

Conditional Cash Transfers and Voting Behavior: Redistribution and Clientelism in Developing Democracies*

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Abstract

In this paper I examine the electoral consequences of conditional cash-transfer programs (CCTs) that provide individualized benefits based on objective eligibility criteria. I employ a variety of statistical approaches to both individual and aggregate data from Brazil and find that even in an environment where citizens' electoral behavior is not monitored they nevertheless reward incumbents who provide CCT benefits, even though these benefits are universally distributed without regard for partisan loyalties. Among non-beneficiaries, greater exposure to the program increases the probability of supporting the incumbent, but there is only very scant evidence that those who must pay for the benefits are motivated by this distributive policy to vote against incumbents. The existence of electoral returns to CCTs questions the notion that targeting and monitoring are needed to solve commitment problems between politicians and voters. The absence of strong backlash suggests that these programs might be a politically viable form of redistribution.

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Now, I will strengthen Bolsa Família, with more people, more money and extra help for children who want to learn a profession. Bolsa Família is a right of the Brazilian people.

JOSÉ SERRA
Main opposition candidate in 2010

Do voters in a traditionally clientelistic developing democracy reward incumbents that deliver individual benefits universally and without monitoring voters electoral behavior? In this paper I examine the electoral consequences of Conditional Cash Transfer programs (CCTs) and show that contrary to expectations and assumptions of the clientelism literature, political targeting of benefits and costly monitoring of voters are not necessary to ensure electoral rewards. This finding is the first step in understanding why governments would undertake a major social program and not build much political discretion into its design, nor attempt to monitor the electoral behavior of beneficiaries

CCTs are arguably the fastest spreading new social policy in the developing world. In 1995, the Brazilian cities of Campinas and Brasilia began making cash payments to low income families that formally agreed to a series of conditions, chief among them keeping children in school (Amaral & Ramos 1999, World Bank 2001). Today, CCTs are present in at least 30 countries, and large scale national level programs such as the Mexican Progresa/Oportunidades and the Brazilian Bolsa Escola/Bolsa Família cover millions of poor families, and have become important pillars of these countries' social protection systems (Fiszbein, Schady, Ferreira, Grosh, Kelleher, Olinto & Skoufia 2009). In particular, the Brazilian CCT program is by far the largest in the world, and has had an important effect in the unprecedented reduction in inequality recently observed in the country (Barros, Carvalho, Franco & Mendonca 2010).

CCTs vary with respect to scope, operational details, and ultimate goals, but all policies within this class seek to induce usage of public services by paying poor families with children to do so. Considerable work by economists has scrutinized many different aspects of CCTs,¹ but less work has been carried out on the political determinants and consequences of these policies, and many interesting questions have not yet been formulated, much less answered. In order to understand the conditions under which CCTs might be a politically attractive policy option it is necessary

¹A comprehensive review of the economic literature is available in Fiszbein et al. (2009).

to establish whether these programs increase the electoral prospects of incumbents, whether they can help create long term partisan support, and whether they affect the political behavior of beneficiaries and non-beneficiaries alike.

While CCTs are relatively new, the idea that citizens vote for politicians who provide them with benefits is common to several different strands of literature in political science. Whether benefits take the form of good economic performance (Lewis-Beck & Stegmeier 2000, Samuels 2004, Hibbs Jr. 2008, and others on retrospective voting), or programmatic redistribution that benefits particular groups at the expense of others (Meltzer & Richard 1981, Boix 2003, Acemoglu & Robinson 2006), a vast literature assumes that it is rational for voters to reward those who make them better off. Monitoring of voter behavior is not typically seen by this literature as necessary to obtaining electoral rewards. Its main concern, instead, is that citizens who have to pay for the resources that makes others better off will vote against politicians who initiate such policies, and possibly even support coups against them (Boix 2003, Acemoglu & Robinson 2006).

Another old and respected tradition in the discipline, however, focuses on the ubiquitousness of clientelistic exchanges between politicians and voters (Scott 1969, and many since then). Clientelism plays a role in all political systems, but research has shown it is more prevalent in poorer countries and for poorer citizens in all countries (Diaz-Cayeros, Magaloni & Estévez Forthcoming, Calvo & Murillo 2004). Politicians choose recipients based on whether they vote, promise to vote, or engage in other type of political activities on behalf of the patron. This process is carried out by partisan networks of political operators in close contact with voters. Maintaining these networks is costly, as they divert resources away from those that need it most in favor of those in the party network, and exclude some beneficiaries who might benefit from the same policies if distribution were universalist. Both voters and politicians could do better in the absence of clientelism and both sides face incentives to renege on the exchange. Despite all these difficulties, clientelism lives on.

In contrast to work on programmatic redistribution, the clientelism literature does not worry much about those paying for the benefits and focuses instead on how the commitment problems in the exchange are solved. Several ideas have been advanced to explain why clientelism is so pervasive, including risk aversion and strong discounting of the future by poor voters (Kitchelt 2000), and difficulties by voters to evaluate the provision of programmatic public goods (Geddes 1994). The explanation that has received most attention in recent times is that clientelistic delivery allows politicians to monitor whether the voters who receive benefits really fulfill their side of the bargain

on election day (Stokes 2005). Such “perverse accountability” in large scale democracies requires politicians to maintain networks of political activists on the ground to gather information about voters. These networks help build long-term exchange relationships with voters, and it is the expectation of repeated interactions over time that helps to sustain the clientelistic exchange (Stokes 2007).

Regardless of the merits of these arguments, there is a clear contradiction between the literature on clientelism and assumptions in the literature on programmatic redistribution with respect to selective targeting of and subsequent monitoring of loyalists. In this paper, I use data from CCT programs in Brazil to evaluate the necessity of political targeting and voter monitoring to ensure incumbent support as well as the extent to which redistribution backlash on the part of those that pay the bill occurs. CCTs are an interesting policy to analyze because they deliver benefits that are at the same time individualized but universalistic, in that they targeted accordingly to objective criteria. These characteristics are typically associated with clientelism and programmatic redistribution respectively, placing CCTs in a theoretical middle ground that allows us to eliminate the difficulties that voters might face to evaluate public good provision. Moreover, Brazil is a particularly good instance in which to test electoral effects of these types of benefits because, as a country with a reputation for clientelistic politics and weak party loyalties (Mainwaring 1999, Ames 2001), it should be the most likely case for the need to monitor voters. If monitoring is not necessary in Brazil, it is probably not necessary elsewhere.

I employ a variety of statistical approaches and both individual and aggregate data to show that citizens reward the politicians who provide CCT benefits. Furthermore, different parties can target and benefit in turn from support of the same socioeconomic groups. I also find very scant evidence that those who must pay for the benefits are strongly motivated by this to vote against politicians who initiated and maintained these policies, though this conclusion is still tentative.

If monitoring is not needed, as this paper suggests, why does clientelism persist as a major aspect of politics in so many places? If universalistic distribution of individualized benefits dominates traditional clientelism, why is it not employed by every incumbent? While this paper does not go as far as completely answering these questions, I discuss some plausible ideas suggesting that clientelism persists because it serves the interests of the local operators of the party networks (as opposed to national level politicians and voters).

The paper proceeds as follows. I start by examining how CCTs can help illuminate important theoretical issues raised by the literature on clientelism and redistribution. Section 2 provides

political context and information on Brazil's experience with CCTs, and in Section 3, I identify electoral effects of Brazil's CCT programs in the 2002, 2006, and 2010 presidential elections. In Sections 4 and 5, I examine different mechanisms by which this effect can be transmitted, and empirically identify indirect and direct paths between voting behavior and CCT coverage. Section 6 concludes with an evaluation of the results, a reconsideration of the role of clientelism, and with directions for future research.

1 Conditional Cash-Transfers, Clientelism and Redistribution

Analyses of politics, parties, and elections in poor and unequal democracies often focus on clientelism and programmatic redistribution. Stokes (2007), however, is one of the few to draw an explicit distinction between what constitutes the former, and what precisely is the latter. Redistributive policies transfer resources from one objectively defined *category* of citizens to another, but within categories benefits are not excludable on any grounds. Clientelism, on the other hand, implies delivery of benefits targeted to specific individuals contingent on their voting behavior. A While these two strands of literature intersect at many points, there are some intriguing disconnections between the two when it comes to their political (typically voting) behavior of beneficiaries and to the reactions of those on the paying side.

Clientelism is the provision of particularist benefits (goods or services) by politicians to voters in *exchange* for political support. The exchange component is its most defining characteristic (Stokes 2005, Kitschelt & Wilkinson 2007), and it relies on the possibility of violating the secrecy of the ballot box — or at least on the belief that it is possible to do so (Weiss-Shapiro 2008). The types of excludable goods delivered through clientelistic exchanges can vary dramatically. They can include promises or actual delivery, as well as threats to suspend benefits or otherwise punish voters. The benefits themselves range from small one-time gifts around election time, to special favors delivered in times of need, to state controlled goods delivered on political grounds when the patron is in office, a subclass of exchange referred to as patronage.

From the politicians perspective, monitoring voters should help reduce the risk that voters will take the benefits but not deliver the votes, even if it is highly distortive and socially wasteful. While non-voters can free-ride on the provision of programmatic redistribution, the threat of exclusion should raise the electoral returns to clientelism relative to other forms of delivery of benefits (Robinson & Verdier 2002, Diaz-Cayeros, Magaloni & Estévez Forthcoming). Excludability alone,

however, does not ensure recipient compliance. There are incentives for both sides to renege on the exchange, and it is only because interactions between patrons and clients are repeated in time that patrons can hold clients accountable for their voting actions (Stokes 2007).² Repeated interactions require networks of local political activists to identify, cultivate, monitor and reward voters, so partisan networks are an essential feature of clientelism.

