Journal of Public Economics 96 (2012) 1047–1060

Contents lists available at SciVerse ScienceDirect

Journal of Public Economics

journal homepage: www.elsevier.com/locate/jpube

Are rules-based government programs shielded from special-interest politics? Evidence from revenue-sharing transfers in Brazil ☆

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ARTICLE INFO

Article history:
Received 29 July 2010
Received in revised form 31 May 2012
Accepted 26 August 2012
Available online 7 September 2012

JEL classification:
H77
D72
D73

Keywords:
Bureaucracy
Institutions
Redistributive politics
Electoral competition

ABSTRACT

Manipulation of government finances for the benefit of narrowly defined groups is usually thought to be limited to the part of the budget over which politicians exercise discretion in the short run, such as earmarks. Analyzing a revenue-sharing program between the central and local governments in Brazil that uses an allocation formula based on local population estimates, I document two main results: first, that the population estimates entering the formula were manipulated and second, that this manipulation was political in nature. Consistent with swing-voter targeting by the right-wing central government, I find that municipalities with roughly equal right-wing and non-right-wing vote shares benefited relative to opposition or conservative core support municipalities. These findings suggest that the exclusive focus on discretionary transfers in the extant empirical literature on special-interest politics may understate the true scope of tactical redistribution that is going on under programmatic disguise.

1. Introduction

Manipulation of government finances for the benefit of narrowly defined groups—special-interest politics, for short—is usually thought to be limited to the part of the budget over which politicians exercise discretion in the short run, such as earmarks. Examples of such tactical redistribution include regulatory or fiscal favors for special interests, such as when particular industries or districts receive public construction projects and government jobs. In contrast, rules-based or programmatic redistribution—carried out using income taxes and the social welfare system—is considered to be relatively stable over time and driven by general-interest politics, which pits the economic interests of large groups of voters against each other (Dixit and Londregan, 1996; Persson and Tabellini, 2000). Whether in practice the scope of tactical redistribution is really limited to discretionary parts of the budget is an important question from both theoretical and policy perspectives. Perhaps surprisingly, however, little is known about this issue because the voluminous empirical literature on redistributive politics—discussed in Section 2 below—has focused almost exclusively on discretionary government spending, implicitly assuming that rules-based programs are implemented without regard to special interests.[1]

In this paper, I examine whether a rules-based transfer program in Brazil, the Fundo de Participação dos Municípios (FPM), which supposedly makes payments to municipal governments exclusively on the basis of population size, was manipulated to favor special interests.[2] The design of the revenue-sharing mechanism considered here is similar to the General Revenue Sharing program used in the US from 1972 to 1986, and is common in many other federations around the world today.[3] These programs bypass the annual budget process and redistribute a substantial part of national tax revenues to local governments based on objective criteria, such as population

☆ An earlier version of this paper was entitled “Rules vs. Political Discretion: Evidence from Constitutionally Guaranteed Transfers to Local Governments in Brazil”. I am grateful to Dennis Epple and two anonymous referees for thoughtful comments and suggestions that have improved the paper a lot. I am also grateful to Antonio Ciccone, Rajeev Dehejia, Albert Fishlow, Macartan Humphreys, Wojciech Kopczuk, David Lee, W. Bentley MacLeod, Kevin Morrison, Kiki Pop-Eleches, Emilia Simeonova, Monica Singhal and Michael Ting for their comments and suggestions. I wish to thank David Samuels for sharing his electoral data with me. The views expressed here are not necessarily those of the national statistical agency, IBGE. All errors are my own.
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[1] Among relatively recent contributions are the following: Ames (1995a, 1995b, 2001), Levitt and Snyder (1995), Schady (2000), Case (2001), Dahlberg and Johansson (2002), Finan (2003), Ansolabehere and Snyder (2006), Khemani (2007), Solé-Ollé and Sorribas-Navarro (2008), Arulampalam et al. (2009).

[2] Municipalities are the lowest level of government in Brazil (below the federal and state governments). The paper refers to counties, communities, and municipalities interchangeably.

[3] Other major federations include Canada, Germany and India (Boadway and Shah, 2007).

0047-2727/$ – see front matter
http://dx.doi.org/10.1016/j.jpubeco.2012.08.010

size. While the explicit goals of such revenue-sharing mechanisms are many, an important common feature is that they aim to redistribute income from rich to poor communities, irrespective of the political characteristics of the community. Ideological alignment with the party in control of the central government should play no role in the allocation of resources under such programs. Indeed, one of the objectives of rules-based programs is to prevent tactical redistribution.

The first result documented here is that the official population estimates that went into the FPM transfer allocation formula for the year 1991 were manipulated, as evidenced by their discontinuous distribution around several population thresholds that determine the amount of transfers received by a municipality. This is in stark contrast to the distribution of *census* 1991 municipality population and to the distributions of official estimates from prior years, which are all smooth around the same thresholds. The 1991 manipulation led to many municipalities receiving higher amounts of transfers than was warranted by their population and resulted in economically important funding differentials. Municipalities that located above the various population cutoffs in 1991 received additional transfers of about US$ 3.6 million on average over the entire decade of the 1990s because the 1991 allocations were subsequently grandfathered.[4] For small local governments, the annual transfer differential amounted to about 15% of the public budget.

In the second step of the analysis, I evaluate which—if any—of several theories about special-interests politics are consistent with the observed program manipulation. According to Cox and McCubbins (1986), a conservative central government, such as the one in Brazil under president Collor from 1990 to 1992, should target core-support conservative municipalities if the electoral response to economic favors among opposition or uncommitted municipalities relative to core-support municipalities is more uncertain and if the central government is risk averse. Similarly, core-support conservative municipalities should be targeted if the central government attempts to buy turnout of existing but unmobilized supporters, instead of votes (Nichter, 2008). In contrast, communities with many non-ideological “swing” voters should be targeted if parties are equally effective in delivering favors to voters across communities (Lindbeck and Weibull, 1987; Dixit and Londregan, 1996).

In order to discriminate between the core-support and swing-voter targeting predictions, I use a non-linear specification in the municipality-level right-wing vote share—defined as the electoral support for right-wing parties in the preceding elections of the Câmara Federal dos Deputados (the Federal Chamber of Deputies).[5] Right-wing parties in Brazil are readily identified—despite the fact that the party system became very fragmented with the transition to democracy—because during the dictatorship period the system was essentially a two-party system, and right-wing parties can for the most part be traced back to the party of the military government.[6] Under the assumption that the right-wing vote share captures the ideological bias of the municipality, a positive relationship with fictitiously high population would indicate core-support targeting, while a non-linear, inverted-U, relationship would be consistent with swing-voter targeting at the expense of opposition or conservative core-support municipalities.[7]

Consistent with swing-voter targeting by the conservative central government, I find that communities with roughly equal right-wing and non-right-wing vote shares had a 16 percentage point higher chance to get favorable population estimates relative to opposition municipalities, and an 8 percentage point higher chance compared to conservative core-support municipalities. In contrast, there is no evidence of swing- or core-voter targeting in the 1985 population estimates—the last estimates made under the military government that had set up the revenue-sharing mechanism in 1965—suggesting that the military government had indeed played by its own rules.[8] The evidence thus suggests that, although the grand redistribution scheme discussed here was shielded from tactical redistribution during the dictatorship, the same program became subject to special-interest politics after the transition to democracy.

Recent theoretical models of special-interest politics in federal systems, such as Brazil, have made further predictions regarding the allocation of intergovernmental transfers, and these depend on the extent of goodwill leakage. Specifically, if voters give enough credit for improvements in their economic conditions to the lower levels of government that turn funds into public services, then allocating grants to non-aligned (non-right-wing) lower levels of governments might actually harm the re-election prospects of the central government (or the election prospects of local conservative parties). If goodwill leakage is sufficiently “large,” non-aligned lower level governments might therefore be expected to receive less transfers than those that are aligned with the right-wing central government (Arulampalam et al., 2008; Khemani, 2007; Solé-Ollé and Sorribas-Navarro, 2007).

Although for the general budget-support transfers considered here, leakage is likely to be relatively large (at least compared to project-specific central government grants), the results suggest a more complex picture because the conditional expectation functions for aligned and non-aligned lower-level governments cross twice. Swing-voter targeting was in fact driven entirely by states with non-right-wing governors and by municipalities with non-right-wing mayors. Among aligned states and municipalities, there is no statistical evidence of core-support or swing-voter targeting. Although statistically the evidence against a common strategy across states and municipalities is not very strong, the pattern suggests that the conservative central government was attempting to bring non-ideological voters in non-aligned lower-level governments into the fold.

A final set of predictions relates to legislative coalition-building in presidential systems. Ames (1995a, 1995b, 2001), among others, argues that presidential coalition-building strategies in Brazil are at least in part based on federal deputies trading votes for discretionary grants from the federal executive. Such grants necessarily flow to individual local governments, however, while deputies compete for votes in their entire state. Any given county thus contributes votes to multiple deputies, which makes it difficult for any one of them to claim credit for the federal financial support he helped to attract. This is particularly true for the unrestricted budget transfers that are the focus of this analysis—as opposed to project-specific grants for which credit-claiming is relatively easier.

[4] The cumulative difference in FPM transfers over the period from 1991 to 1999 was about R$ 5 million in 2008 prices. The Real/$ purchasing power parity exchange rate in 2005 was about 1.36 (World Bank, 2008).

[5] To be precise, right-wing consists of the following political parties: PFL, PDS, PTB, PDC, PL, and PRN. See Table 3 for full party names.

[6] Moreover, the electoral coalitions for the 1990 race for federal deputies I observe in my data are consistent with the definition of right-wing adopted here. As a robustness check I also include minor right-wing parties such as the PSD, PTR, PSC and PST to define the right-wing vote share. See Table 3 for full party names.

[7] I am grateful to an anonymous referee for suggesting this approach. Previous drafts had used interparty fragmentation as an explanatory variable and found that it is positively correlated with favorable population estimates. Although consistent with the results presented here (equal right-wing and non-right-wing vote shares imply high fragmentation) interparty fragmentation is much harder to interpret than the right-wing vote share since fragmentation might be high for other reasons as well, perhaps because the number of political parties is high for example.

[8] Whether special-interest politics was already at play during the 1986–1990 Congress, the first under the new democratic regime, I cannot tell because electoral data from that period are not readily available.

