six-month basis, with winter spanning from October to March, and summer from April to September. 90% and 95% confidence intervals are presented.*

## Tables

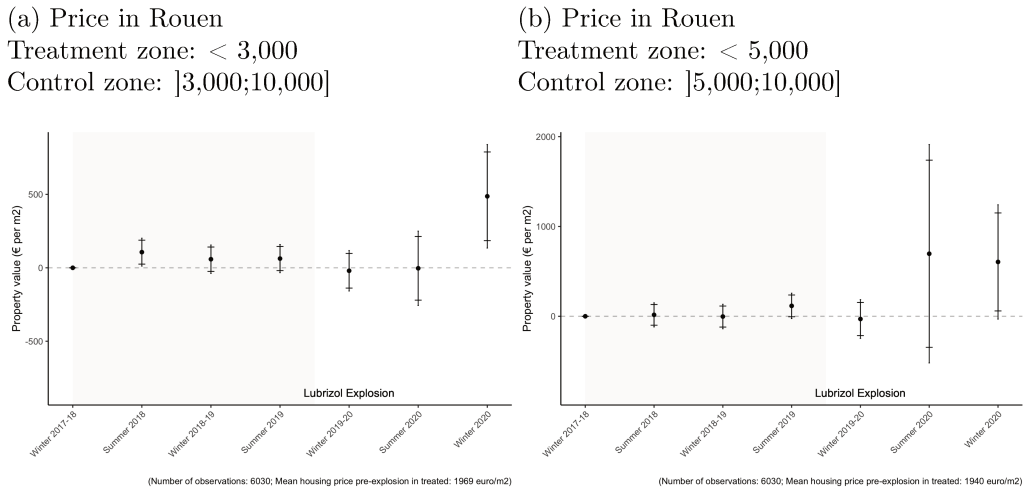
Table 3.2: Results in Rouen

Approach	(1)	(2)	(3)	(4)
	Housing price			Number of transactions
	Price per m2	log Price per m2	Hedonic	Number of transactions
DiD	317.939 (390.202)	0.189 (0.195)	0.178 (0.183)	-48.247*** (10.133)
Mean before the explosion	1398.65	7.22	7.26	47.05
Observations	1,062	1,062	1,062	310
R <sup>2</sup>	0.631	0.625	0.008	0.544

*Note:* A difference-in-difference strategy is adopted. A treatment zone of 1,000m around the Lubrizol explosion is used to estimate the effects on prices. A treatment of 3,000m around the Lubrizol explosion is used to estimate the effects on the number of transactions.

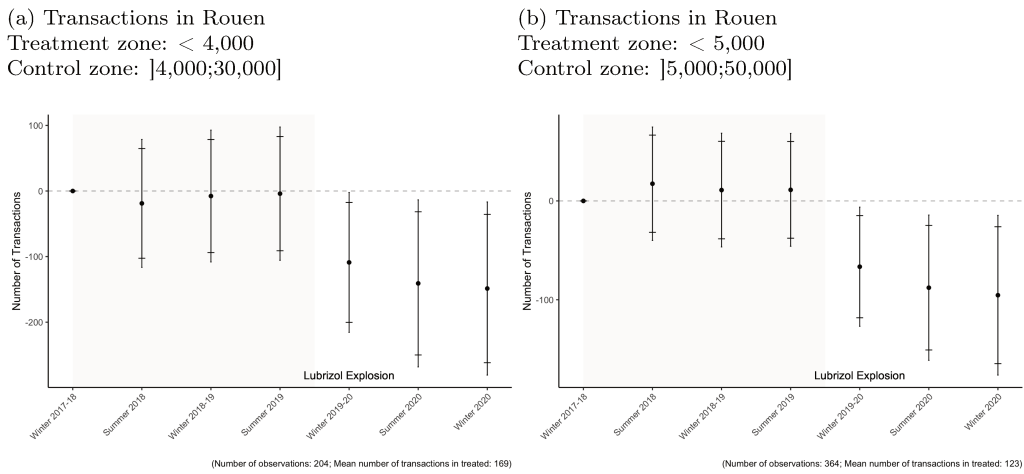
### 3.6.3 Robustness checks

Figure 3.20: Housing Price Effects over Time - Rouen



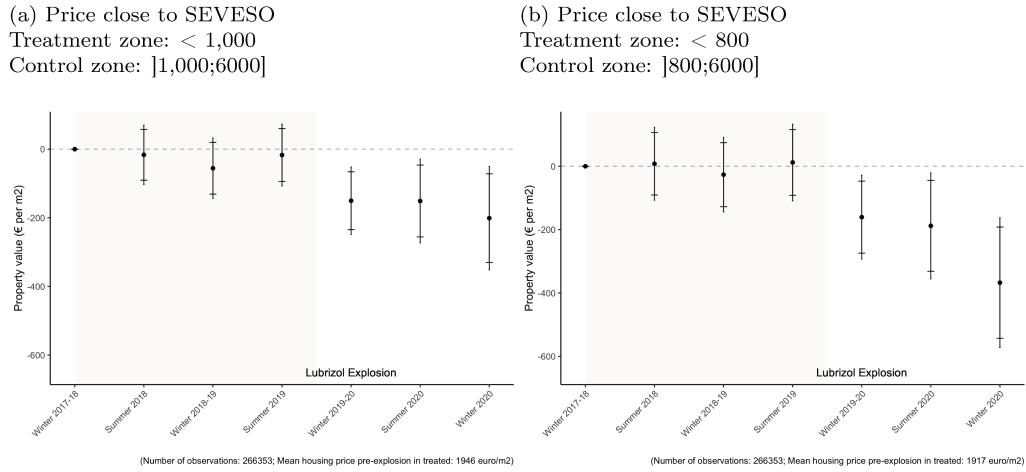
Notes on panels (a) and (b): An event study difference-in-difference strategy is adopted. 95% confidence intervals are presented. Treated transactions are all transactions in the neighborhood of the explosion (2,500-meter radius). The control group comprises all transactions from 3,500 meters to 10,000 meters from the explosion.

Figure 3.21: Number of Transactions over Time - Rouen



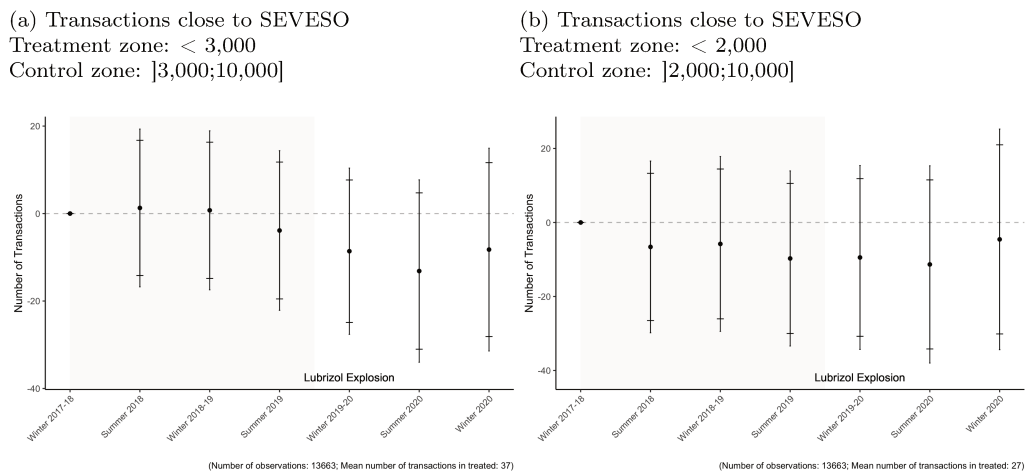
Notes on panels (a) and (b): An event study difference-in-difference strategy is adopted. 95% confidence intervals are presented. Treated transactions are all transactions in the neighborhood of the explosion (2,500-meter radius). The control group comprises all transactions from 3,500 meters to 10,000 meters from the explosion.

Figure 3.22: Housing Price Effects over Time - close to SEVESO Plants



Notes on panels (a) and (b): An event study difference-in-difference strategy is adopted. 95% confidence intervals are presented. Treated transactions are all transactions in the neighborhood of the explosion (2,500-meter radius). The control group comprises all transactions from 3,500 meters to 10,000 meters from the explosion.

Figure 3.23: Number of Transactions over Time - close to SEVESO Plants



Notes on panels (a) and (b): An event study difference-in-difference strategy is adopted. 95% confidence intervals are presented. Treated transactions are all transactions in the neighborhood of the explosion (2,500-meter radius). The control group comprises all transactions from 3,500 meters to 10,000 meters from the explosion.

## Chapter 4

# From Sharing to Capitalizing: Assessing the Rise of Airbnb in Housing Prices

*With Marie Breuillé and Julie Le Gallo*

This paper examines the impact of short-term rental (STR) listing density on real estate transaction prices in Metropolitan France. Using data from Airbnb listings and real estate transaction prices over the period 2018-2020, we cover the entire country, providing robust external validity, and focus on the spatial variations within different zones, including central cities, secondary cities, suburbs, and countryside areas. Using an instrumental variable approach, we show that a 1% increase in STR listing density leads to an 11% increase in real estate transaction prices. We also find that the effects vary across different locations, with central municipalities experiencing higher price increases than other areas. Surprisingly, rural municipalities, particularly those without significant tourist infrastructure, exhibit substantial price increases, suggesting that Airbnb has made these previously less-visited areas more accessible to travelers. This study contributes to the literature in three ways: (i) it provides a comprehensive analysis of the impact of STRs on the real estate market in the most touristic country in the world, enhancing external validity; (ii) it develops a novel instrument capturing the heterogeneity of territories, which is crucial in analyzing tourism dynamics; and (iii) it explores spatial heterogeneity at the national level, considering the unique characteristics of France's diverse municipalities.

**Keywords:** Short-term rentals, Airbnb, Real estate market, Spatial heterogeneity, Instrumental variables

**JEL:** R21, R31, R38, C26

## 4.1 Introduction

Since the 2010s, there has been a surge in the popularity of short-term rentals (hereafter STR) as an alternative accommodation option for travelers. The emergence of platforms like Airbnb and HomeAway has made it easier for property owners to rent their homes or even spare rooms to travelers for short periods. According to Adamiak (2019), Airbnb listings increased worldwide, reaching over 7 million homes, apartments, and shared rooms in 2019. This growth can be attributed to many factors, including the rise of online platforms, the demand for more touristic housing adapted to many kinds of budgets (Guttentag (2015); Farronato and Fradkin (2019)), and the ability for homeowners to earn extra income.

Online short-term rental platforms have profoundly changed the tourist accommodation market, and beyond, the real-estate market. On the one hand, short-term rentals allow property owners to obtain additional income. This additional income could then capitalize on the value of the real estate, making housing less affordable. Moreover, the multiplication and concentration of short-term rentals could increase the tension in the private rental market by diverting usage from the traditional market to the tourist market. On the other hand, this concentration could radically change the neighborhood environment, generating numerous negative amenities (noise, insecurity). This would lead to a decrease in property values and could create an overall decrease in the attractiveness of specific neighborhoods.

Many studies evaluate the effect of short-term rentals on the real estate market. The effects of STR all point in one direction: an increase in real estate or rental market prices, albeit of different magnitudes. Using an instrumental variable method developed by Barron, Kung, and Proserpio (2021), Franco and Santos (2021) show that a one percentage point increase in the volume of Airbnb listings leads to a 3.7% increase in house prices in Lisbon and Porto. On their side, Garcia-López et al. (2020) show using a battery of methods (a panel fixed effect model, an instrumental variable model, and an event study) that STR increases the price of real estate transactions by 4.6% on average in Barcelona. Furthermore, they highlight a substantial heterogeneity within the city where Airbnb's presence is the densest. Property prices in these neighborhoods have increased by 17% on average. In Taiwan, Chang (2020) finds that a one standard deviation increase in the number of Airbnb listings increases rental prices by about 0.38%. Finally, Barron, Kung, and Proserpio 2021 use Google Trends based instrumental variables and demonstrate that a 1 percent increase in the number of Airbnb listings raises real estate transaction prices by about 0.026% on average across the United States. Using an hedonic specification, Ayouba et al. (2020) show that increasing the density of listings in Lyon, Montpellier, and Paris increases the pressure on the rental market.

The empirical literature has also highlighted the strong heterogeneity in the location of STRs within states. In particular, urban centers are more affected by the phenomenon Adamiak 2019. However,

there is also considerable heterogeneity within municipalities on the periphery of urban centers. For example, suburban municipalities may be preferred because they are accessible from the center and more affordable. On the other hand, areas further away from the main urban centers may also be very popular with tourists, as in the case of coastal communities. It is, therefore, essential to explore the spatial heterogeneity of the impact of STR.

