DEPARTMENT
OF ECONOMICS

Essays in Political Economy, Migration, and Public Economics

Nikolaj Broberg

Thesis submitted for assessment with a view to obtaining the degree of Doctor of Economics of the European University Institute

Florence, 19 May 2022

European University Institute
Department of Economics

Essays in Political Economy, Migration, and Public Economics

Nikolaj Broberg

Thesis submitted for assessment with a view to
obtaining the degree of Doctor of Economics
of the European University Institute

Examining Board

Prof. Andrea Ichino, EUI, Supervisor
Prof. Andrea Mattozzi, University of Bologna and EUI, Co-Supervisor
Prof. Frederico Finan, University of California, Berkeley
Prof. Ekaterina Zhuravskaya, Paris School of Economics and EHESS

Researcher declaration to accompany the submission of written work
Department Economics - Doctoral Programme

I Nikolaj Broberg, certify that I am the author of the work “Essays in Political Economy, Migration, and Public Economics” I have presented for examination for the Ph.D. at the European University Institute. I also certify that this is solely my own original work, other than where I have clearly indicated, in this declaration and in the thesis, that it is the work of others.

I warrant that I have obtained all the permissions required for using any material from other copyrighted publications.

I certify that this work complies with the Code of Ethics in Academic Research issued by the European University Institute (IUE 332/2/10 (CA 297).

The copyright of this work rests with its author. Quotation from it is permitted, provided that full acknowledgement is made. This work may not be reproduced without my prior written consent. This authorisation does not, to the best of my knowledge, infringe the rights of any third party.

I declare that this work consists of 53,347 words.

I confirm that chapter 1 was jointly co-authored with Vincent Pons and Clémence Tricaud and I contributed 33% of the work.

I confirm that chapter 2 was jointly co-authored with Lars Ludolph and I contributed 50% of the work.

I confirm that chapter 4 was jointly co-authored with Pietro Panizza and I contributed 50% of the work.

Signature and date:

Wednesday 4th May 2022

Nikolaj Broberg

Abstract

This thesis in four chapters focuses on political economy, migration, and public economics.

The first chapter, joint with Vincent Pons and Clémence Tricaud, investigates the effects of campaign finance rules on electoral outcomes. In French departmental and municipal elections, candidates competing in districts above 9,000 inhabitants face spending limits and are eligible for public reimbursement. Using an RDD around the population threshold, we find that these rules increase competitiveness and benefit the runner-up of the previous race as well as new candidates, in *departmental* elections, while leaving the polarization and representativeness of the results unaffected. These results appear to be driven by the reimbursement of campaign expenditures, not spending limits. We do not find such effects in *municipal* elections, which we attribute to the use of a proportional list system instead of plurality voting.

The second chapter, joint with Lars Ludolph, analyzes the effects of the migration wave from Central and Eastern European countries (AC-12) following their EU accession in 2004 on local level redistribution in England. We apply a difference-in-differences estimation strategy and find that greater migration flows led to spending on means-tested social care services to decrease in relative terms, while spending on education services increased. Our mechanisms suggest that, because of AC-12 migrants' young age at the time of arrival, the 2004 EU enlargement alleviated some of the pressure faced by social care spending in England. We find no evidence that spending shifts are driven by a change in the local willingness to redistribute income.

The third chapter investigates the effect of ideological distance between EU Commissioners for Agriculture and Regional Policy and heads of governments on the allocation of agricultural and regional funds flowing to member states. Results show that ideological distance is a strong deterrent of funds being channeled. The effects are strongest in pre-election years, for countries providing the Commissioners in charge of the given portfolios, and for countries that are single-party-ruled as opposed to coalition-ruled. These results provide first hand evidence that the behavior of European Commissioners follows similar principles to national level elected politicians and can help the debate surrounding EU reforms and the political independence of its executive body.

The fourth chapter, joint with Pietro Panizza, exploits a reform in Italy that granted mayors the right to run for a third consecutive term in towns below 3,000 inhabitants. We employ a difference-in-discontinuity design and find evidence of pandering effects by mayors in both their first and second term

at the time of the reform. Results differ depending on the term of the mayor reflecting the importance of the horizon of when mayors' spending decisions pay off. We also find suggestive evidence of potential capture of first term mayors in the south of Italy.

# Acknowledgements

Writing a PhD at the EUI has been an incredible experience, where I have learnt many important academic and life lessons. This chapter of my life would not have been possible without the intellectual and emotional support of many different people – both in Florence and elsewhere.

Firstly, I am extremely grateful to my advisor Andrea Ichino for his guidance and genuine support throughout this journey. Andrea has brought a great deal of clarity to my work and many of my ideas would never have made the final version of this thesis without his encouragements, his patience, and his strong ability to detect technical hurdles, as well as tease out important questions for me to consider and answer.

I would also like to thank my second supervisor, Andrea Mattozzi, for his availability, his sharp intellect, his calmness, and solid feedback. Our discussions were always very fruitful and helped me see my ideas more clearly when I felt lost.

I must also add that both Andreas are two of the best teachers I have ever had, whose teaching styles and methods I will hopefully try to bring to my own students one day.

Similarly, I am very thankful to the numerous teaching assistants for their dedication, help, and guidance during my first two year at the EUI. I am particularly thankful to Julie Pinole, who on top of top of being such an excellent mathematics TA, strongly recommended I accept the EUI's offer after we had met just before my EUI interview. I also wish to thank other former and current faculty members from the Economics Department, especially Professors Michèle Belot and Juan Dolado, who always kept their doors open whenever I needed advice.

Beyond the EUI Economics Faculty, my profound gratitude goes to Professor Elias Dinas from the SPS department. Beyond our common passion for running and our short collaboration working on my first ever published article, together with Vicente Valentim and Mark Franklin, Elias has always been very kind and available to discuss research ideas with me. I have learnt a lot on the intersections between economics and political science through our exchanges.

Moreover, I would like to express my special gratitude to my jury members Ekaterina Zhuravskaya and Frederico Finan for their availability and for their extremely detailed and valuable comments on my thesis. Their suggestions will without doubt contribute to turning each chapter of my thesis into much improved articles.

I also wish to extend my gratitude to the many students, post-docs, and professors I met and learnt from, whether virtually, at conferences, seminars, during office hours, or simply over lunch, a coffee or a glass of wine. Special mention goes to the members of the EUI Micro-Econometric Working Group, the Levine/ Mattozzi Reading Group, and the Migration Working Group, whose feedback and discussions I have greatly benefitted from.

Three of the chapters in this thesis have been written with co-authors, to whom I owe a huge debt of gratitude.

I feel extremely privileged to have worked with Clémence Tricaud and Vincent Pons, two brilliant scholars, on the first chapter of this thesis. I met Vincent virtually a year before starting my PhD and am very grateful for the advice he gave me on my choice of PhD programme, for being such a great source of inspiration, and for introducing me to Clémence. I have learnt tremendously from working with them and am extremely thankful for their professionalism, patience, advice, support, mentorship, and extremely sharp contributions. I could not have asked for a better learning experience and challenge for a first co-authored project.

The second chapter of this thesis is co-authored with Lars Ludolph. Lars has been a great friend since both of us met during our traineeship at the European Commission and has also been a fantastic co-author since both of us started our PhDs. I feel extremely indebted to Lars' tireless work and many hours of Zoom calls exchanging thoughts on our paper, alongside navigating the challenges of doing a PhD whether at the EUI or LSE.

The fourth and final chapter of this thesis is co-authored with Pietro Panizza, a fellow student from the EUI Economics Department. The collaboration with Pietro started briefly after the outbreak of the COVID-19 pandemic, and more generally speaking during a moment of doubt regarding the direction of my thesis. I would like to extend my deep appreciation to Pietro for contributing to giving my thesis a breath of fresh air, for sharing his innovative ideas with me, for the hours he spent on the phone talking with different Italian statistics authorities and ministries, and more generally, for the time spent together working on this project. I am very lucky to have him as a co-author and now as a friend.

I would also like to thank my supervisors and co-authors, not only for their tremendous efforts to help me become a better researcher, but also for being so understanding of the personal challenges I went through at times during the writing of this thesis.

Writing a PhD thesis can be a very arduous undertaking at times and I would never have made it without the many exceptional friends I have made during my time in Florence starting with the 2016 EUI cohort. Bernhard, Chiara, Christina, Fée, Flavia, Henning, Henrike, Iseabail, Mustafa, Ola, Théo F, and many more... a truly excellent batch. Another thought goes to Alina, Alex, Andrius, Camilla, Caro, Christy, Clara, Jeanne, Jérôme, Johannes, Lazar, Leila, Lisa, Lois, Mar, Marie, Naïs, Puot, Sophie, Thomas T, and Thomas Walsh who I had the chance to meet at later stages of this adventure.

Special mention goes to my classmates Max Brès-Mariolle, Sebastian Rast, and Žymantas Budrys. I probably would not have passed the first-year exams and embarked on the full journey without the "pink of the elephant" study (and after-study) team that we formed.

Seba, it seems that our paths will separate after almost eight years of following each other from UPF in Barcelona, to working for MSY in Frankfut, and finally the PhD in Florence. I wish you all the best for the future and will make sure I look for jobs in Amsterdam... or simply come visit for some cycling on some flat surface for once! Thank you for having been such a great colleague and friend throughout this journey. I will dearly miss both your kindness and calmness, and also want to thank you for sharing your knowledge and wisdom over the years.

Thank you Max for having been such a fantastic classmate and partner in crime in both our academic and non-academic ventures during the first few years of the PhD. No longer being able to bother our fellow PhD friends with discussions on French politics and the Rugby Six Nations since you left has definitely left a void but I am mostly very glad to hear you, Christine, and your expanding family are doing so well in Stockholm. I am very grateful to count you as a friend and look forward to our reunions in the future.

Élise, thank you so much for offering me that spare room on Via Ghibellina. Accepting your pre-condition that we prepare the Florence marathon together is probably one of the best decisions I have made while in Florence. Not only did we improve our physical health, the running itself and the group of friends we formed with Eleni and Thibault have been, and still are, a great source of mental relief. The other flatmates you introduced me to were equally amazing and I am extremely blessed to have spent the first half of the pandemic in such great company. I will never forget the months of March, April, and May 2020 and thank Alessandra, Cecilia, Eleni, Giuliana, and Jaakko for having been such a great family during these difficult and surreal times.

Similarly, I also want to thank my second family away from home during the second wave of the pandemic after I took over Chiara's room on Via Pietrapiana. These were particularly challenging times personally, and I cannot imagine what this year would have been like without the kindness, patience, and emotional support from Chrisitna M, Morgan, Risto, Stella, and Théo B. I cannot wait for our upcoming reunions so that we can all relive Risto spontaneous Friday night aperitivo nights along with Ale, Henning, Jaakko, Margarita, and Seba.

I also want to express my deepest gratitude to my friends from outside the Florence "bubble" who never let me down throughout this long journey. Whether through Whatsapp messages, phone calls, or weekend reunions and holidays, I am so happy we managed to keep in touch despite living in different parts of the world. Special thanks to Adam Mazrani who I cannot thank enough for his emotional support in so many aspects of life. I am also heavily indebted to Danny Walker for being such a great friend and source of intellectual stimulation. Special thanks also to Afrola, Alban, Ana, Andreas, Andrew, Benoit, Daniele, Dimitria, Gilles, JB, Jorge, Justus, Karun, Marcelo, Milena, Numa, Samuel, Surabhi, Swamit, Tamara, Thomas G, Thomas Wood, and Tom. You have all played a great part in this journey.

I also owe a big thank you to Alassane Dumeste-Martin and Linda Gilbert from the 4B for the outstanding support and assistance they give in helping students organise activities beyond the academic curriculum of the EUI. Their efforts have contributed to the great number of races I participated in around Tuscany and more generally the kilometres I ran during my time at the EUI. Special thanks also to Maria who greatly contributed to creating the EUI "Wild Boars" basketball team and to my other fellow "cinghiali" team mates for the many fun training sessions and games played against (and *finally* trying to beat) local Florentine and US university teams. Furthermore, I want to thank all the current and former members of the running group who have been a great source of motivation. I would also like to express my profound gratitude to Anna Choub and Elisabetta Miglietta who helped me a great deal during the final years of the PhD.

Additionally, I would like to thank Anne Banks, Cécile Brière, Ciara Burbridge, Julia Hiltrop, Julia Valerio, Lucia Vigna, Martina Zucca, Rosella Corridori, Sarah Simonsen, Antonella, Cinzia, Giovanni, Loredana, Maurizio, Natasha, and the library staff for making life at the EUI so pleasant that we can focus on our research.

Last but not least, my most special thank you goes to my family who have been unwavering in their love, encouragement, and faith in me. I dedicate this thesis to all of you. The happy and hard times have always been by your side and I know we can always count on each other. I thank my younger sisters Emma, Katharine, Gabrielle, and Rita – you all make me extremely proud and are incredible sources of strength and happiness. Thank you Dimitri, Éva, and Vincent for always believing in me and your support. Thank you, Auntie Nina, for always being such a great source of energy and fun, and for reminding of my wonderful childhood and teenagehood summers spent visiting Farmor and Farfar in Denmark. I will always remember Farmor as a very strong woman, as well as a truly loving and caring grand-mother who taught me to stand up for myself. Je remercie également mes deux cousines Alice et Jessica, mon cousin Antoine avec qui je partage tant, et mes oncles François et Frédéric.

J'ai une pensée particulière pour ma Mamounette chérie – j'ai une chance unique de t'avoir pour grand-mère et me souviendrai toujours de cet amour très puissant que tu me portes. Je te suis très

reconnaissant de m'avoir transmis les histoires de ta famille et de ta jeunesse si difficile - je ne les oublierai jamais et m'efforcerai de les transmettre. Je te remercie aussi pour toutes ces vacances passées ensbemble à la montagne ou au bord de la mer, pour tes appels réguliers sur Messenger et pour tous ces bons moments passés ensemble à Paris. Les souvenirs de notre fabuleux voyage de 2019 à la découverte de tes racines resteront gravés en moi pour toujours. Impossible aussi de ne pas avoir une pensée émue pour Papa Claude qui nous a quitté l'année dernière et dont le sens de l'humour, la gentillesse et la bonté me manquent tellement.

Finally, I wish to thank the two people who made this possible thanks to their efforts and unconditional love since the day I was born: my mum and my dad. Far, I want to thank you for teaching me to always think critically, to never give up, and for all the efforts you make daily to always be there for us all. Maman, tu as fait tous les sacrifices nécessaires pour mon bonheur. Ton courage, ton empathie et ta générosité font de toi une très grande femme et une énorme source d'inspiration. Tu as toujours été la première personne à me soutenir dans les moments difficiles et je t'en suis infiniment reconnaissant. Je tâcherai de toujours être là pour toi.

Grazie a tutti!

Nikolaj

Rome

May 2022

Contents

1

The Impact of Campaign Finance Rules on Candidate Selection and Electoral Outcomes: Evidence from France

Joint with Vincent Pons and Clémence Tricaud

Abstract

[1] This paper investigates the effects of campaign finance rules on electoral outcomes. In French departmental and municipal elections, candidates competing in districts above 9,000 inhabitants face spending limits and are eligible for public reimbursement if they obtain more than five percent of the votes. Using an RDD around the population threshold, we find that these rules increase competitiveness and benefit the runner-up of the previous race as well as new candidates, in *departmental* elections, while leaving the polarization and representativeness of the results unaffected. Incumbents are less likely to get reelected because they are less likely to run and obtain a lower vote share, conditional on running. These results appear to be driven by the reimbursement of campaign expenditures, not spending limits. We do not find such effects in *municipal* elections, which we attribute to the use of a proportional list system instead of plurality voting.

[1]We thank Michèle Belot, Max Brès-Mariolle, Elias Dinas, Alexander Fouirnaies, Abel François, Andrea Ichino, Andrea Mattozzi, Pierre-Guillaume Méon, Pietro Panizza, James Snyder, and seminar participants at the EUI economics May forum, APSA, the Economics and Politics conference in Brussels, and the EUI micro-econometrics working group for their helpful comments and suggestions. We thank Sebastian Calonico, Matias Cattaneo, Max Farrell, and Rocio Titiunik for guiding us through the use of their RDD Stata package "rdrobust" and for sharing their upgrades; Julia Cagé and Laurent Bach for sharing their data on the 2001 municipal elections; Frédérique Dooghe for sharing the CNCCFPC data on campaign expenditures; Brigitte Hazart and Damien Aliaga at the Ministry of the Interior for addressing our questions on population data; and Erik Zolotoukhine and Lorraine Adam from the réseau Quételet for providing data on cantons' population. We are grateful to Salomé Drouard, Eric Dubois, and Thomas Taylor de Timberley, who provided outstanding research assistance.

1.1 Introduction

Policies regulating the influence of money in politics often generate heated debates. Advocates of limited regulation see campaign contributions as a form of political expression and campaign expenditures as an opportunity for candidates to inform voters about their platform. Differences in money raised and spent across competitors may not only be acceptable but even desirable if they help signal their relative quality to the public (e.g., Bailey, 2004). In contrast, supporters of stronger regulation argue that the unregulated use of campaign money can lead to a wasteful arms' race and facilitate the capture of the democratic process by wealthy individuals and interest groups (e.g., Bailey, 2004; Grossman and Helpman, 1992, 2001). They highlight the importance of levelling the playing field for outsider candidates who may not be able to access the same resources as incumbents even if they are of high quality (e.g., Stratmann, 2005).

Despite its importance, much of this debate is framed around principles and anecdotes rather than sound empirical evidence (e.g., Scarrow, 2007). Indeed, while most countries with political pluralism have adopted some form of campaign finance regulation (OECD, 2016), these rules are generally rolled out at the same time throughout the entire territory, rendering their evaluation difficult. A handful of recent papers exploit local variation to estimate the impact of limits to individual campaign contributions and to total campaign expenditures (Avis et al., 2022; Fouirnaies, 2021; Gulzar et al., 2021). However, we lack empirical evidence on rules which go one step further and provide for the reimbursement of campaign expenditures by the state. While such rules have a clear cost, they might further increase the equality of resources across candidates and could therefore be even more impactful than spending limits.

In this paper, we take advantage of reforms implemented in France in the early 1990s to fill this gap and estimate the effects of campaign finance rules on candidate selection and electoral outcomes. Since 1995, all candidates competing in departmental and municipal elections of districts with a population *above 9,000 inhabitants* are subject to a spending ceiling and they are eligible for the reimbursement of their expenditures up to 50 percent of the ceiling if they obtain more than five percent of the votes. Beyond France, rules combining spending limits and reimbursement exist in other countries including Ireland, South Korea, Portugal, Canada, Italy, and the U.S. Importantly for our empirical strategy, in France, campaign expenditures of candidates running in districts *below the 9,000 inhabitants threshold* are neither capped nor reimbursed. We use a Regression Discontinuity Design (RDD) to compare districts located just above the population discontinuity and just below. Differences in electoral results can be attributed to the difference in campaign finance rules since no other regulation changes at this threshold.

The impact of the legislation varies greatly across elections. While we observe strong effects in departmental elections, which use two-round plurality voting in single-member constituencies, we do not find any significant effect in municipal elections, which use a two-round list system with proportional representation.

In *departmental elections*, spending limits and the reimbursement of campaign expenditures do not affect the total number of candidates but they make elections more competitive: the odds that any candidate obtains a majority of votes and wins the election in the first round decrease by 10.9 percentage points. Most importantly, incumbents experience a sharp decline in their reelection rate at the benefit of the runner-up in the previous election and of candidates who were not present in that election. The reimbursement and capping of campaign expenditures causes a reduction in the incumbent's reelection probability by 14.5 percentage points, an increase in the previous runner-up's chances of winning by 5.2 percentage points, and an increase in the likelihood of a victory by a candidate absent from the previous election by 9.2 percentage points.

While the effect on victory by candidates absent from the previous election does not result from an increased number of new entrants, the effects on the likelihood of a victory by the incumbent or the previous runner-up can be decomposed into two parts. First, the treatment reduces the probability that the incumbent runs for reelection by 7.4 percentage points and it increases the likelihood that their challenger in the previous election runs again by 8.4 percentage points. Second, we derive bounds to estimate effects on candidates' chances of winning *conditional on running*. Similarly as the effects on running, conditional effects on winning are negative for the incumbent (between -10.5 and -18.9 percentage points) but positive for the runner-up (between 11.0 and 19.8 percentage points).

In theory, both the reimbursement of candidates' expenditures and spending limits could contribute to levelling the playing field and increase the likelihood of electoral turnovers. We exploit the 1992 and 1994 departmental elections to disentangle the influence of these two dimensions. Unlike the elections in our main sample, these elections were held after the spending ceiling was introduced (above the population threshold), in 1990, but before campaign expenditures started to be reimbursed, in 1995. We do not find any effect in this secondary sample of elections, suggesting that our main effects are driven by the reimbursement of candidates more than expenditure ceilings. Data on candidates' contributions and expenditures above the threshold bring further support for this interpretation. After the 1995 reform, we observe a disproportional increase in the personal contributions and the spending to ceiling ratio for the competitors of the incumbent, who also benefit electorally from the reform. By contrast, spending limits are binding for only a few candidates and they do not become more binding over time: bunching at the ceiling is modest, both before and after 1995.

Finally, the public reimbursement of candidates does not affect the polarization of elections or the representativeness of the winner's orientation, but it increases the probability of a change in the winning orientation and the probability that a candidate from the left is elected. This effect is consistent with the fact that left-wing candidates stand to gain the most from the reimbursement of campaign expenditures since they receive less than half the amount of private donations received by right-wing candidates and contribute less of their own money to their campaign beforehand. After the reform, their expenditure to ceiling ratio increases dramatically relative to candidates on the right.

In contrast to departmental elections, in *municipal elections*, we do not find any significant difference between municipalities immediately to the left and to the right of the campaign finance threshold. To understand this result, we note that mayoral candidates can ask other members of their list to contribute time and money to the campaign, so receiving public funding may make less of a difference for them and have less equalizing power than for candidates in departmental elections. In addition, we provide suggestive evidence that the negative impact of campaign finance rules on incumbents' likelihood to run for reelection, in departmental elections, results in part from political parties asking some incumbents to drop out. Incumbent mayoral candidates may be better able to withstand such pressure because they can invite possible rivals to join their list and they know that they will most likely obtain a seat on the municipal council themselves even if they fail to be reelected as mayor. In other words, the different results we obtain in departmental and municipal elections likely reflect important differences between the single-member constituencies characterizing the former and the list format used in the latter.

1.1.1 Contribution to the literature

We first build on a large theoretical literature studying the relationship between money and politics (see Stratmann, 2005 for a review). Two distinct tradeoffs investigated by theoretical models are directly relevant for campaign finance regulation. First, differences in the amount of money spent by candidates can signal differences in quality, if higher-quality candidates are able to raise more money (Coate, 2004a; Ortin et al., 2000; Prat, 2002), but they may also reflect differences in access to donors that are orthogonal to quality. Spending limits may benefit high quality challengers, if incumbents have easier access to campaign money irrespective of their quality (Iaryczower and Mattozzi, 2012; Pastine and Pastine, 2012), or increase incumbency advantage, if incumbents have non-pecuniary resources which challengers can only hope to overcome by outspending them (Sahuguet and Persico, 2006). The reimbursement of campaign expenditures exacerbates this tension. It decreases imbalances in candidates' access to money but decreases high quality candidates' ability to signal their quality by spending more (Ashworth, 2006; Prat et al., 2010).[2] Our results indicate that, on net, campaign finance regulations do level the playing field and decrease the incumbency advantage.

A second tradeoff relates to the representativeness of elected officials and their policies. On one hand, campaign money funds outreach efforts which educate voters about candidates' policy positions, contributing to the democratic ideal of an informed electorate and increasing the likelihood that the winner's policies are aligned with the preferences of the majority (e.g., Austen-Smith, 1987; Hinich et al., 1989; Prat, 2002). On the other hand, private donors may seek to extract favors in exchange for their contributions, which could instead create a wedge between enacted policies and public interest (e.g., Baron, 1994; Coate, 2004b; Grossman and Helpman, 1996). Limits on individual contributions and on total candidate spending can alleviate the risk of such capture but they also reduce the intensity of campaign communication. While the reimbursement of campaign expenditures by the state generates an obvious burden for the public budget, it can in principle help mitigate this tradeoff (Coate, 2004a). Indeed, we do not find any negative effect on winners' representativeness.

Empirically, we contribute to a burgeoning literature using quasi-experimental evidence to estimate the effects of campaign finance rules. Avis et al. (2022) and Fouirnaies (2021) find that limits on overall spending tend to increase competitiveness and reduce incumbency advantage, and Gulzar et al. (2021) show that looser individual contribution limits increase the number of public contracts assigned to donors of the elected candidate. Existing evidence about the effects of campaign expenditures's reimbursement is much less solid. Malhotra (2008) and Masket and Miller (2015) exploit the fact that some states in the U.S. offer the possibility for candidates to receive public funding in exchange for respecting pre-set spending limits. They find that districts where candidates accept the state's money experience higher competitiveness and lower incumbency advantage as well as more moderate policy outcomes, as a result of not relying on ideological donors. However, candidates who choose public funding may differ from those funded privately on other dimensions, which may bias the comparison between them. Our RDD is insulated from such endogeneity issues. It draws on other studies using RDDs around population thresholds to estimate the impact of other electoral rules and policies (e.g., Bordignon et al., 2016; Corbi et al., 2019; Eggers et al., 2018; Gadenne, 2017).

[2] While we focus on the public funding of individual candidates, a separate literature studies the public funding of national parties, based for instance on their past vote shares. Katz and Mair (1994)'s theory on the cartelization of politics argues that systems of party financing are designed by elected party legislators to prevent the entry of new parties. Interestingly, by facilitating the entry of new candidates, the public funding of individual candidates may work *against* the cartelization of politics (see e.g., Dinas and Foos, 2017; Katz and Mair, 2009).

Beyond studies on campaign finance regulation, our paper also contributes to the broader literature measuring the impact of campaign money on vote shares. Indeed, the differences in campaign finance rules above and below the 9,000 inhabitants threshold generate exogenous variation in the amount of money spent by different types of candidates. Campaign spending limits and reimbursement may advantage challengers if they increase their spending relative to incumbents and if any additional money they spend translates into a larger increase in vote shares. In the U.S., effects of campaign expenditures on vote shares have been found to be modest overall, but larger for challengers than incumbents (Abramowitz, 1988; Jacobson, 1978; Palda and Palda, 1998).[3] However, these results may not apply to our setting since the amount of money spent in French local elections is lower than in the U.S., and campaign money may have decreasing marginal returns. Furthermore, public money (specifically, expenditures that will be reimbursed by the state) may have different effects than private money, which can signal quality but also foreshadow policy bias towards donors' requests. We do find that challengers benefit from the rules prevalent above the threshold, but the effects on the identity of the winner are only present in departmental elections, where campaign expenditures are lower on average. This result is consistent with the possibility that effects of relative spending decrease with the total amount of money spent.[4]

While most of the literature focuses on the distinction between challengers and incumbents, differences across orientations may be even more important. Because left-wing candidates tend to rely less on private donations (Bekkouche et al., 2022), they stand to benefit more from public funding than candidates on the right. Our results confirm this prediction. We cannot measure downstream effects on policymaking but expect them to be important, given evidence that elected officials on the left and on the right implement different policies (Pettersson-Lidbom, 2008, but see Ferreira and Gyourko, 2009) and that electoral turnovers impact performance (Akhtari et al., 2022; Marx et al., 2022).

The remainder of the paper is structured as follows. Section 3.2 introduces our research setting, and Section 3.3 describes our empirical strategy. Sections 1.4 and 1.5 provide the main results for departmental elections and municipal elections, respectively. Section 3.5 discusses the mechanisms at play, and Section 3.6 concludes.

[3] For recent papers measuring the effect of campaign spending on vote shares outside the U.S., see for instance Ben-Bassat et al. (2015), François et al. (2022), and Bekkouche et al. (2022).

[4] Section 1.6.1 discusses the difference between the effects found in municipal and departmental elections at greater length. Regardless of the exact interpretation, these results complement the vast literature studying the impact of differences across voting systems (Bordignon et al., 2016; Eggers, 2015; Myerson and Weber, 1993).

1.2 Research setting

1.2.1 Campaign finance rules in France

Many Western democracies started regulating campaign finance in the 1960s (Alexander and Federman, 1989), hoping to limit the influence of money in politics and to increase the transparency and fairness of the election process (see e.g., Gunlicks, 2019 and The Law Library of Congress, 2009). France did not regulate campaign finance until the late 1980s, prompted by rising amounts of campaign money and numerous scandals uncovering the widespread illegal funding of parties. A series of reforms regulating campaign spending, campaign contributions, and other aspects of political campaigns were adopted from 1988 to 1995. France now has a stable and relatively strict system of campaign finance legislation.

For the sake of brevity and clarity, we focus on the aspects of the French reforms that are relevant to our analysis. Democracies can level the playing field by limiting campaign expenditures or by providing for their reimbursement by the state. France, similarly as other countries including Ireland, South Korea, Portugal, Canada, Italy, and, to some extent, the U.S., does both. In the U.S., presidential election candidates and candidates for state offices in fourteen states face an opt-in system. To receive public funding, they need to respect a spending cap; those who go over this cap become ineligible for public funding.[5] The policy in France and in the other aforementioned countries is more binding. In elections where public reimbursement of expenditures and spending limits apply, complying with them is not at candidates' discretion.

The first reform we exploit is a 1990 law, which introduced spending limits in departmental and municipal districts above 9,000 inhabitants. These limits depend on district size. Candidates must respect these limits, lest they become liable to serious sanctions, up to ten years of prison. Furthermore, all candidates running in districts above the population threshold must provide a detailed account of their expenditures and revenues to a dedicated commission, the CNCCFP.[6] Accordingly, we have comprehensive data on candidate spending above the threshold.

The second reform is a 1995 law which introduced the reimbursement of candidates' expenditures in the same set of districts, with population above 9,000 inhabitants.[7] Candidates running in these

[5] See https://www.fec.gov/introduction-campaign-finance/understanding-ways-support-federal-candidates/presidential-elections/public-funding-presidential-elections/ and https://www.ncsl.org/research/elections-and-campaigns/public-financing-of-campaigns-overview.aspx.

[6] This rule was modified in 2011 such that only candidates obtaining more than one percent of the votes have to submit this information.

[7] Before 1990, candidates had been reimbursed for official propaganda related costs, e.g., the printing of ballots, posters put up in front of polling stations, and manifestos sent to voters, all accounting for a very small share of campaign expenditures. After 1990, candidates remained eligible for the reimbursement of these specific

districts are eligible for the reimbursement of 50 percent of the spending limit,[8] provided they obtain more than five percent of the candidate votes (valid votes cast for a candidate as opposed to blank and null votes) in the first round. Candidates can only ask for the reimbursement of expenditures covered with their own money: expenditures covered by contributions from donors, political parties, etc. are not reimbursed. The 1995 reform also tightened the spending limits first introduced in 1990 to 70 percent of the previous level.

Districts below the population cutoff were not affected by the 1990 and 1995 reforms, such that candidates running in these districts face no spending limit and they are not eligible for reimbursement. We generally measure the combined impact of reimbursement and spending limits, since both vary at the 9,000 inhabitants threshold. We also separately study the 1992 and 1994 departmental elections, where candidates running above the threshold were only subject to the 1990 law, to isolate the effect of spending limits.

The French reforms which started in the late 1980s also changed rules affecting other aspects of elections, including TV and radio advertising (which were prohibited) and contribution limits (Cagé et al., 2021). However, these changes affected cantons and municipalities both above and below the 9,000 inhabitants threshold. Therefore, they do not contribute to the effects we measure at the discontinuity.

1.2.2 French departmental and municipal elections

Our sample includes two types of elections, characterized by different voting rules.

Departmental elections elect members of departmental councils, which exert responsibility over culture, local development, social assistance, education, housing, transportation, and tourism, and account for 7 percent of total public spending. France counts 101 départements divided in single-member constituencies, called cantons. Departmental elections follow a two-round plurality voting rule. In each canton, the top candidate wins the race in the first round if she receives more than 50 percent of the candidate votes, accounting for at least 25 percent of the registered citizens. If no majority is obtained in the first round, the top-two candidates and all other candidates above a certain vote share threshold qualify for the second round. The qualification threshold was 10 percent of registered citizens until 2011, and 12.5 percent afterwards. The second round takes place a week later and uses plurality voting: the candidate receiving the most votes is elected. There is no term limit. Until a 2013 reform, each canton elected one representative for a length of six years, and half of the seats were up for

expenditures provided they obtained more than five percent of the votes, both above and below the population threshold.

[8] The maximum reimbursement was reduced to 47.5 percent in 2011.

election every three years. There were a total of 4,035 cantons, with populations ranging from 270 to 69,335 inhabitants. The reform of 2013 aligned the calendar of all elections, it homogenized cantons' size within departments, cut the number of cantons in half, and led to the redistricting of all cantons' boundaries. Post reform, the population of 98 percent of the cantons was above the 9,000 inhabitants threshold. Therefore, we do not use departmental elections which took place after the reform.[9]

Municipal elections are held every six years and elect the mayor and other members of the municipal council in each of the 35,000 French municipalities, with populations ranging from a handful of inhabitants to 450,000. Around the 9,000 inhabitants threshold, municipal councils count 27 members (including the mayor), so competing lists include 27 candidates. Like in departmental elections, there is no term limit. Municipal councils have discretion over local urban services, municipal police, nurseries, primary schools, sports facilities, road maintenance, and urban public transportation. Their expenditures account for 11 percent of total public spending. We restrict our analysis to the sample of municipalities with more than 3,500 inhabitants because electoral rules differed significantly below this threshold until the 2014 elections. Despite a few municipality mergers, this represents a fairly stable sample of 2,500 to 3,000 municipalities per election year. In these municipalities, elections follow a two-round list system with proportional representation. If a list obtains the absolute majority in the first round, half of the seats are attributed to this list and the other seats are divided proportionally between all the lists which received more than five percent of the votes. If no majority is reached in the first round, the top-two lists and all lists above 10 percent qualify for the second round taking place a week later.[10] Lists with more than five percent of the votes in the first round can merge with lists qualified for the second round.[11]

Since municipal and departmental elections have different voting rules, we study them separately throughout the analysis. These two types of elections also have different electoral calendars (except for 2001 and 2008, when both types of elections coincided) and their districts do not overlap: multiple small municipalities are often included in the same canton and, conversely, large municipalities are generally split into multiple cantons. We find different effects of campaign finance rules in departmental and municipal elections, as shown in Sections 1.4 and 1.5, and interpret these differences in Section 3.5.

[9]The 2013 reform also changed the election format: instead of electing a single representative, each canton elects a ticket composed of a woman and man. Dealing with this additional change would further complicate the analysis, which is conducted at the individual candidate level for all other departmental elections.

[10]The spending limit is looser for lists qualified for the second round than those eliminated after the first round.

[11]This can lead to potential changes in the lists' composition, including the first candidate on each list, as well as changes in the lists' political orientation.

1.3 Empirical strategy

1.3.1 Evaluation framework

Measuring the impact of campaign finance rules is typically difficult as such rules are usually applied uniformly within countries and differences across countries or election types overlap with many other differences. We circumvent this difficulty by exploiting local variation in campaign finance rules in French departmental and municipal elections generated by the 1990 and 1995 reforms. In districts below 9,000 inhabitants, candidates are not reimbursed and they face no spending limits, while candidates running in districts above 9,000 inhabitants must respect spending limits and they are reimbursed provided they obtain more than five percent of the candidate votes in the first round.

Formally, we estimate the impact of these rules with a sharp regression discontinuity design. We use the following specification:

$$Y_{i,t} = \alpha + \tau D_{i,t} + \beta X_{i,t} + \gamma X_{i,t} D_{i,t} + \epsilon_{i,t}, \tag{1.1}$$

where $Y_{i,t}$ is the outcome in district i and election year t, $X_{i,t}$ is the running variable, defined as the district population centered around 9,000 inhabitants, and $D_{i,t}$ is the assignment variable, a dummy taking value one if $X_{i,t}$ is positive.

Following Imbens and Lemieux (2008) and Calonico et al. (2014a), we use a non-parametric estimation, which equates to fitting two linear regressions within a certain bandwidth on either side of the threshold.[12] We follow the optimal MSERD algorithm proposed by Calonico et al. (2019) to construct optimal data-driven bandwidths for each outcome. Applying Calonico et al. (2014a)'s estimation procedure, we obtain robust confidence interval estimators.

We cluster our standard errors $\varepsilon_{i,t}$ at the district level. This allows for the assignment to treatment to be correlated at the district level over time, which is particularly important for the 2008 elections. Indeed, in the majority of districts, population and therefore assignment to treatment remained identical between the 2001 and 2008 elections, since the population was based on the same census for both elections. We discuss the identification assumption required to interpret our estimates causally in Section 1.3.3.

[12] We also show the robustness of our main results to employing a quadratic specification by adding $X_{i,t}^2$ and its interaction with $D_{i,t}$ in equation 1.1 in Appendix Table C11; and to controlling for baseline sociodemographic variables in Appendix Table C12.

1.3.2 Data

Electoral results for all municipalities above 3,500 inhabitants and all cantons come from the Ministry of the Interior. For the 2001 municipal elections, these data aggregate results across candidates of the same political orientation. We obtained candidate-level data from Cagé (2020) and Bach (2012), and completed them by consulting and manually inputting results published in local newspapers present in French archives.

In each district, we pair election results across years to identify which candidates were present in the previous election (which we call "insider" candidates) and which ones were absent ("outsider" candidates).[13] Among insiders, we check whether the incumbent and the runner-up from the previous election (the "challenger") run again.

We exploit political labels attributed by the Ministry of the Interior and information obtained from the research center CEVIPOF to identify "non-party candidates," namely candidates who do not have any party labels. Within this group, we call candidates who cannot be placed on the left-right axis "non-classified." We classified candidates into five orientations, far-left, left, centre, right, and far-right, and place them on ParlGov's 0 to 10 left-right scale (Döring and Manow, 2012).

Importantly, our identification strategy requires to know the exact official population of each district at each election, in order to compute the running and assignment variables $X_{i,t}$ and $D_{i,t}$ accurately. Obtaining reliable population data proved more difficult than anticipated. It required combining and carefully cross-checking many data sources. Changes in the official population can occur following national censuses or out-of-census complementary decrees affecting small subsets of municipalities. Until 1999, national censuses took place every six to nine years. Complementary decrees could occur between censuses, when the population of a municipality had increased by at least 15 percent or following major redistrictings of cantons or municipalities (border changes, mergers, and demergers). Since 2008, yearly national censuses have been published based on the enumeration of one fifth of the territory each year. Our population data come from INSEE (the National Institute of Statistics and Economic Studies) for the national censuses; and from Légifrance (the official website used by the French government to publish new legislation, regulations, and legal information) as well as SIRIUS (IT Service of Interdisciplinary Urban and Spatial Research) for the complementary decrees.

Finally, we digitized booklets from the commission monitoring party and candidate expenditures (CNCCFP), reporting the expenditures and breakdown of contributions received by candidates running

[13] The pairing between the 1995 and 2001 municipal elections also required inputting results from local newspapers for all the 1995 municipal elections.

in all districts above 9,000 inhabitants.[14] These data do not exist for districts below the threshold, where candidates do not need to report their revenues and expenditures to the CNCCFP. While we cannot use our RDD to measure effects on these outcomes, we do provide evidence on the spending patterns of different types of candidates above the threshold and on the changes which followed the introduction of campaign expenditures' reimbursement.

1.3.3 Identification assumptions

The estimates obtained from equation 1.1 identify the local average treatment effect around the threshold conditional on assuming that potential outcomes are continuous at the 9,000 inhabitants threshold (e.g., Hahn et al., 2001; Imbens and Angrist, 1994). We are confident that this assumption is satisfied, first, because no other voting rule or institutional feature changes at this threshold,[15] and second, because districts cannot sort at the threshold. Indeed, the centralized nature of French censuses leaves no room for the manipulation of population figures by mayors or departmental councilors. Furthermore, mayors can only ask for their municipality's population to be updated, leading to a complementary decree, if there is evidence that the population increased by 15 percent at least. In that case, the new official population is established by an independent administrator, preventing the manipulation of the threshold.

We further provide empirical support for our identification assumption by conducting several manipulation tests. First, we check whether the likelihood of experiencing a redistricting between elections *t-1* and *t* or of having been treated at *t-1* jumps at the threshold. Such discontinuities could suggest that incumbents are able to manipulate their population to benefit from the campaign finance regime that they like the most. Fortunately, the results shown in Table 1.1 for both municipal and departmental elections show that this is not the case. Second, we provide a broader test of manipulation by checking if there is a jump in the density of the running variable at the threshold (Cattaneo et al., 2018, 2020; McCrary, 2008). Third, we conduct the following general balance test. We regress the treatment variable T on a set of sociodemographic variables including the age distribution, the share of men in the population, and the distribution of occupations at the district level. Then we use the coefficients from these regressions to predict the treatment status of each district and test whether this predicted treatment value jumps at the discontnuity. Fourth, we conduct balance tests on these census variables taken individually. Fifth, we check that outcomes defined at election *t-1* do not jump at the

[14]We did not digitize the booklets for the 2001 municipal elections, for which the data were only available for half of the candidates.

[15]See Eggers et al. (2018) for a list of policy changes affecting for instance the salary of the mayor or the number of municipal councilors at *other* population thresholds in French municipalities.

threshold either. The results of these tests are shown in Sections 1.4.1 and 1.5 for departmental elections and municipal elections, respectively.

Table 1.1 Changes since election *t-1* - Departmental and municipal elections

	(1)	(2)	(3)	(4)	(5)	(6)
	Departmental elections			Municipal elections		
Outcome	Redistricted	Treated in *t-1*	Linkable	Redistricted	Treated in *t-1*	Linkable
Treatment	0.007	0.052	-0.007	0.004	-0.044	-0.054
	(0.006)	(0.086)	(0.006)	(0.008)	(0.114)	(0.031)
Robust p-value	0.378	0.852	0.378	0.698	0.515	0.117
Observations	2,846	547	2,846	1,605	418	1,006
Polyn. order	1	1	1	1	1	1
Bandwidth	3,186	1,031	3,186	2,001	919	1,331
Mean, left of the threshold	0.000	0.364	1.000	0.004	0.413	0.978

Notes: Standard errors are in parentheses. Robust p-values are used to compute statistical significance. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. Each column reports the results from a separate local polynomial regression. The independent variable is a dummy equal to one if the district has a population above 9,000 inhabitants. Separate polynomials are fitted on each side of the threshold. The polynomial order is one in all columns and the bandwidths are derived under the MSERD procedure. The mean indicates the mean value of the outcome of interest at the cutoff below the discontinuity. We exclude the 2008 elections from the analysis for the outcome "Treated in *t-1*" in columns 2 and 5 since the same major census was in place for both the 2001 and 2008 elections.

1.3.4 Sampling frame

Our main sample includes the 2001, 2008, and 2014 municipal elections and the 1998, 2001, 2004, 2008, and 2011 departmental elections. We also use data from the 1995 municipal elections and the 1992 and 1994 departmental elections to define incumbents, challengers, and outsider candidates in the first elections in the sample (namely, the 2001 municipal elections and the 1998 and 2001 departmental elections).

Table 1.2 indicates the national census used to determine districts' official population, for each election in the sample. We use data from the 1990 and 1999 censuses (as well as complementary decrees which took place in between) to determine the official population for all elections until 2008. We use data from the 2008 and 2011 censuses for the 2011 departmental and 2014 municipal elections, respectively. Importantly, except for the 2008 municipal and departmental elections, each election was preceded by a different national census, leading to changes in all districts' official population.[16] Therefore, our estimates generally capture the impact of being treated once.

Table 1.2 Relevant censuses in place for each election in the main sample

Departmental elections	Relevant census	Municipal elections	Relevant census
1998	1990	2001; 2008	1999
2001; 2004; 2008	1999	2014	2011
2011	2008		

The 2008 municipal and departmental elections are exceptions: in most districts, the population and, therefore, the running and assignment variables, were the same as in the 2001 municipal and departmental elections, respectively. Therefore, we do not use the 2008 elections for the internal validity tests, as keeping them would double count districts where census variables and population figures do not evolve. We include the 2008 elections in all our other analyses but show the robustness of our results to excluding them in Appendices C and E.

We check the consistency of all election results, and drop one race in the 2001 departmental elections, for which we detect inconsistencies.[17] Furthermore, our main outcomes require linking

[16] The 2001 and 2004 departmental elections both used population figures from the 1999 census, but they took place in different sets of districts, since only half of the seats were up for election until the 2013 reform.

[17] We consider elections as problematic if a second round took place even though a candidate obtained a majority of votes and 25 percent of the registered citizens in the first round, or vice versa; if the number of registered voters, turnout, or the number of total candidate votes is missing (we exclude this test for the 1995 municipal elections, as many newspaper sources did not report this outcome); if a candidate appears in the second round even though their first round vote share was below the qualification threshold; or if the sum of individual candidate votes does not add up to the total number of candidate votes.

districts over time: for instance, we cannot define the incumbent, and, thus, we cannot measure effects on the likelihood that they are reelected, if the district is new. We define a district as linkable if it does not experience any major redistricting between elections in *t-1* and *t* and if there were no inconsistencies in the district's electoral results in election *t-1*.[18] In municipal elections before 2014, we further require that the district population was above 3,500 inhabitants both at *t-1* and *t*, so that the electoral rule was identical in both years.

Reassuringly, districts above the discontinuity are not more likely to be linkable with the last election than those below, as shown in Table 1.1. In Appendices C and E, we show the robustness of our results to including non-linkable districts in the sample for outcomes such as turnout or the probability of a candidate's victory in the first round, which can be constructed without linking elections over time.

Overall, our main sample includes 7,653 linkable municipal races (23,709 lists) and 9,938 linkable departmental races (52,651 candidates).[19] Table 1.3 gives summary statistics for both types of elections. In an average departmental race, 5.3 candidates compete in the first round, ten thousand voters are registered to vote, 63.6 percent of them vote and 60.8 percent cast a valid vote for one the candidates. Municipal elections appear less competitive: the number of candidates averages 3.1 and only 36.4 percent of races are decided in the second round, as compared with 68.6 percent for departmental elections. On the other hand, the average number of registered voters, turnout rate, and the share of elections won by the incumbent, challenger, or outsider candidates are very similar across both types of elections.

Beyond our main sample, we use the 1992 and 1994 departmental election results when exploring the mechanisms driving our results, in Section 3.5. These elections help us disentangle the contribution of spending limits and candidate expenditures' reimbursement since the former was implemented before these elections but the latter after.[20]

[18]Overall, we detect inconsistencies in the *t-1* election for one departmental race (corresponding to that 2001 race with inconsistencies) and for 185 races in the 2001 municipal elections (due to inconsistencies in the 1995 election results obtained from newspaper sources).

[19]When we add non-linkable elections, our sample includes 8,604 municipal races (26,164 lists) and 10,083 departmental races (53,600 candidates).

[20]We also use data from the 1985 and 1988 departmental elections to define incumbents, challengers, and outsider candidates in the 1992 and 1994 elections.

Table 1.3 Summary statistics

	Mean	S.D.	Min.	Max.	Observations
Panel A. Departmental elections					
Registered voters	10,010	6,920	289	48,783	9,938
Proportion of turnout	0.636	0.122	0.205	0.919	9,938
Proportion of candidate votes	0.608	0.115	0.197	0.894	9,938
Number of candidates	5.30	1.74	1	15	9,938
Number of female candidates	1.06	1.05	0	7	9,938
Number of non-party candidates	1.50	1.32	0	10	9,938
Number of non-classified candidates	0.23	0.53	0	5	9,938
Proportion of second rounds	0.686	0.464	0	1	9,938
Incumbent victory	0.578	0.494	0	1	9,938
Challenger victory	0.056	0.229	0	1	9,928
Outsider victory	0.348	0.477	0	1	9,938
Panel B. Municipal elections					
Registered voters	9,937	15,029	1,024	254,538	7,653
Proportion of turnout	0.640	0.078	0.330	1	7,653
Proportion of candidate votes	0.605	0.083	0.246	0.908	7,653
Number of candidates	3.10	1.52	1	12	7,653
Number of female candidates	0.53	0.78	0	7	7,653
Number of non-party candidates	1.74	1.22	0	9	7,653
Number of non-classified candidates	0.18	0.48	0	7	7,653
Proportion of second rounds	0.364	0.481	0	1	7,653
Incumbent victory	0.569	0.495	0	1	7,653
Challenger victory	0.065	0.246	0	1	7,219
Outsider victory	0.359	0.480	0	1	7,653

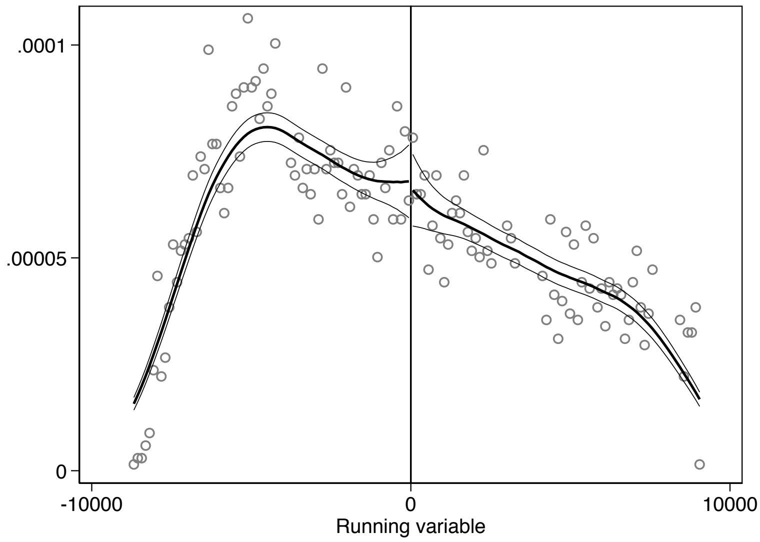
Notes: S.D refers to standard deviation, min. to minimum, and max. to maximum. The outcome “Challenger victory” is missing for districts where only one candidate ran in the previous election.

1.4 Effects in departmental elections

1.4.1 Validity checks

As discussed in Section 1.3.3, our RDD results can only be interpreted causally if districts do not sort across the 9,000 inhabitants cutoff. Figure 1.1 tests this assumption by checking that the density of the running variable does not jump at the threshold, in our main sample of departmental elections, using McCrary (2008)’s test. The Cattaneo et al. (2018) density plots shown in Appendix Figure B1 do not indicate any discontinuity at the threshold either, and the *p*-value of the manipulation test described in Cattaneo et al. (2018) is equal to 0.99. Adding non-linkable districts in the sample yields similar results.

Figure 1.1 McCrary (2008) density test - Main sample of departmental elections



Notes: We test for a jump at the threshold in the density of the running variable (the district population centered around 9,000 inhabitants), using McCrary (2008)'s method. The solid line represents the density of the running variable, while the thin lines represent the confidence intervals. We exclude the 2008 elections since in most districts, the running variable is the same as in 2001 (the same major census was in place for both elections).

Table 1.4 and Appendix Figure B2 show placebo effects on the main outcomes defined in the *previous* elections. None of them is statistically significant.

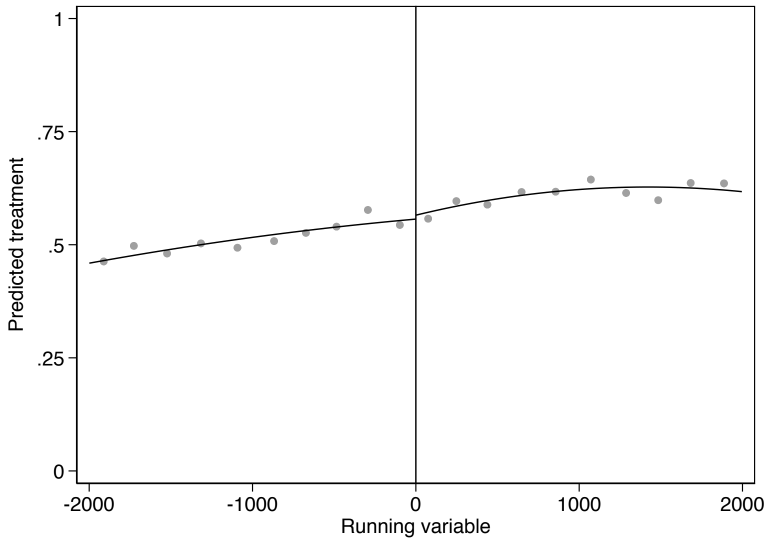
Table 1.4 Placebo tests, main outcomes defined in *t-1* - Main sample of departmental elections

Outcome	(1) Incumbent run	(2) Incumbent win	(3) Challenger run	(4) Challenger win	(5) Outsider run	(6) Outsider win	(7) Victory in first round
Treatment	0.058	0.063	-0.010	0.001	0.006	-0.042	-0.061
	(0.043)	(0.054)	(0.047)	(0.024)	(0.010)	(0.051)	(0.050)
Robust *p*-value	0.284	0.402	0.890	0.963	0.570	0.530	0.195
Observations	1,728	1,471	1,428	1,317	1,030	1,638	1,705
Polyn. order	1	1	1	1	1	1	1
Bandwidth	3,438	2,941	2,848	2,648	2,059	3,284	3,411
Mean, left of threshold	0.728	0.552	0.229	0.046	0.995	0.357	0.322

Notes: Standard errors are in parentheses. Robust *p*-values are used to compute statistical significance. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. Each column reports the results from a separate local polynomial regression. The independent variable is a dummy equal to one if the district has a population above 9,000 inhabitants in election *t*. Separate polynomials are fitted on each side of the threshold. The polynomial order is one in all columns and the bandwidths are derived under the MSERD procedure. The dependent variables refer to our main outcomes defined in election *t-1*. The mean indicates the mean value of the outcome of interest at the cutoff below the discontinuity. We exclude the 1998 (resp. 2008) elections since in most districts, the population and, therefore, the running and assignment variables, were the same as in the 1992 (resp. 2001) elections.

Figure 1.2 shows the lack of jump at the discontinuity when conducting the general balance test described in Section 1.3.3.

Figure 1.2 General balance test - Main sample of departmental elections



Notes: Each dot is the average of the outcome variable within a given bin of the running variable. The running variable (the district population centered around 9,000 inhabitants) is split into quantile-spaced bins. The continuous lines represent a quadratic fit. To facilitate visualization, the graph is truncated at 2,000 inhabitants around the cutoff. The outcome is the value of the treatment predicted by sociodemographic variables: the share of men in the population; the share of under 29 year olds, the share of the population between 30 and 44 years old; the share between 45 and 59 years old; the share above 60 years old; the share of the economically active within the population; the share of unemployed; the share of skilled jobs; the share of workers; the share of employee professions; the share of intermediary professions; the share of artisans; the share of actives working in agriculture. To avoid dropping observations, for each socioeconomic variable, we include a dummy equal to one when the variable is missing and replace by 0s. The independent variable is a dummy equal to one if the district has a population greater or equal to 9,000 in year t.

In this graph, each dot represents the average value of the outcome within a given bin of the running variable. We fit a quadratic polynomial on each side of the population threshold to facilitate visualization. Table 1.5 reports formal estimates obtained using our preferred specification and confirms the absence of a jump: The point estimate on the predicted treatment variable is low and not significant at conventional levels (p-value=0.370). Appendix Table B1 confirms the robustness of this result when studying the sample including non-linkable districts: Predicted treatment does not show evidence of a jump either in this case.

Table 1.5 General balance test - Main sample of departmental elections

	(1)
Outcome	Predicted treatment
Treatment	0.020
	(0.020)
Robust p-value	0.370
Observations	2,143
Polyn. order	1
Bandwidth	3,041
Mean, left of threshold	0.563

Notes: Standard errors are in parentheses. Robust p-values are used to compute statistical significance. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. Each column reports the results from a separate local polynomial regression. The independent variable is a dummy equal to one if the district has a population above 9,000 inhabitants in election t. The outcome is the value of the treatment predicted by sociodemographic variables: the share of men in the population; the share of under 29 year olds, the share of the population between 30 and 44 years old; the share between 45 and 59 years old; the share above 60 years old; the share of the economically active within the population; the share of unemployed; the share of skilled jobs; the share of workers; the share of employee professions; the share of intermediary professions; the share of artisans; the share of actives working in agriculture. To avoid dropping observations, for each socioeconomic variable, we include a dummy equal to one when the variable is missing and replace by 0s. The independent variable is a dummy equal to one if the district has a population greater or equal to 9,000 in year t. Separate polynomials are fitted on each side of the threshold. The polynomial order is one in all columns and the bandwidths are derived under the MSERD procedure. The mean indicates the mean value of the outcome of interest at the cutoff below the discontinuity. We exclude the 2008 elections since in most districts, the population and, therefore, the running and assignment variables, were the same as in the 2001 elections.

Furthermore, Appendix Tables B2 and B3 show balance tests on sociodemographic variables, for the main sample as well as the sample including non-linkable races (see Appendix Figure B3 for the corresponding graphs, for a subset of outcomes). Only one out of 13 variables, the share of 30 to 44 years old, is statistically significant (at the 5 percent level), which is in line with what would be expected and consistent with districts close to the left and to the right of the threshold having similar average characteristics.

Overall, we do not find any evidence that departmental election districts sort at the threshold, increasing our confidence in the reliability of our empirical strategy.

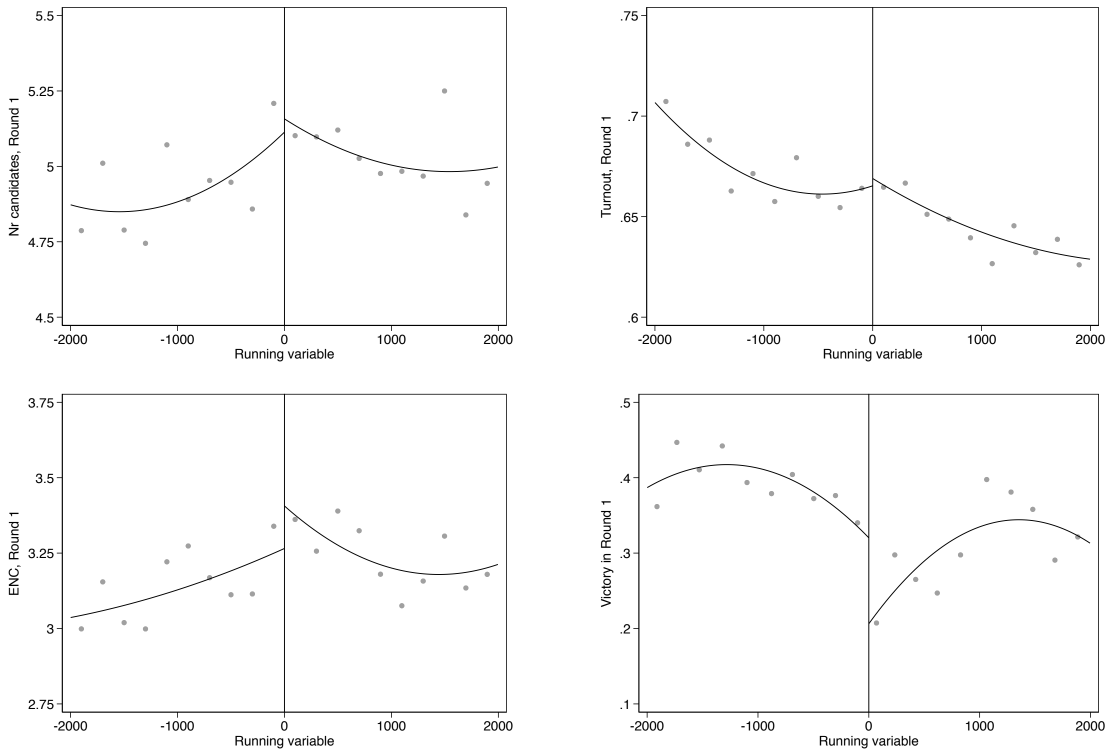
1.4.2 Effects on competition

Our first set of outcomes relate to the competitiveness of elections. We first estimate effects on electoral supply: the total number of candidates, the number of outsider candidates (who were not present in the previous race in the district), and the number of insider candidates (who were present). Outsider candidates might be more likely to run above the threshold, as they know that mainstream candidates face a spending limit and they can expect their own campaign expenditures to be reimbursed, conditional on getting five percent of the votes or more. However, in equilibrium, two forces may limit the number of candidates. First, insider candidates might respond to the increased competition by staying out of the race or striking alliances. Second, if the number of potential candidates is too high, smaller candidates may reason that they are unlikely to obtain the five percent vote share required to get reimbursed and decide to stay out.

Beyond effects on the number of candidates, the campaign finance rules that we evaluate may affect electoral competitiveness through a second channel: by increasing the amount of money spent by smaller candidates relative to established candidates. We measure election competitiveness using two indicators: the fragmentation of vote shares in the first round and, relatedly, the probability of any candidate winning in the first round. Our metric of fragmentation is the effective number of candidates as defined by Laakso and Taagepera (1979): $ENC = \frac{1}{\sum_1^n v_i^2}$, where n is the number of candidates and v_i the first round vote share of candidate i. We also estimate effects on voter turnout, which could increase due to higher competitiveness or to a larger and more diverse set of candidates.

We begin with a graphical analysis, in Figure 1.3, before providing formal estimates.

Figure 1.3 Impact on competition - Main sample of departmental elections



Notes: Each dot is the average of the outcome variable within a given bin of the running variable. The running variable (the district population centered around 9,000 inhabitants) is split into evenly-spaced bins for continuous outcomes and into quantile-spaced bins for binary outcomes. The continuous lines represent a quadratic fit. To facilitate visualization, the graph is truncated at 2,000 inhabitants around the cutoff.

While there is not any clear effect on the number of candidates, turnout, and the effective number of candidates, we observe a large negative jump of the probability of a victory in the first round at the cutoff. These results suggest that, although there is no overall increase in fragmentation, the campaign finance rules penalize front-runners, preventing any of them from winning in the first round.

Table 1.6 reports formal estimates obtained using our preferred specification. Consistent with the graphs, we find that campaign finance rules which apply above the threshold reduce the probability that the election is won in the first round by 10.9 percentage points (30.9 percent), which is significant at the 5 percent level. The point estimates for other outcomes are small and non-significant. These results are robust to excluding the 2008 elections (so that we measure the effect of being treated only once), and to including districts that cannot be linked over time, as shown in Appendix Tables C1 and C2.

Table 1.6 Impact on competition - Main sample of departmental elections

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Number of			Turnout	ENC	Victory
	Candidates	Outsiders	Insiders	r1	r1	in first round
Treatment	0.046	0.010	0.028	0.010	0.086	-0.109**
	(0.119)	(0.119)	(0.065)	(0.009)	(0.089)	(0.044)
Robust *p*-value	0.513	0.855	0.471	0.235	0.246	0.012
Observations	2,326	2,663	2,407	2,306	2,451	2,151
Polyn. order	1	1	1	1	1	1
Bandwidth	2,610	2,993	2,702	2,577	2,741	2,410
Mean, left of threshold	5.055	3.597	1.461	0.656	3.246	0.353

Notes: Standard errors are in parentheses. Robust *p*-values are used to compute statistical significance. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. Each column reports the results from a separate local polynomial regression. The independent variable is a dummy equal to one if the district has a population above 9,000 inhabitants in election *t*. Separate polynomials are fitted on each side of the threshold. The polynomial order is one in all columns and the bandwidths are derived under the MSERD procedure. The mean indicates the mean value of the outcome of interest at the cutoff below the discontinuity.

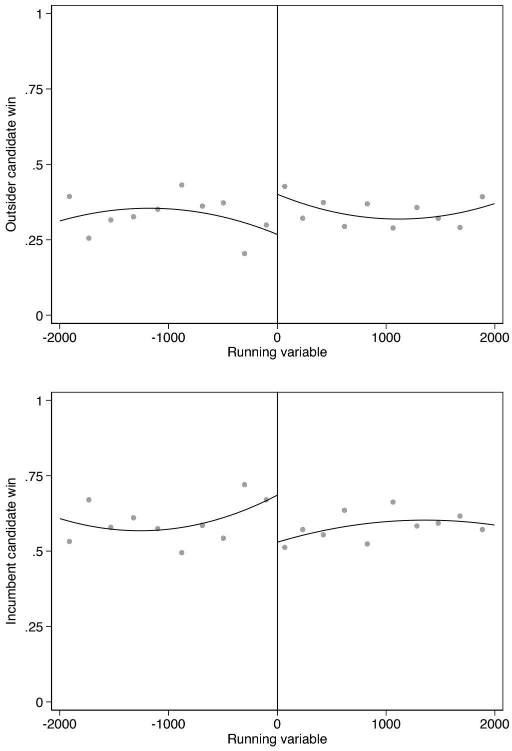
1.4.3 Effects on candidate selection and winner identity

Effects on winner identity

Despite the lack of effect on the total number of candidates, spending limits and the reimbursement of campaign expenditures may affect the selection of candidates who choose to enter the race and, in particular, the likelihood that the incumbent and the challenger of the previous race run again. Furthermore, the increase in election competitiveness indicated by the lower likelihood of a victory in the first round could affect the relative chances of different types of candidates and the identity of the winner. Therefore, we now explore effects on the outcomes of *specific candidates*.

We start with a graphical investigation of the impact of the campaign finance rules on the probability of a victory by an outsider, an insider, the incumbent, and their challenger.

Figure 1.4 Impact on winner identity - Main sample of departmental elections



Notes: Each dot is the average of the outcome variable within a given bin of the running variable. The running variable (the district population centered around 9,000 inhabitants) is split into quantile-spaced bins. The continuous lines represent a quadratic fit. To facilitate visualization, the graph is truncated at 2,000 inhabitants around the cutoff.

Figure 1.4 shows clear positive jumps at the threshold for the probabilities of outsider and challenger candidates winning the election, and negative jumps for incumbents and insider candidates. The corresponding point estimates, shown in Table 1.7, are sizeable and all significant at the 1 or 5 percent level. The probability of outsider and challenger candidates winning increases by 9.2 percentage points (31.9 percent) and 5.2 percentage points (288.9 percent), respectively, while the probability of the incumbent winning declines by 14.5 percentage points (21.2 percent). In absolute terms, the effects on challengers and outsiders almost perfectly add up to the effect on incumbents. In other words, the campaign finance rules level the playing field and increase the winning chances of new candidates and challengers from the previous race at the expense of the incumbent.

Once again, we check the robustness of these results to excluding the 2008 elections, in Appendix Table C3. While the effects on insider and outsider candidates become nonsignificant, our results on challengers and incumbents remain significant at the 5 and 10 percent level, respectively.

Table 1.7 Impact on winner identity - Main sample of departmental elections

	(1)	(2)	(3)	(4)
Outcome	Outsider win	Insider win	Incumbent win	Challenger win
Treatment	0.092**	-0.092**	-0.145***	0.052**
	(0.042)	(0.042)	(0.046)	(0.020)
Robust *p*-value	0.024	0.024	0.002	0.012
Observations	1,686	1,686	1,392	1,819
Polyn. order	1	1	1	1
Bandwidth	1,886	1,886	1,578	2,037
Mean, left of threshold	0.288	0.712	0.683	0.018

Notes as in Table 1.6.

