

When Payouts Pay Off: Conditional Cash Transfers and Voting Behavior in Brazil 2002–10

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This article estimates the electoral effects of conditional cash transfers (CCTs) in three presidential elections in Brazil. It analyzes municipal-level electoral results and survey data and employs matching techniques to reduce causal inference problems typical of observational studies. Results show that CCTs are associated with increased performance by the incumbent party presidential candidate in all three elections but that these effects have been reaped by incumbents from different parties. It also shows that CCTs have had no discernible impacts on party identification and the performance of incumbent parties in legislative elections. Together, these findings suggest that CCTs are significant in the short run, but lack the capacity to induce substantial long-term voter realignments.

What are the long-term effects of targeted social policies? Can incumbents, for instance, reap the electoral benefits of a social policy that was introduced by a predecessor of a different party? There is a considerable body of work that tells us that voters tend to respond retrospectively to changes in their economic situation, but important details of how these results are obtained are not well understood. There are hints that voter responses are often myopic and overly driven by short-term reactions to government actions, but there is also emerging evidence that programs that provide economic gains to voters produce electoral effects that linger on for some time. However, even if voters rely on retrospective evaluations, it is unclear whether they should reward those who initially introduced a benefit or those currently dispensing it.

I address this question by examining the electoral consequences of Brazil's conditional cash transfers (CCTs). CCTs are arguably the fastest-growing type of 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, the most important of which were keeping children in school and regularly visiting a doctor (Amaral and Ramos 1999; World Bank 2001). Today, CCTs are present in dozens of countries, and large-scale national-level programs such as the Mexican *Progresas/Oportunidades* and the Brazilian *Bolsa Escola/Bolsa Família* cover millions of poor families and have become important pillars of their countries' social protection systems (Fiszbein et al. 2009).

CCTs are an ideally suited setting in which to evaluate effects of social policies. As the most important policy innovation in the developing world in decades, CCTs are high-profile programs, providing clear individualized monetary benefits to a well-defined population. Although new, CCTs have now existed long enough so that their electoral effects over several elections can be examined, providing us with empirical leverage on the issue of long-term effects of social policy. Finally, CCTs are often

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implemented in ways that generate a vast amount of data, making it possible to at least partially overcome inferential limitations that typically impinge on observational studies.

This article contributes to the literature by offering a detailed study of the Brazilian experience. It is the first study to cover three consecutive presidential elections in which CCTs were present, allowing examination of electoral effects under two different incumbent parties and over a period in which CCTs expanded and became more salient. This study is also distinctive for its use of different types of data, for its reconciliation of estimates obtained at the aggregate and individual levels, and for the attention paid to minimizing causal inference problems. Despite being a strictly observational study, this combination of approaches boosts our confidence on a causal interpretation of the results.

I show that CCTs can be linked to improved incumbent electoral performance even before pundits and scholars took notice of these programs. Estimated pro-incumbent impacts of CCTs on electoral results have remained roughly constant, which is particularly striking given that different incumbents have benefited from the program. It suggests that voter response to CCTs is similar to ordinary retrospective economic voting. Corroborating this insight, I also show that CCTs have had no noticeable impact on the performance of incumbent parties beyond their presidential candidates, nor have they affected levels of party identification.

The ultimate message of this article is that CCTs matter electorally, but more in the short than in the long run, and not enough to cause long-term voter realignments. The immediate electoral rewards to incumbents who implement and expand such programs can entice rulers around the world to adopt CCTs. This is important because such transfers are believed to be quite effective social policy. However, CCTs are not likely to create a devout following or, by themselves, create clienteles of long-term followers attached to parties or particular politicians.

Should Voters Reward Incumbents for Targeted Social Policies?

Both theoretical reasons and empirical evidence suggest that the economic conditions at the time of elections affect electoral results (e.g., Duch and Stevenson 2006; Fiorina 1981; Lewis-Beck 1988; Stigler 1973). There has been much less work on longer-term effects of economic performance and on how individuals respond to the specific economic conditions they face and to the gov-

ernments' attempts to affect it. On the former point, Achen and Bartels (2008) suggest that changes in income levels could lead to long-term voter realignments. On the latter, most studies tend to converge to the finding that government transfers matter positively (Levitt and Snyder 1997; Pop-Eleches and Pop-Eleches, 2012; Schady, 2000). Research on longer-term effects of government targeted benefits, however, is all but absent.

One recent exception is work by Bechtel and Hainmueller (2011), which examined the long-term effects of disaster relief spending. Making use of a natural experiment afforded by major flooding in Germany, the authors are able to show that expenditures boosted the incumbent's vote share in the election immediately following the flooding and that smaller effects lingered on to the next election before disappearing in the subsequent one.

We do not know, as of yet, whether the same results extend to targeted benefits provided by social policies, and any such analysis has to overcome important complications. Few policy instruments are relevant enough to shape voting behavior at the macrolevel, and it is not always possible to directly assess policy instruments at the microlevel. Moreover, benefits from social policies are dispensed over time, existing policies are typically inherited from previous governments, and many important social policy innovations happened decades ago.

Difficulties notwithstanding, the idea that social policy can affect voting behavior is fairly widespread. In fact, it comes in at least two different versions. On the one hand, bureaucratically administered programs are seen as ways of breaking clientelistic or patronage ties that would bind voters to political machines, giving voters more autonomy (Hunter and Sugiyama 2009; Weyland 1996) and providing incentives for citizens to become more politically active (Campbell 2003). But because social policies vary considerably in the degree to which they can be appropriated politically (De La O 2012), there is also a risk that they could be used to foster clientelism (Weitz-Shapiro 2012).