The link between programmatic redistribution and voting behavior, in contrast, is typically considered much less problematic. Redistributive policies are usually depicted in abstract terms as policy decisions regarding levels of taxation and expenditure. The exact policy instruments are not usually described, and the taxation framework is used as stylized depiction of what are presumably the net effects of a bundle of policies that redistribute resources in society. The most basic result is that the median voter demands some type of redistribution (Meltzer & Richard 1981), and full after-tax convergence of incomes is only prevented by losses incurred in the redistribution process (agency costs, corruption). Though the expectation that greater inequality would lead to greater pressure for redistribution is clear, scant empirical support has been found to support it. Much work, in fact, suggests that electoral rules and geographic dispersion of voters (Jusko 2010), backlash from those footing the bill (Boix 2003, Acemoglu & Robinson 2006), policy implementation difficulties (Przeworski 2010), voter heterogeneity within the same income categories (Mares 2003),³ and several other factors can prevent redistribution from happening. Still, all these assume that voters that benefit or stand to benefit from redistribution will vote for candidates who promise or effectively carry out redistributive policies. No selective targeting of individuals, no monitoring, and consequently no partisan networks are necessary.

Why is enforcement necessary in a clientelistic exchange but not necessary when it comes to redistributive policies? In the redistributive politics literature, voters' electoral preference is itself shaped by how well the voter is doing in economic terms, which is a function of the net effects of redistributive policies on each voter.⁴ The clientelism literature, with its focus on the commitment problem of the exchange, follows the assumption that voters would rather collect the

²There is some additional evidence that the success of the clientelistic exchange cannot be taken for granted. Kitschelt (2010) finds that clientelism is not as effective as it is pervasive, and Brusco, Nazareno & Stokes (2004) find that a majority of people in Argentina do not think those that receive a benefit should feel to return the favor.

³Income, it is argued, is not the only relevant political dimension in which to categorize voters. Redistributive policies, in the real world, single out categories of people within the same income brackets to benefit and pay for redistribution, creating a much more complex political dynamic.

⁴Clientelism is sometimes built into the redistribution literature as a leaky bucket, or informational advantages that make it easier and more effective to target some groups relative than others, and in this sense implies that certain groups will be targeted because of information or logistical advantages from the network (Cox & McCubbins 1986, Dixit & Londregan 1996). This implies that the same "gift" might have a larger effect on some voters than on others.

benefit but behave in some other way, either voting for another candidate or not voting at all. In this case, the utility from the benefit and the voter's electoral preference are separate and whatever is distributed is not enough, on its own, to sway electoral behavior. Hence, the two approaches imply fundamentally different conceptions of how voters' preferences are formed.

The insufficiency of the clientelistic benefit, on its own, is compatible with the notion that patrons have an interest in keeping clients poor, so that their dependency will keep them in clientelistic exchange. It is also consistent with the lack of reaction from those on the paying side of the clientelistic exchange. While the redistributive politics literature is very much concerned and aware of the fact that those paying for redistribution might revolt, leading to democratic collapse (Boix 2003, Acemoglu & Robinson 2006) or false electoral promises of redistribution (Campello forthcoming), this concern is all but absent in the literature about clientelism.

A similar distinction is frequently made between the provision of public and private goods (Mesquita, Smith, Siverson & Morrow 2003, Diaz-Cayeros, Magaloni & Estévez Forthcoming), but it does not map neatly into the clientelism programmatic redistribution dichotomy. While clientelism entails the provision of private goods to voters, its defining characteristic is really the exchange, and not the type of the good. On the other hand, while public goods are always delivered through programmatic redistribution, programmatic redistribution can also entail the provision of private and/or club goods. In fact, despite our tendency to equate redistributive policies with the provision of public goods, most models of redistribution and most of the literature on redistribution in advanced societies focuses on welfare benefits that are delivered discretely to individuals based on universal criteria.

A corollary of this language imprecision is that most arguments about clientelism contrast clientelistic benefits to public goods, but not to *universalist* distribution of *private* benefits. CCTs, therefore, provide the opportunity to evaluate voter behavior in the presence of private benefits and in the absence of monitoring. On one hand, CCTs are typically implemented in contexts that are conducive to clientelism and tend to benefit the same people that would be the natural targets of political exchanges. Well implemented CCTs, however, lack the excludability and the individual targeting typically associated with private goods delivered through clientelism. CCTs, at least as implemented in Brazil,⁵ benefit a whole class of objectively defined citizens, and therefore qualify as programmatic redistribution. On the other, CCTs are different from the provision of public goods typically associated with programmatic redistribution in that the benefit is divisible

⁵The level of political discretion involved in CCTs *can* vary considerably, but there is evidence that Brazil's is pretty well administered (Fiszbein et al. 2009) and has improved considerably over time (Lindert & Vicensini 2010).

and directed to particular households.⁶ Well targeted CCTs blanket *all* members of the class of objectively defined beneficiaries with individual benefits.

This middle ground occupied by CCT benefits is particularly useful from the analytic point of view.⁷ Among many arguments as to why clientelism is attractive, some sustain that poor voters have short time horizons and thus sincerely prefer tangible benefits over policy promises that might deliver much more in the future (Kitchelt 2000). Others claim that voters, especially poor and less educated ones, can monitor the delivery of private, tangible goods, but not whether politicians who promise beneficial policies really introduce them and whether, if introduced, the policies deliver expected benefits (Geddes 1994). Brazilian CCTs allow us to ask how voters behave in the presence of a tangible, identifiable, and quantifiable benefit — that resemble what is delivered by clientelism — but in the absence of any requirement to vote in a certain way, or to be part of specific networks.

The empirical portion of the paper begins by identifying a discernible electoral effect of CCTs. I then investigate the channels by which these programs lead to increased electoral support, examining first whether CCTs function merely as an economic stimulus, in which case CCTs would not differ from any other expansionary fiscal policy, and then whether the aggregate CCT effects are driven by increased support by beneficiaries or a reaction from non-beneficiaries.

2 CCTs in Brazil

Lula da Silva (PT) was reelected president of Brazil in October 2006, propelled to victory by massive support in the poorer places of the country, in the face of a flabbergasted opposition. Only a few months earlier, Lula's government was stuck in a mire of ever expanding corruption allegations, and the opposition had comfortably settled with a strategy of slowly and steadily "bleeding the government" on the daily news, and waiting for the polls to finish him off. Surprisingly, though, Lula's popularity began rising in the run-up to the elections, and by the time it became clear that corruption allegations were not going to defeat him, the opposition was caught without an alternative plan.

When all was said and done, the 2006 elections produced an important shift in voting patterns

⁶Diaz-Cayeros, Magaloni & Estévez (Forthcoming) work with a third intermediate category of local public goods, but this category would not include CCT benefits.

⁷CCT are not the only types of policies that distribute particular benefits using universal criteria, the the conceptual implications of these characteristics have not generally been considered in the debate of the electoral consequences of the provisions of private and public goods.

in the country. Lula's support in the least developed areas of the country stood in sharp contrast with all four previous presidential elections — three defeats and a victory — when he had received more votes in the more well-off municipalities.⁸ In the scramble to find immediate explanations for this electoral realignment, attention turned to the *Bolsa Familia Program* (BFP), a massive conditional cash-transfer program implemented and maintained by the federal government. The BFP is the main component of a larger umbrella program called *Fome Zero* (Zero Hunger) and reaches families with monthly income of up to R\$ 120 (just over US\$ 60). Most of its benefits depend on the number of children in the household, and are conditional on them attending school as well on keeping immunization records and maintaining a schedule of visits to the doctor. Extremely poor families also receive a flat benefit on top of the per child one, in which case total benefits added up to just under R\$ 100 per family. Municipalities enroll potential beneficiaries and enter their information into a national database, which is then centrally purged. The Social Development Ministry (SDM) administers the program, sets targets, determines eligibility, monitors and rewards municipalities that do a good job managing the program, and, more importantly, pays beneficiaries directly through the banking system, though recipients are not required to have bank accounts. The program is audited as part of the executive branch's randomized auditing of federal funds transferred to municipalities, which is carried out by the Comptroller General (Ferraz & Finan 2008), and also by the constitutionally independent auditing authority (TCU).

By October 2006, the BFP covered just over 20% of the entire population of the country — or more than 40 million people in 11 million families. It has since then expanded to 13 million families. Almost 60% of survey respondents at the time of the 2006 election time reported knowing somebody who received the benefit (Vox Populi 2006). In another survey, also carried out close to the 2006 elections, 34% of respondents chose “the fight against poverty” as the most successful policy during Lula's first term out of 15 possible policy areas, with “none of the above” coming a distant second with 9%.⁹ An astonishing 67% approved of the government's policies to “fight hunger and poverty,” and though support dropped with income, just under 60% of those in the highest income category approved of these policies — the highest approval rating for high income respondents among all policies polled (Ibope 2006).

With such notoriety, it is evident that the program's electoral effects have already been noted by the specialized literature. Hunter & Power (2007) published what was probably the first academic

⁸Except for the first election, in 1989, Lula's main contender was always a member of the PSDB. Lula lost twice to Fernando Henrique Cardoso (1994 and 1998), and defeated José Serra in 2002 and Geraldo Alckmin in 2006.

⁹Another 15% of respondents listed “fight against poverty” as the second most successful area.

piece linking the BFP to the 2006 electoral results, arguing that the program had been one of the pillars of Lula's victory. This was soon followed by Nicolau & Peixoto (2007) and Zucco Jr. (2008) who did more data intensive studies using data from Brazil's 5500 municipalities, finding more evidence in support for a BFP electoral effect. Since then, others have dealt with different political aspects of the issue and mostly agreed with the BFP's important role (Soares & Terron 2008, Licio, Castro & Rennó 2009, Fenwick 2009, Canêdo-Pinheiro 2009), though some argue that a stronger economy was a more relevant electoral factor than the BFP in shaping the electoral outcomes (Shikida, Monasterio, Araújo Jr, Carraro & Damé 2009).

