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

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Abstract
This paper examines whether characteristics 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 conditional government transfers among non-beneficiaries. In these 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 of government transfers. We seek to determine whether a “conditionality premium” exists among non-beneficiaries, and whether it is affected by how similar non-beneficiaries perceive themselves 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 are primed to think of themselves as being different from beneficiaries in non-economic dimensions. This is only the case in the two more heterogenous countries that we examine (Brazil and Turkey). The fact that some results only seem to hold in heterogenous societies suggests that conditionals might be more necessary in such societies.

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How do characteristics of government transfers affect the levels of support for these programs? In particular, can ostensively non-redistributive aspects of government transfers affect how the policies are evaluated by the population? Most theoretical approaches and empirical analysis 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 regarding redistributive policies. In this paper, we focus on the fact that individuals assessments of government transfers vary with non-redistributive aspects of these policies. This implies 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 which seek to incentivize usage of public services by paying poor families with children to do so (Fiszbein, Schady, Ferreira, Grosh, Kelleher, Olinto & Skoufia 2009). 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 large scale and highly visible national-level policies. Mexico’s Oportunidades/Progresa and Brazil’s Bolsa Família Program (BFP) are the largest of these, but by no means are they the only examples. CCTs are present in dozens of countries around the world.

CCTs can be highly redistributive (in the sense that they benefit the very poor) but 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 possible mechanisms could account for this fact. 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 therefore, less likely to suffer from leakage or corruption than other transfers. Although the better off should be generally opposed to redistribution because they have to bear its costs, they will also arguably prefer more efficient transfers (i.e. those that achieve most redistribution with the resources are actually spent) over less efficient ones.

In this paper, however, we focus on whether CCTs are popular among non-beneficiaries because they are conditional. The idea that conditional transfers enjoy more support than non-conditional is straightforward to the point of being often 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 average, conditional benefits do enjoy greater support but that this “conditionality premium” is concentrated on those with a higher SES.

We explore the idea that the conditionality premium is derived from the “otherness” problem, which is the well-documented tendency if individuals to resist redistribution towards beneficiaries perceived as different from them. We examine this possibility through survey-experiments carried out in four different countries: Brazil, Chile, Turkey, and Uruguay. In the more heterogenous countries we examine, we find that the conditionality premium is, in fact, larger when 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 heterogenous societies.

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

1 Conditionality and the Acceptability of CCTs

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). 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 poorer counterparts, other individual characteristics should not be discounted. Race and gender consistently affect support for redistribution, as do one’s views about the causes of poverty and personal experiences with economic volatility, misfortune, and social mobility. Recent work examining the developing world has led to further questioning of the Meltzer-Richard assumption of economic self-interest and its applicability in all policy contexts. There is little

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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 (Arдан & 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).
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 surveys 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. If we want to understand whether and how non-redistributive aspects of transfers — such as whether they are conditional or not — affect support for them, the Meltzer-Richard assumption is not of much help.

There is already copious evidence that CCTs are popular enough with beneficiaries to significantly increase their likelihood of 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. 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, depend on the

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3In 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).

4For some consideration about the electoral behavior of non-beneficiaries see Correa (2015) and Zucco Jr. (2015).
support—or at least acquiescence—of non-beneficiaries.

But why might CCTs be more acceptable than other types of transfers? There are many reasons why CCTs might be popular even among the better-off and, as such, 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 better-off (Bowles & Gintis 2000). CCTs also provide small benefits to relative large number of beneficiaries, and this cost-benefit aspect might please those paying for the programs.

In this paper 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 more support than unconditional ones (Fiszbein et al. 2009), but previously mentioned evidence suggests this is might, in fact, be the case. In this paper, therefore, we focus on the possibility that conditionalities are what make CCTs more acceptable to those in higher SES groups, and attempt 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.

Note that what we propose is not the same as exploring the distinction between targeted and universal transfer programs. However interesting it might be such a comparison would not address our question, as it is difficult to compare a universal transfer with a targeted one while holding fixed the redistributive characteristic of such 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 imply the same amount of redistribution.

5 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 more support for targeted programs over universal ones (Lavinas, Cobo, Waltenberg, Veiga & Salazar Méndez 2014). This presents an intriguing (and still unresolved) research question.
Potential effects of conditionalities: There exists considerable debate over the economic rationale for conditioning government benefits on certain behaviors by the recipients\textsuperscript{6} Each family knows best, the argument goes, how to allocate its own resources; reducing their freedom to choose by imposing conditions cannot possibly improve a family’s position. Some purely economic arguments can be made about how conditionalities might help overcome imperfect credit markets, under-investment in education due to positive externalities, or 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: conditionalities make CCTs more palatable to those footing the bill.

