

# Regression Discontinuity Designs Based on Population Thresholds: Pitfalls and Solutions

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## ABSTRACT

In many countries, important features of municipal government (such as the electoral system, mayors' salaries, and the number of councillors) depend on whether the municipality is above or below arbitrary population thresholds. Several papers have used a regression discontinuity design (RDD) to measure the effects of these threshold-based policies on political and economic outcomes. Using evidence from France, Germany, and Italy, we highlight two common pitfalls that arise in exploiting population-based policies (compound treatment and sorting) and we provide guidance for detecting and addressing these pitfalls. Even when these problems are present, population-threshold RDD may be the best available research design for studying the effects of certain policies and political institutions.

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## I. INTRODUCTION

Researchers attempting to estimate the effects of policies face serious endogeneity problems: it is usually impossible to run an experiment in which consequential policies are randomized, and in most observational data it is difficult to locate or construct valid counterfactuals given the various strategic and contextual factors that affect policy choices. In recent years, many researchers have attempted to address these problems by exploiting cases in which policies at the subnational (usually municipal) level depend discontinuously on population thresholds. The use of regression discontinuity designs (RDDs) based on population thresholds was first suggested by Pettersson-Lidbom (2006, 2012), who evaluated the effect of the size of the municipal council on the extent of municipal spending in Sweden and Finland by comparing cities above and below population thresholds that determine council size. Subsequent researchers have used population-threshold RDDs to study the effects of the salary of public officials (see Gagliarducci and Nannicini 2013; Ferraz and Finan 2009; van der Linde et al. 2014; De Benedetto and De Paola 2014), gender quotas (see Campa 2011; Casas-Arce and Saiz 2015), electoral rules (see Fujiwara 2011; Hopkins 2011; Pellicer and Wegner 2013; Barone and De Blasio 2013; Eggers 2015; Gulino 2014), direct democracy (see Arnold and Freier 2015; Asatryan et al. 2013; Asatryan, Baskaran and Heinemann 2014), fiscal transfers (see Litschig and Morrison 2010, 2013; Brollo et al. 2013; Baskaran 2012), and (like Pettersson-Lidbom (2006, 2012)) council size (see Egger and Koethenbueger 2010; Koethenbueger 2012). We survey twenty-eight papers using population-threshold RDDs in Table A1 in the Online Appendix. The existing literature has evaluated population-based policies in twelve countries on four continents, including the U.S., Spain, Brazil, Morocco, India and Japan; our own casual search quickly yielded examples of similar policies in several other countries where to our knowledge no such study has been carried out (e.g., the U.K., Belgium, Austria, Norway, Poland, Slovakia and Mongolia). Fundamentally, the population-threshold RDD is an attractive research design because at the relevant population threshold we can compare sets of cities that implemented different policies but are comparable in other important respects.

In this paper we highlight two pitfalls that (based on our study of France, Italy, and Germany) complicate the use of population-threshold RDDs. The first pitfall is that the same

population threshold is often used to determine multiple policies, which makes it difficult to interpret the results of RDD estimation as measuring the effect of any one particular policy. We show the extent of compound treatment in the three countries we study, emphasizing that extensive institutional background research is necessary before one can interpret the results of a population-threshold RDD as evidence of the effect of a particular policy.<sup>1</sup> When discussing potential remedies, we also highlight the “difference-in-discontinuities” design as a possible solution in cases where a treatment of interest changes in tandem with other policies but one can locate a comparable period or setting where these other policies change on their own.

The second pitfall is sorting – the tendency of municipalities to strategically manipulate their official population in order to fall on the desired side of a consequential population threshold. It is well known that the continuity assumption, necessary for identification in the RDD, may not hold when there is precise manipulation of the running variable (Imbens and Lemieux 2008; McCrary 2008; Lee and Lemieux 2010); evidence of manipulation has been produced by Urquiola and Verhoogen (2009) for the case of class size, Barreca et al. (2011) for birth weight, and Caughey and Sekhon (2011) for close elections (though see also Eggers et al. 2015). Our main contribution here is to show conclusive evidence of manipulative sorting in official population numbers in France, Italy, and Germany;<sup>2</sup> we also show that the standard tests for sorting are biased when the running variable is discrete (as in the case of population-threshold RDDs), and we highlight some of the special challenges involved with assessing covariate imbalance in settings where data is pooled from multiple thresholds.

The evidence we present from France, Italy, and Germany shows why carrying out population-threshold RDD in these countries requires care; readers should not conclude, however, that population-threshold RDDs are always problematic or that there are better ways to study the policies that have been addressed with population-threshold RDDs. We suspect that both compound treatment and manipulative sorting are serious problems in many countries that use population thresholds to assign municipal policies, but even in the countries we study one

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<sup>1</sup>See Keele and Titiunik (2015) for a discussion of compound treatment in RDDs based on geographical boundaries.

<sup>2</sup>Related to our work, Litschig (2012) looks at top-down manipulation of population figures in Brazil; Foremny, Monseny and Solé Ollé (2015) highlight the issue of sorting around population figures in Spain; and a privately-circulated paper by Kristof De Witte, Benny Geys, and Joep Heirman discusses sorting in Belgium.

can identify policies and thresholds such that neither compound treatment or sorting appears to pose much of a problem. When these problems do arise, there are remedies that we discuss that involve weaker assumptions than would be necessary for any feasible alternative design. The countries we study are also not representative of all settings where population-thresholds may be carried out; we chose these countries both because many municipal policies depend on population thresholds (and they have done so for a long time<sup>3</sup>) and because we are familiar with these cases from previous work, but we suspect that compound treatment is less pervasive in countries where fewer policies depend on population thresholds (e.g., see Hopkins (2011) on the U.S.) and sorting is less problematic in countries where municipal population counts are linked more closely to national administrative data (e.g., see Pettersson-Lidbom (2012) on Finland and Sweden). Especially given the general challenges we face in studying the effects of policies, it would be a mistake to conclude from our analysis that population-threshold RDDs should be eschewed in favor of other designs – not just because these problems do not afflict every population-threshold, and not just because there are solutions (which we discuss in depth) to these problems, but also because even in the face of these problems a population-threshold RDD may be preferable to the next best design.

## II. COMPOUND TREATMENT

The population-threshold RDD is appealing because it allows the researcher to compare outcomes in a set of cities where one subset is required to implement one policy (say,  $A$ ) while another identical-in-expectation subset is required to implement another policy ( $A'$ ). The first common problem we highlight in this paper is that often the population threshold that determines whether policy  $A$  or  $A'$  is applied will also determine whether other policies ( $B$  or  $B'$ ,  $C$  or  $C'$ , and so on) are applied; the policy change we hope to study ( $A$  vs  $A'$ ) is thus confounded with other policy changes, undermining the appeal of the RDD. We refer to this

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<sup>3</sup>The first law on municipal government in revolutionary France (passed 14 December of 1789) includes six provisions dictating features of municipal government as a function of population, including a rule specifying six population thresholds determining the council size. An 1808 reform in Prussia used population cutoffs of 3,500 and 10,000 to assign different rules on council size, voting rights, and budget transparency (among others). In Italy, the *Legge Lanza* of 1865 specified population cutoffs determining council size, executive committees, and voting rights in the former Kingdom of Piedmont and Sardinia.

situation as one with “compound treatment.”<sup>4</sup>

#### *A. Documenting the extent of compound treatment*

Figure 1 summarizes the problem based on our investigation of laws applying to municipalities in France, Italy, and German states. Each dot indicates a population threshold at which at least one policy changes; solid dots indicate that more than one policy changes at the same threshold. In every case, there are some thresholds where just one policy changes, but such thresholds are in the minority.

Table 1 provides detail on the various policies that change at population thresholds up to 50,000 in France; the Online Appendix provides details about population-dependent policies in Italy and Germany (see Tables A2 and A3). As Table 1 indicates, at *every* threshold at which council size increases, the maximum number of deputy mayors also increases, which makes it impossible to disentangle the effect of council size from the effect of additional paid council staff. There is only one threshold (1,000 inhabitants) at which the salary of mayors and deputy mayors increases without the council size also increasing. Many of the most interesting policies change at a single threshold of 3,500 inhabitants at which several other policies (including council size and mayor’s wage) also change: the electoral rule used to elect the council, the requirement of gender parity in party electoral lists, and the requirement that the council debate the budget before adopting it.

In the thirteen German states a total of sixty-five different types of municipal policy depend on population thresholds; no state has fewer than fourteen different policies that are determined by population thresholds. (See Appendix Table A3 for details.) The thresholds determining these policies vary across states, ranging from seventy inhabitants to one million. Importantly, of 759 policy-threshold observations across German states (i.e. cases where a policy changes at a given threshold in a given state), only ninety-four do not coincide with another policy change. For mayoral salary, certainly one of the most important of these policies, we find only twelve cases (of 116 in total) in which no other policy changes at the same threshold in the

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<sup>4</sup>Keele and Titiunik (2014) use this term in the same way in their discussion of spatial RDD. Note that in the epidemiological literature Hernan and VanderWeele (2011) use “compound treatments” to refer to situations where there are different versions of treatment.

state.<sup>5</sup>

Detecting whether a given treatment is confounded with another treatment can simply be a matter of scouring the legal code for mentions of population thresholds.<sup>6</sup> In some cases enumerating the full set of policies that change at a threshold is more complicated, however, because some policies depend on population thresholds only indirectly. An example of this type of second-order policy is given in Lyytikäinen and Tukiainen (2013): the maximum number of candidates on electoral lists in Finland is a function of the council size, which changes discontinuously at population thresholds.<sup>7</sup> Another example from Baskaran and Lopes da Fonseca (2015) highlights how subtle the interactions among policies can be: in German municipal elections, parties winning less than a certain vote share are denied representation on the council; this constraint is never binding when the municipal council is below a certain size, which implies that there is a population threshold at which the council size increases *and* a vote share cutoff goes into effect (though this would not be clear without detailed knowledge of the electoral system). In short, a researcher should know a setting intimately before concluding that a given policy (and not other policies) changes at a given population threshold.

### *B. Addressing compound treatment*

Suppose a policy of interest is determined by a population threshold, but other policies change at the same threshold. How can a researcher proceed?

Consider a simple setup where the observed outcome is equal to the potential outcome associated with the set of compound treatments actually received by municipality  $i$  at time  $t$ :  $Y_{it} = Y(\mathbf{K}_{it})$ , where  $\mathbf{K}_{it} \in \mathbb{R}^k$  is a  $k$ -dimensional vector containing the realizations of  $k$

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<sup>5</sup>In Germany and other federal systems the task of locating relevant thresholds is complicated by the fact that higher level authorities may also enact policies based on municipal population thresholds; in Germany, for example, the federal statistical office used a different procedure to implement the 2011 census for municipalities above and below 10,000 inhabitants.