It remains an open question whether social policies lead to captive constituencies or voters who can retrospectively reward politicians for improving their particular economic situation. CCTs, in this context, present an interesting research opportunity because they are "new"—having been initiated in the developing world in the late 1990s—and relatively large social policies—sometimes reaching millions of families. Also very important is the fact that CCTs are often the subject of policy evaluation programs, generating a considerable amount of high-quality data to work with.

Partially for these reasons, CCTs have already attracted considerable scholarly attention. Research on determinants of program initiation and design has found that CCTs are initiated by governments on the right and the left alike, where existing levels of state capacity are high, and as a response to adverse economic conditions (Diaz-Cayeros and Magaloni 2009). There is also evidence that governments constrained by legislative opposition are more likely to implement more rule-bound programs, as opposed to informal programs that can be easily politically manipulated (De La O 2012).

More directly related to the present article, a larger number of studies have analyzed the electoral consequences of CCTs. The emerging consensus, here, is that CCTs improve incumbents' electoral performance (De La O 2013; Manacorda, Miguel, and Vigorito 2011; Zucco 2008). The most robust findings come from De La O (2013), who exploits the fact that Mexico's *Progresa* was started in a randomly selected subset of eligible villages. She finds that in electoral precincts composed of villages with an early onset of the program, there was higher turnout and vote for the incumbent PRI than in comparable precincts with late onset. Estimates of the incumbent boost range from 4 to 10 percentage points.

There is still scant empirical evidence as to whether CCT electoral effects are long lasting, but additional evidence from the Mexican case suggests CCTs have stronger short-term than long-term effects. Using matching techniques to analyze survey data, Diaz-Cayeros, Magaloni, and Estevez (2009) find that while beneficiaries were 17% more likely to support the PRI in 2000 than similar non-beneficiaries, they were 11% more likely to support the new incumbent (PAN) in the 2006 election.

Work by Manacorda, Miguel, and Vigorito (2011) is particularly relevant to this point. It analyzed PANES, a large-scale CCT in Uruguay, conceived as a temporary response to the acute economic crisis of the early 2000s. Using a regression discontinuity design, the authors show that PANES boosted support for the incumbent in the short run (by 11 to 14 percentage points) and continued to produce effects after the program was discontinued. The study's very solid design presents the strongest evidence that exists for longer-term effects of any social policy.

The substantive implications of the lingering effect, however, should not be overstated as it was measured only three months after the end of the program, in a moment in which PANES beneficiaries were still receiving a portion of their benefit. Furthermore, it does not tell us whether PANES beneficiaries would have remained loyal to the *Frente Amplio* government that initiated the program if a subsequent incumbent had continued delivering similar benefits.

I add to our incipient understanding of the longer-term electoral effects of social policies by studying Brazil's experience with CCTs. Brazil runs a very large CCT program and because of shorter presidential terms than Mexico—the other main early adopter—the country has already held three presidential elections under the presence of CCTs, allowing for the observation of electoral effects of these programs over time and under different conditions.

A number of articles have already addressed the electoral effects of CCTs in Brazil, but almost all have focused on the 2006 election¹ when Lula da Silva (Worker's Party-PT) was reelected president with massive support in the poorer regions of the country. This was particularly striking because the government was widely expected to crumble in the face of a massive corruption scandal and because in Lula's first four presidential campaigns, his voting patterns were skewed towards the wealthier regions.² Explanations for both Lula's success and the electoral realignment quickly focused on the *Bolsa Família Program* (BFP), the federal government's massive CCT program, detailed in the next section. Hall (2006) reported on the program just before the election, and Hunter and Power (2007) published what was probably the first academic piece linking the BFP to the 2006 electoral results, arguing through a very insightful analysis of very limited data that the program had been one of the pillars of Lula's victory. This was soon followed by Nicolau and Peixoto (2007) and Zucco (2008), with more data-intensive studies of electoral results that found more evidence in support of a BFP electoral effect. Other studies have included geographical analysis tools (Soares and Terron 2008), ecological inference techniques (Canêdo-Pinheiro 2009) and public opinion survey data (Licio, Castro, and Renno 2009) and mostly concur that CCTs had at a non-negligible effect on voting behavior. Additional work has placed CCTs in the context of a wider political strategy to bypass local-level politicians (Fenwick 2009) and has found little evidence of clientelism in the BFP (Fried 2012; Sugiyama and Hunter 2013).

In contrast to most of the published work, I make use of multiple elections to explore longer-term effects

¹De Janvry, Finan, and 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.

²In 1989, Lula lost the first democratic presidential election to Collor de Mello, by a narrow margin. The PT and PSDB (Party of the Brazilian Social-Democracy) have dominated subsequent elections. In 1994 and 1998, Lula was the runner-up to Fernando Henrique Cardoso. In 2002 and 2006, Lula defeated the PSDB candidates José Serra and Geraldo Alckmin. In 2010, PT's Dilma Rousseff defeated José Serra.

of CCTs and pay particular attention to causal inference problems. There exist a number of empirical challenges that make identification of electoral effects of CCTs in Brazil a nontrivial exercise. There were no randomized pilot programs, there are no obvious discontinuities to be exploited, and CCT eligibility and actual coverage are highly correlated with several other socioeconomic variables. I deal with these issues by using different matching techniques on both aggregate and individual-level data, which generate the best possible estimates of the causal effects of CCTs on election outcomes, considering the intrinsic limitations of an observational study.