Effects on candidate selection

The effects on candidates' probability of winning could come both from voters becoming less likely to vote for incumbents when they are in the race, and from candidates adjusting their entry decision. The outcomes used in Table 1.7 are unconditional winning probabilities, such that candidates who do not compete in the election are assigned a value of 0. Therefore, the negative impact on the reelection of the incumbent could result in part from the fact that some incumbent candidates choose not to run because they know that they will not be able to outspend their competitors. Indeed, they know that their own expenditures will be limited and they can reasonably expect their competitors who are likely to be reimbursed to spend more money than they would otherwise. The same reasoning may increase challengers' likelihood to run, contributing to the positive impact on their likelihood of winning. By contrast, the positive effect on the likelihood of a victory by an outsider candidate should not be driven by increased entry, given the null effect on the number of outsider candidates shown in Table 1.6, column 2.[21]

We test and verify the hypotheses regarding the incumbent and challenger candidates' likelihood of running in Panel A of Table 1.8. Columns 1 and 4 show a reduction in incumbents' probability to run by 7.4 percentage points (9.6 percent) and an increase in challengers' likelihood to run by 8.4 percentage points (47.7 percent). Columns 2 and 5 report effects on the unconditional likelihood of winning which we already showed in Table 1.7, for reference. Columns 3 and 6 show effects on unconditional vote shares. These effects are more difficult to interpret but they are an ingredient of the conditional estimates reported in Panel B, which we turn to now.

Effects on winning conditional on running

We now investigate whether campaign finance rules affect the chances of winning and the vote share of the winner and of their previous challenger, *conditional on participating in the race*. We cannot simply compare the elections below and above the discontinuity in which incumbents or challengers are present. Indeed, the regression discontinuity framework does not imply that incumbents and challengers who choose to run in districts just above the discontinuity are similar to those running in districts just below. In fact, we just showed that the rules affect these candidates' likelihood of entering the race.

To circumvent this difficulty, we follow Anagol and Fujiwara (2016) and Granzier et al. (2019) who adapt Lee (2009)'s method to derive bounds in a regression discontinuity design context. Focusing on incumbent candidates, we define $T = 0$ when districts are below 9,000 inhabitants and $T = 1$ otherwise. We further define R_0 and R_1 as potential outcome indicators for running when $T = 0$ or $T = 1$, respectively. In the data, we only observe $R = TR_1 + (1-T)R_0$. We know whether the incumbent runs for reelection in districts above 9,000 inhabitants but do not know if she would have run again in districts below, and conversely.

In a second step, we define W_0 and W_1 as potential outcomes for winning the election conditional on running, such that we only observe $W = R[TW_1 + (1-T)W_0]$. If the incumbent does not run again $(R = 0)$, she does not win $(W = 0)$, and we do not observe W had she run. If the incumbent runs in a district above 9,000 inhabitants, we observe whether she wins the election but do not know if she would have won in a district below, and conversely.

[21] Moreover, we do not find any significant impact on the likelihood that outsider candidates participate in the election, as shown in Appendix Table A1.

We then classify incumbent candidates as belonging to four categories. "Always takers" are incumbents who always run again, regardless of T; "never takers" are incumbents who never run again; "compliers" are incumbents who run again only if they are in a district below the threshold, where the lack of spending limits and of public reimbursement of campaign expenditures mean they can expect to face less competition; "defiers" are incumbents who would run in a district above the threshold, but not below.

We need to assume that there are no defiers to be able to derive bounds on our estimates: incumbents who run in districts above 9,000 inhabitants would also run in districts below. Assuming away such "defiers" yields $R_1 \leq R_0$, such that we can decompose the impact on the unconditional probability of the incumbent winning as:

$$\underbrace{E(W_1R_1 - W_0R_0|x=0)}_{RD\ effect\ on\ W} = \underbrace{Prob(R_1 > R_0|x=0)}_{RD\ effect\ on\ R} \cdot \underbrace{E(W_1|x=0, R_1 < R_0)}_{Unobservable}$$

$$+ \overbrace{E[W_1 - W_0|x=0, R_0=1]}^{Effect\ on\ win\ cond\ on\ being\ always-taker\ or\ complier} \cdot \underbrace{E(R_0|x=0)}_{lim_{x\uparrow 0}E[R|x]}$$

In words, the impact on the incumbent's victory sums the impact on the incumbent running, multiplied by the probability that an incumbent complier would win if they entered the race, in districts closely above the discontinuity; and the effect of winning conditional on being an always taker or complier, multiplied by the probability that incumbents in districts just below the threshold run for reelection. Rewriting the equation above, we can decompose the impact on the incumbent winning conditional on running as:

$$\overbrace{E[W_1 - W_0|x=0, R_0=1]}^{Effect\ on\ win\ cond\ on\ being\ always-taker\ or\ complier} = \frac{1}{\underbrace{E(R_0|x=0)}_{lim_{x\uparrow 0}E[R|x]}} [\underbrace{E(W_1R_1 - W_0R_0|x=0)}_{RD\ effect\ on\ W}$$

$$- \underbrace{Prob(R_1 > R_0|x=0)}_{RD\ effect\ on\ R} \cdot \underbrace{E(W_1|x=0, R_1 < R_0)}_{Unobservable}]$$

The only unobservable term in this equation, $E(W_1|x=0, R_1 < R_0)$, refers to the probability that a complier would win if she ran in districts closely above the threshold, an outcome which we cannot observe, by definition. Since all the other terms of the equation are observable, we simply need to make assumptions about this term to derive lower and upper bounds on the effects on winning conditional on running.

To derive a lower bound (largest possible impact of spending rules on the incumbent probability of winning), we assume that compliers would never win in districts closely above the threshold: $E(W_1|x = 0, R_1 < R_0) = 0$. To derive an upper bound (lowest possible impact on the incumbent probability of winning), we assume that compliers would, at most, have the same probability of winning as incumbents running in districts below the discontinuity: $E(W_1|x = 0, R_1 < R_0) = 0.871$. This yields a conservative estimate, as this probability is higher than the probability of winning of incumbents who run in districts above the discontinuity: 76.7 percent.

We extend this analysis in two ways. First, we use the same method to derive bounds on *challengers*' probability of winning conditional on running. Since challengers are more likely to run above the discontinuity, our no defiers assumption states that challengers who run in districts below 9,000 (where they might be at a disadvantage due to the lack of limit on incumbents' spending) would also run in districts above. Second, we use our effects on unconditional vote shares to derive bounds on the effects on incumbents and challengers' *vote shares* conditional on running.

We use a bootstrapping procedure to estimate the standard errors of the bounds. For each outcome of interest, we draw a sample of districts with replacement, compute the lower and upper bounds following the method stated above, and repeat these steps 1,000 times.

Panel B of Table 1.8 shows the results. Conditional on running, the campaign spending rules present above the threshold cause a reduction in incumbents' first round vote share and in their probability of getting reelected. Their vote share decreases by 3.0 to 7.6 percentage points (6.3 to 16.1 percent of the mean incumbent vote share in districts just below the cutoff) and their likelihood of reelection by 10.5 to 18.9 percentage points (12.1 to 21.7 percent). By contrast, challengers' vote share and likelihood of winning increase by 3.3 to 13.0 percentage points (13.0 to 51.2 percent) and 11.0 to 19.8 percentage points (79.1 to 142.4 percent), respectively, conditional on running. The upper bounds of these effects are statistically significant, but the lower bounds are not.

These results are robust to excluding the 2008 elections: as shown in Appendix Table C4, the effects on incumbents' winning probability are a bit lower in this sample, but effects on challengers are larger, with lower bounds significant at the 5 percent level for winning, and at the 10 percent level for vote shares.

Overall, our results suggest that the negative impact of campaign spending rules on the incumbent's probability of winning is driven both by their lower probability to enter the race in the first place, and by voters' lower propensity to vote for them conditional on running. Similarly, the positive impact on challengers' probability of winning is driven both by increased entry and an increased vote share, conditional on running.

Table 1.8 Impact on running, winning, and vote shares - Main sample of departmental elections

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Incumbent			Challenger		
	run	win	vote share, R1	run	win	vote share, R1
Panel A. Unconditional effects						
Treatment	-0.074**	-0.145***	-0.058***	0.084**	0.052**	0.034***
	(0.032)	(0.046)	(0.020)	(0.038)	(0.020)	(0.012)
Robust p-value	0.023	0.002	0.005	0.020	0.012	0.003
Observations	2,579	1,392	1,874	1,827	1,819	1,911
Polyn. order	1	1	1	1	1	1
Bandwidth	2,876	1,578	2,113	2,056	2,037	2,159
Mean	0.767	0.683	0.367	0.176	0.018	0.044
Panel B. Conditional effects						
Upper bound		-0.189**	-0.076**		0.198**	0.130***
Boot. std error		(0.096)	(0.034)		(0.081)	(0.042)
Lower bound		-0.105	-0.030		0.110	0.033
Boot. std error		(0.075)	(0.019)		(0.068)	(0.021)
Mean		0.871	0.473		0.139	0.254

Notes: Panel A and Panel B show effects on unconditional outcomes and bounds of effects conditional on running, respectively. The notes for Panel A are as in Table 1.6. In Panel B, the mean, left of the threshold, indicates the value of the outcome for the candidates on the left of the threshold, conditional on running. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively, of the bootstrapped standard errors.

1.4.4 Effects on the winning orientation, polarization, and representativeness

Effects on the winning orientation

To understand how the campaign finance rules affect the political landscape, we now explore their effects on the winner's political orientation

The first outcome that we consider, in Table 1.9, column 1, is a dummy equal to 1 if the orientation of the winner is identical to the orientation of the incumbent. Indeed, the negative impact on the reelection of the incumbent would perhaps be of little consequence if the candidate replacing them (whether this candidate is the previous race's challenger or an outsider) was of the same orientation. Instead, we find that the campaign finance rules increase the likelihood that the seat falls to a candidate of a new political orientation by 8.2 percentage points, which is significant at the 5 percent level, and more than half the size of the effect on incumbents' reelection.

We then go one step further and ask whether changes in the orientation of the winner compensate each other across districts or whether they tend to go in the same direction and to systematically benefit one specific orientation. Spending patterns by candidates on the left and on the right suggest that the former stood to benefit from the reform at the expense of the latter. Appendix Table A2 compares average expenditures to ceiling ratios as well as contributions to ceiling ratios by candidate orientation, in districts just above the threshold, in departmental elections that preceded (1992 and 1994) and followed (1998 and 2001) the introduction of campaign expenditures' reimbursement. Prior to the 1995 reform, expenditures from candidates on the left only accounted for 17.2 percent of the spending limit, compared to 32.8 percent for their counterparts on the right. These differences in spending reflect differences in personal contributions by the candidates (3.2 percent of the ceiling for candidates on the left against 13.9 percent for candidates on the right) and in donations they received (6.2 percent against 14.5 percent). Given these baseline spending patterns, the 1995 reform, that introduced the reimbursement of campaign expenditures, dramatically increased relative spending by candidates on the left. After the reform, personal contributions by right-wing candidates more than doubled, as a ratio of spending limits, but they increased nearly tenfold for candidates on the left. On average, left-wing and right-wing candidates contributed 31.0 percent and 34.4 percent of the ceiling with their own money, and they spent 39.6 and 43.9 percent of the limit. In other words, differences in average campaign expenditures between these two groups were much lower after than before the reform.

Table 1.9 confirms that candidates on the left also benefited from the reform electorally. Campaign finance rules above the threshold increase the likelihood of a victory by a left-wing candidate by 8.5 percentage points (17.9 percent), which is significant at the 10 percent level. Victories by center and right-wing candidates become less likely, by 2.1 and 5.3 percentage points respectively, but these estimates are not statistically significant.

Table 1.9 Impact on winning orientation - Main sample of departmental elections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome	Incumbent or. win	Far-left win	Left win	Center win	Right win	Far-right win	Non-classified win
Treatment	-0.082**	-0.003	0.085*	-0.021	-0.053	-0.000	0.010
	(0.037)	(0.003)	(0.047)	(0.014)	(0.041)	(0.000)	(0.008)
R. p-value	0.024	0.255	0.059	0.149	0.203	0.334	0.263
Obs.	1,534	2,196	2,531	2,576	3,362	1,604	2,126
Polyn.	1	1	1	1	1	1	1
Bdw	1,709	2,459	2,813	2,865	3,784	1,799	2,383
Mean	0.862	0.003	0.475	0.043	0.477	0.000	0.001

Notes as in Table 1.6. “Or.” stands for “orientation.”

Effects on polarization and representativeness

While campaign finance rules level the playing field, improved performance by candidates from non-mainstream platforms could increase polarization. Moreover, by strengthening outsiders, these reforms could lead voters to split their votes across multiple candidates of the same orientation, which could result in suboptimal outcomes such as the defeat of the Condorcet winner (Pons and Tricaud, 2018).

To further characterize the effects of the reforms on electoral outcomes, we first measure the polarization of the results. Using the sample of 86 percent of departmental races for which each candidate can be matched to a ParlGov ranking on the [0-10] left-right scale, we follow Dalton (2008) and build the following measure of polarization: $\sqrt{\sum v_i \left(\frac{p_i-\bar{p}}{0.5}\right)^2}$, where $\bar{p} = \sum v_i p_i$, v_i is candidate i's vote share, and p_i, the ideological positioning of their party or affiliation. This index takes the value 0 when all candidates converge to the same position and 10 when they are equally split between the two most extreme positions. As shown in Table 1.10, the impact on this outcome is small and non-significant, indicating that campaign finance rules do not increase polarization.

Second, we assess whether the legislation affected the representativeness of the winner. We proxy voter preferences using first round results and aggregate first round vote shares by orientation. We measure effects on the first round vote share of the winner's orientation and on a dummy equal to 1 if that orientation had obtained the most votes. We find a negligible effect on the first outcome (column 2) and a negative but small and non-significant effect on the second (column 3), indicating that the rules above the threshold do not decrease the representativeness of the winner with respect to the distribution of first round vote choices.

The results presented in Sections 1.4.4 and 1.4.4 are robust to adding non-linkable districts (Appendix Table C6). The effects on the likelihood of a victory by the incumbent orientation and by a left-wing candidate remain negative and positive, respectively, but they become insignificant when excluding the 2008 elections (p-value=0.31 and 0.11, respectively, Appendix Table C5).

Table 1.10 Impact on polarization and winner's representativeness - Main sample of departmental elections

	(1)	(2)	(3)
Outcome	Polarization	Vote share winner's orientation	Top orientation winning
Treatment	-0.082	-0.002	-0.037
	(0.083)	(0.014)	(0.029)
Robust p-value	0.340	0.888	0.171
Observations	2,161	2,297	1,871
Polyn. order	1	1	1
Bandwidth	2,770	2,565	2,098
Mean, left of threshold	4.868	0.583	0.922

Notes as in Table 1.6.

1.4.5 Additional robustness checks

To assess the robustness of our findings, we first evaluate the possibility that the main results on the probability of victory in the first round and on the likelihood that incumbents, challengers, and outsider candidates run and win may arise from chance rather than reflecting a causal relationship. To do so, we implement our regression discontinuity design at ten false population thresholds below and above the true 9,000 inhabitants cutoff, in Appendix Tables C7 through C10. The number of significant results is not higher than would be expected: six out of 70 point estimates are significant at the 10 percent level, and only one is also significant at the 5 percent level.

Second, we check the robustness of our results to employing a quadratic specification and to controlling for all the sociodemographic variables used in the general balance test in Appendix Tables C11 and C12, respectively. The point estimates and their significance remain very similar.

Finally, we check the sensitivity of the results to bandwidth selection, in Appendix Figures C1 through C4. For each outcome of interest, these graphs plot the point estimates and associated 5 percent confidence intervals for bandwidths ranging from plus to minus 500 inhabitants around the data-driven bandwidth selected based on Calonico et al. (2019). Overall, our results are very robust to changes in bandwidth size, whether we use a linear or quadratic specification.

1.5 Effects in municipal elections

This section investigates the impact of the campaign finance rules in municipal elections.

We first conduct the validity tests discussed in Section 1.3.3. Appendix Tables D1 and D2 show the general balance tests for the main sample and the sample also including non-linkable districts while Appendix Tables D3 and D4 show the balance tests on each sociodemographic variable. The general balance tests show no significant jump while two out of 26 point estimates are significant at the 10 percent level when conducting the individual tests.

Appendix Figures D1 and D2 test the assumption of no sorting across the threshold using the McCrary (2008) graph and the Cattaneo et al. (2018) density plots. Both graphs show positive jumps at the threshold and we reject the null of no manipulation using Cattaneo et al. (2018)'s test, whether non-linkable municipal districts are excluded (p-value=0.032) or not (p-value=0.022). We conduct an election-by-election investigation of this result in Appendix Figures D3, D4, and D5 and notice that the jump in the density of the running variable is driven by the 2014 election (p-value = 0.004), while the 2001 and 2008 elections do not show any evidence of a jump (p-value=0.488 and 0.898). We do not consider the positive jump in the 2014 election as definite evidence of manipulation, given the difficulty to bend the rules used to determine municipalities' official population which we described in Section 1.3.3, and because one would expect manipulation to go in the opposite direction. Indeed, if anything, incumbent mayors may try to maintain the population of their municipality *below* the cutoff in order to limit competition, which would generate a negative jump in the density of the running variable at the threshold. Similar to Corbi et al. (2019), we check the robustness of our results to considering each municipal election separately, to make sure that they are driven neither by the potentially problematic 2014 election year nor by the fact that most treated districts in the 2008 municipal election had already been treated a first time in 2001. Indeed, recall that the populations in place in the 2001 and 2008 elections were mostly identical since no major census took place in between.

Table 1.11 shows the effects on competition in Panel A, and on winner identity in Panel B. These effects are lower in magnitude than in departmental elections, and, unlike in departmental elections, none of them is statistically significant. We obtain similar null results when we consider the 2001, 2008, and 2014 municipal elections separately (Appendix Tables E1 through E3), and when we include non-linkable districts in the sample used to measure effects on competition (Appendix Tables E4 through E7). We investigate the mechanisms driving the difference between results in departmental and municipal elections in the next section.

Table 1.11 Impact on competition and winner identity - Main sample of municipal elections

Panel A. Competition

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Nr candidates	Nr outsiders	Nr insiders	Turnout	ENC	Victory in r1
Treatment	-0.040	-0.034	-0.017	0.003	0.036	-0.008
	(0.135)	(0.131)	(0.069)	(0.009)	(0.099)	(0.059)
Robust *p*-value	0.778	0.763	0.911	0.567	0.762	0.822
Observations	1,426	1,433	2,258	1,189	1,455	1,315
Polyn. order	1	1	1	1	1	1
Bandwidth	1,908	1,913	2,803	1,618	1,939	1,773
Mean, left of threshold	2.920	1.816	1.106	0.637	2.425	0.606

Panel B. Winner identity

	(1)	(2)	(3)	(4)
Outcome	Outsider win	Insider win	Incumbent win	Challenger win
Treatment	-0.022	0.022	-0.030	0.038
	(0.054)	(0.054)	(0.054)	(0.033)
Robust *p*-value	0.653	0.653	0.686	0.209
Observations	1,219	1,219	1,487	1,318
Polyn. order	1	1	1	1
Bandwidth	1,670	1,670	1,975	1,848
Mean, left of threshold	0.374	0.626	0.562	0.0610

Notes as in Table 1.6.

1.6 Mechanisms

The results shown in Sections 1.4 and 1.5 indicate that the effects of campaign finance rules vary across election types. In particular, the rules decrease incumbents' likelihood to run again and get reelected in departmental elections but not in municipal elections. In this section, we discuss the reasons that could account for this difference and we ask whether the effects in departmental elections are driven primarily by campaign spending limits or by the reimbursement of campaign expenditures.

1.6.1 Municipal versus departmental elections

One possible explanation for the differences in results between departmental and municipal elections is that they reflect differences between the voting rules used in these two types of elections. Municipal elections use a two-round list system with proportional representation, while departmental elections are held under a single candidate two-round plurality voting rule. These institutional differences may explain our results through three complementary mechanisms.

First, in municipal elections, candidates' ability to reach their desired amount of spending is likely to depend less on reimbursement by the state. Indeed, campaign costs can be split between the mayoral candidate and the other 26 members of the list, unlike in departmental elections where the campaign is carried out by the candidate alone. In addition, municipal election candidates rely less exclusively on their own contributions because they are more likely to receive private donations: as shown in Table 1.12, in municipalities just above the threshold (with 9,000 to 11,000 inhabitants), donations account for 13.1 percent of the spending ceiling in municipal elections, against 4.1 percent in departmental elections.

Second, in departmental elections, spending limits and reimbursement benefit challengers and outsider candidates because they level the playing field. In municipal elections, the marginal returns of campaign expenditures may be lower, decreasing the equalizing effect of these rules. Indeed, the presence of multiple candidates in each list increases the odds that voters know at least one of them, and voters' higher baseline level of information may make it more difficult and costly to win them over. In addition, all candidates on the list can devote time to reach out to voters, and time may be a substitute for money. Finally, marginal returns may simply be lower due to higher average expenditures in municipal elections: 0.87 euros per capita, versus 0.31 euros per capita in departmental elections (columns 3 and 4 of Table 1.12).

Table 1.12 Composition of candidates' campaign contributions by type of election:

	% of spending ceiling		EUR per capita	
	Municipal elec.	Departmental elec.	Municipal elec.	Departmental elec.
Total expenditures	0.588	0.401	0.87	0.31
Donations	0.131	0.043	0.19	0.03
Party contributions	0.019	0.017	0.03	0.01
Personal contributions	0.439	0.339	0.65	0.26
Natural advantages	0.016	0.016	0.02	0.01
Other contributions	0.001	0.001	0.00	0.00

Notes: This table provides average measures by candidate and by election for each of the defined outcomes as a percentage of the spending ceiling in the first two columns and in EUR per capita in the last two columns. To make districts across municipal and departmental elections comparable, we focus on districts close to the cutoff (between 9,000 and 11,000 inhabitants) and on nearby elections years for which we have expenditure data for both municipal and departmental elections. Namely, we compare the 2008 and 2014 municipal elections with the 2008 and 2011 departmental elections. Note that the sum of contributions does not necessarily add up to total expenditures of candidates, as contributions need not be exhausted.

Third, the factors affecting candidates' decision to compete or stay out of the race may also differ across election types. In departmental elections, we find suggestive evidence that the negative impact on incumbents' likelihood to run for reelection is partly driven by pressure exerted on them by their party. We compare effects for incumbents affiliated with a party (Appendix Table A3) and for those who are not (Appendix Table A4).[22] We find that party-affiliated incumbents are driving the results: In the corresponding districts, campaign finance rules reduce incumbents' probability of running by 9.4 percentage points and their unconditional probability of winning by 16.4 percentage points. Effects on running and winning are much lower, and non-significant, for non-party-affiliated incumbents. These results suggest that, in departmental races above the threshold, where electoral competition is greater, political parties successfully prevent incumbents that they expect to be defeated from running again.

By contrast, as shown in Table 1.13, we do not find any negative effect on incumbents' presence (or on the presence of challengers and outsider candidates) in municipal elections (see Appendix Tables E8 through E10 for separate 2001, 2008, and 2014 results). Incumbents' ability to withstand pressure to drop out of their reelection bid, in these races, may come again from the list format. Incumbents can invite loyal party members as well as possible opponents to join their list, before the first round or between rounds, which increases their bargaining power. In addition, they know that they will most

[22]We identify party-affiliated incumbents as those who had a party label in the previous election, irrespective of the present election, to avoid endogeneity concerns.

likely obtain a seat on the municipal council if they run, even if they fail to be reelected as mayor, which decreases the risk of entering the race. In fact, 99 percent of incumbents who do run again get a seat.

Table 1.13 Impact on running - Main sample of municipal elections

	(1)	(2)	(3)
Outcome	Incumbent	Challenger	Outsider
Treatment	-0.022	0.002	-0.001
	(0.049)	(0.054)	(0.028)
Robust *p*-value	0.788	0.882	0.959
Observations	1,779	1,457	1,774
Polyn. order	1	1	1
Bandwidth	2,298	2,008	2,280
Mean, left of threshold	0.719	0.269	0.908

Notes as in Table 1.6.

1.6.2 Spending limits versus reimbursement

We now investigate whether the effects in departmental elections are driven primarily by spending limits or by the reimbursement of candidate expenditures. While estimating the joint impact of both rules is interesting, as many countries condition public funding of electoral campaigns on complying with spending limits, disentangling their respective importance is helpful to better understand the mechanisms underlying our results and to inform future campaign finance reforms.

The result in Section 1.4.4 showing that left-wing candidates, who benefit from the reimbursement more than their right-wing counterparts, are also those whose electoral outcomes improve the most, is a first piece of evidence suggesting that the reimbursement of campaign expenditures plays an important role.

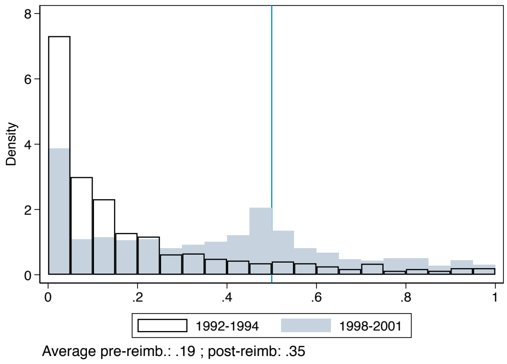
We bring additional evidence by exploiting the departmental elections held in 1992 and 1994. These elections enable us to isolate the effect of spending limits because they took place after the 1990 reform enforcing limits for districts above the discontinuity, but before the 1995 reform enacting the reimbursement of candidates. We should expect null effects in these earlier elections if reimbursement is the main driver of the effects we observe in subsequent elections. This is indeed what we find. As shown in Appendix Table A5, point estimates are of a lower magnitude in the 1992 and 1994 elections than afterwards, and they are generally not significant. The only exception is the effect on challengers' victories, which is significant at the 10 percent level, but has a negative sign, contrary to the positive effect observed after the introduction of reimbursement.

While these results suggest that effects post 1995 are driven by reimbursement rather than spending limits, alternative interpretations remain possible. The tightening of spending limits concomitant to the introduction of public reimbursement, in 1995, could play a role, and limits and reimbursement may be complementary and jointly explain the effects. Therefore, we go one step further and provide direct evidence on changes in candidate spending and contribution patterns between the 1992-1994 and the 1998-2001 departmental elections, in districts just above the threshold. Figures 1.5 and 1.6 plot the distribution of spending to ceiling ratios as well as personal contributions to ceiling ratios for all candidates (upper left graph) and separately for incumbents, challengers, and outsiders (upper right graph and lower graphs).

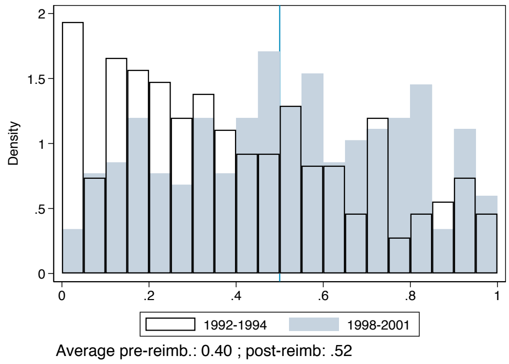
We first observe large outward shifts of both distributions to the right, after the 1995 reform. Expenditures and personal contributions rise as a share of the ceiling for all types of candidates, but the increase is much larger for challengers and outsiders than for incumbents. The fact that these candidates are also the ones who benefit from the reform electorally points to the important role of the reimbursement. Second, both sets of histograms show bunching at 50 percent of the ceiling post 1995 only, particularly for challengers and outsiders. This pattern underlines the role played by reimbursement even more directly, since 50 percent of the ceiling is the maximum amount of expenditures which candidates can submit for reimbursement (conditional on obtaining more than 5 percent of the votes). Interestingly, the bunching is slightly stronger for personal contributions and driven by challengers and outsiders. This is consistent with the fact that the reimbursement only applies to personal expenditures, so that the 50 percent mark is not relevant for other sources of campaign money. Candidates who contribute 50 percent of the ceiling with their own money but also receive private donations or party contributions will fall just below 50 percent in the graph plotting personal contributions but above that mark in the graph plotting total spending. Third, we observe a bit of bunching of overall spending at 100 percent, corresponding to candidates who spend nearly exactly the maximum amount of money authorized. However, this bunching is similar before and after 1995, and it is much lower than the bunching at 50 percent, which again only appears after 1995.

In sum, this graphical evidence underscores the dramatic changes in campaign spending which resulted from the introduction of personal expenditures' reimbursement in 1995. By contrast, while the spending limit does constrain a small subset of candidates, it does not become more binding after 1995. These patterns, combined with the stark difference between effects on our main outcomes in departmental elections before and after 1995 all point to the conclusion that reimbursement, not spending limits, drives our results.

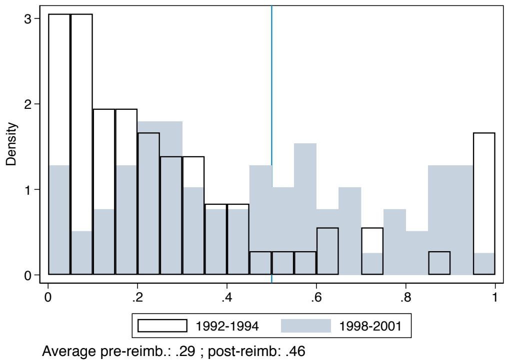
Figure 1.5 Expenditures to ceiling ratios - Main sample of departmental elections



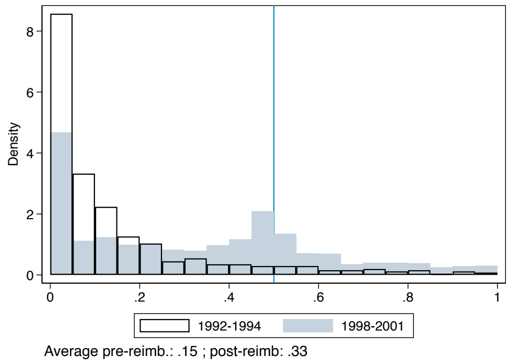
All candidates



Incumbent candidates



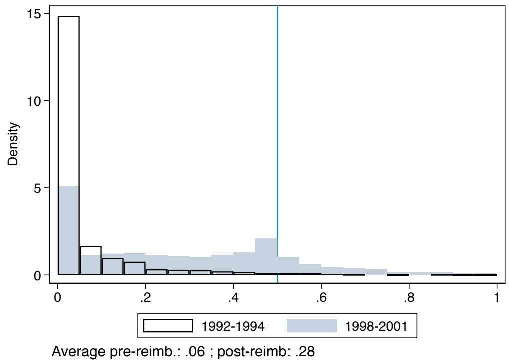
Challenger candidates



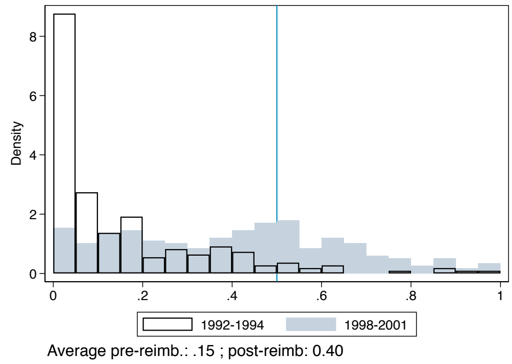
Outsider candidates

Notes: The level of analysis is the candidate and the sample includes only districts above 9,000 inhabitants, for which data on campaign spending are available. We further exclude districts above 11,000 inhabitants to focus on candidates running in districts close to the cutoff. The graphs are trimmed at 1, thus excluding a handful of candidates whose expenditures exceeded the ceiling.

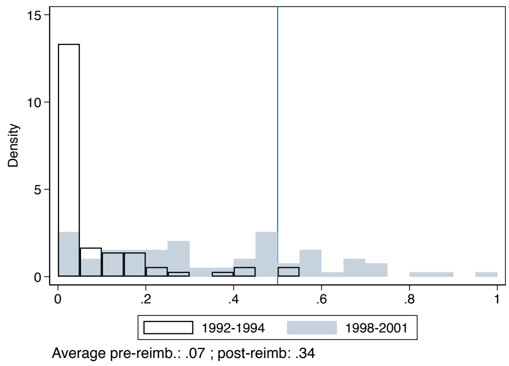
Figure 1.6 Personal contributions to ceiling ratios - Main sample of departmental elections



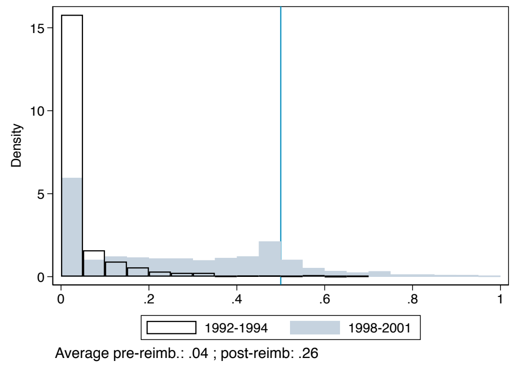
All candidates



Incumbent candidates



Challenger candidates



Outsider candidates

Notes: The level of analysis is the candidate and the sample includes only districts above 9,000 inhabitants, for which data on campaign spending are available. We further exclude districts above 11,000 inhabitants to focus on candidates running in districts close to the cutoff. The graphs are trimmed at 1, thus excluding a handful of candidates whose expenditures exceeded the ceiling.

1.7 Conclusion

This paper investigates how campaign finance rules affect candidate selection and electoral outcomes by exploiting two reforms that took place in France in the early 1990s. After the reforms, the rules differed for cantons and municipalities above and below 9,000 inhabitants, allowing us to estimate their effects with a regression discontinuity design.

Our results first show that the reimbursement of campaign expenditures by the state has the potential to level the playing field and to substantially reduce incumbents' advantage.

In departmental elections, the amount of money spent by competitors increased relatively to incumbents, after the introduction of public reimbursement in districts above the cutoff in 1995. Overall, public funding decreased incumbents' likelihood to be reelected by 14.5 percentage points, due to negative effects on their likelihood to run and on their vote share and winning, conditional on running. The weakening of incumbents benefits the candidate who was their runner-up in the previous race as well as new candidates, who all see their chances of winning improve, and it increases the likelihood of a change in the orientation of the elected official. Overall, these turnovers tend to benefit the left, whose candidates are often outspent by right-wing competitors absent public funding. Importantly, we note that this policy neither increases the polarization of results nor decreases the representativeness of the winner's orientation with respect to the distribution of first round vote choices.

Our results also show that the effects of campaign finance rules can be mitigated due to weaknesses in the exact design of these rules and due to the format of some elections.

First, we do not find any effect of spending limits when we examine the 1992 and 1994 departmental elections in which limits already existed but reimbursement had not been implemented yet. The lack of effects of spending limits contrasts with recent papers finding substantial effects on electoral competition. This difference may come from the fact that the spending ceiling is less stringent and binding in the elections that we study than in other contexts, including the British elections to the House of Commons studied by Fouirnaies (2021), where limits have been tightened over time, or the local Brazilian elections studied by Avis et al. (2022), where ceilings are set based on the maximum spending in the previous race.

Second, unlike the large effects observed in departmental elections post 1995, we do not find any effect of the reimbursement of campaign expenditures in municipal elections. We attribute this difference to important differences in the voting rule used in these two types of elections: plurality voting in single-member constituencies versus a proportional list system. In municipal elections, campaign expenditures can be split across the mayoral candidate and the other members of their list, and the latter can also devote time campaigning on behalf of the list beyond just contributing money. Resources brought by fellow candidates may decrease the scope for public funding to make a difference. In addition, incumbents' ability to invite allies and rivals alike to join their list puts them in a more powerful position to withstand political parties pressuring them to stay out of the race.

In sum, our results suggest that the list format which characterizes proportional elections makes the reimbursement of campaign expenditures less impactful than in elections using single-candidate plurality voting. This insight could inform the design of future campaign finance reforms, in France and beyond. Naturally, further work is needed to verify its external validity.

2

Migration and Redistributive Spending: Evidence from Local Authorities in England

Joint with Lars Ludolph

Abstract

[1] The inflow of migrants can impact public spending through its effect on local preferences for redistribution and through changes in demand for local services brought about by migrants' distinct characteristics. In this paper, we analyse the effects of the migration wave from Central and Eastern European countries following their EU accession in 2004 on local level redistribution in England, specifically disentangling these two channels. We apply a difference-in-differences estimation strategy and find that migrants did not have an effect on local authorities' total service provision per capita. Once we zoom in on the different expenditure items, we find that local authorities experiencing relatively larger migration inflows saw their spending on means-tested social care services decrease in relative terms, while spending on education services increased. Analysing changes in local Council compositions and internal migration flows in response to the arrival of outsiders, we find no evidence that spending shifts are driven by a change in the local willingness to redistribute income. Rather, our results suggest that, due to migrants' young age at the time of arrival, migration following the 2004 EU enlargement alleviated some of the pressure social care spending in England faces.

[1]We thank Josep Amer-Mestre, Brian Bell, Michèle Belot, Max Brès-Mariolle, Riccardo Crescenzi, Andreas Diemer, Elias Dinas, Frédéric Docquier, Gabor Farkas, Jérôme Gonnot, Nancy Holman, Andrea Ichino, Mauro Lanati, Andrea Mattozzi, Kasia Nalewajko, Pieter Nijkamp, David Phillips, Martin Ruhs, Filipa Sá, Olmo Silva, James M. Snyder, Marco Tabellini, Clémence Tricaud, Danny Walker, Thomas Walsh, an anonymous referee, as well as workshop participants at the Max Planck Institute, the European University Institute, the Migration Policy Centre and EPSA for helpful comments.

2.1 Introduction

Concerns about redistributing income to what are considered outsiders has featured as a salient issue in the run up to the 2016 Brexit referendum that ultimately saw the UK leaving the European Union (EU) by popular vote. Both UK transfers to the EU and the pressure EU immigrants allegedly put on public service provision in the UK were platforms the "Vote Leave" campaign heavily relied on to mobilise its supporters (Becker et al., 2017; Gherghina and O'Malley, 2019; Goodwin and Milazzo, 2017).

In this paper, we investigate if the link between the inflow of EU "outsiders" and a local loss of appetite for redistributing income was visible in the UK before the Brexit referendum took place. Specifically, we focus on the time period after the 2004 and 2007 EU enlargement when the historically anchored EU scepticism in the UK took a turn against migration following the granting of free movement rights to citizens from Central and Eastern European countries and the large inflow that followed into UK territory (Gherghina and O'Malley, 2019). We analyse the effect of the unexpected and spatially heterogeneous migration wave from the 12 post-2004 Accession Countries (AC-12) on English local authority level public spending and revenue to answer the question if the local presence of migrants is indeed associated with less redistributive spending patterns. We combine detailed local authority revenue and expenditure data from the Chartered Institute of Public Finance and Accountancy (CIPFA) with annual data on local authority level migrant stocks calculated from the UK Labour Force Survey/Annual Population Survey (LFS/APS) obtained under a special licence agreement over the 2000 to 2015 observation period. Due to the very low number of AC-12 migrants present in England in the pre-enlargement period, the estimated coefficients we obtain from our two-way fixed effects panel regressions correspond to a difference-in-differences research design, allowing us to recover treatment on the treated effects of AC-12 migration on local authority revenue and spending.

Taken at face value, we find ambiguous results with regards to the hypothesis of a closed in-group that cuts down on redistributing income when faced with outsiders. On the one hand, AC-12 migration did not affect local authorities' per capita spending. However, once we zoom in on local revenue and spending patterns, we find that the presence of AC-12 migration is associated with a decrease in locally generated revenue and the unchanged per capita spending was heavily supported by an increase in funding local authorities received from the central government. AC-12 migration is further associated with both a decrease in means-tested social care spending per capita and an increase in education expenditure per capita, an expenditure item where inter-group transfers are likely to be relatively more salient compared to other non-means-tested services (Speciale, 2012; Tabellini, 2020a).

To further explain these results, we then disentangle local preferences for less redistribution from mechanical changes in demand for local services brought about by the distinct characteristics of AC-12 migrants. Our results show that the strong association of AC-12 migration with a decline in social care and a rise in education expenditure per capita is driven by changes this migrant group caused to local demographics. In fact, social care expenditure per population aged 65 and above, the main recipient population of social care services, increased strongly in response to AC-12 inflows. Similarly, when normalising education expenditure per the rising local number of pupils in areas more strongly affected by AC-12 migration, expenditure remained vastly unaffected. Thus, the effects AC-12 migrants had on local authority expenditure patterns were in large parts due to the shifts to local demographics these migrants caused and the corresponding institutional responses that were triggered by the resulting changes in local service demand. The relative shifts from social care towards education expenditure further explain the observed dynamics on the revenue side: In England, local authority education expenditure is almost entirely funded through central government grants, while a larger share of social care spending comes from locally generated revenue (Phillips, 2018). It thus appears that migration decreased the pressure local authorities faced on social care spending over our observation period, allowing local authorities to take better care of their vulnerable older population while decreasing the need for raising revenue locally. On the other hand, we do not find evidence that would support the hypothesis of a shift in local preferences towards less redistribution in response to migration inflows, which we test by analysing local voting patterns and native out-migration ("voting with their feet") following the 2004 EU enlargement and the subsequent wave of AC-12 migration. Our results show that a larger presence of AC-12 migrants is associated with a rise in local Council seats held by the more redistributive Labour party (rather than the Conservatives) and a decline in net-migration outflows. We note that the latter result, coined "fiscal externality" by Tabellini (2020b), may also provide a partial explanation for the changes in local demographics associated with AC-12 migration.

England is a suitable test bed for the local level link between migration and public spending for a number of reasons. First, the country experienced large waves of migration in recent decades, including the both unprecedented and unexpected wave of AC-12 migrants following the 2004 enlargement of the European Union (Becker et al., 2016). These successive waves have led scholars to test the impact of the intensity of migration flows to the UK on numerous outcomes including crime rates (Bell et al., 2013; Jaitman and Machin, 2013), house prices (Sá, 2015), hospital waiting times (Giuntella et al., 2018), public budgets and the wages and employment of natives (Becker et al., 2018; Card et al., 2012; Dustmann et al., 2013; Manacorda et al., 2012). In this study, we exploit the large and spatially heterogeneous shock to local migration stocks that stemmed from EU enlargement in 2004 and led to more than one million people migrating from Central and Eastern Europe to the UK. The fact this stemmed from the granting of free movement rights to new EU citizens explains the size and suddenness of the migration wave and distinguishes it from other migration waves because of the rights framework that enabled movement from the AC-12 to England post-2004. Second, the discretion in raising revenue and spending decisions at the hands of local authorities in the UK makes it an appropriate case study. Funded through a mix of central government grants and locally raised revenue, England, and the UK more generally, is one of the European countries where local governments have discretion over spending decisions that encompass several public expenditure items. UK local authorities are responsible for policies concerning education, social care services, highways, roads and transport, housing, cultural services, environmental services, planning and development and protective services (Gavazza et al., 2019; Phillips, 2018; Sandford, 2018). Local authorities are required to balance their budget but can increase or decrease their total spending through steering the local council tax, a property tax. They can further shift their spending between more or less redistributive spending items, with spending on means-tested social care services in particular reflecting the redistribution of income towards the relatively less wealthy. Third, due to the limited scope the UK central government had under EU law to target EU migrants directly over our 2000 to 2015 observation period, the central government could not use restrictive migration policies in the years prior to the Brexit referendum, such that cutting public spending was the only possible response to a decrease in appetite for redistributing resources. Finally, the wealth of information available allows to disentangle local preferences and fiscal externalities from a more mechanical migrant demand channel, a mechanism frequently neglected in the literature when studying the effect of migration on preferences for redistribution.

2.1.1 Contribution to the Literature

Our results contribute to the literature on the impact of migration on redistribution in destination areas. Pioneered by Freeman (1986), Alesina et al. (2001), Alesina et al. (2004) and Easterly and Levine (1997), an important stream of literature argues that redistributive policies are supported more strongly by homogeneous groups. These findings are driven by in-group biases which translate into greater immigrant diversity lowering preferences for redistribution. Following this work, several scholars have analysed the relationship between immigration and preferences for redistribution in the context of migration to the US and European countries with results pointing towards a negative association of the two (Dinas et al., 2019; Halla et al., 2017; Hopkins, 2010; Senik et al., 2009; Speciale, 2012; Steinmayr, 2020). For example, Senik et al. (2009) finds some evidence of a negative association studying Europe as whole. Speciale (2012) leverages the variation in inflows of migrants to EU-15 countries stemming from the 1990s Balkan wars to study the impact of migration on education expenditure and finds a small and negative association between perceived migration and support for the welfare state. Both authors document considerable heterogeneity across countries and stress the importance of sub-national studies to understand the mechanisms at play and allow for a causal investigation.

Dahlberg et al. (2012) exploit a refugee dispersal mechanism in Sweden and find a significant negative effect of immigration on the local support for redistribution. In Denmark, Harmon (2018) uses an instrumental variable strategy based on historical housing stock data and finds that greater migration inflows leading to increases in local ethnic diversity shift election outcomes from traditional ”big government” left-wing parties towards anti-immigrant nationalist parties. Both studies suggest immigration may lower the level of redistribution or public spending but identify further examination of the effect of immigration and ethnic diversity on more direct measures of redistributive spending as an important topic for future work. Similarly, in more recent work studying the effect of extending the voting franchise to non-natives, Ferwerda (2021) stresses that while evidence points towards European citizens preferring less redistribution with greater migration, the evidence that this leads to a reduction on public good provision is less well understood. Our work contributes directly to filling this gap by measuring the impact of a large migration shock to England on local level redistribution.

Closest to this present work are studies by Tabellini (2020b), Tabellini (2020a), Gerdes (2011) and Jofre-Monseny et al. (2016). Tabellini (2020b) studies the first "Great Migration" when 6 million black Americans migrated from the South to the North of the US. The author specifically focuses on its impact on local public finances due to changes in racial heterogeneity in Northern US cities between 1915 and 1930. After collecting data on local finances for the years 1910, 1919, and 1930 and deploying a version of a shift-share instrument based on historical settlements similar to Card (2001) and Boustan (2010) to predict black migration, the author finds that larger inflows had negative impacts on both public spending and tax revenues. The author then investigates whether this result is driven by a change in local preferences or by second-order effects black migrants had on out-migration of white Americans. While Tabellini (2020b) acknowledges that these mechanisms are not necessarily mutually exclusive, the author argues that the study’s results are rather driven by a negative fiscal externality due to white flight, corroborated by the fact that cities did not change their allocation of spending. In a second related study, Tabellini (2020a) jointly investigates the political and economic impact of European immigration to the US between 1910 and 1930, using a similar shift-share instrumental variable strategy. The author finds that reductions in redistribution stemming from greater migration inflows were more likely driven by natives' preferences and cultural distance. Our study distinguishes itself from these contributions by investigating the role of migration at a time where levels of discretion of municipal councils in Europe differ from the early 20th century US. In addition to the mechanism linking migration to local expenditure and revenue offered by Tabellini (2020a) and Tabellini (2020b), we are able to study a third potential mechanism, namely the change stemming from mechanical demand for local services induced by distinct demographic characteristics of migrants.

Exploiting a refugee placement policy in Denmark, Gerdes (2011) examines the effect of immigration on municipal redistributive spending and does not find any evidence of a change in public sector spending. However, as highlighted in Harmon (2018), the author's empirical strategy might suffer from the endogenous relocation choices of immigrants not covered by the policy as well as the political discretion in assigning migrants to different municipalities. We tackle the empirical issue of endogenous location choices of AC-12 migrants in England by presenting parallel trends comparing heavily and less heavily affected local authority areas in our main outcomes. We further show that all our results are robust to (i) matching local authorities on a wide range of 2001 Census characteristics and (ii) deploying a shift-share instrumental variable strategy based on historical settlement of AC-12 migrants in the tradition of Card (2001).

Jofre-Monseny et al. (2016) focus on the link between municipality-level variation in extra-EU immigrant density and local social spending in Spain. Instrumenting migration flows between 1998 and 2006 using the distribution of rental housing in 1991, the authors find that per capita social spending increases less in municipalities that experience the largest increases in immigrant density. While the authors report strong first-stage results in their instrumental variable regressions, one main concern with this particular identification strategy relates to the exclusion restriction. Municipalities with a relatively large supply of rental housing six years before the migration inflows are likely to also be poorer and larger than other cities and therefore may lie on differential trends that the authors do not account for. Municipalities' public finances might be affected in a way that social services spending may slow down six years later for reasons other than migration. A second concern relates to the authors' interpretation of their results. While Jofre-Monseny et al. (2016) do not study the impact of elections on native outflows and electoral outcomes, they interpret their findings as evidence for a materialisation of a shift in preferences for redistribution among the native population. We argue that this interpretation, although in line with predictions of in-group-out-group theories, is only one possibility. A decrease in redistributive public spending could also reflect a mechanical relationship introduced by migrants' socio-economic characteristics and/or their differing propensity to take up social services if inflows are sufficiently large and migrant characteristics are distinctly different from the local population.

The remainder of this paper is structured as follows. In the following section 2.2, we introduce the institutional setting of local authority spending and describe the nature of AC-12 migration flows. Section 2.3 discusses our data sources. Section 2.4 lays out our main identification strategy. Section 2.5 presents and discusses our results. Section 2.6 discusses alternative identification strategies and robustness. Section 2.7 discusses the main mechanisms of our results and section 2.8 concludes.

2.2 Institutional setting

In this section, we first provide an overview of the level of discretion local authorities hold over spending and revenue in subsection 2.2.1. We then describe the nature of AC-12 migration to England in more detail in subsection 2.2.2.

2.2.1 Local authorities in England

In total, there are 353 local authorities in England. England's local government structure is not homogeneous across the country. Local governments either function under a two-tier or a single-tier regime. The two-tier local authorities consist of an upper-tier, the county councils, and a lower-tier, the district councils. Single-tier authorities encompass 55 unitary authority councils, 36 metropolitan boroughs, 32 London boroughs, the Common Council of the City of London and the Council of the Isles of Scilly for a total of 125. Of these, we exclude nine new unitary authorities from our analyses that were only formed out of tow-tier authorities between 2007 and 2009. Two-tier authorities consist of 27 county councils which in turn are divided into 201 district councils.

While single-tier authorities bear the responsibility for all service spending decisions, county councils and district councils divide responsibilities between themselves in two-tier authorities (Sandford, 2018). To make local authorities comparable across England, we aggregate all lower-tier authority spending up into the upper tiers. This is unproblematic for two reasons. First, the areas where spending decisions are not clearly distinguishable only concern spending on cultural goods such as museums, transport planning, economic development and tourism. Second, spending decisions on the largest expenditure items such as education and social care - the focus of our study - are made on the upper-tier county council level. We provide more information on how areas of responsibility are divided between the two tiers in B.2 of the appendix.

On the revenue side, UK local authorities obtain their funding via a mix of specific, ring-fenced grant funding, general grant funding, the collection of business rate revenue and income from a local council tax. While there is currently a trend towards more devolution in revenue, particularly regarding the retention of local business rates, the discretion of local authorities was limited to steering the local council tax rate over our main observation period from 2000 to 2015 (Phillips, 2018). The council tax is a property tax collected by local authorities and its amount is based on the value of property. Each property is categorised into one of eight bands (A to H) and the tax is then due annually as a fixed fraction of local authority defined baseline tax band D. In 2001, the average band D council tax rate stood at GBP 898, rising to GBP 1,459 in 2015. Total tax income collected by local authorities is then simply the multiple of the band D tax rate and the tax base, i.e. the number of band D equivalent dwellings. Over our observation period, council tax income covers about 25% of annual total service expenditure. We summarise the other main sources of income from central government grant and centrally redistributed business rates in a 'central government transfers' measure.

The discretion of the elected Councillors over local authorities' spending is not limited to revenue collected from council tax, as only a small share of funding from central government grants is explicitly ring-fenced (Phillips, 2018). England, and the UK more generally, is one of the European countries where local governments have discretion over spending decisions that encompass several public expenditure items. UK local authorities are responsible for policies concerning education, social care services, highways, roads and transport, housing, cultural services, environmental services, planning and development and protective services such as fire and rescue (Sandford, 2018). In this context, it is important to note that, unless local authorities can temporarily draw on previously accumulated reserves, they are required to balance their budgets and are unable to borrow on financial markets.

In 2000, British local public spending represented GBP 113 million, a value that increased to GBP 198 million in 2015 in current prices and represents approximately 25% of total government spending.[2] English local authorities spend by far the largest share of their total service expenditure on education.

[2]OECD (2018a), 'Consolidated government expenditure as a percentage of total general government expenditure (consolidated).

In 2001, education made for 53% of all service expenditure, a value that has decreased slightly over time. The second largest share of total expenditure is spent on social care services (23.5% in 2001) which has increased over time. All other expenditure items combined make for less than one fourth of total service expenditure throughout our observation period.

In our analysis, we are particularly interested in means-tested services for which local authorities have discretion during our sample period. Social care services in particular fulfil these criteria while education partially fulfils them.

Social care services in the UK consist of adult social care and child social care. Adult social care entails a range of support services available to the physically or mentally impaired as well as other items where the level of uptake is most highly correlated with advanced age. It also assists disadvantaged groups such as asylum seekers or substance abusers. Adult social care accounts for the bulk of social care service expenditure in England over our main observation period (approximately 70% on average). Social care service minimum eligibility criteria are set by the central government but the amount spent on social care is at local authorities' discretion (Simpson, 2017). Phillips (2018) notes that Councils' discretion extended to determining what kind of services were offered, needs' assessments and eligibility criteria over the 2000 to 2015 observation period. The latter included different thresholds for what Councils considered the risk for an individual in absence of treatment. A detailed overview of local authority social care services and its means-testing criteria is provided in Table B.3 of appendix B.2.

While there has been a trend towards centralisation, the sample period we study still left room for local authority discretion when it comes to education expenditure. Education expenditure in the U.K. has traditionally been locally-managed although school funding was ring-fenced as of 2006 via the Dedicated Schools Grant (DSG), which de facto represents a minimum threshold below which school spending cannot fall for most of the sample period we study. However, local authorities could still exert upward discretion and use their own revenues to additionally fund education. Recent moves by the Conservative government to remove local governments from the education expenditure formula and only have a national funding formula, whereby schools with similar characteristics receive equal funding, are yet to come into place. As highlighted by Phillips (2018), education expenditures was still partially locally managed in our sample period.

In summary, for the purposes of this paper, two observations are therefore important: First, local authorities do have a significant amount of discretion over both the revenue they collect and the allocation of their funds across different spending areas but need to balance their budgets. Second, 1100 statutory spending requirements limit local authorities in their spending decisions to some extent and do not always allow for a clear distinction between mandatory and discretionary spending (Gray and Barford, 2018). This means that the larger the level of disaggregation of spending items, the more detailed knowledge of statutory spending requirements is necessary. We conduct our analysis on expenditure items aggregated at a relatively high level within the different spending areas to minimize this risk.

A local fiscal response to migration in England that reflects a change in redistribution could thus become visible through two main channels. First, migration could affect total spending per capita and revenue. Second, less redistribution could also become visible through a shift between expenditure items more strongly associated with redistribution and those less associated with redistribution. In the analysis below, we focus on social care spending per capita as the main redistributive item due to its free availability only to low-income individuals. It is less clear to which extent education spending falls under redistribution. Some authors have argued that inter-group transfers are more salient in education expenditure than in other non-means-tested services (Speciale, 2012; Tabellini, 2020a). In our analyses, we therefore leave education as a separate expenditure item. We then aggregate all expenditure that falls outside of social care and education into a third category.

2.2.2 The AC-12 migration shock

Following the EU accession of the so-called A-8 countries consisting of the Czech Republic, Estonia, Hungary, Latvia, Lithuania, Slovakia, Poland and Slovenia, as well as Cyprus and Malta in 2004 and Bulgaria and Romania in 2007, the UK experienced a large labour migration inflow of a migrant group commonly referred to as AC-12. Due to the fear of mass migration from the, on average, less wealthy accession countries, EU Member States were given the option to temporarily restrict the fundamental freedom of movement of people originating from the 2004 Central and Eastern European joiners. This was possible "under provisions in the Accession Treaty allowing the existing Member States to apply national measures regulating access to their labour markets for nationals of A-8 countries for up to seven years" (Kennedy, 2011, p.5).

All EU Member States but Sweden, Ireland and the UK applied these regulations on A-8 migrants (Anderson et al., 2006). Becker et al. (2016, p.11) explain that the decision by Tony Blair's Labour government not to limit labour market access to A-8 migrants was driven by "a thriving economy and a misunderstanding of the consequences of decisions by other big EU countries to keep their borders closed to Eastern European workers for a transition period".[3]. As highlighted by the authors, a UK Home Office commissioned study conducted in 2003 by Dustmann et al. (2003) computed their migration projections under the false assumption that other countries such as Germany would also open their borders in 2004. The government then failed to adjust their initial projections when other EU Member States did not open their border immediately and estimated that "only around 5,000-13,000 Eastern Europeans would arrive to the UK per year" (Dustmann et al., 2003) (as cited in Becker et al. 2016, p.11). However, between 2004 and 2007, more than 500,000 migrants arrived to the UK from Central- and Eastern Europe, vastly exceeding the initial projections (D'Auria et al., 2008).[4]

Thus, overnight on 1 May 2004, workers from the A-8 accession countries, Malta and Cyprus obtained full rights to live and work in the UK, including the right to stay permanently and the right to be joined by dependants (Anderson et al., 2006). Workers from A-8 countries (but not Malta and Cyprus) were obliged to register on the so-called Workers Registration Scheme (WRS). Registration on the WRS gave A-8 workers access to in-work benefits but they only had access to out-of-work benefits after they had been in registered employment for 12 months (Kennedy, 2011). For the purposes of this study, it is important to note that local authority services, including social care and education, were accessible to AC-12 workers as soon as they were registered on the WRS.

Characteristics of the AC-12 migrants

Table 2.1 summarises the AC-12 migrants' socio-economic characteristics relative to British natives, EU-15 migrants and the rest of the world based on the 2011 UK Population Census. The table shows age and employment characteristics that could potentially have had an impact on the local population's attitude towards AC-12 migrants. Both employment and young age may thereby reflect a lower likelihood of welfare dependency (Becker et al., 2017; Gherghina and O'Malley, 2019; Goodwin and Milazzo, 2017). As noted above, these characteristics may also reflect the group's need for local authority services. The migration inflow to the UK stemming from the EU enlargement was indeed both sizeable and characterised by the distinctive features of AC-12 migrants in terms of age, education and employment.

[3]One of the other motives cited for the Blair government agreeing to immediate free movement rights for A-8 citizens was that the UK saw a wider Europe as a way to provide the UK with new allies within the EU against the traditionally more pro-integration "old-Europe" States (see e.g., Bulmer 2008)

[4]It has been argued that the post-Brexit registration scheme figures suggest that the number of Central/East European migrants might have been under-estimated (see e.g., migrationobservatory.ox.ac.uk)

Most of the AC-12 migrants living in the UK in 2011 were in the 25 to 39 years' old categories. This contrasts sharply with the age structure of AC-12 migrants we observe in the 2001 UK Population Census where the distribution was much flatter. Other migrants from EU-15 countries and the rest of the world are also younger than the UK born on average but their age-distribution is less skewed to the left than that of AC-12. Cross-checking these numbers with those from the 2001 Census shows little to no movement. Taken together, this suggests that the inflow of migrants from AC-12 countries was distinctively younger than AC-12 migrants living in the UK pre-enlargement and that this change in the pattern of their age-structure was distinct to this group of migrants.

In terms of qualifications, the bulk of AC-12 migrants living in England in 2011 were categorised in the "apprenticeships and other" qualifications section that does not directly translate into the UK system of qualifications but is indicative of relatively low and medium skills. It is further worth noting that the 24% share in the highest skill category did not translate into a similar share of employment in high-skill professions (Drinkwater et al., 2009). Most of the residents born in AC-12 countries as per the 2011 census were working in routine or semi-routine occupations, again contrasting with the 2001 situation for AC-12 migrants residing in England in 2001 and other groups in both 2001 and 2011. Thus, at least over the years following the 2004 accession, the AC-12 migration flow was in its majority a labour migration flow into low and medium skills' professions. 80% of AC-12 migrants were economically active in 2011, a share significantly above that of UK born and other migrant groups.

Table 2.1 Socio-economic characteristics of immigrants vis-à-vis UK citizens and other

Age Structure	UK	EU-15	AC-12	ROW	**Family Structure**	UK	EU-15	AC-12	ROW
Age 0 to 15	21%	12%	10%	7%	No children	29%	34%	35%	24%
Age 16 to 24	12%	13%	15%	12%	One dependent child	20%	20%	29%	22%
Age 35 to 49	12%	23%	41%	23%	Tow or more dependent children	35%	34%	27%	38%
Age 50 to 64	20%	26%	20%	29%	All children non-dependent	16%	12%	9%	16%
Age 50 to 64	18%	13%	9%	18%	**Economic activity**				
Age 65 and over	17%	13%	5%	10%	Economically active: total	63%	69%	80%	63%
Gender					In employment	59%	64%	76%	57%
Males	49%	46%	48%	49%	Full-time	43%	50%	61%	42%
Females	51%	54%	52%	51%	Part-time	15%	14%	15%	15%
Highest level of qualification					Unemployed	5%	5%	4%	7%
No qualifications	23%	13%	16%	19%	Economically inactive: Total	37%	31%	20%	37%
Level 1 qualifications	14%	7%	7%	9%	Retired	23%	15%	6%	11%
Level 2 qualifications	16%	9%	8%	9%	Students	5%	9%	5%	9%
Level 3 qualifications	13%	10%	7%	9%	Looking after home or family	3%	4%	4%	8%
Level 4 and above	26%	40%	24%	37%	Long-term sick or disabled	4%	2%	1%	3%
Apprenticeships and other	7%	21%	37%	18%	Other	2%	2%	3%	5%

Notes: Own table based on data from the 2011 UK Census. ROW refers to rest of the world.

Finally, it should be noted that AC-12 migration inflows into the UK were also geographically and compositionally different from migrants of AC-12 countries that had settled in the UK prior to the 2004 enlargement shock (Becker et al., 2018). Before the AC-12 countries joined the EU, the stock of individuals who were born in any of the ten Central- and Eastern European accession countries was around 193,180. Unlike AC-12 migrants arriving in the UK after 2004, these migrants were mostly concentrated in the London area (Becker et al., 2016). Approximately 30% of this group had arrived before 1981 and consisted mostly of people born in Poland, who made up 42% of the stock of Eastern Europeans having arrived prior to 2004 (Becker et al., 2016). The number of these Polish-born migrants increased by a factor of seven, and the number of Eastern Europeans in the UK by a factor of five, such that the number of AC-12 migrants living in England represented approximately 2% of the English population in 2011, reaching 1,085,351 inhabitants.

2.3 Sampling frame and data sources

In this section, we first describe both the data on local authority expenditure and revenue and the migration data we use for our subsequent analyses, in subsections 2.3.1 and 2.3.2 respectively. We then describe additional data sources we draw on to obtain control variables and for the analyses of mechanisms that may explain the obtained results in section 2.3.3.

2.3.1 Local authority expenditure and revenue

Detailed panel data on local authority public finances is available from the Chartered Institute of Public Finance and Accountancy (CIPFA) website which gathers local authority budgeted expenditure and revenue data. We use this data set to identify exactly how the 116 single-tier authorities and 27 authorities operating under a two-tier regime allocated their funds between all different spending areas annually. We obtain these data for the period from 2000 to 2015 such that our sample consists of a total of 143 local authorities observed over a 16-year period. Table 2.2 summarises our main expenditure related dependent variables and also expresses these as expenditure shares for comparability.

Table 2.2 Summary statistics outcomes

Variable	Mean	Std. Dev.	N
Total expenditure per capita (pc)	1450.07	330.32	2028
Central government transfer (pc)	1086.04	335.83	2028
Council tax required (pc)	364.03	80.90	2028
Social care expenditure (pc)	375.15	102.68	2028
Education expenditure (pc)	734.42	189.07	2028
Other expenditure (pc)	340.66	94.51	2028
Share spent on social care	0.258	0.039	2028
Share spent on education	0.506	0.057	2028
Share spent on other items	0.236	0.037	2028

Notes: The acronym "pc" stands for per capita. Data aggregated and averaged over the 2000 to 2015 observation period for 143 local authorities when data is not missing in either the measures presented here or in the variables of interest in our regression analyses.

The main variables we construct from this data set are our measures of expenditure and revenue per capita: total service expenditure, central government transfers and the required locally generated revenue to balance the budget. We also use this data set to construct our set of expenditure measures on education, social care and aggregate measures of spending on other items.

2.3.2 Migration data

We draw our yearly data on population for each local authority from a special license of the UK Annual Population Survey (APS) between 2000 and 2015. This sample is obtained by aggregating waves of the Labour Force Survey (LFS) and the Labour Force Survey Boosts for England.[5] We refer to this data set as LFS/APS. We are then able to compute immigration figures of AC-12 migrants as well as natives, EU-15 and the rest of the world based on their country of birth. There are approximately 350,000 individuals per wave, making the LFS/APS the largest annual household survey in the U.K.. Although the LFS/APS is more robust than estimates based on one single LFS wave, concerns regarding the accuracy with which this survey precisely measures the shares of immigrant population at the local authority level remain, especially in years preceding the 2004 shock when the AC-12 stock in England was relatively small. In our empirical strategy, we explain that the way we construct our main variable of interest does not require us to make use of the AC-12 migrant population pre-2004 in our regression analyses.

We further verify the robustness of our results by exploiting 2001 and 2011 British Censuses, which capture information for the entire British population by country of birth by local authority at these two points in time. Despite the 2011 Census' advantage of reporting data on the self-reported year of arrival of different migrant groups for the years 2009, 2006, 2003, 2000 and 1990, concerns when using this

[5] See Cangiano (2010) for more details on the APS.

data set remain. Differential rates of out-migration across local authorities by AC-12 migrants by year of arrival could mean that an imputed measure of migration stocks would have a poor relationship with the actual stock for years further in the past. We find a local authority level correlation between the number of foreigners from AC-12 migrants having arrived before 2000 as per the 2011 Census and the number of AC-12 migrants as reported by the 2001 Census of 0.77. Overall, this suggests the LFS/APS is the preferable data set. We nevertheless use the 2001 and 2011 Censuses to check our results for robustness.

In sum, given several missing values in CIPFA and LFS/APS data, our total sample consists of 2,028 observations spanning from 2000 to 2015 for 143 English local authorities.

2.3.3 Additional data sources

Our main specification measuring the effect of AC-12 migration controls for a number of local authority level time varying characteristics. First, we control for local area population obtained from CIPFA to account for potential scale effects in service delivery, especially regarding education. Since the effect of AC-12 on local redistribution may also be conditional of the existing composition and heterogeneity of the population, we further control for the share of EU-15 and non-EU migrants obtained from the LFS/APS (Alesina et al., 2019; Tabellini, 2020b). Finally, we also control for local unemployment rates to account for the fact that local economic conditions are an important pull factor for labour migrants such as those originating from AC-12 countries.

We then construct additional dependent variables to understand the mechanisms driving our main results. These variables include information on the number of pupils per local authority, and the age structure of the population (both obtained from CIPFA). To test for a change in political preferences, we use yearly data on the political composition of local Councils compiled by "The Elections Centre" at Nuffield college, Oxford.[6] In England, local Councillors are elected in a staggered way for 4-year terms by the local population, with some Councils electing all of their Councillors at the same time and other Councils electing half or a third of their Councillors at each election.[7] In our analyses, we are particularly interested in the shares held by the Conservative and Labour party, England's two main parties of government. The Conservative party is traditionally regarded as representing less redistributive "small government" platforms during our period of study as highlighted by Fetzer (2019) who studies the impact of austerity measures conducted by the Conservative government in the 2010s on Eurosceptic attitudes and Taylor-Gooby (2013) who shows that an "analysis of manifestos for the two

[6]The data can be accessed on http://www.electionscentre.co.uk.