These results, which attribute a portion of the increase in rental prices and real estate transactions to STR, support the widespread campaign to regulate STR activity in many cities in Europe, the United States, and worldwide. These regulations can take many forms, including restrictions on the number of days a property can be made available to tourists. These regulations are then used as exogenous variations helping to assess STR effects. Robertson, Dejean, and Suire (2023) show that capping the number of guest days in accommodations decreases tourist traffic in Bordeaux, France, by 44%. Koster, Jos van Ommeren, and Nicolas Volkhausen (2021) studies an ordinance restricting the number of listings in Los Angeles. Using a regression discontinuity design, they show that these rental constraints lowered the number of listings by about 50 percent and reduced the price of rents and real estate transactions by two percent. In Berlin, Duso et al. (2022) take advantage of regulations and show that the increase of STRs listed by professionals reduces the housing supply in the traditional rental market by 0.4 to 0.9 percent while producing an increase in price per square meter of up to 5.6%. Apart from the decisive contribution of Barron, Kung, and Proserpio (2021) analyzing the entire U.S. territory, research making use of a sound identification strategy has focused on cities or regions, which strongly limits the external validity of the studies. In Europe, there are no large-scale studies.

In this context, the objective of this paper is to carry out an analysis across all Metropolitan France of the effect of the density of STR density on real estate transaction prices over two years before the Covid crisis (2018-2020). Specifically, we consider the case of Airbnb and analyze its impact at the scale of the municipality, which is the smallest level of administrative division in France. The contribution to the literature is threefold: (i) we analyze the most touristic country in the world<sup>1</sup> in its entirety, bringing a solid external validity, (ii) we build a novel instrument to capture the heterogeneity of territories, a crucial issue in analyzing tourism dynamics, (iii) we are the first to investigate spatial heterogeneity at the scale of an entire country. This is particularly relevant for France, with its 35,000 municipalities representing 40% of the total number of municipalities in Europe and characterized by a large heterogeneity in size, geographical, socio-economic, and touristic characteristics. We then explore heterogeneity according to zoning in the area of attraction of cities, divided into five categories<sup>2</sup>: central city, other main poles of the urban area, secondary city, suburbs, and countryside.

---

<sup>1</sup>According to the World Tourism Organization (UNWTO) (2020), France is the most touristic country in the world in terms of entries to the territory with 89.4 million entries in 2019, ahead of Spain (82.8) and the United States (72.7).

<sup>2</sup>These categories are defined by the French National Institute for Statistics and Economic Studies (INSEE)



We use multiple databases to assess the effect of STR listing density on the real estate market. The number of listings comes from the scrapped database established by AirDNA, locating listings with an uncertainty radius of about 500 meters<sup>3</sup>. In addition, we use the exhaustive database produced by CEREMA<sup>4</sup> to calculate the price per square meter of real estate transactions in France.

Evaluating the causal impact of Airbnb's activity on real estate prices raises numerous identification issues. In particular, reverse causality is to be feared: homeowners in areas with the highest price per square meter may have a greater incentive to list their property on Airbnb because of the high revenue the activity generates. A causal inference method is thus needed to isolate the effect of the STR activity. Many methods exist, but not all are appropriate for the available data. Since the Airbnb data is scrambled at 500 meters, identification strategies based on the exact geolocation of listings, such as regression discontinuity design (RDD), for example, may yield biased estimates. This is why we opt for an aggregation at the "municipality" level. Furthermore, because there is a clear-cut treatment, a difference-in-differences strategy is not applicable in our case.

The most relevant method then appears to be the instrumental variables. Thus, we build our instruments using the number of views of the online ads on leboncoin, a prominent French player in the trade between individuals<sup>5</sup>. This number of ad views is aggregated at the municipality level, allowing us to predict the number of listings while being exogenous since this is an indicator of demand. We then predict the supply of STR using two demand-side instruments, one of stock containing the number of views of the three most visited ads and the other of variation, with the two-month variation in the number of views of the three most visited ads.

In this paper, we show that a 1% increase in listing density in a municipality causally increases the price of real estate transactions by about 11% in our preferred specification. Turning to heterogeneity, we find that prices increase more in percentage terms in central municipalities (about 9%). Surprisingly enough, the increase is greater in the rural municipalities than in the remaining categories (i.e., except the center), especially those not touristic (8.6 percent). This can be explained by the fact that prices were meager and that Airbnb has made these municipalities not equipped with tourist facilities accessible for travelers.

This paper is organized as follows. First, we present the data and descriptive statistics before detailing the empirical strategy. We then present the main results, followed by an in-depth heterogeneity analysis. Finally, we conclude and highlight future policy challenges.

---

<sup>3</sup>This database is used by almost all papers evaluating the causal impact of Airbnb on various outcomes

<sup>4</sup>French public institution assisting the national government and local authorities in developing, deploying, and evaluating urban planning and transport policies

<sup>5</sup>In France, according to a study by the Observatoire de l'habitat du Crédit Foncier, about 90% of individuals seeking a rental property used the internet in 2019. In October 2019, the 'Real Estate' section on the generalist portal leboncoin exceeded 12 million unique visitors.

## 4.2 Data and Descriptive Statistics

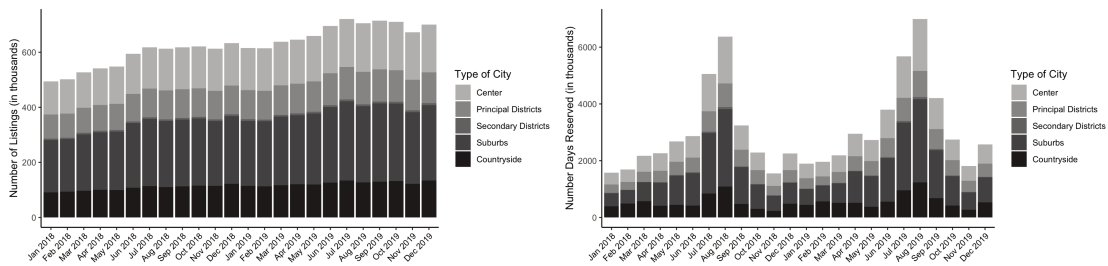
In 2018, there were 35,357 municipalities in France. These are the first level of democracy, followed by inter-municipalities, *départements* and regions. Our sample includes 25,121 municipalities for the period 2018-2020. We start the analysis in 2018, as the instrument data is not available before and we do not consider the period after January 2020 due to the Covid-19 pandemic.

*STR data.* STR listing data comes from AirDNA, a company that scrapes the Airbnb and Home-Away websites on a large scale. The individual data is localized with an uncertainty of 500 meters. It contains information on the number of nights booked, the occupancy rate, and the number of days online in the month. The data covers all of France between January 2018 and December 2019, the last entire period before the Covid-19 outbreak that took a toll on the tourism sector. Figure 4.1 shows the evolution of the number of listings and the number of days reserved by municipality type. The number of listings grew from 500,000 in January 2018 to more than 600,000 in December 2019, following a linear trend marked by a slight seasonality. The number of days reserved on its side follows a more pronounced seasonality, reaching up to 6 million days reserved in August 2018 and 2019. Both variables are driven by suburbs and central cities, while secondary districts cities are in minority.

Figure 4.1: Number of Listings and Number of Days Reserved

(a) Number of Listings

(b) Number of Days Reserved

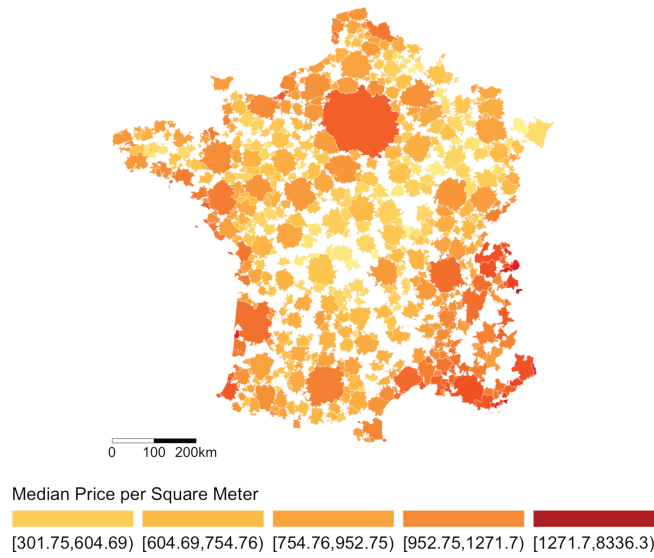


*Real estate data.* The data on the real estate market comes from CEREMA. The data contains all real estate transactions on the French territory for 2018 and 2019, geolocalized at the address level, with numerous structural information, such as the number of rooms or surface of the properties. The price per square meter is calculated by retaining apartments and excluding properties considered exceptional by Cerema<sup>6</sup>. The final sample contains 603,812 transactions in 2018 and 601,731 in 2019. Figure 4.2 shows the median price per square meter aggregated by functional areas. We observe high prices in Paris agglomeration, the Alps, and the thin border facing the Atlantic Ocean and the Mediterranean Sea.

*Ad views data.* The data on the number of ad views comes from the leboncoin. leboncoin is the

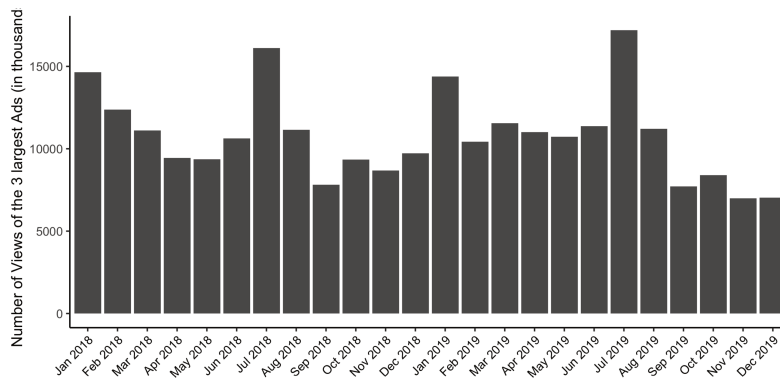
<sup>6</sup>Exceptional properties are dwellings with more than eight rooms and apartments with more than 200m<sup>2</sup> of built-up area

Figure 4.2: Median Price per Square Meter



French leader in the collaborative economy, with more than 45 million ads and 29 million monthly users<sup>7</sup>. The platform is also very well positioned in the holiday market, with more than 87 million visits to leboncoin ads in 2022<sup>8</sup>. We aggregate the data by municipalities and calculate the total number of views of the three most visited ads by municipalities. This is the variable we use as an instrument. Figure 4.3 shows the evolution of the total views of each municipality's three most viewed ads. We observe seasonality, with very apparent peaks in July 2018 and 2019. Figure 4.4 presents the distribution of the views across the territory. The areas with the most views are the region of Paris, the Mediterranean and Atlantic coasts, and large cities such as Lyon or Bordeaux.

Figure 4.3: Number of Views of the Top 3 Ads - Evolution

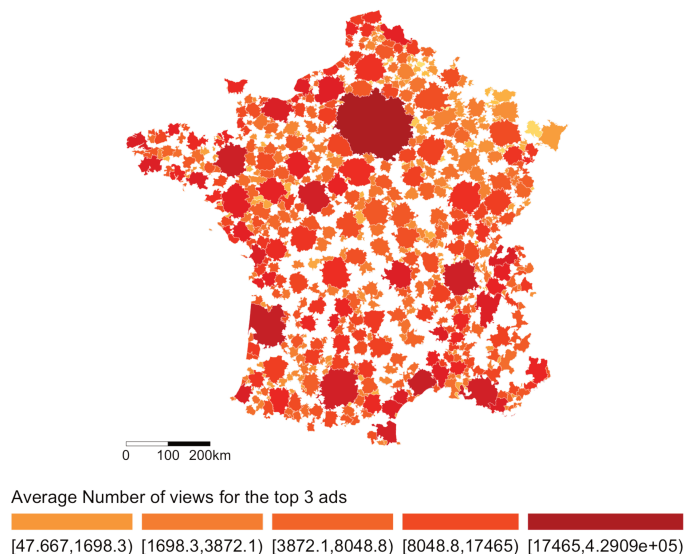


*Tripadvisor data.* To properly integrate the tourist amenities into the database, we collect the

<sup>7</sup><https://leboncoinpublicite.fr/actualites/que-recherchent-les-utilisateurs-leboncoin-tendances-debut-2021>, consulted in May 2023

<sup>8</sup><https://leboncoinpublicite.fr/actualites/comment-les-utilisateurs-leboncoin-vont-ils-voyager-cet-ete/>, consulted in May 2023

Figure 4.4: Number of Views of the Top 3 ads - Zoning



points of interest (POI) listed on the TripAdvisor platform. We complete these points of interest with the number of reviews collected to capture the degree of touristic attractiveness. These number of Tripadvisor POI and reviews are aggregated at the municipality level.