The question of whether CCT beneficiaries shift towards the incumbent in each election, irrespective of who the incumbent is, speaks to the broader point of long-term effects of social policies and carries important implications for how we interpret CCTs. If these programs create long-term partisan attachments and an electoral advantage for the originator of such policies that extends over time, a case could be made that they are, in fact, creating captive clienteles that hinder political competition and ultimately reduce electoral accountability. On the other hand, if any incumbent can benefit from CCTs, the implication would be that these policies are not different from any other form of retrospective economic voting. Retrospective voting—except if extremely myopic and short-term (Bartels 2008)—should help increase electoral accountability.

CCTs in Brazil

The BFP continues to evolve, but by 2005 it had already acquired its current contours. As the main component of a larger umbrella program called *Fome Zero* (Zero Hunger), it pays a variable benefit to families with income per capita under US\$70/month with children under 15 or expectant mothers and a flat benefit to extremely poor families regardless of any children. Benefits can add up to—but are usually much less than—US\$120/month.

Municipalities—autonomous political units—enroll potential beneficiaries and enter their information into a national database, which is then centrally verified. 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. The program is audited by the comptroller general's randomized auditing of federal transfers to municipalities (Ferraz and Finan 2008) and by the constitutionally independent auditing authority.

When Lula took office in early 2003, the federal government already administered several different CCT programs inherited from the preceding Cardoso (PSDB) administration. In 1996, the government had begun a pilot program to remove children from unhealthy work in the coal industry by paying families a stipend and subsequently expanded the program into other pockets of child labor.³ In 1997, inspired by the experience of local governments throughout the country, the government began supporting poor municipalities' social programs that imposed education-related conditions on beneficiaries. This initiative only really left the books in 1999 and was restructured and expanded in 2001 to become *Bolsa Escola*. Less than a year later, and under a different ministry,⁴ the government started *Bolsa Alimentação*, a cash transfer program that imposed health-related conditionalities.

In 2003, the recently inaugurated Lula government added a new program called *Cartão Alimentação* and later that year began unifying all CCT programs under the newly created SDM, which would now be responsible for maintaining a master database of all assisted families (*Cadastro Único*). This database was used not only to administer the CCT program but also served as the platform for planning and implementation of other federal and local targeted policies.⁵ The old programs were gradually phased out as the BFP expanded; although 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.⁶ Some cities and states have added programs of their own, many of which make use of the administrative structure of the BFP and pay benefits through the SDM's payment system.

By October 2006, the BFP covered just over 20% of the entire population of the country—more than 40 million people in 11 million families, and it has since expanded

³This program still exists, but it focuses on educational activities and no longer pays cash benefits.

⁴*Bolsa Escola* was under the Education Ministry, whereas *Bolsa Alimentação* was under the Health Ministry. Another benefit called *Auxílio Gás* was administered by the Ministry of Mines and Energy. Although it is sometimes bundled together with the previous two, it was a targeted substitute to a general cooking-gas subsidy, did not impose conditions, paid very small bimonthly benefits, and, by design, was targeted at families *already covered* by the other programs. For these reasons, I excluded it from the analysis. Results presented in this article do not change if *Auxílio Gás* is included.

⁵This was something that had been contemplated under Cardoso—under the *Projeto Alvorada*—but was never effectively carried out (World Bank 2001).

⁶The decision to unify programs was more than just a managerial issue, and was the product of protracted struggles within the government. Gomez-Bruera (2012) details part of this process.

to 13 million families. Almost 60% of survey respondents at the time of the 2006 election reported knowing somebody who received the benefit (Vox Populi 2006). In another survey, also carried out close to the 2006 election, 34% of respondents chose “the fight against poverty” as the most successful policy out of 15 possible policy areas during Lula’s first term, with “none of the above” coming a distant second. An impressive 67% approved of the government’s policies to “fight hunger and poverty,” and although support dropped with increases in income, just under 60% of those in the highest income category approved of these policies (Ibope 2006).

By 2010, support for CCTs was even more pronounced. After the election, 86% of survey respondents approved specifically of the BFP, with only 2% not expressing an opinion (Ames et al. 2010). Furthermore, six months before the election, 76% understood the BFP as a federal government program, and at election time, this figure had risen to 84% (Ames et al. 2010). Perhaps not surprisingly, Dilma Rousseff—Lula’s anointed successor—cruised to victory, obtaining a similar vote share and geographic vote dispersion as her predecessor.

Inferring CCT Electoral Effects from Aggregate Electoral Data

Assessing CCTs’ electoral effects might seem like a straightforward empirical enterprise. After all, one need only to compare the electoral behavior of similar beneficiaries and non-beneficiaries. Brazil’s CCT programs, however, included no randomized pilots, and because they have been generally well targeted, it is hard to find groups that differ on coverage but are otherwise similar.⁷

Given these limitations, the approach here is to create the best possible conditions in which to infer the causal effects of CCT programs on the electoral support for incumbent party presidential candidates. This was done by employing statistical techniques that attempt to create comparable “treatment” and “control” groups *ex post facto*. These techniques rely on untestable assumptions, so to improve confidence in the results, I show that the analyses of different types of data yield very similar results.