Ames discusses the incentives presidents and federal deputies face under such circumstances. On the one hand, he argues that deputies are more likely to trade votes for grants with the executive when they dominate the municipality vote or at least face limited competition from within their own party, because this makes credit-claiming for the deputy easier. On the other hand, Ames argues that high levels of political competition might reflect weak ideological preferences and a community susceptible to particularistic benefits, provided that the deputy finds a way to claim political credit, for example through an alliance with the local executive. The predicted effect of having a municipality- or coalition-dominant deputy in a given municipality on the likelihood of striking a deal with the executive is thus ambiguous. Empirically, I find some evidence that communities with a municipality-dominant deputy were less likely to receive overly favorable population estimates compared to communities where no deputy dominated the municipality or coalition vote.

In sum, these findings suggest that the exclusive focus on discretionary transfers in the extant empirical literature on special-interest politics may understate the true scope of tactical redistribution that is going on under programmatic disguise. The results are consistent with swing-voter targeting by the conservative central government and there is some evidence of legislative coalition-building between the executive and deputies in electorally fragmented municipalities.

The remainder of the paper is organized as follows: Section 2 discusses existing empirical work on redistributive politics. Section 3 presents institutional background on the revenue-sharing mechanism between the federal and local governments in Brazil and provides evidence of program manipulation in 1991. Section 4 presents the theoretical framework as well as empirically testable hypotheses given the political and institutional environment in Brazil around 1990. Section 5 describes the data. Section 6 gives details on the estimation approach. Estimation results are presented in Section 7. The final section concludes with a discussion of limitations and extensions.

2. Existing empirical work on redistributive politics

Many empirical studies have found that politicians tend to reward their core constituents, as measured by the proportion of votes in a district that go to the party in power at the center. Levitt and Snyder (1995) show that the Democratic vote share is an important predictor of the amount of federal spending across congressional districts for the period 1975–1981, when the federal government was under control of the Democratic party, but not during the 1981–1990 period of divided government. Case (2001) provides evidence of a positive relationship between commune level voting with the central government party in a 1994 constitutional referendum and the subsequent receipt of block grants in Albania. Schady (2000) likewise shows that expenditures by the Peruvian Social Fund over the period 1991–1995 were in part targeted at communities that had helped elect the incumbent government. Using variation in party control of U.S. state governments across states and over time, Ansolabehere and Snyder (2006) also find that the distribution of intergovernmental transfers to local (county) governments was skewed towards loyal constituents.

Some studies have tested explicitly whether transfers are targeted at swing communities. Wright (1974) finds that states exhibiting higher variability in Democratic vote shares for Presidential elections received more federal spending and more work-relief jobs. Case (2001) shows that block grants were also targeted at communes that were relatively swing (close to 50% voting with the central government party on the referendum). Similarly, Schady (2000) also finds that central government funds were targeted at communities where support for the government in previous elections was close to 50%. Dahlberg and Johansson (2002) provide evidence that the central government in Sweden targeted transfers towards regions where the last center government election was close or the estimated proportion of swing voters was high. They find no evidence that core-constituents were favored. In contrast, Ansolabehere and Snyder (2006) find no evidence that parties reward counties where partisan vote shares are close to 50% Democratic and 50% Republican or where the volatility of the Democratic vote share in the past was high.

A number of recent papers test whether center-local alignment matters for the allocation of intergovernmental grants. Arulampalam et al. (2008) show for India that project-specific discretionary grants from the central government are more likely to flow to aligned state governments over the period 1974–1997, but only in those states with a high proportion of close constituency elections. Khemani (2007) finds that over essentially the same time period, aligned Indian states received more general purpose discretionary grants, irrespective of the closeness of previous state legislature elections. Finally, the results in Solé-Ollé and Sorribas-Navarro (2008) suggest that over the period 1993–2003, partisan alignment had a sizeable positive effect on the amount of grants received by Spanish municipalities.

A number of empirical papers deal with special-interest redistributive politics specifically in Brazil. Ames (1995a) demonstrates that federal deputies in the 1987–1990 legislature were more likely to make amendments to the national budget in municipalities where their individual vote share in the previous election was high. He also finds that deputies target vulnerable municipalities, that is, municipalities where incumbent deputies retired, in-migration was high and interparty and intraparty fragmentation was high. Similarly, Finan (2003) investigates federal deputies' amendments to the national budget over the legislative cycle 1995–1998, and finds that they tend to reward municipalities for past electoral support. Arretche and Rodden (2004) find that those states which provided more votes in past presidential elections received more intergovernmental transfers over the period 1991–2000.

There is substantive evidence that Brazilian presidents use public resources to garner legislative support. Ames (1995b) investigates the determinants of voting by federal deputies in Brazil's National Constituent Assembly (ANC) of 1987–1988 and on a set of president Collor's emergency decrees in 1990. He finds that pork in the form of intergovernmental transfers, licenses granted and meetings with ministers is an important determinant of deputy voting behavior. Ames (2001) also examines the allocation of project-specific grants to local governments in Brazil over the period 1986–1994 and finds indirect evidence of presidential vote-buying. In particular, he finds that both the extent of party fragmentation and deputy party affiliation are important determinants of federal project-specific transfers.

Similarly, Arretche and Rodden (2004) find that the spatial allocation of federal transfers to individual states in Brazil over the period 1991–2000 depends on the extent of legislative support for the executive as measured by the share of each state's delegation to the national legislature that belongs to the president's legislative coalition. While the authors interpret their result as evidence of executive-legislative bargaining, it is also consistent with models of unilateral optimization by the central government executive, such as the one outlined in Section 4 below.

3. Institutional background

In this section, I first describe the economic importance, mechanics and origins of the federal revenue-sharing fund for municipal governments in Brazil. Next, I give details on the forecasting procedure for local population estimates in inter-censal years. I then document a manipulation of the program that occurred with the 1991 population estimates and show that this manipulation substantively increased the number of municipalities that were over-classified relative to transfer brackets warranted by census population. I also show that the manipulation had economically significant effects on the distribution of revenue-sharing funds. Finally, I discuss why the effects of the manipulation extended over the entire decade of the 1990s to the present day.

3.1. Importance, mechanics and origins of revenue-sharing in Brazil

Intergovernmental transfers finance most of local government spending on primary education, primary health care, housing and urban infrastructure, and local public transportation in Brazil. Over the period of the 1990s, total government revenue in Brazil was about 28% of GDP, of which municipalities collected about 5%. At the same time, local governments managed about 16% of public resources. Intergovernmental transfers to local governments therefore represented about 3.08% of GDP. The most important among these transfers is the Fundo de Participação dos Municípios (FPM), a constitutionally guaranteed and largely unconditional revenue-sharing grant funded by federal income and industrial product taxes.[9] The FPM grant alone accounted for about 50% of revenue in small to medium sized local governments.

According to the national tax code (Decree 1881/81), transfer amounts depend on municipality population in a discontinuous fashion. More specifically, based on municipality population estimates, pop^e, municipalities are assigned a coefficient $k=k(pop^e)$, where $k(.)$ is the step function shown in Table 1. For municipalities with up to 10,188 inhabitants, the coefficient is 0.6; from 10,189 to 13,584 inhabitants, the coefficient is 0.8; and so forth. There are a total of 18 population brackets and although the population thresholds were supposed to evolve with population growth in Brazil, they remained unchanged since 1966, as further detailed below. The coefficient $k(pop^e_{mst})$ determines the share of FPM resources available for state s that are distributed to municipality m in year t. The amount of transfers to state s in turn depends on a percentage f_s of federal tax collection earmarked for revenue-sharing in year t, rev_t. The state shares are determined in the constitution and have remained unchanged since their introduction in 1989.[10] FPM_{mst} is the amount transferred to municipality m in state s during year t according to the following formula:

$$FPM_{mst} = \frac{k(pop^e_{mst})}{\sum_{i|s} k^e_{ist}} f_s rev_t. \quad (1)$$

Eq. (1) makes it clear that local population estimates should be the only determinant of cross-municipality variation in FPM funding in a given state.

Before proceeding it is worth discussing why politicians would choose to allocate resources based on objective criteria, such as population, rather than use discretion? The answer to this question lies in the political agenda of the military dictatorship which came to power in 1964. As detailed by Hagopian (1996), one of the major objectives of the military was to wrest control over resources from the traditional political elite and at the same time to depoliticize public service provision. The creation of a revenue-sharing fund for the *municípios* based on an objective criterion of need, population, was part of this greater agenda. It reflected an attempt to break with the clientelistic practice of the traditional elite, which manipulated public resources to the benefit of narrowly defined constituencies.

The reason for allocating resources by brackets, i.e. as a step function of population as in Decree 1881/81, is less clear. One explanation could be that compared to a linear schedule, for example, the bracket design mutes incentives for local officials at the interior of the bracket to tinker with their population figures or to contest the accuracy of the estimates in order to get more transfers. A related question is where the exact cutoffs come from—that is, why 10,188, 13,584, 16,980, etc.? While I was unable to trace the origin of these cutoffs precisely, I know roughly how they came about. The initial legislation from 1967 created cutoffs at multiples of 2000 and stipulated that these should be updated proportionally with population growth in Brazil.[11] The cutoffs were thus presumably updated twice, once with the census of 1970 and then with the census of 1980, which explains the “odd” numbers. It is noteworthy that the thresholds are still equidistant from one another, the distance being 6792 for the first 7 cutoffs (except for the second cutoff which lies exactly halfway in between the first and the third cutoffs).

Table 1
Brackets and coefficients for the FPM transfer.

Population bracket	Coefficient
Up to 10,188	0.6
From 10,189 to 13,584	0.8
From 13,585 to 16,980	1
From 16,981 to 23,772	1.2
From 23,773 to 30,564	1.4
From 30,565 to 37,356	1.6
From 37,357 to 44,148	1.8
From 44,149 to 50,940	2
From 50,941 to 61,128	2.2
From 61,129 to 71,316	2.4
From 71,317 to 81,504	2.6
From 81,505 to 91,692	2.8
From 91,693 to 101,880	3
From 101,881 to 115,464	3.2
From 115,465 to 129,048	3.4
From 129,049 to 142,632	3.6
From 142,633 to 156,216	3.8
Above 156,216	4

Source: Decree 1881/81.

