



## Global Development Network Working Paper Series

# **Are Conditional Government Transfers a Politically Acceptable Form of Redistribution?**

Cesar Zucco Jr., Juan Pablo Luna,  
Gokce Ozgen Baykal

**Working Paper No. 93**  
**December 2016**

# **Are Conditional Government Transfers a Politically Acceptable Form of Redistribution?**

**Cesar Zucco Jr.**

Getulio Vargas Foundation

**Juan Pablo Luna**

Pontificia Universidad Catolica de Chile

**Gokce Ozgen Baykal**

Development Analytics

## Abstract

This paper examines whether those aspects of government transfers that do not affect actual levels of redistribution affect support for such transfers. We employ survey experiments in Brazil, Chile, Uruguay, and Turkey to determine whether, and in what contexts, making government transfers conditional on behavior of beneficiaries increases support for these transfers among non-beneficiaries. In the experiments, we prime some respondents to think of themselves as “different” from potential beneficiaries in regional and ethnic/racial dimensions, and manipulate the type of conditionality imposed on beneficiaries. We seek to determine whether a “conditionality premium” exists among non-beneficiaries, and whether it is affected by how similar non-beneficiaries perceive themselves to be relative to beneficiaries of government transfers. Results show that conditional transfers are generally more popular than similar unconditional ones, and also support our “otherness hypothesis”, whereby the “conditionality premium” is greater when non-beneficiaries perceive themselves to be — or are primed to think that they are — different from beneficiaries in non-economic terms. This is the case in the more heterogeneous countries that we examine (Brazil and Turkey). The fact that some results only seem to hold in heterogeneous societies suggests that conditionalities might be more necessary in such environments.

## Introduction

How do different characteristics of government transfers affect the levels of support for these programs? In particular, can ostensibly non-redistributive aspects of government transfers affect the way the policies are evaluated by the population? Most theoretical approaches and empirical analyses of political redistribution rely on stylized depictions that portray redistributive policies as a single tax-and-spend decision. Political redistribution, however, is carried out through a series of different decisions and it is fair to say that almost all government actions have redistributive implications. While the theoretical simplification of treating political redistribution as a bundle of policies has been an extremely fertile area of research, leading to many insights in the political-economy literature, it has prevented us from considering how support for redistribution among non-beneficiaries varies across different policy instruments. We argue that such an understanding is of utmost practical importance for policymakers, especially those in highly unequal and resource-constrained conditions that characterize much of the developing world.

We know that individual and societal characteristics are strong determinants of preferences vis-à-vis redistributive policies. In this paper, we focus on the fact that individual assessment of government transfers varies across non-redistributive aspects of these policies, implying that the basic political-economy assumption that preferences over redistribution are a function of income cannot fully explain why individuals prefer one type of transfer over others. Nor can polity-level characteristics fully explain these preferences. Our claim is that variation across characteristics of the transfers themselves explain at least part

of the support for concrete redistributive policies.

We focus on conditional cash transfers (henceforth CCTs) — a class of non-contributory social policies that seek to incentivize usage of public services by paying poor families with children to do so (Fiszbein, Schady, Ferreira, Grosh, Kelleher, Olinto & Skoufia 2009). In essence, CCTs typically provide small monthly payments to participant families whose children meet health requirements (such as regular checkups and up-to-date immunizations) and educational requirements (such as enrolling and attending school). From their origins in the mid-1990s as a local-level policy innovation in a handful of Brazilian municipalities (Amaral & Ramos 1999, World Bank 2001), CCTs have expanded to become large-scale and highly visible national-level policies. Mexico's Oportunidades/Progresa and Brazil's Bolsa Familia Program (BFP) are the largest of these, but by no means are they the only examples; CCTs are the norm in dozens of countries around the world.

CCTs can be highly redistributive (in the sense that they benefit the very poor) and also fairly popular even among people of higher socio-economic status (SES). In fact, evidence from Brazil suggests that CCTs may be much more popular than other less redistributive transfers, and also more popular than contributory social policies. Several mechanisms could possibly account for this. CCTs, for instance, might enjoy a better "cost-benefit" relationship than other types of transfers, because they are more focused on the needy, and pay relatively small benefits to a large number of beneficiaries. In addition, they are usually closely monitored and evaluated and are, therefore, less likely to suffer leakage or corruption than other transfers. Although the better-off should

generally be opposed to redistribution because they have to bear its costs, they too would, arguably, prefer more efficient transfers (i.e. transfers that achieve most redistribution with the resources that are actually spent) over less efficient ones.

In this paper, however, our focus is on whether CCTs are popular among non-beneficiaries because they are conditional. The idea that conditional transfers enjoy greater support than non-conditional ones is straightforward to the point of often being assumed to be true (Fiszbein et al. 2009). However, the literature has not established the mechanisms that connect the imposition of conditionalities with support among the better-off, and has also not determined the conditions under which conditionalities can affect support for transfers. We find that, on an average, conditional benefits do enjoy greater support but that this "conditionality premium" is concentrated on those of higher SES.

We explore the idea that the conditionality premium is derived from the "otherness" problem which is a well-documented tendency of individuals to resist redistribution towards beneficiaries perceived as being different from them. This possibility is examined through our survey-experiments carried out in four different countries: Brazil, Chile, Turkey, and Uruguay. We find that in the more heterogeneous countries, the conditionality premium is, in fact, larger when actual and perceived differences between beneficiaries and non-beneficiaries are highlighted. This not only sheds light on the mechanism behind the greater support for conditional transfers relative to unconditional ones, but suggests that conditional transfers can help overcome resistance against redistributive policies in heterogeneous societies.

In Section 1, we briefly make a theoretical and empirical case for the need to “unbundle” the idea of redistribution. Section 2 presents our hypothesis on the connection between conditionalities and the support for CCTs. Section 3 looks to derive plausible micro-foundations by describing the utility function and decision problem of a typical non-beneficiary. Section 4 provides a brief overview of our general empirical approach. This is followed by a detailed description and analysis of the three studies presented in Sections 5, 6, and 7. Section 8 is dedicated to a more in-depth analysis of the “Turkish anomaly”, which is identified in the previous empirical sections. Finally, Section 9 summarizes and discusses the results.

## Unbundling Redistribution

Despite mixed empirical evidence at the macro level, most of the academic literature on redistribution builds on the Meltzer-Richard assumption — that, in a democracy, voters who benefit or stand to benefit from redistribution will vote for candidates who promise or effectively carry out redistributive policies (Meltzer & Richard 1981).<sup>1</sup> Conversely, citizens who are required to fund redistributive programs will vote against politicians who initiate such policies, and possibly even support coups against them (Boix 2003, Acemoglu & Robinson 2006).

At the individual level, direct testing of the Meltzer-Richard assumption has led to mixed results. Even though much of the literature finds that richer people are indeed more averse to redistribution than their poorer counterparts, other individual characteristics should not be discounted. Race and gender consistently affect support for redistribution, as do one’s views on the causes of poverty and personal experiences with economic volatility, misfortune, and social mobility.<sup>2</sup> Recent work examining the developing world has led to further questioning of the Meltzer-Richard assumption of economic

---

1. Evidence suggests that the median voter is typically a net payer of taxes (Milanovic 2000), which would muddle the otherwise clean Meltzer-Richard theoretical predictions. The neat and simple majoritarian system posited in the basic theoretical model ignores more complex real-world institutional arrangements. Electoral rules and geographic dispersion of voters’ (Jusko 2010), policy implementation difficulties such as the need for constant redistribution (Przeworski 2010), voter heterogeneity within the same income categories (Mares 2003), malapportionment over-representing conservative voters and allowing them to block redistributive policies (Ardanaz & Scartascini 2011), differential turnout (Kenworthy & Pontusson 2005) and other institutional factors can work against redistribution. And if these were not enough, the raw power of those footing the bill would serve as a last resort impediment to equalization of incomes (Boix 2003, Acemoglu & Robinson 2006).

2. (For a comprehensive review of this literature, see Alesina & Giuliano 2009).

self-interest and its applicability in all policy contexts. There is little evidence that the poor are more likely to vote for higher taxes or for left-leaning parties, at least in Latin America and the United States (Kaufman 2009). Contrary to cross-national comparisons using the World Values Surveys (Alesina & Giuliano 2009), Latinobarometro surveys show no relation between respondents' beliefs about the unfairness of the distribution of income and preferences for higher taxes and more welfare spending (Kaufman 2009). In low-development and high-inequality countries, preferences for redistribution do not vary significantly with income (Dion & Birchfield 2010).

The literature does not discuss in any detail the actual policy instruments by which redistribution is implemented. Instead, redistributive policies are represented theoretically as decision regarding the aggregate amount of taxation and transfers. This is presumably a stylized depiction of the net effects of a bundle of policies that redistribute resources in society. Similarly, empirical work also relies on highly aggregated figures. Macro-level cross-national studies typically employ comparisons of pre- and post-tax distributions of income (e.g. Pontusson 2005), while work on individual support for redistribution relies on survey questions that tend to include some close variant of whether government should be responsible for remedying social inequity (e.g. Dion & Birchfield 2010).

In practice, however, redistribution occurs through a combination of different taxing and spending decisions, only some of which are direct transfers.<sup>3</sup> So what, if anything, does

3. In OECD countries, the size of public transfers can often be used as a proxy for the amount of political redistribution. In Latin America, in contrast, larger transfers or social spending do not necessarily mean more redistribution (Huber & Stephens 2012, Lindert, Skoufias & Shapiro 2006, ECLAC 2005).

the Meltzer-Richard assumption tell us about preferences over different transfers?

Spending decisions vary considerably with regard to how progressive — or redistributive — they are (Lindert, Skoufias & Shapiro 2006). Transfers that cost the same can be more or less redistributive depending on who funds them and who benefits from them. Redistributive transfers, therefore, involve higher net costs to the better-off than regressive transfers. This means that, based on the Meltzer-Richard assumption, we should expect support for a highly redistributive transfer (i.e. one with large net-cost to the better-off) to decline with income, and that the “rich” should support less redistributive policy instruments over more redistributive ones.

Preliminary data suggest that the Meltzer-Richard assumption is not particularly useful for exploring the possible variation of individual preferences over different policy instruments. Figure 1 shows support for CCTs by socio-economic status category (which is a proxy for income). This support does decline with income in Brazil and Uruguay, as predicted, but only in Brazil does it decline monotonically. In Chile, it declines slightly in the three lower categories but increases in the highest SES group. In Turkey, support for CCTs is high across the board; if anything, it, runs counter to the Meltzer-Richard assumption.

Data on preferences over different transfers are even more intriguing, though our preliminary evidence on this point is limited to survey results from Brazil for 2010 (Figure 2). Given the levels of income, regressive transfers such as public sector pensions (Secretaria de Politica Economica 2005), or unemployment benefits (a form of insurance and not exactly a transfer), enjoy much less support than non-conditional benefits to the elderly poor (Beneficio de Prestacao

Figure 1: Support for CCTs by Socio-Economic Status (2015/2016)