Operationalizing CCT Coverage. The municipality is the operational unit for all CCT programs in Brazil,

⁷Interviews with former bureaucrats from both government and international organizations that were privy to the program implementation indicated that in Brazil, there was strong resistance to building evaluation mechanisms into the BFP. This is in contrast to Mexico, where program evaluation was central to *Progres*a from the start.

and programs have relied, at least partially, on geographic proxying to determine eligibility. Hence, I operationalize CCT coverage in terms of both scope and expenditures in each of the country’s more than 5,500 municipalities.

CCT Scope was defined simply as the share of families in each municipality covered by CCT programs. Whereas 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. Although I simply added the number of families in each program, this combined figure overestimates the total number of beneficiaries in 2002. At the time, *Bolsa Alimentação* reached close to 1 million families and *Bolsa Escola* just over 5 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 “leftover” coverage remained in *Bolsa Escola*, *Bolsa Alimentação*, and *Cartão Alimentação*.

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.⁸ 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. The number of families covered increased 80% between 2002 and 2006 and an additional 18% between 2006 and 2010.

Although both scope and expenditures in existing programs were far from negligible in 2002,⁹ CCTs were a relatively low-profile policy at the time and did not receive much media coverage or academic scrutiny prior to the 2006 election.¹⁰ In hindsight, it is not obvious why such little attention was paid to CCTs in 2002. The most likely reasons were a combination of the ultimate defeat of the incumbent party’s candidate and the lack of coordination and visibility of the CCT initiatives under one strong brand.

Data and Methods. I estimated the effects of CCT expenditures and scope on incumbent vote share by OLS and by employing a matching procedure for continuous treatment variables.

⁸All monetary values are adjusted for inflation using the IGP-M index and are reported at 2010 values.

⁹Neither 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).

¹⁰Lindert and Vicensini (2010), for instance, report approximately 200 news articles in six selected outlets in the quarter of the 2002 election, whereas there were more than 800 such articles in the quarter of the 2006 election.

The OLS estimates for each year were obtained by simply regressing the vote share of the incumbent party's presidential candidate on each of the two operationalizations of CCT coverage, controlling for state- and municipal-level variables.¹¹

Controls included the vote share obtained by the party's presidential candidate in 1998 election, the last election prior to the existence of CCTs. As such, the regressions identify the association between CCTs and the change in vote share that occurred since the introduction of CCTs. This is an important point, as it implies that electoral results in each municipality are being compared with results in municipalities that have similar values in the other observed covariates and with the municipality itself before the CCTs could have had any impact. Also accounted for are the average growth in GDP per capita in the year preceding the election.¹² Dummy indicators account for the presence of mayors and governors of the two major parties, share of the population that is non-white, as well as the human development index (HDI-M) and distance to the capital city.

Simply including control variables in a multiple regression does not adequately deal with the nonrandom assignment of coverage. If CCTs were perfectly targeted, all poor municipalities would have high coverage, all rich municipalities would have low coverage, and there would be no contrast between similar municipalities with different coverage levels. CCT effects on vote share would not be identifiable, and OLS would provide an estimate of this effect driven entirely by model assumptions and not by actual contrasts in the data. Moreover, development levels can also affect electoral results through other mechanisms besides CCTs. If these links are ignored, estimates might unduly attribute the effect of these other mechanisms to CCTs.

Fortunately—from an analyst's perspective, that is—CCT targeting is good but 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. To this end, I approach CCT coverage as if it were a continuous “treatment,” matching municipalities on relevant pretreatment covariates to gauge the effect of CCTs by comparing municipalities only with otherwise similar counterparts.

¹¹CCT data were provided directly by the SDM for years prior to 2004, and later data are available on the ministry's website. All other municipal-level data were obtained from public data sources maintained by the Institute of Applied Economic Research (IPEA), the National Geographic Institute (IBGE), and the Superior Electoral Court (TSE).

¹²Growth figures at the municipal level for 2010 are not yet available, so I used growth in 2009.

Granted, random assignment of municipalities to different levels of BFP coverage would be the best way to assess any the program's electoral effects. However, there were no pilots, and the program was phased in rather quickly over the whole country.¹³ As a next best alternative, I employ a generalized propensity score (GPS) matching approach to attempt to hold fixed the connection between development and CCT coverage (Imai and van Dyk 2004) and focus on the independent contribution that CCTs make to incumbent candidate vote share by contrasting municipalities that differ with respect to CCT coverage but that have similar levels of development, size, political background, and other observable covariates.

This matching procedure requires stratifying observations into similar groups. The important assumption here is that within groups, variations in coverage are as good as random. Following (Imai 2004), this was done by computing propensity scores—the treatment levels predicted by pretreatment covariates—and then partitioning the data into strata where all observations have similar propensity scores.¹⁴ The treatment effect is calculated within each strata by a simple linear regression of incumbent vote share on the treatment variable (CCT coverage), controlling for the propensity score itself and growth rates.¹⁵

GPS matching does not overcome the possibility of omitted variable bias. Nonetheless, there are significant advantages to this approach over simply estimating an OLS regression. At a minimum, the matching procedure ensures common support (i.e., that only similar municipalities are actually compared) and relaxes the assumption that causal effects are the same across all types of municipalities (i.e., it allows for heterogeneous treatment effects). These advantages are not trivial, as they make matching procedures less model dependent than OLS regressions. Moreover, the combination of both approaches, as employed here, allows for even greater robustness; regression serves as a correction for potentially faulty matching (Morgan and Winship 2007, 156), and matching serves as a protection from model misspecification (Ho et al. 2007).