[7]See https://www.gov.uk/understand-how-your-council-works/local-councillors-and-elections for details.

main UK parties from 1987 to 2012" shows two patterns: First, Labour manifestos score higher than Conservative manifestos on references to social justice and pro-welfare content. Second, Conservative interest in social justice as equality or redistribution is limited, and is "virtually non-existent for the 1987, 2001 and 2005 elections" (p.36).

We further substantiate the strong differences between the Conservative and Labour party on redistribution in Figure 2.1.

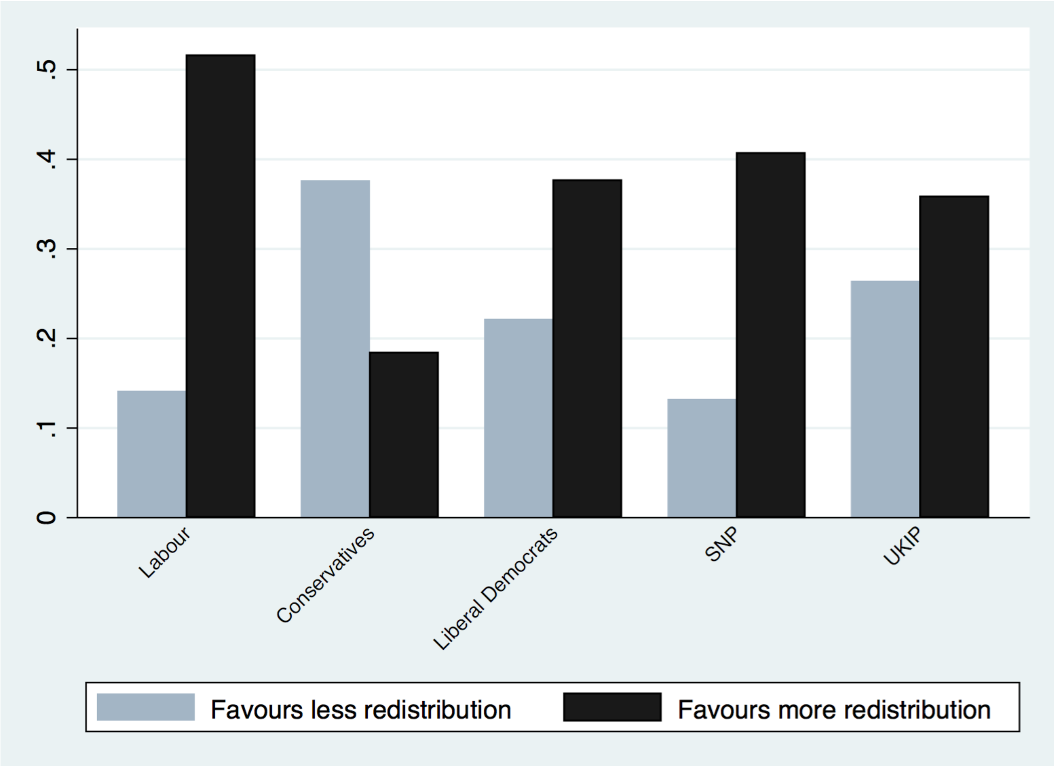


Figure 2.1 Redistribution preferences among voters of major British parties

Note: Figure based on data from the British Election Study (Wave 4, March 2015). The redistribution preference are calculated from the question "Should the government redistribute income?" to which respondents could respond on a scale from 0 ("make much greater effort") to 10 ("be much less concerned"). Respondents answering 0,1,2 or 3 are categorised as "favours more redistribution". Respondents answering 7,8,9 or 10 are categorised as "favours less redistribution". N=2840.

The figure based on data from the March wave of the British Election Study 2015 shows that more than 50% of Labour voters favour more redistribution of income, whereas that share stood at less than 20% among Conservative voters, who tend to favour less redistribution. Voters of Liberal Democrats, SNP and UKIP fall in between.

Unfortunately, the Council composition dataset aggregates information on the Council seat share of the UK Independence Party (UKIP) into an "other" category. Founded in 1991 and until the 2016 Brexit referendum, UKIP was essentially a single-issue party campaigning for an exit from the EU and made strong gains in European elections at the expense of the Conservative Party over our sample period (Becker et al., 2016; Fetzer, 2019; Ziebarth, 2020). However, it is is not clear that more votes for UKIP are a signal of preferences towards less redistribution, as highlighted by Figure 2.1 . Nevertheless, greater vote shares for UKIP in the face of AC-12 migration still suggest greater distaste for migration, which could potentially result in changes in local Councillors' spending decisions. Therefore, we additionally match our database with information on all local election results from Fetzer (2019), which contains election results of almost all parties including UKIP. Finally, we also use information from the second quarter of each LFS wave as well as NHS data to build local authority level measures of internal inflows and outflows.

We note that not all data we use for our analyses of mechanisms is available for the entire 2000 to 2015 observation period. We summarise data availability, sources and the measures we construct in Table 2.11.

2.4 Empirical strategy

In this section, we first lay out our main empirical strategy in subsection 2.4.1. We then turn to the main empirical issue in our setting at hand, the potentially endogenous location choice of migrants within England in subsection 2.4.2.

2.4.1 Main specification

The AC-12 migration wave to England, and the U.K. more generally, was a grand-scale natural experiment due to its large and unexpected nature. To capture its magnitude by local authority, we closely follow Becker et al. (2018) and build the following shock measure:[8]

$$Shock_{c,t} = \frac{AC12migrants_{c,t}}{LApop_{c,t}} - \frac{AC12migrants_{c,2001}}{LApop_{c,2001}} \tag{2.1}$$

where $AC12migrants_{c,t}$ represents the number of AC-12 migrants in local authority c in year t and $LApop_{c,t}$ local authority c's population in year t as per the APS while $AC12migrants_{c,2001}$ and $LApop_{c,2001}$ represent the AC-12 stock and total population in 2001 as per the 2001 Census. The shock measure thus represents the change in percentage points of the share of AC-12 migrants living in a given local authority relative to the share of AC-12 migrants in 2001. As argued earlier, the APS might not capture the share of AC-12 migrants by local authority pre-2004 well because of their low numbers. We

[8]The main contrast in the construction of our shock measure is that we use the LFS/APS instead of only using the 2001 and 2011 Censuses to measure changes in migrant shares.

therefore use the more precise 2001 census as our benchmark share of AC-12 migrants. Furthermore, we never use APS information regarding the AC-12 population pre-2004 as highlighted in our main empirical specification:

$$Y_{c,t} = \alpha_c + \beta_t + \gamma Post_{2004} * Shock_{c,t} + \delta X_{c,t} + \epsilon_{c,t} \tag{2.2}$$

In equation 2.2, $Post_{2004}$ is a dummy equal to 1 after 2004 such that its interaction with $Shock_{c,t}$ identifies a treatment on the treated effect of AC-12 migration on the outcome measures. $X_{c,t}$ represents our vector of controls which includes the local share of EU-15 migrants, the share of non-EU migrants, the local authority population and the local unemployment rate. We further include year fixed effects β_t and local authority dummies α_c to account for unobservable year-specific variation common to all local authorities and time-constant local authority level variation respectively. Due to the spatial nature of our data, we cluster the error term $\epsilon_{c,t}$ at the local authority district level in all analyses to correct standard errors for within local authority correlations (Bertrand et al., 2004). Our estimation is therefore akin to employing a two-way fixed effects panel model that combines features of a continuous Difference-in-Differences and event study as we set the pre-accession AC-12 stocks to zero. We do this due to the to the strong differences between AC-12 pre- and post-2004 and expect a gradual divergence of treated and untreated local authorities in the post treatment period, as not all migrants moved to the UK at the start of our treatment period.

Table 2.3 describes our main variable of interest - AC-12 migration shock - and controls' summary statistics.

Table 2.3 Summary statistics AC-12 shock and controls

Variable	Mean	Std. Dev.	N
AC-12 migration shock	0.011	0.015	2028
Share EU-15 migrants	0.027	0.023	2028
Share non-EU migrants	0.101	0.097	2028
Unemployment	0.068	0.026	2028
Population	358010	270538	2028

Local authorities' average population stands approximately at 358,000 inhabitants with the average increase in the share of AC-12 migrants representing a 1.1 percentage point increase in the share of a local authority's population compared to the 2001 baseline.

2.4.2 Threats to identification

The main concern for causal interpretation of γ is the endogenous location choices of AC-12 migrants. Despite the exogenous nature of the shock at hand, immigrants were not randomly allocated across local authorities and sorting into local authorities might be endogenous to migrants' underlying preferences for redistribution or other unobserved characteristics that determine both migration and trends in local authority spending. We first address this concern by presenting evidence in favour of the common trends assumption by showing that all measures of interest did not move systematically differently between affected and unaffected local authorities prior to 2004 in 2.5 and 2.6.

We also address the recent literature showing that linear regressions with group and time fixed effects are equivalent to estimating a weighted sum of the average treatment effects in each of these time periods and groups (see De Chaisemartin and d'Haultfoeuille 2020). As a result, two-way fixed effects coefficients can be of the opposite sign of the average treatment effect in each group and period in the presence of negative weights and heterogeneous across units treatment effects (see also Borusyak and Jaravel 2017, Callaway et al. 2021, De Chaisemartin and d'Haultfoeuille 2018, de Chaisemartin et al. 2019, De Chaisemartin and d'Haultfoeuille 2020, Goodman-Bacon 2021, and Sun and Abraham 2021). First, we show that we have some negative weights in our regression. These weights follow naturally from the two-way fixed effects procedure and are not of and in themselves concerning (Jakiela, 2021). We also compute the amount of treatment heterogeneity required for the average treatment on the treated and fixed effects coefficient to be of opposite signs (De Chaisemartin and d'Haultfoeuille, 2020). We then follow the diagnostics suggested by Jakiela (2021) to assess the likely severity of treatment heterogeneity. Namely, we test the robustness of our results to excluding later years in our sample when negative weights are most likely to occur (Jakiela, 2021).

We further address potential endogeneity concerns and the fact that one potential source of heterogeneity could stem from sorting into treatment (de Chaisemartin and D'Haultfoeuille, 2019) by showing that our results are practically unchanged when using alternative estimation procedures.[9]

We first show that results are practically unchanged when matching local authorities affected by AC-12 migration inflows to unaffected local authorities using propensity score matching. To do so,

[9] The way sorting into treatment might lead to heterogeneity can be conceptualised using the following intuitive example. Suppose migrants move to local authorities where they are relatively more welcome. As greater numbers of migrants arrive, the preference for redistribution decreases only slightly. On the other hand, local authorities where migrants are less welcome receive fewer migrants but preferences for redistribution could theoretically drop sharply: Local authorities treated with a lower dose of migrants may show a disproportionately larger spending reduction than those treated with a higher dose. The estimated fixed effects coefficient would then be of the opposite sign than the effect on each local authority.

we follow an approach similar to Becker et al. (2018) and first apply a corrected Akaike Information Criterion (AICc) algorithm to a large set of local authority characteristics we gather from the 2001 Census to find the best predictors for AC-12 migration inflows and then match our sample based on these variables (with replacement). While this approach reduces our sample size, it mitigates remaining concerns that destination choices were not picked at random and should be treated as complementary to our main difference-in-differences results.

Finally, we proceed to showing the results when using a shift-share "Bartik" instrumental variable approach based on historical settlement (Bartik, 1991). The problem of endogenous sorting of migrants has indeed often been tackled by using such an approach pioneered by Card (2001). It is based on the premise that immigrant networks are an important determinant of locational choices and allows to identify local average treatment effects of current migration inflows induced by these historical settlement patterns. In the context of the UK, this instrument has been used by Bell et al. (2013), Sá (2015) and Giuntella et al. (2018) in their studies on the impact of migration to the UK and its effect on crime, house prices and hospital waiting times respectively. The validity of such an instrument relies on the assumption that the past settlement of immigrants is uncorrelated with changes in economic outcomes between local authorities prior to 2003. Thus, it assumes that immigrant settlement patterns years before 2004 are only correlated with measures of local authority spending patterns though their effect on post-2004 inflows.

While we show that our results are stable when using such an approach based on 1991 settlements of AC-12 migrants, several problems have been identified in shift-share instrumental variables. Jaeger et al. (2018) first show that such instruments run the risk of conflating the short- and long-run responses to immigration shocks if the spatial distribution of immigrant inflows is stable over time. Second, Lee et al. (2020) find that current practice using instrumental variables typically relies on the first-stage F-statistic exceeding a threshold of 10 as a criterion for showing instrument relevance and trusting t-ratio inference yield an anti-conservative test.

In addition to these concerns, we believe this approach is potentially less relevant when applied to AC-12 migrants. Becker et al. (2018) point out that the historical distribution of Central and Eastern European may not capture subsequent AC-12 migration inflows well because of the stark differences between the two waves. The authors argue that migrants from Poland – the country where the largest share of AC-12 migrants originate from - who resided in the UK in 2001 mainly consisted of people of pension age, having lived in the UK since the second world war as remnants of the Polish Free Army, or of migrants who had entered before 1991 for graduate studies under high-skill visas. Thus, the baseline distribution of Central and Eastern European migration into the UK in 2001 would act as a weak proxy for subsequent migration flows or could pick up very specific parts of these migration flows. This is confirmed when conducting standard first-stage relevance tests but also by comparing the distribution of AC-12 migrants in 1991 and in 2015 as highlighted in Figure 2.2. The figure shows that recent AC-12 migration was more heterogeneously distributed across England than in 1991, when a concentration in London and its surroundings was more clearly visible. Nevertheless, the fact results are not affected by choosing this approach suggests we can be confident in the robustness and interpretation of our results.

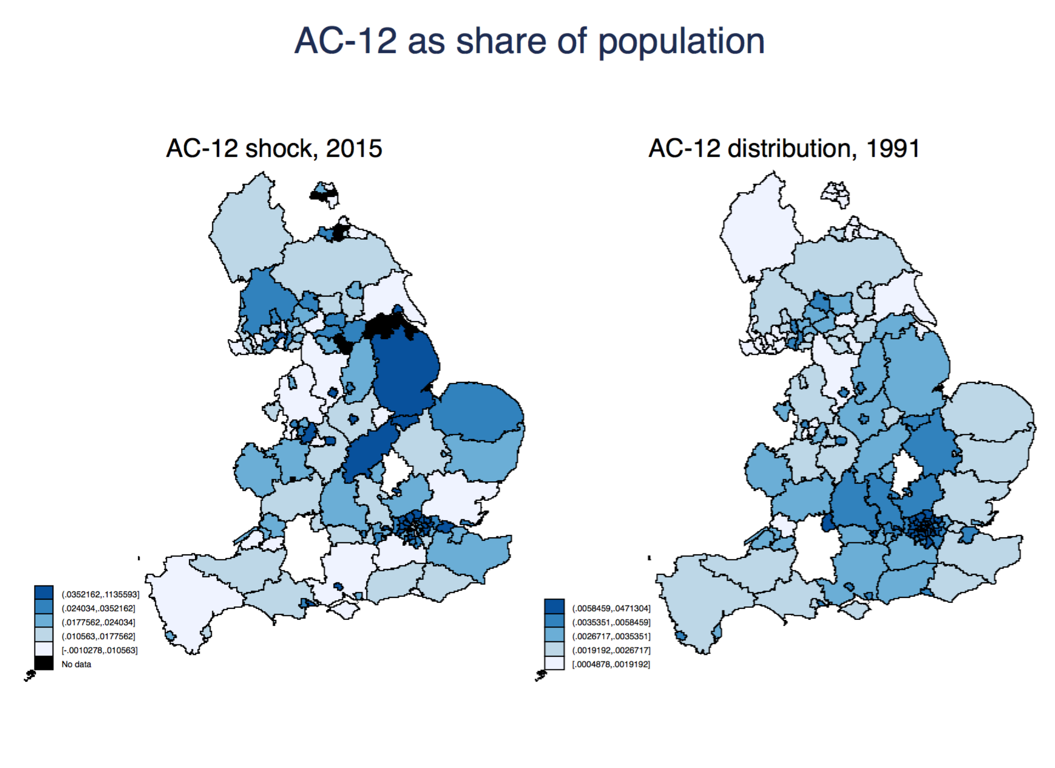


Figure 2.2 Contemporaneous AC-12 shock and historical distribution

2.5 Results

In this section, we turn to the results and split our analyses into two parts. Subsection 2.5.1 provides evidence in support of the identifying common trend assumption. Subsection 4.4 then shows the main results.

2.5.1 Pre- and post trends

In this subsection, we analyse the main outcomes of interest before and after the opening of UK borders to AC-12 migrants in 2004. Figure 2.3 shows that the share of AC-12 migrants indeed gradually picked up after 2004, starting from a level close to zero before the accession of the AC-12 countries.

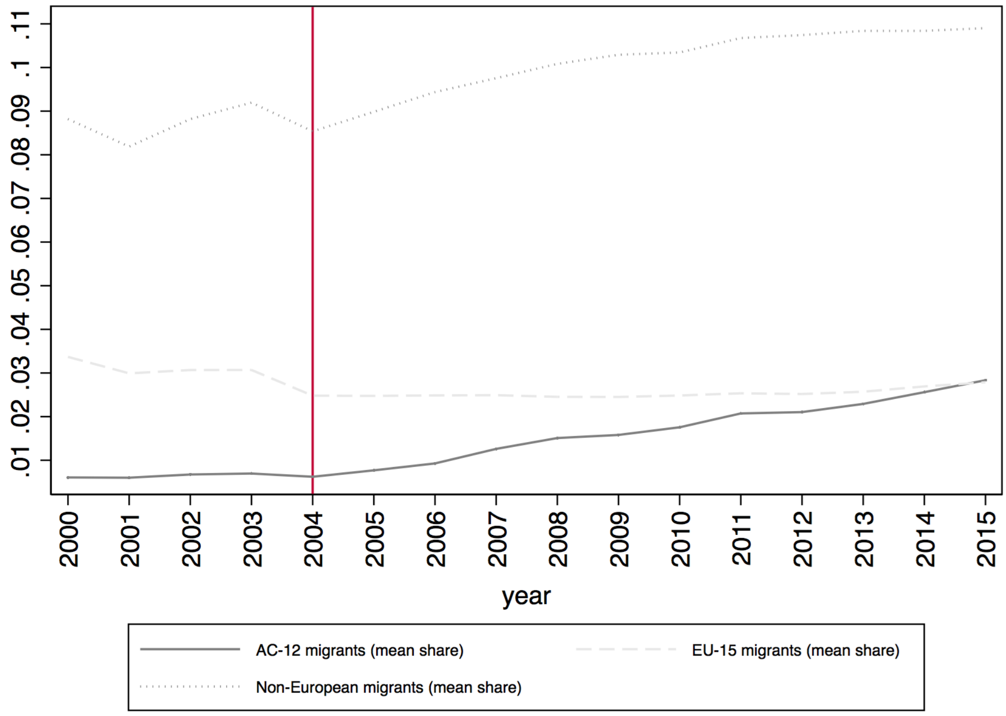


Figure 2.3 Migration to the UK by region of origin

The mean annual local population share of AC-12 born migrants in English local authorities fluctuated between 0.6% and 0.7% in the pre-accession years and then gradually increased to 2.8% in 2015. In relative terms, the average English local authority saw their AC-12 migrant population rising by more than 400% over an 11 year period.

Figure 2.3 further shows an increase in the local authority mean population share of non-EU migrants between 2004 and 2015 while the EU-15 population share remained stable on average. One of the concerns in our empirical analysis when focussing on one migrant group specifically is that location choices of AC-12 migrants may coincide with inflows of other migrant groups. In appendix B.3 we show that the spatial distribution of non-EU and EU-15 migrant inflows does not correlate visibly with the AC-12 shock visualised in Figure 2.2. We nevertheless control for these migrant shares in our preferred specification to adequately capture the diversity of the local area population as discussed in section 2.4.1.

To test for pre- and post-trends in our main outcome variables of interest, we define local authority districts that were in the top 25% (Q4) of the 2015 migration shock distribution as treated ("affected") . We further define all local authority districts that are within the bottom 25% (Q1) of the 2015 migration shock distribution as untreated ("unaffected"). Formally, we define

$$\tau_c = \begin{cases} 1, if \quad AC12Shock_{2015,c} > Q3(AC12Shock_{2015}) \\ 0, if \quad AC12Shock_{2015,c} < Q1(AC12Shock_{2015}) \end{cases} \tag{2.3}$$

The categorisation is motivated by the distribution of the AC-12 shock measure in our final observation year in 2015 shown in Figure 2.4.

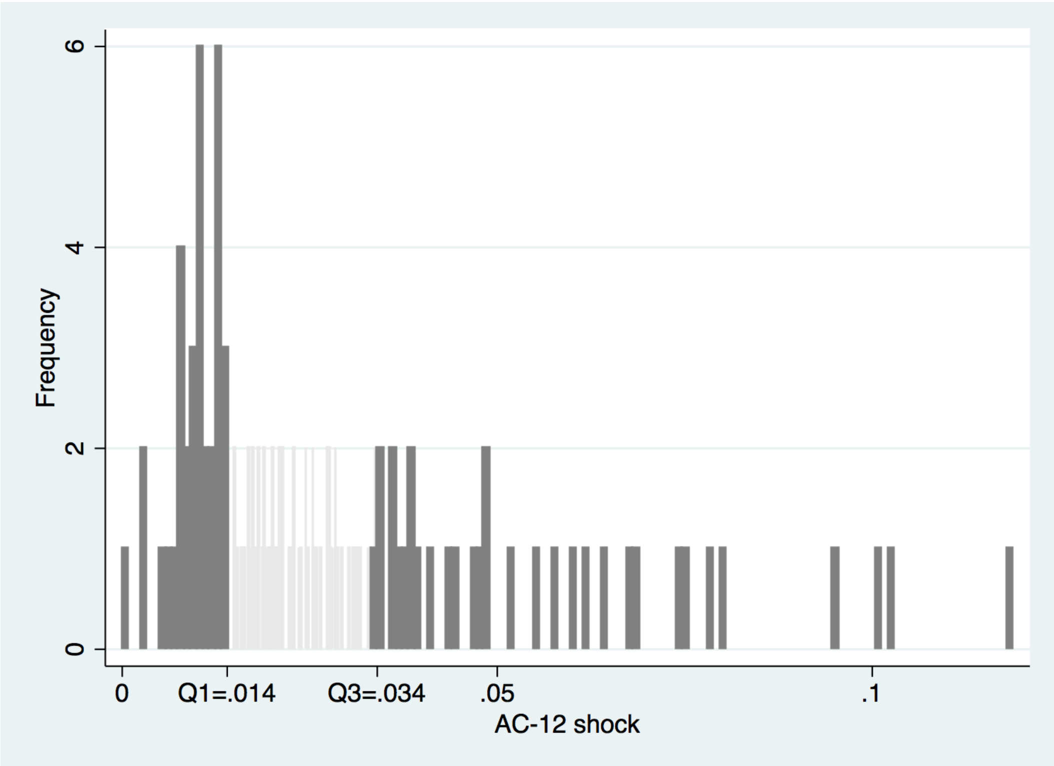


Figure 2.4 Distribution of AC-12 shock in 2015

Local authorities in the bottom quartile of the AC-12 shock distribution only saw their population share of AC-12 migrants rising by between 0.1% and 1.4%. The frequency distribution then has a long right tail with AC-12 migrant shares rising to between 3.4% and 11.9% in local authorities in the top quartile.

Figures 2.5 and 2.6 show the trends in the main outcome variables of interest, with the solid lines corresponding to regions most heavily affected by AC-12 migrant inflows and dashed lines indicating the least affected regions.

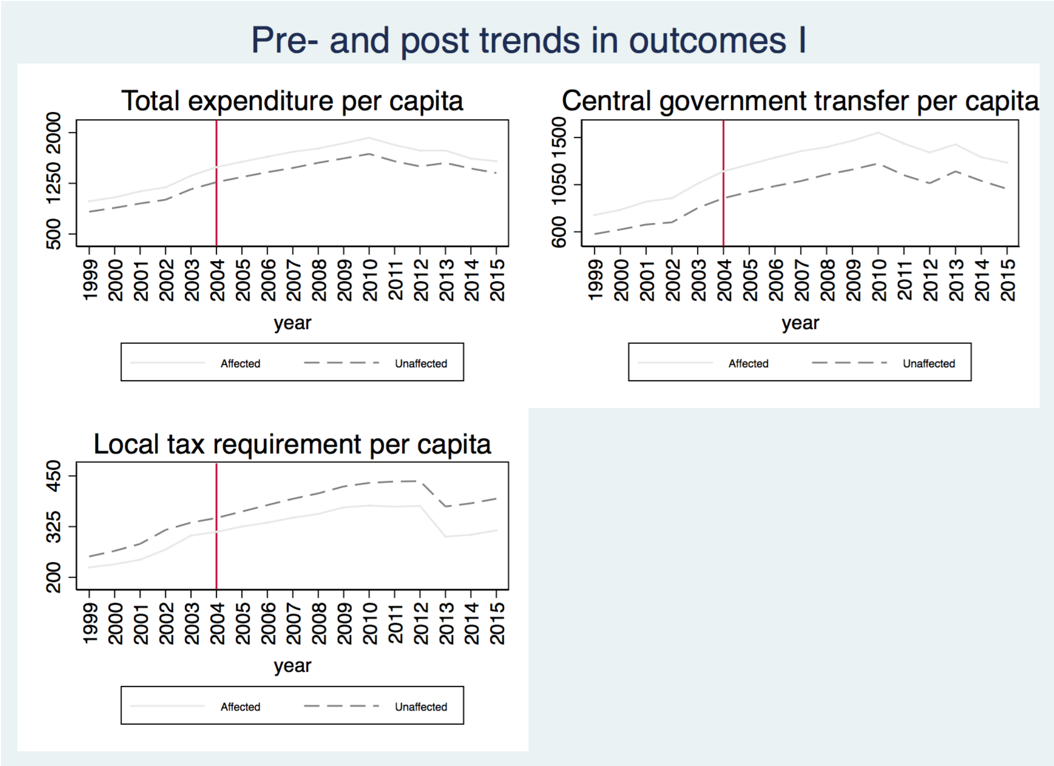


Figure 2.5 Pre- and post trend I

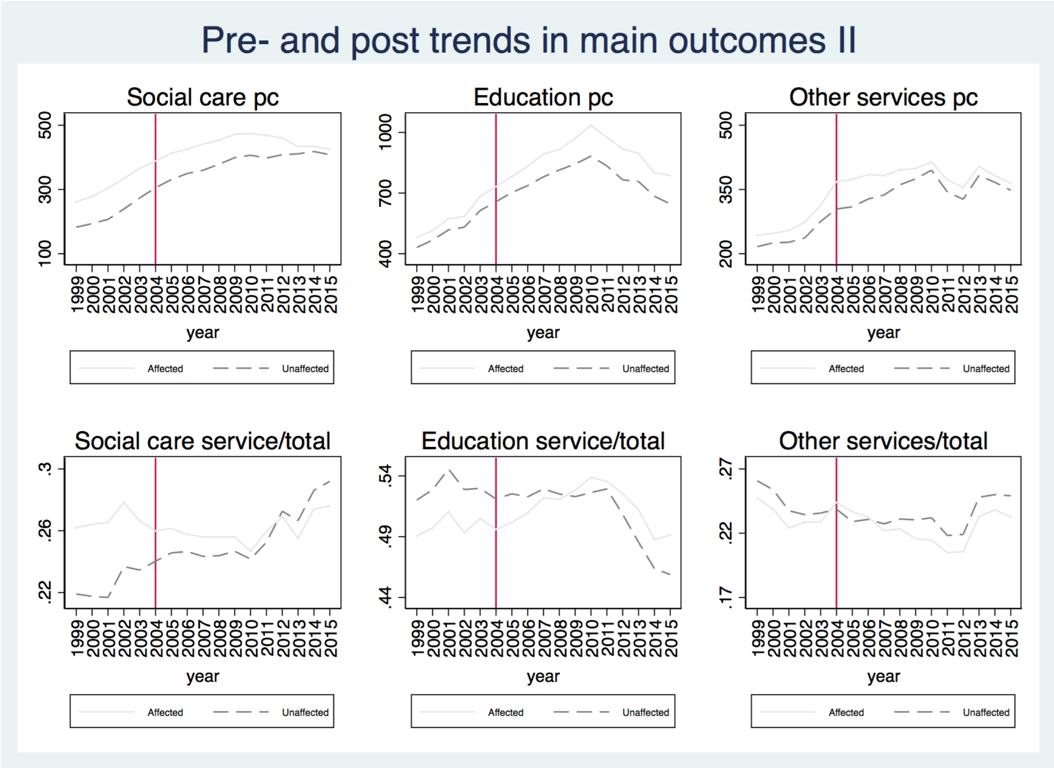


Figure 2.6 Pre- and post trend II

We first note that pre-trends from 1999 to 2003 are parallel in all outcomes, validating the difference-in-differences research design. We detect no visual changes in the total expenditure per capita gap post accession (Figure 2.5, upper left panel). Affected and unaffected local authorities continue to move in parallel after the onset of treatment. The parallel trend continues beyond 2010, when strict austerity measures were imposed on local authorities, a topic we turn to in greater detail in section 2.6.1. The lower left and upper right panel of Figure 2.5 suggest a marginal effect of AC-12 migrant inflows on the funding sources of local authorities. Taken together, the figures suggest that in affected areas, less funding was raised locally and slightly more funding came from central government sources following the inflow of AC-12 migrants, leading to the observed unchanged total per capita spending in an institutional setting where local authorities have to balance their annual budgets.

Figure 2.6 then turns to the spending mix. Social care expenditure per capita starts to rise significantly less in affected local authority areas when AC-12 migrants start to immigrate in 2004. The gap most visibly declines in the years from 2010 onwards (top right panel). The change in trends becomes even more visible when graphing social care as a share of overall local authority spending, where the social care expenditure share in unaffected local authorities passes the share spent in affected local authorities in 2012 (bottom right panel). Similarly, trends in education expenditure per capita start to diverge after 2004, with local authorities most heavily affected by AC-12 migrant inflows spending relatively more on education in per capita terms (top centre panel). The share spent on education in total spending surpasses that of unaffected areas in 2009 and the gap continues to widen up to the final year of the observation period (bottom centre panel). Other expenditure items expressed in per capita terms show slightly more noise but tend to converge after 2004, with affected areas experiencing a small relative decrease (Figure 2.6, upper left and bottom left panel).

In sum, three main findings emerge from this first descriptive analysis. First, total expenditure per capita remained unchanged following AC-12 migrant inflows. Second, the funding mix slightly shifted away from locally raised budget towards central government transfers. And third, AC-12 migrants shifted the expenditure mix away from social care expenditure towards education expenditure.

2.5.2 Main results

In this subsection, we turn to the regression results based on the empirical specification introduced in section 2.4. Table 2.4 shows the results of the effect of AC-12 migration on local spending and revenue sources. For the sake of clarity, we only show the coefficient of interest in the following tables. A regression table also showing the coefficients estimated on our main control variables is shown in Table B.1 of appendix B.1. We will refer to the specification with all controls as our preferred specification.

Table 2.4 Effect of AC-12 migration on local spending and revenue

	Total expenditure pc		*Central government transfer pc*		*Locally raised budget pc*	
	(1)	(2)	(3)	(4)	(5)	(6)
AC-12 shock	460.44	525.65	1014.67**	976.97**	-554.22***	-451.33***
	(459.70)	(457.89)	(483.08)	(472.25)	(96.65)	(87.04)
N	2028	2028	2028	2028	2028	2028
R^2	0.879	0.890	0.821	0.834	0.819	0.826
Time FE	Year	Year	Year	Year	Year	Year
Model	DiD	DiD	DiD	DiD	DiD	DiD
Full set of controls	No	Yes	No	Yes	No	Yes
Sample	LFS/APS	LFS/APS	LFS/APS	LFS/APS	LFS/APS	LFS/APS

Standard errors clustered at the local authority level in parentheses.
* p<0.10, ** p<0.05, *** p<0.01

Notes: Outcomes expressed in per capita terms (pc). The AC-12 shock is defined as the difference in AC-12 population shares in a given local authority-year and its 2001 baseline share as defined in equation 2.1 of section 2.4.1. All regressions include local authority fixed effects. The full set of controls refers to the share of EU-15 migrants, the share of non-EU migrants, the local authority unemployment rate and the total local population. LFS/APS refers to the UK Labour Force Survey and its boost samples in the Annual Population Survey. The observation period is 2000 to 2015.

Column (1) and (2) show that the association between the AC-12 shock measure and total service expenditure per capita is positive, albeit not significant at any conventional level. Similar to the descriptive analysis in subsection 2.5.1, local authority revenue sources were indeed impacted by AC-12 migration: Column (3) indicates that a 1 percentage point increase in the AC-12 shock over the 2001 baseline shares is associated with an annual increase of approximately GBP 10 in central government transfers per capita to affected local authorities ($p<0.05$). The result is remarkably stable to the inclusion of controls for local labour market conditions, population and other migrant groups (column 4). The results of column 5 and 6 show that a percentage point increase in the AC-12 migrant population share over the 2001 baseline is associated with a GBP 5 decline in the locally raised budget per capita ($p<0.01$). The coefficients are again stable when adding controls variables. It is worth noting that the coefficients estimated on EU-15 and non-EU migrant shares point into the same direction. These coefficients shown in columns (1), (2) and (3) of Table B.1, appendix B.1, indicate that AC-12 migration affected central government transfers in a similar manner compared to migrants from other regions.

Table 2.5 then turns to the results of the effect of AC-12 migration on local expenditure by area of spending.

Table 2.5 Local authority spending by area

	Social care expenditure pc		*Education expenditure pc*		*Other expenditure pc*	
	(1)	(2)	(3)	(4)	(5)	(6)
AC-12 shock	-477.89**	-414.47**	1214.59**	1179.50**	-279.10	-238.21
	(219.71)	(204.35)	(574.23)	(564.80)	(178.60)	(152.78)
N	2028	2028	2028	2028	2028	2028
R^2	0.771	0.790	0.750	0.757	0.641	0.654
Time FE	Year	Year	Year	Year	Year	Year
Model	DiD	DiD	DiD	DiD	DiD	DiD
Full set of controls	No	Yes	No	Yes	No	Yes
Sample	LFS/APS	LFS/APS	LFS/APS	LFS/APS	LFS/APS	LFS/APS

Standard errors clustered at the local authority level in parentheses.
* p<0.10, ** p<0.05, *** p<0.01

Notes: Outcomes expressed in per capita terms (pc). The AC-12 shock is defined as the difference in AC-12 population shares in a given local authority-year and its 2001 baseline share as defined in equation 2.1 of section 2.4.1. All regressions include local authority fixed effects. The full set of controls refers to the share of EU-15 migrants, the share of non-EU migrants, the local authority unemployment rate and the total local population. LFS/APS refers to the UK Labour Force Survey and its boost samples in the Annual Population Survey. The observation period is 2000 to 2015.

The results indicate that the AC-12 migrant shock indeed changed the local authority spending mix. The results of our preferred specification of column (2) show that a 1 percentage point increase in the shock measure is associated with an approximate GBP 4 decrease in annual per capita spending on social care ($p<0.05$). Column (4) shows that education spending per capita increased by approximately double that magnitude ($p<0.01$). The increase in AC-12 migrant shares further had a small negative effect on other expenditure items, albeit estimated effects are not significantly different from zero at any conventional statistical level. In comparison, changes in migrant shares from other regions of origin had less impact on the expenditure mix. Column (4) of Table B.1, appendix B.1, suggests that the positive association of EU-15 migrant shares and total expenditure per capita (column 1, B.1) is the result of an increase in additional spending on education associated with this particular migrant group. Neither EU-15 migrants nor non-EU migrants appear to be associated with changes in social care expenditure (column 4, Table B.1).

In sum, three main results emerge. First, the inflow of AC-12 migration does not show a statistically significant association with total service expenditure per capita. Second, AC-12 migration did impact on local authority revenue sources: They are associated with an increase in funding received from the central government but decreased locally generated income. Finally, AC-12 migration decreased social care spending per capita and increased education expenditure per capita, while other expenditure items remained largely unchanged. We note that these findings do not allow for making definite statements about the mechanisms at hand: A reduction in the mostly discretionary means-tested social care services and the relative reduction of locally generated revenue in response to AC-12 inflows could indicate both a shift in local preferences towards less redistribution but could also capture changes in local service demand brought about by distinct socio-economic characteristics of new-arrivals, with repercussions for necessary funding. We analyse these mechanisms in more detail in section 2.7, after testing the robustness of our results in section 2.6.

2.6 Robustness tests

In this section, we test the robustness of our main results along a range of dimensions. Subsection 2.6.1 excludes all years post 2010 to account for the potentially endogenously imposed austerity measures on local authorities in 2010 as well as the expiration of the UK's Worker Registration Scheme (WRS) in 2011. If, for example, austerity measures were imposed on the central government level such that more diverse areas were more heavily affected, these would not necessarily reflect a local change in preferences for redistribution. In subsection 2.6.2 and 2.6.3 we further address potential endogenous sorting of AC-12 migrants by showing the results of a matching and an instrumental variable approach. These robustness tests thus account for the risk that AC-12 migrants potentially chose their destinations within England based on observed or unobserved local authority characteristics that could be correlated with our outcome measures.

In addition to addressing potential endogeneity concerns, these additional checks also address questions related to negative weights and treatment heterogeneity potentially leading the fixed effects coefficient to have an opposite sign to the average treatment on the treated in each group and period.

De Chaisemartin and d'Haultfoeuille (2020) show that the absolute value of the fixed effect coefficient divided by the standard deviation of the weights is equal to the minimum value of the standard deviation of the average treatment effects across time-period treated units under which the the linear regression coefficient and the average treatment on the treated effects may be of opposite signs. Estimating that ratio can thus help assess the robustness of the two-way fixed effect coefficient. If it is close to 0, that coefficient and the average treatment on the treated can be of opposite signs even under a small plausible amount of heterogeneity. In this case, treatment heterogeneity may be a concern. Conversely, treatment heterogeneity is less of a concern if this ratio is large in which case the coefficient and the average treatment on the treated can be of opposite signs only under an implausibly large amount of treatment effect heterogeneity. Focussing on education per capita, we find that 1,005 out of 1,974 ATTs receive negative weights. While negative weights are bound to exist, applying the Stata *twowayfeweights* command proposed by De Chaisemartin and d'Haultfoeuille (2020) shows that the average treatment on the treated and the FE coefficient on education per capita may be of opposite signs if the standard deviation of the treatment effect across the local authority-year observations is equal to 290.87, a large but possible amount of heterogeneity. Given the absence of stable groups between some years in our setting, we cannot compute De Chaisemartin and d'Haultfoeuille (2020)'s proposed alternative estimator. However, the following robustness tests help us alleviate concerns related to treatment heterogeneity. As suggested by Jakiela (2021), many of the negative weights in our setting are likely to affect later years in our sample when treatment intensity is stronger. The stability of results to excluding years post-2010, when the WRS was removed, provides a first reassuring diagnostic. Moreover, showing that results are robust to alternative specifications should further contribute to alleviating these concerns given heterogeneity can also stem from treatment being determined by selection into treatment (de Chaisemartin and D'Haultfoeuille, 2019).

Finally, in subsection 2.6.4 we use UK Census data instead of the more volatile Annual Population Survey data as a more accurate but static measure of AC-12 migration.

2.6.1 Excluding austerity year and the end of WRS

Austerity measures imposed on local authorities by the central government dramatically reduced transfers to local authorities from 2010 onward, putting pressure on local authorities to raise budget locally. These measures have been shown to have uneven effects on local authorities across England, mostly affecting urban areas, the north of England, parts of the East and Cornwall (Gray and Barford, 2018). If these geographically heterogeneous budget cuts imposed by the central government correlate with the different intensity of AC-12 migration inflows for either political or institutional reasons unrelated to *local* preferences for redistribution, they could potentially taint the estimated effects.

A further important institutional change coinciding with the introduction of austerity measures is the expiration of the UK's Worker Registration Scheme (WRS) in 2011. The WRS stipulated that AC-8 migrants - that is, migrants who originate from the 2004 Central and Eastern European EU joiners - could claim out-of-work benefits and tax credits on the same grounds as other EEA nationals only after being registered to the WRS and in continuous employment for 12 months. Giua (2020) shows that the expiration of the WRS had a positive impact on the probability of claiming out-of-work benefits by these migrants. Access to some of the local authority services such as social care services only required registering on the scheme, mitigating the concern of a pick-up in demand after the expiration of WRS (Kofman et al., 2009). However, the pick-up in claims of out-of-work benefits by AC-8 migrants post 2011 may have had a second-order effect on demand for means-tested local authority services due to its direct effect on income. A general link between local unemployment rates and demand for means-tested social care services is indeed visible in our regressions (column 4, Table B.1, appendix B.1). To account for these two important regime changes, we therefore test our results for robustness when excluding post-2010 years.

Table 2.6 shows the results estimated in our preferred specification when excluding all years post 2010.

Table 2.6 Effect of AC-12 migration on local spending and revenue - pre-austerity

	(1)	(2)	(3)	(4)	(5)	(6)
	Total expenditure pc	Central government transfers pc	Locally raised budget pc	Social care pc	Education pc	Other pc
AC-12 shock	787.90*	1027.55**	-239.65**	-184.75	1309.79***	-336.15
	(463.06)	(471.63)	(104.79)	(179.80)	(435.48)	(206.63)
N	1343	1343	1343	1343	1343	1343
R^2	0.953	0.925	0.872	0.874	0.933	0.734
Time FE	Year	Year	Year	Year	Year	Year
Model	DiD	DiD	DiD	DiD	DiD	DiD
Full set of controls	Yes	Yes	Yes	Yes	Yes	Yes
Sample	LFS/APS	LFS/APS	LFS/APS	LFS/APS	LFS/APS	LFS/APS

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.01

Notes: Outcomes expressed in per capita terms (pc). The AC-12 shock is defined as the difference in AC-12 population shares in a given local authority-year and its 2001 baseline share as defined in equation 2.1 of section 2.4.1. All regressions include local authority fixed effects. The full set of controls refers to the share of EU-15 migrants, the share of non-EU migrants, the local authority unemployment rate and the total local population. LFS/APS refers to the UK Labour Force Survey and its boost samples in the Annual Population Survey. The observation period is 2000 to 2010.

The results broadly confirm those of the longer panel shown in Tables 2.4 and 2.5. All coefficients are of the same sign in the early years of AC-12 inflows, suggesting that neither the expiration of the WRS nor the centrally imposed austerity measures fundamentally changed the effect AC-12 migrants had on local authority spending and revenue. Two observations are nevertheless noteworthy. First, all results are slightly less precisely estimated, likely a consequence of the significantly smaller sample size. Second, the coefficient estimated on social care spending per capita is smaller than in the full sample (column 4). One possible reasons for the amplification of observed changes in social care spending during the austerity years is a progressive adaptation by local authorities to changing demographics. A second possible explanation is a delay in the change in local preferences for redistributive polices. On the other hand, the more gradual increase in the demand for education services in local authorities more heavily affected by the migration shock is in line with the observation that many AC-12 migrants were of child-bearing age when they arrived to the UK, or required additional spending due on language schooling. We analyse these hypotheses in more detail in section 2.7.

2.6.2 Matching

The pre-trends we present in Figures 2.5 and 2.6 show that AC-12 migrants did not systematically sort into local authorities based on trends in local spending or revenue regimes. However, a concern remains that destination choices were not picked at random: If AC-12 migrants moved into local authorities of specific underlying unobserved characteristics, these characteristics could have medium- to long-term consequences for local spending and revenue patterns that are then picked up by our estimates. For example, since AC-12 migrants were primarily labour migrants of a distinct skill profile, their destination choices were likely based on labour demand from specific industries; if the presence of these industries was linked to future local economic development in local authority areas, the association of AC-12 migrant shares and local area spending may be spurious.

In this subsection, we address this concern by matching affected and unaffected local authorities based on a wide range of local authority characteristics we draw from the 2001 UK Census. The local area characteristics we use for the matching procedure range from local authority expenditure and revenue measures to local household wealth indicators, local industry composition and demographics. A full list of variables is provided in appendix B.4. The simple matching-with-replacement estimation proceeds in three steps. Similar to our pre and post-trend analysis, we first divide local authorities into affected (=treated) and unaffected (=untreated) based on the increase in AC-12 migrants they experienced between 2004 and 2015. To increase the pool of potential matches, we define the top 50%

receiving areas as affected and the bottom 50% of receiving areas as unaffected for the sake of the matching exercise. In a second step, we then use a simple stepwise Akaike's corrected information criterion (aicc) to select variables that best predict whether or not a local authority was treated, balancing an increase in the goodness-of-fit against the additional information required to achieve it (Cavanaugh and Neath, 2019). The variables selected by the algorithm are highlighted in the full list shown in appendix B.4. In summary, AC-12 migrants were most likely to migrate into local authority areas with relatively larger manufacturing, hotel, health, fishing, financial and domestic work industry shares. Further variables that predict AC-12's propensity to migrate into specific areas include household shares deprived in one dimension and the share of social housing provided by the local Council. In the final step, we then use propensity score matching to match treated and untreated local authorities based on the selected variables.

Table 2.7 shows the results of the matching regressions.

Table 2.7 Effect of AC-12 migration on local spending and revenue - matching

	(1) Total expenditure pc	(2) Central government transfers pc	(3) Locally raised budget pc	(4) Social care pc	(5) Education pc	(6) Other pc
AC-12 shock	664.77	1187.73**	-522.96***	-402.99*	1475.97**	-404.50**
	(520.56)	(539.25)	(115.50)	(233.09)	(616.92)	(174.83)
N	1295	1295	1295	1295	1295	1295
R^2	0.883	0.826	0.808	0.771	0.752	0.651
Time FE	Year	Year	Year	Year	Year	Year
Model	DiD (matched)	DiD (matched)	DiD (matched)	DiD (matched)	DiD (matched)	DiD (matched)
Full set of controls	Yes	Yes	Yes	Yes	Yes	Yes
Sample	LFS/APS	LFS/APS	LFS/APS	LFS/APS	LFS/APS	LFS/APS

Standard errors in parentheses

* p<0.10, ** p<0.05, *** p<0.01

Notes: Outcomes expressed in per capita terms (pc). The AC-12 shock is defined as the difference in AC-12 population shares in a given local authority-year and its 2001 baseline share as defined in equation 2.1 of section 2.4.1. All regressions include local authority fixed effects. The full set of controls refers to the share of EU-15 migrants, the share of non-EU migrants, the local authority unemployment rate and the total local population. Local authorities in the top 50% of AC-12 migrant receiving areas in 2015, measured by the AC-12 shock, are defined as affected and matched with the bottom 50% of AC-12 migrant receiving areas based on 2001 UK Census characteristics described in appendix B.4. Matching conducted with replacement.LFS/APS refers to the UK Labour Force Survey and its boost samples in the Annual Population Survey. The observation period is 2000 to 2015.

Matching local authorities based on their 2001 characteristics does not alter the main results. All estimated coefficients are of similar magnitude compared to the results based on the unmatched sample in Tables 2.4 and 2.5. The results continue to indicate a shift in revenue sources away from locally raised budget towards central government transfers (columns 2 and 3) and a shift in expenditure from social care towards education (columns 4 and 5). The drop in local service expenditure outside education and social care in response is more precisely estimated in the matched sample ($p<0.05$). Overall, the results suggest that spatial sorting among AC-12 migrants based on observable local authority characteristics is not a large concern in the post-accession English setting. To further robustify this finding, we next turn to an an instrumental variable approach in subsection 2.6.3.

2.6.3 Instrumental variable approach

As discussed in subsection 2.4.2, a common way of dealing with endogenous destination choices is to instrument contemporaneous migration inflows by historical settlement patterns. This instrumental variable was pioneered by Card (2001) and is based on the premise that immigrant networks are an important determinant of locational choices (Altonji and Card, 2018). In the context of the UK, such a "shift-share" instrument has been used by Bell et al. (2013), Sá (2015) and Giuntella et al. (2018) in their studies on the impact of migration to the UK and its effect on crime, house prices and NHS waiting times respectively. In our setting, the identifying assumption of such an instrument is that the historical settlement of AC-12 immigrants is correlated with post-2004 changes in local authority spending and revenue only through their effect on post-2004 inflows of AC-12 migrants. In this subsection, we use 1991 UK Census data to construct the instrument similar to Giuntella et al. (2018). Specifically, we define $\lambda_{c,1991}$ as the share of AC-12-borns living in local authority c in 1991 and calculate:

$$\frac{\lambda_{c,1991} \sum AC12_t}{Pop_{c,t}}. \tag{2.4}$$

Thus, for each local authority c and year t, we multiply the 1991 share of AC-12 migrants residing in that local authority by the aggregate national level stock of AC-12 migrants in year t to project the new stock of AC-12 migrants. We then divide this number by the local area population $Pop_{c,t}$ to derive the projected shares.

We do not use the IV approach as our preferred specification for a number of reasons. First, in the context of AC-12 migration, Becker et al. (2016) point out that it is unclear whether such a shift-share instrument captures the skill-composition of AC-12 migrants well. The authors argue that migrants from

Poland – the country where the largest share of AC-12 migrants originate from - who resided in the UK in 1991 mainly consisted of people of pension age, having lived in the UK since the second world war as remnants of the Polish Free Army, or of migrants who had entered before 1991 for graduate studies under high-skill visas. This would mean that the baseline distribution of Eastern European migration into the UK in 1991 would act as a weak proxy for subsequent migration flows, a point confirmed by the relatively weak first-stage F-test of 5.93 shown in Table 2.8. In the context of our research question, the distinct nature of historical inflows from AC-12 settlement in the UK raises an equally important question pertaining to the specifics of the local average treatment effects (LATE) such an instrumental variable identifies. The subset of contemporaneous AC-12 migrants pulled in by historical settlers from AC-12 countries is likely to be different from those AC-12 migrants who came to England for work. Thus, any LATE identified is potentially very different from the average treatment effect of AC-12 migrants on local authority spending researchers and policymakers are ultimately interested in.

With these caveats in mind, Table 2.8 displays the results of the IV-regressions.

Table 2.8 Effect of AC-12 migration on local spending and revenue - IV approach

	(1)	(2)	(3)	(4)	(5)	(6)
	Total expenditure pc	Central government transfers pc	Locally raised budget pc	Social care pc	Education pc	Other pc
AC-12 shock	3092.22	5002.80**	-1910.58**	-3293.37***	5853.21***	551.08
	(2052.34)	(2471.26)	(855.56)	(1215.52)	(2055.41)	(826.78)
N	2028	2028	2028	2028	2028	2028
Time FE	Year	Year	Year	Year	Year	Year
Model	DiD - IV	DiD - IV	DiD - IV	DiD - IV	DiD - IV	DiD - IV
Full set of controls	Yes	Yes	Yes	Yes	Yes	Yes
Sample	LFS/APS	LFS/APS	LFS/APS	LFS/APS	LFS/APS	LFS/APS
First-stage F-test	5.93	5.93	5.93	5.93	5.93	5.93

Standard errors in parentheses

* p<0.10, ** p<0.05, *** p<0.01

Notes: Outcomes expressed in per capita terms (pc). The AC-12 shock is defined as the difference in AC-12 population shares in a given local authority-year and its 2001 baseline share as defined in equation 2.1 of section 2.4.1. DiD - IV refers to a difference-in-differences approach where the AC-12 shock variable is instrumented by the historical distribution of AC-12 migrants across England as defined in 2.4. All regressions include local authority fixed effects. The full set of controls refers to the share of EU-15 migrants, the share of non-EU migrants, the local authority unemployment rate and the total local population.LFS/APS refers to the UK Labour Force Survey and its boost samples in the Annual Population Survey. The observation period is 2000 to 2015. The F-test of the first-stage regression is the Kleibergen-Paap rk Wald F statistic. It is reported only for the post-shock period.

The LATE estimates obtained from the instrumental variable regressions confirm the main results. When interpreted causally, the results show that the subset of AC-12 migrants pulled in by historical networks had sizeable effects on both local authority revenue sources and the spending mix. All estimated coefficients are between five and eight times larger than those obtained in our preferred specifications of Tables 2.4 and 2.5. We note that the difference in the magnitude of the obtained coefficients allows for two possible interpretations: First, the treatment on the treated effects obtained from the difference-in-differences specification underestimate the effects AC-12 migration had on local authority budgets and service provision. A second interpretation is that the estimated LATE is identified based on a subset of migrants that differs significantly from the average group characteristics of AC-12. For the reasons outlined above, we believe that the second is more likely to hold true. We conclude that the estimates obtained in Tables 2.4 and 2.5 are likely to be closer to the true effect AC-12 had on local authority service provision.[10]

2.6.4 Census data

Despite being relied on heavily in the UK-migration literature as a source of tracking local authority level migration stocks over time in the UK (Bell et al., 2013; Giuntella et al., 2018; Sá, 2015), the LFS/APS data we use as our primary data source has some disadvantages regarding its ability to accurately capture migrants of specific countries of origin on a granular geographical level (Cangiano, 2010). The absolute number of AC-12 migrants or other migrant groups sampled in each local authority-year is sometimes too low to rely on in our empirical analyses, leading us to discard observation points when these counts fall below 10. In this section, we therefore confirm the main results using 2001 and 2011 Census data instead of the more volatile LFS/APS. Both censuses provide data at the local authority level of residents by country or region of birth while the 2011 Census provides information on the time of arrival of 2011 residents in two- or three-year brackets. As we do not have information within these year brackets, we calculate the stock of migrants as the last year of each bracket: For example, a migrant reporting to have entered a local authority district five to seven years ago in 2011 enters our stock calculation for the year 2006. This way, we obtain stock data for AC-12 migrants in every local authority district for the years 2000, 2003, 2006, 2009 and 2011. A disadvantage compared to our main AC-12 shock measure is that all results are estimated exploiting year-on-year variation within local authorities only when stock data is available. The way the stock of residents is reported in the Censuses further means that it does not count migrants who arrived in England before 2011 and then left England before 2011 or who died

[10] In theory, the relatively weak first stage F-test (F=5.93) may both lead to a bias of the estimated coefficient towards OLS -in which case the true coefficients would be even larger than those obtained - and an underestimation of the size of the standard errors around these point estimates (Murray, 2006).

before 2011. The stock of migrants reported for every year is therefore a lower bound estimate of the true migration inflow. The alternative shock measure is summarised in Table 2.9.

Table 2.9 Summary statistics

Variable	**Mean**	**Std. Dev.**
AC-12 migration shock (alternative)	0.013	0.017
N	2028	

Table 2.10 then turns to the results using the alternative AC-12 migration shock measure based on Census data.

Table 2.10 Effect of AC-12 migration on local spending and revenue - Census data

	(1)	(2)	(3)	(4)	(5)	(6)
	Total expenditure pc	Central government transfers pc	Locally raised budget pc	Social care pc	Education pc	Other pc
AC-12 shock (alternative)	1322.18***	1779.49***	-457.31***	-622.42***	2153.44***	-208.84
	(330.69)	(344.50)	(94.18)	(195.69)	(438.68)	(160.81)
N	2028	2028	2028	2028	2028	2028
R^2	0.891	0.837	0.825	0.793	0.767	0.654
Time FE	Year	Year	Year	Year	Year	Year
Model	DiD	DiD	DiD	DiD	DiD	DiD
Full set of controls	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Census	Census	Census	Census	Census	Census

Standard errors clustered at the local authority level in parentheses.
* p<0.10, ** p<0.05, *** p<0.01

Notes: Outcomes expressed in per capita terms (pc). The AC-12 shock is defined as the difference in AC-12 population shares in a given local authority-year and its 2001 baseline share as defined in equation 2.1 of section 2.4.1. All regressions include local authority fixed effects. The full set of controls refers to the share of EU-15 migrants, the share of non-EU migrants, the local authority unemployment rate and the total local population. Census refers to the 2001 and 2011 UK Census. The observation period is 2000 to 2015.

The results confirm the main results shown in Tables 2.4 and 2.5. The estimated coefficients on total service expenditure per capita (column 1), central government transfers per capita (2), social care spending per capita (4) and education per capita (5) are by a magnitude of 1.5 to three times larger than those estimated in the baseline specification. All coefficients are slightly more precisely estimated. Due to the shortcomings of the Census data discussed above, we do not overinterpret these differences in effect size; however, the results show that imprecisely measured AC-12 migration stocks are unlikely to be the drivers of the association between AC-12 migration and local authority spending and revenue patterns.

2.7 Mechanisms

The results found in this paper document how a large migration shock translates into the provision of different public goods. In aggregate terms, our results suggest that the AC-12 migration shock only marginally affected the overall supply of public services in both pre and post-austerity years. On the revenue side, our results show a relative increase in central government transfers and a relative decline in locally generated revenue in local authorities affected by AC-12 migration inflows. We also observe important shifts in the types of public goods local authorities provide following the migration shock brought about by AC-12 migrants. Local authorities affected relatively more by migration inflows spent relatively less on means-tested social care services and significantly more on education services.

In this section, we investigate two important channels that might explain our observed results. On the one hand, the change in local authorities' populations stemming from AC-12 migration could lead to changes in redistribution via a change in preferences towards less redistribution or native flight lowering the tax base and local authority revenues. On the other hand, these changes could also reflect a more mechanical response. Migrants' have different socio-economic characteristics and different propensities to uptake services, which could lead to changes in redistribution without reflecting any migration-specific altruistic component. In this case, changes in redistribution from the rich to the poor do not discriminate between native-born versus foreign-born poor and simply reflect changes in the socio-economics characteristics of the population. Conditional on an institutional response by local authority councils, changes in the allocation of spending would then follow a mechanical response. We discuss both these channels separately in the following subsections. While we acknowledge that both channels are non-exhaustive and could be at play simultaneously, we first discuss each of these channels and the hypotheses one can derive from them before arguing, through a number of tests, that the demographic channel better explains the observed changes in redistribution following AC-12 migration. For an overview of all variables we analyse as mechanisms and their source, data availability and construction, see Table 2.11 .

2.7.1 Migration and the altruism towards co-nationals: the preferences channel

The shift in expenditure from social care services towards education and the decline in the locally raised budget we identify could be interpreted as local authorities responding to a change in local preferences towards less redistribution following the migration shock. The link between migrant inflows and local preferences for redistribution could be a reflection of people from different groups disagreeing on the optimal amount and composition of local spending, or the dominant native group being less willing to redistribute towards non-co-nationals. While one has to be careful with using vote shares as an indication for an underlying preference for redistributing wealth or income in the population, we showed in Figure 2.1 that in England, it is clearly the Conservative party that has support from voters less in favour of redistribution. We can thus test whether such a "demand" for less redistribution effect could explain our results by measuring the impact of AC-12 migration on the composition of local Councils and test whether the Conservative less pro-redistributive platform increased its seat share following a stronger migration wave. In this context, it is also worth noting that all EU (and Commonwealth) migrants to the UK have voting rights in local elections. Thus, an increase in the seats' share of Conservative Councillors, who represent the interest of voters with preferences for less redistribution, could also reflect a change in the aggregates populations' preferences if migrants exerted their voting rights.

A second more indirect mechanism through which distaste for outsiders could lead to less redistribution is a mechanism often referred to as "native flight" (see e.g., Cascio and Lewis 2012) or "voting by feet". In addition to a direct reduction in demand for redistribution translating into changes in local spending, distaste for living near foreigners can also affect redistribution via natives moving into different local authorities. Such changes can indeed lead to a lowering of the tax base as the number of taxable properties decline, leading to a deterioration of local authorities' budgets and eventually a decline in local authority government spending. To analyse such second-order internal migration in response to international inflows of migrants, we therefore report the impact of AC-12 migration on the number of taxable dwellings per capita (the tax base) as well as on the Band-D council tax rate, a lump-sum tax due to be paid annually by the dwelling owner. On average, this Band-D council tax stood at GBP 1278 over our observation period. To test the native flight hypothesis, we create two measures of internal migration. First, we draw on National Health Service (NHS) registration data. Getting basic access to free public health care in the UK requires registration with a local general practitioner. If individuals change their residence within the UK, they are required to provide their new general practitioner with any previous registration. The data thus allows to calculate both registrations and de-registrations for any given local authority and year. We then subtract internal migration inflows from internal migration outflows to construct a measure of net outflows of internal migrants at the local authority year level. Since the measure does not allow us to distinguish internal migrants by country of birth, we construct a second similar measure for UK native borns only, following Giuntella et al. (2018). Between 2000 and 2013, the second quarter survey of the UK-LFS contained a variable that asked respondents for their local authority of residence in the year prior to the survey, as well as their current local authority of residence. We use this information and calculate a "net internal native out-migration" variable for UK born individuals for all local authority years. Unlike the measure based on administrative NHS registration data, which is comprehensive, we normalise the UK-LFS data by the total number of respondents in each local authority-year to account for fluctuations in the sample size of the survey over time.

Table 2.11 Summary statistics - mechanisms

Variable	Mean	Std. Dev.	N	Source	Years available	Unit
Share of pupils in population	0.159	0.02	1755	CIPFA	2000-2013	$\frac{Pupils_{c,t}}{Population_{c,t}}$
Education per pupil	4664.09	1033.50	1755	CIPFA	2000-2013	$\frac{Education_{c,t}}{Pupils_{c,t}}$
Population aged >64	0.153	0.034	1881	CIPFA	2001-2014	$\frac{Population\,65+_{c,t}}{Population_{c,t}}$
Social care per population aged >64	2659.26	1365.57	1881	CIPFA	2000-2013	$\frac{Social\,care_{c,t}}{Population\,65+_{c,t}}$
Vote share Labour party	0.398	0.267	2028	UK Elections Centre	2000-2015	$\frac{Council\,seats\,Labour_{c,t}}{Total\,seats_{c,t}}$
Vote share Conservative party	0.371	0.243	2028	UK Elections Centre	2000-2015	$\frac{Council\,seats\,Labour_{c,t}}{Total\,seats_{c,t}}$
Net internal out-migration	198.57	2452.55	1840	NHS registrations	2002-2015	$Net\,outflow\,internal\,migrants_{c,t}$
Net internal native out-migration	0.007	0.049	1756	LFS	2000-2013	$\frac{Net\,outflow\,UK\,natives_{c,t}}{Population_{c,t}}$
Band D equivalent council tax	1278.14	225.49	2028	CIPFA	2000-2015	$Council\,tax_{c,t}$
Dwellings: Tax base per capita	0.34	0.06	2028	CIPFA	2000-2015	$\frac{Band\,D\,dwellings_{c,t}}{Population_{c,t}}$

2.7.2 Migration and changes to local authorities' socio-economic structure: the demographic channel

A second hypothesis that could explain the decline in local revenue and social care spending could simply be an institutional response to changes in impacted local authorities' underlying demographics. In our setting, local authority demographics are likely to have changed significantly in response to the distinct socio-economic characteristics of AC-12 migration inflows shown in Table 2.1. These could have important implications for social care expenditure, which is predominantly consumed by elderly segments of the population. In 2004, the year of EU enlargement, the share of social care consumed by locals aged 65 and above stood at 40.1%. The observed decline in spending on social care could therefore simply reflect the per capita decline in demand for these services. We further recall that English local authorities only have upward discretion over education spending such that its relative increase could directly be linked to the rise in central government transfers that are channeled through local authorities (Phillips, 2018). To investigate this institutional response, we begin by analysing changes in local demographics associated with AC-12 inflows by constructing a measure for the population share of pupils and a measure for the share of the population aged 65 and above to proxy demand for education services and social care services brought about by demographics respectively. In a second step, we then normalise changes in spending by these largest relevant consumer groups for both total spending on education and social care instead of using the previous per capita measure to test whether the observed changes were mechanical.

A second potential explanation for the uncovered association between AC-12 migration inflows and changes in local authority spending and revenue is a second-order effects brought about by an internal migration response to the new-arrivals: If, for example, the availability of relatively cheap labour creates local economic opportunities, this could draw in additional migrants from within the country. This, in essence, is the opposite of the "native flight" mechanism and can therefore be tested using the same internal migration measures as outcome variables as explained in subsection 2.7.3.[11]

[11] An additional mechanism we do not consider due to the lack of reliable data is migrants' propensity to demand local services, even once demographic differences are accounted for. For example, the "healthy migrant effect" could decrease the demand for social care services (Abraido-Lanza et al., 1999). While data on social care referrals in England are available, these cannot systematically be divided into self-referrals and referrals by doctors. This is problematic since doctors are likely to refer their patients to social care services based on their understanding of availability of these services. A further issue with data on referrals is that they do not indicate the type (and thus, cost) of services requested.

2.7.3 The relative importance of the demographic channels: the easing off of pressure on social care stemming from AC-12 migration

Table 2.12 reports the results testing the hypotheses we have brought forward to discuss the relative relevance of the two main channels discussed above.

Table 2.12 Mechanisms

	(1) Share pupils	(2) Share 65+	(3) Education/pupils	(4) Social care/65+	(5) Labour seats	(6) Conservatives seats	(7) Net int. outflows	(8) Native outflows
AC-12 shock	0.15***	-0.26***	2947.91	5559.07***	0.65*	-0.54	-12811.18***	-0.31**
	(0.04)	(0.04)	(2684.36)	(1337.05)	(0.39)	(0.36)	(3968.12)	(0.13)
N	1755	1881	1755	1881	2028	2028	1840	1755
R^2	0.383	0.212	0.790	0.694	0.313	0.241	0.046	0.013
Time FE	Year	Year	Year	Year	Year	Year	Year	Year
Model	DiD	DiD	DiD	DiD	DiD	DiD	DiD	DiD
Full set of controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	LFS/APS	LFS/APS	LFS/APS	LFS/APS	LFS/APS	LFS/APS	LFS/APS	LFS/APS

Standard errors in parentheses

* p<0.10, ** p<0.05, *** p<0.01

Notes: The outcome variables are defined in Table 2.11. The number of observation varies due differences in data availability shown in detail in the same table. The AC-12 shock is defined as the difference in AC-12 population shares in a given local authority-year and its 2001 baseline share as defined in equation 2.1 of section 2.4.1. All regressions include local authority fixed effects. The full set of controls refers to the share of EU-15 migrants, the share of non-EU migrants, the local authority unemployment rate and the total local population. LFS/APS refers to the UK Labour Force Survey and its boost samples in the Annual Population Survey.

We first turn to columns (1) to (4) to analyse the local demographic channel. AC-12 migration is strongly associated with an increase in the local population share of pupils ($p<0.01$; column 1) and is similarly associated with a decline in the local population aged 65 and above ($p<0.01$; column 2). The estimated coefficients indicate that a one percentage point increase in the share of local AC-12 migrants relative to the 2001 AC-12 population share changes the share of pupils in the population and the share of individuals aged above 65 by 0.15 percentage points and -0.26 percentage points respectively. Columns (3) and (4) then show the spending on education and social care relative to their main consumption groups. The coefficient estimated on education expenditure by pupil (3) shows that the large increase in education expenditure per capita we documented in the previous analyses is likely due to the change in underlying demographics associated with AC-12 inflows. The estimated coefficient is still positive, but no longer differs from zero at any conventional statistical level. In column 4, we show that the decline in social care service spending per capita associated with AC-12 inflows disappears when spending is calculated as a share of the main recipient group. In fact, increases in the local AC-12 migration share are associated with a large *inrease* in social care spending by population aged 65 and above ($p<0.01$). The estimated coefficient shows that a one percentage point increase in the AC-12 migrant shock measure leads to a GBP 56 increase in social care spending by the population aged 65 and above.

