Descriptive Representation and Political Participation Among Immigrant Voters: Evidence from France

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Abstract

Immigrant citizens tend to participate less and be less represented in formal politics than natives. Yet, recent years have seen a significant rise in the number of immigrants holding elected office in France and elsewhere. In this thesis, I explore whether this increase in the extent to which the demographic characteristics of elected assemblies mirror those of the population – *descriptive representation* – has a broader effect on immigrants' political participation. To do so, I engage in a large-scale data collection process allowing me to identify immigrant candidates as well as to track turnout levels and immigrants' spatial distribution at a fine level. I then rely on two distinct regression discontinuity designs to estimate the local average treatment effect of the election of an immigrant candidate on immigrants' turnout. Ultimately, I find that descriptive representation does not appear to have a significant positive effect on turnout in subsequent elections.

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1 Introduction

Far from the democratic ideal of equal participation and representation of all citizens, numerous groups have historically remained at the margins of formal politics (Young 2002). Women's marginalization, for a long time enshrined in law, is a particularly striking illustration of this phenomenon. Today, as Western democracies witness a significant rise in inbound migration flows and ethnic diversity[1], I argue that immigrants' under-representation and under-participation in formal politics manifest similarly as a democratic deficit.[2] In France, first and second-generation immigrants represent about 21% of the population (Insee Références 2023), yet they comprise only 4.73% of the National Assembly's composition (Keslassy 2022). Immigrants are also less often registered as voters and vote less than natives (Insee Premières 2012) despite exhibiting similar levels of interest in politics (Tiberj and Simon 2021). The United Kingdom, among others, reveals a similar pattern: only 10.1% of the 650 MPs elected in the 2019 General Elections are from a minority ethnic background, against 16% in the general population (Uberoi and Carthew 2023). Likewise, in the last three elections, minorities' registration and turnout rates were 10 to 15pp lower than those of people from white ethnic groups (Uberoi and Johnston 2022).[3]

Immigrants' political marginalization matters first for the strength and resilience of contemporary political systems. In representative democracies, institutions gain legitimacy in part by representing the population and enacting its preferences (Urbinati 2006). Similarly, representatives' accountability is tied to the expectation that citizens will punish representatives who deviate from their will (Healy and Malhotra 2013). On an even more general level, lively democracies with high civic engagement and strong institutions serve as public goods providers. They provide avenues for peaceful expression of discontent and resolution of political conflict, they aggregate diverging

[1]In France, the proportion of first-generation immigrants has risen from 7.4% in 1975 to 10.3% in 2021, an all-time high in the country's history (Insee 2023e). In England and Wales, where a question on individuals' ethnicity was first included in the Census in 1991, the share of ethnic minorities has risen from 5.35% to 18.3% from then to 2021 (Office for National Statistics 2022). A similar pattern emerges in most European countries (Eurostat 2023).

[2]Throughout the rest of this thesis, I use "immigrants" or "immigrants and immigrant-origin individuals" interchangeably, referring to both first- and second-generation immigrants, with the natural caveat that immigrants can only participate in formal politics once they acquire their host country's citizenship.

[3]Categorizations and definitions of "ethnic background" or "immigrant origin" slightly differ between France and the United Kingdom, but the general argument remains valid.

interests, and they foster a shared identity (Putnam et al. 1993). In this context, the fact that some segments of the population are less involved in their country's political life and suffer from a lack of representation weakens the social contract and undermines the foundations of democratic governance (Chami et al. 2021, p. 113). Further, without claiming that descriptive representation is a panacea, some groups' lack of self-representation erodes their trust in the fairness and legitimacy of democratic institutions, and prevents those institutions from truly representing the population (Williams 2000). Second, it matters for immigrants themselves. Beyond the inherent value of political engagement (Fowler and Kam 2007), increased voting participation would likely enhance their ability to defend their interests and ensure that they are taken into account (Graauw and Vermeulen 2016).[4] Similarly, a greater presence of immigrant representatives may serve as a symbolic and empowering expression of immigrants' legitimacy in the political realm (Banducci et al. 2004; Mansbridge 1999), and lead to greater substantive representation of their interests (Sobolewska et al. 2018). In contrast, the prevailing lack of diversity of Western legislatures arguably sends a message of exclusion (Bloemraad and Schönwälder 2013), while low levels of participation may foster a self-reinforcing cycle of disempowerment (Fowler and Kam 2007). In sum, immigrants' under-participation and under-representation in formal politics stands as an empirical reality that has important theoretical implications.

Despite this general trend, French politics have recently grown increasingly diverse, with a rising number of immigrant-origin citizens holding elected or appointed positions. The *Institut Diderot* (2022), a French think tank, for instance estimated that 6.18% and 4.73% of the National Assembly's MPs elected in the 2017 and 2022 elections respectively were part of a "visible minority". While this value remains lower than the share of immigrants in the population (Insee 2023e), it constitutes a significant evolution. Indeed, only 0.54% of MPs elected in 2007 had a minority background (Keslassy 2022). I argue that these changes call for greater attention to the dynamics of representation and participation among immigrant citizens. In this perspective, in the present thesis, I precisely seek to explore whether these cases, where an immigrant candidate becomes an elected representative, have a broader positive impact on immigrants' political participation as voters. Theoretically, such an effect could stem from two main mechanisms. First, it may be

[4] This reflects the political consequences of women's suffrage, which for instance led to increases in public health spending as women placed greater attention to questions related to child welfare than men (Miller 2008).

that these representatives are able to represent immigrant voters substantially better than their non-immigrant counterparts (Mansbridge 1999). This could for instance be the case if immigrant representatives have a better understanding of immigrants' political preferences and/or if they are more likely to defend their interests (Saalfeld and Bischof 2013). Then, this *substantive* representation could potentially contribute to reducing immigrants' political marginalization, showing them that they can influence political decisions, and plausibly increasing their propensity to vote (Poertner 2023). Second, it could be that the mere presence of immigrant representatives – *descriptive* representation – serves as symbolic and empowering examples of immigrants' legitimacy in the political realm, thereby ultimately increasing their political engagement (Bobo and Gilliam 1990) In practice, disentangling one effect from the other is challenging, as they are likely to be intertwined. In particular, it could very well be that descriptive representation contributes to substantive representation, and that the latter is what ultimately leads to increased political participation (Lowande et al. 2019). Nonetheless, distinguishing the two effects is precisely what I ambition in the present thesis, as I ask whether descriptive representation has a causal effect on immigrants' turnout in subsequent elections.

I argue that France offers a particularly rich setting to explore this question. France is known for its strong Republican tradition, which has sought to minimize the influence of individual ethnic, religious and racial identities in the political realm, to instead promote the ideal of a shared French identity surpassing particular ones (Escafré-Dublet et al. 2023). This stands in relation to France's long-standing emphasis on immigrants' assimilation into French society, in contrast to a perhaps more Anglo-Saxon appraisal of multiculturalism (Rodríguez-García 2010). These cultural specificities have numerous practical consequences. France is for instance known for prohibiting (or strictly restricting) the collection of statistics related to individuals' religion or self-reported ethnicity (Simon 2015). In the political sphere, France's "colorblindness" implies that elected representatives are largely expected not to defend any particular interest linked to their individual identities, but instead to *"collectively care for the will and the interest of the nation"* (Tiberj and Michon 2013). Political parties do not have access to information on voters' ethnic, racial or religious identities and voters cannot be mobilized along these lines (Maxwell 2010). Likewise, candidates' use of their ethno-racial identity as part of their political platform remains largely unthinkable, particularly if these represent "minority" identities (Bird 2005). In a nutshell, French political life is to a large degree "de-ethnicized". This means that the election of an immigrant candidate is for instance unlikely to be publicly portrayed as a victory for the immigrant community, and that immigrant candidates are unlikely to emphasize their immigrant identity. This is important as the effect of descriptive representation on minority voters' political engagement is likely to be mediated by the salience of immigrant candidates' identities and the extent to which minority voters identify with them in that regard. Indeed, one could argue that the effect of descriptive representation is largely a "signaling" effect, where the strength of the signal perceived by immigrant voters is partly a function of the salience of representatives' identity.[5] As a result, I argue that France represents somewhat of a "least-likely case", and that finding a positive effect of descriptive representation on voters' political participation in France would suggest that this effect might generalize to other countries.

To explore whether this is the case, I engaged in a significant data collection effort. I was able to track around 10000 municipal election candidates' origins, as well as to measure turnout levels and the share of immigrants at the level of the voting office, particularly small units containing about 800 voters. Based on this novel dataset, I implement two distinct close-elections regression discontinuity designs to estimate the causal effect of descriptive representation. Ultimately, I find relatively surprising results, which I contend largely support the idea that descriptive representation *does not* have a significant positive effect on immigrants' turnout in subsequent elections. This result appears robust to several sensitivity checks and changes in the model specification.

The rest of this thesis proceeds as follows. In the next section (2), I develop a more general conceptual framework linking descriptive representation to political participation. I first review the literature before briefly describing the present setting and the general methodological strategy. In section 3, I provide some background information, discussing French elections as well as the conditions under which immigrants and immigrant-origin individuals can obtain French citizenship and vote. Then, in section 4, I present the data I collected and detail my approach to operationalizing the main variables of interest. Section 5 allows me to discuss the empirical framework, by presenting the two main identification strategies I rely on. Finally, I present the results of these

[5] In the most extreme scenario, immigrant voters do not even know that a given representative is also an immigrant, in which case we cannot expect descriptive representation to have any effect.

analyses in section 6, before discussing them in greater detail in section 7.

2 Conceptual framework

What does the literature tell us about the effect of descriptive representation, understood as the extent to which the demographic characteristics of elected representatives mirror those of the population (Mansbridge 1999), on minority voters' political engagement? Further, which theoretical mechanisms are likely to come into play?

A number of studies have explored these questions. In a seminal paper, Bobo and Gilliam (1990) suggest that in municipalities where Blacks hold positions of power in local government, Black voters exhibit higher levels of socio-political participation (voting, campaigning, community engagement, etc.). They argue that the presence of Black representatives acts as cues of policy responsiveness, leading to increased interest and political knowledge among Black voters. Noteworthily, this argument ties descriptive representation with the expectation of greater substantive representation, which is then expected to increase engagement. Whitby (2007), Rocha et al. (2010) and Hayes et al. (2022) all find similar relations, studying African American and/or Latino voters in the United States. Geese (2022) presents a cross-national analysis spanning 11 European countries, and argues in favor of a general "empowerment effect", finding that the turnout gap between immigrant-origin and native voters is lower in countries with greater descriptive representation.

Several scholars have then sought to explore more carefully the potential mechanisms underpinning this relation. Banducci et al. (2004) defend that the presence of co-ethnic representatives is associated with increased political knowledge and more positive evaluations of governmental responsiveness among minority voters. Wolak and Juenke (2021) nuance this claim, arguing that co-ethnic representation only leads to increased knowledge about elected officials but not general political knowledge. Pantoja and Segura (2003) contend that Latinos' descriptive representation is associated with lower levels of "political alienation", understood as the belief that officials violate long-standing norms or rules to serve narrow interests. Griffin and Keane (2006) also study minorities' political alienation, arguing that *liberal* African Americans are more likely to vote when descriptively represented but that the effect is negative for *conservative* African Americans. They explain that as African American representatives tend to be more liberal than conservatives, greater descriptive representation tends to lead to a perceived increase in substantive representation among liberals but not conservatives, which then affects political participation. Rosenthal et al. (2018) also defend that descriptive representation leads to increased turnout only when voters expect it to lead to increased substantive representation.

All in all, most studies tend to suggest that descriptive representation contributes to boosting minority voters' political engagement. However, I argue that most of them exhibit important methodological limitations, which raise doubt about the validity of their conclusions. Most importantly, they generally fail to distinguish between cause and effect, leading authors to make claims that go beyond the results of their correlational analyses. Indeed, generally, the authors find that in areas where descriptive representation is higher, voting and socio-political participation among minorities is higher, and they then conclude that the former has an effect on the latter. However, this overlooks significant issues of endogeneity. Simply put, it could very well be that it is instead minorities' higher levels of socio-political participation that leads to increased descriptive representation, as minorities vote and contribute to electing minority candidates. In fact, this hypothesis is supported by the vast literature evidencing voters' tendency to support co-ethnic candidates (Barreto 2007; Heath et al. 2015; Lublin and Wright 2024). It could also be that there is a third unobserved variable such as the presence of a strong ethnic-based community organization that leads to both increased descriptive representation and political participation (Wong 2008).