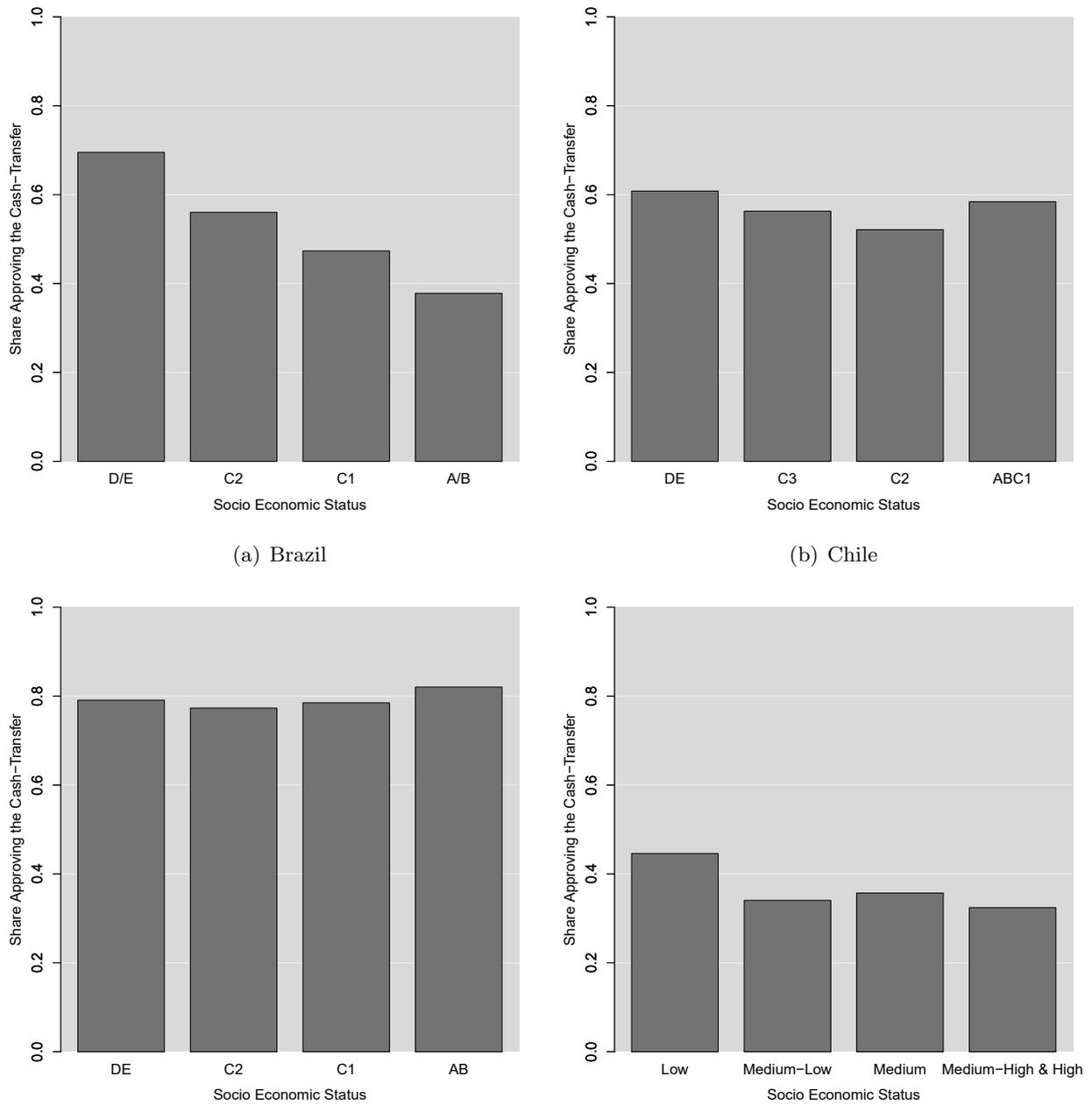


Figure shows shares of respondents who stated that they support or strongly support a hypothetical CCT in each country. Data are extracted from the first study described in detail in Section 5, and refer only to respondents who were asked to evaluate a conditional cash transfer. This amounts to half of the national representative sample in each country.

Figure 2: Support for Increasing Government Transfers in Brazil, 2010

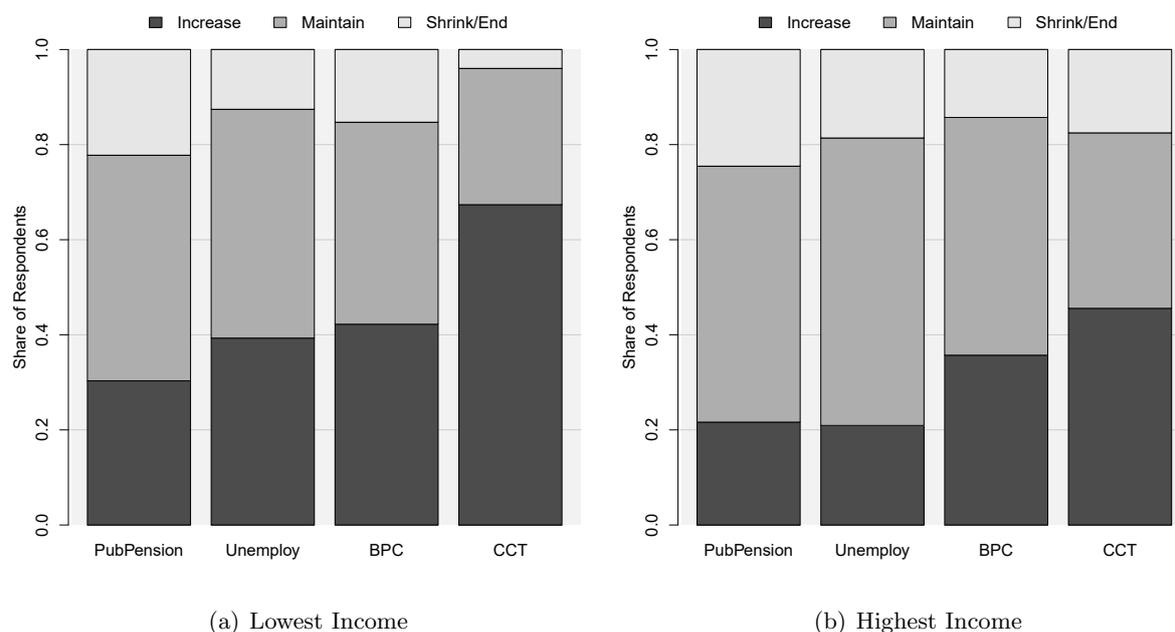


Figure shows respondents' answers to the question "What should be done with transfers given that higher transfers require higher taxes?" for four different types of transfers, in Brazil, in 2010: Conditional Cash Transfers, Beneficio de Prestacao Continuada, Unemployment Insurance, and Public Sector Pensions. Figures exclude "don't know" answers, which amounted to 3.4% for CCTs and between 16 and 17% for the three other policies. Figure (a) includes respondents who declared family income equal to or lower than one minimum wage (24% of the sample), and figure (b) includes only respondents who declared family income higher than five minimum wages (12% of the total sample). The wording of the CCT question was different from the wording for other transfers. Data are from a nationally representative survey (Ames et al. 2010).

Continuada, BPC) and CCT benefits geared towards poor families with children.

We conjecture that the Meltzer-Richard assumption is not the best way to think about variation in support for different transfers because, in practical terms, it is hard to determine the net costs of any government transfer. Spending patterns for each transfer are often fairly transparent, but it is typically next to impossible to determine who is contributing to the funding of each program.

We know, for instance, that social assistance in general, and CCTs in particular, is very well targeted at the lower quartile of the income distribution (Lindert, Skoufias & Shapiro 2006). There is evidence, not surprisingly, that CCTs' success in reducing extreme poverty rates also played a significant role in reducing inequality, even though CCTs were

not designed to reduce inequality per se (Soares, Soares, Medeiros & Osorio 2006, Neri 2008, Barros, Carvalho, Franco & Mendonca 2010). However, CCTs are usually funded by general revenue, which is raised through a wide collection of tax instruments. Some of these are direct and fairly transparent, such as income tax. But only the moderately well-off pay income taxes in Latin America, and governments throughout the region rely substantially on indirect and less transparent taxes on consumption, which tend to be more regressive.<sup>4</sup> Consequently, the beneficiaries of the transfers also contribute to fund these programs.

In this context, one way to make sense of the results in Figure 2 is that, because of the

4. The poor pay a larger overall share of their income in taxes because of relatively high taxes on consumption. See Castaneda & Doyle (2015) for possible political implications of this taxation pattern.

difficulty in assessing net costs, respondents treat the tax-burden as a given, and then attempt to determine how best they would prefer the funds to be spent. Holding taxation fixed, a non-beneficiary might prefer that the money be spent on programs that reduce poverty the most, or on programs that are less wasteful. Respondents are, therefore, most likely not considering the net costs of each program when evaluating them and if this is true then the Meltzer-Richard assumption cannot be applied.

This discussion highlights the fact that neither the data just presented, nor the Meltzer-Richard assumption *per se*, directly addresses the question of our interest. In order to determine how non-redistributive aspects of government transfers affect support for such transfers, it is necessary to hold fixed the net costs of the transfers. Once we do this, however, the Meltzer-Richard assumption gives us no direct prediction. Given a fixed net cost, a non-beneficiary might as well choose a policy that benefits the largest number of people, people who need it the most, or those who are geographically close to or similar to the non-beneficiary in any relevant dimension. It is this problem that we seek to address, both theoretically and empirically.

## Conditionality and the Acceptability of CCTs

There is already abundant evidence on CCTs being popular enough with beneficiaries to significantly increase the likelihood of their supporting incumbent candidates (Diaz-Cayeros, Estevez & Magaloni 2009, De La O 2013, Manacorda, Miguel & Vigorito 2011, Zucco Jr. 2013). There is less extant scholarship on the reactions of non-beneficiaries.<sup>5</sup> While CCTs have proven popular, and have contributed to the decline in inequality in recent years, in no country are CCT beneficiaries a majority of the population. The expansion and continuation of CCTs, therefore, depends on the support — or at least acquiescence — of non-beneficiaries.

But why might CCTs be more acceptable than other types of transfers? There are many reasons as to why these might be popular even among the better-off and, as such, be a particularly acceptable way to redistribute resources. CCTs typically cater only to those whose basic needs are unmet, and experimental studies have shown that need can elicit sympathy from those who are better off (Bowles & Gintis 2000). CCTs also provide small benefits to a relatively large number of beneficiaries, and this cost-benefit aspect might please those paying for the programs.

In this paper, however, we focus on one very conspicuous aspect of CCTs: these transfers are *conditional* on the behavior of beneficiaries, and as such are quite different from other forms of social assistance. Not only is there a common-sense expectation that *conditional* transfers enjoy greater

---

5. For some consideration about the electoral behavior of non-beneficiaries see Correa (2015) and Zucco Jr. (2015).

support than unconditional ones (Fiszbein et al. 2009), but previously mentioned evidence also suggests this might, in fact, be the case. In this paper, therefore, we focus on the possibility that conditionalities are what makes CCTs more acceptable to those in higher SES groups. Our aim is to establish theoretically and test empirically the mechanism through which this happens. We argue that conditionalities are a particularly relevant way of dealing with what we call the “otherness” problem.

It is to be noted that what we propose is not the same as exploring the distinction between targeted and universal transfer programs. Interesting as it might be,<sup>6</sup> such a comparison would not address our question, for it is difficult to compare a universal transfer with a targeted one while holding fixed the redistributive characteristic of such a policy. The universal versus targeted comparison could help us evaluate respondents’ preferences over redistribution more broadly (and perhaps shed light on conceptions of fairness), but such a comparison would not explain the variation between support for different instruments that involve the same amount of redistribution.

#### **Potential effects of conditionalities:**

There exists considerable debate over the economic rationale for making government benefits conditional on certain behaviors by the recipients.<sup>7</sup> Each family knows — so goes the argument — how best to allocate its own resources; reducing their freedom to choose by imposing conditions cannot

6. There are numerous viable practical, symbolic and political-economy arguments for implementing such a transfer — at least for broadening the base of targeted transfers (Prichett 2005) — but evidence from Brazil shows greater support for targeted programs over universal ones (Lavinias, Cobo, Waltenberg, Veiga & Salazar Mendez 2014). This presents an intriguing (and still unresolved) research question.

7. (For a thorough discussion on these points, see Fiszbein et al. 2009).

possibly improve a family’s position. Some purely economic arguments can be made as to how conditionalities might help overcome imperfect credit markets, under-investment in education due to positive externalities, suboptimal outcomes driven by incomplete information, time-inconsistent preferences, or divergence of interest between children and their parents. However, the strongest arguments for conditionalities are pragmatic and related to questions of political economy — that conditionalities make CCTs more palatable to those footing the bill.

Lavinias et al. (2014) found evidence of this in a dedicated, nationally representative survey of the Brazilian population conducted in 2012. A large majority of respondents supported government efforts to reduce poverty and inequality (Lavinias et al. 2014, 64), supported Bolsa Familia specifically, and declared that both child-related conditionalities and work/training requirements for adults were desirable features of social assistance programs (Lavinias et al. 2014, 70). In fact, most Brazilians opposed universal social assistance programs that provided transfers, irrespective of income, in favor of targeted and conditional programs.

Our supposition is that conditionalities affect the preferences of non-beneficiaries in two ways: 1) CCTs increase the perceived worthiness of beneficiaries, and 2) they increase direct spillover effects to non-beneficiaries. The combination of both paths leads to an unequivocal prediction: the expectation that a “conditionality premium” should exist, such that conditional transfers become more popular among the better-off than similar non-conditional transfers. But these two paths also lead to other predictions as to how the conditionality premium will vary.

### **Conditionalities and the “otherness”**

**problem:** We expect that conditionalities will not always increase support for redistribution, or at least not always at the same rate. In fact, we propose that CCTs have a more pronounced effect when non-beneficiaries regard themselves as different from beneficiaries. If true, this would suggest that conditionalities have an important effect on overcoming what we henceforth refer to as the otherness problem, and as such would be particularly useful in increasing support for transfers among those who would be most reluctant to support them. This would be true because conditionalities make those who are being helped more worthy of the assistance, and this increase in worthiness is particularly consequential when beneficiaries are seen as not very worthy by non-beneficiaries to begin with.

What we refer to as the otherness problem is simply an extension of the idea that individuals are more generous towards others who share common characteristics with them (e.g. racial, ethnic or linguistic traits). There is ample empirical evidence to support this contention (Alesina & Giuliano 2009, Fong & Luttmer 2009, Luttmer 2001). Non-economic differences between individuals can help explain individual preferences towards redistribution in the US (Gilens 2000), and are one of the explanations given for differences in levels of redistribution between the US and Europe (Alesina & Glaeser 2004). Non-economic factors also explain why inequality within countries is a more relevant issue than inequality across countries (Milanovic 2005).<sup>8</sup>

---

8. In fact, although the most intuitive mechanism at play is that the better-off are more likely to resist paying for redistribution if funds get spent on people perceived as being different from them, Shayo (2009) has shown that this could be a two-way relationship and that differences in social identities can affect support for redistribution among poorer citizens as well. In this sense, not only are the rich more likely to resist, but the poor too might be more inclined to demand redistribution when the two groups differ significantly from each other.