Lavinas et al. (2014) found evidence for 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 (Lavinas et al. 2014, p. 64), supported Bolsa Família specifically, and declared that both child-related conditionalities and work/training requirements for adults were desirable features of social assistance programs (Lavinas et al. 2014, p. 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 have two effects on the preferences of non-beneficiaries: 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: we expect that a “conditionality premium” should exist, such that conditional transfer will be 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 larger effect when non-beneficiaries regard themselves as different from

\textsuperscript{6}(For a thorough discussion of these points, see Fiszbein et al. 2009).
beneficiaries. If true, this would suggest that conditionalities have an important effect in overcoming what we henceforth refer to as the otherness problem, and as such would be a particularly useful in increasing support for transfers among those that would be the most reluctant to support it. 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 toward others who share common characteristics (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 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 salient issue than inequality across countries (Milanovic 2005).7

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 one is to be willing to help them. As Milanovic states, “inequality may matter when people perceive each other as equals...” (Milanovic 2005, p. 155). In other words, among equals people feel more inclined to address inequalities.

This also resonates with Lieberman’s (2003) argument about the interplay of racial and regional

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7In 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 with people perceived as different than them, Shayo (2009) has shown that this could be a two-way relationship and that different social identities can affect support for redistribution among poorer citizens as well. In this sense, not only the rich are more likely to resist, but the poor might be more likely to demand redistribution when the two groups differ more from each other.
politics. In Lieberman’s study, racial disparities across regions in Brazil undermined national level efforts at redistribution. Had redistribution been conducted among more homogenous 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, by consequence, the definition of “other”. It follows, therefore, that individuals might also be less likely to support others in far away regions because they 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 conditionality that focus on children. Such conditionality does not guarantee that the transfer itself will benefit children, but it could have the added effect of increasing the probability that children will benefit (as they stand from benefit from the conditionality even if not from the transfer). All things equal, it is plausible to suppose that children are seen as more worthy of help than adults.

**Spillover effects of conditionality:** If conditionality generate positive spillover effects to non-beneficiaries, they might also induce more support for the transfer. Requiring 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. Requiring beneficiaries to undergo job-training programs might help increase the quality of the available labor force (something that benefits the community as a whole); requiring pre-natal care for expecting mothers reduces early age complications and saves public resources; requiring vaccinations and regular medical check-ups long term health costs, often paid by public funds. If true, even strictly self-interested non-beneficiaries would likely favor conditional transfers over unconditional ones as long as the conditions provide some direct benefit to them.

If this is the main driver of the conditionality premium, however, it generates 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 non-beneficiaries: the conditionality premium should be larger the closer beneficiaries are to non-beneficiaries. 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 the conditionality premium by the spillover path.

2 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 her preferences over different types of government transfers that have the same “redistributive content.” In other words, our goal is to describe this individual’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 they are conditional or not, 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 her) 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 how stringent the conditionality is. 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:

\[
\text{spillovers} = pe^{-d}.
\]

Conditionalities affect worthiness by increasing 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 the beneficiaries to be 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 \( c \). We are also agnostic about the relative weight of each mechanism, so we assign generic weights \((\beta = [0, 1])\) to each. This leads us to the following utility function:

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

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 this might not be entirely obvious at a first glance.\(^8\)

\(^8\)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.
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 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. $r = f(d)$), for instance, if 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 increase.

If distance is a strong indicator of differences and the worthiness mechanism sufficiently strong, the otherness effect will dominate the spillover effects. If distance is a weak indicator of other differences, and if the worthiness mechanism is weak, then the spillover effects 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 what we refer to as the heterogeneity conjecture: in heterogenous polities, where regional divisions overlap with 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 heterogenous, 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 by the spillover mechanism.

- **Otherness Hypothesis**: The more different a non-beneficiary sees herself from a potential beneficiary in any relevant political dimension, the larger the conditionality premium will be. This should be 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, it should be true in heterogenous countries in which regional differences are politically meaningful, and false in homogenous countries.

3 Empirical Approach

In order to test the hypotheses above, we employ survey experiments. Our 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 stringentness of conditionalities can be thought of as a direct manipulation of $c$ in the simplified model. Perceived racial/ethnic differences are the empirical correspondents of $r$, and geographical differences are the equivalent of $d$ in homogenous countries, and $d$ and $r$ in heterogenous ones.