*Socio-economic characteristics.* Numerous socio-economic characteristics at the municipality level, from INSEE or calculated with the Open Street Map API, are used. These include housing variables such as the number of dwellings, the share of second homes, and the share of social housing. These variables are completed by economic variables such as the unemployment rate and accessibility variables, such as the time to the nearest train station. Table 4.1 details all the variables used in this paper and provides some summary statistics.

### 4.3 Identification Strategy

#### *Baseline equation*

To assess the impact of STR activity density on real estate prices, the following equation is estimated:

$$\log(P_{it}) = C + \alpha \times \log(\text{Density}_{it}) + \beta \times X_i + \gamma \times \text{Trans}_{it} + \text{MonthYear}_t + \text{Zone}_i + \epsilon_{it} \quad (4.1)$$

where  $P_{it}$  is the real estate price per square meter in municipality  $i$  in month  $t$ ,  $\text{Density}_{it}$  is the density of STR listings relatively to the number of dwellings in municipality  $i$  in month  $t$ ,  $X_i$  is the vector of socio-economic characteristics of municipality  $i$ ,  $\text{Trans}_{it}$  is the number of real estate

Table 4.1: Averaged Variables: Descriptive Statistics

	Mean	St. Dev.	Min	Max	N
<b>Airbnb Data</b>					
Number of Listings	55.8	235.4	1	6,663	269,003
Number of Days Booked	273.3	1,346.8	0	76,905	269,003
<b>Real Estate Data</b>					
Mean Price per Square Meter	1,181.4	806.2	0.01	28,927.2	269,003
Number of Real Estate Transactions	4.5	10.5	1	369	269,003
<b>leboncoin Data</b>					
Number of Views of the Top 3 Ads	960.7	1,585.4	1	276,415	269,003
<b>Touristic Amenities</b>					
Number of Tripadvisor POI	3.4	9.1	0	175	269,003
Number of Tripadvisor Reviews	177.4	1,180.5	0	45,377	269,003
Minutes to High Speed Train	48.2	35.9	1.0	356.0	269,003
Annual Temperature	11.4	1.5	0.4	16.2	269,003
<b>Socioeconomic Data (INSEE)</b>					
Unemployment Rate	11.0	4.4	0.0	55.6	269,003
Share with no Degree	27.2	7.5	0.0	69.2	269,003
Share of Industrial Employment	12.5	13.4	0.0	100.0	269,003
Share of Construction Employment	10.1	10.0	0.0	100.0	269,003
Share of Agricultural Employment	12.3	16.9	0.0	100.0	269,003
Number of Dwellings	2,009.3	4,663.6	16.4	104,247.0	269,003
Share of Secondary Housing	11.7	15.5	0.0	96.9	269,003
Share of Vacant Housing	8.4	4.2	0.0	43.3	269,003
Share of Social Housing	6.5	8.1	0.0	65.7	269,003
Share of Immigrants	5.0	4.7	0.0	48.7	269,003
Population Variation	0.3	1.3	-11.5	19.9	269,003
Population Density	345.7	1,197.1	0.9	27,310.0	269,003

transactions of the municipality  $i$  in month  $t$ ,  $MonthYear_t$  and  $Zone_i$  are the time and spatial (at the attraction zoning level) fixed effects,  $C$  is the constant and  $\epsilon_{it}$  is the idiosyncratic the error term. If we had all the variables determining the price per square meter in the municipalities, we could start to get closer to a causal relationship. However, there is a lot of information contained in the error term  $\epsilon_{it}$  that we do not capture. Moreover, even if one could gather all the information determining the price per square meter in the municipalities, reverse causality seriously threatens causal identification. Indeed, if the density of STR listings is likely to affect real estate prices (which we are trying to estimate), we can also fear that the price itself affects the density of STR listings: for example, the higher the price, the more agents are encouraged to list their house online because the gains can be substantial. To address these concerns, we use two instruments on the demand side to predict the density of listings (supply side).

#### *Instruments*

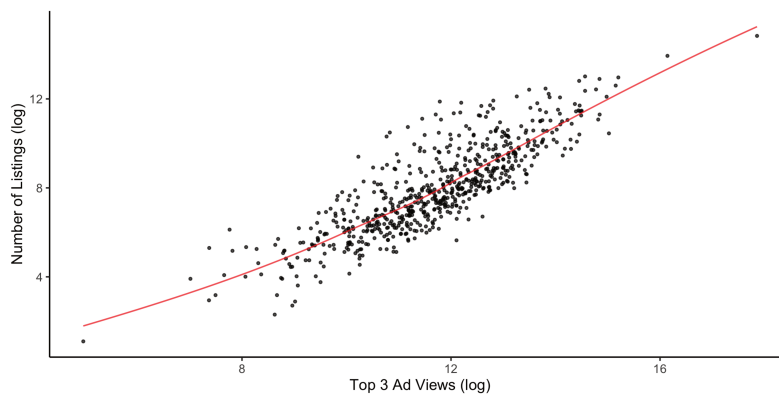
The two instruments we use reflect the demand of users of the platforms listing STRs. We build the instruments by counting the total number of views of the three most viewed ads per municipality. We keep only the three most viewed ads and do not take the whole set of ads to limit the size effect:

small towns with few available housing units would mechanically have fewer ads, and thus fewer views, than large cities with many ads. Moreover, we do not take a single ad to avoid anomalies, such as a price abnormally below the market, inducing an inflated traffic on the ad<sup>9</sup>.

The first instrument is a stock variable, simply counting the number of views of the three most viewed ads in the municipality. The objective here is to capture the raw demand over a given month for a given municipality, allowing us to predict the density of STR listings. Many ad visits capture the intensity of demand for a given city while impacting real estate prices only through the encounter with STR supply. Figure 4.5 shows the positive correlation between the number of listings and the first instrument. This relationship is strong, giving the instrument power and ruling out the risk of a weak instrument.

The second instrument captures a variation in demand. This variation, used over two months, allows us to capture the differences in demand due to seasonality. We use a two-month variation to capture the summer season, which is mainly in July and August (see Figure 4.1 (b)). Both instruments must now be tested to rule out the main causes of non-validity.

Figure 4.5: Number of Listings and Number of Views



Notes: The number of listings and top ad views are aggregated at the attraction zoning level.

Following Barron, Kung, and Proserpio (2021), we perform two validity tests of the instruments<sup>10</sup>.

*IV Has No Effect in Non-Airbnb Zip codes.* The first exercise checks the instrument's correlation with the dependent variable. The existence of a correlation between the square meter prices of real estate transactions and the instruments threatens their validity: the correlation with the square meter price must only pass through the number of Airbnb or HomeAway listings. Figure 4.2 shows the regression of price per square meter on the instruments, with control variables and spatial and temporal fixed effects (our preferred specification, Table 4.4 column (5)). We observe a complete lack of correlation between the instruments and the price per square meter of real estate

<sup>9</sup>The analyses for different instruments, built with the top 1 and 5 Ads, are available in appendices.

<sup>10</sup>Barron, Kung, and Proserpio (2021) perform a third validity test analyzing the evolution of trends before and after Airbnb entered the market. However, it cannot be conducted due to the unavailability of data to construct the instruments before 2018.

transactions in municipalities with no Airbnb or HomeAway listings.

Table 4.2: Instruments Have no Effects on Prices in Cities without Airbnb

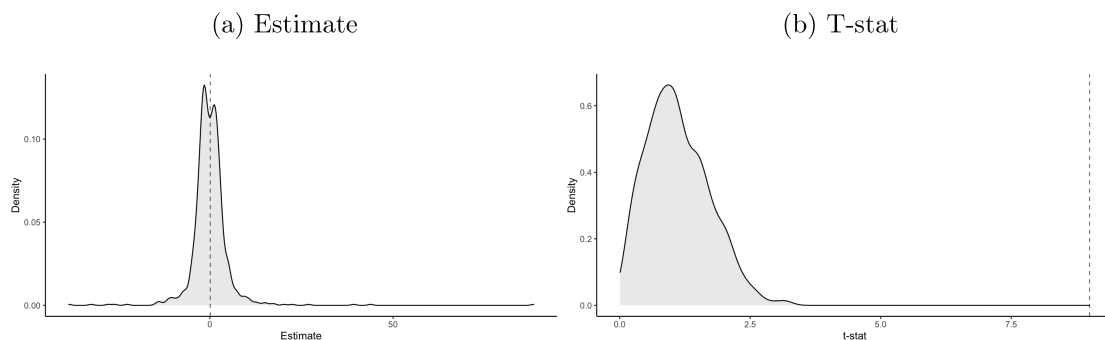
	Price per square meter (log)
Top 3 Ad Views	0.008 (0.006)
Top 3 Ad Views Variation	0.001 (0.001)
Observations	6,891
Adjusted R <sup>2</sup>	0.366

*Note:* The dependent variable is the natural logarithm of the price per square meter in a municipality. Variables undergoing logarithmic transformation have been increased by 1 to avoid logarithms of 0. Standard deviations are reported in parenthesis.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

*Placebo Test* The second exercise consists of a placebo exercise to test the exogeneity of the instrument. To do so, we keep all municipality’s characteristics constant but permute the density of listings across the dataset. Knowing that we are estimating here the effect of the intensity of Airbnb activity on real estate prices, if the instrument captures only this information and is not endogenous, we should have a weak instrument in the first stage, which would lead to insignificant coefficients in the second stage. Thus, we run 1,000 simulations to test this hypothesis. Figure 4.6 shows the density of the estimates and t-stats of the simulations. The estimates are not significant at the 5% level in more than 90% of the cases and not significant at the 1% level in almost 99% of the cases. This allows us to further eliminate risks of endogeneity of the instruments.

Figure 4.6: Placebo Test: Randomized Endogeneous Variable



Notes on panels (a) and (b): The graphics show the density of the estimates and t-stats for 1,000 simulations of the randomized endogenous variable (*i.e.* the density of listings). The dashed vertical lines show the real values of our preferred specification (see Table 4.4 column (5)).

## 4.4 Main Results

Table 4.3 shows the first stage of the estimation of the effect of Airbnb density on prices per square meter of real estate transactions. Five specifications, including different variables, are used alongside the two instruments. The results in column (1) detail the coefficients of both instruments with only municipal characteristics. The specification is then cumulatively augmented with tourism amenities (column (2)), zoning area fixed effects (column (3)), monthly fixed effects (column (4)), and finally, crossed month/year fixed effects (column (5)). The coefficients for each specification and both instruments are significant at the 1% level, showing their strength.

Table 4.3: First Stage Regression

	Density of Listings (log)				
	(1)	(2)	(3)	(4)	(5)
Nb of Views (log)	0.238*** (0.002)	0.133*** (0.002)	0.076*** (0.001)	0.088*** (0.001)	0.089*** (0.001)
Growth of Nb of Views	-0.003*** (0.0002)	-0.001*** (0.0002)	-0.001*** (0.0001)	-0.001*** (0.0001)	-0.001*** (0.0001)
Municipal Characteristics	✓	✓	✓	✓	✓
Touristic Amenities	×	✓	✓	✓	✓
Spatial Fixed Effects	×	×	✓	✓	✓
Monthly Fixed Effects	×	×	×	✓	×
Month × Year Fixed Effects	×	×	×	×	✓
Observations	221,266	221,266	221,266	221,266	221,266
Adjusted R <sup>2</sup>	0.466	0.606	0.684	0.686	0.692

*Note:* The dependent variable is the density of listings in a municipality (number of listings divided by the housing stock). Variables undergoing logarithmic transformation have been increased by 1 to avoid logarithms of 0. Standard deviations are reported in parenthesis.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Regarding the relationship of the instruments to the listing density, the sign of the coefficients indicates that as the touristic demand (total number of the three most viewed ads in the municipality) increases, the listing density increases. However, when the demand increases, the listing density decreases very slightly. This is due to the seasonality of demand, where the number of views gradually increases before reaching a peak and then decreases drastically after that peak (see Figure 4.3). Thus, when demand decreases drastically, the coefficient of variation is negative and is of a larger magnitude than when the variation is positive. Since the total number of listings is stable (because time fixed effects capture the quasi-linear growth), the skewness around the peak of the number of views induces a negative sign in the relationship between the variation in the number of views and the density of listings.