The ultimate psychological driver of this mechanism may lie in biased perceptions of the worthiness of beneficiaries. People are generally more willing to support “industrious” poor via charity or state redistribution policies than they are to support the “lazy” and “unworthy” poor (Fong, Bowles & Gintis 2006, Gilens 2000). An important aspect of this argument is that one tends to see members of one’s own race as more deserving (Fong & Luttmer 2009); it might not be too much of a stretch to say that this kind of bias is generalizable to other differences in a society. In other words, the more similar those being “helped” are perceived to be, the more likely is one to help them. As Milanovic states, “inequality may matter when people perceive each other as equals. . .” (Milanovic 2005, 155). In other words, people are more inclined to address inequalities among their equals.

This argument also resonates with Lieberman’s (2003) argument about the interplay of racial and regional politics. In Lieberman’s study, racial disparities across regions in Brazil undermined national level efforts at redistribution. Had redistribution been conducted among more homogeneous units, this would have generated much more buy-in, and ultimately increased compliance. Regionalism was a proxy for racial, ethnic, and linguistic differences, and though these variables often correlate, regionalism can also independently capture cultural and other non-racial attributes that define one’s identity, and, consequently, the definition of “other”. It follows, therefore, that individuals might also be less likely to support others in far away regions because the latter are perceived as being different.

Perceived differences and industriousness, however, are not the only driver of worthiness. Vulnerable populations, for instance, are probably deemed worthy of help, even if different from non-beneficiaries

and not particularly industrious. This should be the case, for instance, for conditionalities that focus on children. Such conditionalities do not guarantee that the transfer itself will benefit children, but they could have the added effect of increasing the probability that children will benefit (as they stand to benefit from the conditionality even if not from the transfer). All things equal, it is reasonable to suppose that children are seen as more worthy of help than adults.

**Spillover effects of conditionalities:** If conditionalities generate positive spillover effects for non-beneficiaries, they might also help garner greater support for the transfer. Expecting certain behaviors from beneficiaries might generate positive externalities to non-beneficiaries in excess of any externalities induced by the transfer itself. For example, requiring children to attend school might help improve human capital and reduce crime; wanting beneficiaries to undergo job-training programs might help increase the quality of the available labor force (something that benefits the community as a whole); necessitating pre-natal care for expectant mothers would reduce early age complications and save public resources; vaccinations and regular medical check-ups would reduce long-term health costs, often paid by public funds. If this is true, even strictly self-interested non-beneficiaries would likely favor conditional transfers over unconditional ones so long as the conditions provide some direct benefit to them.

If, however, this is the main driver of the conditionality premium, it should generate different empirical predictions than the otherness hypothesis. This is because non-beneficiaries should experience more externalities, to the extent that they are in close proximity to beneficiaries: the closer the beneficiaries are to non-beneficiaries,

the larger should be the conditionality premium. As distance is often a proxy for other differences, it might not be empirically possible to separate the two. Distance, in this case, should increase the conditionality premium via the worthiness path, but decrease this premium by the spillover path.

# Theory

Our theory and hypotheses are motivated and justified by an explicit behavioral model. The individual of interest  $i$  is a non-beneficiary of government transfers and can be thought of as being among the better-off in a given society. We seek to assess the preferences of this individual over different types of government transfers that have the same “redistributive content.” In other words, our goal is to describe individual  $i$ 's preferences among transfers that cost the same and imply the same amount of redistribution towards the “worse-off”, but that differ with respect to whether or not they are conditional, and what type of conditionality they impose on beneficiaries.

We begin with the simple proposition that the utility that individual  $i$  derives from a government transfer is a positive function of the worthiness of the representative beneficiary  $j$  (as perceived by individual  $i$ ) and of the spillover effects (i.e. positive externalities) generated by the transfer. We define worthiness as a function of a one-dimensional summary of differences between  $i$  and  $j$  (as perceived by  $i$ ) in any dimensions that are relevant in the given policy ( $r \geq 0$ ), as well as how industrious ( $n$ ) and vulnerable ( $v$ ) potential beneficiaries are (also as perceived by  $i$ ). Worthiness is higher when perceived differences are small; beneficiaries are perceived as industrious (i.e. high values of  $m$ ) and vulnerable (i.e. high values of  $v$ ). In order to simplify the definition of worthiness, we define merit as a combination of industriousness and vulnerability ( $m = n + v$ ), such that:

$$\text{worthiness} = -e^{r-m}.$$

Spillovers, in turn, are a function of the type of transfer  $p$ . Although many characteristics of the transfer affect the potential spillovers it generates, for our purposes spillovers are

a function of whether the transfer is conditional and the extent to which the conditionalities are stringent. Although we assume that all transfers have some level of potential spillovers  $p > 0$ , the effect of these spillovers on  $i$ 's utility is moderated by geographical distance between  $i$  and  $j$  ( $d \geq 0$ ). Spillovers, therefore, are defined as:

$$\text{spillovers} = p e^{-d}.$$

Conditionalities affect worthiness by increasing the perceived merit  $m$ , and affect the spillovers by increasing  $p$ , which means that  $m=f(c)$  and  $p=f(c)$ . These functions should be different in order to accommodate the fact that each type of conditionality may have different effects on worthiness and on spillovers. Consider, for instance, that a child-based conditionality affects worthiness by means of  $n$  (as it might lead to the beneficiaries being perceived as more industrious because they are doing something to help their children) but also by means of  $v$  (as the conditionalities expand the scope of the transfer to benefit a vulnerable population). For simplicity, however, we assume simple linear functions and replace  $p$  and  $m$  with  $c$ . Also, being agnostic about the relative weight of each mechanism, we assign generic weights ( $\beta = [0, 1]$ ) to each. This leads us to the following utility function:

$$U_i = \beta (-e^{r-c}) + (1 - \beta)c e^{-d}$$

The key expression of interest is the derivative of  $U_i$  relative to  $c$ , in which the first term is the derivative of worthiness, the second of spillovers:

$$\frac{\partial U_i}{\partial c} = \beta e^{r-c} - (\beta - 1) e^{-d}.$$

This expression is always positive, though it might not be entirely obvious at a first

glance.<sup>9</sup> This means that the conditionality premium is completely unambiguous: increasing  $c$  to impose or strengthen conditionalities on transfers always yields greater utility to non-beneficiaries, regardless of the relative weights of the two mechanisms. Moreover, conditionalities have declining marginal utility: their effects are larger when moving from a completely unconditional to a conditional transfer than from further strengthening of the conditionalities. The otherness hypothesis is also borne out by this expression. For larger values of perceived differences between  $r$ , the effects of  $c$  are larger.

While the two predictions are unambiguous theoretically, the expression highlights one possible source of empirical ambiguity. In practice, geographical distance might be strongly correlated with perceived differences. Distance can even be a proxy for other differences. In fact, while we can conceive of implementing a manipulation of perceived differences based on race, it might not be practically possible to manipulate region without manipulating other perceived differences. If perceived differences are a function of distance (i.e.  $r = f(d)$ ) and if, for instance, we manipulate  $d$  we will, in fact, be manipulating  $r$  as well. As long as  $0 < \beta < 1$ , this leads to conflicting effects on  $c$ ; there is no way of theoretically determining whether the positive or negative effect dominates. For some range of  $\beta$ , the effects of  $c$  will decrease with increases in  $d$ , and for others they will increase.

If distance is a strong indicator of differences and the worthiness mechanism is sufficiently strong, the otherness effect will dominate

9. Consider that the first term is strictly positive. As for the second term,  $e^{-d}$  is always strictly positive (and less than or equal to 1) but  $\beta - 1$  is always smaller than or equal to zero. So, if  $\beta - 1$  is zero, the second term is zero and the expression is positive. If, on the other hand,  $\beta - 1$  is negative, then the subtraction becomes an addition and the whole expression is also positive.

the spillover effects. If, on the other hand, distance is a weak indicator of other differences, and if the worthiness mechanism is weak, then the spillover effects will dominate the otherness effects. Empirically, however, we can only observe the net effects of the two mechanisms.

Translating this into a real world setting, we propose that we refer to it as the heterogeneity conjecture: in heterogeneous polities, where regional divisions overlap ethnic or other relevant cleavages, we would expect that the conditionality premium would increase with both  $r$  and  $d$  — not because the theory is ambiguous, but simply because  $d$ , in these contexts, really means  $r$ . In countries that are less regionally heterogeneous, changes in  $d$  should isolate the spillover mechanism and have a negative effect on the conditionality premium.

**The hypotheses:** The reasoning and definitions outlined in the preceding two sections inform the following hypotheses:

- **Conditionality Premium Hypothesis:** A conditionality premium exists whereby conditional transfers enjoy greater support than unconditional transfers, all else being equal. This should be true by the worthiness mechanism and/or the spillover mechanism.
- **Otherness Hypothesis:** The more different a non-beneficiary's perception of self from a potential beneficiary in any relevant political dimension, the larger will be the conditionality premium. This hypothesis should hold true by the worthiness mechanism.
- **Heterogeneity Conjecture:** If geographical distance is a proxy for other relevant political differences, the conditionality premium will be larger when beneficiaries are from a different region than non-beneficiaries. Therefore, this hypothesis

should be true in heterogeneous countries in which regional differences are politically meaningful, and false in homogeneous countries.

- Vulnerability hypothesis: Conditionalities focused on children should yield a higher premium than those focused exclusively on the original beneficiaries. This hypothesis should be true because in addition to any externality and any increase in perceived industriousness, conditionalities that benefit children also increase the vulnerability of potential beneficiaries, which, in turn, increases their perceived worthiness.

## Empirical Approach

To test the hypotheses above, we employ survey experiments. The general approach consists of polling non-beneficiaries of government transfers in order to evaluate a hypothetical transfer. We do this while manipulating experimentally the conditionalities attached to the transfer, the perceived racial/ethnic differences and/or the geographical distance between the respondent and potential beneficiaries. The presence and stringency of conditionalities can be perceived as a direct manipulation of  $c$  in the simplified model, but the differences between child-related and work-related conditionalities can also be thought of as a manipulation of  $v$ . Perceived racial/ethnic differences are the empirical correspondents of  $r$ , and geographical differences are the equivalent of  $d$  in homogeneous countries, and  $d$  and  $r$  in heterogeneous countries.

In order to examine the heterogeneity conjecture in which, in some contexts,  $d$  might be more of a proxy for  $r$  than a clean manipulation of the effects of distance, we fielded survey experiments in four different countries: Brazil, Chile, Turkey, and Uruguay. Brazil and Turkey are large upper-middle-

income countries with high geographical heterogeneity. In addition, Brazil is also racially heterogeneous while Turkey has a sizable ethnic minority. Chile and Uruguay are smaller high-income developing countries, and much less heterogeneous — both ethnically and geographically.

We designed and implemented three separate but related studies. The first was embedded in nationally representative surveys in each country, and included only one simple manipulation pertaining to the conditional nature of a hypothetical government transfer. This study focused primarily on assessing the existence of a conditionality premium, but an examination of heterogeneous effects across subgroups of respondents with different socio-economic status provides a first general assessment of the otherness hypothesis. In short, we expect to find that the conditionality premium is larger in high socio-economic status groups, which presumably see themselves as “more different” from potential beneficiaries.

The other two studies were fielded over the internet and, as such, they disproportionately recruited subjects from the relatively better-off members of each society. In both these studies we manipulated not only the conditionality associated with the hypothetical transfer, but also implemented manipulations that sought to increase the perceived racial/ethnic differences and the geographical distance between respondents and potential beneficiaries. The two studies differ only in the types of conditional transfers that were presented to respondents and in the way we manipulated the distance and differences.

# Study 1

This first study consisted of a single experimental item embedded in nationally representative surveys conducted in Brazil, Chile, Turkey, and Uruguay. The goal was to determine whether a conditionality premium exists, and whether it is larger in subgroups of “privileged” respondents (i.e. an implication of the otherness hypothesis).

## Study Design

The study employed the simplest possible experimental design, with a single response item and a single manipulation with two treatment conditions. Respondents were asked to evaluate their support for a hypothetical government transfer, which could be either unconditional or conditional, depending on the treatment group to which the subject was assigned. The question and answer options were as identically worded as possible in each language, and the value of the benefit was computed to match the average value of CCTs currently in place in each country. The English translation of the two variants of the experimental item can be seen in Box 1.