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
heterogenous while Turkey has sizable ethnic minority. Chile and Uruguay are smaller high-income developing countries, and much less heterogenous, 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, disproportionately recruit subjects from the relatively better-off members of each society. In both of these studies we manipulated not only the conditionality associated to 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 on the way in which we manipulated distance and differences.

4 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).

4.1 Study Design

This 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 were as follows:

<table>
<thead>
<tr>
<th>Condition</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td>Unconditional</td>
<td>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?</td>
</tr>
<tr>
<td>Conditional</td>
<td>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?</td>
</tr>
</tbody>
</table>

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 socio-economic status for each respondent, computed by each country’s polling firm following the customery local practice geographic location, and, in one country, of the respondents race.

### 4.2 Sample and Data Collection

We hired reputable local pollsters that conducted 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 and 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 exact randomization mechanism varied, but in all cases it guaranteed that the probability of being

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9Polling companies aggregate different items to produce a SES indicator variable that is considered acceptable by local marketing and research practices.
assigned to each condition was the same for all participants.  

4.3 Results

We present the main results of Study 1 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 treated 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.

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

All estimates are positive, indicating that conditional transfers enjoy greater support than unconditional ones, in all countries. 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 are not statistically significant in Brazil and Chile,

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

\[\text{By standardized we mean simply that we divided the raw point estimates by the standard deviation of the outcome variable.}\]

\[\text{The set of available control variables in Chile, Turkey Uruguay included gender, age, and a socio-economic status variable (SES) computed by each polling company. In Brazil we also included a control variable for race.}\]
and only statistically significant 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 with higher SES. In all countries, the effects among better-off respondents range from between two and four times greater times larger 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.

Table 2: Average and Heterogenous Treatment Effects — Study 1

<table>
<thead>
<tr>
<th></th>
<th>Brazil</th>
<th>Chile</th>
<th>Turkey</th>
<th>Uruguay</th>
</tr>
</thead>
<tbody>
<tr>
<td>All Respondents</td>
<td>0.038</td>
<td>0.043</td>
<td>0.092</td>
<td>0.085</td>
</tr>
<tr>
<td>Lowest SES</td>
<td>-0.038</td>
<td>-0.024</td>
<td>-0.075</td>
<td>-0.073</td>
</tr>
<tr>
<td>Middle SES</td>
<td>-0.016</td>
<td>-0.021</td>
<td>0.074</td>
<td>0.067</td>
</tr>
<tr>
<td>High SES</td>
<td>0.114*</td>
<td>0.114*</td>
<td>0.396*</td>
<td>0.393*</td>
</tr>
<tr>
<td>Controls</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
</tr>
</tbody>
</table>

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 exists, 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, 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.

These different results for high-SES respondents do not appear to be driven by the simple exclusion of CCT beneficiaries. It could be the case that beneficiaries (for whom conditionalities are a burden) react negatively to these conditionalities, offsetting the positive effect on the rest of the sample. Although we cannot directly exclude beneficiaries from the study, results change very

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13 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.
little relative to the full sample if we exclude only the “poorest” respondents, which should include
the bulk of CCT beneficiaries.\textsuperscript{14} Treatment effects are stronger once we exclude the poorest, but
tend to be closer to the weak results founds 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 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 shortcomings, mostly driven by practical limitations of
including experimental items in commercial surveys. For example, we were constrained to exam-
ining only the effect of one type of conditionality, which focused on children. Moreover, although
we found stronger effects looking only at the subsample of higher socio-economic status respon-
dents, we neither measured nor manipulated these perceived differences directly. To overcome these
limitations, we designed and executed two more sophisticated follow-up studies.

5 Study 2

In Study 2, we expanded on the experimental design 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 promised participants the chance 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 the areas with
higher socio-economic status and belonging to the majority racial/ethnic groups. This recruiting
method yields a sample that is much more diverse than what one would typically obtain using
undergraduate students, but which is not statistically representative of the population.

\textsuperscript{14}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 'low'
socio-economic status, which corresponded to 35.7% of the sample.
5.1 Study Design

We employed a 3×2 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. We manipulated the conditional aspect of the transfer just as we had done in Study #1, presenting an unconditional or a conditional hypothetical cash transfer described.

The six 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 warning stating that there would be an attention check among the subsequent questions that could increase 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 the racial or regional manipulation of otherness, as follows:
Baseline 1  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.

Baseline 2  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.

Control  No further questions

Racial/Ethnic  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 UNDERPRIVILIGED ETHNIC/RACIAL GROUP, among whom X% are considered poor.

Regional  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.