Table 4.4 shows the main results of the effect of STR density on property prices. Columns (1) to



(5) show price elasticity with respect to listing density, including various controls and fixed effects. Sargan’s over-identification test is also performed for each estimate. Depending on the specification used, a 1% increase in listing density implies an increase ranging from 5.5% to 10.9% in the price per square meter of real estate. The effects are all significant at the 1% level. However, there is considerable variability, notably a sharp drop in column (2) specification, when tourist amenities are added. Table 4.5 shows the specifications when integrating spatial spillovers with a variable counting the number of TripAdvisor reviews over the attraction zoning area. We then observe that taking spillovers into account strongly modifies the results in column (2), making them more consistent with the rest of the estimates without significantly modifying the other specifications. A 1% increase in listing density then implies an increase ranging from 8 to 10.9% in the price per square meter of real estate, with coefficients still significant at the 1% level.

Table 4.4: Main Results

	Price per square meter (log)				
	(1)	(2)	(3)	(4)	(5)
Density of Listings (log)	0.088*** (0.004)	0.055*** (0.008)	0.102*** (0.014)	0.108*** (0.012)	0.109*** (0.012)
Sargan Test	0.867	0.816	0.071	0.121	0.146
Municipal Characteristics	✓	✓	✓	✓	✓
Touristic Amenities	×	✓	✓	✓	✓
Spatial Fixed Effects	×	×	✓	✓	✓
Monthly Fixed Effects	×	×	×	✓	×
Month × Year Fixed Effects	×	×	×	×	✓
Observations	221,266	221,266	221,266	221,266	221,266
Adjusted R <sup>2</sup>	0.444	0.472	0.543	0.543	0.542

*Note:* The dependent variable is the density of listings in a municipality (number of listings divided by the housing stock). The p-value of the Sargan test of overidentifying assumption is given in line (2). Variables undergoing logarithmic transformation have been increased by 1 to avoid logarithms of 0. Standard deviations are reported in parenthesis.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Barron, Kung, and Proserpio (2021) find that a 1% increase in the number of listings increases real estate transaction prices by 0.026%, while Garcia-López et al. (2020) find that Airbnb activity increases real estate prices by 4.6% on average. The results presented above align with the few studies analyzing the effect of Airbnb on property prices.

While the average effect ranges from 8% to 11%, the considerable regional heterogeneity in both the volume and seasonality of listings calls for a more detailed analysis. In particular, the need for regulation varies widely across France. On the one hand, some municipalities are declaring

war on STRs, accused of making specific neighborhoods unlivable or of driving up property prices. This is the case in many cities like Paris, Bordeaux, and Saint-Malo. On the other hand, many municipalities need attractiveness and are trying to attract travelers. This dynamic is illustrated, for example, by the partnership initiated between the Airbnb platform and the Association of French Rural Municipalities to increase the visibility of rural towns on the website. Mayors promote STRs and receive 100 euros for each new listing on the platform.

These policy differences illustrate the substantial heterogeneity between different types of municipalities, whether urban or rural, tense or not in terms of access to housing, easily accessible or not *via* transport infrastructures. We will now examine whether these dimensions of heterogeneity play a role in Airbnb's impact.

Table 4.5: Main Results including Spatial Spillovers

	Price per square meter (log)				
	(1)	(2)	(3)	(4)	(5)
Density of Listings (log)	0.088*** (0.004)	0.080*** (0.008)	0.103*** (0.014)	0.108*** (0.012)	0.110*** (0.012)
Sargan Test	0.869	0.146	0.165	0.246	0.282
Municipal Characteristics	✓	✓	✓	✓	✓
Touristic Amenities	×	✓	✓	✓	✓
Spatial Fixed Effects	×	×	✓	✓	✓
Monthly Fixed Effects	×	×	×	✓	×
Month × Year Fixed Effects	×	×	×	×	✓
Observations	221,266	221,266	221,266	221,266	221,266
Adjusted R <sup>2</sup>	0.444	0.477	0.543	0.542	0.542

*Note:* The dependent variable is the density of listings in a municipality (number of listings divided by the housing stock). The p-value of the Sargan test of overidentifying assumption is given in line (2). Variables undergoing logarithmic transformation have been increased by 1 to avoid logarithms of 0. Standard deviations are reported in parenthesis.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

## 4.5 Heterogeneity

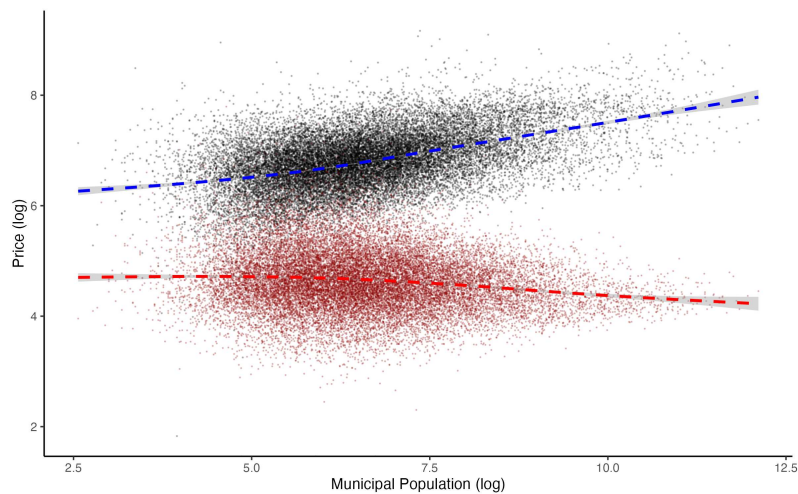
### 4.5.1 Type of Municipality Heterogeneity

We analyze several faces of heterogeneity, considering the territorial diversity of metropolitan France. The first analysis concerns the nature of towns with listings, with the expectation that the effects of listing density differ according to the size of towns, their accessibility, and whether or not they are tourist destinations. We, therefore, use the INSEE classification of urban areas

zoning to divide towns into five categories: (i) central municipalities ("center"), which concentrate a large part of economic life; (ii) other municipalities in the principal centre ("principal"); (iii) municipalities in a secondary centre ("secondary"), with a lesser influence and capacity to attract; (iv) municipalities in the commuting area ("suburbs"); and (v) municipalities outside of functional areas ("countryside"), which benefit from no attraction from other towns and are differentiated according to whether or not they are classified as tourist areas by INSEE.

Table 4.6 presents the estimation results according to this INSEE classification. In our preferred specification (column (5)), we observe that STR density has a positive effect on the price per square meter of real estate, whatever the type of municipality. The highest effect is in central municipalities, where a 1% increase in listing density implies a 9.2% increase in price per square meter. This sharp increase is due mainly to the concentration of STR supply in large cities, as shown by Adamiak 2018. The effect is also substantial in non-touristic countryside municipalities, with an increase of around 8.6%. This slightly surprising effect could be explained by the fact that these municipalities have very few tourist facilities and benefit from particularly low property market values. The possibility of putting one's accommodation on Airbnb is therefore capitalized directly into property values. A slight nominal increase, therefore, has a significant impact on the percentage increase. Figure 4.7 shows that the gap between the price of an overnight stay (in red) and the price per square meter of housing (in blue) is much smaller in communes with low populations. This confirms that Airbnb's profitability is much higher in more rural and unattractive areas, which capitalizes directly on property prices.

Figure 4.7: Price per Square Meters and Average Airbnb Revenue per Municipality Size



For principal, secondary, and suburban communities, the effect is much the same. A 1% increase in listing density raises property market prices by around 7%. Finally, touristic countryside municipalities, comprised of municipalities with a high concentration of cultural heritage or a strong tourism history (coastal municipalities, for example), are impacted with the lowest magnitude, of

around 4.3%. It is explained by the fact that these tourist areas are particularly well-equipped in terms of infrastructure and have a real estate market already geared towards tourism.

Table 4.6: Municipality Profile Heterogeneity

	Price per square meter (log)				
	(1)	(2)	(3)	(4)	(5)
Center	0.116*** (0.005)	0.091*** (0.008)	0.088*** (0.008)	0.091*** (0.008)	0.092*** (0.008)
Principal	0.092*** (0.004)	0.067*** (0.008)	0.062*** (0.008)	0.065*** (0.007)	0.066*** (0.007)
Secondary	0.086*** (0.007)	0.068*** (0.009)	0.062*** (0.009)	0.065*** (0.008)	0.066*** (0.008)
Suburbs	0.090*** (0.004)	0.071*** (0.008)	0.066*** (0.008)	0.069*** (0.007)	0.070*** (0.007)
Countryside (not touristic)	0.112*** (0.005)	0.087*** (0.008)	0.082*** (0.008)	0.085*** (0.008)	0.086*** (0.007)
Countryside (touristic)	0.056*** (0.006)	0.041*** (0.009)	0.039*** (0.009)	0.042*** (0.008)	0.043*** (0.008)
Sargan Test	1.000	1.000	1.000	1.000	1.000
Municipal Characteristics	✓	✓	✓	✓	✓
Touristic Amenities	×	✓	✓	✓	✓
Spatial Fixed Effects	×	×	✓	✓	✓
Monthly Fixed Effects	×	×	×	✓	×
Month × Year Fixed Effects	×	×	×	×	✓
Observations	221,266	221,266	221,266	221,266	221,266
Adjusted R <sup>2</sup>	0.446	0.472	0.473	0.473	0.473

*Note:* The dependent variable is the density of listings in a municipality (number of listings divided by the housing stock). The p-value of the Sargan test of overidentifying assumption is given in line (2). Variables undergoing logarithmic transformation have been increased by 1 to avoid logarithms of 0. Standard deviations are reported in parenthesis.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

## 4.5.2 Tight Real Estate Market Heterogeneity

We also conduct a heterogeneity analysis depending on the French A, B, C zoning. According to article D304-1 of the French Construction and Housing Code, ABC zoning "classifies towns in France into geographical zones according to the imbalance between housing supply and demand." Municipalities are classified in descending order of tension as A, B1, B2, and C<sup>11</sup>.

<sup>11</sup>Zone A contains Paris and 75 municipalities in 5 départements of Ile-de-France (Yvelines, Hauts-de-Seine, Seine-Saint-Denis, Val-de-Marne and Val-d'Oise), the Côte d'Azur, the French part of the Geneva conurbation, certain conurbations or municipalities where rents and housing prices are very high. Zone B1 includes certain large conurbations and municipalities with high rents and housing prices and the French overseas departments. Zone B2 includes city centers of certain large conurbations and some outer suburbs of Paris with fairly high rents and housing

STR may increase the tension in the rental market, which in turn may increase transaction prices. Table 4.7 shows the results based on the classification of towns according to their local housing market.

Table 4.7: Municipality Ranking Heterogeneity

	Price per square meter (log)				
	(1)	(2)	(3)	(4)	(5)
Zone A	0.037*** (0.005)	0.032*** (0.008)	0.101*** (0.014)	0.103*** (0.012)	0.104*** (0.012)
Zone B1	0.081*** (0.005)	0.078*** (0.008)	0.116*** (0.014)	0.118*** (0.012)	0.119*** (0.012)
Zone B2	0.126*** (0.004)	0.116*** (0.008)	0.127*** (0.013)	0.130*** (0.012)	0.131*** (0.012)
Zone C	0.174*** (0.004)	0.157*** (0.008)	0.143*** (0.013)	0.146*** (0.012)	0.147*** (0.012)
Sargan Test	0.535	0.230	0.090	0.110	0.120
Municipal Characteristics	✓	✓	✓	✓	✓
Touristic Amenities	×	✓	✓	✓	✓
Spatial Fixed Effects	×	×	✓	✓	✓
Monthly Fixed Effects	×	×	×	✓	×
Month × Year Fixed Effects	×	×	×	×	✓
Observations	221,266	221,266	221,266	221,266	221,266
Adjusted R <sup>2</sup>	0.457	0.481	0.539	0.539	0.539

*Note:* The dependent variable is the density of listings in a municipality (number of listings divided by the housing stock). The p-value of the Sargan test of overidentifying assumption is given in line (2). Variables undergoing logarithmic transformation have been increased by 1 to avoid logarithms of 0. Standard deviations are reported in parenthesis.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

We note a positive effect of listing density on real estate prices, whatever the zone tested. The amplitude of the coefficients is inversely proportional to the tension of the zones. The effect in zone A (around 10%) is less than the effect in zone C (around 15%), which corresponds to the least tense areas of the French territory. These results are somewhat counter-intuitive and follow almost the same logic as the previous heterogeneity study: particularly tense areas already have exceptionally high property prices. This apparent inferiority in percentage terms thus conceals a significant price increase in nominal terms.