In addition to the answer to this experimental item, which was recorded on a five - point scale, we have data on individual demographic variables that we use for assessment of balance and as controls. We also have a measure of the socio-economic status of each respondent, computed by each country’s polling firm following the customary local practice,<sup>10</sup> geographic location, and, in one country, of the respondent’s race.

## Sample and Data Collection

We hired reputable local pollsters to conduct omnibus surveys that allowed for the inclusion of a single split-sample question. Surveys were conducted between November 2015 and January 2016 in all countries and sample sizes ranged between 700 to 2,002. The surveys were conducted face-to-face in Brazil, Turkey, and Uruguay, and over the phone in Chile.

In all surveys, respondents were randomly assigned to one of the two conditions. The

<sup>10</sup>. Polling companies aggregate different items to produce a SES indicator variable that is considered acceptable by local marketing and research practices.

### Box 1: Study 1 - Experimental Design

---

#### Unconditional

Imagine that the national government proposes a new social program that makes monthly payments of VALUE to each family in COUNTRY with children under 18 years of age and that is considered poor. Would you say that you completely approve, partially approve, neither approve or disapprove, partially disapprove, or completely disapprove of this new social program?

#### Conditional

Imagine that the national government proposes a new social program that makes monthly payments of VALUE to each family in COUNTRY with children under 18 years of age and that is considered poor, as long as the children make regular visits to the doctor and attend school. Would you say that you completely approve, partially approve, neither approve or disapprove, partially disapprove, or completely disapprove of this new social program?

---

Table 1 : Sample Information

	<b>Pollster</b>	<b>Field Dates</b>	<b>Total N</b>	<b>N Uncond.</b>	<b>N Cond.</b>
Brazil	Ibope	Dec 03-Dec 07	2002	998	1004
Chile	Data Voz	Dec 21-Jan 29	1004	509	495
Turkey	TNS	Dec 07-Jan 13	1512	783	729
Uruguay	Equipos Consultores	Dec 01-Dec 10	700	371	329

exact randomization mechanism varied, but in all cases it guaranteed that the probability of being assigned to each condition was the same for all participants.<sup>11</sup>

## Results

The main results of Study 1 are presented in the abbreviated Table 2. The table reports only the average treatment effects, which are measured as differences in average response of subjects in each of the two treatment groups. Positive values indicate greater support for the conditional transfer. For simplicity, we treat the five-point answer scale as a linear variable in the estimation. We report standardized results in order to facilitate a comparison of results with those of the subsequent studies.<sup>12</sup>

The first row reports results for the whole sample. These are, therefore, the average treatment effects in the survey experiment, expressed in standard deviations of the outcome variable. For each country we report estimates with and without controls for individual characteristics.<sup>13</sup> We employed

11. In all cases, the interviewers were not able to interfere in the randomization. In Uruguay, where paper-based surveys were used, we first generated a random sequence of assignments and then physically ordered the questionnaires with either treatment condition to match the random sequence. Interviewers were requested to take the questionnaires in the order they were received. In computer-based surveys, the randomization was done within the system employed, avoiding any interference by the interviewer.

12. By standardized we simply mean that the raw point estimates were divided by the standard deviation of the outcome variable.

13. The set of available control variables in Chile, Turkey Uruguay included gender, age, and a socio-economic status variable (SES) computed by each polling company. In Brazil we also included a control variable for race.

difference-in-means tests to estimate the former, and linear regression with robust standard errors for the latter.

All estimates are positive, indicating that in all countries, conditional transfers enjoy greater support than unconditional ones. Results are similar with or without controls, though we do observe some slight movement in Uruguay. Estimates, however, are small, which is particularly visible if we consider that effects range from 0.04 of a standard deviation of the outcome variable in Brazil to 0.12 in Turkey. Moreover, estimates in Brazil and Chile are not statistically significant; they are statistically significant only in Uruguay, with the inclusion of controls.

These mixed results become substantially clearer if we examine the effects of conditionalities on support for transfers only among those of higher SES. In all countries, the effects among better-off respondents range from between two and four times greater than the average effect in the population. Although effects are not huge, they now range between 0.12 and 0.36 standard deviations, and are statistically significant in all cases. The definition of better-off, it should be noted, is not exactly the same in each country, but in all cases we rely on the SES variable computed by each polling company.<sup>14</sup>

14. In Brazil (26.4%), Chile (28.8%), and Turkey (30.6%), the definition of better-off was those in classes A, B or C1, and excluded, therefore, those in classes C2, D, or E. In Uruguay (35.7%) the definition included all respondents considered as 'high', 'medium-high', and 'medium' socio-economic status, therefore excluding those in the 'low-medium', 'lower' categories.

Table 2: Average and Heterogeneous Treatment Effects — Study 1

	<b>Brazil</b>		<b>Chile</b>		<b>Turkey</b>		<b>Uruguay</b>	
All Respondents	0.038	0.043	0.092	0.085	0.118**	0.113**	0.112	0.145*
Excluding Poorest	0.067	0.071	0.132*	0.131	0.160**	0.157**	0.180*	0.208**
High SES Only	0.116*	0.117*	0.395*	0.392*	0.209**	0.213**	0.318**	0.363***
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Table reports estimates of the treatment effects of the conditional manipulation relative to the unconditional manipulation on support for transfers. Estimates are reported in standard errors of the of the response variable, which was originally measured on a five-point scale. Significance tests were computed with robust standard errors. \*\*\* and \*\* indicate, respectively, p-values < 0.1, < 0.05, and < 0.01. Results with controls include gender, age, and SES for all countries, and also race in Brazil. See text for the definition of "poorest" and "high SES only".

The results suggest that a conditionality premium does exist, but is much more pronounced among the better-off in each country. If we consider that the high-SES respondents are probably “more different” from the potential beneficiaries of the transfer than others, then these results can be read as compatible with the idea that the conditionality premium is a function of perceived differences between beneficiaries and non-beneficiaries.

The results for high-SES respondents do not appear to be driven by the simple exclusion of CCT beneficiaries. If beneficiaries (for whom conditionalities might be a burden) react negatively to the conditionalities, it could offset a plausibly positive effect on the rest of the sample. Although we cannot directly exclude beneficiaries from the study, results change very slightly relative to the full sample if we exclude only the “poorest” respondents, who make up the bulk of the CCT beneficiaries.<sup>15</sup> Treatment effects are stronger once we exclude the poorest, but tend to be closer to the weak results found in the whole sample than the strong effects found only among high-SES (Table 2). We interpret this pattern as evidence that even if we cannot rule out the fact that negative reaction to conditionalities

by beneficiaries might be partially driving the lack of results in the full sample, at least some of the heterogeneity of effects across subsamples are explicable by perceptions of differences between respondents and potential beneficiaries.

This study, however, is not without its shortcomings, mostly driven by practical limitations of including experimental items in commercial surveys. For example, we were constrained to examining the effect of only one type of conditionality, which focused on children. Moreover, although we found stronger effects looking only at the subsample of higher socio-economic status respondents, we neither measured nor manipulated these perceived differences directly. To overcome these limitations, we designed and executed another two, more sophisticated follow-up studies.

15. In Brazil, Chile, and Turkey, we considered as ‘poorest’ those in classes ‘D/E’, which corresponded to 26.4%, 30.6%, and 28.8% of respondents respectively. In Uruguay the definition included respondents considered to be of ‘low’ socio-economic status, which corresponded to 35.7% of the sample.

## Study 2

In Study 2, we expanded on the experimental design such that it would allow us to examine the effects of different types of conditionalities, and enable us to attempt to directly manipulate the level of perceived differences between respondents and potential beneficiaries. To accomplish this, we used an internet convenience sample in each country. The sample was recruited using Facebook ads that offered participants an opportunity to enter a lottery to win an iPad. Our recruiting strategy was focused primarily on the better-off in society, defined as those residing in areas of higher socio-economic status and belonging to the majority racial/ethnic groups. This recruiting method — though not statistically representative of the population — yields a far more diverse sample than what one would typically obtain using undergraduate students.

### Study Design

We employed a 3x3 factorial experimental design in which we manipulated the “perception of otherness” and the “conditionality” of a transfer. There were three “otherness” conditions: a baseline control group; one in which we highlighted the *regional* nature of poverty; and one in which we highlighted the *ethnic/racial* nature of poverty in the country. The idea was that respondents in the two treatment conditions would be led to think that the poor were different from them in some potentially meaningful dimension. In addition to the *unconditional* and *conditional* hypothetical cash transfer described in Study 1, the “conditionality” manipulations included an additional hypothetical conditional transfer that required beneficiaries to attend job training and domestic budgeting courses. No deception was used; all information presented in these manipulations was authentic.

The nine conditions were produced by sequentially manipulating “otherness” and the “conditionality”. These experimental items were embedded in a short survey that started with background demographic questions (age, gender, region, racial group), questions regarding household items, and a standard question on attitudes toward redistribution. We also included a statement that there would be an attention check among the subsequent questions that could increase the respondents’ chances in the lottery.

After the initial background items, respondents were asked two questions on their knowledge of basic facts about poverty and inequality in the country. Those in the control group received no further questions on the topic. The rest received a variant of either *racial* or *regional* manipulation of otherness, as seen in Box 2.

These manipulations sought to increase the perceived differences between respondents and “the poor”, who would be the hypothetical beneficiaries of the cash-transfer program presented for their evaluation in the conditionality manipulation question that followed later in the survey. The answer options to each of these questions were “yes,” “no”, or “don’t know”, but the answers per se were not relevant to our study. We simply opted to present our “primes” as questions so as to not overtly give away the intention of the study.<sup>16</sup>

The conditionality manipulations were straightforward. Respondents were presented with either a conditional or non-conditional hypothetical cash transfer program and asked about their levels of approval on a seven-point scale. The response to this question is the outcome of interest in the study. The three scenarios can be seen in Box 3.

---

16. The actual wording varied from country to country, but deviated very little from the text presented here.

### Box 2: Study 2 - Experimental Design

<b>Baseline 1</b>	Have you read or seen reports in the media recently regarding the following fact? The share of GENTILIC considered poor fell considerably during the last decade.
<b>Baseline 2</b>	Thinking again about media coverage on poverty, have you read or seen reports in the media recently regarding the following fact? According to recent reports, approximately X% of the COUNTRY'S population is still considered poor.
<b>Control</b>	No further questions
<b>Racial/Ethnic</b>	Still thinking about media coverage on poverty, have you read or seen reports in the media recently regarding the following fact? Poverty is a greater problem for GENTILIC that self-identify as UNDERPRIVILEGED ETHNIC/RACIAL GROUP, among whom X% are considered poor.
<b>Regional</b>	Still thinking about media coverage on poverty, have you read or seen reports in the media recently regarding the following fact? Poverty is a greater problem in the UNDERPRIVILEGED REGION, where more than X% of the population is considered poor.

### Box 3: Study 2 - Conditionality Question

<b>Unconditional</b>	The Government is studying a new social program that will pay benefits of about AMOUNT to all verifiably poor people in the country. Do you approve or disapprove of such a proposal?
<b>Conditional-Child</b>	The Federal Government is studying a new social program that will pay benefits of about AMOUNT to all verifiably poor people in the country, as long as they meet a series of conditions such as sending their children to school and taking them on regular visits to the doctor. Do you approve or disapprove of such a proposal?
<b>Conditional-Work</b>	The Government is studying a new social program that will pay benefits of about AMOUNT to all verifiably poor people in the country, as long as they meet a series of conditions such as participate in job training and domestic financial management programs. Do you approve or disapprove of such a proposal?

Immediately after the conditionality question, respondents were subjected to an attention check. The survey concluded with a few additional filler questions and collection of information for the lottery.

## Sample and Data Collection

In each of our countries of interest, we placed a series of Facebook ads for a chance to win an iPad in exchange for completing an academic survey. Facebook users who clicked on the ads were sent to a page on the survey platform Qualtrics which contained the “informed consent form”. Those who accepted the terms of the study were subsequently referred to the survey itself. Facebook ads allow for a few characters of text and one image. We experimented with three different texts with different images for each ad.<sup>17</sup>

We were primarily interested in individuals who could be considered “advantaged” in both racial/ethnic and regional dimensions. We sought to recruit subjects with these characteristics by employing two different strategies simultaneously. First, we employed custom targeting of Facebook ads, focusing only on adult users with at least a high-school diploma and who lived in the “advantaged regions” of each country. Once in Qualtrics, we employed regional and ethnic self-identification questions early in the survey. Respondents who were flagged as being from a disadvantaged ethnic or racial group, or from a disadvantaged region were excluded from the main part of the study.<sup>18</sup>

17. In all countries except Brazil, the most effective ad was one that used the country’s flag as the image.

18. We also excluded those who declared living abroad and/or being younger than 18 years of age. In order to identify respondents who were “advantaged” in both dimensions, we based the Facebook geographical targeting and our internal vetting of respondents on the following definition of disadvantaged regions: North and Northeastern Brazil; Northern departments in Uruguay (Artigas, Tacuarembó, Rivera); La Araucanía and Los Ríos (Reg IX, XIV) in Chile; Southeast Anatolia, East Anatolia, and some regions of

Eligible respondents were then assigned to one of nine treatment conditions in the expanded design, using Qualtrics’ random number generator.