These manipulations sought to increase the perceived differences between respondents and “the poor”, which 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, in and of themselves, are 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.\(^{15}\)

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 were as follows:

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\(^{15}\)The actual wording varied from country to country, but deviated very little from the text presented here.
Unconditional

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?

Conditional

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 to regular visits to the doctor. 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 with the collection of information for the lottery.

5.2 Sample and Data Collection

In each of our countries of interest, we placed a series of Facebook ads that mentioned the 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 and with different images for each ad.\footnote{In all countries except Brazil, the most effective ad was one that used the country’s flag as the image.}

We were primarily interested in individuals who could be considered “advantaged” in both the 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 that 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.\footnote{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 Central Anatolia and Black Sea. Similarly, we...} 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 were not CCT beneficiaries. The logic here is that as our manipulations provide respondents with different information, they will only have an effect if the information was 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 that did not pass the attention screener, as well as data collection dates for each country, are reported in Table 3.

<table>
<thead>
<tr>
<th>Field Dates</th>
<th>Sample Size</th>
</tr>
</thead>
<tbody>
<tr>
<td>Start</td>
<td>End</td>
</tr>
<tr>
<td>Brazil</td>
<td>2015-09-17</td>
</tr>
<tr>
<td>Chile</td>
<td>2015-03-23</td>
</tr>
<tr>
<td>Turkey</td>
<td>2015-05-14</td>
</tr>
<tr>
<td>Uruguay</td>
<td>2015-06-08</td>
</tr>
</tbody>
</table>

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

5.3 Results:

We assess the otherness hypothesis by examining the interaction effects of the two manipulations in the experiment. We present 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, with the central one representing the “control” and with 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 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.

18 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. Being a more homogenous country than Brazil and Turkey, we had mistakenly expected the share of usable responses to be closer to that of Uruguay.
are shown for the conditionality treatments; the difference between the lighter-colored bars to the darker bars 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 left-most cluster should be larger 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 larger 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 due to weaker positive spillover effects of conditionalities when beneficiaries are geographically distant. In practice, following our heterogeneity conjecture, we expect that in Turkey and Brazil — two countries with extreme regional disparities — the conditionality premium will be larger in the regional manipulation than in the control group, and that in Uruguay and Chile — two more homogenous countries — there should be no such difference.

Figure 1 shows a positive conditionality premium in all countries for all otherness manipulations, reinforcing the results reported in Study #1. Both in Brazil and in Turkey we find conditionality premiums that are larger or equal to the conditionality premium in the control group. Results are stronger in Turkey, where the premiums are about twice as large for those who received the otherness manipulation than for respondents in the control group. In Brazil, we observe a very large 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 spillover effects of conditionalities are not relevant for non-beneficiaries, or that the otherness effect through region is much larger 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 homogenous country in the study) effects are outright smaller, and much smaller in the case of the regional manipulation. This supports the heterogeneity
conjecture, and suggests a situation in which positive spillovers exist and other politically-relevant differences do not overlap with regionalism.

In Turkey, both otherness manipulations led to similar increases in the conditionality premium. In Brazil, the regional manipulation led to substantially larger premiums, but the racial manipulation did not (which is something that we also found in the pilot study for the project). In Uruguay and Chile, the otherness manipulations did not increase conditionality premiums. 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.

6 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 with a sample of respondents recruited using Facebook ads.

6.1 Study Design

We employed a $3 \times 2$ experimental design very similar to Study #2, 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 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:
Figure 1: Interaction Effects of Child-Related Conditionality Manipulation — Study 2
The federal government is studying a new social program that will pay benefits of approximately AMOUNT to every poor person in the country.

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 children attend school and make regular visits to the doctor.

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.

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 children attend school and make regular visits to the doctor. This transfer will disproportionately benefit residents of UNDERPRIVILEGED REGION.

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.

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 children attend school and make regular visits to the doctor. This transfer will disproportionately benefit MEMBERS OF UNDERPRIVILEGED GROUP.

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 identify 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.
6.2 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>
<thead>
<tr>
<th>Field Dates</th>
<th>Sample Size</th>
</tr>
</thead>
<tbody>
<tr>
<td>Start</td>
<td>End</td>
</tr>
<tr>
<td>Brazil</td>
<td>2015-12-11</td>
</tr>
<tr>
<td>Chile</td>
<td>2015-11-16</td>
</tr>
<tr>
<td>Turkey</td>
<td>2015-11-18</td>
</tr>
<tr>
<td>Uruguay</td>
<td>2015-11-16</td>
</tr>
</tbody>
</table>

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

6.3 Results

Figure 2 reports the average support for the hypothetical transfers in all six treatment conditions in the study, by country. The data show relatively small effects of conditionality in the otherness control group, which is compatible with results for the previous two studies. Careful scrutiny of these results show 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. From Brazil and Turkey, therefore, we find a similar pattern relative to Study 2, with the conditionality premium being substantially larger for those that 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 indicate essentially no difference in the conditionality premium across otherness conditions.

