Rules vs. political discretion: evidence from constitutionally guaranteed transfers to local governments in Brazil*

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Abstract

Can rules be used to shield public resources from political interference? The Brazilian constitution and national tax code stipulate that revenue sharing transfers to municipal governments be determined by the size of counties in terms of estimated population. In this paper I document that the population estimates which went into the transfer allocation formula for the year 1991 were manipulated, resulting in significant transfer differentials over the entire 1990's. I test whether conditional on county characteristics that might account for the manipulation, center-local party alignment, party popularity and the extent of interparty fragmentation at the county level are correlated with estimated populations in 1991. Results suggest that revenue sharing transfers were targeted at right-wing national deputies in electorally fragmented counties as well as aligned local executives.

Keywords: Bureaucracy, institutions, redistributive politics, electoral competition

JEL: H77, D72, D73

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1 Introduction

In many federations around the world the redistribution of a substantial part of national tax revenues to local governments is prescribed by the constitution and based on objective criteria of need, such as population.[1] While the explicit goal of such revenue-sharing mechanisms is to promote inter-regional equity there is both theory and ample evidence to suggest that much public resource allocation is driven by politicians' electoral goals which are unlikely to coincide with stipulated equity goals. The question thus arises whether shielding public resources from political interference through institutional arrangements works in practice. Little is know about this issue because the empirical literature on redistributive politics has generally taken for granted that constitutionally anchored revenue-sharing mechanisms are implemented without regard to political considerations.

In this paper I demonstrate that the major constitutionally mandated Brazilian intergovernmental transfer program was circumvented and manipulated for political gain over the 1990's. Specifically, I document that the population estimates which went into the transfer allocation formula for the year 1991 were manipulated, as evidenced by their discontinuous distribution around several thresholds determining transfer brackets. The manipulation substantively increased the number of over-classified counties relative to transfer brackets warranted by their actual populations and resulted in economically important transfer differentials. Counties that located above the various population cutoffs in 1991 received additional transfers of about USD 22 million over the entire decade of the 90s and beyond because coefficients were subsequently grandfathered.[2] For small local governments this transfer differential amounted to about 15% of their public budgets.

An important question is whether this manipulation reflects political interference. In order to distinguish between corruption and technocratic judgement as potential explanations, I evaluate which, if any, of several political economy models outlined below best explain the observed program manipulation. In particular, I test whether conditional on

[1] Major federations include Brazil, Canada, Germany and India [Boadway and Shah 2007].

[2] The cumulative difference in FPM transfers over the period from 1991 to 1999 was about R$ 30 million. The Real/$ purchasing power parity exchange rate in 2005 was about 1.4 [World Bank 2008].

county characteristics that might account for the manipulation, center-local party alignment, party popularity and the extent of interparty fragmentation at the county level are correlated with estimated populations in 1991. The findings suggest that the main beneficiaries were right-wing national deputies in electorally fragmented counties. Under the assumption that political fragmentation at the local level proxies for swing constituencies, these results are consistent with the "aligned swing" prediction of Arulampalam, Dasgupta, Dhillon and Dutta's [2008] model. There is also some evidence that intergovernmental transfers were targeted at aligned local executives which is consistent with predictions from the ADDD and Khemani [2007] models.

While little is known about the robustness of attempts to shield public resources from political interference through formal rules, the paper by Khemani [2007] tests the related question whether delegation of fiscal policy to an independent agency can mitigate political distortions. Khemani shows that while discretionary federal transfers to aligned states in India were higher relative to non-aligned states over the period 1972-1995, agency determined transfers to aligned states were actually lower. The net effect of center-state alignment on federal transfers was still positive but statistically insignificant, i.e. the independent fiscal agency substantially offset the effects of political manipulation by the national executive.

The remainder of this paper is organized as follows: Section 2 presents institutional background on the revenue sharing mechanism between the federal and local governments in Brazil and provides evidence of program manipulation. Section 3 gives an overview of the literature on electoral incentives and public spending, including the existing literature on Brazil. Section 4 describes the data. Section 5 shows how I translate the various predictions of the patronage literature into empirically testable hypotheses given the political and institutional environment in Brazil around 1990. Section 5 also gives details on the estimation approach. Estimation results are presented in section 6. The final section concludes with a discussion of the limitations as well as extensions to the analysis presented here.

2 Institutional background

In this section, I first describe the economic importance and mechanics of the federal revenue sharing fund for municipal governments. 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 counties that were over-classified relative to transfer brackets warranted by their actual populations. I also show that the manipulation had economically significant effects on the distribution of revenue sharing funds and discuss why the effects of the manipulation extend to the present day.

2.1 Importance and mechanics of revenue sharing

Intergovernmental transfers finance most of local government spending on primary education, primary health care and local public transportation in Brazil.[3] The most important among these transfers is the Fundo de Participacao Municipal (FPM), a constitutionally guaranteed federal revenue sharing fund.[4] FPM funds alone accounted for 45% of revenue in small to medium sized local governments in 2001 [BNDES 2002]. The FPM is funded by federal income tax and industrial products tax collections. According to the national tax code (Decree 1881/81) the amount of federal FPM transfers a county receives is determined by a rule which depends discontinuously on county population estimates as shown in table I.

Each county is assigned a coefficient $c_{it} = c(pop^e_{it})$ for the following calendar year based on the step function, $c(.)$, from Table I and its estimated population pop^e_{it}. For counties with up to 10188 inhabitants, the coefficient is 0.6, from 10189 to 13584 inhabitants, the coefficient is 0.8 and so forth. The law thus creates discontinuities in FPM transfers at these thresholds. There is 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 $c(pop^e_{it})$ determines the share of FPM resources available for state j

[3] In 2002, local governments were in charge of 16,6 % of total public revenue [BNDES 2003].
[4] Federal Constitution of Brazil, Art. 159 Ib.

that are distributed to county i. The amount of transfers to state j in turn depends on a percentage f_j of federal tax collection in year t, rev_t. The state shares are determined in the constitution and have remained unchanged for decades. For the state of Bahia, for example, the percentage is 9.2695. FPM_{ijt} is the amount transferred to county i in state j during year t as in:

$$FPM_{ijt} = \frac{c(pop^e_{ijt})}{\sum\limits_{s\,|\,j} c^e_{sjt}} f_j rev_t \tag{1}$$

Equation 1 makes it clear that local population estimates are the only determinant of cross-county variation in FPM funding in a given state.

Exact county population estimates are only available for census years or years when a national population count is conducted. For all other years, county population estimates are produced by the national statistical agency, IBGE. Prior to 1989 these estimates were updated only in years ending with the number 5. Since 1989 the estimates are updated on a yearly basis. The model currently used is based on a top-down approach that ensures consistency of estimates for lower level units (counties) 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_{jt}, in proportion to past state level census population numbers. Counties within a given state are grouped by quartile of both census population levels and past population growth between census years and growing counties are separated from shrinking counties. Each of these 20 groups of counties is then assigned its share of the state population estimate, pop^e_{jkt}, proportional to past group level census population. Finally, each county within each group is assigned its population estimate, pop^e_{ijkt}, based on past county level census information.

The specific formula for county population estimates is as follows:

$$pop^e_{ikjt} = (pop_{ikj80}/pop_{kj80})[a_{kj} pop^e_{jt} + b_{kj}] \quad t > 1988 \tag{2}$$

where

$$a_{kj} = \frac{pop_{kj80} - pop_{kj70}}{pop_{j80} - pop_{j70}} \quad k = 1, 2, ..., 20$$

$$b_{kj} = pop_{kj80} - a_{kj} pop_{j80}$$

Since local population estimates directly determine funding levels it is important to verify whether these estimates are indeed derived from the forecasting model described above. Figure 1 plots 1989 official population estimates against predicted estimates calculated using the above formula.[5] It is clear from the scatterplot that the formula predicts 1989 official population estimates quite well although there is some dispersion around the 45 degree line. The dispersion is related to the fact that the predicted estimates are not based on the same 1970 and 1980 census data that were used at the time official estimates were made in 1989. Another and probably more important reason for the dispersion is that origin counties ceded some population to newly created counties.[6] Finally, the dispersion might be related to political manipulation as further discussed below. The important point here, however, is that as a first approximation, 1989 predicted estimates track official estimates fairly closely.