<sup>6</sup>In the first paper for Bavaria in Germany, see Egger and Koethenbueger (2010), the authors studied the effects of council size on municipal expenditures using population threshold rules for council size. Due to the difficulties of detecting the legislation in the vast amount of municipal code, it went unnoticed that the same thresholds are used to determine an array of other policies (e.g. direct funding for the towns of different sizes, direct democratic provisions). As no individual threshold for council size is unique, the effects of those additional treatments cannot be separated from the council size treatment.

<sup>7</sup>Lyytikäinen and Tukiainen (2013) use an instrumental variables approach to tackle the compound treatment issue.

(binary) treatments. Without loss of generality, assume the treatment of interest (say, policy  $A$ ) is contained in the first cell of the vector  $\mathbf{K}_{it}$ , which can thus be decomposed as  $\mathbf{K}_{it} = (A_{it}, \mathbf{V}'_{it})'$ , where  $\mathbf{V}_{it}$  is a  $(k - 1)$ -dimensional vector containing all treatments but policy  $A$ . We refer to  $\mathbf{K}_{it}$  as the vector of all *compound treatments*, and to  $\mathbf{V}_{it}$  as the vector of the *confounding treatments* with respect to the policy of interest  $A$ . Further assume that treatment assignment sharply changes in population size,  $P_{it}$ , at the cutoff  $P_c$ . In particular, at time  $t = t_1$ , policy  $A$  is in place for municipalities above  $P_c$ , but not for those below  $P_c$ . The same cutoff, however, triggers a change in the confounding treatments too. Formally:

$$\mathbf{K}_{it} = \begin{cases} \mathbf{K}_{1v} & \text{if } P_{it} \geq P_c, t = t_1 \\ \mathbf{K}_{0\bar{v}} & \text{if } P_{it} < P_c, t = t_1 \end{cases}$$

where  $\mathbf{K}_{1v} = (1, \mathbf{v}')'$  and  $\mathbf{K}_{0\bar{v}} = (0, (\mathbf{1} - \mathbf{v})')'$ .

To identify any causal effect, the simplest option is to change the quantity of interest and focus on the bundle of policies that simultaneously change at  $P_c$ . In fact, under some circumstances, it may be worth studying the effect of the bundle of policies  $\mathbf{K}_{1v}$  vs  $\mathbf{K}_{0\bar{v}}$ . In France, for example, changes in council size always coincide with changes in the number of deputy mayors; the perfect confounding of these two policies means that it is impossible to separate the effect of the two treatments, but one may still estimate the effect of this bundle of policies. The downside is that, as the number of compound treatments  $k$  increases, it becomes more difficult to motivate and interpret the analysis beyond the immediate setting being considered.

If we want to keep the focus on the policy of interest  $A$ , we need to make some *ignorability* assumption with respect to the effects of the confounding treatments (Keele and Titiunik 2014). One strong assumption would be that:  $Y(\mathbf{K}_{1v}) = Y(\mathbf{K}_{1\bar{v}})$  and  $Y(\mathbf{K}_{0v}) = Y(\mathbf{K}_{0\bar{v}})$ , i.e., the confounding treatments do not affect the outcome. Under this assumption and the standard assumption of *continuity* in potential outcomes (Hahn, Todd and Van der Klaauw 2001), the cross-sectional RDD estimator at  $t_1$  identifies the (local) average treatment effect

of policy  $A$  in the neighborhood of  $P_c$ :

$$\begin{aligned}
\hat{\tau}_{RDD} &\equiv \lim_{p \rightarrow P_c^+} E[Y_{it}|P_{it} = p, t = t_1] - \lim_{p \rightarrow P_c^-} E[Y_{it}|P_{it} = p, t = t_1] \\
&= E[Y(\mathbf{K}_{1v}) - Y(\mathbf{K}_{0\tilde{v}})|P_{it} = P_c, t = t_1] \\
&= E[Y(\mathbf{K}_{1v}) - Y(\mathbf{K}_{0v})|P_{it} = P_c, t = t_1] \\
&= E[Y(\mathbf{K}_{1\tilde{v}}) - Y(\mathbf{K}_{0\tilde{v}})|P_{it} = P_c, t = t_1],
\end{aligned}$$

where the last two expressions represent the (local) average treatment effect of policy  $A$  conditional on the fact that the confounding policies are equal to  $\mathbf{v}$  and  $\tilde{\mathbf{v}}$ , respectively. The fact that they are equal simply means that, under continuity and ignorability, the RDD estimator identifies the (local) average treatment effect of  $A$  with no further restrictions.

The ignorability assumption, however, is hardly plausible in most empirical settings. And if it is not met, the RDD estimator cannot identify any causal effect of policy  $A$  alone:

$$\hat{\tau}_{RDD} = E[Y(\mathbf{K}_{1v}) - Y(\mathbf{K}_{0v})|P_{it} = P_c, t = t_1] + E[Y(\mathbf{K}_{0v}) - Y(\mathbf{K}_{0\tilde{v}})|P_{it} = P_c, t = t_1],$$

where the first term is one of the (local) average treatment effects of policy  $A$  that researchers may want to estimate, and the second is the bias introduced by the confounding policies.

To remove this bias and isolate the causal effect of policy  $A$  alone, the most promising way to proceed is to look for other settings where the confounding policies change but the policy of interest does not; under assumptions we lay out shortly, the difference between the effect of all compound treatments ( $\mathbf{K}_{it}$ ) and the effect of the confounding policies ( $\mathbf{V}_{it}$ ) gives an unbiased estimate of the effect of policy  $A$ . This approach, which combines features of the regression discontinuity design and the difference-in-differences design, is what Grembi, Nannicini and Troiano (2016) call the “difference-in-discontinuity” (diff-in-disc) design. Here, we present this framework to a broader audience, generalize the results to the case of multiple compound treatments, and elaborate on different ways the diff-in-disc estimator can be applied.

To see how the diff-in-disc design can work in practice, assume that policy  $A$  was introduced at time  $t_1$ , but researchers also have information on time  $t_0$ , when the confounding treatments (but not  $A$ ) changed at  $P_c$ . Formally:

$$\mathbf{K}_{it} = \begin{cases} \mathbf{K}_{1v} & \text{if } P_{it} \geq P_c, t = t_1 \\ \mathbf{K}_{0v} & \text{if } P_{it} \geq P_c, t = t_0 \\ \mathbf{K}_{0\tilde{v}} & \text{if } P_{it} < P_c \end{cases}$$



In addition to the standard continuity assumption, identification rests on the following assumption of *local parallel trends*.

ASSUMPTION 1:  $E[Y(\mathbf{K}_{0v}) - Y(\mathbf{K}_{0\tilde{v}})|P_{it} = P_c, t = t_1] = E[Y(\mathbf{K}_{0v}) - Y(\mathbf{K}_{0\tilde{v}})|P_{it} = P_c, t = t_0]$ ,  
 $E[Y(\mathbf{K}_{1v}) - Y(\mathbf{K}_{1\tilde{v}})|P_{it} = P_c, t = t_1] = E[Y(\mathbf{K}_{1v}) - Y(\mathbf{K}_{1\tilde{v}})|P_{it} = P_c, t = t_0]$ .<sup>8</sup>

This assumption can be interpreted from two perspectives. Most directly, it states that the effect of the confounding policies ( $\mathbf{v}$  vs  $\tilde{\mathbf{v}}$ ), holding fixed policy  $A$ , is time invariant. In other words, municipalities just above and just below  $P_c$  would have been on parallel trends between  $t_0$  and  $t_1$  had policy  $A$  not been introduced at  $t_1$ . (Note that this assumption is more local than the standard parallel trends assumption in difference-in-differences, as it must hold only in the neighborhood of the policy threshold  $P_c$ .) From a different angle, the assumption states that the (time) difference in potential outcomes between  $t_0$  and  $t_1$ , again holding fixed policy  $A$ , must be continuous in population size at  $P_c$ . From this second perspective, the assumption is analogous to the RDD assumption of continuity in potential outcomes across the threshold.

In a setting with two compound treatments, Grembi, Nannicini and Troiano (2016) show how the above assumption is sufficient for identification. Indeed, under continuity and local parallel trends, the diff-in-disc estimator yields the (local) average treatment effect of policy  $A$  conditional on  $\mathbf{V} = \mathbf{v}$ :

$$\begin{aligned}\hat{\tau}_{DDISC} &\equiv \left( \lim_{p \rightarrow P_c^+} E[Y_{it}|P_{it} = p, t = t_1] - \lim_{p \rightarrow P_c^-} E[Y_{it}|P_{it} = p, t = t_1] \right) \\ &\quad - \left( \lim_{p \rightarrow P_c^+} E[Y_{it}|P_{it} = p, t = t_0] - \lim_{p \rightarrow P_c^-} E[Y_{it}|P_{it} = p, t = t_0] \right) \\ &= E[Y(\mathbf{K}_{1v}) - Y(\mathbf{K}_{0\tilde{v}})|P_{it} = P_c, t = t_1] - E[Y(\mathbf{K}_{0v}) - Y(\mathbf{K}_{0\tilde{v}})|P_{it} = P_c, t = t_0] \\ &= E[Y(\mathbf{K}_{1v}) - Y(\mathbf{K}_{0v})|P_{it} = P_c].\end{aligned}$$

Note that the estimand identified above is narrower than the standard RDD estimand, as it is conditional on specific realizations of the confounding treatments (i.e.,  $\mathbf{V} = \mathbf{v}$ ). This interpretation reduces the external validity of the results, especially if the number of confounding treatments is large. For example, assuming that  $A$  is the mayor's wage and  $\mathbf{V}$  contains the

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<sup>8</sup>As shown below, only the first part of the assumption (i.e., the part conditional on the case of no treatment,  $A = 0$ ) is needed for identification, but the second part can be used to extrapolate the identified estimand beyond  $t_1$ .

electoral rule (majoritarian vs proportional), the size of the city council, and the amount of federal transfers, the above result tells us that the diff-in-disc design recovers the causal effect of the wage *only* for municipalities that use a majoritarian system, elect a big-sized city council, and receive large transfers from the federal government. In order to identify the standard RDD estimand represented by the average treatment effect of  $A$  in a neighborhood of  $P_c$  (i.e., in our example, the causal effect of the mayor’s wage irrespective of the electoral system, the council size, and the amount of federal transfers), we need to introduce a *separability* assumption about the effects of the compound treatments.

ASSUMPTION 2:  $Y(\mathbf{K}_{it}) = Y(\mathbf{0}) + \tau'_k \mathbf{K}_{it}$ ,

where  $\tau_k$  is a  $k$ -dimensional vector containing the (additively separable) effects of the compound treatments:  $(\tau_a, \tau_b, \dots, \tau_k)'$ . It is straightforward to show that, under this further assumption, the diff-in-disc estimator identifies the (local) average treatment effect of  $A$  in the entire neighborhood of the threshold:  $\hat{\tau}_{DISC} = E[\tau_a | P_{it} = P_c]$ .