We next turn to the local preferences for redistribution channel in column (5) and (6) of Table 2.12. The association of AC-12 migration inflows with the share of local council seats held by Conservatives is negative and not significantly different from zero (6). In fact, we find suggestive evidence that it is the share in Labour held seats that increased following AC-12 migration inflows ($p<0.1$; column 5). Our data does not allow us to disentangle whether the shift towards Labour votes were in parts caused by AC-12 migrant voters themselves or if their presence causally affected the voting behaviour of the local non-migrant population. However, our results allow us to conclude that, if there was indeed a shift of native preferences towards the less-redistributive Conservative party, this shift was smaller than any excess Labour votes cast by migrants themselves, such that the aggregate local preferences showed no signs of a preference shift towards less redistribution as a response to more heterogeneity. In appendix B.5, Table B.5, we find suggestive evidence that AC-12 did indeed increase UKIP votes by approximately the same magnitude as Conservative seats decreased, although these results are only stable when including controls.

Finally, columns (7) and (8) show the association of AC-12 migration with net total UK-internal migrant outflows and the net internal outflow of UK-borns respectively. Unlike previous research by Sá (2015) and Giuntella et al. (2018), who analyse migrants as a homogenous group, we find no evidence for a "native flight" following the inflow of AC-12 migrants into local authority areas. Instead, our estimates in fact suggest a decline in the net outflow of both total and UK native-born internal migrants in response to AC-12 migration.

Taken together, we interpret these results as strongly suggestive of a greater role played by the demographic channel. While the results on native flight and the increase in the more pro-redistributive platform go against the preferences channel, the fact AC-12 migrants were on average younger and more likely to be in employment suggests the demographic channel played a significant role in explaining the reduction in social care spending per capita. Furthermore, education spending per pupil remains stable and can explain the rise in central government transfers per capita for this partially ring-fenced expenditure item. While we cannot fully test this argument, it is also worth noting that this increase in pupils per capita should not be fully attributed to a rise in pupils from AC-12 countries only. As shown in Table 2.1, AC-12 migrants were not particularly more likely to have more children than other groups, such that the relative increase in pupils may also reflect the second order effect of a reduction in net outflows of potentially young natives with children.

Table 2.13 then turns to a more detailed analysis of the effect of AC-12 migration on local revenue sources by breaking up the local revenue measure into the council tax and the tax base.

Table 2.13 Effect of AC-12 migration on local revenue sources

	(1) Council tax (Band-D)	(2) Taxable dwellings pc
AC-12 shock	-395.33*	-0.215***
	(235.61)	(0.050)
N	2028	2028
R^2	0.949	0.789
Time FE	Year	Year
Model	DiD	DiD
Full set of controls	Yes	Yes
Sample	LFS/APS	LFS/APS

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.01

Notes: The Council tax (Band-D) is the baseline annual lump-sum Council tax set by local authorities. Taxable dwellings is the number of Band-D equivalent dwellings that are subject to paying Council tax. "pc" refers to per capita. The AC-12 shock is defined as the difference in AC-12 population shares in a given local authority-year and its 2001 baseline share as defined in equation 2.1 of section 2.4.1. All regressions include local authority fixed effects. The full set of controls refers to the share of EU-15 migrants, the share of non-EU migrants, the local authority unemployment rate and the total local population. LFS/APS refers to the UK Labour Force Survey and its boost samples in the Annual Population Survey. The observation period is 2000 to 2015.

The results shown in Table 2.13 suggest that, while it was indeed the case that the housing stock did not keep up with the increasing number of migrants (column 2), AC-12 migration was also associated with a relative decrease in the local council tax (column 1, p<0.1).

In light of the results on local voting patterns and internal migration responses to AC-12 inflows shown in columns (5) to (8) of Table 2.12, we do not interpret these effects as reflecting the importance of the preferences channel. The provision of local housing units naturally lags changes in the local population size, explaining the decline in the tax base per capita as affected local authorities' populations increased drastically. This decline in the number of taxable dwellings relative to the local population did likely not require a compensation by increasing the local council tax. This interpretation is corroborated by the fact that the reduction in social care spending per capita was not conducted at the expense, but rather at the benefit of the most vulnerable populations (column 4, Table 2.12). Indeed, the most plausible interpretation of our findings is that the inflow of a dynamic and young population eased pressure on local authorities who could now spend relatively more on the social care of those aged 65 and above.

In summary, our analysis of mechanisms that explain the shifts in local authority expenditure and revenue associated with AC-12 migration inflows leads us to three main conclusions: First, the changes AC-12 migrants caused to local authority expenditure and revenue patterns were in large parts due to the shifts these migrants caused to local demographics and the corresponding institutional responses that were triggered by the resulting changes in local service demand. Second, AC-12 inflows were significantly associated with a relative increase in internal migration. The increasing share of pupils associated with AC-12 migration was likely linked to internal migrant inflows into the same local authorities leading to increased education spending per capita. Finally, the relative drop in demand for local social care services associated with AC-12 migrants did not just result in a large *inrease* in social care spending by the population aged 65 and above, but also allowed affected local authorities to keep their council tax rates relatively low. Overall, our results suggest that the demographic channel was most relevant to explain the impact of AC-12 migration. Thus, they further strengthen previous research findings of an overall net positive fiscal contribution migrants from 2004 EU accession countries made to the UK government budget (Dustmann and Frattini, 2014).

2.8 Conclusion

In this study, we investigated the effect of the large and unexpected wave of Central and Eastern European migrants starting in 2004 on local authority redistributive spending in England. Our results suggest that AC-12 migrants indeed impacted on local authority revenue sources and the local spending mix. We do not find evidence in favour of the hypothesis that these large migration inflows impacted on local preferences for redistributing income. We neither observe voting patterns in local elections that would indicate such a shift in preferences, nor did local residents "vote with their feet" in response to migration inflows. While our results clearly favour shifts in demographics as an explanation for the observed changes in local authority revenue and spending, a limitation of our study is the lack of survey data that could capture preferences for redistribution more precisely.

We interpret our results as a word of caution when relating them to the existing literature. A decrease in public spending can mean a lack of demand from newcomers due to their distinct socio-economic characteristics, rather than the outcome of an increasing local insider-outsider dynamic. Thus, our findings rather lend further support to Dustmann and Frattini (2014) who show that AC-12 migrants were positive net contributors to the UK public purse.

It is worth reflecting on our results in light of the Brexit vote, where anti-immigrant sentiment has played an important role (Dennison and Geddes, 2018; Meleady et al., 2017). A possible interpretation of our results is that the national-level distaste for immigrants expressed in the Brexit vote was not driven by local level exposure to foreigners. This explanation finds strong support in a recent study by

Becker et al. (2017), who show that local education, income and unemployment levels are strongly correlated with the local Vote Leave share, whereas exposure to EU migrants has little explanatory power. Such an interpretation would further corroborate the necessity to conduct studies relating the presence of migrants to preferences for redistribution on the subnational rather than the national level if the aim is to study direct exposure: On the national level, changes in preferences for redistribution may capture an increased fear of foreigners in areas not necessarily exposed to migration.

Finally, the interpretation of our findings with regards to the sustainability of social care services in England requires a careful reflection. On the one hand, we show that the distinct demographics of AC-12 migrants eased the pressure these means-tested services face in England in the short-term. On the other hand, those migrants who arrived as part of the post-accession waves are increasingly getting older and will likely demand social care services in larger numbers in the future. Using migration as a tool to permanently ease the pressure on local service provision would thus require a continuous inflow of migrants. However, migration inflows from EU countries to the UK have slowed down in response to the end of free movement for EU citizens and recent political developments in the UK, with net migration flows from the 2004 Central and Eastern European countries turning negative in 2018 for the first time (Sumption and Vargas-Silva, 2021). These developments are likely to have repercussions for local authority revenue and spending in the near future and may call for new reforms to regulate the flow of migrants in the absence of free movement of EU citizens.

3

A Politically Independent Executive Arm? EU Commissioners' Ideological Alignment and Budget Allocation in the European Union

Abstract

[1] This paper investigates the effects of the political distance between European Commissioners and heads of government on the allocation of funds flowing from the European Union to EU member states. The EU's agricultural and regional budgets offer two particularly interesting case studies due to the discretion exerted in these domains by the Commissioner for Agriculture and the Commissioner for Regional Policy, respectively. Leveraging the difference in timing in the turnovers of Commissioners and heads of government, I test whether the political distance between EU commissioners and heads of administrations affects the share of agricultural and regional funds countries receive from 1979 to 2006. Results show that greater ideological distance is a strongly significant deterrent of funds being channelled. The effects are strongest in pre-election years, for countries providing the Commissioners in charge of the given portfolios, and for countries that are single-party ruled, as opposed to coalition ruled. These findings suggest the behavior of European Commissioners follows similar principles to nationally elected leaders and are important given the salience of agriculture and regional funding at the European level and ongoing debates surrounding EU integration and the political independence of the EU's executive body.

[1]I thank Théo Blanc, Max Brès-Mariolle, Anatole Cheysson, Risto Conte Keivabu, Catherine De Vries, Elias Dinas, Jérôme Gonnot, Jaakko Hillo, Simon Hix, Liesbet Hooghe, Andrea Ichino, Vytautas Kuoktis, Lars Ludolph, Andrea Mattozzi, Vincent Pons, Sebastian Rast, James Snyder, Danny Walker, Hong-Il Yoo, as well as seminar participants at MPSA and APSA for their helpful comments and discussions. I am grateful to Clément de Chaisemartin and Xavier D'Haultfoeuille for helping me navigate the *twowayfeweights* command on Stata and thank Holger Döring and Philip Manow for sharing their updates of the ParlGov data set. I particularly wish to thank Chiara Santantonio for a very thorough re-reading of a preliminary version of this paper and for suggesting important edits. All errors are mine.

3.1 Introduction

The European Commission (EC) is often at the heart of debates between those advocating for more or less European integration. The EC is the executive body of the European Union (EU) with features of both an international organization and those of a national ministry with strong executive powers. Supporters of deeper EU integration typically want to give the EC more powers while those in favor of less - or even exiting from the EU - often complain that it lacks political accountability. Most of this debate, however, is framed in terms of principles and ideals but ignores the degree to which the EC has actually been acting as a political player. While European Commissioners are accountable to the European Parliament and, in principle, act as politically independent actors, information on the way decisions are taken is scarce. The procedures underlying negotiations between Commissioners, heads of government, and other EU institutions remain opaque.

Focussing on agriculture and regional funds due to their high salience and the identifiable discretion exerted by the respective Commissioners responsible in these areas, I investigate the impact of ideological distance between EU Commissioners and receiving countries' administrations on the allocation of funds flowing from the EU to member states.

As highlighted in Gehring and Schneider (2018) (hereafter GS, 2018), the link between ideological factors, nationality, the distribution of the EU's budget, and the workings of its executive body have been under-investigated. Following GS (2018)'s study on the impact of the EU agricultural Commissioners' nationality, this paper is, to the best of the author's knowledge, the first to quantitatively measure the effects of political distance between Commissioners and national administrations on the allocation of transfers flowing from the EU to member states. GS (2018) find a significant, positive, and large impact of providing the agriculture Commissioner on the share of agriculture funding a country receives. On average, they find that being represented by the Commissioner for Agriculture leads to a rise by 0.79 percentage points in a given country's agricultural receipts as a share of the total EU agricultural budget. According to their estimates, this represents approximately half a billion euros per year for a fictive average sized country as of the 2006 budget. This paper goes one step further by focussing on the role of ideological proximity, another important dimension, and thus, further sheds light on the EU's executive body's relative independence and political behavior.

To bridge this gap in the literature, I first use the ParlGov database (Döring and Manow, 2012) to construct for each EU country-budget-year observation a measure of the absolute ideological distance between each country's head of government and the sitting Commissioner for Agriculture and the sitting Commissioner for Regional Policy. In a second step, I match this information with data on the share of the specific budget flowing to each country and other observables provided by GS (2018). I then regress these shares, for which I have data from 1979 to 2006, on the ideological distance variable and the controls commonly used in the literature in a two-way fixed-effects setting.

The main concern for a causal interpretation of the results lies in the potential endogeneity of the EU Commissioners' relative ideologies. The major differences in the electoral calendars between EU and national elections, frequent changes in national administrations following votes of no confidence, resignations and elections, and the nomination procedure of both Commissioners of interest, however, greatly contribute to mitigating these concerns. I further help alleviate these concerns by adding country-budget specific linear time trends in my main specification and by showing that results are consistent with a number of robustness and placebo tests.

My estimates show that a one unit increase in the absolute ideological distance leads to a 0.35 percentage point decline in the share of funds received. This implies that a one standard deviation increase in the measure of absolute ideological distance leads to a 0.41 percentage point reduction in the share of funds received. Results are of similar magnitude for both regional policy (-0.35 percentage points) and agriculture (-0.33 percentage points). These changes would translate into reductions of approximately EUR 140 million per year of regional funding and EUR 180 million per year of agriculture funds for a fictive average beneficiary country as of the 2006 budget following a one standard deviation increase absolute political distance between the Commissioner in charge and a given head of government. Overall these can represent substantive differences, especially for smaller economies where such funds often represent large amounts.

Theoretically, these results could reflect two channels. On one hand, Commissioners could be channelling funds to politically aligned member states simply because they believe these funds would be put at better use in countries that pursue a reform agenda they believe in. On the other hand, they could reflect the desire to help political allies stay in power to advance Commissioners' future careers. An investigation of the mechanisms highlights the importance of the second channel suggesting the importance of a mutually beneficial exchange between politically aligned Commissioners and heads of government and is in line with the pork barrel politics theory (Ferejohn, 1974; Shepsle and Weingast, 1981; Weingast et al., 1981): Commissioners disproportionately respond to politically aligned national leaders' demands for more funds in exchange for future favors. The effects are driven by single-party governed countries (-0.39 percentage points) and are absent in coalition-ruled countries where the Commissioner has less clarity on the identity of the transfers' main beneficiary. Furthermore, results are strongest in pre-election years (-0.39 percentage points), when incumbents need the transfers the most, but are still strong in other years (-0.34 percentage points). Finally, political distance matters more for countries providing the Commissioner (-0.48 percentage points) while remaining an important factor in other member states (-0.33 percentage points).

Taken together, these results highlight the importance of Commissioners' ideological alignment with national leaders and complement the results by GS (2018). Providing the Commissioner matters positively for the allocation of the EU budget insofar as the Commissioner is of the same political allegiance as a country's leader. The Commissioner's expected benefit of transferring a disproportionate share of funds to politically aligned incumbents is even higher for the Commissioner's home-country's political allies, as one could expect home partisan support to be the most important factor in determining the Commissioners' future career prospects, both at home or abroad. Beyond nationality, the potential interests of Commissioners in further pursuing an international career also means they seek the support of foreign political allies who also play an important role in shaping their future fortunes.

3.1.1 Contribution to the Literature

The contribution to the literature of this paper is threefold. It first builds on a large body of work exploring the effects of ideological proximity for the transfer of resources from upper tiers of government administration to lower tiers of government with a large focus on the US Congress or President (see e.g., Albouy, 2013; Alt and Lowry, 1994; Berry et al., 2010; Bickers and Stein, 1996; Clemens and Veuger, 2021; Larcinese et al., 2006; Levitt and Snyder Jr, 1995) but also other western democracies (see e.g., Bracco et al., 2015; Brollo and Nannicini, 2012; DenPoemark, 2000; Fouirnaies and Mutlu-Eren, 2015; Hanretty, 2021; Solé-Ollé and Sorribas-Navarro, 2008). The findings in this paper are consistent with these earlier findings and show that the dynamics found at the national level are at also at play in the less integrated, transnational model of the EU.

Secondly, this paper relates to the literature on the allocation of funds in international organizations (see e.g., Dreher et al., 2009; Kuziemko and Werker, 2006) which demonstrates the benefits for countries to hold key positions. More specifically, it contributes to the literature studying EU institutions (see e.g., Aksoy, 2010; Carnegie and Marinov, 2017; Kauppi and Widgrén, 2004; Mazumder et al., 2013; Rodden, 2002; Schneider, 2013).[2] As argued in GS (2018), this literature has mostly focused on EU politics and the EU's legislative body while the workings of its executive body have been under-investigated with the exception of a few qualitative studies (see e.g., Smith, 2003; Wonka, 2007) and the quantitative study by GS (2018). Importantly for this study, none of these studies focus on the impact of partisan alignment between member states' leaders and individuals in charge of EU institutions.

[2]See Alesina et al. (2005) and Baldwin and Wyplosz (2019) for wider reviews of this literature (GS, 2018).

Thirdly, this paper contributes to the literature exploring the effects of national, regional, or ethnic proximity on the allocation of transfers flowing from upper to lower tiers of government (see e.g., Dreher et al., 2016; Franck and Rainer, 2012; GS, 2018; Hodler and Raschky, 2014). In the context of the EC, the results of this study complement the findings by GS (2018) by showing that this dimension is likely to have a different impact depending on partisan alignment between the leader in charge at the upper tier and her home country or region's local leadership.

The remainder of the paper is structured as follows. In Section 3.2, I introduce the institutional setting at hand. Section 3.3 describes the data and empirical strategy. Section 3.4 presents the main results. Section 3.5 discusses the mechanisms underpinning the results while Section 3.6 concludes.

3.2 Institutional Setting

The European Commission is the executive branch of the European Union. Its responsibilities include the proposing of legislation, the enforcing of EU laws, and the directing of the EU's administrative operations. The EC is currently composed of 27 Commissioners who oversee an administration of more than 30,000 public servants. The Commission works in a cabinet structure where Commissioners have responsibilities similar to national ministers (GS, 2018). Apart from the President of the Commission who oversees and coordinates the work of the entire EC, each Commissioner is typically responsible over a particular portfolio such as Economic and Financial affairs, Competition, Regional Policy, Trade, Justice, or Agriculture. These portfolios are usually related to a specific "Directorate-General" EU civil servants work for. The composition of the EC typically changes after elections to the European Parliament to whom Commissioners are accountable to.[3] These elections take place simultaneously in each member state every five years and directly elect by universal suffrage the more than 700 Members of the European Parliament. Importantly for my research design, European elections, in their overwhelming majority, do not coincide with national elections. One of the first tasks of the newly elected European legislature is the vetting process of each Commissioner and the election in a single vote of the full Commission. This takes place after the nomination by each country of one Commissioner.[4] While heads of government and Commissioner candidates often seek to lobby the

[3]Together with the Council of the European Union, the European Parliament constitutes the legislative body of the EU. Its powers and responsibilities include the adoption and amendment of EU legislative proposals, the voting on the EU's budget, and the supervision of the work of Commissioners. The Council of the European Union, often referred to as the Council of Ministers is the only specifically intergovernmental body of the EU's legislative arm. It meets in different configurations of all member states' national ministers (one per state). These ministers vary according to the topic under consideration.

[4]Until the 2004 enlargement, France, Germany, Italy, and the UK were entitled to two Commissioners per cabinet.

nominated President of the EC a specific portfolio, it is the EC President who ultimately allocates the portfolios among each candidate (GS, 2018). As highlighted in Nugent (2001) and GS (2018), the specific choices remain unclear until the announcement is made such that it is nearly impossible to predict which country out of all members is assigned one particular position *ex-ante*. Therefore, the final allocation of Commissioners often results in surprises and can be considered to being close to being as good as random (GS, 2018).[5]

Once in office, Commissioners act as agenda-setters who take legislative and budgetary initiatives and make proposals to other bodies of the EU's governing system. However, information on the way decisions are taken and on the negotiation procedures between Commissioners, national leaders, and other EU institutions remains limited. Although Commissioners swear an oath pledging to respect the EU's treaties and to act as politically independent actors when carrying out their duties during their mandate, the nomination structure of Commissioners is such that national administrations appoint politicians who can be expected to pursue both national and partisan interests once in power (see e.g., Vaubel, 2006; GS, 2018; and https://european-union.europa.eu/institutions-law-budget/institutions-and-bodies/institutions-and-bodies-profiles/european-commission_en). This may potentially give rise to principal-agent problems. National governments increasingly appoint Commissioners who previously worked as high-ranking politicians in their countries of origin (e.g., many former ministers) and more generally active political members of each sending country's ruling party (see e.g., Döring, 2007; Egeberg, 2010). While the nomination of Commissioners should partially reflect the results of EU elections (see the Treaty on European Union, article 17(7)), Wonka (2007) shows that over 65 percent of Commissioners are sent by parties in government and less than 20 percent from opposition parties (GS, 2018). This potentially provides a textbook case study of pork barrel politics whereby Commissioners are faced by national leaders' demands for more bacon in order to be reelected. Commissioners strategically respond to these demands by allocating more resources to copartisans at national levels of government whose support Commissioners need the most to advance their future fortunes both at home and abroad. It is indeed likely that Commissioners are interested in positions which require the support of domestic and foreign political allies once their mandate at the EC is over, thereby preventing them from acting as politically independent actors.

As highlighted by GS (2018), the **Commissioner for Agriculture** offers a great case study to investigate the potential for Commissioners to act as political agents. Up until recently, one of the main responsibilities of this Commissioner was the allocation to different member countries of the

[5]For more information, see: https://www.europarl.europa.eu/news/en/faq/8/how-are-the-commission-president-and-commissioners-appointed and Napel and Widgrén (2008)

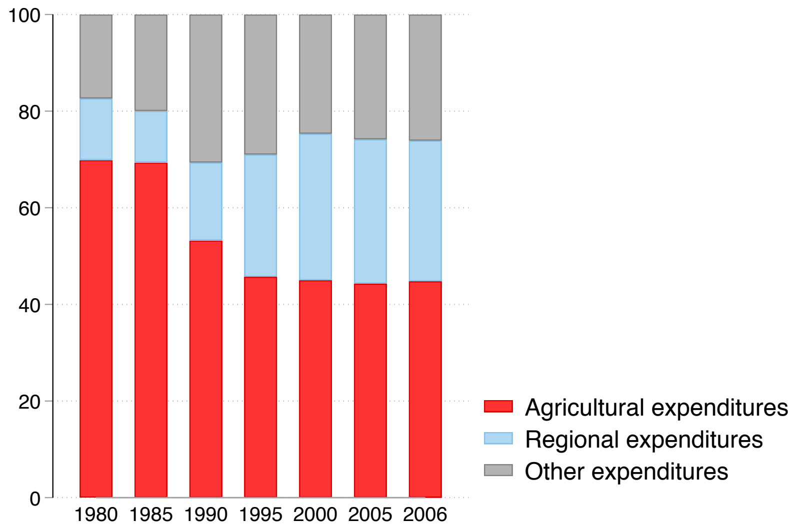
European Agricultural Guidance and Guarantee Fund (EAGGF). Before its replacement by the European Agricultural Guarantee Fund (EAGF) and the European Agricultural Fund for Rural Development (EAFRD), the EAGGF was the main pillar of the EU's Common Agricultural Policy (CAP), representing the largest share of the EU's overall transfers. As highlighted in Figure 3.1, the fund accounted for as much as 70 percent of the total EU budget in the early 1980s and accounted for approximately 45 percent of the total budget in 2006.[6] Its mission included providing direct payments to farmers under the CAP and the financing of measures to regulate agricultural markets such as export refunds and regulation interventions. Up until 2006, CAP budget negotiations began a year ahead of the budget year with the Commissioner for Agriculture making a proposal to the Agricultural Council, a council made up of Ministers for Agriculture in each member state (Fouilleux, 2010). This Council meets monthly with each of these meetings offering possibilities for the Commissioner to influence the share of transfers countries receive due to the Commissioner's agenda-setting power and information advantages (Fouilleux, 2010; GS, 2018).[7]

The Commissioner for Regional Policy also has agenda-setting powers in her realm and oversees the allocation of the European Social Fund (ESF) and the European Regional Development Fund (ERDF). As shown in Figure 1, the importance of these funds rose drastically over the sample period offsetting the reduction in agriculture spending. In 2006, they accounted for approximately 30 percent of the overall EU's transferred funds, representing the second largest share of the EU's budget. The goal of these funds is mainly to promote the development and structural adjustment of EU regions lagging behind, generally assist the social and economic transformation of regions faced with structural problems, and more broadly to promote employment (Butzen et al., 2006). Regional funds' rising importance can be explained by the consecutive accession to the EU of several poorer countries over the sample period and the EU's concern for maintaining social and economic cohesion within the EU (Butzen et al., 2006). In contrast to the Commissioner for Agriculture, GS (2018) do not find any effects of the nationality of the Commissioner for Regional Policy on the allocation of regional funds to member states. The authors argue that this is because the allocation of regional funds are to a much greater extent based on formal criteria giving Commissioners in this field much less room to maneuver. For instance, one of the criteria for European regions to be eligible for ESF/ERDF funding is that their per capita GDP lie below 75 percent of average per capita GDP in the EU (see e.g. Becker et al. 2012; Borin et al. 2021). An alternative hypothesis could be that the regional Commissioner

[6] see Butzen et al. (2006) for a detailed breakdown and explanations of trends in the EU budget over my sample period: http://www.nationalebankvanbelgie.be/doc/ts/publications/economicreview/2006/ecorevii2006_h4.pdf

[7] It has been noted that, up until 2007 and the Lisbon-Treaty, the European Parliament had a low influence on CAP-related budgetary decisions (see, e.g., Crombez and Swinnen, 2011; GS, 2018; Schneider, 2013).

Figure 3.1 Structure of EU expenditures, as percentages of the total budget (%)



over the sample period mostly came from the EU's four largest economies with fewer regions lagging behind, and thus less room for changes in regional policy affecting Commissioners' home countries.[8] This paper argues that the regional policy portfolio still offers an excellent case study for investigating the role of political distance in determining the allocation of funds flowing from the EU to member states. Similarly to agriculture, the regional budget's allocation can still be directly traced to a single Commissioner who sets the agenda and can potentially make regional funds deviate from their trend. The overall size of both funds remains subject to the EU's long-term multi-annual financial framework and decisions by the Commissioner for Budget but discretion within the predefined available budget remains.[9] The annual meetings between the Commissioner and other bodies of the EU should in theory still give important room for regional Commissioners to maneuver and politically reallocate funds between affected countries each year.

To summarize, agriculture and regional policy offer excellent options for studying the extent to which EU Commissioners seek to help politically aligned incumbents stay in power. Both Commissioners in charge of these respective portfolios can be identified as solely responsible over specific, salient, and large budgetary items with a clear redistributive nature and consistent yearly observable flows to individual EU member states.

[8]See Appendix Tables A1 and A2 for a list of the Commissioners in charge of Agriculture and Regional Policy, respectively.

[9]The multi-annual financial frameworks is a seven-year financial constraint negotiated by member states' heads of government where countries "outline EU spending by setting ceilings on expenditures for each budget category and on total expenditure" (Schneider 2013, 465)

3.3 Data and Empirical strategy

3.3.1 Data

The data used in this project were obtained from the replication files in GS (2018) and the ParlGov data set (Döring and Manow, 2012).

Specifically, I use ParlGov, a data infrastructure containing information on party positions measured on an economic and cultural left-right [0-10] scale, to retrieve data on heads of government and Commissioners' ideological positioning. These positions are comparable over countries and are time-invariant unweighted mean values of information from the Castles and Mair (1984), Huber and Inglehart (1995), Benoit and Laver (2006), and Bakker et al. (2015) party expert surveys. First, I retrieve the list of heads of government from 1979 to 2006 for all EU member states. Appendix Table A3 provides for each country in my sample the positions I identify as heads of government. Second, I do a similar exercise for all appointed Commissioners for Agriculture and Regional Policy over the same period. Finally, I assign to each head of government and Commissioner their respective ideological positioning identified by the ParlGov data set at the time of their nomination. Appendix Tables A1 and A2 respectively show the appointment and resignation dates of all agriculture and regional Commissioners in the sample as well as their countries of origin and ParlGov left-right (LR) position at the time of their appointment. For years when a new Commissioner or new head of government is nominated, I compute a weighted average of the LR score based on the number of the month the new Commissioner or leader enters office. A month is counted if the person of interest was in office for the major part of the month.

The data set in the replication files of GS (2018) provides all the remaining data used in this analysis. It contains country-year observations on agricultural funds receipts of a country as a share of the overall EU agricultural budget (*AFS*) and the share of regional structural payments member states receive as a share of the total ESF and ERDF budget (*RSF*) from 1979 to 2006.[10] It also provides information on the proportion of the year (measured as the proportion of months within a year) in which a country appointed the Commissioner, the main independent variable in GS (2018). It also contains the other variables used in the vector of controls of the main regression. The inclusion of such variables is commonly used in the literature on the EU (Bouvet and Dall'Erba, 2010; GS, 2018; Schneider, 2013), is important to condition for potential selection problems in my estimation, and may increase the precision

[10] As explained in GS (2018), the transfers stemming from the European Agricultural Fund for Rural Development (EAFRD), one of the two follow-up funds that replaced the EAGGF, are hard to directly trace back to the actions of the Commissioner for Agriculture. By co-financing economic rural development programs, it is more likely to also depend on other Commissioners' decisions. This explains why the sample used in GS (2018) and this paper ends in 2006.

of the estimates of interest. For reasons of transparency and to allow for the comparison of my results, I include the exact same controls as in GS (2018). These controls relate to the size, economic conditions, importance of agriculture, and the political situation of member states (elections, EU support, and whether the country recently joined the EU). The vector of controls also includes dummies for whether a country holds the European Commission Presidency and whether it holds the EU Council Presidency.

Table 3.1 Summary statistics

	N	Mean	SD	Min	Max
Funds share	669	7.409	7.372	0.004	37.994
Commissioner for Agriculture or Regional Policy	669	0.077	0.264	0	1
Commissioner, ParlGov grade [0-10]	51	5.372	1.382	3.645	7.500
Head of government, ParlGov grade	669	5.501	1.390	2.628	8.496
Absolute Distance between Commissioner and head of government: $AbsDist$	669	1.587	1.177	0	4.418
EC President	669	0.073	0.256	0	1
Pre-election year	669	0.245	0.430	0	1
Election year	669	0.278	0.448	0	1
Employment agriculture (ln)	669	5.635	1.561	0.993	8.010
Gross value added, agriculture	669	3.878	2.887	0.380	14.351
Unemployment rate (%)	669	8.335	3.678	0.700	21.300
Per capita GDP (EU=100)	669	100.260	38.892	23.050	301.183
New Member State	669	0.203	0.403	0	1
Voting Power Council	669	7.441	4.577	0.900	17.857
Domestic EU support	669	46.478	23.021	-30.000	86.000
Council Presidency	669	0.151	0.358	0	1
Coalition government	669	0.256	0.437	0	1
Commissioner for Agriculture:					
ParlGov grade	26	5.992	1.016	3.801	7.292
Providing the Commissioner	364	0.073	0.258	0	1
$AbsDist$ between Commissioner and head of government	364	1.436	1.167	0	4.418
Agricultural Funds Share (%)	364	7.028	6.716	0.004	27.465
Commissioner for Regional Policy:					
ParlGov grade	25	4.727	1.435	3.645	7.500
Providing the Commissioner	305	0.082	0.272	0	1
$AbsDist$ between Commissioner and head of government	305	1.768	1.165	0	4.250
Regional Funds Share (%)	305	7.863	8.074	0.014	37.994

Notes: N refers to the number of observations, mean refers to the mean value of the outcome, SD the standard deviation, min the minimum, and max the maximum. Appendix Table A4 details the sources and definitions of each variable further. The sample size differ for the variables related to the Commissioners for Agriculture and Regional Policy since these Commissioners were not always politically affiliated throughout the sample period.

Moreover, it includes the Shapley-Shubik index, a measure of countries' bargaining power in the EU Council.[11] Finally, this data set also contains information on whether and when countries were ruled by a coalition government, a binary variable I use when investigating potential mechanisms driving the results. I provide a list of the main dependent, independent, and control variables, as well as their sources and definitions in Appendix Table A4.

Merging these two datasets, I obtain a final sample of 669 country-budget-year observations covering 25 countries and 28 years from 1979 to 2006. Note that the 1979 and 1980 agricultural transfers observations are excluded from the main sample. This is because the Commissioner for Agriculture during these years was not politically affiliated such that the ideological distance variable between the Commissioner for Agriculture and heads of government could not be computed. For the same reasons, regional transfers are excluded post-2004. Furthermore, I drop the five country-year observations for which the head of government was unaffiliated during most of a given year from the main sample.[12] I verify that dropping these observations is unproblematic by showing in Appendix Table B1 that the main results found in GS (2018) on their sample of 385 country-year observations are very close to those found when running their exact same estimation procedure on this papers's restricted sample. Table 3.1 provides summary statistics.

3.3.2 Empirical strategy

The empirical strategy of this study closely follows that of GS (2018). Specifically I estimate:

$$BS_{i,f,t} = \beta AbsDist_{i,f,t} + \lambda Comm_{i,f,t} + X'_{i,t}\gamma + \zeta_{i,f} + \rho_t + \alpha_{i,f}t + \epsilon_{i,f,t} \tag{3.1}$$

$BS_{i,f,t}$ refers to budget f 's fund receipts of country *i* in year *t* as a share of the overall EU regional or agricultural budget f in year *t*. $X_{i,t}$ is the vector of controls while $Comm_{i,f,t}$ is the main independent variable used in GS (2018) and gives the proportion of the year in which a country appointed the Commissioner. $AbsDist_{i,f,t} = |NatLeadLR_{i,t} - ComLR_{ft}|$ is the continuous independent variable of interest measuring the absolute distance in LR ParlGov ideology between the Commissioner ($ComLR_{f,t}$) for

[11]The EU Council is formed by member states' heads of the executive, the President of the European Council, and the President of the European Commission. In contrast to the Council of the European Union (the Council of ministers), it has no legislative power but sets the EU's overall political direction, priorities, and policy agenda. It works by traditionally adopting "conclusions" during European Council meetings which typically identify concerning issues and actions to tackle them. Its Presidency rotates between each member state every six months. Since the 2007 Treaty of Lisbon (article 15) the European Council also now appoints a full-time President from one of the member states.

[12]Romania, Bulgaria, and Croatia only joined the EU post-2006. Therefore, these countries are no included in the sample. The UK voted to leave in 2016 and is thus in the sample throughout.

Agriculture or Regional Policy and county i's head of the government in year t ($NatLeadLR_{i,t}$).[13] $\zeta_{i,f}$ and ρ_t represent country-budget and year fixed-effects, respectively. Including such fixed-effects allows me to account for unobservable year and country-budget specific level variation. The standard errors ($\epsilon_{i,f,t}$) are two-way clustered at the country-budget and year level (Baum et al., 2015; Cameron et al., 2011; Schaffer, 2020). This level of clustering is important since the dependent variable is a share out of all EU countries leading to correlation between the receipts of each country at each point in time (GS, 2018). This strategy is therefore akin to a two-way fixed-effects estimation procedure with a staggered and non-stochastic treatment.

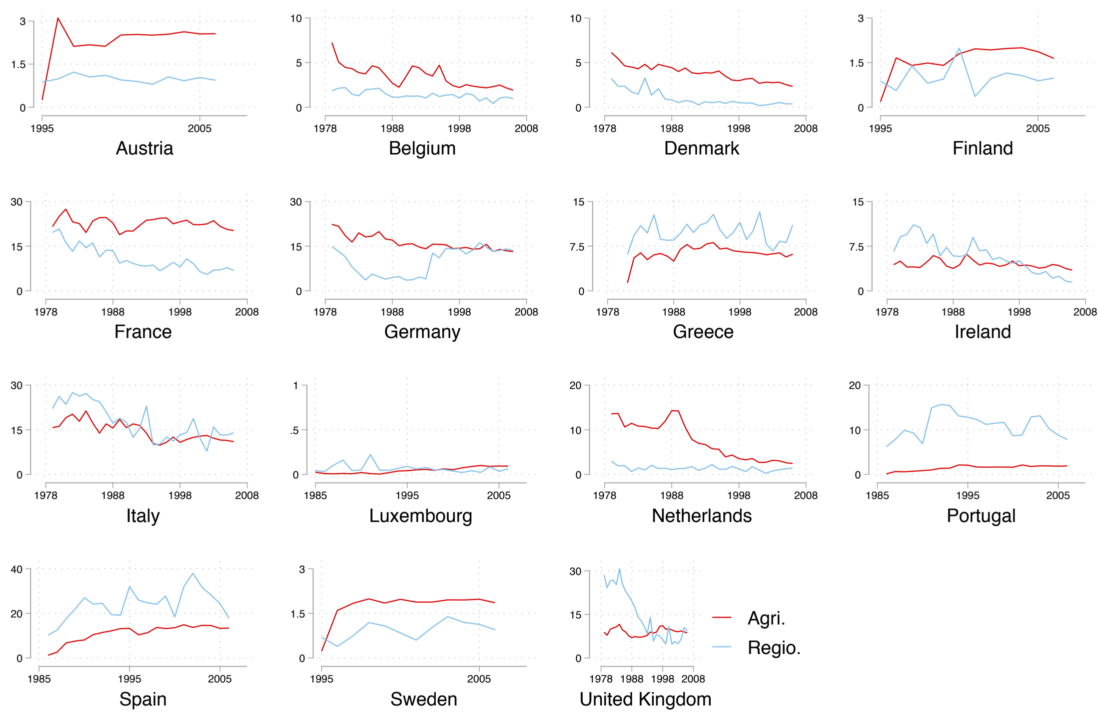
The main threat to causal identification of the procedure laid out above relates to the assumption of common trends between more and less treated countries before changes in ideology of the Commissioner or countries' administrations take place. This assumption being satisfied is plausible given the high frequency at which national leaders are replaced following elections, resignations, or votes of no confidence, the orthogonality between EU and national electoral calendars, and the specificities of the Agriculture and Regional Commissioners nomination procedures described in Section 3.2 often leading to surprises.

However, one might still be concerned that the budgets of countries where the ideology of heads of government and Commissioners are not aligned lie on different trends. For instance, a country experiencing sectoral changes as a result of relative improving economic conditions may see the share of agriculture in its GDP decline and, as as result, receive fewer agriculture funds. More importantly, economic improvements would also mechanically lead to a reduction in the share of regional funds transferred since an important pre-condition for regions being eligible to these funds requires their GDP per capita to be below 75 percent of the average per capita GDP in the EU. These developments might in turn lead to a positive or negative correlation between the political ideology of heads of government and Commissioners, as countries on different economic paths might vote differently to other countries as well as lobby for different posts within the Commission. Such trends can further be explained by successive enlargement rounds diluting the shares most countries receive over time and are clearly visible for certain countries and budgetary items in Figure 3.2.

In light of these concerns, I follow Fouirnaies and Mutlu-Eren (2015) and GS (2018) by adding country-budget specific linear time trends $\alpha_{i,f}t$ to the main specification. The inclusion of country-budget specific linear trends, in addition to the inclusion of control variables, helps alleviate the concerns

[13]The control variables are: a dummy for providing the EC President, a dummy for holding the EU Council Presidency, domestic EU support, a dummy for a country that joined the EU in the latest enlargement round at time t, per capita GDP, unemployment, gross value added in agriculture, employment in agriculture, voting power at the Council, and finally dummies for whether countries are in election and pre-election years.

Figure 3.2 Evolution over time of the share of funds allocated in EU pre-2004 enlargement countries



Notes: Agri. refers to the share of agriculture funds out of the total agriculture budget each member state receives. Regio. refers to the share of regional funds countries receive out of the share of the total regional budget.

mentioned above although non-linear country-budget specific trends could still bias the estimation. Therefore, I also show that results broadly do not change when adding squared trends in the estimation in Appendix Table C3.

I also address the recent literature showing that linear regressions with group and time fixed-effects are equivalent to estimating a weighted sum of the average treatment effects in each of these time periods and groups (De Chaisemartin and d'Haultfoeuille, 2020). As a result, two-way fixed-effects coefficients may be of the opposite sign of the average treatment effect in each group and period in the presence of negative weights and heterogeneous across units treatment effects (see also Borusyak and Jaravel, 2017; Callaway et al., 2021; De Chaisemartin and d'Haultfoeuille, 2018; de Chaisemartin and D'Haultfœuille, 2021; Sun and Abraham, 2021). I first follow the procedure suggested by De Chaisemartin and d'Haultfoeuille (2020) by showing that these coefficients can only be of opposite signs under a relatively large amount of treatment effect heterogeneity. Furthermore, I follow the diagnostics proposed by Jakiela (2021) for assessing the likely severity of heterogeneous treatment effects. Namely, I show that results are robust to excluding one country at a time, one Commission Cabinet at a time, and when performing further heterogeneity analyses in described in Section 3.4.2 suggesting treatment heterogeneity is unlikely to be a problem. I also show that the observed results are robust across a large set of alternative specifications laid out in Section 3.4.2, including a specification accounting for country-year and country-budget fixed-effects.

3.4 Results

3.4.1 Main results

Table 3.2 reports the effects of providing the Commissioner exerting discretion over the budget of interest and, this paper's main variable of interest, the absolute ideological distance between the Commissioner in charge and member states' heads of government, *AbsDist*. Columns 1 to 3 show the results with the inclusion of country-budget and year fixed-effects only, columns 4 to 6 show results after adding controls, while columns 7 to 9 add country-budget specific linear time trends. Columns 1, 4, and 7 report the results of the regression of the *Fund Share* on providing the Commissioner only and does not account for *AbsDist*. This is akin to the main regression employed in GS (2018). Columns 2, 5, and 8 report the results of a regression including the effect of *AbsDist* only, while columns 3, 6, and 9 report the results when including both independent variables.

A first noteworthy observation is that the effects of ideological distance remain stable regardless of whether one also includes the variable for providing the Commissioner across all three specifications. In contrast, the point estimate for providing the Commissioner changes substantially depending on whether one includes the ideological distance variable in GS (2018)'s preferred specification with country-budget specific linear trends. The point estimate on providing the Commissioner indeed declines from 0.51 to 0.36 and is not significant at conventional levels (p-value=0.136).

Whereas the point estimate on providing the Commissioner declines as controls and trends are added, the magnitude of the effects of ideological distance is not sensitive to the inclusion of controls. However the point estimate on absolute ideological distance more than doubles when adopting GS (2018)'s preferred, and what the authors consider to be the most conservative, specification with linear trends. In this case, I find similar point estimates between providing the Commissioner and a one unit increase in political distance. As shown in column 9, a one unit increase in the political distance between the Commissioner and the head of governments is associated to 0.35 percentage point reduction in the share of funds received. This equates to a one standard deviation increase in the absolute ideological distance being associated to a 0.41 percentage points reduction in the share received, representing 5.59 percent of a standard deviation. In this case, results are significant at the one percent level.

Table 3.2 Regression results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome					*Fund Share*				
AbsDist		-0.188	-0.180		-0.194*	-0.173*		-0.359***	-0.350***
		(0.229)	(0.202)		(0.071)	(0.060)		(0.002)	(0.001)
Commissioner	1.667		1.653	1.362*		1.313*	0.513		0.359
	(0.115)		(0.119)	(0.075)		(0.075)	(0.138)		(0.136)
Observations	669	669	669	669	669	669	669	669	669
Country-budget fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No	No	No	Yes	Yes
Country-budget lin. time trends	No	No	No	No	No	No	No	Yes	Yes

Notes: Two-way country-budget year cluster robust *p*-values in parentheses. *** p<0.01, ** p<0.05, * p<0.1 Lin. refers to linear. The control variables used in the baseline are a dummy for providing the EC President, a dummy for holding the EU Council Presidency, domestic EU support, a dummy for a country that joined the EU in the latest enlargement round at time *t*, per capita GDP, unemployment, gross value added in agriculture, employment in agriculture, voting power at the Council, and finally dummies for whether countries are in election and pre-election years.

Table 3.3 shows the results by specific budget by adding interactions between the political distance and providing the Commissioner independent variables with a binary variable, $Agri$, taking value one for the agricultural budget to the main specification:

$$BS_{i,f,t} = \beta AbsDist_{i,f,t} + \kappa AbsDist_{i,f,t} * Agri + \lambda Comm_{i,f,t} + \tau Comm_{i,f,t} * Agri + X'_{i,t}\gamma + \zeta_{i,f} + \rho_t + \alpha_{i,f}t + \epsilon_{i,f,t} \tag{3.2}$$

As in Table 3.2, columns 1 to 3 show the results with the inclusion of country-budget and year fixed-effects only. Columns 4 to 6 show results after adding controls, while columns 7 to 9 show results after adding country-budget specific linear time trends.

A first noteworthy observation is that the effects of political distance are stable for both budgets across specifications which do or do not account for providing the Commissioner.

Starting with the Commissioner for Regional policy, the effects are only present when accounting for linear country-budget specific trends in columns 8 and 9. One can infer from this result that this difference in the regional Commissioner's results is mostly what drives the change in coefficients on the effects of distance between column 5 (resp. 6) and 8 (resp. 9) in the pooled specification presented in Table 3.2. Accounting for these trends is warranted given the observed country-budget specific trends in Figure 3.2. As argued in Section 3.3, accounting for country-budget specific linear time trend alleviates endogeneity concerns stemming from the potential for countries experiencing economic improvements (resp. deterioration) to mechanically receive less (resp. more) regional funds. These developments might in turn be correlated with systematic differences in the political distance between heads of government and the Commissioner for Regional Policy which should be accounted for.

The results in column 9 suggest that a one unit increase in political distance between a country's head of government and the Commissioner for Regional Policy leads to a 0.35 percentage points reduction in the share of the regional budget countries receive and is significant at the five percent level. This equates to a one standard deviation increase in political distance between the Regional Commissioner and heads of government leading to a 0.41 percentage points reduction in the share of funds transferred. For an average sized fictive country such as the Netherlands (approximately 17 million inhabitants), this represents approximately an 11 percent reduction in regional funds' receipts and approximately EUR 140 million as of the 2006 regional budget.[14] In line with GS (2018), I find that providing the Commissioner for Regional policy does not affect the share of regional funds countries receive. While the point estimates are not particularly small, they are far from being significant at any conventional level in all specifications. The effects of providing the Commissioner for Regional Policy are stable across specifications that do or do not account for political distance such that the change in coefficients on the effects of providing the Commissioner depending on whether political distance is included or not in the pooled specification in Table 3.2 are driven by the agricultural Commissioner.

Turning to the Commissioner for Agriculture and the interaction terms, I find that the effects of political distance for the Commissioner responsible in this area decline as linear trends are added to the main specification. However the point estimate remains negative and statistically significant at conventional levels. A linear combination of the coefficients on *AbsDist* and *AbsDist*agriculture* in column 9 suggests that a one unit increase in political distance between a country's head of govenment

[14]The Netherlands received EUR 465 million in regional funds in 2006. This low number can be explained by the fact it is one of the richest countries in the EU.

and the Commissioner for Agriculture leads to a 0.33 percentage points reduction in the share of the agricultural budget countries receive and is significant at the one percent level. This equates to a one standard deviation increase in political distance between the Agricultural Commissioner and heads of government being associated to a 0.39 percentage points reduction in the funds transferred. For an average sized fictive country such as the Netherlands, this represents approximately a ten percent reduction in agricultural receipts and approximately EUR 180 million as of the 2006 agricultural budget.[15] Thus, the effects of political distance on budget allocation are very similar between the two budgets under this preferred specification in column 9. Studying the linear combination between the coefficient for providing the Commissioner and the interaction term between this variable and agriculture, I find that providing the Commissioner for Agriculture increases the share of agriculture transfers received by 0.62 percentage points and is significant at the five percent level. This is a slight decrease relative to the -0.78 percentage point baseline estimate in GS (2018) which does not control for political distance.

[15]The Netherlands received EUR 1,220 million in agriculture funds in 2006 and EUR 2,082 million in overall funds.

Table 3.3 Regression results by Commissioner

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome					*Fund Share*				
AbsDist		0.012	0.004		0.077	0.075		-0.355**	-0.353**
		(0.961)	(0.987)		(0.708)	(0.710)		(0.024)	(0.022)
AbsDist **agri.*		-0.559*	-0.528*		-0.597*	-0.548*		-0.008	0.023
		(0.043)	(0.046)		(0.030)	(0.048)		(0.961)	(0.889)
Commissioner	1.046		1.045	1.116		1.110	0.135		0.133
	(0.575)		(0.571)	(0.404)		(0.402)	(0.825)		(0.790)
Commissioner **agri.*	-0.056		-0.413	0.472		0.143	0.785		0.484
	(0.981)		(0.854)	(0.786)		(0.931)	(0.313)		(0.477)
Country-budget fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Country-budget lin. time trends	No	No	No	No	No	No	Yes	Yes	Yes

Notes: Two-way country-budget year cluster robust p-values in parentheses. *** p<0.01, ** p<0.05, * p<0.1 Agri. refers to a dummy equals 1 for the agricultural budget. Lin. refers to linear. The control variables used in the baseline are a dummy for providing the EC President, a dummy for holding the EU Council Presidency, domestic EU support, a dummy for a country that joined the EU in the latest enlargement round at time t, per capita GDP, unemployment, gross value added in agriculture, employment in agriculture, voting power at the Council, and finally dummies for whether countries are in election and pre-election years.

To make sure results are not driven by the smaller sample size stemming from missing values in the absolute political distance variable, Appendix Table B1 compares the baseline estimates found in GS (2018) using their larger sample versus the smaller sample used in this study. The point estimates show that both samples broadly tell the same story. The point estimate on the agricultural Commissioner declines slightly while the point estimate on the regional Commissioner increases tenfold but remains largely insignificant (p-value=0.631). This provides reassuring evidence that these results are not driven by imposed sample restrictions.

3.4.2 Robustness

To assess the robustness of my findings, I first evaluate the possibility that the main results on the effects of political alignment on the share of allocated funds arise from chance rather than reflect a causal relationship. I do so by verifying in Appendix Table B2 that the political distance between the Commissioner for Agriculture (resp. Regional Policy) and heads of government does not affect the share of transferred regional (resp. agricultural) funds. Reassuringly, the point estimates for political distance are of a much lower magnitude than in the baseline specification and are never significant.

The theoretical premise of this paper is that the political alignment between the Commissioners and heads of government only matters when Commissioners and national leaders are simultaneously in office. Significant lags are in theory possible if Commissioners' legacies take time to reverse. Significant leads, on the other hand, would cast serious doubts on the causal interpretation of the results. Given European elections take place every five years, Appendix Table B3 reports the results of the following:

$$BS_{i,f,t} = \beta AbsDist_{i,f,t} + \sum_{k=-5}^{k=5} \beta_{t+k} AbsDist_{i,f,t+k} + \lambda Comm_{i,f,t} + X'_{i,t}\gamma + \zeta_{i,f} + \rho_t + \alpha_{i,f}t + \epsilon_{i,f,t} \tag{3.3}$$

While this leads to a reduction in the sample's size, given the absence of politically affiliated heads of government and Commissioners both before 1979 and after 2006, results show that the contemporaneous effect of absolute distance remains stable and significant at the one percent level when including lags and leads to the specification. The number of significant leads and lags is not higher than what would be expected: the lead of absolute political distance in *t-4* is negative and significant at the ten percent level while the lag in *t+3* is positive and significant at the five percent level. Most importantly the leads in *t-2*, and *t-1* all have very small point estimates while the leads in *t-3* and *t-5* are of the opposite sign of the main coefficient of interest.

Next, I investigate whether the main results found in Section 3.4.1 are robust to alternative specifications. There is a risk in the main specification that some of the controls in equation (1) may be affected by the treatment themselves and therefore bias the coefficient of interest. Appendix Table C1 shows that the magnitude of the point estimates declines marginally to 0.31 percentage points when lagging all controls and 0.34 percentage points when taking their first-difference.

Several controls in the baseline specification are directly connected to agriculture, which can be explained by GS (2018)'s focus on this budgetary item. Appendix Table C1 shows that excluding these controls virtually does not change the point estimate of the effect of political distance. In all cases, results remain significant at the one percent level.

Moreover, I test the robustness of results to replacing the main independent variable by a binary variable, $Diff$, equals to one if $ComLR_t$ is greater than five (and right-leaning) and $NatLeadLR_{i,t}$ is lower than five (left-leaning) in country *i* and year *t*, and vice-versa.[16] While this measure is arguably less precise and does not for example account for the fact that centre-left politicians may be closer politically to center-right politicians than far-left politicians, results shown in Appendix Table C1 are in line with the baseline. Countries which are governed by a national administration of a different LR orientation to the one of the Commissioner experience a significant at the five percent level and substantial reduction in the share of funds transferred of 0.68 percentage points.

Appendix Table C2 shows that the significance of the result remains unchanged when clustering standard errors at the country-budget and year-budget level as well as at the year and country level. The significance of the effects of interest remains unchanged.

Appendix Table C2 also shows the results of estimating the following:

$$BS_{i,f,t} = \beta AbsDist_{i,f,t} + \zeta_{i,t} + \rho_{f,t} + \alpha_{i,f}t + \epsilon_{i,f,t} \tag{3.4}$$

By including year-budget ($\rho_{f,t}$) and country-year ($\zeta_{i,t}$) fixed-effects, this specification does not account for country-budget specificities but instead accounts for specific shocks that may affect the size of the two budgets each year as well as specific shocks affecting member states each year. While this change in specification greatly changes the point estimate on providing the effect of providing the Commissioner, increasing to 2.67 percentage points, it only moderately changes the main point estimate of interest. The point estimate for political distances rises to 0.42 percentage points and stays significant at the one percent level.

Moreover, Appendix Table C2 shows the results are robust to using year-budget ($\rho_{f,t}$) rather than simply year fixed effect in the main specification :

$$BS_{i,f,t} = \beta AbsDist_{i,f,t} + \lambda Comm_{i,f,t} + X'_{i,t}\gamma + \zeta_{i,f} + \rho_{f,t} + \alpha_{i,f}t + \epsilon_{i,f,t} \tag{3.5}$$

[16] Note that no head of government or Commissioner in the sample has a ParlGov LR score equals to 5

In this case, the point estimate on political distance declines moderately to 0.32 percentage points and remains significant at the one percent level while the effect of providing the Commissioner remains broadly unchanged relative to the baseline.

Recall that the two-way fixed estimator of interest in this paper can only be given a causal interpretation if the assumption of common trends between more and less treated countries is satisfied. While including control variables and country-budget specific linear trends can help mitigate such endogeneity concerns, one might still worried that the common trends assumption is violated because of unobservable nonlinear trends in the budget shares member states receive. Appendix Table C3 shows that the effect of political distance when adding squared country-budget specific trends to the baseline becomes slightly weaker but remains substantial (-0.28 percentage points) and is significant at the five percent level. It also shows that the results are broadly unchanged when using country-specific common trends rather than country-budget specific trends. The point estimate declines slightly in magnitude to -0.33 percentage points when including country specific linear trends only and increases slightly when accounting for both linear and squared common country specific trends. The effects remain significant at the one percent level in both cases.

GS (2018) argue that a potential selection-bias in their study relates to the potential for specific countries to constantly be less likely to provide the Commissioner. In this case, accounting for country-budget specific fixed-effects may not suffice if such countries also have different voting patterns. I follow GS (2018) by showing in Appendix Table C4 that the results are robust to excluding the five most populous countries in the EU (France, Germany, Italy, Spain, and the UK), which potentially have more or less interest in these specific positions. In this case the point estimate of the effect of absolute distance declines in magnitude to -0.19 percentage points but remains significant at the one percent level. Next, I also show that results are stable to excluding each EU member state individually at a time from the main sample. The point-estimates in this case are very close to the baseline and are all significant at the one percent level.

Of the 25 member states in the sample, France, Poland, Portugal, and Lithuania can be characterized as having semi-presidential systems. They differ to parliamentary systems in the sense that these countries have popularly elected heads of states who do not only act as ceremonial figureheads but may also have equal or stronger powers than their heads of government. Since the heads of state and heads of legislature may have different political ideologies, including these countries may add noise to the results, as the identity of the transfers main beneficiary may be unclear. Results remain significant at the one percent level when excluding these countries while the point estimate increases in magnitude to -0.42 percentage points, a result I discuss further in Section 3.5. Appendix Table C4 also shows that the effects increase slightly in magnitude (-0.38 percentage points) and are significant at the one percent level when excluding the ten countries that joined the EU in 2004. Appendix Table C4 also shows that results are robust to dropping country-budget observations for years when these specific countries provide the Commissioner in charge of the specific budget. In this case, the point estimate declines moderately to -0.27 percentage points and remains significant at the five percent level, a result I discuss further in Section 3.5.

The [0-10] LR ideology of the Commissioners in charge and heads of legislatures is computed as a weighted average of the ParlGov grades based on the number of months in power in years that experience turnovers. This might add noise to the data set, particularly during election years when the transition of power may not be immediate. Excluding election years in Appendix Table C4 from the main sample leads to an increase in the magnitude of the point estimate to -0.43 percentage points, which remains significant at the one percent level. As a last heterogeneity test, I show in Appendix Table C5 that result are stable if one excludes one Commission administration at a time from the main sample consisting of eight consecutive Commission Cabinets. The point estimates range from -0.23 to -0.49 percentage points and are significant at the one percent level in all but one case, which is significant at the five percent level.

Finally, I also address questions related to negative weights and treatment heterogeneity potentially leading the fixed-effects coefficient to have an opposite sign to the average treatment on the treated (ATT) in each group and period. De Chaisemartin and d'Haultfoeuille (2020) show that the absolute value of the fixed-effect coefficient divided by the standard deviation of the weights is equal to the minimum value of the standard deviation of the ATT across time-period treated units under which the ATT and the linear regression coefficient may be of opposite signs. If this ratio is close to 0, the two-way fixed-effect coefficient and the ATT can be of opposite signs even under a small plausible amount of heterogeneity, while treatment heterogeneity is less of a concern if this ratio is large. Applying the Stata *twowayfeweights* command described in De Chaisemartin and d'Haultfoeuille (2020), I find that 313 ATTs receive negative weights and that the ATT and the fixed-effect coefficient may be of opposite signs if the standard deviation of the treatment effect across the country-budget year observations is equal to 0.19, a fairly large but possible amount of heterogeneity. Observing negative weights follows naturally from the two-way fixed-effects procedure and is really only of concern in the presence of treatment heterogeneity (Jakiela, 2021). The stability of the results to the multiple sample restrictions shown in this Section, however, greatly mitigates these concerns. Furthermore, I find that there are only 41 negative weights and that the ATT and the fixed-effect coefficient may be of opposite signs if the standard deviation of the treatment effect across the country-budget year observations is equal to 0.73 when using the alternative binary measure equals to one when the Commissioner and head of government are of opposite orientations, *Diff*, as the independent variable. This suggests that the two coefficients can only be of an opposite sign under unrealistically large heterogeneity when using this variable taking only two values and with stable control groups each year, thereby further mitigating concerns related to treatment heterogeneity.

3.5 Mechanisms

This section investigates the mechanisms driving the results found above. One interpretation of less funds being channelled to countries run by ideological distant heads of government could be that Commissioners do not expect the funds from the EU to be put at good use in countries with administrations they ideologically strongly disagree with. In this case, it is not out of self-interest that Commissioners channel relatively less funds to these countries. An alternative could be that the Commissioners allocate more pork to political allies across the EU who are able to lobby for more funds by promising the sitting Commissioners future favors. By obtaining more funds, politically aligned national heads of government increase their chances of reelection. Having allies in power in turn benefits the sitting Commissioner' future career prospects as these allies are then the ones negotiating in favor of the Commissioner's promotion to more important EU roles following Cabinet reshuffles.

The fact that the payments are made directly to farmers and regional projects as opposed to being channeled via centralized national institutions makes the first channel *à priori* unlikely. To investigate whether the main results rather reflect the second "pork barrel" identified channel, I first test if these effects are weaker in countries where identifying whether a political ally will benefit from the payments is inherently more difficult. Specifically, I use the binary variable provided in the GS (2018) replication files indicating whether countries are run by a coalition or a single-party government. Although, the ParlGov data provides a good measure of the political ideology of country *i*'s head of government, it does not account for the fact that the clarity of responsibility of the national leader may be diluted if a given leader is ruling with ministers from other parties. In this case, identifying who benefits from the transfers is more difficult and likely to depend on other aspects such as the balance of power within the coalition or which party controls which ministerial portfolio. The pork barrel hypothesis would therefore predict results to be weaker in coalition run countries than single-party run administrations. I formally test this hypothesis by adding interactions between the political distance and providing the Commissioner independent variables with a binary variable, $Coal$, taking value one if a given country was coalition-ruled during a given year:

$$BS_{i,f,t} = \beta AbsDist_{i,f,t} + \kappa AbsDist_{i,f,t} * Coal + \lambda Comm_{i,f,t} + \tau Comm_{i,f,t} * Coal + \xi Coal_{i,t} + X'_{i,t}\gamma + \zeta_{i,f} + \rho_t + \alpha_{i,f}t + \epsilon_{i,f,t} \tag{3.6}$$

I report the coefficient on *AbsDist*, *Comm*, and their interactions with the coalition dummy, *Coal*, in column 2 of Table 3.4. In line with predictions made by the pork barrel theory, a one unit increase in the constructed measure of political distance between Commissioners and heads of government leads to a 0.39 percentage points reduction in the share of funds transferred for single-party ruled countries while these effects are absent in coalition controlled countries as shown by the large and positive coefficient on the interacted term between political distance and the coalition dummy. This is in line with the results presented in Column 3 and mentioned in Section 3.4.2. Conducting the same exercise but replacing *Coal* in equation 3.6 by a binary variable *Semi-presidential*, taking value one if the country has semi-presidential institutions, indeed shows that effects are stronger in non semi-presidential systems where the identity of the transfers' main beneficiary is also clearer. In this case a one unit increase in the absolute political distance measures is associated to a 0.37 percentage point decline in the share of funds allocated in non semi-presidential systems and is significant at the one percent level. The linear combination between *AbsDist* and its interaction with the *Semi-presidential* dummy suggests that a one unit increase in absolute political distance is associated to a 0.26 percentage points reduction in funds allocated and is significant at the five percent level.

To further explore the relevance of this "pork barrel" channel, I investigate whether more funds flow from the EC to the Commissioner's allies in pre-election years by running the following:

$$BS_{i,f,t} = \beta AbsDist_{i,f,t} + \kappa AbsDist_{i,f,t} * Preelection + \lambda Comm_{i,f,t} + \tau Comm_{i,f,t} * Preelection + X'_{i,t}\gamma + \zeta_{i,f} + \rho_t + \alpha_{i,f}t + \epsilon_{i,f,t} \tag{3.7}$$

The transfer of funds is indeed likely to electorally matter the most to incumbents when seeking their reelection. On the other hand, one would not expect the electoral electoral calendar to affect results if these were purely driven by Commissioners simply disagreeing of the use made of funds in more ideologically distant administrations. Results shown in column 3 of Table 3.4 support the first hypothesis. The effect of a one unit increase in the absolute political distance is associated to a 0.34 decline in the share of funds allocated in other years (significant at the one percent level) while the linear combination of the coefficient on *AbsDist* and its interaction with the pre-election dummy shows that a one unit increase in absolute political distance is associated to a 0.39 percentage points reduction in transferred funds in pre-election years.

As a final test to this hypothesis, I investigate whether Commissioners are more likely to help allies whose support they need the most following their time at the EC. Namely I investigate whether Commissioners transfer more funds to political allies in their home country. Following their mandate, European Commissioners may be interested in continuing their political career either in their home country or working for Organizations abroad. In both cases, the support of home political allies is crucial. It is indeed unlikely that Commissioners would obtain ministerial appointments in their home country once their mandate is over if the party they represent is not in power.Furthermore, the home country's administration's support is typically a pre-condition for nominations for international postings to take place (Wonka, 2007).

Table 3.4 Regression results - Mechanisms

	(1)	(2)	(3)	(4)	(5)
Outcome			*Fund Share*		
AbsDist	-0.350***	-0.386***	-0.373***	-0.337***	-0.334***
	(0.001)	(0.000)	(0.006)	(0.002)	(0.005)
*AbsDist*Coalition*		0.253			
		(0.196)			
*AbsDist*Semi-presidential*			0.112		
			(0.585)		
*AbsDist*Preelection*				-0.050*	
				(0.090)	
Commissioner	0.359	0.102	0.298	0.242	0.545**
	(0.136)	(0.814)	(0.222)	(0.238)	(0.032)
*Commissioner*Coalition*		0.868			
		(0.413)			
*Commissioner*Semi-Presidential*			0.759		
			(0.427)		
*Commissioner*Preelection*				0.412	
				(0.374)	
*AbsDist*Commissioner*					-0.141
					(0.624)
Observations	669	669	669	669	669
Controls	Yes	Yes	Yes	Yes	Yes
Country fixed-effects	Yes	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes	Yes
Country-budget specific linear time trends	Yes	Yes	Yes	Yes	Yes

Notes: Two-way country-budget year cluster robust *p*-values in parentheses. *** p<0.01, ** p<0.05, * p<0.1 Lin. refers to linear. The control variables used in the baseline are a dummy for providing the EC President, a dummy for holding the EU Council Presidency, domestic EU support, a dummy for a country that joined the EU in the latest enlargement round at time *t*, per capita GDP, unemployment, gross value added in agriculture, employment in agriculture, voting power at the Council, and finally dummies for whether countries are in election and pre-election years.

To test this hypothesis, I run the following specification:

$$BS_{i,f,t} = \beta AbsDist_{i,f,t} + \kappa AbsDist_{i,f,t} * Comm_{i,f,t} + \lambda Comm_{i,f,t} + X'_{i,t}\gamma + \zeta_{i,f} + \rho_t + \alpha_{i,f} t + \epsilon_{i,f,t} \tag{3.8}$$

In line with the aforementioned prediction, column 5 in Table 3.4 shows that the effect of ideological distance is stronger in countries that hold the Commissioner position: A one unit increase in political distance is associated to a reduction in the share of funds received of 0.33 percentage points in foreign countries (significant at the one percent level) and to a 0.48 percentage points decline in the Commissioner's home country (significant at the ten percent level) when studying the linear combination of *AbsDist* and the interaction between *AbsDist* and *Comm*.

In this case, providing the Commissioner is associated to a 0.55 percentage point increase in the share of funds received meaning and is significant at the five percent level. This means that Commissioners disproportionatly favor (resp. punish) political allies (resp. rivals) in their home country relative to foreign allies (resp. rivals). A one standard deviation increase in political distance between the Commissioner's ideology and the ideology of the Commissioner's home country's head of government more than cancels out the benefits of being the country providing the Commissioner.

Taken together, these results strongly support the relevance of the second channel with Commissioners allocating more pork to political allies across the EU . These aligned heads of government are able to effectively lobby for more funds in exchange for helping the sitting Commissioners future career prospects once the Commissioners mandates are over.

3.6 Conclusion

In this paper, I show that the ideological distance between heads of EU member state governments and the sitting Commissioners for Agriculture and Regional Policy negatively affects the respective shares of agriculture and regional funds countries receive. The most affected member states are the Commissioners' home countries, countries which are single-party ruled, and countries about to experience elections to the legislature. These results provide first hand evidence that the behavior of European Commissioners follows similar principles to national level elected decision makers and can help the debate surrounding the reform of EU institutions and the EC's role as a more or less independent actor. Despite recent demands for a more political Commission, the results presented in this paper suggest that the structures in place at the EC have, up until recently, already given it the scope to act as a political actor in the fields of agriculture and regional policy.

Avenues for future work include the study of other supranational organizations and investigating whether other Commissioners whose responsibilities cannot be directly traced back to specific budgets are also able to favor political allies.