3.2. The forecasting procedure for local population estimates

According to Eq. (1), municipality population is the key determinant of this revenue-sharing mechanism. However, exact municipality population figures are only available for census years or years when a national population count is conducted. For all other years, official population estimates are produced by the National Statistical Agency, IBGE.[12] Prior to 1989 these estimates were updated only in years ending with the number 5. Beginning in 1989 the estimates were updated on a yearly basis. The currently used forecasting procedure is based on a top–down approach that ensures consistency of estimates for lower level units (municipalities) with the higher levels (states and the country as a whole) (IBGE, 2002).

First, IBGE produces a population estimate for Brazil, pop^e_t, based on estimated birth rates, mortality and net migration for Brazil. Individual states are then assigned their share of the national estimate, pop^e_{st}, in proportion to past state level census population numbers. Municipalities within a given state are grouped by quartile of both census population levels and past population growth between census years and growing municipalities are separated from shrinking municipalities. Each of these 20 groups of municipalities is then assigned its share of the state population estimate, pop^e_{jst}, proportional to past group level census population. Finally, each municipality within

[9] Federal Constitution of Brazil, 1988, Art. 159 Ib. The one condition is that municipalities must spend 25% of the transfers on education (Art. 212). This constraint is usually considered non-binding, in that municipalities typically spend about 20% of their total revenue on education. It is not clear how this provision was enforced in practice, since there is no clear definition of education expenditures and accounting information provided by local governments was not systematically verified.

[10] Supplementary Law no 62/1989 and Decision no 242/1990 of the Federal Court of Accounts. The state shares f_s correspond to the shares of each state in the total population of Brazil according to the 1991 census.

[11] Supplementary Law No. 35, 1967, Art. 1, Paragraphs 2 and 4.

[12] Supplementary Law no 59/1988, Art. 91, Paragraph 3.

each group is assigned its population estimate, pop^e_{mjst}, based on past municipality level census information. The specific formula for municipality population estimates is as follows:

$$pop^e_{mjst} = \left(pop_{mjs80}/pop_{js80}\right)\left[a_{js}pop^e_{st} + b_{js}\right] t > 1988 \tag{2}$$

where

$$a_{js} = \frac{pop_{js80} - pop_{js70}}{pop_{s80} - pop_{s70}} \quad j = 1, 2, \ldots, 20$$
$$b_{js} = pop_{js80} - a_{js}pop_{s80}.$$

According to Eq. (2) local population forecasts are essentially a continuous function of past census information and state level population projections. This top–down procedure ensures the consistency of estimates for lower level units (municipalities) with the higher levels (states and the country as a whole):

$$pop^e_t = \sum_s \sum_j \sum_m pop^e_{mjst}.$$

Since local population estimates directly determine funding levels, it is important to verify whether they are indeed derived from this forecasting procedure. My replication attempt suggests that, as a first approximation, 1989 official population estimates are indeed consistent with the forecasting procedure described above (results available on request).

3.3. Evidence on manipulation of population estimates

The first empirical fact established in this paper is that the tight link between formula-driven predictions and official estimates broke down over the next two years.[13] This point is best demonstrated with the use of histograms for 1989 official estimates, for the 1991 official estimates and for 1991 census population.[14] Figs. 1 and 2 show that, while the distribution of 1989 official estimates is smooth at the thresholds, the distribution of 1991 official estimates exhibits gaps immediately below the thresholds determining transfer brackets and even more obvious spikes immediately above those cutoffs. The histogram for 1991 official estimates actually understates the discontinuity of the density around the cutoffs because the spikes occur at specific points on the support.[15] The total number of municipalities that were placed on any one of these bunching points is 1870, which represents 42% of the municipalities receiving FPM transfers at the time.

Fig. 3 makes it clear that these gaps (to the left) and spikes (to the right) of the thresholds do not reflect 1991 census population. While I was not able to confirm with IBGE what forecast model they were using in 1991, it seems clear that government officials did not rely exclusively on some variant of the forecast procedure outlined above, which is essentially a continuous function of past census information and population projections. The discontinuous distribution of population estimates is thus almost surely the result of an adjustment which went beyond the mechanical application of the forecasting procedure.

The reasons for this manipulation or adjustment of population estimates are less clear. For example, it is conceivable that bureaucrats used some administrative rule to determine which estimates to revise. Officials were likely more averse to underestimate a municipality relative to a given threshold than overestimating it because underestimated municipalities were much more likely to appeal against IBGE's preliminary population estimates. Although IBGE has the final authority to determine official estimates, i.e. there is no external review of IBGE decisions, dealing with municipality complaints involves scarce administrative resources. Bureaucrats' attempts to preempt such complaints would explain the curious gaps in the distribution of estimates just below the thresholds as well as a part of the spikes just above. One sensible administrative rule would be that all municipalities within a given distance to the next higher threshold were placed just above the threshold to take account of the uncertainty surrounding the formula based estimates. The mass of missing municipalities from the gaps to the left of each threshold is too low to account for the mass on the spikes, however. In other words, IBGE officials must have bumped up municipalities for other reasons as well.

Alternatively, administrators might have had access to evidence about actual local population levels justifying selective revision of population estimates. For example, some mayors may have presented IBGE with administrative data, such as local vital and migration statistics indicating that they were in fact eligible for higher transfers. It is also possible that IBGE used electoral data from 1988 to reclassify municipalities. If this were the case—and if the information IBGE acted upon was more reliable than the predictions from the model—one would expect that the number of correctly classified municipalities in terms of transfer brackets increased with the manipulation.

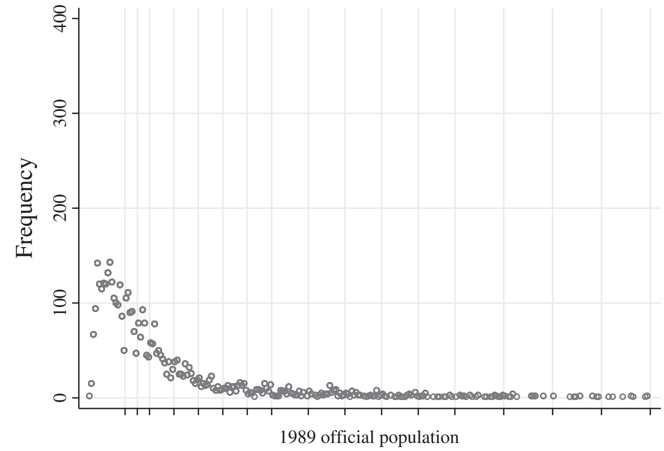

Fig. 1. Histogram for 1989 official population. *Notes*: The bin-width is 566. 1989 official population is from the national statistical agency, IBGE. The vertical lines correspond to the bracket limits from Table 1.

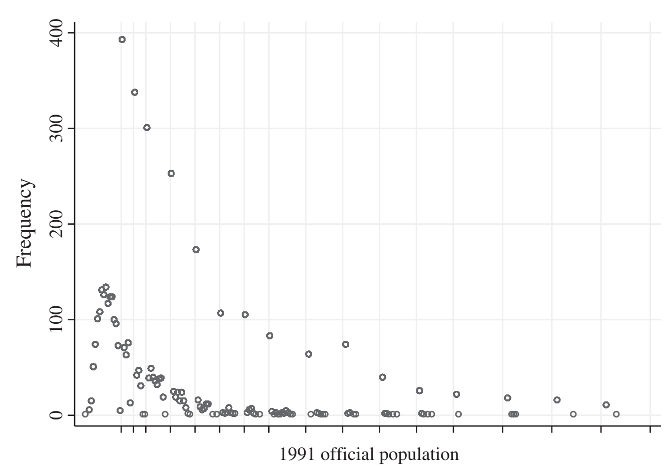

Fig. 2. Histogram for 1991 official population. *Notes*: The bin-width is 566. 1991 official population is from the national statistical agency, IBGE. The vertical lines correspond to the bracket limits from Table 1.

[13] 1990 estimates already exhibit some irregularities but the 1991 manipulation is much more pronounced and produced more lasting effects as further discussed in Section 3.4 below.

[14] The bin-width in these histograms is 566, which ensures that the various cutoffs coincide with bin limits—that is, no bin counts observations from both sides of any cutoff.

[15] The exact bunching points are as follows: 10,189, 10,298, 13,730, 17,162, 24,027, 30,891, 37,756, 44,620, 51,484, 61,781, 72,078, 82,375, 92,671, 102,968, 116,697, 130,426, 144,155, 157,884.

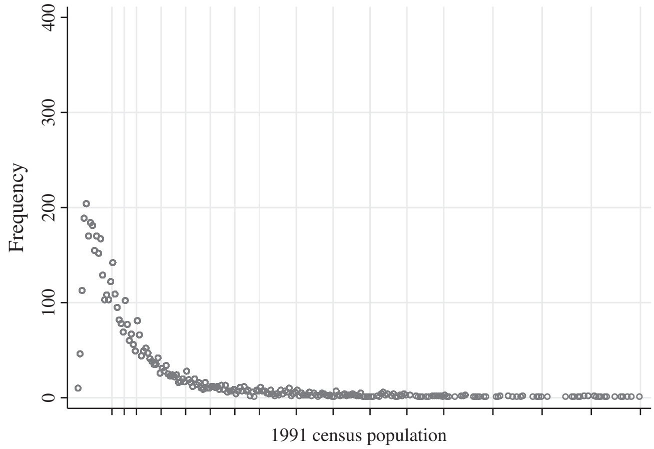

Fig. 3. Histogram for 1991 census population. *Notes*: The bin-width is 566. 1991 census population is from the national statistical agency, IBGE. The vertical lines correspond to the bracket limits from Table 1.

Since populations are known ex post from the 1991 census, I can test whether this is indeed the case by comparing the classification performance that arises using the 1991 manipulated estimates to the classification performance using the 1991 pre-manipulation or first-pass population estimates. Such a comparison holds the inherent uncertainty surrounding population estimates constant and allows a quantification of the distortion of public funds generated by the manipulation.