Given a setting where a policy of interest changes along with other confounding policies, then, we can use the diff-in-disc design to recover the effect of the policy of interest if we have a second setting in which the confounding policies change on their own *and* if we are willing to assume that the effect of changing the confounding policies (holding fixed the policy of interest) is the same in the two settings. In what situations is this possible?

Grembi, Nannicini and Troiano (2016) illustrate what we might call a “longitudinal diff-in-disc design” in order to estimate the effect of fiscal constraints on deficits. Starting in 2001, Italian municipalities below 5,000 were exempted from fiscal constraints that applied to larger cities. A cross-sectional RDD analysis in the post-2001 period using the 5,000 population threshold would thus seem like a good way to study the effect of fiscal constraints vs. no fiscal constraints. The problem is that (as noted in Table A2 of this paper) the salary of the mayor and other executive officers also changes at the 5,000 threshold. Grembi, Nannicini and Troiano (2016) thus implement a diff-in-disc design in which the cross-sectional RDD effect at the 5,000 threshold before 2001 (when fiscal constraints applied to all municipalities) is subtracted from the same effect after 2001 (when fiscal constraints only applied to municipalities above 5,000

in population). This procedure yields a consistent estimate of the effect of fiscal constraints under the local parallel trends assumption that the effect of the other policies that change at this threshold is stable over time and the separability assumption that the effect of fiscal constraints does not depend on these confounding policies. Campa (2011) and Casas-Arce and Saiz (2015) provide other examples of diff-in-disc designs with population thresholds in the estimation of the impact of electoral gender quota in Spain.

Researchers can also consider what we might call a “cross-sectional diff-in-disc design” to address the problem of compound treatment. The key requirement of the cross-sectional diff-in-disc is that the confounding policies also change at some other threshold or in some other region where the local parallel trends assumption and the separability assumption are plausible, i.e., the effect of the confounding policies is plausibly the same in the two settings and does not depend on the value of the policy of interest. Arnold and Freier (2015) and Eggers (2015) provide evidence in this spirit by comparing RDD effects measured at different thresholds in the same system in order to “difference out” the effects of confounding policies. Gagliarducci and Nannicini (2013) compare the RDD effects at the same threshold but for mayors facing different institutional constraints (i.e., term limit), and in order to make the two settings comparable between each other restrict their analysis to mayors who served for two terms. The same approach could of course be used when the effect of the confounding policies can be measured in an entirely different region or country where the policy of interest does not change; the attractiveness of this design of course depends on the plausibility of the local parallel trends assumption.

### III. SORTING

As mentioned above, the appeal of a population threshold-based RDD is partly that the political unit does not choose the policy, which suggests that units just above and below the threshold should be comparable in all respects other than the policy. As is well known, such an RDD (like any RDD) is less appealing when the units can influence the variable that determines treatment assignment (i.e., population). At an extreme, one could imagine that cities near a population threshold could perfectly control whether they end up above or below the threshold,

and thus cities that have policy  $A$  differ from cities that have policy  $A'$  not just in that policy but also in a whole host of background characteristics that affected whether they prefer policy  $A$  or policy  $A'$ .<sup>9</sup> In such a situation, an RDD may be no better than a typical observational study in which political units choose their policies.

The problems of strategic sorting in RDD applications are well known (see Imbens and Lemieux (2008); Lee and Lemieux (2010); Urquiola and Verhoogen (2009); Barreca et al. (2011)).<sup>10</sup> Strategic sorting in population figures has been documented by Litschig (2012) for Brazil and it has been briefly mentioned by Gagliarducci and Nannicini (2013) for the Italian case. One of our contributions here is to provide evidence that sorting in RDD studies based on population thresholds is an issue in all three countries that we study. We also demonstrate techniques for diagnosing and explaining manipulation, as well as potential solutions to address this problem.

#### *A. Aggregate graphical evidence*

The basic pattern of sorting is documented in Figures 2 (France), 3 (Italy), and 4 (Germany). Because the figures use the same format and reflect the same analysis, we explain the French case in detail and subsequently note only the relevant differences between the French case and the others.

In France, we have population data for eight censuses between 1962 and 2011.<sup>11</sup> For each census, we calculate the difference in population between each city and each major population threshold (i.e., one affecting a policy listed in Table 1) that was in force at the time of the census; we store all municipality-years in which a city's population was within 250 inhabitants of a threshold. In the left panel of Figure 2 we plot three histograms of these population

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<sup>9</sup>Alternatively, it may be that only certain cities are able to control whether they end up above or below the threshold, in which case cities that have policy  $A$  may differ from cities that have policy  $A'$  not only in the factors that affect their policy preferences but also in the factors that affect their ability to manipulate their population figures.

<sup>10</sup>Davezies and Le Barbanchon (2015) address the problem of measurement error in the running variable, which stands to manipulative sorting in RDD as weak instruments stand to endogeneity in the instrumental variables framework.

<sup>11</sup>The census years are 1962, 1968, 1975, 1982, 1990, 1999, 2006, and 2011. After 1999, France introduced a new census system that produces annual population estimates for all municipalities; the 2006 census was the first such census.

differences, one for each group of relevant population thresholds (100; 500 or 1,000; and 1,500 and larger). Because there are so many municipality-years, we plot histograms with bin widths of 1. The key evidence of sorting is given by the jumps in each histogram at 0. For example, based on the histogram for the 100-inhabitant threshold, we can see that there were just under 500 cases in which a city was one person short of the 100-inhabitant threshold at which the council size increases, but there were almost 600 cases in which a city cleared that hurdle by one person. The jump is even more striking for the 500- and 1,000-inhabitant thresholds (where the mayor's salary increases).

In the right panel of Figure 2 we depict the McCrary test for all thresholds pooled. This procedure estimates the density of the running variable (i.e., absolute distance in inhabitants to a population threshold) separately on the left and right of the threshold and tests for a jump or drop in the density at the threshold. Not surprisingly (given the histograms in the left panel), the McCrary test indicates a large jump in the estimated density at the threshold.

Figure 3 indicates an even more striking pattern for Italy. Based on the five decennial censuses from 1961 to 2001, we find about 90 cases in which a city cleared the 1,000 or 3,000 population threshold (at which the mayor's wage increases, among other changes) by fewer than 10 inhabitants, but we find only about 20 cases in which a city fell short by fewer than 10 inhabitants; in over 300 cases a city cleared one of the thresholds by fewer than 30 inhabitants, but in fewer than 100 cases did a city fall short by fewer than 30. The pattern of sorting is just as clear (if not as dramatic) at larger thresholds. Again, the McCrary test aggregating all thresholds (right panel) indicates a very large jump in the estimated density at the threshold.

Figure 4 shows the same analysis for Germany. Here we have annual administrative data from 1998-2007 for municipalities from all German states, and our analysis is based on a comparison of each municipality's population to all thresholds in force in that municipality's state. The histograms (left panel) indicate that sorting is nowhere near as severe here, but the McCrary test (right panel) does indicate a significant jump in the density just above the threshold. Note that here we focus on thresholds at which either the mayor's salary or the council size changes, which makes for a closer comparison with the French and Italian analysis; when we include the entire set of thresholds we find less evidence of sorting, as will be seen

in the next section where we carry out McCrary analysis in each country for specific types of thresholds.

### *B. Formal tests at different types of thresholds*

We now carry out the McCrary (2008) test for different types of thresholds within countries, still pooling population figures from the various censuses we have collected. Before showing the results, we note that our analysis here and throughout the paper takes account of two biases (previously unrecognized, as far as we know) that arise when applying the standard McCrary test to a discrete running variable. The McCrary test operates by conducting RDD analysis on an under-smoothed histogram of the running variable.<sup>12</sup> The first bias arises because applying the standard algorithm to a discrete running variable tends to result in a histogram with more observations in the first bin to the right of the threshold than in the first bin to the left, even when the density is perfectly flat; fundamentally, this asymmetry arises because with a discrete running variable one can have observations *exactly* at the threshold, and by default these observations are assigned to the first bin to the right. We address this problem by requiring that the bin width of the histogram take an integer value;<sup>13</sup> alternatively, one can simply set the threshold to be -0.5, which eliminates the asymmetry as long as the bin size is not exactly 0.5, 1.5, etc. The second bias arises when a discrete-valued running variable is analyzed using relative deviations from thresholds of different size, e.g. percentage distance from thresholds of 500, 1000, and 10,000 inhabitants; this creates a bias because all thresholds can produce a relative deviation of 0 (which by default goes into the first bin to the right of the threshold) but only very large thresholds can produce a relative deviation of  $-\epsilon$ . We address this problem by using absolute deviations rather than relative deviations. We explain these biases (both of which tend to increase the likelihood of falsely detecting sorting, especially when data is very plentiful) and our solutions to them in the Online Appendix.

Table 2 reports the results of McCrary analysis (incorporating these adjustments) at different types of thresholds in all three countries. In the top row we assess evidence of sorting in

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<sup>12</sup>For a recent literature that estimates discontinuities in densities and avoids pre-binning of the data see Otsu, Xu and Matsushita (2013); Cattaneo, Jansson and Ma (2015*b,a*).

<sup>13</sup>More specifically, we force the bin size of the McCrary algorithm to the closest integer value to the one chosen by default.

all thresholds, reporting the point estimate (i.e. the effect of crossing the threshold on the log density) and standard error for each test, along with the number of thresholds and observations.<sup>14</sup> Consistent with the previous figures, we find very clear evidence of substantial sorting in France and Italy (with the latter being quite a bit larger) and evidence of small but statistically significant sorting in Germany. In the other rows we assess sorting at particular types of thresholds, e.g., thresholds where the salary of the mayor increases, or thresholds where the council size increases, or thresholds where both increase. In France, we find significant sorting at all types of thresholds, with the smallest effect (and weakest evidence against the null) at thresholds where council size increases (but not mayor’s wage) and thresholds at 3,500 inhabitants or higher. In Italy the estimated effects are much larger, with (as in France) smaller effects at larger population thresholds. To give a sense of magnitude, a McCrary effect size of 1.3 (the effect for Italy at all thresholds) implies that the density on the right of the average threshold is almost four times larger than on the left. In Germany the jumps in density are statistically significant for most subsets and smaller but still fairly substantial in magnitude: at thresholds where both the council size and the mayor’s salary increases, for example, there are about 15% more cities immediately to the right of the threshold than immediately to the left. The fact that sorting appears to be more severe when we focus on thresholds determining salary and council size is consistent with the idea that local officials strategically manipulate population figures to obtain desirable policies; at these thresholds there is a clear incentive to pass the threshold, while at some others (e.g. thresholds above which cities are subject to more stringent financial oversight) we would if anything expect sorting in the other direction.