We further restricted the sample being analyzed to valid respondents who also passed the attention screener and who were not CCT beneficiaries. The logic here was that as our manipulations provide respondents with different information, they will only have an effect if the information is minimally processed. While we do not examine whether respondents processed the information they received, we take it that paying attention to a subsequent question (i.e. the attention check) is a proxy for having paid attention to a previous item (i.e. the manipulation).

Total sample sizes, sample sizes after eliminating those who did not pass the attention screener, as well as data collection dates for each country are reported in Table 3.<sup>19</sup>

## Results for the Conditionality Premium Hypothesis

The research design allows us to explore the main effects of each manipulation as well as the interaction effects. We begin with the main effects, which allows us to examine the conditionality premium and to compare results with those obtained for the conditionality premium in Study 1. We focus only on the main effects of the conditionality manipulation, as we made no prediction of the main effects of the otherness manipulation. We subsequently turn to the

---

Central Anatolia and Black Sea. Similarly, we defined disadvantaged ethnic/racial groups for use in our internal vetting as being comprised of all non-white and non-Asian in Brazil; all non-white in Uruguay; all non-white and non-Asian in Chile; all those whose mother language was not Turkish, English, French, or German in Turkey.

19. Final sample sizes for Chile were lower because we obtained a smaller than anticipated share of “advantaged” respondents, and a smaller than expected share of attentive respondents. Chile being a more homogeneous country than Brazil and Turkey, we had mistakenly expected the share of usable responses to be closer to that of Uruguay.

Table 3: Summary Sample Statistics

	Field Dates		Total	Sample Size
	Start	End		Valid (Advantaged & Attentive)
Brazil	2015-09-17	2015-10-08	1168	317
Chile	2015-03-23	2015-06-23	917	194
Turkey	2015-05-14	2015-06-29	1140	394
Uruguay	2015-06-08	2015-06-10	988	400

Note: Valid responses include only adult non-beneficiaries of CCTs from advantaged regions and race/ethnicity who passed the attention screener.

interaction effects, which are a test of the otherness hypothesis.

Figure 3 depicts graphically the main effects of the conditionality manipulations. The child-related conditionality produced statistically significant increases in support for the transfer in all countries. These effects ranged from 0.5 to 1 standard deviations of the outcome variable, which is larger than what we found in Study 1.<sup>20</sup> We expected larger effects in Study 2, not only because it is a sample of internet users, but also because our recruitment method focused on respondents who were better-off. Such a sample is likely to be quite different from potential CCT beneficiaries, and our own otherness hypothesis leads us to expect larger conditionality effects in this sample than in a sample that is more representative of the population as a whole. As such, the larger conditionality premium in Study 2, relative to Study 1 corroborates the otherness hypothesis.

The work/training conditionality produced significant increases in support for the transfer in Brazil and Uruguay, similar to the effects of the child-related conditionality. In Chile, the effect of the work/training conditionality was less compared to the child-related conditionality, with only

20. Recall that the outcome variables in the two studies were measured in different scales, making it difficult to compare raw estimates. Here we focus on standardized effects.

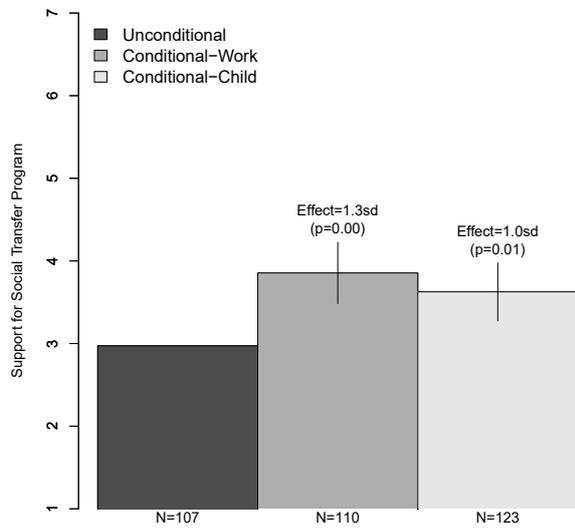
borderline statistical significance. In Turkey, the result was extremely unusual in that the work/training conditionality generated no effect. This particular result was such an unexpected outlier (both across countries and with respect to all other results in this paper) that we return to it in detail in Section 8.

## Results for the Otherness Hypothesis

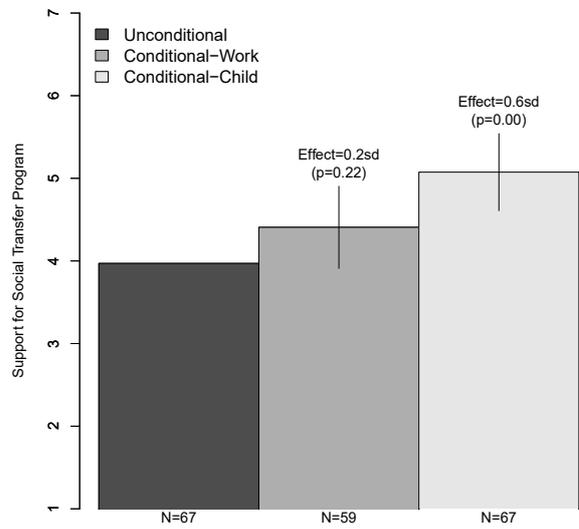
We assess the otherness hypothesis by examining the interaction effects of the two manipulations in the experiment. For ease of presentation, we examine the effects of each of the two conditionalities on approval of cash transfers separately. We present two sets of figures, one for the child-related and another for the work/training conditionality. Each set contains one figure for each country, with three clusters of two columns. Within each cluster, the two columns represent support for the unconditional and the conditional transfer.

The otherness manipulation is represented by the three clusters, the central one representing the “control” and each “otherness” manipulation situated on either side of the control group (racial/ethnic to the left and regional to the right). Confidence intervals of the estimates are shown for the conditionality treatments; the difference between the lighter-colored bars and the darker bars

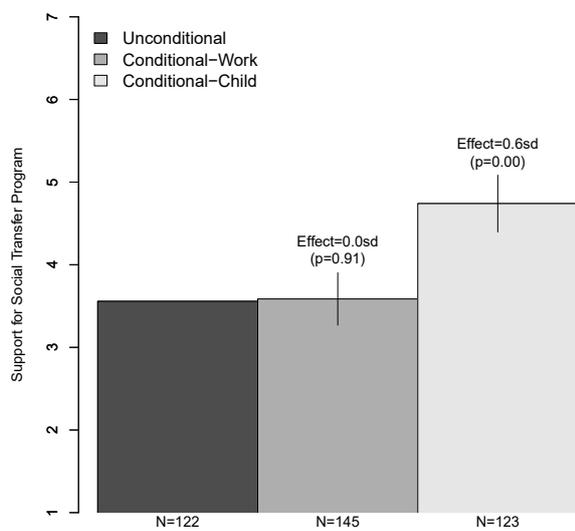
Figure 3: Main Effects of Conditionality Manipulations — Study 2



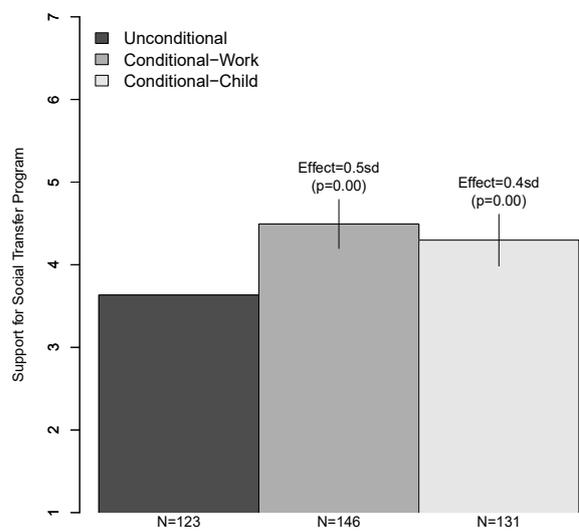
(a) Brazil



(b) Chile



(c) Turkey



(d) Uruguay

adjacent to them represents the conditionality premium.

The otherness hypothesis leads us to expect that the conditionality premium be larger in the racial manipulation than in the control group. In other words, the difference between the lighter and darker bars in the extreme left cluster should be greater than in the central cluster.

To the extent to which regionalism is a proxy for other relevant political differences, we also expect the conditionality premium to be higher in the regional manipulation than in the control group. In contrast, if regionalism is completely unrelated to other differences, then the difference between the two bars should be smaller in the regional manipulation than in the control group. This because of weaker positive spillover effects of conditionalities when beneficiaries are geographically distant. In practice, following our heterogeneity conjecture, we expect that in countries with extreme regional disparities (Turkey and Brazil) the conditionality premium will be higher in the regional manipulation than in the control group, and that in Uruguay and Chile — two countries that are more homogeneous — there should be no such difference.

**Child-related Conditionalities:** Figure 4 reports results for child-related conditionality. We observe a positive conditionality premium in all countries for all otherness manipulations, reinforcing the results reported in the previous section.

Both in Brazil and Turkey we find conditionality premiums that are higher or equal to the conditionality premium in the control group. Results are robust in Turkey, where the premiums are about twice as high for those who received the otherness manipulation than for respondents in the control group. In Brazil, we observe a very

substantial increase in the premiums in the regional manipulation, but no change in the racial one.

Because positive spillovers could offset increases in the effect of conditionalities under the regional manipulation, we conclude that either the spillover effects of conditionalities are not relevant for non-beneficiaries, or that the otherness effect through region is much greater than any possible spillovers. This would suggest that region might be a very strong proxy for other relevant political differences.

In Chile and Uruguay, conditionality premiums are not larger for those in the otherness treatment groups. In fact, in Uruguay (the most homogeneous country in the study) effects are outright smaller, and much smaller in the case of the regional manipulation. This supports the heterogeneity conjecture, and points to a situation in which positive spillovers exist and other politically-relevant differences do not overlap with regionalism.

**Work/Training Conditionalities:** Figure 5 gives results for comparison between an unconditional transfer and a hypothetical one, requiring beneficiaries to attend work/training and household budgeting courses. Results are presented in the same format as in the preceding section.

As observed with child-related conditionalities, the conditionality premium for Brazil and Turkey — for those in the otherness manipulations — is either higher or equal to what we observe in the control group, whereas this is not the case for Chile and, even more clearly, in Uruguay. Results, therefore, support both the otherness hypothesis and the heterogeneity conjecture.

However, results for Turkey are anomalous, with respondents in the otherness control group actually showing preference for the

unconditional transfer over the work/training conditionality. This was the only instance among all the variations of the experiments we fielded for which we observed such a result. The outcome led us to employ additional qualitative analysis which is discussed in detail in Section 8. Consequent to this negative premium, even the relatively small premiums we observe in the otherness treatment are sufficient to conform to the otherness hypothesis.

## Discussion

Despite the above anomaly, results for Turkey, Brazil and Uruguay are consistent across the two types of conditionalities. In Turkey, both the otherness manipulations led to similar increases in the premium. In Brazil, the regional manipulation in both cases led to substantially larger premiums than in the racial manipulations — something that we also found in the pilot study for the project. In Uruguay, both manipulations were associated with similar decreases in the premium. In Chile, the child-related conditionality

Figure 4: Interaction Effects of Child-Related Conditionality Manipulation — Study 2