Since I do not observe 1991 pre-manipulation estimates I use the 1989 official estimates instead.[16] Eq. (2) shows that the only information relevant for local population forecasts that changes between 1989 and 1991 are state-level population estimates. Since these changes are unlikely to be large from year to year, the resulting classification error is likely limited. I focus on the bracket error, defined as the difference between the predicted transfer bracket for 1991, $k(pop_m^e)$, and the correct transfer bracket for 1991, based on census local population (unknown at the time of the forecast), $k(pop_m)$:

$$\text{bracket error} = 5 \times \left[k(pop_m^e) - k(pop_m)\right].$$

Table 2 shows the distribution of bracket errors under the 1989 official estimates (which proxies for 1991 pre-manipulation estimates) and the manipulated 1991 official estimates. From panels A and B it is apparent that for bunched municipalities, that is, those located on any of the bunching points, the manipulation increased the percentage of mis-classified municipalities (bracket error ≠ 0) from about 51% to about 83%. Even more strikingly, the manipulation shifted the entire bracket error distribution to the right, moving the percent over-classified (bracket error > 0) from 31% to 80%. For non-bunched municipalities, the percentage mis-classified increased only slightly from 20% (Panel C) to 21% (Panel D), while the percentage over-classified increased from 10% to 20%. Overall, the manipulated 1991 official estimates increased the number of mis-classified municipalities from 33% to 48% and the number of over-classified municipalities from 19% to 46%.

These results suggest that the information used to revise the formula-driven estimates was not a good predictor of actual levels of population in 1991. It is also worth noting that manipulation may not have been limited to the bunched municipalities since the percentage over-classified also increased for the non-bunched municipalities. Similarly, the 1991 manipulation may not have been an isolated incident. Even prior to 1991 there might have been more subtle manipulations of the program, which left

Table 2
Bracket error distribution.

Panel A, bunched municipalities 1989 official population classification				Panel B, bunched municipalities 1991 official population classification			
Bracket error	Freq.	Percent	Cum.	Bracket error	Freq.	Percent	Cum.
−6	2	0.11	0.11	−6	0	0.00	0.00
−5	7	0.38	0.48	−5	0	0.00	0.00
−4	6	0.32	0.80	−4	2	0.11	0.11
−3	14	0.75	1.56	−3	2	0.11	0.21
−2	49	2.63	4.18	−2	5	0.27	0.48
−1	301	16.15	20.33	−1	38	2.04	2.52
0	911	48.87	69.21	0	318	17.06	19.58
1	387	20.76	89.97	1	1051	56.38	75.97
2	132	7.08	97.05	2	333	17.86	93.83
3	36	1.93	98.98	3	76	4.08	97.91
4	15	0.80	99.79	4	24	1.29	99.20
5	3	0.16	99.95	5	11	0.59	99.79
6	1	0.05	100.00	6	3	0.16	99.95
				7	1	0.05	100.00

Panel C, non-bunched municipalities 1989 official population classification				Panel D, non-bunched municipalities 1991 official population classification			
Bracket error	Freq.	Percent	Cum.	Bracket error	Freq.	Percent	Cum.
−7	1	0.04	0.04	−7	0	0.00	0.00
−6	1	0.04	0.08	−6	1	0.04	0.04
−4	3	0.12	0.20	−5	0	0.04	0.04
−3	4	0.16	0.36	−3	3	0.12	0.16
−2	33	1.34	1.70	−2	9	0.36	0.53
−1	200	8.10	9.81	−1	43	1.74	2.72
0	1976	80.06	89.87	0	1941	78.65	80.92
1	212	8.59	98.46	1	404	16.37	97.29
2	29	1.18	99.64	2	45	1.82	99.11
3	6	0.24	99.88	3	16	0.65	99.76
4	2	0.08	99.96	4	5	0.20	99.96
5	0	0.00	99.96	5	1	0.04	100.00
9	1	0.04	100.00				

Notes: The total number of municipalities in Panels A and B is 1864 and in Panels C and D the total number is 2468. Bunched municipalities refers to those located on any of the bunching points identified in the main text. The tabulation excludes 119 municipalities that were created between 1989 and 1991. Bracket error is defined as 5 × [k(19XX official population) − k(1991 census population)], where k(.) is the step function defined in Decree 1881/81 and XX = 89,91.

the distribution of population estimates smooth at the cutoffs. I take up this issue in Section 4 below, where I test whether conditional on non-political municipality characteristics, political determinants are able to predict official estimates, both in 1991 and in 1985, the last year of the military government.

3.4. Economic significance of the manipulation

The 1991 manipulation resulted in economically important transfer differentials. Municipalities that located above a population cutoff in 1991 received additional transfers of about US$ 3.6 million on average over the entire decade of the 1990s (and beyond) because coefficients were subsequently grandfathered.[17] For small local governments the annual transfer differential amounted to about 15% of their public budgets. Fig. 4 illustrates the persistence of this effect by showing sample average cumulative FPM transfers over the period 1991–1999 against the 1991 official population estimate in a given bin.

Grandfathering began in 1992 when all coefficients remained virtually unchanged, partly because census results had not been available by the end of 1991. When census population estimates were finally released in 1993, the majority of municipalities would have had their coefficients reduced because the law stipulated that the thresholds be adjusted with population growth and these municipalities

[16] I also use the 1989 predicted population estimates discussed above and results are almost identical to those obtained using the 1989 official estimates.

[17] The cumulative difference in FPM transfers over the period from 1991 to 1999 was about R$ 5 million in 2008 prices. The Real/$ purchasing power parity exchange rate in 2005 was about 1.36 (World Bank, 2008).

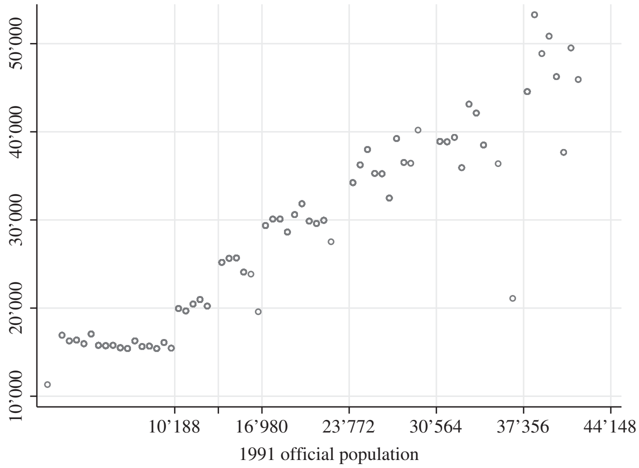

Fig. 4. FPM Transfers, 1991–1999 (in '000 of 2008 Reais). *Notes*: Each dot corresponds to the sample average of FPM transfers in a given bin. The bin-width is 566. FPM transfers are self-reported by municipalities. 1991 official population is from the national statistical agency, IBGE. The vertical lines correspond to the bracket limits from Table 1.

had grown less than the population average for Brazil. Some municipalities would have incurred a significant loss of transfers as a result of this reclassification (Brandt, 2002).

Another law was approved in April 1993, still by the same congress, which determined that both coefficients and population thresholds were to be maintained without adjustment.[18] The only exception was for municipalities that were subdivided and lost population to newly-created municipalities. The revision of coefficients for these types of municipalities was done according to the existing population thresholds using the latest census population figures. Underestimated municipalities' coefficients were updated pursuant to the publication of the census while overestimated municipalities' coefficients were not.

In 1996, there was a population count carried out by IBGE and Congress approved another supplementary law at the end of 1997. It stated that in 1998 all coefficients of the FPM were to remain the same as in 1997.[19] From 1999 onwards however, coefficients would be based on the 1996 population count and the grandfathering would be phased out over the next five years. In each year, coefficients of municipalities that had benefited from the grandfathering would be reduced by 20% of the excess coefficient, the difference between the grandfathered coefficient and that resulting from current population estimates. As a result of the 1997 law, coefficients for fiscal years from 1999 onwards were increasingly based on current population estimates. Denoting $\overline{k}_m$ as the grandfathered coefficient for municipality m, 1[.] as the indicator function and α_t as the percentage reduction in the excess coefficient $\overline{k}_m - k(pop_{mt})$, coefficients are currently calculated as

$$k_{mt} = 1\left[k(pop_{mt}) \geq \overline{k}_m\right] k(pop_{mt}) + 1\left[k(pop_{mt}) < \overline{k}_m\right]\left[\overline{k}_m - \alpha_t\left(\overline{k}_m - k(pop_{mt})\right)\right].$$

In March 2001 a new supplementary law was enacted in order to postpone full adjustment to 2008.[20] The 1991 manipulation thus extends its effects to the present day.

To sum up this section, there is clear evidence that the 1991 official population estimates were somehow adjusted or manipulated. The adjustments resulted in economically important transfer differentials extending up to the present day because coefficients were grandfathered. The fact that the manipulation of municipality population estimates documented above significantly increased the number of misclassified municipalities casts doubts on technocratic explanations. The remainder of the paper turns to political explanations of the program manipulation.

4. Theoretical framework and predictions

4.1. Theoretical framework

This section presents a simple model of central government resource allocation across municipalities, borrowed from Arulampalam, Dasgupta, Dhillon and Dutta, henceforth Arulampalam et al. (2008), and similar to models by Solé-Ollé and Sorribas-Navarro (2007) and Khemani (2007). There are two key predictions from these models: first that a vote-maximizing central government incumbent will favor "swing" municipalities—those with a high proportion of non-ideological voters—and second, that the incumbent may skew fiscal transfers in favor of aligned lower-level governments if credit-claiming is sufficiently difficult, that is, if the implementing lower-level government gets sufficiently high partial credit for turning funding into public services.

There are two parties, L and R, and two levels of government, center and local. The central government incumbent party R, decides on the allocation of transfers and is assumed to care about its own re-election.[21] There is a set of municipalities S^R where the local incumbent party is R, and a set of municipalities S^L where the local incumbent party is L. Transfers from the center to each of M municipalities finance local public services valued by voters. Actual service provision is done by the local government and imperfectly informed voters may not perceive perfectly that the R party is the source of the grants or they may credit the local party for turning extra funding into public services. As a result, the goodwill generated by these transfers is likely shared between incumbent parties at both levels of government. Let $\theta \in [0,1]$ denote the share of goodwill from per capita transfers that accrues to the central incumbent. θ is known by the central government and assumed exogenous. For the kind of transfers considered here, given as general budget support, θ is likely to be relatively low, at least compared to project-specific central government grants.