Comparing the effects by threshold size shows larger effects for smaller thresholds in Italy and France, suggesting that population size is more easily manipulated in smaller towns. Intriguingly, in Germany the pattern is reversed, with somewhat larger effects at larger thresholds, which may be partly explained by the fact that the salary of mayors in Germany often only increases at larger population thresholds.<sup>15</sup>

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<sup>14</sup>We count only thresholds for which we observe cities within 250 inhabitants of the threshold, which explains why some of the counts differ from the analysis above.

<sup>15</sup>In additional analysis, we find that the evidence for sorting in Germany is somewhat sensitive to leaving out one German state at a time: for example, we cannot reject the null for no sorting at salary thresholds when Baden-Württemberg is dropped. Results are robust to dropping regions in France and Italy.

At the bottom of Table 2 we conduct the McCrary tests at thresholds at which no policy changes, as far as we are aware. We generated placebo thresholds by taking the midpoint between each actual threshold in each setting (e.g., in France the smallest placebo threshold is 300, which is halfway between 100 and 500) and adding an arbitrary number (117 was picked). In none of the countries do we find discontinuities in the density at these placebo thresholds.

### *C. How does sorting happen?*

The evidence above is consistent with the view that in many municipalities in France, Italy, and Germany, officials and/or citizens respond to population-based policies by manipulating population numbers. We now ask briefly how such manipulation might take place – both because it might indicate how widespread sorting is likely to be beyond these three countries and because it might help us understand the extent to which sorting endangers our ability to learn from RDD in these and other settings.

It may be useful to distinguish among three distinct types of local behavior that could produce the manipulative sorting we observe. First and most simple is *fraud*: officials could simply falsify population numbers, inventing or ignoring residents in order to achieve desired population numbers. Second is what we call *selective precision*: when a municipality is known to be close to a consequential population threshold, officials can order extra checks or selectively expedite/delay procedures in a way that increases the municipality’s chances of crossing the threshold.<sup>16</sup> (For example: an initial count indicates 999 residents; the mayor asks that the figures be re-checked, perhaps focusing on whether all new arrivals have been properly processed.) Third is *strategic recruitment*: a municipality could make efforts to attract residents (or repel them) by expediting permits or offering tax incentives or simply encouraging friends to change their official residence.

Do local officials have the means, motive, and opportunity to implement these sorting strategies in the countries we study? The assignment of consequential policies (e.g. the salary of the mayor or the electoral system) based on population thresholds in all three countries provides a clear motive. Local officials in each country are also sufficiently involved in the

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<sup>16</sup>Thus selective precision differs from fraud because legitimate procedures are accurately carried out; the key is that officials exercise discretion over which procedures are implemented and/or with what degree of effort.



census and in housing and tax policy to have the means to manipulate. In both France and Italy, mayors are responsible for supervising the census survey at the local level, including hiring and training enumerators; in Germany, municipal registry offices provide reports of births, deaths, and in- and out-flows that state statistical offices use to update census numbers. In all three countries municipalities are also involved in local development and tax decisions, which suggests that they have the means to recruit residents. Whether local officials have the opportunity to implement these strategies is somewhat more difficult to say. Fraudulently adjusting or fabricating census surveys in order to achieve a desired population number seems risky in systems where central authorities oversee local procedures. For all three mechanisms, the pattern of sorting suggests that local officials must have very precise information about the municipality's unmanipulated census count at the time when they decide whether or not to engage in manipulation. To see why, note that the most striking feature of Figures 2, 3, and 4 is the deficit of cities narrowly below the relevant thresholds. This indicates that potential manipulators know not just whether the municipality is likely to be very close to the threshold (because cities 1 inhabitant below the threshold appear to be much more likely to manipulate than cities 5 or 10 below) but also which side of the threshold they are likely to end up on (because cities 1 inhabitant below the threshold appear to be much more likely to manipulate than cities 1 above).<sup>17</sup> This in turn suggests that the manipulation we observe is probably not the result of strategies that would require substantial time to implement, such as building new housing; if new residents are registered two years after permits are issued, for example, then cities would need good information two years before the census about whether the census count would put them just above or just below a threshold in the absence of new housing. The sorting we observe could, however, be the result of calling for an extra check after initial numbers are tallied (i.e., selective precision) or recruiting a friend from a neighboring municipality to move into a vacant apartment before the census takes place (i.e., strategic recruitment).

The case of France may be instructive in highlighting possible mechanisms for manipulative sorting. The French census is a joint project between the national statistics agency (INSEE)

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<sup>17</sup>If manipulation occurs through fraud, the observed pattern could be found if municipalities only knew whether they would be very close to the threshold. See Eggers et al. (2015) for a similar argument about the need for precise information in the case of close elections.

and local municipal authorities: INSEE issues directives; the municipalities hire and train enumerators and submit the results. Municipal authorities are thus involved in interpreting the complex rules that determine how to handle ambiguous cases such as students, members of the military, and people without fixed domiciles. The phenomenon of sorting in the French census was noted as early as 1972 by an INSEE official (Vernet 1972) who suspected that it could be explained by local officials making an extra effort to locate residents when initial tabulations indicated that they would otherwise narrowly fall below an important threshold; to the extent that these efforts involved locating actual residents (e.g., students who should be enumerated in the municipality), the official’s explanation falls under what we call “selective precision”. (If “locating” means “inventing”, we would call it fraud.) Consistent with this explanation, manipulative sorting in France appears to have diminished over time as central authorities have exercised more oversight over municipalities’ data collection procedures. Figure 5 depicts the point estimates and confidence intervals for McCrary tests at three different thresholds over time in France, clearly showing a decline in sorting since the 1980s and a particularly marked drop in the 1999 census. A former census official explained this pattern by noting that for the 1999 census INSEE instituted special measures to strengthen oversight of the census, particularly to ensure that students were only counted once; censuses after 1999 have used a new procedure that uses local tax files (which may be less prone to manipulation) to produce annual population updates.<sup>18</sup> The variation in sorting over time in France suggests that sorting is less likely to be an issue for population-threshold RDDs in countries like Sweden and Finland where local population figures are collected in a highly centralized way and linked to administrative records.<sup>19</sup>

#### *D. Addressing manipulative sorting*

The regression discontinuity design is obviously much less appealing when there is evidence of sorting around the threshold. What can a researcher do in such cases?

One approach is to augment the usual RDD analysis with control variables that capture

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<sup>18</sup>Personal correspondence with Jean-Michel Durr, former Census Director at INSEE.

<sup>19</sup>Consistent with this, Pettersson-Lidbom (2012) and Lyytikäinen and Tukiainen (2013) do not find evidence of sorting in Sweden or Finland.

possible confounding factors. When sorting introduces bias into RDD estimates, it does so because the distribution of covariates differs between the left and right side of the threshold. One way to eliminate this bias, therefore, is to measure these covariates and model their relationship to the outcome at the threshold. In this approach to sorting, an RDD thus becomes more like a typical observational study, in the sense that one must identify, measure, and control for additional variables. The credibility of the resulting model will depend on what we know about the process of sorting, the extent to which we can measure relevant covariates, and the number of observations near the threshold for model-fitting. It also depends on the extent to which the outcome varies with the unmanipulated running variable. In the best case, such analysis will retain much of the appeal of the ideal RDD; in the worst case, such analysis will be no more attractive (and possibly less attractive because of the loss in external validity) than a pure observational study.

To understand some of the considerations in addressing manipulative sorting through covariate adjustment, consider Figure 6, which captures what we think of as the best-case scenario. Because of sorting, a single binary covariate  $X$  is not continuous at the threshold, as shown in the left plot. The right plot shows how this induces bias in the RDD estimate: the expectation of  $Y$  conditional on  $P$  (our running variable) and  $X$  (the covariate) is completely flat everywhere, but due to the imbalance in  $X$  the expectation of  $Y$  conditional on  $P$  (but not conditional on  $X$ ) bends as we approach the threshold, such that the RDD estimate,  $\hat{\tau}_{RDD} \equiv \lim_{p \rightarrow P_c^+} E[Y|P = p] - \lim_{p \rightarrow P_c^-} E[Y|P = p] = Y^+ - Y^-$ , is larger than the effect of the treatment conditional on  $X = 1$  or  $X = 0$  (given by  $Y_{X=1}^+ - Y_{X=1}^- = \lim_{p \rightarrow P_c^+} E[Y|P = p, X = 1] - \lim_{p \rightarrow P_c^-} E[Y|P = p, X = 1]$ , for  $x = 0, 1$ ). The bias due to imbalance in  $X$  can, however, be removed by controlling for  $X$  in the RDD analysis. In this very simple case, where  $E[Y|P, X = 1] - E[Y|P, X = 0]$  is independent of  $P$ , controlling for  $X$  is as simple as additively including  $X$  in the regression. More generally, one could allow the control function to vary across levels of  $X$  or simply estimate the RDD separately across levels of  $X$ .

In practice, addressing sorting by controlling for covariates is typically more difficult than in this best-case scenario for several reasons. First, the task of accurately modeling the re-

relationship between the outcome and the covariate (conditional on the running variable) can be difficult; estimates become more dependent on modeling choices and subject to sampling variation. Second, even when we can address the bias due to imbalance in a covariate  $X$  at the threshold, we can never completely rule out the concern that our estimates are still biased due to imbalance in other (unobservable) covariates. For both of these reasons, we lose some of the attractive simplicity of the ideal RDD analysis, in which the entire focus is on estimating two conditional expectations at the threshold.<sup>20</sup>

To make matters worse, it should be remembered that we cannot rule out the possibility of covariate bias even when there is no sign of discontinuity in the density of the running variable (as McCrary (2008) noted), because sorting may go in both directions.<sup>21</sup> This suggests that every RDD study based on population thresholds should include extensive checks for covariate balance, whether or not there is evidence of sorting in the aggregate – particularly in settings where local officials play a role in producing official figures; when imbalance is evident, the robustness of conclusions to various control strategies should be shown. In the next section (Section IV) we assess the degree of covariate imbalance in the Italian case as an example.

As an alternative to covariate adjustment, researchers can also consider a “donut” RDD analysis that ignores data immediately surrounding the threshold (Barreca et al. 2011). In settings where the sorting appears to be limited to the immediate neighborhood of the threshold, this approach has the advantage that one does not need to measure and control for all potentially unbalanced covariates, nor does one need to worry about measurement error due to misreporting of the running variable. Of course, the very clear disadvantage of the donut approach is that as we drop more data near the threshold, our estimates of the conditional expectation function at the threshold require more extrapolation.

Building on the discussion of the difference-in-discontinuity design in Section III.B, in

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<sup>20</sup>Another problem is that manipulative sorting introduces measurement error that induces bias when the conditional expectation depends on the true value of the running variable. That is, cities just above and below the threshold likely differ in their true population, but this variable is not observed and thus cannot be controlled for in a straightforward way. The best way to address this bias would be to obtain good estimates of the true population and include this as a control variable in the analysis.