Despite this newfound attention, CCTs have existed in Brazil for more than a decade. The origins of the BFP program lay in the preceding Cardoso administration, but the potential impacts of CCTs on earlier presidential elections have not been, to this day, empirically assessed.¹⁰ When Lula took office in early 2003, the Federal Government already administered several different programs that shared characteristics with the BFP. In 1997, inspired by the experience of local governments throughout the country, Cardoso initiated a Federal program to partially fund poor municipalities' social programs that imposed education related conditionalities. This initiative only took off in 1999 (Amaral & Ramos 1999), and was restructured and expanded in 2001 to become Bolsa Escola.¹¹ Less than an year later, the government started Bolsa Alimentação, a cash-transfer program that imposed health related conditionalities.¹² These programs, it should be noted, were administered independently by different ministries.¹³

Soon after being elected in 2002, the Lula government added a new program called "Cartão Alimentação," but in late 2003 it decided to unify all programs, under the newly created SDM and, more importantly, to formulate a single database of families called "Cadastro Único" which would be used to make CCT payouts to the new unified program, and serve as the platform for

¹⁰de Janvry, Finan & Sadoulet (2008) studied the effects of the programs on prospects of mayors in early adopting municipalities, but no analysis of national level elections was done.

¹¹The original 1997 program was instituted by Law 9533. The subsequent "Programa Nacional de Renda Mínima Vinculada à Educação," or Bolsa Escola, was created by Law 10219. Administered by the Ministry of Education, it was targeted to poor families with children between 6 and 15 years old, enrolled in school and required meeting attendance thresholds.

¹²The Bolsa Alimentação Program was created by Provisional Measure (PM) 2206-1, also in 2001, and regulated by Decree 3934. Administered by the Ministry of Health it was targeted at poor households with expectant mothers and/or children below 7 years of age and required that beneficiaries fulfill requirements such as preventive health care, vaccinations, and regular doctor visits.

¹³The Auxílio Gás, created by PM 18-2001 and administered by the Ministry of Mines and Energy was another program that is sometimes bundled together with the previous two. However, it was explicitly formulated as targeted substitute to a general cooking gas subsidy that was being phased out, it did not impose conditionalities, paid very small bi-monthly benefits and, by design, was targeted at families already covered by other programs. For these reasons, I excluded it from the analysis. Results presented in this paper do not change dramatically if Auxílio Gás is included.

planning and implementation of other targeted policies.¹⁴ The old programs were gradually phased out as the BFP expanded to 3.6 million families by the end of 2003, to 6.5 million in 2004, 8.9 million in 2005, 11 million families on the eve of the 2006 elections, and 13 million by the 2010 elections. While the BFP accounted for only a fraction of CCT beneficiaries at its inception, by 2006 almost all CCT benefits paid by the Federal Government were already formally part of this single program.

In hindsight, it is not obvious why so scant attention was paid to CCTs in 2002 as compared to 2006 and 2010. Despite being smaller, the scope of 2002 programs were far from negligible (Figure 1), and there is no *a priori* reason to rule out that they would have affected voting behavior. The most obvious reason for this lack of attention lies in the combination of the ultimate defeat of the incumbent government's candidate, and the lack of coordination and visibility of the CCT initiatives under one strong brand, as was the case in 2006.

Assessing CCTs' electoral effects might seem like a straightforward empirical issue, but there were no randomized pilot programs, programs have been generally very well targeted making it hard to find control groups who do not receive the benefit — at least when using aggregate electoral data, and there are few adequate surveys linking participation in the program with electoral behavior. Confounding factors make the task far from trivial, as economic growth was particularly pro-poor in the years preceding 2006 (Shikida et al. 2009), and there is evidence that a peculiar and significant incumbency effect was also at play (Zucco Jr. 2008, Canêdo-Pinheiro 2009).¹⁵ Making use of the fact that CCTs have now spanned three presidential elections allows us to extend any analysis of the Brazilian experience beyond that of one program and one election, and attempt to draw more general lessons about the electoral implications of such programs.

3 Effects of CCTs on Voting Behavior

To examine the connection between CCTs and voting behavior, it is first necessary to define and operationalize CCT coverage. All CCT programs in Brazil have relied, at least partially, on geographic proxying to determine eligibility, and have been implemented at the municipal level.

¹⁴This was something that had been contemplated still under Cardoso — under the *Projeto Alvorada* — but was never effectively carried out (World Bank 2001).

¹⁵Incumbent party candidates *always* get a proportionally higher share of the vote in poorer places while the reverse applies to the main party in opposition. This does not mean that the incumbent party candidate always wins elections in poorer places, as his average level of support can vary for many different reasons. It does suggest, however, that incumbents have a built-in advantage in reaching these voters and that Lula's vote getting patterns would probably have shifted between 2002 and 2006 even if no CCT existed.

Hence, I operationalize CCT coverage both in terms of scope and of expenditures in each of the country's municipalities.¹⁶

Scope was defined simply as the share of families in each municipality covered by CCT programs. While this is probably the best indicator of coverage, in the early days of CCTs in Brazil, different programs had separate lists of beneficiaries and it is impossible to arrive at a precise combined coverage figure in each municipality. This is specially a problem in 2002. Though I simply added the number of families in each program, this combined figure overestimates the total number of beneficiaries that year. At that point in time, *Bolsa Alimentação* reached close to one million families and *Bolsa Escola* just over five million, but some families were covered by both programs. By 2006, the overwhelming majority of CCT beneficiaries were already in the BFP proper, and only very marginal "left-over" coverage in *Bolsa Escola*, *Bolsa Alimentação* and *Cartão Alimentação*.¹⁷

An alternative conceptualization of CCT coverage focuses on expenditures — defined here as total CCT spending in each municipality by year for each resident, measured in hundreds of Reais — that do not require knowing exactly how many families received the benefits in each municipality.¹⁸ Nationwide, expenditures per capita (adjusted for inflation) were more than three times larger in 2006 than in 2002, and 50% larger in 2010 than in 2006.¹⁹ The number of families covered increased 80% between 2002 and 2006, and an additional 18% between 2006 and 2010.

While both scope and expenditures in existing programs were far from negligible in 2002,²⁰ CCTs were a relatively low-profile policy at the time, and did not receive much media coverage²¹ nor academic scrutiny prior to the 2006 elections. Given this relative lack of interest, it might seem surprising that I find a positive association between program coverage and expenditures and incumbent party candidate electoral performance in 2002.

The first set of estimates reported in Table 1²² were obtained by simple OLS regressions for incumbent party candidate vote share on CCT coverage, including a host of municipal level controls

¹⁶The median population of Brazilian municipalities is just over ten thousand inhabitants. Later in the text I discuss and deal with some of the limitations of aggregate data analysis, and present and analyze individual level data.

¹⁷There were also still beneficiaries of the Auxílio Gas, but as mentioned in footnote 13, the program was excluded from this analysis.

¹⁸The two operationalizations are highly correlated in each election.

¹⁹All figures used in this that compare monetary values across years are adjusted for inflation, and using the IGP-M index, and figures are reported at 2010 values.

²⁰Evidently, neither of these indicators deals with the issue of whether the benefits were effectively making their way to those actually eligible, but there is evidence that the program is quite well targeted (Fiszbein et al. 2009).

²¹Lindert & Vicensini (2010), for instance, report just around 200 news articles in six selected outlets in the quarter of the 2002 election while there were more than 800 such articles in the same outlets in the quarter of the 2006 elections, and before 2006 little thought was devoted to these programs electoral implications.

²²Results for the two different operationalizations of CCT coverage should not be directly compared, as the concepts measured and units employed are quite different.

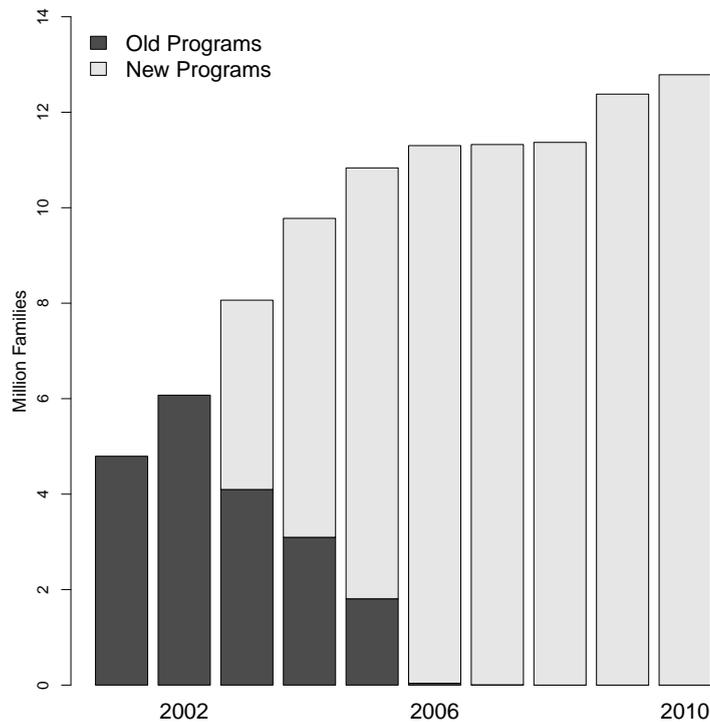


Figure 1: Families Covered by CCT Programs

and state fixed effects.²³

Differently than what happened in most other countries where CCTs are present, in Brazil there were never any randomized pilots that directed benefits to a subset of municipalities or subgroup of the population.²⁴ While regression analysis is an attempt to hold other factors constant, we cannot really be sure to have adequately dealt with the non-random assignment of benefits.²⁵ If CCTs were perfectly targeted, any inference regarding their effects would be driven entirely by the linearity assumed by OLS. If, for instance, all poor municipalities had high coverage and all rich municipalities had low coverage there would be no contrast between similar municipalities with different coverage levels, and hence a “CCT effect” would not be identifiable. Clearly, it is quite plausible that there might exist some common cause to poverty (and consequently prevalence of cash-transfers) and incumbent vote share. To identify a CCT electoral effect, it is necessary to contrast municipalities that are similar with respect to levels of development, size, political

²³The dependent variable is logically bound between 0 and 1. Though some type of transformation could be advised to linearize the variable, the bulk of the municipalities are far from the bounds so results are not substantively affected. In this scenario, ease of interpretation justified maintaining the variable in its original metric.