A limited number of recent studies have acknowledged this issue more directly and have approached these questions with greater methodological care, often nuancing previous findings. Henderson et al. (2016) for instance exploit legislative redistricting and the associated change in electoral districts affecting some voters but not others. Highlighting the limited external validity of their findings, they nonetheless suggest that in California, the effect of being moved to a district with a Hispanic incumbent has little effect on Hispanic voters' participation in subsequent elections. Fraga (2016) extends the analysis to Black and Asian voters in 10 US states, and argues that the effect of being moved to a minority-majority district with a co-ethnic incumbent varies by ethnic group. He finds that African Americans moved to majority-black districts with black incumbents are more likely to vote than "non-moved" Black voters, but that this effect does not generalize to Asian or Latino voters. Finally, Scherer and Curry (2010) show that positively manipulating the number of African American judges in federal courts in an experimental setting leads to an increase in the perceived legitimacy of the institution among African Americans.

In sum, evidence for the effect of descriptive representation has been mixed, and at times conflicting. Still, these studies highlight, albeit with varying methodological rigor, several plausible theoretical mechanisms. Descriptive representation is for instance expected to lead to an increased interest in political life (Bobo and Gilliam 1990), greater political knowledge (Banducci et al. 2004; Wolak and Juenke 2021), or decreased political alienation (Pantoja and Segura 2003). It could also increase the perceived legitimacy of the institutions (Scherer and Curry 2010), or act as a signal of possible greater substantive representation and policy responsiveness on the part of the elected officials (Bobo and Gilliam 1990; Griffin and Keane 2006; Rosenthal et al. 2018). Noteworthily, with rare exceptions (Geese 2022; Rosenthal et al. 2018), these studies largely focus on the American case. However, the political culture around minorities and minorities' (political) integration arguably differs quite widely between the United States and most Western democracies, including France. As evoked in the introduction, France is known for its color-blind Republican tradition that has sought to minimize the influence of individual ethnic, racial or religious identities in political life. In contrast, in the United States, race and ethnicity are arguably central to political affairs (Hutchings and Valentino 2004). This distinction is crucial as most of the theoretical mechanisms described here are tied to voters' identification with the elected officials, generally linked to co-ethnicity. In other words, the salience of race and ethnicity in the political sphere is likely to be an important determinant of the effect of descriptive representation. As a result, the degree to which the findings presented here apply to the French – and more broadly, European – context remains unclear. In this thesis, I thus aim to provide a methodologically rigorous contribution to the literature, exploring the effect of descriptive representation on immigrant-origin voters' political participation in France.

To do so, I rely mainly on a regression discontinuity design, in which I exploit the succession of *unrelated* elections to estimate the effect of the election of an immigrant candidate in a close election on immigrants' turnout in the following election. Schematically, this involves comparing turnout at election $t + 1$ between two electoral units, one in which an immigrant candidate won by a small margin to another in which such a candidate lost by a small margin.

I use French municipal elections as the main setting to determine whether a given electoral unit gets "treated" by the election of an immigrant candidate. Municipal elections are particularly appealing for several reasons. First, the literature has shown that voters were on average more attached to and more knowledgeable about their mayors than to other elected representatives (Foucault 2019). This is important for reasons already detailed, namely that knowledge of and identification with the elected representative is likely a key mediator of the effect of descriptive representation. In practice, this suggests that the effect of descriptive representation is probably greater for municipal elections than for other elections. Second, there are vastly more municipalities than other electoral units (e.g. *départements* or legislative constituencies).[6] This mechanically increases the number of immigrant candidates and representatives, which in turn, increases the statistical power of my analyses, allowing me to recover a lower minimum detectable effect size. The last major advantage of these elections relates to the electoral rules that govern them. French municipal elections serve to elect the municipal council, which will then elect the municipality's mayor. Each running party presents a ranked list of candidates and the first candidate of each list is expected to become the mayor if the list wins. Voters vote for one of these lists, and if one of them obtains more than 50% of the votes in the first round, it is elected. If this is not the case, a second round is organized, where only the lists that obtained more than 10% of the votes in the first round can run. After the election, the seats on the municipal council are distributed based on a majoritarian system that gives a strong premium to the highest-ranked list (Cassette et al. 2013). Indeed, half of the seats are attributed to the list that obtained the most votes, while the other half is distributed proportionately across all competing lists, *including the highest-ranked one*. As Schmutz and Verdugo (2023) point out, this system *"generates a large discontinuity between vote shares and the share of seats in the municipal council"*. In practice, this means that the first candidate on the winning list is bound to become the mayor elected by the municipal council, even if this list obtained just a few more votes than the second one. This offers a particularly appealing setting for a close-elections regression discontinuity design, as I implement in the present thesis.

Then, I choose to measure turnout in the regional elections. These elections have the important advantage of closely following municipal elections: in my sample, they took place between

[6] There are about 35 000 municipalities in France (Insee 2020), but the electoral rules differ between municipalities with fewer vs. more than 1000 inhabitants. I only include the latter in my analysis, but the total number of municipalities with 1000 inhabitants or more remains significantly larger than the number of legislative constituencies for instance.

one and two years later at most. This is crucial as it aligns with my objective of isolating the *signaling* effect of the election of an immigrant candidate, distinguishing it from the effect of substantive representation. In this logic, I suggest that the short time span between the two elections does not give the mayor the time to represent voters substantially better, which keeps the effect of substantive representation to a minimum. Further, the fact that these two elections are largely unrelated to one another limits the risk that the dynamics of political representation at the municipality level have a significant effect in regional elections. Finally, regional elections are known to be relatively scarcely attended, which means that if descriptive representation had an effect, the margin for it to be detected is greater in this setting.[7] In the next section, I provide some additional background information on elections and citizenship in France, before presenting the data used to test my hypothesis.

[7] Over the last three elections, the average turnout rates in the first and second rounds were 43.17% and 51.43% respectively (Ministère de l'Intérieur 2024).

3 Background

Immigration & citizenship in France Access to French citizenship is based on a combination of birthright citizenship (*jus soli*) and "right of blood" (*jus sanguini*). All immigrants may obtain French citizenship after five years of legal residency in France, but there are a number of conditions under which one may apply for citizenship earlier (e.g. being married to a French citizen, having studied in France for more than two years). To obtain citizenship in that way, one undergoes an individual interview with a civil servant, where applicants take a test measuring their command of the French language, their knowledge of French history and culture, and their adherence to French values (République française 2024c). For public statistics purposes, immigrants who have obtained French citizenship remain counted as immigrants (Insee Références 2023). Children of French citizens are granted French citizenship at birth, irrespective of their place of birth and their second parent's nationality (République française 2022). Children born on French territory of non-French parents can acquire French citizenship at the age of 18 if they have lived in France for five years or more (continuously or not) since their 11th birthday, but they may also obtain it sooner if their parents request it (République française 2023). All French citizens, irrespective of their immigrant status, enjoy full civil and political rights, including the right to vote and to run for office (République française 1958).

Elections in France Voting takes place on Sundays, and registered voters go vote in the voting office closest to their place of residence. All French citizens aged 18 or older are allowed to vote, but they have to be registered on the electoral rolls to do so. Unregistered voters who wish to vote in any given election have to register approximately 6 weeks before the ballot, and registration is then permanent (République française 2024b). To combat abstention, the French government now automatically registers all citizens when they reach 18 years old as well as all individuals who acquire French citizenship (République française 2024a). As of today, 95% of the population eligible to vote is registered on the electoral rolls (Insee Focus 2022). Both the municipal and regional elections take place every 6 years, generally in March.[8] They are both held at the universal suffrage with a list-based system and a proportional system. As described in the previous section, municipal elections also include a majoritarian bonus. Finally, these two elections generally include two rounds, organized one week apart.

[8] The 2021 regional elections were postponed to June as a result of the Covid-19 pandemic.

4 Data

Estimating the effect of descriptive representation on immigrant voters' turnout involves operationalizing two main variables: candidates' origins, and immigrants' turnout. I cover these two variables separately.

4.1 Candidates' origins

I choose to proxy candidates' origins using their place of birth, considering that an individual born in a given country is a national of that country. This is a common practice in large-scale operationalizations of individuals' origins (Bloemraad and Schönwälder 2013). Importantly, I focus specifically on extra-European immigrants and their descendants, assuming that they are the most affected by the political marginalization discussed in the introduction. I cover both the first- and second-generation, which means that I consider an individual to be an immigrant if they were born outside the 27 European Union countries as well as the United Kingdom, Canada and the United States, or if at least one of their parents was themselves an immigrant, per the same definition. Covering both the first- and second-generation is in line with the Insee's definition of immigrants.[9] It is also a quite significant improvement vis-à-vis the existing literature on descriptive representation, which generally fails to go beyond the first generation (Bloemraad 2013). Finally, I consider that individuals born in a French colony *before its independence* were born in France and are thus French.[10] As I employ different approaches for the two generations, I treat them separately before summing up, presenting descriptive statistics, and providing an example.

[9]To be precise, the Insee defines a first-generation immigrant as a *"person born without French citizenship outside of France"* (Insee 2023b) and a second-generation immigrant as *"a person born in France who has at least one immigrant parent"* (Insee 2023c).

[10]In practice, this approach is probably over-excluding, in the sense that there are a number of individuals living in French colonies without French citizenship and born before the independence who chose to migrate to France and who do not get counted as such. However, I did not find an alternative approach, and I prefer under- rather than over-classifying individuals as immigrants as the latter would likely dilute the estimated effect of descriptive representation.

4.1.1 First-generation

I was able to identify the first-generation immigrant status of close to all candidates in the 2014 and 2020 municipal elections, and of a portion of the candidates in the 2008 municipal elections. To do so, I engaged in a large-scale data collection process, based on two main insights. First, that to be a political candidate, one first has to be registered as a voter on the electoral rolls (*listes électorales*). Second, that although it is known by relatively few, these rolls are openly communicable. Indeed, the French Electoral Code (2019) states that they constitute public documents, and that any voter who agrees not to make commercial use of them can access them. They are, however, not centralized, and each *préfecture* stores the rolls for their *département* of jurisdiction.[11] I thus contacted the *préfectures* of all 101 French *départements* separately, and I was ultimately able to obtain the 2022 electoral rolls from 99 of them.[12] In total, these rolls cover 47M individuals, about 95% of the population aged 18 or above (Insee Focus 2022). They contain information on individuals' first name, birth name, last name, sex, date of birth, place of birth, and personal address. This is an exceptionally rich resource, which I further discuss in the next section.

Having obtained these electoral rolls, I collected two additional data sources. First, I reasoned that individuals who once ran for office to become their municipality's mayor were probably strongly politically committed, and may thus have been or currently be holding some other elected office today. I thus collected the *Répertoire national des élus (RNE)* from the Ministry of Interior as well as its archived versions from *Regards Citoyens* (2024). The RNE is published every year and contains information on all individuals currently holding an elected political mandate in France (as mayors, MPs, in local government, etc.). It contains individuals' first and last names, sex, date of birth, and place of birth.[13] Second, I collected the *Fichier des personnes décédées* (Register of Deceased Persons – RDP) from the French National Institute of Statistics and Economic Studies (Insee). This dataset contains information on all individuals who have died in France or with

[11]French *départements* are medium-sized administrative units, comprising on average 600 000 individuals. Each *département* has a *préfecture* which serves as the representation of the central government.

[12]The two guilty *préfectures* are the *préfectures* of the Oise and Puy-de-Dôme *départements*, who negatively responded to all my requests, as well as the two decisions of the *Commission d'accès aux documents administratifs* (CADA) in my favor. I do not thank them.

[13]For the full story, the dataset published by the Ministry of Interior used not to include individuals' place of birth, but after I filed a request before the *Commission d'accès aux documents administratifs*, the Ministry of Interior agreed to add the variable to the dataset published online. Long live the rule of law!

French citizenship since 1970 (about 28M individuals at the time of writing), and it also includes individuals' first and last names, sex, date of birth, and place of birth.

I then relied on `fastLink`, Enamorado and colleagues' (2019) probabilistic record linkage algorithm to match the list of municipal election candidates with these three datasets to ultimately recover their place of birth. For the 2014 and 2020 elections, this proved relatively easy as the list of candidates published by the Ministry of Interior includes information on individuals' date of birth in addition to their first and last names. Thus, for these two elections, I combined exact-matching on individuals' sex and birthdate and fuzzy-matching on individuals' first and last names.[14] The 2008 municipal election proved more complex, as the published dataset does not include individuals' birthdates. Thus, I began by performing an exact join between the list of candidates and the RNE based only on individuals' first and last names. This allowed me to recover approximately 800 candidates' birthdates. Then, I used this birthdate to implement the same record linkage approach as for the 2014 and 2020 elections.