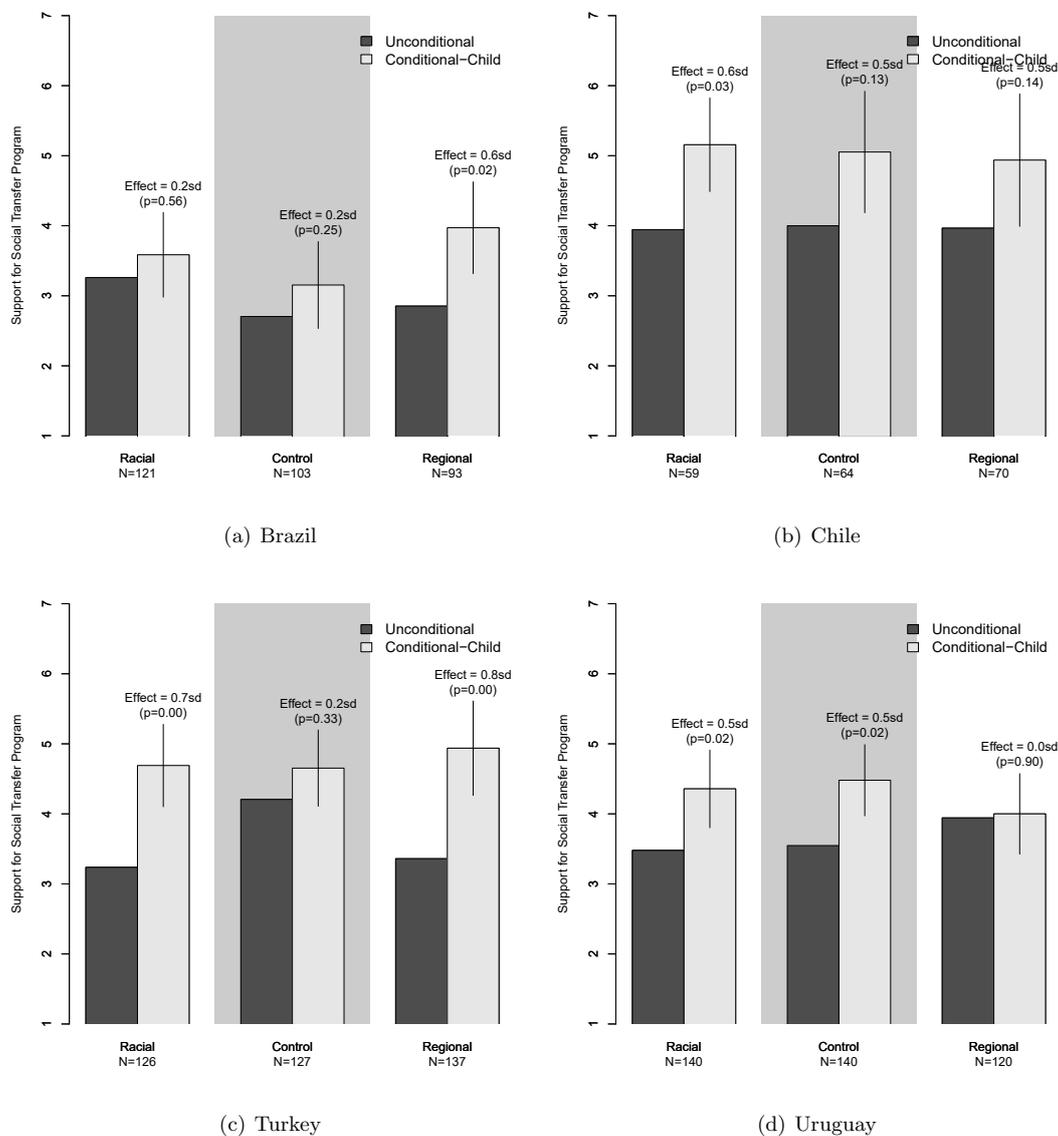
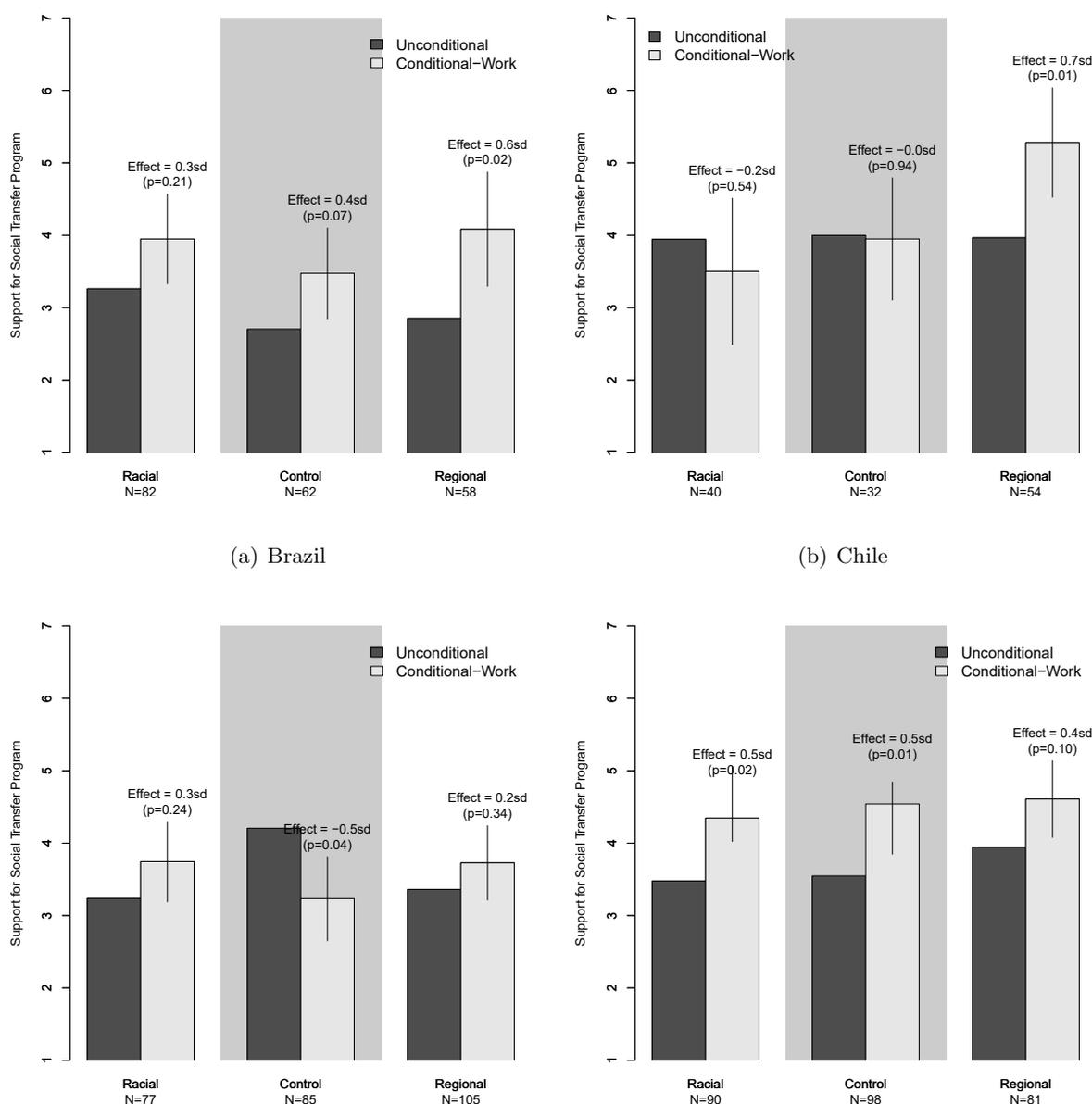


Figure 5: Interaction Effects of Work/Training Conditionality Manipulation — Study 2



essentially yielded no change in premiums, and the work/training conditionality yielded mixed results. While the regional manipulation led to a larger conditionality premium, the racial manipulation was associated with a negative premium, an outcome that we consider anomalous in the overall context of the results.

We obviously did not know — *a priori* — whether regionalism and racial/ethnic differences were equally meaningful in each

of our countries. We are inclined to read the lack of results in Chile and Uruguay as evidence compatible with our heterogeneity conjecture: racial and/or regional differences are not particularly politically relevant, or at least much less so in Chile and Uruguay than in Turkey and Brazil.

One possible drawback of the study is that we were not able to find an established way to manipulate the subjects' perception of similarity relative to potential beneficiaries

of the transfer. Our strategy of providing information highlighting some type of difference between the respondent and the “poor” is reasonable, but quite indirect. We attempted to assess perceived similarity directly by including this post-treatment item in the survey:

Box 4: Study 2 – Assessment of Perceived Similarity

**Similarity**

Based on everything you know about the people considered poor in COUNTRY, how much would you say they have in common with you?

Answers were recorded on a seven-point scale that ranged from “they have nothing in common with me” to “they have a lot in common with me”. Lower values, therefore, indicate increased perceived differences between respondents and the potential beneficiaries of the transfer.

The data in Table 4 show that the four countries line up as we would expect them to in terms of perceived differences between respondents and potential beneficiaries. Perceived similarity was highest in Uruguay, followed by Chile, Brazil, and then Turkey. Data from the manipulation check show that in seven of the eight country/conditionality cases, respondents in the otherness treatment conditions did see themselves as less similar to potential beneficiaries than those in the otherness control group, though this difference was very insignificant. Such a manipulation check is, however, difficult to implement and our assessment is based on a very open-ended question. The weak results might indicate that our manipulation was not strong enough, but could also, quite plausibly, indicate that the manipulation check in and of itself was ineffective.

To address the possibility that our otherness manipulation was not achieving the desired effect, we designed a third study, with a different manipulation of perceived similarity.

Table 4: Assessment of the Similarity Manipulation

	Average	Otherness Condition		
		Average Control	Diff. in Racial	Diff. in Regional
Brazil	4.230	4.137	-0.188	0.182
Chile	4.461	4.500	0.219	-0.074
Turkey	3.747	3.736	-0.002	-0.060
Uruguay	4.671	4.486	-0.114	0.073

## Study 3

The purpose of Study 3 was to test how sensitive our results were to the particular way in which we implemented the manipulation of the perception of similarity. As with Study 2, this study was fielded over the internet; the respondent sample was recruited using Facebook ads. The study design was similar to that of Study 2, but we only used the child-related conditionality, and manipulated the perceived similarity much more directly, as described below.

## Study Design

We employed a 3x2 experimental design, in which we manipulated the “perception of similarity” and the “conditionality” of a transfer. There were three “similarity” conditions: a baseline control group; one in which we highlighted that the transfer would benefit, disproportionately, individuals of a different race/ethnic group; and another that posited that beneficiaries lived, disproportionately, in a different region of the country. The “conditionality” manipulations included the

Box 5: Study 3 – Experimental Design

<b>Unconditional x Control</b>	The federal government is studying a new social program that will pay benefits of approximately AMOUNT to every poor person in the country.
<b>Conditional x Control</b>	The Federal Government is studying a new social program that will pay benefits of about AMOUNT to all verifiably poor people in the country, as long as they meet a series of conditions such as sending their children to school and taking them on regular visits to the doctor. Do you approve or disapprove of such a proposal?
<b>Unconditional x Region</b>	The federal government is studying a new social program that will pay benefits of approximately AMOUNT to every poor person in the country. This transfer will disproportionately benefit residents of UNDERPRIVILEGED REGION.
<b>Conditional x Region</b>	The federal government is studying a new social program that will pay benefits of approximately AMOUNT to every poor person in the country, provided the recipients meet a number of conditions, such as ensuring that children attend school and make regular visits to the doctor. This transfer will disproportionately benefit residents of UNDERPRIVILEGED REGION.
<b>Unconditional x Ethnic/Racial</b>	The federal government is studying a new social program that will pay benefits of approximately AMOUNT to every poor person in the country. This transfer will disproportionately benefit MEMBERS OF UNDERPRIVILEGED GROUP.
<b>Conditional x Ethnic/Racial</b>	The federal government is studying a new social program that will pay benefits of approximately AMOUNT to every poor person in the country, provided the recipients meet a number of conditions, such as ensuring that children attend school and make regular visits to the doctor. This transfer will disproportionately benefit MEMBERS OF UNDERPRIVILEGED GROUP.

*unconditional* and *conditional* hypothetical cash transfer in which the conditionality involved children, very similar to what was described in Study 1 and Study 2.

The experiment consisted of six conditions produced by the combination of the two experimental manipulations. Unlike Study 2, both manipulations were implemented simultaneously, in that respondents saw one of six variations of a single experimental item:

The rationale here was to employ a more direct approach to the manipulation of otherness. Instead of priming respondents to first think of themselves as different from “the poor” the manipulation in Study 3 directly informed respondents about some aspect of the identity of potential beneficiaries of the hypothetical transfer.

As in Study 2, respondents were from advantaged regions and advantaged racial/ethnic groups, ensuring that the beneficiaries in the question were always from a different group than the respondent. The experimental items were embedded in an online survey that was otherwise similar to the one used in Study 2.

## Sample and Data Collection

The data collection process, determination of valid entries, and filtering of non-attentive respondents was done using the same method as in Study 2. Sample sizes were smaller due to the smaller number of experimental conditions.

Table 5: Summary Sample Statistics (Study 3)

	Field Dates		Sample Size	
	Start	End	Total	Valid
Brazil	2015-12-11	2015-12-19	949	413
Chile	2015-11-16	2015-11-25	816	252
Turkey	2015-11-18	2015-12-04	900	265
Uruguay	2015-11-16	2015-11-18	780	403

Note: Valid responses include only adult non-beneficiaries of CCTs, from advantaged regions and race/ethnicity, who passed the attention screener.

## Results for the Conditionality Premium Hypothesis

Results for the analysis of the main effects of the conditionality manipulation are reported in Figure 6. These results corroborate the existence of the conditionality premium. The reported

standardized estimates are slightly lower for all countries, with the exception of Brazil, but are substantively similar to those found in Study 2 (reported in Figure 3).

## Results for Interaction Effects

Figure 7 reports the average support for the hypothetical transfers in all six treatment conditions in the study, by country. Careful scrutiny of these results shows that the conditionality premiums tend to be larger for those respondents who received an otherness manipulation in Turkey and Brazil, except for the essentially null result in the racial manipulation in Turkey. In Brazil and Turkey, therefore, we find a similar pattern relative to Study 2, with the conditionality premium being substantially larger for those

who received the regional manipulation, and larger or equal to the control group for those in the racial/ethnic manipulation. In Chile and

Uruguay, again, results essentially indicate no difference in the conditionality premium across otherness conditions.