2.2 Evidence on manipulation of population estimates

The first empirical fact established in this paper is that this tight link between formula-driven predictions and official estimates broke down over the next two years. This point is best demonstrated with the use of two histograms, one for the distribution of 1989 official estimates and the other for the 1991 official estimates. Figures 2 and 3 document 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

[5] Official estimates come directly from reports issued by the Federal Court of Accounts (TCU).

[6] In order to obtain forecasts for the newly created and origin counties I would need to know which counties lost territory to the newly created counties as well as access to census tract population numbers from 1980 which are not readily available.

transfer brackets and even more obvious spikes immediately above those cutoffs.[7] The histogram actually understates the discontinuity of the density around the cutoffs because the spikes occur at specific points on the support.[8] The total number of counties that were placed on any one of these bunching points is 1870, which represents 42% of the counties receiving FPM transfers at the time. 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 population forecast model 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 population forecast model.[9]

The reasons for this manipulation or adjustment of population estimates is less clear. For example, it is possible that bureaucrats used some administrative rule to determine which estimates to revise. Officials were likely more averse to underestimate a county relative to a given threshold (type I error) than overestimating it (type II error) because underestimated counties 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 county 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 explanation is that all counties 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 counties 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 counties for other reasons as well.

[7] 1990 official estimates exhibit some bunching though not nearly as stark as the 1991 estimates.

[8] The exact bunching points are as follows: 10189, 10298, 13730, 17162, 24027, 30891, 37756, 44620, 51484, 61781, 72078, 82375, 92671, 102968, 116697, 130426, 144155, 157884.

[9] See McCrary [2007] for a formal test of the manipulation hypothesis.

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 counties. If this were the case and if the information they acted upon were more reliable than the predictions from the model, one would expect that the number of correctly classified counties in terms of decree 1881/81 increased with the manipulation. Since actual populations are known ex post, I can test whether this is indeed the case by comparing combined type I (underestimation) and type II (overestimation) errors that arise using the 1991 manipulated estimate 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.[10] Equation 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 to be limited.[11]

Table II below gives the distribution of *bracket errors*, defined as

$$5 * [c(official\ population) - c(1991\ actual\ population)]$$

where c(.) is the step function defined in decree 1881/81, that results from 1989 and 1991 official population estimates. The tabulation shows that the 1991 estimates sub-

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

[11] Alternatively, because the formula is in principle known, 1991 first-pass estimates could be generated given population data from the time the forecasts were made. This approach is complicated by the fact that new counties were created since the last census in 1980. In order to obtain 1991 forecasts for the newly created and origin counties I would need to know which counties lost territory to the newly created counties as well as access to census tract population numbers from 1980. Unfortunately these data are not readily accessible.

stantively increased the number of mis-classified counties from 33.36% to 47.85% (bracket error $\neq 0$) due to a higher number of over-classifications which more than offset a reduction in under-classifications compared to the 1989 estimates. Tables III and IV show that the entire mis-classification difference is driven by the manipulated counties, i.e. those locating on any one of the bunch points identified above. Overall results presented so far 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 pointing out that manipulation may not have been limited to the bunched counties. 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 the distribution of population estimates smooth at the cutoffs.

2.3 Economic significance of the manipulation

The 1991 manipulation resulted in significant transfer differentials. Counties that located above the various population cutoffs in 1991 received additional transfers of about USD 22 million over the entire decade of the 90s and beyond because coefficients were subsequently grandfathered.[12] For small local governments this transfer differential amounted to about 15% of their public budgets. Figure 4 below illustrates the persistence of this effect by scattering cell means of cumulative FPM transfers over the period 1991-1999 against the 1991 official population estimate.

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 counties would have had their coefficients reduced because the law stipulated that the thresholds be adjusted with population growth and these counties had grown less than the population average for Brazil. Some counties would have incurred a significant loss of transfers as a result of this reclassification [Brandt 2002].

[12] The cumulative difference in FPM transfers over the period from 1991 to 1999 was about R$ 30 million. The Real/$ purchasing power parity exchange rate in 2005 was about 1.4 [World Bank 2008].

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.[13] The only exception was for counties that were subdivided and lost population to newly-created counties. The revision of coefficients for these types of counties was done according to the existing population thresholds using the latest census population estimate. Underestimated counties' coefficients were updated pursuant to the publication of the census while overestimated counties' coefficients were not. It is not clear whether this adjustment was legal, given the language of supplementary law n^o 74/1993.

In 1996, there was a population count carried out by IBGE and the two houses of parliament 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.[14] 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 counties 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 $\bar{c}_i$ as the grandfathered coefficient for county i, $1[.]$ as the indicator function and α_t as the percentage reduction in the excess coefficient $\bar{c}_i - c(pop_{it-1})$, coefficients are currently calculated as

$$c_{it} = 1[c(pop_{it}) \geqslant \bar{c}_i]c(pop_{it}) + 1[c(pop_{it}) < \bar{c}_i][\bar{c}_i - \alpha_t(\bar{c}_i - c(pop_{it}))]$$

In March 2001 a new supplementary law was enacted in order to postpone full adjustment to 2008.[15] 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 in 1992. The fact that the manipulation of county population estimates doc-

[13] Supplementary Law n^o 74/1993.
[14] Supplementary Law n^o 91/1997.
[15] Supplementary Law n^o 106/2001.

umented above significantly increased the number of mis-classified counties casts doubts on technocratic explanations. The remainder of the paper turns to political explanations of the program manipulation.

3 Theory and evidence of distributive politics

Both theory and evidence suggest that public resource allocation is at least partly driven by politicians' electoral goals. Whether institutional arrangements restricting the scope of political discretion work in practice is an open empirical question. In this section I first review the theoretical political economy frameworks most relevant for the allocation of intergovernmental grants. I then discuss the existing empirical work on discretionary transfers, including the literature specific to Brazil.

3.1 Single decision-maker models: theory

Most theoretical models considered here assume that decision-makers design income redistribution platforms to maximize expected vote totals. Different predictions regarding optimal redistribution platforms arise as a result of alternative assumptions about the extent and riskiness of various groups' responsiveness to particularistic benefits and politicians' degree of risk-aversion [Cox and McCubbins 1986], or the strength of groups' ideological preferences and parties' relative abilities to efficiently target benefits to particular groups [Lindbeck and Weibull 1987; Dixit and Londregan 1996]. In the Cox and McCubbins model, risk averse politicians are thought to reward their core constituents relative to opposition or uncommitted groups, while candidates with more appetite for risk should actively court uncommitted groups.[16] In the Dixit and Londregan model, parties without a relative advantage to efficiently redistribute benefits but facing groups that are heterogeneous in terms of ideological preferences are expected to target those groups with weak ideological preferences relative to private consumption. Such groups are referred to as swing voters in their model. Alternatively, if parties do differ in how effi-

[16] The prediction also requires that the electoral response to pork is more uncertain for uncommitted groups relative to core support groups [Cox and McCubbins 1986].

ciently they manage to distribute benefits across groups, the model predicts asymmetrical redistribution platforms targeted at parties' respective core constituencies.