Within each municipality m, there is a continuum of voters of mass N_m who may differ in their ideologies. A voter j, located at X_j on the ideology spectrum $[\underline{X}, \overline{X}]$ has preference X_j for party L over party R. X_j is private information while the cumulative distribution function of X in municipality m, denoted $\Phi_m(X)$, is common knowledge. $\Phi'_m(X)$ is strictly positive and continuous for all X. For simplicity, $\underline{X} = -\overline{X}$, so that the midpoint is 0.

Voters in each municipality vote on the basis of ideology and economic benefits generated by grants. Consider a voter j in municipality $m \in S^R$ which has received per capita grant g_m from the center. Party R has received a goodwill of $U(g_m)$, with $U(0) = 0$, $U'(g_m) > 0$, $U''(g_m) < 0$ and so voter j votes for party R iff:

$$U(g_m) - X_j \geq 0 \tag{3}$$

and votes for party L otherwise. In contrast, in a municipality governed by party L, goodwill is split between the two parties: party R gets $\theta U(g_m)$ while party L gets $(1-\theta)U(g_m)$. Voter j will vote for party R iff:

$$\theta U(g_m) - (1-\theta)U(g_m) - X_j \geq 0. \tag{4}$$

[18] Supplementary Law no 74/1993.
[19] Supplementary Law no 91/1997.
[20] Supplementary Law no 106/2001.

[21] The same results obtain if the central government incumbent is assumed to care about election of aligned parties at the local level (Arulampalam et al., 2008).

The inequalities (3) and (4) generate cutpoints, $X(g_m,R)$ and $X(g_m,\theta,L)$ for each municipality such that a voter located at X_j votes for party R iff $X_j \leq X(g_m,\theta,p)$ for $p=L,R$. The central incumbent uses grants in order to shift the location of these cutpoints:

$$\frac{\partial X(g_m,R)}{\partial g_m} = U'(g_m), \frac{\partial X(g_m,\theta,L)}{\partial g_m} = (2\theta-1)U'(g_m).$$

Increasing grants to aligned local governments unambiguously improves electoral prospects of the R party. Increasing grants to non-aligned local governments improves electoral prospects of the R party only if θ is sufficiently high (above 0.5) and hurts party Rs prospects if goodwill leakage is large (θ below 0.5). Unfortunately, θ is not observable. Moreover, it is likely endogenous, since the central government has every incentive to make θ high, while non-aligned local governments want to keep θ as low as possible.

Tactical redistribution by the central incumbent is subject to two constraints. First, transfers must satisfy an overall budget constraint. Second, the incumbent is also interested in maximizing total welfare accruing from transfers. This aspect is captured by specifying a per capita welfare function $\gamma(g_m)$, assumed increasing and concave in g_m. If voters vote along party lines, that is, ideology of voters at the local level is the same as at the central level, it is reasonable to assume that the central incumbent maximizes its vote total across municipalities. The objective function is then:

$$\sum_{m\in S^R} N_m \Phi_m(X(g_m,R)) + \sum_{m\in S^L} N_m \Phi_m(X(g_m,\theta,L)) + \sum_m N_m \gamma(g_m) \quad (5)$$

which the incumbent R government maximizes by choice of grant allocation $\{g_m\}_{m=1}^M$, subject to the budget constraint:

$$\sum_m N_m g_m = B.$$

At an interior solution the first-order condition for a municipality $m\in S^R$ is:

$$\gamma'(g_m^*) + \Phi'_m(X(g_m^*,R))U'(g_m^*) = \lambda \quad (6)$$

and for a municipality $m\in S^L$ it is:

$$\gamma'(g_m^*) + \Phi'_m(X(g_m^*,\theta,L))(2\theta-1)U'(g_m^*) = \lambda \quad (7)$$

where λ denotes the Lagrange multiplier and g_m^* is the optimal allocation of grants for the central incumbent R. If the objective function (5) is concave, then the necessary conditions are also sufficient and the solution $\{g_m^*\}_{m=1}^M$ is unique.

The two main predictions of this model can be seen from the first-order conditions (6) and (7). Define a “swing” municipality s as one where the density of voters in the middle of the ideology spectrum is relatively high compared to municipality l: $\Phi'_s(0) > \Phi'_l(0)$. For “small” levels of grants, $\Phi'_s(X(g_s)) > \Phi'_s(X(g_l))$ if $g_s = g_l$. By concavity of $\gamma(g_m)$ and $U(g_m)$ it follows that $g_s^* > g_l^*$ if the first-order condition is to be satisfied. See Arulampalam et al. (2008) Proposition 2 for a fully rigorous proof.

The second prediction can be seen by inspecting the difference in first-order conditions for an aligned $a\in S^R$ and a non-aligned municipality $n\in S^L$:

$$\gamma'(g_a^*) - \gamma'(g_n^*) = \Phi'_n(X(g_n^*,\theta,L))(2\theta-1)U'(g_n^*) - \Phi'_a(X(g_a^*,R))U'(g_a^*).$$

When goodwill leakage is “large” ($\theta<0.5$), then the right-hand-side of this equation is strictly negative since $\Phi'_m(X)$ is assumed strictly positive for all X and so is $U'(g_m)$ for all g_m. From the concavity assumption on $\gamma(g_m)$ it follows that $g_a^* > g_n^*$ if the equality is to hold. That is, the central incumbent R will allocate higher per capita grants to aligned (R) municipalities than to those that are non-aligned (L) (ADDD, 2008, Proposition 1).

4.2. Testable predictions

In this sub-section I discuss how I translate the above predictions into empirically testable hypotheses, given the political and institutional environment in Brazil around 1990.

Determining incumbent and opposition parties in Brazil's fragmented party system may seem difficult at first, especially during the presidency of Fernando Collor (PRN) from 1990 until 1992, since he did not enter into formal coalitions with other parties until the end of his term. Observers agree, however, that he needed to rely on legislative support from right-wing parties, PDS and PFL in particular, in order to pass legislation (Ames, 1995b, 2001). Other right-wing parties at the time included the PL, the PDC and the PTB.[22] As noted earlier, during the dictatorship the system was essentially a two-party system, and right-wing parties can for the most part be traced back to the party of the military government. Moreover, the electoral coalitions for the 1990 race for federal deputies I observe in my data are consistent with the definition of right-wing adopted here.

Testing the first prediction also requires a measure of the proportion of non-ideological voters in each municipality. I do not have such a measure.[23] Instead, I use the municipality-level right-wing vote share—defined as the electoral support for right-wing parties in the preceding elections to the Câmara Federal dos Deputados (the Federal Chamber of Deputies). The average right-wing vote share across municipalities is 0.5, ranging from 0.01 to 0.99. Under the assumption that the right-wing vote share captures the ideological bias of the municipality, a positive relationship with fictitious population would indicate core-support targeting, while a non-linear, inverted-U, relationship would be consistent with swing-voter targeting at the expense of opposition or conservative core-support municipalities. As a robustness check I also include minor right-wing parties such as the PSD, PTR, PSC and PST for an extended definition of the right-wing vote share (see Table 3 for full party names).

The second prediction obtained above is that aligned lower level governments, that is, states or municipalities that were governed by politicians affiliated with the ruling coalition at the center, were more likely to obtain population estimates above a given threshold and hence receive more federal funding than non-aligned lower level governments. In the empirical analysis below, the binary variable right-wing governor indicates a municipality from a state where a right-wing governor was in power from 1991 to 1994.[24] Similarly, right-wing mayor indicates a municipality headed by a mayor affiliated with any of the above right-wing parties. Over the 1989–1992 term, right-wing mayors governed in 54% of all municipalities (Table 3).

In order to test whether legislative bargaining might explain the observed program manipulation, I construct two indicator variables for municipalities with a right-wing municipality-dominant deputy and with a non-right-wing municipality-dominant deputy, each equal to one if a deputy vote share in the municipality exceeds a given cutoff of the total municipal vote, e.g. 50%, and zero otherwise. With a 50% cutoff, 37% of municipalities had a dominant right-wing deputy while 11% had a dominant non-right-wing deputy (Table 3). For municipalities without a dominant deputy, I construct two indicator variables for those with a right-wing coalition-dominant deputy and with a non-

[22] Partido da Reconstrução Nacional (PRN), Partido Democrático Social (PDS), Partido da Frente Liberal (PFL), Partido Liberal (PL), Partido Democrata Cristão (PDC), Partido Trabalhista Brasileiro (PTB).

[23] It is also not clear whether the central government had such a measure at the time.

[24] There were 13 right-wing governors and 12 non-right-wing governors elected at the end of 1990. The race for governor of the state of Alagoas had to be repeated in early 1991. At the time the 1991 official estimates were issued in late 1990, the party affiliation of the governor was therefore not known and so I drop municipalities from Alagoas state for the specifications in columns 3 and 4 of Table 7.

Table 3
Summary statistics.

Variable	Obs.	Mean	Std. D.	Min	Max
Population data (IBGE)					
1991 official population ('000)	4451	26.89	50.49	0.20	1246.8
1991 census population ('000)	4451	24.32	48.83	0.75	846.4
1991 bracket error using 1991 official population	4451	0.57	0.89	−6	7
1991 bracket error using 1989 official population	4332	0.07	0.9	−7	9
1991 bunch status (0/1)	4451	0.42	0.49	0	1
1985 official population ('000)	3942	22.60	42.54	0.10	1094.8
1985 interpolated population ('000)	3942	24.05	47.56	0.78	1186.9
1985 bracket error	3942	0.10	0.76	−9	8
1980 census population ('000)	3887	23.01	43.11	0.73	1094.8
Elections data (supreme electoral tribunal)					
1990 right-wing vote share	3757	0.49	0.23	0.01	0.99
1990 right-wing vote share (incl. minor)	3757	0.52	0.23	0.02	1.00
1990 right-wing vote share (excl. PRN)	3757	0.38	0.22	0.00	0.98
1990 right-wing municipality-dominant deputy (>0.50)	3686	0.37	0.48	0	1
1990 non-right-wing municipality-dominant deputy (>0.50)	3686	0.11	0.31	0	1
1989 right-wing mayor (0/1)	4276	0.54	0.49	0	1
1988 electorate ('000)	4442	13.6	27.08	0	493.8
1982 right-wing vote share	4086	0.62	0.23	0.03	1.00
Municipality characteristics (census)					
1991 income per capita (% of minimum salary)	4450	0.72	0.42	0.14	3.48
1991 average years of schooling (25 years and older)	4451	3.15	1.22	0.34	8.84
1991 poverty headcount ratio (national poverty line, %)	4450	62.9	21.74	4.83	98.9
1991 Gini index of income inequality	4451	0.53	0.05	0.35	0.79
1991 population living in urban areas (%)	4450	0.52	0.23	0.02	1.00
1980 income per capita (% of minimum salary)	3990	0.82	0.46	0.06	3.57
1980 average years of schooling (25 years and older)	3990	2.08	1.07	0.1	7.2
1980 poverty headcount ratio (national poverty line, %)	3990	56.3	22.7	1.69	97.9
1980 population living in urban areas (%)	3950	0.32	0.21	0.00	1.00

Notes: Right-wing consists of the following political parties: Partido da Frente Liberal (PFL), Partido Democrático Social (PDS), Partido Trabalhista Brasileiro (PTB), Partido Democrata Cristão (PDC), Partido Liberal (PL), Partido da Reconstrução Nacional (PRN). Minor right-wing parties include: Partido Social Democrático (PSD), Partido Trabalhista Renovador (PTR), Partido Social Cristão (PSC), Partido Social Trabalhista (PST).

right-wing coalition-dominant deputy, each equal to one if a deputy vote share in the municipality exceeds a given cutoff of the total coalition vote, e.g. 50%, and zero otherwise. Because electoral coalitions may vary by state, all dummies are calculated for each state separately.