²⁴See de La O (2010a) for insightful study of the electoral effects of CCT in a setting where randomization was employed.

²⁵Empirically, expenditures per capita correlates strongly with level of development in all years, though the correlation is slightly weaker in 2002.

Table 1: Cash-Transfers and Incumbent Candidate Vote-Share

(a) OLS Estimates						
	Serra 02	Lula 06	Dilma 10	Serra 02	Lula 06	Dilma 10
Expenditures (R\$ 100.00)	0.168	0.088	0.044			
SE	0.019	0.005	0.004			
Scope (Share covered)				0.199	0.196	0.159
SE				0.021	0.013	0.013
R2	0.637	0.797	0.848	0.638	0.794	0.848
N	5486	5486	5486	5486	5486	5486

(b) Generalized Propensity Score Matching Estimates						
	Serra 02	Lula 06	Dilma 10	Serra 02	Lula 06	Dilma 10
Expenditures (R\$ 100.00)	0.154	0.139	0.025			
SE	0.035	0.009	0.007			
Scope (Share covered)				0.123	0.189	0.091
SE				0.041	0.030	0.024

Notes: Dependent variable is the incumbent party candidate vote share in the first round of each presidential elections, measure at the municipal level. CCT expenditures per head are measured in R\$ 100 inflation-adjusted to 2010 levels. Scope is measured as share of total families in the municipality covered by CCTs. Top panel shows OLS estimates, control variables and state fixed effects were included in the regression but not reported for simplicity. Lower panel shows the average treatment effect of CCT coverage, estimated as recommended by Imai & van Dyk (2004) through generalized propensity score matching. Predicted treatment regressions are reported in the Appendix and results in panel b were obtained by dividing the data into ten strata as discussed in the text and shown in Figure 2.

background and other covariates but that varied with respect to CCT coverage.

Fortunately — from an analysts’ perspective, that is — CCT targeting is good but is not perfect. There is some variation in coverage across similar municipalities, and this provides leverage to identify a CCT electoral effect at the municipal level. One way to explore this leverage is to approach CCT coverage as if it were a continuous “treatment,” and match municipalities on a vector of other relevant variables to gauge the effect of cash-transfers by comparing municipalities only with otherwise similar counterparts.²⁶

The matching procedure for continuous treatments is slightly more involved than for categorical treatments. It first requires the computation of propensity scores, or the treatment levels predicted by other observed pre-treatment covariates. Following Imai & van Dyk (2004), I then partition the data into subsets according to their propensity scores.²⁷ For each subset, the treatment effect is calculated by a simple regression of incumbent vote-share on the treatment variable (CCT coverage), controlling for the propensity score itself. This procedure only assumes homogeneity of treatment effects within a small subset of similar municipalities and not over the whole set of municipalities, as is the case when estimating a single OLS regression on the sample as a whole. Only municipalities with similar predicted treatments are compared to each other and the inclusion of the propensity score as a control further ensures that the treatment effect is only estimated through the contrast of otherwise very similar observations. I obtain the average treatment effect for the whole sample by weighted-averaging across the treatment effects in each strata of the data. However, I can — and do — analyze the effects in each subset as well.

Granted, the possibility of omitted variable bias is only shifted from the main regression to the predicted treatment regression, but there are theoretical and empirical reasons to believe these preliminary regressions to be very adequate. In the regression for CCT scope I have included the government’s official target of coverage for each municipality. This target was computed once, based on the 2006 national household survey (PNAD). Even though the target did not exist in 2002, it provides a reasonably neutral assessment of need in each municipality because it is based on social indicators that move very slowly over time.²⁸

Besides the target itself, the most powerful predictor of treatment is the HDI-M, but I also

²⁶As reported in the Appendix, there are a handful of municipalities that had zero coverage in 2002, and Serra performed worse in these municipalities than in CCT covered municipalities otherwise similar.

²⁷Imai & van Dyk (2004) actually show that partitioning the data into five subsets is enough, but given the large number of observations I conservatively split the data into more groups. Results hardly change using different number of subsets or partitioning the data by propensity score quantiles or in fixed-width strata.

²⁸See Fried (2010) for more details on who targets are set, and on understanding why coverage might deviate from these targets.

include several other socio-economic controls and political variables. Coverage is not contingent on whether the mayor and governors belonged to the main opposition or government party respectively, which lends confidence that the CCT effects ultimately observed are not driven by pro-government politicians' clientelistic appropriation of public policy. Besides other socio-demographic controls, I also added state fixed-effects to account for further variation not captured by the substantive variables. These dummies, it should be noted, only marginally improve the fit of the regressions, which suggests that the substantive variables are doing most of the work. When all is said and done, the R^2 s of the six treatment regressions reported in the Appendix vary between 0.8 and 0.92, and exhibit equally good fit across the range of the treatment.

The estimated average treatment effects are within the same order of magnitude of those estimated by the simple OLS analysis. For expenditures, effects have declined considerably in 2010. A R\$ 100 increase in yearly per capita coverage lead to as much as a 15% percentage point increase in vote share in 2006, but this has fallen to only 3% in 2010. Though in practice variation in expenditure as large as this would not be observed, the results are still substantively meaningful and statistically significant even when more plausible changes in coverage are considered. Results for scope vary less across different years, and depict a larger effect in 2006, when each additional percentage point in the share of families covered by CCTs increased the incumbent party candidate's vote share by almost a fifth of a percentage point. This has fallen to about one tenth in 2010.

The declining effects between 2006 and 2010 remains to be explained, but it is probably partially driven by a declining returns, specially when we consider that the declining effect is much less pronounced for scope than for spending (whose levels increased considerably more). Another possibility is that the rate of change in both expenditures and coverage, and not only absolute levels, are also driving the results.²⁹ Stronger results for 2006 could also be due to the fact that it was the only election in the set in which the incumbent president himself was running for reelection.

These caveats aside, the punchline is that CCTs have had a substantively meaningful electoral effect in all the last three presidential elections, including the 2002 when CCTs went unnoticed. This result remains robust even when effects are estimated across municipalities that are otherwise similar, irrespective of party or personality of the incumbent candidate.

Figure 2 reports additional details of the results. The dark gray areas indicate the 90% confidence intervals for the CCT effect estimates for each strata. While results are not identical across

²⁹This is one avenue for future work, not yet explored.

all subsets of the data, the average effects reported in Table 1 are not driven by only one or two subgroups of the data. The only clearly outlying segment across all years is leftmost stratum in 2010. This represents generally more developed municipalities, where predicted treatment is low. It is possible that the presence of a third leftist candidate might have affected these results, but more investigation on the subject is warranted.

Several features of the results presented above lend us confidence that the association between CCT and incumbent vote share is indeed a causal effect. OLS estimates are comparable to those obtained with propensity score matching, suggesting that non-random assignment of CCT might not be severely biasing results. Treatment dispensation can be predicted extremely well, and the matching procedure is the best attempt available to assure that the CCT effect is being identified off variations on coverage on otherwise very similar municipalities, making it extremely unlikely that the result can be attributable to other variables.

Moreover, the predicted treatment regressions and OLS results (Appendix) show that political variables are not determinant for CCT coverage and that incumbents do not perform better in municipalities and states held with mayors and governors from their parties. Both of these results are the opposite of what one would expect if CCTs were simply being appropriated by clientelistic machines, and corroborate soft evidence that pointing to the lack of monitoring of voter behavior and political targeting of benefits.

But if CCTs are not part of a clientelistic exchange, and voter behavior is not enforced, how exactly do CCTs increase support of incumbent party candidates? Do CCTs simply act as an economic stimulus that affects both beneficiaries and non-beneficiaries alike, basically serving as public good? Are the estimated effects just described a combination of a sharp increase in the support by those that receive the benefit offset by a decline in support by those that do not? In this next sections I tackle these questions, and attempt to shed some light on the causal mechanisms at play.