¹³I later explore the fact that a few municipalities did not have coverage by the time of the 2002 elections, but the small number of municipalities involved does not provide much leverage.

¹⁴Although Imai and van Dyk (2004) show that partitioning the data into five subsets is enough, I conservatively split the data into more groups. Results hardly change using different numbers of subsets or partitioning the data by propensity score quantiles or in fixed-width strata.

¹⁵Results controlling for growth or not are substantively very similar.

Matching will be a better statistical fix for non-random assignment the better the treatment can be predicted from observed covariates (Morgan and Winship 2007, 114). This is particularly relevant in the present case because there are strong theoretical and empirical reasons to believe the specific propensity score regression employed can ensure proper identification of CCT effects.

Much of the strength of the empirical strategy relies on the inclusion of the government’s official target of coverage for each municipality in the propensity score regression. This target was computed once by the government, based on the 2006 national household survey. 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 slowly over time.¹⁶ Besides the target, the most powerful predictor of treatment is precisely the HDI-M, to which I added squared and cubed terms to improve the fit of the propensity score regression at extreme levels of coverage, as well as the same control variables that were included in the OLS models.¹⁷ State fixed effects were added to account for variation not captured by the substantive variables and only marginally improved the fit of the regressions. In the end, the regressions predicting treatment in each year had R²s of at least 0.8 and exhibited good fit across the full range of the treatment values. As a result, coverage effects are identified based on small deviations from the predicted levels of coverage, and it is reasonable to treat variations in CCT coverage within each strata as random.

Results

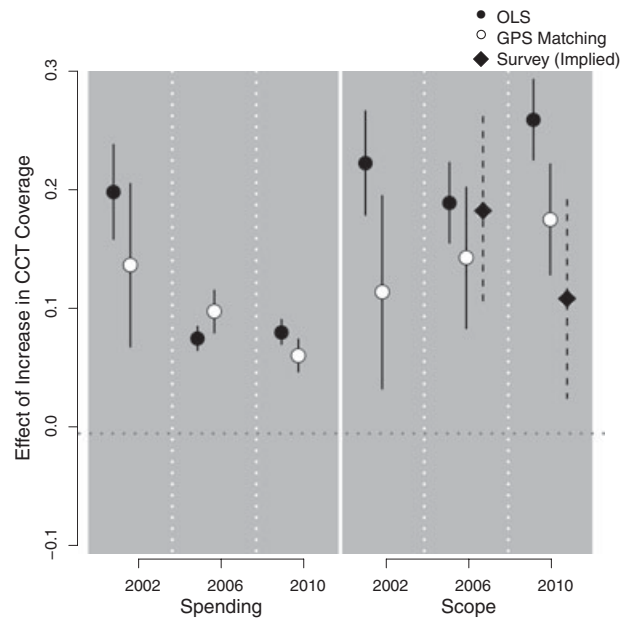
Figure 1 reports both OLS and GPS matching average treatment effects.¹⁸ In all elections, and across all specifications, CCTs are associated with higher incumbent candidate vote share effect in all three elections. Results are statistically significant and substantively meaningful when plausible changes in coverage are considered.

¹⁶See Fried (2012) for more details on how targets are set. No systematic political factor seems to drive deviations from these targets.

¹⁷Analysis of the propensity score regression (in the supplemental materials) shows that coverage was *not* contingent on whether the mayor and governor belonged to the main opposition or government party, respectively.

¹⁸Estimates from individual-level data reported in the figure are discussed below. The graphical depiction of regression results is inspired by Kastelec and Leoni (2007). Figure reports only estimates for the causal variable of interest. Complete estimates are provided in the supplemental materials.

FIGURE 1 CCT Effects on Incumbent Candidate Vote Share (2002–10)



Note: Figure shows estimates of the impact of CCTs on incumbent candidate electoral performance. OLS and GPS matching results rely on municipal-level observations where the dependent variable is incumbent party candidate vote share, spending is measured in R\$100 inflation adjusted, and scope is measured as share of total families in the municipality covered by CCTs. Survey estimates are based on individual-level data—as discussed later in the text—and are available only for 2006 and 2010. Ninety-five percent confidence intervals are shown about the estimates.

For expenditures on CCTs, both GPS¹⁹ and OLS estimates show effects declining over time. The more reliable GPS matching estimates indicate that a R\$100 increase in yearly per capita coverage led to as much as a 15 percentage point increase in vote share in 2002, but this fell to only 6.5 percentage points in 2010. Given that expenditures have increased much more than scope, these results are compatible with the existence of declining returns to expenditures.

For scope of CCT coverage, we see more divergence between GPS matching and OLS. The former is always smaller than the latter, and the difference is statistically significant in 2010. GPS matching estimates support the conclusion that electoral returns for scope have remained roughly constant in the three elections. Each additional percentage point in the share of families covered by CCTs increased the incumbent party candidate’s vote share by about 0.12 percentage points in 2002 and by 0.18 percentage points in 2010. The difference across years is not

¹⁹I refer in the text to average effects across strata. Effects by strata are reported in the supplemental materials.

statistically significant and is probably at least partially driven by two limitations in the data, as I explain below.