A central assumption in these models is that parties are able to claim credit for the benefits they deliver. When benefits take the form of unrestricted budgetary transfers from the central to local governments it seems reasonable to expect that credit-claiming for the central incumbent is less than perfect because at least some of the credit for higher spending goes to the party in power at the local level. Arulampalam, Dasgupta, Dhillon and Dutta [2008], ADDD for short, explicitly allow for credit-claiming imperfections in their model and derive predictions for the redistributive policies of a vote maximizing incumbent party at the center when ideological preferences of local constituencies are heterogeneous and the political orientation of state governments might be different from the orientation of the incumbent party at the center.

Their fist prediction is that when the incumbent at the center receives little or no direct political credit for transferring funds to local governments it should skew the distribution of resources towards aligned local governments, i.e. local governments ruled by the same party as the party at the center ("alignment effect"). Doing otherwise promotes the interests of the non-aligned ruling party at the local level at the expense of the central incumbent. When the potential for credit claiming for the center is low, the authors also argue that among aligned state governments, those with a higher proportion of swing voters should be favored ("aligned swing effect"), while among non-aligned governments swing communities should be discriminated against ("unaligned swing effect").[17] Khemani [2007] also presents a model with the same basic assumptions as in ADDD but focusing on party popularity instead of swing status and assuming zero credit-claiming ability for the center. Her model also predicts that the central government should skew the distribution of resources towards aligned state governments. The model makes no firm predictions about the effect of party popularity but predicts that party popularity should only matter for aligned states.

[17] ADDD actually show that the aligned swing effect obtains irrespective of the extent of goodwill leakage.

3.2 Executive-legislative bargaining models: theory

A final set of predictions relate to legislative coalition-building in presidential systems such as Brazil. Brazilian presidents face the difficult task of building legislative support in an extremely fragmented Congress in order to pass legislation. Ames [1995a, 1995b] argues that presidential coalition-building strategies are at least in part based on 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.

Ames [1995a, 1995b] discusses the incentives presidents and federal deputies face under such electoral rules and argues that deputies are more likely to trade votes for grants with the executive when they dominate a county's votes or at least face limited competition from within their own and other parties because this makes credit-claiming easier. On the other hand, Ames argues that high interparty competition might reflect weak ideological preferences and a community susceptible to particularistic benefits, provided that the deputy finds a way to claim political credit through an alliance with the local executive for example. The predicted effect of interparty fragmentation on redistribution is thus ambiguous. It seems reasonable to speculate, however, that alliances between deputies and local mayors are more likely to happen if they share the same political orientation. Such alliances, in turn make it more likely for a deputy to trade his vote for presidential favors. Similarly, deputies sharing the ideology of the president are more likely to be part of his legislative coalition [Arretche and Rodden 2004].

3.3 Single decision-maker models: evidence

The major problem for empirical testing of the types of models discussed above is that predictions hinge on rarely observed measures of politicians' risk aversion, parties' relative efficiency in distributing benefits to particular groups, the strength of groups' ideological preferences and ease of credit-claiming for favors dispensed. Given the various ways the theoretical concepts have been operationalized for empirical testing and given the differences in political institutions across countries it is not surprising that no general empirical regularities have emerged from this literature.

Some studies of distributive politics find 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. Conditional on the Democratic vote share in a district, these authors find no effect of the representative's party affiliation on federal spending. 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 governments is skewed towards loyal constituents. Similarly, Miguel and Zaidi [2003] find evidence of government targeting of funds to districts that support the ruling party in Ghana.

Some studies have explicitly attempted to test whether transfers are targeted at swing voters. Wright [1974] finds that states exhibiting higher variability in Democratic vote shares for Presidential elections received more federal spending and more work-relief jobs. 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. Ansolabehere and Snyder [2006], on the other hand, 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 is high.

There are also a number of empirical papers that deal with distributive politics in

Brazil. Ames [1995a] demonstrates that federal deputies in the 1987-1990 legislature were more likely to make amendments to the national budget in counties where their individual vote share in the previous election was high. He also finds that deputies target vulnerable municipalities, i.e. municipalities where incumbent deputies retired, in-migration was high and interparty and intraparty fragmentation were high. Similarly, Finan [2003] investigates federal deputies' amendments to the national budget over the entire 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. In addition to rewarding direct electoral support, presidents may also reward local mayors for their endorsement in the presidential race [Ames 1994].

Recent evidence from the Indian federation also suggests that central governments attempt to skew the distribution of resources towards aligned state governments, i.e. state governments ruled by the same party as the party at the center. Specifically, ADDD [2008] find that alignment matters for the allocation of *project-specific* discretionary grants in India over the period 1974-1997 but only in those states where the proportion of close state constituent elections was relatively high. The proportion of close national constituent elections does not seem to matter, irrespective of center-state alignment. Khemani [2007] on the other hand finds that over essentially the same time period, aligned states receive more *general purpose* grants irrespective of the closeness of previous state legislature elections. Khemani's results suggest, however, that among aligned states those with a lower share of national assembly delegates affiliated with the ruling party at the center receive more discretionary transfers while among non-aligned states party popularity does not seem matter. The national ruling party's popularity in state legislatures does not seem to matter either, irrespective of center-state alignment.

3.4 Executive-legislative bargaining models: evidence

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 Collor's emergency decrees in 1990. He finds that pork in the form of intergovernmental transfers, licences 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 the model of unilateral optimization by the central executive developed by Khemani [2007]. Arretche and Rodden do not investigate the interaction effect with state government party affiliation.

4 Data

The data used in this study come from a variety of sources. Official population estimates stem from successive reports issued by the federal court of accounts (TCU). Although estimates are produced by the national statistical agency, it is the responsibility of the TCU to compute counties' brackets in accordance with decree 1881/81. 1991 census population figures come from the national statistical agency (IBGE). Data on FPM transfers were self-reported by county officials and compiled into reports by the secretariat of economics and finance (SEF) inside the federal ministry of finance (MF). The FPM data are somewhat noisy as there is sometimes substantial under-reporting of transfers received from the federal government. Unfortunately, more reliable data from the ministry of finance are not available for the early nineties. The financial data were converted into 2005 currency units using the GDP deflator for Brazil. Electoral data for municipal executive (1988), national congress (1990) and presidential (1989) elections are from the Supreme Electoral Tribunal (TSE). Again these data are somewhat incomplete both in terms of available variables and observations. For the 1988 mayoral elections only the vote total for the winning candidate is available. In order to obtain a measure of local popularity of the executive incumbent I scale the winning candidate's vote by the county's 1988 electorate, rather than by total votes cast. Table V gives summary statistics.

5 Hypotheses and estimation approach

The main goal of this paper is to evaluate which, if any, of the political economy theories outlined above best explain the observed program manipulation. In this section I first discuss how I translate the various predictions into empirically testable hypotheses given the political and institutional environment in Brazil around 1990. I then give details on the estimation approach.

5.1 Hypotheses

The first prediction adapted from the models by ADDD [2008] and Khemani [2007] is that aligned counties, i.e. those that were governed by mayors affiliated with parties of the ruling coalition at the federal level, were more likely to obtain population estimates above a given threshold and hence receive more federal money than non-aligned counties. Both ADDD and Khemani emphasize that this prediction rests on the assumption that the incumbent party at the center receives little or no credit for the financing of local public services. When the transfers are for unrestricted local budget support, as in the Brazilian case and with the general purpose transfers analyzed by Khemani, it seems reasonable to expect that this condition obtains.

Determining allied parties and hence center-local alignment in Brazil's fragmented party system is somewhat difficult, however. It is even more complicated during Collor's presidency 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. In the empirical analysis below I refer to aligned counties as those headed by mayors affiliated with any of these political parties. Table V gives full party names and descriptive statistics of the political determinants used in the empirical analysis.