---

prices. Zone C is the rest of France.

### 4.5.3 Secondary Housing Heterogeneity

The effect of listing density on the real estate market can also vary widely depending on the tourism attractiveness of the municipalities, which can be captured by the share of secondary housing. Table 4.8 shows the interaction of listing density and secondary housing rate on real estate market prices per square meter. The effect is highly significant and positive. The magnitude of the effect of STR activity density, therefore, evolves in conjunction with the rate of secondary housing.

Table 4.8: Secondary Housing Heterogeneity

	Price per square meter (log)				
	(1)	(2)	(3)	(4)	(5)
Density of Listings (log)	0.038*** (0.004)	-0.028*** (0.009)	0.0001 (0.015)	0.010 (0.014)	0.011 (0.013)
Density of Listings (log) × Secondary Housing	0.008*** (0.0002)	0.008*** (0.0002)	0.008*** (0.0003)	0.007*** (0.0003)	0.008*** (0.0003)
Sargan Test	0.429	0.513	0.228	0.339	0.346
Municipal Characteristics	✓	✓	✓	✓	✓
Touristic Amenities	×	✓	✓	✓	✓
Spatial Fixed Effects	×	×	✓	✓	✓
Monthly Fixed Effects	×	×	×	✓	×
Month × Year Fixed Effects	×	×	×	×	✓
Observations	221,266	221,266	221,266	221,266	221,266
Adjusted R <sup>2</sup>	0.447	0.468	0.544	0.545	0.545

*Note:* The dependent variable is the density of listings in a municipality (number of listings divided by the housing stock). The p-value of the Sargan test of overidentifying assumption is given in line (2). Variables undergoing logarithmic transformation have been increased by 1 to avoid logarithms of 0. Standard deviations are reported in parenthesis.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

### 4.5.4 Distance to Train Station Heterogeneity

Finally, we evaluate the heterogeneity of the effect of listing density according to the accessibility of municipalities. To do this, we measure the hourly distance to the nearest high-speed train station in each municipality. We expect that listing density will have a weaker effect if municipalities are more difficult to access.

Table 4.9 shows the effect of listing density interacted with the distance to the nearest high-speed train station on property market prices. As expected, there is a highly significant negative effect of the hourly distance to the nearest station.

Table 4.9: Distance to Train Station Heterogeneity

	Price per square meter (log)				
	(1)	(2)	(3)	(4)	(5)
Density of Listings (log)	0.089*** (0.004)	0.062*** (0.008)	0.108*** (0.014)	0.113*** (0.013)	0.115*** (0.012)
Density of Listings (log) × Distance to Train	-0.0001** (0.00003)	-0.0004*** (0.0001)	-0.0004*** (0.0001)	-0.0004*** (0.0001)	-0.0004*** (0.0001)
Sargan Test	0.383	0.803	0.319	0.454	0.515
Municipal Characteristics	✓	✓	✓	✓	✓
Touristic Amenities	×	✓	✓	✓	✓
Spatial Fixed Effects	×	×	✓	✓	✓
Monthly Fixed Effects	×	×	×	✓	×
Month × Year Fixed Effects	×	×	×	×	✓
Observations	221,266	221,266	221,266	221,266	221,266
Adjusted R <sup>2</sup>	0.444	0.471	0.542	0.542	0.542

*Note:* The dependent variable is the density of listings in a municipality (number of listings divided by the housing stock). The p-value of the Sargan test of overidentifying assumption is given in line (2). Variables undergoing logarithmic transformation have been increased by 1 to avoid logarithms of 0. Standard deviations are reported in parenthesis.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

## 4.6 Conclusion

This paper examines the effect of short-term rentals density on real estate transaction prices in France. The study contributes to the existing literature by providing comprehensive analysis across the entire country, incorporating spatial heterogeneity, and using a novel instrument to capture territory heterogeneities. The study's main finding indicates that a 1% increase in listing density in a municipality leads to an 11% increase in real estate transaction prices. The results also reveal heterogeneity in the effects, with central municipalities experiencing higher percentage price increases than other categories. Surprisingly, rural municipalities, especially those not traditionally touristic, show significant price increases, indicating that STR raised prices everywhere and has made these previously less-equipped municipalities more attractive.

We employ instrumental variables to address potential endogeneity issues and aggregate data at the municipality level due to the availability of Airbnb data. The instruments are constructed based on the number of views of online ads on leboncoin, a prominent French platform. Our findings align with previous studies indicating a positive association between short-term rentals and real estate market prices. The results globally contribute to the ongoing discussions on regulating short-term rental activity in various cities worldwide and more specifically to the ongoing heated French debate on the impact of STR on the real estate market, which for instance manifests itself by cross-party bills to regulate the rental industry.

The increase in the real estate market prices associated with the STR activity can be attributed to several underlying mechanisms. First, the ability of property owners to generate additional income through short-term rentals is capitalized in the value of their real estate assets. The increasing demand for short-term rental properties puts upward pressure on prices in the real estate market. Secondly, the concentration of short-term rentals in certain areas diverts housing usage from the traditional rental market to the tourist market. This increased competition for available housing resources further drives up prices in the real estate market. Additionally, the shift from traditional neighborhood businesses to entertainment-oriented ones, driven by the influx of tourists seeking short-term rentals, can increase real estate prices as demand for commercial spaces rises. Reduced supply of traditional rental housing is also a key driver. Finally, the negative amenities due to the high concentration of short-term rentals in specific neighborhoods are overwhelmed by the capitalization induced by the first four factors. Collectively, these mechanisms contribute to the observed increase in the real estate market associated with the growth of short-term rentals.

Future research could further explore the long-term effects of STR on the real estate market and investigate the potential implications for housing affordability and neighborhood dynamics. Especially the alteration of the local economic environment should be studied. Additionally, analyzing the impact of regulations and policy interventions on short-term rentals and their effects on the real estate market would provide valuable insights for policymakers and urban planners.



## 4.7 Appendices

### 4.7.1 Appendix A. Supplementary Descriptive Statistics

Figure 4.8: Number of Listings

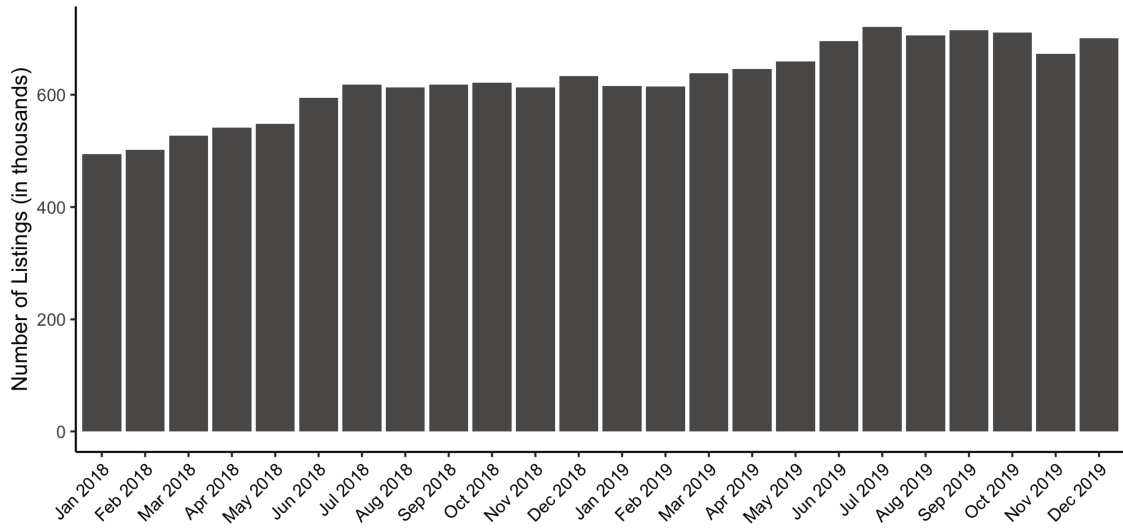


Figure 4.9: Number of Days Reserved

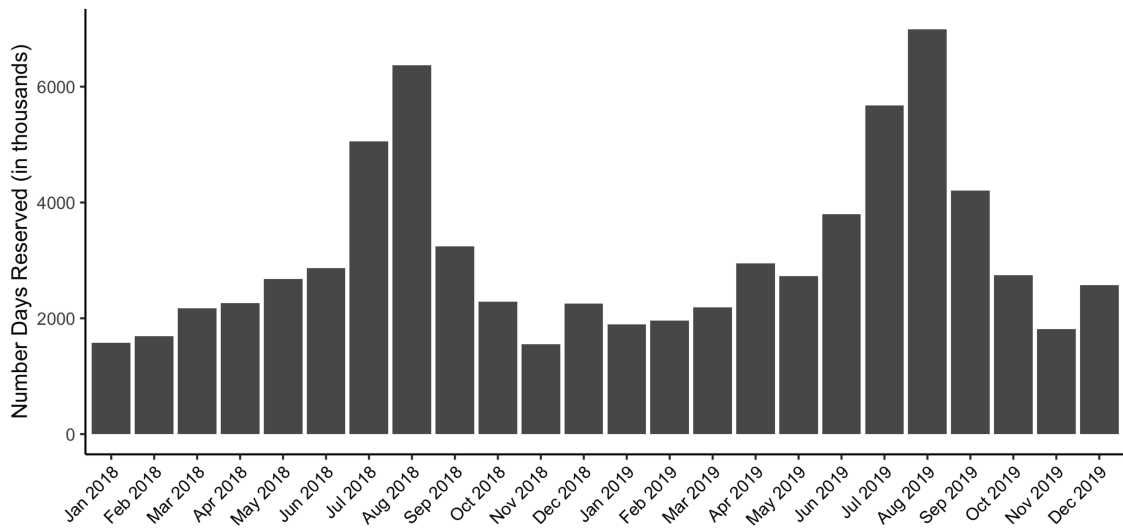


Figure 4.10: Number of Listings Broken Down by Municipality Type

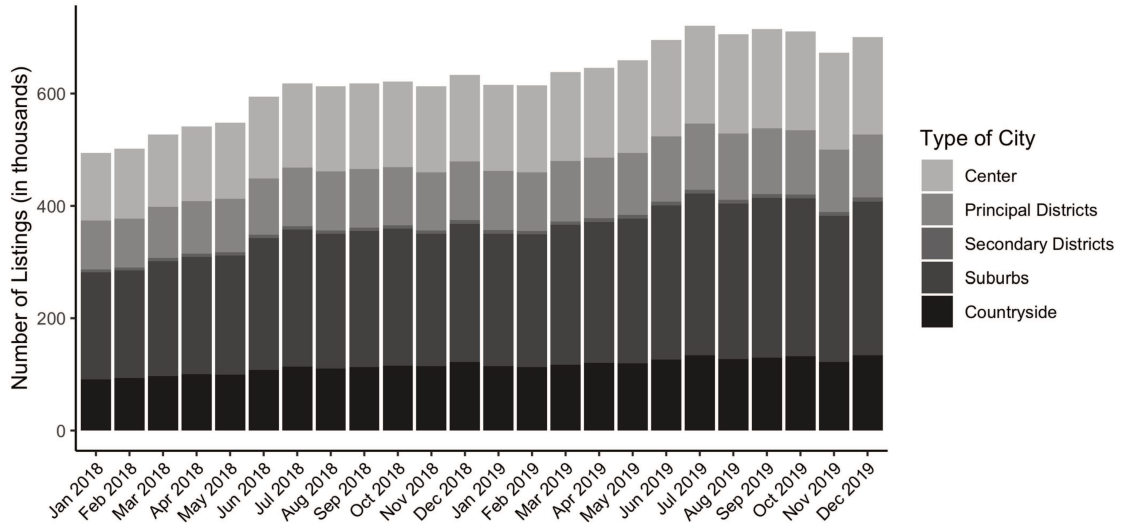
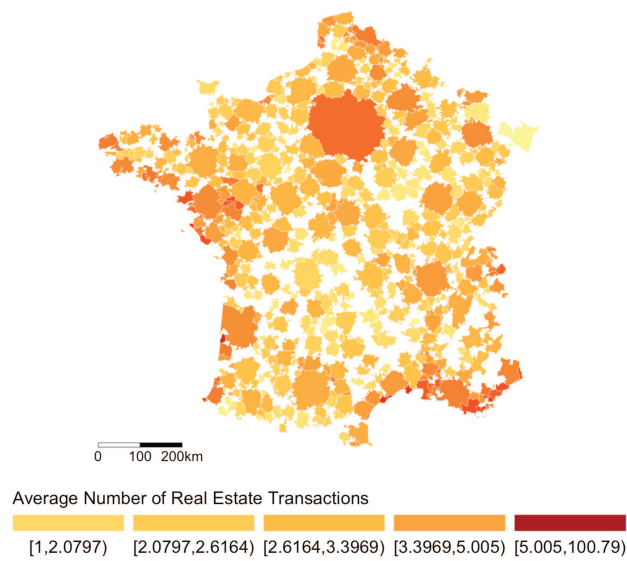