Ultimately, I was able to track 9674 candidates' place of birth out of 10057 candidates across the 2008, 2014, and 2020 elections. For the remaining cases, I proceeded conservatively and simply coded them as being born in France. Recalling that I consider first-generation immigrants the individuals whose place of birth is outside the European Union, UK, Canada and the US *and* who were not born in a French colony *before* its independence, this revealed that 4.75% of the candidates presented in the second-round of a municipal election were first-generation immigrants.[15] I present this information in greater detail in Table 1.

4.1.2 Second-generation

In the absence of data on candidates' parents' names, I was not able to implement the same strategy to track candidates' second-generation immigrant status. To fend off this limitation, I

[14] In its simplest form, fuzzy-string matching involves computing the so-called "edit distance between a pair of strings, such as Kreitmann and Kreitman. This distance corresponds to the number of operations needed to transform one string into the other. Then, the lower the edit distance, the higher the probability that the two strings are the same. Here, I rely on the Jaro-Winkler measure to compute the distance (Winkler 1990).

[15] I consider that the missing values for the 2008 elections are missing at random, and I compute the share of first-generation immigrants based on the number of candidates for whom I recovered the place of birth.

developed an alternative, probabilistic approach. While imperfect, I believe it is a more robust strategy than most other approaches presented in the literature (Portmann and Stojanović 2019). I began by constructing a unique dataset covering about 75M individuals, combining the electoral rolls (∼ 47M) and the Register of Deceased Persons (∼ 28M). As mentioned above, the rolls cover 95% of the population aged 18 or above at the time of data collection (May-June 2023) and the Register of Deceased Persons (RDP) includes all individuals who died in France and/or with French citizenship since 1970. Naturally, one cannot be included in both datasets. I use this unique dataset to estimate, for each candidate, the proportion of individuals from the previous generation who held/hold the same last name and who were first-generation immigrants; this yields a probability that each candidate is a second-generation immigrant. To deal with the fact that some individuals (especially women) change last names when they get married, I instead use candidates' birth names when available (i.e. *via* the electoral rolls). Further, when individuals hold a "composed" last name (such as "Dupont-Durand"), I consider the two parts of the name separately, as such last names are often the product of a marriage between two individuals holding different last names, who decide to combine them. To characterize "the previous generation", I rely on historical birth data from the French Institute for Demographic Studies (Ined 2024). For each calendar year, I create an age range encompassing 95% of mothers' age at the birth of their child. For instance, in 95% of the births that occurred in 1990, mothers were aged between 20 and 38. Thus, for all candidates born in 1990, I consider the individuals born between 1952 and 1970 and holding the same last name as possibly being their parents. Then, I compute the proportion of them who were first-generation immigrants (based on their place of birth), which gives the probability that each candidate is a second-generation immigrant. For individuals holding a composed last name, I perform the same procedure separately for each "root" last name, and I consider a candidate holding this "composed" name as a second-generation immigrant if one of the "root" last names is held by more first-generation immigrants than natives.[16]

[16]One problem with this approach is that despite valuable help from Magali Mazuy (who I thank!), I was not able to find data on fathers' age at the birth of their child, whereas we know that the ages at which men and women become parents differ (Insee 2016). In practice, the effect of this lack of data is probably limited, but warrants being mentioned. More consequentially perhaps is the fact that, by definition, the electoral rolls only include French citizens. This means that all first-generation immigrants who never acquired French citizenship and went back to their host country before their death are not included in either dataset. As such, I am probably *under-estimating* the probability that each candidate is a second-generation immigrant.

4.1.3 Summing up

Summing up, I consider any candidate to be a first-generation immigrant if they were born outside the 27 European Union countries (as well as the UK, Canada and the US), and a second-generation immigrant if more than half of the individuals holding the same last name in the previous generation were themselves first-generation immigrants (per the same definition). This operationalizes the two generations and is in line with the Insee's definition. Before presenting general statistics on the number of immigrant candidates, I provide an example.

One can consider the case of Youssef Lamrini, who ran for office in the 2020 municipal elections in the city of Vernouillet (Eure-et-Loir), and who ended up losing by a margin of 4.66 percentage points. The list of candidates published by the Ministry of Interior reports that M. Lamrini was born on 19/09/1983. Indeed, the record linkage algorithm allowed me to find him in the electoral rolls, which report that he was born in the city of Dreux, in Eure-et-Loir. Thus, M. Lamrini is *not* a first-generation immigrant. Turning to the second generation, historical birth data from the INED indicate that in 95% of the births that occurred in 1983, mothers were aged between 19 and 37 years old. Then, it appears that, in the compiled dataset (electoral rolls and RDP), 28 individuals hold/held the last name Lamrini and were born between 19/09/1946 and 19/09/1964. Out of them, 27 were first-generation immigrants: 1 was born in France, 5 were born in Algeria, and 22 were born in Morocco. Thus, the probability that M. Youssef Lamrini is a second-generation immigrant is $27/28 = 96.43 > 50\%$, and he thus gets coded as such. Combining the two generations, this means that M. Lamrini is an immigrant candidate.

I now print a table detailing the number of immigrant candidates in each of the 2008, 2014 and 2020 municipal elections.

4.2 Immigrant voters' turnout rates

The absolute "gold standard" approach to measuring immigrant voters' turnout rates would involve obtaining individual voting records. However, France has consistently held a strict interpretation of statistical confidentiality, and such data is not available. In its absence, I take advantage of the fact that French electoral results are published at both the levels of the municipality and the voting office. The latter is especially appealing as voting offices are particularly small units of analysis,

Table 1: Distribution of candidates' immigrant-origin status by year

	2008 (∗)	2014	2020	Total
Total number of candidates	1335	4775	3947	10057
1st gen. IO candidates	18	137	112	267
2nd gen. IO candidates	24	109	134	267
Total IO candidates	41	216	221	478

Note: The total number of IO candidates is not equal to the sum of the numbers of first- and second-generation immigrants, as a candidate can be coded as both a first- and second-generation immigrant. For the 2008 municipal elections, I report the number of candidates whose origins I was able to identify. It differs considerably from the total number of candidates, for reasons detailed above. For this election, I consider that the missing candidates are missing at random, and I compute shares based on the number of candidates for whom I have information.

containing on average 800 voters, significantly fewer than most of what the literature has relied on (Zonszein and Grossman 2023).[17] Naturally, this does not preclude concerns linked to ecological inference (Shively 1969), but it still limits them. I then use the 2022 electoral rolls presented above, which cover 95% of the population aged 18 or above, to estimate the share of immigrants at the levels of the voting office and municipality. The rolls include information on individuals' country of birth, and I thus rely on the same approach as presented above to classify individuals' origins: I consider them as first-generation immigrants if they were born in a non-EU country (plus Canada, the UK and the US) and otherwise as natives. To match each individual to a voting office and a municipality, I began by cleaning and geocoding all the addresses included in the electoral rolls (about 20M addresses). I thus obtained geographical coordinates for all individuals' places of residence. Then, I performed a spatial join with a dataset published by data.gouv.fr (2024) which contains the polygons of all voting offices and matches each voting office to a municipality. For missing or problematic cases, I then relied on the cleaned address from the rolls to perform fuzzy-string matching with a dataset published by the Insee (2023a) which associates each address (in text format) to a voting office and a municipality. Combining these two approaches, I was ultimately able to match 99.87% of the individuals included in the electoral rolls to a voting office

[17] In addition, it allows us to bypass the numerous constraints linked to statistical confidentiality imposed by the Insee which usually prevent researchers from tracking immigrants' spatial distribution at a fine level. The smallest geographical unit at which data is available is the IRIS (*ilôts regroupés pour l'information statistique*) but only for highly-populated IRIS, with limited disaggregation based on individuals' origins, and only available through the CASD. Voting offices are about twice as small as IRIS (Insee 2016), and the electoral rolls include significantly more detailed information.

and a municipality.[18] Having done so, I simply divided the number of immigrant voters by the total number of voters in each electoral unit (voting office and municipality) to estimate the share of immigrant voters at this level.

While I believe that this represents a significant improvement compared to most of the literature (notably with regard to the scale of disaggregation), two limitations appear important to highlight. First, my approach to estimating the share of immigrants at the local level is based on individuals' place of birth. This means that I am unable to include second-generation immigrants. This is somewhat of a problem given that they represent half of the total number of immigrants in France (Insee Références 2023). Nonetheless, given that the shares of first- and second-generation immigrants at the local level are most likely highly correlated, this is arguably only a limited concern. Second, and significantly more important is the fact that the electoral rolls which I have access to are only cross-sectional. In that sense, they offer a snapshot of the distribution of immigrants across the French territory in 2022 but not before. This is a significant concern as it means that I use data from 2022 to estimate the share of immigrants at the local level up to 2008, 14 years earlier. Rates of internal migration in France are about 6% per year (Insee 2023d), which means that immigrants' spatial distribution across the French territory may have changed quite significantly over the period covered. I am unable to account for it in my analysis, which is a significant limitation. This is perhaps particularly problematic given that the election of an immigrant candidate in a given municipality might itself affect the proportion of immigrants in that municipality. Indeed, relatedly, Schmutz and Verdugo (2023) for instance show that the election of a left-wing mayor increases the proportion of immigrants at the municipality level, mainly as a result of an increase in the number of social dwellings. Overcoming this (significant) limitation would require accessing the individual voting records (*listes d'émargement*) mentioned above. However, they are exhaustively archived only for presidential elections, they are stored only in departmental archives and in paper-version, and accessing them requires obtaining a special nominative authorization granted by each departmental archive.[19]

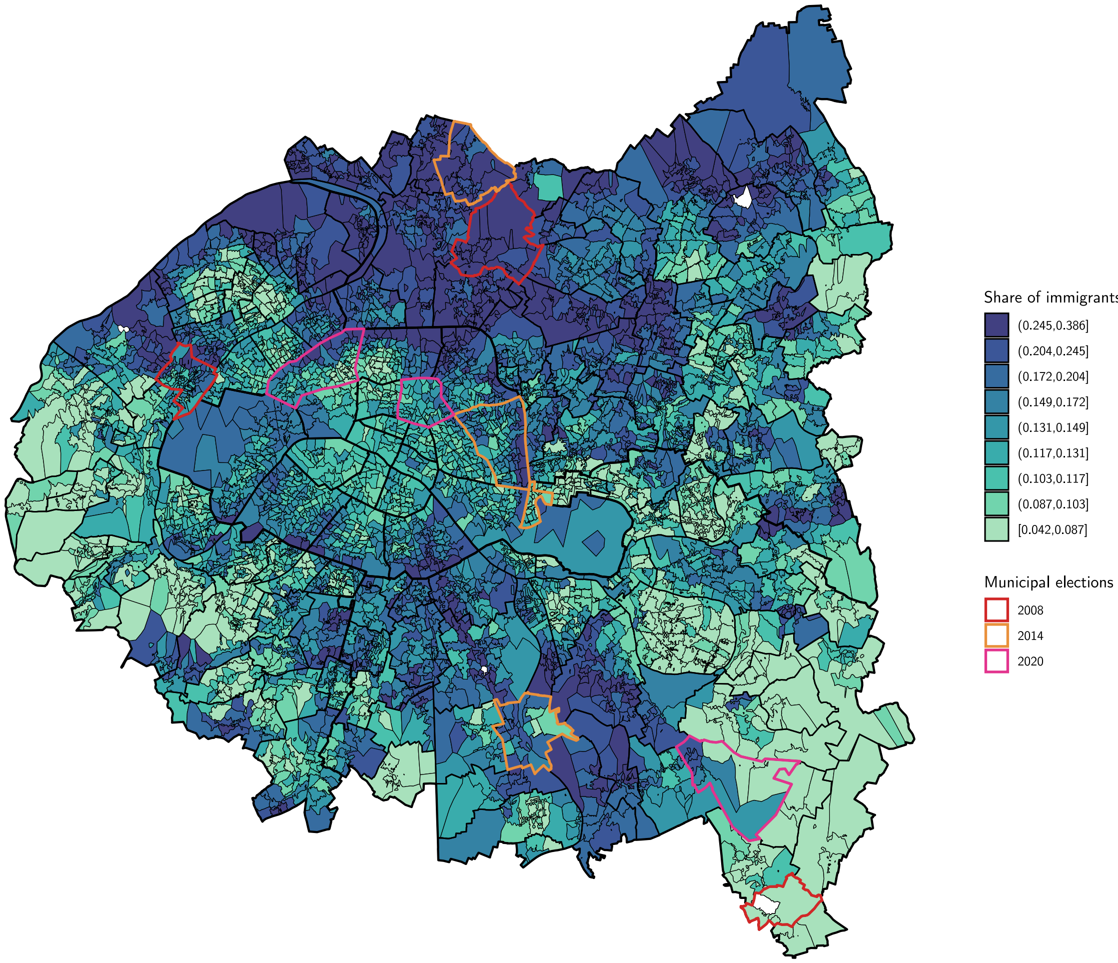
[18]Excluding the cases of the Oise and Puy-de-Dôme *départements*, for reasons beyond my control. The remaining non-matched individuals mainly come from French overseas territories, in particular French Polynesia where addresses are generally not standardized.