ADDD's model also predicts that among aligned counties those with a higher proportion of swing voters in local or national elections should be favored ("aligned swing effect"), while among non-aligned counties swing counties should be discriminated against ("unaligned swing effect"). ADDD use the proportion of close constituent elections in state and national contests in each state as their swing voter measure. Because Brazilian electoral rules for Congress consider each state a single multi-member electoral district, every vote counts equally and hence the notion of a "close" congressional race in a given county does not apply. Instead, I operationalize the swing voter concept as interparty fragmentation of the county vote in the 1990 national legislature elections. Interparty fragmentation is defined as 1-the sum of party vote shares squared, where party vote shares sum across deputy vote shares of a given party. High levels of interparty fragmentation are supposed to proxy for an electorate with relatively weak ideological preferences, i.e. many swing voters.

In Khemani's model, the popularity of the ruling coalition at the center is predicted to matter, though with indeterminate sign and only among aligned counties. Khemani's measure of party popularity is the share of state and national assembly delegates affiliated with the ruling party at the center in state and national legislatures, respectively. As a proxy measure for local popularity of the ruling coalition at the center I use the winning candidate's share of the electorate in the 1988 mayoral contest interacted with county alignment status. As another measure of the national ruling coalition's popularity I use the fraction of county votes cast for right-wing parties in the 1990 elections to the national chamber of deputies.

Similar predictions to those discussed so far can also be formulated regarding the national executive, i.e. presidential, election of 1989. Although presidents could not be re-elected until 1998, Collor might have simply rewarded his supporters or he might have tried to prepare a favorable terrain for his successor. I also tested these hypotheses and found no evidence that the 1989 Collor vote at the county level mattered, alone or interacted with alignment status of the local executive. For the sake of brevity these estimation results are not reported below.

5.2 Estimation approach

The main goal of this paper is to evaluate whether and how FPM transfers were manipulated for political gain. Specifically, I test whether conditional on county characteristics that might account for revisions of population estimates, such as 1988 electorate data and actual 1991 population, the measures of political conditions in local and national arenas described above are correlated with 1991 official population estimates. Controlling for county characteristics is important for all of these tests because revision of estimates may have been based on (local) evidence that a county's actual population placed it into higher transfer brackets as discussed in section 2.2 above. If these counties happened to favor right-wing parties in previous elections for example, the simple correlation of electoral support with transfers received would be an upwardly biased measure of political distortions. If, however, there turns out to be a correlation between past electoral outcomes and official population estimates, *controlling* for actual population, it would be indicative of political interference.

In order to address concerns about unobserved variables that might have been used to improve the county classification in accordance with decree 1881/81, I include second order polynomials of 1989 predicted population, 1988 electorate, 1991 actual local population, a set of indicators for the 1991 actual population classification as well as 1991 county characteristics such as income per capita, average years of schooling, poverty rate, gini index and urbanization rate in the regression specification.[18] Denoting by Y_{cs} the 1991 official population estimate for county c in state s for the year 1991, $\mathbf{X}_{cs}$ the vector of controls mentioned above and a_j a state fixed effect, the estimation equation is as follows:

$$\begin{aligned} Y_{cs} = \; & \alpha_1 \text{Right-wing mayor}_{cs} \\ & + \alpha_2 [\text{1-Right-wing mayor}_{cs}] * \text{Mayor's vote share}_{cs} \\ & + \alpha_3 \text{Right-wing mayor}_{cs} * \text{Mayor's vote share}_{cs} \\ & + \alpha_4 [\text{1-Right-wing mayor}_{cs}] * \text{Interparty fragmentation}_{cs} \\ & + \alpha_5 \text{Right-wing mayor}_{cs} * \text{Interparty fragmentation}_{cs} \\ & + \beta_1 [\text{1-Right-wing mayor}_{cs}] * \text{Right-wing vote share}_{cs} \\ & + \beta_2 \text{Right-wing mayor}_{cs} * \text{Right-wing vote share}_{cs} \\ & + \beta_3 [\text{1-Right-wing mayor}_{cs}] * \text{Right-wing vote share}_{cs} * \text{Interparty fragmentation}_{cs} \\ & + \beta_4 \text{Right-wing mayor}_{cs} * \text{Right-wing vote share}_{cs} * \text{Interparty fragmentation}_{cs} \\ & + \boldsymbol{\gamma} \mathbf{X}_{cs} + a_s + \varepsilon_{cs} \end{aligned} \tag{3}$$

One drawback of this specification is that the dependent variable, estimated county population, is continuous, i.e. it does not explicitly take into account the various brackets mandated by decree 1881/81 on which transfer allocations are ultimately based. As a specification check on this issue I also estimate interval regressions, i.e. ordered probit with the known cutoff points from decree 1881/81. As a further specification check, I also use the 1991 bracket *error,* i.e. 5*[c(1991 official population) - c(1991 actual population)], where c(.) is the step function defined in decree 1881/81, as the dependent variable in the statistical analysis. In order to interpret statistically and economically significant estimation results on the $\alpha's$ and $\beta's$ as evidence of political interference, the untested assumption is that conditional on covariates $\mathbf{X}_{cs}$ there is no omitted variable correlated

[18]Results are not sensitive to higher order polynomial specifications and are omitted for the sake of brevity.

with both 1991 official population and the political conditions variables.

Before presenting estimation results it is useful to briefly discuss the expected signs on the parameters in equation 3. The common prediction from the ADDD and Khemani models is that $\alpha_1 > 0$. The Khemani model further predicts $\alpha_2 = 0$ and leaves α_3 indeterminate while the ADDD model predicts $\alpha_4 < 0$ and $\alpha_5 > 0$. As discussed in section 3.2 above, the predicted effect of interparty fragmentation on redistribution might also be negative irrespective of alignment status, $\alpha_4 < 0$, $\alpha_5 < 0$, because low levels of fragmentation facilitate credit-claiming for deputies that are electorally dominant in a given county.

It also seems plausible that, at least in the Brazilian context, the payoff to the central government takes on other forms not included in the Khemani model, such as kickbacks from local officials [Samuels, 2002]. As a result, the central government might be willing to send additional funds to non-aligned local governments even if it cannot claim any political credit for the resulting public service improvements at the local level. If relatively unpopular local executives are willing to pay a higher price for additional central government transfers one might expect to see a negative correlation between redistribution and the mayor's vote share, irrespective of alignment, i.e. $\alpha_2 < 0$, $\alpha_3 < 0$.

More generally, local governments in Brazil might not be as passive as depicted in the formal models discussed above. In particular, local executives have strong electoral motives for seeking additional funds from the center. While electoral rules at the time prohibited mayors from seeking consecutive terms they were allowed to run again after skipping a term and many of them did so successfully. It is also likely that incumbent mayors had some interest in maintaining the local elite in power even if they themselves had to take a break from office. If better managed local governments (with presumably higher electoral support) were more likely to be successful in obtaining funds from the center (not least because of electoral clout in national elections) one would expect a positive correlation between party popularity at the local level and central government transfers received, i.e. $\alpha_3 > 0$. Grossman [1994] provides a formalization of this idea.

Khemani's model also predicts $\beta_1 = 0$ and is ambiguous about the sign of β_2. Arretche and Rodden's discussion on the other hand would predict $\beta_1 > 0$ and $\beta_2 > 0$ because aligned deputies are more likely to be involved in the president's legislative coalition than non-aligned deputies, irrespective of the mayor's party affiliation.

The prediction $\beta_1 > 0$ and $\beta_2 > 0$ also results from the ADDD model if voters give credit for public spending increases also to national deputies in addition to crediting local executives. This assumption is reasonable for the vast majority of small to medium sized Brazilian local governments which derive most of their financing from upper levels of government as discussed in section 2.1. From the central government's perspective, both high right-wing vote shares in congressional contests and right-wing local executives ensure that most political credit for redistributed benefits goes to aligned politicians.