4

Term Limits and Accountability: Evidence from Italy

Joint with Pietro Panizza

Abstract

[1] This study exploits a 2014 reform that granted mayors of Italian towns with less than 3,000 residents the possibility to run for a third term to analyze the effects of extending term limits on public finance outcomes. Employing a *difference-in-discontinuity* design, we find that mayors who are in their second term at the time of the reform, and would otherwise be forced to step down, tend to increase revenue through debt, transfer resources towards salient spending items such as waste management and public transport, and become less efficient in collecting taxes. First-term mayors, who now face two instead of one potential re-election, increase revenue through the sale of assets and increase investments particularly on public housing. While we interpret the results on second-term mayors as reflecting an act of pandering to voters to be re-elected, we argue two mechanisms are at play for first-term mayors. First, these mayors could be preparing the ground for their potential second re-election by investing in items that only pay off electorally at a later stage. Second, the substantially longer horizon they now face increases the incentives to capture politicians by exchanging reelection promises against public spending in specific items. This second mechanism is substantiated by the fact our main results are driven by municipalities in the south of Italy.

[1]We thank Monika Banaszewska, Flavia Cavallini, Andrea Cintolesi, Thomas Crossley, Francesco Drago, Gemma Dipoppa, Andrea Ichino, Francesca Lotti, Andrea Mattozzi, Kasia Nalewajko, Vincent Pons, Bingham Powell, Chiara Santantonio, Eleanor Woodhouse, Sergio Galletta, Salvatore Piccolo, and seminar participants at the EUI microeconometrics working group, MPSA, and the Sciences Po Economics and Politics EP@L workshop for their helpful comments and suggestions. We also thank Andrea Ferri of Ifel for numerous conversations on Italian local public finance.

4.1 Introduction

Term limits are a restriction on the number of terms a single individual can serve in a given public office. Their use dates back at least to the Athenian democracy (6th century BC) and is still a common arrangement of elected offices in governments and political organizations. Examples range from the President of the United States to sub-national level institutions (e.g., US governors, Brazilian, and Italian mayors) with ongoing debates in countries such as France to extend term limits from national level to local level constituencies. The rationale for adopting term limits can be to mitigate moral hazard and hidden action problems that may worsen with time such as embezzlement or the entrenchment of power, or to promote turnover in the ruling political class. On the other hand, opponents of term limits argue that they may prevent voters from punishing poor leaders or rewarding good and experienced incumbents for their actions (Becker, 1990).

In this study, we contribute to the understanding of the effects of term limits by studying the impact of the introduction of the possibility to run for an additional term in Italian municipalities on local public spending behaviour. We exploit a 2014 reform that introduced the possibility for mayors of municipalities below 3,000 inhabitants to run for a third term, while mayors in districts above this threshold continued to be forced to step down after their second term.

While there is a rich theoretical literature on the effects of term limits (see e.g., Smart and Sturm, 2013; Maskin and Tirole, 2004; and a survey of the theoretical literature by Duggan and Martinelli, 2017), the empirical literature thus far has either conducted analysis requiring strong assumptions for causal interpretation (see e.g., Besley and Case, 1995; Klein and Sakurai, 2015) or focused on comparing narrowly re-elected incumbents to narrowly elected first-term mayors (see e.g., Ferraz and Finan, 2011; Coviello and Gagliarducci, 2017). These studies require mild assumptions for causal interpretation but cannot fully disentangle the effects of re-eligibility incentives from the impact of experience and ability. Fouirnaies and Hall (2021) use difference-in-difference on individual-level data on politicians who served in U.S. State legislatures with term limits and find that legislators that cannot be re-elected exert less effort but do not measure any downstream effects on policy-outcomes.

To the best of our knowledge, this study is the first to measure the effects of extending term limits on local level outcomes by employing a *difference-in-discontinuity* design to account for pre-existing differences in the experience and ability of Italian mayors in small towns. Exploiting the staggered timing of municipal elections in Italy and focusing on towns that experienced an election in 2011, 2012, and 2013, we overcome a main hurdle faced by the empirical literature on electoral accountability.

Mayors with similar levels of experience were indeed suddenly re-eligible for one or two more terms with time to adjust their policies before the next election.

A second contribution of this work is to enable the study of the dynamic effects of a two-term limit. The effects of the policy are indeed likely different between mayors in their first and second term at the time of the reform.

We find that second-term mayors who are suddenly re-eligible for a third term, increase municipal debt (by EUR 200 per capita) and increase public spending on salient items such as waste management and public transportation (EUR 166 per capita). The sudden change in their re-election incentives can explain the increase in spending on items that quickly pay off from an electoral perspective. We also find suggestive evidence of a decline in the efficiency of revenue collection and current spending for second-term mayors. These results confirm that immediate re-eligibility concerns are important for local public finances.

The results are significantly different for mayors in their first term who now become twice re-eligible. Results show that these mayors act significantly on the capital part of the balance sheet by increasing municipal debt (EUR 111 per capita), the revenue accruing from sale of public assets and investments particularly in public housing (EUR 32 per capita).

We attribute these differential results to the relevance of the horizon faced by an elected official on her policy choices. Second-term mayors react to suddenly becoming re-eligible by pandering to their voters. In contrast, mayors in their first term, who perhaps were already pandering to their electorate for a first re-election, now face greater stakes of being reelected as they become eligible for a third consecutive term at the end of their potential second term. The increase in capital spending may be an investment in items that pays off in electoral consensus at the time of their potential second re-election bid.

A second less obvious, and possibly complementary, mechanism driving these results is related to the capture of politicians. Politicians with the prospect of a longer horizon become more appealing to certain special interest groups such as criminal organizations. The existence of such groups is a fairly common phenomenon in Italy, especially in the south. This part of Italy is characterized by a lower level of political accountability and social capital (Nannicini et al., 2013) and a strong presence of criminal organizations (Pinotti, 2015a). There exists empirical evidence that the Italian Mafia has vastly infiltrated the real estate and construction sector in these regions (see e.g., Acconcia et al., 2014; Pinotti, 2015b; De Feo and De Luca, 2017; and Di Cataldo and Mastrorocco, 2020). The building sector with its high profitability and low technological and financial barriers, is an ideal outlet for criminal organizations for the long-term investments of profits obtained from illegal activities.

While we cannot test this hypothesis fully yet, we are in the process of assembling a pre-reform cross-sectional dataset on Mafia's presence in Italian municipalities. This will allow us to investigate whether the results we observe in the south on housing are indeed driven by districts with significant mafia's presence.

The remainder of the paper proceeds as follows. Section 2 describes our research setting. Section 3 explains our research design and the data. Section 4 lays out our main results, while Section 5 is for interpretations and 6 for conclusions.

4.2 Research setting

4.2.1 Municipal elections in Italy

Italian municipalities are composed of a mayor (*Sindaco*), an executive body (*Giunta*), and an elected city council (*Consiglio Comunale*). The electoral system currently regulating municipal elections in Italy was introduced in 1993. In towns with less than 15,000 residents, each candidacy is composed of a candidate mayor and a list of candidates for the city council. The candidacy obtaining a relative majority of votes, elects the mayor and two-thirds of councillors in the council. Elected mayors appoint the executive body and, before 2014, could serve for at most two consecutive terms. A term lasts five years and can be terminated earlier only if the mayor resigns or is forced to step down by a vote of no confidence from the city council. Early terminations of legislatures were common before the 1993 reform and became relatively rare since then (less than five percent of the mandates since 1993 did not reach the end of their cycle). It is further worth noting that districts experiencing an election before the foreseen schedule continue holding a different electoral calendar relative to other towns. Although Italian towns all held their first free elections together in 1946, municipal elections are now staggered across time, as shown in Figure 4.1. The graph shows for each year the number of municipalities with less than 5,000 residents that held elections.

Italian municipalities manage about ten percent of total public expenditure and are in charge of a wide range of services, including water supply, waste management, municipal police, infrastructure, welfare, and housing (Grembi et al., 2016).

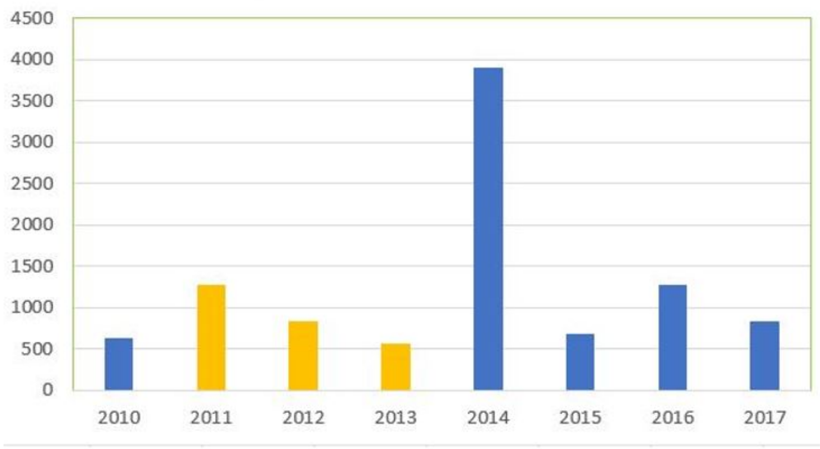


Figure 4.1 Number of Italian municipalities with less that 5000 residents holding elections. Our sample consists of the bars colored in yellow.

4.2.2 The reform of term limits

A 2014 reform (Legge n. 56, 2014) extended to three the maximum number of consecutive terms for mayors of Italian municipalities with less than 3,000 inhabitants. The reform was motivated by the difficulty of finding good quality administrators available to run as mayors (De Benedetto and De Paola, 2019). Besides the term limit extension to districts below 3,000 inhabitants, the law introduced two other policy changes at the same cutoff. First, it introduced a 40 percent gender quota in the composition of the executive body for towns above 3,000 residents. Second, it changed the size of municipalities' city councils and executive bodies.

These changes were to be implemented in the municipal election following the approval of the law and were thus not effective in our selected sample of municipalities. For instance, for municipalities that held elections in 2012 and 2013 the reform was enacted in 2017 and 2018, respectively. As it is unclear how gender quotas and council size would affect our dependent variables before being effectively implemented, we are confident that the effects we measure in this study are driven by the extension of term limits. Furthermore, we interpret the observed differential effects of the reforms between first and second-term mayors as further evidence that the terms limit extension is the main operating factor. We further validate this claim in Section 4.5.

4.3 Research design

4.3.1 Evaluation framework

Measuring the impact of term limits is empirically challenging. Such limits indeed typically apply uniformly *within* countries and many other differences typically overlap with differences *across* countries or *across* election types such that disentangling their impact is not straightforward.

A common approach in the literature has been the application of close election regression discontinuity designs (see e.g., Lee and Lemieux, 2010) to compare the behaviour of narrowly elected first-term mayors facing re-election incentives to narrowly elected second-term mayors with no re-election incentives (see e.g., Lee, 2008; Ferraz and Finan, 2011; and Coviello and Gagliarducci, 2017). While this approach may help mitigate concerns regarding the confounding effect of ability, it cannot perfectly disentangle the effects of re-election incentives from the effects of experience. As, for example in two-term regimes, it compares narrowly re-elected mayors in her final term with narrowly elected first-term mayors. As argued in Ferraz and Finan (2011), the ideal experiment to study the impact of term limits on corruption (or other outcomes) is one where mayors are randomly allocated a term limit. By giving mayors in districts below 3,000 inhabitants the possibility to run for a third and final term, the Italian reform offers a convincing quasi-experiment. The 2014 reform *de facto* quasi-randomly assigned re-election incentives to a subset of incumbent mayors with comparable levels of experience.

Italian mayors' wages and council size already differed at the threshold in 2014. This means we cannot rely on a simple regression discontinuity identification strategy to single out the effect of extending term limits from the pre-existing policy differences (Grembi et al. 2016).

Applying a *difference-in-differences* estimation strategy would also be problematic, as large and small municipalities are typically on differential trends in public policies.[2]

Instead, we closely follow the methodology devised by Grembi et al. (2016) by implementing a *difference-in-discontinuity* design. In concrete terms, we estimate the following regression:

$$
\begin{aligned}
y_{i,t} &= \delta_0 + \delta_1 P^*_{i,t} + S_i(\gamma_0 + \gamma_1 P^*_{i,t}) + T_t[\alpha_0 + \alpha_1 P^*_{i,t} + \\
S_i(&\beta_0 + \beta_1 P^*_{i,t})] + \rho_t + EY_{i,t} + St_{i,t}[\theta_0 + \theta_1 P_{i,t} + \\
S_i(&\zeta_0 + \zeta_1 P^*_{i,t}) T_t\{\lambda_0 + \lambda_1 P^*_{i,t} + S_i(\pi_0 + \pi_1 P^*_{i,t})\} + \rho_t + EY_{i,t}] + \epsilon_{i,t}
\end{aligned}
$$

[2] We verify that the parallel trends assumption does not hold in a standard difference-in-difference design in Appendix Figure D.1.

S_i, a dummy for municipalities below 3,000, T_t a dummy for the post-treatment period, $P^*_{i,t} = P_{i,t} - P_c$, the normalized population and $St_{i,t}$ is a dummy that equals 1 if the incumbent mayor in town i at time t is in her second term. $EY_{i,t}$ represents municipality *i* and election year *t* fixed effects and ρ_t are year fixed effects.

The coefficient of interest, β_0, and the linear combination β_0+π_0, identify the treatment effect of introducing a potential third term for first-term and second-term mayors in districts below the threshold, respectively. We restrict the sample to cities in the interval $P_{i,t} \in [P_c - h, P_c + h]$, where *h* is obtained as the average of optimal bandwidths "MSERD" from Calonico et al. (2014a) and Calonico et al. (2014b) before and after 2015. Finally we cluster the standard errors at the municipality level to allow for the assignment to treatment to be correlated within towns. This is a plausible assumption since no major census updates took place during our sample period such that all districts are always on the same side of the threshold and hence treated repeatedly.

The advantages of this design are twofold. It enables us to exploit both the sharp variation at 3,000 inhabitants and the time variation taking place after the reform by combining elements of *difference-in-difference* and *regression discontinuity* design. Under milder assumptions than a cross-sectional RD or classic difference-in-differences strategy, it identifies the causal effect of giving mayors in districts below the discontinuity the possibility to run for a third term. We discuss these assumptions in Section 4.3.4.

4.3.2 Data

We collected municipal electoral results' data from the Italian Ministry of the Interior. From this data set, we could then construct a variable indicating the term a mayor is in at the time of the reform to identify towns with lame ducks - second-term mayors - and towns with mayors who without the reform could still run for one more term - first-term mayors.

We then matched these electoral results' data with a database of municipal finances collected by the Ministry of Interior. Finally, we matched this data with the 2001 and 2011 Census data provided by *Istat*, the Italian National Institute of Statistics, to determine the population of each municipality and a data set of measures of social capital at the municipal level from Nannicini et al. (2013).

Our main dependent variables are budgetary outcomes of a sub-sample of Italian municipalities in the period 2007-2018. Table 4.1 provide descriptive statistics of most of them. Table 4.1 provides summary statistics while Appendix Table D.1 gives a more specific definition of all the variables we use in this project.

Table 4.1 Descriptive Statistics

	Municipalities that have between 2000 and 3,000 residents	*Municipalities that have between 3,000 and 4000 residents*
	(1)	(2)
Variables	*Mean*	*Mean*
Fiscal revenue	503.51	468.36
Current transfers	178.87	178.34
Non fiscal revenue	262.47	179.73
Revenue from loans	142.14	144.03
Current spending	846.54	751.74
Capital spending	481.04	368.71
Loans repayment	144.80	134.18
Urban planning	62.81	42.35
Public housing	11.89	8.78
Water supply	30.03	36.51
Justice	0.16	0.72
Police	1.12	0.83
Schools	46.42	40.11
Culture	9.81	10.44
Waste management	6.07	6.44
Construction, housing and waste	110.79	94.08
Total revenue	1,672.60	1,417.12
Total spending	1,662.63	1,405.97
Efficiency in collection	0.72	0.70
Efficiency in current spending	0.76	0.75
Efficiency in capital spending	0.28	0.27
Property tax on businesses	8.25	8.32
Share of females in the board	0.17	0.19
Observations	2682	2014
Number of municipalities	289	207

Notes: The budgetary variables are in EUR per capita and are averaged from budget year 2007 to 2018. We have included towns that have between 2000 and 4000 residents as this is close to the sample that we select for the main specification (using the algorithm developed by Calonico et al. (2014a) and Calonico et al. (2014b)).

4.3.3 Sampling frame

Our sample includes yearly fiscal policy outcomes of Italian towns that have between 1,000 and 15,000 residents and that held elections in the years 2011, 2012, and 2013. The sample starts in 2007 in order to include at least a full mayoral term for each groups of towns before the term that goes through the reform's year (2014). We dropped the years after the end of the *relevant* mayoral term.

	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018
2011's batch	✗ Elections	✓	✓	✓	✓	✓ Elections	✓	✓	✓	✓	✓ Elections	✗	✗
2012's batch	✗	✓ Elections	✓	✓	✓	✓	✓ Elections	✓	✓	✓	✓	✓ Elections	✗
2013's batch	✗	✓	✓ Elections	✓	✓	✓	✓	✓ Elections	✓	✓	✓	✓	✓ Elections

Figure 4.2 Sample frame

Figure 4.2 above summarizes how we constructed the sample. We selected the years crossed with a check-mark (✓) and dropped the ones filled with an (**x**) sign. The cells shaded in blue correspond to sub-sample of towns where there are *treated* municipalities. The year of the reform (2014) is shaded in red.

Fiscal policy outcomes in year t are what is reported in year t's final balance. This is the result of the budget drafted in year $t-1$ and the fiscal maneuvers adopted through year t. In case of a power handover, it is possible that fiscal outcomes of election years are determined by two different mayors. We claim that the outgoing mayor is more influential in determining t's fiscal policy as she has been in charge of municipal finances possibly since $t-4$. For this same reason, even though the policy of interest was introduced in April 2014, we let the *treatment* begin in 2015 as this is the first year when fiscal policy was entirely determined after the approval of the reform.

4.3.4 Identification assumptions

Interpreting our results in a causal manner requires several identifying assumptions. These are formalized by Grembi et al. (2016), who also provide precise identification assumptions and testing methods.

Assumption 1: All potential outcomes $E[Yit(w,r)|P_{it}=p,t>t_0]$ and $E[Y_{it}(w,r)|P_{it}=p,t\leq t_0]$, with $w=0,1$ and $r=0,1$, are continuous in p at P_c.

Here $Y_{it}(w,r)$ represents the potential policy outcomes if $W_{it} = w$ and $R_{it} = r$, while r refers to the change in term limit rule and w the pre-existing confounding policy changes at the discontinuity. This assumption is equivalent to stating that there is no sorting of municipalities across the population threshold. We consider this as a plausible assumption given Italian census rules. Censuses are run independently by the national statistics' agency (ISTAT), while the large census of 2011 was conducted three years before the policy was announced and introduced. To test this assumption more directly, we conduct a McCrary (2008) test to verify that there is no evidence of manipulation via a jump in the density of the forcing variable at the relevant threshold. We also conduct a series of placebo on observable tests on a series of pre-existing variables to check whether districts closely below and above the discontinuity were similar on observables prior to the reform.

Assumption 2: The effect of the confounding policy W_{it} at P_c, in the case of no treatment $(r_{it} = 0)$ is constant over time.

This assumption states that observations below and above the discontinuity should satisfy local parallel trends before the introduction of the new policy and is analogous to *difference-in-difference* identifying assumptions but at a more local level. This is also a credible assumption since the rules on mayors' wages had been in place for numerous years before our sample period starts. To test this hypothesis, we estimate the pattern of the discontinuities in Y_{it} before t_0 by showing with yearly regression discontinuity coefficients that towns were not not on locally differential trends before the policy shift.

Assumption 3: The term in which the incumbent mayor is in at the time of the reform should be random.

Since we are interested in the effects of extending the horizon to mayors depending on the number of terms they have left, an additional threat to identification could stem from the fact that the mayor's term at the time of the reform is associated with other observable and unobservable town characteristics.

Given that the reform stemmed from a central government initiative and affected more than half of Italian municipalities, we argue that whether the possibility to run for a third term affected first or second-term mayors was as good as random.[3] To test this formally, we study whether the probability of a mayor being in her second term jumps at the discontinuity in Figure 4.3. In this graph, each dot represents the average value of the likelihood of a district's mayor being in her second term within a given bin of the running variable, while the line fits a linear trend on each side of the population threshold to facilitate visualization. The visual evidence along with the formal *p*-value of the test for a

[3]There are 4,553 municipalities with less than 3,000 residents on a total of 8092 according the 2011 Census.

jump at the threshold developed by Calonico et al. (2014a) and Calonico et al. (2014b) (p-value=0.663) do not show any evidence of a jump and thus alleviate concerns regarding the term mayors were in at the time of the reform.

Proposition: Under assumptions 1, 2 and 3, our *diff-in-disc* estimand of interest identifies the average treatment effect of the possibility to run for a third term on mayors of towns below the threshold, i.e. towns that are *treated* by the policy of interest.

Identifying a more general average treatment effect estimand would require an extra homogeneity assumption stating that there must be no interaction between pre-2014 confounding policies and the policy of interest. While we cannot test this directly, we hypothesize that higher wages would possibly yield stronger re-election incentives such that our estimates for towns below the threshold could represent a lower bound of the estimates given by a more general estimand.

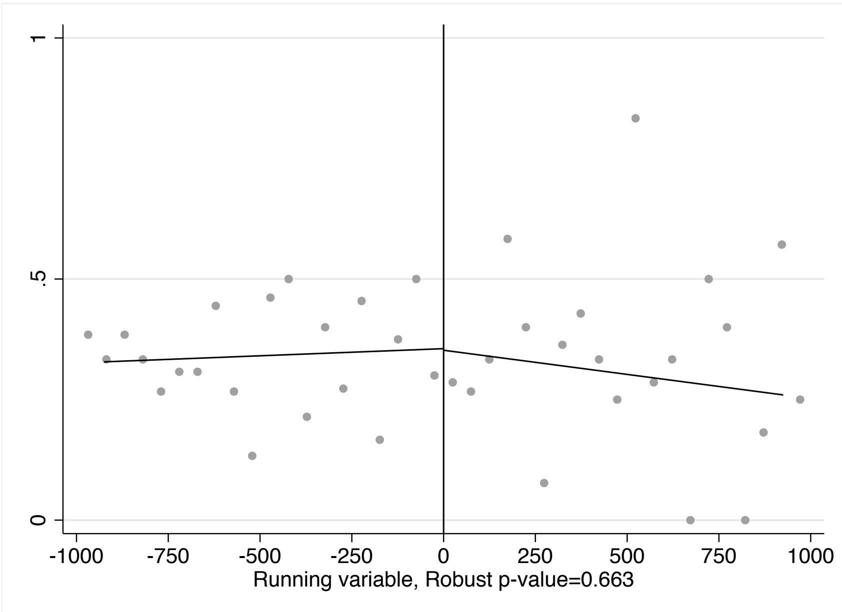


Figure 4.3 Probability of mayors being in their second term in 2014

Notes: Vertical axis: Binary variable for whether a mayor is in her second term. Horizontal axis: 2011 census population size centered around the 3,000 inhabitants discontinuity. Dots represent the average value of the likelihood of a district's mayor being in her second term within a given bin of the running variable. The number of bins used on each side of the threshold is set at 20. The line fits a linear trend on each side of the population threshold to facilitate visualization.

4.4 Main results

4.4.1 Validity checks

We first test whether assumption 1 on the sorting of municipalities is satisfied by conducting the McCrary (2008) manipulation test and reporting the *P*-value of the alternative manipulation test provided by Cattaneo et al. (2018) and Cattaneo et al. (2020) in Figure 4.4.

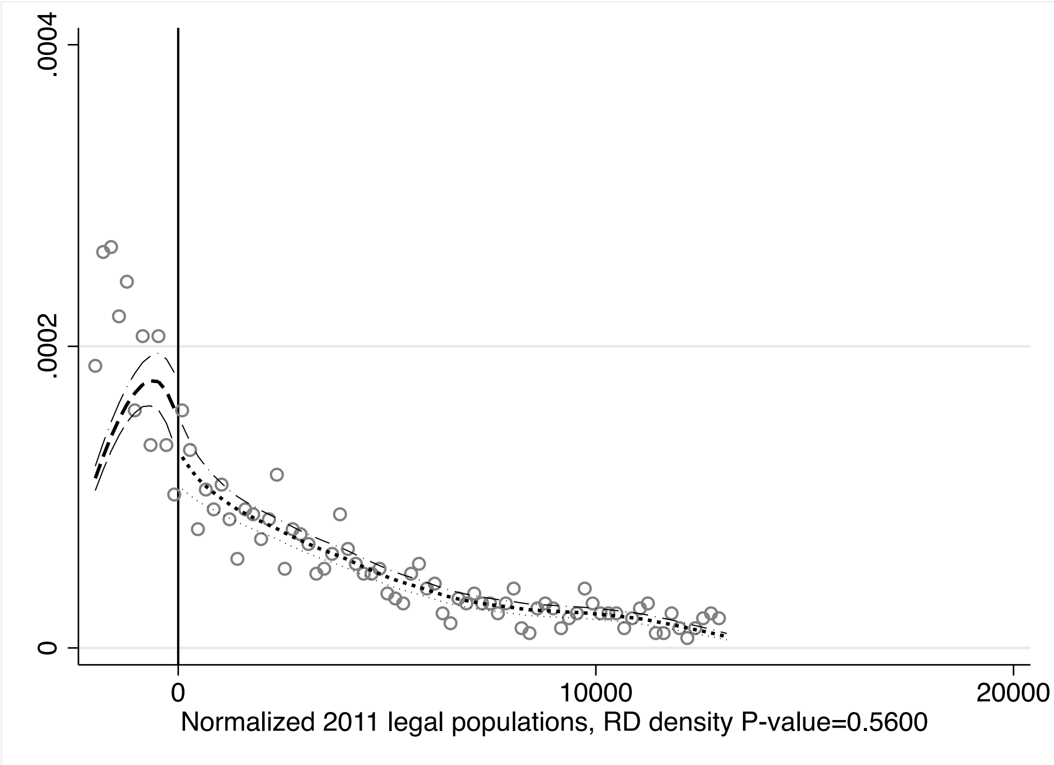


Figure 4.4 McCrary test of manipulation of 2011 census population

Both the McCrary (2008) test for a jump at the discontinuity and the alternative RD density test (Cattaneo et al. (2018)) reported *P*-value (0.56) suggest there is no jump at the discontinuity.

In a second step, we perform placebo observables' tests on pre-reform geographic and socio-economic variables (see Appendix Table D.2) and verify that districts below and above the discontinuity do not differ on observable characteristics that could potentially put in question the quasi-random nature of assignment to the reform of districts below or above 3,000 inhabitants.

Overall this provides reassuring evidence of no manipulation of districts on either side of the discontinuity.

4.4.2 Municipality Revenues

We start by investigating the effects of extending term limits on municipality revenue items in Table 4.2. Each column refers to a specific outcome we study. We report the effects on first-term and second-term mayors in the first row and second row, respectively. We then report the number of observations in the sample chosen by the procedure we described in Section 4.3.1. The bandwidth refers to the average of the two bandwidths selected by the algorithm of Calonico et al. (2014a) and Calonico et al. (2014b), while the mean refers to the pre-treatment value of each outcome in small towns with less that 3000 residents.

Table 4.2 suggests that mayors below the discontinuity react differently based on the term they are in at the time of the reform. According to our estimates, first-term mayors react to the reform by increasing capital revenue. The possibility to run for an additional term after the second is associated with an approximate increase of EUR 269 per capita in capital revenues (89 percent).

Visually, these results are consistent with the top panel of Figure 4.5 where we plot the difference post and pre-2014 of the average capital revenue per capita for each district against the running variable. We notice a clear negative jump above the discontinuity for districts with a mayor in her first term at the time of the reform.

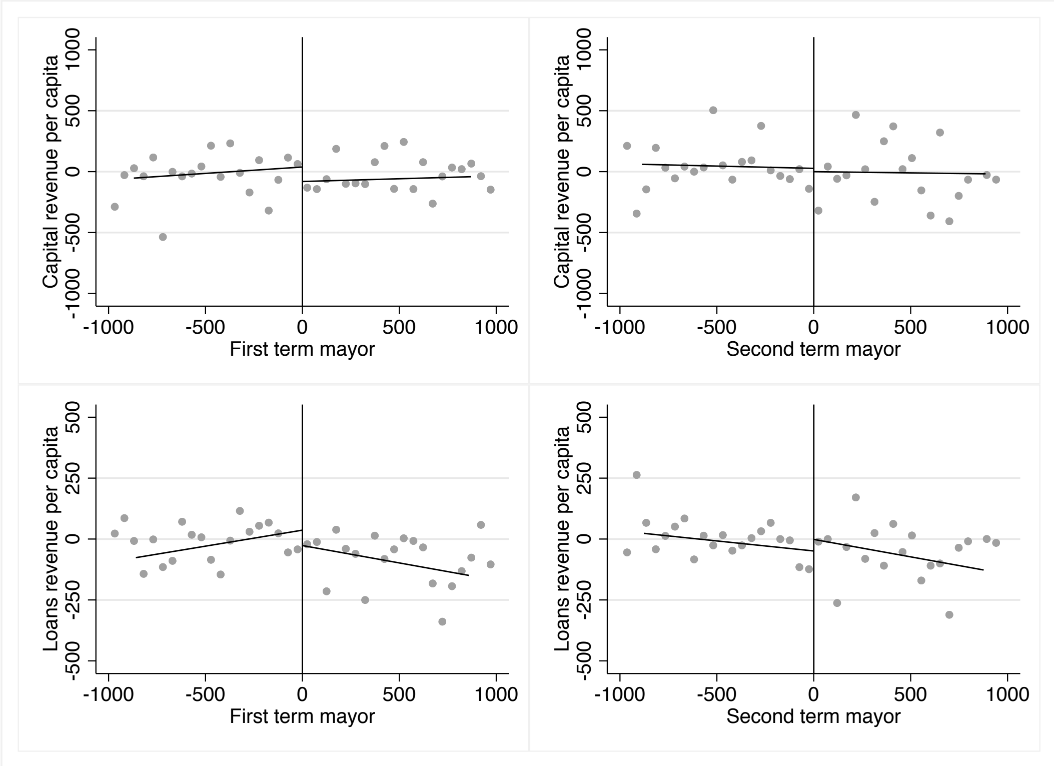


Figure 4.5 Difference in discontinuities - Revenue

Notes: Vertical axis: difference of the average post-reform outcome value and pre-reform outcome value. Results are displayed on the left hand side for first-term mayors and on the right hand side for second-term mayors. Horizontal axis: 2011 census population size centered around the 3,000 inhabitants discontinuity. Dots represent the average value of the likelihood of a district's mayor being in her second term within a given bin of the running variable. The number of bins used on each side of the threshold is set at 20. The line fits a linear trend on each side of the population threshold to facilitate visualization.

Moreover, we provide evidence that trends are locally parallel for first-term and second-term mayors for capital revenue in the top panel of Figure 4.6 by plotting the year-by-year cross-sectional RD estimates. While we notice some changes in the coefficients before the reform, these are only statistically significant in 2011 and hence we can exclude that capital revenues in towns with more versus less than 3000 residents were on a locally different trends before 2014. The variation we observe could be due to the 2011 census updating populations that caused some municipalities to switch sides above and below the discontinuity. Most importantly, we see no clear distinct pattern in the coefficients of interest, especially for first-term mayors. We also provide evidence in Section 4.4.5 that the main results are unchanged when removing the 2014 election from the sample, where we notice a small positive jump.

In the bottom panel of Figure 4.6, we show that while the magnitude of our estimates for capital revenue are somewhat sensitive to the bandwidth chosen, the point estimate for first-term mayors is consistently positive oscillating between EUR 200 and EUR 300 per capita and statistically significant at either the five or ten percent level for a wide range of bandwidths.

Table 4.2 Effect of extending term limits on per capita revenue

Outcome	(1) Fiscal revenue	(2) Capital revenue	(3) Current Transfers	(4) Fees & Tariffs
First-term mayor (β_0)	-25.007	268.878**	11.989	-7.720
	(0.451)	(0.024)	(0.740)	(0.841)
Second-term mayor (β_0+π_0)	91.48	166.127	-45.483	-145.395
	(0.26)	(0.529)	(0.36)	(0.28)
Observations	2,315	2,400	2,752	1,901
Bandwidth	600	622	699	494
Mean	414	303	197	237

Outcome	(5) Loans revenue	(6) Tax rates	(7) Total revenue
First-term mayor	111.312*	0.126	346.047
	(0.056)	(0.574)	(0.169)
Second-term mayor	201.798**	0.192	110.365
	(0.011)	(0.596)	(0.776)
Observations	2,386	2,559	2,672
Bandwidth	619	688	680
Mean	112	8	1,384

Notes: All variables except tax rates are expressed in per capita terms. Municipality level cluster robust p-values in parentheses: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The chosen bandwidth is computed as the average of the two bandwidths selected with the procedure developed by Calonico et al. (2014a) and Calonico et al. (2014b) on each sample obtained by splitting between observations dated before and after 2015. The observations refers to the number of observations within the bandwidth. The mean refers to the average pre-treatment value of the outcome variable for *treated* municipalities.

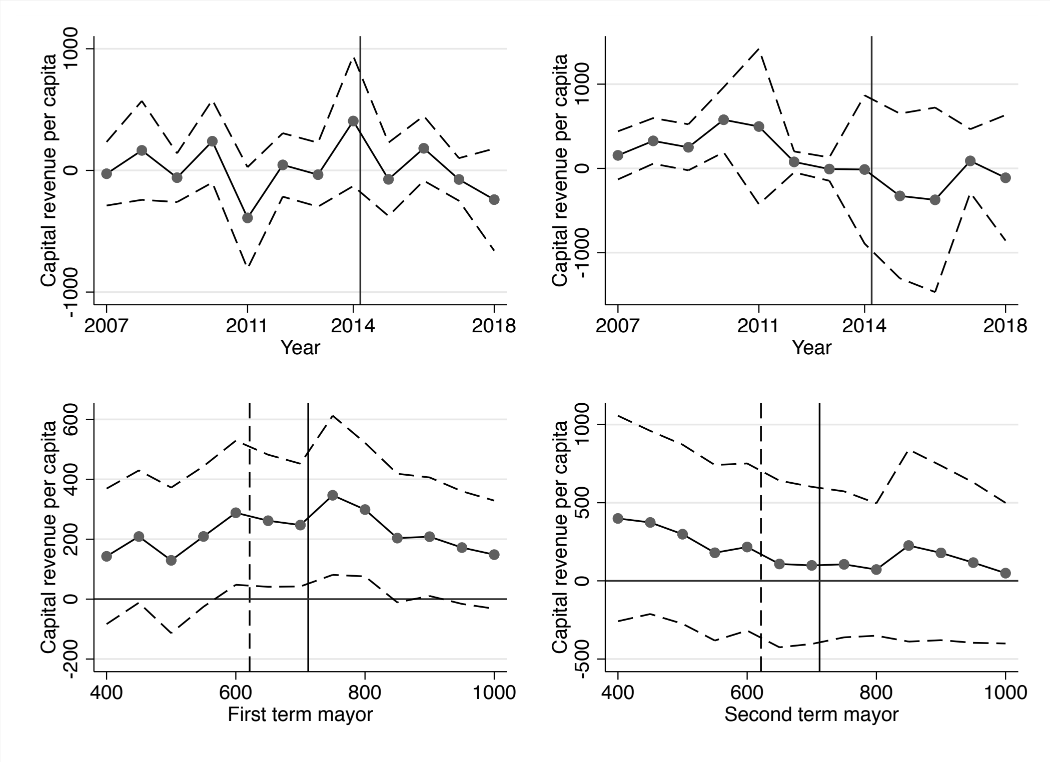


Figure 4.6 Capital revenue

Notes: Top panel: yearly RD coefficients. Vertical axis: point estimates of local linear regressions using optimal bandwidths selected by the procedure developed by Calonico et al. (2014a) and Calonico et al. (2014b). Horizontal axis: years. The vertical line represents the introduction of the law. Bottom panel: difference in discontinuities by bandwidth. Vertical axis: *difference-in-discontinuities* coefficients. Horizontal axis: bandwidth used to estimate the coefficient. The dashed vertical line represents the bandwidth chosen when using local linear regression to determine the estimator (p=1), while the dark vertical line shows the bandwidth when using a quadratic fit (p=2). In both panels the central lines represent the point estimates while the lateral lines the 95 percent confidence interval while results are displayed on the left hand side for first-term mayors and on the right hand side for second-term mayors.

Additionally, mayors affected by the treatment in their first term do not only resort to an increase in capital revenue but also increase loans revenue by approximately EUR 111 per capita. While this result is only significant at the ten percent level, it appears to be robust for a wide range of bandwidths as shown in the bottom panels of Figure 4.7. We find suggestive evidence that second-term mayors also increase revenue from loans by approximately EUR 202 per capita (180 percent). The bottom panel of Figure 4.5 plots the difference before and after 2014 of the loans revenue per capita for each district against the running variable while Figure 4.7 repeats the exercise on local parallel trends and bandwidth sensitivity for this item. Even though the identifying assumption on parallel trends before treatment is verified (See Figure 4.7), we treat this latter result with greater caution as the effect is indeed more sensitive to the choice of bandwidth, as highlighted in the bottom right-hand side graph of Figure 4.7 and we cannot observe visual evidence of the effect in the RD plot at the bottom right hand side panel of Figure 4.5. This, we argue in Section 4.5 may be due to the heterogeneity of results between the south and the rest of Italy.

Finally, other revenue items do not seem to be affected by the treatment. The point estimates for the effect on total revenue for first-term mayors suggests an important increase, although it is not significant. In Appendix Figure D.2, we repeat the exercises conducted in Figure 4.5 for total and other revenue outcomes and show reassuring evidence that the relatively large standard errors for these other revenue outcomes may be due to outliers and no clear visual jump is detected for outcomes other than total revenue. Appendix Figures D.3 through D.5 show the exercise conducted in the top panels of Figures 4.6 and 4.7 for all revenue items for first and second-term mayors, as well as the pooled sample and finds no strong evidence of pre-trends for the great majority of these outcomes.

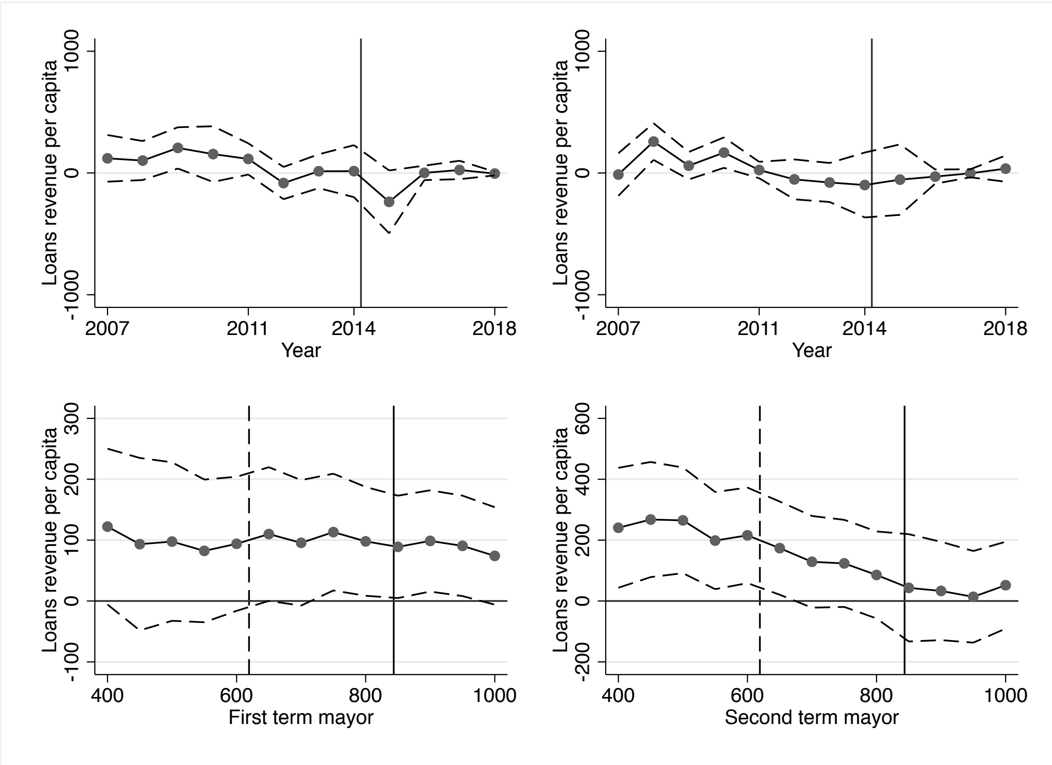


Figure 4.7 Difference in discontinuities - Loans revenue

Notes: Top panel: yearly RD coefficients. Vertical axis: point estimates of local linear regressions using optimal bandwidths selected by the procedure developed by Calonico et al. (2014a) and Calonico et al. (2014b). Horizontal axis: years. The vertical line represents the introduction of the law. Bottom panel: difference in discontinuities by bandwidth. Vertical axis: *difference-in-discontinuities* coefficients. Horizontal axis: bandwidth used to estimate the coefficient. The dashed vertical line represents the bandwidth chosen when using local linear regression to determine the estimator (p=1), while the dark vertical line shows the bandwidth when using a quadratic fit (p=2). In both panels the central lines represent the point estimates while the lateral lines the 95 percent confidence interval while results are displayed on the left hand side for first-term mayors and on the right hand side for second-term mayors.

4.4.3 Municipality Expenditures

Table 4.3 provides the estimates of the effects on the three broad municipal spending categories and total spending per capita. As could be expected, results broadly mirror the findings exposed in Section 4.4.2 on districts' revenue generation. Capital spending in districts run by mayors in their first term, and thus made twice-reeligible, increases by approximately EUR 306 per capita (76 percent). On the other hand, mayors in their second term, who would otherwise be lame ducks, increase loans repayments per capita by approximately EUR 173 (157 percent). Both results are significant at least at the five percent level. Current spending is unaffected while the possibility to run for a third term is also associated to an increase in loan repayments by EUR 86 (78 percent) for districts with a first-term mayor. The point estimate on total spending suggests a strong increase for first-term mayors (EUR 315; 23 percent) although not significant at conventional levels.

Appendix Figure D.6 provides the visual evidence of the jumps for loan repayments and capital spending while Appendix Figures D.7 and D.8 show the results on the sensitivity of the bandwidth and local parallel trends for both outcomes. All three Figures parallel the findings of Section 4.4.2 with a clear visual jump for capital spending per capita in districts run by a mayor in her first term. The point estimates remain stable for a wide range of bandwidth choice and there is no evidence of local pre-trends.

Table 4.3 Effect of extending term limits on per capita expenditure

	(1)	(2)	(3)	(4)
Outcome	Current spending	Capital spending	Loans repayment	Total spending
First-term mayor	-12.397	305.759**	86.326*	314.724
	(0.847)	(0.015)	(0.055)	(0.177)
Second-term mayor	-76.376	273.683	172.575***	188.669
	(0.614)	(0.323)	(0.001)	(0.626)
Observations	2,086	2,309	2,400	2,691
Bandwidth	541	599	622	687
Mean	780	402	110	1,395

Notes: All variables are expressed in per capita terms. Municipality level cluster robust p-values in parentheses: *** p<0.01, ** p<0.05, * p<0.1. The chosen bandwidth is computed as the average of the two bandwidths selected with the procedure developed by Calonico et al. (2014a) and Calonico et al. (2014b) on each sample obtained by splitting between observations dated before and after 2015. The observations refers to the number of observations within the bandwidth. The mean refers to the average pre-treatment value of the outcome variable for *treated* municipalities.

Table 4.4 further investigates the impacts of the reform on several specific expenditure items. According to our point estimates, the potential to run for a third term is associated with an increase in public housing expenditure by approximately EUR 32 per capita (355 percent). This is confirmed visually when plotting the difference of the average expenditure on public housing post and pre-reform against the running variable in Appendix Figure D.9. Although not significant, point estimates suggest first-term mayors react by also increasing urban planning expenditure. Other items are mostly unaffected.

Second-term mayors, on the other other hand, react by reducing spending in public housing by EUR 26 per capita (289 percent) and by increasing transport and mobility related spending by EUR 167 (117 percent) per capita. Although not significant at conventional levels, mayors in their second term also appear to compensate the increase in transport related expenditure by reducing other items such as spending in cultural activities.

In Appendix Figures D.10 and D.11, we show with yearly RDD coefficients that public housing and transport spending were on local common trends in the years leading up to the reform. These latter figures also show that the magnitude of the point estimates we find are also stable over a wide range of bandwidths. The parallel trends assumption is broadly supported for other expenditure items including current spending and total spending in districts run by first-term mayors (Appendix Figure D.12) and second-term mayors (Appendix Figure D.13) as well as for the pooled sample (Figure D.14). Appendix Figures D.16 and D.15 visually confirm that, except for results on total spending and urban planning for first-term mayors, the large standard errors are driven by outliers and that no clear jump is otherwise visible.

Table 4.4 Effect of extending term limits on specific per capita expenditure items

	(1)	(2)	(3)	(4)	(5)
Outcome	Public Housing	Urban planning	Water supply	Waste	Justice
First-term mayor	32.298**	48.313	37.720	-5.650	-0.466
	(0.024)	(0.112)	(0.173)	(0.733)	(0.276)
Second-term mayor	-25.906***	42.292	47.829	36.719	1.006
	(0.005)	(0.379)	(0.274)	(0.471)	(0.584)
Observations	2,823	2,601	1,649	2,672	2,619
Bandwidth	717	663	432	681	668
Mean	9	67	36	134	0.442
	(6)	(7)	(8)	(9)	(10)
Outcome	Police	Culture	Transport	Schools	Social
First-term mayor	-1.533	1.498	41.288	10.844	-7.848
	(0.794)	(0.927)	(0.231)	(0.579)	(0.774)
Second-term mayor	10.964	-19.892	166.84*	-28.533	-21.963
	(0.36)	(0.115)	(0.078)	(0.504)	(0.604)
Observations	2,160	2,271	1,787	2,490	1,831
Bandwidth	561	588	463	639	475
Mean	28	32	143	117	130

Notes: All variables are expressed in per capita terms. Municipality level cluster robust p-values in parentheses: *** p<0.01, ** p<0.05, * p<0.1. The chosen bandwidth is computed as the average of the two bandwidths selected with the procedure developed by Calonico et al. (2014a) and Calonico et al. (2014b) on each sample obtained by splitting between observations dated before and after 2015. The observations refers to the number of observations within the bandwidth. The mean refers to the average pre-treatment value of the outcome variable for *treated* municipalities.

4.4.4 Implications for deficits and efficiency

To assess whether the reform impacts the performance of the municipal government we follow previous literature (see e.g., Drago et al., 2014; Gagliarducci and Nannicini, 2013) and compute two efficiency benchmarks.

We first construct a measure of revenue collection efficiency by calculating the ratio between collected revenues and the total amount of assessed revenues that the municipality should collect within the budget year.

Second we construct a measure of the speed of payment which we define as the ratio between current and capital outlays that were actually paid versus the outlays committed in the municipal budget within the year.

Additionally, we investigate the impact of the policy on overall municipal deficits.

Table 4.5 shows the results and provides suggestive evidence that the policy negatively impacts the revenue collection and current spending efficiency of districts run by mayors in their second term. A term limit extension in these cases is associated with a reduction by 8.1 percentage points (11.6 percent) in revenue collection efficiency and a decline by 5.7 percentage points in current spending efficiency (7.6 percent). The result on revenue collection efficiency is significant at the ten percent level while the result on current spending is not. Despite large standard errors, results also point to a decline in capital spending efficiency and an increase in total deficits per capita in districts run by mayors in their second term.

On the other hand, the point estimates are much weaker in magnitude and far from statistically significant for first-term mayors.

Table 4.5 Effect of extending term limits on efficiency measures and deficits

Outcome	(1) Revenue collection efficiency	(2) Current spending efficiency	(3) Capital spending efficiency	(4) Total deficit per capita
First-term mayor	0.024	0.010	-0.040	-1.557
	(0.508)	(0.647)	(0.648)	(0.971)
Second-term mayor	-0.081*	-0.057	-0.10	74.434
	(0.097)	(0.114)	(0.35)	(0.198)
Observations	2,155	2,357	1,976	2,252
Bandwidth	559	608	517	581
Mean	0.70	0.75	0.18	7

Notes: All variables are expressed in per capita terms. Municipality level cluster robust p-values in parentheses: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The chosen bandwidth is computed as the average of the two bandwidths selected with the procedure developed by Calonico et al. (2014a) and Calonico et al. (2014b) on each sample obtained by splitting between observations dated before and after 2015. The observations refers to the number of observations within the bandwidth. The mean refers to the average pre-treatment value of the outcome variable for *treated* municipalities.

Appendix Figure D.17 shows that the point estimate on current spending efficiency is negative for a broad range of bandwidths while it is stable before increasing at around 800 inhabitants from the cutoff for revenue collection efficiency. Moreover, the point estimates on the deficit outcome variable for second-term mayors are large for a broad range of bandwidths.

In Appendix Figure D.18, we confirm visually the evidence of a jump in revenue collection and current spending efficiency for second-term mayors while Appendix Figures D.19 through D.21 plot the yearly RDD estimates of first, second-term mayors, and the pooled sample, respectively, to check that the local common pre-trends' assumption is satisfied. Importantly, these seem to be satisfied for the deficit and efficiency measures for districts governed by second-term mayors at the time of the reform.

4.4.5 Robustness

We first perform a series of placebo tests to evaluate the chance that our results reflect random chance rather than causal effects. Following DellaVigna and La Ferrara (2010), we conduct for each of our main outcomes - capital revenue, loans revenue, loans repayments, capital spending, public housing spending, transport spending, revenue collection, and current spending efficiency - ten *difference-in-discontinuity* estimations at five false population thresholds below and above the discontinuity. Specifically we run for each of these outcomes the main *difference-in-discontinuity* estimation at false population thresholds increasing in increments of 100 from 1,900 to 2,300 and from 3,700 to 4,100 inhabitants. We choose these false cutoffs sufficiently far from the actual discontinuity of interest and do not expect to find any systematic evidence of treatment effects of similar magnitude to the ones found in the baseline. This is verified in Appendix Tables D.3 to D.10 where the number of significant estimates represents approximately ten percent of the reported coefficients, as would be expected.[4]

In the spirit of Grembi et al. (2016), we further test assumption two of no local common pre-trends further by restricting our sample from 2007 to 2014 and apply a *difference-in-discontinuity* estimation treating the years post-2010 as the post-treatment period. We report the results from this estimation in Appendix Table D.11. Except from the positive significant point estimate on loans repayments per capita of first-term mayors, none of the point estimates on significant results are of the same magnitudes or significance as the ones we find in our baseline results.[5]

Finally we show the robustness of the main results to excluding 2014, the year the policy came into place, from the main sample. Since budgetary decisions are usually taken at the beginning of the year, we consider 2014 as a pre-treatment year. However, different districts may have different timelines and different degrees of flexibility to adjust budgetary decisions such that 2014 for some districts may actually be part of the treatment period, potentially adding some noise to our baseline results. Appendix Table D.12 reports the *difference-in-discontinuity* coefficients of the baseline regressions excluding 2014. For first-term mayors, the coefficients of interest on capital spending, public housing expenditure, and capital revenue are of similar magnitude to the ones of the baselines. Results on loans revenue and repayments lose significance at conventional levels.

For districts with mayors in their second term, results are qualitatively similar for loans repayments, public housing expenditure, loans revenue per capita, and current spending and revenue collection efficiency. The result on transport per capita remains stable in magnitude but is less precisely estimated.

Overall, these placebo tests and alternative regressions provide support for the robustness of the effects we find for most of results exposed in Sections 4.4.2, 4.4.2, and 4.4.4. Specifically, they provide support for the positive effects of the policy found on capital spending, capital revenue, and public

[4]The maximum for an outcome is 4 out of 20 significant coefficients at conventional levels for capital revenues per capita. However, we are not too concerned by this result, as they all concern mayors in their second term, for which we find no result. Furthermore, the false thresholds for which they are significant are in the vicinity of each other.

[5]The point estimates on transport and revenue collection efficiency in this placebo regression are also significant but these are for first-term mayors, as opposed to the main findings that the treatment affects second-term mayors for these outcomes.

housing expenditure for first-term mayors. For second-term mayor, they support results related to the positive impact on loans revenue and repayments, as well as the negative effects on efficiency measures and public housing expenditure. It is worth noting, however, that the results on loans revenue and loans payments for first-term mayors should be treated with greater caution given the sensitivity of results on these outcomes to the exclusion of year 2014.

4.5 Mechanisms

4.5.1 Compound treatment

The main question this paper seeks to address is the effect of sudden new re-election incentives on the behaviour of otherwise comparable mayors. The 2014 reform in Italy specified that mayors in districts below 3,000 inhabitants were now able to be in office for three consecutive terms. However, it also ruled that municipalities with more than 3000 residents, starting from the subsequent elections, will elect more municipal councillors (from 9 to 12 or from 7 to 12 depending on the electoral cycle) and be obliged to respect a 40 percent gender quota in the appointment of mayor's executive board. If mayors of small towns above 3,000 inhabitants were to react to the passing of the law before the next election, i.e. before the law is implemented effectively, then we might be running the risk of estimating the impact of upcoming changes in council size and/or the introduction of gender quotas on the executive body above the discontinuity (see Eggers et al., 2018 for a discussion on compound treatment in RDDs based on population thresholds).

While we cannot unilaterally rule out this second channel can affect our outcomes of interest, we argue that the main operating factor remains the extension of term limits. Unlike reelection incentives, it is unclear how changes in requirements relating to the council size and executive body would affect our dependent variables before being implemented, particularly for second-term mayors above the discontinuity who are serving their final term.

The fact we observe dynamic differential impacts based on the term of the incumbent also seems to confirm this hypothesis, as we would not expect this to be the case if the other upcoming changes were major contributing factors.

Finally, we assess whether the introduction of the gender quota requirement above the discontinuity has a direct impact on the gender composition of the executive board before its actual implementation (or before the next municipal election). A way in which this aspect of the reform could matter before enactment, it is if mayors decided to anticipate the implementation of the policy by appointing more women in their executive board before the gender quota was legally binding. Let us assume that complying with the quota before it was mandatory could bring political consensus. Then, having more women appointed as board members could affect policy choices and confound the effect of terms limit extension.

We test this hypothesis with a *difference-in-discontinuity* design that compares the gender composition of executive boards in municipalities above and below the threshold before and after the approval of the reform (not the actual implementation). We interact the *treatment* dummy ($S_i \times T_t$, where S_i equals 1 for municipalities with less than 3000 residents and T_t equals 1 for years after 2014) with dummies for first and second-term mayors. As shown in Table 4.6, the point estimate on mayors in their first term is extremely low while the point estimate on second-term mayors shows a decrease in the share of women in the council of 5.2 percentage points. As, none of these two effects are significant at any conventional level, we can be more confident that we are mostly measuring the effect of term limit extension.

Table 4.6 Effect of extending term limits on share of women in municipal boards

	(1)
Outcome	Female share in the executive board (*Giunta*)
First-term mayor	0.009
	(0.839)
Second-term mayor	-0.052
	(0.395)
Observations	420
Bandwidth	506
Mean	0.193

Notes: This *difference-in-discontinuity* estimation compares the share of women sitting on each districts' board before and after the reform using the information on the council composition at the beginning and the end of each election district's election cycle. Municipality level cluster robust p-values in parentheses: *** p<0.01, ** p<0.05, * p<0.1. The chosen bandwidth is computed as the average of the two bandwidths selected with the procedure developed by Calonico et al. (2014a) and Calonico et al. (2014b) on each sample obtained by splitting between observations dated before and after 2015. The observations refers to the number of observations within the bandwidth. The mean refers to the average pre-treatment value of the outcome variable for *treated* municipalities.

4.5.2 Voter pandering for first versus second-term mayors

There exists a rich body of theoretical literature that predicts that politicians with re-election incentives tend to please voters to remain in office (Barro, 1973; Banks and Sundaram, 1998; and Maskin and Tirole, 2004). This prediction has also been developed specifically on spending and deficits (Aghion and Bolton, 1990). Term limits may not necessarily need to bind to affect policy-making. Smart and Sturm (2013) use an infinite horizon model where politicians have private information on the first-best policy, to compare a two-term versus longer term limit regimes and show that the policy choices of the more constrained mayors differ both in the first and second term. Hence both when the term limit binds and when it does not. This empirical study allows to test both predictions as it randomly allocated a term limit extension to both first-term and second-term mayors.

Section 4.4 confirms the predictions of these theories. Re-election incentives induce mayors to increase spending on salient items, potentially to please voters, while a longer horizon in office induces mayors to increase investments. Extending terms limit by one term (from two to three) does affect policy choices from first term in office and not merely from the second.

First-term mayors face a longer horizon in office and increase capital revenue and capital expenditure, particularly on public housing. If this increase in investments starts paying off in terms of public approval beyond the next election, then this result can be attributed to an increased foresight In simpler terms, a mayor, that potentially faces two re-election rounds rather than one, makes more far-sighted policy choices.

On the other hand, before the reform *treated* second-term mayors are in their final term. First, they have a shorter horizon and have likely lower incentives to invest in long term projects. It is therefore more useful for them to spend on items that pay off in the immediate short-run such as waste management and public transport. They also have likely less room for manoeuvre and thus resort to loans more than their first-term counterparts. This in turn possibly explains the observed reductions in efficiency in collecting taxes, which comes as a side-effect of pandering to voters. This interpretation is substantiated when comparing the effect of the policy on efficiency for second-term mayors in the south versus the rest of Italy in Table 4.8. Although none of the results are significant at conventional levels, the point estimates suggest that districts in the south of Italy, where politicians are held less accountable by voters (Nannicini et al. (2013)), drive the negative results on efficiency and deficits observed for second-term mayors suggesting that pandering indeed takes place where it may work more effectively.

Table 4.7 Effect of extending term limits on deficit and efficiency by region

Outcome	(1) Revenue collection efficiency	(2) Current spending efficiency	(3) Capital spending efficiency	(4) Deficit per capita
First-term mayor	0.025	0.033	0.068	24.201
North and Center	(0.511)	(0.198)	(0.516)	(0.477)
Second-term mayor	-0.015	-0.002	-0.105	55.669
North and Center	(0.799)	(0.965)	(0.490)	(0.449)
First-term mayor	0.044	-0.004	-0.98	-84.18
south	(0.434)	(0.92)	(0.464)	(0.412)
Second-term mayor	-0.127	-0.058	0.085	157.926
south	(0.14)	(0.403)	(0.537)	(0.143)
Observations	2,155	2,357	1,976	2,252
Bandwidth	559	608	517	581
Mean	0.70	0.75	0.18	7

Notes: All variables are expressed in per capita terms. Municipality level cluster robust p-values in parentheses: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The chosen bandwidth is computed as the average of the two bandwidths selected with the procedure developed by Calonico et al. (2014a) and Calonico et al. (2014b) on each sample obtained by splitting between observations dated before and after 2015. The observations refers to the number of observations within the bandwidth. The mean refers to the average pre-treatment value of the outcome variable for *treated* municipalities.

We investigate differences between the south and rest of Italy further in Section 4.5.3 where we shed light on a possible second mechanism that explains our results on first-term mayors.

4.5.3 The effects in the south of Italy

Given the specificity of the south of Italy regarding social capital, unemployment, and corruption (see e.g., (Nannicini et al., 2013), there are reasons to believe the main results we obtained may differ between the south and rest of Italy. This comparison may also further shed light on the mechanisms at play.

Table 4.8 shows the effects of the policy on revenue items when fully interacting our main specification with a binary variable taking value 1 for municipalities in the south, while Tables 4.9 and 4.10 respectively repeat the same exercise for broader and more specific municipal spending categories.

Table 4.8 Effect of extending term limits on revenue by region

	(1)	(2)	(3)	(4)	(5)
Outcome	Fiscal revenue	Capital revenue	Fees and tariffs	Loans revenue	Total revenue
First-term mayor	2.242	127.334	-50.409	-19.936	-65.329
North and Center	(0.957)	(0.112)	(0.331)	(0.565)	(0.688)
Second-term mayor	95.304	-160.988	-185.109	186.425***	-25.933
North and Center	(0.406)	(0.162)	(0.355)	(0.003)	(0.943)
First-term mayor	-32.303	499.519*	113.497**	400.059***	1257.957**
south	(0.479)	(0.067)	(0.014)	(0.001)	(0.012)
Second-term mayor	37.455	156.066	-103.135	96.313	-517.811
south	(0.608)	(0.722)	(0.167)	(0.611)	(0.401)
Observations	2,315	2,400	1,901	2,386	2,672
Bandwidth	600	622	494	619	680
Mean	414	303	237	112	1,384

Notes: All variables are expressed in per capita terms. Municipality level cluster robust p-values in parentheses: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The chosen bandwidth is computed as the average of the two bandwidths selected with the procedure developed by Calonico et al. (2014a) and Calonico et al. (2014b) on each sample obtained by splitting between observations dated before and after 2015. The observations refers to the number of observations within the bandwidth. The mean refers to the average pre-treatment value of the outcome variable for *treated* municipalities.

Tables 4.8, 4.9, and 4.10 show that the behavior of mayors differ substantially between the south and the rest of Italy. Second-term mayors in the North increase revenue from loans, while southern ones, perhaps more financially constrained, transfer resources from housing investments to waste management and public transports. In this case, we interpret mayors in both the North and south as pandering to voters using different short term tools.

When it comes to first-term mayors, the estimates show that the positive results on capital spending, capital revenue, and public housing are driven by southern first-term mayors who react to the possibility of staying for two more terms by increasing capital spending and public housing per capita by EUR 576 and EUR 72.6, respectively. Additionally, *treated* first-term mayors in the south increase loans repayments and revenues by approximately EUR 305 and EUR 400 per capita, respectively. This leads to a substantial overall increase in total spending by approximately EUR 1,170 per capita in *treated* southern municipalities run by first-term mayors. To fund this additional spending, fees and tariffs are also increased by EUR 114 per capita.

Contrasting with these results, the point estimates on the main outcomes are much lower in magnitude and mostly not significant for first-term mayors in the North.

Given the relevant socioeconomic differences between the south and the rest of Italy there can be several explanations of this result. On of them stems from the fact that mayors' wages are nominally equivalent throughout Italy but the south has significantly lower costs of living and much fewer employment opportunities.[6] This could imply that the stakes of re-election for southern politicians are higher than in the rest of Italy and that mayors who were made re-eligible by the reform had therefore more incentive to pander voters, especially if investing more requires effort. However, the fact that southern incumbent mayors do not on average run more for reelection than mayors in the rest of Italy does not seem to show that re-election is significantly more valuable in the south.

Table 4.9 Effect of extending term limits on spending by region

	(1)	(2)	(3)	(4)
Outcome	Current spending	Capital spending	Loans repayments	Total spending
First-term mayor	7.780	141.487	-3.919	-84.636
North and Center	(0.918)	(0.103)	(0.894)	(0.615)
Second-term mayor	-213.099	-106.101	152.364***	11.401
North and Center	(0.328)	(0.419)	(0.009)	(0.975)
First-term mayor	2.243	576.419**	304.532***	1170.714***
south	(0.981)	(0.044)	(0.002)	(0.010)
Second-term mayor	-59.167	358.593	173.837	-340.076
south	(0.609)	(0.454)	(0.142)	(0.588)
Observations	2,086	2,309	2,400	2,691
Bandwidth	541	599	622	687
Mean	780	402	110	1,395

Notes: as in Table 4.8.

[6] In 2019 in the Northern regions of Italy the employment rate was between 80 and 70 percent while it was generally below 65 percent and below 50 percent in the regions of Campania, Calabria and Sicily (*Source* Eurostat)

Another possible explanation lays in the different political weight of certain special interests in different areas of Italy. De Feo and De Luca (2017), Pinotti (2015b), and Di Cataldo and Mastrorocco (2020) show that there are relatively more investments in public housing and more people employed in the building sector in areas where the local economy and local politics are significantly infiltrated by the mafia. A main source of growth for Italian criminal organizations has been the control and diversion of public funds towards sector of interests like waste management and housing. The observation that results are driven by southern municipalities where the mafia is more present could therefore reflect the fact that granting local politicians a longer horizon might have increased the incentive for institutional capture by local criminal organizations. In this case, higher municipal investments in housing could be the result of a collusive agreement where a first-term mayor, facing two potential re-elections, trades policies that favour local mafias in exchange of electoral support. We can argue that this agreement is mutually more valuable under a triple-term regime than under a double one. The increased political horizon can explain why this mechanism is only at play in cases where mayors were in their first term but not in their first term. The extension of term limits in municipalities run by mayors in their second term reduces the expected horizon of the politician in power from the perspective of a criminal organization that was potentially already betting on a successor mayor with two mandates to go. We are currently in the process of collecting more data on the presence of mafia at the municipal level to validate this hypothesis.

Table 4.10 Effect of extending term limits on specific expenditure items by region

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Public housing	Schools	Waste	Transport	Development	Culture
First-term mayor	15.347	15.316	-25.599	34.495	1.283	-7.524
North and Center	(0.179)	(0.378)	(0.204)	(0.237)	(0.890)	(0.519)
Second-term mayor	-15.133	-26.914	-18.838	-7.964	-19.091	-22.587
North and Center	(0.199)	(0.646)	(0.785)	(0.898)	(0.178)	(0.108)
First-term mayor	72.597**	18.195	23.36	7.161	90.941	27.594
south	(0.027)	(0.660)	(0.399)	(0.943)	(0.23)	(0.495)
Second-term mayor	-41.354***	-14.684	70.846*	236.304*	-150.448	-18.104
south	(0)	(0.773)	(0.073)	(0.093)	(0.137)	(0.431)
Observations	2,823	2,490	2,672	1,787	1,217.	2,271
Bandwidth	717	639	681	463	326	588
Mean	9	117	134	143	-2	32

Notes: as in Table 4.8.

4.6 Conclusion

In this paper we investigate the effects of extending term limits for mayors on public finance outcomes in the context of small Italian municipalities. We exploit a 2014 reform that granted mayors of towns with less than 3,000 residents the possibility to run for a third term while maintaining a two-term limit for those administering cities with more than 3,000 residents. The framework of this study allows us to measure the effect of a term limit extension controlling for experience, ability and other possible sources of bias. We are also able to study the dynamic and horizon effects of the policy as, predictably, mayors react differently depending on whether they are in their first or second term at the time of the reform. We find that second-term mayors tend to increase revenue through debt, transfer resources towards salient spending items such as waste management and public transport, and become less efficient in collecting taxes. This seems to suggest that they adjust their policies to pander to voters and increase their probability of being elected for a third term. First-term mayors increase revenue through debt and sale of assets and increase investments particularly on public housing. They choose an expenditure item that likely pays off later in terms of political consensus. We can argue that the longer horizon granted by the reform favours the adoption of more far-sighted policy choices.

On a more normative note, we plan to further investigate this effect, as it is almost entirely driven by southern municipalities. The south of Italy is characterized by the widespread presence of criminal organizations in local politics and the economy. There exists empirical evidence suggesting that investments in public works and the building sector (including public housing) is strongly correlated with deep mafia infiltration. We may therefore argue that in the context of the south of Italy the beneficial effects of term limits are higher as the low level of social capital (Nannicini et al., 2013) may tame the positive effects of re-election incentives. Avenues for future research we plan to undertake include the study of whether increasing the horizon of politicians in an environment plagued with criminal organizations leads to increases in the likelihood of collusive agreements between the mafia and local politicians and, hence, whether our results reflect a higher level of political capture by the mafia rather then more far-sighted policy choices.

References

Abraido-Lanza, A. F., Dohrenwend, B. P., Ng-Mak, D. S., and Turner, J. B. (1999). The latino mortality paradox: a test of the" salmon bias" and healthy migrant hypotheses. *American journal of public health*, 89(10):1543–1548.