<sup>21</sup>For example, if authoritarian mayors want a small council and inclusive mayors want a large council, and manipulation of population figures is possible, then mayors just above and below a decisive population threshold may differ systematically even though the density is continuous. See Caughey and Sekhon (2011) for another possible example of imbalance without apparent sorting.

some circumstances one could take advantage of multiple thresholds to address sorting or at least give an idea of how problematic it is likely to be for one’s analysis. For example, one could extend the logic of the diff-in-disc to “partial out” the effect of sorting in the special case where a policy of interest changes discontinuously at a threshold at time  $t_1$  but not at time  $t_0$  and sorting occurs (perhaps due to confounding treatments) in both time periods. Under the assumption that the bias due to the combination of sorting and the confounding policies is the same just above the threshold in the two periods (an extension of the local parallel trends assumption), one can use the diff-in-disc to identify the effect of the policy of interest for treated municipalities just above the threshold; under the additional assumption that the effect of the policy of interest is the same for municipalities just above and below the threshold (an extension of the separability assumption), this is equal to the neighborhood average treatment effect. Both of these assumptions are likely to be less attractive than the usual diff-in-disc assumptions: the first assumption will not hold if the policy of interest affects the bias due to sorting, and the second assumption will not hold if the effect of the treatment is different for cities that managed to sort just above the threshold and those that did not.<sup>22</sup>

#### IV. SORTING AND COVARIATE IMBALANCE: THE CASE OF ITALY

The previous section provided clear evidence of manipulative sorting in France, Italy, and Germany around population thresholds determining municipal policies. This evidence indicates that the key assumption of RDD analysis (the continuity of potential outcomes across the threshold) may be violated in these cases. While we cannot directly test this assumption, we can test for covariate imbalance. In this section we conduct tests for covariate imbalance in Italy. Our goal in this section is to assess the extent of covariate imbalance in Italy and identify covariates that should be controlled for in RDD analysis in that setting. Along the way, we highlight some non-obvious issues that are likely to arise when we test for covariate imbalance using data drawn from multiple different thresholds and/or multiple censuses.

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<sup>22</sup>Less formally, and still building on the diff-in-disc idea but in a different way, one could compare RDD estimates at two thresholds where a policy of interest changes but the apparent degree of sorting is much larger at one threshold than the other; if the estimates at these two thresholds are similar, one could conclude that bias due to sorting plays a small role based on the assumption that this bias is increasing in the degree of sorting.

Our main approach to testing for covariate imbalance is to undertake a falsification test in which the covariate is viewed as the outcome in an RDD analysis. Figure 7 shows an example in which the dependent variable is the lagged treatment, meaning an indicator for whether a municipality was above a given population threshold in the *previous* census, given that it was close to that threshold in the current census. The top two plots show this analysis for thresholds at which the salary of the mayor increases. The top left panel shows that the probability of lagged treatment increases with the current running variable (as one might expect) but jumps at the threshold, indicating that cities narrowly above the threshold are more than 10 percentage points more likely to have been above the threshold in the previous census than cities narrowly below the threshold. The top right panel shows how the estimated jump varies with the (triangular) bandwidth employed for the local linear regression; the black dot shows the bandwidth suggested by the Imbens-Kalyanaraman algorithm (see Imbens and Kalyanaraman 2012) and employed in the figure at left and the black triangle shows the bandwidth suggested by the alternate procedure introduced in Calonico, Cattaneo and Titiunik (2014). This clear jump indicates that the RDD analysis for Italy could be biased by the fact that cities above and below the threshold differ systematically in whether they received the treatment in the past. The bottom two plots of Figure 7 use “placebo” thresholds (with no policy changes) and show no evidence of similar persistence, which indicates that the results above cannot be explained by generic stickiness in population figures from one census to the next.

How should this imbalance be interpreted? The most straightforward interpretation is that officials with influence over population figures prefer to prevent cities from crossing thresholds from one census to the next; for example, if a city has shrunk in population such that it is very close to a population threshold, someone is able to influence the final numbers to keep it above the threshold. Note, however, that a different and more subtle interpretation is also available. Recall that our analysis is based on combining observations near multiple different thresholds across multiple censuses. In such cases, covariate imbalance can emerge simply because the value of the covariate varies across thresholds/censuses *and* the degree of sorting varies across thresholds/censuses.

To see this, suppose we were combining data from a single threshold recorded in just two

censuses: one old census, at a time when cities near the threshold were shrinking, and one new census, at a time when cities near the threshold were growing. Suppose sorting was severe in the old census but not the new census. The difference in the severity of sorting means that in the combined data a larger proportion of the observations just above the threshold (compared to just below) will be taken from the old census; because cities were shrinking at the time of the old census, observations from the old census would be more likely to have been above the threshold in the past. Thus even if there were no imbalance in the probability of lagged treatment in either census, we could observe imbalance in this covariate in the combined data.<sup>23</sup> The larger point is that it is tempting to interpret imbalance in a particular covariate as the *cause* of the sorting (e.g. the probability of lagged treatment jumps because municipalities try not to cross thresholds), but it may simply be an artifact of pooling data from multiple censuses or thresholds in which the degree of sorting varies.

Table 3 addresses this complication by assessing imbalance across several covariates (indicated by rows of the table) while adding controls for the year of the census, the type of the threshold, and other factors. Each of the point estimates in this table is an RDD-based estimate of the effect of crossing population thresholds on the covariate.<sup>24</sup> The estimated effect on lagged treatment (examined graphically in Figure 7) is reported in column 1 of the first row as 0.138 (0.037). Columns 2 to 5 carry out the same analysis but additively include covariates in the RDD analysis: dummies for each year of the census (column 2); dummies for each threshold (column 3); both dummies (column 4); and a set of covariates describing Italian municipalities around the year 2002 (column 5).<sup>25</sup> Columns 6 to 8 show the models from columns 1, 3, and 5 but focusing on “placebo” thresholds where no policy changes. Note that in the absence of sorting we expect no effects in any of these tests; in the presence of policy-induced sorting we expect no effects in the placebo thresholds.

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<sup>23</sup>The same argument could be made when we aggregate data from various thresholds at a single point in time.

<sup>24</sup>For each outcome, we estimate the optimal bandwidth using the Imbens & Kalyanaraman approach; this appears in the first column. See Supplemental Information for the same analysis using CCT bandwidths (Calonico, Cattaneo and Titiunik 2014).

<sup>25</sup>The covariates are the (log) number of nonprofits per person, the proportion of inhabitants who give blood, the ratio of young to old inhabitants, an indicator for the South, an indicator for whether the municipality is on the seaside, and the proportion of second homes in the municipality. When a given covariate is used as the outcome it is obviously omitted from the list of regressors.

We have already seen (in Figure 7) that crossing salary thresholds seems to “affect” the probability of treatment in the previous census. In the top row of Table 3 we see that this imbalance persists when we control for the year of the census and the threshold being considered. This suggests that the imbalance in lagged treatment is not simply explained by variation in the extent of sorting over time or across thresholds. This imbalance does, however, mostly disappear when we include municipal covariates in column 5, which suggests that some of these municipal characteristics are unbalanced in a similar way, perhaps because they help explain which cities are able to sort. The second row indicates that we do not find a similar effect for the lagged running variable.

In the third and fourth rows of Table 3 we see strong evidence of imbalance in whether the council size changes at the threshold as well as in the year of the census being considered. This imbalance probably arises for the reason discussed above: sorting is worse at thresholds where both council size and salary change (as shown in Table 2) and in earlier censuses (as shown in Figure A4 in the Online Appendix); in pooled data, therefore, the type of threshold and the vintage of the census is systematically unbalanced, which could cause bias in RDD estimates if the appropriate covariates are not used.

The rest of Table 3 reports similar analysis for a set of covariates we selected because we thought they might explain the aggregate sorting in Italy: two measures of social capital,<sup>26</sup> the proportion of young to old citizens, an indicator for whether the city is in the South of Italy, an indicator for whether the city is located by the sea, and the proportion of second homes. (A large second-home proportion may indicate more opportunities for selective precision.) We find no imbalance in any of these covariates in the raw RDD. In columns 2-5, we find some imbalance in the proportion of young to old citizens: the analysis indicates that cities with an older population are more likely to be found on the left of the threshold than to the right. Similarly, we find imbalances in the proportion of second homes at borderline significance levels ( $p < .1$ ).

Table 4 reports another approach to the same question. We restrict attention to cases in which a municipality was within 50 inhabitants of a threshold that determines the mayor’s

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<sup>26</sup>The measures we use (the number of nonprofit organizations per person and the rate of blood donations) are commonly used in the literature as measures of social capital; see Nannicini et al. (2013).



salary; we then run a weighted OLS regression (with weights proportional to proximity to the threshold) in which the dependent variable is a binary indicator for whether the municipality is above or below the threshold and the independent variables are the covariates we used as outcomes in Table 3. Columns (2)-(4) add dummy variables for year and threshold; Columns (5)-(8) repeat the analysis for placebo thresholds at which no policy changes. The coefficients in column (1) of Table 4 indicate that a municipality close to the threshold is more likely to be above the threshold if it was above that threshold in the previous census and if the council size also changes at that threshold; there is also some evidence that cities with fewer blood donations and more vacation homes are more likely to be found above the threshold, conditional on being close. (Column (5) presents the same regression applied to placebo thresholds. Nothing predicts whether a city is above or below these arbitrary cutoffs; the null hypothesis that all of the coefficients are zero cannot be rejected.) When we add dummy variables for year and threshold in column (4), none of the coefficients shown is statistically significant at the .05 level; this indicates that although sorting differed over time and across thresholds, these covariates are generally well balanced at each threshold and in each census. Still, the results of Table 4 confirm that the cases above and below policy-relevant thresholds *are* different in ways that must be addressed in any population-threshold RDD in Italy. Most tellingly, the F-test of the regression (bottom two lines of the table) indicates that these variables jointly predict whether a municipality-year will be above or below a population threshold (conditional on being close to that threshold) when the threshold determines policy (columns (1)-(4)), but not otherwise (columns (5)-(8)).

What can we conclude from this evidence on covariate imbalance at salary thresholds in Italy? The optimistic conclusion is that researchers can productively conduct RDD in Italy using multiple thresholds and/or multiple censuses as long as they include appropriate controls, which based on this analysis would include an indicator for lagged treatment, indicators for the year and the threshold, and perhaps controls for the age structure of the population and the proportion of second homes. Although it appears that many Italian municipalities attempt to manipulate their population to surpass policy-relevant thresholds, and many succeed, it does not appear that the ones that succeed are much different from those that fail; this suggests

that it may still be a viable strategy to compare cities above and below thresholds. The pessimistic conclusion is that there are many covariates we have not examined (including, of course, unobservable covariates), and thus RDD analysis may be biased even after controlling for the set of covariates we have tested here. The clear implication from this analysis is that studies that pool data across thresholds and years should control for the threshold and year whenever sorting seems like a possibility; whether or not one wants to proceed with a population-threshold RDD setting with evidence of sorting depends, as we discuss in the next section, on what the next best design is.