Finally, ADDD's model would predict $\beta_4 > \beta_3 > 0$ since swing, i.e. highly fragmented, constituencies are more attractive targets the less likely it is that non-aligned politicians are able to take credit for spending increases. This in turn is least likely when the right-wing vote share is high and the local executive's party affiliation is aligned with the center.

As pointed out in section 2.2, there might 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 are not readily available. The analysis presented here is thus only concerned with the 1991 manipulation which had the most persistent effect on the distribution of transfers during the years to come.

6 Estimation results

Table VI presents estimation results for the model in equation 3. Parameter estimates in columns 1 and 2 are all positive but only α_4 is statistically significant, suggesting that official population estimates are increasing with interparty fragmentation among non-aligned, i.e. non-right-wing counties. There is thus no evidence that center-local alignment per se, nor the popularity of the mayor mattered for the revision of population estimates. While positive estimates of both α_4 and α_5 would be consistent with high risk appetite politicians in the Cox-McCubbins model and with swing voter targeting in the Dixit Londregan model, the magnitudes suggest that α_4 is significantly larger than α_5 (p-value=0.02), which is hard to reconcile with the theories reviewed here. Results in Column 3 suggest that both estimates of β_1 and β_2 are positive and statistically significant. This provides support for Arretche and Rodden's view that aligned deputies are more likely to be involved in the president's legislative coalition. These estimates are also consistent with the ADDD model if voters credit both national deputies and local executives for public spending increases. The results so far contradict the predictions of Khemani's model according to which the national coalition's popularity should not matter in non-aligned counties since these should not be targeted at all.

Column 4 tests whether counties that voted for right-wing deputies in the previous congressional elections were treated even more favorably in fragmented local party systems as ADDD's model would predict. The positive and statistically significant point estimates of β_3 and β_4 suggest that among right-wing counties, highly fragmented constituencies were more attractive targets than less fragmented constituencies, irrespective of the mayor's party affiliation. This disaggregation of the right-wing bias also shows that the puzzling favoritism of electorally fragmented non-aligned counties found in columns 1 through 3 was driven entirely by counties which also supported right-wing deputies. Among counties with a high right-wing vote share of say 0.8, a 2 standard deviation difference in interparty fragmentation (0.28) leads to a fictional population gain of about 1000, irrespective of county alignment.

Column 5 shows that these results are robust to the inclusion of other county covariates that might be correlated with electoral outcomes, such as county income per capita or average education levels. Overall, the pattern of results suggests that right-wing national deputies in electorally fragmented counties were the main beneficiaries of the program manipulation. To the extent that political fragmentation proxies for swing constituencies, these results are consistent with the "aligned swing" prediction of ADDD's model.

Table VII reports the results from the same tests discussed above but estimated using ordered probit with the known cutoff points from decree 1881/81. Results are quantitatively similar to those obtained above although it now appears that the interaction effect between party fragmentation and right-wing support is almost twice as large among aligned counties compared to non-aligned counties. Although the magnitudes are not statistically different form each other (p-value=0.51), these estimates provide further support for ADDD's "aligned swing" prediction. Among counties with a high right-wing vote share of 0.8 and governed by a right-wing mayor, a 2 standard deviation difference in interparty fragmentation (0.28) is now associated with a fictional population gain of about 1500. The fact that right-wing support for national deputies only matters in counties governed by right-wing executives is consistent with Khemani's prediction regarding party popularity although the difference is again statistically significant (p-value=0.36).

Table VIII reports the results from the bracket error specification for the dependent variable. For columns 1 and 2 results are qualitatively similar to those obtained above. Although right-wing support in congressional elections (column 3) is still associated with a higher bracket error, the effects are not statistically significant under this specification. Moreover it appears that the interaction effect between party fragmentation and right-wing support is now more than twice as large among aligned counties compared to non-aligned counties, as found in table VII as well. Again, the magnitudes are not statistically different from each other (p-value=0.27). One difference with the previous specifications is that mayor alignment per se, α_1, now seems to matter statistically, although only marginally so.

One advantage of the bracket error specification is that it is easier to interpret whether political conditions mattered economically and not just statistically. It appears, for example, that an interparty fragmentation difference of 2 standard deviations (0.28) at relatively high levels of right-wing support (0.8) in counties with a right-wing mayor amounts to a bracket error increase of about 0.22. This is of the same order of magnitude as the (marginally significant) 0.371 bracket error increase associated with right-wing mayors. The bracket error estimates thus corroborate the earlier finding that right-wing national deputies in electorally fragmented situations were among the main beneficiaries of the program manipulation and there is some evidence that this interaction was stronger in counties governed by right-wing mayors. The fact that right-wing support for national deputies only matters in counties governed by right-wing executives is consistent with Khemani's prediction regarding party popularity. Finally, the bracket error results provide some evidence that the political orientation of the local executive mattered per se as predicted by the Khemani and ADDD models. These findings are also generally in line with previous work on electoral determinants of *discretionary* transfers in Brazil [Ames 2001].

7 Conclusion

In this paper I have presented evidence that even a rule-based transfer program anchored in the constitution and in the national tax code and based on apparently technocratic inputs can be circumvented and manipulated for political gain. Specifically, the findings suggest that the manipulation was economically important and there is robust evidence that the main beneficiaries were right-wing national deputies in electorally fragmented counties. Under the assumption that political fragmentation proxies for swing constituencies, these results are consistent with the "aligned swing" prediction of ADDD's model. There is also some evidence that aligned local executives were targeted with FPM transfers which would be consistent with predictions from the Khemani and ADDD models.

While these results are suggestive of political interference there are two main caveats to the analysis presented here. The first is that it is remains possible that the correlations reported above suffer from omitted variable bias and/or specification error. The second caveat is that the detected effects might only be the tip of the iceberg. For example, bureaucrats may have simply bumped up those counties which paid the highest bribes [Shleifer and Vishny, 1993]. This type of corruption would be exceedingly hard to detect in the data. It is also conceivable that counties that were bumped up were part of influential federal politicians' networks [Fisman, 2001]. 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. Counties that are in the network may not be the counties that provided most electoral support for federal politicians, however [Samuels, 2002]. As a result, the reported correlations between political conditions and manipulation of county population estimates might significantly understate the true extent of patronage dealings.

There are also two main extensions to the analysis presented so far. The first is to investigate the mechanics of distributive politics during the transition towards democracy and in particular during the 1987- 1990 legislature. The second extension is to further disaggregate electoral competition into within and between party components. Previous work has found, for example, that dominant and aligned deputies fared particularly well in a given county when faced with a fragmented opposition [Ames 2001].

Table I

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

Table II: Bracket error distribution, full sample

1989 official population classification				1991 official population classification			
Bracket error	Freq.	Percent	Cum.	Bracket error	Freq.	Percent	Cum.
-7	1	0.02	0.02	-6	1	0.02	0.02
-6	3	0.07	0.09	-4	2	0.05	0.07
-5	7	0.16	0.25	-3	5	0.12	0.18
-4	9	0.21	0.46	-2	14	0.32	0.51
-3	18	0.42	0.88	-1	81	1.87	2.38
-2	82	1.89	2.77	0	2,259	52.15	54.52
-1	501	11.57	14.34	1	1,455	33.59	88.11
0	2,887	66.64	80.98	2	378	8.73	96.84
1	599	13.83	94.81	3	92	2.12	98.96
2	161	3.72	98.52	4	29	0.67	99.63
3	42	0.97	99.49	5	12	0.28	99.91
4	17	0.39	99.88	6	3	0.07	99.98
5	3	0.07	99.95	7	1	0.02	100.00
6	1	0.02	99.98				
9	1	0.02	100.00				

Notes: N=4332. Bracket error is defined as 5*[c(official population)-c(actual 1991 population), where c(.) is the step function defined in decree 1881/81. Tabulation excludes counties that were created between 1989 and 1991.