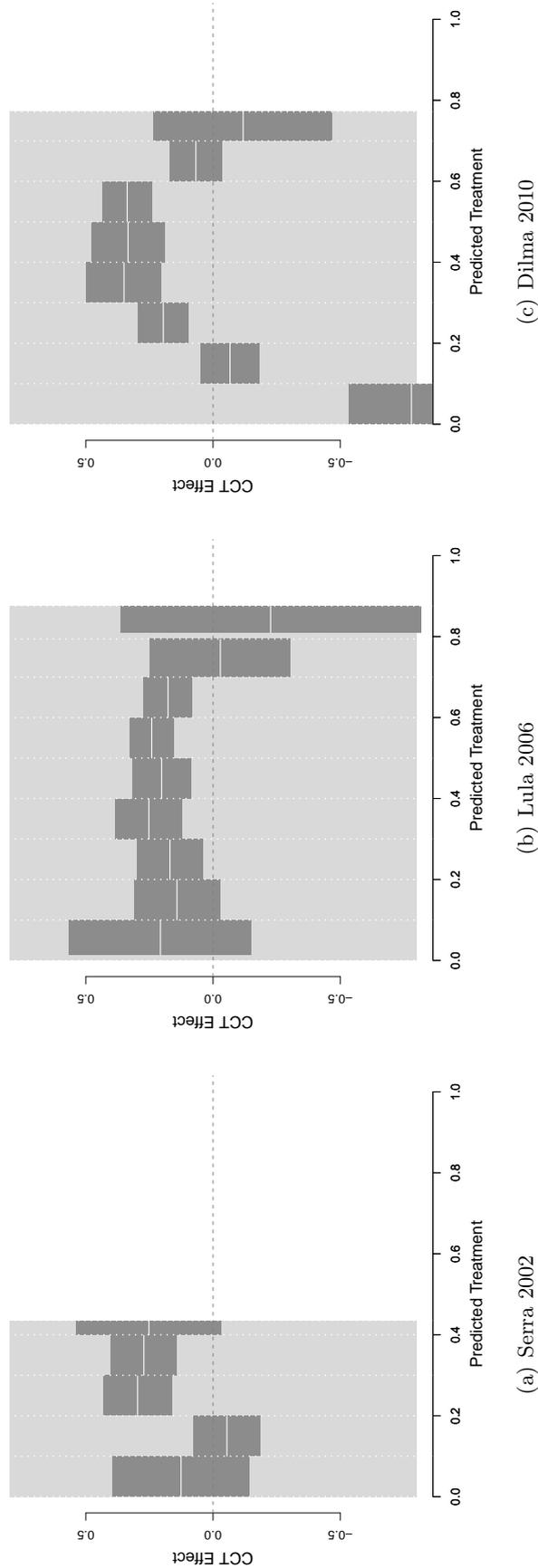


Figure 2: CCT Scope and Vote for Incumbent Party Candidate by Strata (2002–2010)

Notes: Horizontal axis is the propensity score, which is the predicted scope of CCTs given a host of covariates (See Appendix and text for details). Vertical axis is the estimated electoral effect of CCTs on incumbent party candidate's vote in the first round of elections, with the respective 95% confidence interval. The different strata in which the data were partitioned are indicated by the vertical dashed lines, and were defined by dividing the possible range of scope into 10 fixed width intervals. Very similar results obtain using expending per capita as the treatment variable, partitioning the data in different number of strata, partitioning by percentiles, and using second round results.

4 Did CCT's function as economic stimulus?

After *Bolsa Família* was linked to Lula's reelection in 2006, it was suggested by some that economic growth, and not CCTs, were actually behind the shifting vote patterns in the country (Shikida et al. 2009). Given that economic conditions were similarly auspicious at the time of the 2010 election, and given that CCTs might have positively affected economic conditions even of non-recipients, the interplay between CCTs and economic performance is crucial to the understanding of the electoral consequences of CCTs.

CCTs put cash in the hands of the poorest families, and the poor are more likely to spend than to save. In less developed places, where a large share of the population are CCT beneficiaries, this increased spending can be very consequential even for non-beneficiaries. While economic growth is brought about by many different factors, I here simply suggest accounting for this indirect path between CCTs and voting behavior. I test this hypothesis both through mediation analysis and instrumental variable estimation.

The problem can be formulated within the framework of structural equation models (LSEM) and mediation analysis (Baron & Kenny 1986), in which direct and indirect links between the dependent and independent variables are decomposed into two linear regressions as follows:

$$\text{Growth}_i = \alpha_1 + \beta_1 \text{CCT}_i + \xi_1 \text{Controls}_i + \epsilon_{i1} \quad (1)$$

$$\text{Incumbent VS}_i = \alpha_2 + \beta_2 \text{CCT}_i + \delta \text{Growth}_i + \xi_2 \text{Controls}_i + \epsilon_{i2} \quad (2)$$

The direct effects of CCTs on incumbent vote share are represented by β_2 . CCTs indirect economic stimulus effects are defined as $\beta_1 \times \delta$.³⁰ Recently Imai, Keele & Yamamoto (Forthcoming) have generalized this setup to non-linear relationships and shown that the mediation approach can be thought of as a way to isolate causal mechanisms.³¹ Typically, the analyst would be interested in pinning down the specific mechanism represented by the mediation variable, where the rest of the explanation could be composed of many different mechanisms.

The key assumption that permits the identification of mediation effects is sequential ignorability, which requires first that treatment be ignorable given a set of covariates and then, subsequently,

³⁰The sum of direct and indirect effects constitute the total effects, and take on the same value as the coefficient on CCT if Eq. 2 is estimated without the mediating variable (Growth).

³¹See Imai, Keele, Tingley & Yamamoto (2010) for a user-friendly discussion of LSEM and mediation analysis, on which this section draws heavily, and Imai et al. (2010) for the technical details.

that the observed mediator be ignorable given treatment condition and the same set of covariates. The first of these assumptions is common to all observational studies, and in the present case, given that OLS and matching regression results were quite similar, should be a reasonable assumption. The second assumption is harder to gauge, but the approach allows for analysis of how sensitive the results are to deviations from this assumption.³²

Table 2(a) shows the results for the mediation analysis, reporting direct effects of CCTs on incumbent vote-share, its indirect effects via economic growth, and the total effects of economic growth on incumbent party candidate vote share. Regressions included a host of other controls and state fixed effects, which are omitted for simplicity.

Another approach makes use of the fact that growth is an endogenous variable in the non-recursive system defined by Equations 1 and 2.³³ Provided a suitable instrument for economic growth can be found, the system can be estimated by 2SLS.

One readily available instrument for economic growth is a municipality's reliance on exports. It is a well known fact that exchange rates are one of the policies that more clearly generate political and electoral effects, and precisely for that reason exchange rates have been widely used and abused by Latin American governments (Dornbusch & Edwards 1990). Overvalued exchange rate periods tend to be accompanied by consumption expansion and are for this reason generally highly popular with the electorate (Bonomo & Terra 1999). However, overvalued exchange rates do not affect all sectors — and consequently all regions — equally. While consumers may rejoice with an overvalued currency, the exporting sector is generally hurt. This differential impact has been exploited by Carvalho Filho & Chamon (2008), who have recently argued that the mechanism by which exchange rate appreciations are transmitted to voters is mainly through changes in factor incomes rather than by

Between 2002 and 2006 the Brazilian Real moved from an extremely undervalued position against the US Dollar to an extremely overvalued one in historical terms, where it has remained. Hence, it is plausible that, all things equal, municipalities that relied on exports would exhibit lower economic growth in 2006 and 2010 than municipalities that are essentially “consumers”. In 2002 the opposite should hold. Granted, not all people in export relying municipalities earn wages

³²The zero correlation assumption between ϵ_{i1} and ϵ_{i2} , typical of LSEM, is a particular case for linear equations of the more general assumption of sequential ignorability. The more general formulation allows for the identification of mediation effects in non-linear settings as well. While the problem at hand is linear, Imai, Keele & Yamamoto's (Forthcoming) approach makes this assumption clear and allows for sensitivity analysis.

³³This approach is directly analogous to an encouragement experimental design (Imai, Keele & Yamamoto Forthcoming) whereby not only the treatment variable, but also the mediator variable is manipulated in an attempt to rule out confounding factors that could affect both mediator and response variables.

Table 2: Direct and Indirect Effects of CCTs on Incumbent Vote Share (2002–2010)

(a) Mediation Analysis				(b) Instrumental Variable Analysis			
	2002	2006	2010		2002	2006	2010
CCT Indirect	0.002	0.008	0.004	CCT Indirect	0.044	0.066	0.019
(SE)	0.002	0.004	0.003	(SE)	0.032	0.029	0.013
CCT Direct	0.214	0.200	0.185	CCT Direct	0.169	0.140	0.170
(SE)	0.044	0.026	0.027	(SE)	0.039	0.032	0.020
Growth Direct	0.015	0.033	0.017	Growth Direct	0.427	0.286	0.088
(SE)	0.006	0.006	0.006	(SE)	0.224	0.122	0.062

Notes: The mediating variable is average economic growth at the municipal level over the two years prior to the elections. In the 2SLS models, exports per head was used as the main instrumental variable for economic growth. Incumbent party candidate vote share is the dependent variable and scope of CCTs is the treatment variable.

from exports, and even those that do can benefit as consumers from overvalued exchange rates. The assumption here is simply that greater aggregate consumption capacity makes everybody on average better off, and that in non exporting municipalities this improvement is not offset by any loss in welfare resulting from forgone exports. More importantly to the task at hand, this greater capacity to consume is exogenous to CCTs.

We should not expect the results to be the same because the two methods are estimating different quantities.³⁴ This caveat aside, the basic message from both sets of results is very coherent. Economic growth has a positive and almost always statistically significant effect on incumbent vote share, but even after accounting for growth, the direct effect of CCT's is always significant and follows the same patterns across years that were discussed earlier. There is very little support, however, for the hypothesis that CCT's have an economic stimulus effect, except perhaps, in 2006.³⁵

5 CCT effects at the individual level

CCTs have a significant electoral impact, but there is no support in the data for it being just clientelism in disguise, nor there is support for the idea that CCTs are simply an economic stimulus. I turn now to the analysis of individual level survey data to attempt to further identify the causal mechanisms at play.

³⁴The 2SLS approach estimates the effects for a sub sample in which the instrumental variable's effect on growth is stronger.