Abramowitz, A. I. (1988). Explaining senate election outcomes. *The American Political Science Review*, pages 385–403.

Acconcia, A., Corsetti, G., and Simonelli, S. (2014). Mafia and public spending: Evidence on the fiscal multiplier from a quasi-experiment. *American Economic Review*, 104(7):2185–2209.

Aghion, P. and Bolton, P. (1990). Government domestic debt and the risk of default: a political-economic model of the strategic role of debt. *Public debt management: theory and history*, 315.

Akhtari, M., Moreira, D., and Trucco, L. (2022). Political turnover, bureaucratic turnover and the quality of public services. *American Economic Review*.

Aksoy, D. (2010). Who gets what, when, and how revisited: Voting and proposal powers in the allocation of the eu budget. *European Union Politics*, 11(2):171–194.

Albouy, D. (2013). Partisan representation in congress and the geographic distribution of federal funds. *Review of Economics and Statistics*, 95(1):127–141.

Alesina, A., Angeloni, I., and Schuknecht, L. (2005). What does the european union do? *Public Choice*, 123(3-4):275–319.

Alesina, A., Glaeser, E., and Glaeser, E. L. (2004). *Fighting poverty in the US and Europe: A world of difference*. Oxford University Press.

Alesina, A., Glaeser, E., and Sacerdote, B. (2001). Why doesn't the us have a european-style welfare system? Technical report, National bureau of economic research.

Alesina, A., Murard, E., and Rapoport, H. (2019). Immigration and preferences for redistribution in europe. Technical report, National Bureau of Economic Research.

Alexander, H. E. and Federman, J. (1989). *Comparative political finance in the 1980s*, volume 7. Cambridge University Press.

Alt, J. E. and Lowry, R. C. (1994). Divided government, fiscal institutions, and budget deficits: Evidence from the states. *American Political Science Review*, 88(4):811–828.

Altonji, J. G. and Card, D. (2018). *The effects of immigration on the labor market outcomes of less-skilled natives*. Routledge.

Anagol, S. and Fujiwara, T. (2016). The runner-up effect. *Journal of Political Economy*, 124(4):927–991.

Anderson, B., Ruhs, M., Rogaly, B., and Spencer, S. (2006). Fair enough. *Central and East European migrants in low-wage employment in the UK. Joseph Rowntree Foundation*.

Ashworth, S. (2006). Campaign finance and voter welfare with entrenched incumbents. *American Political Science Review*, pages 55–68.

Austen-Smith, D. (1987). Interest groups, campaign contributions, and probabilistic voting. *Public choice*, 54(2):123–139.

Avis, E., Ferraz, C., Finan, F., and Varjao, C. (2022). Money and politics: The effects of campaign spending limits on political entry and competition. *American Economic Journal: Applied Economics*.

Bach, L. (2012). *Faut-il abolir le cumul des mandats ?* Presse de la Rue d'Ulm.

Bailey, M. (2004). The two sides of money in politics: A synthesis and framework. *Election Law Journal*, 3(4):653–669.

Bakker, R., De Vries, C., Edwards, E., Hooghe, L., Jolly, S., Marks, G., Polk, J., Rovny, J., Steenbergen, M., and Vachudova, M. A. (2015). Measuring party positions in europe: The chapel hill expert survey trend file, 1999–2010. *Party Politics*, 21(1):143–152.

Baldwin, R. and Wyplosz, C. (2019). *EBOOK The Economics of European Integration 6e*. McGraw Hill.

Banks, J. S. and Sundaram, R. K. (1998). Optimal retention in agency problems. *Journal of Economic Theory*, 82(2):293–323.

Baron, D. P. (1994). Electoral competition with informed and uniformed voters. *American Political Science Review*, pages 33–47.

Barro, R. J. (1973). The control of politicians: an economic model. *Public choice*, pages 19–42.

Bartik, T. J. (1991). Who benefits from state and local economic development policies?

Baum, C., Schaffer, M., and Stillman, S. (2015). Ivreg29: Stata module for extended instrumental variables/2sls and gmm estimation (v9).

Becker, G. (1990). Reforming congress: Why limiting terms won't work. *Business Week*, 8(Aug):18.

Becker, S. O., Egger, P. H., and Von Ehrlich, M. (2012). Too much of a good thing? on the growth effects of the eu's regional policy. *European Economic Review*, 56(4):648–668.

Becker, S. O., Fetzer, T., et al. (2016). Does migration cause extreme voting? *Center for Competitive Advantage in the Global Economy and The Economic & Social Research Council*, pages 1–54.

Becker, S. O., Fetzer, T., et al. (2018). *Has Eastern European Migration Impacted UK-born Workers?* University of Warwick, Department of Economics.

Becker, S. O., Fetzer, T., and Novy, D. (2017). Who voted for brexit? a comprehensive district-level analysis. *Economic Policy*, 32(92):601–650.

Bekkouche, Y., Cagé, J., and Dewitte, E. (2022). The heterogeneous price of a vote: Evidence from multiparty systems, 1993-2017. *Journal of Public Economics*.

Bell, B., Fasani, F., and Machin, S. (2013). Crime and immigration: Evidence from large immigrant waves. *Review of Economics and statistics*, 21(3):1278–1290.

Ben-Bassat, A., Dahan, M., and Klor, E. F. (2015). Does campaign spending affect electoral outcomes? *Electoral Studies*, 40:102–114.

Benoit, K. and Laver, M. (2006). *Party policy in modern democracies*. Routledge.

Berry, C. R., Burden, B. C., and Howell, W. G. (2010). The president and the distribution of federal spending. *American Political Science Review*, 104(4):783–799.

Bertrand, M., Duflo, E., and Mullainathan, S. (2004). How much should we trust differences-in-differences estimates? *The Quarterly journal of economics*, 119(1):249–275.

Besley, T. and Case, A. (1995). Does electoral accountability affect economic policy choices? evidence from gubernatorial term limits. *The Quarterly Journal of Economics*, 110(3):769–798.

Bickers, K. N. and Stein, R. M. (1996). The electoral dynamics of the federal pork barrel. *American Journal of Political Science*, pages 1300–1326.

Bordignon, M., Nannicini, T., and Tabellini, G. (2016). Moderating political extremism: single round versus runoff elections under plurality rule. *American Economic Review*, 106(8):2349–70.

Borin, A., Macchi, E., and Mancini, M. (2021). Eu transfers and euroscepticism: can?t buy me love? *Economic Policy*, 36(106):237–286.

Borusyak, K. and Jaravel, X. (2017). Revisiting event study designs. *Available at SSRN 2826228*.

Boustan, L. P. (2010). Was postwar suburbanization ?white flight?? evidence from the black migration. *The Quarterly Journal of Economics*, 125(1):417–443.

Bouvet, F. and Dall'Erba, S. (2010). European regional structural funds: How large is the influence of politics on the allocation process? *JCMS: Journal of Common Market Studies*, 48(3):501–528.

Bracco, E., Lockwood, B., Porcelli, F., and Redoano, M. (2015). Intergovernmental grants as signals and the alignment effect: Theory and evidence. *Journal of Public Economics*, 123:78–91.

Brollo, F. and Nannicini, T. (2012). Tying your enemy's hands in close races: the politics of federal transfers in brazil. *American Political Science Review*, 106(4):742–761.

Bulmer, S. (2008). New labour, new european policy? blair, brown and utilitarian supranationalism. *Parliamentary Affairs*, 61(4):597–620.

Butzen, P., De Prest, E., Geeroms, H., et al. (2006). Notable trends in the eu budget. *Economic review*, (ii):49–67.

Cagé, J. (2020). Media competition, information provision and political participation: Evidence from french local newspapers and elections, 1944-2014. *Journal of Public Economics*, 185:104077.

Cagé, J., Le Pennec, C., and Mougin, E. (2021). Money and ideology: Evidence from candidate manifestos. *Working paper*.

Callaway, B., Goodman-Bacon, A., and Sant'Anna, P. H. (2021). Difference-in-differences with a continuous treatment. *arXiv preprint arXiv:2107.02637*.

Calonico, S., Cattaneo, M. D., Farrell, M. H., and Titiunik, R. (2019). Regression discontinuity designs using covariates. *Review of Economics and Statistics*, 101(3):442–451.

Calonico, S., Cattaneo, M. D., and Titiunik, R. (2014a). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, 82(6):2295–2326.

Calonico, S., Cattaneo, M. D., and Titiunik, R. (2014b). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, 82(6):2295–2326.

Cameron, A. C., Gelbach, J. B., and Miller, D. L. (2011). Robust inference with multiway clustering. *Journal of Business & Economic Statistics*, 29(2):238–249.

Cangiano, A. (2010). Uk data sources on international migration and the migrant population: a review and appraisal. *COMPAS Research Resource Paper*.

Card, D. (2001). Immigrant inflows, native outflows, and the local labor market impacts of higher immigration. *Journal of Labor Economics*, 19(1):22–64.

Card, D., Dustmann, C., and Preston, I. (2012). Immigration, wages, and compositional amenities. *Journal of the European Economic Association*, 10(1):78–119.

Carnegie, A. and Marinov, N. (2017). Foreign aid, human rights, and democracy promotion: Evidence from a natural experiment. *American Journal of Political Science*, 61(3):671–683.

Cascio, E. U. and Lewis, E. G. (2012). Cracks in the melting pot: immigration, school choice, and segregation. *American Economic Journal: Economic Policy*, 4(3):91–117.

Castles, F. G. and Mair, P. (1984). Left-right political scales: Some ?expert?judgments. *European Journal of Political Research*, 12:7W8.

Cattaneo, M. D., Jansson, M., and Ma, X. (2018). Manipulation testing based on density discontinuity. *The Stata Journal*, 18(1):234–261.

Cattaneo, M. D., Jansson, M., and Ma, X. (2020). Simple local polynomial density estimators. *Journal of the American Statistical Association*, 115(531):1449–1455.

Cavanaugh, J. E. and Neath, A. A. (2019). The akaike information criterion: Background, derivation, properties, application, interpretation, and refinements. *Wiley Interdisciplinary Reviews: Computational Statistics*, 11(3):e1460.

Clemens, J. and Veuger, S. (2021). Politics and the distribution of federal funds: Evidence from federal legislation in response to covid-19. *Journal of Public Economics*, 204:104554.

Coate, S. (2004a). Pareto-improving campaign finance policy. *American Economic Review*, 94(3):628–655.

Coate, S. (2004b). Political competition with campaign contributions and informative advertising. *Journal of the European Economic Association*, 2(5):772–804.

Corbi, R., Papaioannou, E., and Surico, P. (2019). Regional transfer multipliers. *The Review of Economic Studies*, 86(5):1901–1934.

Coviello, D. and Gagliarducci, S. (2017). Tenure in office and public procurement. *American Economic Journal: Economic Policy*, 9(3):59–105.

Crombez, C. and Swinnen, J. F. (2011). Political institutions and public policy: The co-decision procedure in the european union and the reform of the common agricultural policy. *Available at SSRN 1984572*.

Dahlberg, M., Edmark, K., and Lundqvist, H. (2012). Ethnic diversity and preferences for redistribution. *Journal of Political Economy*, 120(1):41–76.

Dalton, R. J. (2008). The quantity and the quality of party systems: Party system polarization, its measurement, and its consequences. *Comparative Political Studies*, 41(7):899–920.

D'Auria, F., Mc Morrow, K., Pichelmann, K., et al. (2008). Economic impact of migration flows following the 2004 eu enlargement process-a model based analysis. Technical report, Directorate General Economic and Financial Affairs (DG ECFIN), European ....

De Benedetto, M. A. and De Paola, M. (2019). Term limit extension and electoral participation. evidence from a diff-in-discontinuities design at the local level in italy. *European Journal of Political Economy*, 59:196–211.

De Chaisemartin, C. and d'Haultfoeuille, X. (2018). Fuzzy differences-in-differences. *The Review of Economic Studies*, 85(2):999–1028.

de Chaisemartin, C. and D'Haultfoeuille, X. (2019). Two-way fixed effects estimators with heterogeneous treatment effects. Technical report, National Bureau of Economic Research, Inc.

De Chaisemartin, C. and d'Haultfoeuille, X. (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review*, 110(9):2964–96.

de Chaisemartin, C. and D'Haultfœuille, X. (2021). Two-way fixed effects and differences-in-differences with heterogeneous treatment effects: A survey. *Available at SSRN*.

de Chaisemartin, C., D'Haultfœuille, X., and Guyonvarch, Y. (2019). Fuzzy differences-in-differences with stata. *The Stata Journal*, 19(2):435–458.

De Feo, G. and De Luca, G. D. (2017). Mafia in the ballot box. *American Economic Journal: Economic Policy*, 9(3):134–67.

DellaVigna, S. and La Ferrara, E. (2010). Detecting illegal arms trade. *American Economic Journal: Economic Policy*, 2(4):26–57.

Dennison, J. and Geddes, A. (2018). Brexit and the perils of 'europeanised'migration. *Journal of European Public Policy*, 25(8):1137–1153.

DenPoemark, D. (2000). Partisan pork barrel in parliamentary systems: Australian constituency-level grants. *Journal of Politics*, 62(3):896–915.

Di Cataldo, M. and Mastrorocco, N. (2020). Organised crime, captured politicians, and the allocation of public resources. *University Ca'Foscari of Venice, Dept. of Economics Research Paper Series No*, 4.

Dinas, E. and Foos, F. (2017). The national effects of subnational representation: Access to regional parliaments and national electoral performance. *Quarterly Journal of Political Science*, 12(1):1–35.

Dinas, E., Matakos, K., Xefteris, D., and Hangartner, D. (2019). Waking up the golden dawn: does exposure to the refugee crisis increase support for extreme-right parties? *Political analysis*, 27(2):244–254.

Döring, H. (2007). The composition of the college of commissioners: Patterns of delegation. *European Union Politics*, 8(2):207–228.

Döring, H. and Manow, P. (2012). Parliament and government composition database (parlgov). *An infrastructure for empirical information on parties, elections and governments in modern democracies. Version*, 12(10).

Drago, F., Nannicini, T., and Sobbrio, F. (2014). Meet the press: How voters and politicians respond to newspaper entry and exit. *American Economic Journal: Applied Economics*, 6(3):159–88.

Dreher, A., Fuchs, A., Hodler, R., Parks, B., Raschky, P., and Tierney, M. J. (2016). Aid on demand: African leaders and the geography of china's foreign assistance. *Centro Studi Luca d'Agliano Development Studies Working Paper*, (400).

Dreher, A., Sturm, J.-E., and Vreeland, J. R. (2009). Development aid and international politics: Does membership on the un security council influence world bank decisions? *Journal of Development Economics*, 88(1):1–18.

Drinkwater, S., Eade, J., and Garapich, M. (2009). Poles apart? eu enlargement and the labour market outcomes of immigrants in the united kingdom. *International Migration*, 47(1):161–190.

Duggan, J. and Martinelli, C. (2017). The political economy of dynamic elections: Accountability, commitment, and responsiveness. *Journal of Economic Literature*, 55(3):916–84.

Dustmann, C., Casanova, M., Fertig, M., Preston, I., and Schmidt, C. M. (2003). *The impact of EU enlargement on migration flows*. Number 25/03. Research Development and Statistics Directorate, Home Office.

Dustmann, C. and Frattini, T. (2014). The fiscal effects of immigration to the uk. *The economic journal*, 124(580):F593–F643.

Dustmann, C., Frattini, T., and Preston, I. P. (2013). The effect of immigration along the distribution of wages. *Review of Economic Studies*, 80(1):145–173.

Easterly, W. and Levine, R. (1997). Africa's growth tragedy: policies and ethnic divisions. *The quarterly journal of economics*, 112(4):1203–1250.

Egeberg, M. (2010). The european commission. *European union politics*, 3:125–40.

Eggers, A. C. (2015). Proportionality and turnout: Evidence from french municipalities. *Comparative Political Studies*, 48(2):135–167.

Eggers, A. C., Freier, R., Grembi, V., and Nannicini, T. (2018). Regression discontinuity designs based on population thresholds: Pitfalls and solutions. *American Journal of Political Science*, 62(1):210–229.

Ferejohn, J. A. (1974). *Pork barrel politics: Rivers and harbors legislation, 1947-1968*. Stanford University Press.

Ferraz, C. and Finan, F. (2011). Electoral accountability and corruption: Evidence from the audits of local governments. *American Economic Review*, 101(4):1274–1311.

Ferreira, F. and Gyourko, J. (2009). Do political parties matter? evidence from us cities. *The Quarterly journal of economics*, 124(1):399–422.

Ferwerda, J. (2021). Immigration, voting rights, and redistribution: Evidence from local governments in europe. *The Journal of Politics*, 83(1):321–339.

Fetzer, T. (2019). Did austerity cause brexit? *American Economic Review*, 109(11):3849–86.

Fouilleux, E. (2010). The common agricultural policy? *European Union Politics, 3rd ed., edited by Michelle Cini and Nieves Pz-Solno Borrag 34057*.

Fouirnaies, A. (2021). How do campaign spending limits affect elections? evidence from the united kingdom 1885–2019. *American Political Science Review*, 115(2):395–411.

Fouirnaies, A. and Hall, A. B. (2021). How do electoral incentives affect legislator behavior? evidence from us state legislatures. *American Political Science Review*, pages 1–15.

Fouirnaies, A. and Mutlu-Eren, H. (2015). English bacon: Copartisan bias in intergovernmental grant allocation in england. *The Journal of Politics*, 77(3):805–817.

Franck, R. and Rainer, I. (2012). Does the leader's ethnicity matter? ethnic favoritism, education, and health in sub-saharan africa. *American Political Science Review*, 106(2):294–325.

François, A., Visser, M., and Wilner, L. (2022). Using political financing reforms to measure campaign spending effects on electoral outcomes. *Forthcoming at Public Choice*.

Freeman, G. P. (1986). Migration and the political economy of the welfare state. *The Annals of the American Academy of Political and Social Science*, 485(1):51–63.

Gadenne, L. (2017). Tax me, but spend wisely? sources of public finance and government accountability. *American Economic Journal: Applied Economics*, pages 274–314.

Gagliarducci, S. and Nannicini, T. (2013). Do better paid politicians perform better? disentangling incentives from selection. *Journal of the European Economic Association*, 11(2):369–398.

Gavazza, A., Nardotto, M., and Valletti, T. (2019). Internet and politics: Evidence from uk local elections and local government policies. *The Review of Economic Studies*, 86(5):2092–2135.

Gehring, K. and Schneider, S. A. (2018). Towards the greater good? eu commissioners' nationality and budget allocation in the european union. *American Economic Journal: Economic Policy*, 10(1):214–39.

Gerdes, C. (2011). The impact of immigration on the size of government: Empirical evidence from danish municipalities. *Scandinavian Journal of Economics*, 113(1):74–92.

Gherghina, S. and O'Malley, D. J. (2019). Self-determination during the brexit campaign: Comparing leave and remain messages. *Fédéralisme Régionalisme*.

Giua, L. (2020). Removing the wall around welfare: What do we learn from a8 immigrants in the uk? *Economic Policy*, 35(102):305–356.

Giuntella, O., Nicodemo, C., and Vargas-Silva, C. (2018). The effects of immigration on nhs waiting times. *Journal of health economics*, 58:123–143.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*.

Goodwin, M. and Milazzo, C. (2017). Taking back control? investigating the role of immigration in the 2016 vote for brexit. *The British Journal of Politics and International Relations*, 19(3):450–464.

Granzier, R., Pons, V., and Tricaud, C. (2019). *The Large Effects of a Small Win: How Past Rankings Shape the Behavior of Voters and Candidates*. National Bureau of Economic Research.

Gray, M. and Barford, A. (2018). The depths of the cuts: the uneven geography of local government austerity. *Cambridge journal of regions, economy and society*, 11(3):541–563.

Grembi, V., Nannicini, T., and Troiano, U. (2016). Do fiscal rules matter? *American Economic Journal: Applied Economics*, pages 1–30.

Grossman, G. M. and Helpman, E. (1992). Protection for sale. Technical report, National Bureau of Economic Research.

Grossman, G. M. and Helpman, E. (1996). Electoral competition and special interest politics. *The Review of Economic Studies*, 63(2):265–286.

Grossman, G. M. and Helpman, E. (2001). *Special interest politics*. MIT press.

Gulzar, S., Rueda, M. R., and Ruiz, N. A. (2021). Do campaign contribution limits curb the influence of money in politics? *American Journal of Political Science*.

Gunlicks, A. B. (2019). *Campaign and party finance in North America and Western Europe*. Routledge.

Hahn, J., Todd, P., and Van der Klaauw, W. (2001). Identification and estimation of treatment effects with a regression-discontinuity design. *Econometrica*, 69(1):201–209.

Halla, M., Wagner, A. F., and Zweimüller, J. (2017). Immigration and voting for the far right. *Journal of the European Economic Association*, 15(6):1341–1385.

Hanretty, C. (2021). The pork barrel politics of the towns fund. *The Political Quarterly*, 92(1):7–13.

Harmon, N. A. (2018). Immigration, ethnic diversity, and political outcomes: Evidence from denmark. *The Scandinavian Journal of Economics*, 120(4):1043–1074.

Hinich, M. J., Munger, M., et al. (1989). Political investment, voter perceptions, and candidate strategy: An equilibrium spatial analysis. *Models of strategic choice in politics*, pages 49–68.

Hodler, R. and Raschky, P. A. (2014). Regional favoritism. *The Quarterly Journal of Economics*, 129(2):995–1033.

Hopkins, D. J. (2010). Politicized places: Explaining where and when immigrants provoke local opposition. *American political science review*, pages 40–60.

Huber, J. and Inglehart, R. (1995). Expert interpretations of party space and party locations in 42 societies. *Party politics*, 1(1):73–111.

Iaryczower, M. and Mattozzi, A. (2012). The pro-competitive effect of campaign limits in non-majoritarian elections. *Economic Theory*, 49(3):591–619.

Imbens, G. and Angrist, J. (1994). Estimation and identification of local average treatment effects. *Econometrica*, 62:467–475.

Imbens, G. W. and Lemieux, T. (2008). Regression discontinuity designs: A guide to practice. *Journal of econometrics*, 142(2):615–635.

Jacobson, G. C. (1978). The effects of campaign spending in congressional elections. *The American Political Science Review*, pages 469–491.

Jaeger, D. A., Ruist, J., and Stuhler, J. (2018). Shift-share instruments and the impact of immigration. Technical report, National Bureau of Economic Research.

Jaitman, L. and Machin, S. (2013). Crime and immigration: new evidence from england and wales. *IZA Journal of Migration*, 2(1):1–23.

Jakiela, P. (2021). Simple diagnostics for two-way fixed effects. *arXiv preprint arXiv:2103.13229*.

Jofre-Monseny, J., Sorribas-Navarro, P., and Vázquez-Grenno, J. (2016). Immigration and local spending in social services: evidence from a massive immigration wave. *International Tax and Public Finance*, 23(6):1004–1029.

Katz, R. S. and Mair, P. (1994). *How parties organize: change and adaptation in party organizations in Western democracies*, volume 528. Sage.

Katz, R. S. and Mair, P. (2009). The cartel party thesis: A restatement. *Perspectives on politics*, pages 753–766.

Kauppi, H. and Widgrén, M. (2004). What determines eu decision making? needs, power or both? *Economic Policy*, 19(39):222–266.

Kennedy, S. (2011). Eea nationals: the ?right to reside?requirement for benefits. *House of Commons Library Standard Note (Social Policy Section), SN/SP/5972*, pages 1–20.

Klein, F. A. and Sakurai, S. N. (2015). Term limits and political budget cycles at the local level: evidence from a young democracy. *European Journal of Political Economy*, 37:21–36.

Kofman, E., Lukes, S., D'Angelo, A., and Montagna, N. (2009). The equality implications of being a migrant in britain.

Kuziemko, I. and Werker, E. (2006). How much is a seat on the security council worth? foreign aid and bribery at the united nations. *Journal of political economy*, 114(5):905–930.

Laakso, M. and Taagepera, R. (1979). Effective number of parties: a measure with application to west europe. *Comparative political studies*, 12(1):3–27.

Larcinese, V., Rizzo, L., and Testa, C. (2006). Allocating the us federal budget to the states: The impact of the president. *The Journal of Politics*, 68(2):447–456.

Lee, D. L., McCrary, J., Moreira, M. J., and Porter, J. (2020). Valid t-ratio inference for iv. *arXiv preprint arXiv:2010.05058*.

Lee, D. S. (2008). Randomized experiments from non-random selection in us house elections. *Journal of Econometrics*, 142(2):675–697.

Lee, D. S. (2009). Training, wages, and sample selection: Estimating sharp bounds on treatment effects. *The Review of Economic Studies*, 76(3):1071–1102.

Lee, D. S. and Lemieux, T. (2010). Regression discontinuity designs in economics. *Journal of economic literature*, 48(2):281–355.

Levitt, S. D. and Snyder Jr, J. M. (1995). Political parties and the distribution of federal outlays. *American Journal of Political Science*, pages 958–980.

Malhotra, N. (2008). The impact of public financing on electoral competition: Evidence from arizona and maine. *State Politics & Policy Quarterly*, 8(3):263–281.

Manacorda, M., Manning, A., and Wadsworth, J. (2012). The impact of immigration on the structure of wages: theory and evidence from britain. *Journal of the European economic association*, 10(1):120–151.

Marx, B., Pons, V., and Rollet, V. (2022). Electoral turnovers. *Working paper*.

Masket, S. E. and Miller, M. G. (2015). Does public election funding create more extreme legislators? evidence from arizona and maine. *State Politics & Policy Quarterly*, 15(1):24–40.

Maskin, E. and Tirole, J. (2004). The politician and the judge: Accountability in government. *American Economic Review*, 94(4):1034–1054.

Mazumder, S., McNamara, K. R., and Vreeland, J. R. (2013). The buck stops here: What global horse trading tells us about the european project. Technical report, Working Paper.

McCrary, J. (2008). Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of econometrics*, 142(2):698–714.

Meleady, R., Seger, C. R., and Vermue, M. (2017). Examining the role of positive and negative intergroup contact and anti-immigrant prejudice in b rexit. *British Journal of Social Psychology*, 56(4):799–808.

Murray, M. P. (2006). Avoiding invalid instruments and coping with weak instruments. *Journal of economic Perspectives*, 20(4):111–132.

Myerson, R. B. and Weber, R. J. (1993). A theory of voting equilibria. *American Political Science Review*, pages 102–114.

Nannicini, T., Stella, A., Tabellini, G., and Troiano, U. (2013). Social capital and political accountability. *American Economic Journal: Economic Policy*, 5(2):222–50.

Napel, S. and Widgrén, M. (2008). The european commission–appointment, preferences, and institutional relations. *Public Choice*, 137(1-2):21–41.

Nugent, N. (2001). The european commission. *European Union Series. 1st ed. Houndmills, UK: Palgrave Macmillan*.

OECD (2016). *Financing democracy-funding of political parties and election campaigns and the risk of policy capture*. OECD Publishing.

Ortin, I. O., Schultz, C., et al. (2000). Public funding of political parties. In *Econometric Society World Congress 2000 Contributed Papers*, number 0735. Econometric Society.

Palda, F. and Palda, K. (1998). The impact of campaign expenditures on political competition in the french legislative elections of 1993. *Public choice*, 94(1):157–174.

Pastine, I. and Pastine, T. (2012). Incumbency advantage and political campaign spending limits. *Journal of Public Economics*, 96(1-2):20–32.

Pettersson-Lidbom, P. (2008). Do parties matter for economic outcomes? a regression-discontinuity approach. *Journal of the European Economic Association*, 6(5):1037–1056.

Phillips, D. (2018). Equalisation, incentives and discretion in english local public service provision. *Governing England: Devolution and funding*, page 35.

Pinotti, P. (2015a). The causes and consequences of organised crime: Preliminary evidence across countries. *The Economic Journal*, 125(586):F158–F174.

Pinotti, P. (2015b). The economic costs of organised crime: Evidence from southern italy. *The Economic Journal*, 125(586):F203–F232.

Pons, V. and Tricaud, C. (2018). Expressive voting and its cost: Evidence from runoffs with two or three candidates. *Econometrica*, 86(5):1621–1649.

Prat, A. (2002). Campaign advertising and voter welfare. *The Review of Economic Studies*, 69(4):999–1017.

Prat, A., Puglisi, R., Snyder Jr, J. M., et al. (2010). Is private campaign finance a good thing? estimates of the potential informational benefits. *Quarterly journal of political science*, 5(3):291–318.

Rodden, J. (2002). Strength in numbers? representation and redistribution in the european union. *European Union Politics*, 3(2):151–175.

Sá, F. (2015). Immigration and house prices in the uk. *The Economic Journal*, 125(587):1393–1424.

Sahuguet, N. and Persico, N. (2006). Campaign spending regulation in a model of redistributive politics. *Economic Theory*, 28(1):95–124.

Sandford, M. (2018). Local government in england: structures. *House Commons Libr London*.

Scarrow, S. E. (2007). Political finance in comparative perspective. *Annu. Rev. Polit. Sci.*, 10:193–210.

Schaffer, M. (2020). xtivreg2: Stata module to perform extended iv/2sls, gmm and ac/hac, liml and k-class regression for panel data models.

Schneider, C. J. (2013). Globalizing electoral politics: Political competence and distributional bargaining in the european union. *World Politics*, 65(3):452–490.

Senik, C., Stichnoth, H., and Van der Straeten, K. (2009). Immigration and natives? attitudes towards the welfare state: evidence from the european social survey. *Social indicators research*, 91(3):345–370.

Shepsle, K. A. and Weingast, B. R. (1981). Political preferences for the pork barrel: A generalization. *American journal of political science*, pages 96–111.

Simpson, P. (2017). Public spending on adult social care in england. *Institute for Fiscal Studies. https://www. ifs. org. uk/publications/9185*.

Smart, M. and Sturm, D. M. (2013). Term limits and electoral accountability. *Journal of public economics*, 107:93–102.

Smith, A. (2003). Why european commissioners matter. *JCMS: Journal of Common Market Studies*, 41(1):137–155.

Solé-Ollé, A. and Sorribas-Navarro, P. (2008). The effects of partisan alignment on the allocation of intergovernmental transfers. differences-in-differences estimates for spain. *Journal of Public Economics*, 92(12):2302–2319.

Speciale, B. (2012). Does immigration affect public education expenditures? quasi-experimental evidence. *Journal of public economics*, 96(9-10):773–783.

Steinmayr, A. (2020). Contact versus exposure: Refugee presence and voting for the far-right. *Review of Economics and Statistics*, pages 1–47.

Stratmann, T. (2005). Some talk: Money in politics. a (partial) review of the literature. *Policy challenges and political responses*, pages 135–156.

Sumption, M. and Vargas-Silva, C. (2021). Net migration to the uk. *Migration Observatory briefing, COMPAS, University of Oxford*.

Sun, L. and Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225(2):175–199.

Tabellini, M. (2020a). Gifts of the immigrants, woes of the natives: Lessons from the age of mass migration. *The Review of Economic Studies*, 87(1):454–486.

Tabellini, M. (2020b). Racial heterogeneity and local government finances: Evidence from the great migration.

Taylor-Gooby, P. (2013). Why do people stigmatise the poor at a time of rapidly increasing inequality, and what can be done about it? *The political quarterly*, 84(1):31–42.

The Law Library of Congress, G. L. R. C. (2009). Campaign finance : an overview : Australia, france, germany, israel, and the united kingdom. *https://lccn.loc.gov/2018298980*.

Weingast, B. R., Shepsle, K. A., and Johnsen, C. (1981). The political economy of benefits and costs: A neoclassical approach to distributive politics. *Journal of political Economy*, 89(4):642–664.

Wonka, A. (2007). Technocratic and independent? the appointment of european commissioners and its policy implications. *Journal of European Public Policy*, 14(2):169–189.

Ziebarth, D. (2020). Ukip support in local elections: Which factors play a role in determining electoral fortunes? *Available at SSRN 3570053*.

A

Appendix to Chapter 1

Appendix I: Departmental elections

A. Additional tables and figures

Table A1: Impact on outsider and insider candidates - Unconditional outcomes - Main sample of departmental elections

	(1)	(2)	(3)	(4)	(5)	(6)
	Outsider candidates			Insider candidates		
Outcome	run	win	vote share, R1	run	win	vote share, R1
Treatment	-0.004	0.092**	0.018	-0.055**	-0.092**	-0.018
	(0.007)	(0.042)	(0.020)	(0.020)	(0.042)	(0.020)
Robust p-value	0.645	0.024	0.393	0.011	0.024	0.393
Observations	2,153	1,686	2,582	3,289	1,686	2,582
Polyn. order	1	1	1	1	1	1
Bandwidth	2,414	1,886	2,880	3,706	1,886	2,880
Mean, left of threshold	0.995	0.288	0.530	0.932	0.712	0.470

Notes: Standard errors are in parentheses. Robust p-values are used to compute statistical significance. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. Each column reports the results from a separate local polynomial regression. The independent variable is a dummy equal to one if the district has a population above 9,000 inhabitants in election t. Separate polynomials are fitted on each side of the threshold. The polynomial order is one in all columns and the bandwidths are derived under the MSERD procedure. The mean indicates the mean value of the outcome of interest at the cutoff below the discontinuity.

Table A2: Average expenditures and contributions to ceiling ratios in districts between 9,000 and 11,000 inhabitants by candidate orientation

	Far-left	Left	Center	Right	Far-right	Non-classified
Panel A. 1992-1994 elections						
Expenditures	0.104	0.172	*NA*	0.328	0.036	0.083
Personal contributions	0.035	0.032	*NA*	0.139	0.013	0.041
Party contributions	0.008	0.087	*NA*	0.056	0.017	0.019
Donations	0.064	0.062	*NA*	0.145	0.002	0.031
Panel B. 1998-2001 elections						
Expenditures	0.092	0.396	0.558	0.439	0.199	0.134
Personal contributions	0.087	0.310	0.470	0.344	0.178	0.075
Party contributions	0.005	0.035	0.013	0.014	0.004	0.024
Donations	0.005	0.032	0.059	0.077	0.001	0.036

Notes: We focus on districts close to the cutoff (between 9,000 and 11,000 inhabitants). Personal contributions, party contributions, and donations are the three largest sources of candidates' contributions. Other sources of contributions include natural contributions and other contributions such as revenue from investments or of a commercial nature. The sum of contributions does not always add up to total expenditures of candidates, as contributions need not be exhausted. Before 2001, there is no centrist candidate running in departmental elections.

Table A3: Impact on the incumbent's probability of running, winning, and vote share - Main sample of departmental elections - Party candidate

	(1)	(2)	(3)
Outcome	Incumbent		
	run	win	vote share, R1
Panel A. Unconditional effects			
Treatment	-0.094**	-0.164***	-0.082***
	(0.046)	(0.056)	(0.027)
Robust *p*-value	0.036	0.003	0.002
Observations	1,509	1,053	1,209
Polyn. order	1	1	1
Bandwidth	2,530	1,763	2,032
Mean	0.762	0.666	0.366
Panel B. Conditional effects			
Upper bound		-0.215*	-0.108**
Boot. std error		(0.115)	(0.046)
Lower bound		-0.111	-0.050*
Boot. std error		(0.089)	(0.029)
Mean		0.839	0.471

Notes: The sample is restricted to elections where the incumbent is affiliated to a party. Panel A and Panel B show effects on unconditional outcomes and bounds of effects conditional on running, respectively. The notes for Panel A are as in Table A1. In Panel B, the mean, left of the threshold, indicates the value of the outcome for the candidates on the left of the threshold, conditional on running. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively, of the bootstrapped standard errors.

Table A4: Impact on the incumbent's probability of running, winning, and vote share - Main sample of departmental elections - Non-party candidate

	(1)	(2)	(3)
	Incumbent		
Outcome	run	win	vote share, R1
Panel A. Unconditional effects			
Treatment	-0.058	-0.049	-0.024
	(0.054)	(0.076)	(0.033)
Robust *p*-value	0.365	0.455	0.509
Observations	1,098	644	978
Polyn. order	1	1	1
Bandwidth	3,726	2,185	3,295
Mean	0.794	0.660	0.377
Panel B. Conditional effects			
Upper bound		-0.062	-0.030
Boot. std error		(0.108)	(0.050)
Lower bound		0.002	0.005
Boot. std error		(0.083)	(0.031)
Mean		0.878	0.482

Notes: The sample is restricted to elections where the incumbent is not affiliated to a party. Other notes as in Table A3.

Table A5: Impact on the main outcomes - 1992-1994 departmental elections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome	Incumbent		Challenger		Outsider		Victory
	run	win	run	win	run	win	R1
Treatment	-0.013	0.038	-0.045	-0.051*	-0.009	-0.050	-0.015
	(0.051)	(0.066)	(0.061)	(0.030)	(0.009)	(0.070)	(0.064)
Robust *p*-value	0.934	0.535	0.429	0.061	0.186	0.533	0.861
Observations	1,175	1,041	1,021	588	729	871	1,114
Polyn. order	1	1	1	1	1	1	1
Bandwidth	3,818	3,368	3,322	1,910	2,346	2,807	3,601
Mean left of threshold	0.800	0.619	0.309	0.050	1.000	0.345	0.356

Notes as in Table A1.

B. Validity

Table B1: General balance test - All departmental elections, including non-linkable districts

	(1)
Outcome	Predicted treatment
Treatment	0.020
	(0.020)
Robust p-value	0.361
Observations	2,151
Polyn. order	1
Bandwidth	3,031
Mean, left of threshold	0.565

Notes: Standard errors are in parentheses. Robust p-values are used to compute statistical significance. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. Each column reports the results from a separate local polynomial regression. The independent variable is a dummy equal to one if the district has a population above 9,000 inhabitants in election t. The outcome is the value of the treatment predicted by sociodemographic variables: the share of men in the population; the share of under 29 year olds, the share of the population between 30 and 44 years old; the share between 45 and 59 years old; the share above 60 years old; the share of the economically active within the population; the share of unemployed; the share of skilled jobs; the share of workers; the share of employee professions; the share of intermediary professions; the share of artisans; the share of actives working in agriculture. To avoid dropping observations, for each sociodemographic variable, we include a dummy equal to one when the variable is missing and replace by 0s. The independent variable is a dummy equal to one if the district has a population greater or equal to 9,000 in year t. Separate polynomials are fitted on each side of the threshold. The polynomial order is one in all columns and the bandwidths are derived under the MSERD procedure. The mean indicates the mean value of the outcome of interest at the cutoff below the discontinuity. We exclude the 2008 elections since in most districts, the population and, therefore, the running and assignment variables, were the same as in the 2001 elections.

Table B2: Balance tests, sociodemographic characteristics - Main sample of departmental elections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Outcome	Men	Under 29	30-44	45-59	Over 60	Active population	Unemployed	Skilled jobs	Workers	Employees	Intermediary workers	Artisans	Agriculture
Treatment	-0.001	0.005	0.004**	-0.004	-0.007	-0.000	-0.002	0.002	-0.002	-0.003	0.002	0.004	-0.003
	(0.002)	(0.005)	(0.002)	(0.003)	(0.005)	(0.004)	(0.004)	(0.003)	(0.009)	(0.005)	(0.005)	(0.002)	(0.005)
Robust p-value	0.314	0.279	0.043	0.139	0.326	0.780	0.619	0.467	0.931	0.524	0.854	0.102	0.524
Observations	874	2,224	1,998	1,552	2,017	1,453	1,864	2,173	1,852	1,673	2,062	1,459	2,061
Polyn. order	1	1	1	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,814	3,410	3,059	2,365	3,081	2,229	2,819	s3,341	2,812	2,550	3,171	2,244	3,168
Mean, left of threshold	0.493	0.356	0.208	0.186	0.252	0.436	0.116	0.066	0.335	0.278	0.183	0.074	0.064

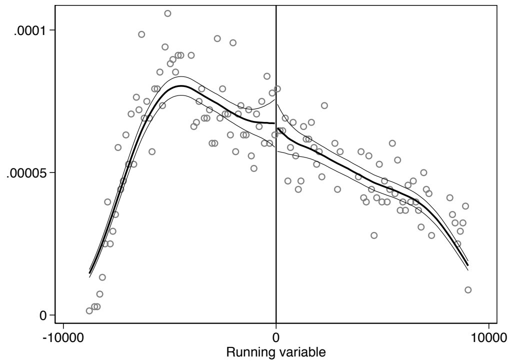
Notes: Standard errors are in parentheses. Robust p-values are used to compute statistical significance. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. Each column reports the results from a separate local polynomial regression. The independent variable is a dummy equal to one if the district has a population above 9,000 inhabitants in election t. Separate polynomials are fitted on each side of the threshold. The polynomial order is one in all columns and the bandwidths are derived under the MSERD procedure. All outcomes refer to shares of the whole population. The mean indicates the mean value of the outcome of interest at the cutoff below the discontinuity. We exclude the 2008 elections since in most districts, the running variable is the same as in 2001 (the same major census was in place for both elections).

Table B3: Balance tests, sociodemographic characteristics - All departmental elections, including non-linkable districts

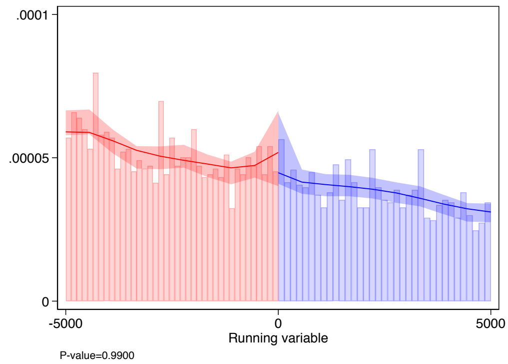
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Outcome	Men	Under 29	30-44	45-59	Over 60	Active population	Unemployed	Skilled jobs	Workers	Employees	Intermediary workers	Artisans	Agriculture
Treatment	-0.001	0.005	0.004**	-0.004	-0.007	-0.000	-0.002	0.002	-0.001	-0.003	0.002	0.004	-0.003
	(0.002)	(0.005)	(0.002)	(0.003)	(0.005)	(0.004)	(0.004)	(0.003)	(0.009)	(0.005)	(0.005)	(0.002)	(0.005)
Robust p-value	0.314	0.274	0.042	0.145	0.319	0.779	0.598	0.485	0.949	0.510	0.887	0.103	0.534
Observations	882	2,245	1,990	1,564	2,020	1,466	1,870	2,188	1,865	1,672	2,044	1,472	2,067
Polyn. order	1	1	1	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,824	3,423	3,035	2,378	3,081	2,244	2,826	3,357	2,830	2,547	3,137	2,256	3,173
Mean, left of threshold	0.493	0.356	0.208	0.186	0.252	0.436	0.116	0.066	0.335	0.278	0.183	0.074	0.064

Notes as in Table B2.

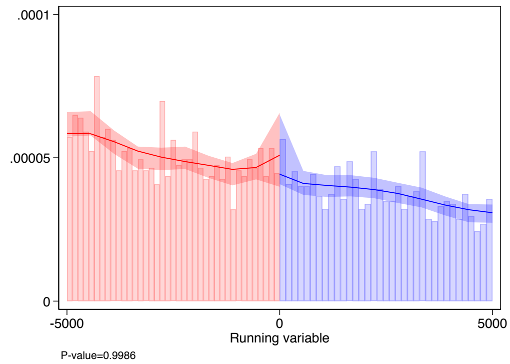
Figure B1: McCrary (2008) and Cattaneo et al. (2018) density tests



McCrary test - All departmental elections, including non-linkable districts



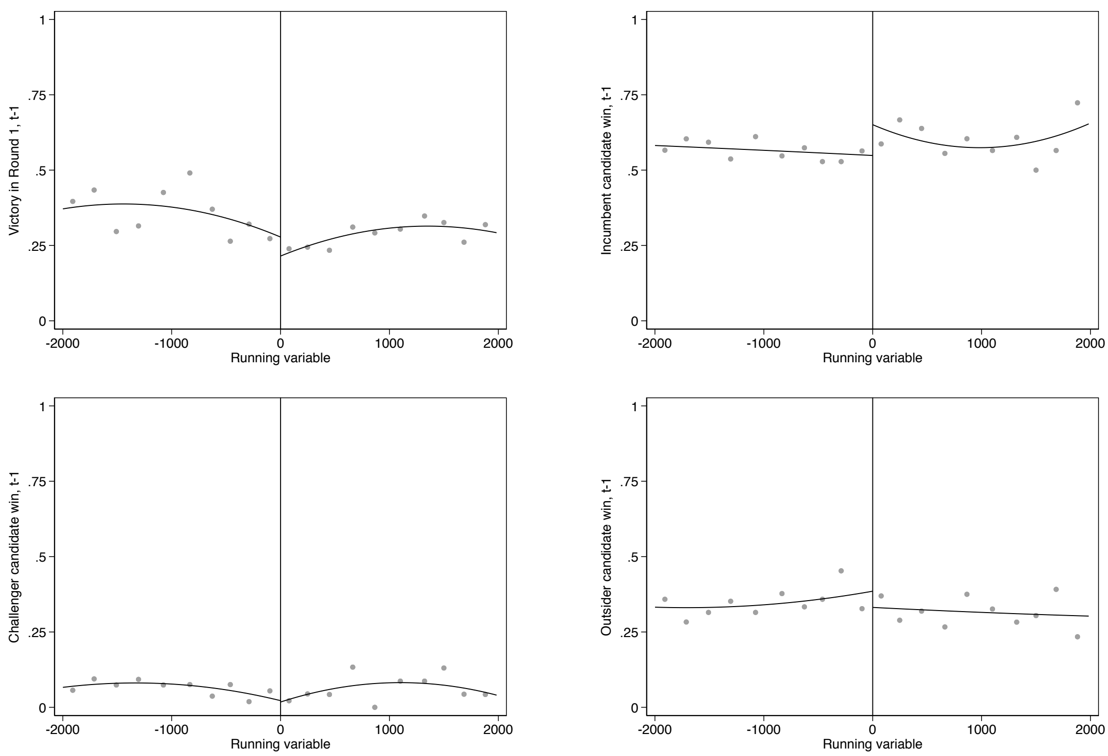
RD Density test- Main sample of departmental elections



RD Density test - All departmental elections, including non-linkable districts

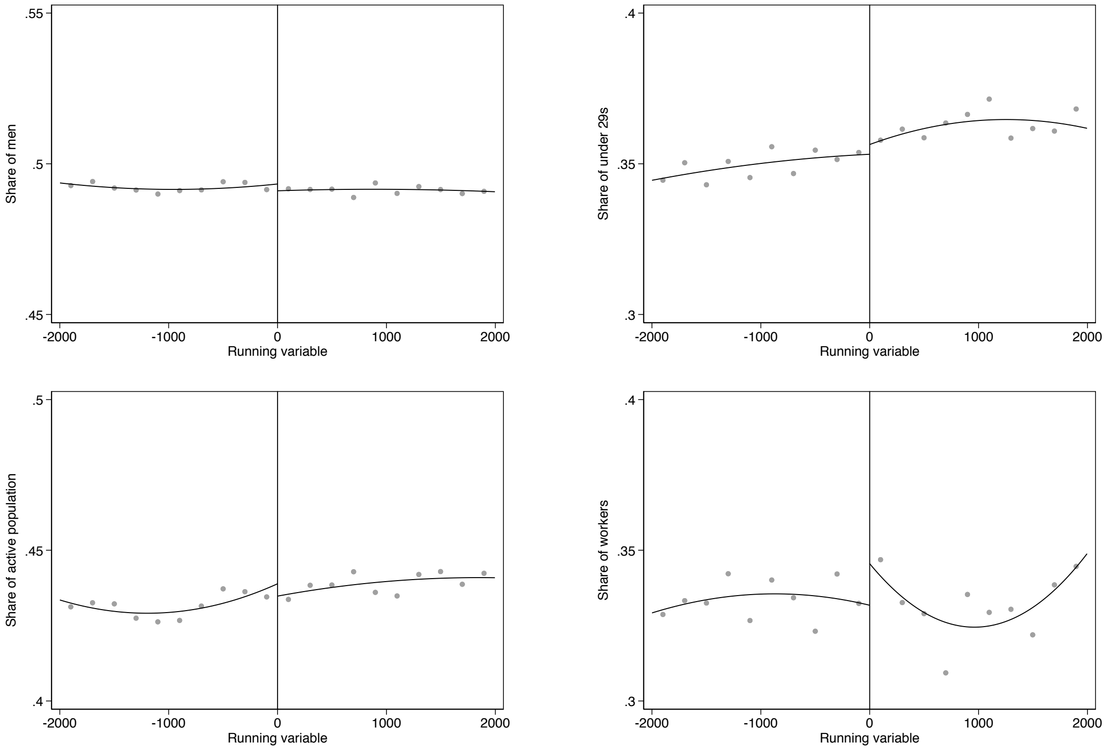
Notes: We test for a jump at the threshold in the density of the running variable (the district population centered around 9,000 inhabitants), using McCrary (2008)'s method in the top panel. The solid line represents the density of the running variable, while the thin lines represent the confidence intervals. The bottom two figures similarly test for a jump at the threshold in the density of the running variable using the method developed by Cattaneo et al. (2018). The solid line represents the density of the running variable, while the shaded bands represent the 95 percent confidence intervals. The graphs also report the *p*-value of the bias-corrected density test. To facilitate visualization, the graph is truncated at 5,000 inhabitants around the cutoff. We exclude the 2008 elections since in most districts, the running variable is the same as in 2001 (the same major census was in place for both elections).

Figure B2: Placebo tests, main outcomes defined in *t-1* - Main sample of departmental elections



Notes: Each dot is the average of the outcome variable within a given bin of the running variable. The running variable (the district population centered around 9,000 inhabitants) is split into quantile-spaced bins. The continuous lines represent a quadratic fit. To facilitate visualization, the graph is truncated at 2,000 inhabitants around the cutoff. We exclude the 1998 (resp. 2008) elections since in most districts, the running variable is the same as in 1992 (resp. 2001).

Figure B3: Balance tests, sociodemographic characteristics - Main sample of departmental elections



Notes: Each dot is the average of the outcome variable within a given bin of the running variable. The running variable (the district population centered around 9,000 inhabitants) is split into evenly-spaced bins. The continuous lines represent a quadratic fit. To facilitate visualization, the graph is truncated at 2,000 inhabitants around the cutoff. We exclude the 2008 elections since in most districts, the running variable is the same as in 2001 (the same major census was in place for both elections).

C. Robustness

Table C1: Impact on competition - Main sample of departmental elections excluding 2008

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Number of			Turnout	ENC	Victory
	Candidates	Outsiders	Insiders	r1	r1	in first round
Treatment	0.103	0.025	0.012	0.014	0.136	-0.103**
	(0.155)	(0.156)	(0.079)	(0.010)	(0.118)	(0.049)
Robust *p*-value	0.356	0.747	0.666	0.117	0.177	0.033
Observations	1,345	1,637	2,099	1,802	1,397	1,737
Polyn. order	1	1	1	1	1	1
Bandwidth	1,910	2,314	2,973	2,545	1,987	2,457
Mean, left of threshold	5.278	3.787	1.499	0.639	3.351	0.312

Notes: Standard errors are in parentheses. Robust *p*-values are used to compute statistical significance. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. Each column reports the results from a separate local polynomial regression. The independent variable is a dummy equal to one if the district has a population above 9,000 inhabitants in election t. Separate polynomials are fitted on each side of the threshold. The polynomial order is one in all columns and the bandwidths are derived under the MSERD procedure. The mean indicates the mean value of the outcome of interest at the cutoff below the discontinuity.

Table C2: Impact on competition - All departmental elections, including non-linkable districts

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Number of			Turnout	ENC	Victory
	Candidates	Outsiders	Insiders	r1	r1	in first round
Treatment	0.044	0.010	0.028	0.009	0.073	-0.111***
	(0.118)	(0.119)	(0.065)	(0.009)	(0.085)	(0.044)
Robust *p*-value	0.524	0.855	0.471	0.263	0.279	0.010
Observations	2,460	2,663	2,407	2,336	2,768	2,222
Polyn. order	1	1	1	1	1	1
Bandwidth	2,736	2,993	2,702	2,604	3,093	2,473
Mean, left of threshold	5.055	3.597	1.461	0.656	3.251	0.355

Notes as in Table C1.

Table C3: Impact on winner identity - Sample of departmental elections excluding 2008

	(1)	(2)	(3)	(4)
Outcomes	Outsider win	Insider win	Incumbent win	Challenger win
Treatment	0.022	-0.022	-0.100*	0.078**
	(0.048)	(0.048)	(0.055)	(0.025)
Robust *p*-value	0.600	0.600	0.065	0.002
Observations	1,769	1,769	1,331	1,299
Polyn. order	1	1	1	1
Bandwidth	2,504	2,504	1,886	1,838
Mean, left of threshold	0.337	0.663	0.635	0.007

Notes as in Table C1.

Table C4: Impact on running, winning, and vote shares - Sample of departmental elections excluding 2008

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Incumbent			Challenger		
	run	win	vote share, R1	run	win	vote share, R1
Panel A. Unconditional effects						
Treatment	-0.073	-0.100*	-0.055**	0.112**	0.078***	0.044***
	(0.043)	(0.055)	(0.025)	(0.050)	(0.025)	(0.015)
Robust *p*-value	0.102	0.065	0.024	0.016	0.002	0.003
Observations	1,799	1,331	1,381	1,322	1,299	1,415
Polyn. order	1	1	1	1	1	1
Bandwidth	2,542	1,886	1,969	1,877	1,838	2,012
Mean	0.745	0.635	0.346	0.180	0.007	0.043
Panel B. Conditional effects						
Upper bound		-0.134	-0.074*		0.265***	0.151***
Boot. std error		(0.102)	(0.041)		(0.084)	(0.043)
Lower bound		-0.053	-0.029		0.152**	0.036*
Boot. std error		(0.077)	(0.024)		(0.074)	(0.021)
Mean		0.835	0.459		0.105	0.253

Notes: Panel A and Panel B show effects on unconditional outcomes and bounds of effects conditional on running, respectively. The notes for Panel A are as in Table C1. In Panel B, the mean, left of the threshold, indicates the value of the outcome for the candidates on the left of the threshold, conditional on running. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively, of the bootstrapped standard errors.

Table C5: Impact on winning orientation, polarization, and winner's representativeness - Sample of departmental elections excluding 2008

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Outcome	Incumbent or. win	Far-left win	Left win	Center win	Right win	Far-right win	Non-classified win	Polarization	Vote share . winner or.	Top or. win
Treatment	-0.039	-0.004	0.081	-0.025	-0.056	-0.000	0.010	0.025	-0.016	-0.039
	(0.042)	(0.004)	(0.054)	(0.018)	(0.049)	(0.000)	(0.010)	(0.087)	(0.016)	(0.034)
Robust p-value	0.308	0.159	0.110	0.157	0.241	0.294	0.322	0.778	0.312	0.246
Observations	1,449	1,574	1,933	2,056	2,341	1,272	1,734	1,800	1,624	1,556
Polyn. order	1	1	1	1	1	1	1	1	1	1
Bandwidth	2,061	2,237	2,732	2,898	3,333	1,804	2,454	2,943	2,298	2,214
Mean, left of threshold	0.840	0.004	0.480	0.048	0.467	0,000	0.004	4.906	0.581	0.918

Notes as in Table C1. "Or." stands for "orientation."

Table C6: Impact on winning orientation, polarization, and winner's representativeness - All departmental elections, including non-linkable districts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Outcome	Incumbent or. win	Far-left win	Left win	Center win	Right win	Far-right win	Non-classified win	Polarization	Vote share . winner or.	Top or. win
Treatment	-0.082**	-0.003	0.084*	-0.021	-0.055	-0.001	0.014	-0.076	-0.003	-0.044
	(0.037)	(0.003)	(0.047)	(0.014)	(0.041)	(0.001)	(0.009)	(0.083)	(0.014)	(0.029)
Robust p-value	0.024	0.266	0.063	0.144	0.189	0.360	0.152	0.368	0.819	0.118
Observations	1,534	2,236	2,559	2,600	3,373	1,730	2,116	2,182	2,320	1,835
Polyn. order	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,709	2,488	2,822	2,881	3,768	1,939	2,350	2,778	2,578	2,046
Mean, left of threshold	0.862	0.003	0.475	0.043	0.477	0,000	0.002	4.868	0.583	0.924

Notes as in Table C1. "Or." stands for "orientation."

Table C7: Placebo discontinuities - Incumbent candidates - Main sample of departmental elections

Panel A. Run

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Discontinuity	5,500	6,000	6,500	7,000	7,500	10,500	11,000	11,500	12,000	12,500
Treatment	-0.022	0.033	0.026	-0.003	0.005	0.052	0.057*	0.009	-0.061	-0.074**
	(0.043)	(0.039)	(0.037)	(0.037)	(0.032)	(0.032)	(0.034)	(0.034)	(0.034)	(0.030)
Robust p-value	0.619	0.496	0.683	0.740	0.847	0.109	0.092	0.717	0.106	0.024
Observations	1,957	2,176	2,274	1,850	2,819	2,851	2,530	2,457	2,433	3,202
Polyn. order	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,783	2,071	2,216	1,879	2,909	3,549	3,262	3,227	3,343	4,496
Mean, left of threshold	0.714	0.705	0.725	0.760	0.754	0.713	0.726	0.755	0.780	0.756

Panel B. Win

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Discontinuity	5,500	6,000	6,500	7,000	7,500	10,500	11,000	11,500	12,000	12,500
Treatment	-0.032	0.015	-0.011	-0.008	0.005	0.049	0.013	0.014	-0.043	-0.045
	(0.042)	(0.043)	(0.039)	(0.045)	(0.044)	(0.036)	(0.032)	(0.038)	(0.034)	(0.038)
Robust p-value	0.601	0.540	0.792	0.744	0.919	0.193	0.680	0.619	0.279	0.321
Observations	2,202	2,004	2,286	1,662	1,907	2,635	3,415	2,679	3,484	2,769
Polyn. order	1	1	1	1	1	1	1	1	1	1
Bandwidth	2,015	1,911	2,232	1,674	1,952	3,298	4,380	3,513	4,767	3,922
Mean, left of threshold	0.629	0.590	0.591	0.599	0.593	0.578	0.596	0.591	0.611	0.598

Notes as in Table C1.

Table C8: Placebo discontinuities - Challenger candidates - Main sample of departmental elections

Panel A. Run

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Discontinuity	5,500	6,000	6,500	7,000	7,500	10,500	11,000	11,500	12,000	12,500
Treatment	0.054	-0.029	-0.024	0.030	-0.049	-0.050	0.073*	0.079*	-0.064	-0.040
	(0.045)	(0.041)	(0.039)	(0.038)	(0.036)	(0.040)	(0.033)	(0.037)	(0.045)	(0.039)
Robust *p*-value	0.211	0.382	0.499	0.380	0.281	0.160	0.050	0.057	0.119	0.250
Observations	1,373	1,990	1,995	1,901	2,630	1,735	2,760	2,194	1,562	1,911
Polyn. order	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,273	1,896	1,979	1,938	2,743	2,106	3,559	2,916	2,180	2,745
Mean, left of threshold	0.194	0.275	0.274	0.255	0.283	0.219	0.180	0.202	0.291	0.258

Panel B. Win

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Discontinuity	5,500	6,000	6,500	7,000	7,500	10,500	11,000	11,500	12,000	12,500
Treatment	0.034	-0.024	-0.003	0.016	-0.015	-0.035*	0.006	0.012	0.014	-0.015
	(0.022)	(0.026)	(0.021)	(0.022)	(0.021)	(0.022)	(0.016)	(0.018)	(0.020)	(0.021)
Robust *p*-value	0.141	0.293	0.921	0.359	0.637	0.094	0.754	0.495	0.461	0.483
Observations	1,598	1,576	2,234	1,565	2,229	1,746	2,963	2,765	2,804	2,568
Polyn. order	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,479	1,503	2,176	1,592	2,301	2,122	3,801	3,673	3,819	3,643
Mean, left of threshold	0.028	0.083	0.070	0.051	0.067	0.076	0.054	0.055	0.056	0.068

Notes as in Table C1.

Table C9: Placebo discontinuities - Outsider candidates - Main sample of departmental elections

Panel A. Run

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Discontinuity	5,500	6,000	6,500	7,000	7,500	10,500	11,000	11,500	12,000	12,500
Treatment	0.006	-0.005	-0.003	-0.009	-0.012	0.002	-0.011	0.007	-0.007	-0.003
	(0.020)	(0.015)	(0.014)	(0.013)	(0.013)	(0.006)	(0.007)	(0.008)	(0.006)	(0.006)
Robust p-value	0.682	0.815	0.845	0.379	0.307	0.476	0.190	0.264	0.382	0.811
Observations	1,432	1,666	1,592	1,709	2,130	2,335	3,244	2,012	3,377	3,330
Polyn. order	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,327	1,593	1,588	1,744	2,194	2,882	4,162	2,691	4,621	4,718
Mean, left of threshold	0.964	0.977	0.978	0.984	0.990	0.995	0.999	0.991	0.996	0.993

Panel B. Win

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Discontinuity	5,500	6,000	6,500	7,000	7,500	10,500	11,000	11,500	12,000	12,500
Treatment	0.010	0.005	0.007	0.007	0.010	-0.029	-0.027	-0.032	0.030	0.073
	(0.041)	(0.043)	(0.040)	(0.042)	(0.040)	(0.030)	(0.032)	(0.036)	(0.031)	(0.033)
Robust p-value	0.887	0.851	0.823	0.787	0.865	0.368	0.378	0.308	0.448	0.046
Observations	2,073	1,903	2,270	1,766	1,995	3,447	3,020	2,585	3,795	3,491
Polyn. order	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,893	1,811	2,208	1,788	2,051	4,251	3,861	3,397	5,184	4,937
Mean, left of threshold	0.323	0.324	0.331	0.321	0.321	0.339	0.335	0.339	0.317	0.310

Notes as in Table C1.

Table C10: Placebo discontinuities - Victory in the first round - Main sample of departmental elections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Discontinuity	5,500	6,000	6,500	7,000	7,500	10,500	11,000	11,500	12,000	12,500
Treatment	-0.094*	-0.074	-0.001	0.020	0.044	-0.016	-0.017	-0.021	0.005	-0.051
	(0.054)	(0.052)	(0.051)	(0.047)	(0.051)	(0.052)	(0.048)	(0.044)	(0.037)	(0.039)
Robust p-value	0.084	0.211	0.924	0.776	0.330	0.556	0.565	0.549	0.865	0.225
Observations	1,834	1,994	1,943	2,170	1,964	1,664	1,894	2,212	3,170	2,951
Polyn. order	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,670	1,897	1,917	2,172	2,009	2,011	2,400	2,941	4,333	4,154
Mean, left of threshold	0.566	0.507	0.431	0.397	0.404	0.358	0.348	0.334	0.294	0.305

Notes as in Table C1.

Table C11: Impact on the main outcomes - Quadratic fit - Main sample of departmental elections

Outcome	(1) Incumbent run	(2) Incumbent win	(3) Challenger run	(4) Challenger win	(5) Outsider run	(6) Outsider win	(7) Victory 1st round
Treatment	-0.088*	-0.139***	0.106**	0.057***	0.000	0.102**	-0.125**
	(0.044)	(0.048)	(0.045)	(0.021)	(0.009)	(0.047)	(0.051)
Robust *p*-value	0.079	0.003	0.021	0.008	0.835	0.027	0.015
Observations	2,848	2,789	2,808	3,483	2,576	2,860	3,375
Polyn. order	2	2	2	2	2	2	2
Bandwidth	3,203	3,146	3,164	3,947	2,871	3,231	3,807
Mean, left of threshold	0.785	0.674	0.169	0.016	0.993	0.281	0.353

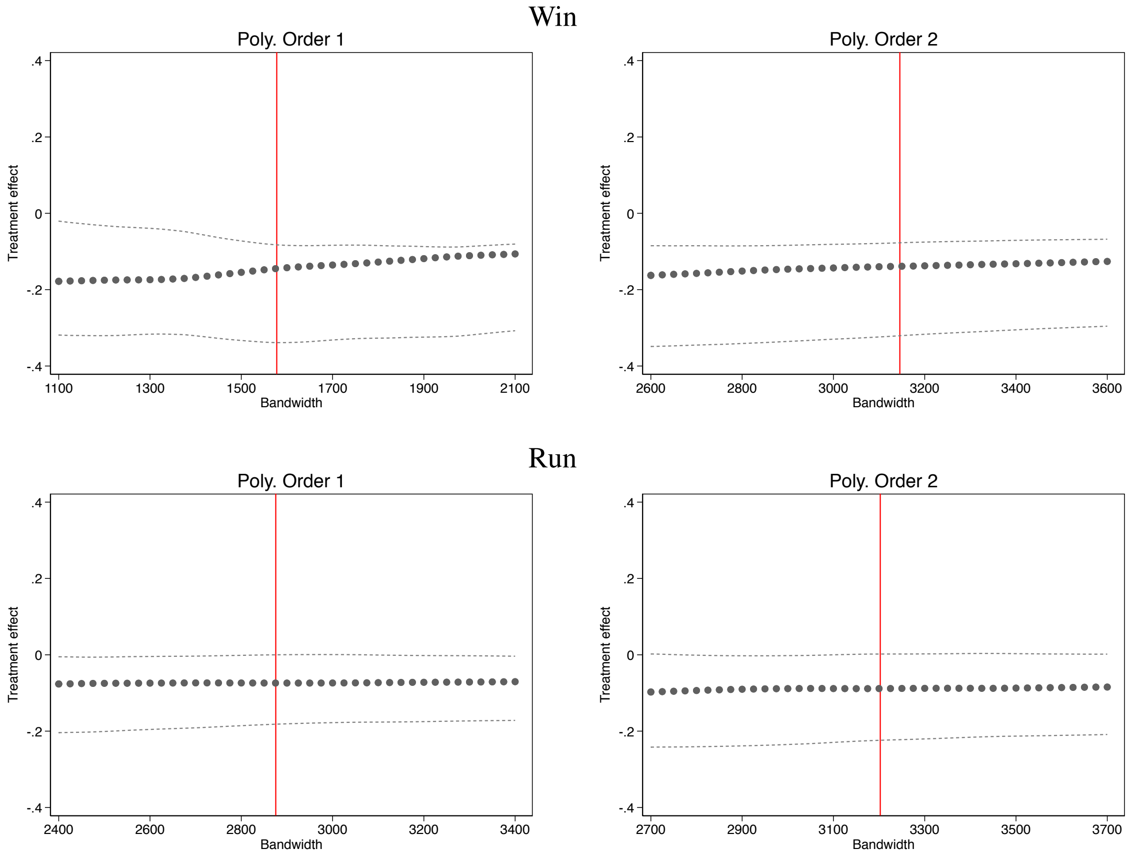
Notes as in Table C1, except for the fact that the polynomial order is two in all columns.