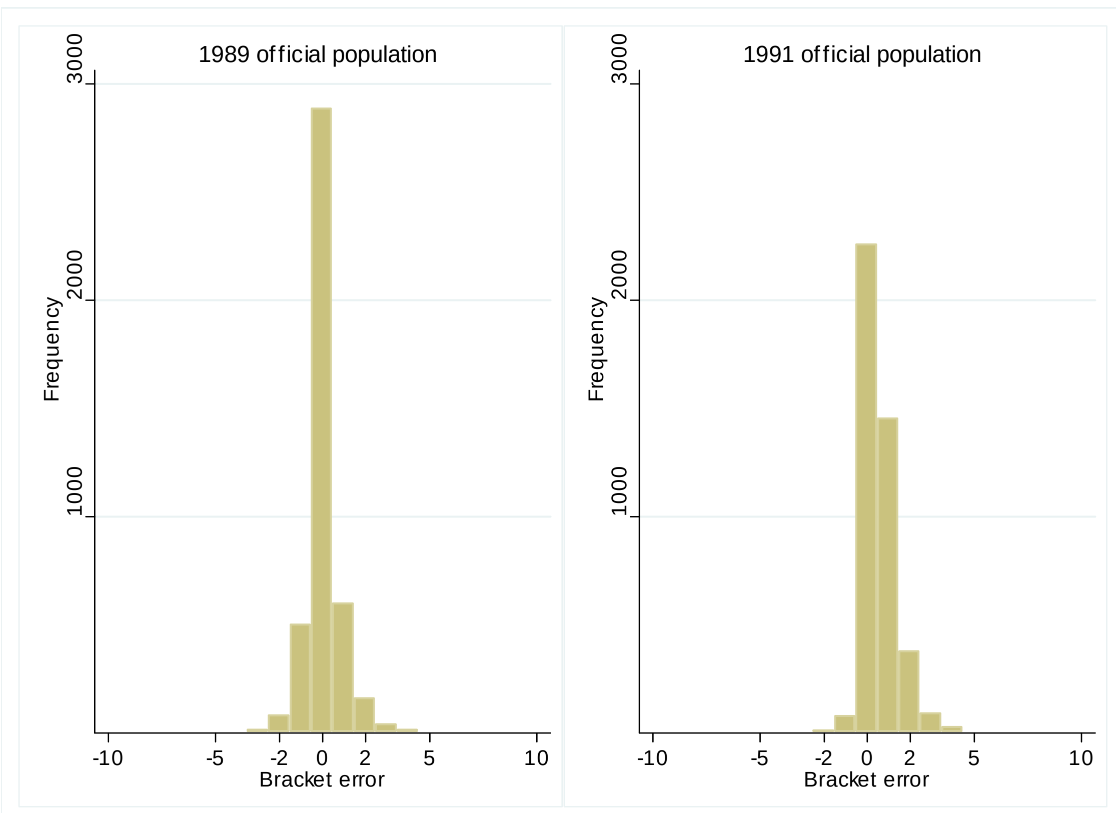


Table III: Bracket error distribution, bunched observations

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

Notes: N=1864. Bracket error is defined as 5*[c(official population)-c(actual 1991 population)], where c(.) is the step function defined in decree 1881/81. Tabulation excludes counties that were created between 1989 and 1991.

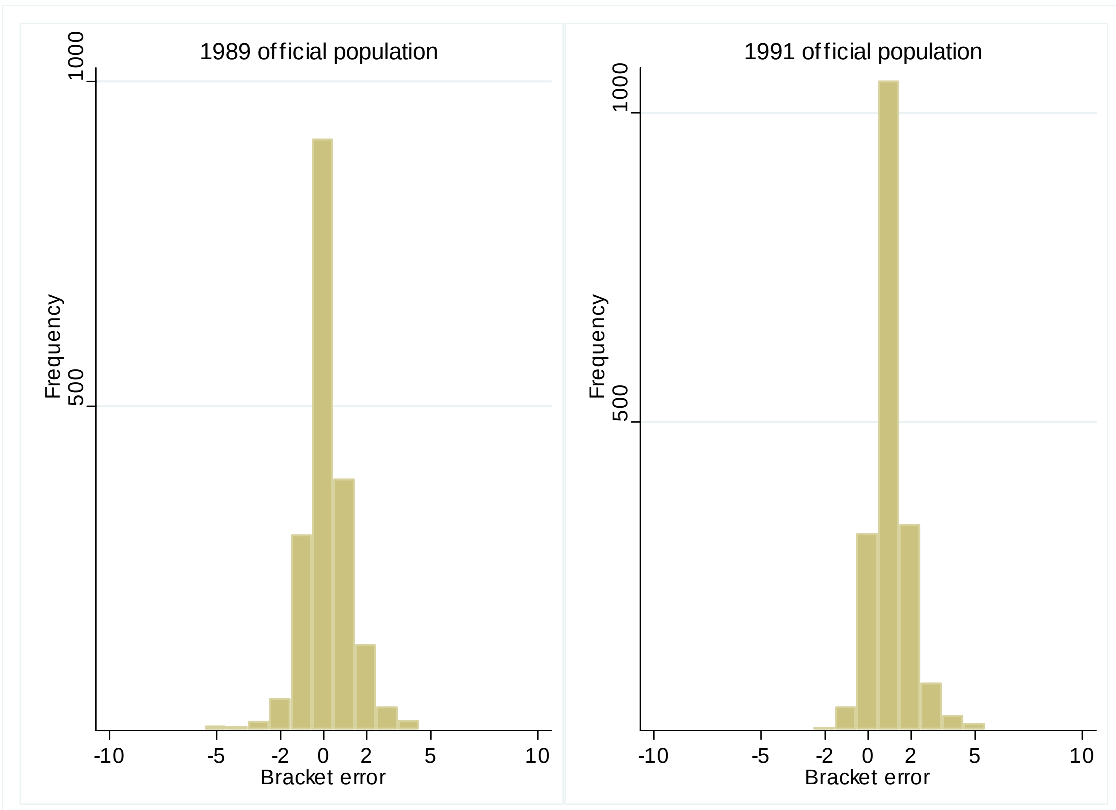


Table IV: Bracket error distribution, non-bunched observations

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

Notes: N=2468. Bracket error is defined as 5*[c(official population)-c(actual 1991 population)], where c(.) is the step function defined in decree 1881/81. Tabulation excludes counties that were created between 1989 and 1991.