[19]The richness of this data might justify overcoming these constraints, but I would probably need *some* funding and *some* help to do so. Feel free to reach out if you have any.

4.3 Constructing the final datasets

Based on this operationalization, I ultimately construct two datasets: one at the level of the voting office, and one at the level of the municipality. To do so, I first match information on each candidate's origins to their electoral results at the second round of each municipal election (2008, 2014, 2020). I then construct my running variable, which corresponds to the difference in vote shares between each immigrant candidate and their highest-ranked competitor. It is thus positive if the immigrant candidate won and inversely. I then filter this dataset to keep only elections where an immigrant candidate was present in the second round. Finally, I match this dataset to the turnout rates at the following regional elections (2010, 2015, 2021), at the level of the municipality for the municipality-level dataset and the level of the voting office for the other. Thus, each immigrant candidate is matched with their electoral results and the turnout rates in the following election, at the level of the voting office or municipality. I now provide a graphical illustration of this dataset, with a map presenting immigrants' spatial distribution at the level of the voting office as well as a random sample of municipalities where a municipal election involving an immigrant took place.

Figure 1: Graphical illustration of the final dataset



Note: The map should be in a sufficiently high resolution to allow you to zoom in and note how small voting offices are. Coloured borders correspond to a random sample (stratified by year) of municipalities where a municipal election involving an immigrant candidate took place over the period covered. To produce the choropleth map, I include only extra-European immigrants, as defined above.

5 Empirical framework

Based on this data, and recalling that I am interested in estimating whether the election of an immigrant candidate leads to an increase in immigrant voters' turnout in unrelated subsequent elections, I proceed in two steps. First, I rely on a regression discontinuity design, where I schematically compare turnout rates in electoral units where an immigrant candidate *just* won vs. *just* lost the previous election. Second, I estimate a difference-in-discontinuities, comparing the RD effect on turnout rates in electoral units with a higher and a lower share of immigrants respectively. This should allow me to determine whether the RD effect of an immigrant candidate's election is greater for more diverse constituencies, in line with our theoretical expectations. In the face of a relatively limited number of close elections involving an immigrant candidate (see Table 2), I pull data on all elections together and estimate the RD effect on the pooled dataset. As the number of observations changes from one election to the other (table 2), the estimated average treatment effect τ presented in the following section does not necessarily recover the average of all the immigrant candidate victory effects but is instead a pooled effect.

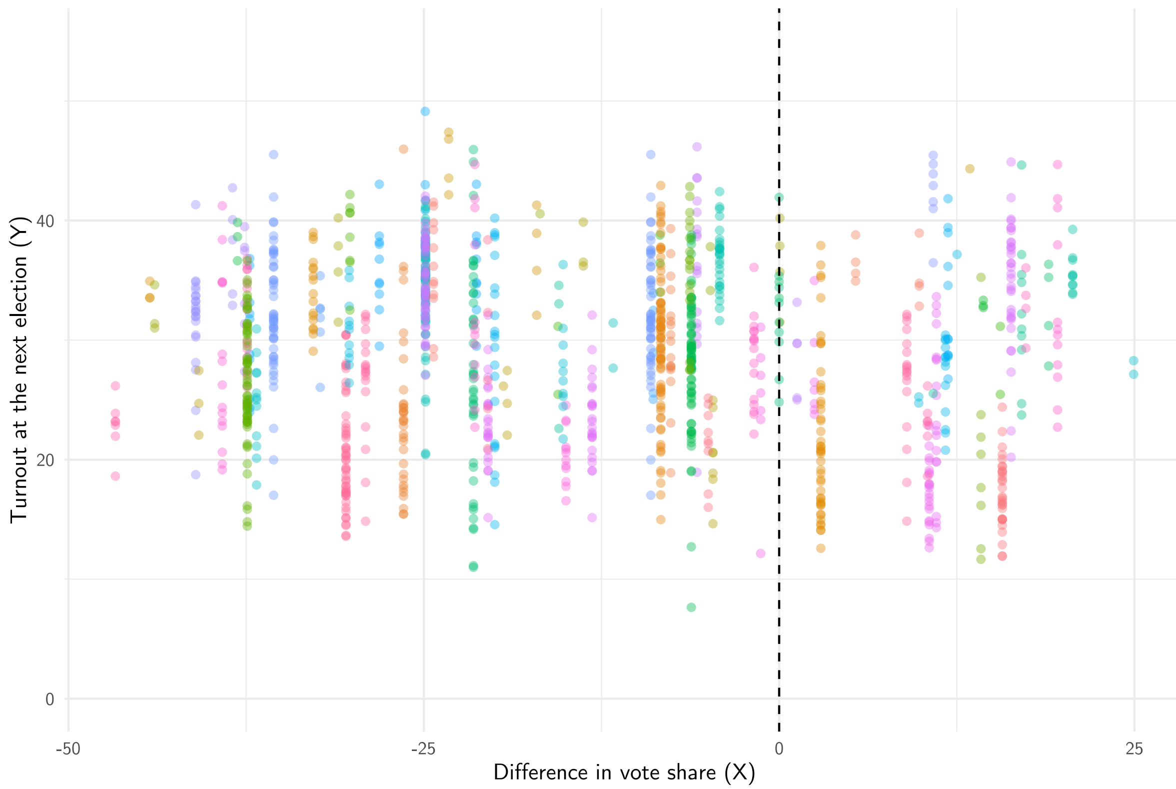
5.1 Regression discontinuity estimation

5.1.1 Setup

I define the running variable X_{it} as the difference between the immigrant candidate's vote share and that of her highest-ranked competitor at election t in municipality i. Thus, if $X_{it} < 0$, the immigrant candidate obtained fewer votes than their competitor and lost the election, and conversely. If $X_{it} > 0$, then municipality i (and the voting offices j it is composed of) is/are treated by the election of an immigrant candidate. This gives us the treatment indicator T_i defined as $T_i = \mathbb{1}\{X_{it} > 0\}$. Finally, we have the outcome variable Y_{t+1} defined as the turnout rate in the next election, either at the level of the municipality i or the voting office j.

Indeed, given that I have access to turnout data at both the levels of the municipality and the voting office, I should be able to estimate the RD effect at these two levels. At face value, the latter option appears more appealing given that a higher number of observations should increase statistical power, allowing me to recover lower effect sizes and increasing the precision of my estimates. However, in practice, my running variable X is in all cases measured at the municipality level, as it is the *election* of the candidate that determines whether a given municipality (and the voting office it is composed of) is treated or not. As such, the regression discontinuity setup in this case can essentially be thought of as an RDD with discrete values for the running variable (Cattaneo et al. 2024a). Indeed, several voting offices within each municipality have similar values for X, despite having different values for Y. This is illustrated in Figure 2, which shows the distribution of the running variable X for a sample of municipalities.

Figure 2: Scatter plot of the running and outcome variables at the level of the voting office



Note: This scatter plot presents data from a random sample of municipalities and voting offices in the 2020 municipal elections. Sampling and selection of the 2020 elections are simply performed to facilitate the visualization. Even though the number of colours in the palette is smaller than the number of distinct municipalities, it appears pretty clearly that each municipality holds a unique value of the running variable X but that the outcome variable Y measured at the level of the voting office varies significantly within each municipality, as illustrated by the vertical distribution of the points.

In practice, the continuity framework for RDD that has now become the reference in empirical social science may remain appropriate in such a setting as long as the number of mass points (i.e. observations sharing the same value for the running variable) is sufficiently high. In this case, the local polynomial methods associated with the continuity framework essentially behave as if each mass point were a single observation, where the outcome variable can be interpreted as the *average* outcome for all observations holding the same value of the running variable. However, in the present case, and as Table 2 illustrates, the number of close elections involving an immigrant candidate is relatively limited, which means that the number of mass points is itself also limited, regardless of the number of voting offices affected by these elections. The methodological literature has thus suggested that relying on the local randomization framework for RD inference could provide a viable alternative in such a setting. This approach has the advantage of allowing for inference even in the face of a limited number of observations unlike the continuity framework (Cattaneo et al. 2024a), as I will come to discuss throughout this section.

Table 2: Number of unique observations in the final dataset

	All elections	Elections with an IO candidate	Close elections with an IO candidate
2008	761	40	20
2014	1650	205	90
2020	1346	204	95
Total	3757	449	205

Note: The total number of elections is obtained from the dataset presenting results at the municipality level. This value may differ slightly in the other dataset which contains covariates, and thus a smaller number of observations without any missing values. The number of *close* elections corresponds to the number of observations within the MSE-optimal bandwidth estimated with the `rdrobust` package.

As such, I ultimately perform two sets of analyses: first, I rely on the continuity framework to estimate the RD effect at the municipality level, and second, I rely on the local randomization framework to estimate the RD effect at the level of the voting office.

5.1.2 Continuity framework

In its purest form, the sharp RD design in the continuity-based approach aims to estimate the treatment effect τ as the difference between the expected value of the potential outcomes $Y_i(1)$ and $Y_i(0)$ at the value $X = c$ where the probability of being treated jumps discontinuously from 0 to 1. In other words, one can think of the ATE τ as being given by:

$$\tau = E[Y_i(1)|X_i = c] - E[Y_i(0)|X_i = c] \tag{1}$$

However, in practice, because the running variable is assumed to be continuous at c, there are no or close to no observations for which $X = c$. Thus, τ is estimated by first approximating the (unknown) regression functions that relate X_i to the outcome variable Y_i on each side of the cutoff c, and then estimating the difference between these two functions at c. In contemporary empirical approaches, the approximation of these regression functions is generally based on local (low-order) polynomial methods. These methods focus on maximizing the fit of the function only vis-à-vis observations that are sufficiently close to the cutoff. This means that the choice of the specific neighbourhood around c (referred to as the bandwidth) that is used to estimate the underlying functions is primordial. Indeed, different bandwidths will yield different regression functions, and thus different RD estimates.

Intuitively, given any polynomial order p, the larger the bandwidth, the poorer the approximation of the regression function within that bandwidth, characterized by an increase in the mean square error (MSE) of its coefficients. This comes from the fact that the function is estimated through *linear* approximation, which means that as the number of observations increases (as the bandwidth is widened), the fit of the linear approximation to the true function (which may follow a different specification than the polynomial model chosen) decreases. Ultimately, this means that the wider the bandwidth, the greater the bias of the RD estimate vis-à-vis the true treatment effect τ. However, in parallel, reducing the width of the bandwidth involves reducing the number of observations used to estimate the regression function, which will in turn increase the variance of the estimator. As such, the choice of the bandwidth h is often said to involve a bias-variance trade-off, and a number of data-driven approaches have been developed to estimate the "optimal" bandwidth. While the intricacies of these approaches are beyond the scope of this paper, they generally aim to derive and optimize an asymptotic approximation of the MSE of $\hat{\tau}$, which varies as a function of its (estimated) variance and squared bias (for details, see Imbens and Kalyanaraman (2012)). In the present case, I rely on the optimal bandwidth selection algorithm developed by Calonico et al. (2020).