Figure 4.11: Number of Transactions



## 4.7.2 Appendix B. First Stage Regression - Spillover Effects

Table 4.10: First stage - Spillover Effects

	Density of Listings (log)				
	(1)	(2)	(3)	(4)	(5)
Nb of Views (log)	0.238*** (0.002)	0.135*** (0.002)	0.076*** (0.001)	0.088*** (0.001)	0.089*** (0.001)
Growth of Nb of Views	-0.003*** (0.0002)	-0.001*** (0.0002)	-0.001*** (0.0001)	-0.001*** (0.0001)	-0.001*** (0.0001)
Growth of Nb of Views (Attraction Area)	-0.004*** (0.001)	-0.0003 (0.001)	-0.0001 (0.001)	-0.0001 (0.001)	-0.0001 (0.001)
Municipal Characteristics	✓	✓	✓	✓	✓
Touristic Amenities	×	✓	✓	✓	✓
Spatial Fixed Effects	×	×	✓	✓	✓
Monthly Fixed Effects	×	×	×	✓	×
Month × Year Fixed Effects	×	×	×	×	✓
Observations	221,266	221,266	221,266	221,266	221,266
Adjusted R <sup>2</sup>	0.466	0.607	0.684	0.686	0.692

*Note:* The dependent variable is the density of listings in a municipality (number of listings divided by the housing stock). Variables undergoing logarithmic transformation have been increased by 1 to avoid logarithms of 0. Standard deviations are reported in parenthesis.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

### 4.7.3 Appendix C. Supplementary Heterogeneity Analysis

Table 4.11: Municipality Size Heterogeneity

	Price per square meter (log)				
	(1)	(2)	(3)	(4)	(5)
<50k inhabitants	0.183*** (0.005)	0.180*** (0.008)	0.177*** (0.008)	0.177*** (0.008)	0.177*** (0.008)
50 - 200k inhabitants	0.176*** (0.005)	0.175*** (0.008)	0.172*** (0.008)	0.172*** (0.008)	0.172*** (0.008)
200 - 700k inhabitants	0.153*** (0.005)	0.155*** (0.008)	0.153*** (0.009)	0.152*** (0.008)	0.152*** (0.008)
+700k inhabitants	0.104*** (0.005)	0.113*** (0.008)	0.111*** (0.008)	0.110*** (0.008)	0.110*** (0.007)
Paris Area	0.081*** (0.004)	0.068*** (0.007)	0.066*** (0.008)	0.065*** (0.007)	0.065*** (0.007)
Countryside - Not Touristic	0.193*** (0.005)	0.185*** (0.008)	0.183*** (0.009)	0.183*** (0.008)	0.183*** (0.008)
Countryside - Touristic	0.147*** (0.006)	0.155*** (0.009)	0.153*** (0.009)	0.153*** (0.009)	0.153*** (0.009)
Sargan Test	1.000	1.000	1.000	1.000	1.000
Municipal Characteristics	✓	✓	✓	✓	✓
Touristic Amenities	×	✓	✓	✓	✓
Spatial Fixed Effects	×	×	✓	✓	✓
Monthly Fixed Effects	×	×	×	✓	×
Month × Year Fixed Effects	×	×	×	×	✓
Observations	221,266	221,266	221,266	221,266	221,266
Adjusted R <sup>2</sup>	0.446	0.472	0.473	0.473	0.473

*Note:* The dependent variable is the density of listings in a municipality (number of listings divided by the housing stock). The p-value of the Sargan test of overidentifying assumption is given in line (2). Variables undergoing logarithmic transformation have been increased by 1 to avoid logarithms of 0. Standard deviations are reported in parenthesis.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

### 4.7.4 Appendix D. Sensitivity to instrument choice

Tables 4.14 to 4.17 show that the number of ads selected per municipality has only a marginal impact on results.

#### Results using the top ad instruments

Table 4.12: Homeowners Heterogeneity

	Price per square meter (log)				
	(1)	(2)	(3)	(4)	(5)
Density of Listings (log)	-0.122*** (0.008)	-0.115*** (0.010)	-0.038** (0.015)	-0.030** (0.014)	-0.028** (0.014)
Density of Listings (log) × Share of Homeowners	0.003*** (0.0001)	0.002*** (0.0001)	0.002*** (0.0001)	0.002*** (0.0001)	0.002*** (0.0001)
Sargan Test	0.549	0.897	0.236	0.352	0.382
Municipal Characteristics	✓	✓	✓	✓	✓
Touristic Amenities	×	✓	✓	✓	✓
Spatial Fixed Effects	×	×	✓	✓	✓
Monthly Fixed Effects	×	×	×	✓	×
Month × Year Fixed Effects	×	×	×	×	✓
Observations	221,266	221,266	221,266	221,266	221,266
Adjusted R <sup>2</sup>	0.444	0.472	0.544	0.544	0.544

*Note:* The dependent variable is the density of listings in a municipality (number of listings divided by the housing stock). The p-value of the Sargan test of overidentifying assumption is given in line (2). Variables undergoing logarithmic transformation have been increased by 1 to avoid logarithms of 0. Standard deviations are reported in parenthesis.

\*p<0.1; \*\*p<0.05; \*\*\* p<0.01

Table 4.13: Vacant Housing Heterogeneity

	Price per square meter (log)				
	(1)	(2)	(3)	(4)	(5)
Density of Listings (log)	0.094*** (0.008)	0.021** (0.010)	0.090*** (0.016)	0.094*** (0.014)	0.096*** (0.014)
Density of Listings (log) × Vacant Housing	-0.001 (0.001)	0.004*** (0.001)	0.002** (0.001)	0.002** (0.001)	0.002** (0.001)
Sargan Test	0.693	0.704	0.232	0.343	0.389
Municipal Characteristics	✓	✓	✓	✓	✓
Touristic Amenities	×	✓	✓	✓	✓
Spatial Fixed Effects	×	×	✓	✓	✓
Monthly Fixed Effects	×	×	×	✓	×
Month × Year Fixed Effects	×	×	×	×	✓
Observations	221,266	221,266	221,266	221,266	221,266
Adjusted R <sup>2</sup>	0.444	0.471	0.542	0.542	0.542

*Note:* The dependent variable is the density of listings in a municipality (number of listings divided by the housing stock). The p-value of the Sargan test of overidentifying assumption is given in line (2). Variables undergoing logarithmic transformation have been increased by 1 to avoid logarithms of 0. Standard deviations are reported in parenthesis.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Table 4.14: First Stage - Top 1 Ads

	Price per square meter (log)				
	(1)	(2)	(3)	(4)	(5)
Nb of Views (log)	0.226*** (0.002)	0.126*** (0.002)	0.072*** (0.002)	0.083*** (0.002)	0.085*** (0.002)
Growth of Nb of Views	-0.004*** (0.0002)	-0.002*** (0.0002)	-0.001*** (0.0002)	-0.001*** (0.0002)	-0.001*** (0.0002)
Municipal Characteristics	✓	✓	✓	✓	✓
Touristic Amenities	×	✓	✓	✓	✓
Spatial Fixed Effects	×	×	✓	✓	✓
Monthly Fixed Effects	×	×	×	✓	×
Month × Year Fixed Effects	×	×	×	×	✓
Observations	221,266	221,266	221,266	221,266	221,266
Adjusted R <sup>2</sup>	0.456	0.603	0.683	0.685	0.691

*Note:* The dependent variable is the density of listings in a municipality (number of listings divided by the housing stock). Variables undergoing logarithmic transformation have been increased by 1 to avoid logarithms of 0. Standard deviations are reported in parenthesis.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Table 4.15: Main Results - Top 1 Ads

	Price per square meter (log)				
	(1)	(2)	(3)	(4)	(5)
Density of Listings (log)	0.098*** (0.005)	0.082*** (0.009)	0.103*** (0.015)	0.108*** (0.014)	0.110*** (0.014)
Sargan Test	0.525	0.513	0.016	0.030	0.038
Municipal Characteristics	✓	✓	✓	✓	✓
Touristic Amenities	×	✓	✓	✓	✓
Spatial Fixed Effects	×	×	✓	✓	✓
Monthly Fixed Effects	×	×	×	✓	×
Month × Year Fixed Effects	×	×	×	×	✓
Observations	221,266	221,266	221,266	221,266	221,266
Adjusted R <sup>2</sup>	0.444	0.471	0.543	0.542	0.542

*Note:* The dependent variable is the density of listings in a municipality (number of listings divided by the housing stock). The p-value of the Sargan test of overidentifying assumption is given in line (2). Variables undergoing logarithmic transformation have been increased by 1 to avoid logarithms of 0. Standard deviations are reported in parenthesis.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Results using the top 5 ads instruments

Table 4.16: First Stage - Top 5 Ads

	Price per square meter (log)				
	(1)	(2)	(3)	(4)	(5)
Nb of Views (log)	0.238*** (0.002)	0.133*** (0.001)	0.077*** (0.001)	0.089*** (0.001)	0.090*** (0.001)
Growth of Nb of Views	-0.003*** (0.0002)	-0.001*** (0.0002)	-0.001*** (0.0001)	-0.001*** (0.0001)	-0.001*** (0.0001)
Municipal Characteristics	✓	✓	✓	✓	✓
Touristic Amenities	×	✓	✓	✓	✓
Spatial Fixed Effects	×	×	✓	✓	✓
Monthly Fixed Effects	×	×	×	✓	×
Month × Year Fixed Effects	×	×	×	×	✓
Observations	221,266	221,266	221,266	221,266	221,266
Adjusted R <sup>2</sup>	0.472	0.608	0.685	0.687	0.692

*Note:* The dependent variable is the density of listings in a municipality (number of listings divided by the housing stock). Variables undergoing logarithmic transformation have been increased by 1 to avoid logarithms of 0. Standard deviations are reported in parenthesis.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Table 4.17: Main Results - Top 5 Ads

	Price per square meter (log)				
	(1)	(2)	(3)	(4)	(5)
Density of Listings (log)	0.085*** (0.004)	0.043*** (0.008)	0.103*** (0.013)	0.109*** (0.012)	0.110*** (0.012)
Sargan Test	0.894	0.827	0.099	0.169	0.201
Municipal Characteristics	✓	✓	✓	✓	✓
Touristic Amenities	×	✓	✓	✓	✓
Spatial Fixed Effects	×	×	✓	✓	✓
Monthly Fixed Effects	×	×	×	✓	×
Month × Year Fixed Effects	×	×	×	×	✓
Observations	221,266	221,266	221,266	221,266	221,266
Adjusted R <sup>2</sup>	0.444	0.471	0.543	0.542	0.542

*Note:* The dependent variable is the density of listings in a municipality (number of listings divided by the housing stock). The p-value of the Sargan test of overidentifying assumption is given in line (2). Variables undergoing logarithmic transformation have been increased by 1 to avoid logarithms of 0. Standard deviations are reported in parenthesis.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01





# Conclusion Générale

Cette thèse de doctorat se concentre sur une série de questions de recherche liées à l'économie publique locale, l'économie politique et l'économie du logement. Dans le premier chapitre, j'explore l'accès aux responsabilités politiques, en examinant la discrimination ethnique subie par les candidats d'origine nord-africaine aux élections municipales. Ce chapitre révèle une discrimination substantielle à l'encontre de ces candidats, avec une baisse d'une dizaine de points de pourcentage des voix collectées. Le deuxième chapitre se penche sur l'effet de l'affiliation politique des maires sur les dépenses publiques municipales. Je mets en évidence de fortes différences de décisions entre les maires de gauche et de droite. Les maires de gauche ont une propension plus élevée à investir dans l'aménagement du territoire, l'environnement et l'aide aux familles, tandis que les maires de droite accordent davantage d'importance à la sécurité. En outre, ces disparités de dépenses se sont principalement manifestées au cours des deux dernières années du mandat, révélant ainsi un cycle politico-économique marqué. Le troisième chapitre explore l'impact des accidents industriels sur le marché immobilier, en analysant l'incendie de l'usine Lubrizol à Rouen en 2019. Nous montrons que les transactions immobilières à proximité de l'incendie chutent drastiquement, tandis que les prix de l'immobilier restent relativement stables. À l'inverse, les logements situés à proximité d'autres installations à haut risque enregistrent une baisse de prix d'environ 15%, et une diminution de 10% est constatée sous le nuage de pollution causé par l'incendie. Ces résultats s'expliquent par l'aversion des vendeurs pour les pertes nominales, qui conduit à une réduction de l'offre de biens immobiliers à proximité de l'incendie, tandis que l'offre augmente à mesure que l'on s'éloigne du site. Enfin, dans le quatrième chapitre, nous examinons les effets de la location touristique meublée sur le marché de l'immobilier en France. Nous montrons qu'une augmentation de 1% de la densité des annonces de meublés touristiques entraîne une hausse de 11% des prix des transactions immobilières. De plus, les effets sont hétérogènes, avec une augmentation plus prononcée des prix dans les communes centres et dans les communes moins touristiques. Ces résultats alimentent le débat sur la réglementation de la location touristique meublée et permettent une élaboration de politiques publiques éclairées.