First, effects for 2002 are stronger if estimated using solely *Bolsa Escola* or *Bolsa Alimentação* (0.15, with $se = 0.059$; 0.27, with $se = 0.086$), suggesting that summing coverage of the two programs introduces measurement error that attenuates estimated effects. Second, CCT effects in 2006 and 2010 would probably be smaller if we could discount changes in voting patterns that would have occurred simply because the PT went from being the challenger to the incumbent (Canêdo-Pinheiro 2009; Zucco 2008). Including the vote share of the PT's presidential candidate in 2006 as a control in the estimation of the CCT effects in 2010, for instance, reduces estimates of CCT effects from 0.18 to just about 0.08 (with $se = 0.023$). While this would eliminate the incumbency effect, it understates the true CCT effects because 2006 electoral results are, themselves, partially produced by CCTs.

I analyze the implications of these results later in the text, but the message is clear. CCTs have had a substantively meaningful electoral effect in each of the past three presidential elections, including in 2002 when CCTs went unnoticed. This result remains robust even when effects are estimated across municipalities that are otherwise similar and seem not to depend on party or personality of the incumbent candidate.

Inferring CCT Electoral Effects from Zero-Coverage Cases

Given the lack of attention paid to CCTs in 2002, the strong results are quite surprising and demand further corroboration. Some empirical leverage can be obtained from the fact that seven municipalities did not have any cash-transfer coverage in 2002. These are all very 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 can be obtained by comparing these municipalities to similar municipalities that had coverage.

I chose the set of municipalities with which to compare the zero-coverage cases from municipalities whose CCT scope in 2002 was “typical,” as defined as lying between the 25th and 75th percentile. Additionally, I only matched “untreated” municipalities to others inside the same state and to municipalities that were similar on predicted coverage, HDI-M, population, and past incumbent electoral performance. Because of the very small sample

size, I allowed for a four-to-one ratio of untreated to treated municipalities to ensure the best possible balance between the two sets of municipalities (Ho et al. 2007). The resulting sample was very similar in all the (few) observed covariates used.

The incumbent party candidate's vote share in the two rounds of the presidential election in municipalities with no coverage was 22.9% and 46.3%, respectively. His vote share in similar municipalities with close to average CCT coverage was 28.7% and 53.9%. After controlling for the incumbent party's candidate in the previous election, the estimated differences between no-coverage cases and some-coverage cases was 5.4 percentage points for the first round (p -value = 0.3) and 6.7 percentage points for the second round (p -value = 0.08).

Given that the average CCT scope among treated municipalities in this small sample was 0.18, the first round estimates correspond to an average effect of roughly 0.3 in the metric reported in Figure 1. This imprecisely estimated effect is larger than OLS and GPS estimates presented earlier. The bluntness of the measure of treatment and the small number of observations are clearly drawbacks, but when taken in context, it adds to the evidence presented earlier that CCTs had electoral effects even before they caught observers' attention.

Inferring CCT Electoral Effects from Individual-Level Data

Up to this point, both electoral behavior and CCT coverage have been observed at the municipal level. However, the ultimate test of CCTs' effects on the voting behavior of beneficiaries must rely on a comparison of individuals who receive the benefit with otherwise similar individuals who do not. A positive effect at the individual level would significantly add confidence that the distribution of targeted benefits can, indeed, increase support for incumbents even in the absence of clientelistic ties between voters and politicians.

Although surveys provide access to individuals' thoughts, they rely on stated—as opposed to revealed—preferences. To minimize the discrepancies between the two, I relied on surveys taken close to the actual election date. Unfortunately, no surveys asked any CCT-related questions close to the 2002 election.²⁰ For 2006, the best available option is the first Vox Populi survey probing voter intention for the second round of the presidential

²⁰This lack of surveys is, in itself, evidence that there was little interest in the topic until the 2006 elections.

TABLE 1 Electoral Effect of Receiving a Benefit (2006 and 2010)

2006			2010			
Prob. Vote Lula		Risk Ratio <i>p-value</i>	Family Income (in Min. Wages)	Prob. Vote Dilma		Risk Ratio <i>p-value</i>
Not CCT	CCT			Not CCT	CCT	
0.61	0.79	1.32 <0.01	Less than 1	0.53	0.64	1.21 0.01
0.61	0.79	1.31 <0.01	1–5	0.46	0.57	1.25 <0.01

Note: Risk ratios refer to the increase in probability of voting for the incumbent associated with participation in a CCT. Data are from surveys taken close to each election (Ames et al. 2010; Vox Populi 2006). Both surveys were preprocessed to ensure balance between CCT recipients and nonrecipients, using genetic matching (Diamond and Sekhon 2005). Predicted probabilities were based on logit regressions. See text and supplementary materials for details.

election (Vox Populi 2006).²¹ Conducted only a few days after the inconclusive first round, it interviewed a total of 2,005 respondents.²² For 2010, I used the last wave of the Brazilian Electoral Panel Survey (BEPS), which interviewed a sample of 1,221 respondents one month after the election (Ames et al. 2010).²³

This analysis seeks to determine whether CCT beneficiaries were more likely to have voted for Lula and Dilma in 2006 and 2010, respectively. Respondents in higher-income brackets do not contribute to the identification of this possible CCT effect, and beneficiaries and non-beneficiaries differ in many more respects besides participation in the BFP. For this reason, I first balanced the data by matching treated and untreated individuals (i.e., beneficiaries and non-beneficiaries) across a number of individual-level pretreatment variables, as proposed by Ho et al. (2007).