Figure 6: Main Effects of Conditionality Manipulations — Study 3

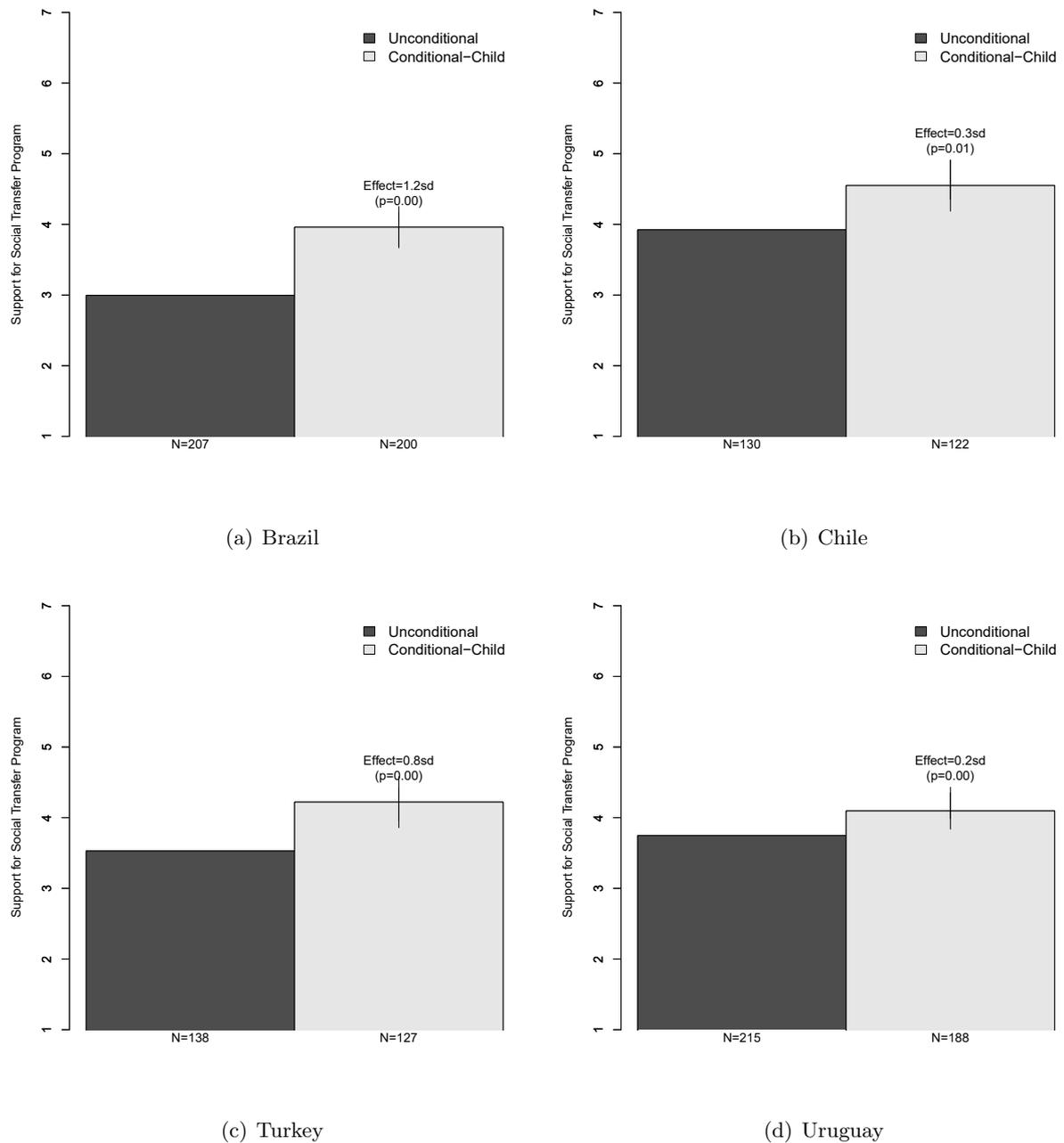
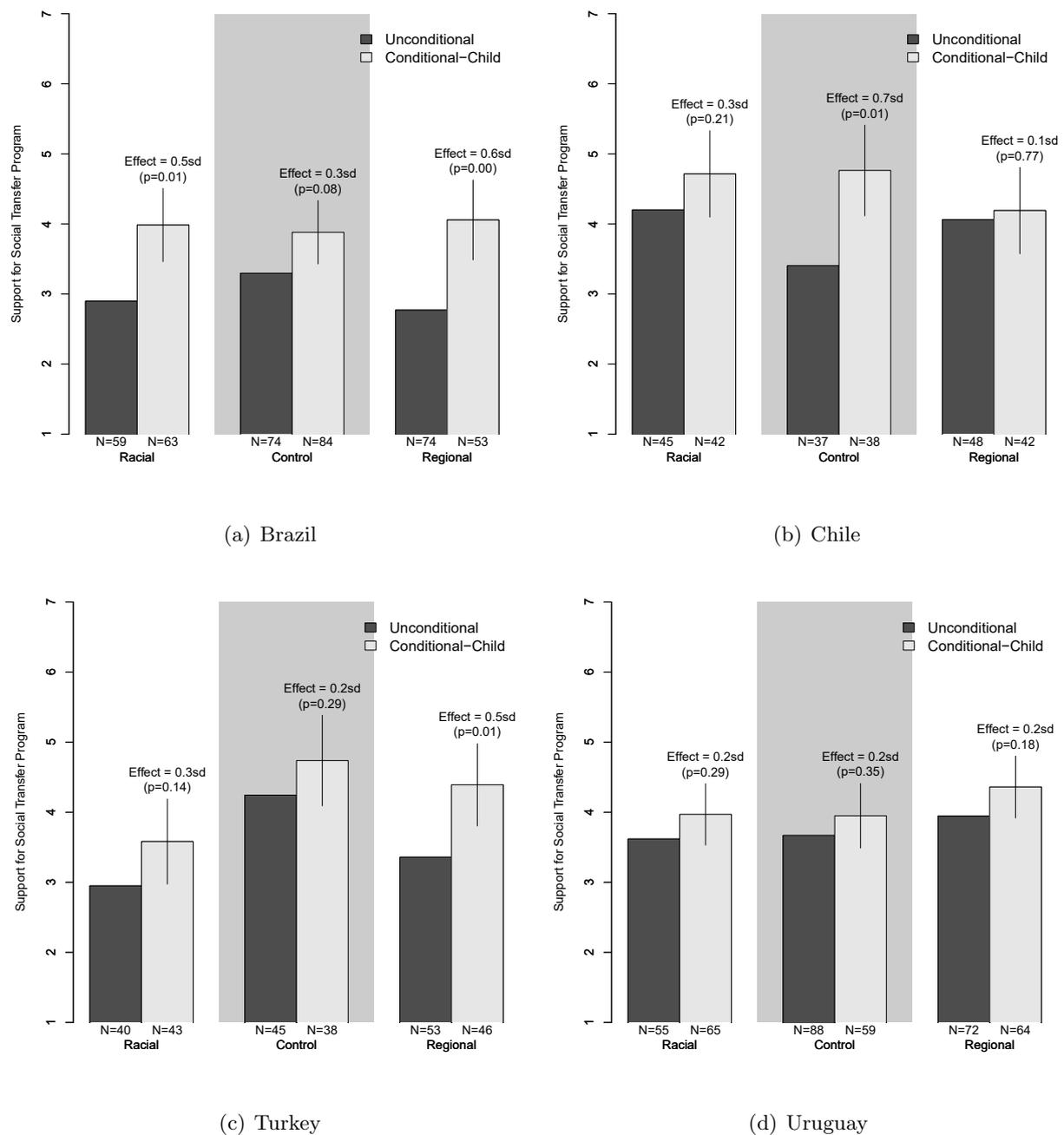


Figure 7: Interaction Effects — Study 3



## Discussion

Overall, we see that results in the heterogeneous countries tend to be stronger for the regional manipulation and this difference between Brazil and Turkey on the one hand, and Chile and Uruguay

on the other, reinforces the heterogeneity conjecture. This also suggests that the worthiness mechanism (by way of perceived differences between beneficiaries and non-beneficiaries) dominates the spillover effects mechanism, if at all it exists.

## The Turkish Anomaly

As reported in the previous sections, results for the work/training conditionality in Turkey were quite anomalous. The main effects of this conditionality (Figure 3) were essentially zero; if we focused only on respondents in the similarity manipulation control condition, they actually preferred the unconditional transfer over the work/training conditional transfer (Figure 5). Turkish results for the child-centered conditionality were in line with our expectations, and comparable to those in Brazil, so we were quite puzzled by this irregularity in the results.

Although these anomalous results were found in a single study, a pilot study fielded over the internet in Turkey yielded similar outcomes. In the pilot, with just 67 respondents, the anomalous results were even stronger than those reported in the previous sections. The main effects of the work/training conditionalities were approximately -0.64 standard deviations and in the similarity control group they were

approximately -1.13 standard deviations. These results were so surprising that we decided to modify the original design to include the child-related conditionality as well.

We sought to determine whether this anomalous result held more broadly in Turkey by examining the issue in a nationally representative survey. We replicated the simple experimental item described in Study 1, but employed a conditionality  $t$  similar to the work-related conditionality used in Study 2. This experiment was embedded in a different, nationally representative omnibus survey, unlike the one used in Study 1, fielded between 9 January to 3 February, 2016.<sup>21</sup> It was conducted by the same polling company, and with the same general sampling and sample sizes as described in Table 1, above.

In the nationally representative survey, we found a statistically significant positive

21. Due to practical constraints, we were not able to field the two experimental items simultaneously in the same omnibus survey.

### Box 6: Turkey Pilot Study – Experimental Design

#### Unconditional

Let us imagine that the national government proposes a new social program that makes monthly payments of VALUE to each family in COUNTRY with children under 18 years of age, and that is considered poor. Would you say that you completely approve, partially approve, neither approve nor disapprove, partially disapprove, or completely disapprove this new social program?

#### Conditional

Let us imagine that the national government proposes a new social program that makes monthly payments of VALUE to each family in COUNTRY with children under 18 years of age, and that is considered poor, so long as the parents attend training programs/courses to learn new work and household management skills. Would you say that you completely approve, partially approve, neither approve nor disapprove, partially disapprove, or completely disapprove this new social program?

conditionality premium for the work-based conditionality that worked out to 0.12 standard deviations of the outcome variable ( $p$ -value=0.031). Although this is a more “normal” result than we found with our internet sample, an important aspect of it suggests that there is in fact a relatively anomalous response to the work/training conditionality in Turkey. When we restrict the analysis to the population with higher SES, which according to theory and all other results should be the ones with a larger conditionality premium, we find just the opposite. Among SES respondents, the standardized conditionality premium is slightly smaller (0.1 sd) — and not statistically significant ( $p$ -value=0.234) — than the average effect of the whole sample. Results confirm the idea that there is something about the work/training conditionality that does not resonate well with the better-off in Turkey.

Given the profile of our respondents and the prevailing political situation in Turkey, we conjectured that they might be concerned with the possibility that the AKP government could use “training” courses in political and/or religious proselytizing campaigns. In order to examine the possible mechanism driving the anomaly, we conducted, in early April 2015, three focused group discussions in Istanbul. Each group was composed of seven or eight people, equally (or almost) balanced in terms of gender, all of whom were non-beneficiaries of conditional cash transfers. The majority of participants were university graduates and none declared any party affiliation. Two of the groups were composed entirely of ethnic Turks while one included three ethnic Kurds.

In all the groups respondents preferred cash transfers with child-based conditionalities over unconditional transfers, which is perfectly in line with our quantitative findings

in Turkey and elsewhere. Two of the three groups replicated the “Turkish anomaly”, in that the respondents preferred unconditional cash transfers over conditional transfers which were based on job training and home economics courses.

In the group that did not replicate the anomaly, both conditionalities were preferred over unconditional transfers. The discussion in this group veered off course, into arguments about the poor being perceived as “lazy”, “not trying hard enough to find a job”, “not being thankful enough” and “greedy”. In particular, job training programs were seen as a positive addition to the transfer. Participants argued that even though transfers were small, job training programs could teach new skills, and it was mentioned that “there exist many skills, vocations that they can learn within few months”. Still, even in this group, there was a minority opinion (held by two participants out of eight) in which unconditional transfers were preferred over the job-training program conditionality transfer.

The discussion in the other two groups revealed that the main reason invoked by participants for this preference was that given that there is no guarantee of beneficiaries eventually finding a job, the effort would be wasted. In general, participants made arguments suggesting that they regarded the benefits of the hypothetical program as being very small. In this context, transportation and time requirements involved in meeting these conditionalities would be counterproductive for beneficiaries. In summary, we did not find any corroboration of our original idea that non-beneficiaries might fear that training programs could allow for indoctrination by the government, and as such, we do not have a good explanation yet as to why there is preference for unconditional transfers over work-related conditionality in Turkey, and only in Turkey.