Then, for a given bandwidth h and a polynomial order p, the regression functions on each side of the cutoff c are typically estimated by weighting each observation so that observations closer to the cutoff receive more weight than those farther away. Again, this stands in the logic of approximating the parameter τ formulated in equation 1. The weight ω_i of each observation is typically calculated as $\omega_i = K\left(\frac{X_i - c}{h}\right)$ where $K(\cdot)$ is referred to as a kernel function. Several kernel functions have been put forward, such as the Gaussian kernel which weights observations according to the normal distribution centered around c. Here, I choose to rely on the triangular kernel – a common choice in the literature – which assigns weights that are made to decrease linearly from the center c to the edges of the bandwidth, after which they reach zero. Worth noting is that in practice, both point estimates and confidence intervals tend not to be sensitive to the choice of the kernel function (Cook and Wong 2008).

Based on these elements, I ultimately move to estimate the following sharp regression discontinuity equation:

$$Y_{it+1} = \alpha + \beta_1 X_{it} + \tau T_{it} + \beta_2 (X_{it} \cdot T_{it}) + \epsilon_{it+1} \tag{2}$$

where τ is the parameter of interest, standing for the RD effect of the election of an immigrant candidate at time t (characterized by $X > 0$ and $T = 1$) on turnout at the following election, Y_{t+1}.[20] Importantly, and consubstantially to the RD design, the parameter τ is by nature a *local* average treatment effect, which stands only for observations that are close to the cutoff c. In practical terms, here, this means that I estimate the effect of the election of an immigrant candidate on turnout rates in subsequent elections *only* for municipalities where the election was close.

I estimate this equation using the MSE-optimal bandwidth for both point estimation and inference, and I cluster standard errors at the level of the municipality to account for within-municipality correlation of the error terms over time (Lee and Lemieux 2010).[21] In line with Gelman and Imbens

[20]Given that the number of elections involving an immigrant candidate varies significantly over time (see Table 2), an option could have been to also include a year fixed effects λ_{t+1}. This would have allowed me to compare turnout rates in municipalities where an immigrant candidate won/lost by a small margin *within the same year*, and would have also increased the efficiency of the point estimate (Calonico et al. 2019; Lee and Lemieux 2010). However, in practice, the vast majority of the municipalities appear only once in the final dataset, for the simple reason that the presence of an immigrant candidate in an election's second round remains rare. Thus, the year fixed effect would absorb the entire variance of the outcome variable for that municipality, mechanically leading to a null point estimate for all these municipalities. I thus do not include any fixed effects.

[21]I use the `rdrobust` package in R (Calonico et al. 2023) to perform the bulk of the estimation.

(2019)'s recommendations, I rely on local linear estimators rather than higher-order polynomials.[22] Importantly, I also follow Cattaneo et al. (2019)'s suggestions and report the robust bias-corrected standard errors and confidence intervals. To detail how *bias correction* is achieved, one should recall that the RD estimator $\hat{\tau}$ is characterized by some degree of unobserved bias (let us denote it b) vis-à-vis the true RD parameter τ. The goal of bias correction is thus to estimate this unobserved bias b with the estimator $\hat{b}$ (as is performed to compute the MSE-optimal bandwidth). Then, the point estimate $\hat{\tau}$ can be corrected by simply computing $\hat{\tau} - \hat{b}$.[23] However, this correction only affects the point estimate but keeps the original standard errors and confidence intervals. Thus, *robustness* precisely involves integrating the increased variability of the corrected RD estimate vis-à-vis the conventional RD estimate that resulted from the bias correction to re-estimate the variance and standard errors. Not doing so would imply using the standard error of $\hat{\tau}$ to perform inference on $\hat{\tau} - \hat{b}$, which appears incorrect.[24]

5.1.3 Local randomization framework

In comparison to the continuity-based approach, the local randomization framework for RD inference is conceptually closer to the logic of classical randomized experiments. Indeed, fundamentally, local randomization rests upon the assumptions that there exists a window W around the cutoff c within which observations are similar to one another except for their treatment value *and* within which treatment assignment is *as-good-as* random. In that sense, within that window W, the running variable X is assumed to be strictly unrelated to the potential outcomes $Y_i(0)$ and $Y_i(1)$.[25] This exactly mirrors the setup of a randomized experiment where the assignment mechanism – in its simplest form, a coin flip – is the running variable, of which the independence vis-à-vis

[22]In the following section, I also present the results with different bandwidths and polynomial orders as a form of robustness check.

[23]In contrast, conventional standard errors and confidence intervals assume that the bias term is exactly zero, and that the chosen polynomial functions perfectly approximate the functions $E[Y_i(1)|X_i]$ and $E[Y_i(0)|X_i]$. This is conceptually inappropriate given that the MSE-optimal bandwidth is obtained precisely based on the bias-variance trade-off, which requires that such bias exists.

[24]The variance of the bias-corrected estimator is necessarily greater than the variance of the conventional estimator, as it incorporates the variance of the bias term $\hat{b}$. This implies that the robust bias-corrected confidence intervals are centered around the bias-corrected point estimate but are wider than the conventional confidence intervals.

[25]This can largely be assimilated to an exclusion restriction, where the potential outcomes are determined by the score only through the score's effect on treatment status (Abadie and Cattaneo 2018).

the potential outcomes is precisely what allows for inference. This is, however, in contrast with the continuity-based approach, which admits that the running variable may be related to the potential outcomes *on its entire support*, as is in fact, generally the case.[26] In practice, here, this assumption of independence between X_i and Y_i corresponds to the idea that within a small window W around c, each voting office's vote share in the second round of election t in municipality j is independent of turnout levels in the voting offices j that compose the municipality i at election $t+1$.

Graphically, this assumption of independence of the running variable and the potential outcomes implies that *within a particular window around the cutoff*, the regression functions on each side of the cutoff are flat. Formally, whereas the continuity-based approach aims at approximating the regression functions $E[Y_i(1)|X_i = x]$ and $E[Y_i(0)|X_i = x]$, the local randomization framework assumes that within the windows W_0 and W_1 around the cutoff c, they are constant. This means that the treatment effect can be estimated as a simple difference-in-means between the two groups. For k_0 and k_1 constants, we can write:

$$\begin{aligned} E[Y_i(0)|X_i = x] &= k_0 \, \forall \, x \in W_0 \\ E[Y_i(1)|X_i = x] &= k_1 \, \forall \, x \in W_1 \end{aligned} \tag{3}$$

Thus, it appears that the most crucial element of the local randomization framework is the choice of this window W which satisfies the conditions just detailed. The general approach to choosing such a window is again closely linked to the setup of randomized experiments. Window selection ultimately aims at finding the largest possible window W within which a set of pre-determined or placebo covariates are perfectly balanced across observations on each side of the cutoff. Specifically, for a vector of covariates $\boldsymbol{Z} = (Z_1, Z_2, \ldots, Z_k)$, the idea is to pose the null hypothesis that $\boldsymbol{Z}$ is balanced between the groups (i.e. that the scores and treatment assignment are unrelated to $\boldsymbol{Z}$) within the smallest possible window W around c. Then, the hypothesis of no difference between the two groups can be tested through a simple difference-in-means. If it fails to be rejected, a larger window is selected and the same null hypothesis is tested in that window. This procedure is repeated until the null hypothesis is rejected. Then, the window of choice is the largest win-

[26] Classically, when estimating the RD effect of a training program targeting students whose grades fall below an arbitrary threshold and where the outcome of interest is each student's grade in the following semester, there are good reasons to believe that the running variable is related to the outcome variable *on its entire support*. Indeed, irrespective of the training program, grades are time t are strongly correlated with grades at time $t+1$.

dow W in which the null hypothesis of no difference fails to be rejected. Cattaneo et al. (2024a) recommend that the significance threshold used to conduct the successive hypothesis tests of no difference between the two groups should be set at 0.15 instead of the conventional 0.05. They reason that one should favour preventing Type II errors (failing to reject a false null hypothesis) over Type I errors (rejecting a true null hypothesis). Indeed, the latter would lead to a *smaller* window than the largest possible window, whereas the former would lead to using a window within which covariates are not actually balanced.

While this window selection procedure has the advantage of being data-driven instead of involving an arbitrary choice, it necessarily involves determining which covariate(s) to include in the successive balance tests. Cattaneo et al. (2024a) recommend that these covariates should be *"related to both the outcome and the treatment assignment"*. In an analysis of the incumbent status advantage in the US Senate that relies on close elections, Cattaneo et al. (2016) include Democrats' vote shares and dummies for victory or defeat in the two previous US Senate elections, Democrats' vote share and a dummy for victory in the previous Presidential elections, as well as the size of the state's population. Focusing on the effect of pools on individuals' turnout intentions, Brugarolas and Miller (2021) use individuals' gender, age and the size of the town they reside in. In the present work, I choose to include the following covariates: the number of voters in the voting office, the share of immigrants in the voting office, and the voting office's turnout level in the first round of election t. I implement this window selection procedure using the `rdlocrand` (2016) package in R, which uses a simple difference-in-means test to assess covariate balance. I report in the next section (6.1.1) the results of this process.

In the context of a low number of observations, inference for the RD estimator can be conducted through Fisherian inference.[27] In this setting, one can pose the sharp null hypothesis that the

[27] When the number of observations within the window W is large, one can conduct inference by relying on Neyman and Super-population approaches. These two approaches are both based on large sample approximation. The former considers the sample at hand as that of a (large) finite population, and sees the potential outcomes within this sample as fixed, with randomness only coming from the treatment assignment. In contrast, the super-population model considers the case of a random sample drawn from a larger population using i.i.d sampling. This means that in this model, both the potential outcomes and the treatment assignments are seen as random (due to the sampling from the super-population, and due to the treatment assignment). In any case, these two models rely on large sample approximation of the normal distribution based on the central limit theorem. Importantly, this

treatment has no effect for *any* unit, that is:

$$H_0 : Y_i(1) = Y_i(0) \, \forall \, i \tag{4}$$

Importantly, this sharp null hypothesis is stricter than the null hypothesis formulated in the continuity framework, which only assumes that the *average* treatment effect is zero, instead of the treatment effect for each unit. The key advantage here is that under the sharp null hypothesis, the potential outcomes of each unit i are known, and simply correspond to the observed outcome for that unit. Indeed, if the treatment has no effect for any unit i, then the potential outcomes with or without treatment are equal, and they are themselves equal. More formally, if we have the treatment effect $\tau_i = 0 \, \forall \, i$, then $Y_i(1) = Y_i(0) + \tau_i = Y_i(1) = Y_i(0)$. Then, as long as the random mechanism determining treatment assignment is known (in practice, assumed) within the window W, we are able to test the sharp null hypothesis formulated in equation 4 by computing the probability that the test-statistic is greater than its observed value.[28] Specifically, given a vector $\boldsymbol{T}_W$ of treatment values $t = \{0, 1\}$ for each unit i and a vector $\boldsymbol{Y}_W$ of observed outcomes, we have, for any test statistic S and its observed value s, the p-value associated with the sharp null hypothesis given by:

$$p = P(S(\boldsymbol{T}_W, \boldsymbol{Y}_W) \geq s) \tag{5}$$

Then, H_0 can be rejected or not based on the value of p vis-à-vis a chosen significance level.[29] Nonetheless, in its current form, the approach is relatively restrictive, in the sense that it does not give any information on the magnitude of the difference nor its direction. Fortunately, confidence intervals can be obtained by inverting a series of hypothesis tests for by inverting a series of test hypothesis conducted for different treatment effect values. Then, the confidence intervals correspond to the set of treatment values where the null hypotheses fail to be rejected at the

has the advantage of allowing for point estimation and inference based on standard approaches, unlike Fisherian inference. For details, see Cattaneo et al. (2024a)

[28]Given that all potential outcomes are known under the sharp null hypothesis, the distribution of *any* test statistic can be obtained from the distribution of the treatment assignments within the window W.