Table 1 presents the predicted probabilities of voting for the incumbent presidential candidate for CCT recipients and non-recipients, for income brackets that are likely to include most beneficiaries.²⁴ Predicted probabilities were estimated on the balanced sample based

on a logit regression of vote for incumbent candidate on CCT participation, income, age, education level, gender, region, municipal characteristics, and economic growth rates, as well as the propensity score of the observation.²⁵

In 2006, the effect of the program among the poorest voters amounted to an increase of about 0.18 (or 32%) in the probability that a voter would select the sitting government’s presidential candidate, and the effect is almost identical in the next income bracket. Effects in 2010 were smaller but still substantively and statistically significant, suggesting an increase of about 0.11 (or 21%) in the probability of voting for the incumbent candidate. These are also in the vicinity of results found in Mexico and Uruguay, reported earlier in the article.

Referring back to Figure 1, the estimates obtained for CCT effects at the level of the individual imply effects similar to those obtained at the aggregate level. Individual-level estimates for 2010 are smaller than those for 2006 and slightly smaller than the aggregate estimates for the same year, but differences between them are within the conventional confidence intervals of all estimates.²⁶

Additional evidence indicates that the link between CCTs and voting behavior is primarily a retrospective one. It could very well be the case that beneficiaries throw their support behind the candidate most likely to continue the policy instead of rewarding the incumbents for

dence between income brackets and eligibility threshold should not be expected.

²⁵After some experimentation, genetic matching was ultimately used (Diamond and Sekhon 2005). Results changed very little with other matching methods. See the supplemental results appendix for additional details.

²⁶Smaller effects at the individual level than at the aggregate level are compatible with the absence of backlash against the program on the part of the wealthier voters, but more research is necessary to confirm this result.

²¹The 2007 edition of LAPOP is an alternative. However, even though this survey has been used by others to analyze related questions (Bohn 2011), it was conducted six months after the election, and recall for Lula is 20 percentage points higher than actual electoral results.

²²The survey question about CCTs asked whether respondents or somebody in their household participated in “Bolsa Família or other similar programs.” The recall question asked for which candidate respondents voted in the first round of the 2006 election.

²³The question about CCTs asked whether respondents or somebody in their household participated in the BFP. The recall question asked for which candidate respondents voted in the first round of the 2010 election.

²⁴Both surveys inquired about “family income,” but BFP eligibility is based on per capita income. For this reason, direct correspon-

past transfers. That the four main candidates in the 2006 presidential election—who 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 2010 election suggested eliminating the program (The Economist 2010), suggests this is not the case.

Data from 2010, in fact, reveal that voters were aware of this convergence and therefore not likely to be driven by fear of termination of the programs. By election time, only an extremely small percentage of voters was unsure about where the candidates stood on the issue, and less than 10% of voters thought any of the three main candidates might shrink or end the program (Ames et al. 2010). Candidates were vocal in their support of CCTs and succeeded in communicating this to voters, which is not surprising given that the issue was featured heavily in the campaign.

CCTs and Attachment to Parties

All the evidence thus far points towards a positive effect of CCTs on the electoral performance of the incumbent party presidential candidates. But have CCTs affected parties' electoral performance more broadly?

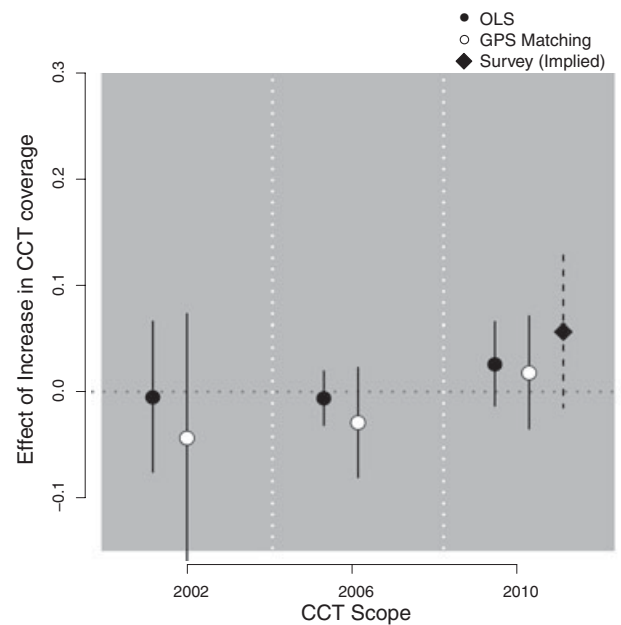
To answer this question, I ran the same aggregate and individual-level analysis reported earlier using party-based dependent variables in the place of presidential election variables. More specifically, I used the vote share of the *party* of incumbent president in legislative elections as the dependent variable in the aggregate data analysis,²⁷ and at the individual level I used identification with the party of the incumbent president.²⁸ As before, I report OLS and GPS matching estimates from municipal-level electoral data and logit estimates after preprocessing individual-level data to ensure balance on observed covariates.

Figure 2 reports the results and shows that CCT scope had no effect on incumbent party vote share or identification. Null results should be taken with skepticism when invoked to support the analyst's hypothesis.

²⁷Lower house vote share is the best indicator of *party* electoral performance because party candidates can receive votes in all municipalities and compete against hundreds of candidates from dozens of parties. In contrast, parties do not field mayoral candidates in every municipality and do not field senatorial and gubernatorial candidates in all states.

²⁸Individual-level data for party identification are only available for 2010 and were taken from the first wave of the BEPS panel survey that went to the field in March 2010 and interviewed a national representative sample of 2,482 voters.