³⁵Sensitivity analysis shows that the indirect effect through economic growth is sensitive to small violations of the sequential ignorability assumption, which is not surprising given the small magnitude of the effect.

The most obvious and intuitive mechanism by which CCTs can affect voting behavior is by making recipients more likely to vote for the incumbent candidate because the cash transfer makes them better off.³⁶ This effect, if present, would suggest that the distribution of particularistic goods can increase support even when done based on universal criteria, and in the absence of enforcement, monitoring, or threat thereof. To identify this effect, I compare voting behavior of CCT beneficiaries with otherwise similar individuals that do not receive the benefit.

5.1 Effects of receiving the benefit

The ultimate test of CCTs direct effects on the voting behavior of beneficiaries must rely on a comparison of individuals who receive the benefit with otherwise similar individual that do not.

While surveys provide access to individuals minds, they require us to rely on stated — as opposed to revealed — preferences. To minimize the discrepancies between the two, it is best to use surveys taken very close to the actual election date. Unfortunately, there are no surveys that asked any CCT related questions close to the 2002 election, and very few asked it in 2006.³⁷ For 2010 I use the last wave of the Brazilian Electoral Panel Survey, a collaborative effort in which I participated, which interviewed a nationally representative sample three times over the course of 2010 (Ames, Machado, Renno, Samuels, Smith & Zucco 2010). This survey interviewed 1221 respondents in 16 states. For 2006, the best available option is the first Vox Populi survey probing voter intention for the second round of the presidential election (Vox Populi 2006). Conducted only a few days after the inconclusive first round, it interviewed a total of 2005 voters in 25 states and asked three questions about CCT benefits.³⁸

Using these surveys, I can estimate whether CCT beneficiaries were more likely to have voted for Lula and Dilma, and by how much more. Evidently, respondents in higher income brackets do not contribute to the identification of this possible CCT effect, as there are no (or almost no) beneficiaries to contrast with non-beneficiaries. For this reason, I first balanced the data by matching treated and untreated observations across a number of individual level variables, as proposed by Ho, Imai, King & Stuart (2007). Exact matches were required for income bracket and region, and nearest neighbor matching was performed on other variables. Table 3 presents both the simple average treatment effects, comparing CCT beneficiaries to non-beneficiaries in the balanced sample, as well as the predicted effect of being a CCT recipient on the probability of voting for Lula,

³⁶Evidently, one could further discuss the precise nature of the causal mechanism, and to whether it is, in fact, retrospective (gratitude) or prospective (fear of losing the benefit if another candidate is elected).

³⁷This lack of surveys is in itself evidence that there was little interest on the topic until after the 2006 elections.

³⁸It should be noted that the questions themselves referred to “Bolsa Família or other similar programs.”

Table 3: Electoral Effect of Receiving a Benefit (2006 and 2010)

(a) 2006					
Family Income (in Min. Wages)	N (Original N)		Prob. Vote Lula		Risk Ratio*
	Not CCT	CCT	Not CCT	CCT	(p-value)
Less than 1	153 (194)	153 (156)	0.59	0.79	1.35 (<0.01)
01–05	230 (808)	230 (230)	0.51	0.73	1.45 (<0.01)
05–10	22 (248)	22 (22)	0.39	0.62	1.63 (<0.01)
More than 10	3 (148)	3 (3)	0.45	0.65	1.57 (<0.01)
ATE	408 (1398)	408 (411)	0.59	0.78	1.32 (<0.01)

(b) 2010					
Family Income (in Min. Wages)	N (Original N)		Prob. Vote Dilma		Risk Ratio
	Not CCT	CCT	Not CCT	CCT	(p-value)
Less than 1	81 (115)	81 (136)	0.65	0.76	1.16 (0.01)
1–2	105 (174)	105 (133)	0.56	0.68	1.22 (<0.01)
2–3	48 (150)	48 (48)	0.46	0.58	1.28 (<0.01)
3–5	32 (131)	32 (32)	0.53	0.64	1.24 (0.01)
More than 5	10 (135)	10 (10)	0.57	0.68	1.22 (0.01)
ATE	276 (705)	276 (360)	0.48	0.59	1.23 (0.01)

Notes: Risk ratios refer to the increase in probability of voting for the incumbent associated with participation in a CCT. The value of the monthly minimum wage was R\$350 (US\$160) in 2006 and it increased considerably to R\$10 (US\$300) in 2010, which is the main reason surveys use markedly different brackets. Data are from the first Vox Populi survey immediately after the first round of the 2006 presidential election (Vox Populi 2006) and from the third wave of the Brazilian Electoral Panel study (Ames et al. 2010) carried out after the second round of the 2010 presidential election. Both were preprocessed using MatchIt (Ho et al. 2007) to ensure balance between CCT recipients and non-recipients. Observations were matched without replacement, exact matches were required for income bracket and region, and nearest neighbor matching was performed on other variables. Income refers to household income, and there was no information as to the size of households. Probability of voting for incumbent was computed from a logit regression, including income, CCT participation, and a series of socioeconomic controls.

by income bracket, as computed through a logit regression of vote for the incumbent candidate on CCT participation, income bracket, age, education level, gender, and rural/urban localities, also estimated on the balanced sample.

The 2006 survey indicates that the effect of the program among the poorest voters amounted to a 35% increase in the probability that somebody will vote for the government. For the next income bracket this effect increases to 45%, and it is even larger for higher household income levels, but these are less substantively significant as almost all beneficiaries are located in the lower two brackets. Across the whole balanced sample (and without controls) the probability of voting for Lula is increased by 0.19, or 32%. Effects in 2010 are smaller, but still substantively and statistically significant, suggesting an increase of 0.13 (or just under 30%) in the probability of voting for the incumbent candidate. This smaller effect found at the individual level in 2010 is compatible with results of the analysis of aggregate data, presented earlier.

5.2 Reactions across different subsets of non-beneficiaries

The existence of CCTs might also affect subgroups of non-beneficiaries differently.³⁹ In general, those on the paying side of redistribution might simply oppose governments that implement redistributive policies, though not all non-recipients are subject, for instance, to income taxes.⁴⁰ Hence, we should expect different reactions from the more well off income tax payers and the less well off non-recipients.⁴¹

In opposition to this redistribution backlash, contrasting non-beneficiaries that know a beneficiary to those that do not can allow us to identify a solidarity effect. Similarly, non-beneficiaries in municipalities where there is high coverage are more likely to see and hear about the results of the program, so contrasting them to non-beneficiaries from municipalities where coverage is smaller is another way to assess the presence of the solidarity effect.

Both surveys under consideration also asked whether respondents had friends or acquaintances who received the benefit, to which just over a half of all non-beneficiaries responded positively. Granted, it would be best to have a measure of whether non-beneficiaries actually approved of CCT programs as well, but unfortunately this question was not part of the 2006 survey.⁴² Obviously, those that know a beneficiary are more likely to share other characteristics with them, and it could be these similarities — and not proximity to beneficiaries — drive pro-incumbent behavior.⁴³ To identify of a possible independent effect of knowing a beneficiary I matched non-beneficiaries who knew a beneficiary to those who did not on a set of pre-treatment observable socio-demographics. I also matched non-beneficiaries in high-coverage municipalities to similar beneficiaries in low-coverage municipalities, and contrasted the probability voting for the incumbent in each group. Results are reported in Figure 3.

All things equal, knowing a beneficiary increased the probability of voting for Lula by about 0.18 (equivalent to an almost 40% increase) and by about 0.11 (just over 20%), in 2010. Likewise, non-recipients with high CCT coverage voted for the respective incumbents at a higher rate than

³⁹While it is safe to assume that beneficiaries know about the program, there is no immediate way of assessing what non-beneficiaries know about it. I assume this problem away based on survey data suggesting that a large majority of Brazilians had some knowledge about CCTs. In a survey taken a month before the 2006 elections less than 3% of respondents did not have an opinion on the government handling of poverty policies, the lowest among all policies that were polled (Ibope 2006). Furthermore, in 2010 close to 90% of respondents could attribute the program to the Federal Government as opposed to other levels of government. (Ames et al. 2010)

⁴⁰While all Brazilians pay hidden consumption taxes, income tax is highly visible.

⁴¹A similar observable difference between the more and less well-off group could be generated by those slightly above the poverty threshold taking comfort in knowing that there is an effective safety-net beneath them in case it becomes necessary.

⁴²Analysis of the full sample (including beneficiaries and non-beneficiaries) suggests that knowing a beneficiary increases the probability of voting for the incumbent by about half of the effect of actually receiving the benefit.

⁴³Empirically, lower income non-beneficiaries have much higher probability of knowing a recipient.