5. Data

The data used in this study come from several sources. Official population estimates stem from successive reports issued by the Federal Court of Accounts (Tribunal de Contas da União, TCU). Although estimates are produced by the National Statistical Agency (Instituto Brasileiro de Geografia e Estatística, IBGE), it is the responsibility of the TCU to compute municipalities' coefficients k_{mt} in accordance with Decree 1881/81 (Table 1). The total number of municipalities in Brazil at the end of 1990 (when the forecast for 1991 was made) was 4490. Of these, 27 were state capitals, and for another 12 the forecast was not available, resulting in a sample size of 4451. 1991 census population figures come from the National Statistical Agency.

Data on FPM transfers were self-reported by municipality officials and compiled into reports by the Secretariat of Economics and Finance inside the federal Ministry of Finance. The FPM data are somewhat noisy as there is sometimes substantial under-reporting of transfers received from the federal government. Unfortunately, more reliable data directly from the Ministry of Finance are not available for the early nineties. The financial data were converted into 2008 currency units using the GDP deflator for Brazil. Electoral data for the Câmara Federal dos Deputados in 1990 and the municipal executive elections in 1989 are from the Supreme Electoral Tribunal (Tribunal Superior Eleitoral, TSE). Again, these data are somewhat incomplete both in terms of available variables and observations.

As discussed below, I include the following municipality characteristics from the 1980 and 1991 census as control variables: the level of municipality income per capita, average years of schooling for individuals 25 years and older, the poverty headcount ratio, the Gini index of income inequality, and the percent of the municipal population living in urban areas. Data on these municipality characteristics are based on the 10% and 20% samples of the 1991 census and the 25% sample of the 1980 census and were calculated by the National Statistical Agency (only a shorter census survey was administered to 100% of the population). Table 3 gives descriptive statistics.

6. Estimation approach

Throughout the analysis, I will focus on the 1991 bracket error, defined as $5\times[k(pop_m^e)-k(pop_m)]$, from Section 3 above. Specifically, the dependent variable is the positive bracket error, equal to one if the bracket error is positive and zero otherwise.[25] The method of estimation is OLS throughout.

To discriminate between swing voter and core-support targeting, I start out with a linear specification in the right-wing vote share, followed by a quadratic specification, followed by specifications that add various control variables, such as polynomials in 1988 electorate and 1991 census population, state fixed effects, census population bracket indicators based on Table 1, and income per capita. Finally I include other municipality characteristics (average years of schooling for individuals 25 years and older, poverty headcount ratio, Gini index of income inequality and percent of population living in urban areas), as well as all these covariates squared.

Controlling for municipality characteristics is important because the revision of population estimates may have been based on (local) evidence that a municipality's actual population placed it into higher transfer brackets. If these municipalities happened to favor right-wing parties in previous elections for example, the correlation between right-wing support and population estimates would be an upwardly biased measure of special-interest influence. If, however, there turns out to be a correlation between political determinants and official population estimates, *controlling* for municipality characteristics that might account for revisions of population estimates, this would be indicative of political interference.

Denoting by Y_{ms} the binary positive bracket error for municipality m in state s, and $\mathbf{X}_{ms}$ the full vector of controls mentioned above, the estimation equation is as follows:

$$\begin{aligned} Y_{ms} = {} & \beta_1 \text{Right} - \text{wing vote share}_{ms} \\ & + \beta_2(\text{Right}-\text{wing vote share}_{ms})^2 + \delta\mathbf{X}_{ms} + U_{ms}. \end{aligned} \quad (8)$$

To test whether states or municipalities that were governed by politicians affiliated with the ruling party at the center were more likely to be favored, I add the binary variables right-wing governor

[25] Results using the bracket error or the raw 1991 official population estimate are qualitatively similar and are available on request.

Table 4
Positive bracket error and right-wing vote share.

Dependent variable: positive 1991 bracket error (0/1)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Right-wing vote share	0.070**	0.582***	0.447***	0.610***	0.489***	0.492***	0.462***	0.469***
	(0.036)	(0.150)	(0.145)	(0.151)	(0.134)	(0.134)	(0.133)	(0.134)
(Right-wing vote share)^2		−0.512***	−0.354**	−0.610***	−0.449***	−0.453***	−0.423***	−0.430***
		(0.147)	(0.141)	(0.146)	(0.130)	(0.130)	(0.129)	(0.129)
Population controls	N	N	Y	Y	Y	Y	Y	Y
State fixed effects	N	N	N	Y	Y	Y	Y	Y
Census population bracket indicators	N	N	N	N	Y	Y	Y	Y
Income per capita	N	N	N	N	N	Y	Y	Y
Other municipality characteristics	N	N	N	N	N	N	Y	Y
(Other municipality characteristics)^2	N	N	N	N	N	N	N	Y
Observations	3716	3716	3707	3707	3707	3706	3706	3706
R-squared	0.001	0.004	0.091	0.159	0.339	0.340	0.347	0.350

Notes: OLS estimations. 1991 bracket error is defined as 5×[k(1991 official population)−k(1991 census population)], where k(.) is the step function defined in Decree 1881/81. Right-wing consists of the following political parties: PFL, PDS, PTB, PDC, PL and PRN. Right-wing vote share is from the 1990 elections for the Câmara Federal dos Deputados. Population controls consist of cubic polynomials in 1988 electorate and 1991 census population. Census population bracket indicators are based on Decree 1881/81. Income per capita and other municipality characteristics (average years of schooling for individuals 25 years and older, poverty headcount ratio, Gini index and percent of population living in urban areas) are based on the 1991 census. Heteroskedasticity-robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

and right-wing mayor, as well interactions with the right-wing vote share. Note that state effects and the right-wing governor indicator cannot both be included at the same time. The full specification is:

$$\begin{aligned} Y_{ms} = {} & \beta_1 \text{Right-wing vote share}_{ms} \\ & +\beta_2(\text{Right-wing vote share}_{ms})^2 \\ & +\gamma_1 \text{Right-wing governor}_s \\ & +\gamma_2 \text{Right-wing vote share}_{ms} \times \text{Right-wing governor}_s \\ & +\gamma_3(\text{Right-wing vote share}_{ms})^2 \times \text{Right-wing governor}_s \\ & +\gamma_4 \text{Right-wing mayor}_{ms} \\ & +\gamma_5 \text{Right-wing vote share}_{ms} \times \text{Right-wing mayor}_{ms} \\ & +\gamma_6(\text{Right-wing vote share}_{ms})^2 \times \text{Right-wing mayor}_{ms} \\ & +\delta \mathbf{X}_{ms} + U_{ms}. \end{aligned} \tag{9}$$

To test whether legislative bargaining might explain the observed program manipulation, I include the right-wing municipality-dominant deputy and non-right-wing municipality-dominant deputy dummies, as well as those for a right-wing coalition-dominant deputy and a non-right-wing coalition-dominant deputy. When all dummies are included, the omitted category is municipalities where no deputy dominated the municipality or coalition vote. The full specification is:

$$\begin{aligned} Y_{ms} = {} & \beta_1 \text{Right-wing vote share}_{ms} \\ & +\beta_2(\text{Right-wing vote share}_{ms})^2 \\ & +\alpha_1 \text{Right-wing municipality-dominant deputy}_{ms} \\ & +\alpha_2 \text{Non-right-wing municipality-dominant deputy}_{ms} \\ & +\alpha_3(1-\text{Municipality-dominant deputy}_{ms}) \\ & \times \text{Right-wing coalition-dominant deputy}_{ms} \\ & +\alpha_4(1-\text{Municipality-dominant deputy}_{ms}) \\ & \times \text{Non-right-wing coalition-dominant deputy}_{ms} \\ & +\delta \mathbf{X}_{ms} + U_{ms}. \end{aligned} \tag{10}$$

There might also have been more subtle manipulations of the program prior to 1991, which left the distribution of population estimates smooth at the cutoffs. Unfortunately, electoral data for the 1987–1990 congressional session, the first under the new democratic regime, is not readily available. Instead, I use data from 1985, the last year of the military government, to run the exact same tests as discussed above. The only right-wing parties in this period were the PDS (the party of the military regime) and the (very minor) PTB. The right-wing vote share in this period is based on the municipality-level vote for right-wing parties in the 1982 elections to the Câmara Federal dos Deputados. To test whether there is evidence of political interference in this period I use specification (8) above.