We see that results in the heterogenous 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, reinforce the heterogeneity conjecture. This also suggests that the worthiness mechanism (by way of the perceived differences between beneficiaries and non-beneficiaries) dominates the spillover effects mechanism, if it exists at all.
Figure 2: Interaction Effects — Study 3
Figure 3: Conditionality Premium Across Otherness Conditions — Pooling Studies 2 and 3

Note: Figure shows estimates of the effects of the conditionality manipulation (i.e. the conditionality premium) and 95 percent confidence intervals for the different otherness conditions, after pooling the data from Studies 1 and 2, for all countries. Effects are reported in standard deviations of the outcome variable.

7 Discussion

Results for the existence and heterogeneity of the conditionality premium were similar in all countries in Study 1. These findings provide some evidence that a conditionality premium exists and that this premium tends to be concentrated among the better-off members of each polity.

Studies 2 and 3 corroborated this proposition that the “natural” conditionality premium is small, but showed that it increased considerably in heterogenous societies when subjects were primed to think of beneficiaries as being different from them. In order to summarize results, we pooled the data from Studies 2 and 3, and estimated the conditionality premium for the different otherness conditions (figure 3).

Pooling these two studies makes for a cogent summary of the results: the regional manipulation more than doubled the conditionality premium in in Brazil and Turkey. The ethnic/racial manipulation doubled the the conditionality premium in Turkey and produced a smaller, though still substantial, increase in Brazil. If respondents are primed to think of themselves as different
from beneficiaries, the conditionality premium is always unambiguously different than zero. In the case of the regional manipulation of otherness, the increase in the conditionality premium is also statistically significant relative to the conditionality premium in the control group.

All results based on Studies 2 and 3 seem to indicate that, in these two countries, priming regional differences has a more consistent effect on the conditionality premium than priming racial or ethnic differences. Our rationalization of this result is that, in these cases, regionalism can be a stand in for any of many politically relevant differences between groups, of which racial and ethnic differences are but one of the possible driving forces.

In Chile and Uruguay, in contrast, the otherness manipulation do not increase support for transfers. This result was expected, and conforms to out heterogeneity conjecture. What was not expected, however, was to find a larger conditionality premium in the control group in these countries than in Turkey and Brazil.

More importantly, however, given the strength of the prior expectations that conditionalities should be consequential for support, we consistently find that the conditionality premium is very small. While we acknowledge that it is not obvious how one should make this assessment, all three studies indicate that simply adding a child-related conditionality to a transfer hardly changes the levels of support we observe. In this sense, the political-economy argument about why one should condition benefits does not carry much empirical weight (however intuitive it may seem), and should not be the driving force behind the adoption of conditionalities. Caution is particularly advisable considering that conditionalities represent an added burden to potential beneficiaries, who are already among the most vulnerable segments of the population.

These results suggest, on the other hand, that conditionalities do seem to matter somewhat for certain groups of the population, especially in heterogenous socieites. Depending on the sociodemographic structure of the country, and on how different each groups sees itself from others, political opposition to cash transfers might be eased by making them conditional.
8 Conclusions and Directions for Future Research

The debate over the long-term consequences of CCTs in 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 in improving the lives of the neediest (Soares, Soares, Medeiros & Osório 2006, Neri 2008, Barros, Carvalho, Franco & Mendonça 2010). But even successful programs are subject to changing political winds, and this risk is exacerbated by a global climate that has seriously impacted most lower income countries, most notably with the sharp fall of commodity prices after one decade of sustained increases[19]. Sound policies have fallen, and will fall, to the imperatives of the governments’ budget priorities and electoral incentives. In this context, a sound policy that is also politically viable has more chances of surviving. While popular among beneficiaries, the political viability of CCTs depends on its acceptance 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 more support (and less resistance) from the better-off than others, and knowing why this is so, are 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.

More specifically, the paper examined 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 more 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. The result is 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 more when they view themselves as similar to the beneficiaries of such policies. This result only holds in the two more heterogenous 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 heterogenous societies also suggests that conditionalities might be less necessary in more homogenous 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 feature 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 primed to think about racial/ethnic differences.

The fact that perceived differences between beneficiaries and beneficiaries have an impact on preferences on redistribution is particularly relevant in an even 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.
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