## V. CONCLUDING REMARKS

We have documented two serious problems with population-threshold RDDs in France, Italy, and Germany. Although important policies depend on population thresholds in each country, these policies often change along with other policies and municipalities seem to strategically manipulate population figures to end up on the desired side of relevant thresholds. We have discussed remedies that researchers might use to address compound treatment and manipulative sorting; applying these remedies requires additional assumptions, which of course makes the analysis less compelling than a standard RDD. The practical question that remains is whether we should bother to undertake population-threshold RDDs in a setting where these remedies (and associated assumptions) are necessary.

The answer to this question of course depends on what the alternative is – i.e., what the next best research design is for addressing the research question. If the alternative is to carry out another population-threshold RDD in a setting that addresses the research question equally well but does not suffer from compound treatment and sorting, clearly the alternative would be better. If the alternative is to conduct an observational study in a setting where the municipalities choose their own policies, the answer is less clear.

Ultimately, the choice depends on how much unobservable imbalance remains in the RDD and the observational study after we apply our various corrections, and how much these omitted variables affect the outcome in each setting; this in turn will depend on how well we understand the process by which municipalities choose their policies in the observational setting and how

sorting takes place in the RDD, but also how well we can measure and control for the covariates that are unbalanced as a result of these processes. All of these considerations are subjective judgments and cannot be measured in the data. Substantive knowledge is thus necessary; the best answer may be to conduct both sets of analysis. What is most clear to us is that a population-threshold RDD should not be dismissed in favor of alternatives simply because there is evidence of compound treatment or manipulative sorting. Observational studies have similar problems: policies tend to be correlated with each other in cross-section, and omitted variable bias is always a concern when units choose their own treatments. Given the difficulty of running experiments on consequential policies, it would be unwise to exclusively rely on purely observational evidence and ignore findings from population-threshold RDDs when these quasi-experiments fall short of the ideal.

More broadly, we also emphasize that, despite the clear challenges of carrying out population-threshold RDDs in the three countries we study, none of our analysis implies that all such designs are problematic. Clearly, researchers should check for compound treatment, sorting, and covariate imbalance whenever they conduct any RDD; the cases we have shown indicate that these problems can be systematic in some settings. But just as it would be a mistake to discard a specific RDD at the first sign of compound treatment or manipulative sorting, it would also be a mistake to conclude based on our analysis that all population-threshold RDDs must suffer from the same problems.

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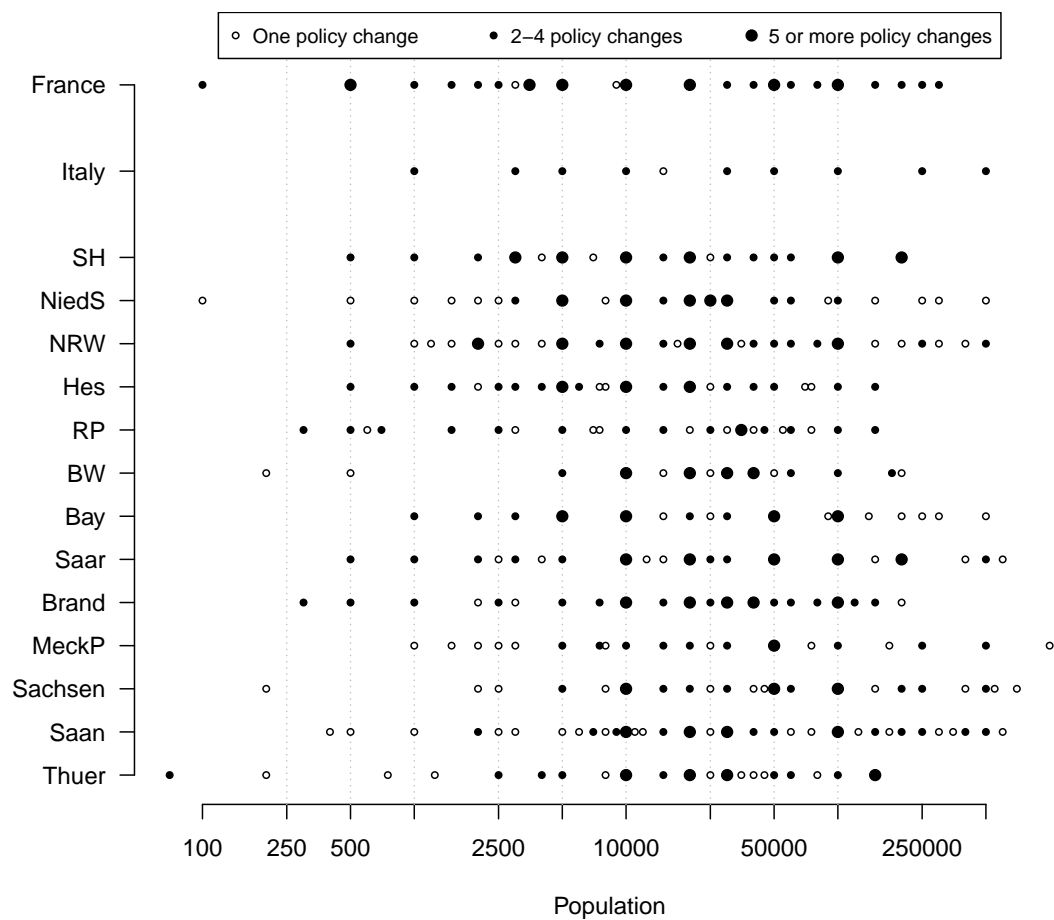
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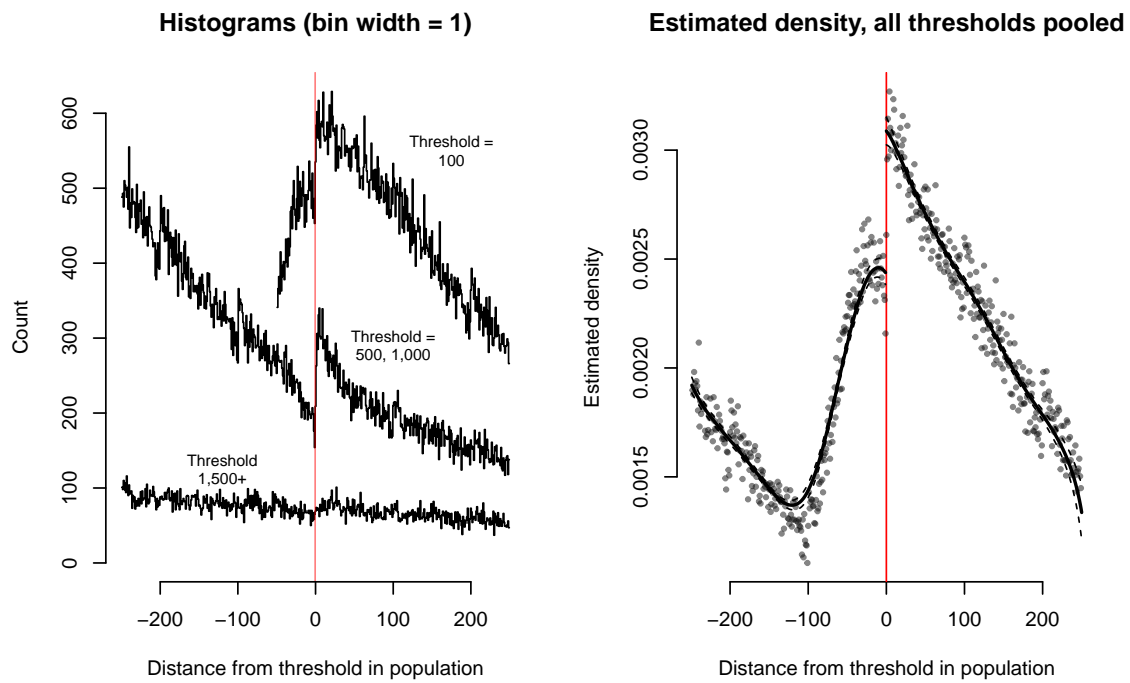
Figure 1: Population thresholds at which municipal policies change: France, Italy, and German states



NOTE: Each dot indicates a population threshold at which a policy changes; solid dots indicate more than one policy changing at the same threshold.