7. Estimation results

Table 4 presents estimates of β_1 and β_2 based on Eq. (8). The results provide clear statistical evidence of a non-linear relationship between the right-wing vote share and the probability of obtaining a fictitious population boost, irrespective of the control variables that are included. Fig. 5 plots the quadratic fit from Table 4, column (1), along with the sample proportion of municipalities with a positive 1991 bracket error in each of 25 non-overlapping bins that partition the support of the right-wing vote share. For opposition municipalities—those with a right-wing vote share close to zero—the probability of a favorable population estimate is about 0.32. As the right-wing vote share increases, so does this probability, until it peaks at about 0.48 (at a right-wing vote share of 0.56). The probability then falls back to a

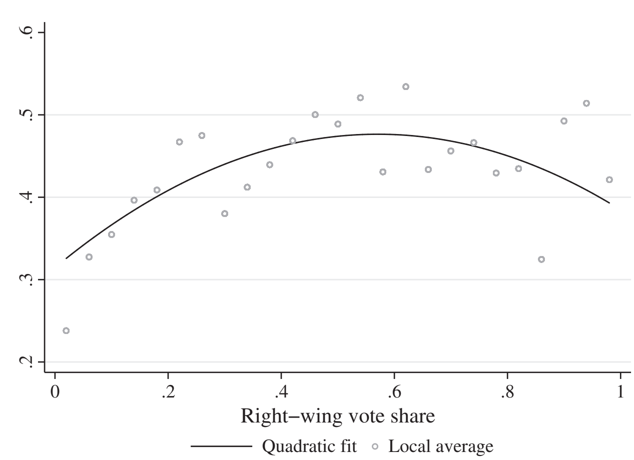

Fig. 5. Positive 1991 bracket error and right-wing vote share. *Notes*: 1991 bracket error is defined as 5×[k(1991 official population)−k(1991 census population)], where k(.) is the step function defined in Table 1. Right-wing vote share is from the 1990 elections for the Câmara Federal dos Deputados and includes the following political parties: PFL, PDS, PTB, PDC, PL, and PRN. Each dot represents the sample proportion of municipalities with a positive 1991 bracket error in a given bin. The bin-width is 0.04.

Table 5
Positive bracket error and right-wing vote share, including minor right-wing parties.

Dependent variable: positive 1991 bracket error (0/1)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Right-wing vote share	0.109***	0.545***	0.446***	0.657***	0.532***	0.540***	0.510***	0.515***
	(0.035)	(0.157)	(0.152)	(0.158)	(0.140)	(0.140)	(0.139)	(0.140)
(Right-wing vote share)^2		−0.411***	−0.292**	−0.621***	−0.462***	−0.471***	−0.440***	−0.449***
		(0.145)	(0.140)	(0.149)	(0.132)	(0.132)	(0.132)	(0.132)
Population controls	N	N	Y	Y	Y	Y	Y	Y
State fixed effects	N	N	N	Y	Y	Y	Y	Y
Census population bracket indicators	N	N	N	N	Y	Y	Y	Y
Income per capita	N	N	N	N	N	Y	Y	Y
Other municipality characteristics	N	N	N	N	N	N	Y	Y
(Other municipality characteristics)^2	N	N	N	N	N	N	N	Y
Observations	3716	3716	3707	3707	3707	3706	3706	3706
R-squared	0.003	0.005	0.092	0.159	0.340	0.341	0.348	0.350

Notes: OLS estimations. 1991 bracket error is defined as 5×[k(1991 official population)−k(1991 census population)], where k(.) is the step function defined in Decree 1881/81. Right-wing consists of the following political parties: PFL, PDS, PTB, PDC, PL, PRN, PSD, PTR, PSC and PST. Right-wing vote share is from the 1990 elections for the Câmara Federal dos Deputados. Population controls consist of cubic polynomials in 1988 electorate and 1991 census population. Census population bracket indicators are based on Decree 1881/81. Income per capita and other municipality characteristics (average years of schooling for individuals 25 years and older, poverty headcount ratio, Gini index and percent of population living in urban areas) are based on the 1991 census. Heteroskedasticity-robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 6
Positive 1985 bracket error and right-wing vote share.

Dependent variable: positive 1985 bracket error (0/1)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Right-wing vote share	0.036**	0.224**	0.049	−0.113	−0.099	−0.110	−0.134	−0.107
	(0.017)	(0.090)	(0.094)	(0.094)	(0.094)	(0.094)	(0.093)	(0.095)
(Right-wing vote share)^2		−0.143**	−0.032	0.082	0.077	0.081	0.093	0.073
		(0.071)	(0.070)	(0.070)	(0.069)	(0.069)	(0.069)	(0.070)
Population controls	N	N	Y	Y	Y	Y	Y	Y
State fixed effects	N	N	N	Y	Y	Y	Y	Y
Census population bracket indicators	N	N	N	N	Y	Y	Y	Y
Income per capita	N	N	N	N	N	Y	Y	Y
Other municipality characteristics	N	N	N	N	N	N	Y	Y
(Other municipality characteristics)^2	N	N	N	N	N	N	N	Y
Observations	3934	3934	3877	3877	3877	3876	3838	3838
R-squared	0.001	0.002	0.296	0.330	0.340	0.340	0.351	0.351

Notes: OLS estimations. 1985 bracket error is defined as 5×[k(1985 official population)−k(1985 interpolated population)], where k(.) is the step function defined in Decree 1881/81. Right-wing consists of the following political parties: PDS, PTB. Right-wing vote share is from the 1982 elections for the Câmara Federal dos Deputados. Population controls consist of cubic polynomials in 1980 census population and 1985 interpolated population. Census population bracket indicators are based on Decree 1881/81. Income per capita and other municipality characteristics (average years of schooling for individuals 25 years and older, poverty headcount ratio and percent of population living in urban areas) are based on the 1980 census. Heteroskedasticity-robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

level of about 0.40 for conservative core-support municipalities (right-wing vote share of 1.00).[26]

Table 5 reports estimates of β_1 and β_2 based on Eq. (8) using an extended definition of the right-wing vote share, including minor right-wing parties. Again, the estimates provide clear statistical evidence of a non-linear relationship between the right-wing vote share and the probability of obtaining an overly favorable transfer bracket, irrespective of the control variables that are included. As an additional robustness check, I also use a restricted definition of the right-wing vote, excluding the party of the president, the PRN. Again, results (not shown) are quantitatively similar to those in Tables 4 and 5. The key result is therefore that municipalities with roughly equal right-wing and non-right-wing vote shares benefited relative to opposition or conservative core-support municipalities, which is consistent with swing-voter targeting by the right-wing central government.

In sharp contrast to the clear evidence of special-interest politics in the 1991 official estimates, there is no evidence of similar interference in the 1985 official estimates. Table 6 shows estimation results for the binary positive 1985 bracket error as the dependent variable based on Eq. (8). While the estimates of β_1 and β_2 in the first two columns indicate swing-voter targeting, adding population controls eliminates this "effect". The evidence thus suggests that, although the grand redistribution scheme discussed here was shielded from tactical redistribution during the dictatorship, the same program became subject to special-interest politics after the transition to democracy.

Table 7 reports estimates of β_1, β_2, and all the γs based on Eq. (9). The first column replicates the results from Table 4, column 8 above. Column 2 drops the state effects and results remain essentially unchanged. Column 3 adds the right-wing governor indicator and its interactions with the right-wing vote share and the squared right-wing vote share. The point estimates on these interactions suggest that swing-voter targeting was in fact driven entirely by states with non-right-wing governors. Among aligned states, there is no statistical evidence of core-support or swing-voter targeting.[27] Although the estimates imply that the two conditional expectation functions are quite different, statistically, the evidence against a common

[26] Going from a right-wing vote share of 0 to 0.56 increases the estimated probability of a positive bracket error by: $0.58\times0.56-0.51\times0.56^2=0.165$.

[27] $\hat{\beta}_1+\hat{\gamma}_2=-0.019$, $se\left(\hat{\beta}_1+\hat{\gamma}_2\right)=0.307$; $\hat{\beta}_2+\hat{\gamma}_3=0.063$, $se\left(\hat{\beta}_2+\hat{\gamma}_3\right)=0.266$. For the joint null hypotheses $H_0\!:\beta_1+\gamma_2=0$ and $\beta_2+\gamma_3=0$ the F-statistic and [p-value] are: 0.39, [0.677].

Table 7
Positive bracket error and right-wing vote share, governors, and mayors.

Dependent variable: positive 1991 bracket error (0/1)	(1)	(2)	(3)	(4)	(5)
Right-wing vote share	0.469***	0.431***	0.581***	0.842***	0.758***
	(0.134)	(0.126)	(0.146)	(0.198)	(0.195)
(Right-wing vote share)^2	−0.430***	−0.357***	−0.521***	−0.791***	−0.765***
	(0.129)	(0.124)	(0.149)	(0.201)	(0.195)
Right-wing governor (0/1)			0.144	0.131	
			(0.088)	(0.091)	
Right-wing vote share × Right-wing governor (0/1)			−0.599*	−0.480	
			(0.343)	(0.356)	
(Right-wing vote share)^2 × Right-wing governor (0/1)			0.584*	0.426	
			(0.306)	(0.320)	
Right-wing mayor (0/1)				0.107*	0.106*
				(0.059)	(0.058)
Right-wing vote share × Right-wing mayor (0/1)				−0.582**	−0.567**
				(0.260)	(0.253)
(Right-wing vote share)^2 × Right-wing mayor (0/1)				0.602**	0.629**
				(0.257)	(0.250)
State fixed effects	Y	N	N	N	Y
F-statistics and [p-values]					
Right-wing governor indicator and all interactions zero H_0:$\gamma_1=\gamma_2=\gamma_3=0$			1.65	0.91	
			[0.176]	[0.437]	
Right-wing mayor indicator and all interactions zero H_0:$\gamma_4=\gamma_5=\gamma_6=0$				1.85	2.53
				[0.136]	[0.056]
Right-wing governor and mayor indicators and all interactions zero H_0:$\gamma_1=\gamma_2=\gamma_3=\gamma_4=\gamma_5=\gamma_6=0$				1.62	
				[0.138]	
Observations	3706	3706	3610	3467	3563
R-squared	0.350	0.322	0.323	0.342	0.367

Notes: OLS estimations. 1991 bracket error is defined as 5×[k(1991 official population) − k(1991 census population)], where k(.) is the step function defined in Decree 1881/81. Right-wing vote share is from the 1990 elections for the Câmara Federal dos Deputados and includes the following political parties: PFL, PDS, PTB, PDC, PL and PRN. Right-wing governor refers to the 1991–1994 term. Right-wing mayor refers to the 1989–1992 term. All regressions include population controls, census population bracket indicators, income per capita, as well as other municipality characteristics, as listed in Table 4. Heteroskedasticity-robust standard errors in parentheses.
*** p<0.01.
** p<0.05.
* p<0.1.

strategy across states (H_0:$\gamma_1+\gamma_2+\gamma_3=0$) is not very strong (p-value = 0.176).