[29]Noteworthily, such a hypothesis test can be conducted for *any* difference in the potential outcomes. In particular, assuming a constant treatment effect τ, one can test for $\tau = \tau_0$ for any τ_0 by posing the sharp null hypothesis that $Y_i(1) = Y_i(0) + \tau_0 \, \forall \, i$.

chosen significance level.[30] Finally, if the local randomization assumptions hold (implying notably constant regression functions, as per equation 3), the parameter of interest θ can be estimated as the difference between the average outcome for treated and non-treated observations within the window W. This is given by:

$$\theta = \frac{1}{N} \sum_{X_i \in W_1} Y_i(1) - \frac{1}{N} \sum_{X_i \in W_0} Y_i(0) \tag{6}$$

5.2 Difference-in-discontinuities

Finally, I recall that I am ultimately interested in determining whether the election of an immigrant candidate increases immigrant-origin voters' turnout in subsequent elections. In the absence of individual-level data, I estimate the share of immigrants at the level of the electoral unit of interest (municipality or voting office). By subsetting my sample based on the median share of immigrants (Zonszein and Grossman 2023), I am able to distinguish between constituencies with a higher vs. lower share of immigrants, which I use as my unit of analysis. Then, I estimate a difference-in-discontinuities (Grembi et al. 2016; Kamel and Woo-Mora 2023) to recover whether the RD effect of an immigrant candidate's election is greater for voting offices with a higher vs. lower share of immigrants. This involves estimating the following parameter:

$$\Delta = \tau_H - \tau_L \tag{7}$$

where τ_H and τ_L are the RD parameters for each group of electoral constituencies. I estimate these two RD effects by re-estimating equation 2 separately for each group, and present the corresponding results in the next section. Next, I estimate equation 7, and conduct inference on Δ through a t-test.[31] Given that I split the sample based on a pre-determined covariate (i.e. the share of immigrants), the RD estimators for each group are independent. Thus, the standard error of their difference is given by the square root of the sum of the squared standard errors (or similarly, of

[30]This successive implementation of hypothesis tests for different values of the treatment effect can be implemented with the `rdlocrand` package (2022) in R.

[31]I first conduct an F-test for the equality of the outcome variable's variance between the two groups. Inequality of these variances would support the use of Welch's t-test (Welch 1947) which adjusts for unequal variances, and is thus more appropriate (Kamel and Woo-Mora 2023). However, here, the p-value associated with the F-test is 0.90, which suggests that the variances are equal, and that a simple t-test is appropriate.

the sum of the variances). Formally, we have:

$$SE(\Delta) = \sqrt{SE(\tau_H)^2 + SE(\tau_L)^2} \tag{8}$$

which allows us to test the significance of Δ through a simple two-sided t-test. We have:

$$t = \left(\frac{\Delta}{SE(\Delta)}\right) \tag{9}$$

which be compared to the critical values of the t-distribution with $df = df_H + df_L - 2$ degrees of freedom.

6 Results

6.1 Regression discontinuity estimation

6.1.1 Voting office-level evidence

Validation of the RD design I begin by validating the recourse to the local randomization framework to estimate RD effects at the level of the voting office. This first involves finding a window W within which covariates are balanced between treated and non-treated observations, using the list of covariates detailed in subsection 5.1.3. In this perspective, I print below the table of the successive balance tests conducted with the `rdlocrand` package in R.

Table 3: Window selection procedure in the local randomization framework

Window	N left	N right	Covariate	p-value	Bin. test
$\pm$ 0.15	6	35	% immigrants	0.001	0.000
$\pm$ 0.64	12	41	% immigrants	0.000	0.000
$\pm$ 1.19	84	47	N voters	0.000	0.002
$\pm$ 1.99	170	102	N voters	0.000	0.000
$\pm$ 2.32	204	109	N voters	0.000	0.000
$\pm$ 2.93	289	164	N voters	0.000	0.000
$\vdots$	$\vdots$	$\vdots$	$\vdots$	$\vdots$	$\vdots$
$\pm$ 4.12	774	272	% immigrants	0.421	0.000

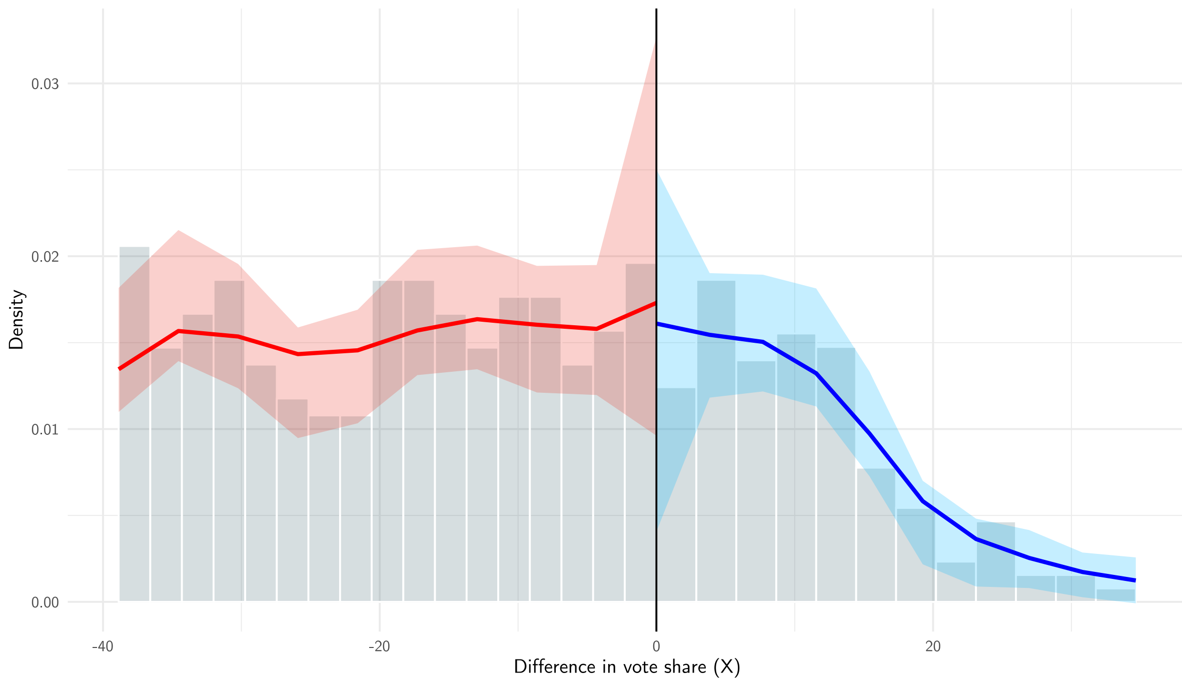
These results suggest that there is simply no window W around the cutoff c within which the selected covariates are balanced. Indeed, the p-values associated with the covariate balance tests are all far below the 0.15 significance threshold discussed in section 5.1.3. At each of these different windows, and for each of the covariates, the null hypothesis that the covariate is balanced between treated and non-treated observations is rejected. In other words, the observations on each side of the cutoff appear to differ systematically on each of the (few) covariates included in the balance tests. This questions the validity of the local randomization assumptions given that if treatment assignment within W was truly as-good-as-random, the observations on each side of c should be approximately similar. This is also supported by the last column of Table 3, which displays the results of a binomial test. Binomial tests are used in the local randomization framework in the same way that tests of the continuity of the running variable in the continuity framework. It assesses the plausibility of the treatment assignment mechanism by comparing the observed distribution of treated and non-treated observations within W to the binomial distribution. The idea is that if treatment assignment was truly as-good-as random, the probability of being "treated" and "non-treated" should be similar. Indeed, in its simplest form, one can assume that assignment is based on a simple coin flip where each observation has a probability $p = 0.5$ of falling in each group. In this setting, the number of observations on each side of c is expected to follow a Binomial distribution based on n repeated Bernoulli trials. Here, the last column of Table 3 shows that the null hypothesis of no difference in the number of observations is rejected at the 0.05 significance level, for all windows.

In sum, these results tend to suggest that the local randomization assumptions do not hold in the present setting. As such, I choose to stop the analysis here and do not proceed to estimate the RD effect at the level of the voting office. For the sake of completeness, I nonetheless present in Appendix A.2.1 the results of the estimation conducted at several arbitrary windows around c.

6.1.2 Municipality-level evidence

Validation of the RD design Before turning to the estimation per se, I recall that point estimation and inference in the continuity-based approach rests upon the assumption that the running variable X is continuous at the cutoff c. Indeed, if that were not the case, the parameter τ formulated in equation 1 would not be identified. Further, discontinuity of the running variable would suggest that observations were able to "sort" by manipulating the score they receive, which would mean that observations that are on each side of the cutoff in its vicinity actually systematically differ from each other, invalidating the estimation of the treatment effect. This assumption can be tested graphically based on a visual inspection of the density of the running variable X, which should exhibit no discontinuity at c. One can also rely on density tests which involve estimating the density of observations on each side of the cutoff, and testing for the significance of the difference between the two. I do so here by implementing Cattaneo, Jansson and Ma's density test (2020) which first estimates the density of the running variable using local polynomial estimators based on the observed cumulative distribution function.[32] Here, this test returns a p-value of 0.4373. I thus fail to reject the null hypothesis of no difference between the two densities, in support of the continuity assumption. I now print a plot presenting the density of the running variable on each side of the cutoff, which shows that the number of observations on each side of the cutoff is approximately similar, and that the density of the running variable is continuous at the cutoff.

Figure 3: Density of the running variable (continuity framework)



Main results I now print the main results of the RD estimation at the municipality level. In Table 4, I present the main model, estimated using the MSE-optimal bandwidth and a local linear estimator.

[32]This test can be implemented using the `rddensity` package in R (Cattaneo et al. 2024b).

Table 4: Main RD results at the municipality level

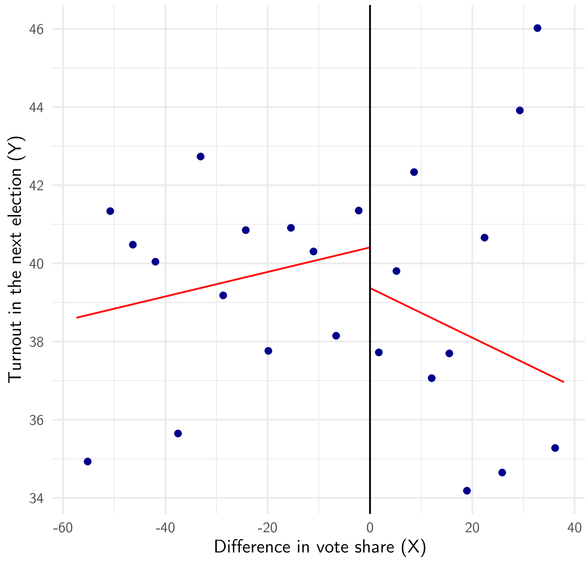
	Conventional	Bias-corrected	Robust
RD Estimate	-10.42	-12.37	-12.37
Standard error	4.88	4.88	5.46
Z-statistic	-2.13	-2.13	-2.13
$P > \|z\|$	0.032	0.011	0.023
95% Conf. int.	[-19.99, -0.84]	[-21.94, -2.81]	[-23.07, -1.68]
Observations left of c		313	
Observations right of c		133	
Effective obs. left		53	
Effective obs. right		52	
Polynomial order		1	
MSE-optimal Bandwidth		7.69	

This table suggests that the RD estimate of the effect of the election of an immigrant candidate on turnout at the following election is negative. Specifically, the robust bias-corrected point estimate is -12.37 on the 0-100 scale that stands for turnout levels at the municipality level. We note that the 95% confidence intervals are particularly wide, most likely resulting from the low number of effective observations on each side of the cutoff (i.e. observations within the bandwidth). Finally, all p-values are below 0.05. All in all, this suggests a statistically significant negative effect of the election of an immigrant candidate on turnout at the following election, *without distinguishing* between municipalities with a higher vs. lower share of immigrants.

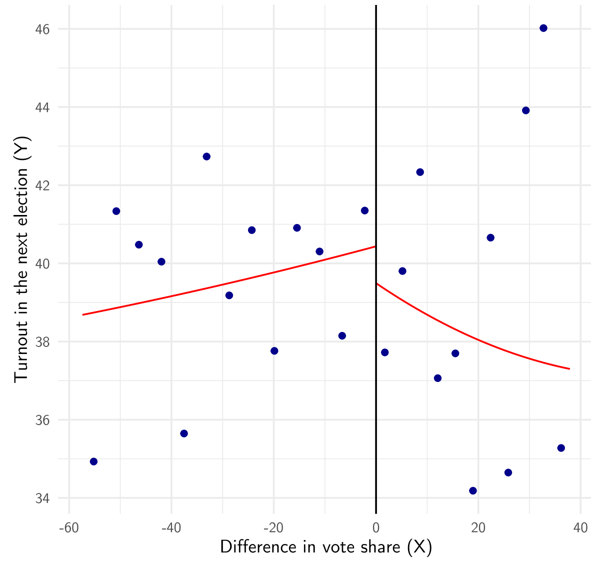
Robustness checks I now conduct several robustness checks aimed at assessing the sensitivity of the RD estimates across specifications. I start by printing the regression discontinuity plots for different polynomial orders.[33] These four plots illustrate the elements just discussed: we notice the existence of a discontinuity at the cutoff c, but we also take note of the great variability of the outcome variable on the right side of the cutoff, most obvious as the number of polynomial orders increases. I further elaborate on these points in section 7.