Table C12: Impact on the main outcomes - Including controls - Main sample of departmental elections

Outcome	(1) Incumbent run	(2) Incumbent win	(3) Challenger run	(4) Challenger win	(5) Outsider run	(6) Outsider win	(7) Victory in first round
Treatment	-0.069**	-0.112***	0.078**	0.048**	-0.005	0.067*	-0.113***
	(0.031)	(0.044)	(0.037)	(0.020)	(0.007)	(0.040)	(0.042)
Robust *p*-value	0.023	0.010	0.026	0.019	0.573	0.079	0.005
Observations	2,809	1,564	1,942	1,856	2,121	1,871	2,207
Polyn. order	1	1	1	1	1	1	1
Bandwidth	3,163	1,756	2,192	2,076	2,375	2,100	2,471
Mean, left of threshold	0.937	-0.076	-0.115	-0.051	1.046	0.907	0.231

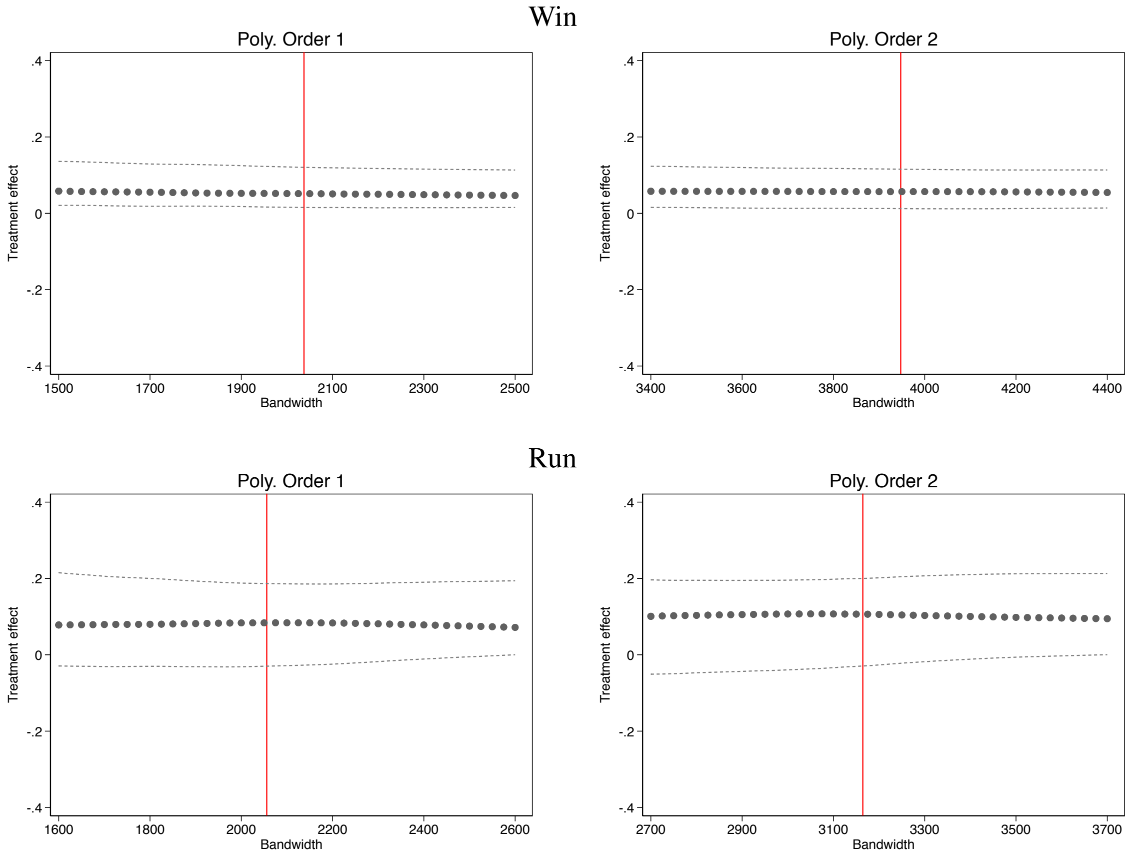
Notes: We add in the regressions the baseline sociodemographic variables shown in Appendix Table B2: the share of men in the population; the share of under 29 year olds; the share of the population between 30 and 44 years old; the share between 45 and 59 years old; the share above 60 years old; the share of the economically active within the population; the share of unemployed; the share of skilled jobs; the share of workers; the share of employee professions; the share of intermediary professions; the share of artisans; the share of actives working in agriculture. To avoid dropping observations, for each variable, we include a dummy equal to one when the variable is missing and replace by 0s. Other notes as in Table C1.

Figure C1: Sensitivity to bandwidth - Incumbent candidate - Main sample of departmental elections



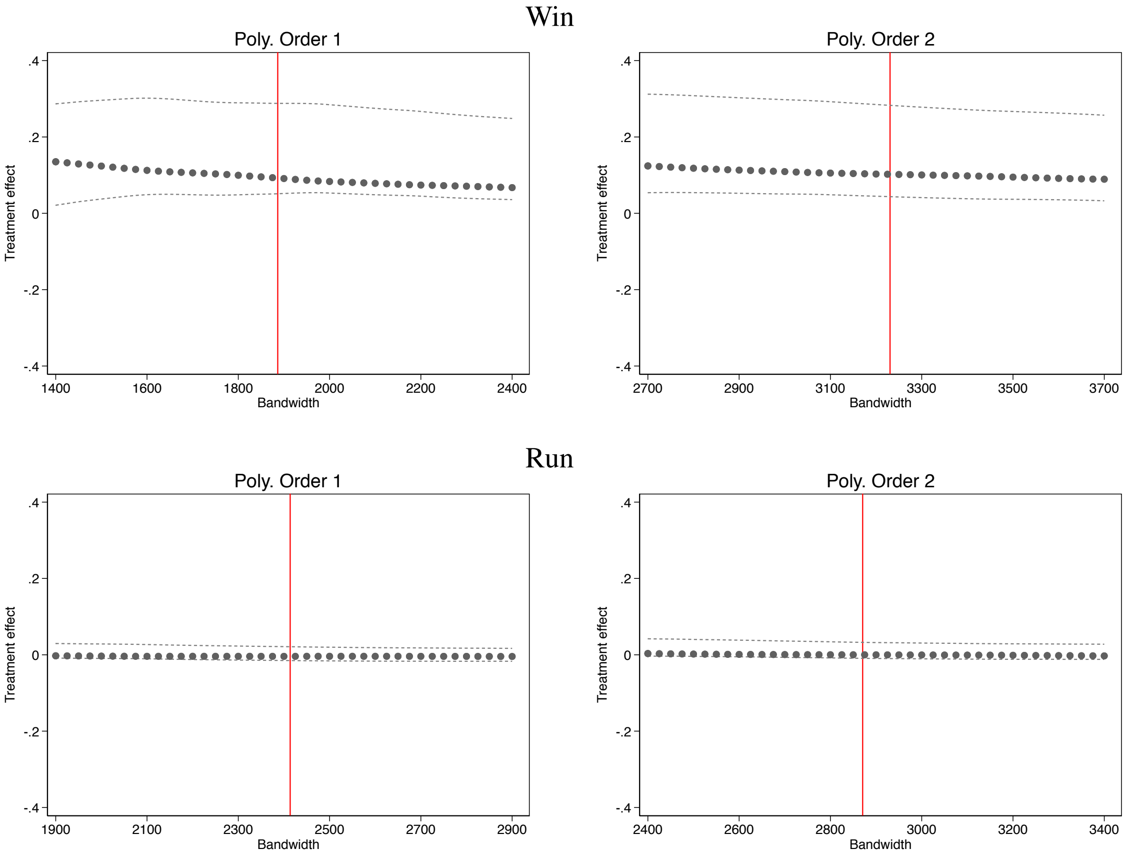
Notes: We show the sensitivity of the effect on the incumbent candidate to bandwidth choice, either using a linear (left-hand side) or quadratic specification (right-hand side). The vertical red line represents the value of the MSERD optimal bandwidth. The dots represent the estimated treatment effect using different bandwidths, while the dotted lines represent the 95 percent confidence intervals. We report all estimates for values of the bandwidth from -500 to +500 inhabitants, in steps of 25 inhabitants.

Figure C2: Sensitivity to bandwidth - Challenger candidate - Main sample of departmental elections



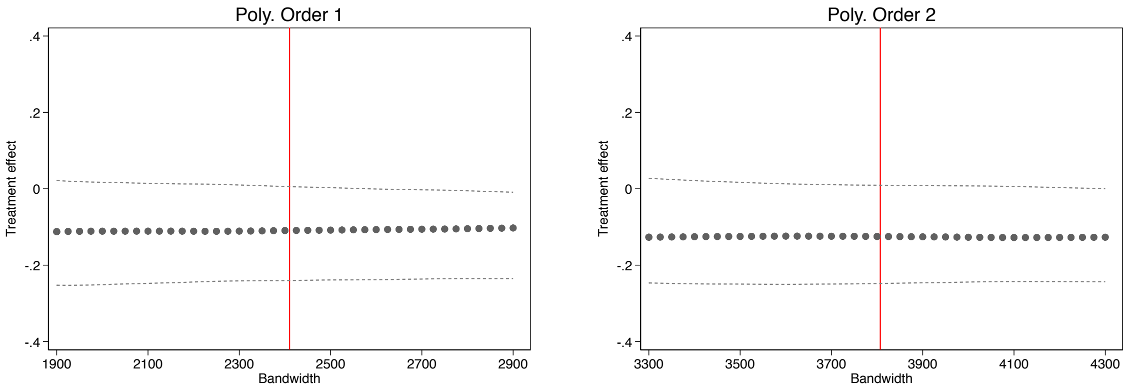
Notes: We show the sensitivity of the effect on the challenger candidate to bandwidth choice, either using a linear (left-hand side) or quadratic specification (right-hand side). The vertical red line represents the value of the MSERD optimal bandwidth. The dots represent the estimated treatment effect using different bandwidths, while the dotted lines represent the 95 percent confidence intervals. We report all estimates for values of the bandwidth from -500 to +500 inhabitants, in steps of 25 inhabitants.

Figure C3: Sensitivity to bandwidth - Outsider candidate - Main sample of departmental elections



Notes: We show the sensitivity of the effect on outsider candidates to bandwidth choice, either using a linear (left-hand side) or quadratic specification (right-hand side). The vertical red line represents the value of the MSERD optimal bandwidth. The dots represent the estimated treatment effect using different bandwidths, while the dotted lines represent the 95 percent confidence intervals. We report all estimates for values of the bandwidth from -500 to +500 inhabitants, in steps of 25 inhabitants.

Figure C4: Sensitivity to bandwidth - Victory in the first round - Main sample of departmental elections



Notes: We show the sensitivity of the effect on the probability of a victory in the first round to bandwidth choice, either using a linear (left-hand side) or quadratic specification (right-hand side). The vertical red line represents the value of the MSERD optimal bandwidth. The dots represent the estimated treatment effect using different bandwidths while the dotted lines represent the 95 percent confidence intervals. We report all estimates for values of the bandwidth from -500 to +500 inhabitants, in steps of 25 inhabitants.

Appendix II: Municipal elections

D. Validity

Table D1: General balance test - Main sample of municipal elections

	(1)
Outcome	Predicted treatment
Treatment	-0.016
	(0.039)
Robust p-value	0.758
Observations	788
Polyn. order	1
Bandwidth	1,640
Mean, left of threshold	0.407

Notes: Standard errors are in parentheses. Robust *p*-values are used to compute statistical significance. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. Each column reports the results from a separate local polynomial regression. The independent variable is a dummy equal to one if the district has a population above 9,000 inhabitants in election t. The outcome is the value of the treatment predicted by the sociodemographic variables: the share of men in the population; the share of under 29 year olds, the share of the population between 30 and 44 years old; the share between 45 and 59 years old; the share above 60 years old; the share of the economically active within the population; the share of unemployed; the share of skilled jobs; the share of workers; the share of employee professions; the share of intermediary professions; the share of artisans; the share of actives working in agriculture. To avoid dropping observations, for each sociodemographic variable, we include a dummy equal to one when the variable is missing and replace by 0s. The independent variable is a dummy equal to one if the district has a population greater or equal to 9,000 in year t. Separate polynomials are fitted on each side of the threshold. The polynomial order is one in all columns and the bandwidths are derived under the MSERD procedure. The mean indicates the mean value of the outcome of interest at the cutoff below the discontinuity. We exclude the 2008 elections since in most districts, the population and, therefore, the running and assignment variables, were the same as in the 2001 elections.

Table D2: General balance test - All municipal elections, including non-linkable districts

	(1)
Outcome	Predicted treatment
Treatment	-0.004
	(0.036)
Robust p-value	0.988
Observations	855
Polyn. order	1
Bandwidth	1,642
Mean, left of threshold	0.386

Notes as in Table D1.

Table D3: Balance tests, sociodemographic characteristics - Main sample of municipal elections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Outcomes	Men	Under 29	30-44	45-59	Over 60	Active population	Unemployed	Skilled jobs	Workers	Employees	Intermediary workers	Artisans	Agriculture
Treatment	0.004	0.000	-0.001	0.002	-0.003	-0.004	0.004	0.001	-0.030	0.013*	0.001	0.000	0.002
	(0.003)	(0.010)	(0.004)	(0.004)	(0.010)	(0.007)	(0.010)	(0.012)	(0.020)	(0.008)	(0.008)	(0.004)	(0.002)
Robust p-value	0.369	0.857	0.705	0.563	0.940	0.524	0.646	0.815	0.102	0.085	0.818	0.703	0.334
Observations	879	792	972	947	1,056	1,072	970	762	540	761	806	662	1,164
Polyn. order	1	1	1	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,818	1,680	2,001	1,953	2,139	2,166	1,994	1,608	1,160	1,605	1,697	1,399	2,346
Mean, left of threshold	0.481	0.378	0.204	0.196	0.223	0.457	0.135	0.121	0.281	0.303	0.241	0.061	0.006

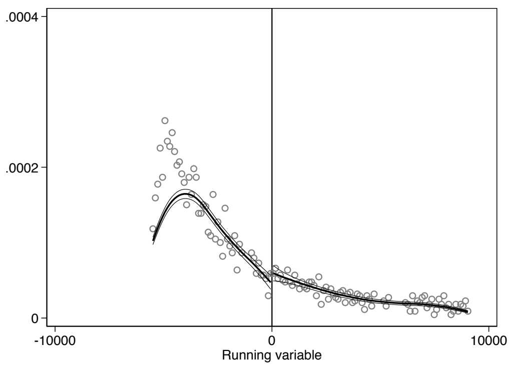
Notes: Standard errors are in parentheses. Robust p-values are used to compute statistical significance. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. Each column reports the results from a separate local polynomial regression. The independent variable is a dummy equal to one if the district has a population above 9,000 inhabitants in election t. Separate polynomials are fitted on each side of the threshold. The polynomial order is one in all columns and the bandwidths are derived under the MSERD procedure. The sample includes the 2001 and 2014 elections. All outcomes refer to shares of the whole population. The mean indicates the mean value of the outcome of interest at the cutoff below the discontinuity.

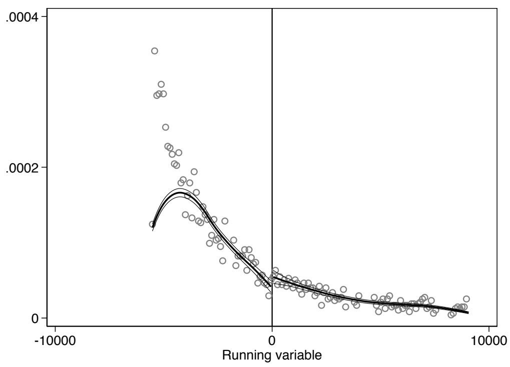
Table D4: Balance tests, sociodemographic characteristics - All municipal elections, including non-linkable districts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Outcomes	Men	Under 29	30-44	45-59	Over 60	Active population	Unemployed	Skilled jobs	Workers	Employees	Intermediary workers	Artisans	Agriculture
Treatment	0.004	0.003	-0.000	0.001	-0.006	-0.002	0.001	0.005	-0.031*	0.010	0.003	-0.001	0.002
	(0.003)	(0.009)	(0.004)	(0.004)	(0.010)	(0.006)	(0.009)	(0.012)	(0.019)	(0.007)	(0.008)	(0.004)	(0.002)
Robust p-value	0.304	0.893	0.945	0.747	0.708	0.762	0.829	0.540	0.081	0.140	0.621	0.995	0.439
Observations	942	869	1,360	959	1,048	1,492	1,131	783	584	926	887	946	1,281
Polyn. order	1	1	1	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,805	1,698	2,516	1,838	2,000	2,714	2,133	1,537	1,185	1,780	1,728	1,814	2,387
Mean, left of threshold	0.482	0.379	0.204	0.196	0.222	0.456	0.137	0.116	0.282	0.304	0.240	0.060	0.006

Notes as in Table D3.

Figure D1: McCrary (2008) density test



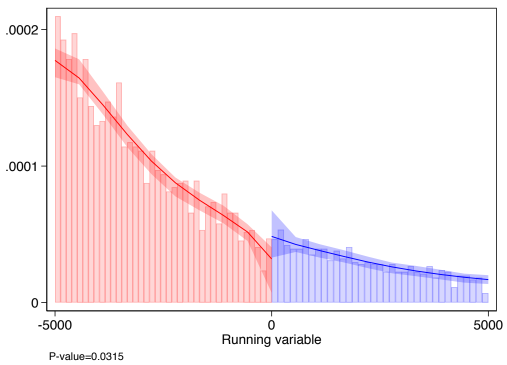


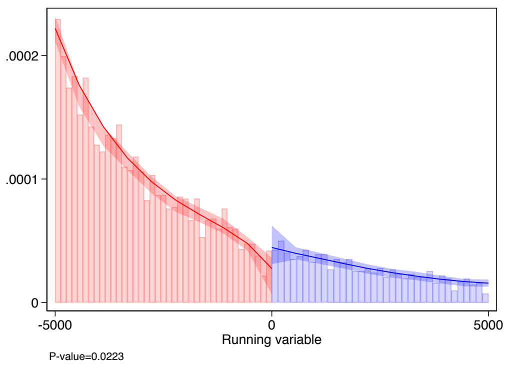
McCrary test - Main sample

McCrary test - Including non-linkable districts

Notes: We test for a jump at the threshold in the density of the running variable (the district population centered around 9,000 inhabitants), using McCrary (2008)'s method. The solid line represents the density of the running variable, while the thin lines represent the confidence intervals. We exclude the 2008 elections since in most districts, the running variable is the same as in 2001 (the same major census was in place for both elections).

Figure D2: Cattaneo et al. (2018) density tests



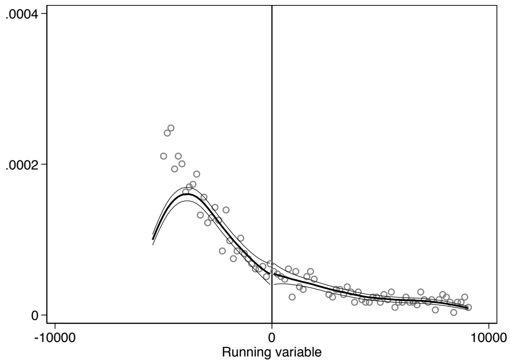


RD Density test - Main sample

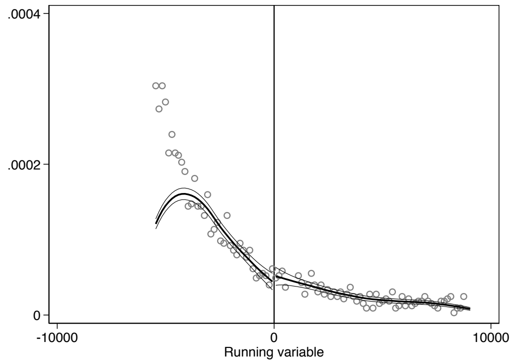
RD Density test - Including non-linkable districts

Notes: We test for a jump at the threshold in the density of the running variable (the district population centered around 9,000 inhabitants), using McCrary (2008)'s method in the top panel. The solid line represents the density of the running variable, while the thin lines represent the confidence intervals. The bottom two figures similarly test for a jump at the threshold in the density of the running variable using the method developed by Cattaneo et al. (2018). The solid line represents the density of the running variable, while the shaded bands represent the 95 percent confidence intervals. The graphs also report the *p*-value of the bias-corrected density test. To facilitate visualization, the graph is truncated at 5,000 inhabitants around the cutoff. We exclude the 2008 elections since in most districts, the running variable is the same as in 2001 (the same major census was in place for both elections).

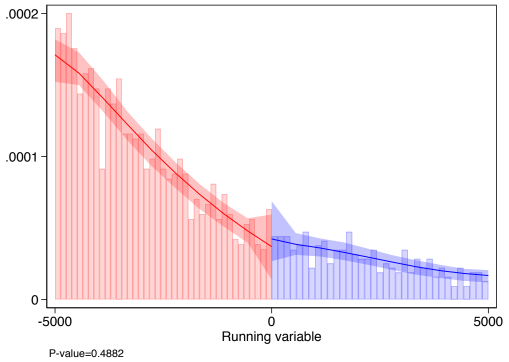
Figure D3: McCrary (2008) and Cattaneo et al. (2018) density tests - 2001 elections



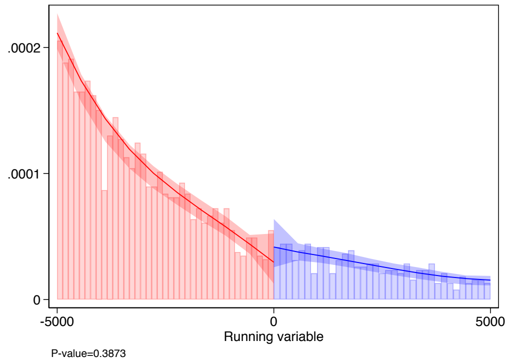
McCrary test - Main sample



McCrary test - Including non-linkable districts



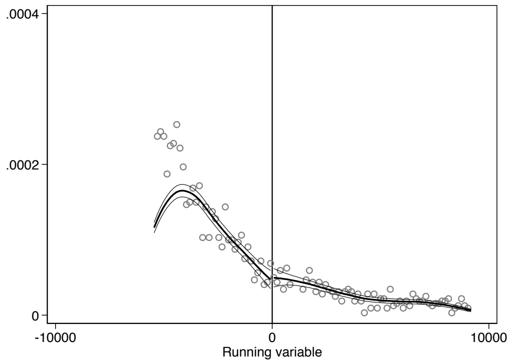
RD Density test - Main sample



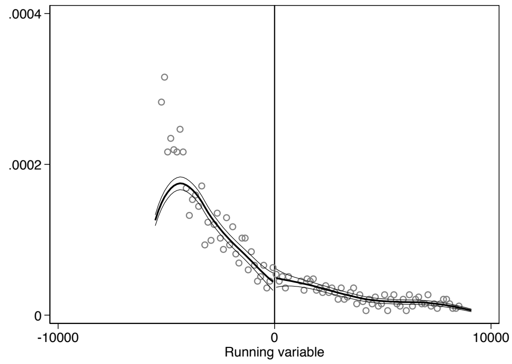
RD Density test - Including non-linkable districts

Notes as in Figure D2.

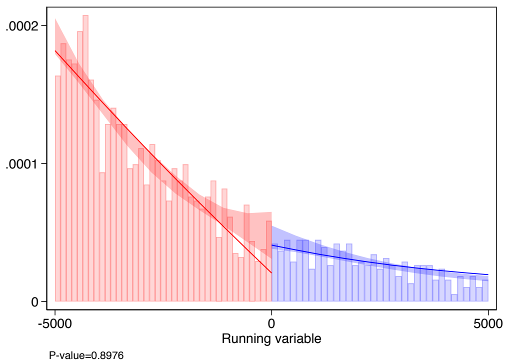
Figure D4: McCrary (2008) and Cattaneo et al. (2018) density tests - 2008 elections



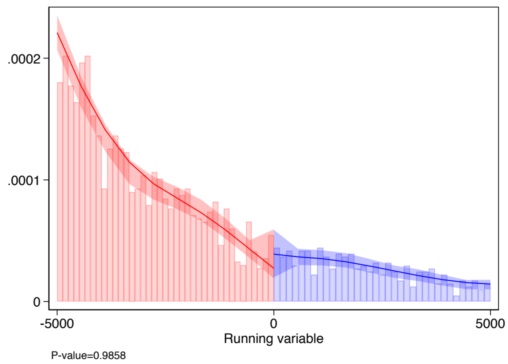
McCrary test - Main sample



McCrary test - Including non-linkable districts



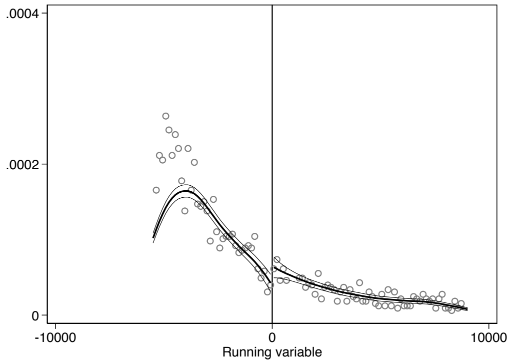
RD Density test - Main sample



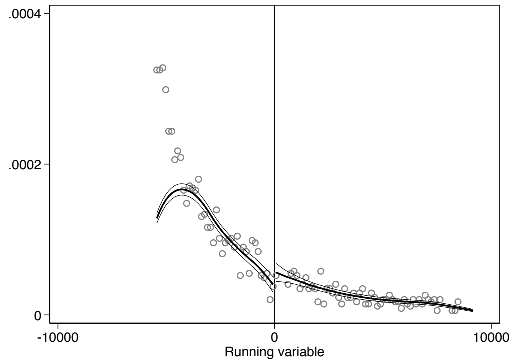
RD Density test - Including non-linkable districts

Notes as in Figure D2.

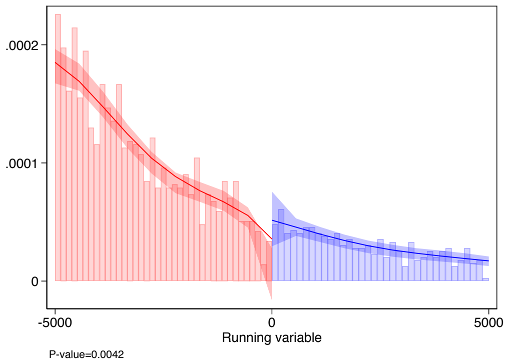
Figure D5: McCrary (2008) and Cattaneo et al. (2018) density tests - 2014 elections



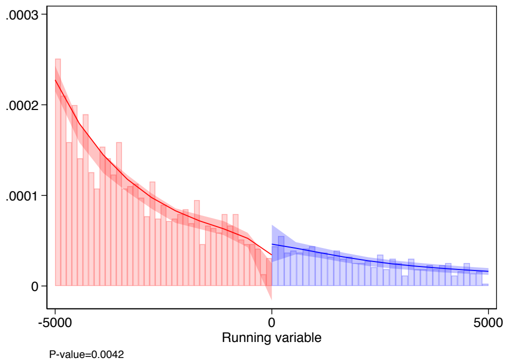
McCrary test - Main sample



McCrary test - Including non-linkable districts



RD Density test - Main sample



RD Density test - Including non-linkable districts

Notes as in Figure D2.

E. Robustness

Table E1: Impact on competition and winner identity - Main sample of municipal elections - 2001

Panel A. Competition

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Nr candidates	Nr outsiders	Nr insiders	Turnout	ENC	Victory in r1
Treatment	-0.026	0.090	-0.125	0.016	0.049	-0.078
	(0.208)	(0.217)	(0.142)	(0.014)	(0.165)	(0.113)
Robust p-value	0.901	0.559	0.346	0.266	0.759	0.469
Observations	590	673	509	342	533	362
Polyn. order	1	1	1	1	1	1
Bandwidth	2,622	2,928	2,332	1,664	2,405	1,741
Mean, left of threshold	2.772	1.631	1.138	0.641	2.355	0.716

Panel B. Winner identity

	(1)	(2)	(3)	(4)
Outcome	Outsider win	Insider win	Incumbent win	Challenger win
Treatment	0.148	-0.148	-0.158	0.025
	(0.095)	(0.095)	(0.114)	(0.048)
Robust p-value	0.206	0.206	0.277	0.586
Observations	574	574	401	379
Polyn. order	1	1	1	1
Bandwidth	2,574	2,574	1,910	1,849
Mean, left of threshold	0.290	0.710	0.697	0.020

Notes: Standard errors are in parentheses. Robust p-values are used to compute statistical significance. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. Each column reports the results from a separate local polynomial regression. The independent variable is a dummy equal to one if the district has a population above 9,000 inhabitants in election t. Separate polynomials are fitted on each side of the threshold. The polynomial order is one in all columns and the bandwidths are derived under the MSERD procedure. The mean indicates the mean value of the outcome of interest at the cutoff below the discontinuity.

Table E2: Impact on competition and winner identity - Main sample of municipal elections - 2008

Panel A. Competition

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Nr candidates	Nr outsiders	Nr insiders	Turnout	ENC	Victory in r1
Treatment	-0.093	-0.043	-0.052	0.001	-0.042	0.040
	(0.216)	(0.201)	(0.134)	(0.012)	(0.169)	(0.103)
Robust *p*-value	0.782	0.882	0.901	0.862	0.941	0.855
Observations	427	498	534	497	407	512
Polyn. order	1	1	1	1	1	1
Bandwidth	1,708	1,936	2,035	1,928	1,639	1,985
Mean, left of threshold	2.827	1.728	1.110	0.642	2.402	0.554

Panel B. Winner identity

	(1)	(2)	(3)	(4)
Outcome	Outsider win	Insider win	Incumbent win	Challenger win
Treatment	-0.129	0.129	0.005	0.099
	(0.091)	(0.091)	(0.108)	(0.062)
Robust *p*-value	0.185	0.185	0.895	0.114
Observations	482	482	411	590
Polyn. order	1	1	1	1
Bandwidth	1883	1883	1657	2304
Mean, left of threshold	0.421	0.579	0.522	0.0569

Notes as in Table E1.

Table E3: Impact on competition and winner identity - Main sample of municipal elections - 2014

Panel A. Competition

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Nr candidates	Nr outsiders	Nr insiders	Turnout	ENC	Victory in r1
Treatment	-0.066	-0.171	0.119	-0.001	0.016	0.011
	(0.212)	(0.214)	(0.159)	(0.013)	(0.174)	(0.095)
Robust *p*-value	0.727	0.319	0.458	0.979	0.949	0.769
Observations	654	712	577	509	563	683
Polyn. order	1	1	1	1	1	1
Bandwidth	2,269	2,484	2,036	1,811	1,993	2,378
Mean, left of threshold	3.173	2.112	1.062	0.626	2.534	0.557

Panel B. Winner identity

	(1)	(2)	(3)	(4)
Outcome	Outsider win	Insider win	Incumbent win	Challenger win
Treatment	-0.046	0.046	0.049	-0.027
	(0.110)	(0.110)	(0.097)	(0.063)
Robust *p*-value	0.640	0.640	0.462	0.760
Observations	485	485	592	501
Polyn. order	1	1	1	1
Bandwidth	1,757	1,757	2,092	1,869
Mean, left of threshold	0.415	0.585	0.482	0.110

Notes as in Table E1.

Table E4: Impact on competition - All municipal elections including non-linkable districts

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Nr candidates	Nr outsiders	Nr insiders	Turnout	ENC	Victory in r1
Treatment	0.030	-0.034	-0.017	-0.001	0.090	-0.038
	(0.137)	(0.131)	(0.069)	(0.008)	(0.101)	(0.056)
Robust *p*-value	0.807	0.763	0.911	0.969	0.394	0.454
Observations	1,429	1,433	2,258	1,562	1,432	1,394
Polyn. order	1	1	1	1	1	1
Bandwidth	1,801	1,913	2,803	1,956	1,807	1,767
Mean, left of threshold	2.918	1.816	1.106	0.637	2.431	0.604

Notes as in Table E1.

Table E5: Impact on competition - All municipal elections, including non-linkable districts - 2001

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Nr candidates	Nr outsiders	Nr insiders	Turnout	ENC	Victory in r1
Treatment	0.049	0.090	-0.125	0.007	0.134	-0.122
	(0.183)	(0.217)	(0.142)	(0.012)	(0.151)	(0.101)
Robust *p*-value	0.647	0.559	0.346	0.526	0.303	0.211
Observations	760	673	509	460	625	442
Polyn. order	1	1	1	1	1	1
Bandwidth	2,874	2,928	2,332	1,881	2,446	1,817
Mean, left of threshold	2.786	1.631	1.138	0.640	2.381	0.695

Notes as in Table E1.

Table E6: Impact on competition - All municipal elections, including non-linkable districts - 2008

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Nr candidates	Nr outsiders	Nr insiders	Turnout	ENC	Victory in r1
Treatment	0.046	-0.043	-0.052	-0.003	0.046	-0.007
	(0.242)	(0.201)	(0.134)	(0.012)	(0.181)	(0.106)
Robust *p*-value	0.764	0.882	0.901	0.939	0.699	0.816
Observations	425	498	534	535	411	497
Polyn. order	1	1	1	1	1	1
Bandwidth	1,663	1,936	2,035	2,002	1,601	1,894
Mean, left of threshold	2.823	1.728	1.110	0.643	2.400	0.553

Notes as in Table E1.

Table E7: Impact on competition - All municipal elections, including non-linkable districts - 2014

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Nr candidates	Nr outsiders	Nr insiders	Turnout	ENC	Victory in r1
Treatment	-0.049	-0.171	0.119	-0.003	0.041	-0.000
	(0.203)	(0.214)	(0.159)	(0.012)	(0.164)	(0.093)
Robust *p*-value	0.722	0.319	0.458	0.718	0.909	0.846
Observations	710	712	577	584	629	728
Polyn. order	1	1	1	1	1	1
Bandwidth	2,413	2,484	2,036	2,014	2,142	2,467
Mean, left of threshold	3.166	2.112	1.062	0.628	2.522	0.566

Notes as in Table E1.

Table E8: Impact on running - Main sample of municipal elections - 2001

	(1)	(2)	(3)
Outcome	Incumbent	Challenger	Outsider
Treatment	-0.105	-0.173	0.074
	(0.107)	(0.116)	(0.078)
Robust p-value	0.464	0.121	0.297
Observations	360	298	405
Polyn. order	1	1	1
Bandwidth	1,734	1,501	1,918
Mean, left of threshold	0.761	0.315	0.824

Notes as in Table E1.

Table E9: Impact on running - Main sample of municipal elections - 2008

	(1)	(2)	(3)
Outcome	Incumbent	Challenger	Outsider
Treatment	-0.050	0.001	-0.010
	(0.094)	(0.104)	(0.047)
Robust p-value	0.563	0.888	0.989
Observations	441	461	685
Polyn. order	1	1	1
Bandwidth	1,744	1,897	2,530
Mean, left of threshold	0.717	0.280	0.944

Notes as in Table E1.

Table E10: Impact on running - Main sample of municipal elections - 2014

	(1)	(2)	(3)
Outcome	Incumbent	Challenger	Outsider
Treatment	0.073	0.091	-0.080
	(0.092)	(0.090)	(0.055)
Robust p-value	0.428	0.331	0.134
Observations	570	622	527
Polyn. order	1	1	1
Bandwidth	2,023	2,293	1,876
Mean, left of threshold	0.692	0.234	0.962

Notes as in Table E1.

-

B

Appendix to Chapter 2

B.1 Main results with controls - full table

Table B.1 shows the coefficients of the AC-12 shock as well as the coefficients of all our control variables on our main outcomes of interest.

Table B.1 Main results: All controls

	(1)	(2)	(3)	(4)	(5)	(6)
	Total expenditure pc	Central government transfers pc	Locally raised budget pc	Social care pc	Education pc	Other pc
AC-12 shock	526.82	978.15**	-451.32***	-414.47**	1179.50**	-238.21
	(457.81)	(472.17)	(87.03)	(204.35)	(564.80)	(152.78)
Share EU-15	1291.99**	1482.37***	-190.38	33.77	1062.35***	195.87
	(514.40)	(441.90)	(156.07)	(230.67)	(365.07)	(231.50)
Share non-EU	247.52	406.58	-159.06**	-62.79	265.37	44.94
	(251.84)	(277.55)	(67.62)	(109.33)	(242.30)	(98.83)
Unemployment rate	51.85	75.64	-23.78	649.06***	-423.88	-173.33
	(343.96)	(355.50)	(106.79)	(187.60)	(405.00)	(145.36)
Population	-0.0020***	-0.0018***	-0.0003**	-0.0006***	-0.0008***	-0.0006***
	(0.0003)	(0.0003)	(0.0001)	(0.0002)	(0.0002)	(0.0002)
N	2028	2028	2028	2028	2028	2028
R^2	0.890	0.834	0.826	0.790	0.757	0.654
Time FE	Year	Year	Year	Year	Year	Year
Model	DiD	DiD	DiD	DiD	DiD	DiD
Sample	LFS/APS	LFS/APS	LFS/APS	LFS/APS	LFS/APS	LFS/APS

Standard errors in parentheses

* p<0.10, ** p<0.05, *** p<0.01

Notes: Outcomes expressed in per capita terms (pc). The AC-12 shock is defined as the difference in AC-12 population shares in a given local authority-year and its 2001 baseline share as defined in equation 2.1 of section 2.4.1. All regressions include local authority fixed effects. LFS/APS refers to the UK Labour Force Survey and its boost samples in the Annual Population Survey. The observation period is 2000 to 2015.

B.2 Local authority spending and funding

Table B.2 is based on Sandford (2018, p.19) and provides a comprehensive overview of how the different functions within two-tier authorities are split between the upper tier (the County) and the lower tier (the District).

Function	**Tier**
Births, deaths and marriage registration	County
Children's services	County
Concessionary travel	County
Education	County
Emergency planning	County
Highways, street lightning and traffic managament	County
Libraries	County
Mineral and waste planning	County
Passenger transport (buses) and transport planning	County
Public health	County
Social care services	County
Trading standards	County
Waste disposal	County
Building regulations	District
Burials and cremations	District
Coastal protection	District
Community safety	District
Council tax and business rates	District
Elections and electoral registration	District
Environmental health	District
Housing	District
Licensing	District
Markets and fairs	District
Public conveniences	District
Sports centres, parks, playing fields	District
Arts and recreation	Country and District
Economic development	Country and District
Museums and galleries	Country and District
Parking and galleries	Country and District
Parking County	Country and District
Planning	Country and District
Tourism	Country and District

Table B.2 Overview of local authority services in England by government tier

Service	Expenditure items included	Means testing (as of 2018)
Adult social care	Physical support Sensory support Memory and cognition support Learning disability support Mental health support Social support (substance abusers and asylum seekers) Assistive equipment Social care activities Early interventions counselling	Needs test: Carried out by local council Asset test: Lower asset threshold GBP 14,250; Upper asset threshold GBP 23,250 Full coverage below lower threshold Shared at the discretion of local authorities in between. Income test: Entitled to GBP 24.90 per week of personal expense allowance fo care home residents; Minimum income guarantee of beween GBP 91.90 and GBP 144.30 per week for other settings
Child social care	Children centres Children looked after Family support services Other children and family support Youth justice Safeguarding children's safety Services for young people	Needs test: Carried out by local council wihtin 45 days after referral

Table B.3 Overview of social care services in England

Table B.3 provides an overview of social care services in English local authorities.[1]

[1]Note that the thresholds for means testing changed over our observation period; however the general setting remains the same

B.3 Spatial distribution of other migrant groups

Figure B.1 shows the change in the shares within the population of non-EU migrants and EU-15 migrants between 2004, the year of enlargement, and 2015.

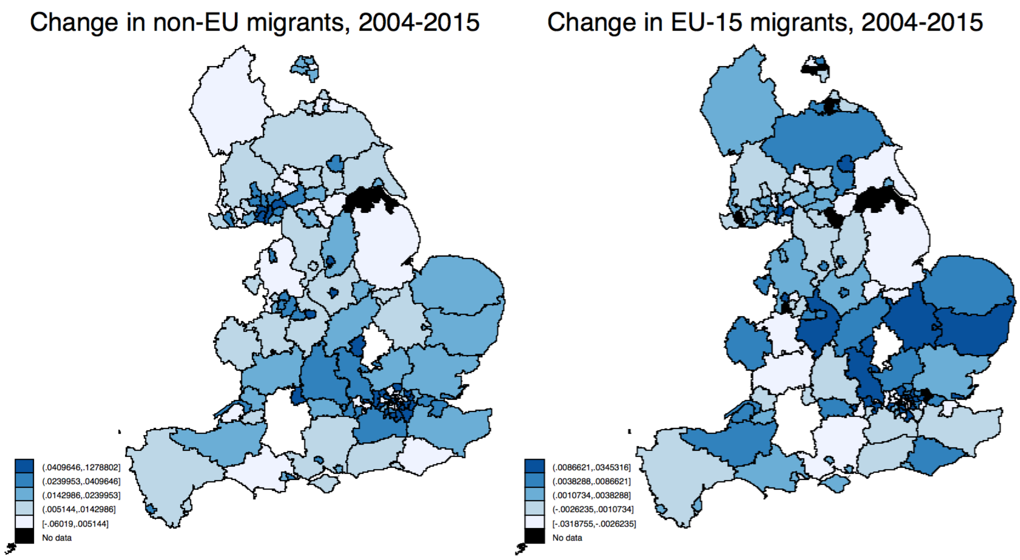


Figure B.1 Changes in non-EU and EU-15 migrants by English local authority

B.4 2001 Census variables for matching

Table B.4 shows the full list of 2001 local authority characteristics drawn from the 2001 UK Census. The variables highlighted in grey were selected as best predictors for AC-12 inflows between 2004 and 2015.

Table B.4 Summary statistics matching variables

Variable	Mean	Std. Dev.	N
Tax base	110.802	94.351	143
Council tax per capita required	304.85	446.339	143
Central government transfer per capita	839.677	1221.178	143
Share spent on social care	0.235	0.048	143
Share spent on education	0.525	0.066	143
Share spent on other items	0.24	0.068	143
Unemployment	0.059	0.025	143
Population	327418.182	257845.906	143
Household share - not deprived	0.413	0.061	143
Household share - deprived in one dimension	0.329	0.019	143
Household share - deprived in two dimensions	0.196	0.036	143
Household share - deprived in three dimensions	0.056	0.019	143
Household share - deprived in all dimensions	0.006	0.003	143
Household share - owns house	0.686	0.123	143
Household share - socially rented	0.196	0.093	143
Household share - socially rented from Council	0.137	0.08	143
Household share - privately rented	0.101	0.053	143
Share houses unoccupied in local authority	0.039	0.025	143
Share houses unoccupied - secondary houses	0.007	0.023	143
Share houses unoccupied - vacant	0.032	0.012	143
Share population in rural area	17.644	24.566	142
Share EU-15 born	0.015	0.015	143
Share AC-12 born	0.006	0.008	143
Share non-EU born	0.079	0.083	143
Share industry: Agriculture	0.011	0.012	143
Share industry: Fishing	0.001	0.001	143
Share industry: Mining	0.002	0.002	143
Share industry: Manufacturing	0.144	0.055	143
Share industry: Electricity	0.007	0.004	143
Share industry: Construction	0.065	0.016	143
Share industry: Whole Sale	0.167	0.025	143
Share industry: Hotels	0.05	0.024	143
Share industry: Transport	0.073	0.019	143
Share industry: Financial	0.051	0.029	143
Share industry: Real Estate	0.137	0.056	143
Share industry: Public sector	0.055	0.016	143
Share industry: Education	0.076	0.012	143
Share industry: Health	0.107	0.018	143
Share industry: Domestic work	0.001	0.001	143
Share aged 64+	0.072	0.016	143

B.5 UKIP results

Table B.5 shows the impact of our AC-12 shock measure on the electoral results of UKIP in local elections that took place between 2000 and 2015.

Table B.5 Effect of AC-12 migration on UKIP vote shares

	(1) Vote share UKIP	(2) Vote share UKIP
AC-12 shock	0.78	0.37*
	(0.23)	(0.22)
N	878	774
R^2	0.715	0.752
Time FE	Year	Year
Model	DiD	DiD
Full set of controls	No	Yes
Sample	LFS/APS	LFS/APS

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.01

Notes: Notes: Outcomes expressed in percentage points. The AC-12 shock is defined as the difference in AC-12 population shares in a given local authority-year and its 2001 baseline share as defined in equation 2.1 of section 2.4.1. All regressions include local authority fixed effects. The full set of controls refers to the share of EU-15 migrants, the share of non-EU migrants, the local authority unemployment rate and the total local population. LFS/APS refers to the UK Labour Force Survey and its boost samples in the Annual Population Survey. The observation period is 2000 to 2015.

C

Appendix to Chapter 3

A. Additional tables

Table A1: List of Commissioners for Agriculture

Name	Nation	ParlGov [0-10] LR score	from	to
Poul Dalsager	Denmark	3.801	January 20, 1981	January 6, 1985
Frans Andriessen	Netherlands	5.938	January 7, 1985	January 5, 1989
Ray McSharry	Ireland	6.071	January 6, 1989	January 5, 1993
René Steichen	Luxembourg	6.447	January 6, 1993	January 24, 1995
Franz Fischler	Austria	6.473	January 25, 1995	November 21, 2004
Sandra Kalniete	Latvia	7.303	May 1, 2004	November 21, 2004
Mariann Fischer Boel	Denmark	7.292	November 22, 2004	February 9, 2009

Sources: ParlGov and GS (2018)

Table A2: List of Commissioners for Regional Policy

Name	Nation	ParlGov [0-10] LR score	from	to
Antonio Giolitti	Italy	3.772	January 6, 1977	January 6, 1985
Grigoris Varfis	Greece	4.497	January 7, 1985	December 31, 1985
Alois Pfeiffer	Germany	3.645	January 1, 1986	August 1, 1987
Peter Schmidhuber	Germany	7.287	September 22, 1987	January 5, 1989
Bruce Millan	United Kingdom	4.356	January 6, 1989	January 24, 1995
Monika Wulf-Mathies	Germany	3.645	January 25, 1995	September 17, 1999
Michel Barnier	France	7.500	September 17, 1999	April 1, 2004
Jacques Barrot	France	7.500	April 26, 2004	November 21, 2004

Sources: ParlGov and GS (2018)

Table A3: Head of government positions by county

Country	Head of government
Austria	Chancellor
Belgium	Prime Minister
Cyprus	President
Czech Republic	Prime Minister
Denmark	Prime Minister
Estonia	Prime Minister
Finland	Prime Minister
France	Prime Minister
Germany	Chancellor
Greece	Prime Minister
Hungary	Prime Minister
Ireland	Taoiseach
Italy	President of the Council of Ministers
Latvia	Prime Minister
Lithuania	Prime Minister
Luxembourg	Prime Minister
Malta	Prime Minister
Netherlands	Prime Minister
Poland	President of the Council of Ministers
Portugal	Prime Minister
Slovakia	Prime Minister
Slovenia	Prime Minister
Spain	President of the Government
Sweden	Prime Minister
United Kingdom	Prime Minister

Table A4: Summary of variables

Agricultural fund share - AFS (%)	The agricultural fund share, refers to the agricultural funds receipts of a country (EAGGF) as a share of the overall EU agricultural budget (EAGGF). Expressed in percentages.
Regional fund share - RFS (%)	Each member state's regional and social fund (ERDF/ESF) receipts as a share of the overall annual regional funds budget. Expressed in percentages.
Fund share - FS (%)	*AFS* when studying the agricultural Commissioner and *RFS* when studying the regional Commissioner.
Placebo Fund share - PFS (%)	*RFS* when studying the agricultural Commissioner and *AFS* when studying the regional Commissioner.
ParlGov Score - [0-10]	ParlGov score of the Commissioner or the head of government in country i in year t. Weighted average of ParlGov scores if the Commissioner or head of government is appointed in year t.
ParlGov Distance - [0-10]	Absolute value of the difference of the ParlGov score of the Commissioner and the head of government in country i in year t.
Election Year	Dummy for election years (1 in years with a national election in country i, 0 otherwise).
Pre-election Year	Dummy for pre-election years (1 in the year before the national election in country i, 0 otherwise).
Employment Agriculture (ln)	Logarithm of the number of employees in the agricultural sector (in millions).
GVA Agriculture	Gross value added of the agricultural industry as a percentage of GDP.
Number of EU Members	Number of EU member states.
Unemployment Rate	Unemployment Rate (in percent).
Per Capita GDP (EU=100)	Normalized per capita gross domestic product (EU average = 100).
New Member State	Dummy for the newest member states (1 for all new members until the next enlargement, 0 otherwise).
Voting Power Council	Shapley-Shubik index of country i in the Council in year t (in %).
Domestic EU Support	The percentage of citizens who think that "EC/EU membership is a good thing". - the percentage of those who think that "EC/EU membership is a bad thing".
Commission President	Proportion of the year in which a country appointed the Commission President. A month is counted, if the respective Commissioner was in office for a majority of this month.
Commissioner	Proportion of the year in which a country appointed the Commissioner for Agriculture or Regional Policy depending on the relevant budget. A month is counted, if the respective Commissioner was in office for more than a half of this month.
Coalition	Dummy for whether country i in year t is run by a coalition government.
European Council Presidency	Binary variable that takes the value 1 if the country holds the EU Council presidency in year t.

Notes: Adapted from GS (2018). The authors draw all budgetary data from the annual reports of the European Court of Auditors; the economic variables on GDP, unemployment, and agriculture from Eurostat and world development indicators; the data on EU support from Eurobarometer; and the other non ParlGov related variables from Schneider (2013)

B. Validity

Table B1: Regression results - Sample restrictions

	(1)	(2)	(3)	(4)
	Agriculture		Regional	
Outcome	*Fund Share*		*Fund Share*	
Commissioner	0.784**	0.726**	0.033	0.297
	(0.016)	(0.030)	(0.966)	(0.631)
Observations	385	364	385	305
Country fixed-effects	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Country-budget specific linear time trend	Yes	Yes	Yes	Yes

Notes: Two-way country-year cluster robust *p*-values in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$
This Table compares the baseline results found in GS (2018) in columns 1 and 3 using the authors' main sample to their baseline results when estimated on this paper's restricted sample in columns 2 and 4. The control variables are the ones used in the baseline: a dummy for providing the EC President, a dummy for holding the EU Council Presidency, domestic EU support, a dummy for a country that joined the EU in the latest enlargement round at time *t*, per capita GDP, unemployment, gross value added in agriculture, employment in agriculture, voting power at the Council, and finally dummies for whether countries are in election and pre-election years.

Table B2: Regression results - Placebo fund shares

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome					*Placebo Fund Share*				
AbsDist		0.076	0.080		0.120	0.127		-0.081	-0.074
		(0.683)	(0.669)		(0.475)	(0.455)		(0.588)	(0.619)
Commissioner	0.708*		0.714*	0.390		0.426	0.390		0.276
	(0.061)		(0.055)	(0.377)		(0.351)	(0.377)		(0.376)
Observations	669	669	669	669	669	669	669	669	669
Country fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No	No	No	Yes	Yes
Country-budget specific lin. time trends	No	No	No	No	No	No	No	Yes	Yes

Notes: Two-way country-budget year cluster robust *p*-values in parentheses. *** p<0.01, ** p<0.05, * p<0.1 Lin. refers to linear. Placebo fund shares refers to budgets the respective Commissioners do not have discretion over, i.e. *RFS* when studying the agricultural Commissioner and *AFS* when studying the regional Commissioner. The control variables are the ones used in the baseline: a dummy for providing the EC President, a dummy for holding the EU Council Presidency, domestic EU support, a dummy for a country that joined the EU in the latest enlargement round at time *t*, per capita GDP, unemployment, gross value added in agriculture, employment in agriculture, voting power at the Council, and finally dummies for whether countries are in election and pre-election years.

Table B3: Regression results - Leads and Lags

	(1)	(2)	(3)	(4)
Outcome		*Fund Share*		
Commissioner	0.823	0.791	0.754	0.843
	(0.148)	(0.163)	(0.200)	(0.129)
AbsDist	-0.463**	-0.460**	-0.471**	-0.438***
	(0.012)	(0.011)	(0.016)	(0.007)
AbsDist (t-1)		-0.064	-0.113	
		(0.653)	(0.439)	
AbsDist (t-2)		-0.016	-0.037	
		(0.889)	(0.755)	
AbsDist (t-3)		0.125	0.080	
		(0.339)	(0.487)	
AbsDist (t-4)		-0.275*	-0.285**	
		(0.051)	(0.032)	
AbsDist (t-5)		0.086	0.029	
		(0.623)	(0.865)	
AbsDist (t+1)		-0.008		-0.031
		(0.922)		(0.654)
AbsDist (t+2)		0.018		0.043
		(0.820)		(0.616)
AbsDist (t+3)		0.253**		0.271**
		(0.048)		(0.027)
AbsDist (t+4)		-0.015		0.024
		(0.851)		(0.792)
AbsDist (t+5)		0.086		0.062
		(0.441)		(0.579)
Observations	444	444	444	444
Country budget fixed-effects	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Country-budget specific linear time trends	Yes	Yes	Yes	Yes

Notes: Two-way country-budget year cluster robust p-values in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$ The control variables are the ones used in the baseline: a dummy for providing the EC President, a dummy for holding the EU Council Presidency, domestic EU support, a dummy for a country that joined the EU in the latest enlargement round at time t, per capita GDP, unemployment, gross value added in agriculture, employment in agriculture, voting power at the Council, and finally dummies for whether countries are in election and pre-election years.

C. Robustness

Table C1: Regression results - Alternative controls and independent variable

	(1)	(2)	(3)	(4)	(5)
Outcome			*Fund Share*		
AbsDist	-0.350***	-0.305***	-0.337***	-0.353***	
	(0.001)	(0.006)	(0.005)	(0.001)	
$Diff$					-0.676**
					(0.019)
Commissioner	0.359	0.067	0.029	0.335	0.415
	(0.136)	(0.866)	(0.934)	(0.208)	(0.138)
Observations	669	669	645	669	669
Country budget fixed-effects	Yes	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes	Yes
Controls	Contemp.	Lagged	First-differenced	Contemp. and excl. agri. variables	Contemp.
Country-budget specific lin. time trend	Yes	Yes	Yes	Yes	Yes

Notes: Two-way country-budget year cluster robust *p*-values in parentheses. *** p<0.01, ** p<0.05, * p<0.1
The Diff dummy takes value one when the incumbent is left-wing and the Commissioner is right-wing or vice-versa. Left-wing is coded for ParlGov values below five and right-wing for ParlGov values above five.
The control variables are the ones used in the baseline: a dummy for providing the EC President, a dummy for holding the EU Council Presidency, domestic EU support, a dummy for a country that joined the EU in the latest enlargement round at time *t*, per capita GDP, unemployment, gross value added in agriculture, employment in agriculture, voting power at the Council, and finally dummies for whether countries are in election and pre-election years. Agricultural control variables refer to the gross value added of the agricultural industry as a percentage of GDP and the logarithm of the number of employees in the agricultural sector (in millions). Excl. stands for excluding, agri. for agricultural, contemp. for contemporaneous and lin. for linear.

Table C2: Regression results - Alternative specifications and standard errors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome					*Fund Share*				
AbsDist	-0.350***	-0.350***	-0.350***	-0.422***	-0.422***	-0.422***	-0.321***	-0.321***	-0.321***
	(0.001)	(0.001)	(0.001)	(0.008)	(0.000)	(0.010)	(0.001)	(0.001)	(0.001)
Commissioner	0.359	0.359	0.359	2.673***	2.673***	2.673**	0.352*	0.352	0.352**
	(0.136)	(0.238)	(0.109)	(0.005)	(0.002)	(0.025)	(0.077)	(0.196)	(0.034)
Observations	669	669	669	669	669	669	669	669	669
Country budget fes	Yes	Yes	Yes	No	No	No	Yes	Yes	Yes
Year fes	Yes	Yes	Yes	No	No	No	No	No	No
Year country fes	No	No	No	Yes	Yes	Yes	No	No	No
Budget year fes	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	No	No	No	Yes	Yes	Yes
Country-budget specific lin. time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Standard error clustering	Two-way country budget - year	Two-way country budget-year budget	Two-way country year	Two-way country budget - year	Two-way country budget-year budget	Two-way country year	Two-way country budget - year	Two-way country budget-year budget	Two-way country year

Notes: p-values in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Lin. refers to linear and fes to fixed-effects. The control variables are the ones used in the baseline: a dummy for providing the EC President, a dummy for holding the EU Council Presidency, domestic EU support, a dummy for a country that joined the EU in the latest enlargement round at time t, per capita GDP, unemployment, gross value added in agriculture, employment in agriculture, voting power at the Council, and finally dummies for whether countries are in election and pre-election years.

Table C3: Regression results - squared and country specific trends

	(1)	(2)	(3)	(4)
Outcome	*Fund Share*			
AbsDist	-0.350***	-0.284**	-0.325***	-0.354***
	(0.001)	(0.026)	(0.001)	(0.003)
Commissioner	0.359	0.757	0.607*	0.661**
	(0.109)	(0.104)	(0.095)	(0.015)
Observations	669	669	669	669
Country budget fixed-effects	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Country-budget specific linear time trend	Yes	Yes	No	No
Country-budget specific squared time trend	No	Yes	No	No
Country specific linear time trend	No	No	Yes	Yes
Country specific squared time trend	No	No	No	Yes

Notes: Two-way country-budget year cluster robust *p*-values in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$ The control variables are the ones used in the baseline: a dummy for providing the EC President, a dummy for holding the EU Council Presidency, domestic EU support, a dummy for a country that joined the EU in the latest enlargement round at time *t*, per capita GDP, unemployment, gross value added in agriculture, employment in agriculture, voting power at the Council, and finally dummies for whether countries are in election and pre-election years.

Table C4: Regression results - Heterogeneity tests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Outcome								*Fund Share*							
AbsDist	-0.355***	-0.357***	-0.350***	-0.350***	-0.393***	-0.350***	-0.361***	-0.387***	-0.307***	-0.342***	-0.350***	-0.360***	-0.262***	-0.350***	-0.365***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.005)	(0.001)	(0.001)
Commissioner	0.354	0.366	0.359	0.358	0.373	0.359	0.355	0.326	0.369	0.306	0.358	0.409	0.205	0.358	0.413*
	(0.145)	(0.102)	(0.136)	(0.136)	(0.173)	(0.136)	(0.137)	(0.272)	(0.449)	(0.107)	(0.136)	(0.128)	(0.477)	(0.139)	(0.087)
Observations	648	618	666	666	618	666	648	620	618	622	666	618	624	666	618
Excluded	Austria	Belgium	Cyprus	CZ	Denmark	Estonia	Finland	France	Germany	Greece	Hungary	Ireland	Italy	Latvia	Luxembourg
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country budget fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-budget lin. time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Continued.	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)	(25)	(26)	(27)	(28)	(29)	(30)
Outcome								*Fund Share*							
AbsDist	-0.350***	-0.313***	-0.353***	-0.371***	-0.351***	-0.351***	-0.356***	-0.353***	-0.360***	-0.350***	-0.193***	-0.422***	-0.380***	-0.270**	-0.431***
	(0.001)	(0.004)	(0.001)	(0.002)	(0.001)	(0.001)	(0.009)	(0.001)	(0.005)	(0.001)	(0.010)	(0.002)	(0.005)	(0.010)	(0.000)
Commissioner	0.359	0.215	0.357	0.348	0.358	0.358	0.297	0.359	0.717***	0.359	0.481**	0.364	0.457*		0.700**
	(0.136)	(0.490)	(0.136)	(0.116)	(0.136)	(0.136)	(0.197)	(0.133)	(0.001)	(0.136)	(0.041)	(0.187)	(0.099)		(0.019)
Observations	666	618	666	630	666	666	630	648	618	666	434	614	575	613	483
Excluded	Malta	Netherlands	Poland	Portugal	Slovakia	Slovenia	Spain	Sweden	UK	Lithuania	Large countries	Semi-presidential systems	Post-2004 enlargement	Countries providing the Commissioner	Election years
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country budget fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fe	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-budget lin. time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Two-way country-budget year cluster robust *p*-values in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Large countries refer to France, Germany, Italy, Spain, and the UK. Semi-presidential systems refer to France, Lithuania, Poland, and Portugal. These countries can be characterized by the the head of government partially sharing executive duties with a directly elected President. Countries providing the Commissioner refers to countries who provide the Commissioner of interest at that given point in time. The control variables are the ones used in the baseline: a dummy for providing the EC President, a dummy for holding the EU Council Presidency, domestic EU support, a dummy for a country that joined the EU in the latest enlargement round at time t, per capita GDP, unemployment, gross value added in agriculture, employment in agriculture, voting power at the Council, and finally dummies for whether countries are in election and pre-election years.

Table C5: Regression results - Sensitivity to excluding specific Cabinets

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Outcome	*Fund Share*									
AbsDist	-0.350***	-0.233***	-0.300***	-0.326***	-0.304**	-0.355***	-0.267***	-0.366**	-0.487***	-0.378***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.011)	(0.001)	(0.007)	(0.023)	(0.004)	(0.004)
Commissioner	0.359	0.555	-0.026	0.735	0.240	0.343*	0.646	-0.077	0.666**	0.436
	(0.136)	(0.167)	(0.897)	(0.169)	(0.513)	(0.082)	(0.127)	(0.872)	(0.012)	(0.112)
Observations	669	633	591	577	573	623	435	551	496	619
Excluded term	None	Jenkins	Thorn	Delors I	Delors II	Delors III	Delors pooled	Santer	Prodi	Barroso
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country budget fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-budget specific lin. time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Two-way country-budget year cluster robust *p*-values in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

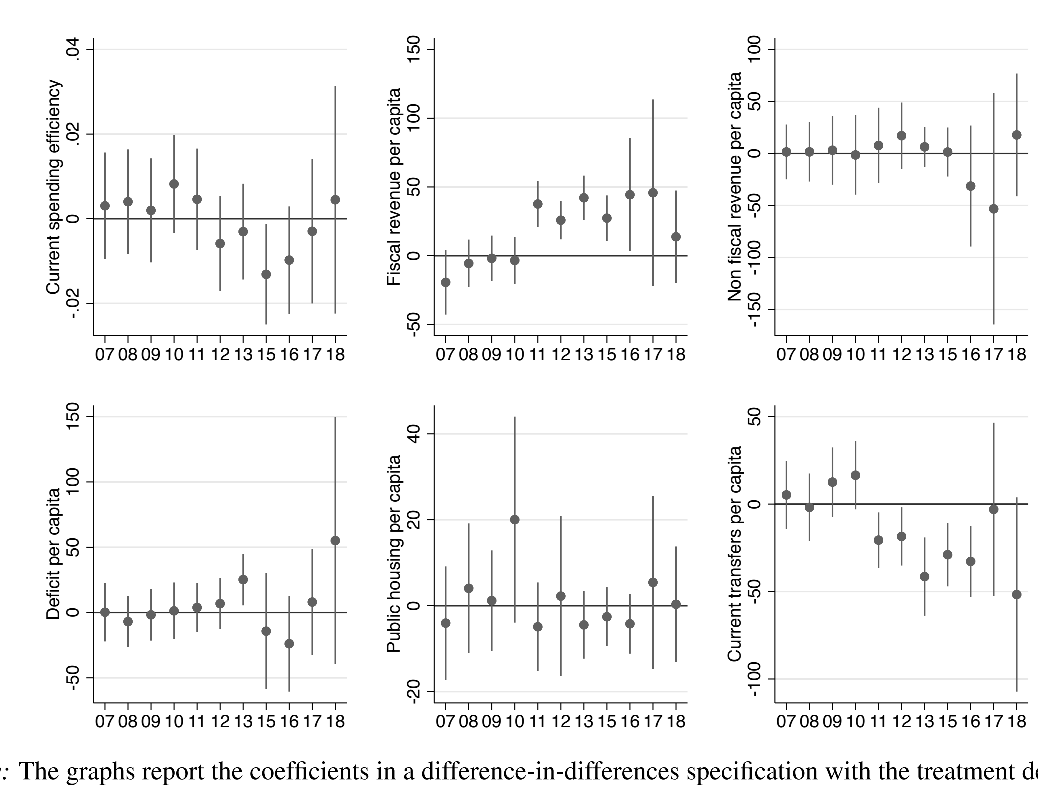
Lin. refers to linear. The excluded term refers to the name of the Commission's President whose term years were excluded from the sample. The control variables are the ones used in the baseline: a dummy for providing the EC President, a dummy for holding the EU Council Presidency, domestic EU support, a dummy for a country that joined the EU in the latest enlargement round at time t, per capita GDP, unemployment, gross value added in agriculture, employment in agriculture, voting power at the Council, and finally dummies for whether countries are in election and pre-election years.

D

Appendix to Chapter 4

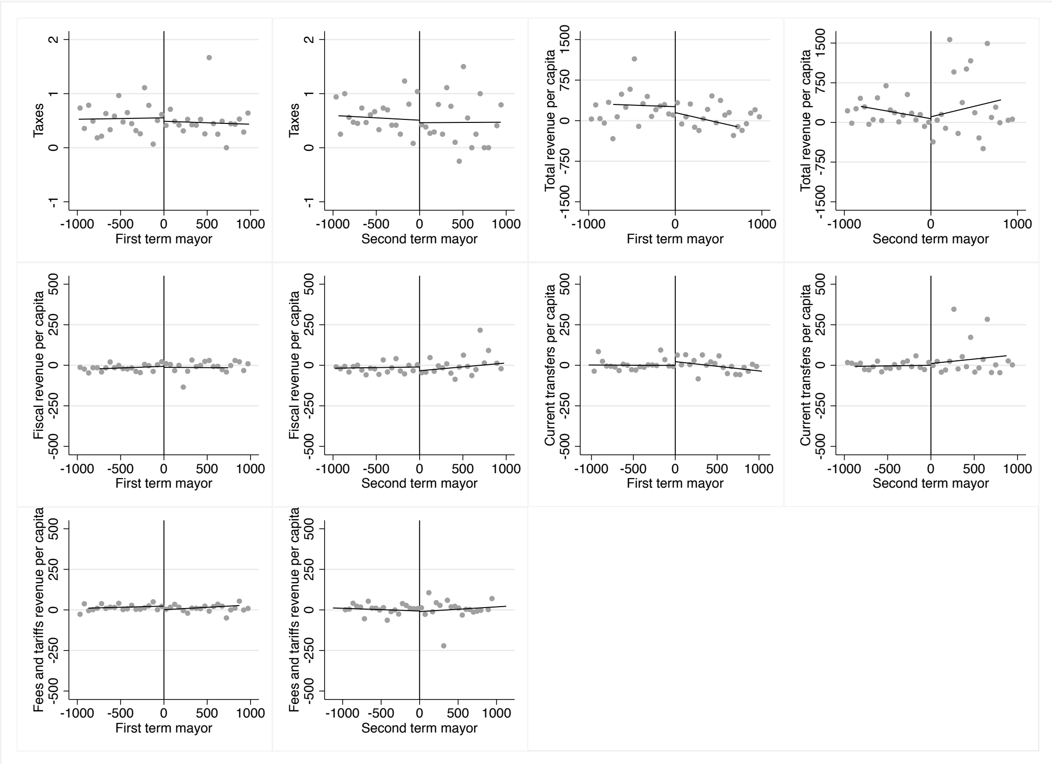
D.1 Figures

Figure D.1 Pre-trends in a difference-in-difference setting



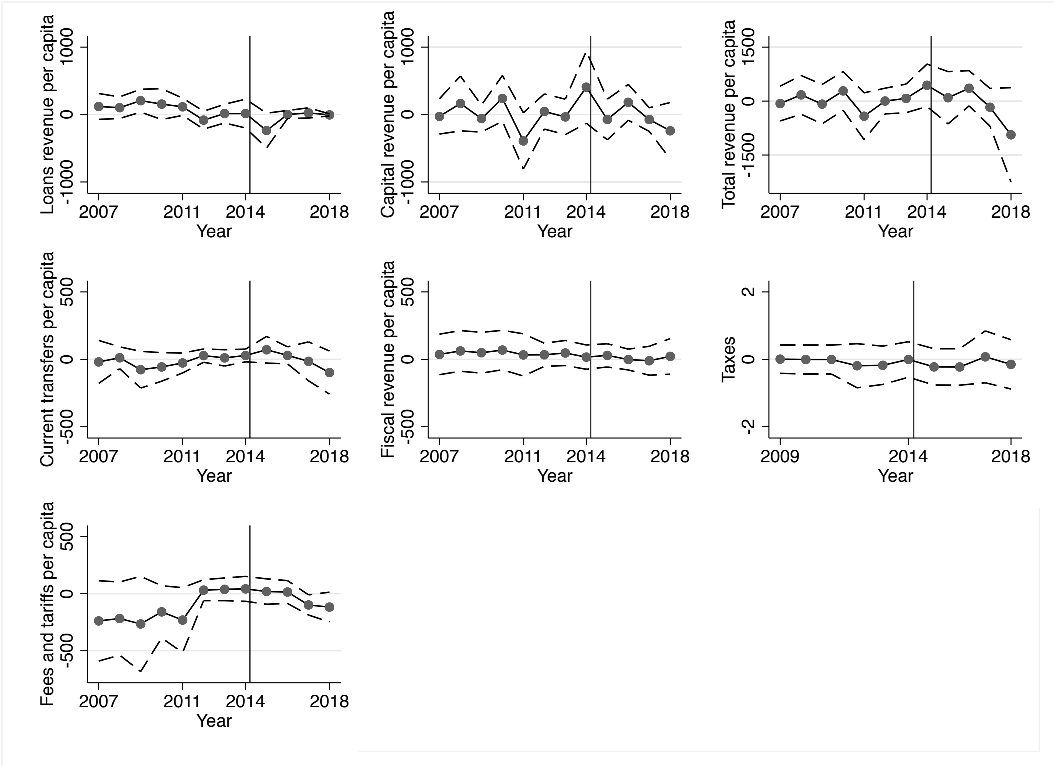
Notes: The graphs report the coefficients in a difference-in-differences specification with the treatment defined as being below 3,000 inhabitants for each year. The regressions include year and municipality fixed effects. For each year, we report the point estimate and the 95 percent confidence interval. The coefficient on 2014 is the omitted category.