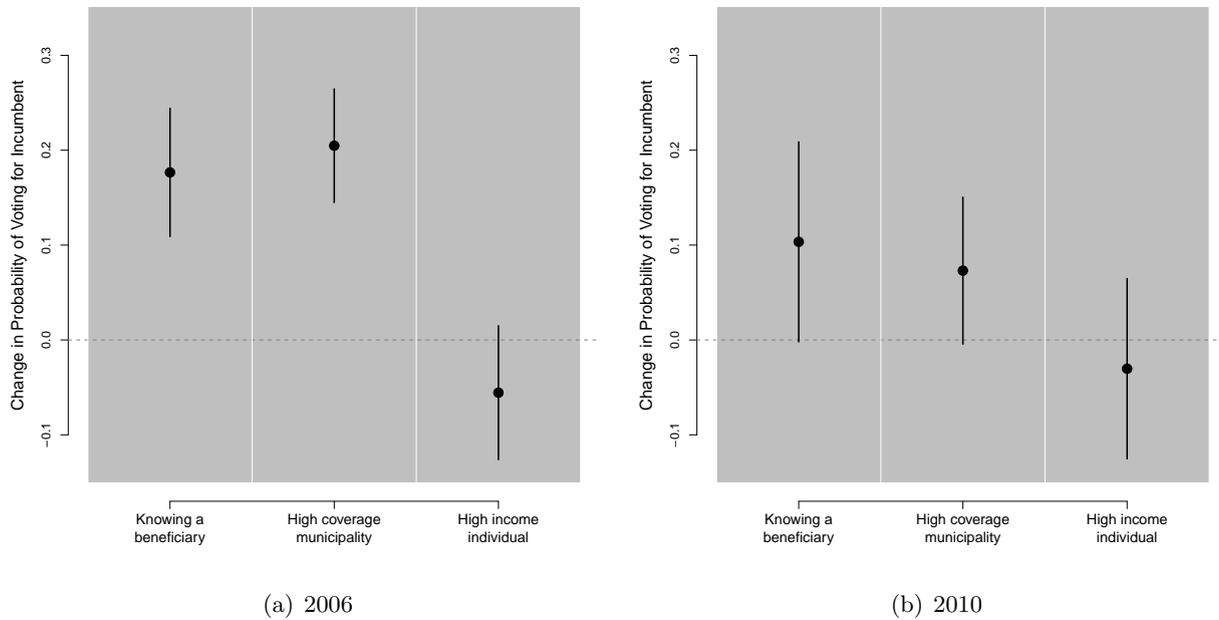


Figure 3: Effects of Non-Beneficiaries (2006 and 2010)

Notes: Figure reports simple differences in the probability of voting for the incumbent for different subgroups of non-beneficiaries, with their respective 0.95 confidence intervals level. Each difference was computed on a reduced sample preprocessed using MatchIt (Ho et al. 2007) to ensure balance on the relevant variable. Estimates are very similar to those obtained controlling for other variables.

their counterparts with less access to the program.

Finally, there is surprisingly little evidence that redistribution backlash is driving reactions of non-beneficiaries. As is also reported in 3, after balancing the sample of non-beneficiaries on income level, it emerges that although higher income non-beneficiaries were less likely to have supported the incumbent candidates, these differences are relatively small (especially in 2010) and not statistically significant. These results, as is true with most “negative” results however, should be taken with a grain of salt. The cutpoint between low and high income was chosen to approximate the threshold for paying income taxes but backlash might manifest itself a higher levels of income, and future research will look into this particular issue.

The precise reason for the lack of backlash is yet to be determined. In general, an extremely skewed income distribution might create space for redistributive policies not be too harshly felt by those paying the bill while still make a significant difference in the lives of those that benefit. A different argument, and one more specific to well administered CCTs, is that the adoption of universalist policies as opposed to clientelistic exchanges might send non-beneficiaries a positive signal about the government’s overall effectiveness and honesty (Stokes 2007, Weiss-Shapiro 2008),

or that the conditionalities imposed by CCTs make it more acceptable to others (Fiszbein et al. 2009). Preliminary results from a survey experiment embedded in the 2010 survey shows that providing respondents with information about the existence of conditionalities does not further increase support for the program, but more research is needed on this topic.

These results are also compatible with the existence of regional sensitivities. Being in a place with low coverage means that the bulk of the resources are being spent elsewhere, in which case voters might be less inclined to support the incumbent because they feel resources are being siphoned away from them, even if personally knowing a beneficiary can compensate for this concern. Disaggregating the data by region further supports this idea, as differences in support for the incumbent vary more across region than between income groups within the same region. In particular, incumbent support in the more affluent Southern region of the county is much lower than in the poorer Northeastern region. This interpretation of the results, however, is still tentative as the data are not particularly well suited for the task. The identification of the precise mechanism — whether a regional distribution effect or simple awareness of the program — will need to rely on more detailed future work.

6 Discussion and Conclusion

CCTs are flexible enough to be implemented at different levels of state capacity, with different long term objectives, and in widely varying scales. Though not designed or implemented to fight inequality per se, there is evidence that CCTs' success in reducing extreme poverty rates can also reduce inequality (Soares, Soares, Medeiros & Osório 2006, Barros et al. 2010). Effective results in fighting malnutrition, in keeping children in school, and in improving the lives of the neediest might make CCTs “good” policy, even though there is still debate about the long term consequences these programs.

But good policies have fallen, and will fall, to the imperatives of the governments' budget priorities and electoral incentives. In this context, a good policy that is also politically viable has more chances of surviving. This paper makes the case that the short term electoral benefits of CCTs are clear. It makes sense, then, that in the 2006 presidential election all four candidates — which spanned the spectrum from the extreme left to the center-right — advocated the expansion of the program (Hall 2006), and that no major candidate in the 2020 election suggested eliminating the program either (The Economist 2010). CCTs now have a clear constituency, and while the

electoral boost for future incumbents might not be as large as in 2006, rolling back such programs is not a feasible alternative.

In all elections held in Brazil in the presence of large CCTs, these programs have generated significant pro-incumbent electoral boosts. There is some evidence that in 2006 CCTs also swayed voters by acting as an economic stimulus, but even after controlling for economic growth, CCTs had significant electoral effects. CCTs substantially increased support for the incumbent candidate among beneficiaries and non-recipients who know beneficiaries and who live in places with higher CCT coverage show higher support for the incumbent candidate than other non-beneficiaries.

Simply knowing that CCTs increased the probability of beneficiaries voting for the incumbent candidate has profound implications for how we think about clientelism. For as long as clear benefits reach voters, it is apparently not necessary to rely on costly monitoring of voters to win elections. The additional finding that there is no evidence that the more well-off resist redistribution through CCTs is also noteworthy. Surveys currently in the field will investigate whether it is the existence of conditionalities that makes CCTs palatable to non-recipients, whether perceptions of competence in administering CCTs affect these perceptions, and also whether information about the origins of the program affects who voters support. Whatever the mechanisms, redistributive policies acceptable to those footing the bill and delivering clear benefits to the neediest sound more like a mythical holly grail of politics than a feasible policy option. Surveying different empirically observed modes of redistribution, Przeworski (2010, p.84) notes that “income-earning capacities can be equalized by policies that are narrowly targeted on increasing the productivity of the poor (...), such as relaxing credit constraints, training for specific skills, subsidizing the necessary infra-structure, focusing on diseases to which poor people are most vulnerable, etc. Such policies, however, take time to bear fruit, and both diagnosing the needs and targeting the policies require a high level of administrative competence.” CCTs, according to the findings of this paper, might constitute a strong candidate for a policy that delivers some of what Przeworski was claiming for, while being both politically and technically viable.

If targeting and monitoring are not necessary to win votes, further and perhaps more intriguing questions come to the fore. The existence of electoral returns to CCTs does not mean that universalist policies will automatically or necessarily replace clientelism. Politicians might not anticipate these positive results, and it is also not clear whether risk-averse politicians would spontaneously try something new even if evidence that it might work existed. In fact, models of redistribution sometimes assume that parties have advantages in targeting some groups over others (Dixit &

Londregan 1996), which can be read as the existence of some type of linkage (possibly clientelistic) between parties and voters.

Even if under certain conditions CCTs dominate clientelism,⁴⁴ it still does not explain why did CCTs emerged when they did. We are only beginning to think about the proper answer to this question, but structural factors will necessarily be part of any complete explanation. One simply cannot ignore that international organizations are actively sponsoring these policies (Milazzo & Grosh 2008), and that the timing of implementation of CCT programs is suspiciously similar across cases (Fiszbein et al. 2009). CCTs are part of a broader move to non-contributory social programs in the region (Mares & Carnes 2010), and the conditions for such a shift can probably be linked to the informal sector's expansion during the lost decade of the 1980s. Previous existing social protection policies were occupationally-based, inherited from a time when those that were excluded of these arrangements were not participants in the democratic process. The severance of formal employment links that coincided with the return to democracy across the region and expanded enfranchisement created the need to cater to the "outs," and the possibility of new electoral coalitions supported on the very poor at the expense of the traditional middle class. The reduction of more traditional contributory social benefits targeted to the "ins" and the expansion of CCTs should be read as two sides of the same phenomenon.

However, even if future research finds support for a structural explanation along these lines, country specific trajectories will continue to inspire different contextual explanations. One such argument is the idea that if incumbents need the support of political adversaries to implement any new social policies, they will have to accept more constraints and less discretion. This is conjectured to be the reason behind the PRI's adoption of *Progres*a (Diaz-Cayeros, Magaloni & Estévez Forthcoming), and de La O (2010*b*) finds preliminary support for the connection between lack of a legislative majority and greater institutionalization of CCTs throughout Latin America.

Thinking about the scope conditions of Brazilian experience, however, might provide us with different insights. In Brazil, less political discretion was not a concession to the opposition. Both the PSDB and the PT governments — parties under which CCTs were introduced and expanded — commanded large legislative coalitions. The opposition did not have the power to demand such

⁴⁴It is not even clear what the electoral returns of clientelism are. Some studies question the electoral effectiveness of clientelism (Kitschelt 2010), others argue that it is not an option to all parties (Calvo & Murillo 2004), but a head-on comparison between clientelism and more programmatic redistribution found the former yielding higher electoral payoffs (Diaz-Cayeros, Magaloni & Estévez Forthcoming). Though in apparent contradiction, these findings are compatible with the idea that clientelism pays electorally when parties have already built their own networks, but not under all circumstances.

bargains, and coalition partners were not likely to do so.⁴⁵ An important feature of the case is that both national parties lacked (and still do) partisan networks on the ground. Both rely on alliances with “traditional” parties, which typically command political machines at the levels of the states (Hagopian 1996). As there cannot be clientelism without partisan networks, the choice facing both parties was not necessarily the same as machine parties elsewhere. On the one hand, any attempt by the PT and PSDB to build their own networks would generate conflict with coalition partners. On the other, using partner’s networks to provide benefits to voters risks empowering less than reliable allies,⁴⁶ and neither PT nor PSDB had interest in feeding the regional clientelistic machines of other parties.