[33]Table 8 in Appendix A.2.2 presents the detailed results of the RD estimation for each polynomial order.

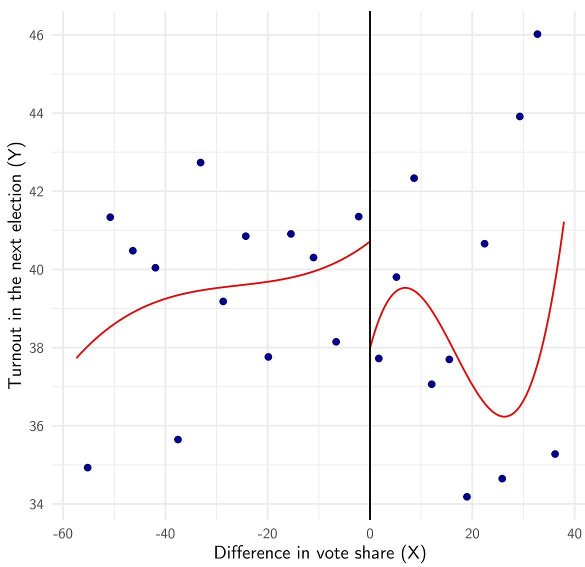
Figure 4: Regression discontinuity plots for all polynomials (continuity framework)



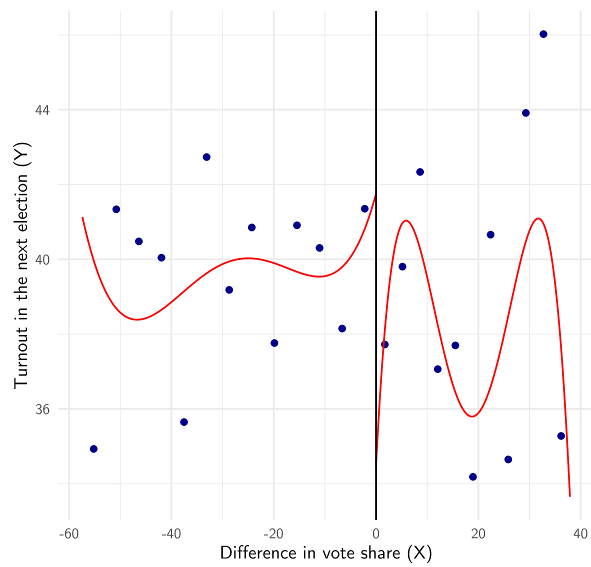
(a) RD Plot ($p = 1$)



(b) RD Plot ($p = 2$)



(c) RD Plot ($p = 3$)

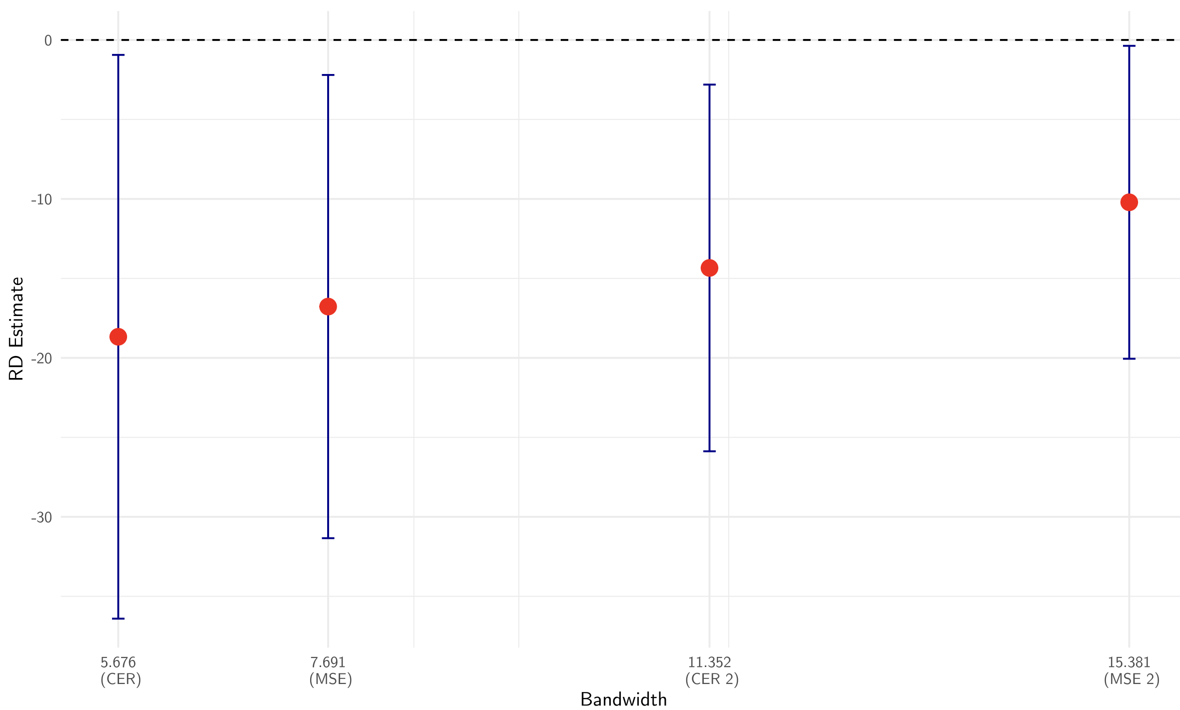


(d) RD Plot ($p = 4$)

Note: The plots show the binned outcomes as a function of the running variable, for each of the four polynomial orders. In each case, the model is estimated with the MSE-optimal bandwidth. The first plot, where $p = 1$, is the main RD plot, which presents graphically the results of Table 4. Detailed results for all polynomial orders are presented in Table 8.

Second, I re-estimate the model using different bandwidths. Figure 5 presents the RD estimates and confidence intervals computed using the MSE-optimal bandwidth, the MSE-optimal bandwidth multiplicated by two, and similarly for the CER-optimal bandwidth.[34] This figure shows, expectedly, that as the number of observations increases, the RD estimates gain in precision, with narrower confidence intervals. Nonetheless, in all cases, the RD estimates remain relatively consistent, fluctuating between approximately -10 and -20 on the 0-100 scale that stands for turnout rate at election $t + 1$. This supports the main results, namely that the election of an immigrant candidate appears to have a negative effect on turnout rates at the next election.

Figure 5: Sensitivity of RD estimates to bandwidth choice (continuity framework)



Finally, I re-estimate the model at alternative cutoffs, with the expectation that the RD effect will remain null at all of them, suggesting that turnout at election $t+1$ does not jump discontinuously

[34]The CER-optimal bandwidth is the bandwidth obtained when one aims to minimize coverage error of the confidence interval, that is the difference between the *nominal* coverage of the interval (i.e. the theoretical probability with which a confidence interval is supposed to contain the true parameter – usually 95%) and the *actual* probability that this interval contains the population parameter, obtained through repeated sampling. The *coverage error* is the difference between the *nominal* and the *actual* coverage probabilities. Thus, the CER-optimal bandwidth aims at minimizing the asymptotic error coverage of the confidence interval, in the same way that the MSE-optimal bandwidth aims at minimizing the asymptotic MSE of the point estimator. For this reason, the MSE-optimal and CER-optimal bandwidths can be combined to optimally estimate the RD treatment effect and its confidence intervals respectively. For details, see Cattaneo et al. (2019).

at these levels. This is precisely the case, as Table 5 below demonstrates: all p-values are superior to 0.05 and all 95% confidence intervals include 0. I discuss these results in detail in the next section, after having explored the heterogeneity of the treatment effect as a function of the share of immigrants in the municipality.

Table 5: RD estimates at alternative cutoffs at the municipality level

Alternative cutoff	Bandwidth	Robust bias-corrected inference			N	
		RD Estimator	p-value	Conf. intervals	Left	Right
-3	15.085	5.711	0.245	[-3.92, 15.34]	110	108
-2	14.303	5.828	0.273	[-4.59, 16.25]	103	102
-1	9.845	12.139	0.087	[-1.75, 26.02]	70	67
1	13.020	-5.859	0.328	[-17.59, 6.88]	92	90
-2	14.517	-4.817	0.422	[-16.58, 6.95]	105	93
-3	16.697	1.480	0.777	[-8.75, 11.71]	120	93

Note: Each model is estimated with the MSE-optimal bandwidth, *via* the triangular kernel function with a polynomial of order 1 (i.e. local linear regression). N denotes the number of observations within the bandwidth. All p-values and confidence intervals support the null hypothesis of no effect.

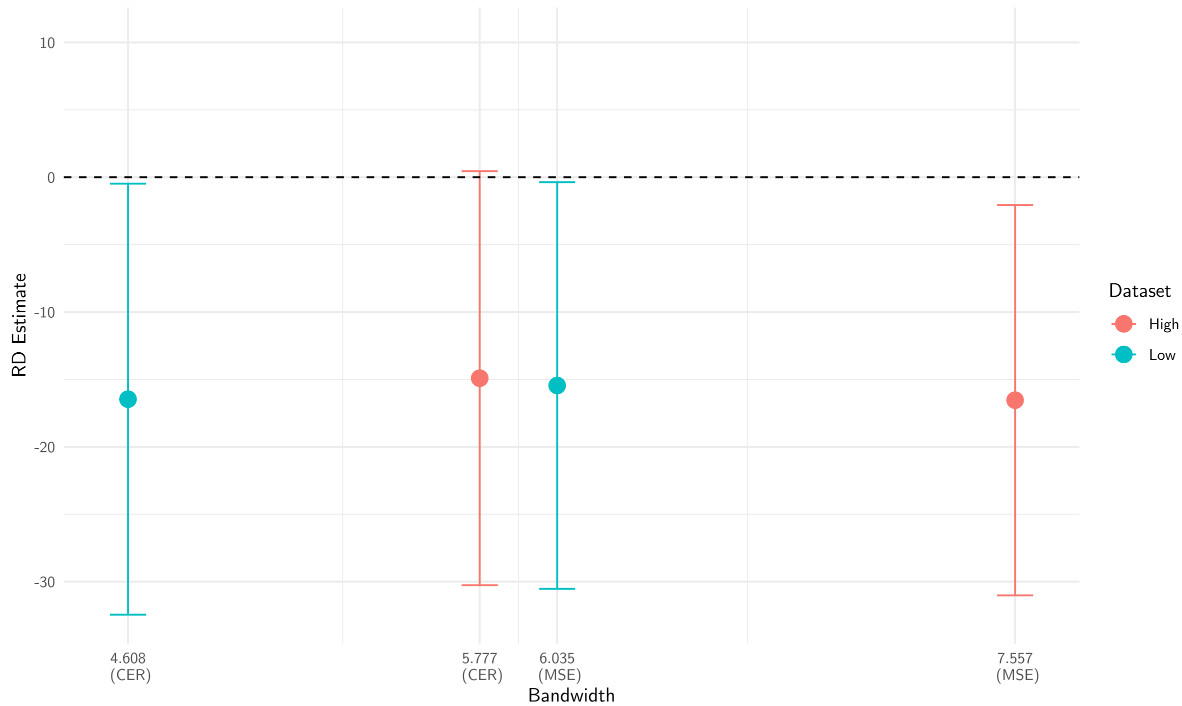
6.2 Difference-in-discontinuities

I now re-estimate the model separately for municipalities with a higher and lower share of immigrants, after having split the main dataset into two datasets at the median share of immigrants.[35] I start by plotting the RD estimates separately for each group, with the MSE-optimal and CER-optimal bandwidths discussed above. This figure reveals that the point estimates are relatively consistent with no distinction between the two groups, for both bandwidths. Nonetheless, the (wide) confidence intervals show that these results do not reach statistical significance, as they include 0. Given the significance of the RD estimates for the entire dataset, it appears likely that this is in part due to a limited number of observations, mechanically reducing the precision of the RD estimates. Table 9 in Appendix A.2.3 presents the results at alternative cutoffs (i.e. different

[35]I present in Appendix A.2.3 additional results based on a split at different quantiles.

quantiles of the share of immigrants in the municipality). The magnitude and the direction of the effect remain relatively stable, but noteworthily, alternative quantiles yield different results in terms of statistical significance. I further discuss these points in the next section.