Figure D.2 Difference in discontinuities - Other revenue items



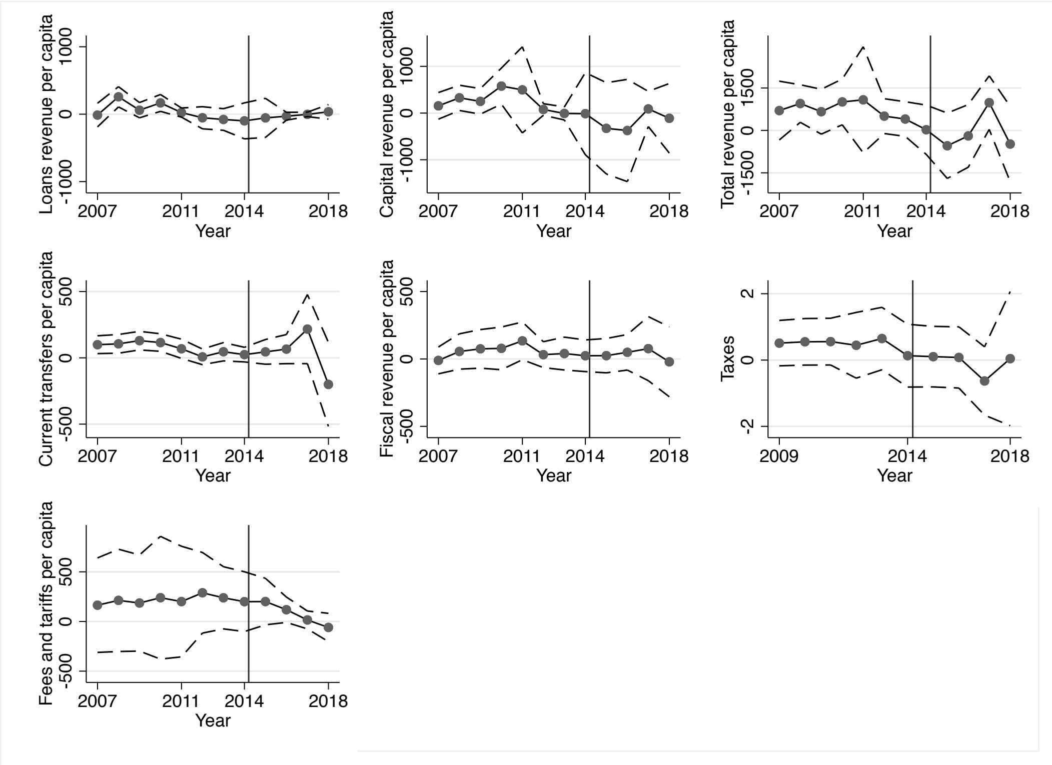
Notes: Vertical axis: difference of the the average post-reform outcome value and pre-reform outcome value. Horizontal axis: 2011 census population size centered around the 3,000 inhabitants discontinuity. Dots represent the average value of the likelihood of a district's mayor being in her second term within a given bin of the running variable. The number of bins used on each side of the threshold is set at 20. The line fits a linear trend on each side of the population threshold to facilitate visualization.

Figure D.3 Revenue items yearly RD coefficients - First-term mayors



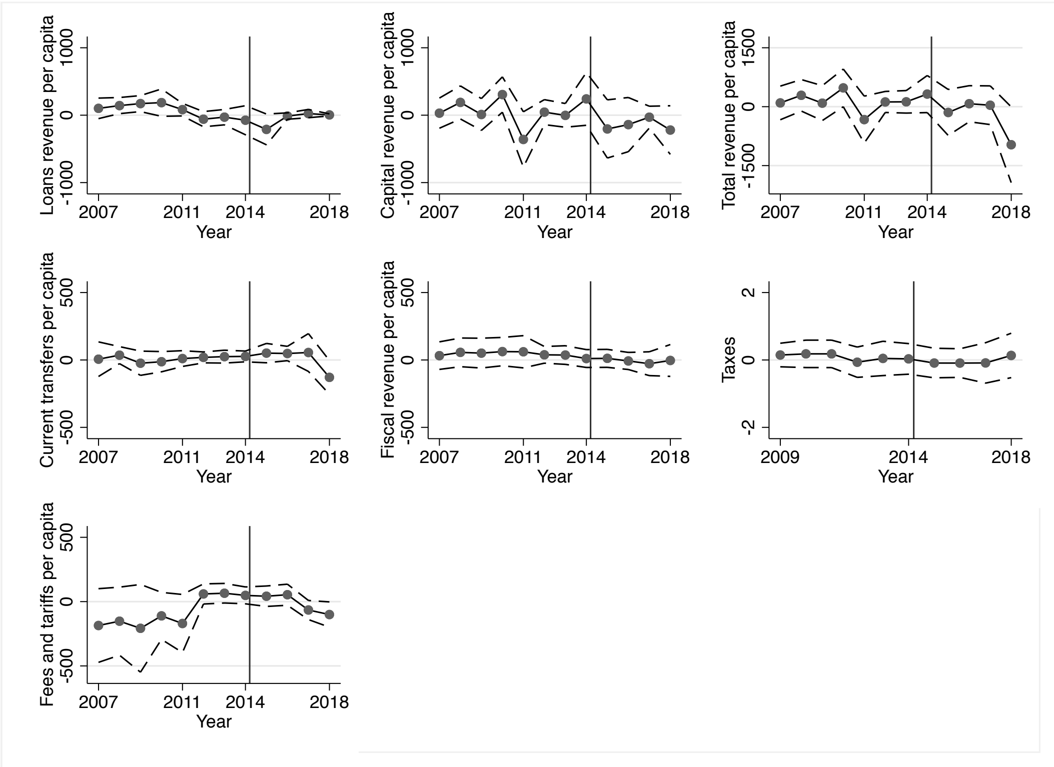
Notes: Yearly RD coefficients. Vertical axis: point estimates of local linear regressions using optimal bandwidths selected by the procedure developed by Calonico et al. (2014a) and Calonico et al. (2014b). Horizontal axis: years. The vertical line corresponds to the introduction of the reform.

Figure D.4 Revenue items yearly RD coefficients - Second-term mayors



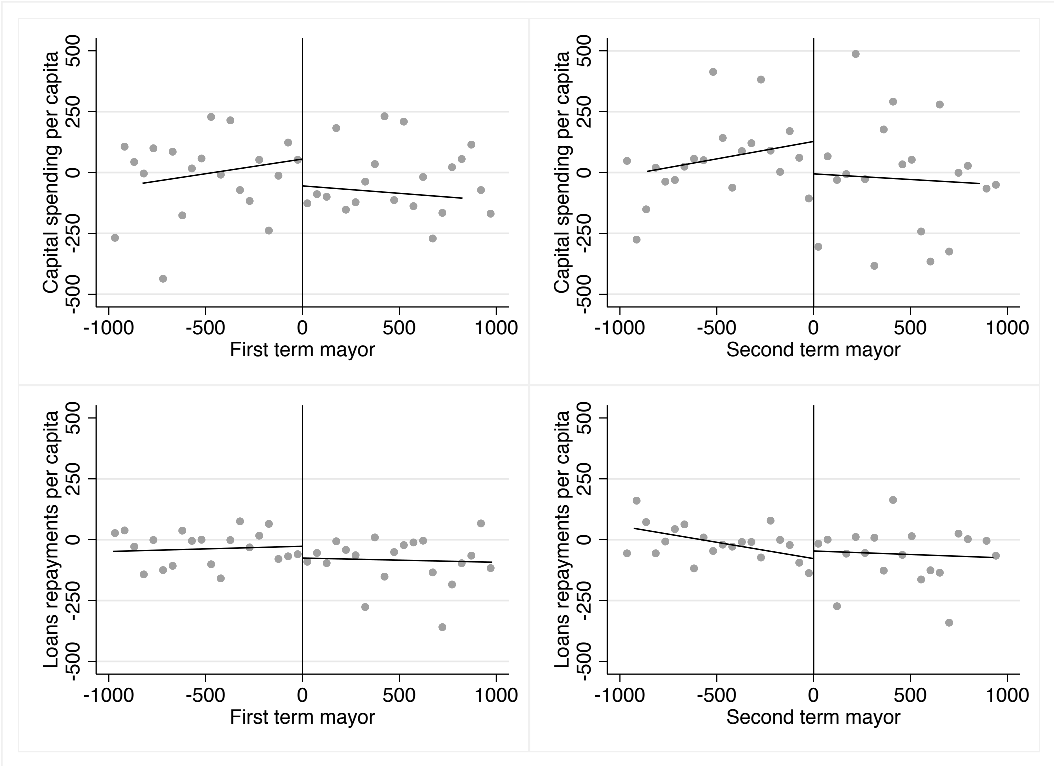
Notes: as in Figure D.3.

Figure D.5 Revenue items yearly RD coefficients - All mayors



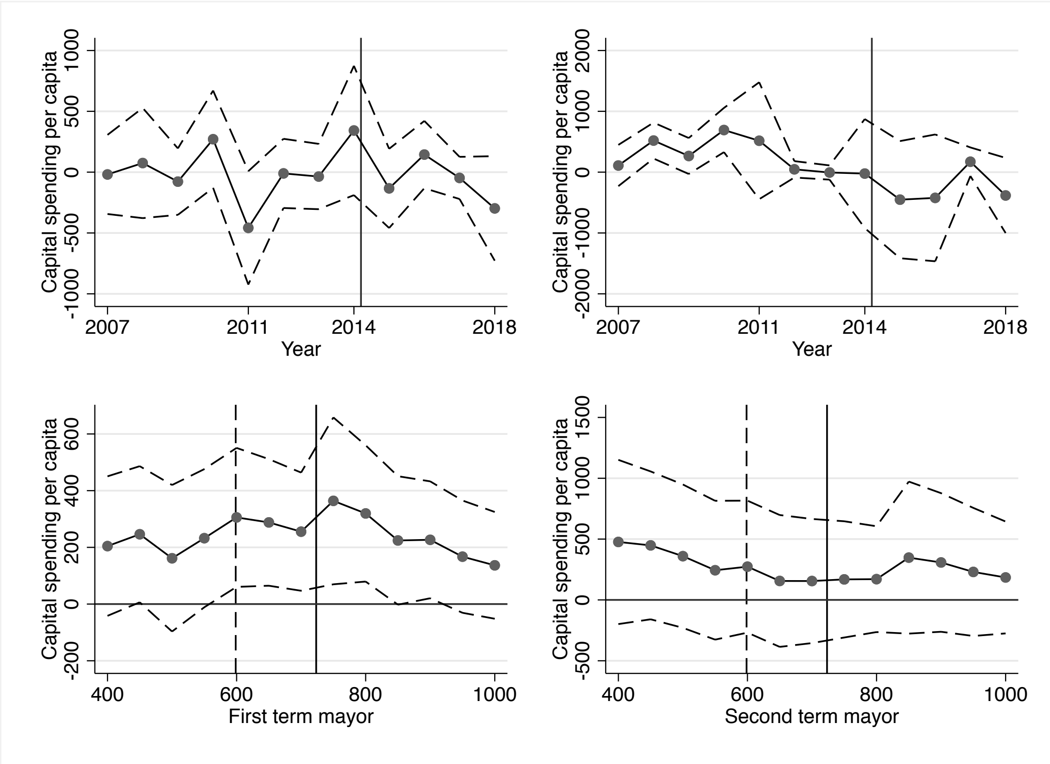
Notes: as in Figure D.3.

Figure D.6 Difference in discontinuities - Capital expenditures and loans repayments



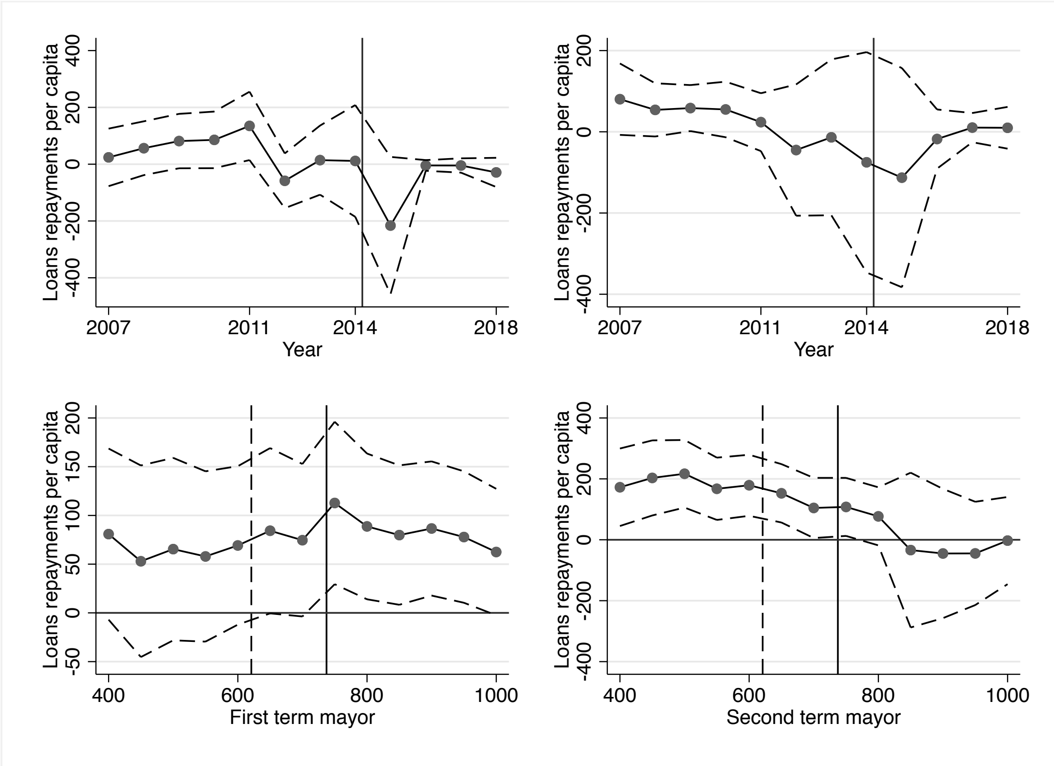
Notes: as in Figure D.3.

Figure D.7 Difference in discontinuities - Capital spending



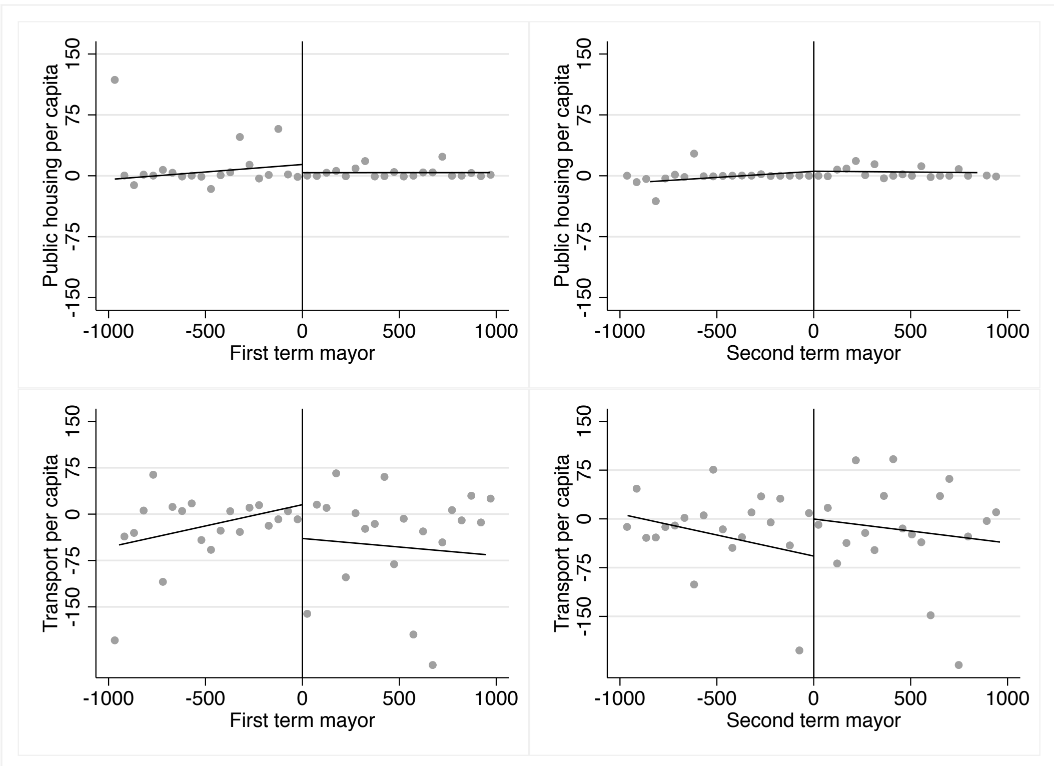
Notes: Top panel: yearly RD coefficients. Vertical axis: point estimates of local linear regressions using optimal bandwidths selected by the procedure developed by Calonico et al. (2014a) and Calonico et al. (2014b). Horizontal axis: years. The vertical line represents the introduction of the law. Bottom panel: difference in discontinuities by bandwidth. Vertical axis: *difference-in-discontinuities* coefficients. Horizontal axis: bandwidth used to estimate the coefficient. The dashed vertical line represents the bandwidth chosen when using local linear regression to determine the estimator (p=1), while the dark vertical line shows the bandwidth when using a quadratic fit (p=2). In both panels the central lines represent the point estimates while the lateral lines the 95 percent confidence interval while results are displayed on the left hand side for first-term mayors and on the right hand side for second-term mayors.

Figure D.8 Difference in discontinuities - Loans repayments



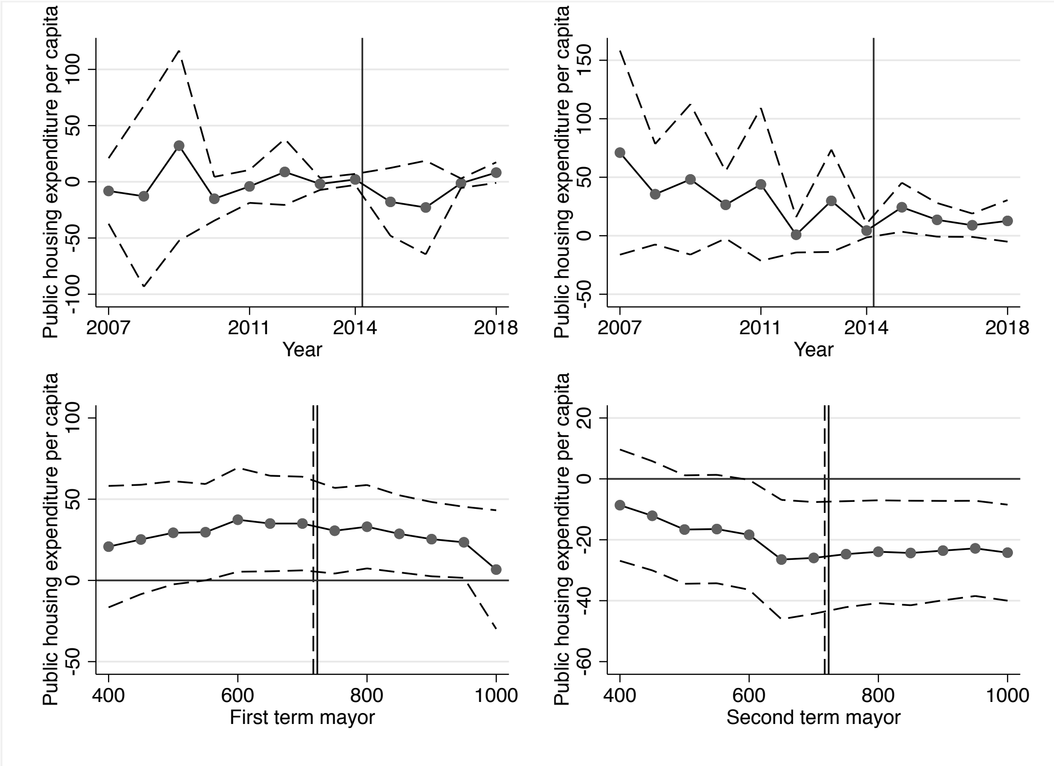
Notes: as in Figure D.7.

Figure D.9 Difference in discontinuities - Public housing and transport



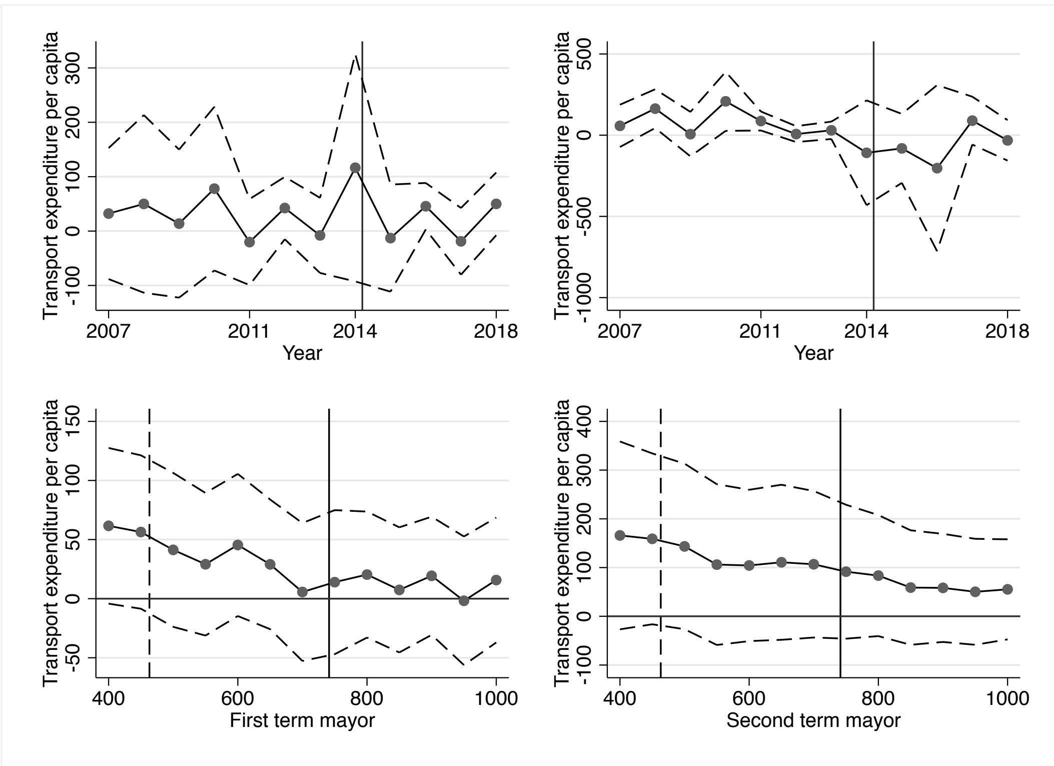
Notes: as in Figure D.7.

Figure D.10 Difference in discontinuities - Public housing



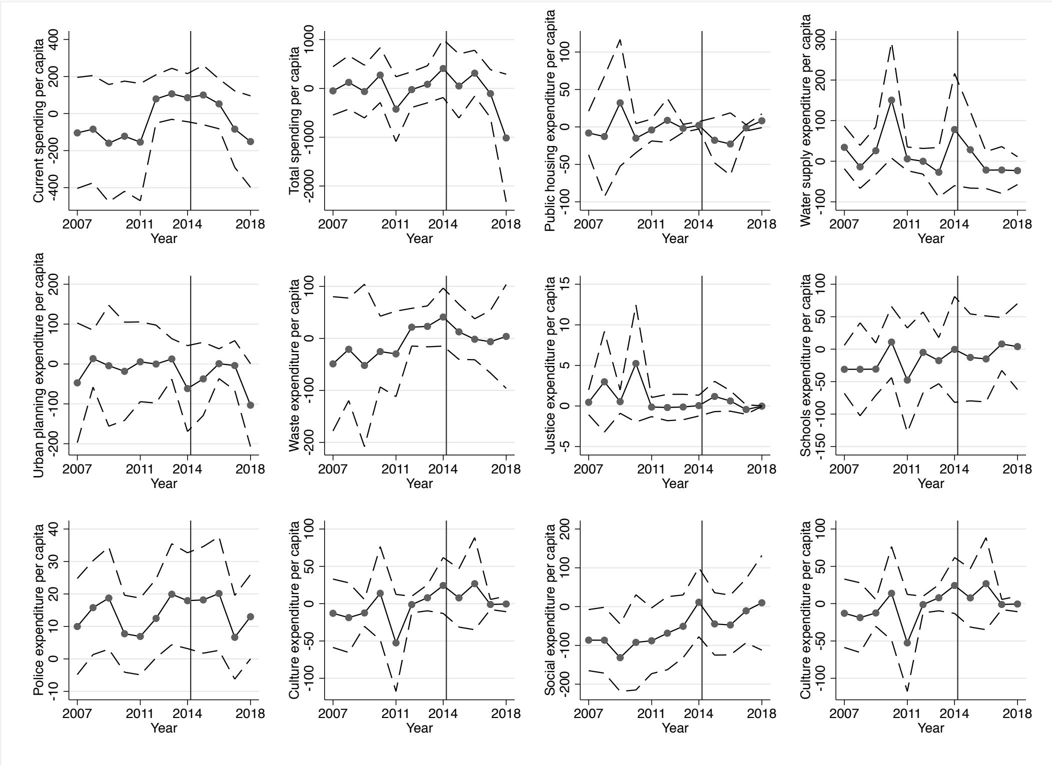
Notes: as in Figure D.7.

Figure D.11 Difference in discontinuities - Transport and mobility



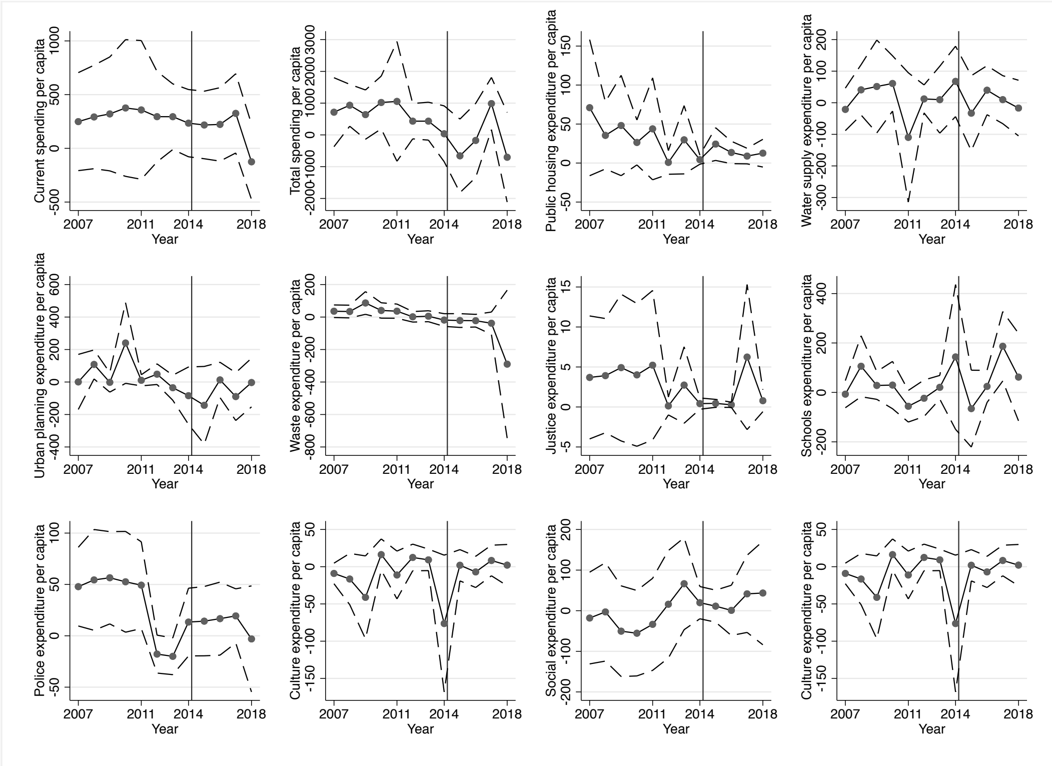
Notes: as in Figure D.7.

Figure D.12 Expenditure items yearly RD coefficients - First-term mayors



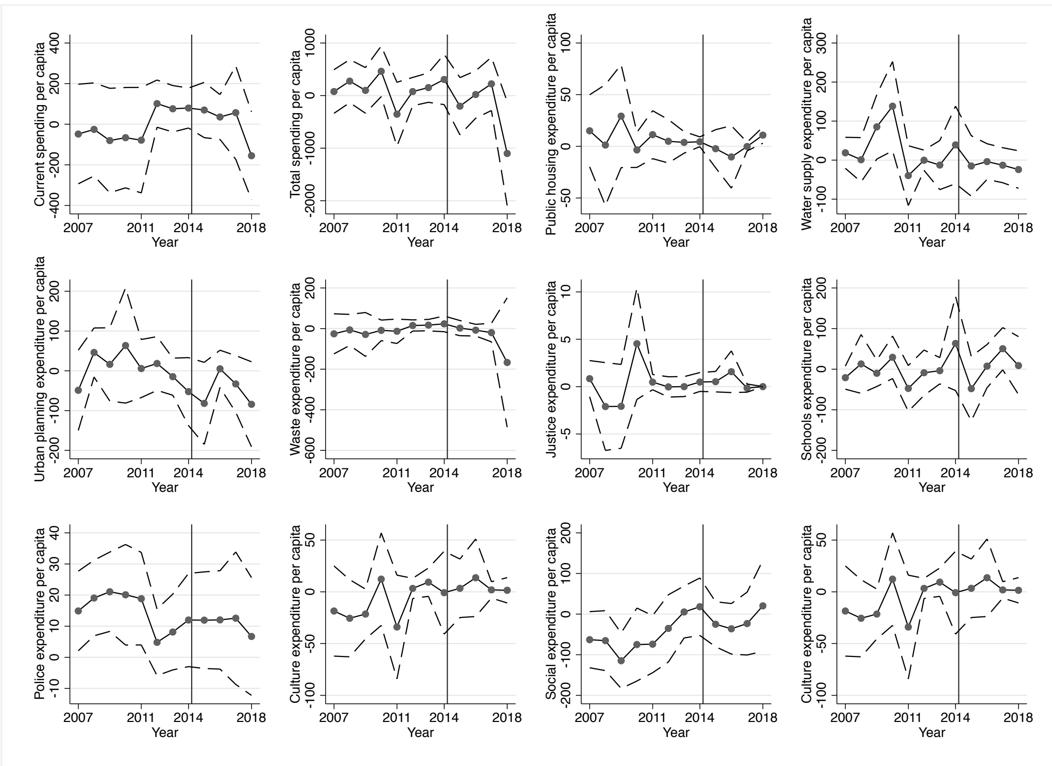
Notes: Yearly RD coefficients. Vertical axis: point estimates of local linear regressions using optimal bandwidths selected by the procedure developed by Calonico et al. (2014a) and Calonico et al. (2014b). Horizontal axis: years. The vertical line corresponds to the introduction of the reform.

Figure D.13 Expenditure items yearly RD coefficients - Second-term mayors



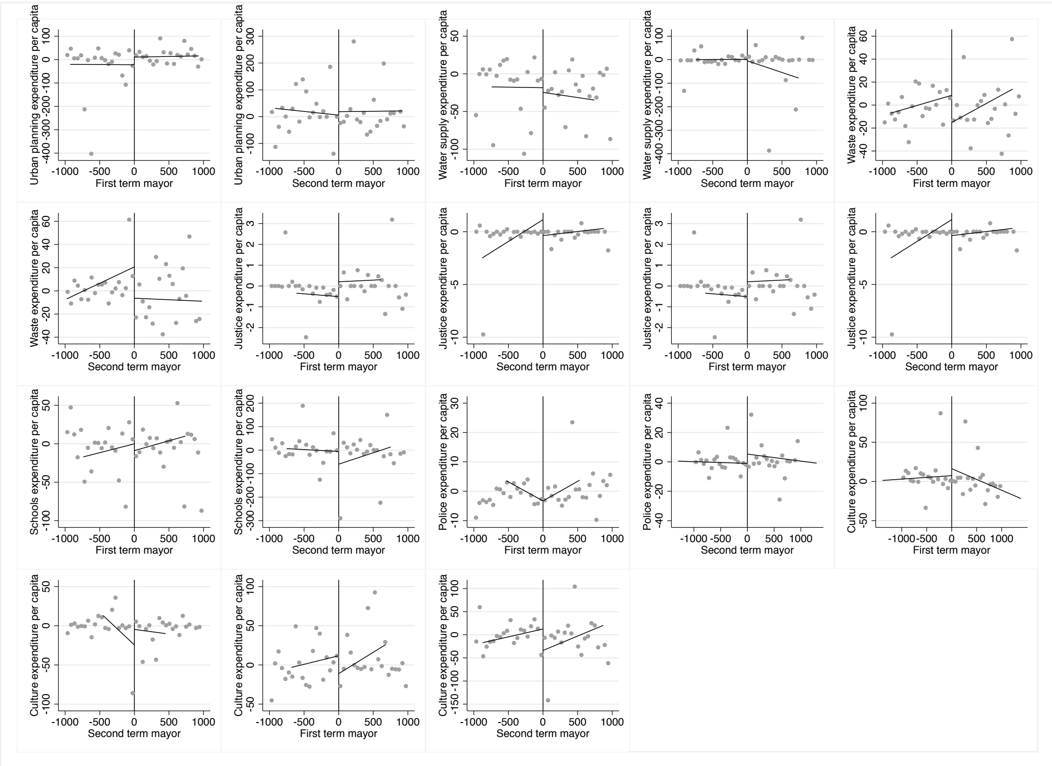
Notes: as in Figure D.12.

Figure D.14 Expenditure items yearly RD coefficients - All mayors



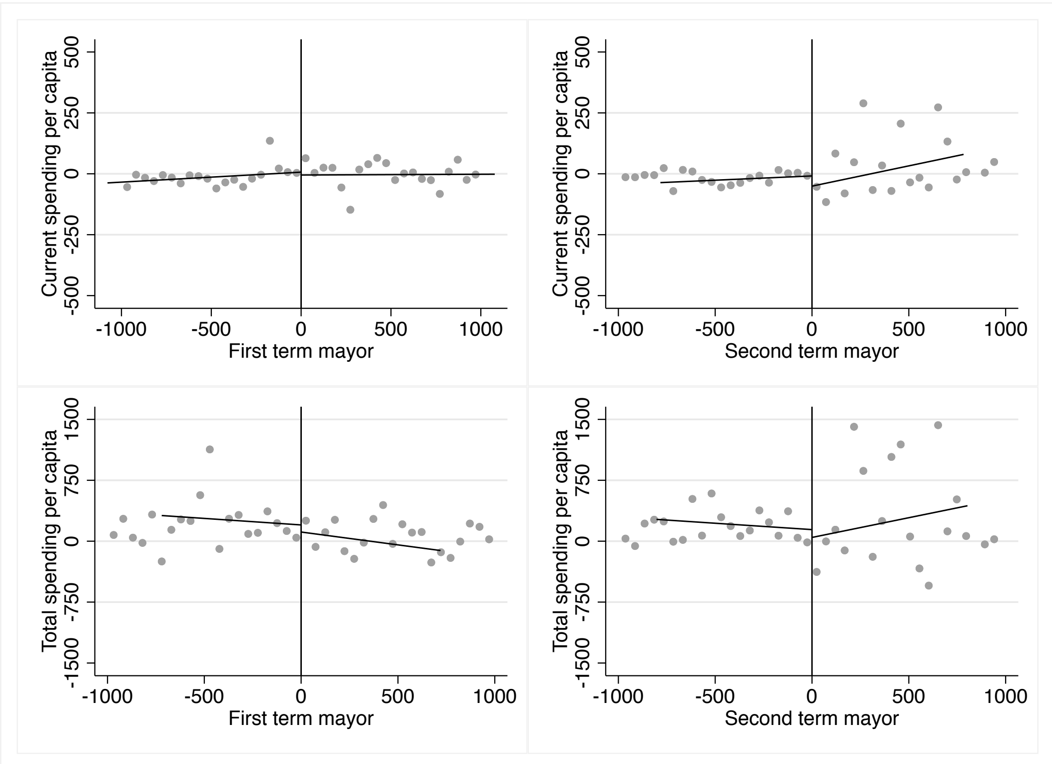
Notes: as in Figure D.12.

Figure D.15 Difference in discontinuities - Specific expenditure items



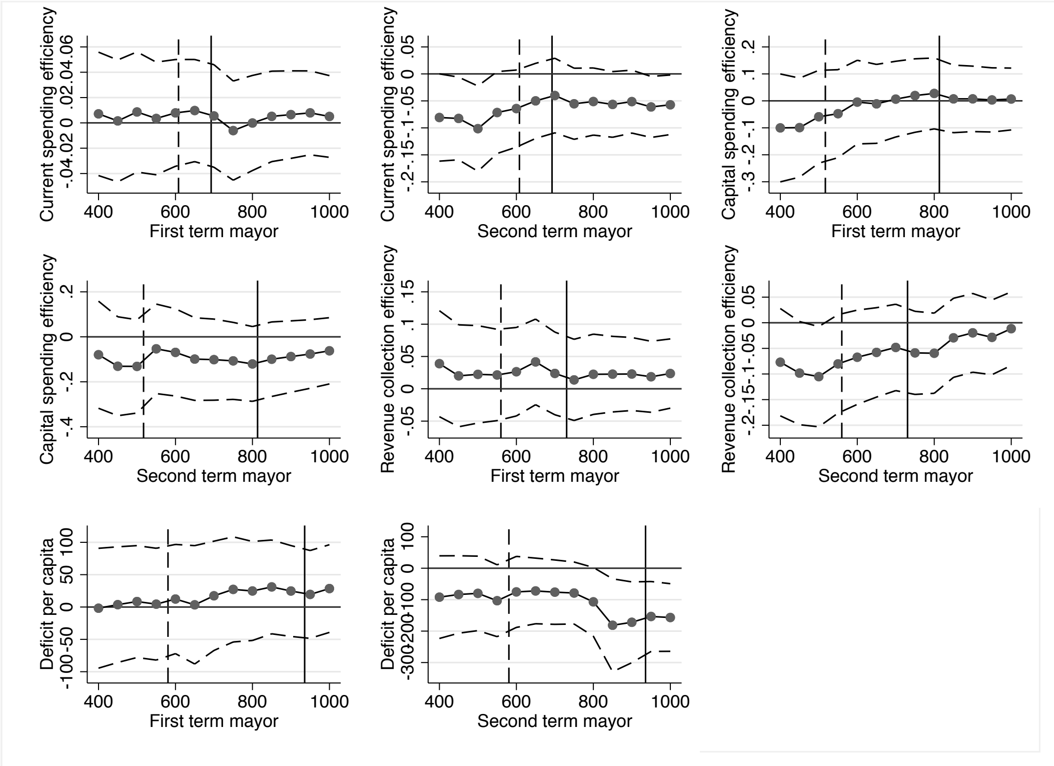
Notes: as in Figure D.12.

Figure D.16 Difference in discontinuities - Current and total expenditures



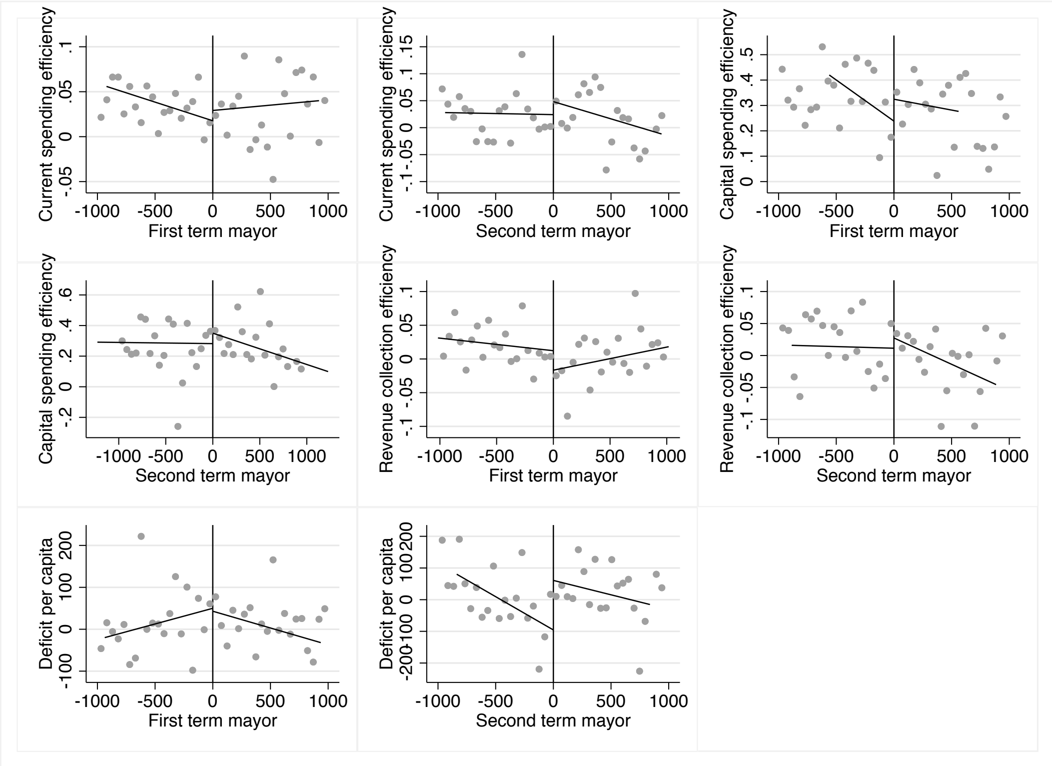
Notes: as in Figure D.12.

Figure D.17 Difference in discontinuity - Efficiency and deficits - Bandwidth sensitivity



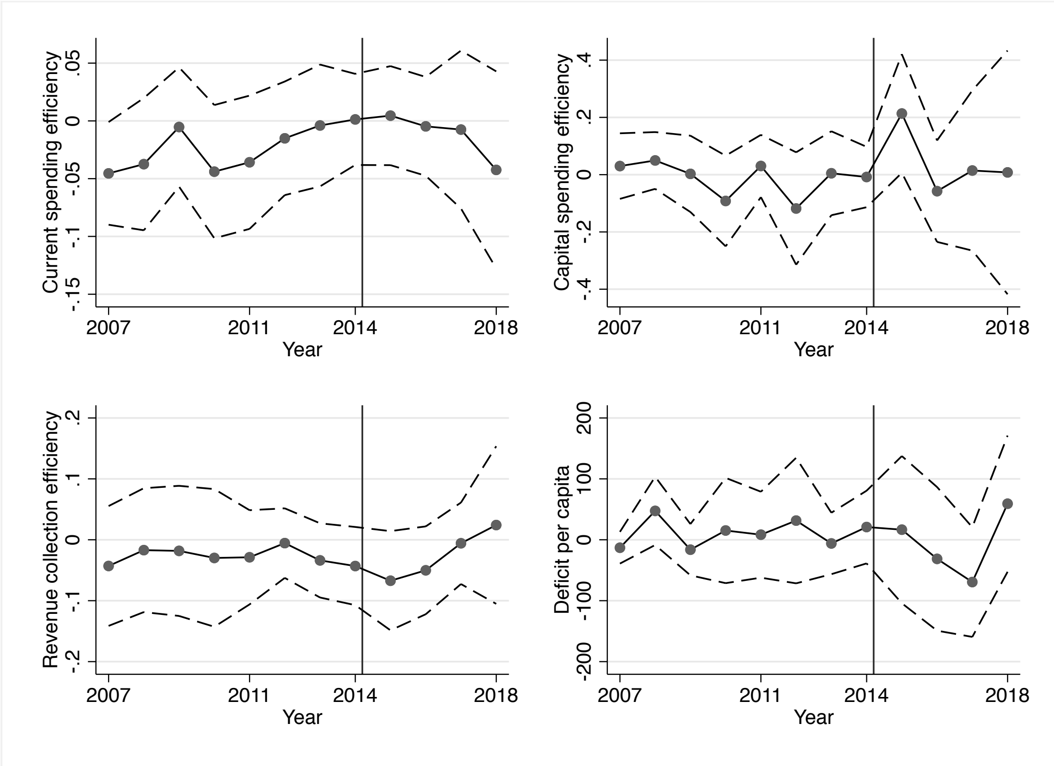
Notes: Vertical axis: *difference-in-discontinuities* coefficients. Horizontal axis: bandwidth used to estimate the coefficient. The dashed vertical line represents the bandwidth chosen when using local linear regression to determine the estimator (p=1), while the dark vertical line shows the bandwidth when using a quadratic fit (p=2). The central lines represent the point estimates while the lateral lines the 95 percent confidence interval.

Figure D.18 Difference in discontinuity - Efficiency and deficits



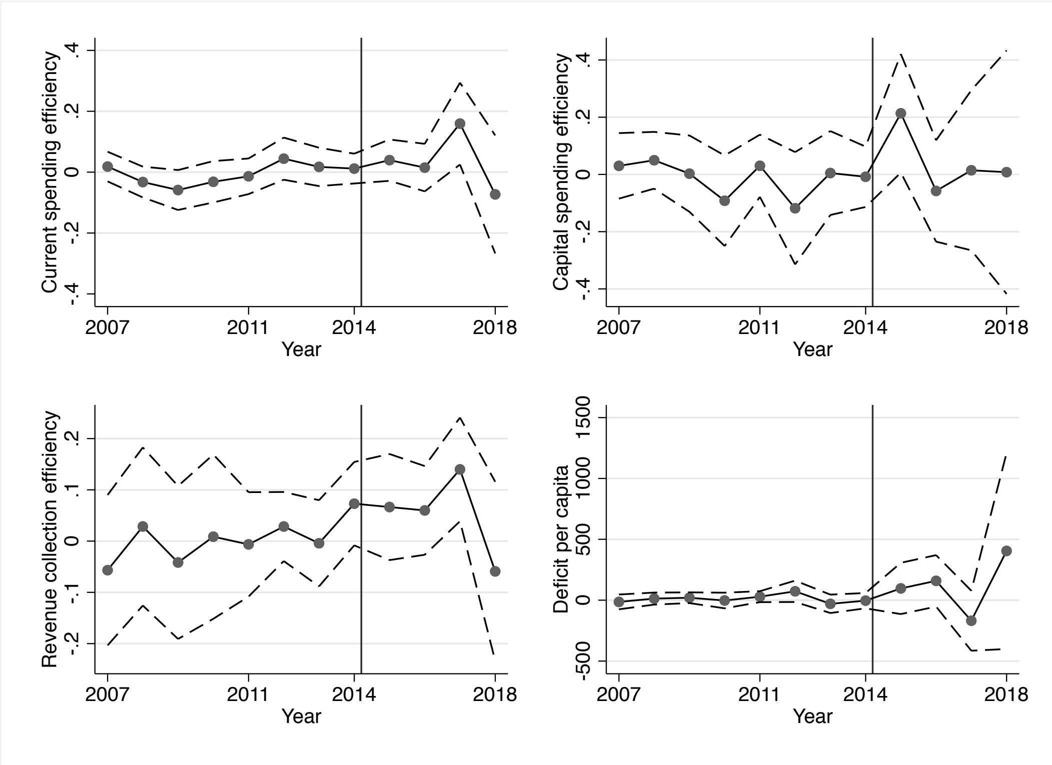
Notes: Vertical axis: difference of the the average post-reform outcome value and pre-reform outcome value. Horizontal axis: 2011 census population size centered around the 3,000 inhabitants discontinuity. Dots represent the average value of the likelihood of a district's mayor being in her second term within a given bin of the running variable. The number of bins used on each side of the threshold is set at 20. The line fits a linear trend on each side of the population threshold to facilitate visualization.

Figure D.19 Efficiency and deficits yearly RD coefficients - First-term mayors



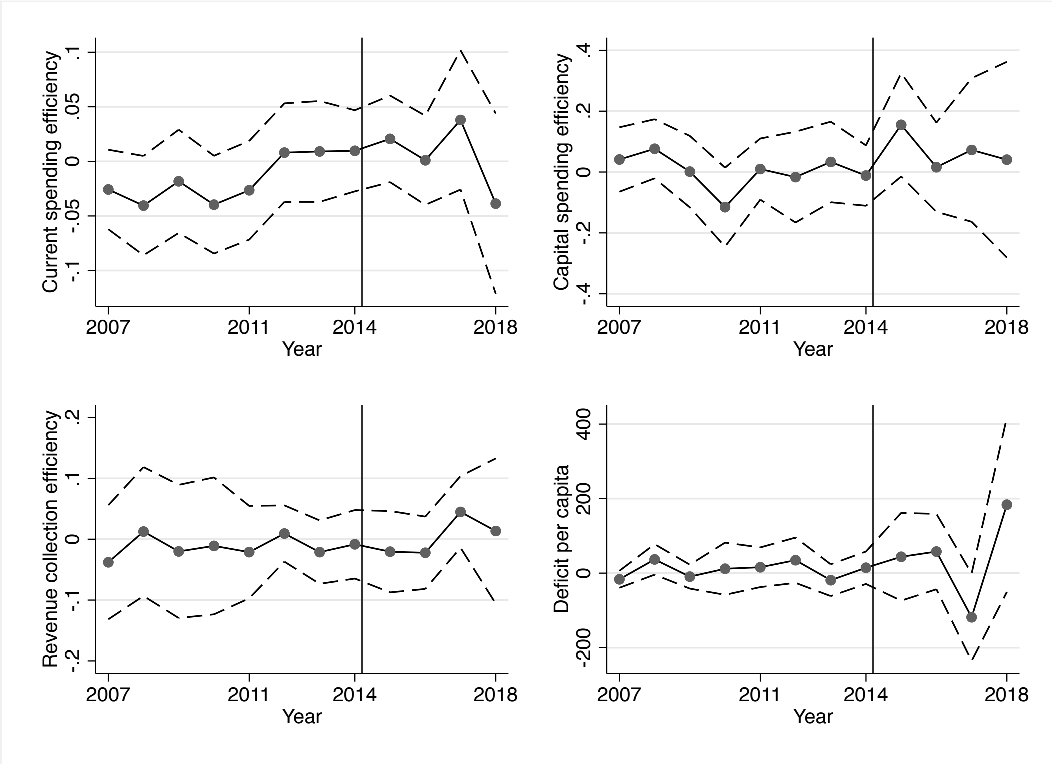
Notes: Yearly RD coefficients. Vertical axis: point estimates of local linear regressions using optimal bandwidths selected by the procedure developed by Calonico et al. (2014a) and Calonico et al. (2014b). Horizontal axis: years. The vertical line corresponds to the introduction of the reform.

Figure D.20 Efficiency and deficits yearly RD coefficients - Second-term mayors



Notes: as in Figure D.19.

Figure D.21 Efficiency and deficits yearly RD coefficients - All mayors



Notes: as in Figure D.19.

D.2 Tables

Table D.1 Data sources and description

Data	*Source*	*Content*	*Time Span*	*Variables*
1.**Municipal Elections**	Ministry of Interior	Municipal election-level: Universe of municipal elections	1991-2019	Elected mayor, margin of victory, number of candidates.
2.**Municipal Budgets**	Ministry of Interior	Municipal budget item-level: Universe of municipal budget items	1998-2018	Current Spending, Capital Spending, Municipal Revenues, Central Transfers, Fees and Tariffs, Debt Service, Deficit.
3.**Registry of Italian politicians**	Ministry of Interior	Individual-level: Universe of Italian politicians	1985-2020	Date and place of birth, gender, party affiliation, public office held.
4.**Census data**	ISTAT	Municipality-level	2001 and 2011	Number of residents, number of people employed, number of people over 65.
5.**Social Capital**	Nannicini et al. (2013)	Municipality-level	2011	Number of associations, turnout in the 1974 referendum on divorce, blood donations.

Table D.2 Balance on observables' tests

Outcome	(1) South	(2) Centre	(3) Less than 6 yrs old	(4) Avg surface of housing	(5) Incidence of young couples	(6) 2nd term mayor	(7) Demographic density	(8) Average household size	
Treatment	0.033	-0.005	-0.114	1.441	-0.313	0.149	20.469	-0.028	
	(0.122)	(0.085)	(0.312)	(1.234)	(0.438)	(0.139)	(57.355)	(0.045)	
Observations	338	433	311	301	230	219	342	265	
Robust p-value	0.893	0.965	0.596	0.171	0.411	0.251	0.556	0.643	
Polynomial	1	1	1	1	1	1	1	1	
Bandwidth	793.3	991.7	728	697.2	540.5	513.3	800.9	618.1	
Mean	0.360	0.180	5.133	42.38	2.855	0.770	217.3	2.443	
Outcome	**(9) Above 75 index**	**(10) Old age index**	**(11) Dependency index**	**(12) Elec in 2011**	**(13) Elec in 2012**	**(14) Elec in 2013**	**(15) Divorce referendum turnout**	**(16) Newspaper sales**	**(17) Blood donations**
Treatment	0.003	9.577	1.094	0.199	-0.190*	0.003	-0.010	-4.404	-0.008
	(0.008)	(15.540)	(2.157)	(0.130)	(0.107)	(0.090)	(0.023)	(9.769)	(0.006)
Observations	325	309	279	295	375	332	310	270	277
Robust p-value	0.684	0.467	0.547	0.112	0.0862	0.905	0.824	0.777	0.253
Polynomial	1	1	1	1	1	1	1	1	1
Bandwidth	767.6	717	648	679.1	864.5	782.8	799.5	626.2	700.7
Mean	0.115	170	32.95	0.487	0.302	0.210	0.858	68.53	0.0265

All variables are expressed in per capita terms. Municipality level cluster robust p-values in parentheses: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The chosen bandwidth is computed as the average of the two bandwidths selected with the procedure developed by Calonico et al. (2014a) and Calonico et al. (2014b) on each sample obtained by splitting between observations dated before and after 2015. The observations refers to the number of observations within the bandwidth. The mean refers to the average pre-treatment value of the outcome variable for *treated* municipalities.

Table D.3 False discontinuities - Capital revenue per capita

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Discontinuity	1.9k	2k	2.1k	2.2k	2.3k	3.7k	3.8k	3.9k	4k	4.1k
First-term mayor	102.609	89.993	239.156	150.537	-184.525	60.440	190.084	-17.777	-10.250	-66.497
	(0.397)	(0.465)	(0.110)	(0.319)	(0.253)	(0.578)	(0.192)	(0.869)	(0.916)	(0.436)
Second-term mayor	217.165	157.206	286.338	-384.459	-379.528	320.299***	307.954**	211.158*	-44.743	166.318*
	(0.456)	(0.653)	(0.577)	(0.353)	(0.243)	(0.008)	(0.025)	(0.087)	(0.621)	(0.08)
Observations	5,719	6,058	4,466	5,078	5,495	3,362	2,608	2,892	2,914	3,061
Bandwidth	961	1044	782	922	1026	1074	855	944	988	1055
Mean	475	478	445	523	532	259	237	301	309	304

Notes: All variables are expressed in per capita terms. Municipality level cluster robust p-values in parentheses: *** p<0.01, ** p<0.05, * p<0.1. The chosen bandwidth is computed as the average of the two bandwidths selected with the procedure developed by Calonico et al. (2014a) and Calonico et al. (2014b) on each sample obtained by splitting between observations dated before and after 2015. The observations refers to the number of observations within the bandwidth. The mean refers to the average pre-treatment value of the outcome variable for *treated* municipalities.

Table D.4 False discontinuities - Loans revenue per capita

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Discontinuity	1.9k	2k	2.1k	2.2k	2.3k	3.7k	3.8k	3.9k	4k	4.1k
First-term mayor	-26.410	20.344	71.784	-5.359	-8.082	-38.824	71.216	-71.253	-116.412**	-106.120**
	(0.557)	(0.626)	(0.111)	(0.895)	(0.843)	(0.546)	(0.194)	(0.142)	(0.013)	(0.013)
Second-term mayor	-125.297*	-67.551	-95.243	47.876	75.197	18.512	-42.079	-34.096	20.692	94.857
	(0.096)	(0.43)	(0.403)	(0.573)	(0.434)	(0.798)	(0.656)	(0.615)	(0.78)	(0.177)
Observations	5,176	5,584	4,286	4,733	4,119	2,633	2,533	3,350	3,060	3,433
Bandwidth	832	952	755	864	793	853	828	1134	1040	1171
Mean	153	134	151	201	145	167	165	163	182	145

Notes: as in Table D.3.

Table D.5 False discontinuities - Capital spending per capita

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Discontinuity	1.9k	2k	2.1k	2.2k	2.3k	3.7k	3.8k	3.9k	4k	4.1k
First-term mayor	62.469	100.212	338.175*	185.453	-218.543	58.681	191.497	-45.998	-109.261	-91.037
	(0.629)	(0.475)	(0.052)	(0.281)	(0.247)	(0.634)	(0.222)	(0.671)	(0.299)	(0.304)
Second-term mayor	278.604	287.045	301.902	-493.206	-473.306	89.262	264.363**	92.365	-170.8*	130.68
	(0.36)	(0.436)	(0.569)	(0.234)	(0.156)	(0.497)	(0.047)	(0.506)	(0.063)	(0.168)
Observations	5,623	5,832	4,374	5,126	5,269	2,952	2,533	3,075	2,718	2,743
Bandwidth	903	991	771	930	982	958	828	1015	929	967
Mean	577	570	544	620	660	345	316	370	375	376

Notes: as in Table D.3.

Table D.6 False discontinuities - Loans repayments per capita

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Discontinuity	1.9k	2k	2.1k	2.2k	2.3k	3.7k	3.8k	3.9k	4k	4.1k
First-term mayor	33.557	8.509	64.767	1.541	-33.831	0.842	83.909*	-8.684	-61.541	-78.016*
	(0.437)	(0.838)	(0.225)	(0.967)	(0.463)	(0.989)	(0.087)	(0.836)	(0.151)	(0.095)
Second-term mayor	-170.433	-103.207	-204.735	177.302	162.713	83.55	-40.629	-43.763	-69.672	95.266
	(0.153)	(0.466)	(0.382)	(0.242)	(0.296)	(0.216)	(0.626)	(0.489)	(0.304)	(0.209)
Observations	4,592	6,245	3,736	5,156	4,764	2,208	2,226	2,914	2,290	2,596
Bandwidth	727	1,138	658	940	901	700	749	954	794	929
Mean	133	119	134	200	176	166	147	147	180	144

Notes: All variables are expressed in per capita terms. Municipality level cluster robust p-values in parentheses: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The chosen bandwidth is computed as the average of the two bandwidths selected with the procedure developed by Calonico et al. (2014a) and Calonico et al. (2014b) on each sample obtained by splitting between observations dated before and after 2015. The observations refers to the number of observations within the bandwidth. The mean refers to the average pre-treatment value of the outcome variable for *treated* municipalities.

Table D.7 False discontinuities - Public housing per capita

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Discontinuity	1.9k	2k	2.1k	2.2k	2.3k	3.7k	3.8k	3.9k	4k	4.1k
First-term mayor	18.991	-54.134	14.902	13.732	4.336	-10.366	-1.549	2.114	12.707	9.993
	(0.625)	(0.212)	(0.513)	(0.513)	(0.827)	(0.235)	(0.787)	(0.649)	(0.179)	(0.117)
Second-term mayor	-50.32	-28.021	-35.941	-18.427	3.496	10.91	11.267	-5.856	-42.135**	-12.077
	(0.164)	(0.285)	(0.114)	(0.331	(0.708)	(0.362)	(0.507)	(0.699)	(0.033)	(0.515)
Observations	4,405	3,798	5,961	5,677	3,732	1,396	1,712	1,982	1,730	2,525
Bandwidth	697	625	1043	1,018	724	459	578	696	644	912
Mean	19	46	32	18	10	5	3	5	8	4

Notes: as in Table D.6.

Table D.8 False discontinuities - Transport per capita

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Discontinuity	1.9k	2k	2.1k	2.2k	2.3k	3.7k	3.8k	3.9k	4k	4.1k
First-term mayor	-22.256	-22.397	3.103	6.097	-5.560	-48.300	-13.651	-38.971	-44.123	-50.660*
	(0.577)	(0.624)	(0.933)	(0.865)	(0.883)	(0.244)	(0.706)	(0.204)	(0.155)	(0.058)
Second-term mayor	-53.628	-94.422	-70.256	-20.739	21.103	54.142	29.151	26.362	51.967	-37.92
	(0.401)	(0.14)	(0.202)	(0.64)	(0.615)	(0.155)	(0.421)	(0.536)	(0.185)	(0.387)
Observations	5,903	4,673	5,435	5,371	4,432	2,615	2,986	3,085	2,765	3,206
Bandwidth	1,086	779	946	971	851	848	969	1,019	948	1,093
Mean	222	219	216	209	191	197	159	164	140	171

Notes: as in Table D.6.

Table D.9 False discontinuities - Revenue collection efficiency

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Discontinuity	1.9k	2k	2.1k	2.2k	2.3k	3.7k	3.8k	3.9k	4k	4.1k
First-term mayor	0.035	0.006	-0.041	-0.020	-0.031	0.050	-0.009	0.017	-0.040	-0.048
	(0.206)	(0.856)	(0.120)	(0.495)	(0.257)	(0.296)	(0.801)	(0.649)	(0.189)	(0.173)
Second-term mayor	0.000	0.003	-0.019	-0.131***	-0.028	0.010	-0.033	0.017	-0.001	-0.057
	(0.996)	(0.934)	(0.574)	(0.000)	(0.445)	(0.869)	(0.542)	(0.782)	(0.988)	(0.352)
Observations	4,804	4,022	4,747	3,976	3,630	1,462	2,502	1,791	2,312	2,077
Bandwidth	763	667	838	738	710	472	820	643	805	757
Mean	0.69	0.71	0.72	0.73	0.66	0.72	0.68	0.69	0.69	0.72

Notes: All variables are expressed in per capita terms. Municipality level cluster robust p-values in parentheses: *** p<0.01, ** p<0.05, * p<0.1. The chosen bandwidth is computed as the average of the two bandwidths selected with the procedure developed by Calonico et al. (2014a) and Calonico et al. (2014b) on each sample obtained by splitting between observations dated before and after 2015. The observations refers to the number of observations within the bandwidth. The mean refers to the average pre-treatment value of the outcome variable for *treated* municipalities.

Table D.10 False discontinuities - Current spending efficiency

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Discontinuity	1.9k	2k	2.1k	2.2k	2.3k	3.7k	3.8k	3.9k	4k	4.1k
First-term mayor	0.023	-0.010	-0.009	-0.001	0.017	-0.013	-0.036	0.040	0.004	-0.021
	(0.202)	(0.608)	(0.625)	(0.961)	(0.493)	(0.641)	(0.116)	(0.171)	(0.870)	(0.374)
Second-term mayor	-0.01	-0.005	0.023	0.01	0.039	0.063	0.024	-0.01	-0.008	-0.06
	(0.685)	(0.849	(0.374	(0.706	(0.243	(0.241	(0.579	(0.825)	(0.817)	(0.129)
Observations	5,139	4,672	4,676	4,193	3,043	1,740	2,222	1,361	2,981	2,862
Bandwidth	826	779	827	777	595	570	746	497	1,005	1,005
Mean	0.75	0.77	0.77	0.77	0.75	0.75	0.75	0.76	0.75	0.75

Notes: as in table D.9.

Table D.11 Main results - False 2010 treatment

Outcome	(1) Capital Spending per capita	(2) Capital Revenue per capita	(3) Loans revenue per capita	(4) Loans repayments per capita	(5) Public housing per capita	(6) Transport per capita	(7) Current spending efficiency	(8) Revenue collection efficiency
First-term mayor	78.460	-13.595	62.135	82.758***	6.675	-74.460*	-0.016	-0.071*
	(0.735)	(0.926)	(0.256)	(0.006)	(0.774)	(0.082)	(0.487)	(0.087)
Second-term mayor	143.689	-22.498	104.78	2.212	0.774	43.225	.017	.016
	(0.453)	(0.898)	(0.236)	(0.978)	(0.942)	(0.523)	(0.452)	(0.674)
Observations	968	866	927	1,249	1,307	1,285	1,031	1,196
Bandwidth	548	502	534	693	722	713	594	672
Mean	460	309	76	69	20	167	1	1

Notes: as in table D.9.

Table D.12 Main results - Excluding 2014

Outcome	(1) Capital Spending per capita	(2) Capital Revenue per capita	(3) Loans revenue per capita	(4) Loans repayments per capita	(5) Public housing per capita	(6) Transport per capita	(7) Current spending efficiency	(8) Revenue collection efficiency
First-term mayor	281.458**	252.422**	82.490	59.512	36.162**	22.603	0.011	0.025
	(0.043)	(0.044)	(0.202)	(0.227)	(0.022)	(0.508)	(0.640)	(0.544)
Second-term mayor	124.964	35.008	231.337**	190.745***	-28.38***	170.121	-.061	-0.098*
	(0.688)	(0.903)	(0.017)	(0.004)	(0.004)	(0.153)	(0.124)	(0.086)
Observations	2,225	2,431	1,910	1,919	2,516	1,736	2,101	1,910
Bandwidth	642	691	551	560	717	510	614	552
Mean	410	300	103	89	11	137	1	1

Notes: All variables are expressed in per capita terms. Municipality level cluster robust p-values in parentheses: *** p<0.01, ** p<0.05, * p<0.1. The chosen bandwidth is computed as the average of the two bandwidths selected with the procedure developed by Calonico et al. (2014a) and Calonico et al. (2014b) on each sample obtained by splitting between observations dated before and after 2015. The observations refers to the number of observations within the bandwidth. The mean refers to the average pre-treatment value of the outcome variable for *treated* municipalities.