The reliance on on-the-ground operators gives clientelism an intrinsically local flavor, as it must involve local level connections, and be sustained by face-to-face dynamics. For this reason, national level parties that adopt clientelistic strategies tend to be the amalgamation of several local networks (Dunning & Stokes 2010). Typically, this amalgamation is done under the same party brand, which probably helps solve commitment problem between national and local politicians. In Brazil, as this common branding is absent, the commitment problem cannot be solved, and national politicians have sought out other alternatives.

Though still very tentative, this discussion brings clientelism’s *raison d’être* back to the fore. Most arguments about the persistence of clientelism relate to the fact that it is necessary to solve commitment problems between voters and politicians. If votes, however, can be obtained without the enforcement mechanisms in the clientelistic exchange, perhaps clientelism really serves the interest of the network operators, more than that of national politicians and voters, as has been suggested by Szwarcberg (2010). This would imply that it is not the partisan networks that exist to allow for clientelism, but rather that clientelism exists to feed the clientelistic machines.

The preceding considerations suggest at least two paths for replacing clientelism: as a concession to emerging competitors or as an attempt to sideline partners that control existing clientelistic networks. Both imply some level of power sharing, and at least one relevant political force that lacks clientelistic networks on the ground and has no incentive — or cannot — to build them. In situations where hegemonic parties exist, or when parties for which achieving electoral hegemony

⁴⁵In fact, anecdotal evidence suggests that junior coalition partners were more likely to require more — and not less — discretion.

⁴⁶There are plenty of examples of local political bosses switching national level allegiances to and from PSDB and PT. The most important local political boss in the PFL — the main PSDB ally throughout the 1990’s — and one of the strongest supporters of Cardoso in 1994 and 1998 — Antonio Carlos Magalhães — supported Lula in 2002. The same applies to the Sarney dynasty in Maranhão that switched from supporting the PSDB to supporting PT in 2006.

is a real possibility, sticking to clientelism or building clientelistic machines might be the more attractive option.⁴⁷ Future work will further specify the structural and contextual conditions under which it makes sense to shun clientelism and initiate programmatic policies.

Appendices

A Zero coverage cases

Given the lack media coverage and expert analysis, the result for 2002 is quite surprising. Hence, alternative tests are important to corroborate these results. Some empirical leverage can be obtained from the fact that seven municipalities in six different states in the 2002 data-set did not have any cash-transfer coverage.⁴⁸ These are all small places, and absence of coverage is almost certainly related to weak bureaucratic capacity, or inattentive mayors. However, these municipalities are not particularly poorer or otherwise significantly different from many others, so a sense of the electoral effects of cash transfers could be obtained by comparing these municipalities to similar municipalities that had coverage.

I matched each of the seven cases with no coverage to the most similar municipality with coverage.⁴⁹ While Serra obtained 46.25% of the vote in the municipalities with no coverage, his performance was 56.01% in similar municipalities with some coverage. This difference is the same within pairs of units, and even after controlling for other factors that could account for, as the matching procedure does not eliminate all difference between groups. In fact, Serra did better in 6 of the 7 cases in which CCTs existed coverage. Coverage, however, ranged from 11% to 32% of families and spending per capita per year ranged from R\$10 to R\$36 in the “treated” group, so simple presence of some coverage is a very blunt measure of the program’s effects. Despite the very reduced number of observations and the bluntness of the measure of treatment, average treatment effects of CCT on Serra’s vote share are large (an increase of 9 percentage points in vote-share), though understandably noisy (P-value=0.053). For 2006 there were four municipalities with no CCT coverage, but all were new municipalities for which several critical socioeconomic indicators

⁴⁷Building networks in the face of opposition from existing clientelistic parties that occupy the space is probably very risky and not a very attractive strategy. However, if there is no clientelistic party in place, and legislative opposition is too weak to mount a challenge, it might be possible.

⁴⁸In reality, there were eight municipalities with no coverage in 2002, but for Santa Cruz do Xingu, MT, there are missing data in several other variables.

⁴⁹Exact match was required for state, and nearest neighbor matches for all other pre-treatment observed covariates.

are lacking, so a similar analysis could not be carried out for that year. All municipalities have coverage in 2010.

B Additional Results

Table 4: Determinants of Incumbent Party Candidate Vote-Share (OLS)

	Expenditures			Scope		
	2002	2006	2010	2002	2006	2010
spentH.cct	0.168	0.088	0.044			
SE	0.019	0.005	0.004			
scope				0.199	0.196	0.159
SE				0.021	0.013	0.013
hdi	-0.123	-0.371	-0.266	-0.102	-0.393	-0.236
SE	0.035	0.036	0.029	0.036	0.037	0.030
previous.vs	0.521	0.411	0.655	0.520	0.414	0.656
SE	0.010	0.011	0.009	0.010	0.011	0.009
mayorpty.PSDB	0.010	0.000	0.002	0.010	0.000	0.002
SE	0.003	0.003	0.002	0.003	0.003	0.002
mayorpty.PT	-0.002	-0.012	-0.009	-0.002	-0.013	-0.009
SE	0.004	0.004	0.003	0.004	0.004	0.003
incgov.PSDB	-0.005	0.006	-0.003	-0.005	0.006	-0.003
SE	0.003	0.004	0.003	0.003	0.004	0.003
incgov.PT	0.002	-0.006	-0.004	0.002	-0.006	-0.004
SE	0.006	0.006	0.004	0.006	0.006	0.004
log.pop	-0.004	0.003	-0.009	-0.005	0.003	-0.009
SE	0.001	0.001	0.001	0.001	0.001	0.001
log.gdpH	-0.010	-0.014	-0.001	-0.010	-0.016	-0.001
SE	0.003	0.003	0.002	0.003	0.003	0.002
log.dgdpH	0.024	0.033	0.017	0.024	0.035	0.016
SE	0.005	0.006	0.005	0.005	0.006	0.005
nonwhite	-0.001	0.002	0.000	-0.001	0.002	0.000
SE	0.000	0.000	0.000	0.000	0.000	0.000
pent	-0.004	0.002	-0.003	-0.004	0.002	-0.003
SE	0.000	0.000	0.000	0.000	0.000	0.000
X.Intercept	0.131	0.273	0.272	0.117	0.279	0.243
SE	0.033	0.032	0.026	0.033	0.032	0.026
State FE	YES	YES	YES	YES	YES	YES
R2	0.637	0.797	0.848	0.638	0.794	0.848
N	5486	5487	5487	5486	5487	5487

Notes: Table reports the full set of OLS estimates for incumbent party candidate vote share.

Table 5: Predicting CCT Coverage

	Expenditures			Scope		
	2002	2006	2010	2002	2006	2010
Target				0.040	0.693	0.123
SE				0.010	0.013	0.012
HDI	10.511	58.917	96.241	5.910	10.396	22.207
SE	1.634	6.125	6.147	1.488	2.002	1.789
HDI ²	-14.825	-86.528	-142.468	-8.830	-14.977	-33.801
SE	2.412	9.046	9.072	2.197	2.957	2.640
HDI ³	6.415	39.684	66.497	3.955	6.879	16.130
SE	1.175	4.404	4.416	1.070	1.439	1.285
Previous Vote-Share	-0.021	-0.006	0.358	-0.014	-0.003	0.080
SE	0.007	0.026	0.032	0.006	0.009	0.009
Gini	0.153	0.789	0.980	0.121	0.108	0.237
SE	0.015	0.057	0.057	0.014	0.019	0.017
Distance to Capital	0.003	0.008	0.014	0.002	0.002	0.005
SE	0.001	0.002	0.002	0.000	0.001	0.001
log(GDP/H)	-0.016	-0.045	-0.094	-0.013	-0.016	-0.039
SE	0.002	0.007	0.007	0.002	0.002	0.002
Share Non-White	0.000	0.001	0.001	0.000	0.000	0.000
SE	0.000	0.000	0.000	0.000	0.000	0.000
Share Pentecostal	-0.001	-0.003	0.002	-0.001	0.000	0.001
SE	0.000	0.001	0.001	0.000	0.000	0.000
Governor PSDB	-0.002	-0.003	-0.017	-0.003	-0.002	-0.004
SE	0.002	0.008	0.008	0.002	0.003	0.002
Governor PT	0.000	0.000	0.000	0.001	-0.003	0.003
SE	0.004	0.015	0.011	0.003	0.005	0.003
Mayor PSDB	0.000	-0.005	-0.005	0.000	-0.003	-0.001
SE	0.002	0.008	0.008	0.002	0.003	0.002
Mayor PT	-0.004	-0.006	-0.009	0.000	0.003	-0.003
SE	0.004	0.011	0.010	0.004	0.004	0.003
Intercept	-2.163	-12.254	-20.185	-1.030	-2.276	-4.445
SE	0.364	1.367	1.372	0.331	0.446	0.399
State FE	YES	YES	YES	YES	YES	YES
R2	0.779	0.807	0.896	0.806	0.881	0.915
N	5469	5469	5469	5469	5469	5469

Notes: Table reports OLS estimates for determinants of treatment, that were used to generate predicted treatment for each municipality. The R^2 for all regressions is high. State fixed effects account for only a small portion of the variance explained. The dependent variables are the two different operationalizations of CCT coverage, as explained in the text. Data are observed at the municipal level. Municipalities with missing data were excluded, but only amount to 1% of total municipalities.

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