Figure 6: RD estimates for municipalities with a higher / lower share of immigrants



Finally, I present below a table containing the detailed results of the RD estimation for each group, using the MSE-optimal bandwidth, and reporting the robust bias-corrected point estimates, standard errors and confidence intervals. This supports the graphical illustration provided in figure 6 as the RD estimates are consistent across groups of municipalities.

Table 6: RD Estimates for municipalities with higher vs. lower share of immigrants

	High	Low
RD Estimate	-16.53	-15.45
Standard error	7.38	7.69
Z-statistic	-2.24	-2.01
$P > \|z\|$	0.025	0.044
95% Conf. int.	[-31.02, -2.52]	[-30.54, -0.37]
Observations left of c	158	155
Observations right of c	65	68
Effective obs. left	26	19
Effective obs. right	18	26
Polynomial order	1	1
MSE-optimal Bandwidth	7.56	6.04

Note: The RD estimates and confidence intervals are obtained by estimating equation 2 separately for each group of municipalities, based on their position vis-à-vis the median share of immigrants. The bandwidth corresponds to each group's MSE-optimal bandwidth. I report robust bias-corrected point estimates, standard errors and confidence intervals.

Finally, using these RD estimates, I estimate the difference-in-discontinuities parameter Δ detailed in equation 7, and conduct inference on it through a t-test. Ultimately, the test yields a t-statistic of -0.102, which is associated with a p-value of 0.919. This means that we *clearly fail to reject* the null hypothesis that the two RD estimates are equal. In other words, there is no evidence that the election of an immigrant candidate has a different effect on turnout rates in municipalities with a higher vs. lower share of immigrants. I discuss these results in the next section.

7 Discussion

Overall, it appears that descriptive representation *does not* have a positive effect on immigrant voters' turnout in subsequent elections. This is evidenced by the results of the difference-in-discontinuity estimation, which suggest that the RD effect does not differ between municipalities with a higher vs. lower share of immigrants (see also Table 9 in Appendix A.2.3). In fact, the RD estimates even suggest that the election of an immigrant candidate at time t has a negative effect on turnout at time $t+1$ in all "treated" municipalities, irrespective of the share of immigrants that reside there. This result is admittedly rather surprising. Two explanations appear conceivable based on the results.

First, it could be that the limited number of observations prevents effective estimation of the true RD parameter. Indeed, the number of observations included within the bandwidth in the main model is only slightly higher than 100, which is far too low. This leads to particularly wide confidence intervals, suggesting that the RD estimates lack precision. We can also note from Figure 4 and Table 8 that changes in the polynomial orders lead to relatively significant changes in the magnitude of the effect. The starting point of the present thesis was precisely that the number of immigrant-origin representatives remained low in France, which justified exploring this topic in greater detail. However, this very fact also hinders systematic exploration of the topic, as the number of cases to study is limited. In addition, while regression discontinuity designs have the advantage of providing clean identification of causal effects, they necessarily involve further reducing the number of observations included in the analysis. Indeed, it brings us to focus on the quite specific (and rare) scenario of a close election involving an immigrant candidate. This explanation would suggest that the RD estimates presented here are biased and imprecise, and that the true effect of descriptive representation remains unknown.

An alternative explanation is that the true population RD effect of the election of an immigrant candidate is in fact negative, and that it is correctly identified in the present analysis. In other words, close elections that lead to the victory of an immigrant candidate could lead to lower participation in subsequent elections, in both municipalities with a high and low share of immigrants. This hypothesis is supported by the fact that despite the variability of the RD estimates and the wide confidence intervals, the direction of the effect remains surprisingly similar across specifications. This is illustrated in Figure 5 which shows that the RD estimates remained stable across bandwidths choice, in Table 8 which suggests that changes in the polynomial order do not alter the direction of the effect, as well as in Table 9 which shows that this holds even when splitting the dataset based on the share of immigrants. Table 5 which showed that the RD effect was null at all alternative cutoffs also supports this explanation. Finally, and although I recall that the local randomization assumptions did not seem to hold in any window around c, one can note that the RD estimates obtained in this framework are relatively consistent with those from the continuity framework, both in direction and in magnitude (see Table 7 in Appendix A.2.1).

While I strongly believe that the first explanation remains the most credible, one can still wonder how a negative RD effect of the election of an immigrant candidate could be explained theoretically. In that regard, it appears worth noting that this result precisely contradicts those of a recent paper published by Zonszein and Grossman (2023). Indeed, they rely on a close-elections regression RDD and suggest that in the United Kingdom, the election of a minority candidate in Parliamentary Elections leads to an *increase* in turnout at the following election. Importantly, they use Parliamentary elections to measure both the treatment and the outcome, meaning that turnout is expected to increase in the next Parliamentary election. They defend that this positive effect is mainly driven by White-majority constituencies, thereby suggesting that the election of a minority candidate presents a threat to the majority group, which mobilizes to prevent the re-election of the minority candidate. Reconciling these two results, it could be that the election of a minority candidate in a given election presents a threat to the majority group, but that this threat generates different reactions depending on the election: it could foster a *discouragement* effect for unrelated elections but a *mobilization* effect for related elections. Regional elections appear as a particularly fertile setting for a discouragement effect, as they are already scarcely attended and often seen as less important than other elections. It could thus be worth exploring how turnout at the next *municipal* election is affected by the election of an immigrant candidate, to explore whether we observe a mobilization effect in this case. Nonetheless, this hypothesis does not explain why the election of an immigrant candidate has a negative effect on turnout even in municipalities with a higher share of immigrants, as Tables 6 and 9 (in Appendix A.2.3) appear to suggest. While some of the literature suggests that only substantive representation has a positive effect and that descriptive representation alone does not (Griffin and Keane 2006), if that were the case, we would expect to find null results in municipalities with a higher share of immigrants, which does not appear to be the case.

In sum, and to conclude, these results are theoretically puzzling, and I argue that the most credible explanation remains that the limited number of observations prevents me from obtaining credible estimates of the effect of descriptive representation on turnout. This is linked to the fact that the presence of an immigrant candidate in the second round of an election remains relatively rare. This rarity is further compounded by the empirical strategy that involves further subsetting the original dataset to focus only on close elections, and then to distinguish between municipalities with a higher vs. lower share of immigrants. Overcoming these limitations would require obtaining individual voting records, which would significantly increase the statistical power of the analysis, and provide a clearer picture of the effect of descriptive representation on turnout.

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A Appendix

A.1 Data & Code

The different datasets used to estimate the results of this paper as well as the code used to replicate the figures are available on a dedicated GitHub repository, [here].

A.2 Additional results

A.2.1 RD Estimation at the level of the voting office in the local randomization framework

The results of the diagnostic tests performed in section 6.1.1 largely suggested that the local randomization assumptions did not hold in any window W around the cutoff. Nonetheless, for the sake of completeness, I present in Table 7 the results of the estimation and inference conducted for different arbitrary windows. I recall that the interpretation of the treatment effect estimator differs from that in the continuity framework. Here, θ corresponds to the constant treatment effect within the window W where the randomization assumptions are invoked.

Table 7: Main results in the local randomization framework

Window	N left	N right	Bin. test	$\hat{\theta}$	p-value	Conf. intervals
± 0.25	6	35	0.000	-12.86	0.001	$[-19.7, -5.7]$
± 0.5	9	35	0.000	-19.30	0.000	$[-20.0, -12.4]$
± 0.75	37	41	0.734	-22.14	0.000	$[-20.0, -18.8]$
± 1	38	41	0.822	-22.54	0.000	$[-20.0, -19.3]$
± 1.25	84	47	0.001	-13.73	0.000	$[-17.0, -10.5]$
± 1.5	116	80	0.012	-9.28	0.000	$[-12.1, -6.4]$
± 1.75	133	80	0.000	-6.17	0.000	$[-9.4, -2.9]$
± 2	170	102	0.000	-7.42	0.000	$[-10.1, -4.7]$

Note: The column "binomial test" reports the p-value associated with the test of the null hypothesis that the number of observations on each side of c within W is consistent with the binomial distribution. θ corresponds to the difference-in-means estimator of the treatment effect defined in equation 6, and is followed by its p-value. The confidence intervals are obtained by conducting successive hypothesis tests for diffent treatment effects and inverting the tests, as described in section 5.1.3.

This table shows that the direction of the treatment effect remains consistent across windows, which is in line with the results presented in the continuity framework (section 6.1.2). The effect sizes are also relatively similar, although smaller in larger windows. Finally, the results of the binomial tests are mostly in line with those presented in Table 3, with the null hypothesis of no difference in the number of treated and non-treated observations being rejected in most cases.

A.2.2 Additional RD results at the municipality level in the continuity framework

Sensitivity to polynomial order I now include a table presenting the results of the RD estimation at the municipality level for all polynomial orders. This table shows that the RD estimates are *relatively* consistent across polynomial orders, at least in terms of the direction and statistical significance of the effect. Nonetheless, we note that as in the main results, the confidence intervals are particularly wide, suggesting that the RD estimates lack precision, possibly due to the limited number of observations.

Table 8: RD point-estimation and inference for all polynomial orders

	Conventional			Bias-corrected			Robust		
	$\hat{\tau}$	p-value	95 % CI	$\hat{\tau}$	p-value	95 % CI	$\hat{\tau}$	p-value	95 % CI
$p = 1$	-10.41	0.03	[-19.99, -0.85]	-12.37	0.01	[-21.94, -2.81]	-12.37	0.02	[-23.07, -1.68]
$p = 2$	-10.87	0.03	[-20.90, -0.84]	-12.60	0.01	[-22.62, -2.57]	-12.60	0.02	[-23.55, -1.65]
$p = 3$	-15.74	0.01	[-28.11, -3.37]	-17.63	0.01	[-30.00, -5.26]	-17.63	0.01	[-31.13, -4.13]
$p = 4$	-17.88	0.001	[-31.64, -4.12]	-19.28	0.01	[-33.04, -5.52]	19.28	0.01	[-34.34, -4.22]

Note: This table extends Table 4 by presenting the same results for all polynomial orders. Each model is estimated with the MSE-optimal bandwidth.

A.2.3 Additional results for the difference-in-discontinuities parameter

Sensitivity to changes in share of immigrants cutoff In the main analysis, I split the original dataset based on the median share of immigrants at the level of the municipality, and presented the results in table 6. I now present the RD results for each group of municipalities after splitting at different quantiles. For interpretation, the fourth group of column for instance reveals that when splitting the dataset at the 70th percentile of the distribution of the share of immigrants in the municipality, the RD estimate of the effect of the election of an immigrant candidate for municipalities with a "high" share of immigrants (i.e. top 30% of the distribution) is -11.9 ($p = 0.023$), while it is -10.51 ($p = 0.041$) when splitting at the 60th percentile. As I discuss in section 7, this supports the idea that descriptive representation does not increase immigrant voters' turnout in subsequent elections.

Table 9: Results of the RD estimation for different cutoffs in the share of immigrants

Quantile	0.3		0.4		0.6		0.7	
	High	Low	High	Low	High	Low	High	Low
RD Estimate	-9.04	-13.24	-7.57	-12.75	-15.76	-10.51	-13.00	-11.90
Standard error	7.93	7.90	6.96	7.08	7.417	5.14	9.48	5.25
Z-statistic	-1.08	-1.54	-1.06	-1.70	-2.08	-1.97	-1.42	-2.17
$P > \|z\|$	0.254	0.094	0.276	0.072	0.034	0.041	0.170	0.023
95% Conf. int.	$[-24.6, 6.5]$	$[-28.7, 2.3]$	$[-21.2, 6.1]$	$[-26.6, 1.1]$	$[-30.3, -1.22]$	$[-20.6, -0.4]$	$[-31.6, 5.6]$	$[-22.2, -1.6]$
Obs. left of c	219	94	190	123	126	187	96	217
Obs. right of c	93	40	77	56	51	82	38	95
Effective obs. left	37	18	35	24	21	47	15	52
Effective obs. right	34	20	29	29	15	50	12	48
Polynomial order	1	1	1	1	1	1	1	1
Bandwidth	7.85	7.92	8.72	7.92	8.23	10.41	7.11	9.43

Note: This table presents the results of the RD estimation for municipalities with a high vs. low share of immigrants, at different cutoffs in the share of immigrants. Each model is re-estimated by spliting the original dataset at different quantiles and estimating equation 2 separately for each group of municipalities, with the MSE-optimal bandwidth and a local-linear polynomial order. The results for a split at the median ($q = 0.5$) with the same specification are presented in Table 6.

A.3 Figures and tables

A.3.1 List of tables

A.3.2 List of figures