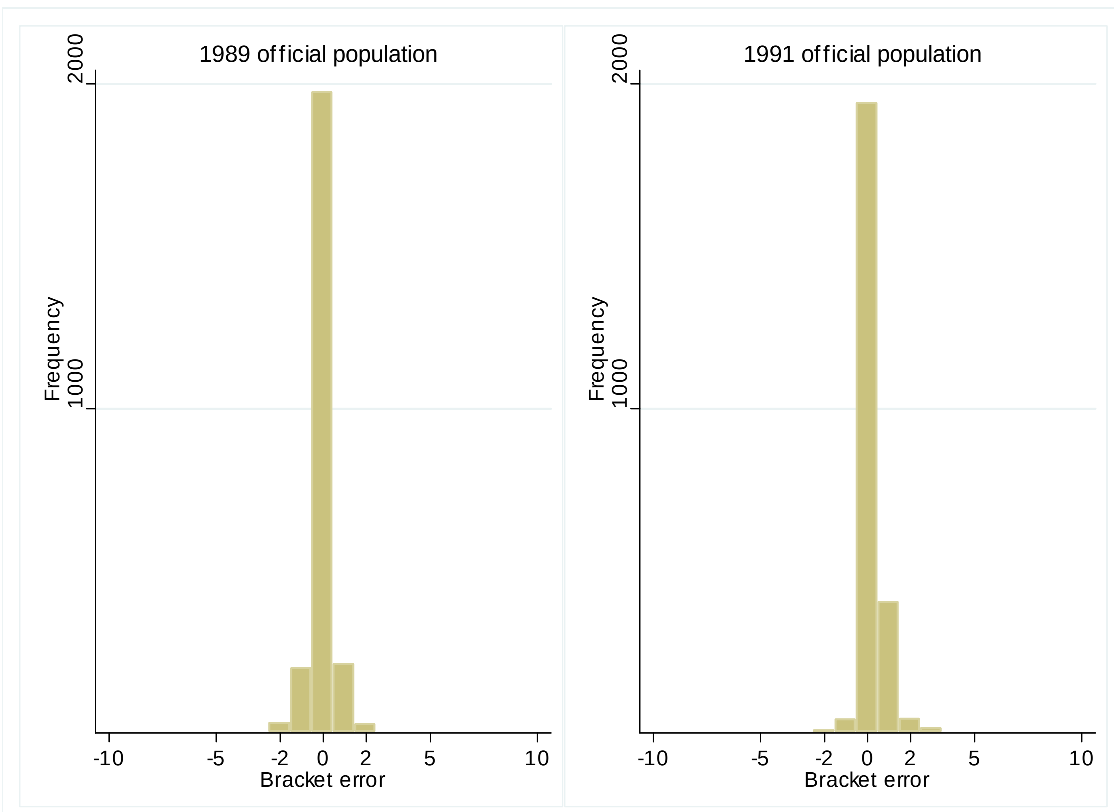


Table V : Summary statistics of covariates

Variable	Obs.	Mean	Std. Dev.	Min	Max
Actual population 1991 census ('000)	4451	24.3	48.8	0.8	846.4
1991 Population forecast error ('000)	4451	2.6	10.1	-108.5	476.9
Bracket error using 1991 official pop.	4451	0.58	0.89	-6	7
Bracket error using 1989 official pop.	4332	0.07	0.90	-7	9
Bunch status	4451	0.42	0.49	0	1
Right-wing mayor 1989-1992	4276	0.53	0.49	0	1
PFL mayor	4276	0.24	0.43	0	1
PDS mayor	4276	0.10	0.30	0	1
PTB mayor	4276	0.07	0.26	0	1
PDC mayor	4276	0.05	0.22	0	1
PL mayor	4276	0.05	0.22	0	1
Electorate 1988 ('000)	4276	18.7	118.6	0.0	6057.6
Mayor's vote share of 1988 electorate	4276	0.37	0.09	0.00	0.82
Interparty fragmentation 1990-1994	3761	0.67	0.14	0.06	0.98
Right-wing vote share	3761	0.37	0.22	0.00	0.98
PRN vote share 1989	4424	0.64	0.15	0.15	0.97

Right-wing mayor includes mayors affiliated with the PFL, PDS, PTB, PDC and PL. Partido Frente Liberal (PFL), Partido Democratico Social (PDS), Partido Trabalhista Brasileiro (PTB), Partido Democrata Cristao (PDC), Partido Liberal (PL), Partido da Reconstrução Nacional (PRN).

Table VI: Political determinants of 1991 official population

Dependent Variable: 1991 official population

Right-wing mayor	0.040	0.921	1.020	1.260	1.125
	(1.693)	(1.193)	(1.261)	(1.361)	(1.385)
Non-right-wing mayor* Mayor's vote share	1.012	0.982	1.084	0.977	1.077
	(2.721)	(1.777)	(1.806)	(1.797)	(1.861)
Right-wing mayor* Mayor's vote share	2.017	1.926	1.765	1.725	1.484
	(1.796)	(1.393)	(1.406)	(1.406)	(1.396)
Non-right-wing mayor* Interparty fragmentation	1.964*	2.540***	2.414***	0.944	0.757
	(1.029)	(0.730)	(0.723)	(0.997)	(0.968)
Right-wing mayor* Interparty fragmentation	1.255	0.553	0.582	-1.359	-1.106
	(0.829)	(0.733)	(0.734)	(1.185)	(1.177)
Non-right-wing mayor* Right-wing vote share			1.272**	-1.579	-2.006
			(0.557)	(1.458)	(1.444)
Right-wing mayor* Right-wing vote share			0.804*	-1.882	-1.869
			(0.478)	(1.226)	(1.212)
Non-right-wing mayor* Right-wing vote share* Interparty fragmentation				4.804*	5.085*
				(2.628)	(2.604)
Right-wing mayor* Right-wing vote share* Interparty fragmentation				4.731**	4.401**
				(2.127)	(2.106)
Predicted 1989 population	Y	Y	Y	Y	Y
Electorate 1988	Y	Y	Y	Y	Y
Actual 1991 population	N	Y	Y	Y	Y
County characteristics	N	N	N	N	Y
Observations	3182	3182	3182	3182	3182
R-squared	0.95	0.97	0.97	0.97	0.97

Notes: See main text for definition of political determinants. Other covariates (not shown) included with actual 1991 population are 1991 actual population bracket classification effects. County characteristics are 1991 income per capita, average years of schooling, poverty rate, gini index and urbanization rate. State fixed effects included in all regressions. Heteroskedasticity-robust standard errors in parentheses.

Table VII: Political determinants of 1991 official bracket classification

Dependent Variable: 1991 official bracket

Right-wing mayor	0.437 (1.896)	1.412 (1.371)	1.595 (1.454)	2.440 (1.579)	2.318 (1.591)
Non-right-wing mayor* Mayor's vote share	0.900 (3.109)	0.866 (2.084)	0.991 (2.116)	0.907 (2.104)	1.004 (2.128)
Right-wing mayor* Mayor's vote share	2.396 (2.000)	1.990 (1.578)	1.826 (1.593)	1.763 (1.593)	1.399 (1.572)
Non-right-wing mayor* Interparty fragmentation	3.071*** (1.137)	3.323*** (0.850)	3.205*** (0.841)	1.901 (1.181)	1.607 (1.154)
Right-wing mayor* Interparty fragmentation	1.483 (0.955)	0.583 (0.864)	0.598 (0.865)	-2.185 (1.407)	-1.977 (1.385)
Non-right-wing mayor* Right-wing vote share			1.324** (0.659)	-1.132 (1.742)	-1.633 (1.704)
Right-wing mayor* Right-wing vote share			0.695 (0.564)	-3.168** (1.490)	-3.267** (1.468)
Non-right-wing mayor* Right-wing vote share* Interparty fragmentation				4.144 (3.105)	4.514 (3.045)
Right-wing mayor* Right-wing vote share* Interparty fragmentation				6.762*** (2.548)	6.582*** (2.518)
Predicted 1989 population	Y	Y	Y	Y	Y
Electorate 1988	Y	Y	Y	Y	Y
Actual 1991 population	N	Y	Y	Y	Y
County characteristics	N	N	N	N	Y
Observations	3182	3182	3182	3182	3182

Notes: Interval regressions (Ordered Probit with known cutpoints from decree 1881/81). See main text for definition of political determinants. Other covariates (not shown) included with actual 1991 population are 1991 actual population bracket classification effects. County characteristics are 1991 income per capita, average years of schooling, poverty rate, gini index and urbanization rate. State fixed effects included in all regressions. Heteroskedasticity-robust standard errors in parentheses.