Cette thèse s'inscrit entièrement dans le champ des politiques publiques grâce aux éclairages qu'elle

fournit<sup>12</sup>. Le premier chapitre fait le constat de la présence de discriminations aux élections, pouvant inciter l'intervention des pouvoirs publics. Le deuxième chapitre éclaire le débat démocratique sur l'utilisation des ressources financières publiques. Le troisième chapitre montre l'effet des accidents industriels et permet de calibrer des mécanismes de compensation pour les populations locales. Enfin, le quatrième chapitre fournit les clés pour une réglementation des meublés de tourisme en prenant en compte les spécificités des communes.

Par ailleurs, cette thèse soulève des interrogations dépassant les sujets d'étude abordés *stricto sensu* dans les différents chapitres. Parmi celles-ci se pose la problématique de l'accès aux données, qui revêt une importance capitale en ce qui concerne la location touristique meublée. Son essor fait l'objet de vifs débats au sein des collectivités territoriales. Bien que de nombreuses municipalités aient mis en place des réglementations pour encadrer cette activité, celles-ci sont souvent établies sans recourir à une étude d'impact préalable, que ce soit à l'échelle locale ou nationale. L'une des raisons réside dans les difficultés d'accès aux données. Les principaux acteurs du marché, à l'instar d'Airbnb, sont réticents à l'idée de partager leurs données à une maille assez fine<sup>13</sup>, ce qui entrave considérablement la prise de décisions éclairées en matière de logement, sujet pour le moins essentiel. Dans ce contexte, l'État et les collectivités territoriales se trouvent confrontés à une impasse, en raison de l'absence de mécanismes leur permettant de contraindre ces acteurs à partager les informations liées à leur activité. L'économie collaborative connaissant un essor fulgurant, il devient impératif d'explorer des solutions afin de pouvoir accéder aux données détenues par ces entreprises, dans le but de mettre en place des politiques publiques éclairées afin de défendre l'intérêt général.

Le premier chapitre soulève également la question des données et plus particulièrement celle des statistiques ethniques. Interdites en France dans l'objectif de *justement* protéger les citoyens, l'absence de mesure ne permet pas de rendre compte des réalités socio-économiques auxquelles sont confrontées certaines catégories de population. Ces dernières sont tout de même identifiables (en raison de leur patronyme ou de leurs traits physiques) et subissent un préjudice, malgré l'absence d'un quelconque "fichage". Il en résulte un constat particulièrement troublant. Les discriminations ethniques peuvent prospérer tandis que le développement de politiques publiques est ardu sans statistiques : comment cibler ? comment en fixer des objectifs ? comment en mesurer des effets ? Il serait plus qu'intéressant d'évaluer les aspects économiques, politiques et sociologiques de la mise en place d'un système de mesure pour lutter contre discriminations.

Pour finir, cette thèse ouvre la voie à de futures recherches, que je souhaite mener. Si les discrim-

<sup>12</sup>D'après France Stratégie, entrent dans le cadre de l'évaluation de politiques publiques "toute publication visant à éclairer un champ de politique publique ou la conduite d'une intervention publique et s'appuyant sur une expertise reconnue et sur des données ayant valeur de preuves (statistiques, témoignages, etc.)". <https://www.vie-publique.fr/eclairage/21987-evaluation-des-politiques-publiques-instrument-de-la-reforme-de-letat>

<sup>13</sup>Les principales plateformes de location touristique meublée ont partagé des relevés mensuels avec Eurostat, au niveau NUTS2. Cette échelle, qui correspond au découpage territorial en région avant la réforme de 2015, n'est pas assez fine afin de mesurer l'impact des meublés de tourisme étant donnée l'hétérogénéité mise en avant dans le quatrième chapitre

inations aux élections sont flagrantes en France, qu'en est-il des États européens possédant une immigration post-coloniale ? Analyser et évaluer les discriminations potentiellement subies par les communautés indiennes et pakistanaises au Royaume-Uni, ou encore celles pouvant toucher la population turque en Allemagne pourraient éclairer les débats sur les politiques d'intégration et d'assimilation. Cette question est d'autant plus cruciale que les migrations extra-régionales sont accentuées par le dérèglement climatique.

Le deuxième chapitre explore les différences dans les dépenses publiques municipales dues à la couleur politique des Maires. Cependant, le paysage politique français a fortement changé ces dernières années, laissant place à une forte polarisation des débats. Éclairer le lien entre discours aux niveaux national et local, et la mise en place de politiques publiques peut s'avérer essentiel pour la préservation de la vie démocratique. En utilisant des modèles linguistiques avancés pour analyser les programmes, les meetings électoraux et les communications sur les réseaux sociaux, nous pourrions évaluer la correspondance entre le discours et les décisions politiques prises une fois élu.

Le troisième chapitre met en évidence les effets surprenants de l'explosion de l'usine Lubrizol sur le marché immobilier, où les prix ne semblent pas intégrer les aménités négatives issues de l'accident. Le développement potentiel de ce papier pourrait prendre la forme d'un modèle. Le modèle théorique serait basé sur l'aversion des vendeurs pour les pertes nominales, et pourrait rationaliser l'ensemble des effets observés. Ce modèle pourrait être augmenté à l'aide de la théorie des jeux coopératifs : les vendeurs se seraient implicitement coordonnés afin de réduire l'offre et maintenir les prix.

Enfin, le quatrième chapitre pose les bases solides d'une compréhension de la location touristique meublée. De futures recherches pourraient capitaliser sur ces enseignements - notamment en termes d'hétérogénéité - afin d'évaluer la gentrification commerciale dans les communes les plus affectées par les meublés de tourisme. Par ailleurs, dans le cas où des données non brouillées seraient accessibles, l'évaluation de la réglementation serait également possible, en utilisant des méthodes telles que la régression sur discontinuités ou encore les différences sur discontinuités. Ce travail a déjà été effectué conjointement avec Marie Breuillé et Julie Le Gallo dans le cadre d'un projet mené avec la Direction de l'Habitat, de l'Urbanisme et des Paysages (DHUP), cependant les données étant brouillées, les limites étaient trop importantes pour inclure les travaux dans cette thèse.



# Bibliography

- Adamiak, Czesław (2018). “Mapping Airbnb Supply in European Cities”. Annals of Tourism Research 71, pp. 67–71.
- (2019). “Current State and Development of Airbnb Accommodation Offer in 167 Countries”. Current Issues in Tourism, pp. 1–19.
- Anenberg, Elliot (2011). “Loss Aversion, Equity Constraints and Seller Behavior in the Real Estate Market”. Regional Science and Urban Economics 41.1, pp. 67–76.
- Anstine, Jeff (2003). “Property Values in a Low Populated Area When Dual Noxious Facilities Are Present”. Growth and Change 34.3, pp. 345–358.
- Arnoult, Emilie et al. (May 2021). “Discrimination à l’embauche des personnes d’origine supposée maghrébine : quels enseignements d’une grande étude par testing ?”
- Athey, Susan and Guido Imbens (July 2016). “Recursive Partitioning for Heterogeneous Causal Effects”. Proceedings of the National Academy of Sciences 113.27, pp. 7353–7360.
- Athey, Susan and Stefan Wager (2019). “Estimating Treatment Effects with Causal Forests: An Application”. Observational Studies 5.2, pp. 37–51.
- Ayouba, Kassoum et al. (2020). “Does Airbnb Disrupt the Private Rental Market? An Empirical Analysis for French Cities”. International Regional Science Review 43.1-2, pp. 76–104.
- Barron, Kyle, Edward Kung, and Davide Proserpio (2021). “The Effect of Home-Sharing on House Prices and Rents: Evidence from Airbnb”. Marketing Science 40.1, pp. 23–47.
- Bauer, Thomas K, Sebastian T Braun, and Michael Kvasnicka (2017). “Nuclear Power Plant Closures and Local Housing Values: Evidence from Fukushima and the German Housing Market”. Journal of Urban Economics 99, pp. 94–106.
- Becker, Gary S. (1957). The Economics of Discrimination. Ed. by 2d edition. Economic Research Studies. Chicago, IL: University of Chicago Press.
- Beland, Louis-Philippe and Sara Oloomi (2017a). “Party Affiliation and Public Spending: Evidence from U.S. Governors: Party Affiliation and Public Spending”. Economic Inquiry 55.2, pp. 982–995.
- (Apr. 2017b). “Party Affiliation and Public Spending: Evidence from U.S. Governors: Party Affiliation and Public Spending”. Economic Inquiry 55.2, pp. 982–995.

- Belloni, Alexandre and Victor Chernozhukov (2013). “Least Squares after Model Selection in High-Dimensional Sparse Models”. Bernoulli 19.2.
- Benjamini, Yoav and Yosef Hochberg (1995). “Controlling the False Discovery Rate: A Practical and Powerful Approach to Multiple Testing”. Journal of the Royal Statistical Society: Series B (Methodological) 57.1, pp. 289–300.
- Benkeser, D and M Carone (2017). “Doubly Robust Nonparametric Inference on the Average Treatment Effect”, p. 18.
- Bertrand, Marianne, Dolly Chugh, and Sendhil Mullainathan (Apr. 2005). “Implicit Discrimination”. American Economic Review 95.2, pp. 94–98.
- Binet, Marie-Estelle, Alain Guengant, and Matthieu Leprince (Nov. 2010). “Superposition des collectivités territoriales, dépenses publiques locales et hétérogénéité spatiale: Le cas des villes françaises de plus de 50 000 habitants”. Revue économique Vol. 61.6, pp. 1111–1122.
- Bird, Karen (Dec. 2005). “The Political Representation of Visible Minorities in Electoral Democracies: A Comparison of France, Denmark, and Canada”. Nationalism and Ethnic Politics 11.4, pp. 425–465.
- Broockman, David E and Evan J Soltas (2020). “A Natural Experiment on Discrimination in Elections”, p. 40.
- Brueckner, Jan K. (Mar. 1979). “Property Values, Local Public Expenditure and Economic Efficiency”. Journal of Public Economics 11.2, pp. 223–245.
- Caliński, T. and J Harabasz (1974). “A Dendrite Method for Cluster Analysis”. Communications in Statistics 3.1, pp. 1–27.
- Calonico, Sebastian, Matias D Cattaneo, and Rocío Titiunik (2014). “Robust Data-Driven Inference in the Regression-Discontinuity Design”. The Stata Journal 14.4, pp. 909–946.
- Calonico, Sebastian et al. (2019). “Regression Discontinuity Designs Using Covariates”. The Review of Economics and Statistics 101.3, pp. 442–451.
- Carolien van Ham et al., eds. (2017). Myth and Reality of the Legitimacy Crisis. First edition. Oxford ; New York: Oxford University Press.
- Challe, Laetitia et al. (2020). “Discrimination dans le recrutement des grandes entreprises: une approche multicanal”.
- Chanfreau, Marie-Catherine Talvikki (Feb. 2006). “Espagnols en territoire français de 1813 à 1971 : circuits ou intégrations d’exilés et d’émigrés”. Mémoire(s), identité(s), marginalité(s) dans le monde occidental contemporain. Cahiers du MIMMOCC 1.
- Chang, Hung-Hao (2020). “Does the Room Sharing Business Model Disrupt Housing Markets? Empirical Evidence of Airbnb in Taiwan”. Journal of Housing Economics 49, p. 101706.
- Chareyron, Sylvain, Louis-Alexandre Erb, and Yannick L’Horty (2022). “Discrimination dans l’accès aux masters : une évaluation expérimentale”.