Column 4 of Table 7 shows that a similar pattern emerges when the right-wing mayor indicator and interactions with the right-wing vote share and the squared right-wing vote share are included. The point estimates on these interactions suggest that swing-voter targeting was driven mostly by municipalities with non-right-wing mayors. Among municipalities with right-wing mayors, there is some evidence of swing-voter targeting, although the estimate of $\beta_2+\gamma_6$ is not significantly different from zero.[28] Fig. 6 illustrates these results. For municipalities with non-right-wing mayors, the probability of a favorable population estimate starts from about 0.28 for opposition municipalities, peaks at about 0.50, when right-wing and non-right-wing vote shares are about equal (0.525 to be precise), and falls back to about 0.30 for conservative core-support municipalities (right-wing vote share of 1.00).[29]

While these estimates imply that the conditional expectation functions are quite different depending on mayors' and governors' alignment with the central government, statistically, the evidence against a common strategy across states (H_0:$\gamma_1+\gamma_2+\gamma_3=0$), municipalities ($H_0$:$\gamma_4+\gamma_5+\gamma_6=0$), or both, is not very strong (p-values = 0.437, 0.136, 0.138), respectively. Only when state effects are included in column 5 can the equality of conditional expectation functions across municipalities with right-wing and non-right-wing mayors be rejected at the 10% level (p-value = 0.056). Overall, this pattern of results suggests that the conservative central government was attempting to

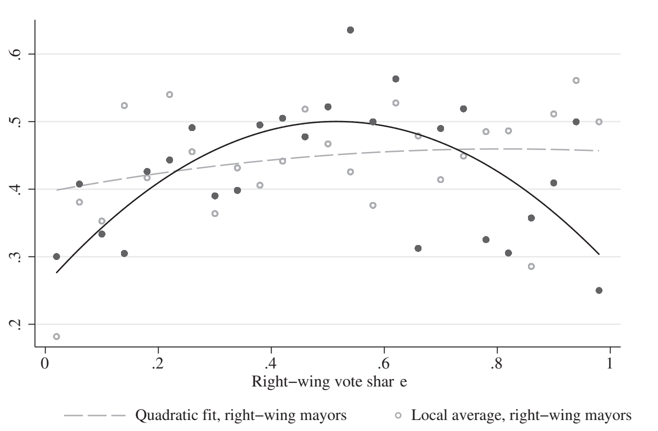

Fig. 6. Positive 1991 bracket error and right-wing vote share, by right-wing status of the mayor. *Notes*: 1991 bracket error is defined as 5×[k(1991 official population) − k(1991 census population)], where k(.) is the step function defined in Table 1. Right-wing vote share is from the 1990 elections for the Câmara Federal dos Deputados and includes the following political parties: PFL, PDS, PTB, PDC, PL, and PRN. Each dot represents the sample proportion of municipalities with a positive 1991 bracket error for a given bin. The bin-width is 0.04.

[28] $\hat{\beta}_1+\hat{\gamma}_5=0.260$, $se\left(\hat{\beta}_1+\hat{\gamma}_5\right)=0.199$; $\hat{\beta}_2+\hat{\gamma}_6=-.189$, $se\left(\hat{\beta}_2+\hat{\gamma}_6\right)=0.198$. For the joint null hypotheses H_0:$\beta_1+\gamma_5=0$ and $\beta_2+\gamma_6=0$ the F-statistic and [p-value] are: 1.73, [0.177].

[29] Going from a right-wing vote share of 0 to 0.525 increases the estimated probability of a positive bracket error by: $0.84\times0.525-0.79\times0.525^2=0.22$.

Table 8
Positive bracket error, right-wing vote share and dominant deputy dummies.

Dependent variable: positive 1991 bracket error (0/1)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Right-wing vote share	0.469***	0.410***	0.412***	0.420***	0.419***	0.389***	0.400***	0.428***	0.441***
	(0.134)	(0.139)	(0.139)	(0.139)	(0.139)	(0.139)	(0.140)	(0.140)	(0.139)
(Right-wing vote share)^2	−0.430***	−0.342**	−0.345**	−0.349**	−0.347**	−0.330**	−0.333**	−0.365***	−0.372***
	(0.129)	(0.136)	(0.136)	(0.136)	(0.136)	(0.136)	(0.138)	(0.137)	(0.136)
Deputy vote share required to qualify as municipality-dominant:		50%	50%	50%	50%	60%	70%	80%	90%
Right-wing municipality-dominant deputy (0/1)		−0.036	−0.049**	−0.049***	−0.041**	−0.039*	−0.048*	−0.030	−0.052
		(0.022)	(0.020)	(0.019)	(0.018)	(0.023)	(0.026)	(0.031)	(0.048)
Non-right-wing municipality-dominant deputy (0/1)		−0.036	−0.049*	−0.045*	−0.037	−0.070**	−0.059	−0.099	−0.098
		(0.029)	(0.027)	(0.026)	(0.025)	(0.035)	(0.043)	(0.064)	(0.098)
Deputy vote share required to qualify as coalition-dominant:		50%	60%	70%	80%	50%	50%	50%	50%
(1 − Municipality-dominant deputy) (0/1) × Right-wing coalition-dominant deputy (0/1)		−0.004	−0.007	0.014	0.044	−0.024	−0.025	−0.032**	−0.030*
		(0.021)	(0.025)	(0.030)	(0.041)	(0.018)	(0.016)	(0.016)	(0.016)
(1 − Municipality-dominant deputy) (0/1) × Non-right-wing coalition-dominant deputy (0/1)		0.003	−0.026	−0.044*	−0.032	0.011	0.006	0.006	0.002
		(0.022)	(0.021)	(0.023)	(0.027)	(0.019)	(0.018)	(0.017)	(0.017)
F-statistics and [p-values]									
All municipality- and coalition-dominant deputy dummies zero $H_0: \alpha_1 = \alpha_2 = \alpha_3 = \alpha_4 = 0$		1.44	1.82	2.43	2.06	2.20	1.63	1.48	1.21
		[0.218]	[0.112]	[0.045]	[0.084]	[0.066]	[0.164]	[0.205]	[0.304]
Observations	3706	3236	3236	3236	3236	3236	3236	3236	3236
R-squared	0.350	0.357	0.357	0.358	0.357	0.358	0.357	0.357	0.357

Notes: OLS estimations. 1991 bracket error is defined as 5 × [k(1991 official population) − k(1991 census population)], where k(.) is the step function defined in Decree 1881/81. Right-wing vote share is from the 1990 elections for the Câmara Federal dos Deputados and includes the following political parties: PFL, PDS, PTB, PDC, PL and PRN. Right-wing and non-right-wing municipality- and coalition-dominant deputy dummies are calculated separately for electoral coalitions of each state. All regressions include all the controls listed in Table 4. Heteroskedasticity-robust standard errors in parentheses.
*** $p<0.01$.
** $p<0.05$.
* $p<0.1$.

bring non-ideological voters in non-aligned lower-level governments—municipalities in particular—into the fold.[30]

Table 8 reports estimates of β_1, β_2, and all the αs based on Eq. (10). The first column replicates the results from Table 4, column 8 above. Columns 2 to 5 use a 0.5 cutoff to determine whether a deputy is considered dominant in a given municipality, and different cutoffs—ranging from 0.5 to 0.8—to determine coalition dominance among municipalities without a dominant deputy. Columns 6 to 9 use cutoffs ranging from 0.5 to 0.8 to determine whether a deputy is considered dominant in a given municipality, and a fixed cutoff of 0.5 to determine coalition dominance among municipalities without a dominant deputy. The first result that stands out in Table 8 is that including the dominant deputy dummies does not alter the estimates of β_1 and β_2. Second, the estimates of α_1 and α_2 in columns 3 to 7 suggest that municipalities with a municipality-dominant deputy (right-wing or non-right-wing) were about 4 to 7 percentage points less likely to receive overly favorable population estimates compared to communities where no deputy dominated the municipality or coalition vote. In columns 4 to 6 there is also some evidence against the joint null hypotheses that all municipality- and coalition-dominant deputy dummies are zero ($H_0: \alpha_1 + \alpha_2 + \alpha_3 + \alpha_4 = 0$) since p-values are below 0.10.

[30] As is evident from Table 7 and Fig. 6, among core opposition municipalities (right-wing vote share = 0), those with right-wing mayors had a 10 percentage point higher chance to get favorable treatment. Among core support municipalities (right-wing vote share = 1.00) those with right-wing mayors had a 13 to 17 percentage point higher chance to get favorable treatment based on estimates in columns 4 and 5.

8. Conclusion

This paper documents that even a rules-based transfer program anchored in the constitution and in the national tax code—as opposed to programs funded through the annual budget—and based on apparently technocratic inputs is not always immune to special-interest politics. The analysis suggests that over the decade of the 1990s, FPM revenue-sharing transfers were targeted at municipalities with roughly equal right-wing and non-right-wing vote shares at the expense of opposition or conservative core-support municipalities. In addition, there is some evidence that is consistent with legislative coalition-building by the central government executive. In contrast, there is no evidence of swing- or core-voter targeting in the last population estimates made under the military government that had set up the revenue-sharing mechanism in 1965.

Additional explanations for the program manipulation are of course possible. For example, bureaucrats may have simply bumped up those municipalities that paid the highest bribes. This type of corruption would be exceedingly hard to detect in the data. It is also conceivable that favored municipalities were part of influential federal politicians' networks. In exchange for funds transferred under the FPM, federal politicians likely received monetary kickbacks, which they used to finance their campaign spending and cultivate their personal vote. Municipalities that are in the network are not necessarily the municipalities that provided most electoral support for federal politicians, however, which makes this type of special-interest politics difficult to detect (Samuels, 2002).

Nonetheless, the results presented here do suggest that the exclusive focus on discretionary transfers in the extant empirical literature on special-interest politics may understate the true scope of tactical redistribution that is going on under programmatic disguise. Investigation of other seemingly special-interest-proof programs, including direct transfer programs to individuals as in Camacho and Conover (2011), is thus an obvious avenue for future research.

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