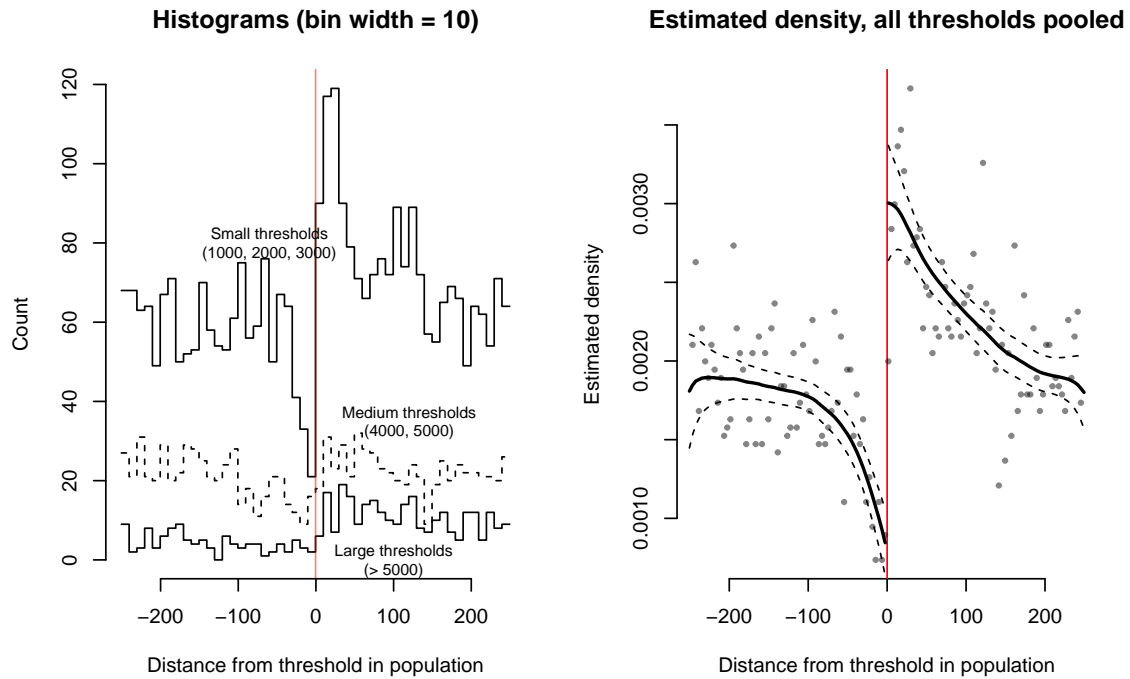


Figure 2: Sorting in municipal population in France, 1962-2011 pooled



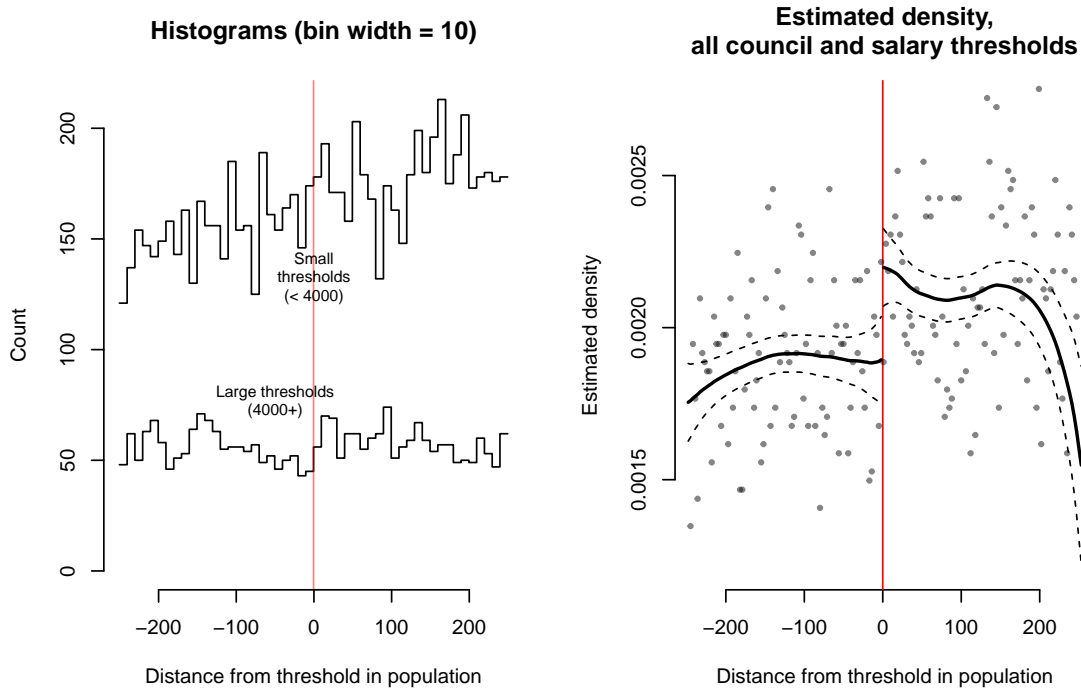
NOTE: The left plot depicts three histograms, one for each group of thresholds (100; 500 & 1,000; 1,500 and larger). In each case the bin width is 1, meaning that the top of the line indicates the number of data points (municipality-years) with population that is exactly a given amount (e.g. 50 inhabitants) from the threshold. The right plot depicts the McCrary analysis for all cases pooled.

Figure 3: Sorting in municipal population in Italy, 1961-2001 pooled



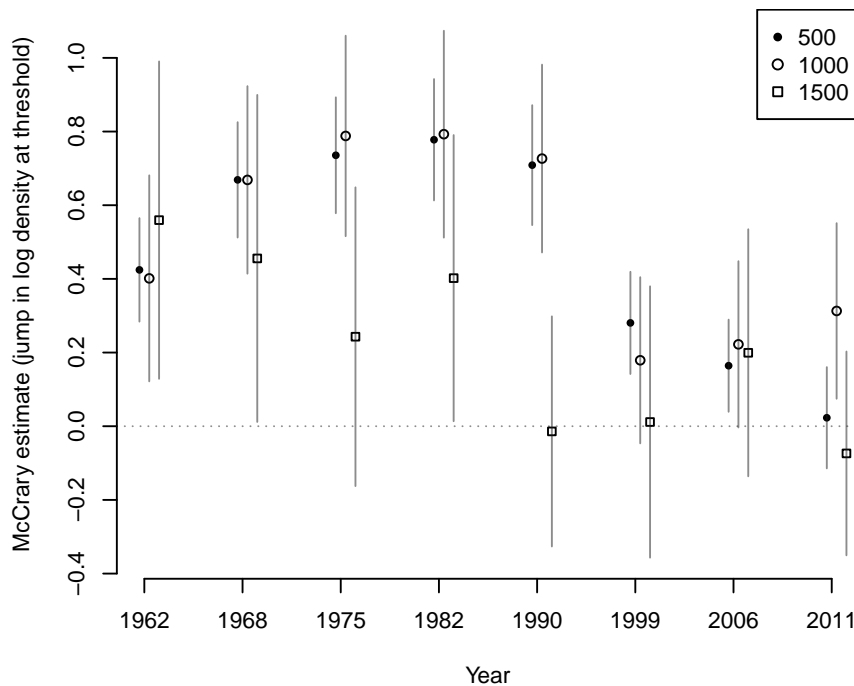
NOTE: In the left plot the bin width is 10, meaning that the top of the line indicates the number of data points (municipality-years) with population that is in a given interval (e.g. 40-49 inhabitants) from the threshold. Otherwise see notes to Figure 2.

Figure 4: Sorting in municipal population in German states, 1998-2007 pooled



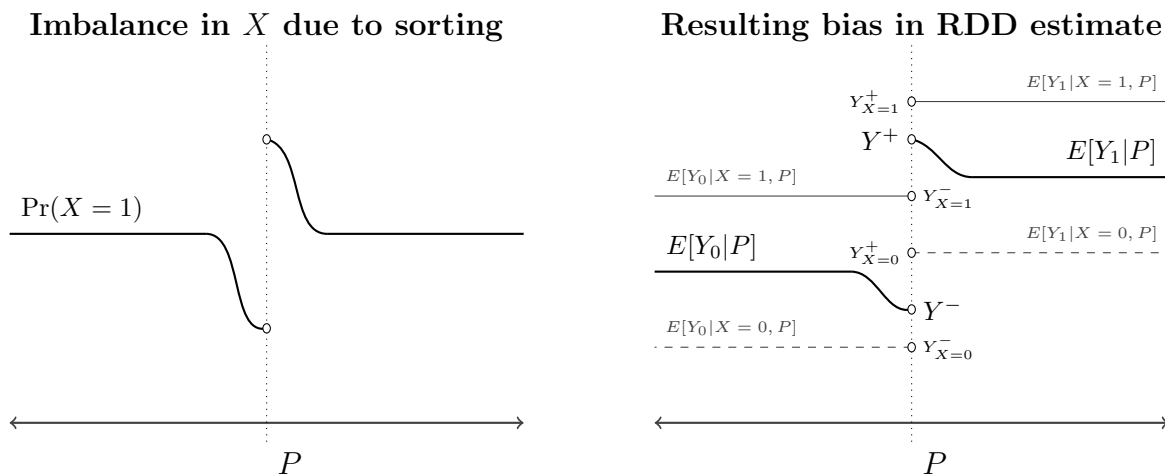
NOTE: In the left plot the bin width is 10, meaning that the top of the line indicates the number of data points (municipality-years) with population that is in a given interval (e.g. 40-49 inhabitants) from the threshold. In both plots we restrict attention to thresholds where both the council size and the mayor's salary change. Otherwise see notes to Figure 2.

Figure 5: Sorting over time in France at the 500, 1,000, and 1,500 population thresholds



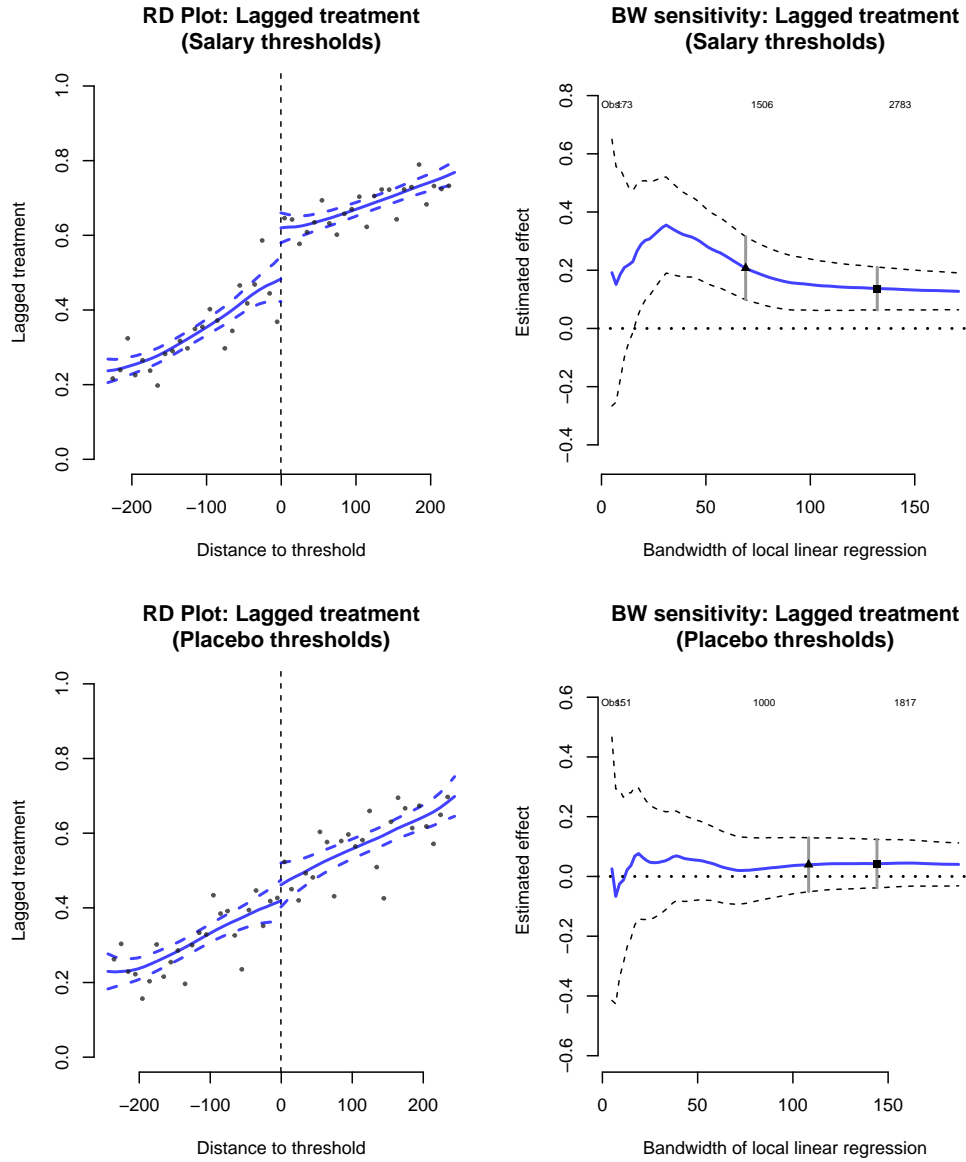
NOTE: Each point corresponds to the McCrary test statistic (the estimated jump in the log density of the running variable at the relevant threshold) for a given threshold in a given census in France. Lines show 95% confidence intervals.