- Chareyron, Sylvain et al. (Feb. 2023). “À la recherche du point de basculement sur le marché du logement : les résultats d’une expérience de terrain.” Revue économique Vol. 74.1, pp. 5–27.
- Chattopadhyay, Raghendra and Esther Dufo (Sept. 2004). “Women as Policy Makers: Evidence from a Randomized Policy Experiment in India”. Econometrica 72.5, pp. 1409–1443.
- Combes, Pierre-Philippe, Gilles Duranton, and Laurent Gobillon (2019). “The Costs of Agglomeration: House and Land Prices in French Cities”. Review of Economic Studies 86.4, pp. 1556–1589.
- de Benedictis-Kessner, Justin and Christopher Warshaw (2016). “Mayoral Partisanship and Municipal Fiscal Policy”. The Journal of Politics 78.4, pp. 1124–1138.
- de la Cuesta, Brandon and Kosuke Imai (2016). “Misunderstandings about the Regression Discontinuity Design in the Study of Close Elections”. Annual Review of Political Science 19.1, pp. 375–396.
- Downs, Anthony (1957). “An Economic Theory of Political Action in a Democracy”. Journal of political economy, p. 16.
- Duboz, Marie-Line, Julie Le Gallo, and Mathieu Houser (2020). “Aux origines des disparités de dépenses des départements français : une analyse empirique (2006-2016)”. Revue d’Economie Politique, p. 26.
- Duso, Tomaso et al. (2022). “Airbnb and Rental Markets: Evidence from Berlin”.
- Eggers, Andrew C. et al. (2015). “On the Validity of the Regression Discontinuity Design for Estimating Electoral Effects: New Evidence from Over 40,000 Close Races: On the Validity of the Electoral Regression Discontinuity Design”. American Journal of Political Science 59.1, pp. 259–274.
- Engelhardt, Gary V. (2003). “Nominal Loss Aversion, Housing Equity Constraints, and Household Mobility: Evidence from the United States”. Journal of Urban Economics 53.1, pp. 171–195.
- Esteve-Volart, Berta and Manuel Bagues (Apr. 2012). “Are Women Pawns in the Political Game? Evidence from Elections to the Spanish Senate”. Journal of Public Economics 96.3-4, pp. 387–399.
- Eymmoud, Jean-Benoit and Paul Vertier (2017). “Gender Biases: Evidence from a Natural Experiment in French Local Elections”. SSRN Electronic Journal.
- Farronato, Chiara and Andrey Fradkin (2019). “The Welfare Effects of Peer Entry in the Accommodation Market: The Case of Airbnb”. Hutchins Center Working Paper #55, p. 60.
- Ferreira, Fernando and Joseph Gyourko (2009). “Do Political Parties Matter? Evidence from U.S. Cities”. Quarterly Journal of Economics, p. 24.
- (Apr. 2014). “Does Gender Matter for Political Leadership? The Case of U.S. Mayors”. Journal of Public Economics 112, pp. 24–39.



- Faroni, Fabrice, Marie Ruault, and Emmanuel Valat (2016). “Discrimination à l’embauche Selon « l’origine » : Que Nous Apprend Le Testing Auprès de Grandes Entreprises ?” Dares Analyses 076.
- Foucault, Martial, Thierry Madies, and Sonia Paty (Oct. 2008). “Public Spending Interactions and Local Politics. Empirical Evidence from French Municipalities”. Public Choice 137.1-2, pp. 57–80.
- Franco, Sofia F. and Carlos Daniel Santos (2021). “The Impact of Airbnb on Residential Property Values and Rents: Evidence from Portugal”. Regional Science and Urban Economics 88, p. 103667.
- Garcia-López, Miquel-Àngel et al. (2020). “Do Short-Term Rental Platforms Affect Housing Markets? Evidence from Airbnb in Barcelona”. Journal of Urban Economics 119, p. 103278.
- Genesove, David and Christopher Mayer (2001). “Loss Aversion and Seller Behavior: Evidence from the Housing Market”. The Quarterly Journal of Economics 116.4, pp. 1233–1260. JSTOR: 2696458.
- George, Pierre (1986). “L’immigration italienne en France de 1920 à 1939: aspects démographiques et sociaux”. Publications de l’École Française de Rome 94.1, pp. 45–67.
- Gerber, Elisabeth R and Daniel J Hopkins (2011). “When Mayors Matter: Estimating the Impact of Mayoral Partisanship on City Policy”. American Journal of Political Science 55.2, pp. 326–339.
- Gourieroux, Christian and Anne Laferrere (2009). “Managing Hedonic Housing Price Indexes: The French Experience”. Journal of Housing Economics 18.3, pp. 206–213.
- Griffin, John D. (May 2014). “When and Why Minority Legislators Matter”. Annual Review of Political Science 17.1, pp. 327–336.
- Grislain-Letrémy, Céline and Arthur Katosky (2013). “Les Risques Industriels et Le Prix Des Logements (Industrial Hazards and the Price of Housing)”. Économie et statistique 460-461, pp. 79–106.
- Gruber, Jonathan (2005). Public Finance and Public Policy. Macmillan.
- Guttentag, Daniel (2015). “Airbnb: Disruptive Innovation and the Rise of an Informal Tourism Accommodation Sector”. Current Issues in Tourism 18.12, pp. 1192–1217.
- Hajnal, Zoltan L. and Jessica Trounstein (Nov. 2010). “Who or What Governs?: The Effects of Economics, Politics, Institutions, and Needs on Local Spending”. American Politics Research 38.6, pp. 1130–1163.
- Hartigan, J. A. and M. A. Wong (1979). “Algorithm AS 136: A K-Means Clustering Algorithm”. Applied Statistics 28.1, p. 100. JSTOR: 10.2307/2346830.
- Hawkins, Stacy (2018). “Batson for Judges, Police Officers & Teachers: Lessons in Democracy From the Jury Box”. Michigan Journal of Race & Law 23.1, p. 1.

- Heinelt, Hubert and Nikolaos-K. Hlepas (2006). “Typologies of Local Government Systems”. The European Mayor. Ed. by Henry Bäck, Hubert Heinelt, and Annick Magnier. Wiesbaden: VS Verlag für Sozialwissenschaften, pp. 21–42.
- Herwartz, Helmut and Bernd Theilen (2017). “Ideology and Redistribution through Public Spending”. European Journal of Political Economy 46, pp. 74–90.
- Hill, Andrew J. and Daniel B. Jones (2017). “Does Partisan Affiliation Impact the Distribution of Spending? Evidence from State Governments’ Expenditures on Education”. Journal of Economic Behavior & Organization 143, pp. 58–77.
- Hsu, Yu-Chin and Shu Shen (2019). “Testing Treatment Effect Heterogeneity in Regression Discontinuity Designs”. Journal of Econometrics 208.2, pp. 468–486.
- Koster, Hans R.A., Jos van Ommeren, and Nicolas Volkhausen (2021). “Short-Term Rentals and the Housing Market: Quasi-experimental Evidence from Airbnb in Los Angeles”. Journal of Urban Economics 124, p. 103356.
- Ladd, Helen F. (Apr. 1992). “Population Growth, Density and the Costs of Providing Public Services”. Urban Studies 29.2, pp. 273–295.
- Lassébie, Julie (Mar. 2020). “Gender Quotas and the Selection of Local Politicians: Evidence from French Municipal Elections”. European Journal of Political Economy 62, p. 101842.
- Le Gallo, Julie et al. (Apr. 2020). “Discrimination in Access to Housing: A Test on Urban Areas in Metropolitan France”. Economie et Statistique / Economics and Statistics 513, pp. 27–45.
- Le Maux, Benoît, Yvon Rocaboy, and Timothy Goodspeed (2011). “Political Fragmentation, Party Ideology and Public Expenditures”. Public Choice 147.1-2, pp. 43–67.
- Lee, D. S., E. Moretti, and M. J. Butler (Aug. 2004). “Do Voters Affect or Elect Policies? Evidence from the U. S. House”. The Quarterly Journal of Economics 119.3, pp. 807–859.
- Leigh, Andrew (Mar. 2008). “Estimating the Impact of Gubernatorial Partisanship on Policy Settings and Economic Outcomes: A Regression Discontinuity Approach”. European Journal of Political Economy 24.1, pp. 256–268.
- McCrary, Justin (2008). “Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test”. Journal of Econometrics 142.2, pp. 698–714.
- Murray, Rainbow (July 2016). “The Political Representation of Ethnic Minority Women in France”. Parliamentary Affairs 69.3, pp. 586–602.
- Musiedlak, Y and B Vignolles (2016). “Les Mouvements Des Prix Immobiliers Dans l’ancien Au Cours Des Années 2000: Des Marchés Locaux Différenciés”. Document de travail du SOeS 22631.
- Oates, Wallace E. (Nov. 1969). “The Effects of Property Taxes and Local Public Spending on Property Values: An Empirical Study of Tax Capitalization and the Tiebout Hypothesis”. Journal of Political Economy 77.6, pp. 957–971.

- Oates, Wallace E. (1988). “On the Measurement of Congestion in the Provision of Local Public Goods”. Journal of Urban Economics 24.1, pp. 85–94.
- Oliver, J. Eric, Shang E. Ha, and Zachary Callen (2012). Local Elections and the Politics of Small-Scale Democracy. Princeton University Press.
- Pande, Rohini (Aug. 2003). “Can Mandated Political Representation Increase Policy Influence for Disadvantaged Minorities? Theory and Evidence from India”. American Economic Review 93.4, pp. 1132–1151.
- Persico, Simon, Caterina Froio, and Isabelle Guinaudeau (2012). “Action publique et partis politiques: L’analyse de l’agenda législatif français entre 1981 et 2009”. Gouvernement et action publique 1.1, p. 11.
- Pettersson-Lidbom, Per (Sept. 2008). “Do Parties Matter for Economic Outcomes? A Regression-Discontinuity Approach”. Journal of the European Economic Association 6.5, pp. 1037–1056.
- Phelps, Edmund S. (1972). “The Statistical Theory of Racism and Sexism”. The American Economic Review 62.4, pp. 659–661. JSTOR: 1806107.
- Piketty, Thomas (2022). “Mesurer Le Racisme, Vaincre Les Discriminations”. Mesurer Le Racisme, Vaincre Les Discriminations. HAL.
- Rensmann, Lars (Sept. 2014). “The Reluctant Cosmopolitanization of European Party Politics: The Case of Germany”. German Politics and Society 32.3, pp. 59–85.
- Robertson, Calum, Sylvain Dejean, and Raphaël Suire (2023). ““Airbnb in the City”: Assessing Short-Term Rental Regulation in Bordeaux”. The Annals of Regional Science.
- Rosen, Sherwin (Jan. 1974). “Hedonic Prices and Implicit Markets: Product Differentiation in Pure Competition”. Journal of Political Economy 82.1, pp. 34–55.
- Rubinfeld, Daniel L. (1987). “Chapter 11 The Economics of the Local Public Sector”. Handbook of Public Economics. Vol. 2. Elsevier, pp. 571–645.
- Sathish, C. (Dec. 2019). “Representation of Minorities in Panchayati Raj Institutions in Rural Tamil Nadu: The Influence of Caste, Class, Religion and Gender”. Sociological Bulletin 68.3, pp. 357–378.
- Stiglitz, Joseph E. (1977). “The Theory of Local Public Goods”. The Economics of Public Services. Ed. by Martin S. Feldstein and Robert P. Inman. London: Palgrave Macmillan UK, pp. 274–333.
- Tiebout, Charles M. (1956). “A Pure Theory of Local Expenditures”. Journal of Political Economy 64.5, pp. 416–424.
- Tricaud, Clémence (2021). Better Alone? Evidence on the Costs of Intermunicipal Cooperation. CESifo Working Paper Series 9000. CESifo.
- Volovitch-Tavares, Marie-Christine (2006). “L’immigration des Portugais en France, une histoire de paradoxes et de contrastes”. Exils et migrations ibériques au XXe siècle 3.2, pp. 57–102.

- Washington, Ebonya (2006). “How Black Candidates Affect Voter Turnout”. Quarterly Journal of Economics.
- Waylen, Georgina (July 2015). “Engendering the ‘Crisis of Democracy’: Institutions, Representation and Participation”. Government and Opposition 50.3, pp. 495–520.
- “UNWTO World Tourism Barometer and Statistical Annex, December 2020” (2020). UNWTO World Tourism Barometer 18.7. Ed. by World Tourism Organization (UNWTO), pp. 1–36.