FIGURE 2 Effects of CCT on Party Vote Share and Party Identification



Note: Figure reports the effects of changes in the scope of CCTs on party performance. The dependent variable was incumbent party vote share in legislative elections in the OLS and GPS matching analyses and identification with the incumbent party in the survey analysis. Ninety five percent confidence intervals about the estimates are shown.

These results, however, are compatible with evidence presented elsewhere. In a survey of federal-level legislators in 2009 (Power and Zucco 2011), over 99% of respondents thought the BFP had had at least some positive effect on Lula's reelection in 2006, but 77% of all respondents and 61% of members of the PT stated it had no impact (positive or negative) on their own election to Congress. The lack of "party effects" is also compatible with evidence from focus groups in the Northeast in which participants rarely ever mentioned the PT as being responsible for the program (Hunter and Sugiyama 2009).

This result could just reflect the notorious weakness of Brazilian parties (Mainwaring 1999). If parties were stronger to start with, they might have been able to find ways to claim credit and enjoy long-term benefits of CCTs. Recent evidence, however, has shown that despite low levels of party identification, party labels—at least for identifiers with the PSDB and the PT—are far from irrelevant (Samuels and Zucco 2012). Evidence from Mexico—where parties are arguably much stronger—suggests that CCT beneficiaries also switch from one incumbent to the next in subsequent elections (Diaz-Cayeros, Magaloni, and Estevez 2009). In this context, it is quite plausible

that the lack of persistent party effects of CCTs extends beyond the Brazilian case.

Conclusion

This article presented estimates of CCT electoral effects that point to a significant boost to incumbent party presidential candidates in the last three Brazilian elections. This boost is constant with the levels of coverage of CCTs and decreases in the total amount of resources transferred to beneficiaries. Aggregate data estimates are confirmed by contrasting the few zero-coverage cases in 2002 with similar municipalities where CCTs were present and with the analysis of individual-level survey data. In addition, CCTs do not generate any noticeable impact on partisanship, be it in terms of the party's performance in legislative elections or in terms of party identification.

The consistency of results over time and under different incumbent parties has important implications for Brazilian politics, the politics of CCTs, and for the broader issue of long-term impacts of social policies.

CCTs have influenced the Brazilian elections since before these policies first caught observers' attention and in periods of poor and good economic performance alike. But more importantly, although CCTs have helped incumbents, there is no evidence as of yet that they can radically reshape the political landscape.

First of all, program coverage cannot expand indefinitely, and simply increasing expenditures will have limited effects, as seen by the declining return to expenditures. Adding to this limitation, CCTs benefited the incumbent party candidate in 2002, but he still lost. Serra would have done better if CCTs were as significant as they were four years later but would probably have lost the election regardless.

The core point, however, is that CCTs benefited a different incumbent in the next election, with presumably a significant number of the same voters who had swung Serra's way in 2002, swinging Lula's way in 2006. This interpretation of the results suggests that a future non-PT incumbent will also receive a boost from the BFP, though we cannot know this for certain until at least 2018.

Finally, there is evidence that CCTs have not created "partisan clientele." This result is compatible with recent microlevel research in Brazil that finds that even where vote buying is rife, the BFP is not perceived by its beneficiaries to be another instrument of vote buying (Sugiyama and Hunter 2013). It is also supported by the lack of association between control of local offices and dispensation of CCT benefits (Fried 2012). In this context, CCTs do not seem like a tool with which to build overall politi-

cal dominance. Instead, they are simply an efficient new mechanism to strengthen the incumbent's advantage in poorer regions of the country—an advantage that already existed before CCTs (Zucco 2008).

On a more general level, the fact that beneficiaries are rewarding incumbents and not program initiators means that effects of social policies get absorbed into retrospective economic evaluations in much the same way in which voters would react to macroeconomic conditions. These results support the view that social policy effects are *continuing* but not *long-lasting*, in the sense that the policy innovation per se does not seem to create stronger and more durable links between voters and parties that can lead to long-term political realignments. Such a conclusion is also backed by results previously found in Mexico, where CCT beneficiaries were also more likely to support incumbents in both 2000 and 2006, even though the incumbents were from different parties.

The short-term proincumbent boost—which is also compatible with results found in previous studies—is, nonetheless, of great substantive importance because it provides enough enticement for vote-seeking politicians to support these programs. CCTs can be implemented at different levels of state capacity, with different long-term objectives, and in widely varying scales. There is evidence that CCTs can help reduce extreme poverty rates, reduce inequality (Barros et al. 2006; Soares et al. 2010), reduce malnutrition, and, perhaps, help keep children in school. So even though there is still debate about the long-term consequences of these programs, and despite the fact that they are only one of the elements of an effective social policy (Diaz-Cayeros and Magaloni 2009), CCTs have made it onto an often too-short list of policies that are politically viable and are also socially desirable.

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Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's web site:

Figure A.1: Families Covered by CCT Programs (2002–2010)

Figure A.2: Voters' Perceptions of Candidates' Preferences About CCT

Figure A.3: Effects of CCT Scope on Vote for Incumbent Party Candidate by Strata (2002–2010)

Figure A.4: CCT Expenditures and Vote for Incumbent Party Candidate by Strata (2002–2010)

Figure A.5: Effect of Control of Local Power on CCT Coverage and Incumbent Vote-Share

Table A.1: Extended OLS Estimates for Determinants of Incumbent Party Candidate Vote-Share (OLS)

Table A.2: Predicting Coverage (First Stage in GPS Matching)

Table A.3: Generalized Propensity Score Matching Estimates

Table A.4: Pre- and Post-Matching Balance Statistics