Table VIII: Political determinants of 1991 bracket error

Dependent Variable: 1991 bracket error

Right-wing mayor	0.307	0.194	0.223	0.385*	0.371*
	(0.193)	(0.172)	(0.181)	(0.210)	(0.211)
Non-right-wing mayor* Mayor's vote share	-0.252	0.124	0.139	0.133	0.152
	(0.249)	(0.253)	(0.257)	(0.256)	(0.259)
Right-wing mayor* Mayor's vote share	-0.047	0.275	0.262	0.254	0.214
	(0.248)	(0.204)	(0.205)	(0.206)	(0.206)
Non-right-wing mayor* Interparty fragmentation	0.669***	0.490***	0.475***	0.345*	0.311*
	(0.143)	(0.118)	(0.118)	(0.177)	(0.174)
Right-wing mayor* Interparty fragmentation	0.135	0.131	0.130	-0.281	-0.250
	(0.145)	(0.111)	(0.111)	(0.184)	(0.183)
Non-right-wing mayor* Right-wing vote share			0.139	-0.106	-0.165
			(0.090)	(0.269)	(0.264)
Right-wing mayor* Right-wing vote share			0.048	-0.520**	-0.524**
			(0.075)	(0.206)	(0.205)
Non-right-wing mayor* Right-wing vote share* Interparty fragmentation				0.417	0.456
				(0.469)	(0.461)
Right-wing mayor* Right-wing vote share* Interparty fragmentation				0.998***	0.956***
				(0.355)	(0.354)
Predicted 1989 population	Y	Y	Y	Y	Y
Electorate 1988	Y	Y	Y	Y	Y
Actual 1991 population	N	Y	Y	Y	Y
County characteristics	N	N	N	N	Y
Observations	3182	3182	3182	3182	3182
R-squared	0.19	0.48	0.49	0.49	0.50

Notes: See main text for definition of political determinants. Bracket error is defined as 5*[c(official population)-c(actual 1991 population)], where c(.) is the step function defined in decree 1881/81. Other covariates (not shown) included with actual 1991 population are 1991 actual population bracket classification effects. County characteristics are 1991 income per capita, average years of schooling, poverty rate, gini index and urbanization rate. State fixed effects included in all regressions. Heteroskedasticity-robust standard errors in parentheses.

Figure 1: 1989 predicted and official populations

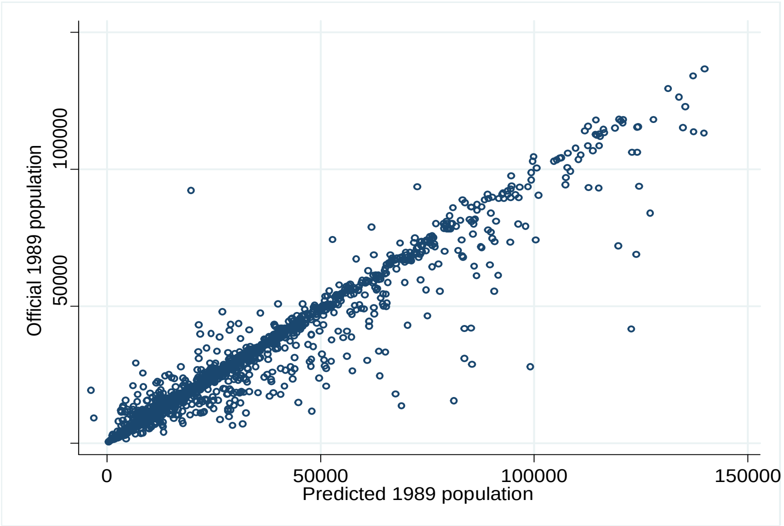


Figure 2: Histogram of 1989 official population

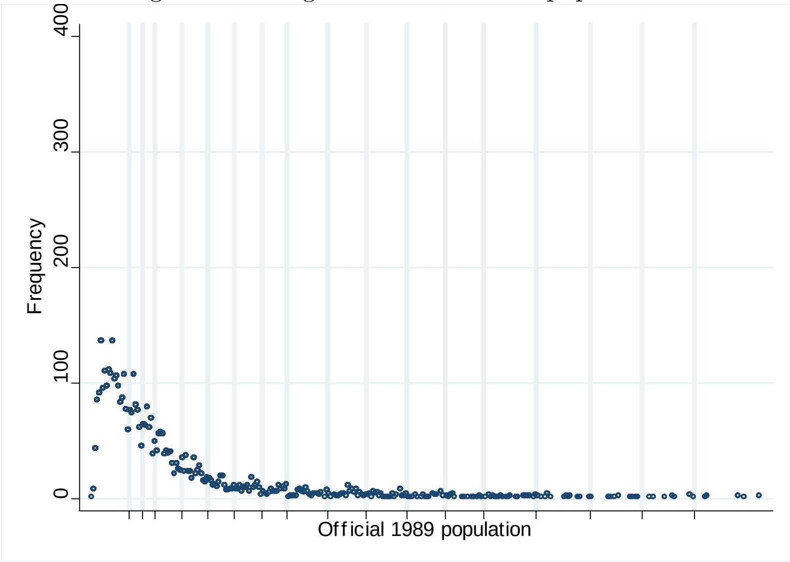


Figure 3: Histogram of 1991 official population

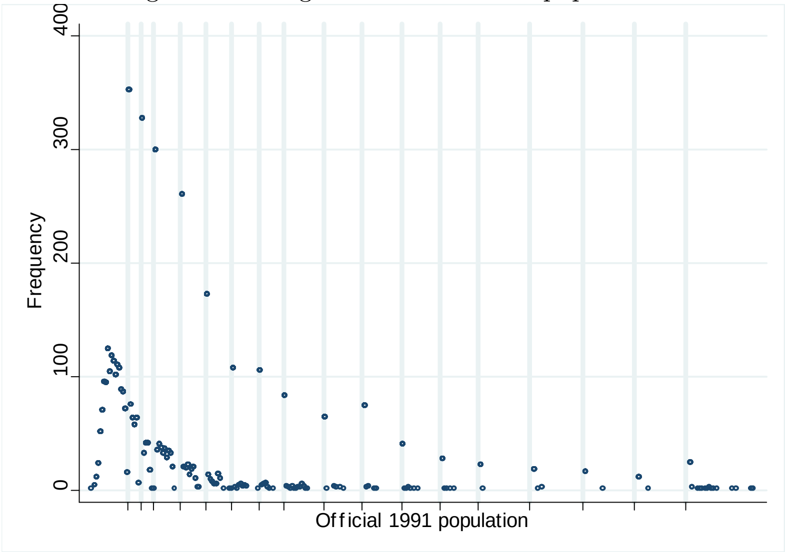
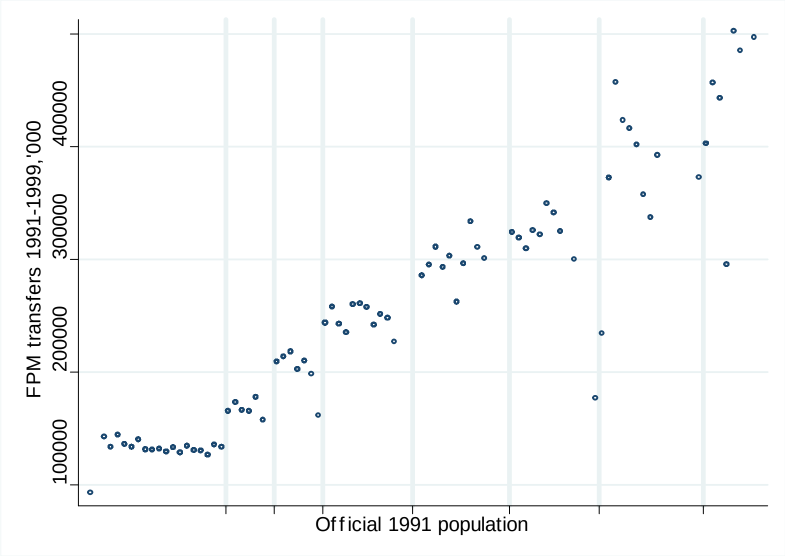


Figure 4: FPM transfers 1991-1999 vs. 1991 official population



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