On the other hand, the focused groups did provide support to the idea that ethnic and regional differences matter vis-à-vis preferences in social programs. While the Kurdish participant preferences were similar to those of other members of the group, their arguments regarding cash transfers included a distinctly more rights-based discourse on social protection programs. When they talked about the transfer, benefits, they used words and expressions like “equality” and “equal opportunity” more frequently. A Kurdish participant also noted that even the unconditional transfer was conditional on the beneficiary being poor. The two Kurdish participants, for instance, also explicitly recognized the ethnic sensitivities when discussing the topics, and both concurred that media attempted to stir anti-government sentiment by “representing the Kurdish people as the ones who get the most benefits.”

## Conclusions and Directions for Future Research

The debate over the long-term consequences of CCTs is ongoing, but there is widespread agreement that CCTs can be an important instrument in a wider set of social policies. CCTs have, in some instances, helped secure positive results in fighting malnutrition, in keeping children in school, and improving the lives of the neediest (Soares et al. 2006, Neri 2008, Barros et al. 2010). However, even successful programs are subject to changing political winds — a risk that is compounded by a global climate that has seriously impacted most lower-income countries, most notably with a sharp fall in commodity prices after a decade marked by sustained increases.<sup>22</sup> Sound policies have fallen, and will continue to fall to the imperatives of the governments’ budget priorities and electoral incentives. In this context, a sound, politically viable policy has greater chances of surviving. Though CCTs are popular among beneficiaries, their political viability depends on the acceptance of these by non-beneficiaries.

Most characteristics of countries and individuals that are known to affect the amount of redistribution a society will accept are immutable, or change only very slowly. Therefore, knowing that some redistributive policies might elicit greater support (and less resistance) from the better-off than others, and knowing why this is so, could prove to be potential game changers when it comes to designing poverty-reducing policies. This paper presents evidence that characteristics of the policy instrument can affect the level of support even if they have no impact on its net-redistributive cost.

---

22. See, for instance, Campello & Zucco Jr. (2016) for a discussion on the political implications of the end of the commodity supercycle.

More specifically, the paper examines whether and why the imposition of conditions on beneficiaries of transfers elicits support from non-beneficiaries. Results suggest that conditionalities in general, and child-related conditionalities in particular, tend to elicit greater support from non-beneficiaries — at least when compared to a similar non-conditional transfer. This result, however, only seems to apply to the better-off in each of the four cases we examined. It is nonetheless relevant, even if restricted to a subset of the general population, because the better-off are precisely the ones more likely to oppose government transfers.

Moreover, conditionalities also seem to mitigate the “otherness” problem, whereby individuals tend to favor redistributive policies when they view themselves as similar to the beneficiaries of such policies. This result only holds in the two more heterogeneous cases that we examined (i.e. Brazil and Turkey), but in these countries it holds for both types of conditionalities examined in Study 2, and in the different implementation of the otherness manipulation examined in Study 3. The fact that results only hold in heterogeneous societies also suggests that conditionalities might not be so relevant in more homogeneous societies. Given that enforcing conditionalities is costly, one can reasonably make the case that they should not be introduced where they are not necessary.

Another important factor to note is that both in Brazil and Turkey, the emphasis on regional disparities appears to trigger a larger conditionality premium than racial or ethnic disparities. While this particular aspect could be influenced by political correctness (to the extent that it affects racial/ethnic considerations more than regional ones) it is a strong indication that spillovers from the conditionalities are not the main mechanism driving the conditionality premiums. If

spillovers were behind people’s reaction to conditionalities, one would expect smaller conditionality effects when subjects were primed to think about regional differences than when they were primed to think about racial/ethnic differences.

The fact that perceived differences between beneficiaries and non-beneficiaries have an impact on redistribution preferences is particularly relevant, even in a broader sense. There is a growing body of literature that shows that political identities are socially constructed. When coupled with our results, we conclude that support for redistributive practices can be affected not only by the design of the transfers but that divisive political discourse can be particularly detrimental to redistribution efforts.

## References

- Acemoglu, Daron, and James A. Robinson. *Economic Origins of Dictatorship and Democracy*. Cambridge University Press, 2006.
- Alesina, Alberto, and Edward Ludwig Glaeser. *Fighting poverty in the US and Europe: A world of difference*. Oxford University Press, 2004.
- Alesina, Alberto F., and Paola Giuliano. "Preferences for Redistribution." *National Bureau of Economic Research Working Paper Series (2009)*: 14825.
- Amaral, Carlos, and Silvia Ramos. "Programas de renda mínima e bolsa-escola: panorama atual e perspectivas." *Interface* 1 (1999).
- Ames, Barry, Fabiana Machado, Lucio Renno, David Samuels, Amy Smith, and Cesar Zucco. *Brazilian Electoral Panel Survey*. IADB Working Paper, 2010.
- Ardanaz, Martin., and Carlos Scartascini. "Why Don't We Tax the Rich? Inequality, Legislative Malapportionment, and Personal Income Taxation Around the World." *IDB Working Paper Series (2011)*:282
- Barros, Ricardo., Mirela de Carvalho, Samuel Franco and Rosane Mendonca. "Markets, the state, and the dynamics of inequality in Brazil." *In Declining inequality in Latin America: A decade of progress?*, edited by Luis F. Lopez-Calva and Nora Lustig. Washington, D.C.: Brookings Institution Press, 2010.
- Boix, Carles. *Democracy and redistribution*. Cambridge University Press, 2003.
- Bowles, Samuel and Herbert Gintis. "Reciprocity, Self-Interest, and the Welfare State." *Journal of Political Economy* 26 (2000): 33-53.
- Campello, Daniela, and Cesar Zucco Jr. "Presidential Success and the World Economy." *The Journal of Politics* 78, no. 2 (2016): 589-602.
- Castaneda, Néstor, and David Doyle. "Redistribution and taxation: how business and informal labor shape partisan tax strategies?." *Unpublished Manuscript, University of Oxford* (2015).
- Corrêa, Diego Sanches. "Os custos eleitorais do Bolsa Família: reavaliando seu impacto sobre a eleição presidencial de 2006." *Opinião Pública* 21, no. 3 (2015): 514-534.
- De La, O., and L. Ana. "Do conditional cash transfers affect electoral behavior? Evidence from a randomized experiment in Mexico." *American Journal of Political Science* 57, no. 1 (2013): 1-14.
- Diaz-Cayeros, Alberto., Federico Estevez and Beatriz Magaloni. "Welfare Benefits, Canvassing and Campaign Handouts." *In Consolidating Mexico's Democracy, the 2006 presidential campaign in comparative perspective*, edited by Jorge Dominguez, Chappell Lawson and Alejandro Moreno, 229-245. Baltimore, Md: Johns Hopkins University Press, 2009.
- Dion, Michelle L., and Vicki Birchfield. "Economic Development, Income Inequality, and Preferences for Redistribution1." *International Studies Quarterly* 54, no. 2 (2010): 315-334.
- ECLAC. *Social Panorama of Latin America 2005*. Santiago: ECLAC, 2005
- Fiszbein, Ariel, and Norbert R. Schady. *Conditional cash transfers: reducing present and future poverty*. World Bank Publications, 2009.
- Fong, Christina M., and Erzo FP Luttmer. "Do race and fairness matter in generosity? Evidence from a nationally representative charity experiment." *National Bureau of*

*Economic Research Working Paper Series*, (2009): 15064.

Fong, Christina M., Samuel Bowles and Herbert Gintis. "Strong reciprocity and the welfare state." In *Handbook of the Economics of Giving, Altruism and Reciprocity: Applications*. Vol. 2., edited by Serge-Christophe Kolm and Jean Mercier Ythier, 1439-1464. Elsevier, 2006.

Gilens, Martin. *Why Americans Hate Welfare: Race, Media, and the Politics of Antipoverty Policy*. Chicago: University of Chicago Press, 2000.

Huber, Evelyne, and John D. Stephens. *Democracy and the left: social policy and inequality in Latin America*. University of Chicago Press, 2012.

Jusko, Karen Long. "The Electoral Foundations of Redistributive Politics." In *Conference on Redistribution, Public Goods Political Market Failures*, Yale University. 2010.

Kaufman, Robert R. "The political effects of inequality in Latin America: some inconvenient facts." *Comparative Politics* 41, no. 3 (2009): 359-379.

Kenworthy, Lane, and Jonas Pontusson. "Rising inequality and the politics of redistribution in affluent countries." *Perspectives on Politics* 3, no. 03 (2005): 449-471.

Lavinas, Lena, Barbara Cobo, Fabio Waltenberg, Alinne Veiga, and Yasmin Salazar Méndez. "Percepções sobre desigualdade e pobreza: o que pensam os brasileiros da política social." *Rio de Janeiro: Centro Internacional Celso Furtado de políticas públicas para o desenvolvimento* (2014).

Lieberman, Evan S. *Race and regionalism in the politics of taxation in Brazil and South Africa*. Cambridge University Press, 2003.

Lindert, Kathy, Emmanuel Skoufias, and Joseph Shapiro. "Redistributing income to the poor and the rich: Public transfers in Latin America and the Caribbean." *Social Safety Nets Primer Series* (2006).

Luttmer, Erzo FP. "Group loyalty and the taste for redistribution." *Journal of political Economy* 109, no. 3 (2001): 500-528.

Manacorda, Marco, Edward Miguel, and Andrea Vigorito. "Government transfers and political support." *American Economic Journal: Applied Economics* 3, no. 3 (2011): 1-28.

Mares, Isabela. *The politics of social risk: Business and welfare state development*. Cambridge University Press, 2003.

Meltzer, Allan H., and Scott F. Richard. "A rational theory of the size of government." *Journal of political Economy* 89, no. 5 (1981): 914-927.

Milanović, Branko. *Worlds Apart: Measuring International and Global Inequality*. Princeton University Press, 2005.

Milanovic, Branko. "The median-voter hypothesis, income inequality, and income redistribution: an empirical test with the required data." *European Journal of Political Economy* 16, no. 3 (2000): 367-410.

Neri, Marcelo. *Income policies, income distribution, and the distribution of opportunities in Brazil*. No. 678. FGV/EPGE Escola Brasileira de Economia e Finanças, Getulio Vargas Foundation (Brazil), 2008.

Pontusson, Jonas. *Inequality and prosperity: Social Europe vs. liberal America*. Cornell University Press, 2005.

Pritchett, Lant. *The political economy of targeted safety nets*. No. 31498. The World Bank, 2005.

Przeworski, Adam. "Self-government in our times." *Annual Review of Political Science* 12 (2010): 71-92.

Secretaria de Política Econômica. "Orçamento Social do Governo Federal, 2001-04." An official Publication of Brazil's Ministério da Fazenda, 2005.

Shayo, Moses. "A model of social identity with an application to political economy: Nation, class, and redistribution." *American Political Science Review* 103, no. 02 (2009): 147-174.

Soares, Fabio Veras, Sergei Soares, Marcelo Medeiros and Rafael Guerreiro Osorio. "Cash Transfer Programs in Brazil: Impacts on Inequality and Poverty." *UNDP International Poverty Center Working Paper Series*, (2006): 21.

World Bank. Brazil: An Assessment of the Bolsa Escola Program. Technical Report 20208-BR Latin America and the Caribbean Regional Office. Washington, D.C.: World Bank, 2001.

Zucco, Cesar. "When payouts pay off: Conditional cash transfers and voting behavior in Brazil 2002–10." *American Journal of Political Science* 57, no. 4 (2013): 810-822.

Zucco Jr, Cesar. "The Impacts of Conditional Cash Transfers in Four Presidential Elections (2002–2014)." *Brazilian Political Science Review* 9, no. 1 (2015): 135-149.

## **The Global Development Network**

The Global Development Network (GDN) is a public international organization that supports high quality, policy-oriented, social science research in developing and transition countries to promote better lives. It supports researchers with financial resources, global networking, and access to information, training, peer review and mentoring. GDN acts on the premise that better research leads to more informed policies and better, more inclusive development. Through its global platform, GDN connects social science researchers with policymakers and development stakeholders across the world. Founded in 1999, GDN is currently headquartered in New Delhi.

---

This research was funded by GDN, as part of a Global Research Competition. The views in the paper are not necessarily those of GDN.

The Global Research Competition is a competitive grant program that provides early-career social science researchers from developing and transition with a platform to investigate international development challenges through collaborative cross-country or inter-regional research projects.



[www.gdn.int](http://www.gdn.int)

**NEW DELHI**

HEAD OFFICE

Global Development Network  
2nd Floor, West Wing, ISID Complex  
4, Vasant Kunj Institutional Area  
New Delhi-110070, INDIA

T: +91 11 4323 9494 / 2613 9494

F: +91 11 2613 6893