Figure 6: Best case scenario for addressing manipulative sorting with covariate adjustment



NOTE: Suppose covariate  $X$  is not continuous at the threshold due to sorting, as shown in the left plot. If  $X$  is also related to the outcome, as shown in the right plot, then the usual RDD estimate ( $Y^+ - Y^-$ ) will be biased. The bias due to imbalance in  $X$  is removed if the RDD is estimated conditional on  $X$ , e.g. as  $Y^+_{x=1} - Y^-_{x=1}$ .

Figure 7: Imbalance in lagged treatment in Italy



NOTE: The dependent variable in the RDD analysis above is “lagged treatment” – an indicator for whether a municipality was above a given threshold in the *previous* census, given that it was close to that threshold in the current census. The left panel in each pair shows the dependent variable in binned means of the running variable (gray dots) and the local linear regression estimate at the Imbens-Kalyanaraman optimal triangular bandwidth; the right panel shows the sensitivity of the estimated effect to the bandwidth, where the Imbens-Kalyanaraman bandwidth is shown with a dot and the CCT bandwidth is shown with a triangle.

Table 1: Population thresholds in French municipalities

	Number of inhabitants (in thousands)													
	0.1	0.5	1	1.5	2	2.5	3	3.5	5	9	10	20	30	50
Council size	x	x		x		x		x	x		x	x	x	x
Salary of mayor and deputy mayors		x	x					x			x	x		x
Max. number of deputy mayors	x	x		x		x		x	x		x	x	x	x
Max. number of non-resident councilors	x	x												
Must have a cemetery					x									
Prohibition on commercial water supply							x							
Campaign leaflets subsidized						x								
Council must approve property sales						x								
Electoral system – PR or plurality								x						
Gender parity								x						
Outsourcing scrutiny								x						
Council must debate budget prior to vote								x						
Committees follow PR principle								x						
Amount of paid leave for council work								x			x		x	
Commission on accessibility									x					
Max. electoral expenditure										x				
Outsourcing commission											x			
Max. municipal tax on salaries											x			x
Debt limit												x		

NOTE: The table identifies population thresholds (in thousands) at which given policies change. This is a partial list of policies, chosen to highlight the variety of policies that depend on population thresholds and the extent to which the same threshold often determines multiple policies. *Source:* French legal code.

Table 2: Summary of McCrary sorting tests

Sample	France		Italy		Germany	
	# of thresholds (# of close obs)	McCrary Test statistic	# of thresholds (# of close obs)	McCrary Test statistic	# of thresholds (# of close obs)	McCrary Test statistic
Total						
All years, all thresholds	21 (311,392)	0.238*** (0.014)	7 (4,756)	1.328*** (0.136)	195 (101,520)	0.068*** (0.025)
Specific thresholds						
Salary increase	14 (140,421)	0.497*** (0.026)	6 (4,730)	1.331*** (0.134)	78 (11,579)	0.135*** (0.061)
Salary increase (no council)	7 (35,329)	0.533*** (0.049)	3 (2,125)	0.840*** (0.211)	21 (447)	0.001 (0.321)
Council increase	15 (267,558)	0.215*** (0.015)	3 (2,605)	1.909*** (0.197)	120 (81,669)	0.071*** (0.026)
Council increase (no salary)	12 (162,466)	0.139*** (0.018)	0 (0)	n.a. n.a.	63 (70,537)	0.063*** (0.029)
Council and/or salary increase	21 (302,887)	0.240*** (0.014)	6 (4,730)	1.331*** (0.134)	141 (82,116)	0.072*** (0.027)
Council and salary increase	7 (105,092)	0.475*** (0.029)	3 (2,605)	1.909*** (0.197)	57 (11,132)	0.149*** (0.063)
Threshold size						
Small thresholds (<3500)	7 (306,520)	0.237*** (0.014)	2 (3,295)	1.644*** (0.178)	61 (93,873)	0.054*** (0.026)
Big thresholds (>=3500)	14 (4,872)	0.239* (0.122)	5 (1,461)	0.700*** (0.247)	134 (7,647)	0.216*** (0.077)
Placebo thresholds	20 (215,986)	0.008 (0.018)	9 (2,800)	-0.044 (0.133)	186 (85,326)	-0.009 (0.025)

*Notes* For each test, we report four numbers: the number of unique population thresholds (e.g. 14 in the first test for France) at which we observe municipalities with populations within 250 inhabitants of the threshold; the number of observations within 250 inhabitants of these thresholds (e.g. 273,274); the estimated difference log frequency above vs. below the threshold (e.g. 0.256) and the standard error of that estimate (0.015). Significance at the 10% level is represented by \*, at the 5% level by \*\*, and at the 1% level by \*\*\*.

Table 3: “Effects” of crossing threshold on covariates (Italy)

Outcome	Obs [BW]	Jump at thresholds where salary changes					Jump at placebo thresholds		
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lagged treatment	2592 [132]	.138*** (.037)	.113*** (.037)	.128*** (.037)	.108*** (.037)	.041 (.036)	.024 (.041)	.028 (.041)	-.055 (.040)
Lagged running variable	2522 [128]	80.115 (63.630)	55.194 (63.642)	84.583 (58.183)	63.567 (57.660)	27.862 (53.600)	95.758 (97.363)	124.452 (79.532)	1.985 (72.847)
Council size also changes	1590 [81.7]	.268*** (.049)	.207*** (.046)						
Year of census	3056 [158.8]	-7.091*** (1.045)		-3.172*** (.889)			-2.871* (1.620)	-1.988 (1.386)	
Population at threshold (in 1000s)	1678 [86.4]	.147 (.401)	-.091 (.396)				.346 (.532)		
Log nonprofits/person	1647 [84.5]	.001 (.057)	.011 (.058)	.026 (.058)	.023 (.058)	-.000 (.058)	.004 (.045)	.002 (.045)	-.052 (.045)
Proportion donating blood	1570 [80.5]	-.003 (.002)	-.003 (.002)	-.003 (.002)	-.003 (.002)	-.004** (.002)	-.001 (.002)	-.001 (.002)	-.001 (.002)
Log young/old ratio	1815 [93.8]	.045 (.056)	-.089** (.040)	-.045 (.047)	-.089** (.040)	-.086** (.039)	.027 (.053)	-.007 (.045)	-.035 (.034)
South	1777 [91.3]	.037 (.048)	.037 (.048)	.036 (.048)	.033 (.048)	-.023 (.040)	-.012 (.050)	-.010 (.050)	-.031 (.041)
Seaside	2077 [106.9]	-.026 (.023)	-.033 (.023)	-.031 (.023)	-.032 (.023)	-.033 (.024)	-.012 (.020)	-.017 (.020)	-.044** (.020)
Proportion vacation homes	1897 [97.8]	.025 (.022)	.043* (.022)	.033 (.022)	.037* (.022)	.037* (.022)	.032 (.022)	.040* (.021)	.021 (.020)
Year dummies:			✓		✓	✓			✓
Threshold dummies:				✓	✓	✓		✓	✓
Other municipal characteristics:						✓			✓

*Notes:* Each point estimate comes from a different RDD analysis in which the row variable is the dependent variable and we pool data from multiple censuses and population thresholds. Model (1) includes no extra control variables; Model (2) includes a dummy for the year of the census; Model (3) includes a dummy for each threshold (e.g. 1000, 2000); Model (4) includes both year and threshold dummies; Model (5) adds controls for municipal characteristics (e.g. indicator for South, proportion of vacation homes) apart from the one being used as the dependent variable. Significance at the 10% level is represented by \*, at the 5% level by \*\*, and at the 1% level by \*\*\*.



Table 4: What predicts being above population thresholds? (Italy)

	Salary thresholds				Placebo thresholds			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lagged treatment	.093* (.040)	.067 (.042)	.093* (.041)	.069 (.042)	.003 (.059)	-.008 (.061)	-.013 (.060)	-.021 (.062)
Lagged pop minus threshold (k)	.031 (.027)	.024 (.028)	.033 (.029)	.026 (.029)	.036 (.033)	.029 (.035)	.050 (.038)	.041 (.040)
Council size also changes	.161*** (.034)	.147*** (.035)						
Log nonprofits/person	.007 (.031)	-.001 (.031)	.007 (.031)	-.001 (.031)	-.052 (.053)	-.067 (.053)	-.061 (.053)	-.075 (.054)
Proportion donating blood	-1.596 <sup>†</sup> (.961)	-1.586 <sup>†</sup> (.961)	-1.534 (.962)	-1.528 (.962)	-.797 (1.460)	-.680 (1.461)	-.616 (1.477)	-.538 (1.479)
Log young/old ratio	.052 (.035)	-.013 (.049)	.038 (.041)	-.015 (.051)	.016 (.053)	-.075 (.086)	-.005 (.061)	-.079 (.087)
South	-.033 (.043)	-.012 (.045)	-.024 (.044)	-.008 (.045)	-.060 (.067)	-.028 (.070)	-.058 (.068)	-.029 (.072)
Seaside	-.061 (.065)	-.074 (.065)	-.080 (.068)	-.088 (.068)	-.122 (.090)	-.118 (.090)	-.155 <sup>†</sup> (.092)	-.148 (.092)
Proportion vacation homes	.127 <sup>†</sup> (.075)	.131 <sup>†</sup> (.075)	.151* (.077)	.151 <sup>†</sup> (.077)	.100 (.118)	.061 (.119)	.143 (.121)	.095 (.123)
Year dummies:		✓		✓		✓		✓
Threshold dummies:			✓	✓			✓	✓
N	980	980	980	980	588	588	588	588
$R^2$	.071	.080	.075	.084	.011	.021	.024	.032
Adj. $R^2$	.060	.065	.059	.065	-.006	-.003	-.007	-.005
F-test of regression (F-stat)	6.368	5.446	4.695	4.302	.644	.869	.769	.855
F-test of regression (p-value)	.000	.000	.000	.000	.741	.571	.703	.629

*Notes:* Each column represents an OLS regression in which the dependent variable is 1 if the municipality was above the relevant threshold in a given census and 0 if it was below. Analysis is restricted to cities within 50 inhabitants of a threshold. Regression weights decrease linearly with distance from the threshold. Columns (1)-(4) apply to thresholds where the salary of the mayor changes; Columns (5)-(8) apply to “placebo” thresholds where no policy changes. Significance at the 10% level is represented by \*, at the 5% level by \*\*, and at the 1% level by \*\*\*.